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The effects of supporting local business: Evidence from the UK☆

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ABSTRACT

This paper assesses the effects of a significant place-based intervention that targeted local businesses in deprived areas in the UK. To gain identification, we use data at a fine spatial scale and a regression discontinuity design exploiting the eligibility deprivation rank rule based on a pre-determined deprivation index. We detect no overall effects on employment in treated areas but find a significant displacement of employment from nearby untreated areas, corresponding to around 10% of local employment. The results suggest that indirect displacement effects may substantially weaken the ability of local support programmes targeting the non-tradable sector to reduce economic inequality.

1. Introduction

Many governments target significant amounts of public spending at areas experiencing poor economic performance. In the US, around $95 billion is spent annually on spatially targeted economic development programs by the federal and state governments (Kline and Moretti, 2014a), and place-based interventions account for around 35 percent of total spending by the EU (EU, 2013). Despite the prevalence of place-based policies, a common concern among economists is that they may shift economic activity from one place to another. Such displacement effects would reduce aggregate net benefits of these interventions.[1] Moreover, because deprivation is often spatially clustered, the nearby areas that may be negatively affected by displacement are often also deprived. Therefore, local displacement effects might also limit the extent to which these policies reduce economic inequality if they merely shift economic activity between deprived areas.

In this study, we assess the impacts of a significant area-based intervention that targeted local businesses in 30 economically deprived areas in England. We start by estimating treatment effects in a standard difference-in-difference framework, comparing treated areas to nearby untreated areas and conditioning on area characteristics and fixed effects. In order to strengthen the internal validity of our results, we also employ a regression discontinuity (RD) design based on a discontinuity in treatment at the eligibility cutoff, which was determined by a pre-determined index of local deprivation. Neither of these approaches detects statistically significant treatment effects on employment, number of local business, or unemployment in treated areas.[2]

Motivated by the fact that most programme support was targeted to local businesses in the non-tradable sector (e.g. restaurants, hairdressers, and retail stores), we continue by assessing the potential displacement of

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[1] For a discussion of the efficiency of spatially-targeted economic interventions, see e.g. Greenstone and Looney (2010) and Kline and Moretti (2013, 2014a).

[2] We show that pre-policy outcomes are balanced between treatment and control groups, there is no bunching at the RD threshold, and that our results are robust to a battery of RD validity checks.

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economic activity from nearby unsupported areas to the treated areas by examining changes in outcomes for 1-km-wide zones. Our test of displacement uses untreated areas that are further from the treatment boundary as a counterfactual for untreated and treated areas at the boundary. This approach identifies displacement effects if indirect effects on the untreated weaken sufficiently quickly as the distance between them and treated locations increases. This condition is likely to hold in the case of interventions that target local non-tradable markets, which typically have limited spatial scope.

Using both the difference-in-difference and RD estimators, we detect significant displacement of employment from unsupported to supported areas. The results indicate that the spatial scope of displacement, and hence the affected local market, is around 2 km wide (1 km on either side of the treatment area boundary). We estimate the magnitude of the local displacement to be around 10% of local employment. While we cannot completely rule out the possibility that control areas further away from the boundary may also be affected by displacement, such effects would bias our estimates downwards, and therefore our analysis provides a lower bound for the displacement effect. We also show that the displacement effects are persistent through the policy-on years and that they disappear after the programme is abolished, suggesting that support for local non-tradable sector is unlikely to have persistent effects on local economies.

In contrast to the strong local displacement of employment, we find weaker evidence of displacement in terms of number of businesses. This finding is consistent with inelastic business premise supply in dense areas, where little free land for new development is available. Finally, we find no effects on unemployment, which is based on residential location. This is consistent with the idea that local labor markets cross programme boundaries and with previous evidence indicating that jobs generated by area-based interventions are often taken by individuals who reside outside the treatment area (e.g., Busso et al., 2013; Freedman, 2015).

Much of previous evidence on the displacement effects of place-based policies concern local tax incentives. In this strand of research, studies using causal research designs find mixed results. For example, Ham et al. (2011) and Givord et al. (2013) find no evidence of employment displacement. Hanson and Rohlin (2013) find evidence of displacement of establishments and employment in unsupported nearby areas, while Hanson and Rohlin (2011), Harger and Ross (2016), and Mayer et al. (2017) provide evidence of industry churning and displacement of economic activity within supported areas. Studies examining the impacts of programmes promoting local investment and infrastructure have also found mixed evidence of displacement. Criscuolo et al. (2019) estimate the effects of a local investment subsidy programme in the UK targeting the manufacturing sector. They find no evidence of indirect effects on employment in nearby untreated areas. On the contrary, in their study on temporary regional transfers targeting local investment in Germany, Ehrlich and Seidel (2018) employ a spatial differencing strategy based on distance to the boundary of the eligible zone and find evidence of displacement of income and employment. Patrick (2016) shows that increasing capital subsidy availability is associated with both capital-labor substitution and changes in local industry mix.

We advance and complement research in this area in several ways. First, we examine the impacts of a programme that targets subsidies predominantly to the local *non-tradable* sector. Because local spillovers are expected to be large in this sector, assessing local displacement effects is especially relevant in the context of such programmes. Second, we provide quasi-experimental evidence of significant displacement of employment in programmes targeting the local non-tradable sector.[3] Third, our research design combines spatial differencing at boundaries of programme areas with an RD design based on pre-determined programme eligibility rule that does not depend on location. This might be important, because the spatial difference-in-difference strategy may not completely account for the potential unobserved factors that may discontinuously change when crossing administrative boundaries (e.g., Ham et al., 2011).

More generally, our study is linked to the literature examining the effects of area-based interventions. Studies examining local tax and block grant schemes with broad target scope in terms of sector and employee type have found positive impacts on employment (e.g., Ham et al., 2011; Busso et al., 2013).[4] Studies examining the impacts of programmes promoting local investment and infrastructure have found also positive effects (e.g., Criscuolo et al., 2019; Kline and Moretti, 2014b; Ehrlich and Seidel, 2018). Moreover, Kline and Moretti (2014b) and Ehrlich and Seidel (2018) find that the effects of investment and infrastructure support can last long beyond the programme period. Freedman (2012) finds modest positive impacts of investor tax incentives supporting investment in low-income communities. Our study complements research in this area by providing evidence of the impacts of a programme that provides public funding mainly for local service businesses. Our finding of substantial displacement of employment in the short-run and no aggregate or long-term effects combined with evidence of positive persistent impacts found in studies examining schemes which have wider sectoral scope and focus on investment highlights the importance of efficient targeting of place-based policies.

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 three key objectives of the Local Enterprise Growth Initiative (LEGI), introduced by the British government in 2006, were to i) increase entrepreneurial activity in deprived areas ii) support growth and reduce exit rates among local businesses in deprived areas, and iii) attract investment into deprived areas (DCLG, 2010a). The primary aim of LEGI funding was to increase resources and develop existing schemes supporting these objectives (HM Treasury, 2006).

91 deprived Local Authorities (LA) in England were eligible to bid for LEGI funding. These were areas that were receiving funds from an existing programme – Neighborhood 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.[5] In order to be eligible, a Local Authority had to rank 50th or worst against any of eleven indices of multiple deprivation (IMD) in 2000 or 2004. These indices were constructed by the central government using a complex three step procedure based on variables pre-dating the introduction of LEGI, some of which dated back as far as the 1991 Census (Appendix B provides details of the construction of these indices).

Selection of treatment areas from the pool of eligible Local Authorities was based on proposals detailing the way in which they planned to achieve the objectives of the initiative. To support their proposals, Local Authorities 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 funding covering three years was allocated in the first two competitive bidding rounds held

[3] For analysis of general equilibrium and spillover effects in the context of other policy interventions, see e.g. Blundell et al. (2004), Bloom et al. (2013), Crépon et al. (2013), and Dechezleprêtre et al. (2016).

[4] Busso et al. (2013) also estimate that the deadweight losses are modest.

[5] At the time, the NRF was the major funding stream used to try to tackle deprivation in England's poorest neighborhoods. 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).

in February and December 2006 with funding awarded to 20 areas comprising 30 Local Authorities displayed in appendix fig. B1 (some areas involved joint bids with the largest awarded joint initiative consisting of five Local Authorities) (DCLG, 2010a).

