Safer Cities and residential burglary

A summary of evaluation results

Paul Ekblom[1]

The Safer Cities programme

Phase 1 of the Safer Cities programme was inaugurated in 1988 and finished in Autumn 1995. Safer Cities was part of the British Government's wider programme to deal with the multiple problems of some of the larger urban areas. The objectives of Safer Cities were to reduce crime, lessen fear of crime, and create safer environments within which economic enterprise and community life could flourish (Home Office, 1993).

Safer Cities initiatives were locally based, with a 'partnership' or multi-agency approach to crime prevention.

The programme was developed in the light of experience of the earlier 'Five Towns' initiative (Liddle and Bottoms, 1991). In each of twenty areas – covering cities or boroughs – a local project was set up with a coordinator and a small team (Tilley, 1992; Sutton, 1996).

Safer Cities projects featured a wide range of activities, including awareness raising among citizens and local agencies, and the development of community safety strategies in local government. But at the core was the initiation of local preventive schemes. These schemes were implemented on the ground by a variety of local organizations, who were invited to bid for funds. The schemes drew on grants from Safer Cities – up to £ 250,000 annually per city – and other local or national resources. Altogether, Safer Cities initiated some 3,600 schemes at a cost of £ 22 million plus £ 8 million administration.

The preventive action was intended to take the rational, problem-oriented

1 Home Office Research and Statistics Directorate, 50 Queen Anne's Gate, London SW1H 9AT, England. I am grateful for assistance from Pat Mayhew, Ho Law, Mike Sutton and other colleagues.

approach developed over the last decade (Tilley, 1993; Laycock and Tilley, 1995; Sutton, 1996). Coordinators were given a limited amount of training and support from professionals in the Home Office and elsewhere (few coordinators had much background in criminology). They were also provided by the Research and Statistics Directorate with an initial 'crime and social profile' of their area, including a beat-by-beat picture of recorded crime rates, to help develop priorities and set up an action plan.

The schemes deliberately addressed a wide range of crime problems using a wide range of methods. The crime problems ranged from residential and commercial burglary, assault, domestic violence, vehicle-related theft, and shop theft. In some cases the focus was more on fear of crime. Preventive methods included both 'situational' and offender-oriented action. The former included measures such as better security hardware, alarms, improved lighting, and surveillance measures. The latter covered youth work, holiday play schemes, credit unions, adventure playgrounds, employment advice, even morality plays in schools. Some schemes focused on the city as a whole (through publicity campaigns, information initiatives such as crime prevention buses, or multi-agency programmes). Many schemes, however, focused on vulnerable individuals, groups of homes, particular institutions (such as schools and clubs), or particular localities (housing estates, car parks or city centres).

The evaluation strategy

The focus in the Research and Statistics Directorate's evaluation was on the impact of the Safer Cities programme as a whole. Our approach was to look at the typical scheme – since this provides the best picture of what a large-scale prevention programme is routinely capable of implementing. The alternative approach – to pick a set of 'good prospects' in advance, or to comb retrospectively for 'success stories' – might say something about good practice, but not much about the cost-effectiveness of the programme. The Dutch government's attempt to evaluate a set of individual preventive schemes identified in advance met with severe attrition problems: poor implementation, poor data and weak scheme evaluations eliminated many (Polder, 1992; Junger-Tas, 1993). Wider discussions of the difficulties of evaluating crime prevention initiatives are in Ekblom (1990) and Ekblom and Pease (1995).

The evaluation required us to link measures of Safer Cities action to measures of outcome. This was challenging (Ekblom, 1992). In particular, many schemes were small in resource terms, or spread thinly over large areas. This meant that the impact of individual schemes was often likely to be modest, and that it was best to consider a large number simultaneously. To minimize the risks of delivering inconclusive findings, and to conduct a 'fair test', the strategy we

devised was path-breaking in several ways (Ekblom and Pease, 1995). It aimed to estimate the size and cost of any preventive impact, required the use of state-of-the art computing (Ekblom et al., 1994) and equally new statistical techniques (Ekblom et al., 1993).
Residential burglary was chosen for this first evaluation because coordinators often targeted it, preventive practice is relatively well-developed, and burglary schemes tend to be local. If the Safer Cities programme was going to have a measurable impact on crime, we reasoned, it would be on burglary.

Safer Cities action against burglary

Up to summer 1992, just under 300 current or completed schemes in the first sixteen cities were targeted at residential burglary at the local level. Three-quarters focused on domestic target-hardening (including door, window and fencing improvements, entry systems, and security lighting around individual houses or blocks). Eight percent were focused on community-oriented action (providing crime prevention outreach workers, raising awareness of prevention, fostering Neighbourhood Watch, and property-marking). Offender-oriented action specifically targeted at burglary was rare. The amount spent per scheme varied from a few pounds to over £ 100,000. The territories covered by the schemes ranged from single blocks of flats to whole districts; the average was about 5,200 households.
The average Safer Cities funds spent per burglary scheme was £ 8,700. For about a third of the schemes there were additional levered-in funds raised from local agencies and institutions, and from other national programmes. For these schemes, the average Safer Cities spend was £ 11,300 and the average levered supplement £ 17,800.

Measuring Safer Cities action

There would be little prospect of finding impact by simply comparing cities. Rather, a fairer test meant taking account of the amount of local action, and looking for impact where one might expect to find it – in the vicinity of schemes.
The amount of action was measured in terms of money spent, combining Safer Cities and levered-in funds. Using data from the Safer Cities Management Information System, maps of scheme locations, and population data from the 1991 Census, an action score was calculated for each small area covered in the evaluation. This score represented the average amount of funds acting on each household over a given year. (It can be regarded as a measure of 'action intensity' – cf. Polder, 1992. It took into account the amount spent on each scheme

affecting the area, the area over which each scheme was spread, and the length of time each scheme had been operating.) The amount spent was averaged over all households in the area because it was not possible to identify which individual household had or had not received action.
Besides this 'hard' data on Safer Cities action, 'softer' information of various kinds was used to guide and interpret our analysis. Brief descriptions of each scheme were available on the Management Information System; informal contacts with coordinators were regular, and open-ended interviews with them (Sutton, 1996) threw light on the process by which they assigned action to particular locations.

Measuring outcome and assessing impact

To measure outcome, two sources of local data were collected: information from sample surveys of adults, and police-recorded crime figures. The two sources were complementary, with different strengths and weaknesses. In each case the evaluation design involved comparing changes in burglary risk over time, between local areas which received Safer Cities action against burglary, areas in the Safer Cities which had no action and other cities matched demographically and by overall crime rate. Of course, the first two sets of areas could only be distinguished retrospectively, when the location of action was known. Our data were hierarchical.
For the survey we covered eleven Safer Cities and eight comparison cities. In these, we sampled over 400 high-crime Census Enumeration Districts (EDs) – areas of about 200 households. The EDs generated some 5,800 respondents, who gave over 7,500 interviews (some were interviewed twice – we used an embedded panel design). Half were interviewed before much Safer Cities action had been implemented (1990), half after (1992).
For the recorded crime data we covered fourteen Safer Cities (with comparison indicators derived from nine other cities). These were broken into 701 police beats (average 2,200 households), each with repeated measures for up to six 'beat-years' from 1987-1992 (according to data availability), making a total of nearly 3,300 observations.
To filter out extraneous factors, such as demographic differences between areas or survey respondents, and background trends in crime, we used statistical modelling (multi-level linear regression with ML3 – Goldstein, 1995). This sought to explain the variation in the risk of burglary over time and between areas, cities and respondents, as appropriate.

This paper is a summary of results reported in full in Ekblom et al. (1996). We first present the findings on Safer Cities impact on burglary from the survey;

then those from the analysis of recorded crime. The results for the survey and the recorded crime analyses are very similar. We then ask the key question: were the Safer Cities schemes value for money? We finally return to the survey to consider some of the less tangible consequences of Safer Cities in terms of people's perceptions of their neighbourhood, and worry about burglary. We also examine the consequences for security-related behaviour, including membership of Neighbourhood Watch and the installation of home security measures.

