Do Neighbourhood Renewal Programs Reduce Crime Rates? Evidence from England

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Abstract

Neighbourhood renewal programs have transformed crime reduction strategies in many developed countries. These place-based initiatives emphasise the preventative value of multi-agency work to enhance community safety and social inclusion. The purpose of this paper is to present empirical evidence on the effectiveness of neighbourhood renewal programs by estimating the impact of the UK's Neighbourhood Renewal Fund (NRF) program on crime rates across England between 2000 and 2007. Because the NRF was only made available to the most deprived local areas in England, we are able to estimate its effects using a Differences-in-Differences (DiD) approach and a Regression Discontinuity (RD) design. Our DiD estimates indicate that the NRF led to improvements in the rates of property and violent crime of between 10-25%, with analysis of treatment intensity effects suggesting that for every £1 per capita of NRF monies, crime rates improved by 0.3-0.6%. Our RD estimates reveal that these improvements are especially strong around the threshold for program eligibility – a finding that is particularly robust for reductions in property crime. Furthermore, using a spatial DiD, we identify the diffusion of crime prevention benefits from areas receiving NRF funding to neighbouring areas that did not receive funding. Our results therefore suggest that neighbourhood renewal programs are effective strategies for reducing crime.

Keywords: Neighbourhood renewal; Crime; England

JEL Codes: H40; R

1. Introduction

Neighbourhood renewal programs are place-based interventions for the regeneration of distressed urban areas, and are a common tool of economic development policy in North America, Europe and elsewhere (Carmon, 1999; Couch, et al., 2008; Givord et al., 2017; Gonzalez-Pampillón et al., 2017; Judd & Parkinson, 1990). Central to most renewal programs is a recognition of the need for partnerships between public, private and non-profit agencies to address the deep-seated problems facing disadvantaged communities (Bailey et al., 1995; van Gent et al., 2009). Private-led actions to encourage co-ordinated strategies can play an important role in driving the renewal of distressed urban areas (see, e.g., Brooks, 2008; Cook & MacDonald, 2011; MacDonald et al., 2013). Nevertheless, government-sponsored multi-agency place-based programs, such as Federal Empowerment Zones (Busso et al., 2013), have also been implemented to promote sustainable community development in a number of countries. One high-profile example of such a place-based program was the Neighbourhood Renewal Fund (NRF), which aimed at narrowing the gap in social and economic outcomes between the most deprived local areas and the rest of England.

The NRF involved the transfer of significant resources to local agencies as a way of enabling them to address the long-term social and economic problems in the most disadvantaged areas across England. It provided about £1.875bn throughout the period 2001 to 2006 to deprived local areas, and a further £525m for the year 2007/08 (Cowen et al., 2008). Local areas identified as being among the top 50 most deprived areas in England on any one of six key measures of deprivation were eligible for the NRF. On this basis, 81 out of 345 local areas across England received NRF funding, amounting to about £4 million per participating area per annum.

This paper focuses on crime reduction, one of the major goals of the NRF, for two key reasons. First, notwithstanding long-standing debates about the structural embeddedness and intractability of crime (Garland, 1996), the reduction of criminal activity is still regarded as a fundamental concern for government. Crime is among the most serious problems faced by neighbourhoods (Brooks, 2008). It is detrimental to economic prosperity, a significant drain on the public purse, harms the quality of life of victims (and perpetrators), and can instigate a vicious circle of social and economic decline in the communities in which it is most prevalent (Becker, 1968; Freeman, 1999). Second, crime is one of the few goals of the NRF for which measures were consistently recorded over time at the local area level for the period under study. Data on rates of burglary, vehicle crime, robbery, violence and sexual offences were published at the local level by the UK Home Office from 2000 onwards, and we are able to utilise these indicators to identify the effects of participation in the NRF.

Place-based policies generally seek to improve the quality of life for residents living in disadvantaged areas (Ladd, 1994). Initiatives intended to redevelop and regenerate distressed urban areas through co-ordinated action by multiple agencies have therefore focused on community as well as economic development (Neumark & Simpson, 2015). Prior research on multi-agency neighbourhood renewal programs shows that they can bring stakeholders together to generate better social and economic outcomes within disadvantaged areas (e.g. Roberts et al., 2017). However, systematic empirical research on the effectiveness of neighbourhood renewal programs is surprisingly scarce. Preliminary evaluations of the NRF suggested that it may have narrowed the “gap” between crime rates in the most deprived areas and the rest (see, Cowen et al, 2008; Amion Consulting, 2010; Lupton et al., 2013). Nevertheless, these evaluations are based on descriptive or qualitative analysis, meaning that the findings should be treated with great caution. Robust empirical evidence on the impact of the NRF, would therefore cast valuable light on the relative merits of neighbourhood renewal programs more generally.

In this study, we estimate the impact of the NRF on crime by analysing the rates of property and violent crime across 345 local areas in England between 2000 and 2007 – during which time the NRF was implemented. To do so, we employ a Difference-in-Differences (DiD) approach to estimate the average effect of the NRF on crime rates, a continuous treatment variable strategy to account for potential differences in “treatment intensity”, and a Regression Discontinuity (RD) design to estimate the local effect of the NRF around the threshold for program eligibility.

Our DiD estimates suggest that the NRF lead to improved community safety in those disadvantaged areas benefitting from the additional resources allocated to them through the NRF. Furthermore, the treatment intensity results indicate that the amount of NRF monies received by each local area is an important predictor of both property and violent crime rates, while RD estimates reveal that these positive effects are especially strong around the program eligibility threshold, particularly for property crime. The additional activities undertaken by local agencies in receipt of NRF funding may therefore have been responsible for the reductions in property crime that we observe and for preventing violent crime. The positive program effects that we identify are substantively important and are resistant to a number of robustness checks. Moreover, using a spatial DiD, we find evidence of the diffusion of crime prevention benefits from areas receiving NRF funding to neighbouring areas without funding. Hence, our estimates of the impact of the NRF on crime rates have the potential to contribute to debates about the effectiveness of neighbourhood renewal programs.

In the following section, we discuss the background to the NRF program. The subsequent section describes the empirical strategy that we employ, including information on the data, methods and estimators that comprise our research design. Thereafter, we present the results of the analyses that we undertake, before concluding with a discussion of the implications of the study.

2. The Neighbourhood Renewal Fund (NRF) in England

The NRF was the principal funding mechanism within the UK government's National Strategy for Neighbourhood Renewal (NSNR) introduced in 2000 (Dickinson, 2014). The overarching aim of the NSNR was to reverse the decline of deprived areas across England by: 'marshalling a coalition of policies, resources and people behind a single Strategy which will focus on: reviving local economies; reviving communities; ensuring decent services; and leadership and joint working' (Social Exclusion Unit, 2000: 42).

Local areas were eligible to receive NRF resources if they were within the top 50 most deprived areas on any of the six Indices of Multiple Deprivation for the year 2000 (IMD2000), namely: the employment scale (the number of people who are unable to work due to unemployment, sickness or disability); the income scale (the number of people in receipt of means-tested welfare benefits); the average of ward scores (the population weighted average of the combined employment and income scales for the neighbourhoods in an area); the average of ward ranks (the population weighted average of the combined employment and income ranks for the neighbourhoods in an area); the extent of deprivation (the proportion of the population living in neighbourhoods which rank within the most deprived 10% of neighbourhoods in the country); and, local concentration (the population weighted average of the ranks of an area's most deprived neighbourhoods that contain exactly 10% of the area's population).

To regenerate disadvantaged local economies and to reconstruct (or rebuild) the most deprived areas in England, NRF resources were given to the Local Strategic Partnerships (LSPs) responsible for all the social and economic outcomes within those areas. LSPs receiving NRF monies were then set targets for delivering improvements on a range of indicators; in particular, crime rates, high school test scores, employment rates, housing quality and mortality rates. At that time, all LSPs liased closely with Crime and Disorder Reduction Partnerships (CDRPs) that brought together representatives of the police, local authorities, probation services, health authorities, and private and non-profit sector organizations within each local area to coordinate crime prevention strategies.[1] LSPs receiving the NRF directed, on average, about 20% of the total funding allocation to CDRPs (Amion Consultion, 2010: Cowen et al., 2008).

It is important to highlight here that, due to the un-hypothecated nature of the NRF allocations, it is conceivable that the percentages allocated to crime prevention may have differed slightly across areas. Nevertheless, LSPs were required to work with UK central government to agree a 'statement of use' indicating how the NRF resources would be deployed. Hence, the central government played a critical role in

[1] As a result of the 2000 Local Government Act, LSPs were established across England to develop a community strategy for each local area. Within this context, CDRPs were made responsible for formulating and implementing initiatives that would contribute to LSPs crime reduction objectives. The territories served by LSPs and CDRPs were coterminous with those for the 345 local government areas across England, with CDRPs having been established by law in all of those areas in 1998. Drawing on the UK Census 2001, the size of the areas served by CDRPs varied from 12.13 Km^2 (London Borough of Kensington and Chelsea) to 2407.63 Km^2 (East Riding of Yorkshire), and from 24,457 inhabitants (Teesdale) to 977,087 inhabitants (Birmingham). For more information about the management and responsibilities of LSPs see (Bailey, 2003), for CDRPs, see Kelman et al. (2013).

regulating and monitoring the allocation of the NRF across England, which is likely to have restricted variations in use of NRF monies. In addition to achieving better outcomes in absolute terms, it was envisaged that the NRF would reduce the gap between deprived areas and the rest of the country. If LSPs were judged to be ineffective or failing in addressing the NRF objectives, UK central government reserved the right to intervene in the management of the funds and if necessary to use alternative institutional means for their disbursement (Social Exclusion Unit, 2001). For the above reasons, we do not expect there to have been substantial differences in the relative priority accorded to crime reduction across LSPs receiving NRF resources.

Within this context, CDRP actions intended to improve crime rates were focused on preventative and community safety initiatives. Examples of such additional activities funded by the NRF, included: target hardening (e.g. security improvements to public spaces); enhancement of visible policing presence (e.g. more Police Community Support Officers (PCSOs) and street wardens); youth diversion activity (e.g. extra-curricular activities and support) and risk offending management (e.g. acceptable behaviour contracts) (Cowen et al., 2008: pp. 27). There are sound theoretical reasons for anticipating that these types of social and situational crime prevention interventions help to decrease criminal activity (see, for example, Becker, 1968; Sutherland & Cressey, 1978; Cohen & Felson, 1979). Nevertheless, the empirical evidence on the efficacy of social preventative crime measures is still mixed.

Some estimates of changes in police presence suggest that an increase in police numbers can result in a corresponding decrease in property and violent crime ranging from around 10% (e.g., Nagin, 2013), rising to as much as 20% (e.g., Vollaard & Koning, 2009). Likewise, estimates of the impact of youth diversion programs suggest that young people benefitting from such schemes are between 1.5 to 2.5 times less likely to reoffend (see, e.g., Gensheimer et al., 1986; Wilson & Hoge, 2013), and estimates of target hardening initiatives such as surveillance cameras suggest a reduction in crime of about 25% (see, Priks, 2015).[2]

As a place-based initiative, the NRF program was utilised by participants to resource a wide range of crime prevention activities, as well as numerous actions intended to improve health, employment and education outcomes. Evaluations of other multi-faceted placed-based policies have found that they can improve crime rates (e.g. Brooks, 2008; Hoyt, 2005), though the specific mechanisms through which this occurs are not precisely identified. However, research on crime prevention strategies that seek to pull multiple levers simultaneously suggests that the effects of such multi-pronged approaches are likely to be more modest and diffuse than those for more narrow and focused initiatives (e.g. Cook, 2012). Hence, while we did anticipate a positive overall program effect for the NRF, we were uncertain whether this would be comparable to that potentially observed for the specific actions CDRPs might undertake, but due to data limitations we were unable to evaluate.

