

# The Local Economic Impacts of Regeneration Projects: Evidence from UK's Single Regeneration Budget

Stephen Gibbons (LSE and SERC)  
Henry Overman (LSE and SERC)  
Matti Sarvimäki (Aalto University and VATT)

**August 2017**

This work is part of the research programme of the Urban Research Programme of the Centre for Economic Performance funded by a grant from the Economic and Social Research Council (ESRC). The views expressed are those of the authors and do not represent the views of the ESRC.

© S. Gibbons, H. Overman and M. Sarvimäki, submitted 2017.

# **The Local Economic Impacts of Regeneration Projects: Evidence from UK's Single Regeneration Budget**

**Stephen Gibbons\***  
**Henry Overman\***  
**Matti Sarvimäki\*\***

**August 2017**

\* Department of Geography and Environment, London School of Economics and SERC/CEP

\*\* Department of Economics and Grantham, Aalto University and VATT

This work was supported by the Economic and Social Research Council grant numbers ES/J021342/1 and ES/G005966/1. It contains statistical data from ONS which is Crown copyright and reproduced with the permission of the controller of HMSO and Queen's Printer for Scotland. The use of the ONS statistical data in this work does not imply the endorsement of the ONS in relation to the interpretation or analysis of the statistical data. This work uses research datasets which may not exactly reproduce National Statistics aggregates.

**Abstract**

We study the local economic impacts of a major regeneration programme aimed at enhancing the quality of life of local people in deprived neighbourhoods in the UK. The analysis is based on a panel of firm and area level data available at small spatial scales. Our identification strategies involve: a) exploiting the fine spatial scale of our data to study how effects vary with distance to the intervention area; and b) comparing places close to treatment in early rounds of the programme with places close to treatment in future rounds. We consider the long run impact of schemes funded between 1995 and 1997 on outcomes up to 2009. Our estimates suggest that the programme increased workplace employment in the intervention area but this had no impact on the employment rates of local residents.

Keywords: single regeneration budget; regeneration; employment; neighbourhoods; urban policy

JEL Classifications: R11; J08; H50

## 1. Introduction

Many governments spend large amounts of money trying to improve economic outcomes in deprived neighbourhoods. Despite their popularity, the economic (and broader) impacts of such programmes are uncertain.<sup>1</sup> This uncertainty persists even though these programmes have been the subject of extensive, and often expensive, evaluations by governments (OECD, 2004). Part of the problem reflects a general weakness in government sponsored evaluations (National Audit Office, 2013). However, it is increasingly recognised that, in part, this uncertainty arises because of methodological challenges: It is often hard to assess the causal impact of policy interventions that are not randomly assigned, especially if evaluation has not been embedded in to policy design.

A further complication arises with spatial initiatives because they are often targeted at many different objectives and involve multiple partners and funding streams. This can mean that data on the location, scale and focus of interventions is often poor, compounding the methodological problems in assessing causal impacts. Furthermore, if one does identify impacts in the location targeted by an intervention, it is important to know whether these effects occur because of the displacement of activity from other areas further away from the scheme. For instance, when evaluating policies attempting to increase local employment, an important question is whether the programme created jobs that would not have existed *anywhere* in some broader area (e.g. the larger neighbourhood) in the absence of the programme? Finally, area based interventions also raise questions about the ‘people versus place’ effects of policies that are usually not an issue for policy interventions aimed at individuals. Specifically, we are often interested in whether policy benefits the local population living close to the scheme. This impact may not be well captured by changes to area level statistics if the latter are driven by the changing composition of the population in areas close to the scheme. These three issues – the causal impact of the scheme, the extent to which

---

<sup>1</sup> For a recent review of evaluation evidence see What Works Centre for Local Economic Growth (2015).

any effects are the result of displacement, and the individual versus the area effects of policy – will be our main focus in this paper.

We address the challenges of evaluating area based policies by focusing on a programme of interventions – the Single Regeneration Budget (SRB) - aimed at enhancing the quality of life of local people in deprived neighbourhoods in the UK. Similar to many other comparable programmes, administrative data on the allocation of funding of SRB is very incomplete and not publicly available. We address this problem by identifying the subset of interventions that involved the building of subsidized business floor space and gathering information on these through an extensive data collection effort. We are able to identify areas targeted by this type of intervention at a relatively fine spatial scale for 165 projects funded between 1994 and 2002 with a total expenditure of £8.2bn. Of this total, £1.5bn is funded by central government through the SRB with the remainder coming from local government, other government bodies, the EU and the private sector.

Our results suggest that the programme increased workplace employment in targeted areas, but had no impact on the employment rates of local residents. We reach this conclusion with the help of remarkably detailed data and several complementary identification strategies. Our data come from the GB Population Census and an administrative register of businesses (the Business Structure Database), which allow us to consider the impacts on a variety of outcomes at a very fine spatial scale. Our first empirical strategy is to simply compare changes in the number of jobs and the employment rates in locations close to an SRB site to observationally identical locations elsewhere. We then compare locations close to an SRB project to locations further away from the same SRB project. Finally, we examine the effect on employment rates by comparing areas close to SRB projects to similarly defined control areas, close to locations that only receive SRB funding in later periods (due to data limitations, we are not able to use this strategy for workplace employment). All of these approaches lead to similar conclusions. Together, they also allow us to assess both the impact on targeted areas as well as possible spill-over effects to the larger neighbourhood.

Our work adds to the small, but growing literature that takes identification issues seriously when evaluating the impact of spatial interventions. Earlier contributions, mostly focusing on US Enterprise or Empowerment Zones (EZ), had often recognised the need for valid controls but had been less convincing in their identification strategies. See, for example, Dabney (1991), Papke (1993, 1994), Boarnet and Bogart (1996), Bondonio and Engberg (2000), Peters and Fisher (2002), O’Keefe (2004), Bondonio and Greenbaum (2007) and reviews by Bartik (1991), Nolan and Wong (2004). Several institutional features of US EZs – specifically the fact that interventions are spatially bounded (i.e restricted to certain areas) and involve a limited number of well documented interventions – have allowed researchers to more effectively deal with the problem of non-random placement. Busso and Kline (2008), and Busso, Gregory and Kline (2013) made significant progress in terms of identification, by using rejected and future EZs as a control group. Neumark and Kolko (2010), Ham et al (2011) and Hanson and Rohlin (2013) developed complementary strategies that used nearby treated areas as controls. A series of papers – Gobillon et al (2012) Givord et al (2013), Mayer et al (2016) – used combinations of these strategies to study the effects of the French Zone Franche Urbaines (ZFU).<sup>2</sup> Gibbons (2015) and Einiö and Overman (2016) – building on methods developed during early stages of the current paper – used more finely spatially detailed data to further develop identification strategies based on comparisons to nearby untreated areas. Our paper contributes to the development of these spatial-differencing strategies, as well as using the timing of SRB projects, to improve identification.

In contrast to this literature, research on the impact of UK government regeneration schemes has paid little attention to issues of identification.<sup>3</sup> The government funded evaluation of the Single Regeneration Budget (SRB) – the programme that is the focus of this paper – assessed ‘additionality’ through “interviews with project managers and beneficiaries that allow relevant counterfactuals, deadweight, displacement and

---

<sup>2</sup> What Works Centre for Local Economic Growth (2015) provides a systematic review of evaluations of Enterprise Zones and related Area Based Initiatives.

<sup>3</sup> Criscuolo, Martin, Overman and VanReenen (2012) use changes in the map of assisted areas to achieve identification on the effect of regional selective assistance. But, as the name suggests, this policy is not specifically targeted at small neighbourhoods.

leakage to be established” (Rhodes, Tyler and Brennan, 2007, Annex A1 p.292). Most economists would view this as a bold claim for research based on 20 case study areas and generating 65 ‘additionality coefficients’. We are unaware of any subsequent research on the impact of SRB which improves on this research design.

The remainder of this paper is organised as following. Section 2 describes the Single Regeneration Budget, which funded the interventions that we evaluate. Sections 3 and 4 introduce our data and present descriptive statistics. Sections 5 and 6 discuss our empirical strategies and results. The final section concludes.

## **2. The Single Regeneration Budget**

From 1994 to 2002, the Single Regeneration Budget (SRB) was the UK government’s main regeneration fund intended to enhance the quality of life of local people in deprived areas.<sup>4</sup> It was launched in November 1993 and replaced 20 existing programmes. The fact that these existing programmes had different objectives was reflected in the variety of objectives to which SRB was expected to contribute. Specifically, projects had to meet at least one of seven strategic objectives: enhancing employment prospects and skills; encouraging sustainable economic growth; improving housing; benefiting ethnic minorities; tackling crime and safety; protecting and improving the environment; and enhancing the quality of life (Rhodes, Tyler, Brennan, 2007).

