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The local economic impacts of regeneration projects: Evidence from UK's single regeneration budget

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ABSTRACT

Many governments aim to improve the labour market outcomes of people living in deprived areas through "place-making" initiatives. Economists are often sceptical about the effectiveness of such policies, but empirical evidence on their impacts remains limited. We examine the impact of building subsidised business floor space in deprived neighbourhoods in the UK. Our estimates suggest that while the £8.2bn investment into these projects increased the number of jobs located in the targeted neighbourhoods, it did little to improve the employment of local residents.

1. Introduction

Many governments spend large amounts of money on area-based initiatives aimed to improve economic outcomes in deprived neighbourhoods. Despite their popularity, the economic (and broader) impacts of such programmes are uncertain both theoretically and empirically. Urban economists have traditionally been sceptical about the potential benefits of such policies (Glaeser and Gottlieb 2008), although recent work presents arguments for their application in the most distressed areas (Austin et al., 2018). The empirical uncertainty persists even though these programmes have been the subject of extensive, and often expensive, evaluations by governments (Nolan and Wong, 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 into policy design.

Academic researchers have developed several identification strategies to help address the problem of identifying the causal effects of area-based initiatives. To date, these methods have primarily been directed at understanding the impact of offering financial incentives to firms to locate in well-defined areas. However, less attention has been paid to widely used policies, where the nature of the treatment and the definition of the target area are less sharply defined. This paper applies state-of-the-art empirical methods to evaluate the long-term effects of such a programme.

We examine the Single Regeneration Budget (SRB) launched in 1994 with the aim of enhancing the quality of life of local people in deprived neighbourhoods in the UK. It offered support through both individual-level investment (e.g. training) and through improvements to the built environment, particularly through investments in subsidised commercial developments and enhancement of physical infrastructure. The rationale was to overcome barriers to small business development in places that had suffered physical decline due, for example, to loss of older traditional industries. The intention was for development and refurbishment of business premises to catalyse further growth in businesses and jobs in the area (see Rhodes et al., 2007, Part 3, Chapter 13 for a more extended discussion). However, in contrast to many other place-based policies, such as US Empowerment Zones, the SRB did not contain direct fiscal incentives to firms or defined sharp boundaries for the targeted areas.

Similar to many other comparable programmes, administrative data on the spatial allocation of funding of SRB is incomplete and not publicly available. We address this problem by identifying the subset of interventions that involved the building of subsidised business floor space and gathering information on these projects through an extensive data collection effort. We are able to identify areas targeted by this type of intervention at a 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

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by central government through the SRB with the remainder coming from local government, other government bodies, the EU and the private sector.

We use a number of alternative approaches to evaluate the causal impact of the scheme, the extent to which any effects are the result of displacement, and the individual versus the area effects of policy. 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 spillover effects to the larger neighbourhood.

This paper adds to the small, but growing literature that takes identification seriously when evaluating the impact of spatial interventions. For a recent review of place-based policies of this type, see Neumark and Simpson (2015). Earlier contributions, mostly focusing on US Enterprise or Empowerment Zones (EZ), had often recognised the need for valid controls but struggled to find appropriate controls. 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 recent papers to more effectively deal with the problem of non-random placement. Busso and Kline (2008), and Busso et al. (2013) made significant progress in terms of identification, by using rejected and future EZs as a control group. Similar strategies have been used in Krupka and Noonan (2009), Hanson (2009), and Reynolds and Rohlin (2015). 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), Mayneris et al. (2017) – used combinations of these strategies to study the effects of the French Zone Franche Urbaines (ZFUs).[1] Gibbons (2015) and Einiö and Overman (2020) – 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 contribution is twofold. First, we evaluate the impacts of the primary place-making policy implemented in the UK in the 1990s and the early 2000s. In contrast to the earlier evaluation of this policy, our work is based on modern empirical approaches and detailed data (partly collected by us).[2] Our results suggest that the £8.2bn investment on the SRB failed to achieve its central objective to improve the employment of people living in the targeted neighbourhoods. We believe this result is important for academics and policymakers contemplating how to best help people living in deprived areas. Second, we combine several empirical strategies to evaluate a place-making policy in a context, where the targeted areas do not have clear boundaries, and the data and policy documentation are incomplete. These features are shared by many place-making policies as well as by other spatial "treatments" such as public transportation stations, road junctions or stadiums. Evaluating such interventions is harder – and the results inevitably less conclusive – than what is the case for policies such as the EZs and the ZFUs discussed above. Nevertheless, we argue that it is important to examine also the harder-to-evaluate policies using the best possible data and appropriate empirical methods. Here, we present an example of how researchers may approach such evaluations.

