

The Effects of Spatially Targeted Enterprise Initiatives: Evidence from UK LEGI[†]

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September 19, 2011

[Preliminary: Please feel free to quote topic, methods etc; but results may change]

Abstract

We investigate the impacts of a significant area-based policy intervention (LEGI) that aimed to increase employment and productivity in 30 disadvantaged areas across England. In order to identify the causal effects of the programme, we use panel data at a fine spatial scale covering years before and after the launch of the program, and exploit several institutional features that determined whether or not an area becomes supported.

K ywords: Employm nt, policy valuation, tr atm nt ff cts

JEL Classification:

1 Introduction

Many governments spend large amounts of money trying to boost productivity, employment and incomes in economically disadvantaged neighbourhoods. Despite the prevalence

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of such policies, there is surprisingly little evidence on their causal impacts on the performance of local economies.

While programme specific outputs (e.g. number of firms assisted) are increasingly well monitored by governments, most so-called evaluations do not use credible identification strategies for assessing whether these outputs translate in to improvements in the local economy that would not have happened in the absence of policy. As emphasised by the literature on program treatment effects (Heckman, LaLonde and Smith, 1999; DiNardo and Lee, 2010) solving this problem requires the construction of a valid counterfactual that can then be compared to observed outcomes. One possibility is to use outcomes for 'untreated' areas (i.e. those that are not eligible for the policy). The problem with this strategy is that areas eligible for treatment are usually not randomly chosen. Another strategy is to look for instrumental variables which are correlated with the likelihood that an area is treated, but that do not in themselves affect area outcomes. In spatial settings, such instruments can be hard to come by either because it is difficult to rule out the possibility that they independently affect outcomes or because, if they don't, they only weakly influence the probability of being treated. Increasingly, however, it has been recognised that using details on the institutional setting combined with these strategies can lead to credible identification. It is this approach that we adopt in this paper.

This paper examines the impacts of a significant area-based policy intervention in England. The Local Enterprise Growth Initiative (LEGI), introduced in 2005, aimed to 'release the economic and productivity potential of the most deprived local areas across the country through enterprise and investment'. Between 2006 and 2009, the programme spent around 80 million pounds per year on 20 LEGI areas located across England (with 30 of the most disadvantaged local authorities receiving funding). With resident working age populations of approximately 1.4 million this equated to expenditure of around £60 per capita per year. Although these per capita expenditures are not huge, performance management data claimed that LEGI had assisted or engaged 240,000 individuals during this period (around 17% of the working age population), created 22,700 jobs (a 2% 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). Clearly, if these truly represent the causal impacts of the programme, effects of these magnitudes should

be amenable to identification in our empirical analysis.

In order to identify the causal effects of the intervention, we use panel data at a fine spatial scale covering years before and after the launch of the program, and exploit several institutional features that determined whether or not an area becomes supported. Our first strategy exploits discontinuities across space arising because the policies apply to specific areas. Specifically, we compare changes in outcomes in treated areas to changes for neighbouring untreated areas. This strategy consistently identifies the causal effect of the policy if: (i) conditional on observable characteristics, the boundaries of treated areas are uncorrelated with any unobserved area characteristics that affect outcomes and (ii) outcomes for un-treated neighbouring areas are unaffected by treatment. In practice we find evidence that (ii) does not hold and that nearby neighbours may be negatively affected by the policy, so that comparisons to immediate neighbours may overestimate impacts. As discussed below, we implement a form of triple-spatial differencing (by comparing treated to nearby untreated neighbours and nearby untreated neighbours to their untreated neighbours that are further from the LEGI boundary) to address this problem. Our second strategy is based on a discontinuity in the eligibility rule determining whether an area is eligible to apply for LEGI funding. According to this rule, a local authority is eligible to apply for funding if it ranks 50th or worse against at least one of six measures of deprivation. This institutional restriction suggests that we might identify the causal impact of the policy by comparing treated and untreated areas just above and below the eligibility threshold. Identification requires that, conditional on observable characteristics, the cut-off for eligibility be uncorrelated with any unobserved area characteristics that affects both outcomes and the probability of treatment. Our final strategy combines these two designs. We discuss these strategies in much more detail below.

Our preliminary results suggest that LEGI increased employment, created businesses and reduced worklessness in treated areas. This effect is more pronounced the closer the control group is to the treated area. As discussed above this indicates that increases in employment and business induced by the programme displaced jobs and firms in unsupported areas. Results from triple-spatial differencing support this interpretation.

The paper adds to the small, but growing literature that takes identification issues seriously when evaluating the impact of spatial interventions. To the best of our knowl-

edge, research on the impact of US Enterprise and Empowerment Zones, particularly the papers by Neumark and Kolko (2008) and Busso, Gregory and Kline (2009), has made most progress in trying to address these problems. Earlier contributions include Dabney (1991), Papke (1993), Boarnet and Bogart (1996), Bondonio and Engberg (2000), Peters and Fisher (2002), O’Keefe (2004), and Bondonio and Greenbaum (2007), while Bartik (1991) and Nolan and Wong (2004) provide useful reviews. Recent work on UK Regional Selective Assistance (Criscuolo, Martin, Overman and Van Reenen, 2011), UK Single Regeneration Budget (Gibbons, Overman and Sarvimaki, 2011) and on French Enterprise Zones (Gobillon, Magnac and Selod, 2010; Mayer, Mayneris and Py, 2011) evaluate the causal impact of spatial policy interventions outside of the US.

Our approach combines and extends approaches that have been used in previous papers assessing the impact of spatial policies, as well as the wider policy evaluation literature. The focus on using detailed data to construct control areas follows Neumark and Kolko (2008) and allows us to control for numerous area characteristics in our analysis, and check for differences in pre-program trends just before the introduction of the intervention. We also use the spatial resolution of our data to help consider the issue of displacement. Becker et al (2010, JPub) use a fuzzy regression discontinuity design and regional data to assess the impact of the EU’s structural fund project. Relative to that paper, we have firm level and census data which allows us to control for unobserved area trends in outcomes at a much finer spatial scale.

The rest of the paper is structured as follows. Section 2 provides more details of the LEGI programme. Section 3 describes our data. Section 4 describes our identification strategy, with results reported in section 5. Finally, section 6 concludes.