Funding was allocated in rounds. The first round of bidding opened in April 1994 with results announced in December 1994 and the funding starting in March 1995. The sixth and final round was announced in January 2000. During the six rounds between 1994 and 2000, the SRB distributed £5.7 billion to 1028 projects. However, it has been estimated that the total expenditure for SRB related projects was £26 billion (Rhodes, Tyler and Brennan 2007 Executive Summary p.ii), with additional funding coming

---

<sup>4</sup> Unless otherwise stated, figures in this section are taken from Rhodes et al (2007).



from Local Authorities, Training and Enterprise Councils (Learning and Skills Councils), the voluntary and private sectors and the European Union.

SRB funds were allocated on a competitive basis. Bids were invited from partnerships which could comprise Local Authorities, Training and Enterprise Councils, private companies, Chambers of Commerce, educational institutions and voluntary organisations (John and Ward, 2005). Hall (2000, p. 4) describes the process as follows: "Each GOR [Government Office of the Region] was issued with an indicative SRB Challenge Fund allocation. Its task was to compile a package of bids to be recommended to central government. Local partnerships were to submit outline bids which would be formally 'encouraged' or 'discouraged' by the GOR. They would then decide, on the basis of this guidance, whether the probability of success merited the submission of a (perhaps amended) formal bid. The GOR would then select which bids would be recommended to central government for funding."

Unfortunately, relatively little information is available on how GORs and Ministers assessed bids. GORs acted in line with recommendations from central government. Bidding Guidance (e.g. Department of Environment, 1994) did contain assessment criteria but these mainly concerned the ability to deliver final outputs and to attract matched funding from sources other than the SRB. It is unclear that these criteria could be used to differentiate between bids that had made it through the GOR screening of bids. What we do know is that even once bids made it through GOR screening, rejection rates were reasonably high. For example, Ward (1997, citing Hall, 1996) reports that only 201 out of 469 final bids were funded in round 1, while 172 out of 329 bids were funded in round 2.

The available guidance and documentation do not resolve all uncertainties about the selection process. However it appears that, despite the strategic objectives of SRB, the underlying economic performance of the area played a relatively minor role in the selection process once a bid was submitted. John, Ward and Dowding (2004) use data

on all submitted bids<sup>5</sup> to examine the likelihood that a bid was successful as a function of the ‘packaging’ of the bid (e.g. whether it included a map), the political characteristics of the location (e.g. whether it was in the constituency of a government minister) and measures of deprivation of the location. They report that “time and money spent on the preparation of bids, rather than the content in terms of the government’s objectives, helps determine success – the triumph of packaging over substance.” (John, et al, 2004, p. 425) Political manipulation also appears to have played a minor role in decisions.

In short, we know that SRB projects target areas that were deprived (roughly a third of the funding was targeted at the 20 most deprived Local Authority districts and 80% at the 99 most deprived). But given the complex decision making process, and the evidence in John et al. that success had relatively little to do with the local economic or political situation, we think it is reasonable to assume that the timing of treatment is independent of area characteristics. This assumption, which we test by comparing observable characteristics of different areas, underpins our strategy of using future SRB intervention areas as suitable controls, as discussed further below.

SRB had no predetermined spatial scale, involved various interventions and targeted numerous objectives. Given that we have data available at a fine spatial scale, our strategy is to focus on one particular set of projects – those that involve the provision or repair of business floor space – and the impact of these projects on a small range of outcomes. Focusing on these projects allows us to precisely locate the project, despite the absence of administrative data on SRB projects. During the six rounds of SRB, 187 projects (18% of the total) include improving or building business floor space amounting to a total expenditure of £8.2bn (SRB share £1.5bn).<sup>6</sup>

---

<sup>5</sup> Sadly, in private correspondence, the authors of this paper confirmed that this data is no longer available.

<sup>6</sup> For the round 1 to 3 projects, we were able to obtain a detailed breakdown of the reported physical outputs for these schemes as including: 4,730,650 metre squared business floor space built/improved, 58,746 dwellings built/improved, 764 hectares of land improved, 401 buildings built/improved, 3km of road built and 8 community facilities built.

These projects also involved other social interventions, to improve local residents' labour market or educational outcomes for example. Overall, our estimates measure the joint effects of both the built environment and social interventions. Two things distinguish our research from much of the available literature focusing on the US Enterprise or Empowerment Zones and French Zone Franche Urbaines. First, most of the SRB interventions were intended to regenerate relatively small local areas.<sup>7</sup> In comparison, many EZs and ZFUs are quite large. Second, while most EZs and ZFUs provide direct financial support to businesses, SRB expenditure involved only indirect support to businesses via improvements to the built environment or through benefits arising from the associated social interventions. The effectiveness of built environment interventions, in particular, has been questioned by the UK government in its review of regeneration funding (Communities and Local Government, 2009) and our findings provide estimates to help inform that debate.

### 3. Data

The SRB dataset that we use is constructed from a variety of sources. First, using project summary documents from the government department in charge of regeneration (Communities and Local Government, or CLG) we identified 187 schemes which included building or improving commercial floor space. In the second stage, we located these 187 schemes using the project summary information provided by CLG and the Regional Development Agencies (which took over responsibility for SRB when they were established in 1999). We also consulted post-scheme evaluations provided by Local Authorities and RDAs, and we used websites of specific schemes where available. The process involved an extensive search for documents held by a variety of organisations and several Freedom of Information requests. Where we succeeded in finding the evaluation document for a particular scheme, we took from it the specific locations (longitude and latitude) which had been the target of physical improvement

---

<sup>7</sup> Among all successful bids, 45% of the projects sought to regenerate a small local area (consisting of a small number of wards, wards being geographical units with an average of around 5000 residents), 20% worked at the level of local authority and the rest at a larger spatial level. But our focus on projects with a significant built environment component means that a much higher percentage of our projects will have targeted small local areas.

works. In this manner we successfully located, to varying degrees of accuracy, 165 schemes which included business floor space improvements. For the remaining 22 projects, we were not able find sufficiently accurate information of their location.

We have data on a number of outcomes. Data on employment of those living in the neighbourhood and demographic characteristics comes from the 1991 and 2001 Censuses. Workplace employment in the neighbourhood is taken from the Business Structure Database (BSD) which provides an annual snapshot of the Inter-Departmental Business Register (IDBR). This dataset contains information on 2.1 million businesses, accounting for approximately 99% of economic activity in the UK and includes each business' name, postcode and total employment.

Our control variables include resident characteristics<sup>8</sup> and population density (from Census 91) and share of land area that is urban. We have also used these data sources to construct control variables measuring the characteristics of the larger neighbourhood in which our unit of observation are located. For each unit of observation (based on 'enumeration districts' – see below), these neighbourhood variables are calculated as the average of census variables in the enumeration districts located within 0.5km, 0.5-1km and 1-5km 'bands'.

As discussed above, our aim is to study the impact of SRB projects at a disaggregated spatial scale. Unfortunately, while all our data sources report data at very fine spatial scales, the reporting units differ between sources. To construct data for a consistent set of spatial units we use the 1991 census enumeration district (ED) as our unit of observation. These EDs were designed to facilitate data census collection and attempted to equalize enumerators' workload.<sup>9</sup> The number of residents in EDs range

---

<sup>8</sup> Shares of residents in different labour market status, age group, education, industry, non-white, foreign born, lone parent, living in owner occupied housing, living in social housing, crowded housing and means of transport.

<sup>9</sup> The design of the 1991 EDs included such factors as density of housing; the number of sub-divided properties (bedsits); flats in which individual front doors are protected by entry-phone systems; and residents who may not have English as their first language. Furthermore, EDs were designed not to straddle major roads, rivers, railway lines or extensive areas of open space. (Martin, 2001)

between 24 and 1797 with an average of 433 inhabitants. In comparison, the US census tracts typically have between 2,500 and 8,000 residents (Census Bureau, 1994).

The BSD and OS Strategi data are available at a very fine spatial level and can easily be aggregated to ED-level.<sup>10</sup> The 2001 census data is reported at Output Area (OA) level. The OAs are smaller than EDs – with the average population of 297 – but their borders are typically not contained within ED borders. We convert the 2001 census data into EDs using weighting based on the overlapping area of the two geographies.<sup>11</sup>

## 4. Descriptive Statistics

We have information on project location and the SRB round in which the project is funded. As we discuss in detail below, we base our identification strategy on either project location or timing (or both). With this in mind, we present descriptive statistics disaggregating by distance to the project and timing of the project in Tables 1 and 2, respectively.