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.[3] 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 et al., 2007).

An important challenge for evaluating the SRB is that (i) it did not have a predetermined scale and (ii) the content of the funded projects varied widely. Individual project typically involved various interventions, targeted numerous objectives, and were vague about what constituted a targeted "neighbourhood". Given these challenges, and data limitations, we 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. These 187 projects (18% of the total SRB projects) amount to a total expenditure of £8.2bn (of which £1.5bn comes directly from the SRB).[4] These projects also involved other social interventions, e.g., programmes aimed to improve local residents' labour market or educational outcomes. Overall, our estimates measure the joint effects of both the built environment and social interventions.

Two factors 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.[5] In comparison, many EZs and ZFUs are quite large. Second, while most EZs and ZFUs provide

[1] See also What Works Centre for Local Economic Growth (2015) for a systematic review of evaluations of Enterprize Zones and related Area Based Initiatives.

[2] In comparison, the government funded evaluation of the SRB assessed 'additionality' through "interviews with project managers and beneficiaries that allow relevant counterfactuals, deadweight, displacement and leakage to be established" (Rhodes et al., 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.

[3] Unless otherwise stated, figures in this section are taken from Rhodes et al. (2007).

[4] 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, 3 km of road built and 8 community facilities built.

[5] 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

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.

The SRB funding was allocated in six 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 et al., 2007 Executive Summary p.ii), with additional funding coming 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 et al. (2004) use data on all submitted bids[6] 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.

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 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 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 to 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 establishments, 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[7] 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), we calculate averages of census variables in the nearby enumeration districts using three distance bands (within 0.5 km, 0.5–1 km and 1–5 km of the unit of observation).

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 equalise enumerators' workload.[8] The number of residents in EDs range between 24 and 1797 with an average of 433 inhabitants. In comparison, the US census tracts typically have between 2500 and 8000 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.[9] The 2001 census data

significant built environment component means that a much higher percentage of our projects will have targeted small local areas.

[6] Sadly, in private correspondence, the authors of this paper confirmed that this data is no longer available.

[7] 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.

[8] 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)

[9] BSD is available at postcode level. OS Strategi is a geometrically structured 1:250 000 scale vector database that defines the real world geographic enti-

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
A: Workplace employment						
1997	448	255	182	156	161	184
2009	470	272	201	172	185	210
Change 1997 to 2009	26	21	22	17	26	27
B: Number of residents						
1991	439	454	452	454	471	446
2001	474	478	474	475	495	475
C: Employment rate of residents						
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
D: Other resident characteristics in 1991						
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^2)	7079	7423	7490	6616	4867	3253
E: Characteristics of the Location						
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	8267	9890	9944	7931	5427	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 5 km of the project location and for the rest of England (RoE).

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.[10]

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 5 km 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, 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 1 km 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 1 km 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.[11] 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 the outcome of interest and is conditioned by the time-span of data available (1991 and 2001 for employment rate data; annually from 1997 to 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. All schemes we consider were designed to increase local employment by increasing the amount, or improving the quality of commercial space in the treated

ties (objects) as point and line features. Each feature consists of geometric and attribute data. Coordinate resolution is 1 metre.

[10] 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.

[11] The most notable difference is that the raw workplace employment growth is clearly smaller in round 5 than in the other rounds (4 and 6) that received SRB funding between 1997 and 2009. This difference is likely to reflect the smaller share of projects located in London in round 5.

Table 2
Descriptive statistics (sample means within 1 km of an SRB site, by round).

