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Reducing Burglary Initiative: an analysis of costs, benefits and cost effectiveness

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1. Introduction

Allocating a finite volume of government resources efficiently across competing calls on funds requires a systematic approach to measuring the costs and benefits to be expected from different projects, pilots and programmes. The cost-benefit analysis and project appraisal techniques widely used for this purpose in other fields such as transport or health are being applied increasingly in the criminal justice area. In the guidance to evaluators of Crime Reduction Programme projects Dhiri and Brand (1999) note a desire on part of both HM Treasury and the Home Office to ensure adequate assessment of both the effectiveness and the cost-effectiveness of new initiatives[1].

It is perfectly possible that the interventions offering the greatest impact on the number of crimes are not necessarily those offering the most cost effective way of using crime reduction resources. It is useful, therefore, to combine study of the *effectiveness* of interventions (how many burglaries they prevented) with an analysis of their *costs***.** Policy-makers can use such comparisons for various purposes ranging from prioritising different types of burglary reduction intervention to comparing burglary reduction measures with other (quite different) kinds of crime reduction activity or comparing the returns from spending on burglary reduction with returns from spending on education or defence or whatever.

The purpose of this report is to build on the findings of the three teams (Matrix MHA, 2002a and b and Midlands Consortium, 2002) who between them faced the daunting challenge of producing economic evaluations of the Reducing Burglary Initiative (RBI) projects. The researchers explore the evidence that has been gathered on costs and benefits across the projects, review some of the limitations of the methods used and make some suggestions about directions in which the developing methodology can be pushed forward.

This report was drafted after the three original economic evaluations had been completed. As will become clear the three evaluation consortia applied different approaches at each of the design, data collection, and analysis phases. This makes efforts to aggregate the three sets of results potentially hazardous. The researchers have made use of a dataset, compiled within the Home Office, integrating data from the studies into a single set of estimates intended to capture consistently the impact of the various projects. [2]

The focus is on the application of cost effectiveness and cost-benefit techniques to estimates of the impact or the effectiveness of interventions. Earlier research on the impact of burglary reduction interventions has concentrated primarily on the effectiveness question (Does an intervention reduce the number of burglaries?) rather than on cost-effectiveness (Which is the cheapest means of preventing a burglary?) or cost-benefit analysis (Are the benefits from burglary reduction greater than the costs of the intervention?).

There are significant limitations to what can be achieved in this study. The researchers are unable to draw strong conclusions about which type of burglary intervention is most beneficial from an economic standpoint although they do identify some project characteristics associated with more successful outcomes. The limitations are attributable to a number of factors. Variation in the composition and implementation of the interventions is part of the explanation and variation across the three evaluations in the way the impact of projects has been estimated accounts for another key part. Neither economic methodology nor the Dhiri and Brand guidelines to evaluators is responsible, in the view of the researchers, for the comparative weakness of the conclusions are drawn.

There is always likely to be some degree of variation in outcomes from projects implementing similar types of intervention, as a result of measurement error, random elements, unobserved variation in the method or style of delivery and so on. The reliability of the inferences that can

[1] Dhiri & Brand (1999), p 3

[2] The researchers comment further on the method below. They did not consult directly with the authors of the original studies in deciding to use this dataset as the basis for their effort to synthesise the work.

be drawn from an intervention (or from a group of pilot interventions) will depend to a considerable degree on whether the interventions have been designed to support such generalisations. It cannot be taken for granted that they have. The degree to which findings from an intervention can be generalised for purposes of inferring the likely impact of repeating the intervention elsewhere may in some instances be very limited. An obvious example of this is that situational crime prevention measures such as alley-gating may be cost effective in one setting (such as an area of terraced houses with back alleys) but not another. The replicability of findings will depend on how closely new interventions will follow the setting, design and implementation of the original intervention. The circumstances that made a particular project cost-effective may simply not apply when a similar intervention is implemented elsewhere.

A feature of the RBI was that it encouraged innovation and flexibility in the design of projects at local level. This approach has some attractive features but it runs exactly counter to the tightly controlled experimental design of interventions advocated by researchers seeking to maximise the robustness of statistical inferences about effectiveness[3]. It therefore inevitably inhibits building an evidence base to which practitioners and policy-makers can refer when designing burglary reduction projects or programmes.

An important complication in trying to draw inferences from RBI projects about the effectiveness of different approaches is that each individual project involved different types of intervention used in a unique, complex combination specially tailored to meet local conditions. To compound this variation, some changes were made to projects as events unfolded at the suggestion of 'project developers'. The result is that projects tend to be idiosyncratic and the programme as a whole is much less like an experiment under (even partially) controlled conditions than it might be. The researchers are therefore starting from a position in which the feasibility of drawing inferences from RBI projects about either the effectiveness or the cost-effectiveness of interventions is very much an open question.

The incorporation of cost information into the Reducing Burglary Initiative is a welcome addition to studies of effectiveness or impact, but it has been achieved at a price. Collecting and analysing the additional information required substantial effort on the part of many researchers and others involved in data collection work on the ground. The guidance given by the Home Office to evaluators (Dhiri and Brand, 1999) and the development of a Data Collection Tool (Emmanuel Solutions, 2001) to encourage and support collection of data in a consistent and uniform way for the Crime Reduction Programme (CRP) as a whole are both signs of a determination to support the assembly of as solid an evidence base as possible.

But variations in the way the guidance notes have been translated into practice have highlighted some areas of methodology where there remains scope for further development. The three teams of evaluators used different methodologies for estimating the scale of burglary reduction. They chose to focus less on some dimensions that the researchers believe to be important from a cost benefit perspective. The researchers conclude that, while the RBI has benefited from the application of benefit-cost analysis, the methodology for evaluating the economic impact of crime reduction projects is not as yet fully settled.

[3] The Randomised Control Trial approach widely used in medical research, and advocated by some researchers in the criminal justice field, represents best practice from the perspective of experimental design. But it is an approach that permits little by way of flexibility or discretion to project designers.

2. Measuring outcomes, costs and benefits

The starting point for a cost-effectiveness or cost-benefit study of a crime reduction intervention is just the same as the starting point for a study of whether the intervention 'works', in the widely accepted sense of having a significant impact in reducing crime rates; Sherman *et al*, 1997, Petrosino *et al*, 2001. If an intervention does not work it cannot be cost effective or cost beneficial since it will show no benefits. But the opposite condition does not hold. An intervention is not cost-effective simply because it 'works' in the sense of producing a reduction in burglaries. An intervention that 'works' will not normally be implemented if a cheaper way can be found of delivering the same outcome. The purpose of cost-effectiveness and cost-benefit analyses is to bring into the sharpest possible relief the relationship between the amounts alternative types of intervention cost to implement and what they deliver by way of crime reduction benefits.

Once the methods for collecting information on costs and benefits are decided it is comparatively straightforward to estimate cost-effectiveness and benefit:cost ratios. The cost-effectiveness ratio, as Dhiri and Brand (1999) outline in their guidance document, is given as the cost per burglary prevented by an intervention. If the number of burglaries expected in the absence of intervention is given by *X* and the number of burglaries when implementing intervention costing *C* is given by *Y* then the cost-effectiveness of the intervention, or the cost per unit outcome, can be estimated by:

$$\text{Cost-effectiveness ratio} = C/(X - Y).$$

This calculation does not rely on an estimate of the value of preventing a burglary. But such a measure of value (*v*) is needed to calculate a benefit:cost ratio. The benefits from burglary prevention are translated into financial terms by multiplying the estimate of the number of burglaries prevented by the average economic and social costs imposed by a burglary. The benefits from the intervention are valued at $v*(X-Y)$. Thus, the ratio of benefits to costs can be written as:

$$\text{Benefit:cost ratio} = v*(X-Y)/C.$$

Compiling such a comparison of the value of the reduction in the number of burglaries committed with the costs of RBI interventions is, however, a less straightforward matter than it sounds. Measuring the scale of the burglary reduction achieved by a project $(X-Y)$ has to confront a number of critical challenges including finding ways of estimating:

- the scale of post-evaluation burglary reduction;
- the impact on unrecorded burglaries;
- the scale of spillovers into neighbouring areas;
- the choice of a comparison area or time period.

Equally, the choice of a figure for the value per burglary prevented (*v*) is not entirely straightforward. The average economic and social costs of a burglary consist, in the standard Home Office publication on the matter, of three elements, namely: the costs in anticipation of burglary (£430), the costs as a consequence of burglary (£1,400), and the cost in response to burglary (£490): Brand and Price, 2000, Table 4.2[4]. The total of these components (£2,300) is

[4] The costs in anticipation of burglary comprise security expenditure and the cost of insurance administration. The costs as a consequence of crime consist of the value of property stolen, property damaged, property recovered, lost output, emotional impact and victim services. The costs of police activities, prosecution, magistrates' courts, crown courts, jury trials, legal aid, non-legal aid defence, probation services, prison service and other criminal justice system

a composite figure for recorded and unrecorded burglaries. The estimates of burglaries prevented by RBI projects normally refer only to the impact on recorded crime, but it is well established from comparisons between recorded burglaries and the proportion of households reporting themselves as victims of burglary incidents that there is a 'recording gap'.