In some supported Local Authorities, support was further targeted to specific areas. While we have some self-reported information on this targeting (e.g. the poorest 10% of areas in Doncaster) we have no information on what factors drove it.[6] Nor do we know if LEGI funding relaxed budget constraints for supported Local Authorities, so that they could re-direct other funds to support non-targeted areas. Following Kline and Moretti (2014b), we focus on identifying the effects of the programme across all areas within supported Local Authorities, rather than on the set of targeted areas, to allow for endogenous policy response within the wider supported area. This allows us to identify the effects of LEGI funding on supported Local Authorities accounting for the potentially endogenous response of other local policies. This approach is further motivated by the fact that outside supported Local Authorities, no areas are targeted. Therefore, dropping untargeted areas in supported Local Authorities may cause selection bias because targeting was likely partly driven by unobserved characteristics.[7]

Projects under the scheme were expected to operate with a ten-year time horizon. In the end, the programme ran for six 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 around £418 ($717) million. Spending was at its highest level in 2008 and 2009 with around £100 ($171) million spent annually. With resident working age populations of around 1.4 million, 234,000 unemployed, and 85,000 businesses in 2006, per year allocation of funding was around £71 ($122) per capita, £427 ($732) per unemployed, and £1176 ($2016) per business in these years.[8] The magnitude of LEGI support is comparable to Federal expenditures on U.S. Enterprise Zones (annually around $142 per capita (Busso et al., 2013)), and it is slightly smaller than expenditures in the German Zonenrandgebiet scheme and in areas receiving the highest EU Structural Funds support (annually around 194–373 and 230 euros per capita, respectively (Becker et al., 2010; Ehrlich and Seidel, 2018)).

Due to the Local Authority specific design, the mix of support activities differed by area but the bulk of programme funding was targeted to local business. Across the programme about 60% of expenditure went on supporting existing and new local business (appendix fig. B2). Major support activities were business advice and direct financial support (grants and loans) which, according to a survey of beneficiary companies, was received by nearly all of them (DCLG, 2010b). Other, less extensive forms of business support included mentoring, coaching, workshops, and training. A very small fraction of companies (less than 3%) reported premises support.

The programme targeted local businesses and according to beneficiary data, around 80% of business support was allocated to the local service sector. Manufacturing and construction received 5.5 and 11.6% of support among start-ups and 10.5 and 8.3% among existing businesses, respectively. The recipient population covered a relatively high share of groups that are typically not reached by other business programmes – for example, around 34% of supported star-ups were by women and 32% by individuals from an ethnic minority (DCLG, 2010a).

[6] Based on self-reported information by programme managers, Local Authorities that had no specific targeting account for around 60% of the total number of spatial units in supported areas in our data.

[7] For instance, supported Local Authorities may have targeted areas that could be expected to benefit the most from the support. We provide a detailed discussion of the identifying assumptions of our difference-in-difference and regression discontinuity approaches below.

[8] Spending per *supported* individual and business were substantially higher, but exact data are unavailable. The figures exclude increases in private outside funding due to the programme support, which can be substantial (see, e.g., Busso et al., 2013).

3. Data

Our units of observation are small geographical areas used as the basis for the UK census. The UK Office for National Statistics calls these areas "Lower Layer Super Output Areas" (LSOA), but we refer to these as "micro-areas" hereafter.[9] Geocoding of micro-areas by treatment status is based on shape files provided by the UK Borders database. As discussed above, an area is considered as treated if it is located within the boundaries of a Local Authority receiving LEGI funding. Because micro-areas are constructed so as not to cross any Local Authority boundaries, the geocoding is exact.

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 (ONS, 2019) which provides an annual snapshot of the Inter-Departmental Business Register (IDBR). This dataset contains information on 2.1 million businesses, accounting for approximately 99% of economic activity in the UK and includes each business' name, postcode, and total employment. We use the Ordnance Survey Code-Point data set to match business postcodes to micro-areas and then construct our measures of employment and number of businesses by aggregating the BSD data by micro-area. One concern with using this source of data is that it does not include new businesses that are below the VAT threshold and do not voluntarily register. However, a large share of active companies below the VAT threshold do register voluntarily because that allows them to claim back the tax that they have paid on taxable supplies.[10] Hence, the data constructed from the IDBR can be expected to have a very good coverage of local employment, but may miss some of smallest companies with activity levels low enough not to benefit from VAT reimbursements.

Unemployment data measures the number of benefit claimants aged 16–64 and is available at micro-area level from the Neighborhood Statistics database maintained by the ONS.[11] This data has otherwise full coverage of claimants, with the exception that the ONS does not disclose information for micro-areas with less than five claimants. However, there are few such cases among micro-areas relevant for our analysis; information on unemployment is available for 99% of them.[12] We also have data for area characteristics measuring acreage, measures of deprivation and economic activity, and ethnic composition of residents by micro-area provided by Neighborhood Statistics.

[9] The Local Super Output Areas ('micro-areas') have a minimum population of 1000. The 32,482 areas 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).

[10] See, e.g., *"The Administrative Costs of Tax Compliance 2004"* by the House of Commons Treasury Committee.

[11] www.neighbourhood.statistics.gov.uk. Unemployment is measured as the count of Job Seeker Allowance claimants within a micro-area. 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 h per week on average, and living in Great Britain.

[12] Because our analysis focuses on disadvantaged areas with relatively high unemployment rates, the micro-areas relevant for us (that is, LEGI areas and 10 km control buffers around them) have better coverage of unemployment than the full sample, in which it is around 97%.

We evaluate the impact of LEGI using data for the period 2001–2012 covering four years before, and eight years after, the announcement of the programme in 2005. We use the first pre-announcement year 2004 as the baseline year. The first funding allocations were made in 2006 and the programme ran until March 2011. 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. Descriptive statistics for the data are presented in Table 1 and appendix table B1. The way in which these statistics are presented relates to our empirical strategies and so we postpone further discussion of these tables until we have outlined the details of our approach.

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 programme evaluation analysis, a common identifying assumption is that, in the absence of the programme, outcomes are, on average, identical for observably similar treated and untreated units (this is the conditional independence assumption or CIA). A second key identifying assumption is that the treatment does not affect outcomes of control units (this is the stable unit treatment value assumption or SUTVA). If these assumptions hold, average outcomes for untreated areas conditional on observable characteristics would provide an unbiased estimate of what would have happened to a treated unit in the absence of support and we could identify the treatment effect by estimating the following equation:

$$\Delta y_{it} = \alpha + \gamma L_{r(i)} + \beta X_i + \varepsilon_{it} \tag{1}$$

where $\Delta y_{it} = y_{it} - y_{i,2004}$ is a log change in the outcome of interest between the baseline year 2004 and year t in micro-area i; $L_{r(i)}$ is a binary indicator for LEGI, equal to one if the micro-area is within a Local Authority r, which was awarded LEGI funding, and zero otherwise; and X_i are observable pre-treatment area characteristics.

Equation (1) compares changes in the outcome between *all* treated and *all* untreated areas. 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 clusters of the treated and nearby untreated micro-areas, such as an area comprising the treatment area and a control buffer around it:

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

where Δy_{it} and $L_{r(i)}$ are as before and α_l are fixed effects for the clusters.

While the introduction of area fixed effects may help with the CIA it may introduce problems if nearby control areas are affected by treatment as a result of general equilibrium effects within local markets (i.e. if SUTVA is violated). For this reason, some previous studies estimating direct treatment effects have excluded nearest control areas to avoid biases arising from spillovers (e.g., Neumark and Kolko, 2010; Kline and Moretti, 2014b). For example, if businesses located in the control buffer compete with businesses located in the treatment area, the control area outcomes are likely to be affected by the treatment. The extent of such spillovers is important to understand for two reasons. First, if they are non-negligible, using nearby areas as a control group will provide biased estimates of treatment effects. Second, if treatment adversely affects nearby untreated locations through negative market spillovers, this may offset potential beneficial effects in the treatment area, reducing or eliminating net effects in the aggregate. In the worst case, net aggregate effects can even be negative if negative spillovers to nearby control areas offset benefits in the treatment areas and, for example, commuting costs increase or aggregate productivity declines.

