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The (Displacement) Effects of Spatially Targeted Enterprise Initiatives: Evidence from UK LEGI

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Abstract
We investigate the impacts of a significant area-based intervention (LEGI) that aimed to increase employment and entrepreneurial activity in 30 disadvantaged areas across England. We examine the spatial pattern of effects at a fine spatial scale using panel data for small geographic units and a regression discontinuity design that exploits the programme eligibility rule. The results indicate considerable local displacement effects. Employment increases in treated areas close to the treatment area boundary at the cost of significant employment losses in untreated localities just across the boundary. These differences vanish quickly when moving away from the boundary and do not persist after the programme is abolished. These findings support the view that area-based interventions may have considerable negative displacement effects on untreated parts of the economy. This displacement can substantially reduce (or in this case eliminate) any net benefits.

Keywords: place-based policy, programme evaluation, displacement, employment, industrial policy
JEL Classifications: R11; H25; J20; O40

1 Introduction

Many governments target large amounts of public spending at areas experiencing high unemployment and poor economic performance. Regional policy accounts for around 35 percent of total community spending in the EU (EU, 2013) and many national governments in Europe run extensive regional programs targeted at disadvantaged areas. In the US, around $95 billion is spent annually on spatially targeted economic development programs by federal and state governments (Kline & Moretti, forthcoming). Despite the prevalence of these policies, a common concern among economists is that they merely shift economic activity from one place to another without any aggregate net benefits. While a number of studies have examined the direct impacts of such policies, these displacement effects are still poorly understood.

We study the impacts of a significant area-based intervention that aimed to increase employment and entrepreneurial activity in disadvantaged areas in England, with a specific focus on identifying spillover effects of the programme on untreated locations. The Local Enterprise Growth Initiative (LEGI) spent 418 million pounds on 30 deprived areas during the period 2006-2011 (DCLG, 2010b). Eligibility for the program was based on a deprivation index rank rule: an eligible area had to rank 50th or below against at least one of a set of pre-determined deprivation indices. We use these features of the programme to identify the causal effects on employment, net business creation and unemployment. Using data at a fine spatial scale, we find local displacement of jobs within 1 kilometre of the boundary of treatment areas. There are no statistically distinguishable differences between the performance of treatment and control locations further from the boundary. Moreover, our results suggest that improved employment in boundary treatment areas is offset by a drop in employment in the nearby untreated areas implying net effects that are much smaller than aggregate. We present evidence that suggests these effects are unlikely to persist much beyond the end of the programme. In contrast to these temporary displacement effects on employment, we find little, if any, effect at all on number of businesses. Finally, consistent with the fact that employment effects occur at a scale that is smaller than the local labour market, we find no effect on unemployment of local residents.

The common criticism of the effectiveness of spatially targeted policies relies on the argument that in competitive labour and property markets spatially-targeted support is offset by increases in rents (e.g. Muth, 1969; Roback, 1982).[3] Our empirical results can be explained by appealing to the fact that, in the UK, commercial rental contracts typically cover a five-year fixed term which implies sticky price adjustment and incomplete capitalization in the short-run (Crosby et al, 2003). As a result, subsidies targeted to specific areas may induce (short-run) spatial reallocation of economic activity towards supported areas. Because similar businesses that are located close to each other often compete in the same local product or service markets, effects of the programme would be more likely to spillover to an untreated area the closer it is to the treatment area boundary. In addition to these effects operating through local product market competition, labour market effects may be larger for untreated locations closer to treated areas as a result of either commuting costs (e.g., Zenou, 2009) or local hiring networks (e.g., Topa, 2001; Ioannides and Loury, 2004).

This paper contributes to the literature concerned with displacement effects of economic policy interventions (Heckman et al, 1998; Blundell et al, 2004). While a growing literature has examined displacement effects within local labour markets in the context of active labour market programmes (e.g. Gautier et al, 2012; Crépon, et. al. 2013; Ferracci et al, 2014), convincing evidence of spillover effects of business support interventions across treatment area boundaries is scarce. Hanson and Rohlin (2013) suggests that areas that border US Empowerment Zones (EZ) experienced a decline in employment and net business creation compared to areas that border rejected EZ applicants, which could indicate negative displacement of economic activity across the treatment area boundary.[4] In contrast, Ham et al (2011) find no evidence of displacement effects of three US place-based programmes when comparing a set of nearest ineligible Census Tracts (contiguous to the treatment area) to a set of second nearest ineligible Census Tracts (i.e. ineligible Tracts that are contiguous to the set of nearest ineligible Tracts). In a study on the long-term impacts of the Tennessee Valley Authority, Kline and Moretti (2014) report results based on comparisons between counties adjacent to the TVA area and other ineligible counties, but find no indication of local spillovers at the TVA boundary. Finally, at a larger spatial scale, Gobillon et al (2012) find no

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[3] For a discussion of the cases in which spatially-targeted economic interventions may be economically efficient, see e.g. Greenstone and Looney (2010) and Kline and Moretti (2013, forthcoming).

[4] In a related study, Givord et al (2013) compare buffers around French enterprise zones that were granted a business tax exemption to observably similar buffers around areas that were in a pool of candidate enterprise zones but were not selected. Their matching approach suggests that the growth rate in the number of establishments was lower within buffers around treated areas compared to buffers around untreated areas, but they find no similar effects on employment. Mayer et al (forthcoming) arrive at a similar conclusion when comparing narrowly defined ZFU areas to non-ZFU areas within the same municipality and further away. See also the study by Hanson and Rohlin (2011) suggesting increasing share of industries that were expected to have the largest gains from tax incentives in Empowerment Zones compared to Enterprise Communities, which may indicate displacement of economic activity across sectors. Finally, a related strand of literature that examines R&D spillovers finds evidence of business stealing effects (e.g. Bloom et al, 2013).

evidence of displacement between treatment and control municipalities located within five kilometres. Likewise, Criscuolo et al (2012) find no evidence of displacement effects for the UK Regional Selective Assistance program when looking at Travel to Work Areas.

Our approach for identifying displacement effects builds on the previous literature to evaluate the impact of place-based policy interventions using fine-resolution spatial data (e.g. Neumark and Kolko, 2010).[5] While previous literature on spatial spillover effects has focused on ineligible areas that border treatment areas, we examine the distribution of changes in the outcomes of interest over a wide spatial range by splitting treatment and control areas into one-km-wide zones based on the distance from the treatment area boundary. This approach allows for identifying displacement effects in local markets around treatment boundaries, provided that spillovers weaken sufficiently quickly as the distance between treated and untreated locations increases. This proves to be the case as we find displacement of employment from narrow control zones to treatment zones at the boundary, while the effects diminish quickly as we move away from the boundary.

The usefulness of comparing nearby treated and untreated locations depends on the extent to which unobserved area characteristics affecting the outcomes vary smoothly across space. If variation is smooth, reducing the distance between treated and control areas alleviates concerns about potentially confounding unobserved differences in the absence of treatment (Duranton et al, 2011). However, while high-resolution spatial data allows for comparisons between very closely located areas at the treatment boundary, crossing such a boundary may change unobserved policy factors, if it separates administrative units that carry out independent local policies (as is the case in our application based on local authority boundaries).

Some reassurance on this point, comes from the fact that trends between treatment and control zones near to the LEGI boundary are similar in the period preceding the implementation of the programme. In order to lend further credibility to our results, we exploit the discontinuity in the rule determining whether an area is eligible to apply for LEGI funding. According to this rule, a local authority (LA) is eligible to apply for funding if it ranks 50th or worse against at least one of twelve pre-determined measures of deprivation. We exploit this institutional restriction to identify the causal impact of the policy for a population of treated and untreated areas just above and below the

[5] Earlier contributions include Dabney (1991); Papke (1993, 1994); Boarnet and Bogart (1996); Bondonio and Engberg (2000); Peters and Fisher (2002); O'Keefe (2004); Bondonio and Greenbaum (2007); Hanson (2009). Bartik (1991) and Nolan and Wong (2004) provide reviews.

eligibility threshold. Identification using this discontinuity requires that LAs near the threshold were unable to manipulate their deprivation rank and eligibility status. This is likely to be the case because deprivation ranks determining eligibility were based on data pre-dating the announcement of the programme. We show that there is no abnormal bunching of LAs at the eligibility threshold and conduct several robustness tests to validate our approach.

The unintended displacement of economic activity across the treatment area boundary is especially relevant in the context of narrowly targeted place-based renewal policies, because the negatively affected nearby neighbourhoods are typically also among the most disadvantaged areas. Thus the impact of policy is simply to shuffle employment from one deprived area to another. Displacement across treatment area boundaries also has important implications for the interpretation of evaluation strategies commonly used to consider the effects of spatially targeted programs. First, if the impact of the treatment spillover to nearby untreated areas, using areas close to the treatment area as a control group yields misleading estimates of the impact of the programme. Second, even if treatment areas were randomly assigned to treatment, comparisons between treatment and control areas which are further apart and thus unlikely to be affected by local displacement effects will provide biased estimates of the net effects of the intervention if spillovers to nearby areas are not taken into account.[6] Our approach utilising detailed spatial data suggests one approach for addressing this problem.

The rest of the paper is structured as follows. Section 2 provides more details of the LEGI programme. Section 3 describes our data while section 4 describes our identification strategy and section 5 reports the results. Section 6 concludes.

2 Institutional Setting

The Local Enterprise Growth Initiative (LEGI), introduced by the British government in 2006, aimed "to release the productivity and economic potential of the most deprived local areas through enterprise and investment, thereby boosting local incomes and employment opportunities." (DCLG, 2010a). The three key objectives of the programme were to i) increase entrepreneurial activity in deprived areas ii) support growth and reduce exit rates among local businesses in deprived areas,

[6] A further concern is general equilibrium effects at a more aggregate geographic level identification of which is out of the scope of this study. However, our empirical approach will detect *local* displacement effects even if wider-area general equilibrium effects are simultaneously at work.

and iii) attract investment into deprived areas. The primary aim of LEGI funding was to increase resources and develop existing schemes supporting these objectives (HM Treasury, 2006).

91 deprived LAs in England were eligible to bid for LEGI funding. These were areas that were receiving funds from an existing programme – Neighbourhood Renewal Fund (NRF) – at the time LEGI was announced in July 2005, and any areas that were named eligible for the 2006-2008 allocation of the NRF.[7] In order to be eligible, an LA had to rank 50th or worst against any of six LA-level indices of multiple deprivation (IMD) in 2000 or 2004. The LA-level IMDs were constructed by the central government using a complex three step procedure based on a number of LSOA-level variables pre-dating the introduction of LEGI, some of which dated back as far as the 1991 Census (Appendix A provides details of the construction of these indices).

Selection of treatment areas from the pool of eligible LAs was based on LA proposals detailing the way in which they planned to achieve the objectives of the initiative. To support their proposals, LAs were expected to provide evidence of the level of deprivation in their area and the gaps in local provision of public services supporting business (HM Treasury 2006). 279 million pounds of LEGI funding covering three years was allocated in the first two competitive bidding rounds held in February and December 2006 with funding awarded to 20 LEGI areas comprising 30 LAs displayed in figure 1 (some LEGI areas involved joint bids with the largest awarded joint initiative consisting of five LAs) (DCLG, 2010a). Projects under the scheme were expected to operate with a ten year time horizon. In the end, the programme ran for 6 years with the initiative abolished from March 2011 following a change of government. By the abolition of the programme, the total spending on assistance had reached 418 million pounds. Spending was at its highest level in 2008 and 2009 with around 100 million pounds spent annually.[8] With resident working age populations of around 1.4 million, 234,000 unemployed, and 85,000 businesses in 2006, per year allocation of LEGI funding was around £71 per capita, £427 per unemployed, and £1,176 per business in these years.

