Relocation of Public Sector Workers:

The local labour market impact of the Lyons Review

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Abstract

This paper assesses the local labour market impact of a UK public sector relocation initiative labeled the Lyons Review. The review resulted in the dispersal of more than 25,000 civil servants out of London and the South East towards other UK destinations. The objective of the paper is to detect whether inflows of public sector jobs have crowded out private sector activity or stimulated the local provision of additional jobs in the private sector. By applying a difference-in-difference approach, I evaluate the policy impact comparing areas in close proximity to a relocation site with areas further away. I find that the dispersal of public sector workers that followed the implementation of the Lyons Review had a positive impact on local services with a negative, but weaker, impact on manufacturing.

JEL classification: O1, R23, R58, J61

Keywords: Economic development; regional labour markets; regional government policy; job displacement

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1. Introduction

Governments design a variety of place-based policies attempting to reverse the fate of economically declining areas and create employment opportunities for local residents. In the US, Enterprise and Empowerment Zone programmes spur the creation of jobs by providing tax incentives to businesses located in designated areas.[2] Similarly, French Enterprise Zone programmes are targeted at discretely bound areas. The UK government follows a slightly different approach by designing either place-based policies with no-predetermined spatial scale (the Single Regeneration Budget programme), or spatially-bound policies whose funding goes indirectly to businesses through local government (the Local Enterprise Growth Initiative)[3]. The UK government also uses relocation programmes of public sector workers to address regional employment problems and to reduce spatial disparities in income. Strictly speaking, relocation programmes of public sector workers are not 'pure' place-based policies. They address a variety of objectives, including delivering cost savings, re-organising the government estate, and enhancing devolution.

When a public sector job is created in an area, it may have a local 'multiplier effect': It may create additional local jobs as a result of the increased demand for locally produced goods and services. Conversely, a rise in public sector employment may trigger general equilibrium effects in the form of higher local wages or higher housing prices (see Moretti, 2011; Faggio and Overman, 2013). These general equilibrium effects may be stronger than the multiplier effect and result in a crowding out or displacement of local businesses.

The debate on the use of public sector worker relocations as a tool to boost regional development is not new. The UK first government-sponsored review was commissioned in the 1960s (Flemming Review, 1963), followed by the Hardman Review (1973) and by the Lawson-Thatcher Review (1988). Notwithstanding the attention given by the government to the subject, there is scarce evidence of the effects of a public sector relocation programme upon local labour markets. This study tries to fill this gap by assessing the local labour market impact of a public sector relocation initiative labelled the Lyons Review.

In 2004, Sir Michael Lyons led a UK government-sponsored independent study on the scope for public sector relocations out of London and the South East towards other UK destinations. The

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[2] See, for recent evaluations of the US programmes, Neumark and Kolko (2010) and Busso, Gregory and Kline (2013).
[3] See Gibbons, Overman and Sarvimäki (2011) and Einio and Overman (2012) for evaluations of the SRB programme and the LEGI initiative, respectively.

review proposed a relocation of about 20,000 civil servants within a six-year period. Thanks to the adoption of effective 'push' factors (such as relocation targets and property controls), the original target was delivered nearly a year ahead of schedule. By March 2010, the program had relocated more than 25,000 jobs. The 2004 relocation programme addressed a variety of objectives including the government desire to stimulate economic activity in less-prosperous areas and, thus, reducing spatial imbalances between London and periphery areas. To the extent that the relocation programme had any impact on local economic conditions, this paper aims to detect the causal effects of the intervention.

In order to detect any causal impact, I use panel data at a detailed geographical scale (Census 2001 Output Areas) covering years before and after the implementation of the programme. Adapting the approach from Gibbons, Overman and Sarvimäki (2011), I construct a measure of treatment intensity that is a non-parametric function of the distance to a relocation site. This approach also provides a parsimonious way to deal with multiple sites. In other words, I assume that effects are additive and vary by distance. My objective is to evaluate the effect and intensity of the treatment by comparing areas in close proximity to a relocation site with areas further away. In order to do that, I apply a difference-in-difference estimation approach and look at the changes in outcome across areas before and after the implementation of the programme. In the main analysis, I compare neighbouring areas that are similar in terms of initial economic and demographic characteristics.

This study finds that the dispersal of public sector workers that followed the implementation of the Lyons Review (2004) had a small positive impact on local services and a negative, but weaker, impact on manufacturing. Results seem robust to a series of robustness checks: including pre-trends in the estimation; conducting a placebo experiment and looking at the impact of the relocation programme on (1998-2002) variation in outcomes; and testing for causality in the spirit of Granger (1969). Furthermore, the analysis is extended to measure the contribution of yearly relocations to total 2003-2008 variation in outcome; to test whether bigger relocations are associated with a larger policy impact; and to explore which demand channel (either consumer demand or intermediate demand) is more likely to explain the positive impact on services. Two robustness checks based on modified versions of spatial differencing conclude the analysis.

This paper makes an original contribution to the literature on the dispersal of public sector workers. To my knowledge, no previous study has looked at the local impact of a public sector relocation programme using a difference-in-difference approach and fine spatial scale data as

presently available. Previous out-of-London relocation studies have focused on the financial costs and benefits of the moves (see, among others, Goddard and Pye, 1977; Ashcroft et al., 1988; Marshall et al., 1991; Deloitte, 2004); some have provided descriptive evidence usually based on interviews with internal managers responsible for implementing relocations and/or secondary data sources (see Marshall et al., 2003; Experian, 2004); others have used regional input-output models[4] aimed at *ex-ante* predicting the local multiplier impact of proposed dispersals (see Ashcroft and Swales, 1982a and 1982b; Ashcroft et al., 1988; Experian, 2004).

Furthermore, the debate on public sector relocation is not limited to the UK.[5] Little attention, however, has been paid in previous research to estimate the local labour market impact of a relocation programme. Faggio and Overman (2013) made a first step in the right direction by looking at the impact of public sector employment on local labour markets. They use fairly detailed geographical information on 352 English LAs and look at changes in total public sector employment during the period 2003-2007, but they do not analyse the local impact of a relocation programme. Interestingly, they find that public sector employment does not have any impact on total private sector employment at the LA level. They do find, however, that public sector employment impacts the local composition of private sector jobs.

A recent paper by Becker, Heblich and Sturm (2012) looks at the rise of Bonn as the new federal capital of Western Germany at the end of World War II. Becker *et al.* (2012) investigate how this historic public sector relocation from Berlin to Bonn changed the local economic structure of the new capital relative to other cities of similar initial size and demographic characteristics. They find limited effects given the extent and relevance of the relocation.

This paper also contributes to the growing literature on the evaluation of place-based government policies. As also noted by Einio and Overman (2012), earlier studies were impaired by the problem of non-random placement[6]. Later studies have combined data at a finer spatial scale with well-designed identification strategies to overcome the problem of causal inference in non-experimental settings. In the US, Enterprise and Empowerment Zone programmes have been successfully evaluated by Neumark and Kolko (2010); Busso and Kline (2008); and Busso, Gregory

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[4] There is an extensive literature on regional input-output models. See Miller and Blair (2009) for a textbook reference and Faggio and Overman (2013) for a discussion in this context.
[5] See, among others, Daniels (1985), Clarke (1998), and Guyomarch (1999) for France; Cochrane and Passmore (2001), Haeussermann and Kapphen (2003) for Germany; Myung-Jin Jun (2007) for Korea.
[6] As pointed out by the literature on program treatment effects (see Heckman, LaLonde and Smith, 1999; DiNardo and Lee, 2011), the problem of causal inference in non-experimental evaluations can be substantial.

and Kline (2013). Other less well-known programmes, like the New Market Tax Credit, have also been carefully evaluated (see Freedman, 2012 and 2013). In Europe and the UK, evaluations of the French Enterprise zone programmes, the UK LEGI and the UK Single Regeneration Budget stand out for accuracy.[7]

The remainder of the paper is structured as follows: Section 2 provides background on the relocation program, Section 3 discusses a simple conceptual framework and Section 4 introduces the empirical strategy. While Section 5 describes the data used, Section 6 presents the main results. Section 7 concludes.

2. The institutional setting

In 2004, Sir Michael Lyons led a government-sponsored review on the scope for relocating central government activities out of London and the South East to more peripheral regions. The review proposed the dispersal of about 20,000 civil service jobs within the six-year period ending in March 2010. The programme developed very strong 'push' factors (such as relocation targets and property controls) to drive posts out of London at an early stage. Such targets were agreed with departments as part of the review process. Each department was then accountable for delivering its own target by March 2010. Property controls stipulated that any government agency wishing to extend the government's property commitment in London or in the South East submit a formal business case for approval. This requirement changed expectations across government: departments needed to justify their presence in London on the grounds of business needs. Thanks to these push factors, the original target was delivered nearly a year ahead of schedule. By its end, the programme relocated more than 25,000 jobs.

The Lyons Review had, among others, several main objectives: delivering cost savings to taxpayers by reducing accommodation and labour costs; allowing the modernization of public services; enhancing devolution; and reversing regional decline.

Property costs tend to be higher in London than elsewhere in the country and, most crucially, 14 per cent of government offices (25 per cent of national expenditure) are located in the prime-cost area of Central London (see Smith, 2010). Despite the national pay scheme, public sector wages also

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[7] See Gobillon, Magnac and Selod, 2012; Mayer, Mayneris and Py, 2011; Einio and Overman, 2012; Gibbons, Overman and Sarvimäki, 2012.

tend to be higher in London than in the rest of the UK because of the London weighting allowance. Due to the allure of private sector job opportunities, there are higher retaining and turnover costs.

In the Expedian (2004) report (which provides background research for the Lyons Review), relocation is also described as a catalyst for re-organising public services and adopting a performance-driven culture across government departments. It is not accidental that the Lyons Review recommendations were implemented as a strand of the Gershon Efficiency Review (2004) whose primary objective was civil service modernisation.

The primary government benefits of devolution, namely reducing cost pressures and relieving spatial constraints, moves hand in hand with the public benefits of important central government organs being close to the people, increased confidence and transparency in government decisions; and an increased sense of belonging. An additional purpose of public sector relocation is to boost regional growth in UK peripheral areas in an attempt to correct the spatial imbalance between a rich South-East and less prosperous regions in the North and the West. This study is about evaluating the programme in light of its ability to achieve this last objective.

Larkin (2010) noted that the size of the government's relocation programme was fairly small. Over the period 2004-2010, the programme dispersed 25,420 jobs out of London and the South East[8]. This figure represents about 5 per cent of total civil service employment (full-time equivalent) working in Britain before the relocations began (Civil Service Statistics, 2003). 5 per cent is not a large number. Looking at the statistics in context, however, reveals that less than one fifth of all civil servants worked in the capital in 2003 and over 70 per cent worked outside London and the South East. Therefore, the programme relocated about 20 per cent of all government jobs initially housed in London or around 17 per cent of those in London and the South East. These are no trivial figures.

What is certainly more interesting is the average number of jobs that successful Local Authorities (LAs) managed to attract under the relocation process. Out of 408, 116 LAs attracted on average 670 jobs with a standard deviation of around 520. The dispersion is large: those 116 LAs received between 1 and 1,600 full-time equivalent jobs. At the 2001 Census Output Area level, which is the detailed level of geography used in the present paper, the average number of jobs relocated in receiving Output Areas (OAs) was 270 with a standard deviation of 348. 282 OAs (out

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[8] I could collect information on the original locations of 20,550 jobs (out of 25,420). While 82 percent of these moves were out of London, 18 per cent were out of the South East region.

of about 160,000)[9] were chosen as the preferred destination of between 0.5 and 1276 full-time equivalent civil service jobs.

When reading background documentation to the Lyons Review (see, e.g., Experian, 2004; Deloitte, 2004), it is not clear why some destinations were chosen instead of others. Experian (2004) recommended the government '*not to choose a building just because is available*', thus suggesting that this might have been the case in past relocations. In addition, it also recommended to phase staff moves in manageable chunks, again endorsing the idea of choosing buildings with a long-run perspective. Furthermore, limited information is available on how relocation decisions were made. Although the Office of Government Commerce (OGC) had the overall responsibility to rationalise the civil service estate and oversee departmental relocations, each individual department was accountable for managing its own relocation programme, including filling posts that were transferred, or created, in the new location.

As documented in Smith (2010), the implementation process lacked transparency: there was no government strategic or unified framework according to which all relocation decisions should have been made. Since coordination across departments was not emphasised, each department sought to maximise its own business interest and not the Exchequer's wider objective. Even within departmental families, departments did not take direct responsibility for the location choices of their own agencies and Non-Departmental Public Bodies (NDPBs).

An additional point of concern (see Larkin, 2010; Smith, 2010) is the amount of public resources used by UK Local Authorities in attracting public sector relocations. Again, information regarding potential destination sites was not collected and made available to all departments in a transparent way. On the contrary, relocation decision-making was open to marketing campaigns (often generic) of individual cities. Finally, there is no central record of how many workers actually moved with the posts and there are no details of relocations packages offered, compensations taken or incentives given. The Smith Review (2010), which followed in the footsteps of the Lyons Review, indicates that lack of planning and transparency resulted in higher-than-expected upfront relocation costs.

As a preliminary way of looking at the relocation data, I collect information on pre-treatment (2003) characteristics of Local Authorities in England, Wales and Scotland (see Faggio and

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[9] Output Areas are very small geographical areas built from clusters of adjacent unit postcodes. See Section 5 for further details.

Overman, 2013)[10]. Geocoding the location of relocation sites, I associated relocation sites with the corresponding Local Authorities. I then distinguish between LAs chosen as actual relocation destinations, LAs not chosen, and LAs in London and the South East from which all relocations originated. Overall, there is a remarkable similarity between LAs chosen and LAs not chosen as a relocation site (see Table 1) giving some support to the idea that the relocation sites might be considered 'almost randomly assigned'. Looking at demographic variables, educational shares are very similar (almost identical) between these two groups of LAs. Conversely, there is a higher proportion of college graduates and a lower share of people with no qualification in London and the South East[11].