4.1. Estimation of local displacement effects

Our test of displacement effects is based on difference-in-difference estimation testing for violations of SUTVA by using micro-areas that are further from the boundary, but still close enough to credibly satisfy the CIA, as a counterfactual. We divide treatment and control areas in to rings based on distance to the treatment area boundary. That is, we augment equation (2) with dummy variables for control and treatment rings that run parallel to the LEGI boundary (i.e. the boundary of the Local Authorities receiving support from the LEGI programme):

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

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 a control micro-area i is between $h-1$ and h kilometers of the treatment area boundary, and zero otherwise; and T_i^k are a set of six treatment ring dummies.[13] Treatment rings are constructed symmetrically for treatment micro-areas, although we pool all micro-areas that are further than 5 km from the nearest control micro-area because the number of observations quickly decreases when moving towards the centre of a LEGI area. More specifically, for $k \in \{1,\dots,5\}$, T_i^k take value one if the distance from a treatment micro-area i to the nearest control micro-area is between $k-1$ and k kilometers, while T_i^6 takes value one if the distance is more than 5 km, and zero otherwise. We define area fixed effects, α_l, at the level we call *LEGI neighborhoods*. A LEGI neighborhood comprises a treatment area and a 10 km-wide buffer area around it. 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 neighborhoods. Finally, although treatment status is invariant within Local Authorities outside LEGI neighborhoods, and we do not assign micro-areas within them to 1-km rings, we include observations and fixed effects for them to allow comparability across specifications and to improve the precision of the estimated coefficients on pre-treatment controls.[14]

Fig. 1 shows 1 km-wide treatment and control rings for the Croydon and Barking & Dagenham LEGI areas in London. The smallest spatial units in the figure are the micro-areas. Estimation based on equation (1), excluding the LEGI neighborhood dummies, compares average performance of LEGI micro-areas to that of all untreated micro-areas, while estimation of equation (2) including fixed effects compares average performance of treated and untreated micro-areas *within* LEGI neighborhoods. 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 treatment area boundary and to that of the treatment rings, within a given LEGI neighborhood. The difference in the average (conditional) growth rate for the outcome of interest between treatment ring k and control ring h is $\theta_k - \xi_h$ in equation (3). This

[13] Technically we use the distance from a control (treatment) micro-area centroid to the nearest treatment (control) micro-area centroid to assign them to control (treatment) rings. In contrast to defining the distance based on the LEGI boundary line, this has the advantage of ensuring that the average distance between locations in, say, a treated micro-area in the 1 km treatment ring and an untreated micro-area in the 1 km control ring is approximately 1 km. Gibbons et al. (2011) use a related approach to study the impact of government subsidized improvements to commercial building supply.

[14] We also present results for FE specifications excluding continuous control variables.

Table 1
Descriptive statistics by 1 km-wide control and treatment rings.

	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.	Diff.	t-stat
	1 km Treatment Ring			1 km Control Ring				
Employment, 2004	391.51	633.01	183	460.66	1080.92	189	−69.148	0.62
Unemployment, 2004	29.67	16.47	183	28.44	21.15	189	1.233	0.44
Number of businesses, 2004	35.28	36.26	183	38.19	39.53	189	−2.912	0.48
Employment Density, 2004	1045	1597	183	1017	1875	189	27.528	0.13
Unemployment Density, 2004	118.34	120.56	183	120.14	128.31	189	−1.794	0.12
Business Density, 2004	113.00	136.65	183	107.81	101.95	189	5.195	0.39
Employment, log change 2001–2004	0.035	0.43	183	0.025	0.452	189	0.010	1.26
Unemployment, log change 2001–2004	−0.014	0.296	182	0.014	0.399	185	−0.028	1.14
Number of businesses, log change 2001–2004	0.009	0.242	183	0.023	0.213	189	−0.014	0.65
Employment, log change 2004–2009	0.106	0.550	183	−0.040	0.659	189	0.146	3.75
Unemployment, log change 2004–2009	0.519	0.314	182	0.505	0.401	188	0.014	0.32
Number of businesses, log change 2004–2009	0.042	0.289	183	0.021	0.263	189	0.021	0.71
Employment, log change 2004–2012	0.163	0.594	183	0.127	0.651	189	0.036	0.57
Unemployment, log change 2004–2012	0.650	0.340	183	0.624	0.416	189	0.026	0.43
Number of businesses, log change 2004–2012	0.230	0.398	183	0.187	0.339	189	0.043	1.08
	2 km Treatment Ring			2 km Control Ring				
Employment, 2004	671.54	2303.37	505	507.66	1031.02	510	163.884	1.43
Unemployment, 2004	28.14	18.91	505	27.64	19.96	510	0.501	0.36
Number of businesses, 2004	49.00	118.80	505	42.33	44.33	510	6.675	1.02
Employment Density, 2004	1092	2565	505	957	1804	510	134.450	0.49
Unemployment Density, 2004	91.07	106.44	505	105.92	130.01	510	−14.849	1.15
Business Density, 2004	97.58	140.03	505	109.59	145.61	510	−12.016	0.53
Employment, log change 2001–2004	0.063	0.417	505	0.026	0.441	510	0.037	1.94
Unemployment, log change 2001–2004	−0.030	0.362	501	−0.006	0.362	500	−0.023	0.76
Number of businesses, log change 2001–2004	0.048	0.235	505	0.033	0.199	510	0.015	1.01
Employment, log change 2004–2009	0.015	0.464	505	0.055	0.454	510	−0.040	1.10
Unemployment, log change 2004–2009	0.566	0.378	502	0.498	0.399	503	0.068	2.50
Number of businesses, log change 2004–2009	0.003	0.262	505	0.029	0.261	510	−0.026	1.09
Employment, log change 2004–2012	0.070	0.519	505	0.090	0.547	510	−0.020	0.57
Unemployment, log change 2004–2012	0.740	0.443	498	0.683	0.463	505	0.057	1.54
Number of businesses, log change 2004–2012	0.126	0.336	505	0.161	0.335	510	−0.035	2.29
	3 km Treatment Ring			3 km Control Ring				
Employment, 2004	570.77	1570.50	596	519.68	912.41	638	51.088	0.52
Unemployment, 2004	27.56	21.26	596	26	16.8	638	1.563	0.77
Number of businesses, 2004	42.01	58.48	596	46.02	45.12	638	−4.004	1.03
Employment Density, 2004	1149	3087	596	987	1567	638	161.780	1.00
Unemployment Density, 2004	87.42	99.18	596	100.78	125.52	638	−13.367	0.81
Business Density, 2004	94.38	124.72	596	111.07	138.19	638	−16.690	1.04
Employment, log change 2001–2004	0.089	0.398	596	0.044	0.402	638	0.045	1.29
Unemployment, log change 2001–2004	−0.040	0.388	588	−0.040	0.398	628	0.000	0.01
Number of businesses, log change 2001–2004	0.055	0.221	596	0.039	0.207	638	0.016	1.00
Employment, log change 2004–2009	0.040	0.470	596	0.032	0.54	638	0.008	0.59
Unemployment, log change 2004–2009	0.539	0.409	591	0.522	0.398	631	0.017	0.59
Number of businesses, log change 2004–2009	0.017	0.261	596	0.011	0.252	638	0.006	0.25
Employment, log change 2004–2012	0.076	0.599	596	0.091	0.594	638	−0.015	0.36
Unemployment, log change 2004–2012	0.739	0.465	590	0.683	0.452	631	0.056	1.24
Number of businesses, log change 2004–2012	0.088	0.345	596	0.133	0.304	638	−0.045	2.19

Notes: Data on employment and number of businesses come from the Business Structure Database provided by the Secure Data Archive at Essex. All other variables come from the Neighborhood 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. T-statistics are adjusted for clustering at the level of LEGI neighborhood.

difference identifies the treatment effect 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 $h+k$ decreases. 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. We test for spillover effects by comparing changes in the boundary control areas to changes in areas further away from the boundary. We also employ a form of this test that is based on a null hypothesis of no boundary effects. This test compares changes in the outcomes between treatment and control areas at the boundary and between treatment and control areas further away from the boundary. Formally, we test for two null hypotheses: (1) $\xi_1 = \theta_1$ and (2) $\xi_h = \theta_h$ for some $h > 1$. A rejection of the former null hypothesis without rejecting the latter indicates boundary spillover effects, which are negative when $\xi_1 < 0 < \theta_1$ and positive when $\theta_1 < 0 < \xi_1$.

4.2. *Regression discontinuity design based on the minimum IMD rank rule*

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 Local Authorities 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.