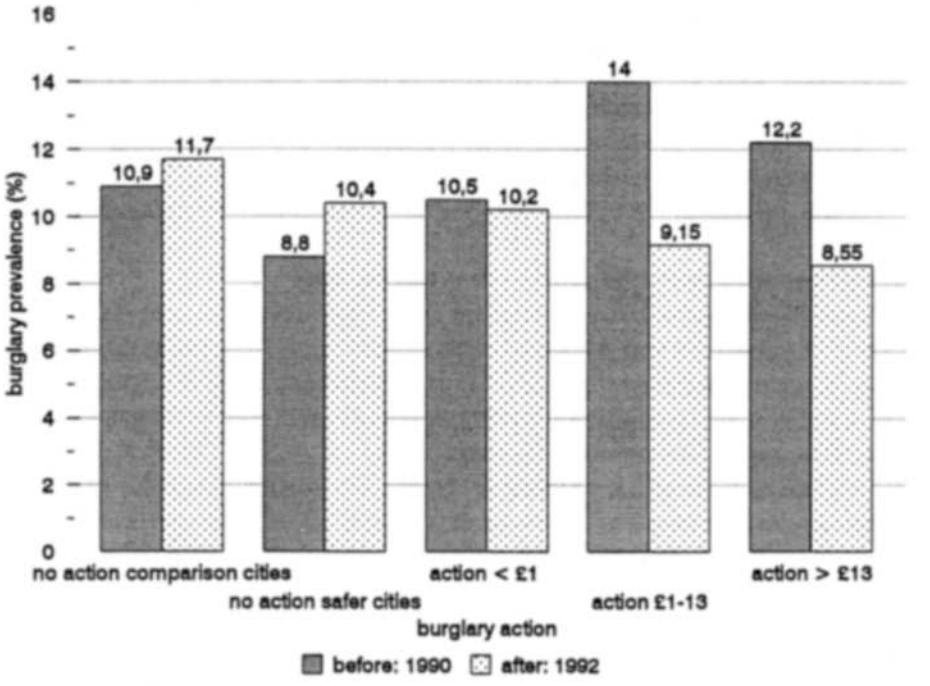
The survey

Our survey obtained a good-sized sample of areas with local action despite this being widely scattered over each city. Of the 300 local schemes targeted on residential burglary, 96 were covered; these were broadly representative, although somewhat larger. They fell in 117 of the surveyed areas. (Some schemes covered more than one area, and some areas received more than one scheme.)
In the 117 surveyed areas in which there was Safer Cities action, the intensity (including levered-in funds) varied from 1p to £ 113 per household over the year preceding the after-survey. The average was £ 16. A distinction was made between EDs in which under £ 1's worth of total action was present per household over the year ('low-action'); £ 1-£ 13 ('medium-action'); and £ 13+ ('high-action' areas).

Did the survey show a Safer Cities effect?

Figure 1 shows how the proportion of households burgled one or more times in a year changed between the 'before' surveys, and the 'after' ones. (These are risks of burglary prevalence, excluding attempts. They are unweighed; variables that would normally be included in weighting are instead incorporated within the statistical model described below.) There are five sets of surveyed EDs. From the left, we have the EDs in the comparison cities; those in the Safer Cities with no action; those with low action; medium action; and high action. Before any Safer Cities action, burglary risks were somewhat higher in the comparison cities than in Safer Cities, reflecting no more than inevitably imperfect matching. Between 1990 and 1992, burglary risks in the comparison cities rose (relative to the before-risk) by 7%; the areas in the Safer Cities where there was no action on residential burglary actually showed a bigger rise, of 18%. However, in areas where there was action, risks fell: by 3% in the low-action areas, by 35% in the medium-action areas and by 30% in the high-action areas. Burglary incidence (the number of burglaries per hundred households) showed a broadly similar pattern. Concentration (the average number of burglaries per burglary

Figure 1: Domestic burglary prevalence before and after implementation (survey)



victim) showed no consistent rise with action, suggesting that the burglary action had not exacerbated repeat victimization.

Regression-to-the-mean?

Safer Cities was meant to target high-crime areas. However, if coordinators targeted areas with temporarily extra-high crime levels, then a downturn in crime might follow whether or not the action itself worked ('regression-to-the-mean'). This would mimic a Safer Cities effect. As figure 1 shows, the prior burglary levels in Safer Cities EDs with medium or high action were indeed markedly higher than in EDs which received less or no action. Could regression-to-the-mean explain away the Safer Cities effect? Three lines of evidence counter this.

1 The tendency for more action to be focused on areas with higher prior burglary risks is rather unreliable, varying strongly between EDs.
2 Safer Cities coordinators consistently stated that targeting of high-crime areas was more on the basis of stable 'bad area reputations' and longer-term high rates of recorded crime, than on short-term 'blips' (Sutton, 1996).

3 Recorded crime data were available going back yearly from 1992 to 1987 for a large number of the surveyed EDs in the Safer Cities. Each surveyed ED was linked (using a geographic information system) to the police beat in which it was sited, and assigned the recorded crime rates of that beat. The areas which subsequently received higher levels of action clearly did tend to have a consistent long-term history of higher recorded burglary rates – they were not just recent fluctuations. This indicated rather conclusively that regression-to-the-mean cannot explain away the Safer Cities effect.

Explaining variation in burglary victimization risks

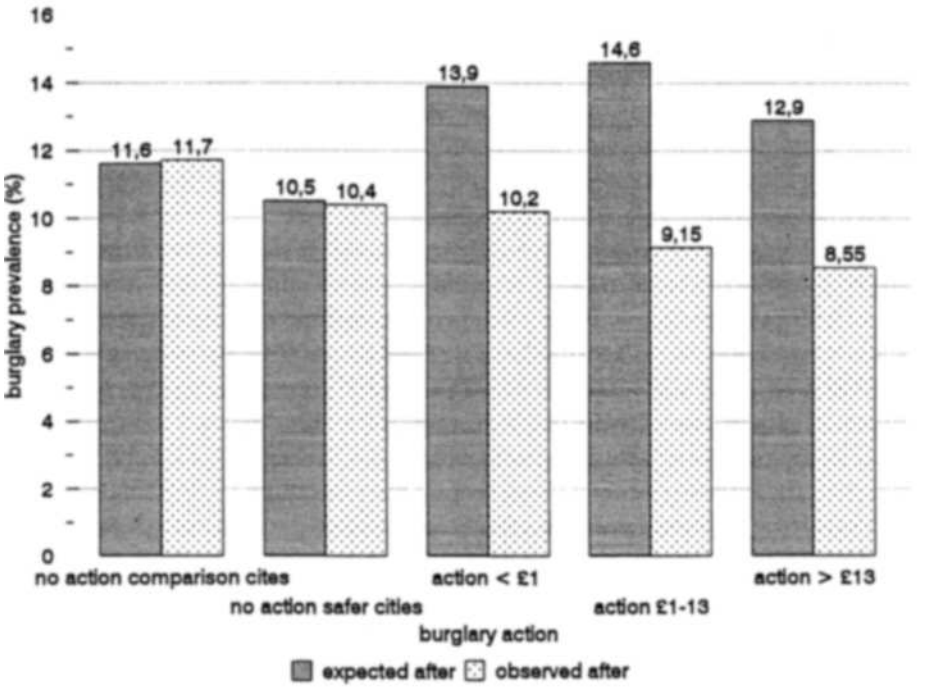
Although regression to the mean was ruled out, figure 1 still remains only prima facie evidence for Safer Cities impact on burglary, because it shows the relationship between just three factors – time, location and action. The statistical modelling (hierarchical logistical regression) enabled us to take account of a wider range of demographic factors.

Overall, net of all the other explanatory factors included in the analysis, Safer Cities burglary action in an ED was associated with a reduction in risk in the after-survey. The 'Safer Cities effect' was not straightforward. Unexpectedly, the mere presence of burglary action seemed to reduce the risk of burglary quite markedly. This could be called the step effect of action. Beyond and above the step effect, the greater the intensity of action, the greater the reduction in the after-risk. This could be called the marginal effect of action. The two effects together give a measure of the overall impact of the Safer Cities action.

Neither step nor marginal effects are constant, but vary with the prior burglary level of the ED where the action was located. The step effect appears to grow somewhat stronger, the higher the prior burglary level in an area. This may mean it is easier to reduce burglary in areas at higher risk, perhaps because offenders are not accustomed to much preventive action and respond more readily. But it could merely be a measurement phenomenon. However, the marginal effect actually fades out in areas with higher burglary levels. Interestingly, this is the opposite of what would be expected with regression-to-the-mean.