For purposes of valuing the benefits from the Reducing Burglary Initiative, evaluators estimated the number of crimes that would have been expected in the absence of intervention. They took no account of the likely impact on unrecorded burglaries. Clearly it would be inappropriate both to neglect unrecorded burglaries whilst also using a 'composite' cost of burglary estimate based on a mix of unrecorded and recorded crime. The possibilities are either (a) to estimate the impact on unrecorded burglaries and apply the composite cost figure to this total or (b) to neglect unrecorded crime but to adjust the average cost figure to reflect the fact that only (more costly) recorded crime is being addressed.

The data used in this analysis are limited to the number of recorded burglaries. Estimating the impact on unrecorded burglaries based on the number of recorded burglaries (i.e. option (a) may exaggerate the impact of interventions. In order to avoid this problem, option (b) followed, whilst accepting that a non-recorded burglary will not in all cases entail lower cost than its recorded counterpart. In addition to this, from the perspective of economic evaluation, method (a) affects both the effectiveness[5] and the benefits[6] of interventions. In contrast, method (b) affects only the benefits of the interventions, without creating the risk of exaggerating the effectiveness of interventions.

Since the total number of burglaries was estimated at around 1.4 million annually (Kershaw *et al*, 2001) compared with recorded burglaries of 443,000, the number of unrecorded burglaries can be estimated at 957,000 incidents. In order to give a weighted average cost for recorded and unrecorded offences of £2,320 the average cost of a recorded burglary must be around £3,378 given that the cost of an unrecorded burglary (anticipation costs plus consequential costs but excluding justice system costs) is around £1,830.[7] The figure of £3,378 for recorded burglaries is the estimate used below in valuing the estimated benefits from reductions in recorded burglaries.

Before saying more about some of the practical issues encountered in estimating the cost-benefit and cost-effectiveness ratios for the RBI, the costs and benefits data from the RBI projects are summarised.

costs are part of the costs in response to crime. For further details regarding the estimation method for the figures above, refer to Brand and Price (2000).

[5] Measures the impact of interventions on the number of burglaries.

[6] Measures the monetary values of the impact of interventions on the number of burglaries.

[7] The likelihood that the cost differential between recorded and unrecorded burglaries is, in fact, greater than this is ignored since in a higher proportion of the unrecorded incidents there is no loss of property involved.

3. Summary of project data and reports

The projects implemented under RBI were selected from proposals received via a tendering procedure. In order to qualify for consideration for funding it was originally planned that an area needed a burglary rate twice the national average.[8] Areas were chosen on the strength of the quality of their funding proposal, and on other criteria such as ensuring balance of various kinds. Generally they covered a population of 3,000 to 5,000 households, so are relatively small.[9]

Three consortia did the economic evaluations of the RBI, each being responsible for 21 projects: Matrix MHA (2002a and b); Midlands Consortium (2002). The consortia used different approaches and had different emphases. There was no uniformity of approach either to the choice of methodology for generating counterfactual burglary estimates[10] or to the definitions of 'cost effectiveness' or 'sensitivity analysis'. Table 3.1 is intended to give a sense of some of the differences, although it does scant justice to the lengthy evaluation reports.

[8] In the event this condition was not fulfilled by some projects. Millie and Hough (this edition) reported, for example, that six out of twenty targeted areas in the Southern Consortium had burglary rates less than twice the national average.

[9] The coverage of target areas varied and some projects could not fulfil the minimum number of households in their target areas. Of the total 51 projects included in the analysis here, 15 projects covered less than 3,000 households. Millie and Hough (this edition) reported that the size of SDPs varied across projects. A Strategic Development Project (SDP) might cover the whole city, a number of police beats or, in one extreme case, a single street.

[10] The Midlands Consortium used a time-series approach while the Northern and Southern Consortia used a cross-sectional approach based on a shift-share analysis.

Table 3.1: Summary of findings reported

	Midlands	**Southern**	**Northern**
SUMMARY	The 21 SDPs are split into 3 groups on the basis of the impact of the projects on crime rates.	The 20 SDPs are split into two groups on the basis of whether the evaluators felt able to "draw conclusions about the effectiveness of burglary reduction interventions". 12 projects are eliminated on this criterion, leaving 8 for which 'meaningful analysis' is possible.	The report covers 21 SDPs each involving a number of interventions (up to 12). The projects are based in 5 Police Force Areas (PFAs).
	The number of interventions implemented across the 21 projects in total was 67 compared with a planned total of 116.	The 8 projects documented involve a total of 54 interventions, with the number of interventions per project varying between 3 and 10.	About 80% of planned interventions were implemented.
PRINCIPAL FINDINGS			
Effectiveness	Outcomes are very mixed across the projects. Group A (6 projects) generated a significant reduction in burglary. Group B (7 projects) gave a reduction in burglary not significant even at the 10% level. Group C (7 projects) generated an increase in burglary in the target area.	Shift-share analysis indicates that in 7 of the 8 areas studied burglary rates fell, with an aggregate maximum net saving in burglaries amounting to 679.	The average net reduction varied widely across projects. 3 SDPs showed a reduction of more than 30%, 4 resulted in net increases, although in only 2 of the 4 was the increase statistically significant at the 5% level.
Cost-effectiveness	Of the 6 projects (Group A) generating a significant reduction in burglary only one did so in a cost effective way: p19.	No measurement of cost per unit outcome.	Summary does not refer to the cost per burglary reduced either by project or by intervention.
	Costs per unit reduction in burglary amongst group A projects ranged from £38,306 to £1,494: Table 7.7.		
Cost-benefit ratios	N/A	Of the 7 projects with a positive impact 3 were judged to have a benefit:cost ratio exceeding one.	Taking all 21 projects the benefits from burglaries saved amounted to £2.6m at a total cost of £2.9m, giving a cost:benefit ratio of 0.89.

Cost variation	Costs are related both to the planned intensity of the intervention and variation in the quality of intervention management: p7.	Substantial variation across SDPs in the cost per unit of output, in part because of differences in the intensity of the intervention.	Cost per unit of output varied widely across interventions, although some of the variation is accounted for by differences in the intensity of intervention.
	The cost per unit of *output* (e.g. costs per gate fitted) for a particular kind of intervention varies widely across projects: p18 and Table 6.6.		Problems with the local to national mapping of the intervention classification are also blamed for artificially inflating the degree of cost variation across projects: p139.
	The cost per unit outcome (i.e. per unit burglary reduction) cannot be identified at the level of an individual type of intervention (para 3, p7).		
CHARACTERISTICS OF SUCCESSFUL PROJECTS	The successful Group (A) contained fewer planned interventions but not necessarily fewer implemented interventions: p17.	With details only available for 3 projects the report does not try to generalise about the characteristics of successful projects.	Regression analysis of the scale of burglary reduction in relation to project characteristics suggested that success seemed to be associated with: location-specific situational crime prevention measures;
	A higher proportion of resources for Group A went on interventions securing vulnerable properties: p17.		
	A lower proportion of resources for Group A went on personnel costs.		stand-alone publicity campaigns; higher degree of partnership working;
	The proportion of resources levered-in did not distinguish Group A projects: p17.		more partner agencies; located in more affluent areas.

METHODOLOGY			
Basis	Effectiveness at project level is explored in relation to various project characteristics: see pp15-17. The basis is a comparison of the group means reported in Table 2.2. Significance levels are not reported.	Quantitative examination of before and after changes within the SDP as compared with a number of different comparators plus a detailed qualitative review of the implementation of the SDP plans to assess the potential scale of impact.	The report endeavours to relate effectiveness at project level to various project characteristics. This analysis is primarily qualitative. It also contains some useful qualitative remarks on the general experience of the implementation process.
Outcome measures	Displacement and diffusion effects (as between areas and between offence types) are identified. Uses a 'buffer area' comprising police beats immediately adjacent to the target area and a category of 'other acquisitive crime': p14. Measures resulting are:	Displacement and diffusion effects (as between areas and between offence types) are identified. Uses a 'buffer area' comprising police beats surrounding the target area to measure displacement effects and a category of 'other acquisitive crime' to estimate diffusion.	Burglary rates are used as the principal outcome measure. An innovation in this report is the identification of anticipatory effects. This refers to burglary reductions experienced ahead of interventions and attributable to publicity about an imminent intervention or project.
	net burglary: total impact in both target area and buffer area;		
	project area: aggregate effects on burglary and Other Acquisitive Crime (OAC) in target area only;		
	overall net impact: effect on both burglary and OAC in both target area and buffer area.		

CHOICE OF BASELINE	Uses GARCH (econometric) methodology based on monthly observations of the number of burglaries for a two-year window (approx.) either side of the project start in mid-1999. A 'project intensity' variable is constructed to indicate how much of the programme had been implemented month-by-month.	The method used is 'shift-share analysis' based on comparisons with neighbouring districts and with related types of crime.	The reduction in burglary in an area was compared with the reduction experienced across the Police Force Area within which a project was located. This method is similar to the 'shift-share analysis' used in the Southern Consortium Report.
COMMENTS	The idea of constructing a 'Calendar of Action' for each project (outlined at p12) is, on the face of it, helpful.	The method of calculating the baseline differs from that used in the other two studies. This is a cost-benefit study rather than a cost effectiveness study.	
Sources:	Original reports: Matrix MHA (2002a and b); Midlands Consortium (2002).		

