Relocation of Public Sector Workers: Evaluating a place-based policy*

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Abstract

This paper investigates the local labor market impact of a UK relocation initiative – the 2004 Lyons Review. The review resulted in the dispersal of about 25,000 civil service jobs out of London and the South East towards other UK destinations. This study aims to detect whether the inflow of public sector jobs crowded out private sector activity or stimulated the local provision of jobs in the private sector. Focusing on short-term effects, I find that the relocation initiative raised private sector employment in receiving areas and changed the sectoral distribution of local employment towards services. I also find evidence of displacement, i.e. a tendency for private businesses to locate closer to a relocation site, moving out of areas at 1-2km and 2-3km distances into areas at 0-1km distance. These agglomeration effects appear highly localized: the largest policy impact is found in areas that received the relocated jobs with spillover effects reducing sharply over distance.

JEL classification: R23, R58, J61

Keywords: Regional labor 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 programs spur the creation of jobs by providing tax incentives to businesses located in designated areas.[1] Similarly, French Enterprise Zone programs are targeted at discretely bound areas.[2] The UK government follows a slightly different approach by designing either place-based policies with no-predetermined spatial scale (the Single Regeneration Budget (SRB)), or spatially-bound policies whose funding goes indirectly to businesses through local government (the Local Enterprise Growth Initiative (LEGI)).[3] The UK government also uses relocation programs of public sector workers to address regional employment problems and to reduce spatial disparities in income. Strictly speaking, relocation programs of public sector workers are not 'pure' place-based policies. They address a variety of objectives, including delivering cost savings, re-organizing 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, it may trigger general equilibrium effects in the form of higher housing prices or higher local wages (see Moretti, 2010; Faggio and Overman, 2014). 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's first government-sponsored review was commissioned in the 1960s (Flemming Review, 1963), followed by the Hardman Review (1973) and then by the Lawson-Thatcher Review (1988). Notwithstanding the attention given by the government to this issue, there is scarce evidence of the effects of a public sector relocation program upon local labor markets. This study tries to fill this gap by assessing the local labor 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 review proposed a relocation of about 20,000 civil service jobs 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. This program addressed a variety of objectives including the government desire to stimulate economic activity in less-prosperous areas with the aim of reducing spatial imbalances between regional UK and London. To the extent that the relocation program had any impact on local economic conditions, this paper

[1] See, for useful evaluations of the US programs, Neumark and Kolko (2010) and Busso *et al.* (2013).
[2] See, e.g., Gobillon *et al.* (2012) and Mayer *et al.* (2017).
[3] See Gibbons *et al.* (2011) and Einio and Overman (2016) for evaluations of the SRB program and the LEGI initiative, respectively.

aims to detect the causal effects of the intervention. Yet, this study is more than a policy evaluation: it informs about what happens when a small number of public sector jobs are moved to or created in some areas.

This paper uses panel data at a detailed geographic scale (the 2001 Census Output Area) covering years before and after the implementation of the program. The use of such detailed level of geography is particularly appropriate for analyzing small size relocations (as those studied here) and for identifying potential multiplier effects that operate at a very small spatial scale. On the other hand, conducting the analysis at the Output Area (OA) level hinders the possibility of detecting potential general equilibrium effects (e.g., changes in housing prices or local wages) operating at a wider geography.

My analysis is complicated by two factors: (1) the geographic spread of the policy is unknown *a priori*; (2) destination areas are not randomly chosen. To solve these issues, I first construct treatment intensity variables: adapting the approach from Gibbons *et al.* (2011), I construct a measure of treatment intensity that is a non-parametric function of the distance to a relocation site. In other words, I assume that effects are additive and vary by distance. Second, I compare neighboring areas at increasing distance from a relocation site. In doing so, I also restrict the comparison to areas that are similar in terms of initial socio-economic and demographic characteristics.

My empirical strategy is to apply a treatment intensity approach, which can be considered an extension of a difference-in-differences approach with heterogeneous (rather than constant) treatment effects (see Angrist and Imbens, 1995). Similarly to a difference-in-differences approach, I exploit the temporal and spatial variation in the data. Differently from a difference-in-differences approach, I cannot clearly distinguish between 'treated' and 'untreated' areas. In fact, all areas can be considered as treated, but they are treated at a different level of intensity. Areas close to a relocation site should be affected (i.e., treated) more intensively than areas further away with intensity decreasing monotonically with distance.

The policy stretches over an eight-year period (2003-2010). Given potential difficulties in disentangling the impact of the 2008 recession from that of the relocation program after 2007, this study focuses on estimating 2003-2007 effects. It finds that the dispersal of public sector jobs that followed the implementation of the Lyons Review (2004) had an overall positive impact on private sector employment at the local level. Results suggest that the arrival of 10 civil service jobs in an area spurs the creation of about 11 jobs in the private sector. It also affects the sector composition of local jobs: it stimulates services, whereas it has no impact on manufacturing. The study also finds evidence of displacement. There is, in fact, a tendency for private businesses to locate closer to a relocation site, moving out of areas at 1-2km and 2-3km distances and into areas at 0-1km distance. In addition, the study finds that effects are highly localized: i.e., the largest impact is found in areas that received the relocated jobs; spillover effects for an average OA are substantially smaller than the direct effect; and spillover effects reduce sharply over distance. There is no impact beyond the 0-3km ring.

These findings seem robust to a series of checks: verifying that central government employment indeed expanded in areas that received the relocated jobs; conducting a falsification or placebo test by

estimating the impact of the relocation program during a pre-treatment period (2001-2003); replicating the analysis using a coarser geographic scale (the 2001 Lower Layer Super Output Area); and investigating whether larger size relocations are associated with a larger policy impact. Furthermore, the analysis is extended to estimate the policy impact over a longer time period (2003-2010); to explore potential changes in the population demographics in the areas that received relocated jobs; and to investigate the impact of removing government jobs from areas of the South East.

This paper contributes to the growing literature on the evaluation of place-based government policies. As also noted by Einio and Overman (2016), earlier studies were impaired by the problem of non-random placement[4]. 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 programs have been successfully evaluated by Neumark and Kolko (2010); Busso and Kline (2008); and Busso *et al.* (2013). Other less well-known programs, like the New Market Tax Credit, have also been carefully evaluated (see Freedman, 2012 and 2015). In Europe and the UK, evaluations of the French Enterprise zone programs, the UK LEGI and SRB stand out for accuracy.[5] To help identification, this study combines a treatment intensity approach, a set of treatment intensity variables and fine spatial scale data as presently available.

This paper also 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 program using detailed spatial data and taking identification issues seriously. 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, e.g., Marshall *et al.*, 2003; Experian, 2004); others have used regional input-output models[6] aimed at *ex-ante* predicting the local multiplier impact of proposed dispersals (see, also, Ashcroft and Swales, 1982a and 1982b).

In addition, the paper contributes to a small literature (see Rosenthal and Strange, 2003, 2008; Arzaghi and Henderson, 2008; Andersson *et al*., 2004, 2009) on the spatial range of agglomeration effects. These effects seem to be local. Looking at the impact of additional employees on small new establishments in the US, Rosenthal and Strange (2003) suggest that external economies of agglomeration are sharply attenuated by distance. When studying the advertising agency industry in New York City, Arzaghi and Henderson (2008) document significant productivity gain from the co-location of firms in Manhattan, but gains attenuate rapidly over space. Turning to Sweden, Andersson *et al*. (2004, 2009) investigate the impact of university decentralization and find substantial but highly localized spillovers on firm productivity over

[4] As pointed out by the literature on program treatment effects (see Heckman *et al.*, 1999; DiNardo and Lee, 2011), the problem of causal inference in non-experimental evaluations can be substantial.
[5] See Gobillon *et al.* (2012); Mayer *et al.* (2017); Einio and Overman (2016); and Gibbons *et al.* (2011).
[6] There is an extensive literature on regional input-output models. See Miller and Blair (2009) for a textbook reference and Faggio and Overman (2014) for a discussion in this context.

distance. The present study also finds that effects are concentrated within the first few kilometers from a relocation site.

Faggio and Overman (2014) is the paper most closely related to the present one. Using data on 352 English Local Authorities (LAs), they look at the impact of changes in public sector employment on the local labor market. They find that public sector employment has no impact on total private sector employment at the LA level. They do find, however, that public sector employment changes the local composition of private sector jobs: it stimulates the provision of locally-produced services (the non-tradable sector), while it has a negative effect on manufacturing (the tradable sector). The results in this paper are largely consistent with those found in Faggio and Overman (2014).

There are, however, important differences between the two studies. First, Faggio and Overman (2014)'s analysis is not a policy evaluation. They do not explore the specific impact of the Lyons Review or of any other place-based policy, but they look at 2003-2007 changes in total public sector employment at the local level. As documented in Cribb *et al.* (2014), these changes were largely driven by the UK public sector expansion in health and education.[7] On the contrary, the present study provides the first thorough evaluation of a relocation program and focuses on the dispersal of 25,000 central government jobs.

Second, as already noted, Faggio and Overman (2014) conduct the analysis at a much higher level of aggregation (using 352 English LAs) than the one used here (based on about 150,000 Output Areas covering England, Wales and Scotland). Conducting the analysis at the Output Area level strengthens the ability to capture potential multiplier effects – particularly if these effects are localized and disappear sharply over distance. Conversely, conducting the analysis at the LA level might hinder the possibility of detecting such localized effects, even though it helps identify potential general equilibrium effects that tend to operate at a larger geographic scale than the OA.

Comparing the results, Faggio and Overman (2014) identify local multiplier and crowding-out effects operating at the LA level: they find that 100 additional public sector jobs in an area spurred the creation of about 50 service jobs while crowding out 40 manufacturing jobs. The present study detects local multiplier and displacement effects operating at the OA level: it finds that the dispersal of civil service jobs has an overall positive impact on private sector employment, mainly driven by the expansion of local services; and that spillover effects are largely contained within the first three kilometers from a relocation

[7] Using ONS Public Sector Employment data, Cribb *et al.* (2014) document that the majority of public sector jobs were created in health and education during the 2000s. While the number in public administration remained stable at around 1.2-1.3 million, NHS workforce grew by 33.3% (from 1.2 million in 2000 to 1.6 million in 2010) and education grew by 14.3% (from 1.4 to 1.7 million during the same period). Hence, Faggio and Overman (2014)'s measure of public sector growth is more likely to pick up the expansion in health and education than any dispersal of central government workers.

site. The seemingly contradicting results of the two studies are explainable in light of the differences just discussed.

It is worth noting that the debate on public sector relocations is not limited to the UK.[8] Little attention, however, has been paid in previous (largely European) research to estimate the local labor market impact of a relocation program. An exception is the paper by Becker *et al.* (2018), which looks at the rise of Bonn as the new federal capital of Western Germany at the end of World War II. They find that total employment and population in Bonn substantially increased after 1949 relative to a group of 40 control cities. They also find that the impact of government jobs on private sector employment was modest and concentrated in the non-tradable sector. Another exception is the work by Jofre-Monseny et al. (2018), which estimates the effects of public job expansions on decennial changes (1980-1990 and 1990-2001) in the employment and population of Spanish cities. They find that one additional public sector job increases non-tradable employment by 0.9 jobs with hardly any impact on the tradable sector. Yet, they point out that these new jobs do not translate into a substantial reduction in local unemployment as better labor market conditions attract new workers to the city.

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 results. Section 7 provides robustness checks and extensions. Section 8 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 program developed very strong 'push' factors, like 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 submits a formal business case for approval. This requirement changed expectations across government: departments needed to justify their presence in London on the grounds of

[8] 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. In 2015, the Danish government announced the relocation of 3,900 jobs out of Copenhagen and into 30 other Danish cities and towns (see Christiansen, F., http://www.pressreader.com/denmark/politiken/20151011/281522224919239/TextView, October 11, 2015; and https://www.thelocal.dk/20151001/denmark-to-move-4000-jobs-out-of-copenhagen, *The Local*, October 1, 2015).

business needs. Thanks to these push factors, the original target was delivered nearly a year ahead of schedule. By its end, the program relocated more than 25,000 jobs.

The Lyons Review had several main objectives: delivering cost savings to taxpayers by reducing accommodation and labor costs; allowing the modernization of public services; enhancing devolution; and boosting regional development.

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 areas of Central London (see Smith, 2010). Despite the national pay scheme, public sector wages also tend to be higher in London than in the rest of the UK because of the London weighting allowance.[9] Due to the allure of private sector job opportunities in the capital, there are higher retaining and turnover costs.

In the Experian (2004) report (which provides background research for the Lyons Review), relocation is also described as a catalyst for re-organizing 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 modernization.

The primary government benefits of devolution, namely reducing cost pressures and relieving spatial constraints, move 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 relocations 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 program in light of its ability to achieve this last objective.

Larkin (2010) notices that the size of the government's relocation program was fairly small. Over the period 2003-2010, the program dispersed 25,420 jobs out of London and the South East[10]. 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). 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 program 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.

What is interesting is the average number of jobs that successful Travel-to-Work-Areas (TTWAs) managed to attract under the relocation process. TTWAs are a measure of contained local labor markets defined such that at least 75% of the resident population works in the area and 75% of the people working in the area are local residents. TTWAs are obviously much larger than Output Areas (OAs), containing an

[9] The London Weighting is an allowance paid to people who work in London's public sector. Its purpose is to compensate London workers for the extra costs they incur in relation to public sector employees elsewhere in the country.

[10] I could collect information on the origin address 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. See section 5 for details.

average of about 1,650 OAs each, and vary in size[11]. There are a total of 232 TTWAs in England, Wales and Scotland, with 26 of these covering London and the South East. Out of 206 potential destination areas, 124 TTWAs received no relocated jobs whereas 82 TTWAs attracted on average 304 jobs each with a standard deviation of 455. The dispersion is large: those 82 TTWAs received between 1 and 1,948 full-time equivalent jobs. At the OA level, the average number of jobs moved was 89 (equivalent to half the number of 2001 public sector workers in these receiving areas) with a standard deviation of 181. 281 OAs (out of about 167,000)[12] were chosen as the preferred destination of between 1 and 1,276 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) recommends the government '*not to choose a building just because it is available'*, thus suggesting that this might have been the case in past relocations. In addition, it also recommends phasing 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 rationalize the civil service estate and oversee departmental relocations, each individual department was accountable for managing its own relocation program, including filling posts that were transferred, or created, in the new location.

The Smith Review (2010), which followed in the footsteps of the Lyons Review, documents that the implementation process lacked transparency: there was no government strategic or unified framework according to which all relocation decisions should have been made. Even within departmental families, departments did not take direct responsibility for the location choices of their own agencies and Non-Departmental Public Bodies (NDPBs). 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, which used substantial amounts of public money (see Larkin, 2009; Smith, 2010). Lastly, there is no central record of how many workers actually moved with the posts and there are no details of relocation packages offered or compensations taken. This lack of planning and transparency resulted in higher-than-expected relocation costs (Smith, 2010).