Table 1 present descriptive statistics for: (a) workplace employment; (b) number of residents; (c) the employment rate of residents; (d) other characteristics of residents in 1991; and (e) location characteristics. These descriptive statistics are presented for 1km ‘bands’ of EDs located within 5km of the project location and for the rest of England (RoE). From the table, a clear pattern emerges where EDs close to SRB sites are home to people who are disadvantaged in comparison to the rest of England, in pretty much all dimensions recorded in the census. In particular, in 1991 before the start of the SRB,

---

<sup>10</sup> BSD is available at postcode level. OS Strategi is a geometrically structured 1:250 000 scale vector database that defines the real world geographic entities (objects) as point and line features. Each feature consists of geometric and attribute data. Coordinate resolution is 1 metre.

<sup>11</sup> For example, consider an OA that has a population of 100, and shares 90% of its area with ED 1 and 10% of its area with ED 2. In this case, we attribute 90 inhabitants to ED 1 and 10 inhabitants to ED 2. We repeat this procedure for each OA and aggregate the resulting data to ED level. That is, each ED may ‘receive’ inhabitants from multiple OAs, which we then sum together to construct our final dataset. We use similar approach to approximate, say, the number of employed residents and calculate the ED level employment rate by dividing the approximated number of employed residents by the approximated working-age population.

those living close to what will become SRB sites tend to have lower employment rates than those living further away.

Table 2 presents averages for the same set of variables for EDs with 1km of SRB projects, broken down by the rounds in which the project was funded. It shows some variation across rounds – particularly in terms of workplace employment in the EDs within 1km of SRB sites – although no systematic pattern emerges. Consistent with this, the number of residents, the employment rate of residents and other demographic characteristics are broadly constant across rounds. Given our discussion in section 3 about the process for decision making, we view these variations as a random outcome rather than systematic and assume that interventions in later rounds are not targeted at areas that are systematically any different from areas targeted in earlier rounds.

The differences and similarities documented in Tables 1 and 2 motivate the identification strategies discussed in the following two sections. The key challenge in evaluating the impact of any policy intervention is the construction of a plausible control group that allows us to assess what would have happened in the absence of intervention. The way in which we achieve this varies by outcome of interest and is conditioned by the time-span of data available (1991 and 2001 for employment rate data; annually from 1997-2009 for employment) and the relationship of this to the timing of the different SRB rounds.

## **5. Effect on workplace employment**

We start with the effect on workplace employment given that all the schemes we consider have a substantive component of commercial development designed to increase workplace employment in the treated area (and it was this development that we used to geo-locate the SRB project). We have workplace employment data for 1997-

2009. Areas close to SRB projects in rounds 1 to 3 (1995/6 to 1997/8) have already begun to receive treatment by 1997 so we have no pre-treatment employment data for rounds 1 to 3, given the timing of the rounds. Thus we have to focus attention on rounds 4 to 6 in order to consider changes over time.

Our aim is to estimate whether the change in workplace employment ( $\Delta y_{it}$ ) in enumeration district  $i$  between 1997 and time  $t$  is affected by SRB policy ‘treatment’. We start with regressions that define an enumeration district (ED) to be “treated” if it is within a given distance of a round 4 to 6 SRB project. More precisely, we define treatment using indicator variables  $D_i^K$  that take the value 1 if there is a round 4 to 6 SRB site within distance  $K$  of enumeration district  $i$ , and zero otherwise. Using these distance bands, we estimate regressions:

$$\Delta y_{it} = \mu + \beta^K D_i^K + x'_{i0} \gamma + \varepsilon_{it} \quad (1)$$

where  $\Delta y_{it}$  and  $D_i^K$  are as defined above,  $x_{i0}$  are observable factors specific to ED  $i$  in the pre-policy period that may affect changes in employment over time, and  $\varepsilon_{it}$  is an error term capturing the impact of unobservable factors that vary over time and place. Since the spatial scale of the potential treatment effect is not known *a priori*, we report estimates using different distance bands to define whether an ED is ‘close’ to a SRB site.<sup>12</sup> We start by considering the longest possible time difference (to 2009) but then use shorter time windows to see whether the effects differ across time. In our preferred specifications, the vector  $x_{i0}$  also controls for nearest SRB site-specific constants (SRB site fixed effects). The estimation sample is restricted to the subset of observations for which the dependent and observable variables are available in all years 2003 to 2009.<sup>13</sup>

---

<sup>12</sup> This feature sets SRB apart from programs such as US Enterprise/Empowerment Zones, in which the interventions are targeted at improving outcomes for discretely bounded areas. In contrast, SRB interventions were designed to benefit loosely defined areas ‘close to the scheme’.

<sup>13</sup> This sample restriction facilitates comparisons across specifications and time periods. Results available on request show that findings are robust to dropping this sample restriction.

As usual, the identification challenge arises because unobservable factors that affect employment may be correlated with SRB treatment, not least because policy deliberately targeted SRB sites to economically disadvantaged areas. The fact that we examine changes in employment helps deal with time invariant unobservable factors that may affect both the level of employment and treatment. Consistent estimation of the treatment effects thus requires that ED-specific unobservables which affect *changes* in employment over time ( $\varepsilon_{it}$ ) are independent of SRB treatment status (i.e. a ‘parallel trends’ assumption), at least conditional on the set of included control variables (a ‘Conditional Independence Assumption’ or CIA).

Table 3 presents the coefficients and standard errors when estimating equation (1) for long differences from 1997 to 2009. The standard errors are clustered by nearest SRB site across all rounds. The first row reports results when including no additional control variables. The point estimate suggests that EDs close to SRB sites added 17 jobs per ED more than EDs elsewhere in England. Note, however, that the estimate is not statistically significant.

We next add nearest SRB fixed effects to control for time invariant unobservables that are common to neighbouring EDs (second row). Controlling for geographical location in this way leads to a point estimate of 22 jobs per ED, and makes the association between employment growth and proximity to an SRB site statistically significant at 10% level. Adding a full set of residential characteristics of the ED in 1991 (third row) further increases the point estimate to 27 jobs per ED and makes the estimates significant at 5% level. Finally, the estimates are not affected by controlling for residential characteristics of neighbouring EDs in 1991 (fourth row).<sup>14</sup>

---

<sup>14</sup> Results available on request show that findings are robust to using fixed effects based on Local Authority, rather than nearest SRB. Results are also robust to clustering by LA for EDs that are more than 5km from the nearest SRB.



The estimates reported in the bottom two rows of Table 3 suggest that areas within 1km of SRB sites experienced faster employment growth than comparable locations elsewhere in England. In the remaining columns, we report estimates using wider distance bands. The estimates become gradually smaller as we loosen the definition of being “close” to an SRB site. This pattern of results suggests that employment growth mainly occurs within 1km of where the subsidized business floor space was built: As we move from <1km to <2km the number of EDs roughly doubles, and the effect halves consistent with positive employment effects at <1km now being averaged across more EDs.

Comparison of the estimates across the columns of Table 3 suggests that part of the increase employment in the “treated” EDs (<1km) may be due to displacement of jobs from locations further away in the larger neighbourhood. Given the number of EDs in each of the distance rings (see Table 1), we would expect the coefficient in the <2km, <3km, <4km and <5km bands to be, respectively, around one-half, one-third, one-quarter and one-fifth of that in the 0-1km band if the employment effects are positive within 1km and zero elsewhere (relative to the >5km control group). This is indeed what we see up to 3km in Table 3, but not for the final two columns suggesting that some displacement may be occurring from places further than 3km from the SRB site.

Table 4 shows the pattern of results over time for specifications including nearest SRB fixed effects, residential characteristics of the ED in 1991 and residential characteristics of neighbouring EDs in 1991. These specifications are comparable to those in the fourth row of Table 3 (indeed, the final row simply replicates the results for 1997 to 2009). The table shows that positive employment effects can be detected around 3 years after round 6 is completed (i.e. 2005) and they grow somewhat over time.<sup>15</sup> As in the final row of Table 3, the coefficients are only ever significant in the closest distance bands.

---

<sup>15</sup> Results not reported here show that the statistical significant of findings in early years are more reliant on the introduction of controls - fixed effects at a minimum, but some years need all controls for significance.