Contrary to expectations, actual relocations are in LAs with a higher initial share of public sector employment. If anything, I would expect additional public sector jobs to go to areas with a limited public sector presence. Relocations are also in areas with higher unemployment rates; slightly higher inactivity rates; and a lower share of entrepreneurial activity.

Looking at political variables (see Table 1), the share of Labour votes at the 1983, 1997 and 2005 general elections is consistently higher (although not statistically different) for the group of LAs chosen as a relocation site as opposed to the group of LAs not chosen. In addition, support for Labour seems to grow over time within the group of chosen LAs.

Finally, as I restrict the analysis to the 352 LAs in England for which the 2004 Index of Multiple Deprivation (IMD) is available [12], a larger proportion of deprived areas are within the group of LAs chosen as opposed to the other group. Nevertheless, there were a considerable share (31%) of deprived areas that were not chosen as a relocation site.

Overall, descriptive statistics presented so far show an important similarity between the two groups of LAs (chosen and not chosen), thus suggesting that the problem of non-random placement of this relocation exercise might not be as serious as initially appeared.

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[10] LA data are retrieved from BIS Local Authority data (2003-2007); ABI Local Authority data (2003-2007); Local Area Labour Force Survey (1999-2003); ONS Model-based Estimates of Unemployment (1999-2007); Land Registry data (2000-2010); 2004 Index of Multiple Deprivation statistics. Shares of labour votes at the 1983, 1997 and 2005 elections as well as real mix-adjusted house price index have been kindly provided by Christian Hilber and Wouter Vermeulen.
11 By conducting a simple T-test on the equality of means, educational shares are not significantly different between the groups of potential and actual relocation areas.
[12] The IMD index identifies the poorest LAs in England ranking areas from least deprived to most deprived according to six social and economic dimensions.

3. Conceptual Framework [Incomplete – Please skip this part]

On pure economic grounds, place-based policies do not appear fully justifiable (see Glaeser and Gottlieb, 2008). As the government relocates economic activity into unproductive areas, spatial equilibrium considerations suggest that local residents may not truly benefit from the policy. They could find themselves in a new equilibrium characterised by higher wages and higher prices (including housing prices)—all feeding through to a possible higher cost of living.

Obviously, there are differences between moving jobs or workers in terms of local supply and demand for labour in the destination area. As the public sector expands, the local demand for labour also expands. If workers move with jobs, the local supply expands with the demand with no (or little) impact on local wages. Sure, the public sector could open additional vacancies in the new area attracting local workers who are unemployed or employed somewhere else (assuming complete mobility of workers across sectors within an area but no mobility across areas). In addition, the effect on local wages could be complicated by the existence of a national pay scheme: the higher wage earned in the public sector would draw an increasing number of workers out of the private sector and into the public sector.

If workers do not move with jobs, the local supply does not expand with the demand creating upward pressure on local wages (again, there is no mobility across areas). In order to keep labour costs down, private sector employers (mainly in tradable goods) will be tempted to leave the area. Private sector workers might then choose whether to follow their current employer outside the area or search for a new job inside the area (either private or public). As in the previous case, higher wages in the public sector guaranteed by a national pay scale would attract workers in the public sector.

In the former case, public sector employment would crowd out private sector employment if a national pay scheme kept public sector wages artificially high or the public sector offered additional vacancies expanding its local presence even further. In the latter case, general equilibrium considerations would already encourage tradable sector employers to leave the area. Again, higher public sector pay might pull high quality private sector workers into public employment and push private businesses out of the area.

If I relax the assumption of no worker mobility across local labour markets, workers will come from adjacent labour markets in response to an upward pressure on wages brought about by an inflow of public sector jobs (not workers) in to the area or by additional public sector local

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vacancies. Yet, it is reasonable to suggest that longer term higher public sector salaries do create a local distortion that works against the private sector.

4. The Empirical Strategy

There are methodological problems associated with ex-post evaluations and the two concepts of additionality and deadweight may be the most challenging of them. Additionality refers to the 'amount of output from a policy as compared with what would have occurred without the government intervention' (HM Treasury, 1988). Expressed otherwise, additionality would require to answer the following question: what would have happened in a situation in the absence of a particular policy intervention or measure? Needless to say, it is impossible to know what would happen in any of the chosen locales had they not been allocated any public sector jobs. In the literature (see Heckman et al.,1999, DiNardo and Lee, 2011), a way of solving this additionality problem is by comparing treated sites with a suitable control group.

The related concept of deadweight can be defined as 'that part of a public expenditure programme which is taken up by recipients other than those to whom the expenditure should, if possible, be directed…' (HM Treasury, 1988). Some amount of deadweight is inevitable in every policy intervention. Often is it difficult to evaluate the extent of the loss. Special forms of deadweight are displacement and crowding out. For instance, businesses might decide to relocate in the proximity to a treated site where the demand is higher, pulling up employment in nearby areas and down in areas further away (displacement effect).[13] Alternatively, public sector employment might put upward pressure on local wages forcing local businesses to move out of the areas into less costly locales (crowding out effect). Evaluating the extent of additionality, crowding out and displacement are the main issues of this paper.

My ex-post evaluation has additional methodological challenges. Firstly, I deal with places instead of people (see Glaeser and Gottlieb, 2008; Einio and Overman, 2012, for a discussion). It is typically harder to identify suitable control groups for places rather than individuals: detailed geographical level statistics are not easy to find. In addition, area-based policies raise questions about 'people versus area' effects. When investigating a place-based policy, we are often interested in detecting its impact on the people originally living or working in the area. Unfortunately, area

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[13] Displacement refers to the extent to which the generation of a desirable outcome in one area leads to a loss of the same output in another areas.

level statistics may be contaminated by people leaving the treated areas during the implementation of the policy; thereby reflecting both the change in neighbourhood composition and the extent of any policy impact.

Secondly, it is hard to measure the causal impact of interventions that are not randomly assigned (see DiNardo and Lee, 2012). Recent studies (see, e.g., Busso and Kline, 2008; Busso, Gregory and Kline, 2013; Neumark and Kolko, 2010; Einio and Overman, 2012) have successfully combined empirical strategies, such as spatial differencing as well as comparing earlier and later rounds of specific policies, with institutional details for helping identification. In this study, limited information is available on how government selected relocation sites. The little we know is that relocations were implemented outside a unified framework (Smith, 2010). Coupled with this, the similarity (documented in Section 2) between areas chosen as relocation sites and areas not chosen suggest that the non-random nature of this relocation exercise might not be as severe as initially thought. Furthermore, the difference-in-difference approach used in the present study deals with time-fixed area unobservables (but not time-varying shocks).

A third methodological challenge is the fact that the spatial scale of the potential treatment effect is not known *a priori*. I can identify relocation sites at the level of a postcode, but I do not know the spatial impact of the relocation. On one hand, the effects of creating additional public sector jobs may be very localised to the point of impacting only the persons previously working in the chosen building. On the other hand, they might spread across several kilometres, when they stimulate additional activity in terms of public workers' intermediate demand for consultancy and legal work or final demand for catering and personal services. Furthermore, crowding out and displacement effects might operate: public sector employment might crowd out local activity in the receiving areas or it might cause businesses in outlying areas to displace themselves and relocate into the local area. Finally, locations might be affected by multiple relocations further complicating the picture.

Using a difference-in-difference approach, this study investigates the impact of treatment intensity variables on the outcome focusing on locations with similar pre-treatment observational characteristics used as control group. Consider the estimation equation:

$$\Delta y_i = \sum_c^C I_i^c \Delta N_i^c \gamma^c + \sum_n^N \beta^n X_{i,2001\,(or\,1991)}^n + \Delta \varepsilon_i \tag{1}$$

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where Δy_i is the log change in the outcome measure of interest over the period 2003-2008 in a Output Area i. I_i^c is a binary indicator for OA i and buffer or control ring c, equal to 1 if the OA has at least one relocation site within distance c and equal to 0 otherwise. All buffers have a 1km width. ΔN_i^c refers to the total number of jobs moved (i.e., the size of government relocations) an OA faces in control ring c. Alternatively, I assume equal size across all relocations (i.e., $\Delta N_i^c = 1$) so that the binary indicators I_i^c are also used as treatment intensity variables. To be precise, ΔN_i^c (when allowed to vary) refers to the change in the number of public sector jobs moved between March 2003 and December 2007.[14] Since I consider the cumulative dispersal of public sector workers between 2003 and 2007, this variable was clearly zero for all OAs in 2003 and turned positive for some OAs in 2007. $\sum X_{i,2001}^n$ refer to a set of pre-treatment area controls that may include economic activity of residents, industry and occupation structures, age of residents, population density, education shares, household size and dwelling characteristics.[15] ε_i is an error term. All specifications also include Travel-To-Work-Area (TTWA) fixed effects and observations are clustered at the TTWA level.

Adapting the approach from Gibbons, Overman and Sarvimäki (2011), I estimate the impact for several treatment intensities defined as a non-parametric function of the distance to a relocation site. I proceed as follows: I split Britain into about 190,000 census Output Area, which is the unit of observation chosen for the analysis (see Section 5 for more details); I measure the centroid of each OA; I compute the Euclidean distance between each government relocation site and all OA centroids (both expressed in National Grid references); I then compute 1km-wide buffers from each OA centroid and count the number of relocations in each buffer. In doing so, I make the assumption that the effects are additive. I then measure the treatment intensity as an interaction of distance with the size of the relocation, where size refers to the number of jobs moved.

It is worth noting that treatment intensity variables included in the estimation are constructed in a cumulative way: ΔN^1 refers to all relocations (and associated jobs moved) within a 0-1km distance band, ΔN^2 refers to all relocations within a 0-2km distance band (which obviously includes relocations considered in the 0-1km ring also), ΔN^3 refers to all relocations within a 0-3km band (which obviously includes relocations considered in the 0-1km and 1-2km rings also), and so for. This approach captures the impact of relocations in a given distance band isolating their impact from

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[14] While local employment is computed as a change between April 2003 and April 2008, the total number of jobs moved is aggregated up over the period March 2003-December 2007. In doing so, I leave a three-month period between counting the jobs moved and analyzing any policy impact.
[15] For a full list of control variables, see Appendix B.

that of all other relocations. A graphical representation, also adapted from Gibbons, Overman and Sarvimäki (2011), helps clarify. Consider two Output Areas, area A and area B, and two relocation sites, site 1 and site 2 (see Figure 1). Remember that buffers or control rings are constructed around OA centroids and not around relocation sites. If I consider only three 1km-wide buffers, the three treatment intensity vectors in this graphical representation are: $\Delta N^1 = (0,1)$; $\Delta N^2 = (0,1)$; $\Delta N^3 = (2,1)$.

Looking at equation 1, γ^c are the parameters of interest. Time differencing removes OA fixed effects that may be correlated with outcomes, meaning that the OLS estimates of γ^c are robust to time invariant unobserved heterogeneity at the OA level. However, time variant unobserved heterogeneity at the OA level could still affect the results. In other words, equation 1 will provide consistent estimates of the treatment variables as long as, conditional on initial characteristics, unobserved OA level trends in the outcome variables are uncorrelated with the treatment intensity variables. Even with a very rich set of pre-treatment variables, this condition is unlikely to hold.

An emerging literature[16] uses spatial differencing as an initial way to solving this problem. Spatial differencing (i.e., measuring the difference between an area and its neighbour) removes unobserved factors that are common across neighbouring sites and attempts to reduce the bias if these unobservable characteristics are not common but vary across space.

In a standard framework, spatial differencing would, for instance, require to choose two distance thresholds (d_1 and d_2, with $d_2 > d_1$) – defined both as distances to a relocation site– and restrict attention to compare treated areas within d_1 of a relocation site and untreated areas, those between d_1 and d_2. Considering how I construct my treatment intensities, as buffers around OA centroids and potentially reflecting the impact of multiple relocation sites, my empirical strategy already compares areas in close proximity to a relocation site with areas increasingly further way. This comparison is done in a consistent way, kilometre after kilometre (within a TTWA). In principle, equation 1 could include all treatment intensities from 1km distance (to a relocation site) to 50km distance and each treatment intensity would pick up the impact of relocations on outcomes in a given control ring. In practice, this over-parameterisation would not be necessary. Partly, it will not be needed if effects are very localised. Partly, equation 1 could simply include the first 5km or 10km or 15km treatment intensities and then a cumulative treatment intensity variable picking up the policy impact from 6km (or 11km or 16km) to 50km.

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[16] Recent papers have successfully used spatial differencing in evaluating the impact of place-based policies in the US (Neumark and Kolko, 2010; Busso, Gregory and Kline, 2013) and the UK (Gibbons, Overman, Sarvimäki, 2011; Einio and Overman, 2012).

5. Data Construction

This study uses three data sources: Government relocation data provided by the UK Office of Government Commerce (OGC)[17]; the Business Structure Database (BSD); and the UK 1991 and 2001 Censuses of Population.

The Government relocation data are comprehensive: They list the total number of actual job moves within government departments following the implementation of the Lyons Review (2004). They provide information on 25,408 public sector jobs relocated out of London and the South East into peripheral UK destinations between June 2003 and December 2010. The data give details on the date of the move; the government department and business unit involved; the origin or exporting address of the building from which a job was relocated; and the destination or importing address of the building receiving the job.

Not all public sector workers were involved, but only those civil servants working for central government (including government departments, non-ministerial departments and executive agencies) or for special entities called executive Non-Departmental Public Bodies (NDPBs). UK NDPBs are, for instance, the Care Quality Commission or the Competition Commission[18].

In a substantial number of cases, the geographical information on origin and destination addresses was missing or misreported. I checked every address in the dataset and filled out the postcodes when missing, using old government archives, internet search engines and government agency websites. Since staff moves were phased in manageable chunks, I could identify 1,486 distinct relocations (involving more than 25,000 jobs)[19] defined by a precise moving date, the number of jobs moved, a government department, a business unit, and a destination address. The majority of these locales were in England (1126), followed by Wales (222), Scotland (119), and Northern Ireland (19). Given the limited numbers of relocations into Northern Ireland and the usual

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[17] When the Coalition Government came to power in May 2010, the OGC was dismantled and its main functions became part of the Efficiency and Reform Group at the Cabinet Office.