In order to lend further credibility to the causal interpretation of our results, we estimate the effects with an RD design which is based on the funding eligibility rule. According to the rule, a Local Authority was eligible to apply for LEGI funding if it ranked among the 50 worst Local Authorities against any of eleven indices of deprivation in 2000 or

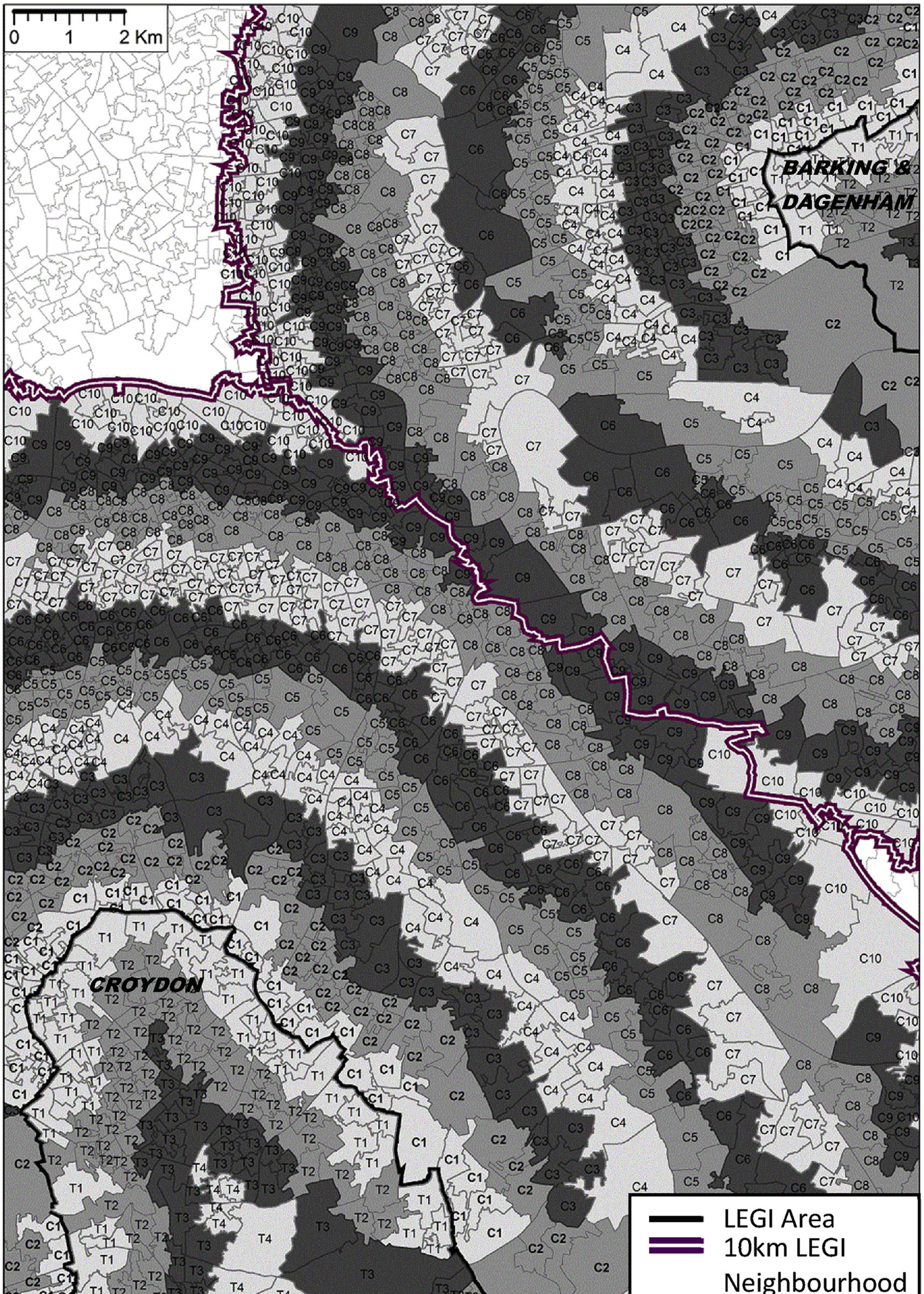

Fig. 1. 1 km-wide Control and Treatment Rings (Croydon and Barking & Dagenham). Notes: The figure depicts 1 km-wide control and treatment rings in the Croydon and Barking and Dagenham areas. Labels indicate the ring. For example, the 1 km treatment ring is labelled "T1" and the 2 km control ring is labelled "C2". Distances based on micro-area centroids. For example, the centroid of a 1 km control (treatment) micro-area is within 0–1000m from the nearest treatment (control) micro-area centroid on the other side of the boundary. GIS data from Edina's UK BORDERS portal.

2004.[15] Formally, this rule can be written as:

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

where $R_{r(i)}$ is Local Authority r's minimum rank across the eleven deprivation indices (five indices for the year 2000 and six for the year 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. The RD design is fuzzy because not all Local Authorities received funding on the eligible side of the threshold. Hence, we estimate the effects by employing a fuzzy RD design (see e.g., Hahn et al., 2001; Van Der Klaauw, 2002) where we use the threshold indicators as an instrument for the treatment status in a Two-Stage Least Squares (TSLS) procedure based on the following equations:

$$L_{r(i)} = \alpha_1 + \rho E_{r(i)} + \boldsymbol{\tau}_1' \mathbf{R}^P_{r(i)} + v_{1it} \tag{4a}$$

$$\Delta y_{it} = \alpha_2 + \tilde{\theta} L_{r(i)} + \boldsymbol{\tau}_2' \mathbf{R}^P_{r(i)} + v_{2it} \tag{4b}$$

where $\mathbf{R}^P_{r(i)}$ is a vector of P polynomial terms of the minimum IMD rank. We allow for different coefficients on the (first and second order) terms of minimum IMD rank on each side of the eligibility threshold. Here $\tilde{\theta}_s$ is the parameter of interest. The key identifying assumption underlying the RD approach is that Local Authorities did not manipulate their minimum deprivation ranking to receive LEGI funding. We believe that manipulation was highly unlikely for several reasons. First, we can rule out direct manipulation through false reporting because the deprivation ranks are constructed by central government based on national statistics. Second, the timing rules out any manipulation of underlying socio-economic characteristics in response to the announcement of the rules for funding because the deprivation rule was based on data pre-dating the announcement of the programme. Third, in the RD analysis below, we can detect no abnormal bunching of observations below the deprivation rank cut-off of 50. Moreover, the minimum IMD rank is based on eleven indices with each, in turn, based on numerous pre-determined variables and complicated formulas so that the outcome of any such manipulation on own IMD score would have been highly uncertain. The final ranking of a Local Authority also depends on the performance of other Local Authorities which further decreases the ability to precisely manipulate one's own ranking. Therefore it is very unlikely that Local Authorities manipulated own deprivation scores to affect LEGI funding and inconceivable that they could affect their rankings.

5. Results

5.1. Descriptive analysis

Table 1 displays descriptive statistics for outcomes for the three 1 km-wide treatment and control rings nearest to the treatment boundary.[16] We also show differences in means between treatment and control rings and the corresponding t-statistics. 1 km control rings are, on average, similar to the 1 km treatment rings in terms of employment, unemployment and number of businesses in 2004 (i.e. pre-announcement). The same is true for changes in these three outcomes over the pre-treatment period between 2001 and 2004. In contrast, the change in employment in the programme period is statistically significantly different between these rings. Over the period 2004–2009, on average, employment increased in the 1 km treatment rings by around 10.6% and decreased in the 1 km control rings by around 4%. Differences in 2004–2009 growth rates of unemployment and number of businesses across 1 km control and treatment rings are smaller and statistically insignificant. Between 2004 and 2012 (i.e. from pre-programme till after the programme was abolished) the differences between 1 km control and treatment rings are insignificant for all outcomes.

Looking at the 2 km and 3 km treatment and control rings, we find no statistically significant differences in most of the pre-programme outcomes either in levels (2004) or changes (2001–2004).[17] Looking across all three outcomes we see three significant differences in the post-treatment period[18] all of which suggest *poorer* outcomes in the 2 km and 3 km treatment rings.

We do not spend more time on these differences as the mean comparisons in Table 1 do not account for unobserved differences in trends across LEGI neighborhoods and for the potential confounding selection of Local Authorities to the programme. We now turn to our econometric analysis that is more robust against these potential sources of bias.

5.2. Difference-in-difference estimates

We start by displaying results for pooled and area fixed-effects regressions without ring dummies. In all difference-in-difference estimations, we cluster standard errors by LEGI neighborhoods and by Local Authority for micro-areas not in a LEGI neighborhood. Table 2 displays the results for pre-trends between 2001 and 2004, treatment effects for changes between 2004 and 2009, and long-term effects for changes between 2004 and 2012. In column 1, the naïve estimate is insignificant across all outcomes except for the 2001–2004 and 2004–2012 change in unemployment. When conditioning on 10 km neighborhood fixed effects (column 2) and adding census controls for area characteristics (column 3), all coefficients become insignificant. The coefficient (standard error) for the 2004–2009 log-change in employment in column 3 is 0.020 (0.022). This point estimate recovers the impact of the programme over the announcement year and four programme years and corresponds to only around a 0.4 percentage point annual increase in the growth rate of employment. Similarly, the standard error for the annualized estimate corresponds to around 0.44 percentage points. The precision of our estimation is therefore sufficient to reject the null hypothesis of no impacts on the annual growth rate of employment for estimates above around 0.86 percentage points at the 95% confidence level.

While the standard difference-in-difference analysis does not detect statistically significant differences between treatment and control areas, the analysis comparing all treated and all control areas may hide treatment effects at a finer spatial scale. To examine this, we estimate regressions corresponding to equation (3), including dummies for ten control and six treatment rings and controlling for LEGI neighborhood fixed effects and census control variables.

