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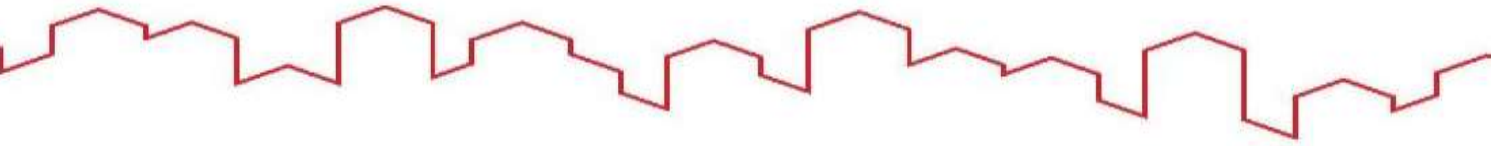

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Relocation of Public Sector Workers: Evaluating a place-based policy

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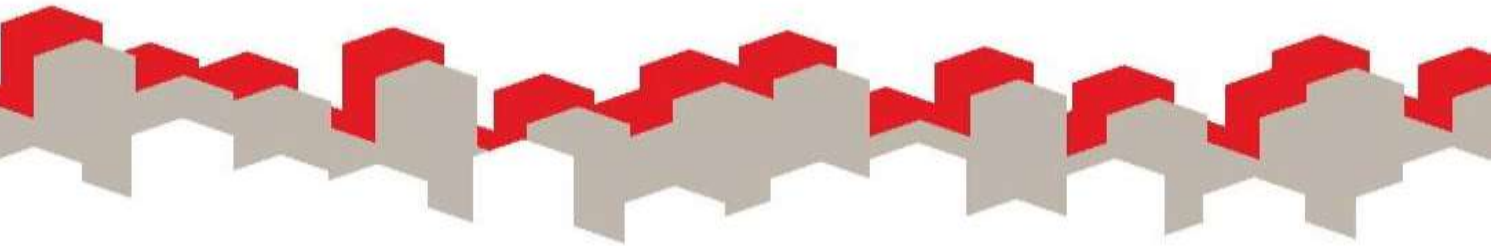

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Relocation of Public Sector Workers: Evaluating a place-based policy*

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

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. The paper aims to detect whether inflows of public sector jobs have 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 programme raised private sector employment in the receiving areas and changed the sectoral distribution of local employment towards services and away from manufacturing.

JEL classification: O1, R23, R58, J61

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

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1. Introduction

Governments design a variety of place-based policies attempting to reverse the fate of economically declining areas and create employment opportunities for local residents. In the US, Enterprise and Empowerment Zone programmes spur the creation of jobs by providing tax incentives to businesses located in designated areas.[1] Similarly, French Enterprise Zone programmes 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 or SRB programme), or spatially-bound policies whose funding goes indirectly to businesses through local government (the Local Enterprise Growth Initiative or LEGI).[3] The UK government also uses relocation programmes of public sector workers to address regional employment problems and to reduce spatial disparities in income. Strictly speaking, relocation programmes of public sector workers are not 'pure' place-based policies. They address a variety of objectives, including delivering cost savings, re-organising the government estate, and enhancing devolution.

When a public sector job is created in an area, it may have a local 'multiplier effect': It may create additional local jobs as a result of the increased demand for locally-produced goods and services. Conversely, a rise in public sector employment may trigger general equilibrium effects in the form of higher 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 first government-sponsored review was commissioned in the 1960s (Flemming Review, 1963), followed by the Hardman Review (1973) and by the Lawson-Thatcher Review (1988). Notwithstanding the attention given by the government to the subject, there is scarce evidence of the effects of a public sector relocation programme upon local labour markets. This study tries to fill this gap by assessing the local labour market impact of a public sector relocation initiative labelled the Lyons Review.

In 2004, Sir Michael Lyons led a UK government-sponsored independent study on the scope for public sector relocations out of London and the South East towards other UK destinations. The 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 programme had relocated more than 25,000 jobs. The 2004 relocation programme addressed a variety of objectives including the government desire to stimulate economic activity in less-prosperous areas and, thus, reducing spatial imbalances between

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

London and periphery areas. To the extent that the relocation programme had any impact on local economic conditions, this paper aims to detect the causal effects of the intervention.

In order to detect any causal impact, I use panel data at a detailed geographical scale (2001 Census Output Areas) and covering years before and after the implementation of the programme. My analysis is complicated by two factors: (1) the geographical spread of the policy is unknown *a priori*; (2) destination areas are not randomly chosen. To solve these issues, I firstly 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. Secondly, I compare neighbouring 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-difference approach with heterogeneous (rather than constant) treatment effects (see Angrist and Imbens, 1995). Similarly to a difference-in-difference approach, I exploit the temporal and spatial variation in the data. Differently from a difference-in-difference 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 eighth-year period (2003-2010). Given the complexities surrounding the 2008 recession, this study provides short-run estimates of the policy effects. It finds that the dispersal of public sector jobs that followed the implementation of the Lyons Review (2004) has 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 5.5 jobs in the private sector. It also affects the sector composition of local jobs: it stimulates services, whereas it has a negative but weaker impact on manufacturing. The study also finds evidence of displacement – particularly for local services. There is, in fact, a tendency for private businesses to locate closer to a relocation site, moving out of areas at 1-2km distance and into areas at 0-1km distance. In addition, the study finds that effects are highly localised: 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 0-2km 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 programme on changes in outcomes during a pre-treatment period (2000-2001); and replicating the analysis using a courser geographical scale (2001 Lower Layer Super Output Area). Furthermore, the analysis is extended to investigate whether larger size relocations are

associated with a larger policy impact and to explore which demand channel (either consumer demand or intermediate demand) is more likely to explain the positive impact on services.

This paper contributes to the growing literature on the evaluation of place-based government policies. As also noted by Einio and Overman (2012), earlier studies were impaired by the problem of non-random placement.[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 programmes have been successfully evaluated by Neumark and Kolko (2010); Busso and Kline (2008); and Busso *et al.* (2013). Other less well-known programmes, 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 programmes, the UK LEGI and the UK Single Regeneration Budget stand out for accuracy.[5] To help identification, this study combines a treatment intensity approach with 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 programme 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 (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 case of 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 distance. The present study also finds that effects are concentrated within the first few kilometres from a relocation site.

Faggio and Overman (2014) is the paper most closely related to the present one. Using geographical information on 352 English Local Authorities (LAs), they look at the impact of changes in public sector employment on the local labour market. They find that public sector employment has no

[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.* (2012); Einio and Overman (2012); 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.

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, while it has a negative effect on manufacturing jobs. 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. Firstly, 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] Conversely, this study provides the first thorough evaluation of a relocation programme and focuses on the dispersal of 25,000 central government jobs. Secondly, as already noted, they conduct the analysis at a much higher level of aggregation (using 352 English LAs) than the one used here (based on about 167,000 Output Areas covering England, Wales and Scotland). Thirdly, they apply a Bartik-IV estimation approach, whereas this study uses treatment intensity variables and exploits data variation across space and over time. As for the results, Faggio and Overman (2014) 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 finds that the dispersal of civil service jobs has an overall positive impact on total private sector employment. It also provides evidence that the positive impact on services more than compensates the negative impact on manufacturing.

It is worth noting that the debate on public sector relocation is not limited to the UK.[8] Little attention, however, has been paid in previous (largely European) research to estimate the local labour market impact of a relocation programme. An exception is the paper by Becker *et al.* (2012), which looks at the rise of Bonn as the new federal capital of Western Germany at the end of World War II. Becker *et al*. (2012) investigate how this historic government relocation from Berlin to Bonn changed the local economic structure of the new capital relative to other cities of similar initial size and demographic characteristics. They find limited effects, given the extent and relevance of the relocation.

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

[7] Using ONS Public Sector Employment Statistics, 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.

[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. As of April 2015, the Danish parliament is debating the relocation of 4,000 jobs out of Copenhagen to less prosperous areas (see Christiansen, F., 2015, http://www.pressreader.com/denmark/politiken/20151011/281522224919239/TextView, *Politiken*, October 11).

empirical strategy. While Section 5 describes the data used, Section 6 presents the results. Section 7 concludes.