Figure 2 focuses on burglary prevalence in the after-survey only, to illustrate these findings from the statistical model. It compares, for each of the sets of surveyed EDs, what was actually observed in the after-survey with our best estimate from the model of what we would have expected to have found in the same areas, had the Safer Cities action not been implemented, but all else had remained the same. From left to right, the EDs in the comparison cities and the Safer Cities with no burglary action both show the observed prevalence close to the expected. However, all three sets of EDs with Safer Cities burglary action show the observed prevalence in the after-survey to be markedly less than

Figure 2: Domestic burglary prevalence after implementation: expected and observed (survey)



expected. The step effect is visible as the common drop, in the three action sets, from observed to expected.

On the face of it, this evidence for Safer Cities impact is extremely welcome. However, before we can convincingly attribute the reductions in burglary risk to the Safer Cities action, we have to examine the part played by other Safer Cities action not targeted on burglary; and action outside the Safer Cities programme which may also have influenced crime.

The role of other Safer Cities action

Obviously, burglary was not the only target of Safer Cities action. Schemes were implemented to tackle other problems such as vandalism and disorder, or to reduce the propensity to offend. The presence of this 'other Safer Cities action' could well have affected burglary risks in the surveyed areas.

There was a strong tendency for burglary schemes to be located in areas which also had other action. It was therefore important to investigate whether the impact of the former was gaining strength from the latter. If this were so, our

estimates of the effectiveness of action targeted on burglary would be over-generous. Our original statistical model already included the 'other action' score, but this appeared, strangely, to be associated with an increase in risk. We therefore extended it to explore how burglary risk differed between EDs with, and without, other Safer Cities action. We compared three types of ED: those with burglary action alone (25 EDs); other Safer Cities action alone (96 EDs); and burglary plus other action together (92 EDs). Dividing the areas into these subsets considerably reduced the reliability of the findings, but the more robust ones are worth reporting for diagnostic purposes.

The impact of burglary action on burglary itself seems to depend on the presence of other action in the same area. The kind of burglary action implemented in the Safer Cities might not work by itself, even though the amount of action in the 'burglary action alone' areas was in fact quite high. The step effect of burglary action in particular seemed to disappear when there was no other action. (This may help explain why the step effect existed at all – after all, it is puzzling that the mere presence of burglary action in an ED substantially reduced risk even when the intensity of action was very small. (Low burglary action EDs, whilst receiving an average of only 11p of burglary action per household, were also receiving some £ 5.30 input of other action.)

The marginal effect of burglary action seemed more robust when accompanied by other action. There was some evidence of 'inward' crime switch: the presence of other action alone in an area appeared to increase the risk of burglary by possibly causing offenders to switch from other crimes to burgling homes. As a corollary to the last point, there may be a kind of 'protective' effect of burglary action: in areas where other action is accompanied by burglary action, there is no evidence of crime switch to burglary. Indeed, there may be a synergy – perhaps one that is necessary for the burglary action to work at all.

The role of action outside the Safer Cities programme

The Safer Cities programme did not exist in isolation. Urban areas with multiple problems received a great deal of remedial action – social, economic and architectural. Some of this other action is likely to have influenced burglary risks and its effects could, therefore, be confused with those of Safer Cities schemes targeted on burglary. If there was any tendency for Safer Cities coordinators to direct their schemes towards areas in receipt of extraneous action, then this could have boosted the measured impact of the Safer Cities schemes as a whole. Unfortunately, we could not measure such other action directly (it would have been a further major undertaking), so it cannot be ruled out as a factor in the results. But overall, our interviews with coordinators revealed they had no consistent tendency to site, or to avoid siting, schemes where extraneous action was present (Sutton, 1996). The coordinators had

to respond to a variety of policy considerations, and experienced a variety of constraints in deciding where to locate action.
Having eliminated a number of alternative explanations for the apparent Safer Cities effect, we can now estimate its size.

Size of reduction in risk

From the statistical model of burglary risk, we were able to produce numerical estimates of the overall and marginal impact of action. It is important to remember that the estimates relate to the impact of action on all households in an area – it is impossible from our data to estimate the impact of a certain sum spent on individual households. It should also be borne in mind that these are generalized estimates of impact in the kinds of areas we sampled. Unlike the reductions in risk in figure 2, they are not specific to the composition of areas and individuals in our sample. (For the moment they apply to all burglary schemes irrespective of whether these are accompanied by other action.)
At the burglary prevalence of 10% (average in our survey, but relatively high nationally), the best estimate of the step effect of action is that it reduced burglary risks by 29%. In other words, *the mere presence of Safer Cities action against burglary seemed to reduce the risk of burglary by over a quarter.* On the marginal impact, given the presence of action, for every additional pound of action per household the risk of burglary fell by a further 0.1%. Step and marginal effects combined showed an overall reduction of almost 30%.
Table 1 shows how these reductions vary with the prior burglary risk. We can see the fairly modest increase in the step effect with prior burglary risk, and the decrease in the marginal effect. At a prevalence rate of a little over 20%, the marginal effect drops out altogether, and in fact thereafter is linked to a rise in risk, which is difficult to interpret. However, as said, there were indications that this fade-out was confined to circumstances where burglary action was implemented alone, in the absence of other Safer Cities action. The impact on risk in the majority of burglary action covered by the survey, which was accompanied by other Safer Cities action, is indicated by the numbers in brackets in table 1. The step effect is rather less; the marginal effect is rather more, and it continues to exist at very high levels of risk. However, these estimates are less reliable.

Geographic displacement

If Safer Cities burglary action was doing no more than move some of the crime to neighbouring areas then, obviously, the cost-effectiveness picture would appear less favourable. We therefore took a close look at geographic displace-

Table 1: Reductions in burglary prevalence risk associated with Safer Cities action against burglary (survey results), in %[1]

prior burglary prevalence	reduction in risk[2] step effect[3] 6	7	marginal effect[4] 6	7	overall effect[5] 6	7
3[8]	27	(17)	0.18	(0.61)	30	(25)
5	27	(17)	0.16	(0.60)	30	(24)
10[9]	29	(16)	0.11	(0.57)	31	(23)
15	31	(15)	0.06	(0.54)	32	(22)
20	32	(14)	0.02	(0.51)	32	(20)
25	32	(13)	-	(0.47)	33	(19)
30	33	(12)	-	(0.44)	33	(18)
35	34	(11)	-	(0.41)	33	(16)

1 The action input comprises both Safer Cities and levered funds.

2 The reductions are estimated relative to the expected risk in the after-survey, in the absence of Safer Cities action (*not* proportional falls from the prior burglary risk).

3 'Step effect': the reduction in risk associated simply with the presence of Safer Cities action in the relevant ED in the year of the after-survey. (It should be noted that while the step effect of the burglary plus other action decreases, this does not imply an increase in the step effect of burglary action alone. Rather, under these conditions some of the strength of the step effect has been transferred to the corresponding marginal effect.)

4 'Marginal effect': the further reduction in risk for an extra £ 1 of action per household, beyond the average (£ 16), spent in the ED at the time of original implementation.

5 'Overall effect': the reduction in risk associated with the presence of Safer Cities burglary action in an ED, at the average intensity of £ 16 per household over the year preceding the after-survey.

6 This column: all burglary action.

7 Column in brackets: from EDs where burglary action is accompanied by other Safer Cities action (less reliable).

8 The 3% burglary prevalence risk is the national average from the British Crime Survey.

9 The 10% risk is the average for the present survey.

ment. We took account of any burglary action in rings of EDs that surrounded each surveyed ED in the Safer Cities (the 'bull's-eye'). This was 'extra' action only; it excluded schemes which covered both the surrounding neighbourhood and the surveyed ED itself. 'Extra adjacent action' scores were generated for each surveyed ED and these were incorporated in a slightly simplified version of the existing statistical model. We distinguished between our surveyed EDs on the basis of whether or not they had burglary action in the bull's-eye, and whether or not they had extra burglary action in the surrounding ring. Of the 280 surveyed EDs in the Safer Cities, 109 had extra burglary action in the ring. The findings are tentative, but in summary suggest the following results. Burglary schemes in the bull's-eye and adjacent rings seem to work together to reduce the risk in the bull's-eye, often to a substantial degree. For example,

in the 28 EDs with action in the bull's-eye and extra action in the immediately adjacent ring and an additional outer one, the overall reduction in risk is in the 60-70 percent range. Burglary action in the bull's-eye seems to deflect displaced burglars elsewhere.