The method of classifying the interventions used in the RBI programme is an important matter because it influences the shape in which findings are reported. Virtually all projects comprised a bundle of different types of activity or 'intervention'. There is a listing of 93 different activities aggregated into a seven-way classification[11] of the principal intervention types. The 'intervention' a project represents could thus be characterised in terms of the settings selected for each of the 93 possible activities. The number of different combinations this permits is, obviously, enormous. Even when aggregating up to the six or seven-way classification there is still very great scope for variation across projects.

To give some indication of the variety of activities in the RBI projects, the Northern Consortium reports its 21 projects as including a total of 179 interventions in different combinations ranging between three and fifteen per project, with an average of 8.52 per project. The most frequently represented types of intervention in the Northern Consortium areas were target hardening of individual properties (implemented in 18 of the 21 projects) and publicity leaflets (implemented in all but 2 of the projects). The former was one of the activities in the 'location specific situational crime prevention' group of interventions whilst the latter is classified in the 'stakeholding' group.

From the analyst's perspective the key decision that has to be made is how (if at all) to aggregate across projects. One approach is to treat each project as 'unique' and to document its evolution and key features in such a way as to help inform policy-makers in the future seeking guidance as to which combinations of activity and styles of implementation seem to work best. Each project can be thought of as an entry in a database holding all the relevant details including qualitative information on implementation. This database comprises a pool of evidence that practitioners can consult when designing interventions in the future.

An approach from a rather different direction is to ask what generalisations can be made about project effectiveness. This might be framed as an effort to identify, for example, whether greater representation of one group of activities is associated with a stronger performance from a benefit:cost or cost-effectiveness perspective or whether the incidence of significant implementation difficulties is correlated with project outcome. The emphasis of the consortium reports is more at the individual project level whilst in this paper looks for more general findings.

[11] In fact there are really six main groups of interventions: the seventh category contains a project for which the principal intervention type is unknown. The groupings are listed at the foot of Table 3.2.

Table 3.2: Summary project data

SDP	Consortium*	Households in target areas	Burglaries in target area before project T(0)	Initial burglary rate per household (%)	Project cost (£'s)	Capital spending (%)	Principal Intervention**	Implementation problems	Calculation of net effect size, used in Kodz and Pease, Findings 204
Phase 1 - 2	S	6,134	794	12.94	408,792	7.45	3	some	191.12
Phase 1 - 1	S	3,952	629	15.92	42,829	33.79	6	serious	68.62
Phase 1 - 11	S	4,182	331	7.91	609,273	89.00	2	some	4.16
Phase 1 - 13	S	3,117	898	28.81	172,241	22.25	1	some	494.94
Phase 1 - 20	S	4,515	491	10.87	38,252	45.72	1	some	-0.59
Phase 1 - 16	S	2,992	778	26.00	45,640	28.35	4	serious	120.01
Phase 1 - 12	S	3,569	501	14.04	105,134	8.03	4	some	99.09
Phase 1 - 14	S	2,580	284	11.01	1,844,204	2.14	6	none	121.77
Phase 1 - 17	S	3,557	361	10.15	150,932	68.14	2	none	48.45
Phase 1 - 6	S	4,909	473	9.64	124,935	42.23	4	serious	100.48
Phase 1 - 9	S	3,076	494	16.06	63,100	50.52	2	some	28.76
Phase 1 - 19	S	3,483	82	2.35	80,483	79.25	2	none	-4.14
Phase 1 - 15	S	4,115	324	7.87	100,171	20.64	n.a.	none	42.84
Phase 1 - 38	M	3,557	86	2.42	97,172	27.16	2	some	6.79
Phase 1 - 34	M	1,443	101	7.00	49,852	27.39	2	serious	-44.91
Phase 1 - 60	M	1,595	193	12.10	98,401	51.40	1	none	54.12
Phase 1 - 31	M	2,981	598	20.06	53,140	28.02	5	none	95.83
Phase 1 - 33	M	2,371	203	8.56	71,646	31.07	1	some	69.65
Phase 1 - 47	M	4,324	103	2.38	64,302	4.90	4	some	60.61
Phase 1 - 43	M	3,302	676	20.47	173,422	42.33	1	some	-110.47
Phase 1 - 55	M	6,313	431	6.83	157,559	37.69	1	none	30.51
Phase 1 - 22	M	3,304	296	8.96	249,213	11.46	1	none	13.75
Phase 1 - 23	M	4,355	1118	25.67	65,089	33.67	1	none	-173.49
Phase 1 - 25	M	3,188	421	13.21	86,544	35.05	2	some	-5.53
Phase 1 - 41	M	4,610	740	16.05	62,301	39.57	1	none	147.94
Phase 1 - 32	M	5,771	639	11.07	86,889	34.94	2	serious	59.89
Phase 1 - 58	M	2,213	203	9.17	63,603	70.45	1	none	18.28
Phase 1 - 24	M	4,059	394	9.71	76,269	NA	1	some	57.01
Phase 1 - 30	M	4,125	382	9.26	74,483	37.57	1	none	-78.92
Phase 1 - 29	M	4,091	486	11.88	162,618	1.84	4	none	138.24
Phase 1 - 46	N	2,603	175	6.72	55,652	41.48	4	some	-6.72
Phase 1 - 53	N	1,652	314	19.01	100,749	66.51	1	none	-49.34
Phase 1 - 62	N	2,590	253	9.77	140,824	35.97	3	some	77.54
Phase 1 - 54	N	3,461	216	6.24	83,273	51.77	5	none	-19.24
Phase 1 - 50	N	2,323	190	8.18	190,153	11.94	4	some	51.22
Phase 1 - 51	N	3,053	451	14.77	219,479	17.84	4	none	-22.95
Phase 1 - 61	N	3,517	364	10.35	151,524	18.48	4	none	35.31
Phase 1 - 44	N	3,500	395	11.29	375,021	19.64	4	none	118.11
Phase 1 - 49	N	3,292	307	9.33	404,032	18.80	4	none	40.83
Phase 1 - 21	N	5,044	436	8.64	202,034	38.46	1	none	116.48
Phase 1 - 35	N	3,291	317	9.63	108,338	53.89	1	none	40.04
Phase 1 - 36	N	3,387	501	14.79	356,070	89.13	2	none	29.20
Phase 1 - 39	N	3,396	648	19.08	54,599	45.04	1	none	264.01
Phase 1 - 37	N	2,088	276	13.22	165,477	23.09	5	some	50.29
Phase 1 - 42	N	923	257	27.84	162,997	25.67	1	none	-6.84
Phase 1 - 52	N	3,503	767	21.90	142,416	20.92	1	some	172.63
Phase 1 - 40	N	3,210	356	11.09	78,953	48.94	5	none	74.99
Phase 1 - 57	N	2,271	504	22.19	243,309	17.05	4	serious	205.46
Phase 1 - 48	N	2,271	300	13.21	331,069	2.74	4	serious	73.09

Phase 1 - 45	N	3,300	1,071	32.45	256,421	4.48	4	serious	-32.66
Phase 1 - 27	N	3,309	454	13.72	196,011	19.66	5	none	45.47
Total		**173,767**	**22,062**		**£9,496,890**				
Average		**3,407.20**	**432.59**	**13.17**	**£186,214**	**20.60**			

*: S = Southern Consortium; M = Midland Consortium; N = Northern Consortium
**: Intervention 1 = Location specific situational crime prevention.
Intervention 2 = Area-wide situational crime prevention.
Intervention 3 = Stakeholding.
Intervention 4 = Enforcement.
Intervention 5 = Offender Challenge Actions.
Intervention 6 = Property.
Intervention 7 = Others.

Table 3.2 summarises some basic information on most of the projects comprising the RBI. It omits 12 of the 63 projects funded, because of gaps in the data available. Nevertheless it gives an overall view of the costs and some of the other characteristics of the projects. It identifies the 'principal intervention type' used in each project, namely the type on which the largest proportion of a project's budget was spent. This is a rather crude way of characterising an intervention or project. In a later section a more sophisticated variant is pursued in which a project is characterised by the proportion of its budget spent on each of the six main groups of activities. From a practitioner's perspective this kind of approach can be useful in narrowing down more quickly the search for an appropriate format for some future intervention.

For purposes of measuring burglary reduction the simple calculation of net effect size as used in Kodz and Pease (2003)[12] is used. This method is not without its weaknesses, but it has the advantage of applying a unified methodology across all projects to generate, arguably, a consistent set of burglary reduction estimates. Variations in the date on which interventions began constrain the length of the 'before' and 'after' data collection windows: Kodz and Pease (2003). The burglary estimates that result vary widely ranging from substantial savings in areas such as Phase 1 - 13 (495) and Phase 1 - 39 (264) to a worsening picture in areas such as Phase 1 - 23 (-173) and Phase 1 - 43 (-110). Of course it should be kept in mind that many of these more 'extreme' results may be a product of local factors not known to evaluators such as movements of prolific offenders so results at the site level should be interpreted with some caution.