3. Conceptual Framework

This section presents a simple theoretical framework. The objective is to outline the mechanisms through which local multiplier effects or crowding out/ displacement effects may come about. Extending the work by Moretti (2010), and Faggio and Overman (2014), I discuss the case of a small geographic area

[11] Out of London, the smallest TTWA contains 34 OAs whereas the largest has 5,500 OAs. Output Areas are very small geographic areas built from clusters of five or six adjacent unit postcodes. See Section 5 for further details.
[12] There are about 218,000 Output Areas covering England, Wales and Scotland; about 50,300 of these are in London and the South East.

characterized by a fixed amount of land and the arrival into the area of public sector workers offering a tradable service. My purpose is to detect the implications of such a move for local businesses.

A nationwide economy is made up of many cities. Each city consists of $\boldsymbol{n}$ output areas, where $\boldsymbol{n}$ is large. Output areas are characterized by Labor (L) and Terrain (T). Labor is free to move across output areas within a city and across cities. The assumption of perfect worker mobility within a city appears justifiable, as workers commute to work from outlying residential areas to the business district, typically located in the city center. The assumption of perfect worker mobility across cities is, in this context, partly justifiable. Worker relocation programs implies a certain degree of worker mobility from origin addresses to destination areas. Even though the Lyons Review did not provide information on how many people move with their job, the design of the program envisaged the possibility that people would move across cities in order to keep their job, being compensated for the cost they would face.

Terrain is an area-specific factor. Each OA is characterized by an endowment of terrain, T. OAs are small geographic areas of about $1km^2$ on average and $7km^2$ in standard deviation. Real estate space for both residential and commercial purposes needs terrain to be built (i.e., land scarcity) and the availability of developable land is further constrained by planning restrictions. Planning restrictions are particularly binding in the UK: a few studies (e.g., Cheshire and Sheppard, 2002; Cheshire and Hilber, 2008; Hilber and Vermeulen, 2016) have documented a substantial impact of regulatory constraints on office space costs and house prices across UK cities. Cheshire and Sheppard (2002) evaluate the net welfare costs these restrictions have on local residents using the town of Reading as a test bed. Cheshire and Hilber (2008) show how regulatory constraints explain the higher cost of office space in Britain relative to other European business locations. Given this evidence, it seems reasonable to assume that the supply of residential and non-residential property is inelastic for any OA in the UK, particularly for the short five-year period considered here.

There are three sectors in the local economy: a public sector which provides a tradable service; a private sector which provides a non-tradable service; and a private sector which provides a tradable good.

A tradable public sector

This sector consists of service jobs in the public sector that do not require face-to-face contact with the public on a regular basis. Therefore, their service could equally be provided in the capital or anywhere else in the nation. This sector includes, for instance, statistician jobs at the Office for National Statistics; accountant jobs at the HM Revenue & Customs; and economist jobs at the Office for Fair Trading. These jobs are likely to involve positions in government and non-ministerial departments, executive agencies and Non-Departmental Public Bodies (NDPBs), i.e. organizations characterized by a limited contact with the general public. These are precisely the types of public sector jobs involved in the relocation program (see

Section 5). Given the nature of these jobs and the fact that they were actually moved, these jobs must offer a tradable service.

A non-tradable private sector

This sector consists of local service jobs in consultancy, real estate, finance and insurance. These jobs tend to be spatially concentrated, typically around the city business district, and serve a relatively large business community. The sector also consists of local service jobs in catering, dry-cleaning and other personal services, which are spread out more evenly across space driven by population demand.

The arrival of a substantial number of public sector jobs in an area could stimulate (through a local multiplier effect) the local demand for intermediate inputs in terms of consultancy, legal, accounting and real estate jobs. Alternatively, the arrival of public sector jobs could increase the consumer demand for catering, hairdressing and other personal services. Considering the spatial distribution of service jobs, increases in intermediate demand are likely to occur near the relocated workplace, spillovers in consumer demand are expected to be more ubiquitous. They could partly occur near the relocated workplace and partly occur near worker homes. In the empirical analysis, I will test whether effects are highly localized, i.e., concentrated around OAs receiving the relocated jobs. If this were the case, I would expect the analysis to capture mostly the impact on intermediate demand instead of consumer demand.

Assuming that the rise in intermediate demand dominates, the arrival of civil servant jobs into an area will result in an increase in revenues paid to the non-tradable private service sector. This surge in revenues will likely spur the sector's jobs. Coupled with this, there will be an increase in the demand for Terrain, because either the government searches a building where to house its relocated jobs or transferred workers look for housing. Given the supply of T is fixed, the increased demand for T will result in upward pressure on housing costs. Higher rents might drive employers and workers out of the area. The positive effect on revenues and jobs in the private sector is the so-called 'local multiplier effect'; the moving-out of local businesses associated with rising housing costs has been labelled 'crowding out effect'.

Time plays a crucial role in understanding the interplay between these two forces. Local multiplier effects are likely to follow swiftly the arrival of public sector jobs into an area, whereas crowding out effects need time to materialize. Local services may promptly react to the surge in consumer demand. Likewise, the government may quickly outsource some activities to local businesses. Conversely, employers tend to face higher rents only at the end of their tenancy agreement and, then, decide how to respond to these upward pressures. As this study focuses on the short-run impact of the relocation, it is more likely to capture local multiplier (rather than crowding out) effects affecting services.[13]

A tradable private sector

I also consider a local private sector which sells a homogenous good which is tradable. Production firms can move anywhere across OAs within a city and across cities. In the empirical analysis (see Section

[13] Besides, as noted in the introduction, conducting the analysis at the OA level hinders the possibility of identifying potential general equilibrium effects operating at a wider geographic scale.

6), I take the conventional view and consider manufacturing activities as providing a tradable good and private service activities as providing a non-tradable service, although recent work in the offshore literature has suggested that service activities can also be ordered by various degree of tradability (see Jensen and Kletzer, 2006).

According to the simple framework sketched so far, the arrival of additional civil servant jobs into an area might also stimulate the demand for the tradable good (e.g., clothing or cars). Since the local production of the homogenous good is small relative to total production, any rise in local demand is likely to be satisfied by national (rather than local) supply. As a consequence, in the short run, no effect prevails. The rise in product demand is unlikely to trigger higher local production; and the upward pressure on *T* needs time to build up. In the long run, the impact on *T* is likely to dominate.

To summarize, the inflow of additional public sector workers into a small geographic area can have local multiplier and crowding out effects. For the service sector, local multiplier effects are likely to dominate in the short run. In the long run, it is hard to know *a priori* which effect will prevail. For the manufacturing sector, both effects are subdued in the short run, whereas crowding out effects are expected to dominate in the long run. According to these predictions, a short-run analysis of the relocation program is likely to detect a positive impact on local services and no clear impact on manufacturing jobs.

4. The Empirical Strategy

There are methodological problems associated with ex-post evaluations and the two concepts of potential outcomes and deadweight may be the most challenging of them. Potential outcomes or counterfactuals refer to the outcome of a policy as compared with what would have occurred without the government intervention. Needless to say, it is impossible to know what would have happened in any of the chosen locales had they not been allocated any public sector jobs. In the literature on causal inference (see Heckman *et al.*, 1999, DiNardo and Lee, 2011), a way of solving this potential outcome problem is to compare treated sites with a suitable control group, but this approach in its simplest form is not viable here.

The related concept of deadweight can be defined as "that part of a public expenditure program which is taken up by recipients other than those to whom the expenditure should, if possible, be directed" (Mceldowney, 1997, p176). Some amount of deadweight is inevitable in any policy intervention. It is often difficult to evaluate the extent of the loss. Special forms of deadweight are displacement and crowding out. As noted in the theoretical framework, public sector employment might put upward pressure on local rents forcing local businesses to move out of the areas into less costly locales (crowding out effect). Alternatively, businesses might decide to relocate in the proximity to a treated site where their product demand is higher, pulling up employment in nearby areas and down in areas further away (displacement effect). Evaluating the extent of crowding out and displacement are the main issues of this paper.

My ex-post evaluation has additional methodological challenges. First, area-based policies raise questions about 'people versus area' effects (see Glaeser and Gottlieb, 2008). When investigating a place-based intervention, we are often interested in detecting its impact on the people originally living or working

in the area. Unfortunately, area level statistics may be contaminated by people leaving the treated areas during the implementation of the policy; thereby reflecting both the change in neighborhood composition and the extent of any policy impact.[14]

Second, 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 *et al.*, 2013; Neumark and Kolko, 2010; Einio and Overman, 2016) have successfully combined empirical strategies such as comparisons of policy applicants and grant recipients; early and late policy rounds; and spatial differencing, with institutional details for helping identification. In this study, limited information is available on how government selected relocation sites. Particularly, I do not know how many potential places were initially considered and according to what criteria the final ones were chosen. This lack of information hampers the possibility of adopting an applicant-recipient or loser-winner comparison. What I know is that about two-third of all destinations were hit by multiple job moves. This last institutional detail hinders the possibility of using early and late policy rounds.

Third, worker relocation programs like the Lyons Review are policies with a potential nationwide impact and of which the geographic spread is not known at the start. As a consequence, the use of spatial differencing is unsuitable in this case. Spatial differencing (i.e., measuring the difference between an area and its neighbor) is explicitly about evaluating neighboring effects. It cannot be used to assess the effects of policies that have a national impact, since it relies on (untestable) assumptions that the spillovers of these policies are limited geographically.

In an attempt to solve some of the identification challenges listed here above – namely, dealing with a non-randomized intervention and not knowing *ex-ante* the geographic spread of the policy, this paper applies a treatment intensity estimation approach, which allows areas to be treated at a different level of intensity instead of assuming a constant treatment effect (see Angrist and Imbens, 1995). The chosen approach replaces (or combines) a treatment dummy, typical of a standard difference-in-differences approach, with treatment intensity variables. In constructing these variables, I do not confine effects to be within certain geographic boundaries. I assume, instead, that all areas can be treated, although areas close

[14] See Section 7 for a robustness check on this potentially confounding issue.

to a relocation site should be affected (i.e., treated) more intensively than areas further away with intensity decreasing monotonically with distance.

In formal terms, this study investigates the impact of a treatment variable and several treatment intensity variables on outcome (e.g., local private sector employment) using the following estimation equation:

$$\Delta \boldsymbol{y_i} = \boldsymbol{\gamma^0 \Delta N_i^0} + \sum_c \boldsymbol{\gamma^c \Delta N_i^c} + \sum_n \boldsymbol{\beta^n X_{i,2001\ (or\ 1991)}^n} + \Delta \boldsymbol{\varepsilon_i} \quad (1)$$

where $\Delta \boldsymbol{y_i}$ is the raw change in the outcome measure of interest over the period 2003-2007 in a Output Area *i*. $\Delta \boldsymbol{N_i^0}$ refers to the total number of jobs moved an $\boldsymbol{OA_i}$ received between 2003 and 2007. Since $\boldsymbol{OA_i}$ could be hit by multiple rounds of relocations during the 2003-2007 period, $\Delta \boldsymbol{N_i^0}$ refers to the cumulative sum. $\Delta \boldsymbol{N_i^c}$ refers to the total number of jobs moved an $\boldsymbol{OA_i}$ faces within distance band *c*, with $\boldsymbol{c = 1, 2, \dots, 6}$. All distance bands have a 1km width. $\sum \boldsymbol{X_{i,2001(or\ 1991)}^n}$ refers to a set of pre-treatment (either 2001 or 1991) area characteristics that include economic activity of residents, age structure, population density, education shares, household size and dwelling characteristics.[15] $\boldsymbol{\varepsilon_i}$ is an error term. All specifications also include Travel-To-Work-Area (TTWA) fixed effects and standard errors are clustered at the TTWA level.

The first treatment variable ($\Delta \boldsymbol{N_i^0}$) identifies the treatment effect on OAs that received public sector jobs during 2003-2007 relative to areas that did not receive such jobs. This is equivalent (apart from measuring the size of the relocation rather than the status of being treated) to a typical treatment dummy in a standard difference-in-differences approach. If the analysis focused exclusively on this variable, it would only capture the policy impact on receiving areas. By introducing treatment intensity variables, the analysis allows for potential spillovers into neighboring areas to be identified.

In investigating the impact of subsidizing commercial space in deprived areas, Gibbons *et al.* (2011)'s analysis is also impaired by not knowing *a priori* the geographic scale of the treatment. To solve this issue, they introduce a set of treatment intensity variables to capture the intensity of the effect for each location by recording the number of subsidized sites within distance bands. Adapting Gibbons *et al.* (2011)'s intuition, I construct a set of treatment intensity variables defined as a non-parametric function of the distance to a relocation site. I proceed as follows: I split Britain into about 218,000 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 draw 1km-wide buffers from each OA centroid and count the total number of jobs moved in each buffer. In doing so, I make the assumption

[15] For a full list of control variables, see Tables A.2 and A.3.

that the effects are additive. I then measure the treatment intensity as an interaction between distance and size, where size refers to the number of jobs moved.

The final sample used in the estimation consists of all OAs (except those in London and the South East from which all relocations originated) that received public sector relocations and/or face relocations occurring within a 50km radius – the radius being drawn from the centroid of the OA. In other words, I assume that the policy is unlikely to affect areas more than 50km away from a relocation site and, thus, drop those areas. In the main analysis (2003-2007) the resulting sample includes 151,912 OAs, of which 227 are receiving areas.

The treatment variable indicated with $\Delta \boldsymbol{N_i^0}$ in eq.(1) is either zero or takes positive values. If $\boldsymbol{OA_i}$ receives no relocated jobs over the period 2003-2007, $\Delta \boldsymbol{N_i^0 = 0}$; if $\boldsymbol{OA_i}$ receives $\boldsymbol{n > 0}$ jobs, $\Delta \boldsymbol{N_i^0 = n}$. Thus, coefficient $\boldsymbol{\gamma^0}$ in eq.(1) captures the direct impact of the number of relocations received in $\boldsymbol{OA_i}$ on employment in $\boldsymbol{OA_i}$, conditional on the number of relocations occurring elsewhere within the 50km radius. The coefficients of the treatment intensity variables (labelled 'spillover' coefficients and indicated as $\boldsymbol{\gamma^c}$ with $\boldsymbol{c = 1, 2, \dots, 6}$ in eq.1) parameterize the spillover effects to $\boldsymbol{OA_i}$ of relocations occurring in different distance bands $\boldsymbol{c}$ drawn from the $\boldsymbol{OA_i}$ centroid up to a 50km distance, each spillover effect being conditional on relocations occurring at a greater distance. Generalizing, spillover coefficients measure the indirect impact of relocations occurring at distance $\boldsymbol{c}$ on an average output area $\boldsymbol{OA_j}$ located $\boldsymbol{c}$ kilometers away from these relocations.

The empirical specification indicated in eq.(1) has two features worth noting. First, it has no explicit control group in term of distance bands. This is because treatment variables are measured in term of relocation size. If these variables were defined in terms of any relocation occurring rather than the number of jobs moved (e.g. using dummy variables that take 0/1 values rather than continuous treatment variables), the 0-50km band effectively would provide the baseline control group.