3. Empirical strategy

3.1. Data

To estimate the potential effect of the NRF program on crime reduction, we collected data from 345 local areas for the period 2000-2007, 81 of which received NRF resources.[3] The analysis presented here focuses on publicly available measures of

[2] For a useful review of the literature on crime deterrence policies we refer the reader to Chalfin and McCrary, 2017

[3] As discussed, local areas in England within the top 50 most deprived areas on any of the six IMD2000 indices were targeted for the distribution of the NRF. On this basis, 81 out of 345 areas

crime that were collected as part of the police recorded crime series published by the UK's Home Office. To assess the impact of the NRF program, we draw upon measures of property and violent crime.[4] We estimate separate models for the following crimes against property: domestic burglary, measured as the number of burglaries in a dwelling recorded by the police per 1,000 households in each area, and vehicle crime and robbery, measured as the number of such offences per 1,000 population.[5] For violent crimes, we analyse violence against the person and sexual offences per 1,000 population. These indicators are particularly appropriate for our analysis, not only because they reflect key NSNR priorities regarding crime reduction (Lupton et al., 2013), but also because they capture the focus on social (dis)order that influenced much of the UK Labour government's neighbourhood renewal agenda in the 2000s (Levitas, 2005).[6]

Table 1 summarizes the baseline characteristics of English local areas by program participation status. The following characteristics are included from the 2001 UK Census: the average earnings (£ per week) of employees in the area; the percentage of young people in each local area grouped into three age categories (25-

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across England were eligible to receive funding from the financial year 2001/02, when the scheme was inaugurated, to the financial year 2007/08, when the scheme was closed. It is important to note, however, that the UK Government proposed that at the start of the program seven further local areas that were among the top 50 most deprived areas according to the 1998 Index of Local Deprivation should also receive transitional protection/funding for three years (Tunstall & Lupton, 2003). The transitional nature of the funding received by those seven areas may bias our results, hence we have excluded those areas from the sample used for our analysis.

[4] Crime is generally divided in the economics literature into property and violent crime (see, e.g., Doyle et al., 1999; Fajnzylber et al., 2002; Gibbons, 2004; Levitt, 1997, among others), so we follow that convention and group our crime measures into those dimensions of crime.

[5] Although robbery is defined by the UK Office for National Statistics as "an offence in which violence or the threat of violence is used during a theft", it is not recorded by the police in England as violence against the person.

[6] It is important to acknowledge the limitations of crime statistics based on police records, since they may understate the incidence of crime, which is known in the criminology literature as the "dark figure" of non-reported and/or non-recorded crime (Biderman & Reiss, 1967). For England, studies have flagged substantial discrepancies between victimisation surveys, such as the British Crime Survey (BCS), and police recorded statistics, indicating that many crimes are not reported or recorded by the police (see, Maguire, 2012). Although our data likely underestimate the actual rate of crime, formal reporting of such incidents arguably reflects their relative seriousness (see, Tarling & Morris, 2010, among others).

29 years, 20-24 years, and 15-19 years); the percentage of low skilled people; an indicator of ethnic diversity; population density; and an indicator of population concentration. According to the table, while both groups of local areas seem relatively similar in terms of average income, areas that received NRF funding have a larger proportion of low skilled and young residents, higher population density, a higher number of residents concentrated in a small number of neighbourhoods, and are more diverse in terms of residents' ethnicity. It should be noted that although there are clear cross-sectional differences between areas receiving NRF resources and those that were not eligible for funding, our research design should not be affected by those differences (see section 3.2).

[Table 1 about here]

To provide a sense of the study's geographical context, Figure 1 shows the spatial distribution of the amount of NRF monies received per capita for each program participant, and Figure 2 shows the change in crime rates for all measures of criminal activity analysed in our study. In addition, Table 2 reports summary statistics for the number of criminal offenses before and after the NRF implementation. The table shows that local areas receiving NRF monies have much higher average crime rates before 2002, which is not surprising since the NRF program addressed the most deprived areas in England, deprivation being consistently associated with crime incidence (e.g. Kawachi et al., 1999). After 2002, the average change in crime rates diverges for each group. In particular, domestic burglaries, vehicle theft and robbery offences in local areas receiving NRF funding decreased at a faster rate than in those local areas that did not receive funding. As regards violent crime, our data suggest that there was an increase for both crime measures, though violent crime rates rose less in local areas receiving NRF resources. Overall, the results in Table 2 seem to indicate that the NRF had a positive effect on reducing crime victimization.

[Figure 1 about here]

[Figure 2 about here]

[Table 2 about here]

3.2 Methods

We use three different empirical approaches in this paper, a DiD approach, a continuous treatment variable strategy, and a RD design. We begin by estimating the following generalized DiD model that includes area fixed effects to estimate the impact of the NRF program on crime rates:

$$Log(y_{it}) = \alpha_i + \delta_t + \beta D_{it} + \varepsilon_{it} \qquad (1)$$

where y_{it} represents crime outcomes, i.e., domestic burglary, vehicle crime, robbery, violence and sexual offences, for year t (t = 2000, …, 2007) in local area i (i = 1, …, 345), α_i denotes local area fixed effects, and δ_t represents time (yearly) effects. D_{it} is a binary indicator equal to one for those areas being treated after year 2001, and zero otherwise. In mathematical notation, D_{it} = ($treated_i$ * d_t), where $treated_i$ is a dummy variable coded one for participants in the NRF program and zero for those areas non-eligible for funding, and d_t is a time dummy that switches on for post 2001 observations, i.e., after the NRF program was introduced. [7] In this model specification, the fixed effects control for time-invariant differences in crime rates

[7] It should be noted that local areas started to receive NRF monies from mid-2001 onwards, hence our decision to consider the beginning of the treatment period in 2002.

due to unobserved factors that differ across local areas, while time effects control for common time shocks affecting all areas.

As discussed, although there might be cross-sectional differences between program participants and non-participants, the DiD identification strategy rests on the assumption that both groups would have followed a common trend in crime rates in the absence of the NRF program. To investigate the common trend assumption, we undertake Mora and Reggio's (2015) parallel-trends test to establish if there are systematic pre-treatment trend differences between both groups.[8] For all crime outcomes, we cannot reject the null hypothesis of common pre-treatment dynamics (p-values equal to 0.90, 0.79, 0.83, 0.90, and 0.85, for the DiD models predicting domestic burglary, vehicle theft, robbery, violence against the person and sexual offences, respectively).

In addition to identifying an average treatment effect on the program participants the generalized DiD approach enables us to exploit the time variation in our data. More specifically, we extend our analysis by deploying a model including lead and lagged effects to explore whether NRF effects changed over time. Formally:

$$Log(y_{it}) = \sum_{j=-1}^{0} \gamma_j \left(treated_i * d_j\right) + \sum_{j=1}^{6} \rho_j \left(treated_i * d_j\right) + \alpha_i + \delta_t + \varepsilon_{it} \qquad (2)$$

Leads and lags in Eq. (2) are a series of dummy variables, where each lead is set to one if local area *i* will be eligible for the NRF program *j* years in the future and zero otherwise; the estimates of γ_j measure, therefore, potential lead effects, capturing

[8] We perform the parallel-trend test using the STATA command "*dqd*" developed by Mora and Reggio (2015). The command "*dqd*" calculates the test with a null of the parallel paths based on auxiliary regressions in which year dummies are interacted with the treatment dummy. Rejection of this test indicates the violation of the parallel path assumption.

non-parallel trends (if any) on crime outcomes between program participants and non-participants before the NRF implementation. Similarly, lags are dummy variables for each year j following the NRF implementation, where ρ_j measure the lagged effects of the NRF program and tests whether the effects are sustained, decrease or increase over time.

A potential disadvantage of the above approaches is the assumption that "treatment" is a binary outcome, i.e. both strategies consider that program effects within the treated areas would be the same. It is conceivable, however, that the NRF effects on crime might not be homogenous across program participants. Therefore, an alternative way to measure the program effect is to replace the binary treatment group variable in model (1) with a continuous treatment variable. More specifically, we estimate the following model:

$$Log(y_{it}) = \alpha_i + \delta_t + \beta_5(TI_{it} * d_t) + \varepsilon_{it} \qquad (3)$$

here, the intensity of treatment for program participants (TI_{it}) is proxied by the amount of NRF funding per inhabitant for each local area i in year t. The total amount of NRF monies allocated to each local area was determined by the UK central government. More specifically, the amount of funding allocated to each local area was related to the number of local residents living in deprived neighbourhoods, hence treatment intensity, like program participation, was not self-determined by the local areas but was determined by the UK Central government.

As a fourth step, we complement the generalized DiD results by estimating a number of RD models after the NRF implementation. The nature of the NRF program makes it a good candidate for a RD design to estimate a local average treatment effect around the threshold.[9] As mentioned above, local areas were eligible to receive funding if they were among the top 50 most deprived areas on *any* of the six indices in the IMD2000. When multiple rating scores determine assignment to only two treatment conditions, i.e. treated or non-treated, a "single" rating score can be created that alone determines treatment assignment, the so-called "binding score" approach (see, Reardon & Robinson, 2012). Hence, to reduce the multidimensionality of the problem, we propose here to create a new deprivation variable to capture the binding score.

As noted in section 2, the eligibility of local areas for the NRF is based on six individual indices of deprivation, i.e., employment scale, income scale, average of ward scores, average of ward ranks, extent, and local concentration. Because the NRF was made available only to areas among the top 50 most deprived on these indices of socio-economic disadvantage, the program represents an ideal setting for the application of quasi-experimental methods. The distribution of the six indices of deprivation, however, varies substantially, differing also in the unit of measurement. For these reasons, we need to deploy a standardization strategy in order to make the six different indices comparable. In doing so, we consider the conventional standardization approach given by the following expression:

$$z_{ik} = \frac{w_{ik} - \mu_k}{\sigma_k} \tag{4}$$

where w_{ik} is the deprivation of local area i in index k, μ_k is the mean of the index of deprivation k, and σ_k its standard deviation. Once the six deprivation indices are standardized, we construct a new rating variable (X_i) defined as the maximum of the difference between the individual indices and the cut-off of each dimension (i.e., the

[9] A detailed explanation of RD designs can be found in Angrist and Pischke (2008), Imbens and Lemieux (2008), Lee and Lemieux (2010), and Cattaneo et al., (2017), among other sources.

value for the 50th most deprived local area). The rationale behind the choice of this functional form is that the distance from the threshold would be comparable for each rating score (Robinson, 2011; Reardon & Robinson, 2012). Formally,

$$X_i(z_{ik}) = \max_k\{z_{ik} - z_{(50)k}\} \tag{5}$$

where X is a continuous variable that perfectly determines program participation, z_{ik} is the standardised value of the deprivation index k in local area i and $z_{(50)k}$ is the cut-off of index k. Following Reardon and Robinson (2012: 96), given X, we can use single rating RD methods to estimate the effect of the NRF program on crime.[10]

As in our DiD models, our outcome variables of interest are the five indicators of crime victimization (y_{it}). Local areas are assigned to the treatment status if the running or forcing variable (X_i) is equal or greater than the cut-off point or threshold, i.e. if $X_i \geq 0$, hence $T_i = 1(X_i \geq 0)$. In our RD analysis, we use a non-parametric local polynomial approach with a triangular kernel, optimal bandwidth selection and robust confidence intervals as described in Calonico et al. (2014). Specifically, we estimate the following local linear polynomial approach:

$$Log(y_{it}) = \alpha + \tau T_{it} + \beta(X_{it} - \bar{x}) + \gamma T_{it}(X_{it} - \bar{x}) + \varepsilon_{it} \tag{6}$$

where the estimate of τ is our coefficient of interest, which approximates the effect on crime of the NRF program at the cut-off. To estimate this model, we first restrict the sample to local areas within some optimal bandwidth[11], and then estimate a weighted least-squares regression with a triangular kernel weighting function. To evaluate our results' robustness, we also report RD estimates using different polynomial orders and alternative kernel functions in Appendix B.