When there is no action in the bull's-eye, the direction of the effect of extra adjacent action depends on its intensity. With low amounts of adjacent action, there is an overall increase in risk in the bull's-eye (for example, an almost 70 percent increase in risk with £ 1 extra action intensity in the rings). Burglary is therefore displaced into the bull's-eye. With high amounts of adjacent action, by contrast, the marginal effect prevails and there is an overall decrease in risk in the bull's-eye (for example, a decrease in risk of nearly 80 percent). In these circumstances, the more intense action may have driven offenders further off, caused them to switch to other targets, or forced them to give up altogether. This latter case may therefore be diffusion of benefit, although to be certain, we would have needed to measure burglary risk over a wider area. (Our survey design confined us to measuring outcome in the bull's-eye only.)

Did some types of action work better?

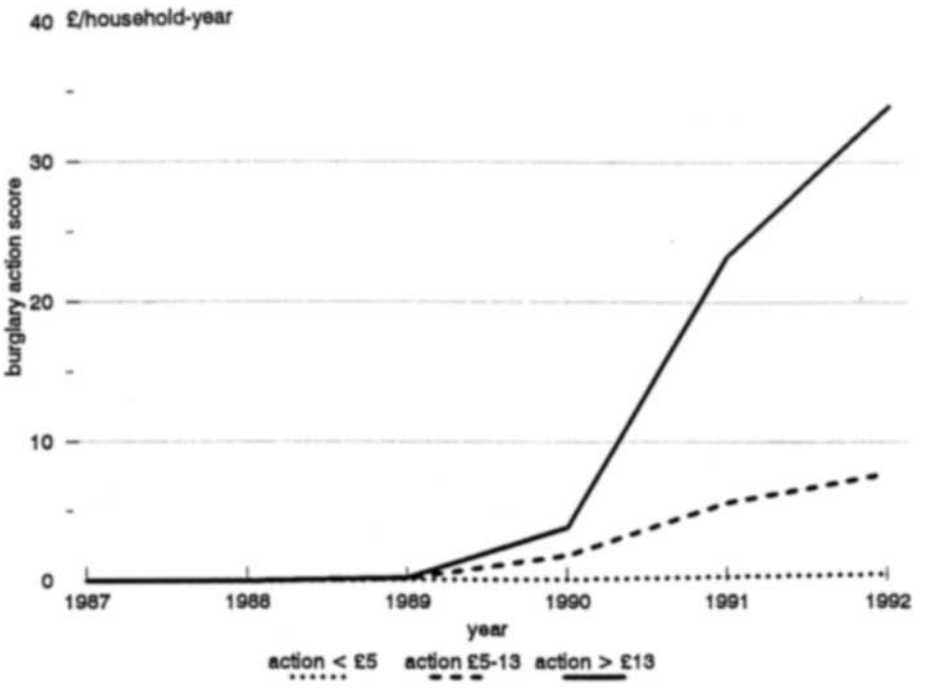
Basically, the answer here is no, since there were too few distinct differences in the types of action that schemes took. Nearly all schemes took some target-hardening action, and where it appeared a scheme's main focus was something else (e.g., fostering Neighbourhood Watch, or general anti-burglary publicity), target hardening was often implemented through other schemes in the same area. Moreover, coordinators indicated that they tended to implement 'other' action in areas which had already had target hardening installed by agencies outside the Safer Cities Programme. There appeared, however, to be a syndrome whereby 'target hardening, plus other action' had a particularly strong effect per pound of input. This was consistent with Tilley and Webb's (1994) finding (based on case studies of 12 Safer Cities burglary schemes) that comprehensive approaches to target hardening seemed especially beneficial.

The recorded crime statistics

As with the survey, we identified local Safer Cities schemes targeted on residential burglary which were in the right time and place to link up with our outcome measures. We succeeded in covering 240 schemes out of the total of 300 current or completed by summer 1992.

Almost half of the beats (325 out of 701) had burglary action at some point. These units of place we call 'action beats'. We calculated the burglary action score as the average input of Safer Cities funds per household in the relevant

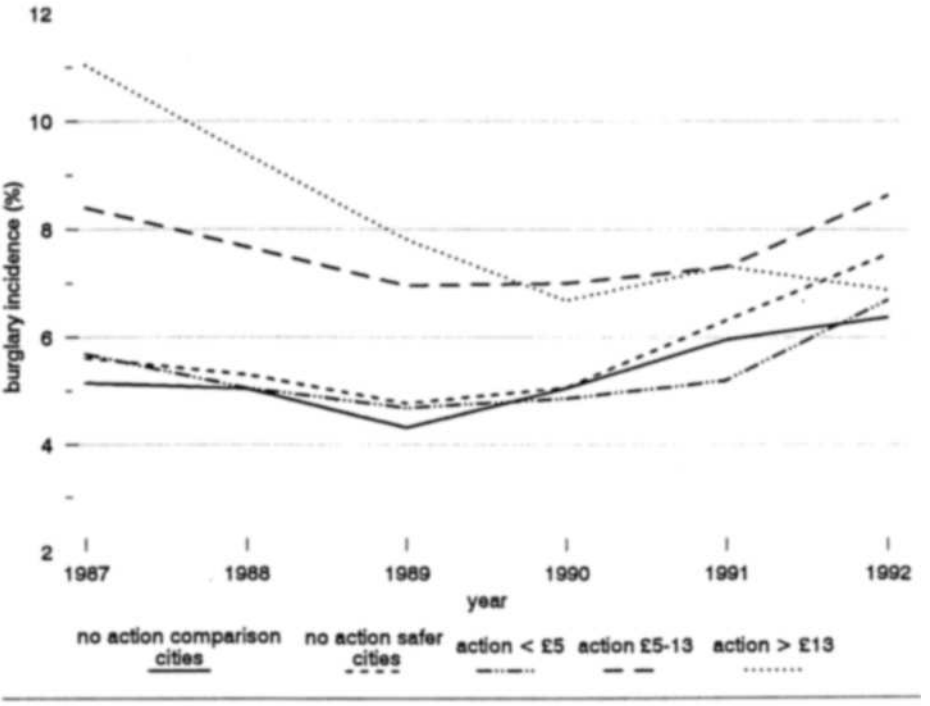
Figure 3: Burglary action scores 1987-1992



beat and over the year in question. While the scores in the survey had a once-only value (i.e., for 1992, the year of the after-survey), the scores for the recorded crime analysis were calculated separately for each beat-year in which there was action. These we call 'action beat-years'. They are units of both time and place. Altogether out of 3,277 beat-years for which we had recorded crime data, 734 had some action, mostly after 1989. The average action intensity in each of these action beat-years was just over £ 3.50 per household, combining Safer Cities and levered money.

For purposes of presentation, we divided the beats into sets on the basis of the total action present in this final year. There were 375 beats which never had action; 266 which ended up in 1992 with under £ 5-worth of action (average just under 50p); 26 with action between £ 5-£ 13 (average nearly £ 8); and 33 with action over £ 13 (average £ 34). Figure 3 shows, for these sets of low-, medium- and high-action beats, the time course of action over the years 1987-1992. The action in each set starts to appear between 1989-1990, and reaches highest cumulative levels in 1992.

Figure 4: Domestic burglary incidence 1987-1992 (recorded crime)

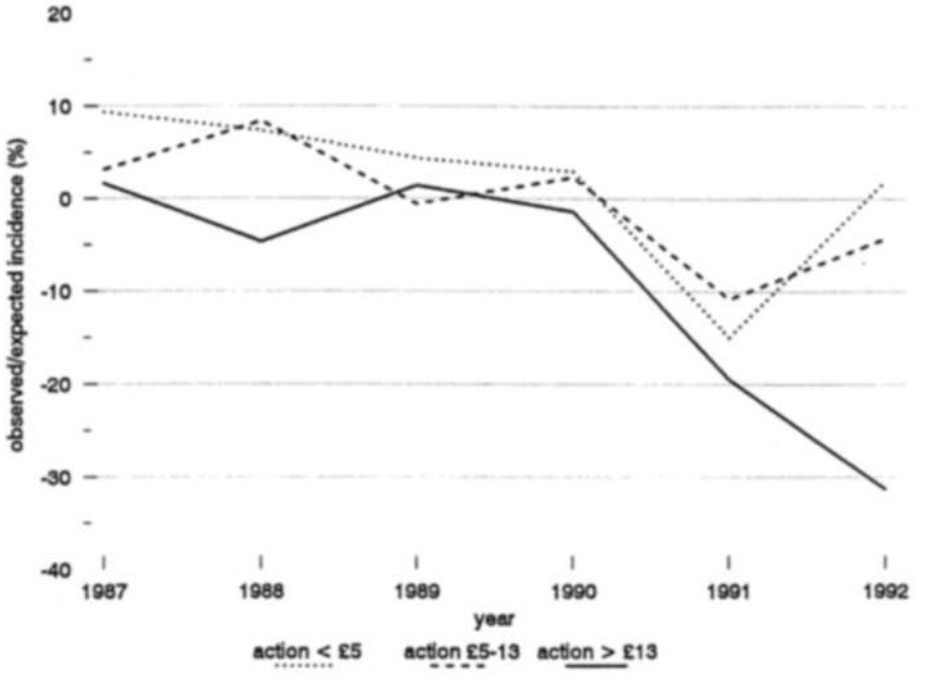


Did the recorded crime data show a Safer Cities effect?