Second, eq.(1) includes treatment variables constructed in a cumulative way. A graphical representation helps clarify (see Figure 1). Consider two Output Areas, area A and area B, and two relocation sites, site LL137YY and site LL111BW (in Figure 1, sites are identified by postcodes). Assume for simplicity that each site is endowed with one relocated job. Noticing first that the relocation sites are not located within the boundaries of area A or B, the treatment variable in this graphical representation (with the first element referring to area A and the second element referring to area B) is $\Delta \boldsymbol{N^0}$= (0,0). Considering next the three subsequent 1km-wide buffers, the three treatment intensity vectors in this illustration are: $\Delta \boldsymbol{N^1}$= (1,0); $\Delta \boldsymbol{N^2}$= (2,1); $\Delta \boldsymbol{N^3}$= (2,2). If an additional relocation site were located within area A boundaries, this would affect all treatment variables as follows: $\Delta \boldsymbol{N^0}$= (1,0); $\Delta \boldsymbol{N^1}$= (2,0); $\Delta \boldsymbol{N^2}$= (3,1); $\Delta \boldsymbol{N^3}$= (3,2).

Using separate 1km-wide (rather than cumulative) rings in eq.(1) would lead to the same findings. The actual estimates of the two specifications would be different, but it would be possible to derive estimates of one specification from those of the other. The main advantage of using a cumulative definition

of treatment intensity variables is that it simplifies the comparison across coefficients and thus the interpretation of the results. Each coefficient (γ^c) captures the marginal effect of each additional treatment intensity *c* included in the estimation. No *t*-test between γ^c and γ^{c-1} would be necessary in order to verify whether the impact of the two treatment intensity variables are significantly different from each other.[16]

5. Data Construction

This study uses three data sources: Government relocation data provided by the UK Office of Government Commerce (OGC)[17]; the ONS 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 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 other UK destinations between 2003 and 2010. The data give details on the quarter and year 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 geographic 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 defined by the year of the move, the number of jobs moved and a destination address. The majority of these destinations

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[16] If cumulative distance bands were defined as:

$$y = b^1 x_{0-5} + b^2 x_{0-15} + b^3 x_{0-30}$$

Then, non-cumulative distance bands could be defined as:

$$y = (b^1 + b^2 + b^3) x_{0-5} + (b^2 + b^3) x_{5-15} + b^3 x_{15-30}$$

Because:

$$x_{0-30} = x_{0-5} + x_{5-15} + x_{15-30}$$
$$x_{0-15} = x_{0-5} + x_{5-15}$$

The cumulative specification gives coefficients which capture the direct impact of each band (b_1, b_2 and b_3) on the variable of interest. Conversely, the non-cumulative specification of distance bands needs to be followed by *t*-tests on the equality of the coefficients for the different distance bands.

[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 departments (such as Food Standard Agency or Ofsted); executive agencies (some 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 (see Table A.1 for details).

were in England (1,126), followed by Wales (222), Scotland (119), and Northern Ireland (19). Given the limited numbers of relocations into Northern Ireland and the usual difficulties in collecting good quality data for this country, I drop relocations occurring in Northern Ireland. Furthermore, in order to distinguish clearly between origin (London and the South East) and destination (everywhere else in the UK) areas, I also exclude 21 relocations occurring in the South East[19] from the final sample, which consists of 1,445 relocations involving 24,476 job moves within 20 government departments. The bulk of these changes (about 64% of relocations and about 65% of job moves) occurred between 2003 and 2007 (see Figures 2 and 3).[20]

It is worth noting three things: first, my analysis focuses on destination areas. It investigates what happens in areas receiving the additional public sector jobs; it does not try to identify a policy effect in the capital where there are buoyant private sector opportunities easily filling up vacant buildings as they appear. Second, it proved harder to identify the exact postcodes in London or the South East where jobs originated. Out of the initial 1,522 moves, 407 origin addresses report no geographic detail. Conversely, only 36 destination postcodes were not identified. Lastly, the data provide 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. What I know is that civil servant numbers fell by about 16,500 full-time equivalent workers in London and the South East during 2002-2010 against an increase of about 18,900 in the rest of England, Wales and Scotland (Civil Service Statistics, 2002-2010).

The second database I use is the Business Structure Database (BSD), which contains information on about 2.4 million business establishments per year over the period 1997-2011 and includes information on each business' postcode, ownership type, sector of activity (up to 5-digit SIC 2003 code) and total employment.[21] Geocoding the postcodes, I assign each local unit active in England, Wales and Scotland to a 2001 Census Output Area. BSD data classify establishments according to their ownership status, distinguishing between private sector and public sector types.[22] Since the study looks at the policy impact on private businesses, I focus on the former group of establishments and also exclude private sector plants operating in two sectors: Private Households with Employees (SIC95) and Extra-Territorial Organizations and Bodies (SIC99).

Third, I use the UK 1991 and 2001 Census data. From the UK 2001 Census, I select a rich set of Output Area variables measuring local labor market characteristics; demographics and population density; household size and types of dwelling; means of transport; and average commuting distance.[23] Figures are

[19] These 21 relocations are small in size (mean 22; SD 15; min 1; max 60); spread across the whole time period (2003-2010); and do only affect two locations in the South East (Hastings and Milton Keynes).
[20] The first job moves were recorded in Q2 2003, thus preceding the publication of the Lyons Review (2004).
[21] The initial raw data include approximately 3 million local units every year. However, I carry out a series of checks and drop a number of units as detailed in the Appendix.
[22] The private sector consists of all plants registered as Company, Sole Proprietor, Partnership, Non-profit Making Body or Mutual Association. Public sector plants are those defined as Public Corporation/ Nationalized Body, Central Government, and Local Government.
[23] See Tables A.2 and A.3 for details.

available for a range of geographic boundaries. I choose the most local, the 2001 Census Output Area, which builds to larger areas, such as wards, local authorities and commuting zones. The UK 1991 Census also provides a rich set of similarly defined area characteristics. The smallest level of geography available in this older 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 1991 Scottish Output Area) level and then mapped into 2001 Output Areas.[24]

Introduced in England and Wales in 2001, OAs are built from clusters of five or six adjacent unit postcodes. They were designed to have similar population sizes[25] and be as socially homogenous as possible (based on tenure of household and dwelling type).[26] When first delineated, OAs largely consisted of entirely urban postcodes or entirely rural postcodes. In total, there are 165,665 OAs in England; 9,769 in Wales; and 42,604 in Scotland.

The final data issue to be resolved concerns the choice of the time period. Government relocation data cover the eight-year period 2003-2010; there are, however, concerns about including the 2008 recession which might have played out unevenly across space, i.e. being more detrimental to areas that had previously benefited from public sector investment. If a smaller policy impact is found in receiving OAs after 2007 relative to the period before, it would be hard to know whether the smaller impact is solely the result of the policy or the result of the combination of the policy, the recession and the subsequent austerity measures.[27] Thus, confounding issues are the primary reason why my preferred specification ends in 2007. In the analysis that follows, Section 6 focuses on the first five years (2003-2007) of the program, which were also the most intense (see Figure 2); Section 7 extends to a longer time period (2003-2010).

The main analysis focuses on 2003-2007 changes in total private sector employment as the outcome variable. To this end, I aggregate employment data for all BSD establishments that operate in the private sector (SIC11-SIC93) and belong to a given OA. In addition, I aggregate employment data

[24] Enumeration Districts (ED) are the smallest areal unit used in the 1991 Census for England and Wales. These countries are divided into 106,865 EDs with an average size of 420 residents or 175 households.

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

[26] OAs were introduced in Scotland with the 1981 Census, although their definition changed over time. In Scotland, OAs are of relatively smaller size (the minimum OA size is 20 households and 50 residents, 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.

[27] Following the 2008 recession and the rescue of three troubled banks (Royal Bank of Scotland, Northern Rock and Lloyds Banking Group), the UK government introduced departmental spending cuts as part of a fiscal consolidation aimed at bringing the public finances back on to a sustainable path. As a result, the size of public sector employment fell from 6.4 million in 2010 to 5.3 million in 2018, reflecting a drop from 21.9% in 2010 to 16.5% in 2018 in the share of public sector workforce (ONS Public Sector Employment Time Series, 1991-2018). Much of the reduction was concentrated in local government employment (rather than central government or public corporations/ nationalized bodies) which fell by about 850,000 workers during the same period. Furthermore, Cribb *et al.* (2014) document that regions with larger cuts to public employment during 2010-2013 were not those with faster growth in private sector activity.

distinguishing between manufacturing and services, holding the conventional view that classifies the manufacturing sector as the tradable sector whereas the service sector as the non-tradable sector. The former consists of employment in all manufacturing industries (SIC15-SIC37); the latter includes employment in construction (SIC45), transport (SIC60-SIC64), finance, insurance, real estate (FIRE) & business activities (SIC65-SIC74), and trade, catering & personal services (SIC50-SIC55; SIC92-SIC93). More refined industry splits are also considered.

6. Results

6.1. Preliminary steps

As a first preliminary step, I conduct a direct test of the treatment. It would be reassuring to see that public sector employment had indeed risen in destination areas, with no effect observed in areas that did not receive public sector jobs. As noticed in Section 5, BSD data distinguish between private and public sector establishments. In addition, the data list different types of public sector employers. Only establishments whose employer has been classified as central government are included in this preliminary analysis.[28] 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 changes for this type of workers. Results confirm these expectations (see Table 1).

Table 1 shows results using model specifications with an increasing number of distance bands: one-ring (0-50km), three-ring (0-3km; 0-5km; 0-50km) and six-ring specifications (the full model as indicated in eq.1). Experimenting with alternative specifications allows me to verify whether the direct impact on areas that received the relocated jobs (top panel) is affected by the number of distance bands included in the estimation (bottom panel). Table 1 confirms that results are not affected by the number of distance bands included. It also shows that there is no explicit control group in terms of distance bands. This is true across all specifications, thereby highlighting that the absence of a control group in terms of distance bands does not depend on the number of distance bands included. It depends, however, on the use of continuous rather than discrete treatment variables (as discussed in Section 4).[29]

Focusing on the six-ring specification, there is about a 1:1 correspondence between central government employment and relocation size (see Table 1, Columns 7-9, top panel). The arrival of one civil servant job in an area resulted in one additional employee working locally for the central government. The coefficient of 0.9-1.0 is statistically significant and robust to the inclusion of initial area characteristics (Column 8) and pre-trend changes in central government employment (Column 9). Results also indicate the presence of some spillover effects in the first and second rings in proximity to a relocation site (see

[28] See footnote 21.

[29] Among the robustness checks, I use an alternative model specification which includes as a control group all OAs that are more than 50km away from a receiving area and that are not in London and the South East. In this new specification, total observations increase to about 167,000 (212 TTWAs) from about 152,000 (186 TTWAs). As expected, results (see Table W.1 in the Web Appendix) hardly change.

Column 7, bottom panel). These spillover effects, however, disappear when area characteristics and pre-trends are controlled for (see Column 9, bottom panel).

As a second preliminary step, I provide evidence on pre-trends of the main outcome variables (total private sector employment, manufacturing employment, and service employment). Looking at the years preceding the relocation program (up to 2002), receiving and non-receiving OAs show similar trends in the evolution of these variables (see Figure 4, Panels B-D). For instance, the average number of workers in services changed little during 1998-2002 in both receiving and non-receiving OAs: it decreased by 1.38% in the former; it increased by 1.27% in the latter. As for manufacturing, average employment decreased for both groups between 1998 and 2002: it dropped by 11.8% and 8.6% in receiving and non-receiving areas, respectively.

Figure 4 also shows the evolution of the outcome variables during the policy implementation. Looking at the first five years (2003-2007) into the relocation program, both total private sector employment and service employment rose more substantially in receiving than non-receiving areas: total private sector employment rose by 20.7% and 8.8% in receiving and non-receiving OAs, respectively; the corresponding figures for service employment are 29.2% and 16.9%. As for manufacturing, average employment decreased in both groups: -6.2% in receiving OAs; -13.6% in non-receiving OAs.

6.2. Main analysis

The main objective of this study is to evaluate whether the arrival of public sector workers into an area had any impact on local private businesses. In the analysis that follows, I report results with treatment intensity variables defined as the total number of jobs moved within a given distance band.[30] It seems plausible to expect that not only receiving a relocation, but also the number of jobs relocated matters for a local area.

Table 2 shows results for three outcome variables: total private sector employment; manufacturing employment; and service employment. Table 2 is organized as follows: Columns (1), (4) and (7) report baseline results without controls; Columns (2), (5) and (8) include pre-treatment area characteristics as controls; Columns (3), (6) and (9) include both area characteristics and pre-trends. There are concerns that the negative trend in manufacturing employment that started in the 1980s (and continues today) could affect the estimation. In addition, area-specific shocks could be driving the response of businesses in areas local to a relocation site. Therefore, I construct pre-trend variables measuring the changes in total private, manufacturing and service employment during 1998-2002 – a period before the implementation of the program. I then include the 1998-2002 changes in total private sector employment as an additional control in Columns (3). Pre-trends for manufacturing and service employment are added to Columns (6) and (9), respectively.

I start presenting results for total private sector employment across all sectors of the economy (see Table 2, Columns 1-3). Evidence suggests that the implementation of the Lyons Review had a positive

[30] Results obtained using treatment intensity variables defined as binary indicators are available upon request.

impact on total private sector employment in areas that received the relocated jobs (see Table 2, Column 1, top panel). A coefficient of 1.173 (s.e. 0.545) implies that the arrival of 10 public sector jobs in an area triggered the creation of 11.7 additional jobs in the private sector. Estimates are slightly lower (coeff. 1.146; s.e. 0.544), when both area characteristics and pre-trends are included in the estimation. Looking at spillovers (see Table 2, Column 3, bottom panel), there seems to be evidence of displacement: the policy had a positive impact on private sector employment in areas within a 0-1km distance from a relocation site, whereas it had a negative impact on areas at 1-2km and 2-3km distances.

Treatment intensity parameters in Table 2 have two related interpretations: *i*) they parametrize the spillover effects to a receiving area $\boldsymbol{OA_i}$ of relocations occurring in different distance bands, up to 50km from the $\boldsymbol{OA_i}$ centroid; *ii*) they measure the indirect impact of relocations occurring at distance c on an average output area OA_j located c kilometers away from these relocations. Applying this second interpretation to the spillover estimates reported in Table 2, we note that the number of OAs contained in each band increases with distance. For example, the first, second and third rings consist of about 39, 85 and 117 OAs, respectively. By multiplying the coefficients reported in Table 2, Columns 3 (Rows 0-1km, 0-2km and 0-3km) with their corresponding count of OAs, I obtain effects of 3.003, -1.020 and -1.053 for the 0-1km, 1-2km and 2-3km rings, respectively. These effects capture the impact of one relocated job in a receiving OA on all surrounding OAs at a given (i.e. 0-1km, 1-2km and 2-3km) distance. Results indicate that displacement is not complete: the positive impact within the 0-1km band more than compensates the negative impact within the (larger) 1-2km and 2-3km bands. Furthermore, I can compute the average effect for any OA included within the 0-3km circle as a weighted average of direct and indirect effects. By using the coefficients reported in Column 3 (Rows 0km, 0-1km, 0-2km and 0-3km), I obtain an average impact of 0.009.[31] That is, the arrival of 100 civil servant jobs in an area (central to the 0-3km circle) spurs the creation of 0.9 new jobs in the private sector in any of the 242 OAs within the 0-3km space – including the area that received the relocated jobs.[32]

Consider now the results for manufacturing employment. Looking at receiving OAs (see Table 2, Columns 4-6, top panel), the dispersal of public sector jobs had no impact on manufacturing employment

[31] I first define γ^0, γ^1, γ^2, γ^3 as the parameters for the treatment variable and the first three treatment intensity variables (see eq.1) and N^1, N^2, N^3 as the average numbers of OAs in the 0-1km, 1-2km, 2-3km rings, respectively. I then express the average effect for any OA within the 0-3km space (including the receiving area) as follows:

$$avg = (\gamma^0 \times 1 + \gamma^1 \times N^1 + \gamma^2 \times N^2 + \gamma^3 \times N^3)/(1 + N^1 + N^2 + N^3)$$

This computation takes the gains obtained locally around the relocation site and spread them evenly across all OAs within the 0-3km space.