[10] To check the sensitivity of our results to alternative binding scores, we report RD estimates in Appendix A using different standardization approaches and different functional forms of Eq. (5).

[11] In this paper, we follow Calonico et al. (2017) and use data-driven optimal bandwidths such as mean square error optimal bandwidths and coverage error rate optimal bandwidths (see Section 4).

Identification in RD designs relies on the assumption that individuals cannot manipulate the treatment assignment variable, or running variable (Lee & Lemieux, 2010). In our context, assignment manipulation is unlikely to happen given the nature of the eligibility rule. First, deprivation indices are constructed by the UK central government based on official statistics. Secondly, the eligibility rule was based on deprivation scores constructed and made publicly available before the NRF announcement. Nevertheless, to add confidence in our approach, we formally evaluate the assumption of absence of manipulation by means of the test developed by Cattaneo et al. (2018), which extends the continuity test first proposed by McCrary (2008). This tests the null hypothesis of continuity of the running variable around the threshold by means of a local polynomial distribution regression approach. Using three different polynomial degrees the results of this analysis indicate that there is no evidence of discontinuity around the threshold (p-values equal to 0.533, 0.102 and 0.324, using polynomials of order one, two and three, respectively).

To conclude this section, it is important to note that DiD and RD models do not estimate the same quantities. DID estimates are usually interpreted as the relative effects of a policy on treatment versus control groups, while RD estimates are generally interpreted as local average treatment effects around the threshold. Hence, program effects estimated with the RD approach are based on differences between areas immediately above and below the thresholds determining program participation status, while DiD estimates should capture the program effects on all of the participating areas. Therefore, although RD designs are potentially more credible, in terms of identification, than alternative strategies such as DiD (Lee & Lemieux, 2010), the latter still provide valuable insights about the generalizability of RD estimates outside the neighbourhood of the threshold. This combination of identification strategies is being increasingly deployed in policy/program evaluation studies using observational data (see, e.g., Feng et al., 2018; Hanson & Rohlin, 2017; Kogan et al., 2016; Meghir et al., 2018, among others).

4. Results

In this section, we begin by assessing the impact of the NRF program on property and violent crime using the generalized DiD models described above, before presenting and discussing estimates of RD models.

4.1. Difference-in-differences results

The results of our DiD analyses (Eq. 1) are reported in the two panels of Table 3. Panel A presents estimates from models predicting property crime, while Panel B shows the results from DiD models predicting violent crime. Overall, the results suggest crime rates in local areas participating in the NRF program fell faster than in "non-treated" areas. Starting with the NRF effect on property crimes, DiD estimates suggest that the NRF program is associated with a reduction of domestic burglary victimisation by about 13% (95% CI [-0.18, -0.07]), vehicle crime incidence by about 9% (95% CI [-0.14, -0.06]), and robbery by about 24% (95% CI [-0.31, -0.18]). When we turn to violent crime, the results are very similar in both magnitude and significance; our results show that receiving NRF monies is associated with a positive effect in terms of preventing violence against the person (β=-0.12; 95% CI [-0.20, -0.04]), and sexual offences (β=-0.15; 95% CI [-0.22, -0.09]). Since violent crime was rising across England at this time, the estimates indicate that it did so at a slower rate in areas receiving NRF resources.

[Table 3 about here]

The initial DiD specification presented above provides no information about treatment dynamics. In other words, our first model does not indicate whether the effect of the NRF program on crime varies over time. Analysis of the effect dynamics might help to better understand the impact of the NRF program. To explore these dynamics, Figure 3 visually depicts estimates for the leads and lags model described in Eq. (2).

[Figure 3 about here]

Looking first at the parallel trend assumption, our results suggest that there are no anticipatory effects or differences between both groups before 2002 in any of the estimated models. It should be highlighted, however, that although leads and lags models, and Mora and Regio's test both point to the absence of differential pre-treatment trends, given the low number of pre-treatment periods we cannot entirely discard potential region-specific trends which might bias our DiD estimates. In particular, it is conceivable that those areas exhibiting persistent increases in crime rates would likely have been eligible to participate in the program. If this was the case, our DiD estimates would be biased downwards, i.e. DiD models would understate the effects of the program, sometimes called "Ashenfelter's dip" or "pre-programme dip" (Ashenfelter, 1978; Heckman & Smith, 1999).

As regards inter-temporal effects, our results suggest that domestic burglary rates decreased substantially from year 2003, the positive effect reaching its peak in

2007, where the estimated impact of the program is a reduction of about 17%. Looking now at the NRF impact on the other two indicators of property crime, i.e. vehicle crime and robbery, the results almost mirror our findings regarding domestic burglary; the positive impact of the NRF is particularly evident after 2003, reaching its peak in 2007, with an estimated reduction of crime victimization of about 16% and 33%, respectively. Regarding violent crime, our results suggest that the increase in both violence against the person and sexual offences was slower in areas receiving NRF funds from year 2002. The estimated dampening effect reaching its peak in 2007 for violence and 2006 for sexual offences, where the estimated impact of the program is about -17% and -19%, respectively.

4.2. Continuous treatment variable strategy

Table 4 reports estimates for the continuous treatment variable strategy depicted in Eq. (3). The results suggest that the amount of NRF monies received by each local area is a non-trivial predictor of both property and violent crime. The size of the point estimates for the continuous treatment variable indicate that a £1 per capita increase in NRF funding is associated with a reduction in domestic burglary and vehicle crime of about 0.3%, and a reduction of 0.6% in robbery victimisation. Regarding violence against the person and sexual offences, point estimates suggest that additional funding is associated with a decrease of violent crime victimisation (in comparative terms) of about 0.3% and 0.5%, respectively. On average, treated areas in our sample received each year £21.64 per capita in NRF monies, and so the coefficient estimates suggest an economically significant impact.

[Table 4 about here]

4.3. Regression Discontinuity analysis

We now turn to the effect of the NRF program on crime in the RD set up. First, Figure 4 depicts RD plots for property and violent crimes. These plots suggest a potential positive effect of the NRF program on property crime in England: the domestic burglary, vehicle crime and robbery plots show a downward discontinuity just at the right side of the threshold (or cut-off) vertical line. On the other hand, RD plots for violent crimes are less supportive of a positive program effect; although there seems to be a slight downward discontinuity around the threshold for both violent crime indicators, this discontinuity is not as evident as for property crimes.

[Figure 4 about here]

Moving now to the regression results, Table 5 reports point estimates, and robust 95% confidence intervals for the local linear polynomial approach described in Eq. (6). In RD non-parametric analyses, the choice of bandwidths is an important matter, since those bandwidths define the weight assigned to each observation. Hence, we present a variety of RD estimates using mean square error (MSE) optimal bandwidths (h_{MSE}) and coverage error rate (CER) optimal bandwidths (h_{CER}), alternatively (for a detailed explanation of bandwidth selection alternatives see Calonico et al., 2017).

Starting with the NRF effect on property crime, RD estimates indicate larger effects than DiD estimates; first, the estimated impact of the NRF program is a reduction of domestic burglary rates of about 42% (95% CI[-0.68, -0.25]) based on MSE-optimal bandwidths. When using CER optimal bandwidths, the RD estimate points to a decrease of about 43% (95% CI[-0.70, -0.19]). Similarly, when looking at the NRF effect on vehicle crime rates, our results suggest again that the program did help to reduce the incidence of vehicle crime in areas receiving NRF funding. Based on MSE optimal bandwidths, the RD estimate is -0.2375 (95% CI[-0.44, -0.11]), which suggests that the NRF program reduced vehicle crime rates by about 24%. When using CER optimal bandwidths, the RD estimate for vehicle crime rates is again very similar (-0.2547; 95% CI[-0.47, -0.07]), hence giving us additional confidence in the robustness of our findings. The RD estimates of the impact of the NRF on robbery are again negative, suggesting a reduction of robbery rates of about 67%-65% depending on the optimal bandwidth used. Turning to the results for violent crime reported in Table 5; Panel B. RD estimates for both types of violent crime, i.e. violence against the person and sexual offences are again negative. However, an analysis of the 95% CIs suggests that these estimates are not statistically different from zero in some specifications, particularly as regards sexual offences.

[Table 5 about here]

One potential concern with the RD results presented thus far is the relatively low number of observations at each side of the threshold, which might bias our RD estimates. To alleviate this potential small sample bias, we propose to include in our models a set of pre-treatment covariates. The inclusion of control covariates, though not necessary for identification purposes, may help to increase the precision of the RD estimates and might eliminate some of the small sample bias in cases where the number of observations close to the threshold is small (Imbens & Lemieux, 2008).

Here, it is important to note that including covariates that might be affected by the treatment may bias RD estimates, hence just pre-treatment characteristics should be included as controls (Pettersson-Lidbom, 2008). Therefore, we report in Table 6 RD estimates including the baseline characteristics depicted in Table 1 (i.e., the average earnings of the employees in the area, the percentage of young people across different age categories, , the percentage of low skilled people, an indicator of ethnic diversity, population density, and an indicator of population concentration) taken from the UK Census 2001.

Two important patterns emerge from Table 6; first, the estimated coefficients seem more pronounced across almost all model specifications when compared to RD estimates without controls. Second, the inclusion of additional control covariates seems to reduce the variance of our estimates, which now point to a statistically significant negative association between the NRF and property and violent crime rates in all model specifications.

[Table 6 about here]

In addition to the results reported in this section, Appendix A and B include a series of further specifications to check our results' sensitivity to alternative approaches to constructing the binding score and to alternative polynomial orders and other kernel functions. Firstly, we report RD estimates in Appendix A using different standardization approaches (Eq. 4) and different functional forms of Eq. (5). The results of our analysis do not seem to depend on the choice of binding score for property crime, with all alternative approaches producing similar results. For violent crime, i.e. violence against the person and sexual offences, we again obtain negative point estimates. In line with our baseline RD estimates including controls, the results are statistically different from zero (see Tables A.1, A.2 and A.3; Figure A.1). Finally, we report estimates of our RD model using different polynomial orders and other kernel functions in Appendix B. Table B.1 and B.2 show RD estimates considering a quadratic polynomial, and Tables B.3 and B.4 present non-parametric local linear estimates using Epanechnikov kernel functions, with Table B.5 and B. reporting non-parametric local linear estimates using Uniform kernel functions. Consistent with previous estimates, the results reported in Appendix B suggest that the NRF program had a positive effect in reducing property crime. As regards violent crime, most of these alternative RD estimates are negative and statistically significant in line with the previous results.

