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Crime and Police Resources: the Street Crime Initiative

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Abstract
In this paper we look at links between police resources and crime in a different way to the existing economics of crime work. To do so we focus on a large-scale policy intervention - the Street Crime Initiative - that was introduced in England and Wales in 2002. This allocated additional resources to some police force areas to combat street crime, whereas other forces did not receive any additional funding. Estimates derived from several empirical strategies show that robberies fell significantly in SCI police forces relative to non-SCI forces after the initiative was introduced. Moreover, the policy seems to have been a cost effective one, even after allowing for possible displacement or diffusion effects onto other crimes and adjacent areas. There is some heterogeneity in this positive net social benefit across different SCI police forces, suggesting that some police forces may have made better use of the extra resources than others. Overall, we reach the conclusion that increased police resources do in fact lead to lower crime, at least in the context of the SCI programme we study.

JEL Classification: H00, H5, K42
Keywords: Street crime; Police resources; Cost effectiveness

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> "We are facing head-on the thuggery and violence on our streets. We literally must reclaim our streets for the decent law-abiding citizens who want no more than to be able to walk safely, to live peacefully, and to go about their business freely, untroubled by attack.
>
> We want more police visible on our streets, immediate action to speed the perpetrators through the system, action to protect victims and witnesses and to ensure that those who are remanded or convicted don't walk freely on our streets."
>
> Home Secretary, David Blunkett,
>
> Street Crime Initiative announcement on 17 March 2002

I. Introduction

Standard economic models of crime predict that increased resources, like extra spending on police or on modes of deterrence, are associated with reduced crime. In the Becker (1968) or Ehrlich (1973) models, where individuals choose whether or not to commit crime by weighing up the expected costs and benefits relative to legal alternatives, extra resources should affect the probability that an individual is detected committing a crime and therefore should, on the margins, cause potential offenders to desist. Similarly, in criminological models of crime participation (Sherman, 1995; Nagin, 1998) more spending on police can, if effectively utilised, improve police productivity and this can result in reductions in crime.

Obtaining sound evidence that additional police resources can reduce crime has, however, proven elusive. Surveys of empirical research on police and crime (e.g. Cameron, 1988; Marvell and Moody, 1996; Eck and Maguire, 2000) report that the majority of studies fail to find any relationship between the two; in fact some actually report there to be a positive association. The reason is perhaps not very surprising as most of these studies face severe difficulties in attempting to unravel a causal relationship between police resources and crime. This is because additional police resources may impact on crime, but a causal relationship running in the opposite direction is equally likely. The problem is clearly stated by Levitt (1997) who observes that higher crime rates may well increase the marginal productivity of police (e.g. cities with higher crime rates are likely to have bigger police forces, even if police reduce crime).

Weaknesses in research design in existing studies, and high public policy relevance, make the issue of police resources and crime a very important research question. This is especially the case when one notes how much is spent on crime prevention and given the very high social costs of crime (see Brand and Price, 2000, and Dubourg and Hamed, 2005, for England and Wales or Cohen, 2002 for the US). Not much work gets to grips with the problems of reverse causation from crime to police that plague the existing literature. However, there are some rare exceptions. For instance, Levitt's (1997) study of crime in 59 large US cities does address the endogeneity of police resources. By noting that in many countries crime is an important political question he sets out to resolve the endogeneity issue via an instrumental variable[1] strategy that uses election years as instrument for police in a crime equation. Doing so he identifies a causal negative effect running from police to crime, but even here Levitt acknowledges his estimates to be 'imprecise, making it difficult to draw strong public policy conclusions' [Levitt, 1997, page 271].[2]

A more recent study by Di Tella and Schargrodsky (2004) also tried to circumvent the police and crime causality problem, by looking at a quasi-natural experiment. Their very specific study makes use of intensified police presence that occurred around religious buildings in Buenos Aires following a terrorist attack.

[1] Identification is achieved by including an election year indicator in the police equation, but by excluding it from the crime equation.

[2] See also McCrary (2002) and Levitt's (2002) response who discuss some worries with the data and approach used in the Levitt paper.

Comparing car theft in the vicinity of the buildings before and after the attack, relative to unaffected areas, isolates a substantial impact of police on crime.

In this paper we adopt what we believe to be a strong, yet rather different, research design to look at the impact of additional police resources on crime. We base our analysis on a policy intervention that occurred in England and Wales, beginning in the 2002/03 financial year. This is the Street Crime Initiative, hereafter referred to as SCI, an intervention aimed at reducing robbery. In the SCI some police forces were given extra resources, to the tune of £24 million per year in the first two years, to participate in the initiative. A key feature of this policy was that only some forces (10 out of the 43 police forces of England and Wales) received the additional funding.

The quasi-experimental nature of the SCI policy means that we are able to consider several empirical strategies designed to look at what happened to robbery rates in SCI police forces relative to non-SCI police forces before and after the introduction of the initiative. Subject to being able to counter possible biases resulting from the design of the SCI policy, this provides a strong research design that is different to the existing work on police resources and crime (since it is based on a policy intervention at a large-scale level[3]), and that can enable us to identify any impact of police on crime. The paper is careful to describe how we can adopt empirical approaches that are able to utilise the SCI policy intervention, and its quasi-experimental nature, to identify an impact of the policy intervention on crime.

Our empirical estimates show that the SCI introduction significantly reduced robbery rates in SCI areas relative to non-SCI areas. We reach this conclusion after a detailed empirical analysis that takes considerable care to ensure that the estimated SCI

[3] Though there are interesting smaller scale studies based on treatment-control type comparisons (see, for example, Sherman and Weisburd's, 1995, very interesting analysis of the Minneapolis Hot Spots Patrol Experiment of 1988/89).

non-SCI differences we report come from comparing areas that (in the statistical models we present) have similar pre-policy trends in robbery rates. This is important since, as discussed in more detail below, the selection of forces to participate in the SCI was not random and did depend on the number of robberies before the initiative came in. Moreover, our results show that the SCI policy seems to have been a cost effective one since the monetary benefits from robbery reductions we identify are large relative to the costs of the programme. This is the case even after allowing for possible displacement/diffusion effects onto other crimes and adjacent areas. That said there is some evidence of heterogeneity in the estimated effects of increased resources on robbery across areas, suggesting some police forces may have made better use of the extra resources than others.

The rest of the paper is structured as follows. In Section II we describe the data we use and discuss the nature of Street Crime Initiative, paying particular attention to the institutional features of the intervention programme and providing some descriptive statistics. In Section III we present estimates of the impact of SCI participation on robbery, taking time to introduce and carefully discuss the empirical strategies we use. In Section IV we describe and implement a cost-benefit analysis of the programme. Section V concludes.

II. Crime Data and the Street Crime Initiative

Data Description

Our aim is to study the scope for additional police resources to impact on crime using the quasi-experimental nature of the SCI. To do so, we use Home Office data on reported crimes in localised areas before and after SCI introduction in England and Wales. Since the financial year 1999/00 the Home Office has published annual crime data for local contained areas known as Crime and Disorder Reduction Partnerships (CDRPs). These have boundaries corresponding to administrative local authorities and there are 376 CDRPs across the 43 police force areas (PFAs) of England and Wales. Figure 1 shows the CDRPs, together with their PFAs, on a map of England and Wales.

At CDRP level we have annual data available for analysis for the five financial years 1999/00, 2000/01, 2001/02, 2002/03 and 2003/04. The SCI was introduced in most of its participating police forces in April 2002 and, since financial years run from April 1 to March 31, we can consider the years 2002/03 and 2003/04 as the years corresponding to the SCI and the other years as pre-policy years.

However, one feature of the SCI was its early introduction in one particular police force, the London Metropolitan police service, in February 2002. This came before the introduction to the other 9 participating police forces (Avon and Somerset; Greater Manchester; Lancashire; Merseyside; Nottinghamshire; South Yorkshire; Thames Valley; West Midlands and West Yorkshire) in April 2002. It is evident that we cannot accurately analyse the February introduction in London using annual data but, with considerable help from the Home Office team working on the SCI, we have been given access to monthly data on robberies at the Basic Command Unit (BCU) level from October 2000 to March 2004. Basic Command Units are operational policing sub-divisions within the police force areas. We have been able to put together a monthly panel covering 235 BCUs that allow us to study the impact of the different starting times of the SCI.