The methodology used in Kodz and Pease 2003, relies on an estimate of the impact of a project based on a comparison between changes in burglaries in the 'target area' where a project is implemented and a 'reference area'. For purposes of data collection it had been expected that evaluators would track the numbers of burglaries in a 'buffer area' as well as in target and reference areas. The reason for this was the conjecture that when an intervention is implemented in a target area some burglary activity will be displaced into neighbouring (buffer) areas as offenders seek to avoid the 'heat'. This spillover or 'displacement effect' will neutralise some of the benefits enjoyed in the target area itself. The original rationale was that by ensuring evaluators collected data on target, buffer and reference areas it would be possible to ensure a unified treatment of such effects. The expectation was that target areas would see a fall in burglaries, offset to some degree by an increase in buffer areas. The reference area, once burglaries in the target and buffer areas were excluded, would be the comparison. It would be unaffected by spillover effects and thus suitable as an indicator as to what would have happened in the other two areas had the intervention not occurred.

[12] For further discussion of the method of estimating the intervention effect and the displacement effect/diffusion effect of benefits of location-specific intervention in criminal justice, see Bowles and Pradiptyo (2004).

Table 3.3: Original data from each consortium

	Target	Buffer	Reference
Southern*	Yes	Yes	Yes
Midland**	Yes	Yes	Yes
Northern***	Yes	Yes	No

*Target (area) = SDP; Buffer (area) = police beats surrounding SDP; Reference (area) = police Basic Command Unit (BCU) (or similar) – SDP.
**Target = SDP; Buffer = Police beats immediately adjacent to the target area.
***Target = SDP, Buffer = BCU – SDP

In the event the three teams of evaluators used different approaches, as summarised in Table 3.3. There is not a unified definition of buffer and reference areas across the three consortia. The Southern Consortium defined the buffer and reference areas as the police beats surrounding the SDP and the BCU minus the SDP areas respectively (Millie and Hough, 2003). The Northern Consortium defined the buffer areas as the BCU minus the target (SDP) area, but the data for the reference areas are missing. Hirschfield (2003) reported that the data for 21 SDPs in the Northern Consortium had been collected, together with their constituent police beats and BCU, and also for the rest of police beats and BCUs in the remainder of the Police Force Area. Nevertheless, the data received from the Home Office for the Northern Consortium are limited to burglaries in target (SDP) areas and in buffer areas (BCU-SDP).[13]

The simple calculation of net effect size, used in Kodz and Pease, Findings 204, of unifying data from the three evaluations involved relabelling buffer areas as reference areas for the Northern Consortium projects and then applying the measure set out in footnote 14 below to compare target areas and reference areas without any mention of buffer areas. It thus dispenses with displacement effects altogether for the sake of consistency.

Comparing the change in the target area burglary rate with the change in the burglary rate in the reference area is not without its problems. Since the burglary rate in a target area has to be high initially for the area to qualify for funding, the likelihood is that the reference area will have a lower initial burglary rate. The question then is whether it is appropriate to use a lower burglary rate area as a comparator. Might it not be better, for example, to use as a comparator the change in the burglary rate across high burglary rate BCU's in England and Wales where a project was not implemented?

Leaving aside these complications, the method of estimating the impact of the intervention is to assume that, had it not been for the intervention, burglaries would have risen (or fallen) at the same speed in both the target and reference areas. Any difference in the growth rates is attributed to the impact of the intervention.[14] If this difference can be demonstrated to be significant and in the right direction across a series of projects then the intervention is said to be effective. Figure 3.1 plots the rate of change in burglaries in both the target and reference areas for each project. It demonstrates that, for the most part, the projects are effective since burglaries fall more quickly in the target areas than in the reference areas.[15]

[13] Relabelling the Northern Consortium data implies that in order to guarantee that the displacement and the diffusion effects are zero, the strong assumption should be made that the target area was 'surrounded by a strong fence', allowing no burglaries to spill into the surrounding area. This strong assumption is not imposed for the Southern and Midland Consortia. In order to be consistent, the estimation of the total effect of the RBI should use one or other of the assumptions across all consortia. Imposing both assumptions simultaneously makes the interpretation of the results problematic.

[14] In simple algebraic terms, denoting the number of burglaries in the target area as T_0 prior to intervention and T_1 afterwards while burglaries in the reference area go from R_0 to R_1, the 'Intervention Effect' (= burglary saving/initial burglaries) is given by: $[T_0.R_1/R_0 – T_1]/T_0 = [R_1/R_0 – T_1/T_0]$.

[15] It is a temptation to estimate the mean rate of change in each case (-15.4% in target areas and –3.9% in reference areas) but this implicitly gives equal weight to each project. Statisticians would normally incline to giving greater weight to the results from larger project areas because these estimates will tend to be more reliable, see Aos *et. al* (2001) and Lipsey and Wilson (2000). This method would require information on the sizes of both target and reference areas and unfortunately there was no access to information on the number of households in all the relevant areas.

Figure 3.1: Rates of changes in burglaries

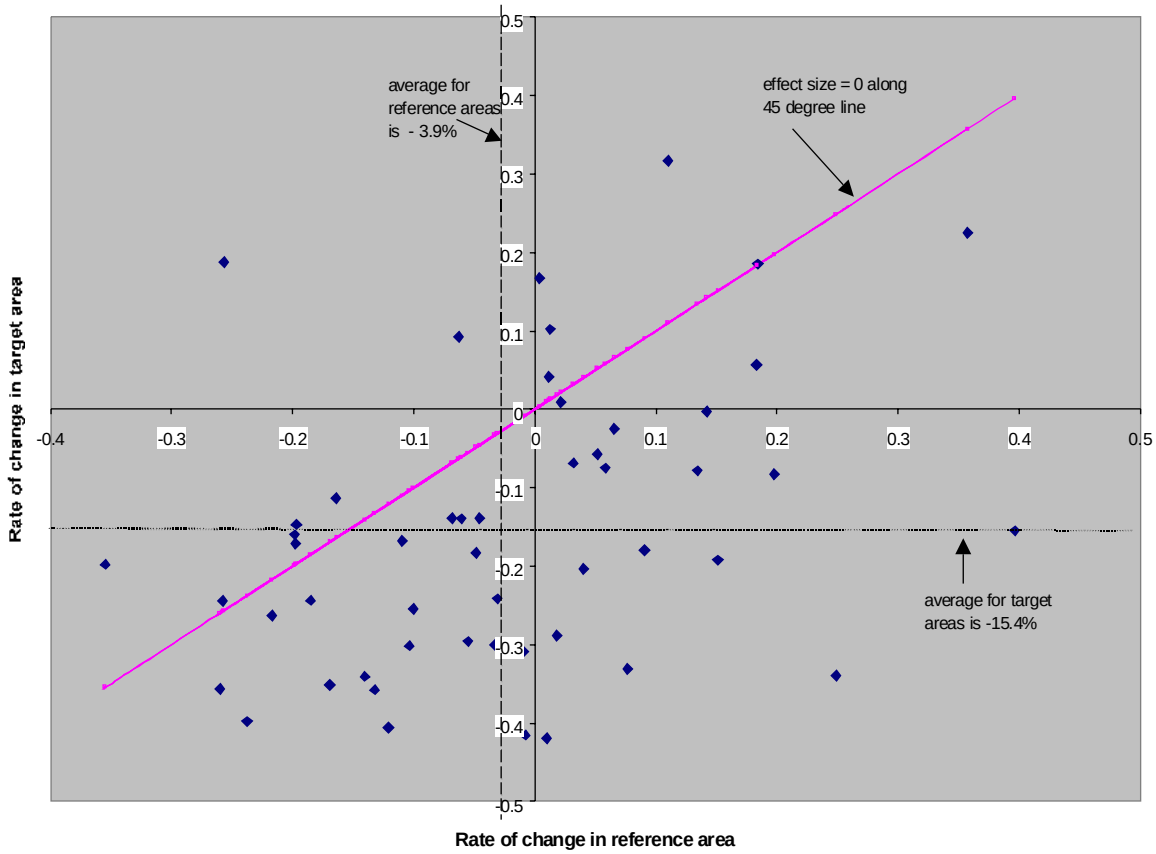

Table 3.2 reports 'implementation problems' as occurring in half of the projects. Many of the reports on individual projects give substantial weight to discussion of these difficulties. The problems most often seemed to stem from developing effective methods of co-operation between partner agencies. Again, from a practitioner's perspective the most useful guidance may be about which sorts of project ran into difficulties.

Moving now to the costs side, it can be seen from Table 3.2 that project costs varied widely. The average is about £180,000 per project including resources 'levered into' projects from outside sources in addition to funding under the Crime Reduction Programme. But there is a lot of variation around this central figure, with 22 projects costing less than £100,000 and four costing over £400,000.

Table 3.2 also includes information on the proportion of a project's total costs represented by capital, as distinct from current, spending. This measure of capital intensity is likely to play a major role when it comes to exploring the longer-term sustainability of projects[16], since the benefits from more capital-intensive projects might be expected to persist for a longer time.

[16] This matter is under further review by the Home Office.