Finally, although we are not aware of other policies that might have altered crime rates in the areas receiving NRF monies that perfectly overlapped in time and participants, 35 small neighbourhoods in 34 of our treated local areas and one neighbourhood in one of the control areas were also targeted by the New Deal for Communities (NDC).

As part of the NSNR, the UK government announced the NDC program in 1998, which had similar goals to the NRF, but was aimed specifically at about 1% of the most deprived neighbourhoods in England: 39 small areas of about 10,000 people each (Gutierrez Romero, 2009; Social Exclusion Unit, 2001). Although most NDC projects were implemented from 2002 onwards (Gutierrez Romero, 2009), hence clearly overlapping with the NRF, we believe that the potential effect of the NDC in our estimates should be minimal due to the narrow geographical scope of the NDC and its implementation via bespoke NDC neighbourhood partnerships. Nevertheless, as a further robustness check we report in Tables C.1 and C.2 (Appendix C) our baseline RD estimates (with and without control covariates) excluding those local areas where any small neighbourhood was selected for the NDC. The exclusion of these local areas does not lead to a re-evaluation of our findings.

In sum, there is evidence of a significant association between the NRF and property crime reductions; using a generalized DiD approach, we find that the NRF had substantial effects on reducing domestic burglary, vehicle crime and robbery. Taking advantage of the program eligibility rule and using a RD design, we find even larger effects. Moreover, our continuous treatment variable strategy suggest that additional funding made a difference in terms of property crime reduction. Regarding violent crime, our findings point in the same direction. However, the latter results should be treated with caution, since the violent crime estimates are less robust to alternative model specifications; DiD and continuous treatment variable models affirm that the NRF was effective in preventing violent crime, but the estimated effects are not statistically different from zero in some RD specifications.

Overall, it would appear that the activities made possible through the NRF contributed to a reduction in crime in the most deprived areas. This may in part reflect the impact of the program on education, health and employment outcomes in disadvantaged areas, but is also likely to be a result of the additional crime prevention initiatives that were undertaken by CDRPs in the areas receiving NRF monies. In particular, due to participation in the program CDRPs were able to support: security upgrades (e.g. the Street Lighting and Highway Signs PFI Project – Safer Sunderland Partnership, 2005); new youth diversion schemes (e.g. the Youth Inclusion Project - Barking & Dagenham Partnership, 2005); additional community policing (e.g. the Street Crime Warden Service – City Safe: Liverpool a Safe City, 2005), support services for offenders and victims of crime (the Rape Crisis Helpline - Leicester Partnership Against Crime & Disorder, 2005).[12]

4.3. Less crime or displaced crime?

A crucial issue in determining the effectiveness of the NRF in terms of crime reduction is whether crime relocates as a result of the activities that it funded, such as target hardening and enhancement of visible policing presence – what is usually referred to as crime displacement (Guerette & Bowers, 2009). Prior research suggests that crime reduction interventions can sometimes generate negative displacement effects of one kind or another (Barr & Pease, 1990). In this instance, it is conceivable that criminals operating in disadvantaged areas benefitting from the NRF simply switch their attention to neighbouring areas, which may be perceived to be more vulnerable or attractive options than their 'home turf'. If criminal activity is indeed displaced from areas participating in the NRF program to adjacent areas, the overall effectiveness of the NRF in terms of crime reduction would be partly diminished.

To test for crime displacement effects, we follow Delgado and Florax (2015) and deploy a spatial DiD model that accounts for potential spatial spillovers of program participation. Specifically, we estimate the following model:

$$Log(y_{it}) = \alpha_i + \delta_t + \beta D_{it} + \theta \sum_{j=1}^{345} w_{ij} D_{jt} + \varepsilon_{it}$$

where w_{ij} is an element of the spatial matrix (W) reflecting the relative connectivity between local areas. We present estimates based on a row-normalized spatial contiguity matrix (common border between local areas) and, to check the results' sensitivity to alternative spatial matrices, we also report estimates based on a row-normalized spatial inverse distance-squared matrix, where the magnitude of the

[12] Unfortunately, comprehensive data on all of the initiatives undertaken by CDRPs are not publicly available, so we are unable to incorporate information on those activities directly into our research design.

spatial interaction is measured by the squared inverse distance between the geographic centres of the studied areas. Hence, the potential local spatial spillover of receiving NRF funding would be that associated with the spatially lagged variable indicating program participation (θ).

Table 7 presents results for the spatial DiD models. We find no evidence of displacement effects. On the contrary, our results suggest, overall, that there may exist what are known in the criminology literature as “diffusion benefits” (Bowers et al., 2011) or, in other words, that the benefits of crime reduction activities/policies may spread to neighbouring areas. These findings are in line with recent evidence suggesting that diffusion of crime prevention benefits is more likely to occur than spatial crime displacement (see, e.g. Bowers et al., 2011; Johnson et al., 2014).

[Table 7 about here]

5. Conclusion

In carrying out this study, we anticipated that rates of property and violent crimes would improve in disadvantaged areas receiving support for crime reduction activities through a major neighbourhood renewal program – the NRF. Using rigorous quasi-experimental techniques our results indicate that the NRF did indeed lead to lower crime rates in the most deprived local areas of England. Moreover, the crime reduction associated with the NRF is not only statistically significant, but substantively important. On average, the rate of victimisation from burglary, robbery and vehicle crime in areas receiving the NRF decreased by almost 30% during the study period (2000-2007). In addition, the rate of victimisation from violent crimes increased by around 10% less than in areas not participating in the NRF, though this finding was less robust to alternative model specifications. These estimates therefore provide strong evidence of the potentially beneficial impact of neighbourhood renewal programs on rates of criminal activity in disadvantaged areas.

To date, research on the efficacy of preventative approaches to crime reduction is somewhat equivocal about the impact of such strategies (Nagin, 2013) – as too is empirical evidence on the effectiveness of neighbourhood renewal programs more generally (van Gent et al., 2009), and the NRF in particular (Lupton et al., 2013). Whatever the merits of preventative and place-based approaches to crime reduction for generating feelings of community safety (Crawford & Evans, 2016), few studies provide robust analysis that demonstrate improvements in recorded crime rates. Our analysis indicates that disadvantaged areas can benefit from additional publicly funded crime prevention activities. This beneficial effect was particularly apparent in those areas that were only slightly more deprived than others failing to meet the eligibility criteria for the NRF. One important implication of this might therefore be that the most deprived areas require considerably more resources to match the rate of crime reduction achieved in less deprived areas - as our treatment intensity analysis also suggests. In addition, the activities aimed at reducing crime in disadvantaged areas appear to have had a range of positive 'spillover' effects on criminal behaviour and activity in more prosperous neighbouring areas.

While the empirical strategy that we employ gives us considerable confidence in the results that we present, there are other aspects of crime reduction in local areas that we have not been able to fully incorporate within our research design. Firstly, as for evaluations of other place-based policies, it is not possible to accurately identify all the additional activities that were undertaken by CDRPs receiving NRF monies. Examples of such actions can be garnered from the small sample of CDRP community safety strategies that are still available via The National Archives UK, but systematic information on how the NRF resources were allocated to specific initiatives is needed to facilitate a fine-grained understanding of the most (and least) effective interventions. Secondly, since criminal activity tends to be under-reported in recorded crime figures, the observable reductions in crime that we identify may actually underestimate the 'true' effect of the NRF on crime rates. Whether the activities supported by the NRF were associated with a complementary decrease in less serious property and violent crime is something that could usefully be explored through in-depth case studies with police forces in England. Likewise, the impact of the NRF on the willingness of victims to report crimes to the local police force is something that would cast further valuable light on the nature of the evidence that we present. Thirdly, it is also possible that the fear of crime in disadvantaged areas decreased as a result of NRF-resourced interventions. Although perceptions of crime and recorded crime rates are not always correlated (Hale, 1996), subsequent research could seek to analyse the connection between the NRF, crime rates and fear of crime by drawing upon data sources capturing public attitudes towards criminal activity, such as the British Crime Survey.

In sum, this study indicates that it is possible to achieve desired outcomes through place-based interventions that provide local agencies with the resources needed to develop and implement additional preventative approaches to crime reduction. Although further investigation is required to identify the precise interventions through which agencies were able to achieve improved crime rates, there is strong reason to believe that the NRF was instrumental in enabling the implementation of these interventions. The finding that disadvantaged areas can benefit from neighbourhood renewal programs may therefore be a finding that is generalizeable to other contexts than the UK.

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Table 1. Characteristics of local areas from 2001 census.

	All (N=345)		Non-participants (N=264)		Participants (N=81)	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Average earnings of residents[a]	362.92	53.18	363.67	50.50	360.48	61.35
Proportion of residents						
between 25-29 years of age	6.14	1.47	5.85	1.15	7.09	1.93
between 20-24 years of age	5.46	1.65	5.13	1.48	6.50	1.74
between 15-19 years of age	6.07	0.60	5.95	0.55	6.45	0.60
Proportion of low skilled residents[b]	45.72	7.12	44.36	6.33	50.14	7.79
Ethnic diversity[c]	1571.70	1631.45	1272.54	1120.48	2546.73	2461.64
Population density[d]	1287.02	1777.32	816.32	1082.72	2821.15	2572.00
Population concentration[e]	21.83	7.86	19.63	4.82	29.00	11.00

Notes: S.D. stands for standard deviation; (a) median gross weekly pay of full-time employees on a workplace basis (in British Pounds); (b) residents with no qualifications or only UK Level 1 qualifications; (c) the proportion of the different ethnic sub-groups within the local population identified in the UK national census was squared, and the sum of the squares was subtracted from 10,000, with a higher level of diversity reflected in a higher score of the index; (d) persons per Km^2; (e) To construct this indicator, we gathered information on the population density for the 32482 English Lower Layer Super Output Areas (LSOAs). These LSOAs are a geographic disaggregation of our unit of analysis, i.e. English local areas that typically contain, on average, 1500 individuals each. An index of population concentration was then created for each local area by calculating the standard deviation of the population density of all the LSOAs within each area. Larger index values indicate that a larger share of the population is concentrated in a small number of neighbourhoods, whereas if the population were distributed evenly across a local area, the index value would be close to zero.

Table 2. Summary statistics for crime data.

	All (N=345)		Non-participants (N=264)		Participants (N=81)	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Criminal offences 2000-01						
Domestic burglary	15.19	9.80	11.72	6.18	26.30	10.93
Vehicle crime	15.96	9.49	13.32	7.09	24.45	11.15
Robbery	1.28	2.26	0.66	0.79	3.32	3.80
Violence against the person	9.84	5.91	8.15	3.96	15.36	7.61
Sexual offences	0.63	0.35	0.53	0.23	0.96	0.46
Criminal offences 2002-07						
Domestic burglary	12.20	7.96	9.91	5.61	19.63	9.73
Vehicle crime	12.41	6.92	10.68	5.47	18.03	8.07
Robbery	1.21	1.81	0.72	0.78	2.81	2.93
Violence against the person	16.26	7.15	14.25	5.68	22.82	7.52
Sexual offences	1.00	0.45	0.89	0.38	1.34	0.48

Notes: S.D. stands for standard deviation. Domestic burglary reflects the number of burglaries per 1,000 households. Vehicle crime, robbery, violence against the person and sexual offences reflects the number of such offences per 1,000 population

Table 3. Difference-in-differences estimates.