The SCI is focused explicitly upon reducing robberies. A person is deemed to be guilty of robbery if he/she 'Steals, and immediately before or at the time of doing so, and in order to steal uses force or puts or seeks to put a person in fear of being then and there subjected to force'.[4] Therefore the outcome of interest in our empirical analysis is the number of robberies recorded by the police.[5]

We have constructed annual and monthly panels at the CDRP, PFA and BCU level and, where possible, have matched to these various other data. First, we have access to other measures of crime from the Home Office. This will be important to consider since, although the SCI focuses on robberies, there is scope for an impact on other crimes, which could be negative through diffusion effects or positive through displacement effects (see below for more detail on the forms of displacement/diffusion we consider). Second, we have matched point-in-time data on area characteristics from the 2001 Census to the panels, and annual time-varying information from the local area Labour Force Surveys to the annual CDRP panel.[6,7]

The Street Crime Initiative

The SCI was introduced to combat robbery in 10 police force areas of England and Wales in 2002. The remaining 32 police force areas of England and Wales received no extra resources.[8] Figure 1 shows the SCI and non SCI areas on the England and Wales map. The 10 participating police forces were given significant additional resources from being in the SCI. Table 1 shows, for anonymised police forces aggregated into three

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[4] Section 8: 'Robbery'. Theft Act 1968.

[5] We use recorded crime rather than available data on victimisations (e.g. from the British Crime Survey, BCS) for several reasons. The BCS data does not include crime experienced by people aged under 16 and, even more importantly, is not sufficiently reliable at the local level to permit meaningful analysis.

[6] The LFS data cannot be matched to BCU crime data due to boundary definition issues and is, in any case, not reported monthly. One very small CDRP has no data in the LFS and so we base most of our analysis on 375 CDRPs, of which 111 are SCI areas and 264 are non-SCI areas. Of the 235 BCUs 90 and 145 are SCI and non-SCI BCUs respectively.

[7] The Census data is on the following characteristics in terms of proportions of the relevant population: unemployed; aged under 25; ethnic minorities; no educational qualifications; economically active; working part-time; living in social housing; in lowest social grade; working in manufacturing. The local area LFS data we use reports socio-economic statistics at the local authority level from a sample of 60,000 households for every year between 1999/00 and 2003/04. We have matched the following proportions to the annual CDRP crime data: unemployed; economically active; aged 16-24; working part time; ethnic minority; aged 16-19 in full time education; working in manufacturing.

[8] A single London area is generated by merging the Metropolitan police service with the City of London Police Force (owing to the artificially high crime rate per population of the latter since very few people actually live in the City). The total number of police forces is thus 42 in the rest of the analysis.

groupings[9], the average robbery levels in the year preceding SCI introduction, the share of robberies in all crimes, the pre-policy robbery rates per 1,000 population, and the amounts of money received for financial years 2002/03 and 2003/04, in absolute terms (in £million) and as a share of the total police budget.

The numbers in the Table make several things clear about SCI introduction. First of all, the policy was not introduced randomly to police force areas. It was introduced through a rule based on the number of robberies in the year prior to introduction. Thus the number of robberies is significantly higher in the SCI areas prior to introduction, at 100,840 incidents, as compared to 20,535 in the non-SCI areas. SCI areas are also more robbery intensive since robberies make up a higher proportion of all recorded crimes (at about 3.3 percent) as compared to non-SCI areas (where robberies comprise .8 percent of all recorded crimes). And the robbery rate, the number of robberies per 1000 people, is higher at 4.4 robberies per 1000 people in the SCI forces as compared to .7 in the non-SCI forces.

The second point of note in Table 1 is that SCI forces received a sizable amount of money, at just over £48 million across the 10 areas in the first two years of the policy. These extra funds amounted to about 1 percent of their usual allocation, a significant amount when one bears in mind that robberies in these areas comprise about 3 percent of all crimes. The extra funds were mostly spent on overtime, additional staffing and information technology. In the first year resources were spent on both capital investment and police staffing, but in later years moved to staffing alone. A key part of the SCI was targeted policing strategies and inter-agency collaboration. Significant numbers of police officers were re-deployed in SCI forces to focus on robbery reduction functions.

[9] The forces are anonymised and grouped since the Home Office very kindly provided us with confidential data on additional resources by police force that is not available in the public domain.

Because of the way it was implemented, the SCI emphasised using the additional resources in conjunction with a problem-solving approach to combating street crime. As such it combines increased police numbers/hours with attempts to enhance police efficiency via a targeted programme. The SCI involves a number of agencies working in partnership, by adopting and developing practical measures to generate crime reduction.[10] Tilley et al (2004) identify four key mechanisms for reducing street crime: policing and criminal justice mechanisms (to incapacitate and deter offenders); social interventions to reduce crime (to divert crimes and improve social control mechanisms); individual treatments (e.g. trying to reduce drug dependency, or changing thinking about crime); situational mechanisms (increasing risk and effort facing potential criminals, decreasing rewards and reducing excuses criminals may make). These mechanisms underpin the strategies which were used within the SCI to combat street crime.

Preliminary Descriptive Analysis

Figure 2 shows the robbery rate in SCI and non-SCI areas in the annual and monthly panels we have constructed. The vertical lines show the date of SCI introduction, with the time varying feature of SCI introduction built into the monthly Figure. There is an intriguing pattern that occurs before and after the policy introduction. It is evident that the robbery rate rises and falls after introduction in the SCI areas as the dotted lines in the left hand panels of the Figure show, but continues to rise in the non-SCI areas (as shown in the solid lines in the right hand panels).

Table 2 shows average robbery rates (per 1,000 people) for the year before and the two years after SCI introduction. There is a clear pattern, with robbery rates falling significantly after the policy introduction in the SCI police force areas, and rising in the

[10] The agencies involves include those traditionally involved in crime reduction (Home Office, the police service, the Crown Prosecution Service and the courts), but also some collaboration with other government departments.

non-SCI areas. The difference-in-difference, calculated as the change in the robbery rate in the SCI areas before and after the policy intervention relative to the change in the non-SCI areas, shows a marked fall in the robbery rate occurring in the participating police forces. The difference-in-difference amounts to a .91 fall in the robbery rate in SCI Year 1, and a slightly larger fall of 1.21 in SCI Year 2, both of which are strongly significant in statistical terms. In logs the falls are -.28 and -.38, showing the reduction in the robbery rate across the two years to be around one-third.

Pre-Policy Trends in Robberies

Robbery rates therefore seem to have fallen in SCI areas relative to non-SCI areas. However, to reach a conclusion that this was due to the policy intervention would be premature. Whilst the fact that the SCI was introduced to some areas and not others appears to offer a highly attractive research design for looking at the impact of increased police resources on crime, one should be very careful to note the non-random selection of SCI areas. Indeed it is very important to establish that pre-SCI trends were not different in the two sets of areas. For the comparison group to be valid in a non-experimental study like the one we are considering, the key conditions are that there are common trends and stable composition of the treatment and comparison areas over the time period to be studied.

This does seem to be the case. Figure 3 shows indexed changes in the robbery rate in SCI and non-SCI areas for annual data starting in 1982/83 (in the upper panel of the Figure) and for monthly data starting in October 2000. The temporal patterns of both are very striking. The rates of change of robberies are highly similar in both the annual and monthly data before the SCI was introduced. In fact, in no period in the pre-policy introduction time can we reject the null hypothesis of equal growth rates in robberies for SCI and non-SCI areas.

It is important to realise that the indexing pertains to the changes in robbery: that is, pre-policy trends in the growth of robberies over time are highly similar (despite a permanently higher level). Our empirical models will mostly look at growth in robberies so as to ascertain the impact of SCI introduction on robberies (i.e. since they will control for area fixed effects that will condition out the pre-policy level). Looking at the post-SCI time periods, makes it clear that there was a very strong divergence in robbery rates after SCI introduction. In SCI areas the robbery rate fell very sharply; in non-SCI areas, it continued to rise.