2 The Institutional Setting

The Local Enterprise Growth Initiative (LEGI) was introduced by the previous government in 2005. As discussed above LEGI aimed “to release the productivity and economic potential of our most deprived local areas and their inhabitants, through enterprise and

investment, thereby boosting local incomes and employment opportunities.”¹ Total expenditure for the programme was budgeted at around £100 million per year, although actual expenditure appears to have been about 20% lower (DCLG, 2010). Programmes under the scheme were expected to operate with a ten year time horizon, although in 2006 funding was only confirmed for a three year ‘comprehensive spending review’ period. In the end, the LEGI programme ran for 6 years with the incoming coalition government abolishing the programme from March 2011.

LEGI funding was provided to local authorities who then had considerable flexibility in determining how best to “increase local entrepreneurial activity and support the growth, and reduce the failure rate, of locally owned businesses”. As a result, the specific mix of activities differed quite significantly by area. Across the programme as a whole about 30% of expenditure went on supporting existing local 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. See DCLG (2010) for more details.

Funding was allocated in two competitive bidding rounds held in February and December 2006. All local authorities eligible for Neighbourhood Renewal Fund (NRF) were eligible to apply for LEGI funding. At the time, the Neighbourhood Renewal Fund 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 targetted 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.

Eligible local authorities were invited to apply and “two competitive bidding rounds were held in February and December 2006 with funding awarded to 20 areas comprising of 30 local authorities.” The fact that our data covers two rounds of funding suggests that one possible identification strategy would be to follow Busso, Gregory and Kline (2009)

¹Unless otherwise indicated, quotes in this section taken from <http://www.communities.gov.uk/regeneration/regenerationfunding/legi/> (accessed 23/06/11).

and exploit the different timing of interventions. That is, we could compare changes up to December 2006 in areas that received funding in the first round to those that only received funding in the second round. In practice, however, the 11 month gap between rounds makes this strategy less attractive. Instead, we treat both rounds symmetrically and assume that impact on areas receiving either round 1 or 2 funding potentially occurs from 2006 onwards. We think this assumption is particularly appropriate for LEGI given the 10 year planning framework and the initial commitment to reasonably generous levels of funding for a 3 year period (even if in practice the scheme was curtailed after 5 years in March 2011). Alternatively, we could follow the alternative strategy of Busso et al (2009) and compare winners to losers. To date, however, the government department in charge of administering the scheme has been unable to provide us with details on unsuccessful applicants.

Instead of using timing or unsuccessful applicants as part of our identification strategy, we focus on two other characteristics of the scheme that can help with identification. The first of these builds on the fact that LEGI funding was granted to specific geographic areas which were demonstrated to be performing badly on some economic criterion (e.g. number of local businesses). Conditional on these observable characteristics, it seems reasonable to assume that the exact placing of boundaries is arbitrary with respect to other unobserved characteristics that might affect area level outcomes. If we further assume that these unobservable characteristics vary continuously over space then we can use areas just outside the LEGI area boundary as control areas for comparison to those areas that get treated because they are inside the boundary. As we describe in more detail below, we follow Duranton, Gobillon and Overman (2011) and implement this idea empirically by using 'spatial differencing' across the LEGI area boundary. The second institutional characteristics that we use to achieve identification builds on the fact that LEGI applicants need to be eligible for the Neighbourhood Renewal Fund (NRF). This restriction means that local authorities were able to apply for LEGI funding if they ranked among the 50 worst local authorities against any of six local authority level Index of Deprivation (ID) indices.² Based on the 2004 indices, 80 out of 354 local authorities

²The indices on which eligibility was based on were the following 2004 local authority level summary ID indices based on ID indices at the Super Output Area (SOA) level: 1) *Local Concentration* is the population weighted average of the ranks of a district's most deprived SOAs that contain exactly 10% of the district's population; 2) *Extent* is the proportion of a district's population living in the most deprived

met this criteria (and hence were eligible for both NRF and LEGI). In addition to this IMD rule, 7 local authorities who did not meet the criteria were able to apply because they were eligible for transition period funding.³ As discussed in more detail below, we can exploit this discontinuity to compare outcomes for small geographical areas that receive treatment because their local authority is 'just' eligible for LEGI to similar small geographical areas that do not receive treatment because their local authority is 'just' *ineligible* for LEGI. Again, conditional on observable characteristics, the key identifying assumption is that the exact choice of cut-off is arbitrary and hence uncorrelated with unobservable characteristics that may influence area outcomes.

3 Data

Our units of observation for the analysis 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.⁴ These super output areas are sufficiently small that we can classify them as either treated or untreated on the basis of LEGI boundaries, but large enough that we can construct area level data for three area outcomes of interest - employment, number of businesses and worklessness. These three outcomes 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 for approximately 99% of economic activity in the UK and includes

SOAs in the country; 3) *Income Scale* is the number of people who are Income deprived. 4) *Employment Scale* is the number of people who are Employment deprived. 5) *Average of SOA Ranks* is the population weighted average of the combined ranks for the SOAs in a district. 6) *Average of SOA Scores* is the population weighted average of the combined scores for the SOAs in a district.

These were local authorities that were eligible for NRF in the 2000 IMD, but did not meet eligibility criteria in the 2004 IMD.

⁴LSOAs have a minimum population of 1,000. The 32,482 LSOAs in England were built from groups of Output Areas (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)

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. Worklessness data measures benefit claimants as a percentage of working age population and is available at LSOA level from the Neighbourhood Statistics database maintained by the ONS.⁵ We also have data for a rich set of area characteristics measuring age of residents, economic activity of residents, property values, ethnic composition, population density, acreage and measures of deprivation at LSOA level provided by Neighbourhood Statistics. To control for industry structure, we use information on the number of businesses (local units) categorized in to one of 14 industries and 3 age groups available for Middle Super Output Areas (MSOA) from Neighbourhood Statistics.⁶

Geocoding of super output areas by treatment status is based on shape files provided by the UK Borders database. A super output area is considered as treated if it is located within the boundaries of a local authority receiving LEGI funding. Because super output areas are constructed so as not to cross any local authority boundaries, the geocoding of treated super output areas is exact. Figure 1 provides an example, showing three LEGI areas to the South of London. The smallest areal units in the figure are the super output areas - our units of observation. Bold lines mark out the boundaries of the Croydon, Barking and Dagenham and Hastings and Rother LEGI areas (two in the north-west corner of the figure, one on the south-west corner). The shaded super output areas comprise the control areas that we use when spatial-differencing as explained below.