Due to the LA-specific design, the mix of support activities differed quite significantly by area. Across the programme as a whole about 30% of expenditure went on supporting existing local

[7] At the time, the NRF was the major funding stream used to try to tackle deprivation in England's poorest neighbourhoods. In contrast to LEGI, with its clear economic focus, NRF had much wider objectives with about 20% of expenditure targeted at crime, 20% on education (school and pre-school provision), 13% on employment, 15% on health, 7% on housing and physical environment and 7% on transport (with the remainder spent on miscellaneous other local priorities and administration). See DCLG (2008) for more details of the NRF programme.
[8] The annual allocation figures are drawn from an unpublished document "LEGI Capital and Revenue Split" provided to the authors by the UK Department for Communities and Local Government.

businesses, with projects supporting new business start-ups receiving a similar share. Support to residents, e.g., in acquiring skills or a job, accounted for about 20% of expenditure, while about 10% was spent on area improvements or promotion. Management and administration accounted for the remaining 10% of expenditure. The majority of LAs put emphasis on business assistance with 16 out of 20 LEGI areas targeting at least 50 percent of their total LEGI funding on businesses, while spending targeted to local residents accounted for a much smaller proportion in majority of support areas with its funding share being less than 25 percent in 15 areas (figure 2).

In a 2010 evaluation, the government reported that LEGI had assisted or engaged 240,000 individuals by December 2009 (around 17% of the working age population), created 19,500 jobs (a 1.9% job creation rate from a baseline of around 1 million area jobs in 2006) and 13,700 new business (a 17% creation rate from a baseline of around 78,500 in 2006) (DCLG 2010a; DCLG 2010b). The evaluation employs propensity score matching at LSOA level (based on observed area characteristics) which does not control for unobserved heterogeneity in local economic trends. As will become clear in our analysis below, ignoring such confounding variation across localities massively over-estimates the benefits from the programme.

3 Data

Our units of observation are so-called Lower Layer Super Output Areas, henceforth referred to as super output areas or LSOA. LSOA are small geographical areas used as the basis for the UK census.[9] The boundaries of super output areas coincide with LA boundaries and they can be classified exactly as either treated or untreated on the basis of LEGI boundaries.

Our three outcomes of interest – employment, number of businesses, and unemployment – correspond closely to the objectives of LEGI. Data on employment and number of business come from the Business Structure Database which provides an annual snapshot of the Inter-Departmental Business Register (IDBR). This dataset contains information on 2.1 million businesses, accounting

[9] LSOAs have a minimum population of 1,000. The 32,482 LSOAs in England were built from groups of Output Areas (OA) (typically 4 to 6) and constrained by the boundaries of the Standard Table wards used for 2001 Census outputs. 2001 Census OAs were built from clusters of adjacent unit postcodes. They were designed to have similar population sizes and be as socially homogenous as possible (based on tenure of household and dwelling type). OAs preferably consisted entirely of urban postcodes or entirely of rural postcodes. They had approximately regular shapes and tended to be constrained by obvious boundaries such as major roads. The minimum OA size is 40 resident households and 100 resident persons but the recommended size was rather larger at 125 households. (http://www.ons.gov.uk/about-statistics/geography/products/geog-products-area/names-codes/soa/index.html and http://www.statistics.gov.uk/geography/census_geog.asp, accessed 27/06/2011)

for approximately 99% of economic activity in the UK and includes each business' name, postcode and total employment. We use the Ordnance Survey Code-Point data set to match business postcodes to LSOA and then construct our measures of employment and number of businesses by aggregating the BSD data by LSOA. Unemployment data measures the number of benefit claimants aged 16-64 and is available at LSOA level from the Neighbourhood Statistics database maintained by the ONS.[10] We also have data for area characteristics measuring acreage, measures of deprivation and economic activity and ethnic composition of residents at LSOA level provided by Neighbourhood Statistics.

Geocoding of LSOA by treatment status is based on shape files provided by the UK Borders database. An LSOA is considered as treated if it is located within the boundaries of an LA receiving LEGI funding. Because LSOA are constructed so as not to cross any LA boundaries, the geocoding of treated LSOA is exact.

We evaluate the impact of LEGI using data for the period 2002-2012 covering three years before, and eight years after, the announcement of LEGI in 2005. We use 2004 – the year before LEGI was announced – as the base-line year. As discussed above, the policy was announced in 2005, the first funding allocations were made in 2006 and the programme ran until March 2011. Thus the data window allows us to examine potential pre-treatment trends prior to the announcement, treatment effects in the programme period, and whether effects persist once the programme ends. We draw additional data from the 2001 census to control for pre-treatment area characteristics. Descriptive statistics for the data are presented in Tables 1 and 2. In addition to levels, we present statistics for log changes for key outcomes for pre-treatment period 2002-2004 and for two different treatment periods: 2004-2009 and 2004-2012 (on which our empirical specifications are based). The way in which these statistics are presented relates to our empirical strategy and so we postpone further discussion of these tables until we have outlined the details of our approach (see section 5).

4 Empirical Strategy and Identification

In this section, we explain our empirical strategies for identifying the causal effects of LEGI funding on economic outcomes in local super output areas (LSOA) that are affected by treatment.

[10] www.neighbourhood.statistics.gov.uk. Unemployment is measured as the count of Job Seeker Allowance claimants within an LSOA. To get Jobseeker's Allowance a job seeker must be available for, capable of and actively seeking work, aged 18 or over (except in some special cases) but below State Pension age, working less than 16 hours per week on average, and living in Great Britain.

In the conventional programme evaluation framework it is typically assumed that, in the absence of the programme, area outcomes of observably similar LSOA would be, on average, identical for treated and untreated units (this is the conditional independence assumption or CIA). A second key assumption for identifying the treatment effect of the programme is that the treatment did not affect outcomes for the control group (this is the single unit treatment value assumption or SUTVA). If these assumptions hold, average outcomes for untreated LSOA conditional on observable characteristics would provide an unbiased estimate of what would have happened to a treated LSOA in the absence of LEGI funding and we could identify the LEGI treatment effect by estimating the following equation:

$$\Delta y_{it} = \alpha + \gamma L_{r(i)} + \epsilon_{it}, \tag{1}$$

where Δy_{it}=$y_{it} - y_{i,2004}$ is a log change in the outcome of interest between the baseline year 2004 and year t in an LSOA i; $L_{r(i)}$ is a binary indicator for LEGI, equal to one if the LSOA is within a Local Authority r, which was awarded LEGI funding, and zero otherwise.

Equation (1) compares changes in the outcome between *all* treated and *all* untreated LSOA. A potential concern with this approach is that, to the extent that treatment is not assigned randomly (as is the case with LEGI), unobserved factors affecting the outcome may vary across treated and untreated locations, and this would invalidate the CIA. This concern has led many scholars to use nearby untreated locations as a control group (e.g. Neumark and Kolko 2010). This approach is based on the idea that if unobserved characteristics vary smoothly across space, the CIA is more likely to hold the smaller is the distance between treated and untreated locations (Duranton et al 2011). A standard way to implement this idea empirically would be to introduce fixed effects for some appropriate set of spatial units that represented aggregations of similar LSOA. In line with this approach, we estimate:

$$\Delta y_{it} = \alpha_l + \gamma L_{r(i)} + \beta X_i + \epsilon_{it} \tag{2}$$

where Δy_{it} and $L_{r(i)}$ are as before and α_l are a set of fixed effects defined as follows: For each LEGI area we define a neighbourhood comprising all LSOA within the LEGI boundary (the treated area) and all LSOA outside the LEGI boundary but within 10 kilometre of the LEGI boundary (the control area). We call these *LEGI neighbourhoods*. For the remaining LSOA that are located outside these LEGI neighbourhoods we assign fixed effects by LA. Identification of the impact of the programme now comes from comparisons *within* LEGI neighbourhoods between treated and untreated LSOA (i.e. LSOA inside the LEGI area versus LSOA outside, but close to the LEGI area). Although treatment status is invariant within LAs outside LEGI neighbourhoods, we keep them in the sample to allow comparability across specifications and improve precision of the estimated coefficients on pre-treatment area characteristics X_i (these include ethnic composition and economic activity of residents, acreage, and minimum IMD rank as defined below).

Unfortunately, while the introduction of LEGI neighbourhood fixed effects may help with the CIA it may undermine validity of the SUTVA. For example, if economic agents in nearby treated and untreated locations compete in the same local markets and treatment affects the outcomes in the untreated areas as a result, decreasing the distance between the treatment and control group makes it more likely that treatment also affects the control group (i.e. SUTVA does not hold). Indeed, the concern that spillovers are stronger the closer control units are to the treatment area has led some scholars to drop nearest untreated locations from the control group (e.g. Kline and Moretti 2014). As we now explain, we use the fine spatial detail of our data to better address this trade-off between the CIA and SUTVA.

Identifying Local Displacement Effects at Treatment Area Boundary

In order to disentangle the extent to which the observed differences between treated and untreated LSOA are due to direct impacts of the program on treated LSOA as opposed to spillover effects on untreated LSOA, we divide treatment and control areas in to rings based on their distance to the boundary of the treated area.[11] That is, we augment equation (2) with dummy variables for control and treatment rings that run parallel to the LEGI boundary:

$$\Delta y_{it} = \alpha_l + \sum_{k=1}^{6} \theta_k T_i^k + \sum_{h=1}^{10} \xi_h C_i^h + \beta X_i + u_{it}, \tag{3}$$

[11] Gibbons et al (2011) use a similar approach to study the impact of government subsidised improvements to commercial building supply. The approach employed in our study develops this idea one step further and divides both control *and* treated areas in to rings based on the distance to the LEGI boundary.

where Δy_{it} and X_i are as before; the C_{i}^{h} are a set of ten one-km-wide *control* ring dummies that take value one if the distance from an untreated LSOA *i* to the nearest treated LSOA is between h-1 and h kilometres, and zero otherwise; and $\mathrm{T}_{\mathrm{i}}^{k}$ are a set of six treatment ring dummies. Treatment rings are constructed symmetrically for treated LSOA, although we pool all LSOA that are further than five kilometres from the nearest untreated LSOA because the number of observations quickly decreases when moving towards the centre of a LEGI area. More specifically, for k∈ $\{1, \dots, 5\}$, $\mathrm{T}_{\mathrm{i}}^{k}$ take value one if the distance from a treated LSOA *i* to the nearest untreated LSOA is between k-1 and k kilometres, while $\mathrm{T}_{\mathrm{i}}^{6}$ takes value one if the distance is more than five kilometres, and zero otherwise. Including α_l in the estimating equation means that ring indicators identify the spatial pattern of the changes in the outcome of interest *within* LEGI neighbourhoods. Finally, as with equation (2), although treatment status is invariant within LAs outside LEGI neighbourhoods, and we do not assign LSOA within them to one-kilometre rings, we include them to allow comparability across specifications and to improve the precision of the estimated coefficients on pre-treatment controls (i.e. β**)**.

In equation (3), $\theta_k - \xi_h$ is the difference in the average (conditional) growth rate for the outcome of interest between treatment ring *k* and control ring *h.* This difference identifies the treatment effect of LEGI if both CIA and SUTVA hold for the ring pair. As discussed above, if unobserved characteristics vary smoothly across space, the CIA will be more likely to hold the smaller is $h + k$. However, spillovers in local markets are likely to increase as $h + k$ is decreased. This implies that while comparisons between ring pairs are likely to be the least confounded by unobserved characteristics when $h = 1$ and $k = 1$, spillover effects are likely to be the largest for such pairs. On the other hand, local spillover effects are likely to diminish as $h + k$ increases, but this will come at a cost of reducing the credibility of CIA. For example, consider the case where the program has a positive impact on treated areas at the expense of nearby control areas. If such displacement effects decay as we move away from the boundary, then $\xi_1 < \xi_h$ for some $1 < h \leq h_B$ where h_B refer to the last control ring within a buffer of comparable LSOA. Furthermore, we can consider a special case where the policy only has displacement effects close to the boundary. This may be the case if, for example, the only impact of the policy is to reallocate market shares across the treatment area boundary within localized markets. This implies an additional condition $\theta_k = \xi_h$ for some $1 < k \leq k_B$ and $1 < h \leq h_B$. Testing displacement at the boundary may then be based on a null hypothesis $\theta_k = \xi_h$ for all $k \leq k_B$ and $h \leq h_B$ against the alternative hypothesis $\theta_1 > \xi_1$.[12] In the context of our study, it proves to be sufficient to set $k_B = h_B = 2$. This test is based on very narrow zones around the treatment area boundary within which unobserved characteristics that vary smoothly across space are likely to be sufficiently similar. We show below that pre-treatment trends in treatment and control areas within a reasonable distance of the treatment area boundary are similar, lending credibility to the CIA in the relevant subsample.