Fig. 2 shows placebo and treatment effect estimates of the impacts of LEGI funding across rings using log-changes between 2001 and 2004 and between 2004 and 2009.[19] The figure displays the estimates and 95% confidence intervals for the ring dummies using the 2 km control ring as the reference category.

Starting with employment, we detect no statistically significant differences in pre-trends between the 2 km control ring and any other ring within 10 kms of the boundary. When comparing the 1 km control ring to

[15] For data and methodology for calculating Local Authority-level IMD rankings see Appendix B.

[16] Appendix table B1 presents summary statistics for all treated and all control micro areas.

[17] The only exception is the growth rate of employment between the 2 km rings. The difference becomes statistically insignificant, however, when we control for area fixed effects and pre-programme characteristics below.

[18] For the 2 km rings we find significant differences for unemployment 2004–2009 and number of businesses 2004–2012, for the 3 km rings for number of businesses 2004–2012.

[19] The choice of the period 2004–2009 is motivated by the fact that programme funding was at its highest levels in 2008 and 2009. Using alternative treatment years does not affect our conclusions (estimates for alternative treatment years are available in appendix table B2). Our analysis includes 247 clusters of which 20 are treatment clusters. Hence cluster-robust regression standard errors can be expected to perform well in our setting.

Table 2
Difference-in-difference estimates without ring dummies.

	(1) Pooled OLS	(2) Area FE	(3) Area FE + control variables
A. Employment, log change 2001–04			
LEGI	−0.002	0.013	0.003
	(0.024)	(0.025)	(0.019)
N	32473	32473	32473
B. Employment, log change 2004–09			
LEGI	0.001	0.007	0.020
	(0.013)	(0.018)	(0.022)
N	32473	32473	32473
C. Employment, log change 2004–12			
LEGI	−0.017	0.006	0.019
	(0.017)	(0.021)	(0.025)
N	32474	32474	32474
D. Number of Businesses, log change 2001–04			
LEGI	−0.007	−0.013*	−0.012
	(0.007)	(0.008)	(0.011)
N	32473	32473	32473
E. Number of Businesses, log change 2004–09			
LEGI	−0.003	0.005	0.008
	(0.009)	(0.010)	(0.012)
N	32473	32473	32473
F. Number of Businesses, log change 2004–12			
LEGI	−0.013	0.015	0.007
	(0.013)	(0.016)	(0.018)
N	32474	32474	32474
G. Unemployment, log change 2001–04			
LEGI	−0.051**	0.019	0.009
	(0.026)	(0.022)	(0.023)
N	31135	31135	31135
H. Unemployment, log change 2004–09			
LEGI	−0.010	−0.040	−0.021
	(0.033)	(0.041)	(0.048)
N	31472	31472	31472
I. Unemployment, log change 2004–12			
LEGI	0.101***	0.028	0.016
	(0.038)	(0.042)	(0.051)
N	31380	31380	31380
10 km *LEGI Neighborhood Fixed Effects*		Yes	Yes
Pre-determined Area Characteristics			Yes

Note: Panel labels refer to the outcome. Pre-determined area characteristics are the log of micro-area acreage, minimum IMD rank, and variables listed under the labels "population characteristics" in table B1. 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 Neighborhood Statistics database provided by the ONS except acreage which is based on LSOA boundary files drawn from Edina's UK BORDERS portal. Standard errors are clustered by LEGI neighborhoods and by Local Authority for micro-areas not in a LEGI neighborhood. *, **, *** denote significance at 10%, 5% and 1% confidence levels, respectively.

other rings, the pattern of estimates suggests that 2001–2004 employment growth was *worse* in the 1 km treatment ring compared to the 1 km control ring. In a regression using the 1 km control ring as the reference category, the coefficient on the 1 km treatment ring dummy is −0.058 with a standard error of 0.029 and it is statistically significant at the 5% risk level (appendix table A1). If this difference persisted, it would work against finding negative displacement effects in the 1 km control ring. However, for the period 2002–2004, the difference in employment growth between the 1 km treatment and control rings is only −0.001 with a standard error of 0.036, suggesting that the difference was not persistent.[20]

The pattern of estimates for the period 2004-09 indicates that the programme induced marked divergence of employment at the treatment area boundary. This displacement effect is also highly persistent across treatment years (see appendix table B2). We detect large relative employment losses in the 1 km control ring compared to other control rings. Between 2004 and 2009, employment dropped around 10.3% in the 1 km control ring when compared to the 2 km control ring. This corresponds to around a 2.1 percentage point annual deviation in the growth rate of employment. This economically and statistically significant negative effect, together with the insignificant estimates across a wide range of control rings further from the boundary suggests that the programme induced substantial displacement of employment from untreated micro-areas just outside the LEGI boundary. In the treatment area, we detect the largest positive estimates for the 1 km treated ring and the largest divergence in employment occurs between the 1 km treated and 1 km control rings. The *difference* in employment growth between these two boundary rings is around 15 percentage points and statistically significant at the 5% level.

The results for the number of business units provide little evidence of pre-trends or displacement in terms of the number of business units. For instance, the difference between the 2 km and 1 km control rings is only −0.015 log-points with a standard error of 0.018 in the period 2004-09.

For 2001-04 changes in unemployment, the estimates for the 3 km treatment ring and 3 km and 9 km control rings are statistically significant at the 5% level while the estimates for other rings are not significant. However, all coefficients on ring dummies are insignificant when we use midpoint %-changes as the outcome to account for censoring and zero values in the unemployment data (appendix fig. A1). For 2004-09 change in unemployment, only the estimate for 1 km control ring is significant at the 10% confidence level. We detect no statistically significant differences between the 1 km treatment and control rings at the boundary, and therefore cannot reject the hypothesis of no displacement for unemployment (appendix table A2, column 5). Overall, we find little evidence of displacement for unemployment either before the programme or in the programme period.

Previous studies have found that targeted economic development policies can benefit some industries and hurt others.[21] Because the LEGI programme targeted local markets and, in particular, local services, these industries could be expected to be most affected. To examine this, we estimated the main specifications for 2004–2009 log-changes in employment and number of businesses for service industries. These industries cover 65.9% of employment in our data in 2004.[22] We find a very similar and statistically significant employment displacement pattern for these industries (see appendix fig. A2).

Fig. 3 examines the persistence of the effects on employment after the programme was abolished in March 2011, using log-changes between 2004 and 2012 as the outcome. We are unable to detect statistically significant displacement of employment over this longer period, suggesting that the effects vanished quickly after the programme ended.[23]

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 treatment area. Furthermore, we detect no impact on the number of businesses. This finding suggests that the displacement effect we identify for employment is 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; Duranton et al., 2011). As a result, an economic intervention supporting local businesses need not generate higher entry rates as

[20] Moreover, we find no evidence of different pre-treatment trends in our regression discontinuity design.

[21] See e.g. Hanson and Rohlin (2011), Freedman (2015), Patrick (2016), Harger and Ross (2016), and Patrick et al. (2017).

[22] We exclude the education sector and public administration as these were not targeted by the programme.

[23] We also found no evidence of displacement in terms of number of businesses or unemployment in the long-term (appendix table A3).