[18] The Lyons Review (2004) gave guidance for the dispersal of civil servants working in government departments (such as HM Treasury or Department of Health); non-ministerial department (such as Food Standard Agency or Ofsted); executive agencies (many of which provide service to the citizens, such as Jobcentre Plus and HM Courts Service); and executive Non-Departmental Public Bodies (NDPBs). Face-to-face public services not provided directly by central government, such as those in health (NHS) trusts, schools, police forces, local authorities were outside the scope of the review. For the rest of the paper, I use government departments as synonymous for central government without making any distinction between the types of public entities considered by the review.

[19] The total number of relocations is 1,522, but 36 destination postcodes were not identified.

difficulties in collecting good quality data for this country, I exclude Northern Ireland from the final sample, which consists of 1,467 relocations, involving about 24,950 job moves within 20 government departments. The bulk of these changes (about 64% of relocations and about 65% of job moves) occurred between June 2003 and December 2007.

It is worth noting two things: Firstly, my analysis focuses on destination areas. It is clearly much more interesting to investigate what happens in areas receiving the additional public sector jobs than knowing what might have happened in the capital where there are buoyant private sector opportunities easily filling up vacant buildings as they appear. Secondly, it proved much harder to identify the exact postcodes in London or the South East where jobs originates. Out of 1522, 407 relocations report no geographical detail. Thirdly, the data provides information on the number of jobs (not workers) moved. I do not know whether a worker who filled the job in London (or in the South East) actually moved with the relocated job[20]. What I do know is that a public sector job was reduced in the capital while an additional public sector job was created in a peripheral area.

The second database I use is the Business Structure Database (BSD), which provides an annual snapshot (taken in April at the closing of the fiscal year) of the Inter-Departmental Business Register (IDBR). The IDBR consists of administrative data collected for revenues and taxation purposes and is constantly updated. Any business liable for value-added taxation (VAT) and/or with at least one employee registered for tax collection will appear on the IDBR. For the year 2012, the VAT threshold for registration was a turnover of taxable goods and services of £77,000, thus suggesting that the BSD might not sample small and very small businesses. Nevertheless, the ONS estimated that for 2004 the businesses listed on the IDBR accounted for approximately 99 per cent of economic activity in the UK.

The BSD data contains information on about 2.4 million business establishments per year over the period 1997-2011 and includes information on each business' date of birth, date of death, postcode, sector of activity (up to 4-digit Standard Industry Classification (SIC) 2003 code) and total employment.[21] Using the postcode, I assign each local unit active in England, Wales and Scotland to a Census 2001 Output Area.

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[20] Civil servants were firstly asked to move with the job. If they did not agree, they could either accept a redundancy package or apply internally for openings in different departments. Anecdotal evidence suggests that a relatively small number of employees actually move with the job, them being usually young and of junior rank positions.

[21] The initial raw data includes approximately 3 million local units every year. However, I carry out a series of checks and drop a number of units as detailed in Appendix A.

Since the study looks at the policy impact on private businesses, I exclude establishments operating in one of the following sectors: Public Administration and Defence; Education; Health and Social Work; Public Utilities (Electricity, Gas and Water). I also exclude businesses operating in Agriculture; Hunting and Forestry; Fishing; Private Households with Employees; and Extra-Territorial Organizations and Bodies.

From the UK 2001 Census, I select a rich set of Output Area variables measuring local labour market characteristics; industry and occupation shares; demographics and population density; household size and types of dwelling, local presence of ethnic minorities; means of transport and average commuting distance.[22] Most figures refer to the people who live in the area, but some (like industry and occupation structures) refer to the people who work in the area. Figures are available for a range of geographical boundaries. I choose the most local, the Census 2001 Output Areas, which builds to larger areas, such as parishes, wards, local and health authorities, constituencies, and towns and cities.

The UK 1991 Census also provides a rich set of similarly defined control variables. The smallest level of geography available in the UK 1991 Census is the 1991 Enumeration District for England and Wales and 1991 Output Area for Scotland. In order to apply a consistent geography over time, 1991 Census data were retrieved at the Enumeration District (and at the 2001 Scottish Output Area) level and then converted into 2001 Output Areas.

Introduced in England and Wales in 2001, output areas are built from clusters of five or six adjacent unit postcodes and represent the smallest standard unit for representing local statistical information. Census OAs were designed to have similar population sizes[23] and be as socially homogenous as possible (based on tenure of household and dwelling type).[24] When first delineated, OAs largely consisted of entirely urban postcodes or entirely rural postcodes. Thus, urban/rural mixes were avoided where possible. In total, there are 165,665 OAs in England; 9,769 in Wales; and 42,604 in Scotland.[25]

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[22] See Appendix B for details.

[23] OAs were required to have a specified minimum size to ensure the confidentiality of data. In England and Wales, the minimum OA size is 40 resident households and 100 resident people, but the recommended target was rather larger at 125 households. UK OAs are significantly smaller than US Census tracts. According to the US Census Bureau, census tracts usually have between 2,500 and 8,000 resident persons.

[24] OAs were introduced in Scotland at the 1981 Census, although their definition changes over time. In Scotland OAs are of relatively smaller size (the minimum OA size is 20 resident households and 50 resident people, with a target size of 50 households) than those in England and Wales. In addition, social homogeneity was not used as a factor in designing Scottish OA boundaries.

[25] See Figure 2 for a geographical illustration of UK 2001 Census Output Areas in central London.

In this version of the paper, I focus on private sector employment as the outcome variable. I compute the rate of private employment growth between 2003 and 2007 for each output area in Britain that reports positive employment in both years. To this end, I aggregate employment data for all BSD establishments that belong to a given OA distinguishing between manufacturing and services. The former consists of employment in all manufacturing activities; the latter includes employment in construction activity, transport, FIRE services as well as trade, catering and personal services. I also use total private employment defined as the sum of manufacturing and services as an additional outcome variable.

I also experiment with splitting services within the private sector by type. I expect newly located civil servants to outsource part of their work to local businesses such as consultancies, legal offices, external auditors or accounting firms. At the same time, I expect newly located civil servants (who earned a higher salary than local private sector employees because of the national pay scheme) to outsource part of their home production activity to coffee shops, takeaways and restaurants; dry-cleaning services; house cleaners; etc.

The final data issue to be resolved concerns the choice of time period. Government relocation data cover the eight year period 2003-2010, but there are some concerns about incorporating the recession which might have played out unevenly across space. In this version of the paper, I focus on a relatively short time period (2003-07) and look at the impact of government relocations after the first five years of the program. Given those five years were also the most intense (counting for about 65% of both relocations and jobs moved), I would be able to detect an impact, if the program had any impact at all.

6. Results

6.1. Preliminary steps

As a preliminary step, I check whether the policy impact is 'local'. Since the analysis uses small scale geographical areas (with an average dimension of 1km^2 and 7km^2 in standard deviation) as well as relocations of fairly modest size, I expect effects (if any) to be quite localised. Using a baseline specification equivalent to the one described in equation 1, I regress the rate of growth in total private employment between 2003 and 2008 on fifty 1km-wide treatment intensities within a 50km area. I find that the policy has an impact and, as expected, its impact is localised, i.e., concentrated within the first one or two kilometres (see Figure 3). Similar pictures are obtained when

splitting total private sector employment into manufacturing and services, although estimates on the 1km treatment intensity seem to go in opposite directions for the two subsectors: falling for manufacturing and rising for services (see Figure 4). This initial exercise suggests reducing substantially the number of treatment intensities included in the estimation without fear of losing crucial information.

Secondly, I conduct a direct test of the treatment. It would be re-assuring to see that public sector employment has indeed risen in destination areas, with no effect observed in areas that did not attract public sector jobs. As noticed in Section 5, the BSD focuses on private businesses and is, thus, less comprehensive in collecting information on public sector employment. In addition, BSD data does not distinguish between private and public sector establishments. A 'crude' but rather common way of splitting the data consists in attributing sectors SIC75 (public administration and defence), SIC80 (education) and SIC85 (health and social work) to the public sector; all the remaining sectors to the private sector. Since the Lyons Review gave guidance for the dispersal of civil servants working for central government and executive agencies, a direct test of the treatment would entail positive employment changes in public administration and defence with no or limited impact on the other two categories. Results seem to confirm these expectations. The binary indicator D_0 identifies those OAs which succeeded in attracting government jobs during the implementation of the Lyons Review. Looking at the coefficient on D_0 in Column (1) of Table 2, total public sector employment rose by about 30 percentage points between 2003 and 2008 in treated OAs, i.e. those receiving government jobs, relative to non-treated ones.

When public sector employment is split into sectors, the 30 percentage point impact appears indeed driven by public administration and defence (see Column 2) as well as health services (see Column 3) with education services playing no role. The positive and significant effect for health services might be counter-intuitive at first. Two alternative explanations are possible, both linked to data limitations. The crude public/private split I use might lead to erroneously classify services within the private sector as public or vice versa. This problem is obviously more severe for sectors characterised by a large private sector presence (education and health rather than public administration and defence). Therefore, the positive rise in health services shown in Column (3) of Table 2 might be explained by an increase in private face-to-face (rather than public) health services in destination areas. An alternative explanation concerns the government relocation data I received from the OGC which list relocations by government departments. Particularly, they list 38 distinct

relocations (involving about 900 jobs, equivalent to about 5.5% of the total jobs relocated) which originated from the UK Department of Health during the period 2003-2007. With the employment data currently available, it is not possible to verify whether employment changes in executive agencies attached to the Department of Health or in the Department of Health itself have been classified as changes in public administration and defence (SIC75) or health services (SIC85). In the latter case, the health coefficient would pick up part of the policy impact in successful areas.

Despite these data limitations, results are encouraging. Looking at Table 2, what is most re-assuring is that all the increase in public sector employment (and particularly public administration and defence employment) is concentrated in the OAs receiving civil servant jobs during the implementation of the Lyons Review. Conversely, public sector employment and its sub-components remained largely unchanged for any area in close proximity to a relocation site (but excluding the relocation site itself), with proximity measured at 1-, 2-, 3-, 4- and 5km. Each binary indicator (D_1 to D_5) captures the policy impact at a given distance band and, apart from some noise in the health sector, estimates are largely insignificant[26].

6.2.Main analysis

My strategy is to assess post-treatment outcomes for areas with similar pre-treatment characteristics. In doing so, I compare OAs in close proximity to a government relocation site with areas further away. In other words, the analysis aims to capture the intensity of the impact assuming it decreases over distance. The final sample of OAs excludes destination areas that were successful in attracting government jobs; that is, the 0-1km distance band includes all OAs apart from those actually receiving the relocated jobs. Only if a destination area was located in close proximity to another destination area, the former would be included in the analysis and treated as an area neighbouring a relocation site.

OAs are also compared within the same TTWA and observations are clustered at the TTWA level in order to (1) partial out fixed TTWA effects and (2) attempt to address any remaining within-group correlation. TTWAs are a measure of local labour markets defined such that at least 75% of the resident population also work in the area. At the same time, 75% of the people working in the area must be resident there. These areas are obviously much larger than OAs, containing an average

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[26] Each coefficient in Table 2 is derived from a separate regression.

of about 1,650 OAs each, and vary in size[27]. There are currently 232 TTWAs across England, Wales and Scotland.

Tables 3a and 3b show results for three outcome variables: total private sector employment, manufacturing and services. Table 3a includes treatment intensity variables defined as binary indicators, equal to 1 if an OA faces, at least, one relocation within a given distance band (0-1km, 0-2km, 0-3km, etc.) or 0 otherwise. Table 3b includes treatment intensities defined as the total number of jobs moved (expressed in logs) within a given distance band. It seems plausible to expect the impact of a policy intervention to vary by size. Thus, Table 3b shows my preferred specifications.

In both tables, Columns (1)-(3) report baseline results without pre-treatment characteristics; Columns (4)-(6) include pre-treatment characteristics as controls. Looking at Columns (1)-(3) in Tables 3a and 3b, there is some evidence that the implementation of the Lyons Review has been detrimental to manufacturing employment in OAs in close proximity to a relocation site. The estimate in the second column of Table 3a is about -0.08 and significant at the 10% level suggesting that manufacturing employment fell on average by 8 percentage points over the period 2003-2008 in OAs at 1km distance to a relocation site. When treatment intensities are based on size (see Column 2 Table 3b), the corresponding estimate is smaller (3 percentage points) and yields a slightly different interpretation: a 1% increase in the number of jobs moved leads to about a 3 percentage point drop in manufacturing employment in locales within a 0-1km distance band. Manufacturing results are, however, not robust to the inclusion of pre-treatment area controls (see Column 5 in Tables 3a and 3b).

On the other hand, the Lyons Review seems to have exerted a positive impact on employment in services within the private sector. Throughout all the analysis, I focus on services only (instead of the total private sector) since results for services mirror those for total private sector employment.[28] Regarding services, results without area controls (see Column 3 in Table 3b) suggest a positive impact of the Lyons Review in areas at 1km distance from a relocation site with a coefficient of about 1 percentage point and significant at the 10% level. That is, a 1% rise in relocated civil servant jobs is associated with about 1 percentage point rise in local services. There is also evidence of a displacement effect, i.e. a tendency for private businesses to reduce the geographical distance to a relocation site, moving out of areas at 2km distance into areas at 1km

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[27] Out of London, the smallest TTWA contains 34 OAs whereas the largest has 5500 OAs.
[28] In this study, total private sector is the sum of manufacturing and services. With manufacturing being a very small part of the total, results on services are essentially the same as those on total private sector employment.

distance. In fact, estimates on the 1km and 2km treatment intensities are of similar magnitude but opposite sign.

When area controls are included in the estimation (see Column 6 in Tables 3a and 3b), results remain largely unchanged in terms of magnitude, but slightly increased in significance. This improvement shows that controlling for pre-treatment characteristics refines the comparison between areas at different distance bands. The estimate of 0.034 in Column (6) in Table 3a indicates that services increased by about 3 percentage points in areas at 1km distance; the corresponding estimate for Table 3b is 1 percentage point, but now significant at the 5% level. Displacement is still apparent when treatment intensities are based on size, whereas there is no evidence of displacement when they are defined as binary indicators.