The time profile of estimated employment effects does raise the concern that the results in Table 3 may underestimate the effects of rounds 4 to 6 if EDs close to rounds 1 to 3 appear in the controls. Results in Tables 4A and 4B of the appendix suggest that these concerns are largely unwarranted. To produce the results in Table 4A we drop any observations that are within  $k$  km of a round 1 to 3 project (with  $k$  varying from 1 to 5 km as we move across the columns). Table 4B takes the more conservative approach of dropping all observations within 5km of a round 1 to 3 project. As is clear from both tables, we still find a positive significant effect of round 4 to 6 on employment from around 2005 onwards.

An alternative approach for examining the impact of the SRB is to exploit the spatial detail in our data and to directly compare EDs close to an SRB scheme to EDs somewhat further away from the same scheme. This approach builds on the insight that the largest workplace employment effects should occur at (or near to) the commercial development that is located at the ‘centre’ of the scheme.<sup>16</sup> As noted above, the results reported in Table 3 and 4 are in line with this assumption.

As in Gibbons (2015) and Einiö and Overman (2016), we implement this idea by using EDs that are within 5km of a round 4 to 6 SRB site to estimate:

$$\Delta y_{it} = \mu + \sum_K \beta^K D_i^K + x'_{i0} \gamma + \varepsilon_{it} \quad (2)$$

where  $\Delta y_{it}$  is defined as above, and  $D_i^K$  are a series of indicator variables taking value one if the ED is within  $k$  to  $k-1$  kilometres of an SRB site, zero otherwise, and all other variables are defined as before. We use  $D_i^5$  as the omitted category. Thus, the parameters  $\beta^K$  measure the change in employment for EDs located  $k$  to  $k-1$  kilometres from an SRB site in comparison to EDs 4 to 5 kilometres of an SRB site (the omitted category). As before, in our preferred specifications the vector  $x_{i0}$  controls for nearest

---

<sup>16</sup> To be precise this is the centre of the scheme given the way in which we have geo-located projects. It is possible that other SRB activities are not necessarily centred on the commercial development site introducing some measurement error for the employment rate regressions as we discuss further below.

SRB site-specific constants (SRB site fixed effects) and we restrict the sample to the subset of observations for which the dependent and observable variables are available in all years 2003 to 2009.<sup>17</sup> The restriction to EDs within 5km of a round 4 to 6 site helps control for time varying shocks that are common across all areas close to SRB round 4 to 6 sites.

Table 5 presents the coefficients and standard errors when estimating equation (2) where the sample is restricted to EDs within 5km of an SRB site, and the coefficients estimate the impact on the 1997-2009 change in employment in each distance ring relative to the 4-5km. The standard errors are robust to clustering by ring and SRB site.<sup>18</sup> The first column reports results when including no additional control variables. EDs close to round 4 to 6 SRB sites experienced larger changes in employment than EDs 4-5km from 4 to 6 SRB sites, although the difference is not statistically significant. The remaining three columns sequentially add fixed effects for the nearest SRB project (column 2), residential characteristics of the ED in 1991 (third row) and residential characteristics of neighbouring EDs in 1991 (fourth row). The resulting pattern is very similar to that reported in Table 3, using the alternative specification of equation (1): the estimates become larger and statistically significant as we add control variables to the specification.

Table 6 shows the pattern of results over time for specifications including nearest SRB fixed effects, residential characteristics of the ED in 1991 and residential characteristics of neighbouring EDs in 1991. These specifications are comparable to those in the fourth column of Table 5 (again, the final column replicates the results for 1997 to 2009). The table shows that for equation (2) positive employment effects can be

---

<sup>17</sup> This sample restriction facilitates comparisons across specifications and time periods. Results available on request show that findings are robust to dropping this sample restriction.

<sup>18</sup> That is, we have one cluster for EDs within 1km of SRB round 4 to 6 project A, one cluster for EDs within 2km of SRB round 4 to 6 project A, ..., one cluster for EDs within 1km of round 4 to 6 SRB project B, etc.

detected earlier than for equation (1) – specifically in the year after round 4 is completed (i.e. 2003) and they again grow somewhat over time.<sup>19</sup>

As with equation (1), the time profile of estimated employment effects raises the concern that the results in Table 3 may underestimate the effects of rounds 4 to 6 if EDs close to rounds 1 to 3 appear in the controls. To check for this, we drop any ED that is within 5km of a round 1 to 3 SRB site, as these will have already been treated at least once by 1997. This gives us a set of ED that are within 5km of a round 4 to 6 SRB project, but more than 5km from a round 1 to 3. Results reported in Table 6A in the appendix suggest that, if anything, including these EDs causes us to slightly over-estimate, rather than under-estimate the effects of treatment.

Overall, these results suggest that employment increased at SRB project sites but there are no statistically significant impacts beyond 1km.<sup>20</sup> In line with the results reported in Table 3, the coefficients in Table 5 suggest that the positive effects within 1km of the site do not come at the expense of areas immediately nearby: The signs on the coefficients in the 1-2 and 2-3 km band are positive, although insignificant. If there is displacement, it is from areas more than 3km away from the SRB site, where the sign turns negative. Either way, SRB generates ‘additional-to-the-area’ employment close to SRB sites. The question remains as to whether these employment increases benefited the policy target group, that is the people living nearby. To answer this question we now turn to whether the SRB commercial space projects and their associated active labour market measures lead to higher employment rates for local residents.

## **6. Effect on residence-based employment rates**

There are two reasons why we might see an effect on employment rates for residents living close to SRB projects. First, because there are local employment effects as

---

<sup>19</sup> Results available on request report no positive effect for 2002.

<sup>20</sup> Although, formally, we cannot reject equality of the coefficients. For example a t-test for the equality of the coefficients on kilometre 0-1km and 1-2km has a p-value of 0.33.

documented in the previous section. Second, because we know that SRB projects involve other activities that are specifically aimed at improving employment rates for local residents. If those additional local jobs go to local residents, or if the other support measures are effective, then local employment rates should improve.

As for employment, we start by estimating equation (1) which allows for an effect on employment rates if an ED is within K kilometres of an active SRB site. We have employment rate data for 1991 and 2001 and so focus only on the treatment effect of projects funded in rounds 1 to 2. Results when estimating equation (1) using the change in employment rates between 1991 and 2001 as the outcome variable are presented in Table 7. Standard errors are clustered by nearest SRB site, as for equation (1), Table 3.

The structure of the Table is exactly as for Table 3. To reiterate, the first row in each panel presents results when including no additional control variables. Treatment is defined as within K km of SRB project rounds 1 to 2 (with K increasing across columns from K=1, within 1km; to K=5 within 5km). In order to provide more informative comparisons, we gradually add nearest SRB project fixed effects (second row), residential characteristics of the ED in 1991 (third row) and residential characteristics of neighbouring EDs in 1991 (fourth row).

The baseline estimates show that residents living close to an SRB site experience slower growth in their employment rates than those living elsewhere. Given that the SRB projects were targeted at declining areas, this comparison is unlikely to measure the causal impact of the programme. However, once we add SRB fixed effects and pre-treatment residential characteristics we continue to find no significant effect on employment rates.<sup>21</sup> Areas close to SRB sites tended to experience changes in employment rates that were no different to other *comparable* areas. Thus these

---

<sup>21</sup> Results in Table 7A show that this finding is robust to dropping all ED within 5km of a round 3 or 4 project (which may have received some treatment by 2001).

estimates suggest that while the SRB projects appear to have affected local workplace employment, the new jobs seem to have little, if any, effect on local employment rates.

For employment rates, we can achieve more credible identification by following Busso, Gregory and Kline (2013) and using projects in later rounds, yet to be funded, as a control group for the projects treated prior to 2001. Specifically, we compare changes over time for EDs that benefit from SRB-interventions in early rounds 1 and 2 to EDs that will benefit from SRB interventions in later rounds 5 and 6. The idea underlying this approach is that EDs receiving SRB-treatment at a given point in time should be much more comparable to EDs receiving an intervention at some other time, than to EDs that never receive treatment.

We implement this idea by restricting the sample to EDs close to schemes in rounds 1, 2, 5 and 6 and estimating equation (1), but with treatment  $D_i^K$  redefined to be an indicator variable taking the value 1 if there is a round 1 to 2 SRB site within distance  $K$  km of enumeration district  $i$ , and zero for EDs within  $K$  km of a round 5 to 6 project. As before, we allow  $K$  to increase across columns from  $K=1$  (within 1km) to  $K=5$  (within 5km) and restrict the sample to EDs within  $K$  km of a round 1 to 2 or 5 to 6 project. For example, when  $K=1$ , we compare changes in employment rate of residents living in EDs within 1km of round 1 to 2 SRB site the change in employment rate of those living within 1km of round 5 to 6 site. Errors are clustered by nearest SRB site across all rounds.