	Locations within 1 km of SRB site in round					
	1	2	3	4	5	6
A: Workplace employment						
1997	459	755	514	323	305	548
2009	497	741	509	397	324	605
Change 1997 to 2009	44	−12	4	77	21	66
B: Number of residents						
1991	437	364	428	469	468	425
2001	481	446	463	489	489	462
C: Employment rate of residents						
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
D: Other resident characteristics in 1991						
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	6361	10,023	6848	6625	6153	6876
E: Characteristics of the Location						
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	1054	1827	1693	1384	2092
Number of SRB projects	27	22	36	23	25	33

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

area (and it was this development that we used to geo-locate the SRB project). That is, they provided a subsidy to a factor of production (built space) on the assumption that it was complementary to labour and so would increase employment, and that physical redevelopment would lead to additional employment gains by attracting more capital to the surrounding area. Thus, a logical first step is to examine whether the additional commercial space led to more jobs being located in the targeted neighbourhoods. 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.

5.1. Baseline estimates

Our aim is to estimate whether the change in workplace employment (Δy_{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 (launched in 1998–2000). More precisely, we define treatment using indicator variables D_i^K that take the value one 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 Δy_{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 ε_{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 an SRB site.[12] 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 1997 and in all years 2003 to 2009.[13]

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 (ε_{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 Eq. (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). Con-

[12] 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'.

[13] This sample restriction facilitates comparisons across specifications and time periods.

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	9.223	5.592	0.511	0.353
	(12.76)	(7.834)	(7.873)	(4.645)	(3.900)
Controlling for nearest SRB fixed-effects	22.17*	12.23	7.671	0.185	0.554
	(12.34)	(8.441)	(10.27)	(5.034)	(3.634)
... and 1991 residental characteristics	27.21**	17.89**	14.25	7.160	7.869**
(aEDED level)	(12.30)	(8.207)	(10.27)	(5.157)	(3.955)
... and 1991 residental characteristics	27.16**	14.50*	8.567	−0.847	−2.614
(at neighborhood level)	(12.45)	(7.802)	(9.180)	(4.814)	(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 5 km. 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).

trolling 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).[14]

The estimates reported in the bottom two rows of Table 3 suggest that areas within 1 km 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 1 km of where the subsidised business floor space was built: As we move from <1 km to <2 km the number of EDs roughly doubles, and the effect halves consistent with positive employment effects at <1 km now being averaged across more EDs.

Two limitations need to be taken into account when interpreting the estimates reported in Table 3. First, the SRB was targeted towards declining neighbourhoods. While we are able to condition on the ED fixed-effects and a rich set of background characteristics – and the allocation process appeared to contain random elements as discussed in Section 2 above – it is possible that the policymakers had information on the expected decline of a neighbourhood that is not available for us. If such information were used to target declining areas, our estimates would be biased downwards. This conjecture is supported by the fact that the estimates become larger as we condition on observable characteristics. Thus, if we could also condition on the relevant unobservable characteristics, the estimates would likely become even larger than those reported in Table 3. In the appendix, we use the formalisation of this idea by Altonji et al. (2005) and Oster (2019) to examine the likely extent of the bias. The Oster bounds for the specifications conditioning on 1991 residential characteristics are about 30 jobs per ED. Importantly, all estimates and bounds suggest that the SRB increased the number of jobs located at or close to the SRB sites.

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

[14] These 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 5 km from the nearest SRB.

Table 4
Effect of treatment rounds 4 to 6 on change in workplace employment.

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

5.2. Dynamics

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 three years after round 6 is completed (i.e. 2005) and they grow some-

what over time.[15] As in the final row of Table 3, the coefficients are only ever significant in the closest distance bands.

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 the online appendix suggest that these concerns are largely unwarranted. We can 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) or even take the more conservative approach of dropping all observations within 5 km of a round 1 to 3 project. In both cases, we still find a significant positive effect of round 4 to 6 on employment from around 2005 onwards.

5.3. Local comparisons

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.[16] As noted above, the results reported in Tables 3 and 4 are in line with this assumption.