[32] Alternatively, I can derive the overall spillover effect within 0-3km by taking into account the average number of jobs moved by relocation site. To this end, I proceed in three steps: by summing the three ring effects (3.003-1.020-1.053 = 0.930), I first obtain the total spillover effect (0.93) within 0-3km, which captures the impact of one relocated job in a receiving OA on all surrounding 0-3km OAs. Knowing that treated areas received, on average, 70.2 relocated jobs, I next multiply 0.93*70.2=65.30, which gives the total number of jobs created in the surrounding 0-3km OAs. Since there are 227 relocation sites included in my estimation, I then derive the total effect for Britain as 227*65.30=14,823 additional jobs created within 0-3km. This computation potentially ignores any overlapping of 0-3km rings around relocation sites.

during the period 2003-2007. All estimates in the top panel are small in magnitude and not statistically significant. In addition, there are no significant spillover effects.

Turning to services (see Table 2, Columns 7-9), I detect a positive impact of the relocation program. Results without area controls (see Columns 7, top panel) suggest that the arrival of 10 additional public sector jobs in an area triggered the creation of about 12 additional jobs in local services. As area controls and pre-trends are included in the estimation (see Columns 8-9, top panel), coefficients slightly decrease in size, but remain significant at the 5% level. There is also some evidence of displacement, i.e. a tendency for private businesses to locate closer to a relocation site. It is worth noting that spillover effects into areas at 0-1km distance are positive and highly significant (coef. 0.081; s.e. 0.028), whereas spillover effects out of areas at 1-2km and 2-3km distances are smaller in size and not statistically significant. Besides, by taking into account the average number of OAs in the first three rings, the effect within the first band (3.159) is 2.4 times larger than the sum of the negative effects within the second and third (-0.595; -0.702).[33]

Consistent with Faggio and Overman (2014), the results presented in Table 2 indicate that public sector dispersal affects the sector composition of local jobs. While it stimulates local services, it has no impact on manufacturing employment. Differently from Faggio and Overman (2014), Table 2 shows that the dispersal of public sector jobs has an overall positive impact on private sector employment – which is essentially driven by local services; it shows that manufacturing employment is not affected by the relocation program; and it gives evidence of some displacement.

Consistent also with a growing literature on the spatial range of agglomeration effects (see, e.g., Rosenthal and Strange, 2003; Arzaghi and Henderson, 2008), Table 2 shows that effects are highly localized: i.e. the largest impact is found in receiving OAs; average spillovers effects into neighboring OAs are substantially smaller than the direct effect; and these spillovers are largely concentrated within the first three kilometers. For instance, Andersson *et al.* (2004; 2009) look at university decentralization in Sweden as a form of regional fiscal policy. They find that the creation of new research centers and universities is beneficial for local businesses in terms of higher firm productivity and creativity. These productivity gains, however, attenuate sharply over distance. Evidence reported in Table 2 suggests that the implementation of the Lyons Review generates patterns in the UK that are similar to those found in other countries.

6.3. Industry disaggregation

Results presented so far rely on aggregated industry groups. An interesting way of slicing the data is to use a more detailed industry classification which splits the private sector into sub-groups. Coupled with manufacturing and services, the private sector includes agriculture and mining, also referred to as primary industries. Applying eq.(1) to agriculture and mining employment (either combined or separate), I detect essentially no impact of the Lyons Review on primary industries' workers.[34] This is no surprise,

[33] Figures are derived as 3.159 = (0.081 × 39); -0.595 = (-0.007 × 85); and -0.702 = (-0.006 × 117) for the first, second and third rings, respectively.
[34] Results available from the author on request.

given that jobs in those industries are mostly concentrated in rural areas while central government jobs tend to be located in urban areas. Additionally, input-output linkages are weak between primary industries and central government activities.

I also use a more detailed classification which splits the service sector into four types: 1) construction; 2) transport; 3) FIRE, consultancy & business services; 4) trade, catering & personal services. Results indicate that the recent government relocation exercise has spurred the provision of local services in the form of FIRE & business activities as well as trade & catering (see Table 3, Columns 3 and 4). I find a negligible effect on transport and a limited impact on construction. Focusing on receiving OAs (see Table 3, top panel), evidence suggests that the arrival of 10 public sector jobs in an area stimulates the creation of about 7 additional jobs in FIRE & business activities (coeff. 0.671; s.e. 0.334) as well as about 3 jobs in trade & catering (coeff. 0.333; s.e. 0.157). When focusing on spillover effects, I find evidence of some displacement for both sectors. Considering jobs in trade & catering, an OA needs to receive 100 additional public sector jobs in order to attract about 190 jobs in shops and restaurants within the 0-1km distance band while experiencing a loss of about 60 jobs within the 1-2km distance band, being 1.911 and -0.595 the effects for the first and the second rings, respectively.[35]

As pointed out by the relocation literature (see, e.g., Marshall *et al.*, 2005), 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, 2014), both in terms of intermediate demand for consultancy and legal work and/or in terms of consumer demand for catering and personal services. While increases in intermediate demand tend to occur near the relocated workplace, increases in consumer demand might partly occur near the relocated workplace and partly occur near worker homes. With the data presently available, i.e. there is no information about worker homes, I can only capture effects around the relocated workplace. Thus, I would expect increases in intermediate demand to dominate. Table 3 confirms these expectations.

To investigate the issue further, I split the two groups of FIRE & business services and trade & catering activities into their respective sub-groups (see Table 4). Within FIRE & business activities, the strong direct effect is largely explained by the expansion in business services, followed by real estate activities with finance playing a positive (but not statistically significant) role. Regarding spillover effects, business activities matter the most. Within trade & catering, hotels & restaurants account for a larger part of the overall direct effect as shown in Table 4, Column 5, top panel. Both trade and personal services report a positive but not significant coefficient – although the coefficient on personal services is one-fifteenth of that on trade. Regarding spillovers, the sub-groups of trade and hotels & restaurants contribute the most, but personal services also play a role. These findings are consistent with a story that documents

[35] Figures are derived as 1.911 = (0.048 × 39) and -0.595 = (-0.008 × 85).

larger increases in intermediate (rather than consumer) demand mostly occurring around the relocated workplace.

6.4. Placebo test or test for pre-trends

A placebo test is conducted in order to test the validity of the results obtained so far. The test consists in analyzing the policy impact on changes in OA employment prior to the Lyons Review. If Tables 2-4 captured a true policy impact, searching for effects before the implementation of the policy would lead to no significant results. To this end, I use 2001-2003 employment changes as the dependent variable in a regression similar to that of eq.(1). The specification also includes pre-treatment area characteristics retrieved from the 1991 Census and 1998-2000 pre-trend changes. Table 5 distinguishes between total private sector, manufacturing and service employment (see Columns 1-3) as well as splitting services into four main groups (Columns 4-7). The placebo exercise confirms that there were no large differences between receiving and non-receiving OAs before the implementation of the Lyons Review, whereas Tables 2-4 show that these two types of OAs behaved differently thereafter. Considering private sector employment as a whole (Column 1), both direct and indirect effects are not significant. Looking at the split between manufacturing and service employment (Columns 2-3), almost all coefficients do not satisfy the standard levels of significance with the exception being the estimates for the 0-2km spillover effect. Looking at the industry split (Columns 4-7), I also find that almost all coefficients do not satisfy the standard levels of significance with the exceptions being construction and, to a smaller extent, FIRE & business and trade & catering. A negative but small estimate of -0.010 (s.e. 0.005) indicates that construction employment decreased slightly (on average by 1 worker) during 2001-2003 in OAs that would receive 100 relocated jobs at a later period (2003-2007) relative to OAs that would not. Conversely, the coefficient of 0.002 (Column 4, Row 0-1km) indicates that construction employment increased on average by less than 1 worker ($0.078 = 0.002 \times 39$) during 2001-2003 within the first 1km-wide ring surrounding relocation sites at a likewise future period (2003-2007). These findings could signal the presence of very small anticipation effects in and around receiving areas for the construction industry.

6.5. Larger versus smaller size relocations

The underlying assumption in constructing treatment intensities based on size is that the intensity of a relocation does not vary only by geographic distance, but also by the number of jobs moved. Interacting distance with size affects 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 expect that larger relocations should have a larger impact. The direction of the effect, however, is unclear. 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 a relocation. A large mass of public sector workers would strongly stimulate demand for locally-produced goods and services. What was not mentioned in the review is that moving a substantial number of public sector jobs to a specific

area, where housing/commercial real estate supply is limited, could also have an adverse impact on pre-existing activities, e.g. leading to crowding out and displacement.

A way of testing whether larger size relocations are associated with a larger policy impact involves splitting government relocations by distance band and quartile class, thereby creating 24 treatment intensity variables (in the form of binary indicators). These new treatment intensities replace previous treatment intensity variables in regressions similar to those presented in Table 3. For simplicity, Table 6 reports results for receiving areas and spillovers within the first distance band only.[36] As expected, the policy impact varies by the size of the relocation (see Table 6). Focusing on OAs that received the relocated jobs (see Table 6, 0km panel), estimates for the top (i.e. fourth) quartile are substantially larger than those for the bottom (i.e. first) quartile. This holds true across all industry groups.[37] Larger relocations are particularly important in explaining the expansion of transport employment. For this sector, only the estimate for the top quartile is statistically significant. Conversely, relocation size does not seem to matter for construction, for which all estimates are not statistically significant. The picture is different for FIRE & business and trade & catering activities. Relocations of all sizes seem important in explaining the expansion of these two sectors, although larger size relocations report larger coefficients.

Looking at the first set of spillovers (Table 6, 0-1km panel), there is again a difference between FIRE and catering on one side and transport and construction on the other side. Both larger and smaller size relocations exert a positive impact for FIRE & business and trade & catering, whereas transport and construction employment is hardly affected by relocations of any size. Looking at subsequent sets of spillovers (see Table W.2 in the Web Appendix), I also find evidence of displacement with effects concentrated within the first three kilometers from a relocation site. While trade & catering employment is affected by relocations of almost all sizes (particularly at 1-2km distance), FIRE & business activities are influenced by medium-sized relocations (2nd and 3rd quartile).

6.6. A larger geography

Up to now, I have conducted the analysis at a fine spatial scale by using 2001 Census OAs. The rationale for going so local is the following: in order to overcome one of the challenges my analysis faces, i.e. not knowing *a priori* the geographic spread of the policy, I chose the smallest areal unit and built treatment intensity variables starting from the centroid of each OA. One drawback of going so local is that treatment effects might be less precisely estimated than when choosing a coarser geographic scale. In order to verify the stability of my results, this section replicates the analysis using a larger geography, the 2001

[36] The complete table of results is available in the Web Appendix, Table W.2.

[37] Performing F-tests on the equality of the top and bottom quartile coefficients, differences are statistically significant for transport and FIRE & business, whereas they are not for construction and trade & catering. The test has p-value=0.052 (F-stat=3.82) for FIRE and p-value=0.072 (F-stat=3.26) for transport. The corresponding values for catering and construction are p-value=0.138 (F-stat=2.22) and p-value =0.521 (F-stat=0.41).

Lower Layer Super Output Area (LSOA). Built up from groups of four to six adjacent OAs, there are 40,883 LSOAs in England, Wales and Scotland, with an average size of 5.6 km^2 and a S.D. of 27.3 km^2.

First, I construct a new set of treatment intensity variables. After splitting Britain into LSOAs[38], I compute the Euclidean distances between each relocation site and all LSOA centroids. I then draw 1-km wide buffers from each LSOA centroid and count the total number of jobs relocated in each buffer. Treatment intensity variables are defined as the interaction between distance and size. Second, I create sets of 1991 and 2001 LSOA characteristics using information from the Censuses. Third, I apply eq.(1) using (2003-2007) employment changes at the LSOA level as the new outcome variable.

Again, I start with looking at the link between the number of jobs moved and central government employment at the LSOA level (see Table 7). Results shown in Table 7 mirror those reported in Table 1. Focusing on the six-ring specification (Columns 7-9), a coefficient of 1.282 (s.e. 0.506) reported in Column 7, top panel, indicates that for each government job relocated during 2003-2007, about 1.3 new jobs were created in central government in a receiving LSOA over the same period. The coefficient declines to 1.07 (s.e. 0.431) after controlling for initial LSOA characteristics and pre-trends (see Column 9, top panel).

Next, I look at the main effects. I analyze the impact of the policy on the three variables of interest: total private sector employment; manufacturing employment; and service employment (see Table 8). In order to ease the comparison between LSOA and OA estimates, variables in Table 8 are expressed as number of jobs/workers per square kilometer so that LSOA estimates can be interpreted as average effects per square kilometer within a LSOA. Table 2 estimates, which report OA effects, have a similar interpretation since OAs have an average size of 1 km^2. Still, differences remain. LSOA effects are average effects per km^2 over a larger geography (of 5.6 km^2 on average) than OA effects are. Also, the sample of OAs included in the estimation has an average size slightly smaller than 1.[39]

Consistent with the results presented in Table 2, Table 8 shows that the dispersal of public sector jobs had a positive impact on private sector employment in receiving LSOAs. A coefficient of 0.993 (s.e. 0.464) indicate that the arrival of 10 civil service jobs in an area triggered the creation of about 10 additional jobs in the private sector (see Column 1, top panel) – the corresponding figure in Table 2 is 11.7. This impact declines to 7 when initial area characteristics and pre-trends are included in the estimation (see Column 3, top panel) – the corresponding figure in Table 2 is 11.5. As before, I find that the policy changed the sector composition of local jobs: it had essentially no impact on manufacturing while stimulating jobs in local services. Evidence suggests that for 10 new central government jobs created in an area, manufacturing employment does not change whereas service employment rises by about 7 (see Column 9,

[38] While England and Wales introduced LSOAs in 2001, in the same year Scotland introduced data zones (DZ) that are roughly equivalent to LSOAs. DZs in Scotland are smaller in population size (between 500 and 1,000 household residents) than their LSOA counterparts, which have minimum and maximum population thresholds of 1,000 and 3,000 with an average of 1,500 residents.

[39] Although OAs have an average size of 1 km^2, the sample of OAs included in the estimation has an average size of 0.91 km^2: 0.88 km^2 for the group of receiving OAs and 0.91 km^2 for the non-receiving group.

top panel). Regarding spillover effects, I also find evidence of displacement. When splitting services by group and sub-group, results confirm that business services are driving the expansion of local services[40].