A second important selection issue highlighted in some of the Figures is one that has similarities to what has become known in the programme evaluation literature as 'Ashenfelter's Dip' (Ashenfelter, 1978, Ashenfelter and Card, 1985). This is the idea that selection for treatment (in our case being an SCI police force) can be explained by a recent shock in the outcome variable of interest (in our case the robbery rate). If this shock is only transitory, we could expect the outcome variable to revert to its mean after selection, but not as a result of the treatment received. Whilst the longer run trends are very similar in terms of growth rates of robberies in SCI and non-SCI areas, close inspection of Figure 2 does indeed show a 'hump' shape in the SCI areas in the year immediately before and after SCI introduction, which may reflect the presence of mean reversion. If this is the case we may well over-estimate an SCI introduction effect on robberies by using a standard before/after approach.

In our empirical analysis we are careful to consider several empirical strategies that try and ensure our estimates are not contaminated by selection biases resulting from any differences in pre-policy trends. We take care to condition on various observable and unobservable area characteristics through different estimation methods, to look at alternative control periods and to examine long term robbery trends to net out these potential problems. We discuss these more advanced estimates of the policy effect, which consider and address these important selection issues, in the empirical section of the paper that comes next.

III. Empirical Analysis

Empirical Strategy

Our aim is to use the variation in police expenditures induced by the Street Crime Initiative as a means to identify the impact of police on crime. Since SCI and non-SCI areas had very similar pre-policy evolutions in robbery growth, we are able to do so in a panel data area-level model for area a in year t that includes area fixed effects as follows:

$$R_{at} = \alpha_a + \varphi SCI_a * PolicyOn_t + \varphi Z_{at} + u_{at} \quad (1)$$

where R is the robbery rate (in logs), SCI a dummy variable equal to one for SCI areas (and 0 for non-SCI areas), *PolicyOn is* a binary dummy equal to one for the periods after SCI introduction (and 0 in the pre-policy periods), Z is a set of time varying controls (matched to areas from the local area Labour Force Survey) and u is an error term. In this framework, φ is a difference-in-difference estimate of the impact of SCI introduction on the robbery rate. This is the key parameter on which we focus in our empirical work.

This difference-in-difference estimate offers an estimate of the impact of police on crime to the extent that SCI introduction raised expenditure on police. As already discussed above, SCI introduction did raise police expenditures, but we only have limited information on exactly what is was spent upon. Thus our identification relies on the reduced form regression, equation (1). This is because it is not feasible to adopt a two-stage instrumental variable approach using SCI introduction as an instrument for police since we do not have good enough data on the nature of the additional police expenditures that occurred for SCI areas (e.g. we cannot get data on overtime hours, or total hours of police officers, or on capital expenditures at the area level we study).

Basic Empirical Estimates

We consider our baseline estimates of the impact of SCI introduction on robbery rates in Table 3. As one moves across the columns of the Table the specifications gradually build up. The basic fixed effects model just including year dummies is given in column (1) and the same model plus time varying controls from the local Labour Force Survey are added in column (2).[11] The estimates are very robust and sizable, showing a significant -.34 effect of SCI introduction on the log(robbery rate). They point to an important robbery reducing effect of the SCI in the areas where it was introduced. The research design we have implemented, at least for these baseline CDRP models, thus points to an important negative association running from increased police to crime reduction.

Annual Versus Monthly Estimates

The February 2002 introduction of the SCI in the London area took place two months earlier than in the introduction to the other nine SCI police forces. This does raise an issue with the annual CDRP data, from which we cannot discern an effect from the different timing of the policy implementation. We therefore make use of the monthly BCU panel to try and measure the magnitude of a possible London effect due to differential timing of policy introduction. The bottom left graph in Figure 2 reports monthly robbery rates in the SCI and shows a drop between the February and April SCI

[11] We also looked at non-fixed effects specifications to try and get a better understanding of whether the higher pre-policy robbery rates in SCI areas can be accounted for. In a basic specification including only year dummies the estimated coefficient (standard error) on the SCI dummy variable was 1.802 (.313); this effect can almost entirely be explained by observable pre-policy characteristics because the effect falls to .230 (.142) on the inclusion of a set of pre-policy Census control variables (listed in footnote 6), pre-policy burglary and vehicle crime rates and an average trend in pre-policy robberies at police force area level.

introduction dates. This further illustrates the need for more thorough scrutiny of the timing of introduction issue.

Column (3) of Table 3 considers this question showing a specification based on monthly BCU data, but altering the dating of the *PolicyOn* variable to two months earlier for the London area. The estimated policy impact is slightly bigger (in absolute terms) than in the comparable yearly CDRP estimates of column (2), but is very close in terms of magnitude. Thus it seems that the results are very similar whether or not we take into account the early London introduction. We therefore continue our analysis treating April 2002 as the start date and using the CDRP data for which we have observations for a longer time period. [12]

Defining Comparison Time Periods and the Hump

We have so far not paid attention to the 'hump' around the policy introduction period we identified earlier in the descriptive analysis. The hump is evident when one looks at the top left graph of Figure 2, which shows what appears to be a large relative increase in robbery rate in the SCI areas in the selection year compared to the two previous years. If this was only a transitory shock with a subsequent regression to the mean of the outcome variable we are at serious risk of overestimating the policy effect (as in the Ashenfelter, 1978, case on the evaluation of work training programs in the US).

To deal with this issue of possible mean reversion we adopt several approaches. First we consider the effect on our estimates of using different pre-policy periods. So far we have only used 2001/02 (the selection year) to define the pre-policy control period. In columns (4) and (5) of Table 3 we report estimates that utilise two alternative pre-policy periods. The results from these specifications are indeed suggestive of there being some

[12] The monthly BCU data is only available from October 2000 and can consequently only be compared with yearly CDRP data starting in financial year 2001/02.

degree of mean reversion associated with the different pre-policy robbery patterns in the years immediately prior to policy introduction.

When all three years are used as control years, as shown in column (4), there is a significant reduction in the SCI effect, going from -.338 to -.242. However, the impact remains strongly significant, still showing a significant fall in the robbery rate post-SCI introduction. The column (5) model is even more stringent, in that it drops the year immediately prior to SCI introduction from the definition of control years. This drops the year where SCI area robbery rates did seem to accelerate, the source of possible worries about mean reversion in the analysis. The magnitude of the policy impact again falls (in absolute terms), to -.192, again this is strongly significant, showing falls in the robbery rate to have occurred in the SCI areas after the policy introduction.

Long Term Police Force Area Models

A sceptic might argue that the limited time period of pre-policy CDRP data available does not fully guarantee that we are able to wipe out any hump effect that could cause mean reversion. For example, this would be the case if the robbery rate had been increasing unusually fast in the SCI relative to the non-SCI areas for more than the three pre-policy years available in the CDRP data.

Whilst this seems unlikely we have addressed the question by adopting a much longer term perspective using 22 years of robbery data for the 42 police force areas. Results are reported in columns (6) and (7) of Table 3 and show that even after controlling for twenty pre-policy years we find relatively comparable SCI effects to those previously estimated.[13] This remains the case when we additionally control for area-specific trends, as shown in column (7).

[13] It was not possible to find socio-economic variables consistent over this time period to use as controls but, given the length of the time series, we now can include area-trends.

We carried out one more investigative analysis of this long run data to verify that the relative magnitude of the fall in robbery rates in the treated areas following the SCI introduction was an unprecedented occurrence. We estimate twenty-two year on year difference-in-difference coefficients across SCI and non-SCI areas. In Figure 4 we show these coefficients with an upper and lower bound of two standard errors highlighting the significance threshold at zero. Only two of the differences in difference coefficients are revealed to be negative and significantly different from zero, as denoted by having a top bar of the line beneath the zero line on the chart. By far the most negative of these is the coefficient on the first post policy year 2002/03. Remembering that these are difference-in-difference estimates for each year, the 2003/04 effect is also reassuring in that it shows the robbery rate stays lower after the initial fall in the first post-policy year. Moreover, our earlier models looking at three years prior to the policy introduction as the pre-policy period seem to be validated by the numbers in the Figure since the difference-in-difference estimates of those three years are very similar to one another and insignificantly different from zero.