2. The institutional setting

In 2004, Sir Michael Lyons led a government-sponsored review on the scope for relocating central government activities out of London and the South East to more peripheral regions. The review proposed the dispersal of about 20,000 civil service jobs within the six-year period ending in March 2010. The programme developed very strong 'push' factors, 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 submit a formal business case for approval. This requirement changed expectations across government: departments needed to justify their presence in London on the grounds of business needs. Thanks to these push factors, the original target was delivered nearly a year ahead of schedule. By its end, the programme relocated more than 25,000 jobs.

The Lyons Review had several main objectives: delivering cost savings to taxpayers by reducing accommodation and labour 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-organising public services and adopting a performance-driven culture across government departments. It is not accidental that the Lyons Review recommendations were implemented as a strand of the Gershon Efficiency Review (2004) whose primary objective was civil service modernisation.

The primary government benefits of devolution, namely reducing cost pressures and relieving spatial constraints, moves hand in hand with the public benefits of important central government organs being close to the people; increased confidence and transparency in government decisions; and an increased sense of belonging. An additional purpose of public sector relocation is to boost regional growth in UK peripheral areas in an attempt to correct the spatial imbalance between a rich South-East and less

[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 that they incur in relation to public sector employees elsewhere in the country.

prosperous regions in the North and the West. This study is about evaluating the programme in light of its ability to achieve this last objective.

Larkin (2010) notices that the size of the government's relocation programme was fairly small. Over the period 2003-2010, the programme 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 programme relocated about 20 per cent of all government jobs initially housed in London or around 17 per cent of those in London and the South East.

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 local labour markets defined such that at least 75% of the resident population works in the area and 75% of the people working in the area reside there. These areas are obviously much larger than Output Areas (OAs), containing an 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 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 as recorded in the Census) 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 rationalise the civil service estate and

[10] I could collect information on the original locations of 20,550 jobs (out of 25,420). While 82 percent of these moves were out of London, 18 per cent were out of the South East region.
[11] Out of London, the smallest TTWA contains 34 OAs whereas the largest has 5,500 OAs. Output Areas are very small geographical areas built from clusters of 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.

oversee departmental relocations, each individual department was accountable for managing its own relocation programme, including filling posts that were transferred, or created, in the new location.

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 their job and there are no details of relocation packages offered or compensations taken. This lack of planning and transparency resulted in higher-than-expected upfront 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 geographical area characterised 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 j. Each city consists of N_j output areas, where N_j is large. Output areas are characterised by Labour (L) and Terrain (T). Labour 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 centre. The assumption of perfect worker mobility across cities is, in this context, partly justifiable. Worker relocation programmes 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 programme 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 characterised by an endowment of terrain, T. OAs are small geographical 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: recent 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 NDPBs, i.e. organisations characterised by a limited contact with the general public. These are precisely the types of public sector jobs involved in the relocation programme (see Section 5). Given the nature of these jobs and the fact that they were actually moved, these jobs must offer a tradable service. I abstract from considering the presence of a public sector providing a non-tradable service. Non-tradable service jobs are those in public schools, NHS trusts, police forces, fire stations, post offices, etc. They typically involve a direct involvement with the general public. Neither were they the focus of the Lyons Review, nor are they the focus of this analysis.

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'.

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 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., bread, 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 this case, the effect on *T* is likely to dominate any positive impact on demand. Because of higher housing rents, manufacturing employers may decide to relocate themselves moving out of costly locales and into less costly areas. In other words, for the tradable private sector, the crowding-out effect tends to dominate any local multiplier effect.

To summarise, the inflow of additional public sector workers into a small geographical area is likely to raise housing costs, creating incentives for local employers to move out of the area (i.e. a crowding-out effect). In addition, it might stimulate the demand for tradable goods and for locally-produced services (i.e. local multiplier effects). For the manufacturing sector, the crowding-out effect is likely to dominate (being the local demand a very small fraction of total demand). For the service sector, it is hard to know *a priori* which effect will prevail. Local service firms face a trade-off between incurring higher rental costs and satisfying a larger customer base.

4. The Empirical Strategy

There are methodological problems associated with ex-post evaluations and the two concepts of additionality and deadweight may be the most challenging of them. Additionality refers to the 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 additionality problem is by comparing 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 programme which is taken up by recipients other than those to whom the expenditure should, if possible, be directed"

(Mceldowney, 1997, p177). 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 additionality, 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 neighbourhood composition and the extent of any policy impact.

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, 2012) 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 programmes like the Lyons Review are policies with a potential nationwide impact and of which the geographical 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 neighbour) is explicitly about evaluating local 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.

One way of solving these issues is to apply 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). In other words, it replaces (or combines) a treatment dummy, typical of a standard difference-in-difference approach, with treatment intensity variables. In constructing these variables, I do not confine effects to be within certain geographical boundaries. I assume, instead, that all

areas can be treated, although areas close to a relocation site are 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:

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

where $\Delta \boldsymbol{y_i}$ is the raw change in the outcome measure of interest over the period 2003-2008 in a Output Area *i*. $\Delta \boldsymbol{N_i^0}$ refers to the total number of jobs moved (i.e. the size of the relocation) an OA *i* received between 2003 and 2007. Since the 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.[13] $\Delta \boldsymbol{N_i^c}$ refers to the total number of jobs moved an OA *i* faces within distance band *c*, with $\boldsymbol{c \geq 1}$. All buffers 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.[14] $\boldsymbol{\varepsilon_i}$ is an error term. All specifications also include Travel-To-Work-Area fixed effects and standard errors are clustered at the TTWA level. In an alternative specification, I assume equal size across all relocations so that the binary indicators $\boldsymbol{I_i^c}$ are also used as treatment intensity variables. $\boldsymbol{I_i^c}$ is a binary indicator for OA *i* and distance band *c*, equal to 1 if the OA faces at least one relocation site within distance *c* and equal to 0 otherwise. Similarly, $\boldsymbol{I_i^0}$ refers to a binary indicator for OA *i* equal to 1 if the OA was chosen as a destination.

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-difference 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 neighbouring 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 geographical 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

[13] While local employment is computed as a change between April 2003 and April 2008, the total number of jobs moved is aggregated up over the period June 2003-December 2007. In doing so, I leave a three-month period between counting the jobs moved and analyzing the impact.
[14] For a full list of control variables, see Tables A.1 and A.2.

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 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.

It is worth noting that the treatment intensity variables included in the estimation are constructed in a cumulative way: $\Delta \boldsymbol{N_i^1}$ refers to all relocations (and the associated number of jobs moved) that an OA *i* faces within a 0-1km distance band, but outside the OA boundaries.[15] The comparison group consists of OAs that do not face relocations within the 0-1km band, but face relocations at a greater distance. $\Delta \boldsymbol{N_i^2}$ refers to all relocations that an OA *i* faces within a 0-2km distance band (which also includes relocations considered in the 0-1km ring) relative to areas that face relocations beyond the 0-2km ring. $\Delta \boldsymbol{N_i^3}$ refers to all relocations an OA *i* faces within 0-3km band (which also includes relocations considered in the 0-1km and 1-2km rings) relative to areas that face relocations further away, and so forth. 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). Remember that buffers or distance bands are constructed around OA centroids and not around relocation sites. If I consider only three 1km-wide buffers, the three treatment intensity vectors in this graphical representation (with the first element referring to area A and the second element referring to area B) are: $\Delta \boldsymbol{N^1}$= (1,0); $\Delta \boldsymbol{N^2}$= (2,1); $\Delta \boldsymbol{N^3}$= (2,2).

Defining treatment intensity variables in a cumulative way allows me to identify the effects of relocations within a given distance band (and related $\boldsymbol{\gamma^c}$ parameters) separately from one another. Consider the first two treatment intensity variables, $\Delta \boldsymbol{N_i^1}$ and $\Delta \boldsymbol{N_i^2}$, included in equation (1). $\boldsymbol{\gamma^1}$ measures the policy impact for OAs that face relocations within a 0-1km distance band. Even though $\Delta \boldsymbol{N_i^2}$ includes relocations at both 0-1km and 1-2km distance bands, $\boldsymbol{\gamma^2}$ picks up the policy impact for OAs facing relocations at 1-2km distance band only, since the 0-1km impact is already captured by $\boldsymbol{\gamma^1}$. The same line of reasoning applies to all subsequent treatment intensity variables.