The final data issue to be resolved concerns the choice of time period. We treat data from 2005 as capturing pre-treatment area characteristics. The recession causes some difficulties in terms of deciding on the period over which we should look for an impact of the policy. For this preliminary version of the paper we focus on a short time period

⁵www.neighbourhood.statistics.gov.uk. Worklessness 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.

⁶MSOAs were built from groups of LSOAs and constrained by the local authority boundaries of 2003. These areas have a minimum population of 5,000, with an overall mean of 7,200. The industry classification is based on the following 14 industries: Agriculture; Production; Construction; Motor Trades; Wholesale; Retail; Hotels & Catering; Transport; Post & Telecommunications; Finance; Property & Business Services; Education; Health; Public Administration & Other Services. The company age groups are 0-9, 10-19, and more than 19.

to 2007. The short time period has the benefit of not incorporating the recession (which may play out unevenly over space) but at the expense of capturing only the immediate effects of treatment. Data is available that will allow us to assess changes up to 2010 in future versions of the paper.

4 The empirical strategy

In this section, we explain our empirical strategies for identifying the causal effects of LEGI funding on economic outcomes in treated super output areas. The central problem that these strategies need to address arises because we do not know what would have happened to a treated LSOA in the absence of LEGI funding. The simplest approach to this problem assumes that, in the absence of the programme, area outcomes would be determined linearly by the observable characteristics of areas (plus a random component that is independent of treatment). Under this assumption we can identify the causal impacts of LEGI by comparing outcomes for treated LSOA to outcomes for untreated LSOA that are assumed to be 'similar' once we control for observable characteristics. To implement this idea empirically, we use a baseline difference-in-difference analysis that compares *changes* in employment, number of businesses and worklessness between treated and untreated LSOA. That is, we estimate the following equation:

$$\Delta y_{ir} = \gamma L_{r(i)} + \beta X_{ir} + \epsilon_{ir} \quad (1)$$

where Δy_{ir} is a log change in the outcome measure of interest over the period 2005-2007 in an LSOA i located within local authority r . $L_{r(i)}$ is a binary indicator equal to one if the LSOA is within a local authority r which was awarded LEGI funding in the year 2006 (Round 1) or 2007 (Round 2), and zero otherwise. X_{ir} , are a set of pre-treatment controls which may include industry structure, firm size, age of residents, economic activity of residents, property values, ethnic composition, population density, acreage and measures of deprivation. Time differencing removes LSOA fixed effects that may be correlated with outcomes meaning that the OLS estimate of γ is robust to time invariant unobserved heterogeneity at LSOA level. In other words, equation 1 will provide a consistent estimate of the effect of LEGI funding if, conditional on observed characteristics included in X_{ir} ,

unobserved LSOA level trends in y_{ir} are uncorrelated with LEGI treatment. Clearly, even with a relatively rich set of controls, we might still worry that this condition does not hold. A first strategy for dealing with this is to assume that there are differences in unobserved characteristics across space but that treated areas are similar to neighbouring untreated areas in terms of these unobservable characteristics. This identifies the effect of LEGI by comparing outcomes for treated LSOA to *neighbouring* LSOA that are untreated but otherwise 'similar' once we control for observable characteristics. To implement this idea empirically we use a difference-in-difference estimation that compares changes in outcome between treated areas and *neighbouring* untreated areas. Specifically, we estimate:

$$\Delta y_{ir} = \alpha_{l(i)}^m + \gamma L_{r(i)} + \beta X_{ir} + \epsilon_{ir} \quad (2)$$

where, as before, Δy_{ir} is a log change in the outcome measure of interest, $L_{r(i)}$ is a binary indicator for LEGI and X_{ir} are pre-treatment characteristics. Note that, relative to equation 1, this specification 'spatial differences' by including $\alpha_{l(i)}^m$ - a set of fixed effects defined as follows: For each LEGI local authority we define a "LEGI neighbourhood" comprising all super output areas within its boundary (a treated zone) and within m kilometer of the boundary (a control zone); for the remaining super output areas we assign fixed effects by local authority. Identification of the LEGI effect now comes from comparisons within LEGI neighbourhoods. Super output areas outside LEGI neighbourhoods do not help with the identification of the treatment effect, but do improve the precision with which we estimate the coefficient on pre-treatment controls.

Restricting our attention to LEGI neighbourhoods naturally raises the concern that this may overestimate the impact of LEGI if funding displaced economic activity in untreated super output areas close to the LEGI boundary to treated areas inside LEGI. To consider this possibility we follow Neumark and Kolko (2008) and Gibbons, Overman and Sarvimaki (2011) divide untreated super output areas within LEGI neighbourhoods in to control rings based on the distance to the LEGI boundary. That is, we augment equation (2) as follows:

$$\Delta y_{ir} = \alpha_{l(i)}^m + \sum_{c \in \{1: \lceil m/g \rceil\}} \times \xi^c D_{ir}^c + \gamma L_{r(i)} + \beta X_{ir} + u_{ir} \quad (3)$$

where g is the maximum width of each control ring, $\lceil m/g \rceil = \min\{z \in \mathbb{Z} \mid z \geq m/g\}$

is the number of control rings, and D_{ir}^c is a binary indicator for control ring c equal to one if a super output area is within a distance $d_c \in ((c-1) \cdot g, \min(c \cdot g, m)]$ of the border of the LEGI zone and zero otherwise. For example, for a LEGI neighbourhood with a 5 km control zone ($m = 5$), and 2 km control rings ($g = 2$) we have $\lceil m/g \rceil = 3$. In this case we have three control rings: $d_1 \in (0, 2]$, $d_2 \in (2, 4]$, and $d_3 \in (4, 5]$. Our test of displacement is based on comparing coefficients on control rings. Under the null hypothesis of no displacement $\xi^{c_1} = \xi^{c_k}$ for all $1 < k \leq \lceil m/g \rceil$. In contrast, if $\xi^{c_1} < \xi^{c_k}$ this suggests that the average change in, say, employment was smaller in super output areas closer to a LEGI area compared to the super output areas further away from it. Provided that LEGI funding had a positive effect on employment and that unobserved shocks to employment were similar across these control rings, this would be an indication of displacement to treated areas from the untreated super output areas closest to LEGI.