4. Review of findings

Table 4.1 records the cost-effectiveness and benefit:cost ratios for each project. The table is organised into groups of projects according to the principal intervention on which a project's budget is spent. The cost-effectiveness ratio (the cost per burglary prevented) is omitted for projects where the estimate is that a reduction in burglaries has not been achieved.

The average cost per burglary prevented (£3,275) is exceeded by the value of a burglary prevented (£3,378) suggesting that, even on the conservative approach to benefit valuation used thus far, the programme as a whole breaks even. For a project to be a worthwhile investment the benefit:cost ratio needs normally to exceed one although there are various complications, such as making allowance for the income levels of beneficiaries[17], to be taken into account. But on a simple 'break-even' criterion 26 of the 51 projects pass muster. The most successful projects on this test are Phase 1 - 29, Phase 1 - 1, Phase 1 - 13, Phase 1 - 16, Phase 1 - 31 and Phase 1 - 41, all of which have benefit:cost ratios in excess of five.

At the aggregate level, projects with principal interventions comprising location-specific situational crime prevention, stakeholding, enforcement and offender challenge actions all produce benefits that exceed costs. By contrast, projects featuring principal interventions of area-wide situational crime prevention and property marking produce benefit:cost ratios less than one[18].

Having summarised findings from the three consortia it is useful to take stock of progress thus far and to identify any opportunities for deriving implications and conclusions plus any pointers for further analysis.

In terms of the overall design of the RBI programme the projects are heterogeneous and idiosyncratic. They employ a very wide range of combinations of activities and vary widely in the success with which they have been implemented and the impact they are estimated to have had. The wide range reflects the desire to encourage innovation and the tailoring of projects to local conditions. From a research perspective there is the disadvantage that this increases the complexity of the programme, making it more difficult to produce findings that can be readily generalised. Compare this broad approach, for example, with an experiment in which a particular type of target-hardening, say alley-gating, is implemented in a range of areas where rear-entry burglaries are common. A tightly controlled experiment would implement this (single) intervention in a number of areas selected randomly from a group of

[17] Although this point is not pursued further here it might be significant because high burglary rate areas tend to have an above-average level of deprivation. Their income levels will thus be below average and thus any benefits from burglary reduction would attract a heavier distributional weight under the new Treasury Green Book approach: Treasury (2003). Offsetting this may be a smaller economic and social cost per burglary in these areas.

[18] There is always a temptation to estimate the intervention effect of a principal intervention by summing the intervention effects across individual projects within the respective group. This can be misleading as the method gives equal weight to each project. Table 3.2 shows that the size or the coverage of target areas varies across projects. Lipsey and Wilson (2000) and Aos *et.al* (2001) argue that the average effect size across projects should be estimated by taking into account the sizes (i.e. the number of households) in target and reference areas. In the absence of information about the size of reference areas, the effect size of a principal intervention is estimated by comparing the total burglaries in the target and reference areas for the intervention group *as a whole*. This implicitly follows the method suggested by Lipsey and Wilson (2000) and Aos *et.al* (2001).

similar areas, the remainder of the areas being used as a control group. By comparing the evolution of burglary rates in the intervention and control areas it would be possible to draw inferences about the effectiveness of the intervention and about its costs relative to the benefits. By setting up a series of such bilateral comparisons based on different types of intervention it would be possible to compare the cost-effectiveness of different interventions.

Table 4.1: Benefit:cost ratios by principal intervention

				Data used in the Kodz and Pease paper (2003)**				Kodz and Pease net effect size calculation				
	Consortium*	Households in target areas	Project cost	T(0)	T(1)	R(0)	R(1)	Effect size (%) ^	Intervention effect (number of burglaries [illegible]^^	Benefits (£3,378 per burglary saved)^^^	Cost-effective-ness ratio (cost per burglary prevented)	Benefit: cost ratio
Location-specific situational crime prevention												
Phase 1 - 13	S	3,117	172,241	898	759	386	539	55.12	495	1,671,917	348	9.71
Phase 1 - 20	S	4,515	38,252	491	582	3,758	4,450	-0.12	-1	-1983		-0.05
Phase 1 - 60	M	1,595	98,401	193	177	2,152	2,577	28.04	54	182,803	1,818	1.86
Phase 1 - 33	M	2,371	71,646	203	164	1,000	1,151	34.31	70	235,288	1,029	3.28
Phase 1 - 43	M	3,302	173,422	676	789	5,347	5,367	-16.34	-110	-373,173		-2.15
Phase 1 - 55	M	6,313	157,559	431	371	5,246	4,887	7.08	31	103,047	5,165	0.65
Phase 1 - 22	M	3,304	249,213	296	218	3,764	2,947	4.65	14	46,452	18,123	0.19
Phase 1 - 23	M	4,355	65,089	1,118	1221	2,633	2,467	-15.52	-173	-586,034		-9.00
Phase 1 - 41	M	4,610	62,301	740	488	9,330	8,018	19.99	148	499,741	421	8.02
Phase 1 - 58	M	2,213	63,603	203	198	1,238	1,319	9.01	18	61,756	3,479	0.97
Phase 1 - 24	M	4,059	76,269	394	393	3,827	4,371	14.47	57	192,567	1,338	2.52
Phase 1 - 30	M	4,125	74,483	382	503	3,749	4,162	-20.66	-79	-266,584		-3.58
Phase 1 - 53	N	1,652	100,749	314	252	722	466	-15.71	-49	-166,654		-1.65
Phase 1 - 21	N	5,044	202,034	436	305	6,067	5,865	26.72	116	393,481	1,734	1.95
Phase 1 - 35	N	3,291	108,338	317	335	1,917	2,268	12.63	40	135,263	2,706	1.25
Phase 1 - 39	N	3,396	54,599	648	379	3,764	3,735	40.74	264	891,817	207	16.33
Phase 1 - 42	N	923	162,997	257	213	2,659	2,133	-2.66	-7	-23,104		-0.14
Phase 1 - 52	N	3,503	142,416	767	493	3,125	2,712	22.51	173	583,155	825	4.09

			2,073,612	**8,764**	**7,840**	**60,684**	**59,434**	**8.48**	**743**	**2,511,457**	**2,789**	**1.21**
Area-wide situational crime prevention												
Phase 1 - 11	S	4,182	609,273	331	334	4,902	5,008	1.26	4	14,044	146,548	0.02
Phase 1 - 17	S	3,557	150,932	361	295	3,230	3,073	13.42	48	163,674	3,115	1.08
Phase 1 - 9	S	3,076	63,100	494	411	4,417	3,932	5.82	29	97142	2194	1.54
Phase 1 - 19	S	3,483	80,483	82	70	4,003	3,215	-5.05	-4	-13991		-0.17
Phase 1 - 38	M	3,557	97,172	86	74	1,536	1,443	7.90	7	22947	14305	0.24
Phase 1 - 34	M	1,443	49,852	101	120	1,680	1,249	-44.47	-45	-151710		-3.04
Phase 1 - 25	M	3,188	86,544	421	318	3,084	2,289	-1.31	-6	-18668		-0.22
Phase 1 – 32	M	5,771	86,889	639	550	878	838	9.37	60	202303	1451	2.33
Phase 1 - 36	N	3,387	356,070	501	379	2,003	1,632	5.83	29	98650	12193	0.28
			1,580,315	**3,016**	**2,551**	**25,733**	**22,679**	**3.55**	**107**	**361649**	**14761**	**0.23**
Stakeholding												
Phase 1 - 2	S	6,134	408,792	794	559	2,895	2,735	24.07	191	645595	2139	1.58
Phase 1 - 62	N	2,590	140,824	253	180	2,563	2,609	30.65	78	261933	1816	1.86
			549,616	**1,047**	**739**	**5,458**	**5,344**	**27.33**	**286**	**966552**	**1921**	**1.76**
Enforcement												
Phase 1 - 16	S	2,992	45,640	778	580	5,427	4,883	15.43	120	405406	380	8.88
Phase 1 - 12	S	3,569	105,134	501	350	5,704	5,113	19.78	99	334728	1061	3.18
Phase 1 - 6	S	4,909	124,935	473	436	3,487	3,955	21.24	100	339430	1243	2.72
Phase 1 - 47	M	4,324	64,302	103	68	539	673	58.84	61	204729	1061	3.18
Phase 1 - 28	M	4,091	162,618	486	289	3,201	2,814	28.45	138	466984	1176	2.87
Phase 1 - 46	N	2,603	55,652	175	147	1,779	1,426	-3.84	-7	-22716		-0.41
Phase 1 - 50	N	2,323	190,153	190	156	2,174	2,371	26.96	51	173011	3713	0.91
Phase 1 - 51	N	3,053	219,479	451	400	1,854	1,550	-5.09	-23	-77526		-0.35
Phase 1 - 61	N	3,517	151,524	364	234	2,614	1,934	9.70	35	119277	4291	0.79
Phase 1 - 44	N	3,500	375,021	395	273	2,134	2,113	29.90	118	398985	3175	1.06
Phase 1 - 49	N	3,292	404,032	307	284	2,118	2,241	13.30	41	137919	9896	0.34
Phase 1 - 57	N	2,271	243,309	504	337	3,171	3,413	40.77	205	694056	1184	2.85
Phase 1 - 48	N	2,271	331,069	300	239	3,375	3,511	24.36	73	246894	4530	0.75
Phase 1 - 45	N	3,300	256,421	1,071	1,116	2,604	2,634	-3.05	-33	-110330		-0.43
			2,729,289	**6,098**	**4,909**	**40,181**	**38,631**	**15.64**	**954**	**3221825**	**2862**	**1.18**