Regression Discontinuity Design Based on the Minimum IMD Rank Rule

Although our analysis is based on data at a very fine spatial scale as a result of which we can compare very nearby locations, exploiting variation across administrative boundaries may raise the concern that locations on different sides of the border, however closely located to each other, are exposed to different local policies. For example, if LAs that submitted successful bids were more capable of carrying out successful economic policies, economic performance in treated areas may have been better even in the absence of LEGI. In this case the CIA may not hold even for pairs of contiguous treatment and control areas.

As argued in detail in Duranton et al (2011), this is unlikely to be a problem in our setting as LAs have a limited number of policy tools with which to affect local development. However, in order to assess the validity of this argument we consider whether non-random assignment of LAs to treatment is driving our results by exploiting the eligibility rule for LEGI funding that was based on pre-announcement deprivation rankings of LAs. According to this rule, an LA was eligible to apply for funding if it ranked among the 50 worst LAs against any of six LA level indices of deprivation in 2000 or 2004.[13] Formally, this rule can be written as:

$$E_{r(i)} = I(R_{r(i)} \leq 50)$$

where $R_{r(i)}$ is LA r's minimum rank across the twelve deprivation indices (six indices for each of the years 2000 and 2004) and $E_{r(i)}$ is a binary indicator taking the value of one if this minimum rank is less than or equal to 50 which unambiguously determined eligibility for LEGI funding.

[12] In our statistical inference, we rely on a more conservative tests based on the alternative hypothesis $\theta_1 \neq \xi_1$, that allows for the sign of the treatment effect (and hence also the sign of the displacement effect) to be unknown a priori.

[13] For details of this rule see section 2. For data and methodology for calculating LA-level IMD rankings see Appendix A.

Figure 3 displays the discontinuity of treatment intensity at the minimum rank cut-off of 50. Among eligible LAs below the cut-off the receipt of treatment was not completely determined by eligibility because constraints on the overall level of funding for LEGI meant that only 30 LAs out of 87 eligible received LEGI funding. 29.4 percent of eligible LSOA are located within a treated LA while ineligible LSOA are never treated. The estimated change (standard error) in treatment intensity at the cut-off is 0.213 (0.010).

Because the jump in the fraction of treated areas at the minimum rank cut-off is less than one we implement a fuzzy regression discontinuity (RD) design (see e.g., Hahn et al 2001; Van der Klaauw 2002) where we use eligibility as an instrument for treatment. The discontinuity at the threshold only provides one instrumental variable, which is insufficient for identification of all coefficients on treatment and control ring dummies in equation (3). Instead, we restrict attention to two subsamples of treated and untreated LSOA constructed on the basis of distance from the treatment area boundary, with distances selected on the basis of estimation results for equation (3) reported below. Specifically, we use (i) a sample of contiguous treatment and control rings located within 1km of the boundary and (ii) a sample of treatment and control rings that are 2-3km away from the treatment boundary (i.e. located further away and non-overlapping with the first sample). Separately for these samples, we estimate a Two-Stage Least Squares (TSLS) procedure based on the following equations:

$$L_{r(i)} = \alpha_{1s} + \rho_s E_{r(i)} + \boldsymbol{\tau}'_{1s}\mathbf{R}^P_{r(i)} + v_{1its} \tag{4a}$$

$$\Delta y_{it} = \alpha_{2s} + \tilde{\theta}_s L_{r(i)} + \boldsymbol{\tau}'_{2s}\mathbf{R}^P_{r(i)} + v_{2its} \tag{4b}$$

where *s* denotes the subsample and $\mathbf{R}^P_{r(i)}$ is a vector of *P* polynomial terms of the minimum deprivation rank. Here $\tilde{\theta}_s$ is the parameter of interest. For a subsample of one-km-wide treatment and control rings that are between *d* and *d-1* kilometres from the boundary, the estimate of $\tilde{\theta}_s$ recovers an effect corresponding to $\theta_d - \xi_d$ in equation (3). It is worth remembering, however, that if treatment effects are heterogeneous across LSOA, the RD procedure will recover the local average treatment effect (LATE) for the population at the cut-off, which need not be equivalent to the average treatment effect for all LSOA.[14] However, as demonstrated in figure 3, none of the

[14] Imbens and Angrist (1994) show that when the effects of the treatment are heterogeneous the IV estimate of the treatment parameter is the LATE. Abadie (2003) provides conditions under which the TSLS estimator recovers the LATE when covariates are included in the model.

ineligible LSOA above the threshold are treated. This implies that the treated and the complier populations are equivalent and thus our estimation strategy recovers the average treatment effect on the treated (ATT) at the cut-off.

The key identifying assumption underlying the RD approach is that LAs did not manipulate their minimum deprivation ranking in order to receive LEGI funding. Although we cannot rule this out completely, it is highly unlikely for several reasons. First, we can rule out direct manipulation through false reporting because the deprivation ranks are constructed by central government on the basis of national statistics. Second, the timing rules out any manipulation of underlying socio-economic characteristics in response to the announcement of the rules for LEGI funding because the deprivation rule was based on data pre-dating the announcement of the programme. Third, we can detect no abnormal bunching of observations at the deprivation rank cut-off of 50 (figure 4).[15] It is possible, however, that LAs just above the threshold may have anticipated that future funding would depend on future deprivation rankings. This seems unlikely, but even if anticipated it is hard to see how this could affect our results because the minimum IMD rank is based on twelve indices with each, in turn, based on complicated formulas so that the outcome of any such manipulation on own IMD score would have been highly uncertain (see appendix A for details of data and calculations used to construct the IMD indices). In addition, even in the unlikely event that a LA could control own scores on which the rankings are based, its final ranking also depends on the performance of other LAs which further decreases the ability to precisely manipulate one's own ranking. In short, it is very unlikely that LAs manipulated own IMD scores to affect LEGI funding and inconceivable that they could affect their rankings.

Before turning to results, a map helps clarify our identification strategy. Figure 5 shows 1km-wide treatment and control rings for the Croydon and Barking & Dagenham LEGI areas in London. The smallest areal units in the figure are the LSOA - our units of observation. LSOA labels indicate the relevant treatment or control ring within which the LSOA is located. For example, the label "T1" refers to the 1km treatment ring while the label "C1" refers to the 1km control ring. As discussed above, a LEGI neighbourhood comprises a LEGI area and a control area around it. The shaded LSOA are those within a 10km LEGI neighbourhood and are defined on the basis of the distance of

[15] Note that skewness towards the lower tail of the probability distribution is typical for a minimum of a random variable.

the LSOA to the nearest LEGI boundary.[16] Estimation based on equation (1) excluding LEGI neighbourhood dummies compares average performance of LEGI LSOA to that of all untreated LSOA, while estimation of equation (2) including fixed effects compares average performance of treated and untreated LSOA *within* LEGI neighbourhoods. The shading identifies LSOA within different 1km-wide control rings. Our test of displacement based on equation (3) compares average performance of the nearest control rings to that of the control rings further away from the LEGI area boundary and to that of the treatment rings within LEGI area boundary within a given LEGI neighbourhood. As should be clear, the non-uniform size of both LEGI areas and individual super output areas introduces some unevenness in terms of the exact shape of the control rings. Finally, our RD approach is based on samples including (i) the 1km treatment rings and the 1km control rings contiguous to the LEGI boundary and (ii) 2-3km treatment rings and the 2-3km control rings.

5 Results

Descriptive Analysis

Table 1 displays descriptive statistics for outcomes and area characteristics for the full sample, treated and control areas. Comparing the second block of statistics to the third we see that, compared to the average control LSOA, treated LSOA start with smaller employment, fewer businesses, a larger unemployed population and a lower employment rate. This is hardly surprising as LEGI is specifically targeted at disadvantaged neighbourhoods. When we turn to changes in outcome variables, we see that LEGI areas do better in terms of changes to employment and number of businesses but worse in terms of unemployment over the period 2004-2009. However, when looking at longer-term changes from 2004 to 2012 (remembering that the programme was abolished in 2011), employment and number of business decline and unemployment increases in LEGI areas compared to the control areas. The growth in average employment is also slower in the treatment area in the pre-treatment period, while pre-trends between the treatment and control areas are very similar in terms of business and unemployment.

The table also presents descriptive statistics for our set of control variables on population characteristics. As expected, there are some systematic differences between treated and control

[16] Technically we use the distance from a control (treatment) LSOA centroid to the nearest treatment (control) LSOA centroid to assign control (treatment) LSOAs to control (treatment) rings. In contrast to defining the distance on the basis of the LEGI boundary line, this has the advantage of ensuring that the average distance between locations in, say, a treated LSOA in the 1km treatment ring and an untreated LSOA in the 1km control ring is approximately 1km.

areas. In 2001, treated LSOA have, on average, a lower proportion of economically active, larger proportion of young unemployed, and higher proportion of long-term unemployed reflecting, again, the selection of economically distressed areas. Treated LSOA also have a larger population with white British ethnic background.

Table 2 displays descriptive statistics for outcomes and area characteristics for the three 1km-wide treatment and control rings nearest to the LEGI boundary. Comparison of tables 1 and 2 shows that control ring LSOA within 1-3km of the LEGI boundary are more similar to the average treatment area LSOA than the average control LSOA in terms of pre-announcement employment and unemployment in 2004. This provides some initial support for the assumption that nearby LSOA are more similar in terms of these outcomes in the absence of treatment. Similarly, the difference in terms of number of businesses is considerably reduced when comparing treatment and control LSOA close to the LEGI boundary as opposed to the comparison to the average control LSOA. Note that, while the average employment trends over the period 2002-2004 are fairly similar across rings, 1km treatment and control rings diverge dramatically by the third full programme year 2009: Over the period 2004-2009 employment decreased in the 1km control ring by around 4%, on the average, while all other rings improved, with the largest increase in the 1km treatment ring (around 10.6%, on the average). Finally, differences across 1km control and treatment rings are much less pronounced in terms of unemployment and number of businesses. It is also worth noting that over a longer period 2004-2012 (ending after the programme was abolished) the differences between 1km control and treatment rings are, once again, small.

These descriptive statistics are suggestive of two effects. First LEGI causes displacement at the programme area boundary. Second, these displacement effects appear not to persist post programme. We now turn to more rigorous econometric results that confirm and extend these findings. We start by presenting basic difference-in-difference estimates that compare all treated LSOA to all control LSOA and to control LSOA within 10km of the treatment area boundary. We then turn to our tests of differential change across 1km-wide control and treatment rings in terms of these variables. These regression approaches allow us to control for the observed differences across locations while maintaining the assumption that (conditional on observable characteristics) unobserved differences across treatment and control locations sufficiently close to the treatment area boundary are not confounding our estimates. Finally, we relax this assumption by exploiting the predetermined eligibility rule that induced exogenous variation in treatment at the minimum IMD rank cut-off of 50.

Difference-in-Difference Estimates

Table 3 displays the results from specifications corresponding to equations (1) and (2) (i.e. *without* treatment and control ring dummies). It shows results for pre-trends between 2002 and 2004, treatment effects for changes between 2004 and 2009 (the third full year of the programme) and longer-term effects for changes between 2004 and 2012 (the year following the abolition of the programme in March 2011).[17] The first column reports naïve estimates of the impact of the program from a pooled regression comparing treated and untreated LSOA (i.e. equation (1)). The second column adds area fixed effects, as discussed in section 4, to control for unobserved area-specific trends (i.e. equation 2 with no X_i). The third column adds control variables for a rich set of area characteristics, while the fourth column displays estimates for the same specification for the sample of LSOA that are in LAs that have a minimum IMD rank of 5 to 145, which is the sample used in the RD analysis below.