Thus consideration of the longer term trends does seem to pick up a strong and marked shift reduction in the robbery rate that occurred after the introduction of the SCI policy. This, taken with the statistical estimates that try to carefully net out SCI selection effects owing to different pre-policy means and trends, is evidence that robberies did fall significantly in SCI areas relative to non-SCI areas after the policy was introduced and is in line with the idea that additional police resources are able to curb crime.

Matching Estimates

The final statistical approach we adopt to try and ensure we are identifying a policy impact is a propensity score matching model. We use this to even more stringently ensure that we are comparing like with like by removing areas for which we cannot find a suitable control area. To do so we select a sample of matched CDRPs using propensity score matching techniques based on the pre-policy Census data control variables. The propensity score distributions and probit models used to generate them are shown in Table A1 of the Appendix.[14] The basic method used is that of Heckman, Ichimura and Todd, 1997, where propensity scores are estimated and the sample then trimmed to exclude poorly matched areas without common support.[15]

Carrying out the matching exercise trims the sample of 29 unmatched CDRPs, 23 of which come from the SCI areas and 6 from the non-SCI areas, leaving us with a matched sample of 346 CDRPs. We thus estimate policy effects using this new sample using the same modelling specifications as earlier and the estimates are reported in Table 4. They display a very similar pattern to the results already reported. All the SCI effect estimates are slightly smaller compared to earlier as we have purged the data of 'extreme' areas with respect to their characteristics. The estimate excluding the SCI selection year is -.161 (see column (3)), and remains strongly significant. This can be interpreted as a more conservative estimate of the success of SCI in reducing robberies.

Heterogeneity of the SCI Impact

The policy effect estimated so far is an average treatment effect for all the areas where the SCI was implemented. However, we know from the descriptive statistics presented in Table 1 that there is a substantial degree of heterogeneity between the three groups of SCI police forces on various measures. There is therefore scope for

[14] Close inspection of the probit coefficients and associated standard errors in the Appendix Table makes it evident that not many of the pre-policy Census characteristics are statistically significant. This is basically since they are correlated with one another. However, if the trimming was based on a more parsimonious model with only the two most important determinants entered as independent variables (proportion ethnic minorities, proportion lone parents) both of these are significant and trimming of the sample using this model made little difference to the results - estimated coefficient = -.172, with associated standard error .056 as compared to the -.161 (.056) reported in column (3) of Table 4.

[15] See Rosenbaum and Rubin (1983, 1984) for the initial statements on how to use propensity score matching as a means of reducing bias in observational studies designed to compare treatments and controls (= 0 or 1, in our case being distinguished by the SCI variable).

investigating the impact of the policy on robberies in these different areas. We have therefore estimated models with separate coefficients for the three groups of forces considered earlier.

Results using different pre-policy control years are reported in Table 5 for the full and matched samples of CDRPs and for the long term PFA panel. We find the same pattern as before with all estimates decreasing (in absolute terms) as we change the pre-policy period to eradicate the hump effect and as we move to the matched sample. However, there is evidence of heterogeneity in the policy impact across the three police force groupings. These are statistically significant at better than the 10 percent level for all specifications, as the P-Values testing equality across forces show, thus rejecting the null hypothesis of constancy of the SCI policy effect. The effect of the SCI on robberies is of greater magnitude Forces 1-3, the three police forces with the largest volumes of pre-policy robberies and the ones who received more resources to combat street crime.

IV. Cost Benefit Calculations

In this section of the paper we describe our attempts to assess the effectiveness of the SCI policy. To do so, we compare the estimated benefits of reduced robberies derived from the statistical models with the costs of implementing the policy.

Approach

The starting point to the cost-benefit calculations comes from comparing the estimates of the benefits due to crime reduction with the costs of the SCI programme. A basic measure of the social benefits from robbery reduction, SB_1, is given by

$$SB_1 = \hat{\varphi} \cdot \overline{R}_{pre} \cdot SC_R \qquad (2)$$

where $\hat{\varphi}$ is the estimated policy impact (with a hat denoting an estimate), $\overline{R}_{pre}$ is the number of robberies in the policy areas in the pre-policy period and SC_R denotes the social costs of a robbery. SB_1 thus measures the value of the benefits of robbery reduction due to the SCI programme. This can be compared with the costs of the policy, C, to give an estimate of the net social benefits of the policy SB_1- C.

Of course this measure precludes possible displacement or diffusion effects resulting from the SCI policy. There are different aspects to this, including the possible impact of the policy on other crimes, and the possible movement of robberies to other geographical areas.

Consider first a possible impact on other crimes. It is evident that crime reducing policies which give incentives to the police to target a particular crime, such as robberies in the case of the SCI, can have either a negative or positive side effect on other types of crime. Crime can rise if the police focus on combating one type of crime resulting in a displacement to other crimes which they pay relatively less attention to and might have to shift resources from fighting. On the contrary, an increase in police effort might increase the probability of being caught for other crimes and consequently also reduce their occurrence.

Ultimately which effect dominates is an empirical question and we consider this below. But we are also interested in how this may affect the cost-benefit calculations we propose to undertake. In our analysis we consider two other crimes, burglary and vehicle crime. One can define the negative/positive monetary cost/benefit resulting from displacement/diffusion effects as follows:

$$SB_2 = \hat{d}_B \cdot \overline{B}_{pre} \cdot SC_B + \hat{d}_V \cdot \overline{V}_{pre} SC_V \quad (3)$$

where B and V denote burglary and vehicle crimes respectively and $\hat{d}$ denotes the estimated displacement/diffusion effect on to the other crimes. It is clear that SB_2 can be positive or negative depending on the empirical estimates of the impact of SCI on other crimes.

The second relevant issue concerns the fact that the SCI was only introduced into ten areas, which gives scope for the spatial displacement of crime across police force area borders as potential criminals shift their activities to places not covered by the SCI. There is thus a concern about what happened to crime in adjacent areas not receiving the SCI treatment. However, whilst displacement seems most likely, diffusion is also possible. On the former, one could predict that robberies will increase there as criminals displace their activity to the non-SCI area in their vicinity. Or, in the latter case, it could be argued that improved identification and incapacitation of offenders, police force competition, with limited crime commuting all can have a reducing effect on robberies in the non-treated areas.

In this case one needs to think carefully how to uncover a spatial effect. We discuss the modelling approach we adopt below, but defining an estimate of spatial displacement/diffusion as $\hat{g}$, we can define a social cost/benefit from spatial displacement/diffusion as

$$SB_3 = \hat{g} \cdot \overline{\mathrm{RADJ}}_{\mathrm{pre}} \cdot SC_R \tag{4}$$

where $\overline{\mathrm{RADJ}}_{\mathrm{pre}}$ measures the pre-policy robberies in the adjacent areas (i.e. those where spatial displacement/diffusion may take place).

It is evident that, whilst a reduction in robberies will mean that SB_1 will be positive, SB_2 and SB_3 may be positive (if diffusion results in reduced crime) or negative (if displacement raises crime). A more general cost-benefit calculation thus results in a net social benefit of $SB_1 + SB_2 + SB_3 - C$. In what follows, we begin by reporting the basic cost-benefit calculations, looking only at the direct SCI impact in the net social benefit calculation SB_1- C. We then further extend this to allow for crime displacement/diffusion and spatial displacement/diffusion effects to see how they amend calculations regarding the cost effectiveness of the SCI initiative.

Basic Cost Benefit Analysis

To calculate SB_1- C we need to obtain our preferred estimates from the barrage of empirical results we reported in Section III of the paper. We adopt a conservative approach, using estimates from the models excluding the pre-policy introduction selection year for the full (-.192) and matched (-.161) samples. Table 6 reports cost-benefit calculations based upon these estimates, which correspond to percentage decrease in robberies in SCI areas of 17.4 and 14.8 percent respectively.[16] To establish the volume of robberies these reductions mean we must relate them to the appropriate SCI baseline levels. This is the average number of robberies recorded during our control period, 1999/00 and 2000/01[17], and translates into between 12,751 and 10,846 fewer robberies resulting from the SCI introduction.