Table 8 presents results from this comparison of round 1 and 2 treatment with round 5 and 6 controls. The dependent variables is, as before, the change in residence-based employment rates from 1991 to 2001. The first row reports results when including no additional control variables. We progressively add in nearest SRB fixed effects (second row), residential characteristics of the ED in 1991 (third row) and residential characteristics of neighbouring EDs in 1991 (fourth row). Providing that SRB neighbourhoods are defined to be smaller than local labour markets (which seems

likely), estimation of equation (3) should provide us with a reasonable estimate of the effect on employment rates of the increase in employment identified in Tables 3 to 5. Once again, we find no significant (positive) effects on employment rates across all specifications and distance bands.<sup>22</sup>

For completeness, we end with a similar spatial differencing approach to that we used in Table 5 to examine workplace employment. That is, we once again exploit the spatial detail in our data to compare EDs close to an SRB scheme to EDs somewhat further away from the same scheme. This approach will capture the impact of other interventions – e.g. employment training – provided as part of the SRB projects that were targeted at smaller spatial scales than the 5km SRB neighbourhoods that we have constructed. We implement it by estimating equation (2) for employment rates, with timing changed to reflect the availability of data. Specifically, we now use EDs that are within 5km of a round 1 to 2 SRB site.

Table 9 presents the results of this approach, estimating equation (2) for long differences of employment rates from 1991 to 2001 on this restricted sample. The first column reports results when including no additional control variables. The remaining three columns sequentially add fixed effects for the nearest SRB project (column 2), residential characteristics of the ED in 1991 (third row) and residential characteristics of neighbouring EDs in 1991 (fourth row). As with Tables 7 and 8 we find no (positive) significant effects of SRB on employment rates.

---

<sup>22</sup> As before, results in Table 8A show that this finding is robust to dropping all ED within 5km of a round 3 or 4 project (which may have received some treatment by 2001).

## 7. Implied cost per job

In order to interpret the magnitude of our estimates, and to compare them to previous studies, we end with rough cost per job calculations. The estimates reported in Tables 3 and 5 suggest that the local average impact of an SRB project was an increase of around 25 jobs per ED within 1km of a round 4 to 6 project. There were 8,267 ED within 1km of a round 1 to 6 project. Assuming that the scale and pattern of employment effects were similar for round 1 to 3 projects as for round 4 to 6 projects, this suggests a total increase in workplace based employment of 206,675 jobs. With a total cost of £8.2 billion the implied cost per job created is £39,675.

Even ignoring the possibility that these jobs may have been displaced from elsewhere, the implied cost per job is higher than for other labour market interventions in the welfare-to-work field (e.g. Van Reenen, 2004; Black et al, 2003). It is also high relative to other UK area-based policies. For example, Criscuolo et al (2012) estimate a cost per job of £6,885 for UK Regional Selective Assistance. In short, although we cannot say anything about the type and quality of jobs created given the data available, the cost per job figure for SRB seems high.

Turning to the employment of local residents, our point estimates are generally negative and statistically insignificant. An optimistic assessment – based on the impacts within 3km using the upper 95% confidence interval from Table 5 – would imply a positive gain of 4 jobs per 1000 population ( $-0.00195 + 1.96 \times 0.00313$ ). The mean population in a 3km circle of an SRB project is around 200,000 (multiplying the area of the circle by the population density of around 7300, see Table 1). Therefore, this effect implies 8 jobs for local residents within 3km of each SRB site, at a staggering cost of £6.2 million per *local* person employed (£8.2 billion/(165 x 8)). Of course, a more realistic interpretation of our results is that building new business floor space in deprived neighbourhoods had no effect on the employment of local residents.



## 8. Conclusions

Many governments attempt to help people living in deprived neighbourhoods by providing financial incentives for firms to locate into these areas. While such “place-making” policies are often popular among policy makers, economists typically remain sceptical about the cost-efficiency of these initiatives. However, empirical evidence informing this debate remains limited due to the scarcity of data and research designs that would allow for plausible impact evaluations.

In this paper, we study the local economic impacts of major regeneration programmes that aimed to enhance the quality of life of local people in deprived neighbourhoods in the UK. We focus on subset of projects implemented as part of UK’s Single Regeneration Budget (SRB) between 1994 and 2002. During this period, the SRB was the main regeneration fund in the UK and it allocated considerable amount of public funds to local projects. The total expenditure of the 165 projects we examine was £8.2bn.

Using several identification strategies and remarkably detailed data, we find that subsidising the development of commercial space through the SRB created some additional workplace employment in the targeted places (although we can only partially assess to what extent these were displaced from further afield). However, despite the increase of new local jobs, we find no evidence that these jobs went to local people or improved the employment outcomes of local residents. Moreover, we can comfortably rule out the possibility that these projects were a cost-efficient way to improve local employment. Indeed, our results suggest that the cost of creating an additional job for a person living in the target areas was *at least* £6 million! Thus our study provides a striking example of the challenges government face when trying to help the residents of deprived neighbourhoods by “bringing jobs” to them.

## References

Bartik, Timothy. 1991. Who Benefits from State and Local Economic Development Policies? Kalamazoo, Michigan: W.E. Upjohn Institute for Employment Research.

Black, D., J. Smith, M. Berger and B. Noel (2003) "Is the Threat of Reemployment Services More Effective Than the Services Themselves? Evidence from Random Assignment in the UI System" *American Economic Review*, 93(4), 1313-1327.

Boarnet, M., and Bogart, T. (1996): Enterprise Zones and employment: evidence from New Jersey. *Journal of Urban Economics*, 40(2):198-215.

Bondonio, D., and Engberg, J. (2000): Enterprise Zones and local employment: evidence from the states' programs. *Regional Science & Urban Economics*, 30(5), 519-549.

Bondonio, D., and Greenbaum, R. (2007): Do local tax incentives affect economic growth? What mean impacts miss in the analysis of enterprise zone policies. *Regional Science and Urban Economics*, 37, 121-136.

Busso, M, and Kline, P. (2008): Do Local Economic Development Programs Work? Evidence from the Federal Empowerment Zone Program. Cowles Foundation Discussion Paper No. 1638.

Busso, M., J. Gregory & P. Kline (2013). "Assessing the Incidence and Efficiency of a Prominent Place Based Policy," *American Economic Review*, American Economic Association, vol. 103(2), pages 897-947, April.

Census Bureau (1994): Geographic Areas Reference Manual. U.S. Department of Commerce.

Criscuolo, C., Martin, R., Overman, H. G., and Van Reenen, J. (2012). The causal effects of an industrial policy. NBER Discussion Paper No. 17842.

Dabney, D. (1991): Do Enterprise Zone incentives affect business location decisions? *Economic Development Quarterly*, 5, 325-334.

Department of Environment (1994) Bidding Guidance: A Guide to Funding Under the Single Regeneration Budget (London: DoE, 1994)

Einiö, E. and H. G. Overman (2016), The (Displacement) Effects of Spatially Targeted Enterprise Initiatives: Evidence from UK LEGI (February 2016). CEPR Discussion Paper No. DP11112.

Gibbons, S. (2015), Gone with the Wind: Valuing the visual impacts of wind turbines through house prices, *Journal of Environmental Economics and Management*, 72, July, 177-196

Givord, P., Rathelot, R., and Sillard, P. (2013): Place-based tax exemptions and displacement effects: An evaluation of the Zones Franches Urbaines program. *Regional Science and Urban Economics*, 43(1), 151-163.

Gobillon, L., Magnac, T., and Selod, H. (2012): Do unemployed workers benefit from enterprise zones? The French experience. *Journal of Public Economics*, 96(9), 881-892.

Hall, S. (1996) The Single Regeneration Budget: A Review of Challenge Fund Round II Processed Birmingham: School of Public Policy, University of Birmingham.

Hall, S. (2000) The way forward for regeneration?: Lessons from the single regeneration budget challenge fund. *Local Government Studies*, 26 (1). pp. 1-14.

Ham, J. C., Swenson, C., İmrohoroglu, A., and Song, H. (2011): Government programs can improve local labor markets: evidence from state Enterprise Zones, federal

Empowerment Zones and federal Enterprise Community. *Journal of Public Economics*, 95(7-8), 779-797.

Hanson, A., and Rohlin, S. (2013): Do spatially targeted redevelopment programs spillover? *Regional Science and Urban Economics*, 43(1), 86-100.

John, P., H. Ward and K. Dowding (2004) The Bidding Game: Competitive Funding Regimes and the Political Targeting of Urban Programme Schemes. *British Journal of Political Science*, 33: 405-28.