The recorded crime data were adjusted by population to produce burglary incidence rates for each beat-year. Figure 4 shows the average incidence rates for the low, middle and high sets of action beats, as they changed over time. It also presents the same burglary trends for two other series: the 375 beats with no burglary action, and the global comparison indicator, a weighted aggregate of the nine matched comparison cities.

Several things are apparent from figure 4. First, there is a trough in each series at about 1989 or 1990, corresponding to a trough in national crime rates at that time. Second, as with the survey, the middle- and high-action sets start off with markedly higher risks of burglary. Third, while all other series continue to rise through to 1992, the high-action set alone shows a return to a falling trend. These patterns show some prima facie evidence of a Safer Cities effect, but this is confined to the high-action set. There is, moreover, a possibility that the final fall is no more than a resumption of the earlier fall.

Figure 5: Domestic burglary incidence 1987-1992 (recorded crime) relative to expected incidence



Explaining variation in burglary incidence risks between beat-years

The statistical modelling sought to take account of several features of the crime data which could have masked, or mimicked, a Safer Cities effect. These include the high burglary risk in the early years in the middle- and high-action beats, and their steeper decline, from 1987, before Safer Cities action was implemented. Results from modelling confirm the indication in figure 4 of a possible Safer Cities effect.

Net of all the other explanatory factors included in the analysis, Safer Cities action in a beat, in a given year, was associated with a reduction in risk. Again, the mere presence of action in a particular beat-year, and the intensity of that action, showed independently measurable reductions in risk.

Figure 5 presents the differences between observed and expected burglary risk, as a proportion of the expected risk level. Up to 1990, this figure remains close to zero and fairly flat for each beat set, indicating that the beats with action were displaying trends that were expected on the basis of all the extraneous

factors taken into account in the model. In 1991, however, all three sets show marked dips (ranging from 10-20 percent below expected). The only ones to continue below expected in 1992, though, are the medium- (4 percent below expected) and high-action beats (30 percent below, continuing on down). This suggests (but does not prove) that the effects of action of lesser intensity in a beat may be rather more short-lived.

The role of other Safer Cities action

It was hard to disentangle the effects of other Safer Cities action from those of burglary action, because over 90 percent of beat-years with burglary action also had other action present. We did, though, confirm the survey finding that where other action was present but burglary action was absent, there seemed to be an increase in risk of burglary, albeit statistically not very robust. This suggested (like the survey) that the other action may have led to a crime switch into burglary, fended off by the presence of burglary action.

Size of reduction in risk

Table 2 presents the estimated reductions in burglary incidence risk associated with the presence, and intensity, of Safer Cities action. Again, we are dealing with step effects (from the mere presence of Safer Cities action in a particular beat, in a particular year), marginal effects (from an extra £ 1-worth of action invested) and overall effects. The recorded incidence risks in this table are approximately equivalent to the prevalence risks in table 1 (for example a recorded incidence risk of 10 percent is equivalent to a 'real' incidence risk of 15 percent due to under-reporting of crime. But since we found an average 1.5 incidents per victimized household, a 'real' incidence risk of 15 percent is equivalent to a 'real' prevalence risk of 10 percent, in the survey.)

At a 10 percent incidence level of risk (equivalent to the average prevalence risk in the survey) the mere presence of Safer Cities burglary action seemed to reduce the risk of burglary by about 7 percent. On the marginal impact, given the presence of action at the average intensity (£ 3.57), for an additional £ 1 of action the risk of burglary fell by a further 0.8 percent. Step and marginal effects combined showed an overall reduction of some 10 percent at the average action intensity.

These estimates differ from the main figures from the survey in table 1. However, the recorded crime estimates in table 2 were based on action beats in virtually all of which, the burglary action was accompanied by other action. The differences are rather less if we compare like with like, and use the estimates in brackets in table 1 – from only those surveyed areas in which burglary action was accompanied by other action. Reasons for differences that

Table 2: Reductions in burglary incidence risk associated with Safer Cities action (recorded crime results), in %[1]

baseline burglary incidence	reduction in risk[2] step effect[3]	marginal effect[4]	overall effect[5]
3[6]	11	1.1	15
5	9	1.0	13
10[7]	7	0.8	10
15	6	0.7	9
20	6	0.6	8
25	5	0.6	7
30	5	0.5	6
35	4	0.5	6

1 The baseline incidence rates in these example calculations have been chosen as equivalent to the corresponding prevalence rates for the survey results in table 1. (For every victim in the current survey there are an average 1.5 incidents; for every surveyed incident there are an average 0.66 recorded incidents, from the 1992 British Crime Survey.) The action input comprises both Safer Cities and levered funds.

2 The reductions are estimated relative to the expected risk on a given beat-year, in the absence of Safer Cities action.

3 'Step effect': the reduction in risk associated simply with the presence of Safer Cities action in the relevant beat in the relevant year.

4 'Marginal effect': the further reduction in risk for an extra £ 1 of action per household, beyond the average (£ 3.57), spent in an area at the time of original implementation.

5 'Overall effect': the reduction in risk associated with the presence of Safer Cities burglary action in an area, at the average value of £ 3.57 per household over the year.

6 The 3% burglary risk is equivalent to the national average from the British Crime Survey.

7 The 10% risk is equivalent to the average risk for the present survey.

remain are discussed in the full report (Ekblom et al., 1996), but the most obvious is that the larger area of the police beats than the survey EDs considerably reduced the average intensity of action in the former. It is thus not surprising that the step effect was somewhat smaller with the recorded crime. (Since this difference in average intensity is taken account of in the costing – below – the cost estimates from the two sources come considerably closer.)

Sort of money to prevent a burglary

We converted the figures in Table 2 into estimates of the average amount that one would need to spend, on local action of the kind and quality implemented in Safer Cities, to prevent one burglary incident. (We costed for a 'real' incident – not a 'recorded' incident, which would have cost about 1.5 times more to prevent.) There are two types of cost estimate that are of interest to decision

makers concerned with implementation of cost-effective preventive action. Overall cost is the cost of preventing one burglary, taking all the Safer Cities effects into account – both the presence of action and the amount of action. This figure informs the decision 'Is this preventive action worth implementing at all?' Marginal cost is of interest when the first decision has already been taken. It is the cost of preventing one more burglary through extra action. This figure informs the decision 'Given that we have already decided to set up some preventive action, how much should we implement in the target area?' All other things being equal, marginal costs are likely to be greater than overall costs, because they ignore the effects of the mere presence of action.
Both Safer Cities scheme expenditure and leverage are included in our cost estimates. Local and central administrative costs over the lifetime of the programme were also taken into account (adding 50p overhead to every £ 1 spent on scheme funding). Finally, we assumed that any effect of Safer Cities action would endure for two years.

Overall costs
It was not straightforward to calculate the cost estimates from the values obtained in the statistical model and our other cost figures just described. In particular, the 'mere presence' of action (the step) had no cost dimension. To produce a sensible answer, we costed this at the average input intensity of £ 3.57. Since there was some evidence that the step effect gained strength from the presence of other Safer Cities action, it is reasonable to add the average amount of other action – namely £ 7.73. (The other action, may, of course, reduce other types of crime risk – but this cannot be estimated.) In total, the average input associated with the presence of Safer Cities burglary action plus other supporting action was taken to be £ 11.30, including levered funds.