To obtain the monetary benefits from robbery reduction we multiply these figures with the average social cost of a recorded personal robbery of £12,094 as estimated by the Home Office.[18] After deducting the £24.1 million average annual cost of the SCI we find a net social benefit of the policy of between £107 and £130 million. This appears to make the SCI a cost effective policy as it amounts to roughly 4.5 to 5 times the initial investment.

Displacement/Diffusion to Other Crimes

To analyse possible displacement/diffusion on to other crimes, we have considered what happened to the other main property crimes, burglaries and theft from

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[16] Calculated as {exp(coefficient)-1]X100} from the log(robbery rate) models.

[17] We also use this baseline for the matched sample estimation although it could be argued that we should deduct the number of robberies in the trimmed areas from this figure. This would generate a hardly comparable result for our lower bound estimate even after scaling down the cost of the policy accordingly.

[18] We are grateful to the Home Office for providing us with their up to date social costs of crime estimates (these are updates of the earlier numbers in Brand and Price, 2000 as reported in Dubourg and Hamed, 2005). The £12,094 average social cost of a personal robbery (kindly supplied to us by Richard Dubourg and Joe Hamed) takes into account the larger impact on the criminal justice system component of the cost associated with recorded crimes – it is larger than the £7357 number given in the Dubourg-Hamed work for British Crime Survey robberies (based on victimisations) since we use data on recorded robberies and so need to add a criminal justice component to the costs and because it nets out robbery against businesses.

and of vehicles, in SCI compared to non SCI areas. Results reported in Table 7 are estimates from the same modelling specifications used to obtain our robbery estimates, but instead using the other crimes as the dependent variable of interest. The estimated effects of the SCI on both burglaries and vehicle crime are small and negative and we cannot reject the hypothesis that they are not significantly different from zero. The small negative (albeit insignificant) coefficients mean we can rule out any evidence of displacement effects to these other crimes, but equally this does not either allow us to clearly substantiate any diffusion effect. However, on the basis of these estimates it seems that SB_2 approximates zero and thus we do not need to amend our earlier cost-benefit calculation.

Displacement/Diffusion to Other Areas

To measure geographical displacement/diffusion we cannot simply estimate a 'non-policy' effect on the robbery rate in the non-SCI areas as this would simply be a mirror image of the SCI effects on robberies already estimated. We need to design a more subtle way of estimating spatial displacement/diffusion effects. We are able to do this we have data on multiple CDRPs within SCI and non-SCI areas. We sort the non-SCI areas into two groups, those adjacent and non-adjacent to the SCI areas.[19] The adjacent areas are all CDRPs where the SCI was not implemented which border SCI areas. The non-adjacent area comprises the rest of the non-SCI area which does not have any common border with SCI areas. It seems likely that if geographical displacement/diffusion occurs it will be in the adjacent areas and not in the non-adjacent areas.

In Table 8 we thus estimate models which compare what happened to robberies in SCI areas relative to the adjacent and non-adjacent CDRPs (i.e. separately for the preferred modelling specifications from above). For the full and matched samples, the

[19] See Bowers and Johnson (2003) for a related approach referring to these as buffer and non-buffer areas.

estimates show that robberies were reduced in SCI areas relative to both adjacent and non-adjacent areas. However, the reduction was by more in the comparison with the adjacent areas. This is in line with possible displacement to adjacent areas. However, the differences between adjacent and non-adjacent are not large and, moreover, we find that the estimated reductions are not statistically distinguishable from one another. Hence, we cannot reject a null hypothesis of no spatial displacement (or diffusion) due to the SCI. Again then, as with the case of displacement/diffusion to other crimes, it seems that SB_3 approximates zero and thus we do not need to amend our earlier cost-benefit calculation.

Heterogeneity Across Police Forces

We established earlier that the impact effect of SCI did differ across the three groupings of police force areas, so it also seems sensible to carry out the cost-benefit calculations for the different forces. This exercise is reported in Table 9. Again we compute the basic cost-benefit calculation, but also look for possible crime and spatial displacement/diffusion effects that may be operating, this time separately by police force groupings. On the latter only one displacement/diffusion effect is estimated to be statistically significant, with there being a significant diffusion effect on burglaries in Forces 1-3 (the estimate is a -.091 reduction in burglaries). Hence the only augmentation of the basic cost benefit calculation $SB_1 - C$ occurs for Forces 1-3 where we also report $SB_1 + SB_2 - C$.

The Table shows that whilst the overall SCI was cost effective, there are differences across areas. In fact the net social benefits are high for Forces 1-3, moderate for Forces 4-7 and essentially zero for Forces 8-10. This suggests some police forces may have made better use of the extra resources than others. It is also consistent with the notion that the forces that received more resources (in absolute terms) delivered more in terms of reducing street crime. However, it is evident that more research, focussing specifically on the actions taken in different police force areas and the way in which resources were utilised, would be required to better pin down the reasons for variations in effectiveness across different police forces.

V. Conclusions

In this paper we have utilised a different research approach to that used in most of the literature on crime and police resources. We focus on a policy intervention - the Street Crime Initiative – which was introduced in England and Wales in 2002. This policy allocated additional resources to some police force areas to combat street crime, whereas other forces did not receive any additional funding. The quasi-experimental setting induced by the SCI enables us to get a handle on the direction of causation in the police resources-crime relationship since we can, with a suitable empirical research design, look at the impact of additional resources on street crime by comparing the evolution of street crime before and after the policy introduction in areas exposed to the policy as compared to those that were not.

Our estimates, derived from a number of empirical strategies, show that robberies did fall significantly in SCI police forces relative to non-SCI forces after the initiative was introduced. Moreover, the policy seems to be a cost effective one, with the annual net social benefits being somewhere in the range of £107 to £130 million, as compared to a cost of £24 million. This is the case even after allowing for possible displacement/diffusion effects onto other crimes and adjacent areas Moreover, there is some heterogeneity in this positive net social benefit across different SCI police forces, suggesting that some police forces may have made better use of the extra resources than others.

We believe that these estimates make a significant contribution to the literature on police resources and crime which, for the most part, has been plagued by problems of reverse causation. Applying a programme evaluation type analysis to the police resources-crime question in a large-scale setting is, to our knowledge, a novel approach in the economics of crime field and it leads to a conclusion that increased police resources do in fact lead to lower crime. This finding should be placed firmly into the context of the SCI, which is a highly targeted programme and one which emphasises the role of communities operating in conjunction with police forces to try and combat street crime. It seems evident that the additional resources made available in the SCI were used in a planned and systematic manner and therefore that the programme was able to significantly reduce the serious social problem of street crime.

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Figure 1: Map of CDRPs in England and Wales