John, P. and H. Ward (2005) How Competitive Is Competitive Bidding? The Case of the Single Regeneration Budget Program, *Journal of Public Administration Research and Theory* 15(1)

Mayer, T., Mayneris, F., and Py, L. (forthcoming): The impact of Urban Enterprise Zones on establishment location decisions and labour market outcomes: evidence from France. *Journal of Economic Geography*.

OECD (2004) Evaluating Local Economic and Employment Development: How to Assess What Works Among Programmes and Policies. <http://www.oecd.org/leed-forum/publications/Evaluating%20Local%20Economic%20and%20Employment%20Development.pdf>

O'Keefe, S. (2004): Job creation in California's enterprise zones: a comparison utilizing a propensity score matching model. *Journal of Urban Economics*, 55, 131-150.

Papke, L. (1993): What do we know about enterprise zones? NBER Working Paper No. 4251.

Papke, L. (1994): Tax policy and urban development: evidence from the Indiana Enterprise Zone program. *Journal of Public Economics*, 54(1), 37-49.

Peters, Alan H., and Peter S. Fisher. 2002. "The Effectiveness of State Enterprise Zones." *Employment Research* 9 (4): 1–3.

Rhodes, J., Tyler, P. and Brennan, A. (2007): *The Single Regeneration Budget: Final Evaluation*. Department of Land Economy, Cambridge University.

Martin. D. (2001): *Geography for the 2001 Census in England and Wales*. London, Office for National Statistics.

Neumark, D. and Kolko, J. (2010): *Do Enterprise Zones Create Jobs? Evidence from California's Enterprise Zone Program*. *Journal of Urban Economics*, 68 1-19.

National Audit Office (2013). *Evaluation in Government*  
[https://www.nao.org.uk/wp-content/uploads/2013/12/10331-001-Evaluation-in-government\\_NEW.pdf](https://www.nao.org.uk/wp-content/uploads/2013/12/10331-001-Evaluation-in-government_NEW.pdf)

Van Reenen, J. (2004) "Active Labour Market Policies and the British New Deal for Youth in Context" in R. Blundell, D. Card, and R. Freeman (eds) *Seeking a Premier Economy*, Chicago: Chicago University Press

Ward, K. (1997) *The single regeneration budget and the issue of local flexibility*. *Regional Studies*, Vol. 31, No. 1, 02.1997, p. 78-81

What Works Centre for Local Economic Growth (2016) *Area Based Initiatives Evidence Review: Enterprise Zones*  
[http://www.whatworksgrowth.org/public/files/Policy\\_Reviews/16-01-04-Area-based-initiatives-EZ.pdf](http://www.whatworksgrowth.org/public/files/Policy_Reviews/16-01-04-Area-based-initiatives-EZ.pdf)

**Table 1: Descriptive Statistics (sample means by distance to a SRB site)**

	Distance from the nearest SRB site					
	<1km	1-2km	2-3km	3-4km	4-5km	RoE
<i>A: Workplace employment</i>						
1997	448	255	182	156	161	184
2009	470	272	201	172	185	210
Change 1997 to 2009	26	21	22	17	26	27
<i>B: Number of residents</i>						
1991	439	454	452	454	471	446
2001	474	478	474	475	495	475
<i>C: Employment rate of residents</i>						
1991	0.50	0.52	0.53	0.54	0.55	0.56
2001	0.54	0.57	0.58	0.59	0.60	0.63
Change 1991 to 2001	0.04	0.05	0.05	0.05	0.05	0.06
<i>D: Other resident characteristics in 1991</i>						
Has a higher degree	0.01	0.01	0.01	0.01	0.01	0.01
Has a degree	0.05	0.07	0.07	0.08	0.07	0.07
Has diploma	0.04	0.05	0.05	0.06	0.06	0.07
Lone parent	0.02	0.02	0.02	0.02	0.01	0.01
Non-white	0.13	0.12	0.12	0.09	0.06	0.03
Foreign born (Commonwealth)	0.06	0.06	0.06	0.05	0.04	0.02
Foreign born (RoW)	0.05	0.06	0.07	0.06	0.04	0.03
Lives in owner occupied housing	0.55	0.59	0.61	0.64	0.71	0.72
Lives in social housing	0.33	0.29	0.28	0.26	0.20	0.18
Does not have a car	0.48	0.43	0.41	0.38	0.32	0.25
Moved from outside of the ward within x years	0.09	0.08	0.08	0.08	0.07	0.08
Population density (per km <sup>2</sup> )	7,079	7,423	7,490	6,616	4,867	3,253
<i>E: Characteristics of the Location</i>						
in London	0.23	0.31	0.36	0.33	0.24	0.04
Urban	0.96	0.96	0.94	0.91	0.85	0.63
Number of EDs	8,267	9,890	9,944	7,931	5,427	61,637

Source: Authors own calculations using BSD, Census 91 Ordnance Survey Strategi land use database. Statistics are presented for 1km 'bands' of EDs located within 5km of the project location and for the rest of England (RoE).

**Table 2: Descriptive Statistics (sample means within 1km of an SRB site, by round)**

	Locations within 1km of SRB site in round					
	1	2	3	4	5	6
<i>A: Workplace employment</i>						
1997	459	755	514	323	305	548
2009	497	741	509	397	324	605
Change 1997 to 2009	44	-12	4	77	21	66
<i>B: Number of residents</i>						
1991	437	364	428	469	468	425
2001	481	446	463	489	489	462
<i>C: Employment rate of residents</i>						
1991	0.51	0.53	0.49	0.48	0.49	0.49
2001	0.57	0.56	0.54	0.52	0.53	0.54
Change 1991 to 2001	0.06	0.03	0.05	0.04	0.04	0.05
<i>D: Other resident characteristics in 1991</i>						
Has a higher degree	0.01	0.02	0.01	0.00	0.00	0.01
Has a degree	0.07	0.1	0.06	0.04	0.03	0.05
Has diploma	0.05	0.05	0.05	0.04	0.04	0.04
Lone parent	0.02	0.02	0.03	0.02	0.02	0.02
Non-white	0.12	0.14	0.14	0.16	0.12	0.1
Foreign born (Commonwealth)	0.07	0.07	0.07	0.08	0.06	0.05
Foreign born (RoW)	0.06	0.13	0.05	0.03	0.03	0.05
Lives in owner occupied housing	0.56	0.44	0.53	0.55	0.59	0.55
Lives in social housing	0.32	0.36	0.35	0.35	0.31	0.32
Does not have a car	0.45	0.5	0.48	0.48	0.47	0.49
Moved from outside of ward within x years	0.10	0.12	0.09	0.07	0.08	0.09
Population density	6,361	10,023	6,848	6,625	6,153	6,876
<i>E: Characteristics of the Location</i>						
in London	0.38	0.54	0.28	0.15	0.06	0.17
Urban	0.98	0.97	0.98	0.93	0.95	0.95
Number of EDs	982	1,054	1,827	1,693	1,384	2,092
Number of SRB projects	27	22	36	23	25	33

Source: Authors own calculations using BSD, Census 91 Ordnance Survey Strategi land use database

**Table 3: Effect of Treatment Rounds 4 to 6 on change in workplace employment 1997 to 2009**

	Bandwidth				
	<1km	<2km	<3km	<4km	<5km
Baseline	17.50 (12.76)	9.223 (7.834)	5.592 (7.873)	0.511 (4.645)	0.353 (3.900)
Controlling for nearest SRB fixed-effects	22.17* (12.34)	12.23 (8.441)	7.671 (10.27)	0.185 (5.034)	0.554 (3.634)
... and 1991 residential characteristics (at ED level)	27.21** (12.30)	17.89** (8.207)	14.25 (10.27)	7.160 (5.157)	7.869** (3.955)
... and 1991 residential characteristics (at neighborhood level)	27.16** (12.45)	14.50* (7.802)	8.567 (9.180)	-0.847 (4.814)	-2.614 (4.013)
Number EDs	96,473	96,473	96,473	96,473	96,473
Adj-R squared	0.01	0.01	0.01	0.01	0.01
Number SRB site f.e.	103	111	121	132	138

Note: \*\*\*, \*\*, \* indicate significance at 1%, 5% and 10% respectively. Dependent variable is change in workplace employment 1997 to 2009. First row reports results from OLS regression for coefficient on dummy variable taking value 1 if the ED is within km of an SRB site and zero otherwise. Each column presents results as  $k$  increases from 1 to 5km. Rows 2 to 4 in each panel add additional controls as described in the text. Standard errors (in parentheses) clustered by nearest SRB. Adjusted R-squared is for final specification (including 1991 residential characteristics).