In column 1, the naïve estimate is insignificant across all outcomes except for the 2002-2004 and 2004-2012 change in unemployment. All coefficients are insignificant when conditioning on 10km neighbourhood fixed effects (column 2) and adding census controls for area characteristics (column 3). Restricting estimation to the RD sample (column 4) has little impact on the estimates, except the coefficient for the 2002-2004 change in number of business becoming weakly significant.[18] Overall, these results suggest that pre-treatment, during treatment and post-treatment, LEGI areas performed no better or worse than the control areas.

Local Displacement Effects

We now turn to the question of displacement across the LEGI boundary. Table 4 reports estimates for different time periods, for 1km-wide treatment and control rings, from specifications corresponding to equation (3). These results are for specifications including LEGI neighbourhood fixed effects and area controls, with the 1km control ring as the omitted category. As explained in section 4, the size of LEGI LAs limits us to using five 1km-wide rings and a residual 5km-plus ring for treated LSOA inside LEGI. For control areas we use ten 1km-wide rings. Results, available on

[17] For brevity, we display results for three periods. The choice of the period 2004-2009 is motivated by the fact that programme funding was at its highest levels in 2008 and 2009.

[18] Our more detailed difference-in-difference specifications and RD analysis below indicate that this is not driven by pre-policy divergence between treatment and control LSOA relevant for our displacement analysis.

request, are very similar for specifications excluding area controls as well as for the sample of LSOA that are in LAs that have a minimum IMD rank of 5 to 145 (the sample used in the RD analysis below).

As for Table 3, table 4 shows results for pre-trends between 2002 and 2004, treatment effects for changes between 2004 and 2009, and longer-term effects for changes between 2004 and 2012. Results in column (1) show that pre-treatment trends in employment are similar across the 1km-wide rings around the LEGI boundary, with no evidence of significant differences in employment growth before treatment. This stands in marked contrast to estimates for the period from 2004 to 2009 reported in column (2) suggesting that employment grew significantly faster within the LEGI treatment area compared to the 1km control ring just outside it. If nearby treatment and control rings are comparable and treatment did not spill over to nearby untreated areas, comparisons between 1km treatment and control rings would provide an unconfounded estimate of the treatment effect of the programme. However, the control rings at 2km or greater also experience significantly higher employment growth in the treatment period compared to the 1km control ring, while differences in growth rates are much less pronounced between these control rings and any treatment ring. For example, the differential between the 1km treatment and 2km control rings is around 0.152-0.103 = 0.049. Finally, our long data window allows us to examine the persistence of the employment effects after the programme was abolished in March 2011. Column (3) displays results for long-term changes from 2004 to 2012. It shows that the local displacement effects for employment do not persist after the programme is abolished. Over the 8 year period the treated LSOA grew no faster than the untreated control areas.

To statistically test for displacement, we re-estimate the same specification, but with the 2km control ring as the omitted category. Figure 6 provides a graphical presentation of the results for employment which is the only outcome with statistically significant coefficients within 4km off the boundary (corresponding estimates and standard errors for all outcomes are reported in appendix table B1). When looking at a wide range of rings close to the LEGI boundary, the figure highlights the fact that compared to 2km control ring, employment grew the fastest in the 1km treated ring just inside the LEGI area. Moreover, it reveals dramatic, statistically significant divergence in employment trends at the boundary: employment growth was around 15 percentage points slower among treated LSOA 1km outside the treatment area as compared to treatment LSOA that are 1km inside it while we detect no significant differentials between the 2km control ring and any other treatment or control ring within 9km of the boundary at conventional confidence levels. In sum, the results suggest that LEGI induced worse employment growth in untreated LSOA 1km from the boundary compared to all other LSOA in both the nearby control area and treatment area. They also suggest that 1km treatment ring improved employment the most compared to rings across a wide spatial range. Taken together, these results suggest displacement of employment from the 1km control ring with the largest spillovers within 1km of the LEGI boundary.

Finally, going back to table 4, in contrast to these findings for employment, we are unable to detect any statistically significant displacement effects for number of business (columns (5) and (6)) and unemployment (column (8) and (9)). Looking at the pre-trends of these variables in columns (4) and (7), only the 5 and 6km control rings for number of business and 10km control and 5km plus treatment rings for unemployment show weakly significant coefficients, indicating, again, similar LSOA trends across a wide range of rings. For number of business, only the coefficient for the 8km control ring is significant for the 2004-2012 change. For unemployment, only the coefficient for the 10km control ring is weakly significant for the 2004-2012 change.

Overall, these findings indicate that LEGI funding resulted in the displacement of employment from untreated to treated areas. The pattern of point estimates also suggests that the displacement seems to be stronger the closer an untreated area is to the border of a LEGI area. Furthermore, we detect no impact on number of local units or unemployment when we compare outcomes for treated LSOA to neighbouring LSOA (controlling for observable characteristics). Taken at face value, the positive effect for employment combined with the insignificant effect for number of local units suggests that the displacement effect on employment we identified in table 4 is being created in existing rather than new firms. This is consistent with the fact that, in the UK, commercial rental contracts typically cover a five-year fixed term which implies sticky price adjustment and incomplete capitalization in the short-run for incumbent businesses (Crosby et al, 2003). As a result, an economic intervention supporting local businesses need not generate higher entry rates, as businesses entering the local market have to sign new contracts with rental rates potentially inflated by the intervention. The finding of no effect on unemployment, which is based on the residential rather than work location of a worker, is easy to reconcile with our findings of positive impacts on employment, once we recognise that local labour markets almost certainly cross LEGI boundaries so that workers who take the new jobs in the LEGI-side of the boundary may easily be from neighbouring untreated areas. The results also indicate that the spatial scope of job displacement is too narrow to generate any statistically detectable differences in employment trends at a more aggregate level.[19] Finally, we find no evidence that the effects on employment persist long beyond the abolition of the programme in March 2011. This suggests that LEGI appears to have created temporary employment displacement around the treatment boundary, but have induced no long run divergence in employment growth.

Results Based on the Minimum IMD Rank Rule

The variation exploited to estimate differences between treatment and control rings comes from between LA differences in LEGI funding. Although we compare rings within a relatively small area, the control and treatment rings are administrated by different LAs which may raise the concern that LAs which won a bid for LEGI funding may have been successful in attracting funding for other local development projects as well, or that they may have been more efficient in administrating their area in general. If this was the case, comparisons across the LA boundary may be confounded.

We address the potential concern that more efficient LAs received treatment with the RD approach described in section 4. Given that the estimates in figure 6 suggest very local displacement of employment within 1km of the treatment area boundary, we calculate RD estimates for two samples: (i) a 1km boundary sample containing treated and untreated LSOA within 1km of the LEGI border and (ii) a sample containing LSOA within 2 and 3km treatment and control rings. We implement estimations with this pairwise approach because the minimum IMD rank rule has only one cut-off point (at 50) and thus it only allows us to estimate one treatment dummy coefficient at a time.[20] We allow for first and second order polynomials of the minimum IMD rank and restrict the sample to a band around the cut-off. The width of the band is constrained by the limited number of observations in the subsamples. We use a band running from 5 to 145 as it drops the most deprived LAs from the sample, is symmetric around the cut-off (it includes all observations within 30 percentile points of the cut-off), and retains enough observations to achieve statistical precision. To avoid overfitting with limited number of observations, we restrict estimation to models imposing equivalent functions of the running variable on both sides of the cut-off (that is, the coefficients on

[19] The 1km treatment ring accounted for around 3.1% of total employment in LEGI areas in 2004.

[20] Alternatively, we could interact all right-hand-side variables with a subsample dummy indicating each relevant ring pair, but this is equivalent to estimating the model separately for each subsample.

the first and second order terms of the minimum IMD rank are unchanged when crossing the cut-off).[21]

Table 5 displays IV estimates of the impact of LEGI on employment, net business creation, and unemployment for the different time periods labelled by the row panel titles. The IV specification uses a binary indicator for LSOA at or to the left of the cut-off of 50 as an instrument for LEGI treatment. All first-stage discontinuity estimates on the binary treatment indicator are highly significant and close to one (ranging from 0.894 to 1.170). Therefore, they and reduced form estimates are omitted for brevity. The first four columns show estimates based on a sample of treatment and control LSOA located within 0-1km from the treatment area boundary while the last four columns show estimates for LSOA within 1-3km from the boundary. Results for a specification using a first order polynomial of the minimum IMD rank are displayed in columns 1, 2, 5, and 6, while columns 3, 4, 7, and 8 show results for a second order polynomial specification. In each even numbered column we control for the log level of the outcome in the baseline year in order to reduce sampling variation and improve the precision of the estimation (e.g. Lee and Lemieux 2010).

The 2002-04 estimates suggest no evidence of differences in pre-treatment trends in terms of employment, number of businesses or unemployment. Looking at the effects up until the third full programme year 2009, consistent with figure 6, there is a robust and highly significant divergence for employment between the 0-1km control and treatment zones, and no statistically significant difference between the 1-3km treatment and control zones. The coefficients for the 0-1km treatment and control zones (between 0.358 and 0.425) are larger than the corresponding estimate in table 4 (0.152), but the latter estimate is based on the full sample, and when the difference-in-difference estimation is restricted to a sample with the minimum IMD rank between 5 and 145, the difference is slightly smaller.[22] Looking at the longer term effects up until 2012 (after the programme was abolished), the estimates for employment are insignificant and considerably smaller than for the 2004-2009 changes across all specifications. Overall, these estimates suggest, again, displacement of employment at the treatment boundary, but these effects diminish quickly after the programme is abolished in March 2011.

[21] See Gelman and Imbens (2014) for a thorough discussion of unreliability of RD estimates in over-fitted settings.

[22] The corresponding difference-in-difference estimate (standard error) for the restricted sample is 0.188 (0.051).

The coefficients for the 2004-2009 change in number of business are smaller than the corresponding estimates for employment and have lower statistical significance, although estimates based on the second order specification are significant at the 5% level (columns 3 and 4). For the 1-3km zone, none of the estimates is significant. Estimates for number of business are insignificant for the longer time period 2004-2012, except one weakly significant coefficient in column 2. Finally, the results for unemployment indicate no significant differences in either 0-1km or 1-3km zones.

Overall, we conclude that the RD results are consistent with the pattern of estimates reported in figure 6 which suggests displacement of employment at the treatment boundary and little, if any, persistence of effects after the programme is cancelled.

6 Summary and Conclusions

This paper evaluates the impact of a large-scale area-based intervention that aimed to improve employment and productivity in the most deprived areas of the UK. We assess the causal impacts of the policy by exploiting high-resolution spatial data on employment, number of businesses and unemployment. We show that treated locations within 1km of the treatment area boundary performed substantially better in terms of employment when compared to control locations within 1km of the boundary. However, this difference vanishes quickly when moving away from the boundary: we find no statistically distinguishable effects when comparing control locations 1-3km away from the boundary to treatment locations 1-3km away from the boundary. Moreover, we show that the local displacement effects diminish quickly after the programme is abolished. A regression discontinuity analysis based on a deprivation rank rule that determined eligibility to bid for program funding confirms the robustness of our difference-in-difference estimates to potential biases that may arise from the selection of treatment areas.

Our analysis suggests weaker, if any, impacts on the number of businesses. This and our finding of significant local displacement of employment suggest that employment increased mainly at the intensive margin. We argue that this is in line with the fact that, in the UK, commercial rental contracts typically cover a five-year fixed term (Crosby et al, 2003) and incumbent companies with existing contracts gain competitive advantage from the intervention if entrants face higher rental rates. The results also suggest that unemployment, which is based on residential rather than work location, was unaffected consistent with the fact that the scale of the local labour market is larger than the narrow spatial range over which employment displacement occurs.