Marginal costs
The marginal cost is, as said, how much more money needed to be invested in an area, at the time the action was originally implemented, to prevent one more burglary. The marginal cost of preventing one extra burglary was estimated in a similar way to the overall cost, with one additional step. This involved comparing the estimated overall effect on risk of £ 3.57-worth of burglary action (the average intensity) and £ 4.57-worth (the input of other action is taken into account by multiplying the burglary-action-only cost figure by average inputs of [burglary + other] / burglary).
The following estimates of overall and marginal cost from the recorded crime analysis have wide margins of error (from statistical uncertainty and from assumptions we had to make in calculation). Equivalent cost estimates from

the survey (for areas with burglary plus other action only) are in brackets. They are remarkably close.
Where risks are very high – where there are 20 recorded burglary incidents per hundred households in a year – the Safer Cities cost of preventing one ('real') burglary is estimated to be about £ 360 (£ 200) overall, and £ 1,300 (£ 900) at the margin.
Where risks are high – in an area where there are 10 recorded burglary incidents per hundred households in a year – the cost of preventing one burglary is estimated to be about £ 550 (£ 400) overall, and a little over £ 2,000 (£ 1,500) at the margin.
For those in areas with a risk of 3 recorded incidents per hundred households in a year – equivalent to the nationally average prevalence risk from the British Crime Survey, of 3% – the cost of preventing a burglary is estimated at a little over £ 1,400 (£ 1,400) overall, and about £ 4,700 (£ 4,800) at the margin.
The figures for both overall and marginal costs show that when burglaries are common, it needs less expenditure in an area to prevent them than when burglaries are rare. This is consistent with common sense. A risk reduction of a fifth in an area suffering from a risk rate of six percent prevents twice as many burglaries for the money, as the same one fifth reduction in an area with a risk rate of only three percent. This underlines the importance of targeting, which the Safer Cities coordinators found difficult for various practical and policy reasons (Sutton, 1996).

Were the Safer Cities burglary schemes value for money?

To assess this requires estimates of what it cost, under various conditions, to prevent a burglary through Safer Cities action, to be set against the costs of burglary itself, to victims and the State. This comparison cannot be made with perfect rigour, even though our methods have taken us much further than other programme evaluations in assessing whether the preventive action was good value for money. For one thing, there are the inevitably complex problems discussed above in assessing the costs of preventing a burglary in Safer Cities action areas, and of knowing for certain whether the reduction in risk observed was entirely due to Safer Cities.
There is also the uncertainty of knowing how much crime in the action areas was displaced elsewhere. If one allows, conservatively, for the possibility of some displacement, then cost estimates would obviously be higher than we have given, because some of the apparent reduction in risk is shifted elsewhere into higher risks for others. By the same token, if diffusion of benefit extended the effects of higher-intensity burglary schemes beyond their intended (and funded) boundaries, then our cost estimates would be too high. Evidence for

both displacement and possible diffusion was found under different conditions in the survey analysis (it was not possible to do this for the recorded crime in the time available), and the conclusion there was that the most sensible position to adopt for costing was a neutral one.

1992 British Crime Survey figures indicate that burglaries with entry, on average, cost victims living in 'Inner City' areas (the nearest equivalent to where the Safer City action was targeted) about £ 800 gross. About half of this, on average, is recouped through insurance – though this does not take away the cost, but merely redistributes it, socially, in terms of the cost of insurance premiums.

A current Home Office estimate of the cost of a residential burglary to the criminal justice system – for the police, courts and prisons, etcetera – is about £ 300. (This is for a 'real' burglary, not a 'recorded' one.) The total financial cost of a burglary to victim and state is therefore £ 1,100.

In areas of high burglary risk (10 recorded incidents per 100 households), our estimate of the overall costs of preventing a burglary amounts to £ 550, with levered-in funds and administrative costs taken into account. The marginal cost is about £ 2,000. From the recorded crime analysis, typical Safer Cities burglary action would easily pay its way in overall terms, and is not far short in marginal terms. Only in areas of national average risk (3 recorded incidents per 100 households in a year) does the overall cost of prevention (£ 1,400) exceed the direct costs of burglary. (The marginal cost, at £ 4,700, is considerably greater.)

Very similar cost estimates emerged from the survey (although the marginal costs were much higher, if we included the minority of burglary schemes that were implemented without the 'boost' from other Safer Cities action). It is sometimes claimed that the introduction of a crime prevention scheme raises the proportion of crimes in an area that are reported to the police. This would mask the impact of the action on recorded crime. We found, among our surveyed burglary victims, an above-average increase in reporting in the low-action EDs but a fall in the high action EDs. This fall was based on a small sample of victims (only 81 interviews produced victims in the high-action EDs). But if it was more widely representative, it could have caused the recorded crime analysis to overestimate the size of impact.

The main report explores reasons for remaining differences and concludes in favour of the estimates from recorded crime, whilst acknowledging that the marginal cost may be somewhat higher. In particular, while there were no consistent changes in reporting burglary to the police across all burglary action areas, the areas with the highest action showed a decline in reporting. On the other hand, because the survey covered deprived areas it had a rather low response rate (60%), which may have introduced bias. Given the greater size of the police beats, it is likely that the recorded crime estimates will have automati-

cally taken account of a proportion of any displacement or diffusion of benefit that occurred. This is because some of these unintended consequences will have been contained within the beat boundary. But accounting for displacement is by no means complete.

We did not assess 'crime switch' from burglary to other offences. (This has not yet been explored. There was some evidence of crime switch to burglary when Safer Cities action not targeted on burglary was implemented in the absence of burglary action.) Taking a conservative view, we should continue to regard the current cost estimates as gross local estimates – i.e., applying to the gains in the action areas only. However, we should also take note of the indication that diffusion of benefit may have outweighed displacement in areas with more intense burglary action.

This means that we can conclude, moderately confidently, that on the financial balance sheet, the Safer Cities action against residential burglary was cost-effective, when targeted on areas above the national average burglary risk. Taking account of any non-financial benefits of action, such as the avoidance of misery, upset and worry suffered by burglary victims and fellow residents, would make for an even more favourable picture. Some of these are explored next.

Impact on crime prevention behaviour and worry about burglary

The survey enabled us to look for evidence of the impact of Safer Cities burglary schemes on membership of Neighbourhood Watch, domestic security levels, people's worry about burglary and their perceptions of the local area.

Neighbourhood Watch

Only one scheme covered by the survey explicitly set out to establish Neighbourhood Watch (through the employment of a community worker), although nine others also employed 'outreach workers' or sought to raise the profile of community safety through publicity. However, according to our interviews, membership of Neighbourhood Watch went up by over 70 percent in high-action areas, compared to under 7 percent in no-action areas, and 5 percent in the comparison cities. Could the increased Neighbourhood Watch membership in the high-action areas actually have contributed to the Safer Cities effect? Statistical analysis found no difference in the reduction in burglary risk, between high-action areas with (what appeared to be) old Neighbourhood Watch schemes, new schemes and those without any at all. However, low- and middle-action areas only showed a reduction in risk when Neighbourhood Watch was present. Neighbourhood Watch may therefore have been a

necessary ingredient for lesser amounts of Safer Cities burglary action to work, while more intense action seemed to function adequately alone.
This may help explain the rather puzzling impact of the mere presence of Safer Cities action, and is consistent with the better performance of 'target hardening plus' burglary action, and 'burglary action plus' action against other crime problems. (Alternatively, the presence of Neighbourhood Watch may have made no contribution in itself, but merely indicated greater social cohesion in an area capable of founding a Watch scheme. The cohesion may have been the necessary ingredient.)
The overall effect of Neighbourhood Watch (with or without Safer Cities burglary schemes present in the area) seemed to be associated with an increase in risk. But it did appear that the presence of tangible Safer Cities burglary action was necessary for Neighbourhood Watch to work. (However, we did not set out to conduct a full and fair test on the effectiveness of Neighbourhood Watch, it must be said.)
Comparison with the Kirkholt burglary prevention project is instructive here (Forrester et al., 1988, 1990). This demonstrated that a combination of Neighbourhood Watch and target hardening worked well.

Domestic security

People were also asked, in the after-survey only, about a range of crime prevention measures they had taken in their home over the previous two years, or which their landlords had taken for them. We focused on the kinds of measures implemented within the Safer Cities schemes in the surveyed areas – mostly door locks, bolts, chains and viewers, and window locks. In some cases, the Safer Cities scheme had been directly involved in supplying and fitting the equipment; in others, there was more 'promotional' activity in publicizing available devices. From the survey data, we calculated a score representing the number of such measures installed.
The average number of domestic security measures installed proved to be greater in the high-action areas, relative to the low- and middle-action areas. There were, though, some complications – for example, there was a greater increase in the numbers installed in the comparison cities.