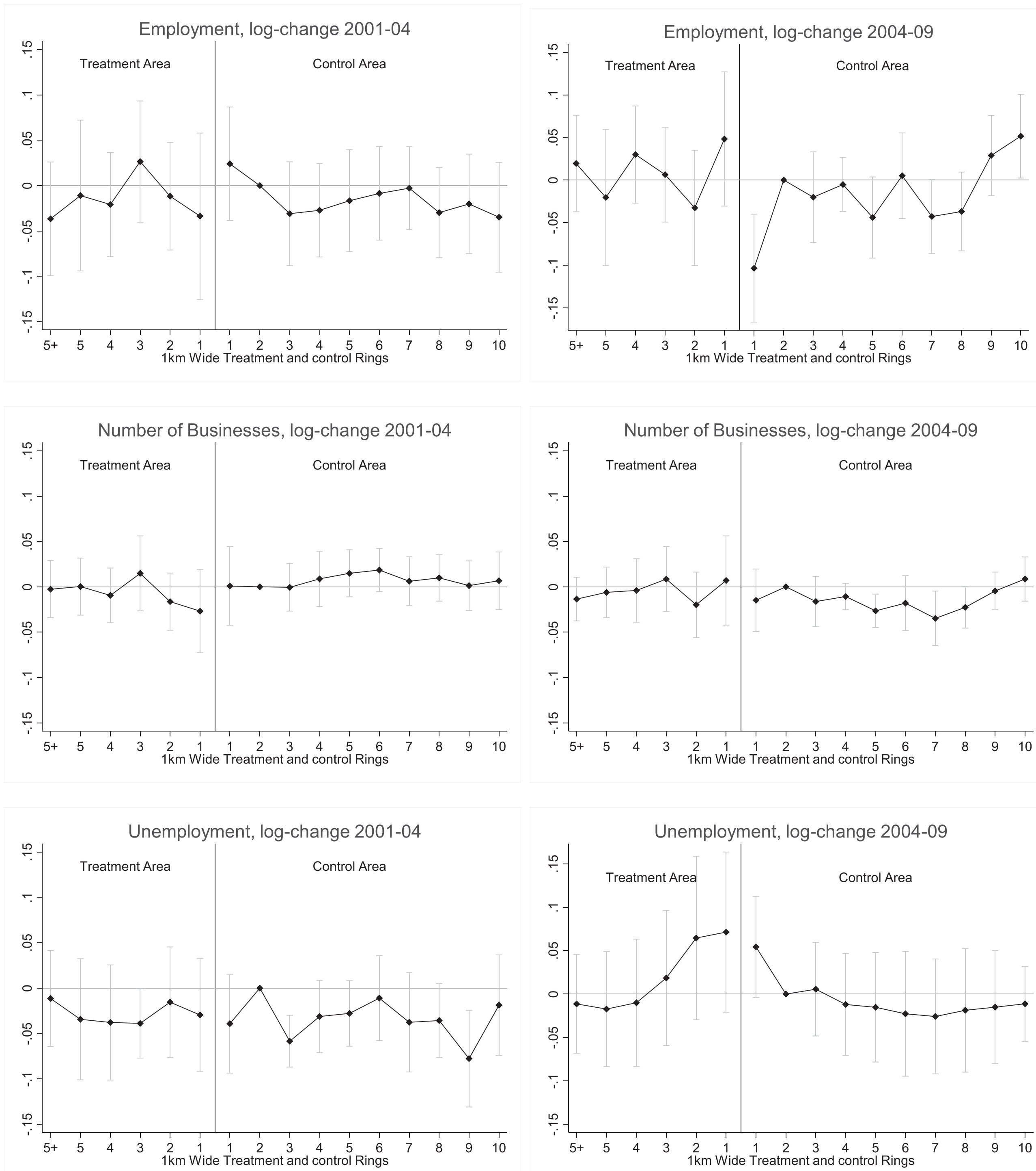

Fig. 2. Effect of LEGI by 1 km-Wide Treatment and Control Rings, Placebo and Treatment Effect Estimates. Notes: Difference-in-difference estimates of the effect of LEGI for 1 km-wide control and treatment rings. Specification controls for 10 km LEGI neighborhood fixed effects and pre-determined characteristics, which are the log of micro-area acreage, minimum IMD rank, and variables listed under the label "population characteristics" in table B1. The gray capped lines display the 95% confidence intervals, based on standard errors clustered by LEGI neighborhoods and by Local Authority for micro-areas not in a LEGI neighborhood. The reference category is the 2 km control ring. Corresponding estimates and standard errors are reported in appendix tables A1 and A2.

businesses entering the local market must sign new contracts with rental rates potentially inflated by the intervention.[24] On the other hand, at boundary areas, funding may help supported incumbent firms to win a larger share of local markets from incumbent unsupported firms.[25]

[24] The effects of spatially targeted support may be offset if rents are flexible and support is fully capitalized (e.g. Muth, 1969; Roback, 1982). However, both the long contract periods and the finding of strong displacement of employment suggest that rents are not completely elastic.

[25] The point estimate for employment in the 1 km treatment ring can be different to the point estimate for the 1 km control ring if, for instance, supported incumbents that increase market share increase hours of employment while unsupported firms reduce number of jobs.

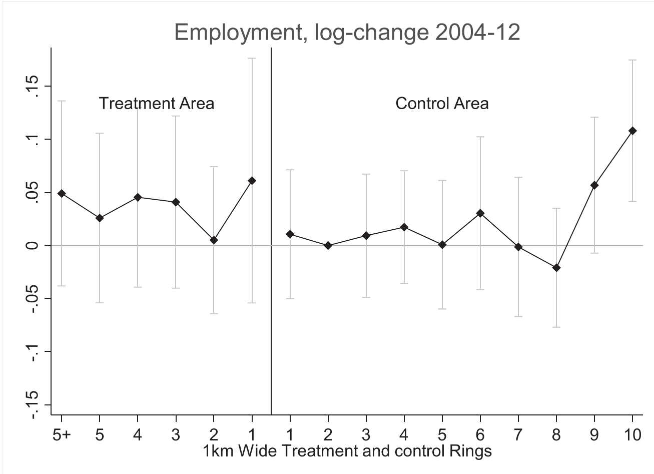

Fig. 3. Long-Term Effect of LEGI by 1 km-Wide Treatment and Control Rings. Notes: Difference-in-difference estimates of the effect of LEGI for 1 km-wide control and treatment rings. Specification controls for 10 km LEGI neighborhood fixed effects and pre-determined characteristics, which are the log of micro-area acreage, minimum IMD rank, and variables listed under the label "population characteristics" in table B1. The gray capped lines display the 95% confidence intervals, based on standard errors clustered by LEGI neighborhoods and by Local Authority for micro-areas not in a LEGI neighborhood. The reference category is the 2 km control ring. Corresponding estimates and standard errors are reported in appendix table A3.

Weaker effects on unemployment, which is based on the residential rather than work location of a worker, is easy to reconcile with significant employment displacement: it is consistent with the idea that local labor markets cross programme boundaries and with previous evidence indicating that jobs generated by area-based interventions are often taken by individuals who reside outside the treatment area (e.g., Busso et al., 2013; Freedman, 2015). The results also suggest that the spatial scope of job displacement is too narrow to generate any statistically detectable differences in employment trends at a more aggregate level.[26] Finally, the finding that the effects on employment do not persist long beyond the abolition of the programme in March 2011 suggests that the programme appears to have created temporary employment displacement around the boundary of the treated Local Authorities, but have induced no long-run divergence in employment growth.

5.3. RD results

The variation exploited to estimate differences between treatment and control rings comes from between Local Authority differences in programme funding. Although our difference-in-difference approach compares rings within a relatively small area, the control and treatment rings are administrated by different Local Authorities which may raise the concern that areas 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 Local Authority boundary may be confounded.

We address the potential concern that more efficient Local Authorities received treatment with the fuzzy RD approach described in section 4. Because the estimates in Fig. 2 suggest very local displacement of employment within 1 km of the treatment area boundary, we test for this pattern in the RD design for two samples: (i) a boundary sample containing micro-areas within the 1 km treatment and control rings and (ii) a sample containing micro-areas within 2–3 km treatment and control rings. The choice of rings is motivated by the findings in Fig. 2, which suggest that the displacement of employment occurs only within 1 km of the boundary. We limit the samples to 3 km treatment and control rings, which are close enough to the boundary to plausibly satisfy the CIA assumption. An advantage of restricting estimation to subsamples is that we can estimate differential impacts of the programme within these samples between control and treatment micro-areas with a just-identified single-instrument IV procedure.[27] Our baseline specification uses a linear function of the minimum IMD rank allowing for different coefficients across the threshold.[28] Standard errors are clustered by minimum IMD rank in all RD specifications (see, e.g., Lee and Card, 2008).[29] We also show that our results are robust when we add a second-order polynomial of the minimum IMD rank, use a weighting kernel, and restrict the sample to minimum IMD rank range of 10–100, which is the optimal CCT bandwidth.[30] We detect no abnormal bunching to the left of the threshold (appendix fig. B3).[31] This is expected, because manipulation of minimum IMD rank is very unlikely due to its complex determination and because it is pre-determined, as discussed in section 4.2. We provide results for RD specifications testing for pre-trends in appendix table B3. We detect no pre-trends on any of the outcomes in subsamples including micro-areas within 1 km rings, 2–3 km rings, and 10 km LEGI neighborhoods.