Our results indicate that improvements in employment in the treatment area are offset by worsening employment in nearby untreated areas with no overall effect on unemployment. They also suggest that the intervention was unlikely to have any positive impact on the treated economy in the long run. If the ultimate objective was to improve economic outcomes for residents then this represents a costly policy failure.

The findings of the study are in line with the concern among economists that the direct impacts of economic interventions on a target population may well be attenuated by general equilibrium effects. Although we are unable to identify general equilibrium effects at a wider spatial scale (national or global, for example), our study makes an important contribution to the debate by showing that such effects seem to be at play at small spatial scales, within local markets. Moreover, our findings support the view that even relatively large place-based interventions may struggle to shift the economy into a new spatial equilibrium (so that effects persist post-funding). The findings are also important for future research as they imply that control groups constructed from untreated locations near to treatment areas, may significantly confound treatment effect estimates. In the context of this study such an approach would lead to a large overstatement of employment effects of the policy and badly misleading recommendations for policy makers. Our approach utilising detailed spatial data that allows us to consider the trade-off between CIA and SUTVA suggests one approach for addressing this problem.

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Table 1: Descriptive Statistics by Treatment Status

	All			Treated			Controls		
	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.
Employment, 2004	684.14	2351.8	32476	606.57	2196.5	3791	694.39	2371.4	28685
Unemployment, 2004	21.57	19.51	32476	28.06	21.89	3791	20.72	19.01	28685
Number of businesses, 2004	64.9	217.15	32476	48.45	143.28	3791	67.07	225.02	28685
Employment Density, 2004	1310	4304	32476	1118	2456	3791	1336	4491	28685
Unemployment Density, 2004	75.87	130.15	32476	90.54	114.55	3791	73.94	131.96	28685
Business Density, 2004	130.97	312.61	32476	104.31	401.19	3791	134.5	298.77	28685
Employment, log change 2004-2009	0.039	0.447	32473	0.040	0.490	3791	0.038	0.441	28682
Unemployment, log change 2004-2009	0.534	0.444	31461	0.562	0.397	3758	0.531	0.45	27703
Number of businesses, log change 2004-2009	-0.002	0.238	32473	-0.005	0.27	3791	-0.002	0.234	28682
Employment, log change 2004-2012	0.084	0.500	32474	0.069	0.553	3791	0.086	0.493	28683
Unemployment, log change 2004-2012	0.656	0.507	31380	0 .745	0 .456	3758	0 644	0 .513	27622
Number of businesses, log change 2004-2012	0.085	0.284	32474	0.074	0.325	3791	0.086	0.278	28683
Employment, log change 2002-2004	0.066	0.386	32470	0.054	0.424	3791	0.067	0.381	28679
Unemployment, log change 2002-2004	-0.033	0.425	31087	-0.032	0.390	3744	-0.033	0.430	27343
Number of businesses, log change 2002-2004	0.047	0.184	32470	0.044	0.213	3791	0.047	0.180	28679
Acreage, LSOA	4.094	13.58	32477	1.566	5.717	3791	4.428	14.26	28686
Minimum IMD Rank 2004, LA (NRF Eligibility Rule)	81.42	60.99	32477	16.9	21.31	3791	89.95	59.40	28686
Population Characteristics:									
Economic Activity (% of working-aged pop.), 2001:									
Economically active	66.80	8.25	32477	62.76	9.02	3791	67.33	7.99	28686
Self-employed	8.265	3.972	32477	6.297	3.153	3791	8.525	3.996	28686
Young unemployed aged 16-24	24.88	10.39	32477	27.21	9.45	3791	24.57	10.47	28686
Long-Term Unemployed	27.86	10.89	32477	31.35	9.83	3791	27.40	10.94	28686
Full-time Students	2.525	1.653	32477	2.510	2.094	3791	2.527	1.585	28686
Population by Ethnic Background (fraction of total pop.), 2001									
White: Other White	0.027	0.038	32477	0.014	0.015	3791	0.028	0.039	28686
Black or Black British	0.023	0.057	32477	0.017	0.044	3791	0.024	0.059	28686
Asian or Asian British	0.045	0.106	32477	0.05	0.121	3791	0.044	0.103	28686
Chinese or Other Ethnic Group	0.009	0.014	32477	0.006	0.011	3791	0.009	0.014	28686
Mixed	0.013	0.013	32477	0.012	0.013	3791	0.013	0.013	28686
White: Irish	0.013	0.013	32477	0.010	0.010	3791	0.013	0.014	28686
White: British	0.871	0.176	32477	0.892	0.159	3791	0.868	0.177	28686

Notes: Authors own calculations. Data on employment and number of business come from the Business Structure Database provided by the Secure Data Archive at Essex. All other variables come from the Neighbourhood Statistics database provided by the ONS except acreage which is based on LSOA boundary files drawn from Edina's UK BORDERS portal. Variables are measured at the LSOA level. Aggregate employment rate is the ratio of aggregate employment to the sum of aggregate employment and unemployment.

Table 2: Descriptive Statistics by 1km-wide Control and Treatment Rings

	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.
	3km Control Ring			2km Control Ring			1km Control Ring		
Employment, 2004	519.68	912.41	638	507.66	1031.02	510	460.66	1080.92	189
Unemployment, 2004	26	16.8	638	27.64	19.96	510	28.44	21.15	189
Number of businesses, 2004	46.02	45.12	638	42.33	44.33	510	38.19	39.53	189
Employment Density, 2004	987	1567	638	957	1804	510	1017	1875	189
Unemployment Density, 2004	100.78	125.52	638	105.92	130.01	510	120.14	128.31	189
Business Density, 2004	111.07	138.19	638	109.59	145.61	510	107.81	101.95	189
Employment, log change 2004-2009	0.032	0.54	638	0.055	0.454	510	-0.040	0.659	189
Unemployment, log change 2004-2009	0.522	0.398	631	0.498	0.399	503	0.505	0.401	188
Number of businesses, log change 2004-2009	0.011	0.252	638	0.029	0.261	510	0.021	0.263	189
Employment, log change 2004-2012	0.091	0.594	638	0.090	0.547	510	0.127	0.651	189
Unemployment, log change 2004-2012	0.683	0.452	631	0.683	0.463	505	0.624	0.416	189
Number of businesses, log change 2004-2012	0.133	0.304	638	0.161	0.335	510	0.187	0.339	189
Employment, log change 2002-2004	0.044	0.402	638	0.026	0.441	510	0.025	0.452	189
Unemployment, log change 2002-2004	-0.040	0.398	628	-0.006	0.362	500	0.014	0.399	185
Number of businesses, log change 2002-2004	0.039	0.207	638	0.033	0.199	510	0.023	0.213	189
	3km Treatment Ring			2km Treatment Ring			1km Treatment Ring		
Employment, 2004	570.77	1570.50	596	671.54	2303.37	505	391.51	633.01	183
Unemployment, 2004	27.56	21.26	596	28.14	18.91	505	29.67	16.47	183
Number of businesses, 2004	42.01	58.48	596	49.00	118.80	505	35.28	36.26	183
Employment Density, 2004	1149	3087	596	1092	2565	505	1045	1597	183
Unemployment Density, 2004	87.42	99.18	596	91.07	106.44	505	118.34	120.56	183
Business Density, 2004	94.38	124.72	596	97.58	140.03	505	113.00	136.65	183
Employment, log change 2004-2009	0.040	0.470	596	0.015	0.464	505	0.106	0.550	183
Unemployment, log change 2004-2009	0.539	0.409	591	0.566	0.378	502	0.519	0.314	182
Number of businesses, log change 2004-2009	0.017	0.261	596	0.003	0.262	505	0.042	0.289	183
Employment, log change 2004-2012	0.076	0.599	596	0.070	0.519	505	0.163	0.594	183
Unemployment, log change 2004-2012	0.739	0.465	590	0.740	0.443	498	0.650	0.340	183
Number of businesses, log change 2004-2012	0.088	0.345	596	0.126	0.336	505	0.230	0.398	183
Employment, log change 2002-2004	0.089	0.398	596	0.063	0.417	505	0.035	0.43	183
Unemployment, log change 2002-2004	-0.040	0.388	588	-0.030	0.362	501	-0.014	0.296	182
Number of businesses, log change 2002-2004	0.055	0.221	596	0.048	0.235	505	0.009	0.242	183

Notes: Authors own calculations. Data on employment and number of business come from the Business Structure Database provided by the Secure Data Archive at Essex. All other variables come from the Neighbourhood Statistics database provided by the ONS except acreage which is based on LSOA boundary files drawn from Edina's UK BORDERS portal. Variables are measured at the LSOA level.

Table 3: Difference-in-Difference Estimates, All Treated vs. All Controls

	(1)	(2)	(3)	(4)
A. Employment, log change 2002-04				
LEGI	-0.013	-0.001	-0.010	-0.010
	(0.017)	(0.018)	(0.018)	(0.022)
N	32470	32470	32470	20431
B. Employment, log change 2004-09				
LEGI	0.001	0.007	0.020	0.004
	(0.013)	(0.018)	(0.022)	(0.018)
N	32473	32473	32473	20432
C. Employment, log change 2004-12				
LEGI	-0.017	0.006	0.019	0.002
	(0.017)	(0.021)	(0.025)	(0.023)
N	32474	32474	32474	20433
D. Number Business, log change 2002-04				
LEGI	-0.003	-0.006	-0.010	-0.020*
	(0.006)	(0.006)	(0.009)	(0.011)
N	32470	32470	32470	20431
E. Number Business, log change 2004-09				
LEGI	-0.003	0.005	0.008	-0.001
	(0.009)	(0.010)	(0.012)	(0.015)
N	32473	32473	32473	20432
F. Number Business, log change 2004-12				
LEGI	-0.013	0.015	0.007	0.000
	(0.013)	(0.016)	(0.018)	(0.022)
N	32474	32474	32474	20433
G. Unemployment, log change 2002-04				
LEGI	-0.059***	0.021	0.027	-0.002
	(0.020)	(0.021)	(0.025)	(0.029)
N	31108	31108	31108	19977
H. Unemployment, log change 2004-09				
LEGI	-0.010	-0.040	-0.021	-0.012
	(0.033)	(0.041)	(0.048)	(0.060)
N	31472	31472	31472	20083
I. Unemployment, log change 2004-12				
LEGI	0.101***	0.028	0.016	0.036
	(0.038)	(0.042)	(0.051)	(0.058)
N	31380	31380	31380	20054
10km Treatment Neighbourhood Fixed Effects		Yes	Yes	Yes
Pre-determined Characteristics			Yes	Yes
Restricted Sample				Yes

Note: Panel labels refer to the outcome. Standard errors clustered at the level of 10km treatment neighbourhoods (program area plus 10km control buffer) in parentheses. Pre-determined characteristics are the log of LSOA acreage, minimum IMD rank, and variables listed under the labels "population characteristics" in table 1. These variables are included in the regressions reported in columns (3) and (4) (coefficients are not shown). Data on employment and number of business come from the Business Structure Database provided by the Secure Data Archive at Essex; All other variables come from the Neighbourhood Statistics database provided by the ONS except acreage which is based on LSOA boundary files drawn from Edina's UK BORDERS portal. Variables are measured at the LSOA level. *, **, *** denote significance at 10%, 5% and 1% confidence levels respectively.