Consistent with Faggio and Overman (2013), Tables 3a and 3b find evidence that public sector dispersal stimulates local services in neighbouring areas to a relocation site. Conversely, it seems to have a negative impact on manufacturing employment, although evidence on the latter effect is weaker. There are, however, differences between this study and Faggio and Overman (2013). They conduct the analysis at a much higher level of aggregation (using 352 English Local Authorities) than the one used here (based on about 160,000 OAs covering England, Wales and Scotland). In addition, they do not analyse the specific impact of the Lyons Review, but they look at changes in public sector employment as a whole, which includes employees in central and local government, police forces, public schools, NHS trusts, etc. Furthermore, they find that 100 additional public sector workers created in an area between 2003 and 2007 crowds out 50 manufacturing jobs while spurring the creation of about 40 new service jobs. This study finds that the dispersal of civil servant jobs has a positive impact on local services in surrounding areas , but the crowding-out effect on local manufacturing is not robust to the inclusion of pre-treatment area characteristics.

6.3. Robustness Checks

A series of robustness checks is conducted in order to test the validity of the results obtained so far. Readers might be concerned that the negative trend in manufacturing employment that started in the 1980s (and continues today) might affect the estimation. In addition, area-specific unobservables could be driving the positive response of services in areas local to a relocation site. Therefore, I construct pre-trend variables measuring the changes in total private, manufacturing and

services employment during the years 1998-2002 and include them as additional controls in the estimation. Results are hardly changed by the inclusion of pre-trend effects (see Table 4).

As a second robustness check, I conduct a placebo experiment (see Table 5) which consists of analysing the impact of the treatment intensity variables on changes in OA employment prior to the Lyons Review. To this end, I use log 1998-2002 employment changes as the dependent variable in a regression similar to that of equation 1. Because of the different time span, area control variables were retrieved from the 1991 UK Census of Population and constructed similarly to those derived from the 2001 Census. The placebo experiment confirms that there were no OA differences before the implementation of the Lyons Review; but Tables 3a and 3b show that areas behaved differently thereafter.

In a third robustness check, I perform a test for causality in the spirit of Granger (1969) by applying a methodology used by Autor (2003) and described by Angrist and Pischke (2009). The methodology infers causation by looking at the impact of a policy on outcome using data before and after its implementation. Granger causality means that the cause precedes the effect. In this context, the introduction of a policy should precede the effect of it. A way of conducting the test for causality used by Autor (2003) is to investigate the impact of treatment intensity variables on log employment changes before and after the implementation of the Lyons Review. The test consists of running a series of consecutive regressions where the dependent variable is expressed as log changes in my chosen three outcomes from five years ahead to one year behind. Full specifications include initial area controls, TTWA fixed effects and observations are clustered at the TTWA level. For simplicity, Table 6 only reports estimates for the 0-1km distance band in regressions that use treatment intensities based on size.[29] Looking at Table 6, results are consistent with a causal interpretation of my findings. Treatment intensities do not have any impact on local employment in years preceding the implementation of the Lyons Review, but they do when the recommendations of the review are implemented. I cannot, however, detect any impact in the years 2008-2009. Possibly, the 2008 recession continues to play out unevenly across space making harder to disentangle any impact. Alternatively, even if I focus on relocations during the period 2003-2007, the relocation programme lasted until March 2010, further complicating the interpretation of the results. Finally, it could simply be that the impact is short lived.

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[29] Consistent estimates are obtained when using treatment intensities expressed as binary indicators and are available from the author on request.

6.4. Extensions

In order to avoid over-estimating the policy impact, all relocations have been pooled together over the period 2003-2007. Another interesting way of slicing the data is to measure the relative contribution of annual relocations to log 2003-2008 changes in private sector employment. Table 7 reports the results of an estimation similar to the one reported in Table 3b with treatment intensities now varying by distance and time. For instance, the treatment intensity referring to a 0-1km distance over the period 2003-2008 is now split into four components, each capturing a different timing of the relocations: 2003-2004; 2005; 2006; and 2007.[30] Consistent with the results shown in Table 3b, the positive impact of the policy on services employment is concentrated in areas within a 0-1km distance band. Table 7 refines the analysis showing also that the positive impact is driven by relocations occurring both at the beginning and at the end of the period. Interestingly, those estimates are of comparable magnitude and significance (between 1.9 and 2.4 percentage points and at the 5% significance level). Consistent with results shown before, there is also evidence of a displacement effect. Table 7 also shows that displacement is driven by relocations occurring during the years 2003-2004; but it does not affect later relocations. Furthermore, Table 7 indicates that there are additional small positive effects of relocations occurring in 2007 within locales at both 2-3km distance and 3-4km distance bands. Finally, estimates report a negative effect of treatment intensities on both manufacturing and services for relocations occurring in 2006. Manufacturing employment is also affected negatively by early relocations at the 5km distance.

The underlying assumption in constructing treatment intensities based on size is that the intensity of a relocation does not vary only by geographical distance, but also by the number of jobs moved by a particular relocation. Interacting distance with size changes the relative weight given to observations, giving more weight to OAs in proximity to relocations that moved a larger number of jobs relative to OAs close to relocations that moved fewer jobs. In principle, it is reasonable to assume that larger relocations should have a larger impact on employment. The direction of the total effect is unclear, though. The Lyons Review (2004) argued that reaching a critical mass of public sector workers in an area would be crucial for reaping the benefits of the relocation. A large mass of public sector workers (who are, in most areas, also higher wage earners) would stimulate demand for locally produced goods and services. What was not mentioned in the Lyons Review is that moving a substantial number of public sector jobs in a specific areas, where housing/commercial real estate

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[30] Given the limited number of jobs moved in 2003, I combine relocations in 2003 with those in 2004.

supply is limited, could also have an adverse impact on other types of activities located there. For instance, it may cause local activities to close or move somewhere else (an example of crowding out).

A way of testing whether larger relocations are associated with a larger policy impact involves splitting government relocations by distance and quartile class and, so, creating 24 additional treatment intensities variables (in the form of binary indicators). These new treatment intensities replaced previous treatment intensity variables in regressions similar to those presented in Table 3b, Columns (4)-(6). Contrary to expectations, Table 8 shows that the impact of treatment intensities does not vary significantly by size. Particularly, within a 0-1km control ring, the impact of smaller relocations is similar to that of larger relocations. Size seems to matter at a 4km distance only, showing that the intensity of the treatment being negative for smaller relocations and significantly larger and positive for relocations in the top quartile. Conversely, at the 5km distance, small relocations have a much greater impact than larger ones.

Results reported in Tables 2-8 show that the Lyons Review had a positive impact on local services and essentially no impact, when controlling for pre-treatment characteristics, on manufacturing. As pointed out by the relocation literature (see, e.g., Marshall et al, 1991), the arrival of a substantial number of public sector jobs in an area could stimulate demand for local activities (through a multiplier effect, see also Moretti 2010; Faggio and Overman, 2013), both in terms of intermediate demand for consultancy and legal work and/or in terms of consumer demand for catering and personal services. Increases in both types of demand might allure private businesses (if tradable) to displace themselves and relocate into areas at a relative short distance to a relocation site.

In order to investigate which demand channel is more likely to operate, I use a more detailed classification which splits the service sector into four types: construction; transport; trade, catering and personal services; finance, insurance, real estate (FIRE) and business services. The broader category of 'services minus construction' is also added to Tables 9a and 9b (Column 1) to aid comparison. Results indicate that the recent government relocation exercise has spurred intermediate demand and, thus, the provision of local services in the form of FIRE, consultancy, legal and auditing services. Surprisingly, I cannot detect any effect on catering and personal services nor transport nor construction. Considering that business and FIRE services tend to concentrate in certain areas, being driven by the existence of agglomeration economies, whereas catering and

personal services are more evenly spread across space, it seems reasonable to conclude that it is the tradable side of services that is stimulated by the policy. In fact, when using an alternative classification which splits services by tradability (see Jensen and Kletzer, 2006; and Faggio and Overman, 2013), results confirm that tradable (as opposed to non-tradable or medium tradable) services significantly increase in areas likely affected by the Lyons Review.[31]

Splitting services within the private sector by tradability, Faggio and Overman (2013) find that non-tradable (rather than tradable) local services respond positively to a rise in public sector employment. Faggio and Overman (2013) ascribe the result to the differential impact that public sector employment (defined as a non-tradable sector) is likely to have on tradable and non-tradable sectors: that is, a negative impact on tradables and a positive impact on non-tradables. The less tradable is a sector, the more important is local demand and local income in spurring employment in that sector.

A few explanations for these differences are possible. Firstly, it is worth noting that government relocation exercises typically involve the dispersal of public sector jobs that are tradable, i.e., jobs that can be performed almost everywhere because they do not require face-to-face customer interactions. Conversely, NHS staff and teachers (which represents the bulk of public sector employment in Faggio and Overman, 2013) tend to offer face-to-face services that are locally needed. Secondly, the local impact of civil servant dispersals could be quite different from that of a generalised increase of public sector jobs (which likely includes NHS staff, police forces, public school teachers, etc.) because the task content of jobs in different parts of the public sector varies greatly. On the one hand, senior civil servant positions in government are as competitive as senior managerial positions in the private sector. On the other hand, NHS trusts and private health practices compete for workers with a completely different set of skills and training relative to those applying for government jobs. Thirdly, because of the nature of the job itself, civil servants in government tend to create an intermediate demand for consultancy, auditing and legal work than is not common across NHS staff or teachers.

6.5. Pseudo Spatial Differencing

As noted in Section 4, spatial differencing (i.e., measuring the difference between an area and its neighbour) removes unobserved factors that are common across neighbouring sites and attempts to reduce the bias if these unobservable characteristics are not common but vary across space. As

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[31] Results available from the author on request.

already mentioned, a recent strand of the literature has successfully used spatial differencing in evaluating place-based policies[32]. In order to detect whether the intensity of the treatment varies in distance, this paper compares OAs across subsequent control rings, but the analysis so far does not limit the number of control rings included in the estimation (covering an area up to 50km). In this context, robustness checks in a spirit of spatial differencing could entail the following exercises: (1) reducing substantially the number of control rings included in the estimation (up to 5km); (2) expanding incrementally the number of distance bands included. Both exercises allow me to check whether results are sensitive to the number of rings and, thus, the geographical dimension of the 'intensity area' included in the estimation.

Using the smaller sample of OAs within a 0-5km (instead of a 0-50km) distance band, I reproduce the analysis conducted so far. As expected, given the way I define my treatment intensities, which capture the policy impact at each 1km-wide control ring, I find very little difference in the results relative to those obtained before. If any, estimates slightly increase in significance because the exercise allows me to reduce the number of OAs included in the estimation. Only neighbouring areas within a 0-5km zone are compared and, among these, differences in unobservables are likely to be less severe. By improving the quality of the comparison, the crowding-out effect on manufacturing employment becomes robust to the inclusion of area characteristics as well as pre-trends (see Columns 2 and 4 in Table 10b). The policy impact on manufacturing can be detected when the analysis uses treatment intensities defined as size; estimates are still not significant when treatment intensities are expressed as dummies (see Columns 2 and 4 in Table 10a).

The second exercise consists of expanding gradually the treatment intensities or control rings included in the estimation. This exercise allows me to check whether results are sensitive to the number of control rings and, thus, the geographical dimension of the 'intensity area' included in the estimation. Tables 11a and 11b report three and four sets of results, respectively. Panel A presents results comparing Output Areas in the first and second control rings; Panel B adds a third ring expanding to three the rings compared; Panel C adds a fourth ring; and Panel D adds a fifth one. As estimates in Table 10a are based on treatment intensities defined as binary indicators and a constant is included in all specifications, one relocation dummy must be dropped. Thus, Table 11a reports results for Panels A to C; whereas Table 11b for Panel A to D. In both tables, estimations include a

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[32] See, e.g., Neumark and Kolko (2010); Busso, Gregory and Kline (2013); and Einio and Overman (2012).

full set of pre-treatment area characteristics, TTWA fixed effects and observations are clustered at the TTWA level. Looking at Panel A-C of Table 11a, I find that results are consistent across the three specifications and, thus, robust to the inclusion of additional control rings. In addition, estimates are very similar to those reported in Tables 3a and 9a, confirming the existence of a positive impact of the Lyons Review on local services (with an estimate between 3.5 and 4.0 percentage points). Turning to Table 11b, I also find that estimates are consistent across the four panels, showing that the policy intervention had a positive impact on services and a negative, although weaker, impact on manufacturing employment. When comparing OAs within the first and second control rings (see Panel A in Table 11b), there is evidence of a displacement effect: businesses operating in services appear to relocate themselves closer to a relocation site. Displacement is, however, not robust to the inclusion of additional control rings: as the geographical dimension of the 'intensity area' expands, estimates on the 2km treatment intensity remain negative but lose significance.

7. Conclusions

Since World War II, the UK government has used relocation programmes of public sector workers as a tool to address employment problems in declining regions (see Jefferson and Trainor, 1996). In recent years, the move of the BBC from London to Manchester and the relocation of the Office for National Statistics (ONS) headquarters from London to Newport have attracted public attention.[33] Notwithstanding the attention given by the government and the media to the subject, there is scarce evidence of the effects of a public sector relocation programme upon local labour markets. This study has tried to fill this gap by assessing the local labour market impact of a public sector relocation initiative labelled the Lyons Review.

In 2004, Sir Michael Lyons led a UK government-sponsored independent review on the scope for public sector relocations out of London and the South East towards periphery destinations. The review proposed a relocation of about 20,000 civil servants within a six-year period. Thanks to the adoption of effective 'push' factors, the original target was delivered nearly a year ahead of schedule. By March 2010, the program had relocated more than 25,000 jobs.