We start with baseline RD estimates of the impact of the programme on employment growth between 2004 and 2009 for 1 km rings. The first-stage and IV estimates and standard errors are reported in the first column of panel A in Table 3 while the corresponding first-stage and reduced-form RD plots are displayed in Figs. 4 and 5. Fig. 4 shows a sharp first-stage discontinuity on LEGI treatment. The estimate of the size of this discontinuity and its standard error are shown in the second row of column 1 in Table 3. The point estimate is 1.148, and significant at the 1% level, indicating that the eligibility rule provides strong first-stage variation in treatment status.[32] The reduced-form RD plot for this specification is shown in Fig. 5. The graph shows that the micro-areas on the eligible side of the eligibility threshold experience higher employment growth between the years 2004 and 2009; the point estimate of the size of this discontinuity is 0.391. The corresponding IV estimate (standard error) of the impact of the programme on the difference in the growth rates between 1 km treatment and control rings is 0.341 (0.109) and highly significant. The corresponding IV estimate for the 2–3 km

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

[27] Alternatively, we could interact all right-hand-side variables with a subsample dummy indicating each relevant ring pair. However, this would lead to an over-identified IV model which may be susceptible to weak instrument bias. We prefer the pairwise ring estimation because it uses only a single instrument and allows for the graphical presentation of the RD results.

[28] We prefer the first-order polynomial specification due to the concern that the relatively small number of observations in some of our subsamples could lead to problems related to over-fitting in RD settings (see Gelman and Imbens, 2019 for a thorough discussion of unreliability of RD estimates in over-fitted settings).

[29] Kolesár and Rothe (2018) suggest an alternative way to calculate confidence intervals in sharp RD settings with a discrete running variable, but their method is not applicable in our fuzzy RD setting.

[30] The CCT bandwidth is calculated with the Stata rdrobust procedure (see Calonico et al., 2014a, 2014b).

[31] We tested the null hypothesis of no manipulation with the robust test of Cattaneo et al. (2017). The p-value was 0.595, indicating no manipulation.

[32] The estimate of the discontinuity on treatment status is slightly larger than one, which may raise the concern that the first-order polynomial is not the correct specification for the first-stage equation. However, because the IV estimate of the treatment effect in the fuzzy RD model is equivalent to the ratio of the reduced-form discontinuity estimate and the first-stage discontinuity estimate, the potential over-estimation of the first-stage discontinuity due to misspecified first-stage equation provides conservative results. We also show below that our results are robust when we allow for second order polynomials of the minimum IMD rank, which provide first-stage estimates that are below one.

Table 3
Fuzzy regression discontinuity estimates.

	(1) 1 km rings	(2) 2–3 km rings	(3) 10 km LEGI Neighborhood
A. Employment, log change 2004–09			
LEGI	0.341***	−0.016	−0.051
	(0.109)	(0.040)	(0.053)
1st stage			
$I(R \leq 50)$	1.148***	0.881***	0.607**
	(0.142)	(0.227)	(0.278)
Observations	372	2249	10317
B. Number of Businesses, log change 2004–09			
LEGI	0.041	−0.064*	−0.027
	(0.034)	(0.036)	(0.037)
1st stage			
$I(R \leq 50)$	1.148***	0.881***	0.607**
	(0.142)	(0.227)	(0.278)
Observations	372	2249	10317
C. Unemployment, log change 2004–09			
LEGI	0.015	0.054	0.042
	(0.096)	(0.128)	(0.155)
1st stage			
$I(R \leq 50)$	1.146***	0.881***	0.609**
	(0.142)	(0.227)	(0.277)
Observations	370	2227	10182

Notes: Fuzzy RD estimates using an indicator for observations left to the minimum IMD rank cut-off of 50 as an instrument for the LEGI area dummy. All specifications use first order polynomial of the minimum rank variable allowing for different coefficients across the cut-off. The dependent variables are indicated by the panel labels. Estimates in columns 1, 2, and 3 are based on a sample of treatment and control micro-areas located within 0–1000m, 1000–3000m, and 10,000m from the treatment area boundary. Standard errors (in parentheses) are clustered by minimum IMD rank. *, **, *** denote significance at 10%, 5% and 1% confidence levels, respectively.

treatment and control ring sample is small (−0.016) and insignificant.

The IV estimate of the impact of the program on the employment growth differential between the 1 km treatment and control rings from the RD design is larger than the corresponding difference-in-difference estimate (0.152, see column 1 of appendix table A2), although the difference-in-difference estimate falls within the 95% confidence interval of the RD estimate. These estimates do not necessarily recover the same underlying parameter, because the RD design identifies the impacts for the threshold population while the difference-in-difference analysis identifies the effects for a wider population.[33] Nevertheless, the RD estimates in columns 1 and 2 of panel A provide additional support, with stronger internal validity, for the displacement effects detected by the difference-in-difference analysis above.[34]

We also estimated specifications using the 2 km control ring as the reference category and separately for each 1 km-wide treatment ring as the comparison group (appendix table B5, panel A). The results from these specifications indicated a similar displacement pattern for employment as the other estimation approaches.[35]

The RD setting also allows identification of the causal impacts of the programme for the whole treatment area. Column 3 of Table 3 does this by providing RD estimates for a sample including all micro-areas within 10 km LEGI neighborhoods. In panel A, the IV estimate of the treatment effect for employment is negative and insignificant and does not allow us to reject the null hypothesis that the programme had no impact on overall employment in LEGI areas.

Panels B and C of Table 3 provide RD estimates for number of businesses and unemployment. For neither outcome do we detect significant divergence between the 1 km boundary rings due to the programme. For number of businesses the estimate for the 2–3 km zone is −0.064, and weakly significant. We also detect a small negative, but insignificant estimate in column 3. These estimates suggest that the programme, if anything, may have reduced the number of local businesses in the 2–3 km treatment rings compared to the 2–3 km control rings.[36] Finally, the RD design detects no statistically significant impacts on unemployment.

In Table 4, we examine the robustness of the RD results for the boundary and 2–3 km ring samples. Column 1 replicates the baseline estimates in columns 1 and 2 of Table 3. Column 2 allows for a second order polynomial of the minimum IMD rank. In panel A, this increases the RD estimate of the impact of the programme on the employment growth differential between the 1 km treatment and control rings to 0.625. In column 3, we add a control variable for 2003 log-level of the outcome (e.g., in panel A, the log of 2003 employment) to further test the validity of the RD design. This approach is an alternative robustness check to adding unit fixed effects, but is more efficient because it sacrifices fewer degrees of freedom (Lee and Lemieux, 2010). The IV estimate for employment in panel A is very little affected by this alternative specification. Column 4 is similar to column 3 but weights the regression with a triangular weight equal to one at the cut-off and linearly declining to zero towards the left and right tails of the minimum IMD range. This weighting procedure has also very little impact on the estimate. Column 5 restricts the estimation to optimal CCT bandwidth (Calonico et al., 2014a) of 10–100. This, again, has little impact on the point estimate, which in this most demanding specification is 0.521. Combined with the finding of no statistically significant differences in employment growth between the 2–3 km treatment and control rings across specifications in panel B, these results are in line with the significant displacement of employment at the treatment area boundary.

Panels C and D report the robustness results for number of businesses. In panel C, which displays estimates for the boundary rings, we report one marginally significant estimate in column 3, which becomes insignificant when we allow for kernel weights. For 2–3 km rings in panel D, we report a marginally significant *negative* coefficient in columns 1–3. This coefficient increases and becomes significant at the 5% level in columns 4 and 5. These findings strengthen the conclusion that the number of businesses may have declined within these rings in the treatment area. The impacts of the programme on number of businesses could well be negative in some parts of the treatment area if, for instance, a larger fraction of support goes to companies that are not the smallest in the local economy, and rivalry effects force a non-negligible fraction of small companies to exit. For this reason, we consider the results for employment of first order importance when assessing the impacts of the policy. Finally, the estimates for unemployment are all insignificant (panels E and F), which is in line with the corresponding difference-in-difference results. Overall, the regression discontinuity analysis provides robust additional evidence, based on weaker identifying assumptions, of the displacement effects on employment in areas close to the treatment area boundary.

6. Summary and conclusions

Improving the economic and social conditions in deprived neighborhoods is a central urban policy objective in many countries. This paper evaluates the impacts of a large-scale area-based intervention that

[33] The larger RD point estimate could arise, for instance, from heterogeneous treatment effects with larger impacts among the least deprived eligible areas that are closer to the eligibility cut-off.

[34] We also found no evidence of the displacement effects for employment persisting in the long-term. The IV estimates (standard errors) for baseline RD model using log-changes in employment between 2004 and 2012 as the outcome are 0.059 (0.108) for micro-areas within 1 km rings and −0.053 (0.077) for micro-areas within 2–3 km rings.

[35] We also do not detect any statistically significant pre-treatment trends for these specifications (appendix table B6).

[36] The estimates by 1 km-wide treatment rings with 2 km control ring as the reference category lead to the same conclusion, with IV coefficients for some treatment ring dummies being negative and statistically significant (see appendix table B5, panel B).