In principle, equation (1) could include all treatment intensities from 0-1km distance to the maximum distance and each variable would pick up the impact of relocations on outcomes within a given distance band. In practice, this over-parameterisation is not necessary. On one hand, it would not be needed if effects were highly localised. On the other hand, equation (1) could simply include the first 5km or 10km treatment intensities and then a cumulative treatment intensity variable picking up the policy impact from 5km (or 10km) onwards. For tractability, I will consider 50km as the maximum distance in the empirical

[15] In other words, $\Delta \boldsymbol{N_i^0}$ is not included in $\Delta \boldsymbol{N_i^1}$(and in any subsequent $\Delta \boldsymbol{N_i^c}$ with $c \geq 2$).

analysis and include the first 5km treatment intensity variables as well as a cumulative 5-50km impact variable in the main specification.[16]

5. Data Construction

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

The Government relocation data are comprehensive: They list the total number of actual job moves within government departments following the implementation of the Lyons Review (2004). They provide information on 25,408 public sector jobs relocated out of London and the South East into other UK destinations between June 2003 and December 2010. The data give details on the date of the move; the government department and business unit involved; the origin or exporting address of the building from which a job was relocated; and the destination or importing address of the building receiving the job. Not all public sector workers were involved, but only those civil servants working for central government (including government departments, non-ministerial departments and executive agencies) or for special entities called executive Non-Departmental Public Bodies (NDPBs). UK NDPBs are, for instance, the Care Quality Commission or the Competition Commission.[18]

In a substantial number of cases, the geographical information on origin and destination addresses was missing or misreported. I checked every address in the dataset and filled out the postcodes when missing, using old government archives, internet search engines and government agency websites. Since staff moves were phased in manageable chunks, I could identify 1,486 distinct relocations defined by a moving date, the number of jobs moved and a destination address. The majority of these destinations 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 exclude Northern Ireland from the final sample, which consists of 1,467 relocations, involving 24,950 job moves within 20 government departments. The bulk of these changes (about 64% of

[16] I have also experimented with alternative ways of aggregating treatment intensities over the 5-50km distance (5-10km, 10-30km and 30-50km). Since effects are highly localized, it makes no difference.

[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.

relocations and about 65% of job moves) occurred between June 2003 and December 2007 (see Figures A.3 and A.4).[19]

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 1,522 moves, 407 origin addresses report no geographical detail. Conversely, only 36 destination postcodes were not identified. Lastly, the data provides information on the number of jobs (not workers) moved. I do not know whether a worker who filled the job in London (or in the South East) actually moved with the relocated job.[20] What I know is that civil servant numbers fell by about 16,500 FTE[21] 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' date of birth, date of death, postcode, sector of activity (up to 5-digit SIC 2003 code) and total employment.[22] 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 type.[23] 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).

From the UK 2001 Census, I select a rich set of Output Area variables measuring local labour market characteristics; demographics and population density; household size and types of dwelling; means of transport and average commuting distance.[24] Figures are available for a range of geographical 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

[19] The first job moves were recorded in June 2003, thus preceding the publication of the Lyons Review (2004).
[20] Civil servants were firstly asked to move with the job. If they did not agree, they could either accept a redundancy package or apply internally for openings in other departments. Anecdotal evidence suggests that a relatively small number of employees actually moved with the job, them being usually young and of junior rank.
[21] Staff numbers on a full-time equivalent basis.
[22] The initial raw data includes approximately 3 million local units every year. However, I carry out a series of checks and drop a number of units as detailed in Appendix A: Data Construction.
[23] 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 Authority.
[24] See Tables A.1 and A.2 for details.

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.[25]

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[26] and be as socially homogenous as possible (based on tenure of household and dwelling type).[27] 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. Figure 2 (Panel 1) compares the evolution of central government employment between receiving and non-receiving OA over 1998-2011. During the first five years of the programme (2003-2007), central government employment increased substantially in receiving OAs, rising from an average number of 124 workers in 2002 to about 470 in 2007. During the same period, central government employment in non-receiving OAs rose from 84 to 96 workers. After 2007, although the relocation programme was still active, employment numbers in receiving OAs remained roughly constant at 400-450. Non-receiving OAs registered another small increase (from 96 to 114) between 2007 and 2011. It is unclear why the number of civil service workers did not continue to rise in receiving areas after 2007. The OGC relocation data show that job moves were recorded during 2008-2010. Alternative explanations are possible: inconsistencies between BSD and OGC data; job moves were scheduled but did not take place; job moves occurred but were offset by the impact of other policies. The 2008 recession and the financial crisis that preceded it might have played out unevenly across space.[28] In addition, government austerity measures might have been more detrimental to areas that have previously benefited from public investment. With the data currently available, I cannot distinguish between alternative hypotheses. Nonetheless, Figure 2 (Panel 1) validates the choice of focusing on a shorter time period.

The main analysis looks at the impact of government relocations after the first five years of the programme (2003-2007) and focuses on 2003-2008 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

[25] 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.

[26] 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 have between 2,500 and 8,000 resident persons.

[27] 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 resident households and 50 resident people, with a target size of 50 households) than those in England and Wales. In addition, social homogeneity was not used as a factor in designing Scottish OA boundaries.

[28] The 2008 recession was triggered by the global financial crisis of 2007-2008. During those years, the UK government was an active player in supporting the banking sector. It led the nationalization of Northern Rock in February 2008; the part-nationalization of Bradford and Bingley in September 2008; the recapitalization of the Royal Bank of Scotland, Lloyds TSB and Halifax Bank of Scotland in October 2008.

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) and business activities (SIC65-SIC74); and trade, catering and 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, empirically confirming what observed in Figure 2 (Panel 1). During 2003-2008, central government employment indeed expanded in OAs receiving the relocated jobs relative to non-receiving areas. Looking at Table 1 (Columns 1-3), there is about a 1:1 correspondence between central government employment and the size of the relocation. The arrival of one civil servant job in an area resulted in one additional employee working locally for the central government. The coefficient of 1.0-1.1 is highly statistically significant and robust to the inclusion of initial area characteristics and pre-trend changes in central government employment. Also as expected, there are no spillover effects: none of the coefficients on the treatment intensity variables included in Table 1 is statistically significant. Furthermore, findings are robust to the adoption of an alternative specification that expresses treatment variables as binary indicators. The binary indicator for receiving areas (at 0km) is reported in the top panel of Table 1, Columns 4-6. A coefficient of 87-107 indicates that central government employment on average expanded by between 87 and 107 workers during the period 2003-2008 in receiving versus non-receiving areas. Again, there are no spillover effects.

As a second 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 programme (up to 2002), receiving and non-receiving OAs show similar trends in the evolution of these variables (see Figure 2, Panels 2-4). For instance, the average number of workers in private services changed little during 1998-2002 in both receiving and non-receiving OAs: it decreased by 1.34% in the former; it increased by 0.67% in the latter. As for manufacturing, average employment decreased for both groups between 1998 and 2002: it dropped by 14.5% and 16.3% in receiving and non-receiving areas, respectively.

Figure 2 also shows the evolution of the outcome variables during the policy implementation. Looking at the first five years (2003-2007) into the relocation programme, both total private sector employment and service employment rose more substantially in receiving than non-receiving areas. Between 2003 and 2007, total private sector employment rose by 20.7% and 9.6% in receiving and non-receiving OAs, respectively. The corresponding figures for service employment are 29.2% and 17.1%. As

for manufacturing, average employment decreased by 2.5% in receiving OAs while it increased by about the same amount (2.1%) in non-receiving areas.