By and large, the results obtained using a larger geography are consistent with those obtained using a finer spatial scale. The first stage results are closely replicated by using LSOAs instead of OAs. The main effects are still highly localized: i.e. the largest policy impact is found in receiving LSOAs and spillover effects decline over distance. There are also a few differences, largely due to the reasons pointed out before: direct effects are smaller and spillover effects are larger when using a coarser geography; spillover effects spread over a longer distance – mainly for services (see Column 9, bottom panel); and the positive impact on local services is now split more equally between trade & catering and FIRE & business services.

7. Further robustness checks and extensions

7.1. Further robustness checks

Figure 4 shows that receiving and non-receiving OAs have similar pre-trends in the main outcome variables and Table 5 presents a statistical test of that null hypothesis. Nevertheless, receiving and non-receiving areas might continue to differ in other observable and unobservable characteristics. In order to rule out the possibility that these area differences may confound my estimates, I conduct a further robustness analysis involving an inverse-probability weighted regression-adjustment (IPWA) estimation.[41]

I first construct a balance table using a subset of demographic, labor market and social-economic characteristics included as covariates in eq.(1). The balance table reports estimated coefficients derived from regressions that use the same model specification as the one used in Table 5, the only difference being that the demographic, labor market and social-economic characteristics are used as dependent variables, expressed both in levels (both 1991 and 2001) and in changes (1991-2001 change). Results suggest that there are significant differences between receiving and non-receiving areas. This is particularly true for variables such as population density; shares of the young and elderly in the population; the proportion of females; and shares of inactive and part-time workers.

Having shown that characteristics are not well-balanced between the two groups, I next provide a robustness analysis in which I estimate a propensity score model for the probability of being a receiving OA (where the independent variables are the same covariates included in the balance table) and then reweight the data using inverse probability weighting (see, e.g., Busso *et al.*, 2014). My objective is twofold: (*i*) verifying that after re-weighting covariates balance between receiving and non-receiving areas and (*ii*) confirming that the main findings hold.

Following Busso *et al.* (2014), I then apply an IPWRA estimation and compute the average treatment effect among treated areas (ATET). I choose this approach for three reasons: first, I focused on estimating the ATET because it requires weaker versions of the conditional independence assumption and

[40] Results available from the author on request.
[41] For this robustness analysis, results are reported in the Web Appendix, Tables W.3-W.5.

of the overlap assumption.[42] Given the nature of the dataset I use, some OAs (particularly those in rural areas) are very unlikely to be considered as potential receiving areas, namely their probability to be treated is close to zero. This implies that, for this set of OAs, the overlap assumption is likely to be violated. Second, I used the IPWRA estimator instead of the inverse-probability weighted (IPW) estimator because IPW estimators can become extremely unstable as the overlap assumption gets close to be violated (as is the case here). Third, the IPWRA estimator is considered 'double-robust'. Double-robust estimators lay down both the outcome and the treatment probability models but require only one of the two models to be correctly specified to consistently estimate the treatment effects.

Comparing the IPWRA treatment-effects estimation with a simple relocation dummy estimation, results are similar across the two methods. Looking at total private sector employment, the estimated ATET is 264.573 (s.e. 56,162) whereas the binary indicator for receiving areas (at 0km) shows a coefficient of 274.139 (s.e. 48,858). A coefficient of about 265-275 indicates that total private employment had expanded by 265-275 workers on average during the period 2003-2007 in receiving versus non-receiving areas. The two estimations also report similar results for manufacturing and service employment.

The binary indicator for receiving areas was derived from a specification similar to those reported in Table 2; the only difference being that relocation dummy variables at 0km and at the six consecutive distance bands (0-1km; 0-2km; 0-3km; 0-4km; 0-5km; 0-50km) were used instead of continuous treatment intensity variables. In addition, both the treatment effects specification and the relocation dummy specification include the same number of controls, i.e. the variables listed in the balance table.

The covariate-balance summary associated with the IPWRA estimation shows standardized differences and variance ratios for the raw data and the matched sample. Results indicate that the covariate balance between treatment and non-treatment groups has largely improved after applying IPWRA. In fact, standardized differences are closer to zero and variance ratios are closer to 1. Results also indicate that for some covariates (e.g., unemployment-to-population ratio; population density; the proportion of females; the share of non-whites), balance has not been perfectly achieved.

7.2. Extensions

This section extends the analysis in three ways: 1) estimating the policy impact over a longer time period (2003-2010); 2) exploring potential changes in the population demographics or skill composition of the areas that received relocated jobs; and 3) investigating the impact of removing government jobs from the areas of the South East.

As discussed in Section 5, the main analysis ends in 2007 because there are concerns about incorporating the 2008 recession which might have played out unevenly across space. Nevertheless, extending the analysis to 2010 is an exercise worth pursuing. Table 9 shows the impact of public sector relocations occurring between 2003 and 2010 on private sector activity over the same period. Looking at

[42] The conditional independence assumption means that, after controlling for all observables, the potential outcomes are independent of treatment; the overlap assumption requires that each subject has a positive probability of receiving treatment.

the coefficients on receiving areas (Row 0km), results indicate that public sector employment has a positive but not statistically significant impact on total private sector activity, while affecting the sectoral distribution of jobs towards services and away from manufacturing.

Comparing the 2003-2010 results (see Table 9) with those referring to a period preceding the 2008 recession (see Table 2), the significant policy impact on total private activity has disappeared; the positive impact on services is now one-third of what it was before; and the negative coefficient on manufacturing has doubled and turned statistically significant. Still, the policy continues to affect the sectoral distribution of local jobs towards services and away from manufacturing, with the positive impact on services twice as large as the negative impact on manufacturing.

In discussing potentially confounding issues related to place-based policy evaluations, Section 4 raises questions about 'people versus area' effects. As I aim at estimating a clean policy impact on people originally working in a treated area, I want to verify that changes in the area population demographics during the implementation of the policy do not confound my estimates. Since this study focuses on civil servants moving out of London and the South-East – the two most prosperous UK regions, I find it reasonable to assume that the relocated jobs involve higher-than-average skilled employees. By transferring high-skilled government jobs to an area, the policy might attract additional high-skilled workers, thereby changing the skill composition of the local workforce.

In order to verify whether changes in population demographics may be present and therefore confound my estimates, I proceed as follows: first, I use three editions of the UK Census of Population (1991, 2001 and 2011) and construct two new outcome variables – namely the (2001-2011) change in the number of residents with at least a college degree and the (2001-2011) change in the number of residents with a managerial or professional occupations. Next, I construct (1991-2001) changes in the same outcome variables and use them as pre-trends. Then, I adopt an empirical specification similar to eq.(1), which uses decennial (2001-2011) changes in the number of high-skilled residents (defined either by education or occupation) as the dependent variable; and treatment variables based on the full set of (2003-2010) government relocations (plus pre-trends) as regressors. Results reported in the Web Appendix (Table W.6) indicate that government relocations had no impact on the skill composition of the workforce, with skills identified by either education or occupation.

Although somewhat surprising, these results might be explained by the following reason: population census data collect information on residents rather than people working locally whereas information on relocated jobs is about places where people work rather than places where people live. At the detailed level of geography used here – the 2001 Census Output Area, it is unlikely that people working in an area are also locally resident. This data mismatch is likely to explain my inability to detect a policy impact on the skill composition of the workforce at the OA level. Still, I cannot exclude the possibility of capturing an impact at a higher level of geography. What is relevant for this study is that conducting the analysis at the OA level is robust to confounding issues linked to changes in population demographics.

Up to now, the analysis has estimated the impact of moving government jobs into an area. Estimating fully the impact of moving government jobs out of an area is hindered by data limitations. As pointed out in Section 5, information on where government jobs originated is incomplete. Out of a total of 1,522 moves, geographic details on 407 origin addresses (26.7% of all moves) could not be identified. Out of the remaining 1,115 moves, 82% originated from London whereas 18% came from the South East. Moreover, it would be hard to explore the potential impact of removing government jobs from the capital given its high turnover of private sector opportunities.

In an attempt to extend the analysis by looking at the removal of government jobs from an area, I focus on the OAs of the South East only. In that region, there were 198 government job moves during 2003-2010. I use these 198 moves (mean size 33; S.D 121.7; min 0.25; max 1229) in order to study the impact of the removal of government jobs on private sector activity in the same OAs that lost these very jobs as well as in neighboring OAs (up to 50km distance). I conduct the analysis for 2003-2007 (involving 80 relocations) and for 2003-2010 (involving all 198 relocations).

Results for 2003-2007 indicate that an outflow of public sector jobs from an area significantly reduces total private sector and service employment without any impact on manufacturing activity (see Table 10). The magnitude of the effects is small, ten times smaller than in the case of an inflow of government jobs. A coefficient of -0.08 (s.e. 0.028) indicates that the outflow of 10 government jobs from an area reduces private sector employment by less than one (-0.80) worker. The corresponding coefficient for service employment indicates that the impact is nearly one (-0.96). Extending the analysis to 2010, I find that the negative impact on services slightly decreases (-0.089; s.e. 0.033) whereas the negative impact on total private sector employment disappears. Spillovers seem important (particularly over the period 2003-2007) in explaining the distribution of total private sector activity following an outflow of government jobs. While private sector activity goes down in OAs that lost government jobs, activity goes up in areas at 0-1km distance and drops again in areas at 1-2km distance.

Overall, results seem to indicate that the removal of government jobs from an area has a reverse but more subdued impact on local private sector activity relative to adding government jobs. It is worth noting that since I cannot fully assess the effect of the policy on all the OAs that saw a reduction in public employment (since London is excluded), such a relocation exercise could still be a zero-sum game with the total number of jobs created in receiving OAs being the same as the total number of jobs destroyed in London and the South East.

8. Conclusions

Since World War II, the UK government has used relocation programs 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 two thousands BBC London-based posts to MediaCityUK in Salford[43] and the

[43] Together with the posts, a number of London-based departments were moved to Salford, including parts of Radio 5 Live, BBC Sport, Children's, Learning and BBC Breakfast.

relocation of the Office for National Statistics (ONS) headquarters from London to Newport have attracted much public attention.[44] Advocates of relocation programs believe that such policies help lagging regions through public investment. Opponents of this view consider relocation programs (and the associated redundancy packages for workers) as a waste of taxpayers' money. Despite the media attention, there is scarce evidence of the effects of a public sector relocation program upon local labor markets. This study has tried to fill this gap by assessing the local labor market impact of a public sector relocation initiative, the 2014 Lyons Review.

The study has found that the implementation of the Lyons Review had a short-run multiplier effect on private sector employment at the local level. The arrival of 1 civil service jobs in an area triggered the creation of 1.1 local jobs in the private sector. It also changed the sector composition of local jobs: it spurred the provision of locally-produced services, whereas it had no impact on manufacturing. The study has also found evidence of displacement: service employers seem to move out of areas at 1-2km and 2-3km distances from a relocation site into areas at 0-1km distance. These agglomeration effects appear highly localized: i.e., the largest policy impact is found in areas that received the relocated jobs; spillover effects for an average OA are substantially smaller than the direct effect; and spillover effects reduce sharply over distance. Evidence has also shown that the policy had no impact beyond the 0-3km ring.

The analysis was extended in several directions, of which two are worth recalling here. When looking at a longer time period (2003-2010), the significant policy impact on total private activity disappeared; the positive effect on services was reduced by two-third; and the negative coefficient on manufacturing doubled, turning statistically significant. Still, the policy continued to affect the sectoral distribution of local jobs towards services and away from manufacturing, with the positive effect on services twice as large as the negative impact on manufacturing. When looking at the removal of government jobs from an area, the policy had a reverse but more subdued impact on local private sector activity (mainly services), with estimated effects being ten times smaller than in the case of an inflow of government jobs.

Overall, the policy appears to have been beneficial. It acted as a Keynesian-type fiscal stimulus at the local level. It triggered the much sought-after local multiplier effect, even though its impact was restricted to service activities. Two caveats remain: first, this analysis has provided reliable short-run estimates of the policy impact. Although indicative, longer-time estimates need to be taken with caution given potentially confounding issues in the years after 2007. Second, my conclusions are based on a partial analysis. This study has presented an accurate evaluation of the policy impact on private sector employment, but the study still provides a partial analysis. Evaluating both the costs and benefits of the policy at the local level requires a thorough analysis of the long-run evolution of labor costs and housing

[44]For recent press articles, see Johnston, C., 2014. "BBC now has more staff outside London than in the capital". *The Guardian*, October 31. http://www.theguardian.com/media/bbc-salford-move; Giles, C., 2014. "UK GDP figures harmed by statistics office move to Wales". *The Financial Times*, July 8. http://on.ft.com/1vWNBKn.

prices before and after the implementation of the program – because of data limitation, such evaluation is outside the scope of this study.