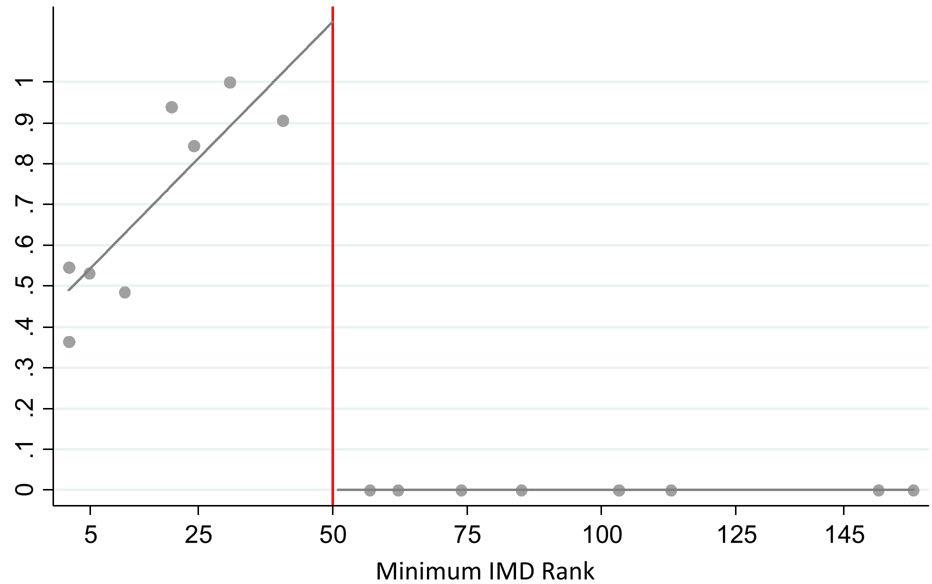

Fig. 4. First-Stage Regression Discontinuity Plot, 1 km Rings. Notes: N = 372. Outcome is the binary indicator for LEGI treatment. Plots are means by minimum IMD rank bins. The number of observations within bin ranges from 13 to 33. See appendix B for the corresponding plots for micro-areas within 2–3 km rings and 10 km LEGI neighborhoods.

aimed to improve employment and entrepreneurial activity in the most deprived areas of the UK by providing targeted support to businesses in the local non-tradable sector. We assess the causal impacts of the policy by combining spatial difference-in-difference and RD strategies based on programme eligibility rule. We show that pre-policy outcomes are balanced between treatment and control groups, there is no bunching at the RD threshold, and that our results are robust to a battery of RD validity checks.

While we find no effects on aggregate employment in treated areas, data at fine spatial scale reveals significant displacement of employment from unsupported to supported areas close to the treatment area boundary. While the displacement pattern is persistent in the programme period, it diminishes quickly after the programme is abolished. Our finding of the unintended displacement of economic activity across the boundary of treated areas is especially important because the negatively affected nearby neighborhoods in our study are also, on average, economically distressed. The results suggest that the programme may have simply shuffled jobs from one deprived area to another with no aggregate net effects.

Our results are consistent with the idea that the net impacts on employment of area-based interventions may be attenuated by general equilibrium effects. Although we are unable to identify general

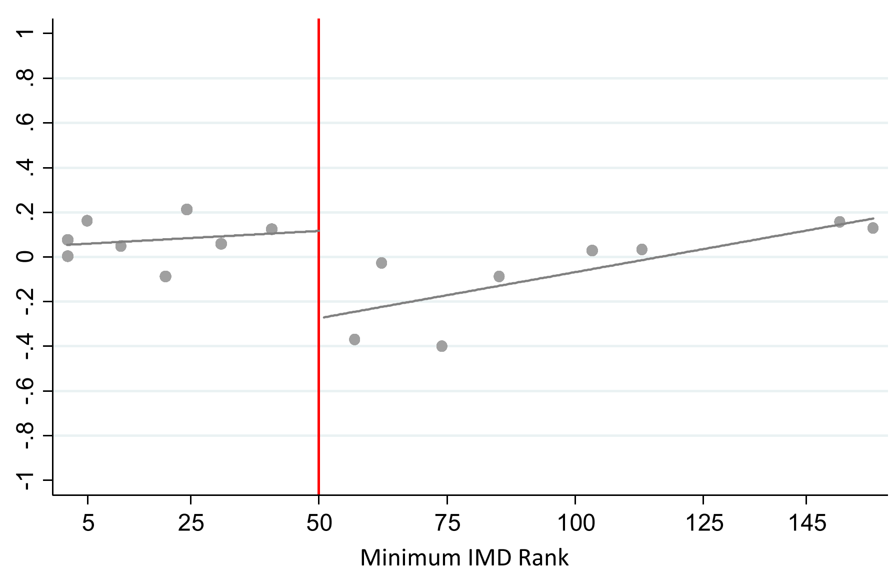

Fig. 5. Reduced-Form Regression Discontinuity Plot for Employment, 1 km Rings. Notes: N = 372. Outcome is the log-change in employment between 2004 and 2009. Plots are means by minimum IMD rank bins. The number of observations within bin ranges from 13 to 33. See appendix B for the corresponding plots for micro-areas within 2–3 km rings and 10 km LEGI neighborhoods.

Table 4
Robustness specifications for regression discontinuity analysis.

	(1)	(2)	(3)	(4)	(5)
A. Employment, log change 2004-09, 1 km rings					
LEGI	0.341***	0.625***	0.540***	0.606***	0.521***
	(0.109)	(0.111)	(0.087)	(0.062)	(0.060)
1st stage					
$I(R \leq 50)$	1.148***	0.927***	0.924***	0.932***	0.945***
	(0.142)	(0.142)	(0.143)	(0.086)	(0.079)
Observations	372	372	372	282	227
B. Employment, log change 2004-09, 2–3 km rings					
LEGI	−0.016	0.015	0.063	−0.000	−0.039
	(0.040)	(0.082)	(0.063)	(0.060)	(0.089)
1st stage					
$I(R \leq 50)$	0.881***	0.796***	0.812***	0.814***	0.842***
	(0.227)	(0.281)	(0.280)	(0.231)	(0.207)
Observations	2249	2249	2249	1917	1256
C. Number of Businesses, log change 2004-09, 1 km rings					
LEGI	0.041	0.054	0.063*	0.033	0.001
	(0.034)	(0.035)	(0.033)	(0.024)	(0.020)
1st stage					
$I(R \leq 50)$	1.148***	0.927***	0.913***	0.924***	0.945***
	(0.142)	(0.142)	(0.140)	(0.086)	(0.074)
Observations	372	372	372	282	227
D. Number of Businesses, log change 2004-09, 2–3 km rings					
LEGI	−0.064*	−0.100*	−0.079*	−0.132**	−0.150**
	(0.036)	(0.056)	(0.046)	(0.062)	(0.070)
1st stage					
$I(R \leq 50)$	0.881***	0.796***	0.816***	0.817***	0.843***
	(0.227)	(0.281)	(0.278)	(0.228)	(0.205)
Observations	2249	2249	2249	1917	1256
E. Unemployment, log change 2004-09, 1 km rings					
LEGI	0.015	−0.112	−0.028	−0.114	−0.225
	(0.096)	(0.103)	(0.121)	(0.108)	(0.150)
1st stage					
$I(R \leq 50)$	1.146***	0.927***	0.931***	0.935***	0.920***
	(0.142)	(0.142)	(0.139)	(0.087)	(0.099)
Observations	370	370	370	281	226
F. Unemployment, log change 2004-09, 2–3 km rings					
LEGI	0.054	−0.048	−0.028	0.079	0.164
	(0.128)	(0.157)	(0.108)	(0.128)	(0.123)
1st stage					
$I(R \leq 50)$	0.881***	0.795***	0.825***	0.815***	0.840***
	(0.227)	(0.281)	(0.276)	(0.230)	(0.207)
Observations	2227	2227	2205	1883	1236

Notes: Column 1 replicates the baseline results in Table 3 for 1 km and 2–3 km rings. Column 2 allows for a second order polynomial of the minimum IMD rank. Column 3 adds a control variable for 2003 log-level of the outcome (e.g., in panel A, the log of 2003 employment). Column 4 is similar to column 3 but weights the regression with a triangular weight equal to one at the cut-off and linearly declining to zero towards the left and right tails of the minimum IMD range. Column 5 is similar to column 4 but restricts the estimation to minimum IMD rank range 10–100. Standard errors (in parentheses) are clustered by minimum IMD rank. *, **, *** denote significance at 10%, 5% and 1% confidence levels, respectively.

equilibrium effects at a wider spatial scale (e.g., national or global), our study makes an important contribution by providing evidence that such effects can occur within local markets. Our findings are consistent with the view that supporting the non-tradable sector may be a highly inefficient instrument for transferring jobs to disadvantaged areas.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.regsciurbeco.2019.103500.

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