The picture drastically changed in 2008. Between 2007 and 2008, receiving OAs experienced a fall in average employment across all sectors of the economy, with the largest drop recorded in manufacturing (-26.7%). A possible explanation is as follows: as the financial crisis deepens and the recession kicks in, cost-cutting strategies lead employers to move out of costly areas into less expensive areas. The reality of a recession approaching tilts the balance towards crowding-out effects and away from local multiplier effects. Manufacturing employers, who are particularly sensitive to crowding-out effects, are affected the most. It is worth noting that crowding-out effects appear to dominate six years into the programme and only when general economic conditions deteriorate.

Looking at service employment, the 2008 drop in employment is relatively smaller (-10%) than manufacturing. As the financial crisis deepens, it is possible that cost-cutting strategies become attractive to local service firms too. It is also possible that during the initial phase of a relocation programme, the government outsources some activities to the private sector, thus generating an extra-boost for the local economy. As the programme enters a more mature phase and services are again insourced, this boost is likely to fade. Given the difficulty in distinguishing between alternative hypotheses and the complexities surrounding the recession, the remainder of the paper focuses on 2003-2008 changes, thus providing a short-term analysis of the relocation programme.

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.[29] It seems plausible to expect that not only receiving a relocation, but also the number of jobs moved 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 organised 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 programme. I then include the 1998-2002 changes in total private sector employment as an additional

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

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 impact on total private sector employment in areas that received the relocated jobs (see Table 2, Column 1, top panel). A coefficient of 0.599 (s.e. 0.253) implies that the arrival of 10 public sector jobs in an area triggered the creation of 6 additional jobs in the private sector. Estimates are slightly lower (coeff. 0.548; s.e. 0.257) when both area characteristics and pre-trends are included in the estimation. Looking at spillovers (see Table 2, Column 3, bottom panel), there is evidence of displacement: the policy had a positive impact on private sector employment in areas within a 0-1km distance to a relocation site, whereas it had a negative impact on areas at 1-2km distance.

Treatment intensity parameters in Table 2 capture the average impact on an OA within a given distance band. Nonetheless, the number of OAs contained in each band increases with distance. For example, the first and the second rings consist of about 39 and 85 OAs, respectively. By multiplying the coefficients reported in Table 2, Columns (3) with their corresponding counts of OAs, I obtain average total effects of 2.379 and -1.870 for the 0-1km and 1-2km ring, respectively (see Table 3, Column 2). These coefficients suggest that the arrival of 10 civil service jobs in an area spurs the creation of about 24 additional jobs in the private sector within the first distance band and the destruction of about 19 jobs within the second band. These results also 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 band. Furthermore, I can compute the average effect for any OA included within the 0-2km circle as a weighted average of direct and indirect effects.[30] By using the coefficients reported in Table 2, Column (3) (0km, 0-1km and 0-2km rows), I obtain an average effect of 0.008. That is, the arrival of 100 civil servant jobs in an area (central to the 0-2km circle) spurs the creation of 0.8 new jobs in the private sector in any of the 125 OAs within the 0-2km space.

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 a negative but limited impact on manufacturing employment. The estimate in Column (4) is -0.169 (s.e. 0.092) suggesting that for 10 new central government jobs created in an area, manufacturing employment on average falls by 1.7. When including both area characteristics and pre-trends (see Column 6, top panel), estimates become smaller and

[30]Defining γ^0, γ^1, γ^2 as the parameters for the treatment variable and the first two treatment intensity variables (see equation 1); and N^1, N^2 as the numbers of OAs in the 0-1km and 1-2km rings, respectively, the average effect for any OA within the 0-2km space can be expressed as $avg = (\gamma^0 + \gamma^1 * N^1 + \gamma^2 * N^2)/1 + N^1 + N^2$. This computation takes the gains obtained locally around the relocation site and spread them evenly across all OAs within the 0-2km space. See Table 3.

turn borderline significant (coeff. -0.141; s.e. 0.087). For manufacturing, there are no significant spillover effects.

Turning to services (see Table 2, Columns 7-9), I detect a positive impact of the relocation programme. 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 7 additional jobs in local services. As area controls and pre-trends are included in the estimation (see Columns 8-9, top panel), coefficients decrease in size, but remain highly significant. There is also evidence of a displacement effect, i.e. a tendency for private businesses to locate closer to a relocation site. As noted earlier, displacement is not complete: by taking into account the average number of OAs in the first two rings, the average total effect within the first band (3.003) is more than double the negative effect within the second (-1.105). Moreover, the average effect for any OA included within the 0-2km space is 0.020 (see Table 3, Column 4). In other words, the arrival of 100 civil servant jobs in an area (central to the 0-2km circle) spurs the creation of 2 new jobs in local services in any of the 125 OAs contained in that circle.

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 a negative but weaker 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 and that the positive impact on services more than offsets the negative impact on manufacturing. Furthermore, Table 3 gives evidence of incomplete displacement, showing that average spillover effects within the first and the second ring do not cancel each other out, with the resulting impact being mostly positive.

Consistent with a growing literature on the spatial range of agglomeration effects (see, e.g., Rosenthal and Strange, 2003; Arzaghi and Henderson, 2008), Table 2 also shows that effects are highly localised: i.e. the largest impact is found in receiving OAs, spillovers effects for an average OA are substantially smaller than the direct effect, and there are no spillovers beyond the 0-2km ring. 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 centres and universities is beneficial to 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 generated 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 equation (1) to agriculture and mining employment (either combined or

separate), I detect essentially no impact of the Lyons Review on primary industries' workers.[31] This is no surprise, given that jobs in those industries are mostly concentrated in rural areas while central government jobs tend to locate 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) trade, catering & personal services; 4) FIRE, consultancy & business 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 4, Columns 3 and 4). I find no effect on transport and a limited impact on construction. Focusing on receiving OAs (see Table 4, top panel), evidence suggests that the arrival of 10 public sector jobs in an area stimulates the creation of about 4 additional jobs in FIRE & business activities (coeff. 0.425; s.e. 0.159) as well as about 2.5 jobs in trade & catering (coeff. 0.265; s.e. 0.111). When focusing on spillover effects per distance band, I find evidence of some displacement for both sectors. Looking at trade & catering (see Table 3, Column 5), the inflow of 10 additional public sector jobs in an area spurs the creation of 19 additional jobs in shops and restaurants within the 0-1km distance band and the destruction of 9.4 similar jobs within the 1-2km band, being 1.989 and -0.935 the average total effects for the first and the second ring, respectively.

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 4 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 5). 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 negative but not statistically significant role. Regarding spillover effects, both finance and business activities matter. Within trade & catering, hotels & restaurants account for a larger part of the overall direct effect as shown in Table 5, Column 5, top panel. Both trade and personal services report a positive but not significant coefficient – although the coefficient on personal services is less than one-fifth of that on trade. Regarding spillovers, the sub-groups of trade and hotels & restaurants

[31] Results available from the author on request.

contribute the most. These findings are consistent with a story that documents larger increases in intermediate (than consumer) demand mostly occurring around the relocated workplace.

6.4. Placebo test or test for pre-trends

A placebo or falsification test is conducted in order to test the validity of the results obtained so far. The test consists in analysing the policy impact on changes in OA employment prior to the Lyons Review. If Tables 2-5 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 2000-2001 employment changes as the dependent variable in a regression similar to that of equation (1). Pre-treatment area characteristics are retrieved from the 1991 UK Census of Population; 1998-1999 pre-trend changes are also included in the estimation. Table 6 distinguishes between manufacturing and service employment (see Columns 1-2) as well as splitting services into four main groups (Columns 3-6). 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 2-5 have shown that these two types of OAs behaved differently thereafter. When considering manufacturing and services, both direct and indirect effects are not significant. When looking at the industry split, almost all coefficients do not satisfy the standard levels of significance with the exception being the estimate for trade & catering. A positive but small estimate of 0.030 (s.e. 0.018) indicates that trade & catering employment grew slightly more (on average 3 additional workers) during 2000-2001 in OAs that would receive 100 relocated jobs at a later period (2003-2007) relative to OAs that would not. This could signal the presence of small anticipation effects in receiving areas for the trade & catering industry.