In order to detect any causal impact of the intervention, I have used panel level data at a detailed geographical scale (UK Census 2001 Output Areas) covering years before and after the implementation of the programme. I have also constructed a measure of treatment intensity (adapted from Gibbons, Overman and Sarvimäki, 2011) that is a non-parametric function of the distance to a relocation site. It is hard to know a priori the spatial extent of the policy. In theory, all locations might be affected by the policy intervention. By constructing these treatment intensities, I have assumed that the policy impact varies over distance with areas in proximity to a relocation site likely to be more affected than areas further away.

Using a difference-in-difference approach, I have evaluated the intensity of the treatment by looking at the changes in outcome across neighbouring areas before and after the implementation of the programme. This study finds that the implementation of the Lyons Review had a positive impact on local services; whereas it had a negative, but weaker impact on manufacturing. The effects appear to be small and concentrated within the first one or two kilometres from a relocation site. Results are robust to a series of robustness checks and extensions. As robustness checks, I have included pre-trends in the estimation; conducted a placebo experiment; looked at the impact of the relocation

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[33]For a recent press article, see Daily Mail (16 May 2013): http://www.dailymail.co.uk/news/article-2325212/BBC-black-hole-Salford-swallows-cash--15k-Gary-Linekers-taxis-talent-commuting-jet.html

programme on (1998-2002) variation in outcomes; and tested for causality in the spirit of Granger (1969). In extending the main analysis, I have found that relocations at the beginning and at the end of the sample period contribute the most towards explaining the total 2003-2008 variation in employment. Contrary to expectations, I have also found that larger relocations do not appear to be associated with a larger policy impact. When exploring alternative demand channels through which the positive impact on services could come about, I found that increased intermediate demand for consultancy and legal work (rather than consumer demand for catering and personal services) is more likely to explain the results. Finally, results were also confirmed when applying two modified versions of spatial differencing.

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Figure 1: Graphical representation of treatment intensity variables
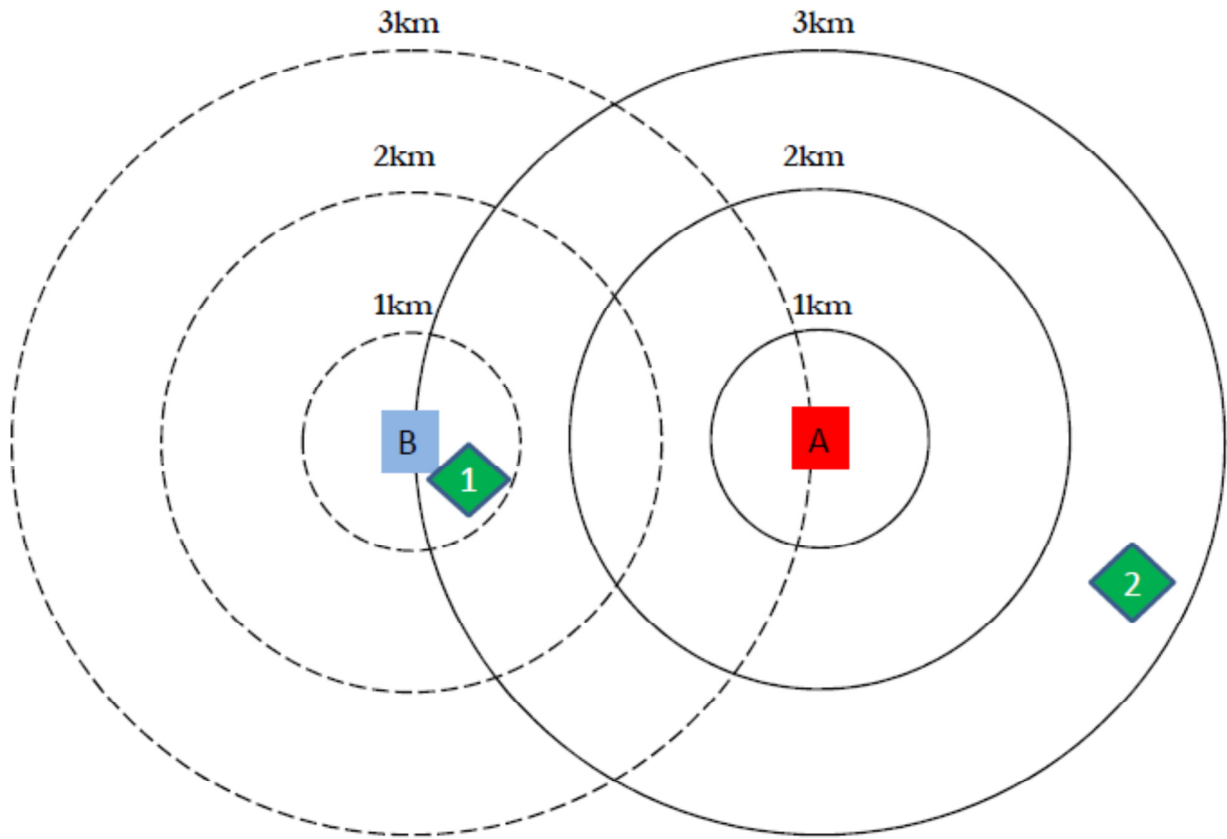
Note: A and B are two Output Area centroids, whereas 1 and 2 are two relocation sites.
Source: author's adaptation from Gibbons, Overman and Sarvimäki (2011).

Figure 2: Geographical illustration of UK 2001 Census Output Areas

St Pancras and Somers Town and surrounding area % of people born outside UK - 2001 Census

% of population born outside UK

ELSEPERC
- 12 - 26
- 27 - 35
- 36 - 44
- 45 - 55
- 56 - 81
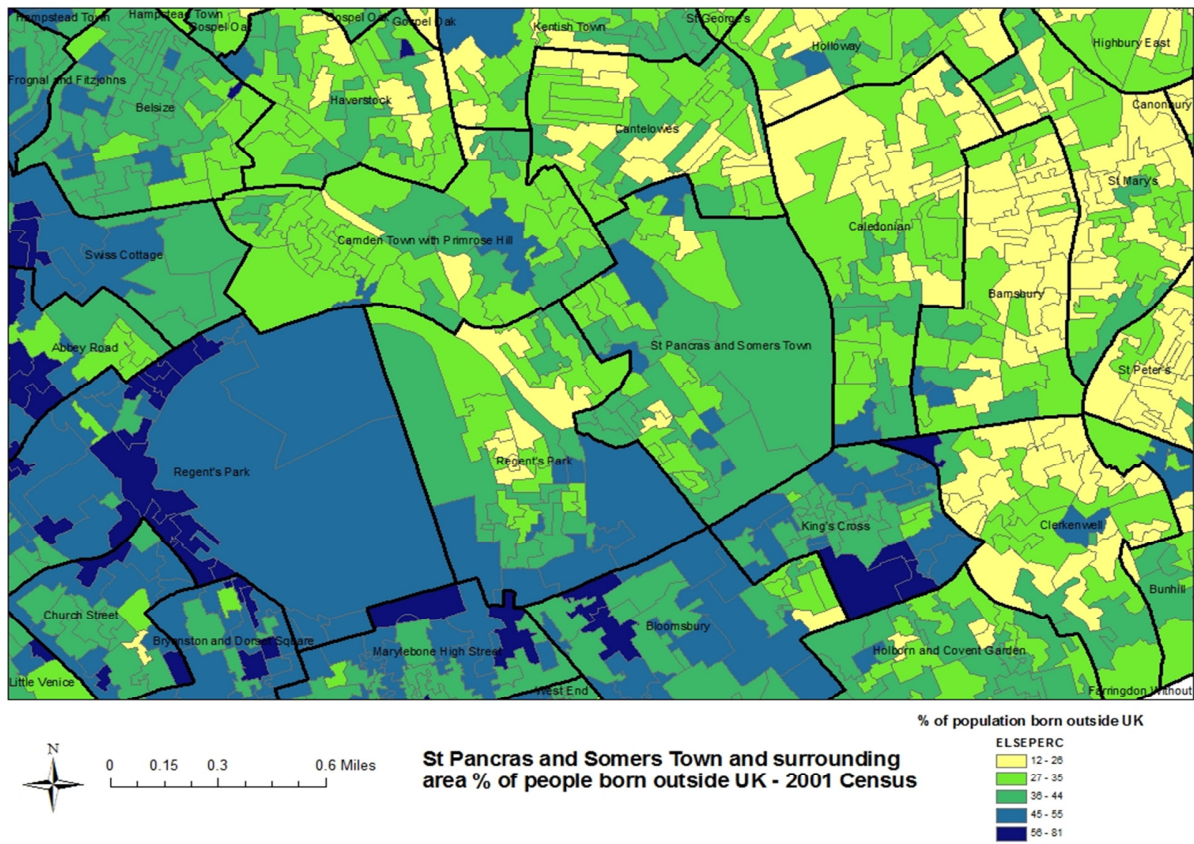
Source:

Figure 3: The impact of the Lyons Review on private sector employment, Log changes 2003-2008
Full specification including 50km treatment intensities and 2001 Census controls
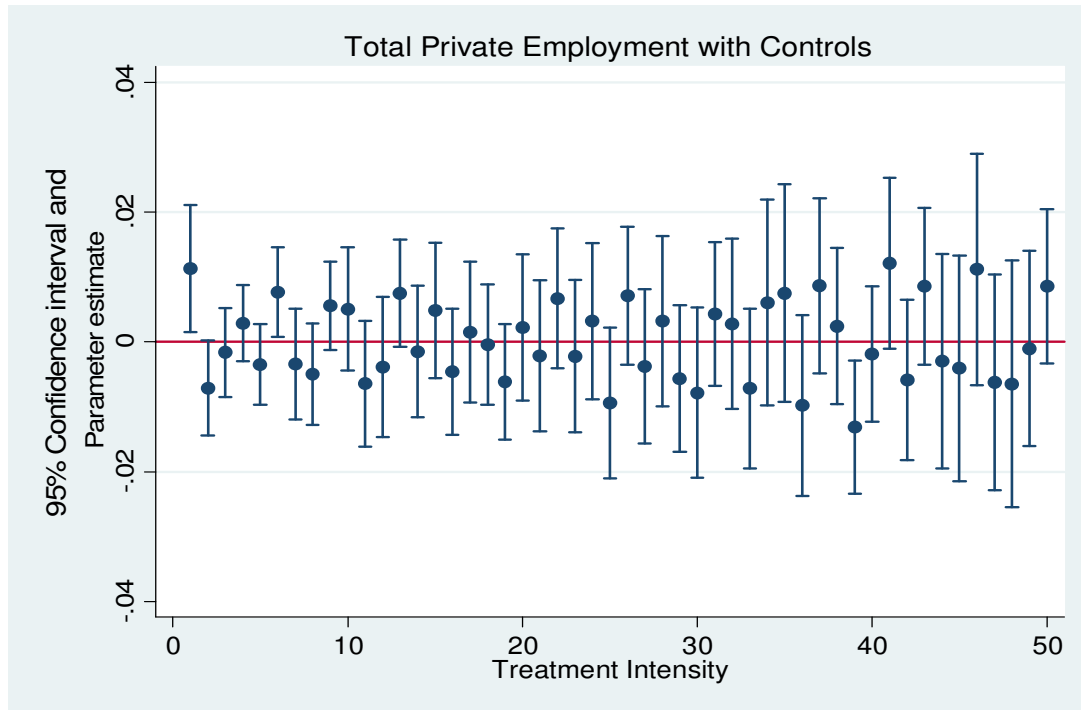
Note: The dependent variable is expressed as the Log (2003-2008) changes in total private sector employment.
Source: BSD establishment data aggregated up to the 2001 Output Area level (2003-2008); 64 OA control variables retrieved from the UK 2001 Census (See Appendix B for details).

Figure 4: The impact of the Lyons Review on manufacturing and services employment, Log changes 2003-2008

Panel A
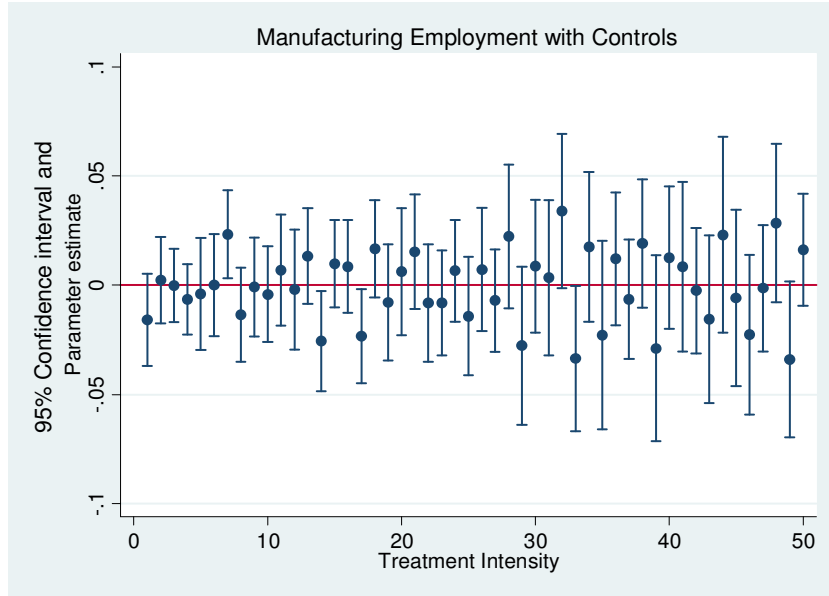
Panel B
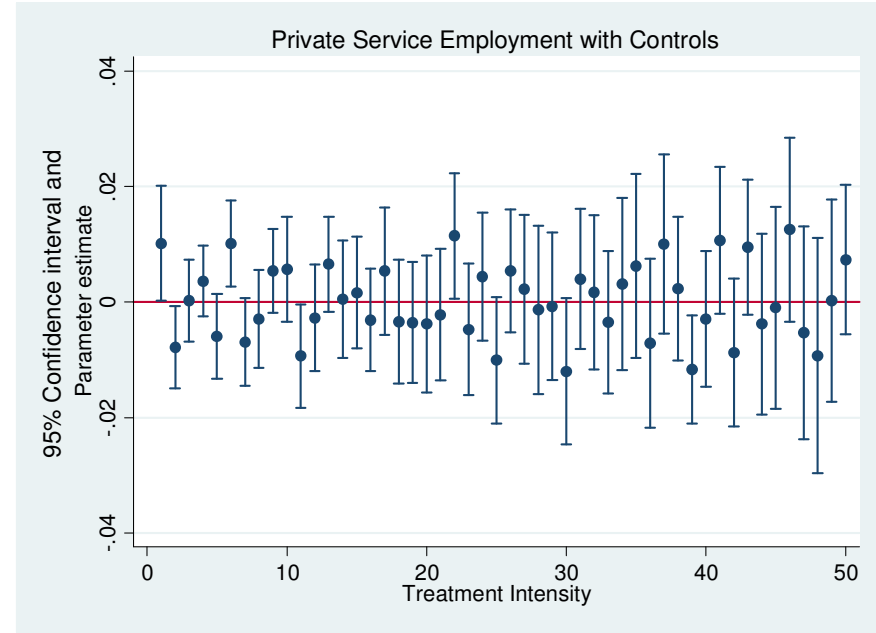
Note: The dependent variable is expressed as the Log (2003-2008) changes in manufacturing employment (Panel A) or services employment (Panel B). Full specification including 50km treatment intensities, UK 2001 Census controls, TTWA fixed effects and observations clustered at the TTWA level.
Source: BSD establishment data aggregated up to the 2001 Output Area level (2003-2008); 64 OA control variables retrieved from the UK 2001 Census (See Appendix B for details).