As in Gibbons (2015) and Einiö and Overman (2020), we implement this idea by using EDs that are within 5 km 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 Δy_{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 km 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 β^K measure the change in employment for EDs located *k* to *k-1 km* from an SRB site in comparison to EDs 4 to 5 km of an SRB site (the omitted category). As before, in our preferred specifications, the vector x_{i0} controls for nearest 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 1997 and in all years 2003 to 2009.[17] The restriction to EDs with 5 km 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.

Estimates of Eq. (2) measure the impact of the SRB on local jobs under the identifying assumption that areas 4–5 km away from an SRB site are not affected by the intervention. This assumption would be violated if the SRB led to a relocation of jobs from the 4–5 km area into or close to the SRB sites. In this case, the control group would be negatively affected and the estimates for β^K would be biased upwards. Thus, the resulting estimates can be interpreted as upper bounds for the true effect. On the other hand, the baseline estimates are likely to be, if anything, biased downwards and can thus be interpreted as lower bounds (see above). Thus, these two sets of estimates together provide plausible bounds for the true treatment effect.

Table 5 presents the results from estimating Eq. (2).[18] The first column reports results when including no additional control variables. The estimates show that EDs close to round 4 to 6 SRB sites experienced larger changes in employment than EDs 4–5 km 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 Eq. (1): the estimates become larger and statistically significant as we add control variables to the specification.

Table 5
Effect of treatment rounds 4 to 6 on change in workplace employment 1997 to 2009, by distance to SRB project for EDs within 5 km of Round 4 to 6 project.

	(1)	(2)	(3)	(4)
0 – 1 km	15.74	19.44	23.23*	26.18**
	(15.73)	(13.45)	(12.95)	(12.98)
1 – 2 km	2.709	5.799	8.017	5.915
	(15.19)	(13.81)	(13.68)	(12.15)
2 – 3 km	10.12	12.07	13.37	14.18
	(13.98)	(15.94)	(15.37)	(14.05)
3 – 4 km	−11.59	−11.80	−8.886	−5.324
	(8.424)	(12.47)	(12.24)	(10.79)
4 – 5 km	.	.	.	.
Number EDs	25,866	25,866	25,866	25,866
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 residentialEDED)	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 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 Eq. (2) positive employment effects can be detected earlier than for Eq. (1) – specifically in the year after round 4 is completed (i.e. 2003) and they again grow somewhat over time.

As with Eq. (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 5 km 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 5 km of a round 4 to 6 SRB project, but more than 5 km from a round 1 to 3. Results reported in the online 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 1 km.[19] In line with the results reported in Table 3, the coefficients in Table 5 suggest that the positive effects within 1 km 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 3 km away from the SRB site, where the sign turns negative. Clearly, the existence of this kind of displacement implies that treatment by an SRB site is affecting the control EDs, so our estimate is not causal, in the sense of an SRB site adding to overall employment. Even so, SRB generates 'additional-to-the-area' employment close to SRB sites, relative to those further away,

[15] 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.

[16] 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.

[17] This sample restriction facilitates comparisons across specifications and time periods.

[18] The standard errors are robust to clustering by ring and SRB site. That is, we have one cluster for EDs within 1 km of SRB round 4 to 6 project A, one cluster for EDs within 2 km of SRB round 4 to 6 project A, …, one cluster for EDs within 1 km of round 4 to 6 SRB project B, etc.

[19] Although, formally, we cannot reject equality of the coefficients. For example, a t-test for the equality of the coefficients on kilometre 0-1 km and 1-2 km has a p-value of 0.33.

Table 6
Effect of treatment rounds 4 to 6 on change in workplace employment by distance to SRB project for all EDs within 5 km of Round 4 to 6 project.