	A. Property crime		
	Domestic Burglary	Vehicle Crime	Robbery
D estimate	-0.1290	-0.0937	-0.2425
	[-0.1839, -0.0740]	[-0.1360, -0.0508]	[-0.3067, -0.1782]
N	2743	2746	2734
Unit effects	Yes	Yes	Yes
Time effects	Yes	Yes	Yes
	B. Violent crime		
	Violence	Sexual offences	
D estimate	-0.1214	-0.1556	
	[-0.2007, -0.0420]	[-0.2241, -.0870]	
N	2760	2760	
Unit effects	Yes	Yes	
Time effects	Yes	Yes	

Notes: Robust 95% CI in brackets

Table 4. Treatment intensity estimates.

	A. Property crime		
	Domestic Burglary	Vehicle Crime	Robbery
Treatment intensity	-0.0026	-0.0030	-0.0062
	[-0.0052, -0.0001]	[-0.0046, -0.0014]	[-0.0088, -0.0036]
N	2743	2746	2734
Unit effects	Yes	Yes	Yes
Time effects	Yes	Yes	Yes
	B. Violent crime		
	Violence	Sexual offences	
Treatment intensity	-0.0030	-0.0048	
	[-0.0057, -0.0002]	[-0.0073, -0.0024]	
N	2760	2760	
Unit effects	Yes	Yes	
Time effects	Yes	Yes	

Notes: Robust 95% CI in brackets

Table 5. Local polynomial RD estimates.

	A. Property crime					
	Domestic burglary		Vehicle crime		Robbery	
	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}
RD coefficient	-0.4209	-0.4279	-0.2375	-0.2547	-0.6706	-0.6496
Robust 95% CI	[-0.6818, -0.2447]	[-0.6971, -0.1928]	[-0.4414, -0.1100]	[-0.4657, -0.0654]	[-0.9940, -0.4294]	[-0.9999, -0.3344]
N_- / N_+	144/90	102/66	160/102	132/72	144/84	90/60
H	0.1874	0.1279	0.2303	0.1572	0.1730	0.1181
	B. Violent crime					
	Violence		Sexual offences			
	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}		
RD coefficient	-0.0752	-0.1247	-0.0730	-0.0749		
Robust 95% CI	[-0.1935, 0.0041]	[-0.2451, -0.0204]	[-0.1877, 0.0019]	[-0.1973, 0.0279]		
N_- / N_+	144/84	90/66	312/156	192/114		
H	0.1801	0.1229	0.3757	0.2565		

Notes: RD estimates computed using local linear methods with triangular kernel function. Bias-corrected robust estimators of standard errors developed by Calonico et al. (2014). Optimal bandwidth selection relies on two different procedures: mean square error optimal bandwidth selector (h_{MSE}); coverage error rate optimal bandwidth (h_{CER}) (see Calonico et al. 2015).

Table 6. Local polynomial RD estimates with covariates included.

	A. Property crime					
	Domestic burglary		Vehicle crime		Robbery	
	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}
RD coefficient	-0.3511	-0.4369	-0.2899	-0.2940	-0.6520	-0.6462
Robust 95% CI	[-0.5134, -0.2067]	[-0.6056, -0.2596]	[-0.4178, -0.1744]	[-0.4276, -0.1572]	[-0.8333, -0.4966]	[-0.8423, -0.4418]
N_- / N_+	221/126	149/102	297/138	184/114	222/132	156/102
H	0.3064	0.2092	0.3579	0.2444	0.3215	0.2197
	B. Violent crime					
	Violence		Sexual offences			
	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}		
RD coefficient	-0.1204	-0.1463	-0.2361	-0.2067		
Robust 95% CI	[-0.1830, -0.0619]	[-0.2164, -0.0777]	[-0.3359, -0.1271]	[-0.3168, -0.0797]		
N_- / N_+	449/204	263/138	318/162	192/114		
H	0.4999	0.3413	0.3881	0.2650		

Notes: RD estimates computed using local linear methods with triangular kernel function. Bias-corrected robust estimators of standard errors developed by Calonico et al. (2014). Optimal bandwidth selection relies on two different procedures: mean square error optimal bandwidth selector (h_{MSE}); coverage error rate optimal bandwidth (h_{CER}) (see Calonico et al. 2015).

Table 7. Spatial difference-in-differences estimates.

Estimates based on a spatial contiguity matrix

	A. Property crime		
	Domestic Burglary	Vehicle Crime	Robbery
D estimate	-0.0923	-0.0411	-0.1301
	[-0.1589, -0.0257]	[-0.0930, 0.0107]	[-0.2116, -0.0486]
Spatially lagged D	-0.1000	-0.1427	-0.3053
	[-0.2053, 0.0053]	[-0.2312, -0.0542]	[-0.4404, -0.1702]
N	2743	2746	2734
Unit effects	Yes	Yes	Yes
Time effects	Yes	Yes	Yes

	B. Violent crime	
	Violence	Sexual offences
D estimate	-0.0502	-0.0865
	[-0.1340, 0.0337]	[-0.1617, -0.01125]
Spatially lagged D	-0.1937	-0.1879
	[-0.3326, -0.0549]	[-0.3108, -0.0650]
N	2760	2760
Unit effects	Yes	Yes
Time effects	Yes	Yes

Estimates based on an inverse distance-squared matrix

	A. Property crime		
	Domestic Burglary	Vehicle Crime	Robbery
D estimate	-0.0729	-0.0494	-0.1472
	[-0.1370, -0.0088]	[-0.0998, 0.0009]	[-0.2292, -0.0652]
Spatially lagged D	-0.2283	-0.1791	-0.3836
	[-0.3721, -0.0844]	[-0.3004, -0.0579]	[-0.5723, -0.1950]
N	2743	2746	2734
Unit effects	Yes	Yes	Yes
Time effects	Yes	Yes	Yes

	B. Violent crime	
	Violence	Sexual offences
D estimate	-0.0620	-0.1066
	[-0.1465, 0.0225]	[-0.1822, -0.0310]
Spatially lagged D	-0.2413	-0.1990
	[-0.4209, -0.0316]	[-0.3732, -0.0248]
N	2760	2760
Unit effects	Yes	Yes
Time effects	Yes	Yes

Notes: Robust 95% CI in brackets

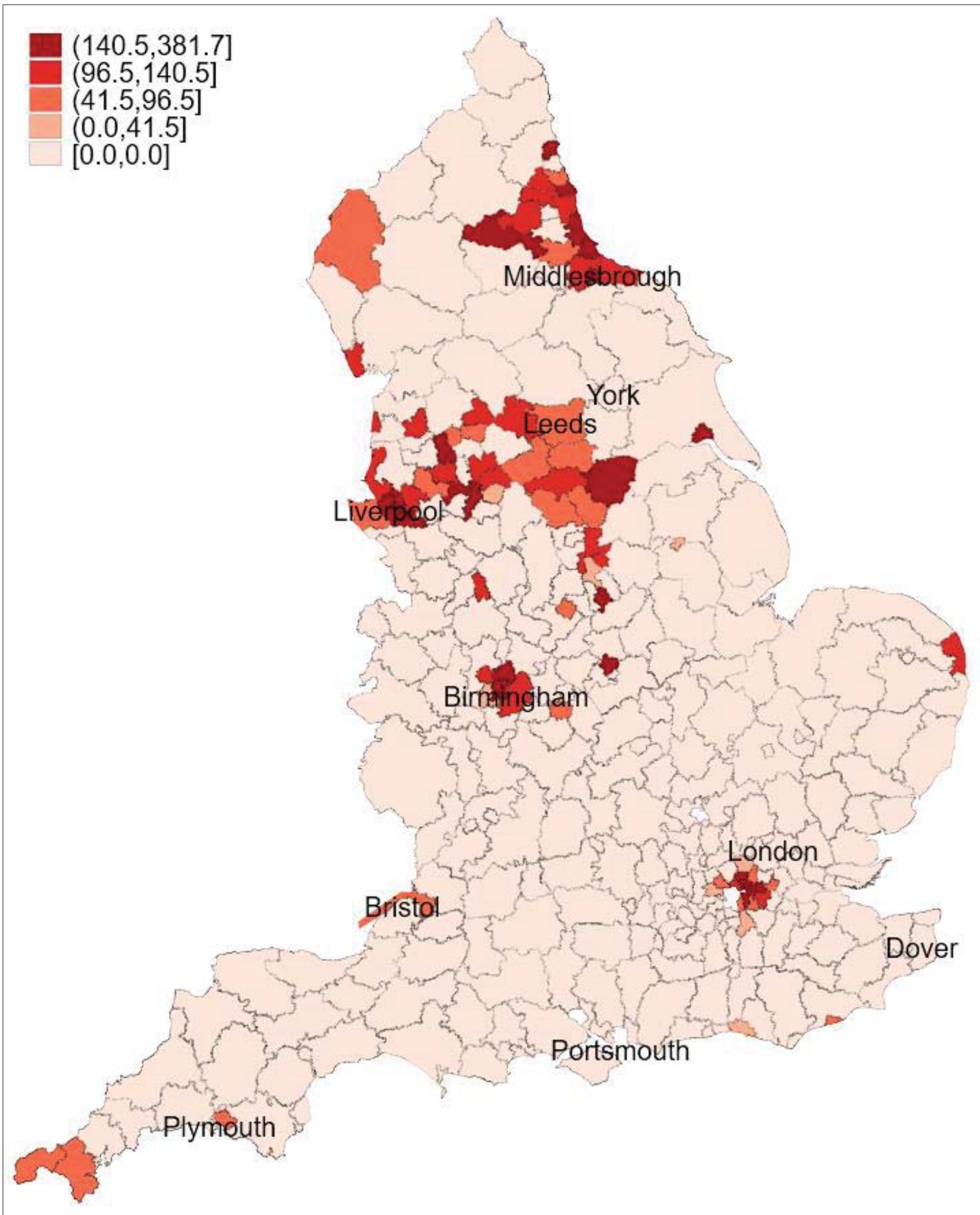

Figure 1. Total NRF allocation. This figure shows the spatial distribution of NRF monies (in British pounds per capita) received by each local area between years 2002 and 2007. The figure highlights that the average NRF allocation to treated areas over the period under study was £129.88 per capita (S.D. 86.44), which is a substantial amount of additional resource when the average local government expenditure per capita in each area across England at that time was about £1,500 - (see also Lupton, Fenton & Fitzgerald, 2013, for a discussion of this issue).