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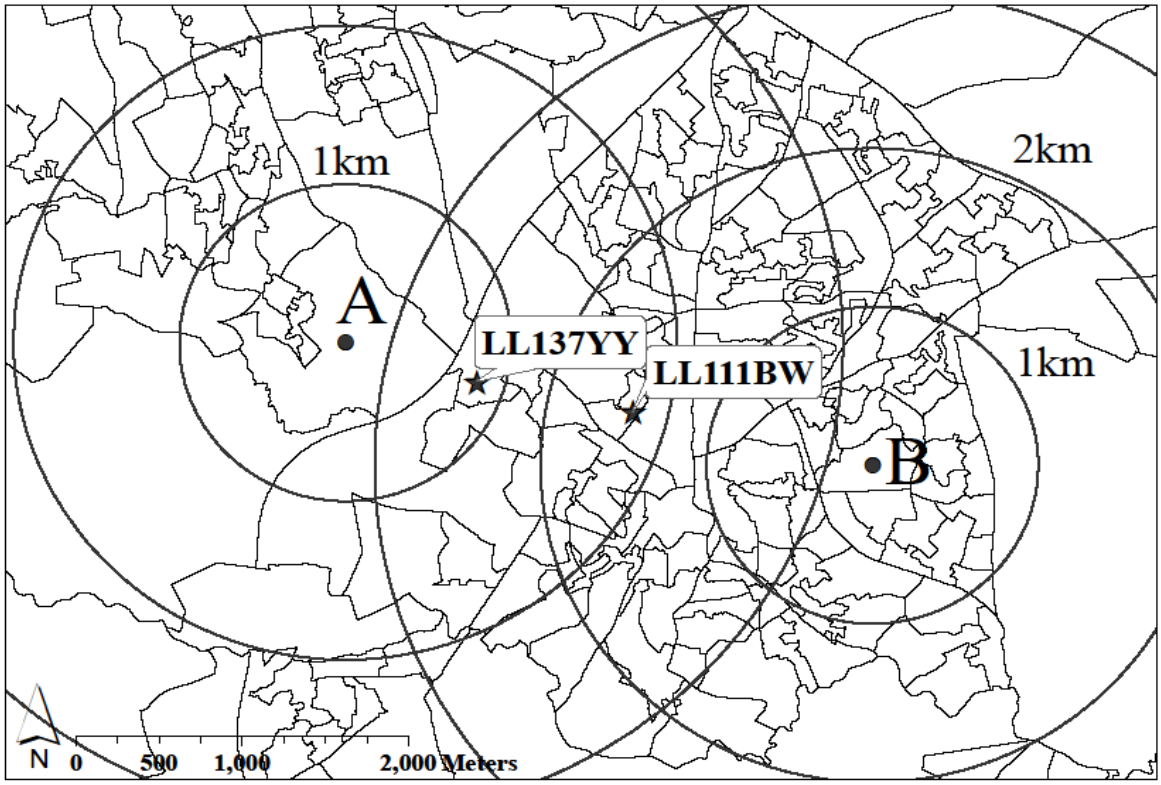
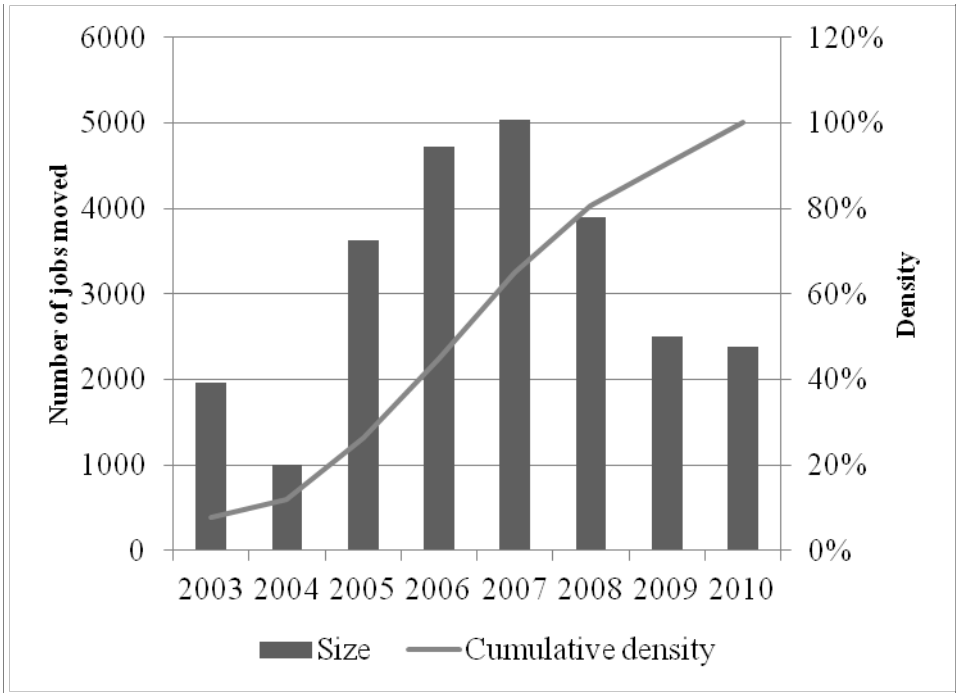
Figure 1: Graphical representation of treatment intensity variables



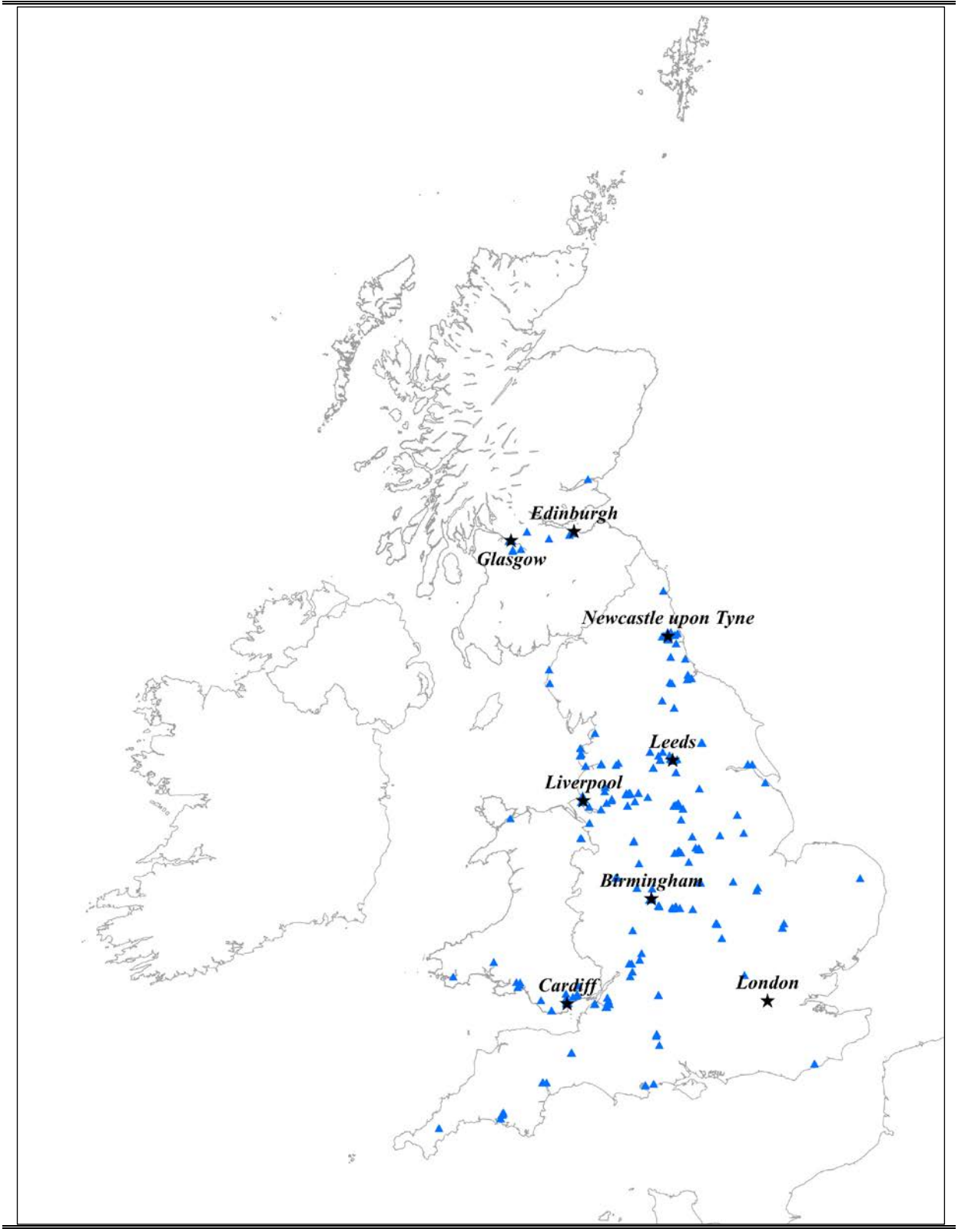
Note: A and B are two Output Area centroids, whereas LL137YY and LL111BW are the postcodes of two actual relocation sites in Wrexham, North Wales.
Sources: OGC Government relocation data, 2003-2010; UK 2001 Census Output Area shape-files; ONS Postcode Directory, May 2016.

Figure 2: Size and density of government relocations by year



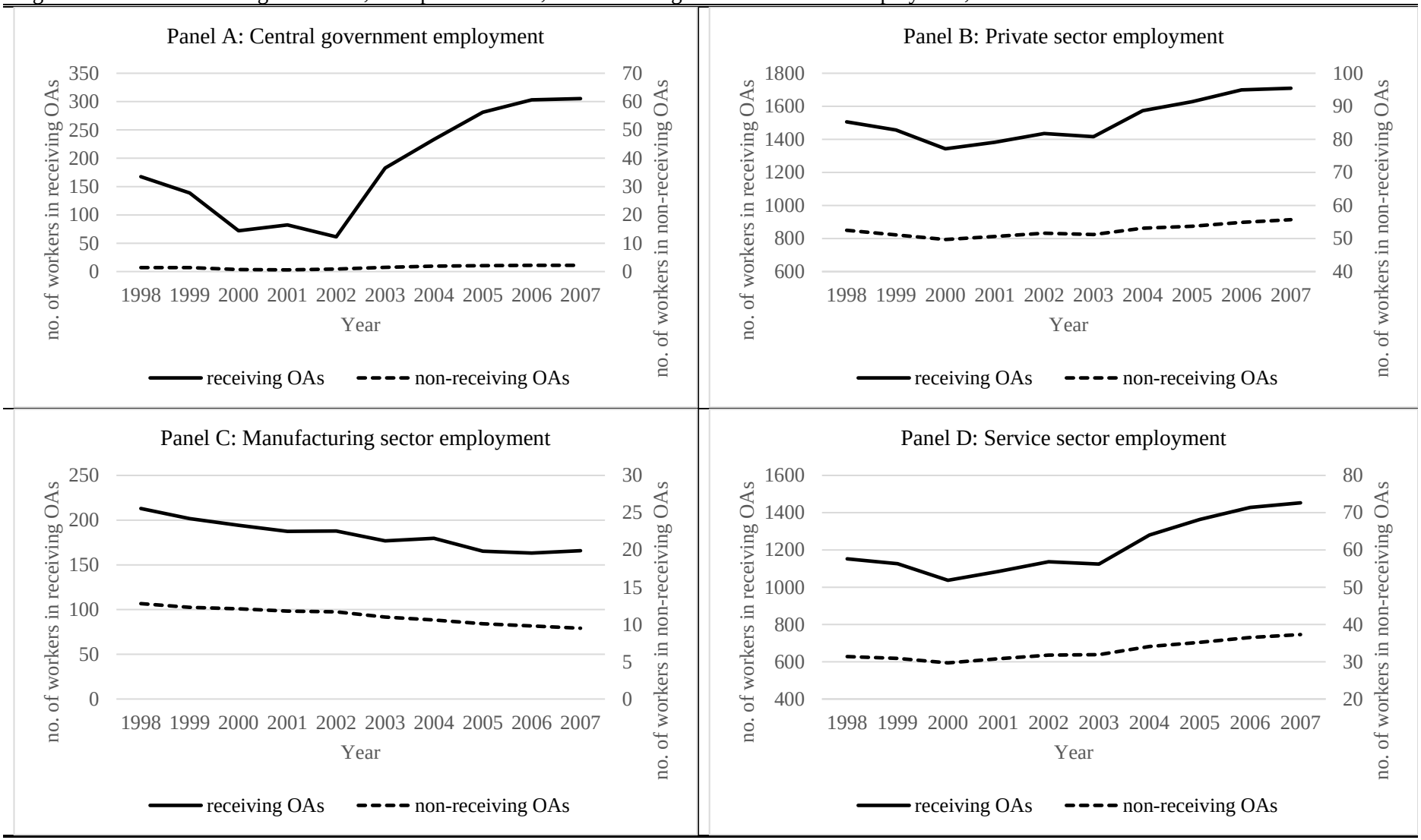
Note: Size refers to the number of jobs relocated each year; the size axis is on the left-hand side; the density axis is on the right-hand side. Even though the Lyons Review was published in 2004, the first job moves were recorded in Q2 2003. Between Q2 2003 and Q4 2004, about 3,000 jobs (12% of all jobs) were relocated.
Source: OGC Government relocation data, 2003-2010.

Figure 3: Spatial distribution of government relocations, 2003-2007



Note: the graph plots government relocations that occurred between Q2 2003 and Q4 2007. Plotting 2003-2010 relocations would give a similar picture, since more than 2/3 of sites were hit by multiple relocations.
Source: OGC Government relocation data, 2003-2010.

Figure 4: Trends in central government, total private sector, manufacturing and service sector employment, 1998-2007



Note: The sample consists of 151,912 observations: 151,685 non-receiving OAs and 227 receiving OAs.
Sources: OGC Government relocation data, 2003-2007; BSD local unit data, 1997-2011.

Table 1: The impact of 2003-2007 cumulative relocations on (2002-2007) changes in central government employment

	One-ring specification			Three-ring specification			Six-ring specification		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Receiving areas									
0km	1.076**	1.046**	0.929**	1.069**	1.046**	0.929**	1.039**	1.026**	0.915**
	(0.453)	(0.452)	(0.415)	(0.453)	(0.452)	(0.415)	(0.455)	(0.454)	(0.417)
Spillovers									
0-1km							0.029***	0.022***	0.013
							(0.010)	(0.008)	(0.008)
0-2km							0.007**	0.002	0.004
							(0.003)	(0.003)	(0.003)
0-3km				0.007***	0.001	0.001	0.000	-0.002	-0.002*
				(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)
0-4km							0.001	0.000	-0.000
							(0.002)	(0.002)	(0.001)
0-5km				-0.001	-0.001	-0.001	-0.001	-0.001	-0.001
				(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
0-50km	-0.002***	-0.002***	-0.002***	-0.003***	-0.002***	-0.002***	-0.003***	-0.002***	-0.002***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
Area controls		√	√		√	√		√	√
Pre-trends			√			√			√
Observations	151,912	151,912	151,912	151,912	151,912	151,912	151,912	151,912	151,912
# of clusters	186	186	186	186	186	186	186	186	186

Note: Robust standard errors are reported in parentheses; *, **, *** indicate significance at the 10%, 5% and 1% levels, respectively. In all columns, the dependent variable is defined as (2002-2007) changes in central government employment. Columns (1)-(3) use a one-ring specification that includes the 0-50km treatment intensity variable only; Columns (4)-(6) use a three-ring specification that includes three treatment intensity variables: 0-3km, 0-5km and 0-50km; Columns (7)-(9) use the full specification as indicated in eq.(1) which includes six treatment intensity variables. Columns (1), (4) and (7) do not include controls; Columns (2), (5) and (8) include UK 2001 Census area controls (see Appendix, Tables A.2 and A.3 for details); and Columns (3), (6) and (9) include both area controls and pre-trends. Pre-trends are defined as (1998-2001) changes in central government employment at the OA level. All regressions include 186 TTWA fixed effects and standard errors are clustered at the TTWA level (186 clusters).

Sources: OGC Government relocation data, 2003-2007; BSD local unit data (public sector employers only), 1997-2011; UK Census of Population, 2001.