As a further robustness check, I conduct an IV estimation. There might still be concerns that locations where service industries were expanding attracted government relocated jobs and not vice versa. In regressions similar to those presented in Table 2, I regress (2003-2008) changes in total private sector employment for each OA on simultaneous changes in central government employment in the receiving area (0km) and in each ring c (with c = 0-1km, 0-2km, …, 0-50km) using the corresponding number of jobs moved between 2003-2007 – i.e. the previously constructed treatment intensity variables – as instruments. Although I can identify significant second-stage spillover effects that are comparable with the OLS results, I cannot identify significant direct effects because of weak first-stage estimates (see Table B.1).

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 geographical 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 is unclear, though. The Lyons Review (2004) argued that reaching a critical mass of public sector workers in an area would be crucial for reaping the benefits of 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 in 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 and quartile class and, so, 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 4. As expected, the impact of treatment intensities significantly varies by size (see Table 7). Focusing on OAs that received the relocated jobs (Table 7, 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 for FIRE & business, trade & catering and transport activities with construction showing an opposite effect.[32] Larger relocations are particularly important in explaining the expansion of FIRE & business and transport employment. For these two sectors, only estimates for the top quartile are statistically significant. The picture is different for trade & catering activities. Relocations of all sizes seem important in explaining this sector's expansion, although larger size relocations report larger coefficients. Conversely, bottom quartile relocations seem to matter most for construction.

Looking at the first set of spillovers (Table 7, 0-1km panel), there is again a difference between FIRE & business and trade & catering activities. Regarding FIRE & business employment, larger size relocations exert a positive impact, while smaller size relocations have a negative impact – although the estimate for the bottom quartile (coeff. -3.913, s.e. 2.304) is less than half that of the top quartile (coeff. 9.088, s.e. 4.737). Regarding trade & catering, both larger and smaller relocations have a positive impact. Again, larger size relocations matter more. Looking at subsequent sets of spillovers (Table 7, 0-2km and 0-3km panels), I also find evidence of displacement: service employment tend to decrease in areas at 1-2km and 2-3km distances from a relocation site and to increase in areas at 0-1km distance. As previously noted, the policy impact on trade & catering activities is associated with relocations of different sizes (see Table 6, 0-2km panel). Conversely, larger size relocations exert a more significant impact on FIRE & business services, particularly at 2-3km distance (see Table 7, 0-3km panel).

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 was the following: in order to overcome one of the challenges my analysis faces, i.e. not knowing *a priori* the geographical 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 courser geographic scale. In order to verify the stability of my results, this section replicates the analysis using a larger geography,

[32] Performing F-tests on the equality of the top and bottom quartile coefficients, differences are not statistically significant. The test has p-value=0.156 (F-stat=2.02) for both FIRE and transport. The corresponding values for catering and construction are p-value=0.269 (F-stat=1.23) and p-value =0.633 (F-stat=0.23), respectively.

the 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[33], 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 equation (1) using (2003-2008) 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. Results shown in Table 8 closely mirror those reported in Table 1. A coefficient of 0.779 (s.e. 0.404) reported in Column 3, top panel, indicates that for each government job relocated during 2003-2007, about 0.8 new jobs were created in central government in a receiving LSOA over the period 2003-2008.

Next, I look at the main effects. I analyse the impact of the policy on three variables of interest: total private sector, manufacturing and service employment (see Table 9). In order to ease the comparison between LSOA and OA estimates, variables in Table 9 are expressed as number of jobs (workers) per squared kilometre so that LSOA estimates can be interpreted as average effects per squared kilometre 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.[34]

Consistent with the results presented in Table 2, Table 9 shows that the dispersal of public sector jobs had a positive impact on private sector employment in receiving LSOAs. A coefficient of 0.486 (s.e. 0.291) indicate that the arrival of 10 civil service jobs in an area triggered the creation of about 4.9 additional jobs in the private sector (see Column 3, top panel). The corresponding figure in Table 2 was 5.5. As before, I find that the policy changed the industry composition of the local economy: it depressed manufacturing employment while stimulating jobs in local services. Evidence suggests that for 10 new central government jobs created in an area, manufacturing employment on average falls by about 2 whereas service employment raises by about 7.9. The corresponding figures in Table 2 were 1.4 and 6.5, respectively. I find that spillover effects spread over a longer distance (0-5km) than before (0-2km) but there is limited evidence of displacement – mainly for local services (see Column 9).[35] When splitting

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

[34] Although OAs have an average size of 1 km^2, the sample of OAs included in the estimation has an average size of 0.84 km^2: 0.94 km^2 for the group of receiving OAs and 0.84 km^2 for the non-receiving group.

[35] Combining Table 9 estimates with the corresponding counts of LSOAs in each ring, I found effects that are comparable to those reported in Table 3: a positive total spillover effect within the 0-5km distance band (0.550); a small and positive average effect for any of the 100 LSOAs included within the 0-5km circle (0.013).

services by group and sub-group, results confirm that business services are driving the expansion of local services.[36]

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 localised: i.e. the largest policy impact is found in receiving LSOAs. There are also a few differences, largely due to the reasons pointed out before: both the negative impact on manufacturing and the positive impact on services are larger when using LSOAs than when using OAs; the crowding-out effect on manufacturing employment is more precisely estimated with a larger scale; the positive impact on local services is now split more equally between trade & catering and FIRE & business services; and comparable spillovers effects that spread over a longer distance.

7. Conclusions

Since World War II, the UK government has used relocation programmes of public sector workers as a tool to address employment problems in declining regions (see Jefferson and Trainor, 1996). In recent years, the move of two thousands BBC's London-based posts to MediaCity UK, Salford[37] and the relocation of the Office for National Statistics (ONS) headquarters from London to Newport have attracted public attention.[38] Advocates of relocation programmes believe they help lagging regions through public investment. Opponents view relocation programmes (and the associated redundancy packages for workers who chose not to relocate) as wastes of taxpayers' money. Despite the media attention, there is scarce evidence of the effects of a public sector relocation programme upon local labour markets. This study has tried to fill this gap by assessing the local labour market impact of a relocation initiative, the 2004 Lyons Review.

The study has found that the implementation of the Lyons Review had a multiplier effect on private sector employment at the local level. The arrival of 10 civil service jobs in an area triggered the creation of 5.5 local jobs in the private sector. It also affected the sector composition of local jobs: it spurred the provision of locally-produced services, whereas it had a detrimental but weaker impact on manufacturing. The study has also found evidence of displacement. Service employers seem to move out of areas at 1-2km distance to a relocation site and move into areas at 0-1km distance. Displacement is, however, not complete: the positive effect within the 0-1km distance band more than offset the negative impact within

[36] Results available from the author on request.

[37] 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.

[38] 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.

the 1-2km band. These agglomeration effects appear highly localised: i.e. the largest policy impact is found in areas that received the relocated jobs; and spillover effects reduce sharply over distance.

Results are also robust to a series of checks and extensions. I have verified that central government employment actually expanded in areas that received the relocated jobs, conducted a placebo experiment by estimating the impact of the relocation programme on outcomes during a pre-treatment period (2000-2001), and replicated the analysis using a larger geographic scale (LSOAs). In extending the main analysis, I have found that larger size relocations are associated with a larger policy impact. Consistent with the effects being highly localized around the relocation workplace, I have also found that increased intermediate demand for business services, consultancy and legal work (rather than consumer demand for personal services) largely explains the positive impact on the service sector.

Overall, the policy appears to have been beneficial. It acted as a Keynesian-type fiscal stimulus at the local level. It triggers the much sought-after local multiplier effect. Even though it had a crowding-out effect on manufacturing activities, this negative impact was more than compensated by the multiplier effect on local services. Three caveats remain. First, the analysis provides short-run estimates of the policy impact. It focuses on estimating 2003-2008 effects. It would be hard to pin down the long-run effects of the policy because of the difficulty in disentangling the impact of the 2008 recession from that of the relocation programme on areas that have been beneficiaries of public support through the relocated jobs.

Second, this type of policies might have unintended effects. Although the relocation programme was not envisaged to differentially stimulate the provision of (locally-produced) goods and services, this is *de facto* what it did. It was good for services; it was not so good for manufacturing. With the evidence presented so far, it is impossible to quantify whether those changes were net welfare improving for the local economy and its residents.