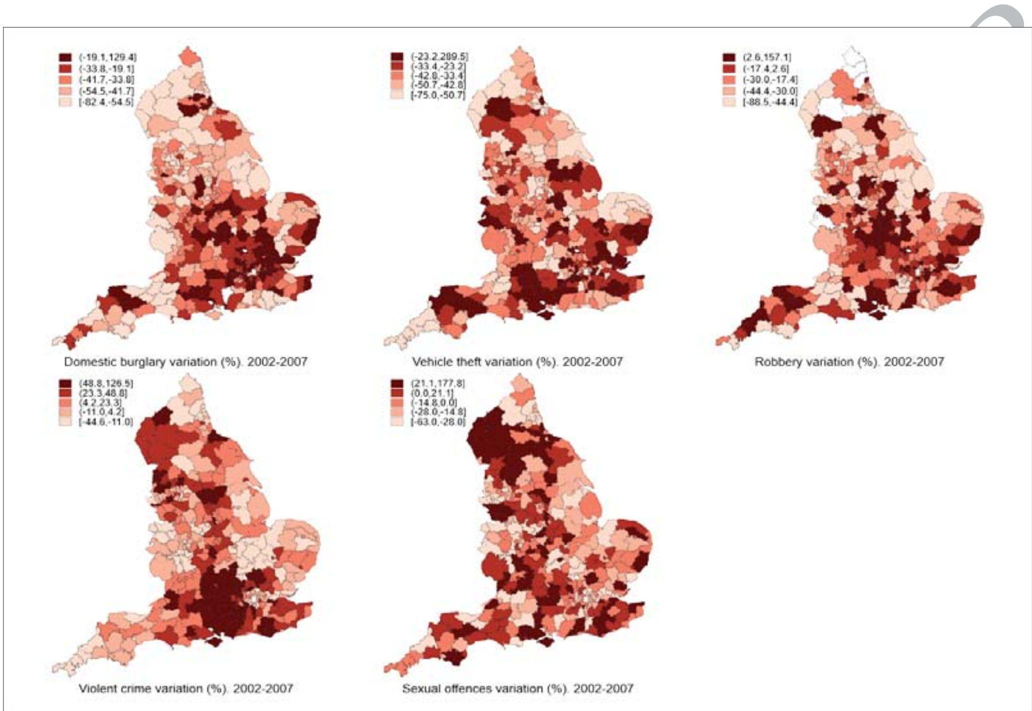

Figure 2. Crime variation in England (2002-2007). This figure shows the spatial distribution of crime rate variation (in percentage points) in English local areas for the analysed crime measures during the treatment period.

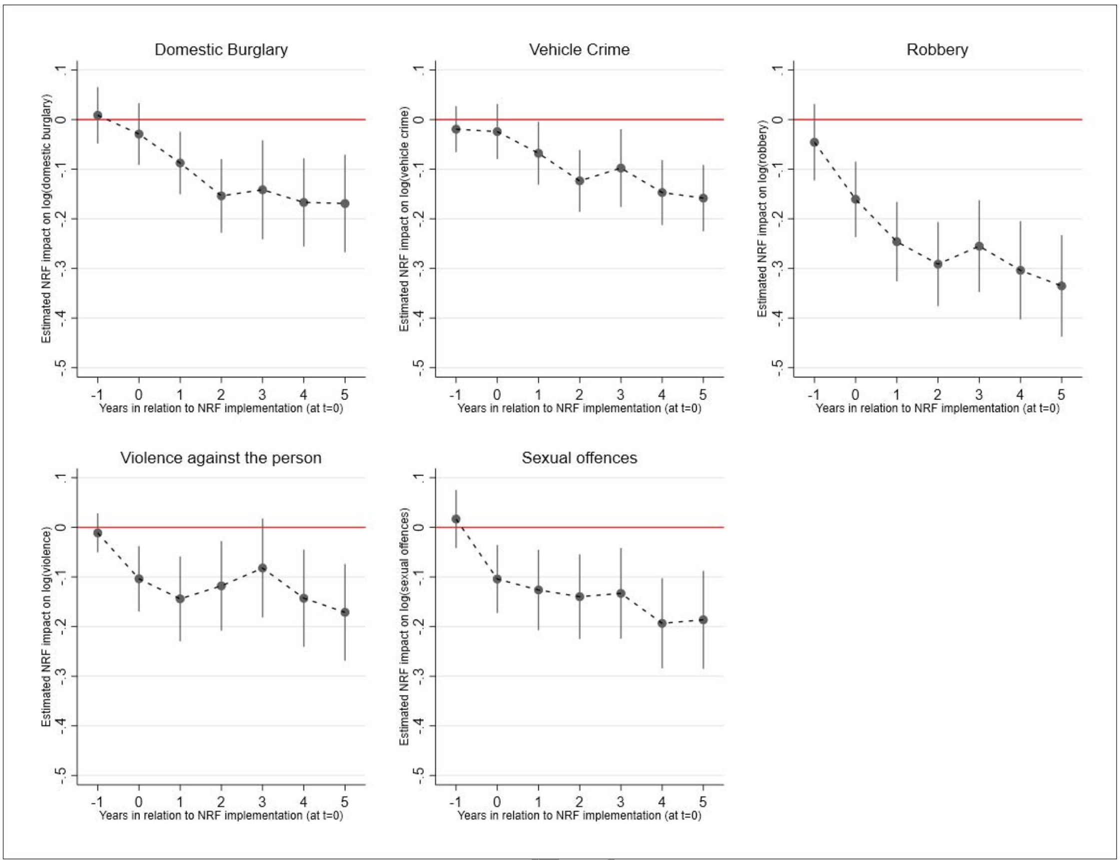

Figure 3. Lead and lag effects of the NRF on crime. Black dots represent point estimates and vertical bars show robust 95% CIs of the NRF impact on crime outcomes, estimated at the year of the effective implementation (year 2002; represented as t=0 in the figure), and for one year before and five years after the policy was implemented. Reference year in our models is year 2000 (t=-2).

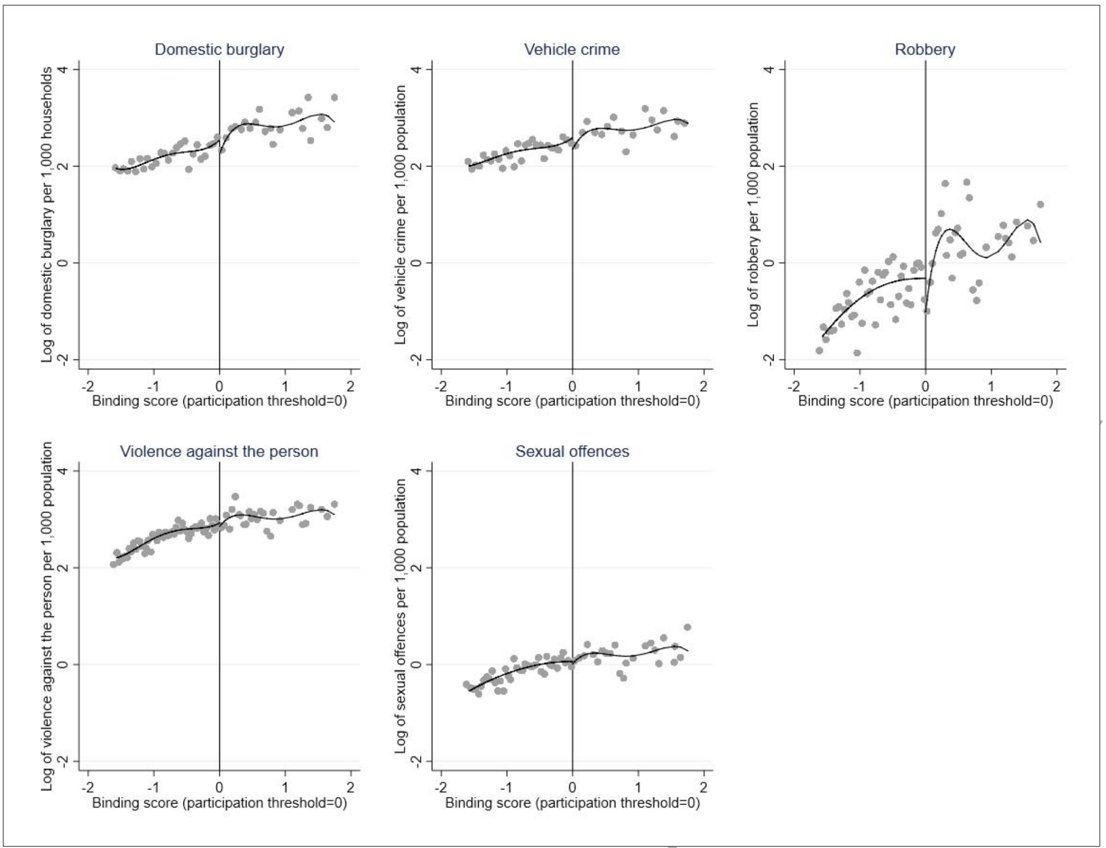

Figure 4. Estimated impact of the NRF on crime rates; This figure shows standard RD plots for our five crime measures over the treatment period (2002-2007). Observations are averaged within bins using the mimicking variance evenly-spaced method described in Calonico et al. (2015). Each plot includes 4th order global polynomial fits represented by the solid lines. Plots are constructed using the software developed by Calonico et al., (2017).

APPENDIX A: Sensitivity of RD estimates to different cut-off definitions

The eligibility rule for participation in the NRF program is based on the deprivation level of local areas, measured using the six summary indices of the IMD2000. Deprivation is, therefore, assumed to be multidimensional. Such multidimensionality poses the fundamental question of how to integrate the six deprivation indices in a single binding score, which can provide a reliable multidimensional measure of deprivation. As described in the main text, eligible local areas are within the 50 most deprived areas for any of the indices incorporated in the IMD2000. Therefore, the UK government is implicitly imposing the so-called *union* approach (Duclos, Sanh & Younger, 2006) to measure multidimensional deprivation, i.e. a local area is considered multidimensionally deprived whenever it is deprived on at least one of the deprivation indices.

To satisfy such a criterion, baseline results are computed using a measure of multidimensional deprivation constructed as the maximum absolute distance between the standardised indices and their standardised cut-off, given by the value of the 50th most deprived area for each of the indices (see Eq. 5). Although this approach has been used for similar purposes in previous studies (see, e.g., Reardon & Robinson, 2012), there are no theoretical reasons for preferring it over other suitable functional forms. In particular, the maximum of the relative difference between the cut off and the standardized value of the indices can be used instead of the absolute one:

$$X_i(\mathrm{z}_i) = \max_k \left\{ \frac{z_{ik}}{z_{(50)k}} \right\}, \qquad \text{(A.1)}$$

where z_{ik} is the standardised value of the deprivation index k in local area i and $z_{(50)k}$ is the cut-off of index k. It should be noted that under this specification, the condition for participation in the NRF program holds for $X_i \geq 1$.

As regards the standardisation procedure, the baseline results are reported for the conventional method of subtracting the mean and dividing by the standard deviation (Eq. 4). To investigate the robustness of our results to other standardization procedures, we consider an alternative method that yields individual indicators for each index ranging from zero (for the least deprived local area) to one (for the most deprived local area).

$$z_{ik} = \frac{w_{ik} - min_k}{max_k - min_k}, \tag{A.2}$$

where w_{ik} is the deprivation level of local area i for index k, while min_k is the minimum of the index of deprivation k and max_k is its maximum.

We combine different standardisation procedures and functional forms to analyse the effect of different binding scores on the impact of the NRF. Hence, we report RD estimates using different approaches to construct the running variable (Tables A.1 and A.2). We also report results without applying any standardisation to the separate indices of the IMD2000 and using Eq. (A.1) to construct the index of multidimensional deprivation (Table A.3). The use of this ratio yields dimensionless variables, thus making standardisation unnecessary. The results of the sensitivity analysis reported in Tables A.1, A.2 and A.3 confirm the positive effect of the NRF on property crimes. The effect size is slightly different depending on the structure of the deprivation measure, but its direction is robust to the different approaches used to construct the running variable. Although for violent crime the RD coefficients were not significant for the baseline results, the sensitivity analysis performed in this section points towards a positive impact of the NRF program in terms of violent crime prevention.

[Table A.1 about here]

[Table A.2 about here]

One of the main limitations of the so-called "*union* approach" is that the overall level of multidimensional deprivation is assessed using only the variable which presents the highest level of deprivation. To illustrate the potentially misleading conclusions that can be reached under this approach, let us assume that two local areas are equally deprived in terms of unemployment, one of them is not deprived on the other dimensions at all, whereas the other is deprived on all dimensions, but less severely than in terms of unemployment. Under all of the specifications used so far, we would classify both areas as equally deprived, even when one exhibits arguably higher levels of deprivation than the other.