Table 2: The impact of (2003-2007) relocations on total private sector, manufacturing and service employment, (2003-2007) employment changes

	Total Private Sector			Manufacturing			Services		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Receiving areas									
0km	1.173**	1.136**	1.146**	0.076	0.084	0.081	1.173**	1.130**	1.152**
	(0.545)	(0.543)	(0.544)	(0.090)	(0.090)	(0.093)	(0.521)	(0.517)	(0.518)
Spillovers									
0-1km	0.094***	0.077***	0.077**	-0.001	0.002	0.002	0.102***	0.082***	0.081***
	(0.031)	(0.029)	(0.030)	(0.004)	(0.004)	(0.004)	(0.030)	(0.028)	(0.028)
0-2km	0.002	-0.011*	-0.012*	-0.005	-0.003	-0.004	0.007	-0.007	-0.007
	(0.006)	(0.006)	(0.006)	(0.004)	(0.004)	(0.004)	(0.005)	(0.006)	(0.005)
0-3km	-0.004	-0.009**	-0.009*	-0.003	-0.002	-0.002	-0.000	-0.007	-0.006
	(0.005)	(0.005)	(0.005)	(0.002)	(0.002)	(0.002)	(0.004)	(0.004)	(0.004)
0-4km	0.003	0.003	0.003	-0.001	-0.001	-0.001	0.001	0.001	0.001
	(0.006)	(0.005)	(0.005)	(0.002)	(0.002)	(0.002)	(0.006)	(0.005)	(0.005)
0-5km	-0.003	-0.001	-0.001	0.001	0.001	0.001	-0.002	-0.000	-0.001
	(0.002)	(0.003)	(0.003)	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)	(0.003)
0-50km	-0.004***	-0.003**	-0.003**	0.000	0.000	0.000	-0.004***	-0.003***	-0.003***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)
Area controls		√	√		√	√		√	√
Pre-trends			√			√			√
Observations	151,912	151,912	151,912	151,912	151,912	151,912	151,912	151,912	151,912
# of clusters	186	186	186	186	186	186	186	186	186

Note: Robust standard errors are reported in parentheses; *, **, *** indicate significance at the 10%, 5% and 1% levels, respectively. In columns (1)-(3), the dependent variable is defined as (2003-2007) changes in total private sector employment across all sectors of the economy except agriculture and forestry (SIC11-SIC93). In columns (4)-(6), the dependent variable is defined as (2003-2007) changes in manufacturing employment (SIC15-SIC37). In columns (7)-(9), the dependent variable is defined as (2003-2007) changes in service employment (SIC45-SIC93). Columns (1), (4) and (7) do not include area controls; Columns (2), (5) and (8) include area controls; and Columns (3), (6) and (9) include both area controls and pre-trends. Pre-trends are defined as (1998-2002) changes in total private sector employment (Column 3), manufacturing (Column 6) and services (Column 9). All regressions include 186 TTWA fixed effects and standard errors are clustered at the TTWA level (186 clusters).
Sources: OGC Government relocation data, 2003-2007; BSD local unit data (private sector employers only), 1997-2011; UK Census of Population, 2001.

Table 3: Splitting services by type, (2003-2007) changes in employment

	Construction (1)	Transport (2)	FIRE & Business (3)	Trade & Catering (4)
Receiving areas				
0km	0.040*	0.115	0.671**	0.333**
	(0.021)	(0.073)	(0.334)	(0.157)
Spillovers				
0-1km	-0.001	0.002	0.031**	0.049***
	(0.001)	(0.004)	(0.015)	(0.016)
0-2km	0.000	0.001	-0.002	-0.007***
	(0.000)	(0.001)	(0.003)	(0.002)
0-3km	0.000	-0.001**	-0.002	-0.003
	(0.000)	(0.001)	(0.002)	(0.002)
0-4km	-0.000	0.001	-0.000	0.001
	(0.000)	(0.001)	(0.002)	(0.003)
0-5km	-0.000	0.000	0.000	-0.001
	(0.000)	(0.000)	(0.001)	(0.001)
0-50km	-0.000	-0.000	-0.002***	-0.001**
	(0.000)	(0.000)	(0.001)	(0.000)
Observations	151,912	151,912	151,912	151,912
# of clusters	186	186	186	186

Note: Robust standard errors are reported in parentheses; *, **, *** indicate significance at the 10%, 5% and 1% levels, respectively. Dependent variables are defined as (2003-2007) employment changes in construction (Column 1), transport (Column 2), FIRE & business (Column 3), and trade & catering (Column 4). All columns include 2001 area characteristics and (1998-2002) pre-trends. All regressions include 186 TTWA fixed effects and standard errors are clustered at the TTWA level.
Sources: See Table 2.

Table 4: Splitting selected services by sub-group, (2003-2007) employment changes

	FIRE & business			Trade & Catering		
	Finance	Real Estate	Business	Trade	Hotels & restaurants	Personal services
	(1)	(2)	(3)	(4)	(5)	(6)
Receiving areas						
0km	0.162	0.066**	0.397**	0.148	0.168**	0.010
	(0.162)	(0.031)	(0.186)	(0.112)	(0.079)	(0.021)
Spillovers						
0-1km	0.002	0.003	0.025***	0.024***	0.020**	0.005***
	(0.004)	(0.002)	(0.009)	(0.008)	(0.008)	(0.002)
0-2km	-0.001	0.001***	-0.002	-0.006***	0.000	-0.001*
	(0.001)	(0.000)	(0.002)	(0.001)	(0.001)	(0.001)
0-3km	-0.002*	0.000	0.001	-0.002	-0.000	-0.000
	(0.001)	(0.000)	(0.002)	(0.001)	(0.001)	(0.000)
0-4km	0.001	-0.000	-0.001	0.001	-0.000	0.000
	(0.001)	(0.000)	(0.001)	(0.002)	(0.001)	(0.000)
0-5km	-0.000	0.000	0.001	-0.000	-0.001	-0.000
	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)
0-50km	-0.000**	-0.000***	-0.001*	-0.001***	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	151,912	151,912	151,912	151,912	151,912	151,912
# of clusters	186	186	186	186	186	186

Note: Robust standard errors are reported in parentheses; *, **, *** indicate significance at the 10%, 5% and 1% levels, respectively. In Column (1), the dependent variable is defined as (2003-2007) employment changes in finance and insurance (SIC65-SIC67). Similarly, Columns (2)-(6) use (2003-2007) employment changes in real estate activities (SIC70-SIC71), business services (SIC72-SIC74), wholesale and retail trade (SIC50-SIC52), hotels and restaurants (SIC55), and personal services (SIC92-SIC93), respectively. All columns include 2001 area characteristics and (1998-2002) pre-trends. All regressions include 186 TTWA fixed effects and standard errors are clustered at the TTWA level.
Sources: See Table 2.

Table 5: Placebo Test, (2001-2003) employment changes adding (1998-2000) pre-trends

	Total Private Sector	Manufacturing	Services	Construction	Transport	FIRE & Business	Trade & Catering
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Receiving areas							
0km	0.140	0.012	0.154	-0.010*	0.084	0.052	0.037
	(0.126)	(0.044)	(0.105)	(0.005)	(0.057)	(0.100)	(0.048)
Spillovers							
0-1km	0.008	-0.002	0.008	0.002***	-0.000	0.006	-0.004
	(0.009)	(0.003)	(0.007)	(0.001)	(0.003)	(0.006)	(0.005)
0-2km	-0.001	-0.006**	0.006**	-0.000	-0.000	0.005**	0.002
	(0.004)	(0.003)	(0.002)	(0.000)	(0.001)	(0.002)	(0.002)
0-3km	0.001	0.001	-0.002	-0.000	-0.001	0.001	-0.002
	(0.003)	(0.002)	(0.002)	(0.000)	(0.001)	(0.001)	(0.001)
0-4km	0.000	0.000	0.000	-0.000	0.000	-0.001	0.001
	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)
0-5km	-0.002	-0.000	-0.001	0.000	0.000	-0.000	-0.001**
	(0.002)	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)
0-50km	-0.000	0.000	-0.001	-0.000***	-0.000	-0.001	-0.000
	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	151,912	151,912	151,912	151,912	151,912	151,912	151,912
# of clusters	186	186	186	186	186	186	186

Note: Robust standard errors are reported in parentheses; *, **, *** indicate significance at the 10%, 5% and 1% levels, respectively. Dependent variables are defined as (2000-2003) employment changes in total private services (Column 1), manufacturing (Columns 2), services (Columns 3), construction (Column 4), transport (Column 5), FIRE & business (Column 6), trade and catering (Column 7). All columns include UK 1991 Census area characteristics (see Appendix for details) and (1998-2000) pre-trends. Pre-trends are defined as (1998-2000) employment changes in total private services (Column 1), manufacturing (Columns 2), services (Columns 3), construction (Column 4), transport (Column 5), FIRE & business (Column 6), trade and catering (Column 7). All regressions include 186 TTWA fixed effects and standard errors are clustered at the TTWA level.

Sources: OGC Government relocation data, 2003-2007; BSD local unit data (private sector employers only), 1997-2011; UK Census of Population, 1991.

Table 6: The impact by quartile class and distance band, (2003-2007) employment changes

	Bottom →						**Top**	
	1st quartile		2nd quartile		3rd quartile		4th quartile	
	Coeff.	SE	Coeff.	SE	Coeff.	SE	Coeff.	SE
Receiving areas 0km								
Construction	5.001	(3.181)	1.368	(3.766)	1.069	(2.810)	9.348	(5.668)
Transport	11.288	(11.572)	2.610	(7.079)	1.490	(12.478)	72.526**	(28.573)
FIRE	67.653***	(23.817)	122.969***	(41.040)	131.547***	(48.107)	267.652***	(97.455)
Catering	69.373**	(28.555)	106.959***	(27.579)	136.327***	(36.207)	166.701***	(53.494)
Spillover 0-1km	1st quartile		2nd quartile		3rd quartile		4th quartile	
Construction	0.059	(0.255)	-0.140	(0.230)	-0.161	(0.259)	0.115	(0.257)
Transport	-0.528	(0.697)	0.379	(0.568)	-0.116	(0.701)	0.313	(1.159)
FIRE	-1.262	(1.397)	3.782*	(2.185)	0.740	(1.551)	5.893*	(3.012)
Catering	1.671	(1.235)	1.567*	(0.845)	1.032	(1.930)	6.374***	(1.904)
Observations	151,912		151,912		151,912		151,912	
# of clusters	186		186		186		186	

Note: Robust standard errors are reported in parentheses; *, **, *** indicate significance at the 10%, 5% and 1% levels, respectively. Treatment intensity variables are split by distance and quartile classes. For instance, 0-1km treatment intensity variable is split into four components, each capturing a different quartile of the size of the relocations. All regressions include 2001 area controls, (1998-2002) pre-trends, 186 TTWA fixed effects and standard errors are clustered at the TTWA level. The sample includes 151,912 observations. A full table of results is available in the Table W.2 in the Web Appendix.
Sources: See Table 2.

Table 7: The impact of 2003-2007 cumulative relocations on (2002-2007) changes in central government employment, LSOA level

	One-ring specification			Three-ring specification			Six-ring specification		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Receiving areas									
0km	1.409***	1.296***	1.079**	1.385***	1.310***	1.087**	1.282**	1.258**	1.071**
	(0.499)	(0.477)	(0.426)	(0.498)	(0.479)	(0.427)	(0.506)	(0.486)	(0.431)
Spillovers									
0-1km							0.096**	0.064**	0.017
							(0.040)	(0.030)	(0.025)
0-2km							0.027*	-0.012	0.003
							(0.016)	(0.013)	(0.010)
0-3km				0.028***	-0.012**	-0.006*	0.007	-0.010	-0.006
				(0.005)	(0.005)	(0.004)	(0.012)	(0.010)	(0.008)
0-4km							0.001	-0.003	-0.005
							(0.011)	(0.010)	(0.010)
0-5km				-0.003	-0.006	-0.005	-0.003	-0.005	-0.003
				(0.003)	(0.005)	(0.005)	(0.004)	(0.007)	(0.008)
0-50km	-0.015***	-0.013***	-0.012***	-0.015***	-0.012***	-0.011***	-0.015***	-0.012***	-0.011***
	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)
Area controls		√	√		√	√		√	√
Pre-trends			√			√			√
Observations	28,154	28,154	28,154	28,154	28,154	28,154	28,154	28,154	28,154
# of clusters	184	184	184	184	184	184	184	184	184

Note: Standard errors are reported in parentheses; *, **, *** indicate significance at the 10%, 5% and 1% levels, respectively. All regressions include 184 TTWA fixed effects and standard errors are clustered at the TTWA level (184 clusters). All variables are defined as in Table 1, but they are computed at the LSOA level.

Sources: See Table 1.

Table 8: The impact on total private sector, manufacturing and service employment, (2003-2007) employment changes, LSOA level

	Total Private Sector			Manufacturing			Services		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Receiving areas									
0km	0.993**	0.747*	0.680*	-0.052	-0.010	-0.015	1.088**	0.800*	0.735*
	(0.464)	(0.429)	(0.417)	(0.108)	(0.107)	(0.111)	(0.465)	(0.416)	(0.409)
Spillovers									
0-1km	0.751***	0.498***	0.513***	-0.041	0.008	0.010	0.798***	0.518***	0.518***
	(0.188)	(0.153)	(0.166)	(0.039)	(0.034)	(0.034)	(0.189)	(0.157)	(0.168)
0-2km	-0.026	-0.200***	-0.200***	-0.049	-0.019	-0.023	0.049	-0.151***	-0.142***
	(0.059)	(0.066)	(0.066)	(0.031)	(0.029)	(0.027)	(0.043)	(0.045)	(0.042)
0-3km	-0.107	-0.127**	-0.128**	-0.040**	-0.020	-0.018	-0.094	-0.125**	-0.130***
	(0.082)	(0.057)	(0.055)	(0.020)	(0.017)	(0.017)	(0.073)	(0.051)	(0.048)
0-4km	0.073*	0.068	0.075	0.009	0.022	0.023	0.018	0.004	0.014
	(0.043)	(0.047)	(0.047)	(0.019)	(0.015)	(0.015)	(0.033)	(0.038)	(0.038)
0-5km	0.047	0.037	0.033	-0.009	0.004	0.006	0.076**	0.066**	0.059**
	(0.051)	(0.046)	(0.044)	(0.011)	(0.011)	(0.010)	(0.037)	(0.032)	(0.029)
0-50km	-0.035	0.015	0.011	0.003	-0.003	-0.002	-0.025	0.032	0.028
	(0.038)	(0.031)	(0.030)	(0.020)	(0.018)	(0.018)	(0.032)	(0.023)	(0.023)
Area controls		√	√		√	√		√	√
Pre-trends			√			√			√
Observations	28,154	28,154	28,154	28,154	28,154	28,154	28,154	28,154	28,154
# of clusters	184	184	184	184	184	184	184	184	184

Note: Robust standard errors are reported in parentheses; *, **, *** indicate significance at the 10%, 5% and 1% levels, respectively. Variables are expressed as employment density, i.e. number of jobs divided by the number of employees per square kilometer. In columns (1)-(3), the dependent variable is defined as the (2003-2007) changes in total private sector employment per square kilometer (SIC11-SIC93). Columns (4)-(5) refer to manufacturing employment per square kilometer; Columns (7)-(9) refer to service employment per square kilometer. Treatment intensity variables are expressed as the weighted average of the number of civil service jobs relocated into a band *c*, with weights being the surface (expressed in square kilometers) of the OAs receiving the relocated jobs within that band *c*. Pre-trends are defined as the (1998-2002) changes in total private sector, manufacturing, and services employment, with variables expressed as employment density. All regressions include 184 TTWA fixed effects and standard errors are clustered at the TTWA level (184 clusters).
Sources: See Table 2.