**Table 4: Effect of Treatment Rounds 4 to 6 on change in workplace employment**

	Bandwidth				
	<1km	<2km	<3km	<4km	<5km
1997-2003	8.079 (9.693)	-4.164 (5.645)	-5.945 (4.946)	-6.812* (3.668)	-0.514 (3.626)
1997-2004	6.620 (8.383)	2.332 (5.562)	0.621 (7.079)	-4.910 (3.263)	-2.697 (3.061)
1997-2005	22.10 (14.18)	7.554 (6.068)	1.936 (7.898)	-1.572 (3.266)	-1.769 (3.130)
1997-2006	24.79* (13.07)	9.489 (6.841)	3.701 (8.650)	-0.793 (3.767)	-1.500 (3.342)
1997-2007	23.67* (12.18)	8.247 (6.931)	3.177 (8.909)	-2.208 (3.945)	-3.138 (3.694)
1997-2008	20.94** (10.38)	9.508 (6.396)	1.732 (6.882)	-4.943 (4.136)	-5.353 (3.797)
1997-2009	27.16** (12.45)	14.50* (7.802)	8.567 (9.180)	-0.847 (4.814)	-2.614 (4.013)
Number EDs	96,476	96,476	96,476	96,476	96,476
Number SRB site f.e.	103	111	121	132	138

Note: \*\*\*, \*\*, \* indicate significance at 1%, 5% and 10% respectively. Dependent variable is change in the workplace employment for years as indicated in column 1. All rows report results from OLS regression for coefficient on dummy variable taking value 1 if the ED is within km of an SRB site and zero otherwise. Each column presents results as  $k$  increases from 1 to 5km. All rows include nearest SRB fixed effects and full set of controls. Standard errors (in parentheses) clustered by nearest SRB. Adjusted R-squared is for final specification (including 1991 residential characteristics).

**Table 5: Effect of Treatment Rounds 4 to 6 on change in workplace employment 1997 to 2009, by distance to SRB project for all ED within 5km of Round 4 to 6 project**

	(1)	(2)	(3)	(4)
0 – 1 km	15.74 (15.73)	19.44 (13.45)	23.23* (12.95)	26.18** (12.98)
1 – 2 km	2.709 (15.19)	5.799 (13.81)	8.017 (13.68)	5.915 (12.15)
2 – 3 km	10.12 (13.98)	12.07 (15.94)	13.37 (15.37)	14.18 (14.05)
3 – 4 km	-11.59 (8.424)	-11.80 (12.47)	-8.886 (12.24)	-5.324 (10.79)
4 – 5 km	.	.	.	.
Number EDs	25866	25866	25866	25866
Adj R-squared	0.000	-0.000	0.009	0.016
Number SRB sites	76	76	76	76
Controlling for				
Nearest SRB FE	no	yes	yes	yes
1991 residential (ED)	no	no	yes	yes
1991 residential (neighbourhood)	no	no	no	yes

Note: Reports results from OLS regression for coefficients on distance band dummy variables as defined in the text. Additional controls are as described in the text. Standard errors (in parentheses) clustered by nearest SRB.

**Table 6: Effect of Treatment Rounds 4 to 6 on change in workplace employment by distance to SRB project for all ED within 5km of Round 4 to 6 project**

	-2003	-2004	-2005	-2006	-2007	-2008	-2009
0 – 1 km	16.87 (11.14)	10.43 (9.343)	26.70* (14.42)	28.22** (13.08)	28.14** (12.10)	22.99** (10.96)	26.18** (12.98)
1 – 2 km	-6.661 (10.09)	1.584 (9.350)	0.723 (10.65)	0.561 (10.71)	-0.211 (11.08)	8.197 (10.23)	5.915 (12.15)
2 – 3 km	-1.094 (11.89)	9.185 (12.26)	1.808 (13.74)	3.554 (14.26)	6.543 (14.95)	6.497 (12.14)	14.18 (14.05)
3 – 4 km	-12.21 (8.976)	-8.972 (10.32)	-2.986 (10.34)	-1.852 (10.56)	-3.285 (11.14)	-4.586 (9.505)	-5.324 (10.79)
4 – 5 km	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)
Number EDs	25866	25866	25866	25866	25866	25866	25866

Note: Reports results from OLS regression for coefficients on distance band dummy variables as defined in the text. Additional controls are as described in the text. Standard errors (in parentheses) clustered by nearest SRB.

**Table 7: Effect of Treatment Rounds 1 and 2 on change in employment rate 1991 to 2001**

	<1km	<2km	Bandwidth		
			<3km	<4km	<5km
Baseline	-0.013** (0.00629)	-0.010*** (0.00378)	-0.0077*** (0.00294)	-0.0083*** (0.00258)	-0.0077*** (0.00246)
Controlling for nearest SRB fixed-effects	-0.0137** (0.00629)	-0.0105*** (0.00378)	-0.0077*** (0.00294)	-0.0083*** (0.00258)	-0.0077*** (0.00246)
... and 1991 residential characteristics (at ED level)	-0.0104 (0.00693)	-0.00730* (0.00438)	-0.00412 (0.00369)	-0.00338 (0.00318)	-0.00182 (0.00274)
... and 1991 residential characteristics (at neighborhood level)	-0.00783 (0.00607)	-0.00500 (0.00379)	-0.00195 (0.00313)	-0.00085 (0.00298)	0.00112 (0.00273)
Number EDs	101,570	101,570	101,570	101,570	101,570
Adj-R squared					
Number SRB sites	63	63	63	63	63

Note: Dependent variable is change in workplace employment 1997 to 2009. First row reports results from OLS regression for coefficient on dummy variable taking value 1 if the ED is within km of an SRB site and zero otherwise. Each column presents results as  $k$  increases from 1 to 5km. Rows 2 to 4 in each panel add additional controls as described in the text. Standard errors (in parentheses) clustered by nearest SRB.

**Table 8: Effect of Treatment Rounds 1 and 2 on change in employment rate 1991 to 2001, relative to rounds 5 to 6**

	Bandwidth				
	<1km	<2km	<3km	<4km	<5km
Baseline	-0.00097 (0.00816)	0.0030 (0.0056)	0.0060 (0.0047)	0.0038 (0.0043)	0.0024 (0.0042)
Controlling for nearest SRB fixed-effects	-0.014 (0.0099)	-0.0024 (0.0054)	0.0011 (0.0047)	-0.0022 (0.0040)	-0.0059 (0.0042)
... and 1991 residential characteristics (at ED level)	-0.0150 (0.0096)	0.00296 (0.0059)	0.00049 (0.0062)	-0.0031 (0.0050)	-0.0027 (0.0039)
... and 1991 residential characteristics (at neighborhood level)	-0.016** (0.0073)	-0.0016 (0.0063)	- (0.0050)	-0.0030 (0.0045)	-0.0017 (0.0034)
Number EDs	5212	12210	19911	26854	32192
Adj-R squared	0.382	0.360	0.365	0.372	0.380
Number SRB sites	63	63	63	63	63

Note: Dependent variable is change in workplace employment 1997 to 2009. First row reports results from OLS regression for coefficient on dummy variable taking value 1 if the ED is within km of an SRB site and zero otherwise. Each column presents results as  $k$  increases from 1 to 5km. Rows 2 to 4 in each panel add additional controls as described in the text. Standard errors (in parentheses) clustered by nearest SRB.

**Table 9: Effect of Treatment Rounds 1 to 2 on change in employment rate 1991 to 2001, by distance to SRB project for all ED within 5km of Round 1 to 2 project**

	(1)	(2)	(3)	(4)
0 – 1 km	-0.00654 (0.00756)	-0.00866* (0.00520)	-0.00908* (0.00527)	-0.00684 (0.00441)
1 – 2 km	-0.00437 (0.00503)	-0.00627*** (0.00230)	-0.00635*** (0.00208)	-0.00447** (0.00194)
2 – 3 km	0.00506 (0.00531)	0.000624 (0.00248)	-0.00134 (0.00261)	0.00108 (0.00230)
3 – 4 km	-0.000848 (0.00577)	-0.00300 (0.00286)	-0.00310 (0.00282)	-0.00159 (0.00236)
4 – 5 km	.	.	.	.
Number EDs	17574	17574	17574	17574
Adj R-squared	0.001	0.067	0.313	0.345
Number SRB sites	63	63	63	63
Controlling for				
Nearest SRB FE	no	yes	yes	yes
1991 residential (ED)	no	no	yes	yes
1991 residential (neighbourhood)	no	no	no	yes

Note: Reports results from OLS regression for coefficients on distance band dummy variables as defined in the text. Additional controls are as described in the text. Standard errors (in parentheses) clustered by nearest SRB.