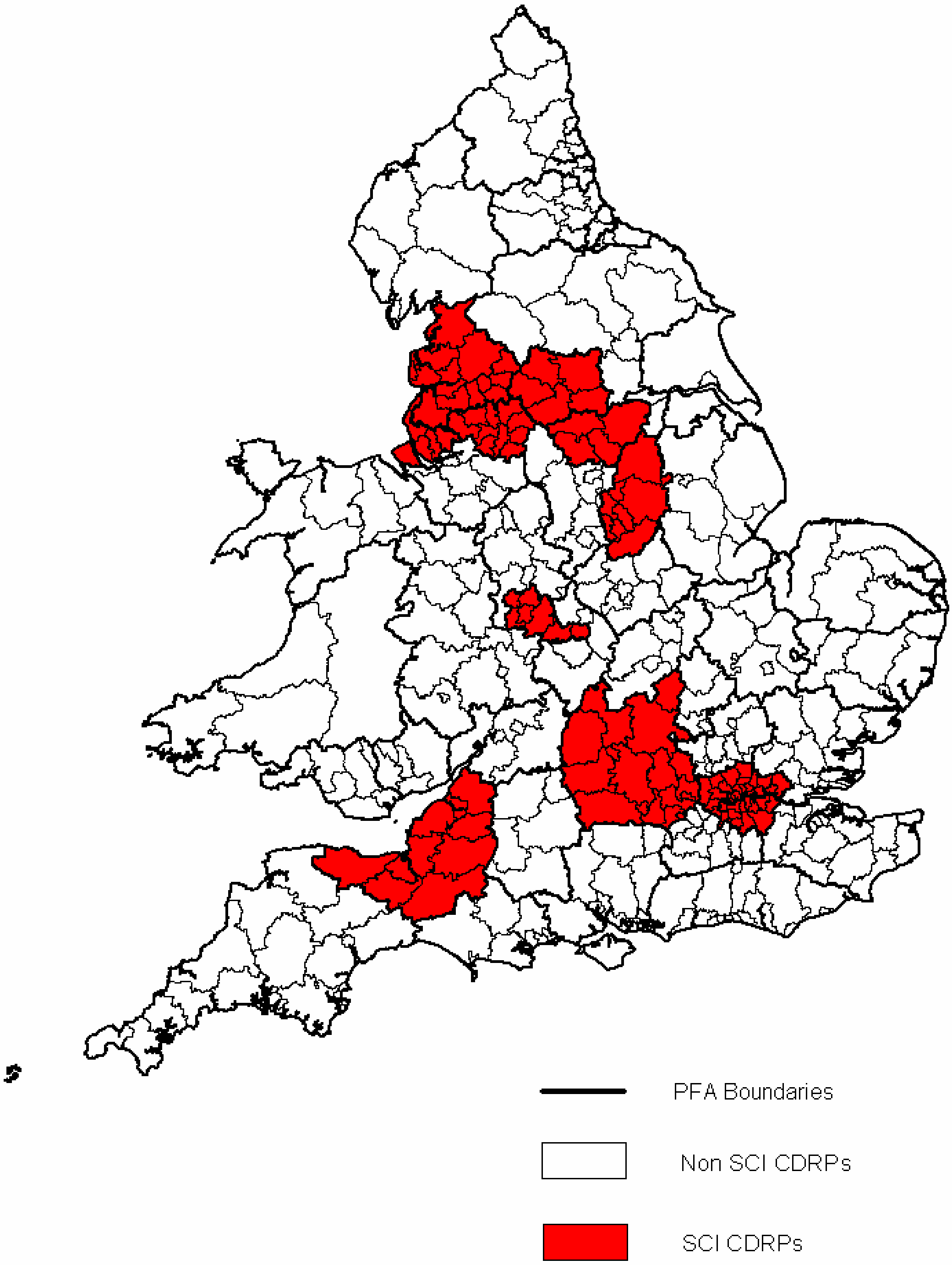
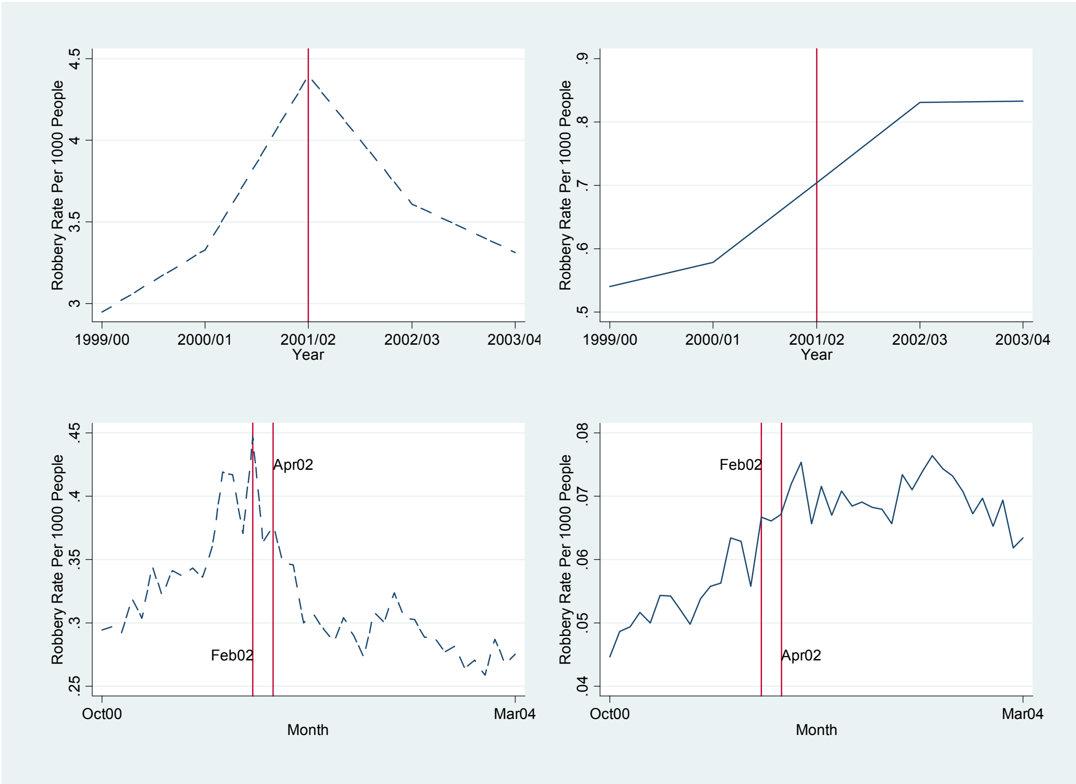


Figure 2: Yearly and Monthly Robbery Rates in SCI and non-SCI Areas



Notes: SCI areas are in the left panel (dotted lines), non-SCI areas in the right panel (solid lines).

Figure 3: Indexed Changes in Robbery Rate

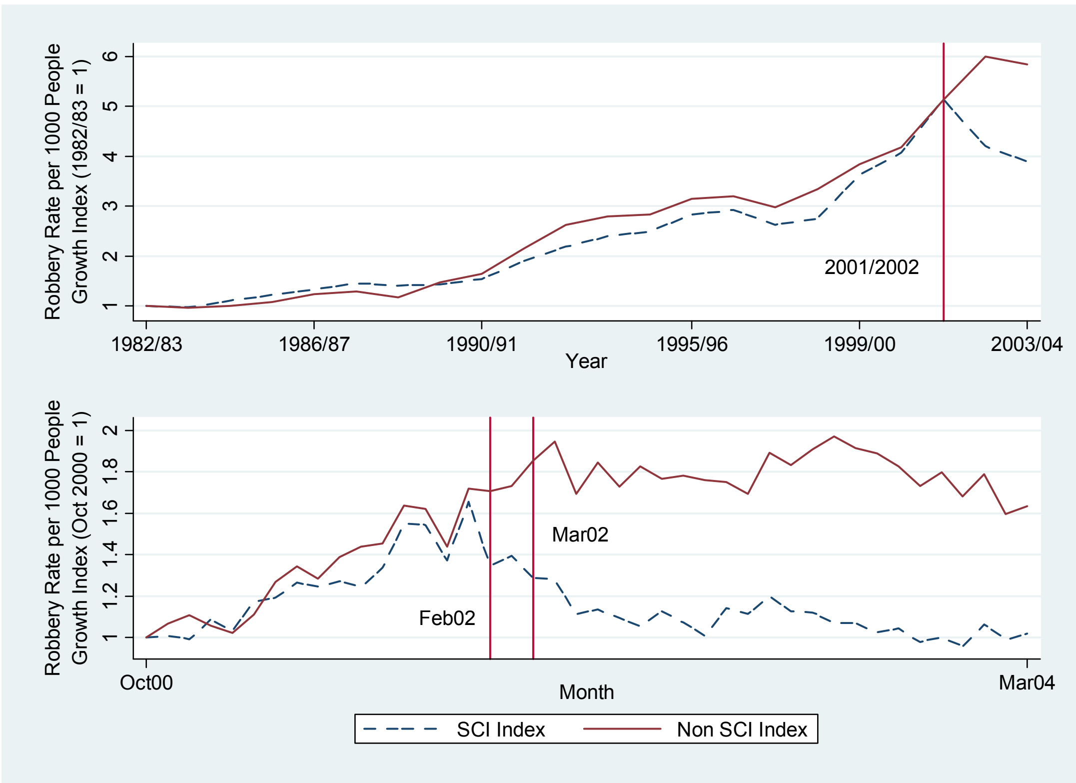
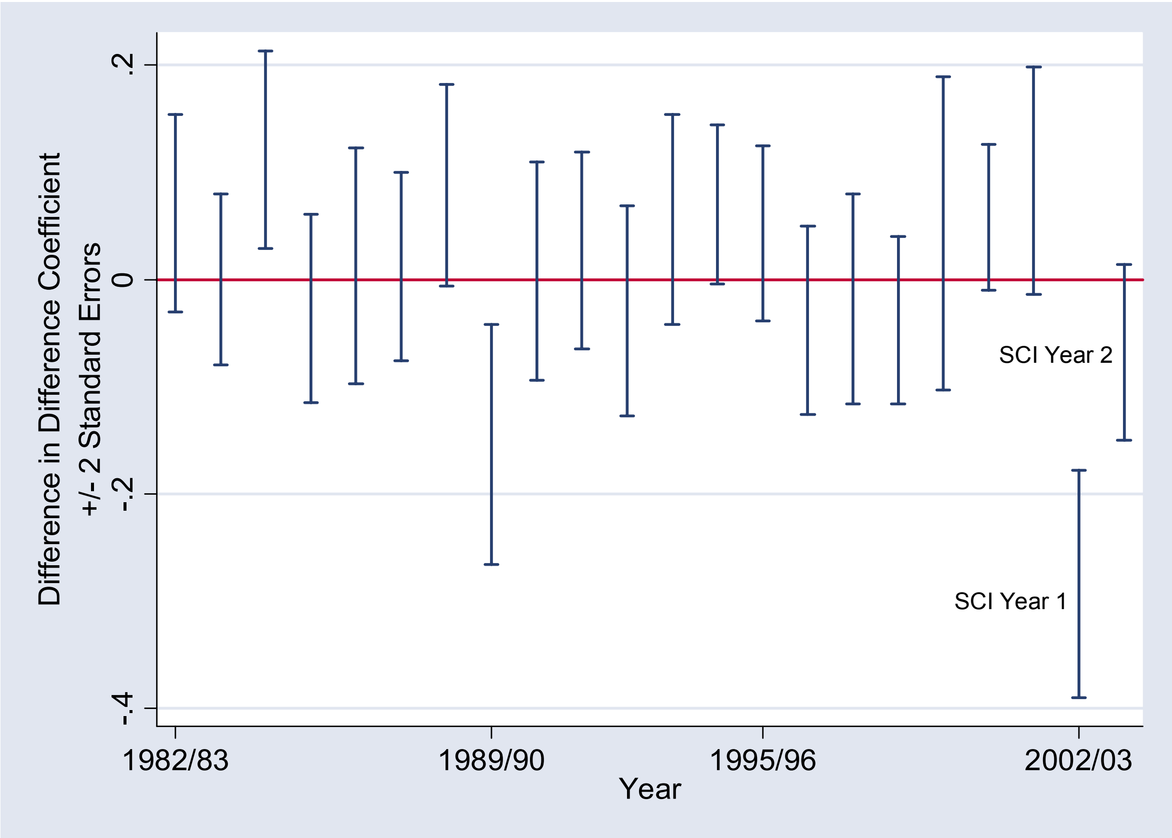