Alternative strategies to the "*union* approach" have been adopted to determine eligibility for neighbourhood renewal funding in other countries. For example, the National Anti-Poverty Strategy in Ireland only considers a local area to be multidimensionally deprived if it is deprived across all dimensions (Layte, Nolan & Whelan, 2000) – the "*intersection* approach". In our case, however, this approach would not identify program eligibility adequately. For this reason, we opt for an "*intermediate* approach" in which the joint distribution of all indices will be considered before identifying which local areas are deprived (Duclos & Tiberti, 2016). To evaluate the level of deprivation, we use the overall index proposed by Bourguignon & Chakravarty (2003) given by:

$$X_i(z_i;\alpha,\varepsilon)=\left[\sum_{k=1}^{K}\gamma_k(g_{ik})^{\varepsilon}\right]^{\frac{\alpha}{\varepsilon}}, \tag{A.3}$$

where g_{ik} are the individual indices of deprivation, constructed using Eq. (A.1) so that $g_{ik} = z_{ik}/z_{(50)k}$. The overall index is defined only for non-negative values of g_{ik}. Hence, the individual indices of deprivation (w_i) are standardised using Eq. (A.2), thus ensuring that the non-negative condition holds. γ_k stands for the weight of index k, which, for simplicity, is assumed to be the same for all dimensions. The parameter ε determines the elasticity of substitution between different indices of deprivation (g_{ik}). For $\varepsilon = 1$, the indices are considered perfect substitutes, which means that deprivation on one index (say $g_{i,unemployment} = 1.1$) can be compensated by the relative absence of deprivation on another (say $g_{i,income} = 0.9$). As ε tends to infinity, the different indices become perfect complements, so Eq. (A.3) tends to

$$X_i(z_i;\alpha) = max\{g_{ik}\}^{\alpha}. \tag{A.4}$$

The α parameter captures the degree of multidimensional deprivation, so that the larger the value, the higher the weight given to multidimensionally deprived areas. If we set α equal to one in Eq. (A.4), we get the particular case given by the *union* approach used to compute the results presented Table A.2.

To analyse the sensitivity of the RD estimates to different degrees of substitutability between the indices of deprivation, we re-estimate Eq. (6) using Eq. (A.3) to construct the running variable for different values of the ε parameter and setting $\alpha = 1$ in order to facilitate the comparison with previous results. Figure A.1 presents the RD coefficients for both dimensions of crime, property and violent crime, and robust 95% confidence intervals for the non-parametric local polynomial model, using a triangular kernel and the optimal bandwidth selection that minimises the mean square error. As explained above, these estimates should converge, by definition, to the RD coefficients presented in Table A.2 (represented in Figure A.1 by the dashed lines) because, when ε tends to infinity, the functional form used to construct the running variable of the RD estimates presented in this figure (Eq. A.3) tends to the expression of the running variable used to obtain the results presented in Table A.2. Finally, it is worth noting that, in spite of the decreasing trend observed for the point estimates, the effect of the program does not differ depending on the ε parameter because the confidence intervals are wide enough to not reject the hypothesis of a constant effect.

[Figure A.1 about here]

Table A.1. Local linear non-parametric RD estimates. Running variable constructed by using Eq. (A.1) and the standardisation formula given in Eq. (4).

	A. Property crime					
	Domestic burglary		Vehicle crime		Robbery	
	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}
RD coefficient	-0.4249	-0.4238	-0.2730	-0.2651	-0.7306	-0.7058
Robust 95% CI	[-0.677, -0.2569]	[-0.6856, -0.2009]	[-0.4727, -0.1397]	[-0.4698, -0.0941]	[-1.0369, -0.5233]	[-1.0283, -0.4316]
N_{-} / N_{+}	138	102	154	126	150	114
H	90	72	96	78	90	72
	B. Violent crime					
	Violence		Sexual offences			
	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}		
RD coefficient	-0.1242	-0.1214	-0.1145	-0.1580		
Robust 95% CI	[-0.2313, -0.0338]	[-0.2315, -0.02]	[-0.2676, -0.0094]	[-0.3155, -0.0276]		
N_{-} / N_{+}	156	126	168	132		
H	96	78	96	84		

Notes: RD estimates computed using local linear methods with triangular kernel function. Bias-corrected robust estimators of standard errors developed by Calonico et al. (2014). Optimal bandwidth selection relies on two different procedures: mean square error optimal bandwidth selector (h_{MSE}); coverage error rate optimal bandwidth (h_{CER}) (see Calonico et al. 2015).

Table A.2. Local linear non-parametric RD estimates. Running variable constructed by using Eq. (A.1) and the standardisation formula given in Eq. (A.2).

	A. Property crime					
	Domestic burglary		Vehicle crime		Robbery	
	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}
RD coefficient	-0.3158	-0.3313	-0.2962	-0.2710	-0.8615	-0.5251
Robust 95% CI	[-0.5139, -0.1919]	[-0.5346, -0.1697]	[-0.4617, -0.1942]	[-0.4376, -0.1405]	[-1.0388, -0.591]	[-0.7743, -0.2365]
N_{-} / N_{+}	437	275	436	280	282	174
H	96	90	96	90	90	72
	B. Violent crime					
	Violence		Sexual offences			
	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}		
RD coefficient	-0.1143	-0.0760	-0.1669	-0.1881		
Robust 95% CI	[-0.2063, -0.0584]	[-0.1662, -0.0031]	[-0.3007, -0.0857]	[-0.3220, -0.0843]		
N_{-} / N_{+}	354	198	432	258		
H	96	84	96	90		

Notes: RD estimates computed using local linear methods with triangular kernel function. Bias-corrected robust estimators of standard errors developed by Calonico et al. (2014). Optimal bandwidth selection relies on two different procedures: mean square error optimal bandwidth selector (h_{MSE}); coverage error rate optimal bandwidth (h_{CER}) (see Calonico et al. 2015).

Table A.3. Local linear non-parametric RD estimates. Running variable constructed by using Eq. (A.1) and non-standardised variables.

	A. Property crime					
	Domestic burglary		Vehicle crime		Robbery	
	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}
RD coefficient	-0.2620	-0.3042	-0.2505	-0.2889	-0.9058	-0.8382
Robust 95% CI	[-0.4399, -0.1545]	[-0.4858, -0.1566]	[-0.3985, -0.1633]	[-0.4365, -0.1699]	[-1.1203, -0.7331]	[-1.0592, -0.6414]
N_{-} / N_{+}	597	425	615	424	476	330
H	108	96	108	96	96	90

	B. Violent crime			
	Violence		Sexual offences	
	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}
RD coefficient	-0.1072	-0.0881	-0.1291	-0.1863
Robust 95% CI	[-0.1948, -0.0517]	[-0.1729, -0.0197]	[-0.2514, -0.0508]	[-0.3072, -0.0874]
N_{-} / N_{+}	426	270	582	384
H	96	84	102	90

Notes: RD estimates computed using local linear methods with triangular kernel function. Bias-corrected robust estimators of standard errors developed by Calonico et al. (2014). Optimal bandwidth selection relies on two different procedures: mean square error optimal bandwidth selector (h_{MSE}); coverage error rate optimal bandwidth (h_{CER}) (see Calonico et al. 2015).

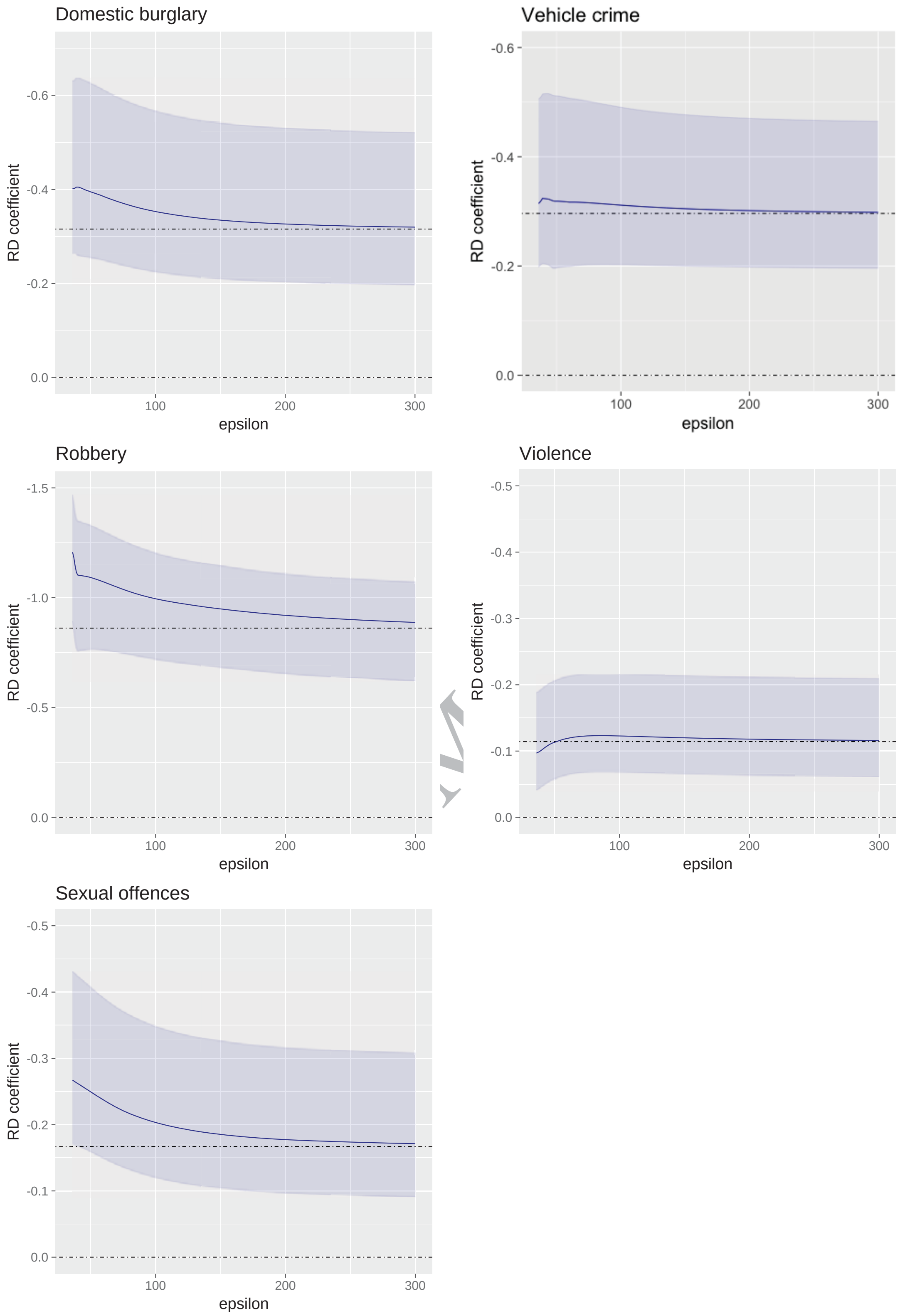

Figure A1. RD estimates for alternative definitions of the running variable.

APPENDIX B: Additional robustness checks

Table B.1. Quadratic RD estimates.