The identification of the treatment effect (γ) in equation (2) is based on the assumption that LEGI treatment does not affect nearby untreated areas and that potential unobserved trends in outcomes (y_{ir}) are independent of LEGI treatment conditional on observable area characteristics. The first problem may be exacerbated because our approach is based on comparing treated and untreated super output areas within a small distance. As discussed above, this may cause problems because we cannot rule out the possibility that there is displacement between the treated and control groups given their geographical closeness. In addition, we cannot rule out the possibility that unobserved shocks to economic performance are different across supported and unsupported super output areas in a way that is correlated with LEGI. It may be, for example, that local authorities that submitted successful bids had suffered shocks to outcomes that then persisted over time. In this case, we would underestimate the impact of treatment.⁷ Another concern is that LEGI areas may have been selected based on central government expectations about future economic performance. Depending on the nature of the selection bias, this could lead us to under or overestimate the impact of LEGI if these expectations proved accurate. In order to address whether displacement and non-random assignment of treatment is driving our results, our second strategy exploits discontinuities in the eligibility rules. Specifically, we exploit the Index of Multiple Deprivation rule determining eligibility for LEGI funding.

⁷Rodrik (2007) makes this argument in the context of industrial support to firms or industries that have experienced negative shocks.

According to this rule, a local authority was eligible to apply for LEGI funding if it ranked among the 50 worst local authorities against any of six local authority level IMD indices.⁸ Formally, this rule can be written as:

$$E_{r(i)} = I \quad R_{r(i)} \leq 50 \quad (4)$$

where $R_{r(i)}$ is Local Authority i 's minimum rank across the six IMD indicators and $E_{r(i)}$ is a binary indicator taking the value of one if this minimum IMD rank is less than or equal to 50. There are two factors that make the receipt of treatment not completely dependent on being eligible for treatment. First, constraints on the overall level of funding for the LEGI programme meant that only 30 local authorities out of 80 eligible areas received LEGI funding. Second, as noted above, 7 local authorities who did not meet the criteria were able to apply because they were eligible for transition period funding. As a result of both these factors the discontinuity at the threshold of the IMD rule is fuzzy. To allow for this, we implement a fuzzy regression discontinuity (FRD) design by including in equation (2) a higher order polynomial of $R_{r(i)}$ and using the eligibility status $E_{r(i)}$ as an instrument for the treatment status $L_{r(i)}$ (see e.g., Hahn, Todd and Van der Klaauw 2001; Van der Klaauw 2002) . That is, we estimate a 2SLS procedure based on the following equations:

$$\begin{aligned} L_{ir} &= \tilde{\alpha}_i^m + \rho E_{r(i)} + \tilde{\beta} X_{ir} + \tilde{\tau}^P R_{r(i)}^P + \tilde{v}_{ir} \\ \Delta y_{ir} &= \alpha_i^m + \gamma L_{r(i)} + \beta X_{ir} + \tau^P R_{r(i)}^P + v_{ir} \end{aligned} \quad (5)$$

where $\tau^P R_{r(i)}^P = \sum_{p=1}^P \tau_p R_{r(i)}^p$ is a P th order polynomial of the minimum IMD rank. This approach identifies the impact of LEGI using those super output areas that were drawn into LEGI because their local authority ranked 50th or worse against at least one of the IMD indexes compared to observably similar super output areas that did not receive funding because their local authority ranked above 50 against all six IMD indices. If we drop the assumption of a constant causal effect of LEGI and instead assume that the effects are heterogenous then this provides a causal estimate of the local average treatment effect (LATE) which does not necessarily equal the average treatment effect for moving a randomly chosen LA from non-treated to treated status. In other words using the

⁸For definitions of these indices, see section 2.

fuzzy regression discontinuity design to reduce concerns about the endogeneity of LEGI treatment (internal validity) comes at the cost of increased concerns about the general applicability of the identified effect (external validity). See Imbens and Angrist (1994) and Angrist and Pischke (2009) for further discussion.

Figure 1 shows three LEGI areas to the South of London. The smallest areal units in the figure are the super output areas - our units of observation. Bold lines mark out the boundaries of the Croydon, Barking and Dagenham and Hastings and Rother LEGI areas (two in the north-west corner of the figure, one on the south-west corner). As discussed above, a LEGI neighbourhood comprises of a LEGI area and a control zone around it. The shaded super output areas are those within a 10km control zone and are defined on the basis of the distance of the super output area from the centroid of the *nearest* LEGI super output area. In contrast to defining on the basis of the LEGI boundary, this has the advantage of ensuring that the average distance between a location in a treated LSOA at the LEGI boundary and an untreated LSOA at the outer boundary is approximately 10km. Estimation based on equation (1) compares average performance of LEGI super output areas to that of all control zone super output areas, while an approach based on the fixed effects specification of equation (2) compares average performance of treated and untreated zones *within* LEGI neighbourhoods. Different shading identifies super output areas within different 1km control bands (again based on distance of the LSOA centroid to the centroid of the nearest LEGI LSOA). 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 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'.

5 Results

[TO DO: Results from equation 1 excluding LEGI neighbourhood dummies.]

Tables 1-3 display the results from the specifications including LEGI neighbourhood fixed effects (equations 2 and 3). Each table has three panels, reporting results using three different LEGI neighbourhoods (all super output areas within 2km, 5km and 8km of the LEGI boundary, respectively). The first column in each table reports the results

from the specification comparing treated super output areas to untreated super output areas within the same LEGI neighbourhood, i.e. the coefficient on $L_{r(i)}$ in equation 2. Columns 2-5 report the results from a specification corresponding to equation 3 where we address concerns that any positive effect of LEGI may be at the expense of displacement of economic activity from control rings near the LEGI border. These specifications add binary indicators for 1km control rings based on the distance of untreated LSOA from the LEGI boundary. For the 2km LEGI neighbourhoods this gives two control rings (at 1km and 2km). We report results first with the 1km ring omitted (column 2) and then with the 2km ring omitted (column 3). We think that the availability of multiple comparison groups helps highlight the role of displacement in driving results. In panel 2, for 5km LEGI neighbourhoods, this gives 5 control rings (at 1km, 2km, 3km, 4km and 5km). For brevity, we report estimates for the four closest control rings and, once again, present results omitting each ring in turn in columns 2-5.⁹ Finally, in panel 3, we report results for 8km LEGI neighbourhoods for the closest four (out of 8) control rings omitting each ring in turn as before.