Table 4: Difference-in-Difference Estimates for Employment, Number Business and Unemployment 1km Treatment Rings vs. 1km Control Rings

	Employment			Number of business			Unemployment		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	2002-04	2004-09	2004-12	2002-04	2004-09	2004-12	2002-04	2004-09	2004-12
Treated, >5km	-0.016	0.123***	0.038	0.013	0.001	-0.022	0.055*	-0.086	-0.088
	(0.031)	(0.038)	(0.043)	(0.021)	(0.019)	(0.024)	(0.031)	(0.057)	(0.063)
Treated, 5km	0.008	0.083*	0.015	0.006	0.009	-0.016	0.030	-0.080	-0.041
	(0.034)	(0.044)	(0.045)	(0.022)	(0.019)	(0.022)	(0.026)	(0.058)	(0.059)
Treated, 4km	-0.016	0.133***	0.035	0.013	0.011	-0.023	0.006	-0.051	-0.039
	(0.020)	(0.038)	(0.040)	(0.022)	(0.022)	(0.022)	(0.025)	(0.052)	(0.064)
Treated, 3km	0.022	0.110***	0.030	0.014	0.023	-0.008	0.003	-0.046	-0.005
	(0.021)	(0.039)	(0.046)	(0.028)	(0.028)	(0.016)	(0.030)	(0.056)	(0.058)
Treated, 2km	0.012	0.071*	-0.006	0.012	-0.005	-0.006	0.021	-0.005	0.025
	(0.021)	(0.041)	(0.046)	(0.028)	(0.026)	(0.020)	(0.041)	(0.076)	(0.076)
Treated, 1km	-0.001	0.152***	0.051	-0.022	0.022	0.045	0.053	-0.035	0.005
	(0.036)	(0.040)	(0.068)	(0.025)	(0.029)	(0.045)	(0.043)	(0.086)	(0.052)
Control, 1km					*Reference Ring Category*				
Control, 2km	-0.017	0.103***	-0.011	0.005	0.015	0.008	0.009	0.003	-0.006
	(0.016)	(0.032)	(0.031)	(0.018)	(0.018)	(0.010)	(0.021)	(0.023)	(0.023)
Control, 3km	-0.007	0.083**	-0.001	0.010	-0.001	-0.012	-0.028	0.003	-0.029
	(0.031)	(0.037)	(0.032)	(0.019)	(0.018)	(0.021)	(0.020)	(0.036)	(0.039)
Control, 4km	-0.011	0.098***	0.007	0.017	0.004	-0.023	-0.028	-0.018	-0.042
	(0.021)	(0.031)	(0.025)	(0.019)	(0.016)	(0.018)	(0.026)	(0.044)	(0.048)
Control, 5km	0.020	0.059**	-0.010	0.031*	-0.012	-0.024	0.010	-0.049	-0.053
	(0.016)	(0.026)	(0.013)	(0.019)	(0.017)	(0.019)	(0.017)	(0.042)	(0.050)
Control, 6km	0.044**	0.108***	0.020	0.032*	-0.003	-0.029	0.018	-0.037	-0.072
	(0.019)	(0.028)	(0.025)	(0.019)	(0.011)	(0.024)	(0.017)	(0.046)	(0.058)
Control, 7km	0.029	0.061**	-0.012	0.027	-0.020	-0.035	-0.001	-0.043	-0.039
	(0.020)	(0.028)	(0.031)	(0.018)	(0.014)	(0.024)	(0.021)	(0.047)	(0.059)
Control, 8km	-0.000	0.067*	-0.032	0.017	-0.008	-0.038***	0.004	-0.040	-0.036
	(0.018)	(0.037)	(0.031)	(0.021)	(0.023)	(0.011)	(0.021)	(0.055)	(0.062)
Control, 9km	0.011	0.132***	0.046	0.026	0.010	-0.006	0.005	-0.083	-0.098
	(0.026)	(0.038)	(0.036)	(0.025)	(0.020)	(0.017)	(0.021)	(0.051)	(0.067)
Control, 10km	0.012	0.155***	0.098***	0.017	0.023	0.010	0.045*	-0.057	-0.103*
	(0.022)	(0.039)	(0.034)	(0.019)	(0.021)	(0.014)	(0.023)	(0.049)	(0.057)
N	32470	32473	32474	32470	32473	32474	31108	31472	31380

Note: In columns 1 to 3 outcome variable is log change in LSOA employment for time period indicated. Similarly for columns 4 to 6 (log change in LSOA number of business units) and for columns 7 and 8 (log change in LSOA unemployment). Variable labels refer to 1km-wide treatment and control rings. For example, the label "Control, 1km" refer to LSOA with distance 0-1000m from the nearest treatment LSOA. Distances between LSOA are based on LSOA centroids. Standard errors clustered at the level of 10km treatment neighbourhoods (program area plus 10km control buffer) are in parentheses. All specifications control for 10km treatment neighbourhood fixed effects and pre-determined characteristics, which are the log of LSOA acreage, minimum IMD rank, and variables listed under the label "population characteristics" in table 1 (coefficients are not shown). Data on employment and number of business come from the Business Structure Database provided by the Secure Data Archive at Essex; All other variables come from the Neighbourhood Statistics database provided by the ONS except acreage which is based on LSOA boundary files drawn from Edina's UK BORDERS portal. Variables are measured at the LSOA level. *, **, *** denote significance at 10%, 5% and 1% confidence levels respectively.

Table 5: IV Estimates for Employment, Number Business, and Unemployment

	0-1km Treated vs. 0-1km Untreated				1-3km Treated vs. 1-3km Untreated			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
					Employment			
A. 2002-04								
LEGI	-0.102	-0.104	-0.090	-0.090	0.082	0.080	0.085	0.084
	(0.067)	(0.067)	(0.065)	(0.065)	(0.068)	(0.067)	(0.066)	(0.065)
B. 2004-09								
LEGI	0.394***	0.358***	0.425***	0.392***	-0.036	-0.041	-0.028	-0.032
	(0.132)	(0.122)	(0.123)	(0.090)	(0.054)	(0.052)	(0.043)	(0.044)
C. 2004-12								
LEGI	0.111	0.073	0.151	0.116	-0.109	-0.108	-0.098	-0.096
	(0.153)	(0.124)	(0.146)	(0.086)	(0.091)	(0.095)	(0.092)	(0.093)
N	279	279	279	279	1877	1877	1877	1877
					Number Business			
D. 2002-04								
LEGI	-0.017	-0.027	-0.012	-0.023	0.010	0.009	0.006	0.006
	(0.044)	(0.025)	(0.047)	(0.027)	(0.034)	(0.033)	(0.028)	(0.028)
E. 2004-09								
LEGI	0.049	0.054*	0.075**	0.082***	-0.065	-0.058	-0.072	-0.065
	(0.040)	(0.029)	(0.033)	(0.030)	(0.041)	(0.040)	(0.046)	(0.042)
F. 2004-12								
LEGI	-0.181	-0.163*	-0.132	-0.110	-0.119	-0.105	-0.133	-0.116
	(0.127)	(0.099)	(0.113)	(0.100)	(0.104)	(0.096)	(0.117)	(0.103)
N	279	279	279	279	1877	1877	1877	1877
					Unemployment			
G. 2002-04								
LEGI	-0.129	-0.088	-0.112	-0.071	0.042	0.052	0.048	0.058
	(0.103)	(0.077)	(0.105)	(0.061)	(0.100)	(0.093)	(0.106)	(0.099)
H. 2004-09								
LEGI	-0.060	-0.065	-0.077	-0.067	-0.062	-0.075	-0.025	-0.034
	(0.097)	(0.096)	(0.118)	(0.104)	(0.158)	(0.143)	(0.094)	(0.083)
I. 2004-12								
LEGI	-0.087	-0.098	-0.098	-0.100	0.026	0.010	0.051	0.039
	(0.146)	(0.144)	(0.160)	(0.157)	(0.154)	(0.138)	(0.118)	(0.105)
N	261	261	261	261	1727	1727	1727	1727
Polynomial Order	1st	1st	2nd	2nd	1st	1st	2nd	2nd
Lagged Outcome Control	No	Yes	No	Yes	No	Yes	No	Yes

Notes: Fuzzy RD estimates using an indicator for observations left to the minimum IMD rank cut-off of 50 as an instrument for LEGI treatment. Standard errors are clustered by minimum IMD rank. Specifications in columns 1, 2, 5, and 6 (3, 4, 7, and 8) include first (second) order polynomial terms for the minimum IMD rank. First-stage coefficients on the instrument ranging from 0.821 to 1.252 are highly significant (not displayed). The outcomes are in log changes. Estimates in columns 1-4 (5-8) are based on a sample of treatment and control LSOA located within 0-1000m (1000-3000m) from the treatment area boundary. The sample is restricted to a minimum IMD rank band 5-145. Specifications in columns 2, 4, 6, and 8 control for the log-level of the outcome one year before the baseline (e.g. in the first row this is log of employment in 2001; in the second row it is log of employment in 2003). Data on employment and number of business come from the Business Structure Database provided by the Secure Data Archive at Essex; Data on unemployment come from the Neighbourhood Statistics database provided by the ONS. *, **, *** denote significance at 10%, 5% and 1% confidence levels, respectively.

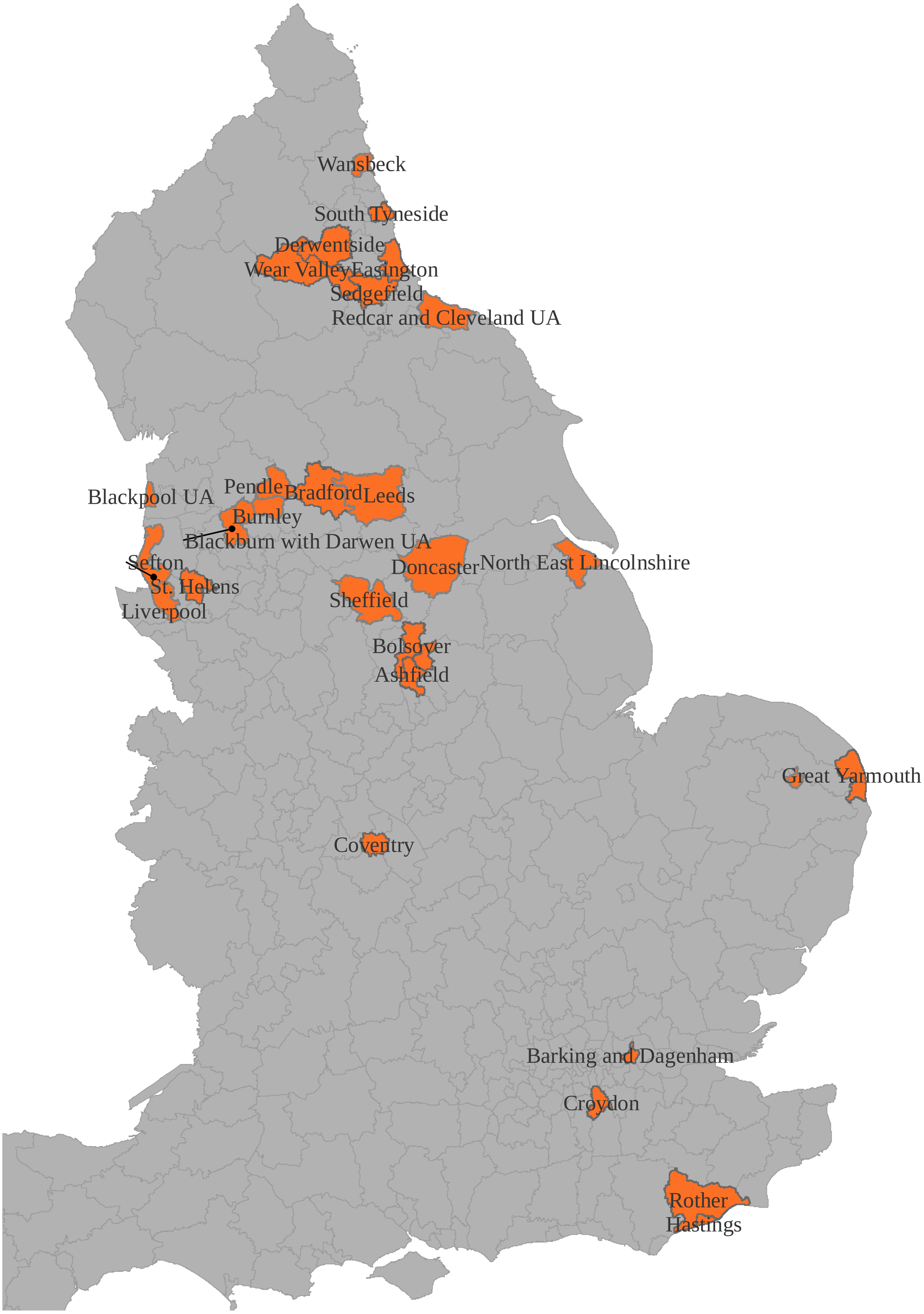
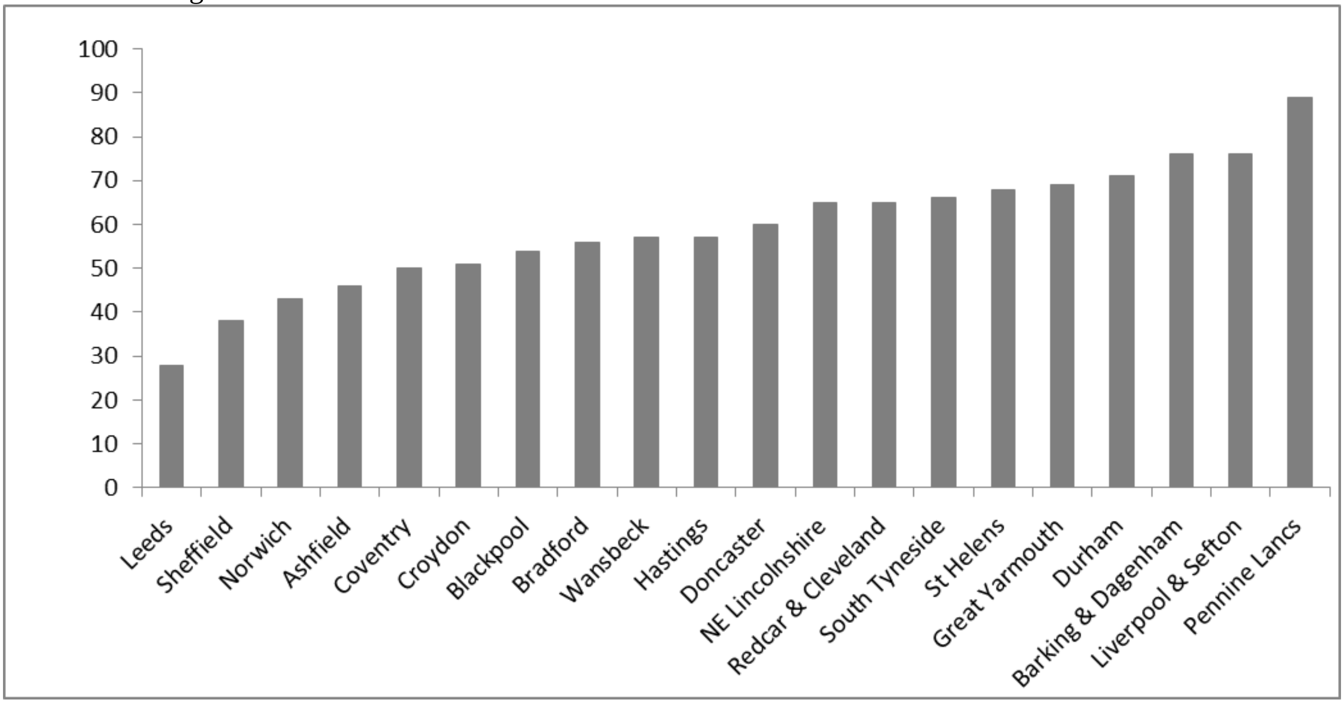