	−2003	−2004	−2005	−2006	−2007	−2008	−2009
0 – 1 km	16.87	10.43	26.70*	28.22**	28.14**	22.99**	26.18**
	(11.14)	(9.343)	(14.42)	(13.08)	(12.10)	(10.96)	(12.98)
1 – 2 km	−6.661	1.584	0.723	0.561	−0.211	8.197	5.915
	(10.09)	(9.350)	(10.65)	(10.71)	(11.08)	(10.23)	(12.15)
2 – 3 km	−1.094	9.185	1.808	3.554	6.543	6.497	14.18
	(11.89)	(12.26)	(13.74)	(14.26)	(14.95)	(12.14)	(14.05)
3 – 4 km	−12.21	−8.972	−2.986	−1.852	−3.285	−4.586	−5.324
	(8.976)	(10.32)	(10.34)	(10.56)	(11.14)	(9.505)	(10.79)
4 – 5 km	.	.	.	.	.	.	.
Number EDs	25,866	25,866	25,866	25,866	25,866	25,866	25,866

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.

but within 5 km. The question remains as to whether these localised 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 more local jobs as documented in the previous section, employment rates of residents should increase if they take some of those jobs. 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.

6.1. Baseline estimates

As for employment, we start by estimating Eq. (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.[20] Results when estimating Eq. (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 Eq. (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 1 km; to $K = 5$ within 5 km). 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.[21] Areas close to SRB sites tended to experience changes in employment rates that were no different to other *comparable* areas.

Similar to our analysis of workplace employment, we carried out an Oster bounds analysis (Oster, 2019). The Thin the online appendix). As we discuss in Section 5 (and in the appendix), these estimates answer the question: "What would the treatment effect be, if selection on unobservables would be as important as selection on observables?" All of our estimates addressing this question are close to zero and slightly negative. Furthermore, the observable characteristics included in our data explain more than 40% of the variation in the change of local employment rates. Taken together with, Tables 3, 5 these results thus strongly suggest that while the SRB projects affected local workplace employment, the new jobs seem to have little, if any, effect on local employment rates.

6.2. Using later rounds as a control group

For employment rates, we can achieve more credible identification by following Busso et al. (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. Round 5 provided funding for projects lasting from 1 to 7 years after June 1999, but the overall spend for Round 5 was anticipated to be £1.25 billion with only £75 million of this in 1999 and 2000.[22] Given this timing, we assume that any impacts from the larger Round 5 projects and all Round 6 projects will have post-dated the April 2001 Census, and use EDs exposed to projects in these final rounds as control areas. 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 Eq. (1), but with treatment D_i^K redefined to be an indicator variable taking the value one 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 1 km) to $K = 5$ (within 5 km) 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 the employment rate of residents living in EDs within 1 km of round 1 to 2 SRB site the change in employment rate of those living

[20] We focus on these two rounds in order to make timing of the analysis of workplace and resident based employment as comparable as possible. That is, rounds 4-6 were launched in 1998-2000 and the estimates for workplace employment become statistically significant in 2006 (see Table 4). In comparison, SRB rounds 1-2 were launched in 1994-1995 and hence the 2001 measures employment rates 6-7 later.

[21] Results in show that this finding is robust to dropping all ED within 5 km of a round 3 or 4 project (which may have received some treatment by 2001).

[22] These figures come from the "SRB Round 5 bidding guidance: a guide for partnerships" available from the National Archives http://webarchive.nationalarchives.gov.uk/20120919132719/www.communities.gov.uk/documents/regeneration/pdf/155889.pdf (accessed May 2018).

Table 7
Effect of treatment rounds 1 and 2 on change in employment rate 1991 to 2001.