The first panel of table 1 displays the results for employment based on comparisons within 2km LEGI neighbourhoods (i.e. areas comprising of a LEGI area and super output areas within 2km of the LEGI boundary). The estimate in the first column is 0.0453 and significant at the 5% level suggesting that LEGI funding resulted on average in 2.25 percentage points higher annual growth rate of employment over the two year period 2005-2007. Columns 2-5 display results for the specification including binary indicators for 1km control rings. For each specification the reference group is the control ring for which an estimate is not displayed. For example, in column 2 the reference control ring is the 1km control ring and the coefficient in the row labeled “LEGI” displays an estimate for how much larger on average the two-year growth rate was in the treated super output areas compared with super output areas in the 1km control ring. This estimate is 0.0985 and significant at the 5% level, while the coefficient on the LEGI dummy in column 3 (representing the difference between treated super output areas and super output areas in the 2km control ring) is three times smaller and insignificant. In other words, the two-year growth rate for LSOA in the 1km control ring was 6.69 percentage points *lower* than that

⁹A full set of estimates is available from the authors upon request.

of LSOA in the 2km control ring. These findings suggest that LEGI funding may have resulted in a considerable displacement of employment from untreated to treated areas, and that the displacement is stronger the closer an untreated area is to the border of a LEGI area. The results based on 5km and 8km LEGI neighbourhoods display a similar pattern. In both of these specifications, note that the estimate in column 1 comparing treated output areas to *all* untreated output areas within the LEGI neighbourhood is smaller than the corresponding coefficient for 2km LEGI neighbourhoods. Again, this is consistent with significant displacement to treated LSOA from untreated LSOA close to the LEGI boundary. This displacement pushes down the estimated coefficient as we widen the LEGI neighbourhood, because the estimate for 2km neighbourhoods includes only output areas nearest the LEGI border and is thus more affected by displacement than the corresponding estimates for broader LEGI neighbourhoods where the control group also includes untreated super output areas further away from the LEGI border.

Results for the impact of LEGI on worklessness are reported in Table 2. In this table, none of the coefficients on the LEGI dummy are significant although the standard errors are in general of a similar magnitude as for employment. It is worth noting, however, that the most precise point estimates of the treatment effect in column (1) are all negative. This is in line with the positive effects of LEGI on employment (Table 1) as higher growth rate of employment should lower the growth rate of unemployment. That said, overall, these results indicate that LEGI had no detectable impact on worklessness when we compare outcomes for treated LSOA to neighbouring LSOA (controlling for observable characteristics). This finding is easy to reconcile with our findings of positive impacts on employment, once we recognise that local labour markets almost certainly cross most LEGI boundaries so that workers who take the additional jobs may easily be from neighbouring untreated areas. Finally, Table 3 displays the results for business formation measured as a log-change in the number of businesses. Again while the point estimates are positive, all coefficients on the LEGI dummy are insignificant. Taken at face value, this would suggest that the additional employment we identified in table 1 is being created in existing rather than new firms. Caution is needed in interpreting the results for worklessness and number of businesses, because our estimates using Fuzzy Regression Discontinuity design do detect positive impacts, suggesting that spatial differencing may

not eliminate all unobserved heterogeneity correlated with LEGI treatment. It is worth noting, however, that the OLS estimates based on spatial-differencing and IV estimates based on the eligibility rule may be different even if both are consistent, because the OLS procedure recovers an estimate for the average treatment effect (ATE) while the IV procedure recovers an estimate for the local average treatment effect (LATE). Indeed, the negative point estimate for worklessness and positive point estimates for number of businesses are in line with the significant coefficient estimates we get for these two outcomes when using FRD. It is to these results that we now turn.

Results from the Fuzzy Regression Discontinuity design (equation 5) are reported in tables 4-6. Again, there are three panels, reporting results using three different LEGI neighbourhoods (all super output areas within 2km, 5km and 8km of the LEGI boundary, respectively). Within each panel, the first row reports the coefficient on the LEGI dummy in the second stage (i.e. the coefficients from the regression involving changes in the outcome of interest regressed on the LEGI dummy, a higher order polynomial of the IMD rank plus controls). The second row reports the first-stage coefficient on the NRF instrument, a dummy that indicates whether or not the LSOA is in a Local Authority eligible for NRF (i.e. the coefficients from the regression of the LEGI dummy on NRF eligibility status, a higher order polynomial of the IMD rank plus controls). Columns 1 and 2 in each panel display IV estimates for specifications excluding LEGI neighbourhood fixed effects. Columns 3 and 4 display results for specifications with these LEGI dummies included so that identification comes from variation in LEGI status induced by the NRF instrument within LEGI neighbourhoods. This addresses the potential concern that unobserved neighbourhood level effects may be correlated with the proportion of NRF-eligible LSOA across neighbourhoods. As discussed above, we still include LSOA that are located outside any LEGI neighbourhood to help with the precision of the estimated coefficients on the control variables. Results reported in columns 1 and 3 are based on estimations using a sample that includes all LSOA. We also examine the robustness of our estimates by narrowing the sample around the eligibility threshold (as recommended, for example, by Lee and Lemieux 2009). To do this, we re-run the estimations using a sample that excludes 8293 super output areas located in local authority areas with a minimum IMD rank higher than 175. Results based on this restricted sample are displayed in columns 2

and 4 of each panel.