	A. Property crime					
	Domestic burglary		Vehicle crime		Robbery	
	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}
RD coefficient	-0.4389	-0.4651	-0.2818	-0.2796	-0.8491	-0.6455
Robust 95% CI	[-0.6707, -0.2723]	[-0.7194, -0.2234]	[-0.4571, -0.1530]	[-0.4678, -0.1036]	[-1.1531, -0.6287]	[-0.9959, -0.3111]
N_{-} / N_{+}	401/186	215/114	502/222	304/138	312/138	174/102
H	0.4576	0.2959	0.5555	0.3591	0.3609	0.2334
	B. Violent crime					
	Violence		Sexual offences			
	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}		
RD coefficient	-0.1639	-0.047	-0.1518	-0.1335		
Robust 95% CI	[-0.2744, -0.0861]	[-0.1610, 0.0603]	[-0.3010, -0.0401]	[-0.3004, 0.0260]		
N_{-} / N_{+}	366/168	192/114	402/174	216/114		
H	0.4142	0.2677	0.4492	0.2904		

Notes: RD estimates computed using quadratic polynomial methods with triangular kernel function. Bias-corrected robust estimators of standard errors developed by Calonico et al. (2014). Optimal bandwidth selection relies on two different procedures: mean square error optimal bandwidth selector (h_{MSE}); coverage error rate optimal bandwidth (h_{CER}) (see Calonico et al. 2015).

Table B.2. Quadratic RD estimates with covariates included.

	A. Property crime					
	Domestic burglary		Vehicle crime		Robbery	
	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}
RD coefficient	-0.3699	-0.4237	-0.2955	-0.2831	-0.7191	-0.6862
Robust 95% CI	[-0.5292, -0.2150]	[-0.6031, -0.2462]	[-0.4540, -0.1356]	[-0.4672, -0.0986]	[-0.9651, -0.4669]	[-0.9835, -0.3778]
N_{-} / N_{+}	538/240	329/162	394/186	214/114	318/162	192/114
H	0.6219	0.4022	0.4566	0.2953	0.3868	0.2503
	B. Violent crime					
	Violence		Sexual offences			
	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}		
RD coefficient	-0.1332	-0.1036	-0.2215	-0.2834		
Robust 95% CI	[-0.2278, -0.0436]	[-0.2105, 0.0046]	[-0.3768, -0.0742]	[-0.4616, -0.0993]		
N_{-} / N_{+}	414/192	222/120	306/156	186/114		
H	0.4660	0.3013	0.3776	0.2441		

Notes: RD estimates computed using quadratic polynomial methods with triangular kernel function. Bias-corrected robust estimators of standard errors developed by Calonico et al. (2014). Optimal bandwidth selection relies on two different procedures: mean square error optimal bandwidth selector (h_{MSE}); coverage error rate optimal bandwidth (h_{CER}) (see Calonico et al. 2015).

Table B.3. Local linear RD estimates; Epanechnikov kernel function.

	A. Property crime					
	Domestic burglary		Vehicle crime		Robbery	
	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}
RD coefficient	-0.4384	-0.4238	-0.2301	-0.2774	-0.6190	-0.5813
Robust 95% CI	[-0.6940, -0.2690]	[-0.6880, -0.1947]	[-0.4332, -0.1118]	[-0.4858, -0.0918]	[-0.9249, -0.4258]	[-0.8979, -0.3013]
N_{-} / N_{+}	144/84	90/66	148/102	120/66	144/90	102/66
H	0.1786	0.122	0.2176	0.1486	0.1848	0.1262

	B. Violent crime			
	Violence		Sexual offences	
	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}
RD coefficient	-0.0838	-0.1165	-0.0622	-0.0672
Robust 95% CI	[-0.2023, -0.0019]	[-0.2395, -0.0134]	[-0.1708, 0.0056]	[-0.1816, 0.0275]
N_{-} / N_{+}	132/78	78/60	312/156	192/114
H	0.1590	0.1085	0.3792	0.2589

Notes: RD estimates computed using local linear methods with Epanechnikov kernel function. Bias-corrected robust estimators of standard errors developed by Calonico et al. (2014). Optimal bandwidth selection relies on two different procedures: mean square error optimal bandwidth selector (h_{MSE}); coverage error rate optimal bandwidth (h_{CER}) (see Calonico et al. 2015).

Table B.4. Local linear RD estimates; Epanechnikov kernel function with covariates included.

	A. Property crime					
	Domestic burglary		Vehicle crime		Robbery	
	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}
RD coefficient	-0.3537	-0.4771	-0.2819	-0.3327	-0.6175	-0.6507
Robust 95% CI	[-0.5164, -0.2155]	[-0.6458, -0.3012]	[-0.4204, -0.1728]	[-0.4737, -0.1996]	[-0.7525, -0.4772]	[-0.8196, -0.4770]
N_{-} / N_{+}	215/114	144/90	220/132	154/102	263/138	168/102
H	0.2876	0.1964	0.3205	0.2189	0.3409	0.2329

	B. Violent crime			
	Violence		Sexual offences	
	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}
RD coefficient	-0.1411	-0.1526	-0.2355	-0.1996
Robust 95% CI	[-0.2160, -0.0774]	[-0.2322, -0.0770]	[-0.3363, -0.1339]	[-0.3072, -0.0799]
N_{-} / N_{+}	318/162	192/114	306/156	192/114
H	0.3821	0.2609	0.3719	0.2539

Notes: RD estimates computed using local linear methods with Epanechnikov kernel function. Bias-corrected robust estimators of standard errors developed by Calonico et al. (2014). Optimal bandwidth selection relies on two different procedures: mean square error optimal bandwidth selector (h_{MSE}); coverage error rate optimal bandwidth (h_{CER}) (see Calonico et al. 2015).

Table B.5. Local linear RD estimates; uniform kernel function.

A. Property crime						
	Domestic burglary		Vehicle crime		Robbery	
	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}
RD coefficient	-0.4348	-0.4431	-0.2600	-0.2655	-0.8197	-0.7355
Robust 95% CI	[-0.6839, -0.2844]	[-0.7024, -0.2171]	[-0.4665, -0.1394]	[-0.4734, -0.0922]	[-1.1140, -0.6086]	[-1.0590, -0.4574]
N_{-} / N_{+}	138/78	84/60	144/84	96/66	114/66	60/48
H	0.1647	0.1124	0.1826	0.1246	0.1297	0.0886

B. Violent crime				
	Violence		Sexual offences	
	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}
RD coefficient	-0.1448	-0.0879	-0.0647	0.0194
Robust 95% CI	[-0.2703, -0.0633]	[-0.2160, 0.0209]	[-0.1776, 0.0017]	[-0.1002, 0.1147]
N_{-} / N_{+}	102/66	60/42	222/126	150/102
H	0.1267	0.0865	0.305	0.2082

Notes: RD estimates computed using local linear methods with uniform kernel function. Bias-corrected robust estimators of standard errors developed by Calonico et al. (2014). Optimal bandwidth selection relies on two different procedures: mean square error optimal bandwidth selector (h_{MSE}); coverage error rate optimal bandwidth (h_{CER}) (see Calonico et al. 2015).

Table B.6. Local linear RD estimates; uniform kernel function with covariates included.

A. Property crime						
	Domestic burglary		Vehicle crime		Robbery	
	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}
RD coefficient	-0.3591	-0.7090	-0.2457	-0.3873	-0.6115	-0.6372
Robust 95% CI	[-0.5251, -0.2021]	[-0.8765, -0.5079]	[-0.3580, -0.1217]	[-0.5189, -0.2440]	[-0.7359, -0.4937]	[-0.7625, -0.4710]
N_{-} / N_{+}	161/102	132/72	190/114	144/84	306/156	192/114
H	0.2305	0.1574	0.2533	0.1730	0.3674	0.2510

B. Violent crime				
	Violence		Sexual offences	
	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}
RD coefficient	-0.1339	-0.0981	-0.2624	-0.2204
Robust 95% CI	[-0.2086, -0.0564]	[-0.1816, -0.0118]	[-0.3849, -0.1712]	[-0.3439, -0.0978]
N_{-} / N_{+}	216/120	150/96	192/114	144/84
H	0.2993	0.2043	0.2647	0.1808

Notes: RD estimates computed using local linear methods with uniform kernel function. Bias-corrected robust estimators of standard errors developed by Calonico et al. (2014). Optimal bandwidth selection relies on two different procedures: mean square error optimal bandwidth selector (h_{MSE}); coverage error rate optimal bandwidth (h_{CER}) (see Calonico et al. 2015).

APPENDIX C: RD estimates excluding local areas where any neighbourhood was selected for participation in the NDC.

Table C.1. Local polynomial RD estimates.

	A. Property crime					
	Domestic burglary		Vehicle crime		Robbery	
	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}
RD coefficient	-0.4635	-0.4253	-0.2718	-0.2727	-0.7395	-0.6543
Robust 95% CI	[-0.7337, -0.2848]	[-0.6993, -0.1909]	[-0.4834, -0.1463]	[-0.4863, -0.0877]	[-1.0160, -0.3827]	[-1.0008, -0.2786]
N_- / N_+	138/84	102/60	160/96	132/72	114/60	72/54
H	0.1840	0.1263	0.2324	0.1595	0.1439	0.0988
	B. Violent crime					
	Violence		Sexual offences			
	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}		
RD coefficient	-0.1013	-0.1156	-0.1397	-0.2114		
Robust 95% CI	[-0.2227, -0.0210]	[-0.2387, -0.0116]	[-0.3144, -0.0331]	[-0.3874, -0.0613]		
N_- / N_+	138/78	90/54	138/84	114/60		
H	0.1716	0.1177	0.1967	0.1350		

Notes: RD estimates computed using local linear methods with triangular kernel function. Bias-corrected robust estimators of standard errors developed by Calonico et al. (2014). Optimal bandwidth selection relies on two different procedures: mean square error optimal bandwidth selector (h_{MSE}); coverage error rate optimal bandwidth (h_{CER}) (see Calonico et al. 2015).

Table C.2. Local polynomial RD estimates with covariates included.

	A. Property crime					
	Domestic burglary		Vehicle crime		Robbery	
	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}
RD coefficient	-0.3674	-0.4332	-0.3115	-0.3228	-0.5939	-0.6121
Robust 95% CI	[-0.5316, -0.2202]	[-0.5993, -0.2537]	[-0.4381, -0.1947]	[-0.4550, -0.1858]	[-0.7608, -0.4242]	[-0.7963, -0.4076]
N_- / N_+	215/126	149/96	298/132	184/108	275/126	180/102
H	0.3189	0.2190	0.3650	0.2506	0.3502	0.2406
	B. Violent crime					
	Violence		Sexual offences			
	h_{MSE}	h_{CER}	h_{MSE}	h_{CER}		
RD coefficient	-0.1525	-0.1407	-0.2201	-0.2311		
Robust 95% CI	[-0.2265, -0.0894]	[-0.2209, -0.0626]	[-0.3323, -0.0670]	[-0.3642, -0.0609]		
N_- / N_+	306/132	186/108	186/108	138/78		
H	0.3807	0.2613	0.2615	0.1795		

Notes: RD estimates computed using local linear methods with triangular kernel function. Bias-corrected robust estimators of standard errors developed by Calonico et al. (2014). Optimal bandwidth selection relies on two different procedures: mean square error optimal bandwidth selector (h_{MSE}); coverage error rate optimal bandwidth (h_{CER}) (see Calonico et al. 2015).