Third, my conclusions are based on a partial analysis. This study has presented an accurate evaluation of the impact of the policy on private sector employment, but it still provides a partial analysis. Evaluating both the costs and benefits of the policy at the local level would require a thorough analysis of the evolution of labour costs and housing prices before and after the implementation of the programme, which is outside the scope of this study and is left for future research.

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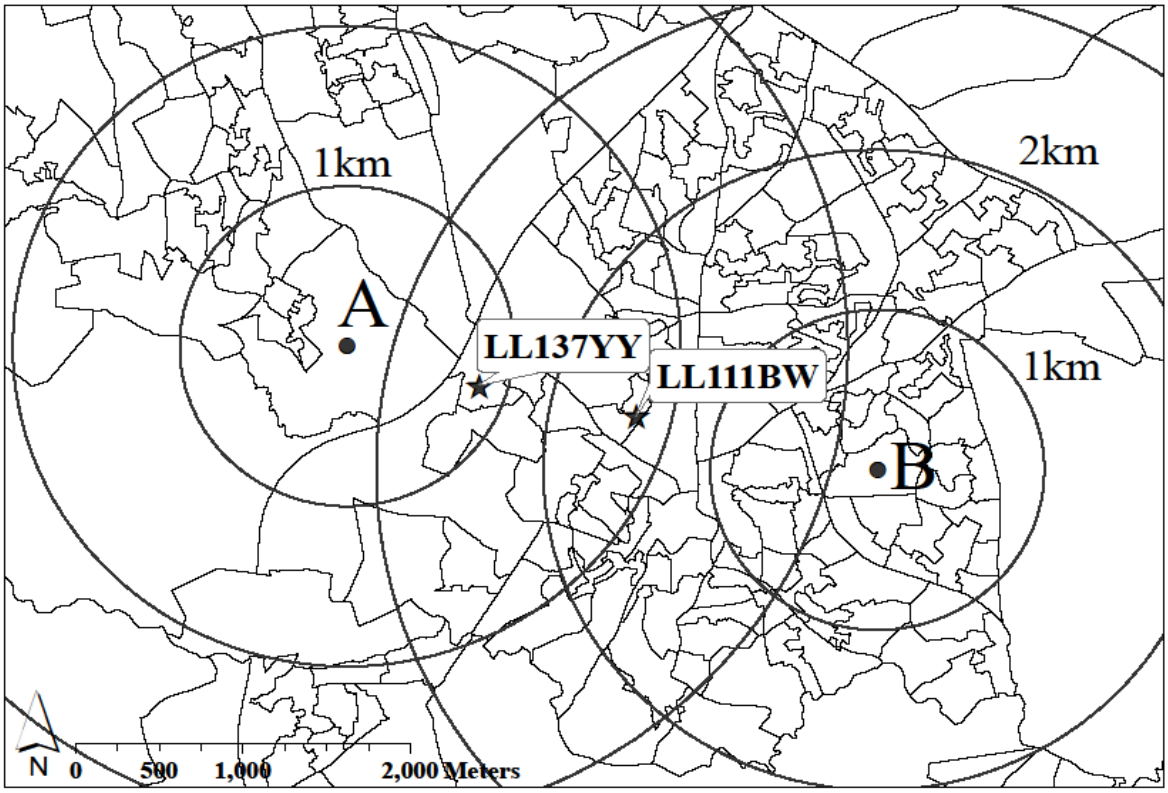
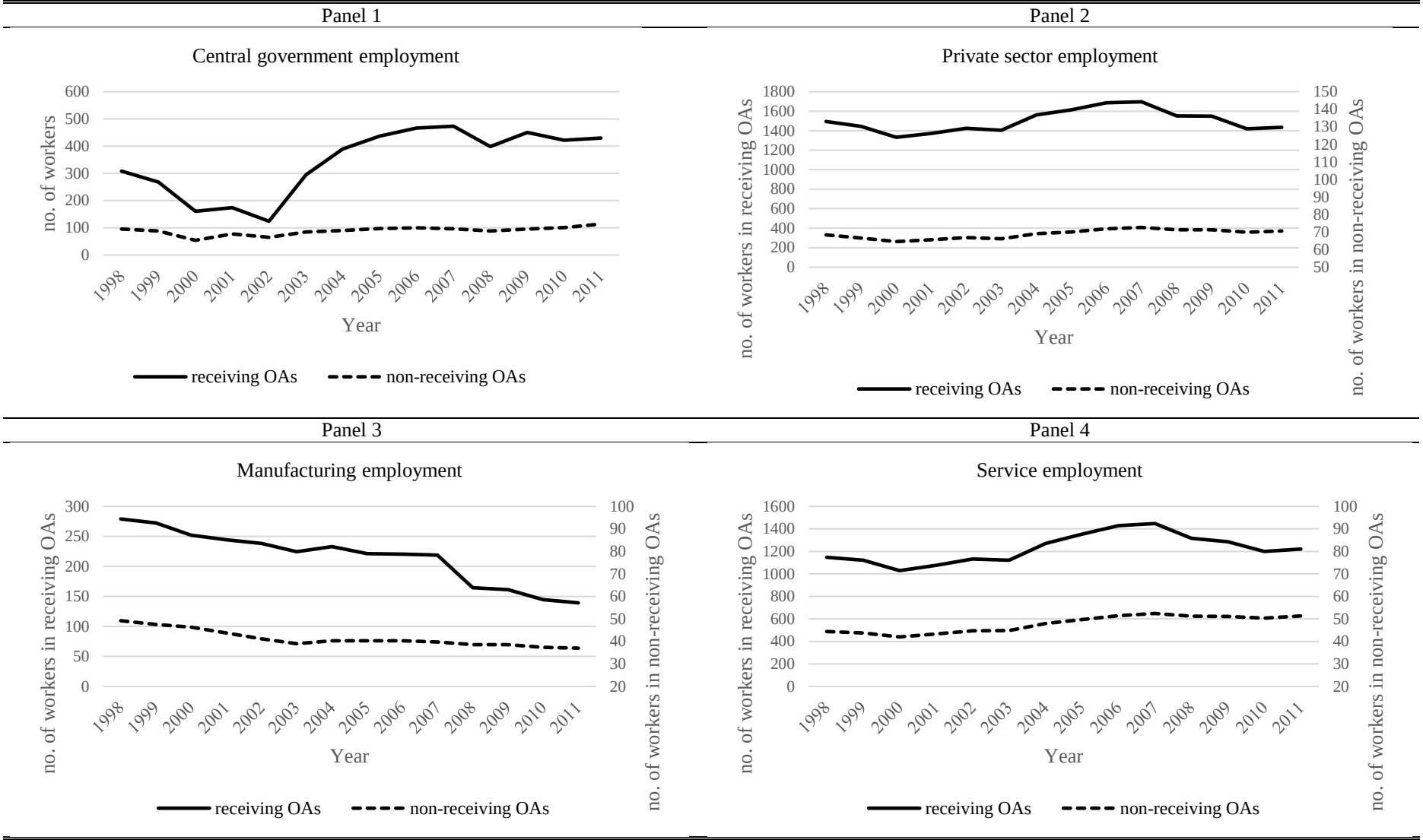
Figure 1: Graphical representation of treatment intensity variables



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.
Source: Government relocation data (2003-2010); 2001 UK Census Output Area shape-files; and NSPD directory.

Figure 2: Trends in central government, private sector, manufacturing and service sector employment, 1998-2011



Note: Figures report the total number of employees in each sector.
Sources: BSD local unit data (public sector and private sector employers), 1998-2011.