Table 4 reports the FRD results for employment. Notice that the coefficient estimates reported in the first column are equal across the different panels because the specification includes no area fixed effects. Standard errors may differ across panels, however, because we cluster errors at the LEGI neighbourhood level (even when we do not include LEGI fixed effects). For this specification we find no significant effect of LEGI on employment although the first-stage coefficient on the NRF instrument is large and highly significant. The fact that the first stage coefficient on the NRF instrument is highly significant holds across most specifications pointing to the strength of our instrument. The second column displays results for the pooled restricted sample specification where the sample excludes LSOA located in local authorities ranked above 175 against all six indices of deprivation. The exclusion of super output areas located within the least deprived local authority areas increases the treatment effect estimate, although the precision of the estimation is again low. The standard errors are such that we should not draw strong conclusions on the basis of the differences in the coefficient estimates, but the fact that they do differ underlies our decision to replicate results for this sample when we move to the specification including LEGI neighbourhood fixed effects.

As discussed above, even though we condition on a rich set of observable area characteristics and the minimum IMD rank determining the NRF status, NRF status may be correlated with spatially heterogenous trends in employment (thus making it invalid as an instrument). To address this problem, we estimate specifications combining the FRD and spatial-differencing approaches by including LEGI neighbourhood fixed effects into our IV model. Columns (3) and (4) display the results for the full and the restricted samples, respectively. For all LEGI neighbourhoods (2km, 5km, and 8km) and for both samples the first-stage coefficient on the NRF instrument is more than twice as large as the corresponding IV estimate when we exclude LEGI neighbourhood fixed effects. This is because estimation is based on variation in NRF status within LEGI neighbourhoods, and within these neighbourhoods the fraction of super output areas eligible for NRF is larger than in the full sample. Comparing across the panels, we see that the precision of the estimation improves as the LEGI neighbourhood is expanded because more untreated super output areas (that act as controls for treated LEGI LSOA) are included in

estimation. This is reflected in the decreasing standard error for the first-stage coefficient on the NRF instrument as the LEGI neighbourhood is expanded. With a 2km LEGI neighbourhood the fact that we are trying to identify the effect from a small number of LSOA causes the estimates to be insignificant (although the point estimate is positive). For the 5km LEGI neighbourhoods results are significant at the 10% level, at least when we restrict the sample by excluding LSOAs located in local authorities ranked above 175 against all six indices of deprivation (column 4). For 8km neighbourhoods the coefficient estimates on LEGI treatment are significant at the 10% level for both samples, with the restricted sample giving a positive significant coefficient at the 5% level. To give some idea of magnitudes, the coefficient of 0.0726 for the 8km LEGI neighbourhood estimated on the restricted sample suggests that the average annual growth rate among LEGI super output areas was about 3.6 percentage points higher than for control areas.

As discussed in the methodology section, the FRD approach identifies the LATE. If we assume that the effects of LEGI are heterogenous across areas then this estimate gives us the effect of LEGI on treated LSOA compared to nearby untreated non-NRF super output areas which would have received LEGI funding if the minimum of six IMD ranks in their local authority area was 50 or less. This estimate is clearly of significant policy interest, because it tells us what would have happened if the IMD criterion for NRF funding had been relaxed to make more Local Authorities eligible. If we are willing to assume that the effects of LEGI are similar across super output areas, then the LATE is equal to the average treatment effect (ATE) and tells us what would have happened under a number of different changes to the policy framework. Specifically, it gives the effect if (i) more LEGI areas were funded under the existing scheme, (ii) NRF funding criterion had been relaxed or, indeed, (iii) a Local Authority had been randomly allocated LEGI treatment.

Turning to worklessness, results for the same specifications and LEGI neighbourhoods are reported in Table 5. When we do not include LEGI neighbourhood fixed effects, results suggest no impact of LEGI on worklessness (Table 5, Columns 1 and 2) in either sample. Once we introduce LEGI fixed effects (columns 3 and 4) looking across the panels, we see that, as with employment, we cannot obtain precise estimates using 2km LEGI neighbourhoods. For 5km and 8km LEGI neighbourhoods the results for worklessness are slightly weaker than those for employment in terms of statistical significance. In our

preferred specification (i.e. column 4, included fixed effects on the restricted sample) we found significant effects on employment for both the 5km and 8km LEGI neighbourhood definitions, with the latter coefficient significant at the 5% level. For worklessness, results are only significant for the 8km LEGI neighbourhood definition and then only at the 10% level. That said, point estimates are similar across both 5km and 8km neighbourhoods highlighting the fact that this is mostly an issue of the precision of estimates rather than the magnitudes of the estimated effects on worklessness. These point estimates are similar in magnitude to the corresponding results for employment. This is what we should expect, as increases in unemployment are expected to show up in reduced worklessness provided that incoming residents or commuters from other areas are not occupying all new jobs.

Finally, when we consider the number of business, in Table 6 we find some weak evidence of positive impacts on business formation even when we exclude LEGI neighbourhood dummies, providing that we restrict the sample by excluding LSOAs located in local authorities ranked above 175 against all six indices of deprivation (column 2). As for both employment and worklessness estimates of the treatment effect for IV specifications with LEGI neighbourhood fixed effects defined on 2km LEGI neighbourhoods are insignificant (Table 6, Columns 3 and 4). For 5km and 8km LEGI neighbourhoods we find highly statistically significant effects (with the significance somewhat stronger than for employment and considerably stronger than for worklessness). Again, as for worklessness, the estimated effects are of very similar magnitude to the corresponding estimates for employment. The similar magnitude of the impact of the programme on employment and business formation suggests that a substantial fraction of employment resulting from LEGI funding resulted from new businesses starting up in the LEGI area.

[TO DO: Results combining rings and FRD to look at the issue of displacement]

[TO DO: Robustness tests for FRD approach experimenting with different order of IMD rank polynomials and different bands around the threshold]

[TO DO: Descriptive and diagnostic figures for the FRD design.]

6 Conclusions

We assess the causal impact of a significant area based initiative that aimed to raise productivity and employment in England’s most deprived neighbourhoods. To identify

causal impacts of the scheme we use detailed spatial data combined with strategies based on two institutional features of the scheme. Specifically, we use the fact that treatment areas are spatially bounded to compare treated areas to nearby untreated areas and the fact that only certain local authorities are eligible for funding to compare treated areas to similar areas that are untreated due to the ineligibility of the local authority of which they are a part. Our preliminary results suggest that the scheme may have positively effected both employment and number of businesses as well as reducing worklessness. However, we also find evidence that these impacts come at the expense of nearby areas. In future versions of the paper we intend to improve the analysis along a number of dimensions, in addition to calculating whether the overall scheme has positive impacts net of displacement.