Offender challenge actions												
Phase 1 - 31	M	2,981	53,140	598	360	1,998	1,523	16.03	96	323,723	555	6.09
Phase 1 - 54	N	3,461	83,273	216	238	2,977	3,015	-8.91	-19	-65,002		-0.78
Phase 1 - 37	N	2,088	165,477	276	179	1,678	1,394	18.22	50	169,870	3,291	1.03
Phase 1 - 40	N	3,210	78,953	356	270	2,232	2,163	21.07	75	253,332	1,053	3.21
Phase 1 - 27	N	3,309	196,011	454	423	4,297	4,434	10.02	45	153,614	4,310	0.78
			576,854	**1,900**	**1,470**	**13,182**	**12,529**	**17.68**	**336**	**1,134,600**	**1,717**	**1.97**
Property												
Phase 1 - 1	S	3,952	42,829	629	593	4,937	5,193	10.91	69	231,784	624	5.41
Phase 1 - 14	S	2,580	1,844,204	284	165	3,480	3,514	42.88	122	411,355	15,144	0.22
			1,887,033	**913**	**758**	**8,417**	**8,707**	**20.42**	**186**	**629,850**	**10,120**	**0.33**
Phase 1 - 15	S	4,115	100,171	324	397	951	1,291	13.22	43	144,700	2,338	1.44

*) S = Southern Consortium; M = Midland Consortium; N = Northern Consortium.
**) Data for the buffer areas in Southern and Midland Consortia were excluded from the analysis.
Data for the buffer areas in Northern Consortium have been considered as data for the reference areas.
^) The total effect size for each principal intervention is estimated by applying the method of effect size used in the Kodz and Pease (2003) paper to the sum of burglaries in the target and reference areas before and after intervention for the respective principal intervention. This method is consistent with the estimation of total effect size by Aos, *et, al* (2001) and Lipsey and Wilson (2000).
^^) Total intervention effect for each principal intervention = [total effect size].[total burglaries in target areas before project in the respective principal intervention].
^^^) Benefit = [intervention effect].[value per burglary saved (i.e. £3,378)].
Total benefit for each principal intervention = [total intervention effect of the respective principal intervention].[value per burglary saved (i.e. £3,378)]

But, given that the RBI programme did not set out with this more narrow focus, the more general question is: what indicators (if any) can we derive from RBI as to the likely success of projects? One approach is to accumulate qualitative information from which generalisations can be made about success factors. This can translate into useful data for developers of projects in the future seeking to replicate the practice followed in successful projects. The reports from the consortia contain a lot of data potentially useful for this purpose.

An alternative approach is to apply quantitative methods in order to derive some inferences about the characteristics of successful projects. The simplest way to approach this is to investigate the correlation between project success, as measured by the benefit:cost ratio, and the various project characteristics documented in Tables 3.2 and 4.1. A formal version of the model used is not presented here but, in essence, a multiple regression model is estimated(for the 51 projects on which there is reasonable data) in which the dependent variable is the project's estimated benefit:cost ratio. Each project is characterised by reference to a series of independent variables.

For this purpose it is necessary first to specify a number of dummy variables and to create some new variables in order to capture the various influences to be explored. A consortium dummy is specified for two of the three consortia in order to account for any variation in the measurement methodology or data collection processes being used. The researchers define a Southern Consortium variable that takes the value one for a project evaluated by the Southern Consortium and zero if evaluated by either of the other two consortia. Likewise they identify a Midlands Consortium dummy. There is no need to specify a third dummy variable for the Northern Consortium since it acts as a control.

Similarly, when it comes to the implementation issue, where there are three possible outcomes (no problem, some problems and serious problems), two dummy variables are needed to capture the 'deviations' from a control position. The 'serious problem' dummy takes a value of one for projects where the problems were serious and zero otherwise. A second dummy variable takes value one if there are 'some' problems and zero otherwise. If there are no implementation problems then both variables take the value zero. The purpose of including these intervention dummies is to explore the hypothesis that the returns on a project may be higher or lower depending on how effectively the project has been implemented. On the face of things the argument might be that the greater the implementation problems encountered the lower the return will be. But of course one kind of possible implementation problem is that activity on the project was lower than originally anticipated. Such an effect will manifest itself however in lower spending so that, although the level of activity is lower, costs are lower as well. The ratio of benefits to costs may not be much affected as a result.

The next four variables are taken directly from the earlier tables. The initial burglary rate per household is used because the hypothesis is that projects located in high burglary rate areas may be more or less productive (other things being equal) than similar projects implemented in lower burglary rate areas, an effect suggested by findings from the Safer Cities Project: Ekblom *et al* (1996). Spend per household is included in order to explore the hypothesis that projects with a greater 'intensity' are more likely to be successful. The proportion of the project's total cost accounted for by capital spending is included to check the hypothesis that more capital intensive projects may be more or less successful, a possibility explored in greater depth below where the implications of limiting the measurement of project benefits to the first two years following the start of intervention is considered. The last of the four variables taken from earlier tables is the size of the project area size, as measured by the number of households living there. This is included to check the possibility that interventions in more concentrated geographical areas may have a systematically different impact.

The final group of variables included measure the proportion of a project's budget going on each of the first five of the principal types of intervention. The hypothesis here is that the composition of spending will play a significant part. If two otherwise similar projects differ in the shares of spending going to two different types of intervention might be expected that this will be reflected in the overall return on the project.

A correlation coefficient matrix summarising the pair-wise relationships between the variables is given in Table 4.2. The results are a bit disappointing, especially in regard to the benefit:cost ratio which does not seem to be very highly correlated with other variables. There are various factors that may contribute to this result. The benefits of each project are estimated based on the simple calculation of net effect size used in Kodz and Pease (2003). Table 3.3 shows that there is no homogeneous method used by the three consortia to defined the buffer and the reference areas. It is apparent that the lack of consistency in defining the reference areas across consortia increases the variability of the benefit:cost ratio across projects.

In addition, due to the lack of data and for the sake of consistency in estimating the method used in the Kodz and Pease (2003) publication across consortia, data for the buffer areas in the Northern Consortium have been relabelled to be data for reference areas. This creates a serious problem because by definition the reference areas should be free from both the direct and indirect impacts of interventions (e.g. displacement effects or diffusion of benefits). The data from the Southern and Midlands Consortia may satisfy the definition; however, this is not the case for the Northern Consortium data.

The method used in the Kodz and Pease (2003) calculation takes into account neither the possibility that burglaries in the target area may be displaced to its surrounding areas, nor the possibility that the interventions may reduce burglaries beyond the target area. Even if the displacement effect/diffusion of benefits in the benefit:cost ratio were incorporated, this would not eliminate the problem of inconsistency in defining the buffer and the reference areas. The total impact of the programme is discussed in chapter 7 below.

As we discussed in chapter 2, there is great variation in the intervention content across projects in the RBI programme. There are 93 different activities, and any possible combinations of them, aggregated into six major categories of intervention. It would not be surprising if, despite classifying activities into six principal intervention categories, the variety of intervention across projects were to remain high.

Finally, there is variation in the time profile of burglary reduction across interventions. Some interventions, because of their character, can be expected to have an effect immediately (e.g. target hardening, alley-gating, etc). Other interventions (such as anti-burglary campaigns or distribution of crime prevention packs, etc.), however, may take a longer period to contribute to a reduction in the number of burglaries.

There are nevertheless a few interesting observations to emerge. Serious implementation problems seem to be more closely associated with some interventions than others. There is a negative link with location-specific situational crime prevention suggesting that these projects, that tend to follow well established paths, are less susceptible to serious implementation problems (partial correlation coefficient: -.32). The reverse is true for the enforcement activities comprising intervention group four (partial correlation coefficient: +.41). As is clear from the relationship between the proportion of capital spending and the proportion of the budget going on capital (partial correlation coefficient: -.47) these group four interventions tend to be less capital intensive. It seems reasonable to suppose that this is consistent with organisational challenges being greater in the case of labour-intensive projects. However this link does not work entirely smoothly because the intervention type two (i.e. area-wide situational crime prevention) content is quite highly correlated with capital intensity (partial correlation coefficient: +.41) but does not seem to be as subject to serious implementation problems (partial correlation coefficient: +.01). The proportion of a project's budget going on type three intervention activities (stakeholding) seems to be the only factor likely to increase susceptibility to less serious implementation problems (partial correlation coefficient: +.29).