Appendix: Not for publication

**Table 4A: Effect of Treatment Rounds 4 to 6 on change in workplace employment (dropping observations within  $k$  km of rounds 1 to 3)**

	Bandwidth				
	<1km	<2km	<3km	<4km	<5km
1997-2003	9.030 (9.947)	4.839 (3.995)	-5.983 (4.349)	1.187 (2.557)	-0.166 (2.818)
1997-2004	6.299 (8.098)	2.442 (4.954)	-4.433 (4.829)	-0.728 (2.549)	-3.796 (2.749)
1997-2005	22.30 (15.08)	9.352 (6.434)	-2.701 (5.859)	1.852 (2.752)	-1.323 (2.903)
1997-2006	24.50* (13.39)	9.411 (7.144)	-1.567 (6.692)	1.863 (3.380)	-1.183 (3.613)
1997-2007	23.80* (12.84)	9.627 (6.862)	-2.861 (5.973)	0.0791 (3.666)	-2.189 (3.901)
1997-2008	19.81* (10.56)	12.62** (5.892)	-0.852 (5.401)	0.374 (3.416)	-2.748 (3.655)
1997-2009	23.13* (12.47)	12.54* (7.224)	2.073 (5.985)	1.884 (4.294)	-1.432 (4.597)
Number EDs	93215	88253	82260	76920	72901

Note: \*\*\*, \*\*, \* indicate significance at 1%, 5% and 10% respectively. Dependent variable is change in workplace employment for years as indicated in column 1. All rows report results from OLS regression for coefficient on dummy variable taking value 1 if the ED is within  $k$  km of an SRB site and zero otherwise. Each column presents results as  $k$  increases from 1 to 5km. All rows include nearest SRB fixed effects and full set of controls. Standard errors (in parentheses) clustered by nearest SRB. Adjusted R-squared is for final specification (including 1991 residential characteristics).

**Table 4B: Effect of Treatment Rounds 4 to 6 on change in workplace employment  
(dropping all observations within 5 km of rounds 1 to 3)**

	Bandwidth				
	<1km	<2km	<3km	<4km	<5km
1997-2003	10.96 (7.429)	5.548 (4.827)	-1.435 (4.230)	-0.419 (2.417)	-0.166 (2.818)
1997-2004	7.940 (9.147)	2.672 (6.498)	-2.806 (4.731)	-2.210 (2.734)	-3.796 (2.749)
1997-2005	19.46 (12.85)	7.933 (7.882)	-1.418 (6.067)	0.690 (2.977)	-1.323 (2.903)
1997-2006	23.58* (13.31)	11.13 (9.159)	0.0680 (7.212)	0.532 (3.596)	-1.183 (3.613)
1997-2007	25.84* (15.40)	11.40 (8.748)	-0.0888 (6.431)	-0.961 (3.866)	-2.189 (3.901)
1997-2008	25.01* (12.67)	13.55* (7.385)	2.569 (5.667)	-0.486 (3.620)	-2.748 (3.655)
1997-2009	29.40* (15.37)	14.35 (9.271)	4.376 (6.598)	-0.203 (4.608)	-1.432 (4.597)
Number EDs	72901	72901	72901	72901	72901

Note: \*\*\*, \*\*, \* indicate significance at 1%, 5% and 10% respectively. Dependent variable is change in workplace employment for years as indicated in column 1. All rows report results from OLS regression for coefficient on dummy variable taking value 1 if the ED is within km of an SRB site and zero otherwise. Each column presents results as  $k$  increases from 1 to 5km. All rows include nearest SRB fixed effects and full set of controls. Standard errors (in parentheses) clustered by nearest SRB. Adjusted R-squared is for final specification (including 1991 residential characteristics).



**Table 6A: Effect of Treatment Rounds 4 to 6 on change workplace employment by distance to SRB project for all ED within 5km of Round 4 to 6 project, excluding ED within 5km of rounds 1 to 3 project**

	-2003	-2004	-2005	-2006	-2007	-2008	-2009
0 – 1 km	10.23 (7.938)	10.45 (9.595)	22.20* (12.35)	23.89** (12.09)	27.60* (14.40)	22.34* (12.24)	27.82* (14.46)
1 – 2 km	10.52* (6.132)	8.909 (6.573)	10.56 (7.557)	13.62* (7.945)	13.50 (8.258)	16.33* (8.383)	13.70 (8.329)
2 – 3 km	-9.616 (7.068)	-6.998 (7.515)	-14.17 (9.342)	-12.34 (10.44)	-10.02 (8.804)	-5.216 (7.580)	0.564 (8.621)
3 – 4 km	4.137 (6.546)	6.054 (6.784)	9.452 (8.749)	7.189 (9.486)	1.693 (8.121)	2.235 (7.839)	-3.085 (8.043)
4 – 5 km	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)	0 (.)
Number EDs	14502	14502	14502	14502	14502	14502	14502

Note: Reports results from OLS regression for coefficients on distance band dummy variables as defined in the text. Additional controls are as described in the text. Standard errors (in parentheses) clustered by nearest SRB.

**Table 7A: Effect of Treatment Rounds 1 and 2 on change in employment rate 1991 to 2001 (dropping all observations within 5 km of rounds 3 or 4)**

	<1km	<2km	Bandwidth		
			<3km	<4km	<5km
Baseline	-0.0148* (0.00778)	-0.0134*** (0.00498)	-0.0105** (0.00411)	-0.0111*** (0.00377)	-0.0105*** (0.00363)
Controlling for nearest SRB fixed-effects	-0.00737** (0.00315)	-0.00737** (0.00315)	-0.00737** (0.00315)	-0.00737** (0.00315)	-0.00737** (0.00315)
... and 1991 residential characteristics (at ED level)	-0.00358 (0.00332)	-0.00358 (0.00332)	-0.00358 (0.00332)	-0.00358 (0.00332)	-0.00358 (0.00332)
... and 1991 residential characteristics (at neighborhood level)	-0.00117 (0.00337)	-0.00117 (0.00337)	-0.00117 (0.00337)	-0.00117 (0.00337)	-0.00117 (0.00337)
Number EDs	78058	78058	78058	78058	78058
Adj-R squared					
Number SRB sites	63	63	63	63	63

Note: Dependent variable is change in workplace employment 1997 to 2009. First row reports results from OLS regression for coefficient on dummy variable taking value 1 if the ED is within km of an SRB site and zero otherwise. Each column presents results as  $k$  increases from 1 to 5km. Rows 2 to 4 in each panel add additional controls as described in the text. Standard errors (in parentheses) clustered by nearest SRB.

**Table 8A: Effect of Treatment Rounds 1 and 2 on change in employment rate 1991 to 2001, relative to rounds 5 to 6 (dropping all observations within 5 km of rounds 3 or 4)**

	Bandwidth				
	<1km	<2km	<3km	<4km	<5km
Baseline	-0.0012 (0.010)	-0.0014 (0.0072)	0.00060 (0.0058)	-0.0024 (0.0054)	-0.0033 (0.0053)
Controlling for nearest SRB fixed-effects	0.00780 (0.0193)	0.00603 (0.00515)	0.0017 (0.0075)	-0.0025 (0.0047)	-0.0061 (0.0047)
... and 1991 residential characteristics (at ED level)	0.026*** (0.0086)	0.00697 (0.0084)	0.00297 (0.0060)	-0.00478 (0.0057)	-0.00544 (0.0060)
... and 1991 residential characteristics (at neighborhood level)	0.0195 (0.0163)	0.00615 (0.0097)	0.00391 (0.0068)	-0.00427 (0.0056)	-0.00535 (0.0054)
Number EDs	3009	6715	10737	14395	17496
Adj-R squared	0.417	0.392	0.391	0.400	0.406
Number SRB sites	63	63	63	63	63

Note: Dependent variable is change in workplace employment 1997 to 2009. First row reports results from OLS regression for coefficient on dummy variable taking value 1 if the ED is within km of an SRB site and zero otherwise. Each column presents results as  $k$  increases from 1 to 5km. Rows 2 to 4 in each panel add additional controls as described in the text. Standard errors (in parentheses) clustered by nearest SRB.



### **Spatial Economics Research Centre (SERC)**

London School of Economics  
Houghton Street  
London WC2A 2AE

**Web:** [www.spatial-economics.ac.uk](http://www.spatial-economics.ac.uk)