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Figure 1: South-East England LEGI areas (Croydon, Barking & Dagenham, and Hastings & Rother) and 1km control rings within 10 km control zones around them.

Table 1: The Impact of LEGI on Employment. Log changes 2005-2007.

		1km Control Rings			
	(1)	(2)	(3)	(4)	(5)
2 km LEGI Neighbourhoods					
LEGI	0.0453** (0.022)	0.0985*** (0.022)	0.0316 (0.024)		
1km control ring			-0.0669*** (0.023)		
2km control ring		0.0669*** (0.023)			
#(LEGI neighbourhoods)	354	354	354		
5 km LEGI Neighbourhoods					
LEGI	0.0261 (0.019)	0.102*** (0.029)	0.0341 (0.026)	-0.00855 (0.025)	0.0262 (0.025)
1km control ring			-0.0682*** (0.024)	-0.111*** (0.040)	-0.0761*** (0.019)
2km control ring		0.0682*** (0.024)		-0.0427* (0.024)	-0.00795 (0.022)
3km control ring		0.111*** (0.040)	0.0427* (0.024)		0.0347 (0.032)
4km control ring		0.0761*** (0.019)	0.00795 (0.022)	-0.0347 (0.032)	
#(LEGI neighbourhoods)	353	353	353	353	353
8 km LEGI Neighbourhoods					
LEGI	0.0251 (0.021)	0.102*** (0.028)	0.0366 (0.025)	-0.00456 (0.026)	0.0311 (0.025)
1km control ring			-0.0650*** (0.025)	-0.106*** (0.040)	-0.0705*** (0.018)
2km control ring		0.0650*** (0.025)		-0.0412* (0.024)	-0.00548 (0.021)
3km control ring		0.106*** (0.040)	0.0412* (0.024)		0.0357 (0.032)
4km control ring		0.0705*** (0.018)	0.00548 (0.021)	-0.0357 (0.032)	
#(LEGI neighbourhoods)	348	348	348	348	348
#(Observations)	32473	32473	32473	32473	32473

Notes: All specifications include a full set regional controls and LEGI neighbourhood fixed effects. For control ring specifications indicators for all control rings are included but coefficients are shown only for the four closest rings. 90, 95 and 99 % confidence levels are denoted by “*”, “**”, and “***”, respectively.

Table 2: The Impact of LEGI on Worklessness. Log changes 2005-2007.

		1km Control Rings			
	(1)	(2)	(3)	(4)	(5)
2 km LEGI Neighbourhoods					
LEGI	-0.00463 (0.0208)	0.00269 (0.0283)	-0.00651 (0.0209)		
1km control ring			-0.00920 (0.0213)		
2km control ring		0.00920 (0.0213)			
#(LEGI neighbourhoods)	354	354	354		
5 km LEGI Neighbourhoods					
LEGI	-0.00696 (0.0182)	0.00231 (0.0259)	-0.00249 (0.0220)	-0.00722 (0.0195)	-0.00466 (0.0204)
1km control ring			-0.00480 (0.0199)	-0.00953 (0.0195)	-0.00697 (0.0179)
2km control ring		0.00480 (0.0199)		-0.00473 (0.0154)	-0.00217 (0.0149)
3km control ring		0.00953 (0.0195)	0.00473 (0.0154)		0.00256 (0.0144)
4km control ring		0.00697 (0.0179)	0.00217 (0.0149)	-0.00256 (0.0144)	
#(LEGI neighbourhoods)	353	353	353	353	353
8 km LEGI Neighbourhoods					
LEGI	-0.00551 (0.0163)	0.00911 (0.0247)	0.00284 (0.0209)	-0.00407 (0.0190)	-0.00303 (0.0206)
1km control ring			-0.00627 (0.0201)	-0.0132 (0.0198)	-0.0121 (0.0186)
2km control ring		0.00627 (0.0201)		-0.00692 (0.0152)	-0.00587 (0.0147)
3km control ring		0.0132 (0.0198)	0.00692 (0.0152)		0.00104 (0.0142)
4km control ring		0.0121 (0.0186)	0.00587 (0.0147)	-0.00104 (0.0142)	
#(LEGI neighbourhoods)	348	348	348	348	348
#(Observations)	32473	32473	32473	32473	32473

Notes: All specifications include a full set regional controls and LEGI neighbourhood fixed effects. For control ring specifications indicators for all control rings are included but coefficients are shown only for the four closest rings. 90, 95 and 99 % confidence levels are denoted by “*”, “**”, and “***”, respectively.

Table 3: The Impact of LEGI on Number of Businesses. Log changes 2005-2007.

		1km Control Rings			
	(1)	(2)	(3)	(4)	(5)
		2 km LEGI Neighbourhoods			
LEGI	0.0116 (0.0097)	0.00635 (0.015)	0.0130 (0.012)		
1km control ring			0.00661 (0.018)		
2km control ring		-0.00661 (0.018)			
#(LEGI neighbourhoods)	354	354	354		
		5 km LEGI Neighbourhoods			
LEGI	0.0151 (0.014)	0.0131 (0.016)	0.0160 (0.014)	0.0168 (0.015)	0.0107 (0.016)
1km control ring			0.00299 (0.019)	0.00375 (0.017)	-0.00233 (0.016)
2km control ring		-0.00299 (0.019)		0.000762 (0.010)	-0.00532 (0.0090)
3km control ring		-0.00375 (0.017)	-0.000762 (0.010)		-0.00608 (0.0085)
4km control ring		0.00233 (0.016)	0.00532 (0.0090)	0.00608 (0.0085)	
#(LEGI neighbourhoods)	353	353	353	353	353
		8 km LEGI Neighbourhoods			
LEGI	0.0128 (0.013)	0.00714 (0.016)	0.0130 (0.014)	0.0146 (0.016)	0.00906 (0.016)
1km control ring			0.00582 (0.019)	0.00749 (0.017)	0.00192 (0.017)
2km control ring		-0.00582 (0.019)		0.00167 (0.010)	-0.00390 (0.0089)
3km control ring		-0.00749 (0.017)	-0.00167 (0.010)		-0.00557 (0.0085)
4km control ring		-0.00192 (0.017)	0.00390 (0.0089)	0.00557 (0.0085)	
#(LEGI neighbourhoods)	348	348	348	348	348
#(Observations)	32473	32473	32473	32473	32473

Notes: All specifications include a full set regional controls and LEGI neighbourhood fixed effects. For control ring specifications indicators for all control rings are included but coefficients are shown only for the four closest rings. 90, 95 and 99 % confidence levels are denoted by “*”, “**”, and “***”, respectively.