Table 1: Descriptive Statistics for Local Authorities chosen/not chosen as a relocation site

Variables	LAs chosen		LAs not chosen		LAs in London and the South East	
	Mean	Standard Deviation	Mean	Standard Deviation	Mean	Standard Deviation
Labour Market variables:						
Share of public sector employment	0.239	0.062	0.201	0.068	0.178	0.060
Unemployment rate	4.9	1.7	4.1	1.4	4.8	2.2
Inactivity rate	0.222	0.050	0.198	0.050	0.204	0.060
Employment rate	0.739	0.058	0.769	0.056	0.758	0.072
Share of full-time workers	0.752	0.029	0.740	0.041	0.762	0.045
Share of part-time workers	0.248	0.029	0.260	0.041	0.237	0.045
Self-employment	0.114	0.038	0.134	0.050	0.150	0.040
Demographic variables:						
Share of Whites	0.948	0.067	0.974	0.039	0.865	0.153
Skills: College degree and above	0.236	0.065	0.236	0.068	0.292	0.079
Skills: A-Levels and Equiv.	0.149	0.026	0.149	0.028	0.144	0.029
Skills: O-Levels and Equiv.	0.156	0.027	0.162	0.028	0.148	0.038
Skills: Apprenticeship	0.069	0.020	0.075	0.024	0.053	0.022
Skills: Foundation	0.153	0.029	0.153	0.036	0.137	0.042
Skills: Other	0.076	0.019	0.075	0.024	0.107	0.054
Skills: No Qual.	0.161	0.050	0.151	0.051	0.118	0.047
Political and Housing variables:						
Share of Labour Votes 1983 Election	0.203	0.090	0.161	0.093	0.131	0.072
Share of Labour Votes 1997 Election	0.332	0.105	0.285	0.112	0.244	0.100
Share of Labour Votes 2005 Election	0.398	0.129	0.321	0.139	0.285	0.126
House prices in Land Registry data	126,364	42,060	145,625	46,047	227,206	75,216
Real mix-adjusted house prices index composed of CML and Registry data	223.2	35.7	229.1	31.1	261.9	41.8
2004 Index of Multiple Deprivation:						
Total number of English LAs	88		165		99	
Number of Poor Areas (according to the Index of Multiple Deprivation)	34		25		21	
As % of all IMD areas	0.425		0.313		0.263	

Note: All but the Index of Multiple Deprivation (IMD) figures are as of 2003. Education variables are expressed as the 2003 local share of working age population (16-64 years old) with the relevant education qualification. The 2003 local unemployment rate refers to the model-based unemployment rate derived by the ONS which combines unemployment information from the Annual Population Survey and the number of beneficiaries of Jobseeker's Allowance. English LAs are classified as poor areas according to the 2004 Index of Multiple Deprivation if they ranked among the 50 worst local authorities against any of the six local authority IMD indices.
Source: BIS Local Authority data (2003-2007); ABI Local Authority data (2003-2007); Local Area Labour Force Survey (1999-2003); ONS Model-based Estimates of Unemployment (1999-2007); Land Registry data (2000-2010); 2004 Index of Multiple Deprivation statistics. Shares of labour votes at the 1983, 1997 and 2005 elections as well as real mix-adjusted house prices index have been kindly provided by Christian Hilber and Wouter Vermeulen. Because of missing data, two LAs are excluded: City of London and Isles of Scilly.

Table 2: The impact of the treatment on total public sector employment and its components

	Total Public (SIC75,80,85) (1)	Public administration and defence (SIC75) (2)	Education (SIC80) (3)	Health (SIC85) (4)
D_0	0.29***	0.32***	-0.044	0.21***
	(0.074)	(0.12)	(0.089)	(0.079)
D_1	0.0034	0.091	-0.0062	-0.0032
	(0.023)	(0.081)	(0.034)	(0.023)
D_2	-0.037*	-0.059	0.026	-0.045**
	(0.020)	(0.061)	(0.029)	(0.020)
D_3	-0.018	0.026	0.027	-0.025
	(0.019)	(0.074)	(0.027)	(0.020)
D_4	0.034*	-0.12	0.0029	0.034
	(0.018)	(0.078)	(0.029)	(0.022)
D_5	-0.0090	0.071	-0.016	-0.0065
	(0.017)	(0.075)	(0.023)	(0.017)
Controls	√	√	√	√
Observations	65,822	7,257	26,319	43,383

Note: Robust standard errors are reported in parentheses; *, **, *** indicate significance at the 10%, 5% and 1% levels respectively. All regressions include TTWA fixed effects and are clustered by TTWAs. Each coefficient is derived from a different regression. Treatment is defined as a binary indicator D_0 equal to1 if an OA is a relocation site and 0 otherwise. For robustness checks, other dummies are used: D_1 identifies OAs 1km apart from a relocation site; D_2 identifies OAs 2km apart from a relocation site. Dummies from D_3 to D_5 are defined in a similar way. Total public sector employment is also split into public administration and defence, education and health and social work (see column 2-4). Correspondently, the dependent variable in column (1) refers to the Log changes (2003-2008) in total public sector employment. The dependent variable used in column (2) refers to the Log changes (2003-2008) in workers employed in public administration and defence. The dependent variables for columns (3) and (4) are similarly defined for education and health.

Table 3a: The Impact of the Lyons Review on private sector employment, Log 2003-2008 Employment Changes, treatment intensities measured as relocation dummies

	Private	Manuf	Services	Private	Manuf	Services
	(1)	(2)	(3)	(4)	(5)	(6)
0-1km dummy	0.028	-0.079*	0.031	0.036**	-0.027	0.034*
	(0.019)	(0.046)	(0.020)	(0.018)	(0.046)	(0.019)
0-2km dummy	-0.031***	-0.022	-0.027**	-0.017	0.014	-0.017
	(0.011)	(0.035)	(0.011)	(0.011)	(0.034)	(0.011)
0-3km dummy	-0.012	-0.014	-0.012	-0.006	-0.002	-0.008
	(0.0097)	(0.032)	(0.010)	(0.009)	(0.034)	(0.010)
0-4km dummy	-0.011	-0.034	-0.0037	-0.008	-0.020	-0.001
	(0.011)	(0.027)	(0.012)	(0.010)	(0.025)	(0.011)
0-5km dummy	0.0072	-0.0076	-0.0025	0.022**	0.024	0.014
	(0.011)	(0.029)	(0.012)	(0.009)	(0.026)	(0.010)
Constant	0.061***	-0.12***	0.091***	-0.220	-0.490	-0.100
	(0.002)	(0.004)	(0.002)	(0.160)	(0.390)	(0.180)
Controls	No	No	No	√	√	√
Observations	160,874	39,187	158,954	155,608	38,353	153,747
R-squared	0.004	0.010	0.004	0.008	0.017	0.008

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Table 3b: The Impact of the Lyons Review on private sector employment, Log 2003-2008 Employment Changes, treatment intensities measured as relocation size

	Private (1)	Manuf (2)	Services (3)	Private (4)	Manuf (5)	Services (6)
0-1km size	0.010*	-0.028***	0.010*	0.011**	-0.016	0.010**
	(0.005)	(0.011)	(0.005)	(0.005)	(0.011)	(0.005)
0-2km size	-0.009**	-0.004	-0.009**	-0.007*	0.003	-0.008**
	(0.004)	(0.010)	(0.004)	(0.004)	(0.010)	(0.004)
0-3km size	-0.003	-0.005	-0.001	-0.002	-0.001	0.000
	(0.004)	(0.008)	(0.004)	(0.003)	(0.008)	(0.004)
0-4km size	0.001	-0.010	0.002	0.003	-0.007	0.003
	(0.003)	(0.008)	(0.003)	(0.003)	(0.008)	(0.003)
0-5km size	-0.002	0.0002	-0.005**	0.001	0.005	-0.001
	(0.003)	(0.007)	(0.002)	(0.003)	(0.007)	(0.003)
0-50km size	0.004	-0.012	0.004	0.005	-0.008	0.004
	(0.003)	(0.008)	(0.003)	(0.003)	(0.007)	(0.003)
Constant	0.041**	-0.063	0.067***	-0.25	-0.46	-0.13
	(0.017)	(0.043)	(0.017)	(0.17)	(0.39)	(0.18)
Controls	No	No	No	√	√	√
Observations	160,874	39,187	158,954	155,608	38,353	153,747
R-squared	0.004	0.010	0.004	0.008	0.017	0.008

Table 4: The Impact of the Lyons Review on private sector employment, Log 2003-2008
Employment Changes adding pre-trends

VARIABLES	Dummy			Size		
	Private	Manuf	Services	Private	Manuf	Services
	(1)	(2)	(3)	(4)	(5)	(6)
0-1km	0.041***	-0.021	0.040**	0.011**	-0.016	0.011**
	(0.015)	(0.047)	(0.017)	(0.0048)	(0.010)	(0.0049)
0-2km	-0.013	0.021	-0.012	-0.0060	0.0055	-0.0070**
	(0.011)	(0.035)	(0.011)	(0.0037)	(0.010)	(0.0035)
0-3km	-0.016*	-0.011	-0.018*	-0.0037	-0.0015	-0.0021
	(0.0085)	(0.037)	(0.0095)	(0.0034)	(0.0097)	(0.0033)
0-4km	0.000080	-0.018	0.0073	0.0059**	-0.0086	0.0068**
	(0.011)	(0.030)	(0.012)	(0.0029)	(0.0093)	(0.0031)
0-5km	0.017*	0.033	0.0084	-0.0015	0.0081	-0.0037
	(0.0098)	(0.030)	(0.011)	(0.0026)	(0.0074)	(0.0024)
0-50km				0.0035	-0.0019	0.0027
				(0.0035)	(0.0069)	(0.0034)
Pre-trends: Ln(private)$_{1998-2002}$	-0.20***			-0.20***		
	(0.0055)			(0.0055)		
Pre-trends: Ln(manuf)$_{1998-2002}$		-0.18***			-0.18***	
		(0.0082)			(0.0082)	
Pre-trends: Ln(services)$_{1998-2002}$			-0.21***			-0.21***
			(0.0051)			(0.0050)
Constant	-0.26	-0.54	-0.12	-0.28*	-0.55	-0.14
	(0.16)	(0.45)	(0.17)	(0.16)	(0.44)	(0.18)
Controls	√	√	√	√	√	√
Observations	146,651	31,107	144,236	146,651	31,107	144,236

Table 5: Placebo experiment: Impact of treatment intensities on Log 1998-2002 employment changes

VARIABLES	*Dummy*			*Size*		
	Private	Manuf	Services	Private	Manuf	Services
	(1)	(2)	(3)	(4)	(5)	(6)
0-1km	0.0078	0.0024	0.0018	0.0015	0.0057	-0.0015
	(0.018)	(0.029)	(0.017)	(0.0047)	(0.0093)	(0.0055)
0-2km	0.0039	-0.00037	0.0087	0.0013	-0.011*	0.0016
	(0.012)	(0.022)	(0.013)	(0.0041)	(0.0061)	(0.0038)
0-3km	-0.030**	-0.017	-0.030**	-0.0055*	0.012*	-0.0063*
	(0.012)	(0.029)	(0.013)	(0.0033)	(0.0068)	(0.0037)
0-4km	0.0091	0.0055	0.00051	0.0013	-0.0078	-0.00065
	(0.012)	(0.027)	(0.013)	(0.0034)	(0.0086)	(0.0036)
0-5km	0.017	0.027	0.020	0.0025	0.0072	0.0035
	(0.012)	(0.020)	(0.013)	(0.0024)	(0.0061)	(0.0024)
0-50km				-0.0014	-0.013	-0.0014
				(0.0025)	(0.0086)	(0.0029)
Constant	0.20***	0.093	0.18***	0.21***	0.16	0.19***
	(0.053)	(0.12)	(0.059)	(0.054)	(0.13)	(0.061)
Controls	√	√	√	√	√	√
Observations	160,164	45,382	158,024	160,164	45,382	158,024
R-squared	0.009	0.012	0.009	0.009	0.013	0.009

Table 6: Test for causality: Impact of treatment intensities on Log employment changes before and after the implementation of the Lyons Review

Dep.Var.	Private (1)	Manuf (2)	Services (3)
Log 1998-1999 emp	-0.00070	0.0032	-0.0012
	(0.0028)	(0.0050)	(0.0030)
Observations	165,671	55,207	163,714
Log 1999-2000 emp	-0.0012	-0.00041	-0.0021
	(0.0027)	(0.0058)	(0.0029)
Observations	165,113	53,708	163,188
Log 2000-2001 emp	0.0013	0.0061	0.000027
	(0.0027)	(0.0049)	(0.0028)
Observations	165,498	54,178	163,617
Log 2001-2002	0.0032	-0.0024	0.0021
	(0.0034)	(0.0056)	(0.0032)
Observations	165,498	54,178	163,617
Log 2002-2003 emp	-0.0011	-0.0079	0.000034
	(0.0032)	(0.0049)	(0.0033)
Observations	159,275	58,561	157,511
Log 2003-2008 emp	*0.011***	*-0.016*	*0.010***
	(0.0050)	*(0.011)*	*(0.0051)*
Observations	*155,608*	*38,353*	*153,747*
Log 2008-2009 emp	-0.0021	0.00018	-0.0025
	(0.0024)	(0.0058)	(0.0023)
Observations	164,710	49,235	163,359

Note: Robust standard errors are reported in parentheses; *, **, *** indicate significance at the 10%, 5% and 1% levels respectively. All regressions include area controls (1991 or 2001 UK Census), TTWA fixed effects and are clustered at the TTWA level. Each row refers to a different set of regressions using the dependent variable indicated in the far-left column.