Table 1: The impact of 2003-2007 cumulative relocations on (2003-2008) changes in central government employment

	Relocation size variables			Relocation dummy variables		
	(1)	(2)	(3)	(4)	(5)	(6)
Receiving areas						
0km	1.106***	1.078***	1.037***	107.568***	98.239***	87.561***
	(0.377)	(0.377)	(0.346)	(31.947)	(30.597)	(27.574)
Spillovers						
0-1km	0.079	0.052	0.042	18.704	6.968	5.024
	(0.062)	(0.056)	(0.058)	(18.491)	(19.289)	(19.518)
0-2km	0.054	0.021	0.024	-9.747	-12.244	-10.309
	(0.078)	(0.086)	(0.081)	(22.511)	(22.903)	(22.099)
0-3km	-0.017	-0.026	-0.031	10.312	5.975	4.699
	(0.058)	(0.063)	(0.059)	(22.947)	(22.367)	(21.452)
0-4km	0.000	0.019	0.020	-6.895	-6.375	-6.138
	(0.067)	(0.065)	(0.062)	(18.047)	(17.818)	(17.808)
0-5km	-0.008	-0.010	-0.000	4.395	14.139	16.852
	(0.033)	(0.032)	(0.031)	(16.669)	(17.090)	(17.210)
0-50km	-0.017	-0.013	-0.011			
	(0.015)	(0.014)	(0.014)			
Area controls		√	√		√	√
Pre-trend			√			√
Observations	2,228	2,228	2,228	2,228	2,228	2,228
# of clusters	189	189	189	189	189	189

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 (2003-2008) changes in central government employment. Columns (1)-(3) use treatment intensity variables expressed as the total number of jobs moved (labelled as relocation size variables); Columns (4)-(6) use treatment intensity variables expressed as binary indicators (labelled as relocation dummy variables). Columns (1) and (4) do not include area controls; Columns (2) and (5) include 2001 UK Census area controls (see Appendix B for details); and Columns (3) and (6) include both area controls and pre-trends. Pre-trends are defined as (1998-2002) changes in central government employment at the OA level. All regressions include 189 TTWA fixed effects and standard errors are clustered at the TTWA level. The sample consists of OAs with non-zero central government employment in 2008.
Sources: OGC Government relocation data, 2003-2010; BSD local unit data (public sector employers only), 1998-2011; UK Census of Population, 2001.

Table 2: The impact on total private sector, manufacturing and service employment, (2003-2008) employment changes

	Total Private Sector			Manufacturing			Services		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Receiving areas									
0km	0.599**	0.538**	0.548**	-0.169*	-0.135	-0.141*	0.712***	0.644***	0.652***
	(0.253)	(0.256)	(0.257)	(0.092)	(0.088)	(0.087)	(0.249)	(0.248)	(0.249)
Spillovers									
0-1km	0.077***	0.061**	0.061**	-0.009	0.002	0.004	0.097***	0.077***	0.077***
	(0.025)	(0.025)	(0.025)	(0.012)	(0.011)	(0.011)	(0.027)	(0.025)	(0.025)
0-2km	-0.010	-0.021***	-0.022***	-0.016	-0.008	-0.011	0.001	-0.013***	-0.013***
	(0.006)	(0.007)	(0.007)	(0.013)	(0.013)	(0.012)	(0.004)	(0.004)	(0.004)
0-3km	-0.004	-0.008	-0.008	-0.011	-0.009	-0.009	-0.002	-0.007	-0.007
	(0.006)	(0.006)	(0.006)	(0.009)	(0.009)	(0.008)	(0.004)	(0.004)	(0.004)
0-4km	0.002	0.002	0.003	-0.005	-0.003	-0.003	0.002	0.003	0.003
	(0.007)	(0.006)	(0.006)	(0.009)	(0.009)	(0.009)	(0.005)	(0.005)	(0.005)
0-5km	-0.002	0.001	0.001	0.001	0.002	0.003	-0.003	0.000	0.000
	(0.003)	(0.003)	(0.003)	(0.005)	(0.005)	(0.005)	(0.002)	(0.002)	(0.002)
0-50km	-0.002*	-0.001	-0.001	0.001	0.001	0.001	-0.002**	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Area controls		√	√		√	√		√	√
Pre-trends			√			√			√
Observations	161,864	161,864	161,864	54,040	54,040	54,040	158,223	158,223	158,223
# of clusters	202	202	202	201	201	201	202	202	202

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 (2003-2008) 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 (2003-2008) changes in manufacturing employment (SIC15-SIC37). In columns (7)-(9), the dependent variable is defined as (2003-2008) 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 TTWA fixed effects and standard errors are clustered at the TTWA level. In total, there are 202 TTWAs in the dataset. Regarding sample restrictions, OAs with zero private sector (Columns 1-3), manufacturing (Columns 4-6) and service employment (Columns 7-9) in both 2003 and 2008 are dropped from the estimation.

Sources: OGC Government relocation data, 2003-2010; BSD local unit data (private sector employers only), 1998-2011; UK Census of Population, 2001.

Table 3: Average total effects per distance band

	Average OA count (1)	Total Private Sector (2)	Manufacturing (3)	Services (4)	Trade & Catering (5)	FIRE & Business (6)
Receiving areas						
0km	1	0.548**	-0.141*	0.652***	0.265**	0.425***
Spillovers						
0-1km	39	2.379**	0.156	3.003***	1.989**	1.872***
0-2km	85	-1.870***	-0.935	-1.105***	-0.935***	-0.765*
Total spillover effect within the 0-2km band						
(0-1km+0-2km)		0.509	-0.779	1.898	1.054	1.107
Average effects for any OA included within the 0-2km circle						
0-2km circle	125	0.008	-0.005	0.020	0.011	0.012

Note: The underlying OA estimates for Columns (2)-(4) are taken from Table 2, Columns (3), (6) and (9); the underlying estimates for Columns (5)-(6) are taken from Table 4, Columns (3)-(4). The average total effect per distance band is computed multiplying the average OA effect by the average number of OAs in each band. The average effect for any OA included within the 0-2km circle is computed as a weighted average of OA effects within the 0-2km circle with weights being the OA count in each band. Defining γ^0, γ^1, γ^2 as the parameters for the treatment variable and the first two treatment intensity variables (see equation 1); and N^1, N^2as the numbers of OAs in the 0-1km and 1-2km rings, respectively, the average effect for any OA within the 0-2km circle can be expressed as $avg = (\gamma^0 + \gamma^1 * N^1 + \gamma^2 * N^2)/(1 + N^1 + N^2)$. This computation takes the gains obtained locally around the relocation site and spread them evenly across all OAs within the 0-2km circle.

Table 4: Splitting services by type, (2003-2008) changes in employment

	Construction (1)	Transport (2)	Trade & Catering (3)	FIRE & Business (4)
Receiving areas				
0km	0.026*	0.073	0.265**	0.425***
	(0.013)	(0.107)	(0.111)	(0.159)
Spillovers				
0-1km	-0.004	0.010	0.051**	0.048***
	(0.004)	(0.016)	(0.022)	(0.018)
0-2km	-0.001	0.000	-0.011***	-0.009*
	(0.001)	(0.008)	(0.003)	(0.005)
0-3km	-0.000	-0.003	-0.003	-0.005
	(0.001)	(0.005)	(0.003)	(0.004)
0-4km	0.000	-0.000	0.002	0.003
	(0.001)	(0.003)	(0.003)	(0.004)
0-5km	0.000	0.001	-0.001	0.001
	(0.000)	(0.001)	(0.001)	(0.001)
0-50km	0.000	0.000	-0.001	-0.001
	(0.000)	(0.001)	(0.001)	(0.001)
Observations	84,935	42,391	125,040	112,993
# of clusters	201	201	202	201

Note: Robust standard errors are reported in parentheses; *, **, *** indicate significance at the 10%, 5% and 1% levels, respectively. Dependent variables are defined as (2003-2008) employment changes in construction (Column 1), transport (Column 2), trade & catering (Column 3), FIRE & business (Column 4). All columns include 2001 area characteristics and (1998-2002) pre-trends. All regressions include TTWA fixed effects and standard errors are clustered at the TTWA level. In total, there are 202 TTWAs in the dataset. For each industry group, OAs with zero employment in both 2003 and 2008 are excluded from the estimation.
Sources: See Table 2.