Table 4: The Impact of LEGI on Employment. Log changes 2005-2007. IV estimates.

Minimum IMD rank	Pooled		FE	
	>0 (1)	0-175 (2)	>0 (3)	0-175 (4)
<hr/>				
2 km LEGI neighbourhood				
LEGI	0.0196 (0.056)	0.0363 (0.052)	0.151 (0.11)	0.161 (0.17)
1 km control ring				
<i>1st stage regressions</i>				
NRF	0.293*** (0.10)	0.331** (0.14)	0.762** (0.36)	0.729 (0.52)
NRF / neighbouring LEGI				
#(Observations)	32473	24180	32472	24179
#(LEGI neighbourhoods)			353	220
<hr/>				
5 km LEGI neighbourhood				
LEGI	0.0196 (0.053)	0.0363 (0.050)	0.0516 (0.035)	0.0630* (0.034)
1 km control ring				
<i>1st stage regressions</i>				
NRF	0.293*** (0.097)	0.331** (0.14)	0.863*** (0.24)	0.994*** (0.38)
NRF / neighbouring LEGI				
#(Observations)	32473	24180	32472	24179
#(LEGI neighbourhoods)			352	219
<hr/>				
8 km LEGI neighbourhood				
LEGI	0.0196 (0.055)	0.0363 (0.052)	0.0648* (0.037)	0.0726** (0.036)
1 km control ring				
<i>1st stage regressions</i>				
NRF	0.293*** (0.093)	0.331** (0.14)	0.781*** (0.20)	0.966*** (0.31)
NRF / neighbouring LEGI				
#(Observations)	32473	24180	32470	24177
#(LEGI neighbourhoods)			345	213

Notes: All specifications include a full set regional controls. FE specifications include LEGI neighbourhood fixed effects. 90, 95 and 99 % confidence levels are denoted by “*”, “**”, and “***”, respectively.

Table 5: The Impact of LEGI on Worklessness. Log changes 2005-2007. IV estimates

Minimum IMD rank	Pooled		FE	
	>0 (1)	0-175 (2)	>0 (3)	0-175 (4)
<hr/>				
2 km LEGI Neighbourhoods				
LEGI	-0.0114 (0.0993)	-0.00160 (0.0911)	-0.0402 (0.0448)	-0.0426 (0.0576)
1 km control ring				
<i>1st stage regressions</i>				
NRF	0.295*** (0.100)	0.333** (0.137)	0.760** (0.357)	0.726 (0.516)
NRF / neighbouring LEGI				
#(Observations)	31959	24018	31958	24017
#(LEGI neighbourhoods)			353	math
<hr/>				
5 km LEGI Neighbourhoods				
LEGI	-0.0114 (0.0943)	-0.00160 (0.0871)	-0.0779 (0.0565)	-0.0757 (0.0535)
1 km control ring				
<i>1st stage regressions</i>				
NRF	0.295*** (0.0969)	0.333** (0.137)	0.862*** (0.239)	0.991*** (0.378)
NRF / neighbouring LEGI				
#(Observations)	31959	24018	31958	24017
#(LEGI neighbourhoods)			352	219
<hr/>				
8 km LEGI Neighbourhoods				
LEGI	-0.0114 (0.0941)	-0.00160 (0.0860)	-0.0780* (0.0469)	-0.0611* (0.0365)
1 km control ring				
<i>1st stage regressions</i>				
NRF	0.295*** (0.0937)	0.333** (0.136)	0.779*** (0.200)	0.964*** (0.315)
NRF / neighbouring LEGI				
#(Observations)	31959	24018	31956	24015
#(LEGI neighbourhoods)			345	213

Notes: All specifications include a full set regional controls. FE specifications include LEGI neighbourhood fixed effects. 90, 95 and 99 % confidence levels are denoted by “*”, “**”, and “***”, respectively.

Table 6: The Impact of LEGI on Number of Businesses. Log changes 2005-2007. IV estimates.

Minimum IMD rank	Pooled		FE	
	>0 (1)	0-175 (2)	>0 (3)	0-175 (4)
<hr/>				
	<hr/> 2 km LEGI Neighbourhoods <hr/>			
LEGI	0.0325 (0.029)	0.0559* (0.033)	0.0143 (0.029)	0.0224 (0.033)
1 km control ring				
<i>1st stage regressions</i>				
NRF	0.293*** (0.10)	0.331** (0.14)	0.762** (0.36)	0.729 (0.52)
NRF / neighbouring LEGI				
#(Observations)	32473	24180	32472	24179
#(LEGI neighbourhoods)			353	220
<hr/>				
	<hr/> 5 km LEGI Neighbourhoods <hr/>			
LEGI	0.0325 (0.029)	0.0559* (0.033)	0.0497** (0.023)	0.0621*** (0.022)
1 km control ring				
<i>1st stage regressions</i>				
NRF	0.293*** (0.097)	0.331** (0.14)	0.863*** (0.24)	0.994*** (0.38)
NRF / neighbouring LEGI				
#(Observations)	32473	24180	32472	24179
#(LEGI neighbourhoods)			352	219
<hr/>				
	<hr/> 8 km LEGI Neighbourhoods <hr/>			
LEGI	0.0325 (0.028)	0.0559* (0.032)	0.0450*** (0.016)	0.0575*** (0.019)
1 km control ring				
<i>1st stage regressions</i>				
NRF	0.293*** (0.093)	0.331** (0.14)	0.781*** (0.20)	0.966*** (0.31)
NRF / neighbouring LEGI				
#(Observations)	32473	24180	32470	24177
#(LEGI neighbourhoods)			345	213

Notes: All specifications include a full set regional controls. FE specifications include LEGI neighbourhood fixed effects. 90, 95 and 99 % confidence levels are denoted by “*”, “**”, and “***”, respectively.