Table 7: The relative importance of yearly relocations, Log 2003-2008 employment changes

	Private	Manuf	Services
	(1)	(2)	(3)
0-1km $_{2003\text{-}2004}$	0.024**	-0.015	0.019**
	(0.0093)	(0.020)	(0.0094)
0-1km $_{2005}$	-0.0021	-0.0043	-0.0063
	(0.0082)	(0.026)	(0.0082)
0-1km $_{2006}$	-0.013*	-0.012	-0.014
	(0.0076)	(0.020)	(0.0085)
0-1km $_{2007}$	0.019*	-0.0077	0.023**
	(0.010)	(0.025)	(0.011)
0-2km $_{2003\text{-}2004}$	-0.021***	0.00043	-0.020***
	(0.0057)	(0.012)	(0.0061)
0-2km $_{2005}$	0.013*	-0.0015	0.0099
	(0.0077)	(0.013)	(0.0075)
0-2km $_{2006}$	-0.0044	0.0066	-0.0028
	(0.0064)	(0.014)	(0.0061)
0-2km $_{2007}$	-0.00082	-0.0071	-0.00060
	(0.0065)	(0.017)	(0.0063)
0-3km $_{2003\text{-}2004}$	0.0017	-0.0029	0.0034
	(0.0058)	(0.011)	(0.0057)
0-3km $_{2005}$	0.0025	0.020	0.0018
	(0.0061)	(0.014)	(0.0058)
0-3km $_{2006}$	-0.013***	-0.031**	-0.011**
	(0.0050)	(0.012)	(0.0050)
0-3km $_{2007}$	0.0059*	0.016	0.0068*
	(0.0032)	(0.014)	(0.0036)
0-4km $_{2003\text{-}2004}$	0.0054	0.012	0.0037
	(0.0052)	(0.013)	(0.0052)
0-4km $_{2005}$	-0.0033	-0.019	-0.0020
	(0.0058)	(0.018)	(0.0053)
0-4km $_{2006}$	-0.0030	0.011	-0.0029
	(0.0053)	(0.013)	(0.0051)
0-4km $_{2007}$	0.0061	-0.0089	0.0071*
	(0.0041)	(0.018)	(0.0039)
0-5km $_{2003\text{-}2004}$	0.0011	-0.018*	0.0020
	(0.0041)	(0.010)	(0.0038)
0-5km $_{2005}$	-0.0031	0.0099	-0.0034
	(0.0045)	(0.011)	(0.0044)
0-5km $_{2006}$	0.0064	0.00012	0.0053
	(0.0044)	(0.011)	(0.0040)
0-5km $_{2007}$	-0.0032	0.0084	-0.0055
	(0.0045)	(0.012)	(0.0043)

0-50km $_{2003\text{-}2004}$	-0.00025	-0.00070	0.00094
	(0.0032)	(0.0061)	(0.0032)
0-50km $_{2005}$	0.0059	-0.0054	0.0072*
	(0.0046)	(0.0096)	(0.0041)
0-50km $_{2006}$	-0.0045	-0.016	-0.0064*
	(0.0037)	(0.011)	(0.0036)
0-50km $_{2007}$	0.0047	0.011	0.0042
	(0.0036)	(0.0069)	(0.0035)
Constant	-0.25	-0.47	-0.14
	(0.16)	(0.39)	(0.18)
Controls	√	√	√
Observations	155,608	38,353	153,747

Table 8: Impact of treatment intensity by quartile class and distance band, Log (2003-2008) employment change (0-5km sample)

		1 quartile		2 quartile		3 quartile		4 quartile	
1km									
	Private	0.057**	(0.028)	0.0017	(0.027)	0.057**	(0.022)	0.068*	(0.035)
	Manuf	0.030	(0.072)	-0.077	(0.076)	-0.045	(0.092)	-0.063	(0.065)
	Services	0.056*	(0.030)	0.0056	(0.024)	0.056**	(0.025)	0.056	(0.036)
2km									
	Private	0.0011	(0.014)	0.0043	(0.014)	-0.017	(0.022)	-0.032	(0.027)
	Manuf	0.033	(0.042)	0.015	(0.051)	0.0060	(0.057)	0.030	(0.060)
	Services	0.0049	(0.017)	-0.0044	(0.016)	-0.019	(0.019)	-0.032	(0.027)
3km									
	Private	0.0046	(0.013)	-0.020	(0.013)	-0.016	(0.020)	-0.0052	(0.025)
	Manuf	-0.023	(0.037)	0.077*	(0.046)	-0.0049	(0.057)	-0.043	(0.049)
	Services	-0.0047	(0.015)	-0.024*	(0.014)	-0.010	(0.021)	0.0050	(0.022)
4km									
	Private	-0.025**	(0.011)	0.0040	(0.017)	0.0029	(0.023)	0.034*	(0.017)
	Manuf	-0.0100	(0.028)	-0.031	(0.052)	0.0027	(0.043)	-0.041	(0.059)
	Services	-0.015	(0.011)	0.0049	(0.017)	0.0079	(0.022)	0.044**	(0.020)
5km									
	Private	0.026	(0.026)	-0.0066	(0.020)	-0.0099	(0.021)		
	Manuf	0.015	(0.074)	0.018	(0.062)	-0.0025	(0.061)		
	Services	0.035	(0.027)	0.0071	(0.024)	-0.0060	(0.022)		

Note: There are 42378 observations in the regression on total private employment; 9310 in manufacturing employment; 41727 in services. The sample included all OAs within a 0-5km distance band from relocation site only.

Table 8bis: Impact of treatment intensity by quartile class and distance band, Log (2003-2008) employment change (0-50km sample)

		1 quartile		2 quartile		3 quartile		4 quartile	
0-1km									
	Private	0.052*	(0.029)	-0.0061	(0.026)	0.048**	(0.024)	0.059*	(0.035)
	Manuf	0.043	(0.075)	-0.06	(0.076)	-0.041	(0.088)	-0.058	(0.063)
	Services	0.052*	(0.03)	-0.0015	(0.024)	0.049*	(0.027)	0.047	(0.035)
0-2km									
	Private	-0.0065	(0.015)	-0.0031	(0.014)	-0.028	(0.022)	-0.04	(0.026)
	Manuf	0.034	(0.039)	0.0098	(0.05)	-0.0022	(0.057)	0.025	(0.059)
	Services	-0.0026	(0.017)	-0.011	(0.015)	-0.031	(0.02)	-0.038	(0.026)
0-3km									
	Private	-0.0037	(0.013)	-0.017	(0.013)	-0.017	(0.02)	-0.012	(0.027)
	Manuf	-0.028	(0.037)	0.061	(0.046)	-0.0041	(0.049)	-0.026	(0.053)
	Services	-0.012	(0.015)	-0.02	(0.014)	-0.013	(0.021)	-0.0026	(0.025)
0-4km									
	Private	-0.026**	(0.011)	0.0033	(0.016)	0.0017	(0.022)	0.033*	(0.017)
	Manuf	-0.014	(0.027)	-0.025	(0.049)	-0.0024	(0.043)	-0.043	(0.059)
	Services	-0.013	(0.01)	0.005	(0.017)	0.007	(0.021)	0.046**	(0.019)
0-5km									
	Private	0.041***	(0.012)	0.0093	(0.013)	0.0048	(0.014)	0.01	(0.021)
	Manuf	0.021	(0.034)	0.026	(0.035)	0.018	(0.033)	-0.0015	(0.057)
	Services	0.035***	(0.013)	0.0032	(0.013)	-0.0086	(0.013)	-0.0069	(0.021)
0-50km									
	Private	-0.015	(0.022)	-0.0043	(0.014)	0.014	(0.011)		
	Manuf	0.093**	(0.044)	0.053*	(0.031)	0.073***	(0.021)		
	Services	-0.011	(0.026)	0.004	(0.016)	0.016	(0.014)		

Table 9a: Splitting services by type (relocation dummy) adding trends

	Baseline (1)	Transport (2)	FIRE and business (3)	Trade and Catering (4)	Construction (5)	Baseline (1)	Transport (2)	FIRE and business (3)	Trade and Catering (4)	Construction (5)
0-1km dummy	0.035*	-0.022	0.050*	-0.019	0.012	0.045***	-0.046	0.071***	-0.0050	0.0040
	(0.019)	(0.065)	(0.027)	(0.021)	(0.027)	(0.016)	(0.070)	(0.025)	(0.019)	(0.033)
0-2km dummy	-0.018*	0.015	-0.019	-0.0048	-0.031	-0.018*	0.023	-0.013	-0.010	-0.030
	(0.0098)	(0.057)	(0.020)	(0.014)	(0.021)	(0.0098)	(0.067)	(0.017)	(0.014)	(0.025)
0-3km dummy	-0.012	-0.021	-0.037*	-0.010	0.0032	-0.020**	-0.011	-0.058***	-0.014	0.0056
	(0.0096)	(0.043)	(0.019)	(0.013)	(0.026)	(0.0088)	(0.048)	(0.018)	(0.014)	(0.025)
0-4km dummy	0.0037	-0.0020	-0.0040	0.020	-0.016	0.0073	-0.022	0.014	0.016	-0.0055
	(0.011)	(0.038)	(0.019)	(0.017)	(0.024)	(0.013)	(0.040)	(0.018)	(0.016)	(0.023)
0-5km dummy	0.012	-0.038	0.031**	0.0071	0.037**	0.013	-0.034	0.038***	0.0079	0.014
	(0.010)	(0.027)	(0.013)	(0.012)	(0.017)	(0.011)	(0.035)	(0.014)	(0.012)	(0.019)
Constant	-0.30	0.36	-0.17	0.17	-0.21	-0.22	-0.13	-0.047	0.064	-0.32
	(0.19)	(0.51)	(0.28)	(0.21)	(0.34)	(0.20)	(0.58)	(0.29)	(0.22)	(0.38)
Controls	√	√	√	√	√	√	√	√	√	√
Pre-trends						√	√	√	√	√
Observations	144,986	29,016	100,800	110,402	67,482	134,351	20,698	81,634	99,858	52,020

Note: Baseline regressions (Column 1) use log 2003-2008 employment changes in services without construction (SIC50-SIC93) as dependent variable; Transport (Column 2) uses log 2003-2008 employment changes in transport services (SIC60-SIC64); FIRE and Business (Column 3) uses log 2003-2008 employment changes in finance, insurance, real estate and business services (SIC65-SIC74); Trade and Catering (Column 4) uses log 2003-2008 changes in wholesale and retail trade, hotel and restaurants, personal services (SIC50-SIC55; SIC92-SIC93); Construction (Column 5) uses log 2003-2008 employment changes in construction workers (SIC45).

Table 9b: Splitting services by type (relocation size) adding trends

VARIABLES	Baseline (1)	Transport (2)	FIRE and business (3)	Trade and Catering (4)	Construction (5)	Baseline (1)	Transport (2)	FIRE and business (3)	Trade and Catering (4)	Construction (5)
0-1km size	0.0093*	0.0064	0.012*	-0.0029	0.0057	0.011**	0.0089	0.015***	0.00082	0.0081
	(0.0051)	(0.020)	(0.0065)	(0.0071)	(0.0090)	(0.0045)	(0.019)	(0.0052)	(0.0066)	(0.010)
0-2km size	-0.0053	-0.013	-0.0027	-0.0054	-0.018**	-0.0070**	-0.015	-0.0024	-0.0077*	-0.016**
	(0.0033)	(0.013)	(0.0050)	(0.0038)	(0.0071)	(0.0034)	(0.014)	(0.0052)	(0.0040)	(0.0078)
0-3km size	-0.0031	-0.0079	-0.0083	0.00020	0.0022	-0.0039	0.00058	-0.010*	-0.0010	0.0024
	(0.0036)	(0.011)	(0.0051)	(0.0048)	(0.0060)	(0.0037)	(0.011)	(0.0056)	(0.0049)	(0.0074)
0-4km size	0.0028	0.0083	-0.0032	0.0016	0.0034	0.0043	-0.00023	-0.0014	0.0021	0.0065
	(0.0031)	(0.012)	(0.0055)	(0.0053)	(0.0066)	(0.0037)	(0.013)	(0.0053)	(0.0049)	(0.0077)
0-5km size	0.00035	-0.011	0.0058	0.0018	0.0038	-0.00020	-0.0075	0.0073*	0.0015	-0.0018
	(0.0023)	(0.0086)	(0.0039)	(0.0032)	(0.0043)	(0.0024)	(0.010)	(0.0038)	(0.0030)	(0.0046)
0-50km size	0.0025	0.0011	0.0062	0.0035	0.015***	0.0016	-0.0022	0.0055	0.0034	0.0066
	(0.0039)	(0.0098)	(0.0055)	(0.0046)	(0.0052)	(0.0041)	(0.012)	(0.0058)	(0.0043)	(0.0073)
Constant	-0.32*	0.36	-0.21	0.14	-0.31	-0.24	-0.100	-0.090	0.038	-0.36
	(0.19)	(0.51)	(0.29)	(0.22)	(0.34)	(0.20)	(0.60)	(0.30)	(0.22)	(0.38)
Controls	√	√	√	√	√	√	√	√	√	√
Pre-trends						√	√	√	√	√
Observations	144,986	29,016	100,800	110,402	67,482	134,351	20,698	81,634	99,858	52,020

Note: Baseline regressions (Column 1) use log 2003-2008 employment changes in services without construction (SIC50-SIC93) as dependent variable; Transport (Column 2) uses log 2003-2008 employment changes in transport services (SIC60-SIC64); FIRE and Business (Column 3) uses log 2003-2008 employment changes in finance, insurance, real estate and business services (SIC65-SIC74); Trade and Catering (Column 4) uses log 2003-2008 changes in wholesale and retail trade, hotel and restaurants, personal services (SIC50-SIC55; SIC92-SIC93); Construction (Column 5) uses log 2003-2008 employment changes in construction workers (SIC45).