Figure 1: Map of Local Enterprise Growth Initiative Areas in England

A. Funding Share to Businesses



B. Funding Share to Residents

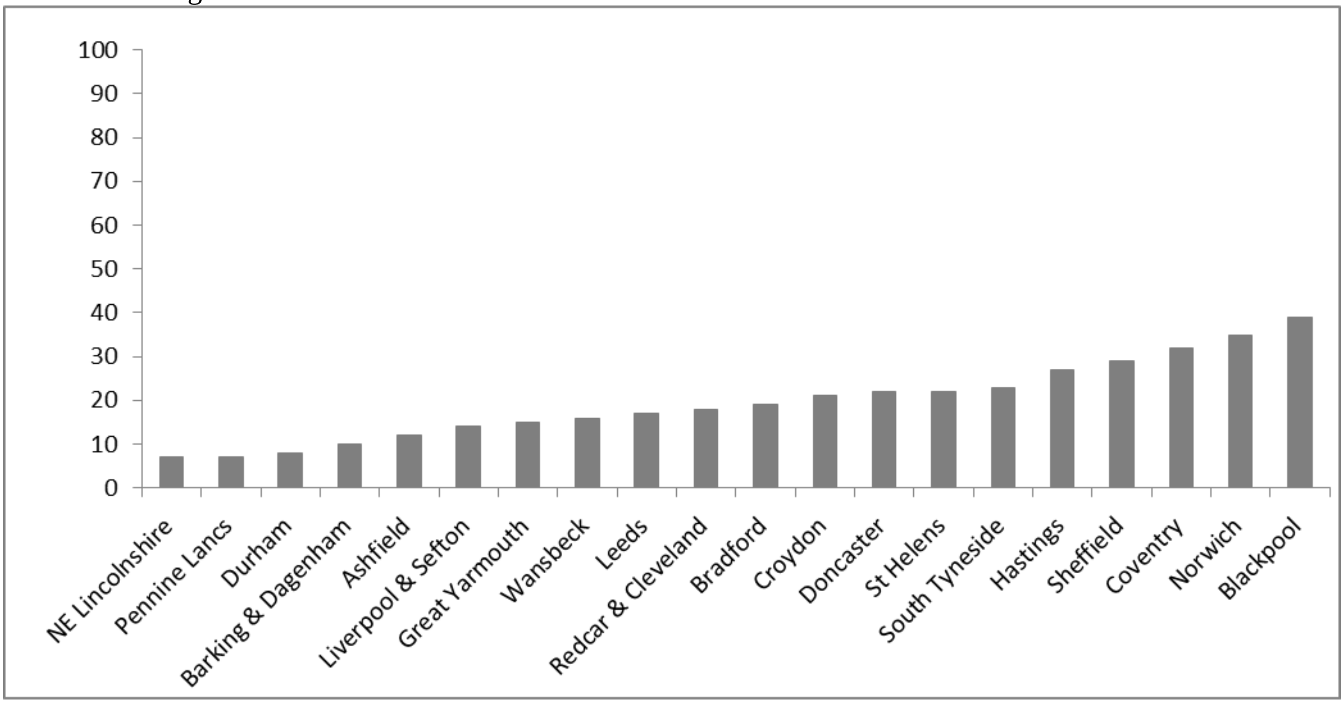


Figure 2: Distribution of LEGI area Funding Share to Businesses and Residents

Source: DCLG (2010b)

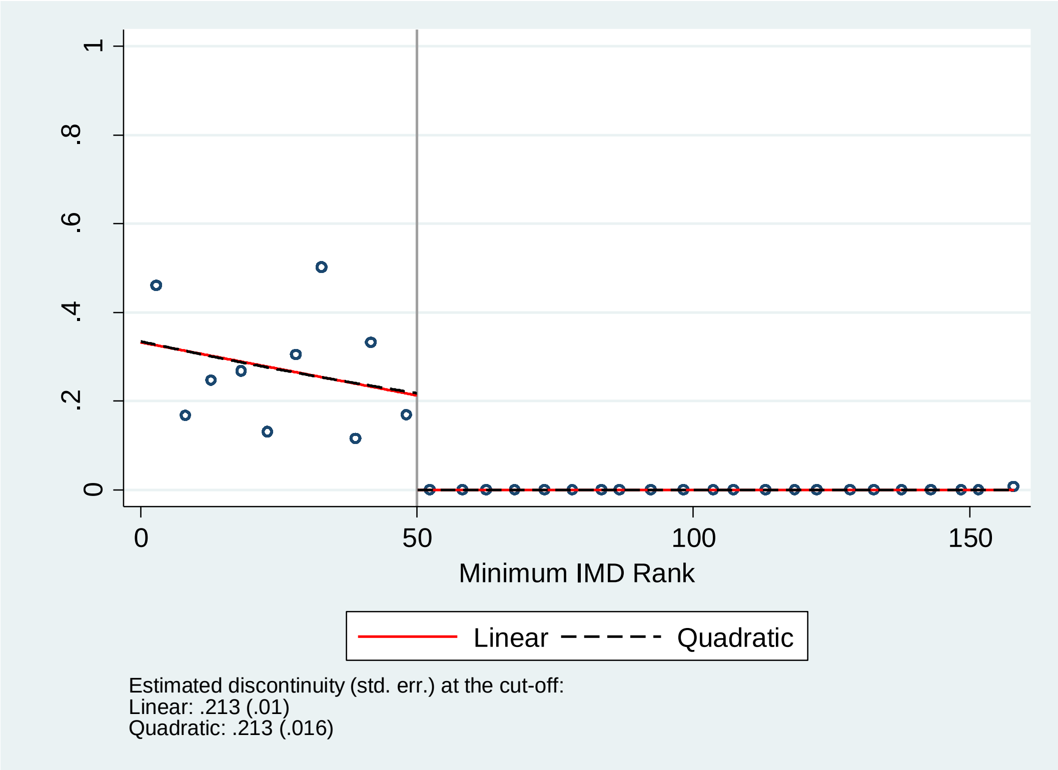


Figure 3: Discontinuity on LEGI at the Minimum IMD Rank Cut-Off

Notes: The figure depicts the share of treated LAs by minimum IMD rank bins. The bin width is five ranks. The lines are fitted regression functions from an LA-level regression of LEGI treatment status on a first and second order polynomials of the minimum IMD rank and a dummy indicator equal to one to the left of and at the eligibility cut-off of 50, and zero elsewhere. Data on minimum IMD rank are drawn from the Neighbourhood Statistics database provided by the ONS.

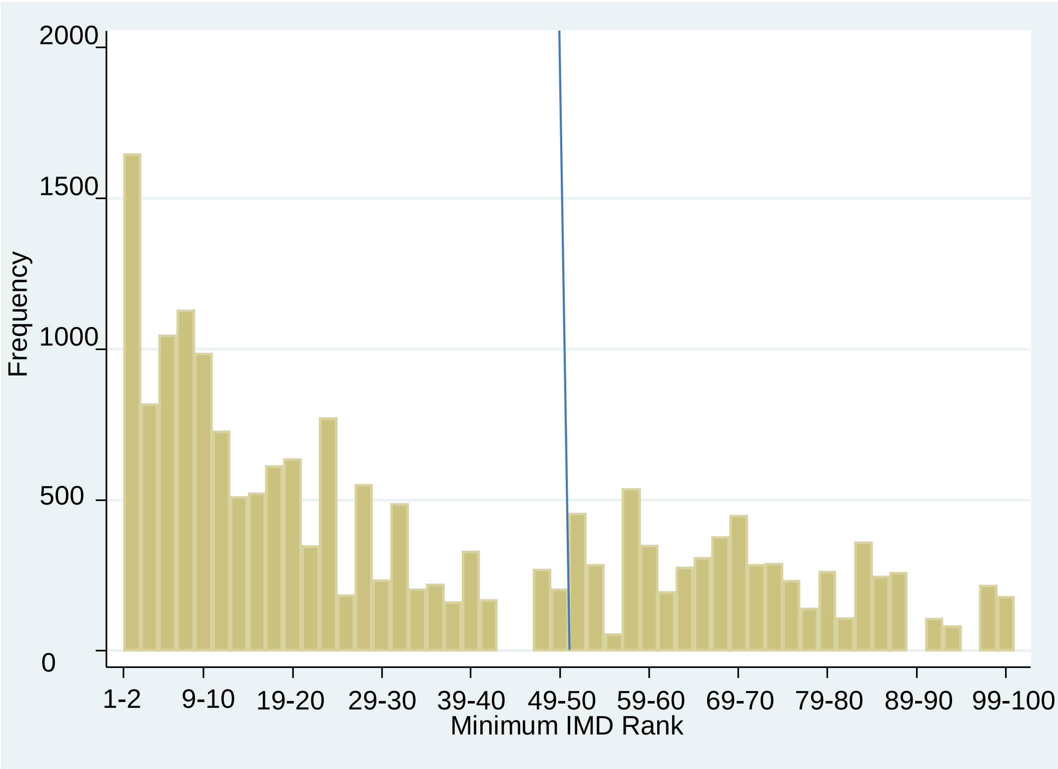


Figure 4: Histogram of the Minimum IMD Rank

Notes: The figure shows counts of LAs by minimum IMD rank. The bin width is two ranks. Data drawn from the Neighbourhood Statistics database provided by the ONS.