Perceptions of local area

Safer Cities was also intended to improve general community life. Although burglary is only one kind of crime – and 'incivilities' such as litter and vandalism have been shown to be more closely associated with people's feelings about their area – the results are interesting. People were asked to say whether the

area within 10 minutes' walk of their home was a good or a bad place to live. There were increases in the proportion perceiving their locality as bad in every type of area surveyed, including the comparison cities, except the areas receiving high levels of burglary action, which showed a 13 percent decrease. There appears to be a threshold below which action fails to make people feel happier about their surroundings. Below that threshold, it may serve only to draw attention to an area's problems.

Worry about burglary

Reducing fear of crime was the second Safer Cities objective. Worry about burglary fell more in the comparison cities. Those in areas selected for action were significantly more worried about burglary initially, and this is not surprising. But the more action was taken the significantly more worried householders were in the after-surveys. Why did this occur? One explanation is that coordinators had targeted areas where fear was rising. Another is that action itself awakened fear by focusing attention on the burglary problem.

There is an important difference, though, between those people in scheme areas who were aware of action, and those who were not. Awareness of action was uniformly low – and this is true of any kind of preventive action, implemented by any agency, anywhere in the city or borough (it was difficult for people to distinguish Safer Cities from any other action). Only in Safer Cities EDs which received high action, was there an increase in the proportion of people saying they were aware of action (from 17% to 25%).

We looked at whether there was a 'Safer Cities effect' on worry separately for those who were and were not aware of preventive action, as broadly defined. This showed that generally people who were aware of any action experienced reduced worry. Further, the more Safer Cities action was taken, the less they were worried. The one exception to this was the people in the low-action EDs: they showed a nearly 10 percent rise in worry, even if they were aware of action. This picture is broadly confirmed by more detailed statistical analysis which sought to explain variation in people's worry, taking account of some additional extraneous factors such as respondents' gender, and whether they lived alone, or had been burgled.

There are difficulties in establishing a firm link between people's general awareness of crime prevention action, and their awareness of Safer Cities action in particular. Nevertheless, it is helpful to try to establish a coherent view from our results, even if a fairly speculative one.

First, it seems that unless action is particularly intensive, or of a public nature such as Neighbourhood Watch, people remain unaware of what is being done, even if it is in their immediate locality. The only increase in awareness (just

over 50 percent) was in the small areas receiving high action. This seems to have limited the impact of Safer Cities schemes generally on worry about burglary.

Second, where people are aware of action, that action will only have a measurably beneficial effect on worry if it is substantial either in terms of the numbers of households targeted, or the amount of action per household, or both. This action may serve to reduce worry either indirectly, by reducing 'real' burglary risk, or directly (and subjectively), by convincing people that something substantial is being done to tackle their local burglary problem. Weak action (or action implemented in only a few households in the neighbourhood) may serve only to draw attention to burglary without reassuring those few householders who are aware of it, that something is being done for them. This may be so even though (as our earlier analysis showed) it is the presence of action as much as the amount which reduces objective burglary risk.

What produced the Safer Cities effect on burglary?

Among the findings we have so far reported are the following.

1 There was only limited awareness of the Safer Cities Programme and local preventive action among those exposed to it.
2 There was only a modest tendency for households within the Safer Cities action areas to report having more home security measures installed during the main phase of Safer Cities activity.
3 Both main analyses of Safer Cities' impact on burglary – survey and recorded crime – suggested that the presence of action was as important in reducing risk as the intensity of action, if not more so.
4 'Mixed' action against burglary seemed to perform better than target hardening alone.
5 Other Safer Cities action (not targeted on burglary) seemed to have an important role in shoring up the burglary effect.
6 There was an increase in Neighbourhood Watch membership in high-action areas in the survey, although it was only low levels of action that seemed to need the extra presence of Neighbourhood Watch for the action to work. And Neighbourhood Watch itself appeared to work only in the presence of tangible Safer Cities action.
7 Geographic displacement seemed to have occurred from action areas to adjacent zones, but also, possibly, diffusion of benefit when action was high-intensity. Burglary action in an area seemed to protect against geographic displacement from other schemes in adjacent areas, deflecting it elsewhere perhaps; and against crime switch into burglary from schemes in the same area but targeted on different offences.

There is an apparent paradox between the tangible impact of Safer Cities action on burglary, and people's lack of awareness of that action, and only weak evidence of increased domestic target hardening. How, then, did the Safer Cities schemes have their impact on burglary? This evaluation was not designed to explore the causal mechanisms by which action may have led to outcome (Pawson and Tilley, 1994; Ekblom and Pease, 1995). A detailed study of individual schemes would have been a more appropriate strategy. But it is important to impose some sense on the results, although not all of the above observations are underpinned by rock-solid evidence. A number of possibilities emerge, which relate to measurement issues and mechanisms themselves. The measurement issues (such as the difficulty of getting accurate reports from survey respondents) are left aside here.

Preventive action against burglary may operate at two levels – individual and area – and in two ways – heightening objective effort and risk to burglars, and heightening subjective perceptions of effort and risk. With the protection of individual homes, the Safer Cities action could have physically blocked the offence, or made burglary seem more risky and less rewarding. At the area level, offenders may perceive that security has been enhanced in a particular neighbourhood, and avoid the whole area. Whether the area is objectively more risky for them (bristling with active Neighbourhood Watchers scanning across improved sight-lines, perhaps), or whether the risk is only subjectively perceived (the mere knowledge that something has been done to tackle burglary in an area) may not matter. Individual householders need not be aware of the presence of action in their neighbourhood, nor even of security measures installed in their homes for the action to have its impact through subjective enhancement of risks to offenders.

To the question 'If householders seem not to be aware of preventive action, why should burglars be?' it may be that burglars are more sensitive to changes in security measures than ordinary, honest residents. One recent study has, in fact, demonstrated that burglars were markedly better than non-burglars at recognizing security changes, such as fitting of new locks, to photographed houses (Wright et al., 1995).

Many of the findings from both survey and recorded crime seem to point to the operation of area processes. The fact that the mere presence of burglary action reduces burglary risk, suggests this. Other supporting evidence – ironically – comes from displacement to adjacent areas, and the 'protective' effects of existing burglary action in an area (against both displacement and crime switch into burglary). Any diffusion of benefit in particular would imply that offenders are being guided by illusory risks beyond the boundaries of objective action, when that action is of sufficiently high intensity. The better performance of mixed methods rather than 'pure' target hardening, and the importance of support

from 'other' Safer Cities action (and Neighbourhood Watch in lower-action areas) suggest more broadly that specific security improvements on specific homes may not always be enough to achieve reductions in risk (a finding consistent with the experience of the Kirkholt project – Forrester et al., 1988 and 1990).

An overview

The fairly 'typical' Safer Cities burglary prevention schemes evaluated here seemed to reduce the risk of burglary. The mere presence of action was as significant as the intensity of action – perhaps more so. The overall cost per burglary prevented – albeit estimated with a fairly wide latitude of uncertainty – was under most conditions rather less than the direct financial costs of that burglary to the household and the criminal justice system. With some reservations (notably from the survey results) the marginal cost, of preventing one more burglary by investing more funds at the inception of a scheme, was also less than the cost of the burglary itself when burglary was common. Overall, the cost of preventing a burglary diminished in areas where burglary was more common (although reducing the risk of burglary seemed in some cases to become more difficult). The ability of coordinators to select the highest-crime area seems constrained by lack of routinely available and easily retrievable local crime data and other information. The possibilities offered by geographic information systems should be pursued. There was some evidence for displacement, but intense action may possibly have led to diffusion of benefit.
Less tangible gains in high-action areas, in any event, may include reduced worry, and increased confidence which may be reflected in wider social and economic benefits in the area. It needs to be said, here, that the findings on worry were not straightforward. It is quite usual for crime prevention evaluations to report little impact on crime but, as a 'consolation prize', that worry or fear went down. Paradoxically, this study found the reverse – tangible effects on crime, but little consistent impact on worry or on perceptions of improvement in the neighbourhood. The key to this seemed to be the overall lack of awareness of action except where this was most intensive. Future interventions should make sure that householders are aware that action is being taken. Such a message might have an additional effect in deterring offenders. (Tilley and Webb (1994) also emphasised the importance of publicity in these respects.) It goes without saying, though, that the protection delivered needs to be credible to both householders and to burglars. Among the minority of householders that were aware of action, low levels of action seemed actually to raise worry.
Political constraints may also be important here. Coordinators and their steering committees were often concerned with issues of inequity ('Why should one

house, or one area, get preventive action, and not another?'). Publicity could exacerbate this. Under such circumstances, it is important to establish a clear and defensible policy, in consultation with those residents with a stake in security. (One example is the policy of targeting repeat victims (Farrell, 1995).) They could stress benefits to the whole area from action on individual homes or streets (as the 'step' effect of Safer Cities action suggests). However, monitoring against the possibility of displacement should also be offered as part of the package.