Figure 4: Year on Year Differences-in-Differences Estimates (+/- 2 Standard Errors), 1982/83-2003/04



Notes: The vertical lines are year-on-year estimated difference-in-difference coefficients (+/- two standard errors) for adjacent years with no SCI policy (from 1982/83 to 2001/02) and for the two SCI years (2002/03 and 2003/04).

Table 1: Characteristics of SCI and Non-SCI Police Forces

	Pre-Policy Number of Robberies	Pre-Policy Robberies as Percent of All Crimes	Pre-Policy Robbery Rate Per 1000 People	SCI Extra Funding in Millions of Pounds	SCI Extra Funding as Percent of Police Grant
Time Period	2001/02	2001/02	2001/02	2002/04	2002/04
SCI Police Forces (10)					
All	100,840	3.28	4.39	48.181	1.05
Forces 1 to 3	77,942	4.28	6.37	30.172	1.00
Forces 4 to 7	16,612	2.01	2.37	12.526	1.22
Forces 8 to 10	6,286	1.46	1.70	5.483	1.02
Non-SCI Police Forces (32)					
All	20,535	0.84	0.70	n/a	n/a
Forces 11 to 13	3,901	1.13	1.14	n/a	n/a
Forces 14 to 16	3,377	1.14	1.21	n/a	n/a

Notes: Police force names are anonymised (on request of the Home Office). The forces are grouped according to the pre-policy number of robberies.

Table 2: Descriptive Statistics (One Year Before Vs Two Years After)

	Robbery Rates Per 1000 People			Changes and Difference-in-Differences	
	Year Before SCI Introduction, 2001/02	SCI Year 1, 2002/03	SCI Year 2, 2003/04	Change: SCI Year 1 – Year Before Introduction (Standard Error)	Change: SCI Year 2 – Year Before Introduction (Standard Error)
SCI Police Force Areas	4.39	3.61	3.31	-.79 (.32)	-1.08 (.30)
Non-SCI Police Force Areas	.70	.83	.83	.13 (.03)	.13 (.03)
Gap (Standard Error)	3.69 (1.07)	2.78 (.79)	2.48 (.80)	Difference-in-Difference **Levels: - .91 (.31)** **Logs: -.28 (.05)**	Difference-in-Difference **Levels: - 1.21 (.29)** **Logs: -.38 (.04)**

Notes: Based on 375 aggregated Command and Disorder Reduction Partnerships (CDRPs) in 42 police force areas of England and Wales (Metropolitan and City of London are amalgamated). There are 111 CDRPs in 10 police force areas that were exposed to the SCI, and 264 CDRPs in 32 police force areas that were not part of the SCI. Standard errors clustered by police force area in parentheses.

Table 3: Basic CDRP and BCU Models

	Dependent Variable = Log Robbery Rate Estimation Period: Control Year(s) and 2002/2003, 2003/2004						
	Yearly CDRPs		Monthly BCUs	Yearly CDRPs		Yearly PFAs	
Control Year(s)	2001/02	2001/02	2001/02	1999/00, 2000/01, 2001/02	1999/00, 2000/01	1982/83 to 2001/02	1982/83 to 2001/02
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
SCI Area*Policy On	-.343 (.049)	-.338 (.050)	-.349 (.034)	-.242 (.050)	-.192 (.059)	-.217 (.079)	-.294 (.045)
Year or Month Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Area Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
LFS Controls	No	Yes	No	Yes	Yes	No	No
Area Trends	No	No	No	No	No	No	Yes
R-Squared	.980	.980	.897	.973	.974	.969	.986
Sample Size	1125	1125	8438	1875	1500	924	924
Number of Areas	375	375	235	375	375	42	42

Notes: Robust standard errors clustered at the PFA level in parentheses. Regressions weighted by area population. Controls from LFS used are the following proportions: unemployed; economically active; population aged 16-24; working part time; ethnic minority; aged 16-19 in full time education; working in manufacturing.

Table 4: Matched Areas Based on Pre-Policy Census Data

	Dependent Variable = Log Robbery Rate, Estimation Period: Control Year(s) and 2002/2003, 2003/2004		
	Yearly CDRPs		
	Matched Sample	Matched Sample	Matched Sample
Control Year(s)	2001/02	1999/00, 2000/01, 2001/02	1999/00, 2000/01
	(1)	(2)	(3)
SCI Area*Policy On	-.318 (.047)	-.214 (.047)	-.161 (.056)
Year or Month Dummies	Yes	Yes	Yes
Area Fixed Effects	Yes	Yes	Yes
LFS Controls	Yes	Yes	Yes
R-Squared	.972	.961	.963
Sample Size	1038	1730	1384
Number of Areas	346	346	346

Notes: Robust standard errors clustered at the PFA level in parentheses. Regressions weighted by area population. Controls from LFS used are the following proportions: unemployed; economically active; population aged 16-24; working part time; ethnic minority; aged 16-19 in full time education; working in manufacturing. The matched estimates trim the sample of 29 unmatched areas (23 SCI and 6 non-SCI areas) based on propensity scores from the probit model given in the Appendix.