Table 10a: The Impact of the Lyons Review on private sector employment, Log 2003-2008 Employment Changes, restricting the sample to 0-5km (relocation dummy)

	Private (1)	Manuf (2)	Services (3)	Private (4)	Manuf (5)	Services (6)
0-1km dummy	0.044**	-0.033	0.041**	0.047***	-0.036	0.046***
	(0.018)	(0.043)	(0.019)	(0.015)	(0.045)	(0.017)
0-2km dummy	-0.0085	0.017	-0.0081	-0.0045	0.023	-0.0024
	(0.011)	(0.033)	(0.011)	(0.011)	(0.033)	(0.011)
0-3km dummy	-0.0020	0.0035	-0.0039	-0.010	-0.0018	-0.012
	(0.0090)	(0.033)	(0.010)	(0.0086)	(0.036)	(0.0093)
0-4km dummy	-0.0060	-0.019	-0.00039	0.0014	-0.019	0.0080
	(0.010)	(0.026)	(0.012)	(0.011)	(0.029)	(0.012)
Controls	√	√	√	√	√	√
Pre-trends				√	√	√
Observations	42,378	9,310	41,727	39,234	7,622	38,399

Table 10b: The Impact of the Lyons Review on private sector employment, Log 2003-2008 Employment Changes, restricting the sample to 0-5km (relocation size)

	Private (1)	Manuf (2)	Services (3)	Private (4)	Manuf (5)	Services (6)
0-1km size	0.013**	-0.018*	0.011**	0.012**	-0.021*	0.012**
	(0.0049)	(0.011)	(0.0050)	(0.0047)	(0.011)	(0.0048)
0-2km size	-0.0052	0.0031	-0.0059	-0.0042	0.0062	-0.0051
	(0.0038)	(0.0097)	(0.0037)	(0.0038)	(0.0096)	(0.0036)
0-3km size	-0.00073	-0.00017	0.0011	-0.0024	-0.00078	-0.00061
	(0.0034)	(0.0087)	(0.0034)	(0.0034)	(0.0097)	(0.0031)
0-4km size	0.0032	-0.0061	0.0037	0.0061**	-0.0068	0.0068**
	(0.0030)	(0.0080)	(0.0032)	(0.0029)	(0.0092)	(0.0032)
0-5km size	-0.0017	-0.0032	-0.0032	-0.0028	0.0025	-0.0041
	(0.0045)	(0.010)	(0.0045)	(0.0046)	(0.0091)	(0.0046)
Controls	√	√	√	√	√	√
Pre-trends				√	√	√
Observations	42,378	9,310	41,727	39,234	7,622	38,399

Table 11a: The Impact of the Lyons Review on private sector employment, Log 2003-2008
Employment Changes, expanding gradually the areas compared (relocation dummy)

	Private	Manuf	Services	Private	Manuf	Services	Private	Manuf	Services
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Panel A			Panel B			Panel C		
0-1km dummy	0.038**	-0.014	0.035*	0.042**	-0.019	0.040**	0.043**	-0.026	0.040**
	(0.017)	(0.047)	(0.019)	(0.017)	(0.043)	(0.019)	(0.017)	(0.043)	(0.019)
0-2km dummy				-0.0081	0.017	-0.0073	-0.0078	0.016	-0.0077
				(0.011)	(0.032)	(0.011)	(0.011)	(0.033)	(0.011)
0-3km dummy							-0.0024	0.0059	-0.0048
							(0.0091)	(0.033)	(0.010)
Constant	-0.55	0.040	-0.64	-0.45	0.81	-0.42	-0.39	0.35	-0.31
	(0.56)	(1.20)	(0.58)	(0.44)	(0.94)	(0.42)	(0.34)	(0.85)	(0.36)
Controls	√	√	√	√	√	√	√	√	√
Observations	14,591	3,606	14,369	24,499	5,627	24,115	33,745	7,528	33,220

Table 11b: The Impact of the Lyons Review on private sector employment, Log 2003-2008
Employment Changes, expanding gradually the areas compared (relocation dummy)

	Private	Manuf	Services	Private	Manuf	Services
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A			Panel B		
0-1km size	0.011**	-0.014	0.0099*	0.012**	-0.016	0.011**
	(0.0051)	(0.012)	(0.0052)	(0.0050)	(0.011)	(0.0051)
0-2km size	-0.0077*	-0.0017	-0.0087*	-0.0051	0.0031	-0.0058
	(0.0045)	(0.011)	(0.0050)	(0.0040)	(0.0095)	(0.0039)
0-3km size				0.0017	0.00075	0.0044
				(0.0047)	(0.0069)	(0.0041)
Constant	-0.52	0.054	-0.60	-0.46	0.83	-0.44
	(0.56)	(1.21)	(0.58)	(0.43)	(0.94)	(0.41)
Controls	√	√	√	√	√	√
Observations	14,591	3,606	14,369	24,499	5,627	24,115
	Panel C			Panel D		
0-1km size	0.012**	-0.017	0.011**	0.013**	-0.018*	0.011**
	(0.0049)	(0.011)	(0.0050)	(0.0049)	(0.011)	(0.0050)
0-2km size	-0.0050	0.0025	-0.0058	-0.0052	0.0031	-0.0059
	(0.0038)	(0.0099)	(0.0037)	(0.0038)	(0.0097)	(0.0037)
0-3km size	-0.00084	0.00043	0.0011	-0.00073	-0.00017	0.0011
	(0.0033)	(0.0088)	(0.0034)	(0.0034)	(0.0087)	(0.0034)
0-4km size	0.0057	-0.0065	0.0040	0.0032	-0.0061	0.0037
	(0.0039)	(0.0075)	(0.0037)	(0.0030)	(0.0080)	(0.0032)
0-5km size				-0.0017	-0.0032	-0.0032
				(0.0045)	(0.010)	(0.0045)
Constant	-0.42	0.43	-0.34	-0.20	0.99	-0.15
	(0.34)	(0.85)	(0.36)	(0.33)	(0.72)	(0.34)
Controls	√	√	√	√	√	√
Observations	33,745	7,528	33,220	42,378	9,310	41,727

Appendix A

The Business Structure Database

The second database I use is the Business Structure Database (BSD), which provides an annual snapshot (taken in April at the closing of the fiscal year) of the Inter-Departmental Business Register (IDBR). The IDBR consists of administrative data collected for revenues and taxation purposes and is constantly updated. Any business liable for value-added taxation (VAT) and/or with at least one employee registered for tax collection will appear on the IDBR. For the year 2012, the VAT threshold for registration was a turnover of taxable goods and services of £77,000, thus suggesting that the BSD might not sample very small-businesses. Nevertheless, the ONS estimated that for 2004 the businesses listed on the IDBR accounted for approximately 99 per cent of economic activity in the UK.

The BSD data is structured into enterprises and local units. An enterprise is the overall business organisation, whereas the local unit can be thought of as a plant or establishment. In the majority of cases (about 70 per cent), enterprises only have one local unit, while in the remaining 30 per cent of cases enterprises have multiple local units. I focus on data at the establishment or plant level. The BSD data contains information on about 2.4 million business establishments per year over the period 1997-2009 and includes information on each business' date of birth, date of death, postcode, sector of activity (up to 4-digit Standard Industry Classification (SIC) 2003 code) and total employment. Using the postcode, I assign each local unit active in England, Wales and Scotland to a Census 2001 Output Area – the unit of observation I have chosen to conduct the analysis.

The initial raw data includes approximately 3 million local units every year. However, I carry out a series of checks and drop a number of units. In particular, I drop cases where I identify establishments opening/closing in a specific year, disappearing/reappearing in a subsequent year only to open/close again in a subsequent wave. Stated differently, we only count firms' birth and death once. In addition, I drop active units with zero employment (this figure includes the owners/managers of the establishment, so it cannot be zero for an active unit) and postcodes that include an anomalous number of units in the same 3-digit industry. The adjustment for anomalous postcodes consists of three parts: When I observe two or more plants operating in the same 3-digit industry, sharing the same postcode[34] and that are under common ownership, I believe this being an

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[34] A UK postcode or postal unit usually corresponds to a limited number of addresses or a single large delivery point. It is not necessarily a geographically accurate description of where a property is located, but it is a good approximation.

error in reporting and drop them. In a non-trivial number of cases, I also observe plants sharing the same 3-digit industry and the same postcode. I believe this being another coding error and trim the top 5% of those plants' distribution. Finally, looking at the employment distribution by 3-digit industry, I also trimmed the top 1%.

Finally, I focus on private businesses and exclude active units operating in one of the following sectors: Public Administration and Defence; Education; Health and Social Work; Public Utilities (Electricity, Gas and Water). I also exclude businesses operating in Agriculture; Hunting and Forestry; Fishing; Private Households with Employees; and Extra-Territorial Organization and Bodies.

We also use the National Statistics Postcode Directory (NSPD) which is a list of all the current and terminated postcodes that have ever existed in the UK, together with geographical links or matches for each postcode to a variety of different administrative, health, electoral and other geographies that are, or have been, used within the UK.

The NSPD is a collection of over 29 million Royal Mail postal addresses and postcodes. I use the NSPD for matching postcodes of government relocation to in order to match government relocation data with business postcodes to Census Output Area (OA) and then construct our measure of employment and number of businesses by aggregating the BSD data by OA.

For instance, a building which contains several flats or businesses but only one external door, will only have the external door listed as a delivery point. This example also shows that UK postcodes are geographically accurate up to the level of a front door in a particular street.

Table A.1: List of control variables retrieved from the UK 1991 and 2001 Census
All control variables are constructed at the 2001 Output Area (OAs) level.

pop_less25	Proportion of population aged 24 or younger
pop_25_44	Proportion of population aged 25 to 44
pop_45_64	Proportion of population aged 45 to 64
pop_65_74	Proportion of population aged 65 to 74
pop_over75	Proportion of population aged 75+"
noqual_wap	Proportion of WAP without qualifications
qlev1_wap	Proportion of WAP with qualification level 1
qlev2_wap	Proportion of WAP with qualification level 2
qlev3_wap	Proportion of WAP with qualification level 3
qlev4o5_wap	Proportion of WAP with qualification level 4 o 5
qoth_wap	Proportion of WAP with other qualifications
pop_male	Proportion of male population
pop_female	Proportion of female population
pop_density	Population density
emp_xstud	emp-to-pop ratio - excluding students
inact_xstud	inact-to-pop ratio - excluding students
un_xstud	un-to-pop ratio - excluding students
sh_stud	share of students in the WAP(16-74)
sh_ret	share of retired individuals in the WAP(16-74)
work_se_rate	Proportion of self-employed workers
Work_ft_rate	Proportion of full-time employees
work_pt_rate	Proportion of part-time employees
comm_emp	Proportion of employees that travel to work
home_emp	Proportion of employees working from home
Agri_sh	Proportion of total employment in agriculture, hunting and forestry
Fish_sh	Proportion of total employment in fishing
Min_sh	Proportion of total employment in mining
Elect_sh	Proportion of total employment in electricity and water supply
Man_sh	Proportion of total employment in manufacturing
Const_sh	Proportion of total employment in construction
Who_sh	Proportion of total employment in wholesale and retail
Hot_sh	Proportion of total employment in hotels/catering
Trans_sh	Proportion of total employment in transport/communication
Fin_sh	Proportion of total employment in financial interm
Est_sh	Proportion of total employment in real estate/renting
Pub_sh	Proportion of total employment in public admin and defense
Ed_sh	Proportion of total employment in education
Hea_sh	Proportion of total employment in health and social
Other_sh	Proportion of total employment in other industries
elem_so_sh	Proportion employed in elementary occupations over total
proc_so_sh	Proportion employed as process/plant and machine operatives over total

sale_so_sh	Proportion employed in sales and customer service occupations over total
pers_so_sh	Proportion employed in personal service over total
skl_so_sh	Proportion employed in skilled occupations over total
admin_so_sh	Proportion employed in administrative/secretarial occupations over total
apro_so_sh	Proportion employed in technical occupations over total
pro_so_sh	Proportion employed in professional occupations over total
mangr_so_sh	Proportion employed as managers/senior officials over total
white_sh	Proportion of all whites over population
nowhite_sh	Proportion of all non-whites over population
lone_hous	Proportion of lone parent households over total households
migr_pop	Proportion of non-UK born over total population
hous_overcr	Proportion of overcrowded households
hous_avsize	Average household size
hous_nbrooms	Average number of rooms per household
hous_noheat	Proportion of households without heating
hous_wocars	Proportion of households without a car
Hous_allrent	Proportion of households living in rented houses
hous_own	Proportion of households living in owned houses
hous_privrent	Proportion of private renters on total renters
hous_council	Proportion of social renters on total renters
pop_estab	Share of total population living in communal establishments
travel_undergr	Proportion of employees travelling to work by tube
travel_train	Proportion of employees travelling to work by train
travel_bus	Proportion of employees travelling to work by bus
travel_motocy	Proportion of employees travelling to work by motorcycle
travel_car	Proportion of employees travelling to work by car
travel_carpas	Proportion of employees travelling to work by common car
travel_taxi	Proportion of employees travelling to work by taxi
travel_bike	Proportion of employees travelling to work by bike
travel_foot	Proportion of employees walking
travel_oth	Proportion of employees travelling to work by other means
av_commdist	Average distance (km) travelled to fixed place of work
pub_trans	People in employment who use public transport
Source: UK Census data 2001.	