Table 9: The impact of (2003-2010) relocations on total private sector, manufacturing and service employment, (2003-2010) employment changes

	Total Private Sector			Manufacturing			Services		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Receiving areas									
0km	0.218	0.199	0.205	-0.184*	-0.170*	-0.173*	0.351**	0.334**	0.344**
	(0.187)	(0.188)	(0.188)	(0.099)	(0.097)	(0.096)	(0.153)	(0.153)	(0.152)
Spillovers									
0-1km	-0.007	-0.009	-0.008	-0.011**	-0.006	-0.005	0.015*	0.011	0.012
	(0.012)	(0.012)	(0.012)	(0.005)	(0.005)	(0.005)	(0.008)	(0.008)	(0.008)
0-2km	-0.006	-0.007	-0.008	-0.007**	-0.004	-0.005	-0.002	-0.004	-0.004
	(0.006)	(0.006)	(0.006)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
0-3km	-0.004	-0.004	-0.004	-0.001	0.000	0.000	-0.001	-0.002	-0.001
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
0-4km	-0.000	0.001	0.001	-0.003**	-0.003*	-0.003*	0.002	0.002	0.002*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
0-5km	-0.000	0.002	0.002	0.002**	0.002***	0.002***	-0.002**	-0.000	-0.000
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
0-50km	0.000	0.000	0.001	-0.000	-0.000	-0.000	0.000	0.000	0.000
	(0.001)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Area controls		√	√		√	√		√	√
Pre-trends			√			√			√
Observations	152,035	152,035	152,035	152,035	152,035	152,035	152,035	152,035	152,035
# of clusters	187	187	187	187	187	187	187	187	187

Note: Robust standard errors are reported in parentheses; *, **, *** indicate significance at the 10%, 5% and 1% levels, respectively. In Columns (1)-(3), the dependent variable is defined as (2003-2010) changes in total private sector employment across all sectors of the economy except agriculture and forestry (SIC11-SIC93). In Columns (4)-(6), the dependent variable is defined as (2003-2010) changes in manufacturing employment (SIC15-SIC37). In Columns (7)-(9), the dependent variable is defined as (2003-2010) changes in service employment (SIC45-SIC93). Columns (1), (4) and (7) do not include 2001 area characteristics; Columns (2), (5) and (8) include area controls; and Columns (3), (6) and (9) include both area controls and pre-trends. Pre-trends are defined as (1998-2002) changes in total private sector employment (Column 3), manufacturing (Column 6) and services (Column 9). All regressions include 187 TTWA fixed effects and standard errors are clustered at the TTWA level.
Sources: OGC Government relocation data, 2003-2010; BSD local unit data (private sector employers only), 1997-2011; UK Census of Population, 2001.

Table 10: The impact of removing government jobs from areas of the South East of England (2003-2007)

	Total Private Sector			Manufacturing			Services		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Receiving areas									
0km	-0.046*	-0.084***	-0.080***	0.005	0.016	0.015	-0.042**	-0.101***	-0.096***
	(0.026)	(0.028)	(0.028)	(0.010)	(0.012)	(0.011)	(0.020)	(0.024)	(0.023)
Spillovers									
0-1km	0.039***	0.025***	0.023***	0.004	0.007	0.007*	0.026**	0.008**	0.007
	(0.010)	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.012)	(0.004)	(0.004)
0-2km	-0.003	-0.010***	-0.008*	-0.002	0.000	0.001	-0.001	-0.012	-0.011
	(0.003)	(0.004)	(0.005)	(0.004)	(0.004)	(0.004)	(0.006)	(0.007)	(0.008)
0-3km	0.002	0.001	0.002	0.001	0.001	0.000	0.003	0.001	0.002
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.005)	(0.005)	(0.005)
0-4km	0.002	0.004**	0.004**	-0.003***	-0.003***	-0.002**	0.010***	0.013***	0.012***
	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)
0-5km	-0.006***	-0.004***	-0.004***	-0.000	-0.000	-0.001**	-0.009***	-0.007***	-0.006***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)
0-50km	-0.000	-0.002	-0.002	0.000	0.001	0.001	-0.000	-0.002**	-0.002**
	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Area controls		√	√		√	√		√	√
Pre-trends			√			√			√
Observations	43,786	43,786	43,786	43,786	43,786	43,786	43,786	43,786	43,786
# of clusters	51	51	51	51	51	51	51	51	51

Note: Robust standard errors are reported in parentheses; *, **, *** indicate significance at the 10%, 5% and 1% levels, respectively. In Columns (1)-(3), the dependent variable is defined as (2003-2007) changes in total private sector employment across all sectors of the economy except agriculture and forestry (SIC11-SIC93). In Columns (4)-(6), the dependent variable is defined as (2003-2007) changes in manufacturing employment (SIC15-SIC37). In Columns (7)-(9), the dependent variable is defined as (2003-2007) changes in service employment (SIC45-SIC93). Columns (1), (4) and (7) do not include 2001 area characteristics; Columns (2), (5) and (8) include area controls; and Columns (3), (6) and (9) include both area controls and pre-trends. Pre-trends are defined as (1998-2002) changes in total private sector employment (Column 3), manufacturing (Column 6) and services (Column 9). All regressions include 51 TTWA fixed effects and standard errors are clustered at the TTWA level. The sample includes Output Areas that lost government jobs located in the South East of England and Output Areas within 50km of the areas that lost these jobs (London is excluded from the analysis).
Sources: OGC Government relocation data, 2003-2007; BSD local unit data (private sector employers only), 1997-2011; UK Census of Population, 2001.

Appendix: Data construction

The Business Structure Database (BSD)

The main analysis uses (2003-2007) changes in private sector employment (and its various sub-groups) at the 2001 Census Output Area level as the main outcome variable. This measure is constructed aggregating micro-level data from the Business Structure Database (BSD) covering the period 1998-2007. The BSD is an annual snapshot (taken at the closing of the fiscal year) of the Inter-Departmental Business Register (IDBR), which consists of constantly-updated administrative data collected for taxation purposes. Any business liable for value-added taxation (VAT) and/or with at least one employee registered for tax collection appears 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 data are structured into enterprises and local units. An enterprise is the overall business organization. The local unit can be thought of as a plant or establishment. In the majority of cases (70 per cent), enterprises only have one local unit, while the remaining 30 per cent of the cases represent enterprises with multiple local units. In this work, I make use of data at the local unit level including plants belonging both to single- and multi-plant enterprises and located in England, Wales and Scotland. I neglect Northern Ireland because of poor data coverage.

The initial raw data include approximately three million local units every year. However, before using the data for the analysis, I carry out a series of checks and drop a number of units. First, I investigate the consistency of opening and closing dates of BSD units with their actual existence in the dataset and drop a number of anomalous 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, I only count firms' birth and death once. Second, I check the consistency of units' postcodes and sectors of activity over the years, and drop cases with missing or anomalous information.[45] For example, when I observe two or more plants operating in the same 3-digit industry, sharing the same postcode and being part of the same enterprise, I believe this being a reporting error and drop them. Similarly, I observe a non-trivial number of same-postcode same-three-digit industry combinations representing anomalous concentration of identical activities at a single address. I believe this is another coding error and drop the plants that belong to the top 5% of the distribution of local units sharing same three-digit industry and the

[45] A UK postcode usually corresponds to a very limited number of addresses or a single large delivery point. While it might not always be a geographically accurate description of where a property is located, it is generally a good approximation. 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 shows that UK postcodes are geographically accurate up to the level of a front door in a particular street.

same postcode. Third, I drop active units with zero employment since this figure includes the owners/managers of the establishment, so it cannot be zero for an active unit, as well as units with an unusually large size (i.e., total employment above the 99th percentile of the distribution for each three-digit industry sector). After applying these restrictions, the final dataset still comprises of more than two million plants annually over 10 years (1998-2007).

Table A.1: Government relocations by UK Department and Non-Departmental Public Body

Department Name	Relocation size		Frequency of relocations
	Mean	SD	
Department for Business, Innovation and Skills	13.2	20.2	58
Competition Commission	25	0	1
Commission for the Compact*	11.2	4.4	6
Crown Prosecution Service	7.2	8.2	6
Department for Communities and Local Government	5.2	10.7	76
Department for Culture, Media and Sport	22.5	29.9	42
Department for Children, Schools and Families	26.3	37.7	58
Department for Environment, Food and Rural Affairs	16.1	21.4	49
Department for International Development	2.3	2.5	39
Department for Work and Pensions	33.7	75	125
Department for Transport	10.3	8.3	6
Department of Health	23	32.1	53
Foreign and Commonwealth Office	24.1	14.6	19
HM Revenue and Customs	5.7	16	635
HM Treasury	3.7	4	10
Home Office	54.9	89.9	56
Ministry of Defence	207.6	282.3	29
Ministry of Justice	16.7	44.8	58
Office of Fair Trading	3	0	1
Office for National Statistics	4.3	5.5	139

Note: *: The Commission for the Compact was an independent body responsible for overseeing the Compact and its five Codes of Good Practice. The Compact aims to promote good partnership working between the public and the voluntary sector by outlining how the two sectors should behave towards each other. The Commission for the Compact has been dismantled in March 2011. Responsibility for the Compact are now shared between the Office for Civil Society and Compact Voice.
Source: OGC Government relocation data, 2003-2010.

Table A.2: List of OA characteristics retrieved from the UK 2001 Census of Population

Variable Name	Variable Definition	Mean	S.D.
Demographic variables			
pop_density	Population density (in square kilometers)	47.8	47.1
pop_less25	Proportion of population aged 24 or younger	0.299	0.091
pop_25_44	Proportion of population aged 25 to 44	0.283	0.083
pop_45_64	Proportion of population aged 45 to 64	0.246	0.074
pop_65_74	Proportion of population aged 65 to 74	0.091	0.050
pop_over75	Proportion of population aged 75+	0.081	0.068
noqual_wap	Proportion of WAP without qualifications	0.328	0.144
qlev1_wap	Proportion of WAP with qualification level 1	0.187	0.066
qlev2_wap	Proportion of WAP with qualification level 2	0.183	0.057
qlev3_wap	Proportion of WAP with qualification level 3	0.072	0.048
qlev4o5_wap	Proportion of WAP with qualification level 4 o 5	0.174	0.120
qoth_wap	Proportion of WAP with other qualifications	0.056	0.038
pop_female	Proportion of female population	0.517	0.039
Labor Market Variables			
emp_xstud	Employment-to-population ratio - excluding students	0.626	0.135
inact_xstud	Inactivity-to-population ratio - excluding students	0.334	0.120
un_xstud	Unemployment-to-population ratio - excluding students	0.040	0.033
sh_stud	Share of students in the WAP(16-74)	0.064	0.067
sh_ret	Share of retired individuals in the WAP(16-74)	0.149	0.080
work_se_rate	Proportion of self-employed workers	0.122	0.081
work_pt_rate	Proportion of part-time employees	0.237	0.070
home_emp	Proportion of employees working from home	0.082	0.059
Socio-economic Variables			
nowhite_sh	Proportion of all non-whites over population	0.045	0.104
lone_hous	Proportion of lone parent households over total households	0.067	0.061
migr_pop	Proportion of non-UK born over total population	0.050	0.061
hous_overcr	Proportion of overcrowded households	0.068	0.077
hous_avsize	Average household size	2.3	0.4
hous_nbrooms	Average number of rooms per household	5.3	1.0
hous_noheat	Proportion of households without heating	0.088	0.108
hous_wocars	Proportion of households without a car	0.284	0.201
hous_own	Proportion of households living in owned houses	0.683	0.260
hous_privrent	Proportion of private renters on total renters	0.483	0.363
hous_council	Proportion of social renters on total renters	0.481	0.363
pop_estab	Share of total population living in communal establishments	0.012	0.053
Commuting Variables			
travel_undergr	Proportion of employees traveling to work by tube	0.005	0.024
travel_train	Proportion of employees traveling to work by train	0.024	0.038
travel_bus	Proportion of employees traveling to work by bus	0.110	0.102
travel_motocy	Proportion of employees traveling to work by motorcycle	0.010	0.015
travel_car	Proportion of employees traveling to work by car	0.604	0.161
travel_carpas	Proportion of employees traveling to work by common car	0.083	0.043
travel_taxi	Proportion of employees traveling to work by taxi	0.007	0.015
travel_bike	Proportion of employees traveling to work by bike	0.026	0.035
travel_foot	Proportion of employees walking	0.125	0.096
travel_oth	Proportion of employees traveling to work by other means	0.006	0.013
av_commdist	Average distance (km) travelled to fixed place of work	13.0	6.3
pub_trans	People in employment who use public transport	0.129	0.106

Note: WAP stands for working age population. Total number of observations: 151,912 OAs.
Source: UK Census of Population, 2001.

Table A.3: List of OA characteristics retrieved from the UK 1991 Census of Population

Variable Name	Variable Definition	Mean	S.D.
Demographic Variables			
pop_density	Population density (in square kilometers)	46.5	46.6
pop_less25	Proportion of population aged 24 or younger	0.321	0.075
pop_25_44	Proportion of population aged 25 to 44	0.287	0.070
pop_45_64	Proportion of population aged 45 to 64	0.224	0.061
pop_65_74	Proportion of population aged 65 to 75	0.095	0.047
pop_over75	Proportion of population aged over 75	0.073	0.053
qoth_wap	Proportion of WAP without higher degree, degree or diploma	0.871	0.126
qleva_wap	Proportion of WAP with higher degree	0.008	0.023
qlevb_wap	Proportion of WAP with degree	0.055	0.075
qlevc_wap	Proportion of WAP with diploma	0.064	0.066
pop_female	Proportion of females over tot pop	0.517	0.033
Labor Market Variables			
emp_xstud	emp_to_pop ratio - excluding students	0.569	0.128
inact_xstud	inact_to_pop ratio - excluding students	0.375	0.110
un_xstud	un_to_pop ratio - excluding students	0.060	0.044
sh_stud	share of students	0.041	0.029
sh_ret	share of retired individuals	0.195	0.090
work_se_rate	Proportion of self-employed workers	0.118	0.077
work_pt_rate	Proportion of part-time employee workers	0.212	0.056
home_emp	Proportion of employees working from home	0.050	0.091
Socio-economic Variables			
nowhite_sh	Proportion of all non-whites over population	0.031	0.084
hous_lone	Proportion of lone parent households over total households	0.037	0.041
migr_pop	Proportion of non-UK born over total population	0.042	0.052
hous_own	Proportion of households living in owned houses	0.658	0.270
hous_council	Proportion of social renters on total renters	0.541	0.370
hous_privrent	Proportion of private renters on total renters	0.436	0.365
hous_overcr	Proportion of overcrowded households	0.022	0.029
hous_avsize	Average household size	1.7	0.7
hous_nbrooms	Average number of rooms per household	3.4	1.5
hous_noheat	Proportion of households without heating	0.200	0.187
hous_wocars	Proportion of households without a car	0.342	0.210
pop_in_est	Share of total population living in communal establishments	0.017	0.089
Commuting Variables			
travel_train	Proportion of employees traveling to work by train	0.024	0.064
travel_bus	Proportion of employees traveling to work by bus	0.132	0.163
travel_motocy	Proportion of employees traveling to work by motorcycle	0.014	0.032
travel_car	Proportion of employees traveling to work by car	0.555	0.219
travel_carpas	Proportion of employees traveling to work by common car	0.091	0.094
travel_bike	Proportion of employees traveling to work by bike	0.029	0.055
travel_foot	Proportion of employees traveling to work on foot	0.137	0.144
travel_oth	Proportion of employees traveling to work by other	0.005	0.028

Note: WAP stands for working age population. Total number of observations: 151,912 OAs.
Source: UK Census of Population, 1991.