Table 5: Variations Across SCI Police Forces, Within Area Models

	Dependent Variable = Log Robbery Rate Estimation Period: Control Years and 2002/2003, 2003/2004				
	Yearly CDRPs				Yearly PFAs
	All	Matched Sample	All	Matched Sample	All
	(1)	(2)	(3)	(4)	(5)
Control Years	1999/00, 2000/01, 2001/02	1999/00, 2000/01, 2001/02	1999/00, 2000/01	1999/00, 2000/01	1982/83 to 2001/02
	(1)	(2)	(3)	(4)	(5)
Forces 1 to 3	-.303 (.049)	-.270 (.051)	-.264 (.059)	-.228 (.059)	-.354 (.089)
Forces 4 to 7	-.209 (.053)	-.210 (.055)	-.148 (.070)	-.149 (.071)	-.162 (.098)
Forces 8 to 10	-.131 (.054)	-.131 (.055)	-.076 (.065)	-.077 (.066)	.078 (.049)
Year Dummies	Yes	Yes	Yes	Yes	Yes
LFS Controls	Yes	Yes	Yes	Yes	No
Area Fixed Effects	Yes	Yes	Yes	Yes	Yes
P-Value of F- Test of Equality Across Forces	.005	.049	.014	.068	.000
R-Squared	.975	.961	.975	.964	.967
Sample Size	1875	1730	1500	1384	924
Number of Areas	375	346	375	346	42

Notes: Robust standard errors clustered at the PFA level in parentheses. Regressions weighted by area population. Controls from LFS used are the following proportions: unemployed; economically active; population aged 16-24; working part time; ethnic minority; aged 16-19 in full time education; working in manufacturing. The matched estimates trim the sample of 29 unmatched areas (23 SCI and 6 non-SCI areas) based on propensity scores from the probit model given in the Appendix.

Table 6: Cost Benefit Calculations, Year Following SCI Introduction

Control Years: 1999/00 and 2000/01	Estimate = -.192	Estimate = -.161
Effect on Robberies in Percentage Terms: [Exp (Coefficient) – 1]*100	- 17.4	- 14.8
SCI Areas Baseline Number of Robberies: Average Recorded Robberies in Control Years	73,282	73,282
Robberies Reduction in SCI Areas: Baseline*Effect	12,751	10,846
Benefits from Robbery Reduction in SCI Areas: Effect*Baseline*£12,094 (Millions £)	154.2	131.2
Average Annual Cost of SCI over 2002/03-2003/04: (Millions of £)	24.1	24.1
Net Social Benefit (Millions of £):	**130.1**	**107.1**

Notes: The Net Social Benefit is SB_1 – C as defined in the text of the main body of the paper. The £12,094 average social cost of a personal robbery (kindly supplied to us by Richard Dubourg and Joe Hamed) takes into account the larger impact on the criminal justice system component of the cost associated with recorded crimes – it is larger than the £7357 number given in the Dubourg-Hamed (2005) work for British Crime Survey robberies (based on victimisations) since we use data on recorded robberies and so need to add a criminal justice component to the costs and because it nets out robbery against businesses

Table 7: Crime Displacement/Diffusion Effects of SCI, Burglaries and Vehicle Crime

	Dependent Variable = Log Burglary Rate		Dependent Variable = Log Vehicle Crime Rate	
	Estimation Period: Control Years, 2002/03 & 2003/04			
	Yearly CDRPs			
	All	Matched Sample	All	Matched Sample
	(1)	(2)	(3)	(4)
Control Years	1999/00, 2000/01	1999/00, 2000/01	1999/00, 2000/01	1999/00, 2000/01
SCI Area*Policy On	-.031 (.050)	-.013 (.049)	-.007 (.049)	-.004 (.053)
Year Dummies	Yes	Yes	Yes	Yes
Area Fixed Effects	Yes	Yes	Yes	Yes
LFS Controls	Yes	Yes	Yes	Yes
R-Squared	.957	.951	.960	.955
Sample Size	1500	1384	1500	1384
Number of Areas	375	346	375	346

Notes: Robust standard errors clustered at the PFA level in parentheses. Regressions weighted by area population. Controls from LFS used are the following proportions: unemployed; economically active; population aged 16-24; working part time; ethnic minority; aged 16-19 in full time education; working in manufacturing. The matched estimates trim the sample of 29 unmatched areas (23 SCI and 6 non-SCI areas) based on propensity scores from the probit model given in the Appendix.

Table 8: Spatial Displacement/Diffusion,: SCI and Adjacent and Non-Adjacent Areas

	Dependent Variable = Log Robbery Rate Estimation Period: Control Years, 2002/03 & 2003/04			
	Yearly CDRPs			
	SCI Vs Adjacent		SCI Vs Non-Adjacent	
	All	Matched Sample	All	Matched Sample
Control Years	1999/00, 2000/01	1999/00, 2000/01	1999/00, 2000/01	1999/00, 2000/01
	(1)	(2)	(3)	(4)
SCI*Policy On	-.239 (.062)	-.206 (.060)	-.174 (.063)	-.144 (.061)
Year Dummies	Yes	Yes	Yes	Yes
Area Fixed Effects	Yes	Yes	Yes	Yes
LFS Controls	Yes	Yes	Yes	Yes
R-Squared	.980	.970	.976	.967
Sample Size	736	640	1208	1096
Number of Areas	184 (SCI: 110; Adjacent: 74)	160 (SCI: 87, Adjacent: 73)	302 (SCI: 110, Non-Adjacent: 192)	274 (SCI: 87, Non-Adjacent: 187)

Notes: Robust standard errors clustered at the PFA level in parentheses. Regressions weighted by area population. Controls from LFS used are the following proportions: unemployed; economically active; population aged 16-24; working part time; ethnic minority; aged 16-19 in full time education; working in manufacturing. The matched estimates trim the sample of 29 unmatched areas (23 SCI and 6 non-SCI areas) based on propensity scores from the probit model given in the Appendix.

Table 9: Cost Benefit Calculations, Variations Across SCI Areas

	Forces 1 to 3		Forces 4 to 7		Forces 8 to 10	
Control Years: 1999/00 and 2000/01	Estimate = -.264	Estimate = -.228	Estimate = -.148	Estimate = -.149	Estimate = -.076	Estimate = -.077
Effect in Percentage Terms	-23.2	-20.3	-13.8	-13.8	-7.4	-7.3
Robberies Baseline in Control Years	58,546	58,546	10,444	10,444	4,293	4,293
Baseline*Effect	-13,583	-11,885	-1,441	-1,441	-318	-313
Benefits from Robbery Reduction	164.3	143.7.	17.4.	17.4	3.8	3.8
Average Annual Cost of SCI	15.6	15.6	6.3	6.3	2.3	2.3
Net Social Benefits	**148.7**	**128.1**	**11.1**	**11.1**	**1.5**	**1.5**
Diffusion Effect Benefit	55.8	-	-	-	-	-
Total Net Social Benefits	**204.5**	**128.1**	**11.1**	**11.1**	**1.5**	**1.5**

Note: The Net Social Benefit is $SB_1 + SB_2 + SB_3 - C$ as defined in the text of the main body of the paper. The estimates from Table 5 are used for this table. In terms of additional displacement/diffusion effects for disaggregated areas, the only crime type or geographical displacement/diffusion effect which is statistically significant is for burglaries in Forces 1 to 3 (coefficient = -.091, with associated standard error .053). To value this, the average control year burglary baseline is 147,088 and we use a £4,361 social cost from Home Office figures to calculate the diffusion benefit. Estimated displacement/diffusion effects are not significantly different from zero for all other specifications.

Appendix

Table A1: Probit Models of SCI Participation as Function of Pre-Policy Census Area Characteristics

	Pr[SCI = 1]
	Annual CDRPs
Proportion of population under 25	- .896 (3.097)
Proportion ethnic minorities	3.393 (1.471)
Proportion with no educational qualifications	- .868 (1.972)
Proportion economically active	2.072 (2.452)
Proportion unemployed	- 6.403 (7.642)
Proportion working part-time	-.3.449 (4.098)
Proportion living in social housing	- 1.403 (1.599)
Proportion lone parents	10.397 (6.587)
Proportion living in lowest social grade	7.322 (7.428)
Proportion working in manufacturing	- .642 (1.394)
Sample size	375

Notes: Marginal effects reported, standard errors in parentheses. Weighted by population.

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