Table 4.2: Correlation coefficient matrix

										Share of spending				
	B:C ratio	**Southern consortium**	**Midlands consortium**	**Serious intervention problems**	**Some intervention problems**	**Initial burglary rate per household (%)**	**Spend per household**	**Capital spending (%)**	**Project area size (households)**	**Intervention 1**	**Intervention 2**	**Intervention 3**	**Intervention 4**	**Intervention 5**
B:C ratio	1.000													
Southern Consortium	0.189	1.000												
Midlands Consortium	-0.169	-0.397	1.000											
Serious intervention problems	0.097	0.134	-0.072	1.000										
Some intervention problems	0.022	0.211	-0.021	-0.308	1.000									
Initial burglary rate per household (%)	0.220	0.038	-0.183	0.249	-0.103	1.000								
Spend per household	-0.123	0.181	-0.227	-0.045	-0.116	0.031	1.000							
Capital spending (%)	-0.080	0.153	-0.057	-0.206	-0.093	-0.194	-0.236	1.000						
Project area size (households)	0.060	0.236	0.137	-0.006	0.015	-0.208	-0.239	0.002	1.000					
% on intervention 1	0.113	-0.226	0.360	-0.318	-0.092	0.207	-0.239	0.179	0.072	1.000				
% on intervention 2	-0.247	0.123	0.201	0.012	0.165	-0.231	-0.045	0.413	0.002	-0.366	1.000			
% on intervention 3	0.212	0.328	-0.325	0.040	0.285	-0.045	0.021	-0.116	0.176	-0.223	-0.278	1.000		
% on intervention 4	0.015	-0.071	-0.292	0.410	-0.156	0.158	0.194	-0.471	-0.120	-0.345	-0.391	-0.015	1.000	
% on intervention 5	0.004	-0.055	-0.210	-0.222	0.013	-0.134	-0.066	-0.046	-0.035	-0.173	-0.253	0.018	-0.223	1.000

Having summarised the findings in a preliminary way, some of the outstanding issues will be pursued in greater depth. In particular:

- take further steps towards transforming the analysis from a project-base to an intervention-base;

- look more closely at the time profile of projects and the implications for the sustainability and replicability of projects; and

- look further at the method used to compute baseline measures for making before and after comparisons of burglary rates and for taking proper account of displacement effects.

5. Intervention-based analysis

For the policy-maker and for the practitioner working at the 'sharp end' of crime reduction, a critical question is what the experience of the RBI can contribute to an analysis of the choice at operational level between various types of intervention. It would be very helpful if evidence from the projects could be used to tackle questions such as: Is target-hardening a better investment of resources than property marking? In what circumstances will alley-gating be effective? The individual project reports contain a wealth of material about implementation problems and about steps that have proved effective locally. Assembling a database of the evidence in a format that makes it easy for local Crime and Disorder Reduction Partnerships, for example, to review the various interventions available and to select the one most likely to be successful in their own area would be a challenging but worthwhile extension to the present analysis.

The purpose of this chapter is to look more closely at the prospects for developing an analysis at the broad intervention category level. Table 4.1 illustrated that the average benefit:cost ratio varied across groups of projects classified by reference to the principal type of intervention implemented, suggesting that some intervention types will be more successful than others. The sheer length of the list of individual types of interventions (93) implemented in various combinations in 63 projects is such that there is little or no prospect of producing evidence as to the effectiveness of each type of intervention on the 'long list'. The best that can be done is to find systematic forces operating at the level of the six broad groups of interventions.

The next step is to run a regression model based on the variables listed in Table 4.1. But this effort yields a blank, perhaps not surprising given the relative weakness of many of the partial correlation coefficients. We can find no systematic relationship between a project's benefit:cost ratio two years into projects with any of the other project characteristics listed above. The regression model has little explanatory power (F statistic insignificant) and none of the independent variables has a significant influence (t statistics insignificant at 5% level).

This does not, of course, mean that the return on the RBI projects is an entirely random matter. Given the wide range of interventions used, the characterisation of the intervention type is very crude. The fact that no link can be established between the benefit:cost ratio and the principal intervention type represented in a project does not imply that the benefit:cost ratio is independent of the type of intervention undertaken. It may just mean that a more thorough way of distinguishing between the 'intervention content' of projects needs to be developed, a point returned to below.

There are some important caveats here. First, as indicated below, there are grounds for arguing that benefits have not been captured very reliably because of the methodological problems in estimating the scale of burglary reduction effects. Secondly, the complexity of project composition allows a lot of scope for noise to enter the model and for the burglary reduction impact to be influenced by factors other than the composition of the intervention. The characterisation of composition is very crude because, although it distinguishes the proportion spent by type of intervention it does not disaggregate below that level. The type of activity within an intervention group (or the particular combination of activities) may be having an influence that the model used cannot detect. Another way of expressing this is to say that although the benefit:cost ratio of a project is not a purely random matter the researchers have not succeeded in identifying the shape of the systematic influences.

6. The time profile of benefits

The evaluation of RBI projects took place a comparatively short time after implementation, typically within two years. Many of the projects, particularly the more capital-intensive ones, have potentially a substantial life span extending many years beyond the evaluation date. An obvious danger in conducting an interim evaluation, before all a project's effects have had time to unwind, is that there will be uncertainty as to the reliability of estimates of the costs and benefits expected over the remainder of the intervention's lifetime. An even greater danger is that evaluators will be tempted by the expedient of simply ignoring future costs and benefits. By doing this, and basing cost effectiveness estimates purely on a retrospective account of events during the first two years of a project's life, analysts risk the 'premature evaluation' problem analysed in more detail elsewhere: Bowles and Pradiptyo (2003c).

Consider as an example the installation of alley-gates. The main cost is incurred 'up front' in identifying suitable locations for this type of target-hardening measure, in resolving legal disputes about access and in installing gates. The gates might be estimated to have a useful life of 10 or 15 years before needing replacement, and so the benefits will extend to burglaries potentially prevented over the whole of this time horizon. Whatever the success of the installation in the first year or so the critical factor will be whether any crime reduction benefits can be expected to continue into the future. Quite different results will obtain depending on whether it is assumed that the same burglary reduction effects will persist or whether they are anticipated, say, to decay exponentially as assumed in some models.[19] From an investment appraisal perspective, the evaluation is being done 'too early' in the project's life to make a reliable inference about the likely benefit:cost ratio over its lifetime.

From a cost perspective RBI evaluators have made serious efforts to distinguish the capital and recurrent costs of projects and to spread capital costs appropriately over a project's life. This is a necessary condition for making rational decisions about whether it is worth continuing to support projects, but it is not sufficient. If first-year costs (capital plus recurrent) are compared with first-year benefits then there is a high risk that capital-intensive projects will be undervalued and not rolled out elsewhere even if, over their lifetime, they would be beneficial. But for the full picture it is essential that future benefits be estimated as well as future costs. The downside to this forward-looking investment appraisal kind of an approach is that project benefits are mostly in the form of potential, future benefits whose scale is not known for sure at the time of evaluation. These are more difficult to estimate even than current outcomes, leaving evaluators in the position usually of choosing between assuming that the benefits will continue at their initial level, that they will decay at some steady rate or that all (or most) benefits will be confined to the early stages of the project.

This problem was anticipated before the RBI evaluations began[20], but its ramifications were not fully appreciated. The Dhiri and Brand (1999) guidelines for evaluating CRP projects set out a standard cost-benefit/project appraisal approach in which the streams of benefits and costs are estimated for the whole of a project's lifespan and then discounted to get a Net Present Value. This is the same model used more generally for the analysis of public investment as, for example, advocated in the Treasury Green Book guidelines: Treasury (2003).

The practical effect of the neglect of burglary saving during the post-evaluation phase is likely to be a significant underestimation of project benefits. A good way to get a sense of the scale of this effect is to consider the example in Table 5.2 of the Dhiri and Brand (1999) guidelines book. The costs and benefits in their project are given in the following table:

[19] This issue is raised, but not explored in depth, in the path-breaking work of Aos *et al* (2001).
[20] The short time-scales were decided on so that findings from round 1 could feed into rounds 2 and 3 of the RBI.

Table 6.1: Time profile of hypothetical costs and benefits

	Year 1	Year 2	Year 3	Year 4	Total
Start-up costs, £	55,000	0	0	0	55,000
Recurrent costs, £	10,000	11,000	12,000	0	33,000
Total costs, £	65,000	11,000	12,000	0	88,000
Outcomes (number)	100	150	140	50	440
Benefits, £	30,000	45,000	42,000	15,000	132,000
Cumulative net benefit, £	(35,000)	(1,000)	29,000	44,000	44,000

Source: Dhiri and Brand (2000)

The method used in the evaluations of the RBI is equivalent to 'stopping the clock' at the end of year 2 in this example and concluding (erroneously) that the project is non-viable because it shows a cumulative loss of £1,000. The great bulk of costs (£76,000 of the total of £88,000) have been incurred by this stage but only a smaller proportion of the benefits (£75,000 of the total of £132,000) has accrued to that point. If things continue to unfold as originally predicted the project will move into surplus in year 3 and end up by bestowing a substantial cumulative net benefit by the end of its lifetime at the end of year 4. Any backward-looking evaluation done before benefits have all been exhausted is inevitably prone to this kind of underestimation. The appropriate approach from a benefit:cost perspective is to make a new estimate of expected future benefits at the time of the evaluation based on the information gathered over the first two years of the project.[21]

[21] As Dhiri and Brand (1999) p33 point out the costs and benefits in this example have not been discounted, but that makes relatively little difference to the arithmetic. There is a more general analytical treatment in Bowles and Pradiptyo (2003b).