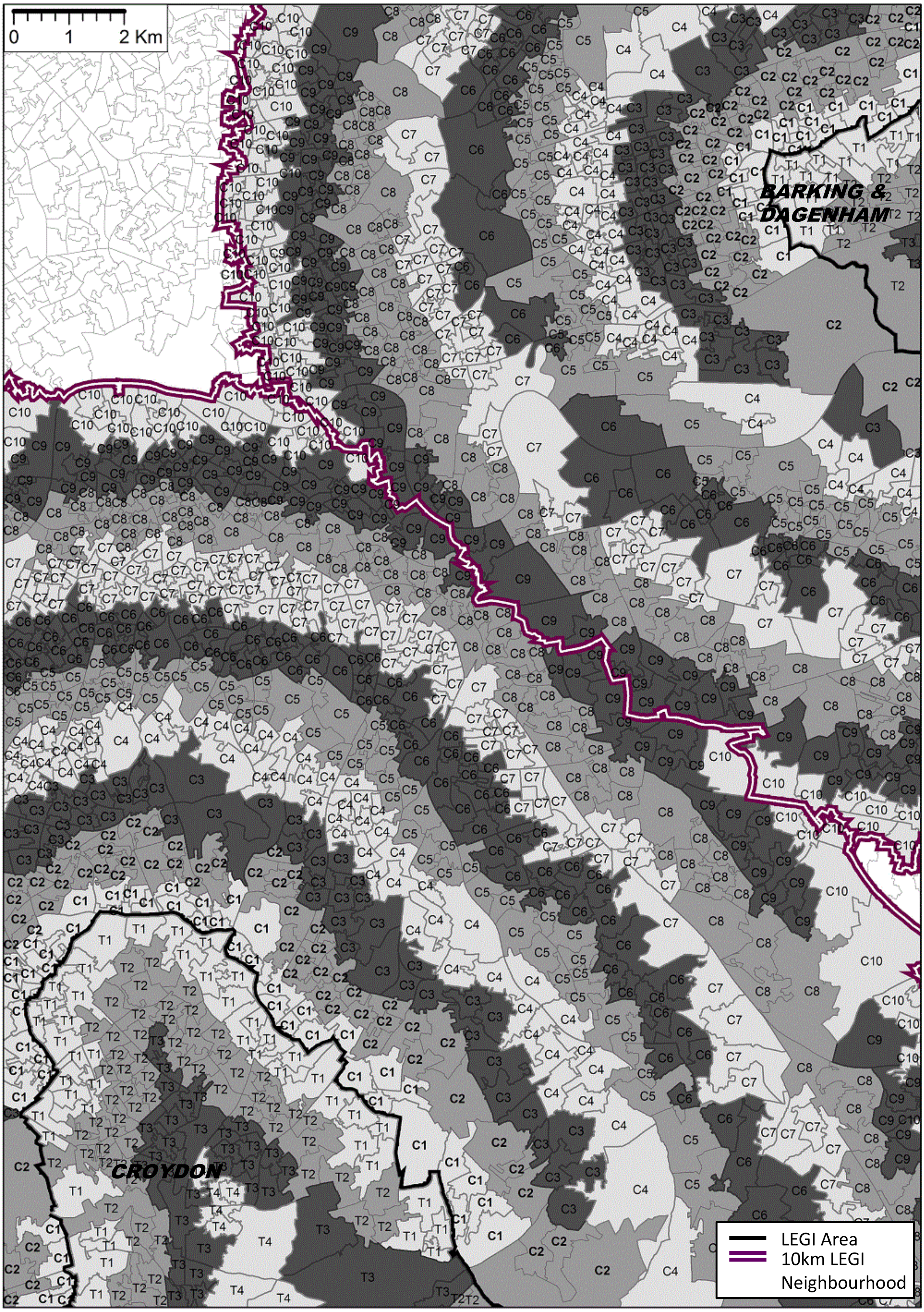


Figure 5: 1km-wide control and treatment rings (Croydon and Barking & Dagenham)

Notes: The figure depicts 1km-wide control and treatment LSOA rings in the Croydon and Barking and Dagenham areas. LSOA labels indicate the ring. For example, the 1km treatment ring is labelled “T1” and the 2km control ring is labelled “C2”. Distances based on LSOA centroids. For example, the centroid of a 1km control (treatment) ring LSOA is located in non-LEGI (LEGI) area and within 0-1000m from the nearest treatment (control) LSOA centroid. GIS data from Edina’s UK BORDERS portal.

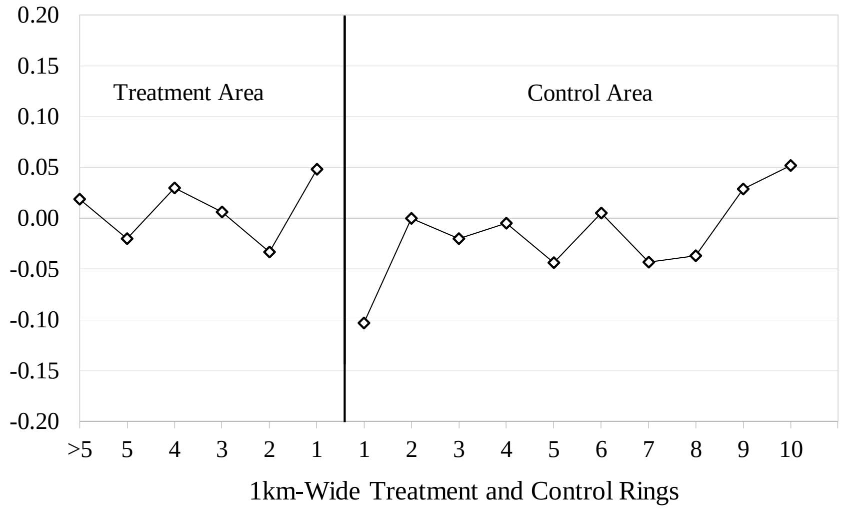


Figure 6: Displacement of Employment at the LEGI Boundary

Notes: Difference-in-difference estimates by 1km-wide control and treatment rings from column 2 of table 4. The outcome is the log change in employment from 2004 to 2009. The reference category is the 2km control ring. For further details, see notes in table 4. Corresponding estimates and standard errors are reported in column 1 of appendix table B1.

APPENDIX A: Data and Methodology for Calculating LA-level Deprivation Ranks

The LA-level indices on which eligibility for NRF and LEGI was based were constructed by the government as follows:[23]

1. Calculation of LSOA-level sub-indices for different deprivation domains. In 2000, the deprivation domains were i) income deprivation, ii) employment deprivation, iii) health deprivation and disability, iv) housing deprivation, v) education, skills, and training deprivation, and vi) geographical access to services. The 2004 sub-indices were calculated for domains i)-iv), vi), and two new domains on crime and living environment deprivation. The sub-indices were based on 32 variables in 2000 and on 37 variables in 2004. The set of underlying variables had some overlap between the years (9 exactly equivalent variables) but due to new domains introduced in the 2004 index and numerous changes within continuing domains a substantial number of variables are different in the 2000 and 2004 indices (the 2004 index introduced 11 variables not used in the 2000 index, while 3 variables were excluded). The 2000 sub-indices were mainly based on data from 1996 to 1999, although some variables dated back to the 1991 Census. The 2004 sub-indices were mainly based on data from 2001-2002 with the exception that some geographical accessibility measures – such as distance to the nearest post office – and crime measures, used data from 2003. In some domains the underlying variables are not measured in the same units. In such cases the sub-indices are formed by ranking and transforming to a normal distribution, or by factor analysis. Furthermore, before calculating the sub-indices, many variables were transformed with a "shrinking" procedure which shifted values towards the mean of the corresponding LA, with more shrinking for small LSOAs, on the average.

2. Calculation of a single LSOA Index of Multiple Deprivation (IMD). The sub-indices were combined by ranking the sub-indices and then transforming the ranks to an exponential distribution. The single LSOA index was calculated as a weighted average of the transformed sub-index ranks. The 2000 and 2004 indices used different weights. For example, the income and employment domains each had a combined weight of 25% in 2000, while they had a weight of 22.5% in 2004.

3. Calculation of LA-level IMDs. Six LA-level IMDs were used:
    a. *Degree* was the population weighted average of the LSOA IMD within an LA
    b. *Extent* was the proportion of a district's population living in LSOA which rank within the most deprived 10 percent in the country.
    c. *Intensity* was the average of LSOA IMD within the most deprived LSOAs in the LA that contained 10 percent of the LA's population
    d. *Income Scale* was the sum of LSOA sub-index in the Income deprivation domain (number of people experiencing "income deprivation" in the LA)
    e. *Work Scale* was the sum of LSOA-level sub-index in the employment deprivation domain (number of people experiencing "employment deprivation" in the LA)

    In 2004, a sixth LA-level IMD was introduced. It was the population weighted average of the LSOA IMD rank. Eligibility for NRF, and therefore for LEGI, was determined by ranking LAs by each of the 2000 and 2004 LA-level IMDs. An LA that ranked among the 50 worst LAs along at least one IMD was eligible for LEGI funding.

[23] This appendix is based on DETR (2000) and DCLG (2006).

APPENDIX B: Additional Tables

Table B1: Difference-in-Difference Estimates for 1km-Wide Rings Comparisons to 2km Control Ring (2004-09)

	(1) Employment	(2) Number Business	(3) Unemployment
Treated, >5km	0.019	-0.013	-0.090**
	(0.029)	(0.012)	(0.042)
Treated, 5km	-0.020	-0.006	-0.084*
	(0.041)	(0.014)	(0.045)
Treated, 4km	0.030	-0.004	-0.054
	(0.029)	(0.018)	(0.041)
Treated, 3km	0.006	0.009	-0.049
	(0.028)	(0.018)	(0.046)
Treated, 2km	-0.033	-0.020	-0.008
	(0.035)	(0.018)	(0.064)
Treated, 1km	0.048	0.007	-0.039
	(0.040)	(0.025)	(0.075)
Control, 1km	-0.103***	-0.015	-0.003
	(0.032)	(0.018)	(0.023)
	2km Control Ring Omitted		
Control, 3km	-0.020	-0.016	0.000
	(0.027)	(0.014)	(0.026)
Control, 4km	-0.005	-0.011	-0.021
	(0.016)	(0.007)	(0.035)
Control, 5km	-0.044*	-0.026***	-0.053*
	(0.024)	(0.009)	(0.030)
Control, 6km	0.005	-0.018	-0.041
	(0.026)	(0.015)	(0.032)
Control, 7km	-0.043*	-0.035**	-0.046
	(0.022)	(0.015)	(0.035)
Control, 8km	-0.037	-0.023*	-0.043
	(0.024)	(0.012)	(0.041)
Control, 9km	0.029	-0.004	-0.086**
	(0.024)	(0.011)	(0.034)
Control, 10km	0.052**	0.009	-0.060*
	(0.025)	(0.012)	(0.032)
Intercept	0.003	0.337	-1.319***
	(0.434)	(0.269)	(0.397)
N	32473	32473	31472
10km Treat. Neigh. FEs	Yes	Yes	Yes
Pre-Determined Controls	Yes	Yes	Yes

Notes: See notes in table 4. Estimates in column (1) are plotted in figure 6.

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