The evaluation broke new ground in linking 'micro' analysis of small areas, and the action they received, to the 'macro' scale of cities and to the overall performance of a major programme of prevention. Two very different sources of outcome measures – surveys and recorded crime statistics – produced answers which were in most cases remarkably similar, although some loose ends and uncertainties in the evaluation inevitably remain. (If such local data was available to a common standard this would also greatly facilitate both local and national evaluations of crime prevention initiatives.) Our attempts to estimate cost-effectiveness were also pioneering – and may well be open to debate. Having estimated costs of prevention through Safer Cities action, we still lack evidence of the cost-effectiveness of other ways of investing our resources for crime control – whether by other arrangements for implementing local preventive action, or wider alternatives such as police patrolling. Quantitative evaluation of these alternatives should be developed.

Burglary was, as explained at the beginning, the 'best bet'. We have yet to see whether action targeted on other kinds of crime has the same measurable impact. But phase 1 of the Safer Cities Programme seems to have achieved an impact on burglary through interventions by local agencies with relatively limited experience of practical crime prevention, in the absence of particularly efficient targeting, and perhaps without full exploitation of deterrence through offenders' awareness of action. Given this, the potential for further gains in phase 2 of the Safer Cities Programme is considerable. In a context where few large-scale interventions against social problems seem to have much measurable effect, this is good news.

The lessons for practice and policy

Our recommendations are brought together as they apply to several key decisions. These decisions are relevant to both central policy and local practice. Where relevant, we have also drawn on the conclusions of our implementation study (Sutton, 1996).

The decision

Action against burglary of the type taken in Safer Cities phase 1 seems worth implementing, in cost-effectiveness terms. We found some evidence of geographical displacement, but also (weaker) signs of the opposite effect – diffusion of benefit causing offenders to avoid wider areas than strictly necessary, when action was intense. We nonetheless lack evidence of the cost-effectiveness of other ways of arranging for local preventive action, or wider alternatives such as police patrolling. Quantitative evaluation of these alternatives should be developed.

Where to target action

Targeting very local high-crime areas, other things being equal, promises the best returns on investment in preventive action; however, this does not necessarily mean that it will be markedly easier to accomplish a given reduction in burglary risk. Any targeting policies adopted, however, must be comprehensible and justifiable to the local community. Clarification of such policies is needed.

The ability of coordinators to select the highest-crime area seems constrained by lack of routinely available and easily retrievable local crime data and other information. The possibilities offered by geographic information systems should be pursued. If such local data was available to a common standard this would also greatly facilitate both local and national evaluations of crime prevention initiatives.

How much action

With some reservations, the more intense the action in an area, the greater the reduction in risk – so deciding to spend more rather than less is justified. However, the reduction at the margin may diminish if the intended area of action has higher risk of burglary and burglary action is introduced alone. Against this, the more intense the action, the greater may be the gains from diffusion of benefit.

Moreover, there appeared to be a certain threshold of action, beneath which there was little measurable impact on people's perceptions of their area, their awareness of preventive action, or their worry about burglary. A tendency to avoid spending more on action in the highest-crime localities should be explored further as it raises the question of effectiveness and other policy issues.

What sort of action

Safer Cities action on burglary in a locality appeared to have the strongest effect, pound for pound, where target hardening was combined with other measures, and where action on burglary was itself combined with wider preventive action. (There were some doubts about whether burglary action alone had much effect at all.) The idea of synergistic, concerted action rather than isolated single-track schemes makes intuitive sense.

More generally, local area processes seem important to exploit, in addition to action targeted simply on a home-by-home basis. Bringing schemes together, or into adjacent areas, seems better at deflecting burglary elsewhere, if not preventing it completely.

More could be done to publicize action, thereby both reassuring householders and more deliberately sending a deterrent message to offenders. The action actually taken of course would need to be of an appropriate kind and intensity. The publicity would need to be sensitively handled to avoid raising unrealistic expectations, or awakening feelings of inequity of treatment.

References

Ekblom, P.
Evaluating crime prevention: the management of uncertainty. In: C. Kemp (ed.), *Current Issues in Criminological Research*
Bristol, Bristol Centre for Criminal Justice, 1990, pp. 65-89

Ekblom, P.
The Safer Cities Programme impact evaluation: problems and progress
Studies on Crime and Crime Prevention, vol. 1, no. 1, 1992, pp. 35-51

Ekblom, P.
Scoping and scoring: linking measures of action to measures of outcome in a multi-scheme, multi-site crime prevention programme. In: D. Zahm, P. Cromwell (eds.), *Proceedings of the International Seminar on Environmental Criminology and Crime Analysis*
University of Miami, May 1993
Florida Statistical Analysis Center / Florida Criminal Justice Executive institute, 1994

Ekblom, P., D. Howes, H. Law
Scoping, scoring and modelling: linking measures of crime preventive action to measures of outcome in a large, multi-site evaluation using a GIS and multi-level modelling. Unpublished paper presented at *GIS Research UK 1994*, Leicester University, April 12, 1994

Ekblom, P., H. Law, M. Sutton
Safer Cities and Residential Burglary
London, HMSO (forthcoming)

Ekblom, P., K. Pease
Evaluating crime prevention. In: M. Tonry, D. Farrington (eds.), *Building a Safer Society: Strategic Approaches to Crime Prevention*

London/Chicago, University of Chicago Press, 1995, pp. 585-662
Crime and Justice: a Review of Research, vol. 19

Ekblom, P., M. Sutton, R. Wiggins
Scoping, scoring and modelling. Paper presented to the *seminar of the Royal Statistical Society*, London, December 14, 1993

Farrell, G.
Preventing repeat victimisation. In: M. Tonry, D. Farrington (eds.), *Building a Safer Society: Strategic Approaches to Crime Prevention*
London/Chicago, University of Chicago Press, 1995, pp. 469-534
Crime and Justice: a Review of Research, vol. 19

Forrester, D.P., M.R. Chatterton, K. Pease
The Kirkholt Burglary Prevention Demonstration Project
London, Home Office, 1988
Crime Prevention Unit Paper, no. 13

Forrester, D.P., S. Frenz, M. O'Connell, K. Pease
The Kirkholt Burglary Prevention Project: Phase II
London, Home Office, 1990
Crime Prevention Unit Paper, no. 23

Goldstein, H.
Multilevel Statistical Models
London, Edward Arnold, New York, Halsted Press, 1995

Home Office
Safer Cities Progress Report, 1991-1992
London, Home Office, 1993

Junger-Tas, J.
Policy evaluation research in criminal justice
Studies on Crime and Crime Prevention, vol. 2, 1993, pp. 7-20

Laycock, G., N. Tilley
Implementing crime prevention. In: M. Tonry, D. Farrington (eds.), *Building a Safer Society: Strategic Approaches to Crime Prevention*
London/Chicago, University of Chicago Press, 1995, pp. 535-584
Crime and Justice: a Review of Research, vol. 19

Liddle, A.M., A.E. Bottoms
Implementing Circular 8/84: a Retrospective Assessment of the Five Towns Initiative
Cambridge, University of Cambridge, Institute of Criminology, 1991 (unpublished report)

Pawson, R., N. Tilley
What works in evaluation research
British Journal of Criminology, vol. 34, no. 3, 1994, pp. 291-306

Polder, W.
Crime prevention in the Netherlands: pilot projects evaluated
Dutch Penal Law and Policy, no. 7, 1992

Sutton, M.
Implementing Crime Prevention Schemes in a Multi-Agency Setting: Aspects of Process in the Safer Cities Programme
London, HMSO (forthcoming)

Tilley, N.
Safer Cities and Community Safety Strategies
London, Home Office, 1992
Crime Prevention Unit Paper, no. 38

Tilley, N.
Crime prevention and the Safer Cities story
Howard Journal of Criminal Justice, vol. 32, no. 1, 1993, pp. 32-57

Tilley, N., J. Webb
Burglary reduction: Findings from Safer Cities schemes
London, Home Office, Police Research Group, 1994
Crime Prevention and Detection Series, no. 60,

Wright, R., R. Logie, S. Decker
Criminal expertise and offender decision making: an experimental study of the target selection process in residential burglary
Journal of Research in Crime and Delinquency, vol. 32, no. 1, 1995, pp. 39-53