	Bandwidth				
	<1km	<2km	<3km	<4km	<5km
Baseline	−0.013**	−0.010***	−0.0077***	−0.0083***	−0.0077***
	(0.00629)	(0.00378)	(0.00294)	(0.00258)	(0.00246)
Controlling for nearest SRB fixed-effects	−0.0137**	−0.0105***	−0.0077***	−0.0083***	−0.0077***
	(0.00629)	(0.00378)	(0.00294)	(0.00258)	(0.00246)
... and 1991 residental characteristics	−0.0104	−0.00730*	−0.00412	−0.00338	−0.00182
(at ED level)	(0.00693)	(0.00438)	(0.00369)	(0.00318)	(0.00274)
... and 1991 residental characteristics	−0.00783	−0.00500	−0.00195	−0.00085	0.00112
(at neighborhood level)	(0.00607)	(0.00379)	(0.00313)	(0.00298)	(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 residential employment rate 1991 to 2001. 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 5 km. 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	0.0030	0.0060	0.0038	0.0024
	(0.00816)	(0.0056)	(0.0047)	(0.0043)	(0.0042)
Controlling for nearest SRB fixed-effects	−0.014	−0.0024	0.0011	−0.0022	−0.0059
	(0.0099)	(0.0054)	(0.0047)	(0.0040)	(0.0042)
... and 1991 residental characteristics	−0.0150	0.00296	0.00049	−0.0031	−0.0027
(at ED level)	(0.0096)	(0.0059)	(0.0062)	(0.0050)	(0.0039)
... and 1991 residental characteristics	−0.016**	−0.0016	−0.00089	−0.0030	−0.0017
(at neighborhood level)	(0.0073)	(0.0063)	(0.0050)	(0.0045)	(0.0034)
Number EDs	5212	12,210	19,911	26,854	32,192
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 residential employment rate 1991 to 2001. 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 5 km. Rows 2 to 4 in each panel add additional controls as described in the text. Standard errors (in parentheses) clustered by nearest SRB.

within 1 km 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 are, 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 Eq. (3) should provide us with a reasonable estimate of the effect on employment rates of the increase in employment identified in Tables 3–5. Once again, we find no significant (positive) effects on employment rates across all specifications and distance bands.[23]

6.3. Local comparisons

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 5 km SRB neighbourhoods that we have constructed. We implement it by estimating Eq. (2) for employment rates, with timing changed to reflect the availability of data. Specifically, we now use EDs that are within 5 km of a round 1 to 2 SRB site.

Table 9 presents the results of this approach, estimating Eq. (2) for long differences in 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.

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 average local impact of an SRB project was an increase of around 25 jobs per ED within 1 km of a round 4 to 6 project. There were 8,267 EDs within 1 km 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

[23] As before, results in show that this finding is robust to dropping all ED within 5 km of a round 3 or 4 project (which may have received some treatment by 2001).

Table 9
Effect of treatment rounds 1 to 2 on change in employment rate 1991 to 2001, by distance to SRB project for alEDED within 5 km of Round 1 to 2 project.

	(1)	(2)	(3)	(4)
0 – 1 km	−0.00654	−0.00866*	−0.00908*	−0.00684
	(0.00756)	(0.00520)	(0.00527)	(0.00441)
1 – 2 km	−0.00437	−0.00627***	−0.00635***	−0.00447**
	(0.00503)	(0.00230)	(0.00208)	(0.00194)
2 – 3 km	0.00506	0.000624	−0.00134	0.00108
	(0.00531)	(0.00248)	(0.00261)	(0.00230)
3 – 4 km	−0.000848	−0.00300	−0.00310	−0.00159
	(0.00577)	(0.00286)	(0.00282)	(0.00236)
4 – 5 km	.	.	.	.
Number EDs	17,574	17,574	17,574	17,574
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.

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 £6885 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, even with adjustments for a plausible degree of selection on unobservables. It seems likely that building new business floor space in deprived neighbourhoods had no effect on the employment of local residents, despite creating some jobs at a high cost.

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 policymakers, 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 a major regeneration programme that aimed to enhance the quality of life of local people in deprived neighbourhoods in the UK. We focus on a subset of projects implemented as part of the 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 a 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 rule out the possibility that these projects were a cost-efficient way to improve local employment. Thus our study provides an example of the challenges government face when trying to help the residents of deprived neighbourhoods by "bringing jobs" to them.

CRediT authorship contribution statement

Stephen Gibbons: Conceptualization, Formal analysis, Funding acquisition, Investigation, Methodology, Project administration, Writing - original draft, Writing - review & editing. **Henry Overman:** Conceptualization, Formal analysis, Funding acquisition, Investigation, Methodology, Project administration, Writing - original draft, Writing - review & editing. **Matti Sarvimäki:** Conceptualization, Formal analysis, Investigation, Methodology, Project administration, Writing - original draft, Writing - review & editing.

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Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jue.2020.103315.

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