7. Programme-wide impact

Estimating what would have happened had an intervention (or a programme of interventions) not been implemented is a generic problem not confined to burglary or even crime programmes as a whole. In the analysis of health care, for example, comparison of how a patient will fare under alternative possible treatments is central to decisions about which option to choose. In medicine the approach of clinicians, increasingly, is to rely on results from experiments designed to yield reliable inferences about the impact of alternative treatment regimes. These experiments are usually based on the methodology of Randomised Control Trials. They involve following up members of the treatment and control groups so as to keep close track of health outcomes for the two groups and to enable valid comparisons to be made.

These sorts of experiments are costly to run and the tradition in the criminal justice field is to use much more informal methods for estimating outcomes. Comparison groups, or areas, are a popular method. In the evaluation of offender treatment programmes, for example, it is common practice to compare reconviction rates as between (a) a group of offenders who have been though a programme designed to reduce their propensity to reoffend and (b) a comparable group (with similar age, gender and offending histories) who have not been through the programme. The effectiveness of the programme is estimated on the basis of the difference in the reconviction rates between the two groups.

In most of the RBI projects the burglary saving is inferred from the change in the burglary rate in the intervention area relative to the change in the burglary rate across the remainder of the Basic Command Unit within which the intervention area sits[22]. An alternative to this 'bottom-up' approach, using individual projects or intervention type as the unit for analysis, is a 'top-down' approach that compares areas as a whole where RBI projects were implemented with areas as a whole where projects were not implemented. Ideally this would distinguish high burglary rate areas and concentrate on them. This would not only give a consistency check on results from aggregating across individual projects, but would also give a basis for making an assessment of the overall impact of the programme.

It has not been possible to assemble the data to construct such an analysis although, in principle, the calculation could be done. It would require an identification of all high burglary rates and measurement of the change in the number of burglaries in these areas net of project areas. Instead, the researchers have taken the short cut of comparing the change in project areas with the change in all non-project areas, i.e. non-project areas with both high and low burglary rates: see Table 7.1. The position across England and Wales can be characterised as follows:

[22] This is not the best method of choosing the reference areas. The underlying assumptions of the above method are: a) an SDP has similar characteristics to the rest of the BCU; and b) the target area (SDP) is an isolated area such that incidents cannot displace to other areas within the respective BCU, or the impacts of interventions cannot affect burglaries beyond the SDP's boundary (i.e. the rest of the BCU). The former may well be satisfied, however; the latter may not be fulfilled as the size of the target areas varies across projects. For further discussion see Bowles and Pradiptyo (2004).

Table 7.1: Programme-wide estimates of burglary reduction

	Pre-intervention period (19 months)	Intervention period (21 months)	Rate of change %
England and Wales, all areas	761,491	739,554	-2.88
51 RBI project areas Total (T + B + R)	208,383	198,102	-4.94
E & W excluding RBI project areas	553,108	541,452	-2.11

In the non-intervention areas burglaries fell at a rate of 2.11 per cent. If this rate had been experienced in the RBI areas but for the intervention then burglaries in those 51 areas would have been expected to fall from 208,383 to 203,986. In the event they fell to 198,102 suggesting an overall saving of something like 5,884 burglaries. The benefits from this burglary reduction (at £3,378 per burglary) would have been of the order of £19.877 million compared with total project costs of £9.496 million as recorded in Table 3.2. This finding suggests savings on a substantially greater scale than would be suggested by summing the burglary savings across individual projects as set out in Table 3.2.

The analysis has neglected any diversionary impact the projects might have had on crimes other than burglary. The evaluations on which the researchers synthesis builds do not report on other crimes in a consistent way and thus there is little scope for assessing the diversion effects arising from the implementation of the RBI.

8. Sustainability and replicability

From a practical perspective there are two layers of questions about projects and interventions. First there is the sustainability question: is it worth continuing to spend on the intervention following evaluation? And secondly there is the replicability question: is it worth implementing similar new projects or interventions elsewhere? The reason why the answer to these two questions might be different is that for the first question capital costs are ignored whilst for the second they are included. As an example take the case of the Concorde aircraft project. The plane continued (for many years) in commercial use so long as passenger revenues were sufficient to cover maintenance and running costs. But no further supersonic planes were built because the revenues over the plane's lifetime were not believed to be sufficient to cover the combined (enormous) capital construction costs plus current running costs.

The analysis of the time profile of costs and benefits in chapter 6 above helps distinguish these questions effectively. In the Dhiri and Brand (1999) example set out in Table 6.1 above the sustainability question is: at the end of year 2 is it worth spending 12,000 in year 3 in order to generate benefits of 42,000 and 15,000 in years 3 and 4? The answer, of course, is yes: the prospective benefits from then on are well worth the extra spending in year 3, on the assumption that stopping spending would reduce benefits in years 3 and 4 to zero. This answer to the sustainability question has ignored the capital costs and the recurrent costs incurred up to the point of evaluation. It makes use only of expected future costs and benefits.

The replicability question is different. The test as to whether the intervention is worth repeating will, in essence, be whether the *net cumulative benefit expected over the whole project, as judged at the time of the interim evaluation at the end of year 2, is estimated to be positive*. In the Dhiri and Brand example the net cumulative benefit is (plus) £44,000 so the intervention is undoubtedly worth repeating. But it is very easy to change the numbers a bit and to come to different conclusions. If the benefits were reduced to £15,000 per annum for example then they would total only £60,000 over the four years compared with total costs of £88,000. In such circumstances the project would have been sustainable since looking forward from the end of year 2 it brings benefits of £15,000 for each of years 3 and 4 at a cost of only £12,000 in year 3. But it would not have been worth replicating since the cumulative net benefit would have been negative (benefits of £60,000 less costs of £88,000 giving a net loss of -£28,000).

In the RBI it is difficult to find much appreciation of the overall profile of benefits and costs expected either at the point of implementation or at the evaluation stage. The absence of predictions of expected future costs and benefits, in the final analysis, makes it difficult to put confidence in the estimates of cost effectiveness and to derive inferences about either sustainability or replicability.

9. Conclusions

A standard cost-benefit approach following the approach recommended to evaluators in the Dhiri and Brand (1999) guidelines has been applied to RBI projects. The validity of attempting such an exercise has been questioned by at least one reviewer of this paper on the grounds that the researchers are making unwarranted assumptions about the likely costs and benefits that might result if the projects were to be repeated elsewhere. That criticism may be justified in part but the alternative of encouraging project heterogeneity, flexibility and innovation has the disadvantage that it militates against efforts to make any generalisable estimates of the effectiveness and cost-effectiveness of interventions.

Such results as have been established here are broadly encouraging from a cost-effectiveness and benefit:cost ratio perspective. From the evidence available the benefits generated by the projects implemented, in aggregate, exceeded their costs. Groups of projects with a focus on location-specific situational crime prevention, stakeholding, enforcement and offender challenge as the principal type of intervention all gave rise to benefit:cost ratios in excess of one.

However, there are grounds for supposing that project benefits may have been consistently underestimated across the programme, in which case the benefit:cost ratios would have been somewhat higher than the estimates presented here. One of the factors causing an underestimate of the benefits of interventions is the omission of estimates of future benefits from interventions during the post-evaluation phase. Another factor contributing to underestimation of benefits is a lack of consistency in defining reference areas across the three consortia. If the reference areas were defined consistently, as discussed in chapter 7, an increase in estimated benefits might have been expected. A third possibility, albeit one which has not been tested empirically, is that the projects fostered innovative approaches to burglary reduction that, if main-streamed, may be less costly to replicate. A cost-benefit approach can be rather hard on such 'prototypes' unless they are designed specifically to generate cost and benefit data.[23]

In any event there are some useful lessons to emerge:

- the guidelines given to evaluators in the future can be developed further in light of the methodological limitations that have been identified;
- the design of future major crime reduction investment programmes can be improved to help mitigate some of the difficulties in drawing inferences about the cost effectiveness of interventions;
- the 'premature evaluation' problem identified can be confronted directly and remedies developed to prevent the recurrence of systematic undervaluation of project benefits;
- a more systematic treatment of displacement and diffusion effects is needed to inform the identification of appropriate 'buffer' and 'reference' areas;
- greater use can be made of secondary data to give more context to cost-effectiveness explorations.

Methodological advances have the potential to improve the raw material from which evaluators are able to work and thus the inferences they can draw from the findings. From an 'experimental design' perspective the set of projects comprising the RBI has important weaknesses. The 'gold standard' applied by analysts (such as those involved with the Campbell Collaboration: see Petrosino *et al*, 2001), assembling an international pool of

[23] If, as one reviewer of this paper has suggested, the programme was intended to explore which interventions were most likely to bear fruit if implemented elsewhere then one would conclude that a somewhat different programme design, identifying initially promising ideas then implementing them in a variety of settings, might have been more appropriate.

evidence on effectiveness and cost-effectiveness imposes much heavier demands than can be met by the RBI projects. But these are early days in the development of sound methodology for applying economic evaluation to crime reduction projects. Additional development work would be necessary to allow further progress towards assembling high quality evidence on the effectiveness of crime reduction interventions in general and reducing burglary interventions in particular.

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