Table 5: Splitting selected services by sub-group, (2003-2008) employment changes

	FIRE & business			Trade & catering		
	Finance	Real Estate	Business	Trade	Hotels & restaurants	Personal services
	(1)	(2)	(3)	(4)	(5)	(6)
Receiving areas						
0km	-0.449	0.109**	0.466***	0.086	0.181*	0.016
	(0.284)	(0.050)	(0.171)	(0.110)	(0.095)	(0.022)
Spillovers						
0-1km	0.052**	0.004	0.038**	0.025**	0.044**	0.006
	(0.022)	(0.005)	(0.015)	(0.012)	(0.020)	(0.005)
0-2km	-0.030**	0.002	-0.007	-0.009***	-0.003	-0.003**
	(0.012)	(0.002)	(0.005)	(0.003)	(0.002)	(0.001)
0-3km	-0.024**	-0.002	-0.000	-0.003	-0.001	-0.000
	(0.011)	(0.002)	(0.003)	(0.002)	(0.004)	(0.001)
0-4km	0.018*	-0.001	0.000	0.002	0.001	0.001
	(0.010)	(0.002)	(0.003)	(0.002)	(0.003)	(0.001)
0-5km	0.002	0.002*	0.000	0.000	-0.001	-0.001
	(0.004)	(0.001)	(0.001)	(0.001)	(0.001)	(0.000)
0-50km	-0.001	-0.001	-0.000	-0.000	-0.001	0.000
	(0.003)	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)
Observations	13,053	38,408	104,842	100,299	52,346	64,703
# of clusters	196	201	201	201	202	201

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 (2003-2008) employment changes in finance and insurance (SIC65-SIC67). Similarly, Columns (2)-(6) use (2003-2008) 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 TTWA fixed effects and standard errors are clustered at the TTWA level. In total, there are 202 TTWAs in the dataset. For each industry group, OAs with zero employment in both 2003 and 2008 are excluded from the estimation.
Sources: See Table 2.

Table 6: Falsification test, (2000-2001) employment changes adding (1998-1999) pre-trends

	Manufacturing	Services	Construction	Transport	FIRE & Business	Trade & Catering
	(1)	(2)	(3)	(4)	(5)	(6)
Receiving areas						
0km	-0.035	0.077	-0.000	0.008	0.045	0.030*
	(0.057)	(0.047)	(0.007)	(0.028)	(0.059)	(0.018)
Spillovers						
0-1km	0.006	0.007	-0.001	0.016	-0.002	0.005
	(0.007)	(0.005)	(0.002)	(0.026)	(0.006)	(0.006)
0-2km	-0.008	-0.006	0.001	-0.022	-0.002	-0.001
	(0.006)	(0.005)	(0.001)	(0.018)	(0.003)	(0.002)
0-3km	0.003	-0.000	-0.001	0.003	-0.001	-0.001
	(0.005)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
0-4km	-0.005	-0.001	0.001**	-0.002	-0.002	0.000
	(0.007)	(0.001)	(0.000)	(0.002)	(0.002)	(0.001)
0-5km	0.001	0.001	-0.000	0.000	0.001	0.000
	(0.005)	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
0-50km	0.001	-0.000	-0.000	-0.001	0.000	-0.000
	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)
Observations	49,545	155,295	73,639	35,797	100,732	119,672
# of clusters	201	202	201	202	201	202

Note: Robust standard errors are reported in parentheses; *, **, *** indicate significance at the 10%, 5% and 1% levels, respectively. Dependent variables are defined as (2000-2001) employment changes in manufacturing (Columns 1), services (Columns 2), construction (Column 3), transport (Column 4), FIRE & business (Column 5), trade and catering (Column 6). All columns include 1991 area characteristics and (1998-1999) pre-trends. Pre-trends are defined as (1998-1999) employment changes in manufacturing (Columns 1), services (Columns 2), construction (Column 3), transport (Column 4), FIRE & business (Column 5), trade and catering (Column 6). All regressions include TTWA fixed effects and standard errors are clustered at the TTWA level. In total, there are 202 TTWAs in the dataset. For each industry group, OAs with zero employment in both 2003 and 2008 are excluded from the estimation.

Sources: OGC Government relocation data, 2003-2010; BSD local unit data (private sector employers only), 1998-2011; UK Census of Population, 1991.

Table 7: The impact by quartile class and distance band, (2003-2008) employment changes

	Bottom →						**Top**	
	1st quartile		2nd quartile		3rd quartile		4th quartile	
	Coeff.	SE	Coeff.	SE	Coeff.	SE	Coeff.	SE
Receiving areas 0km								
FIRE	41.811	(27.386)	61.019	(44.878)	51.331	(43.559)	142.390**	(63.587)
Catering	56.117**	(22.151)	60.639**	(25.983)	83.768***	(27.185)	112.908***	(42.023)
Construction	8.977**	(4.292)	0.139	(3.024)	4.506	(5.357)	5.302	(6.123)
Transport	7.812	(16.854)	2.806	(5.899)	10.716	(20.824)	56.834**	(24.047)
Spillovers 0-1km	1st quartile		2nd quartile		3rd quartile		4th quartile	
FIRE	-3.913*	(2.304)	4.064	(3.320)	-2.694	(3.589)	9.088*	(4.737)
Catering	2.958**	(1.443)	-0.317	(1.820)	1.674	(2.327)	7.359**	(3.294)
Construction	0.182	(0.532)	-0.646	(0.676)	-1.219	(0.838)	0.055	(0.885)
Transport	-1.566	(4.303)	4.547**	(2.265)	-4.133	(5.397)	1.080	(3.582)
0-2km	1st quartile		2nd quartile		3rd quartile		4th quartile	
FIRE	1.204	(1.267)	0.334	(1.144)	0.638	(1.657)	-1.698	(1.312)
Catering	-1.635*	(0.962)	-1.599**	(0.744)	-4.130***	(1.060)	-3.604***	(1.207)
Construction	0.141	(0.435)	-0.032	(0.331)	-0.379	(0.491)	-0.584	(0.506)
Transport	0.123	(1.216)	-0.162	(1.133)	-2.713	(1.704)	-0.247	(1.867)
0-3km	1st quartile		2nd quartile		3rd quartile		4th quartile	
FIRE	-0.329	(0.738)	-1.081	(1.126)	-2.844**	(1.145)	-3.134*	(1.645)
Catering	1.189	(0.854)	-0.043	(0.862)	-1.116	(1.106)	-1.517	(1.416)
Construction	0.155	(0.329)	-0.252	(0.305)	0.129	(0.421)	-0.422	(0.514)
Transport	0.471	(1.143)	-1.283	(0.991)	-1.681	(1.448)	-1.851	(1.903)
0-4km	1st quartile		2nd quartile		3rd quartile		4th quartile	
FIRE	-0.237	(0.564)	-1.185	(1.148)	-0.991	(1.180)	2.048	(1.296)
Catering	-0.651	(0.787)	-0.842	(0.812)	1.468	(1.046)	1.669	(1.021)
Construction	-0.544	(0.336)	0.291	(0.429)	-0.255	(0.413)	0.178	(0.385)
Transport	0.263	(0.943)	1.366	(1.501)	2.400**	(0.981)	0.648	(1.693)
0-5km	1st quartile		2nd quartile		3rd quartile		4th quartile	
FIRE	0.829	(0.552)	2.292**	(0.968)	2.521***	(0.775)	1.537*	(0.860)
Catering	1.062*	(0.601)	2.094***	(0.609)	0.063	(0.585)	-0.236	(0.699)
Construction	0.677***	(0.252)	0.359	(0.320)	0.517**	(0.257)	0.442	(0.291)
Transport	-0.639	(0.697)	0.276	(1.235)	-1.209	(0.780)	0.362	(1.047)
0-50km	1st quartile		2nd quartile		3rd quartile		4th quartile	
FIRE	-63.297	(130.278)	-62.523	(130.545)	-62.745	(130.598)	-62.963	(130.558)
Catering	-67.469	(82.638)	-66.892	(82.747)	-66.333	(82.960)	-66.551	(82.936)
Construction	0.282	(5.477)	0.468	(5.495)	0.331	(5.477)	0.474	(5.550)
Transport	73.792**	(35.036)	74.190**	(35.035)	75.240**	(35.424)	76.850**	(35.490)

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, the 0-1km treatment intensity variable is split into four components, each capturing a different quartile of the size of the relocations. In the sample, there are 112,993 observations (201 clusters) in FIRE & business services; 125,040 (202 clusters) in trade & catering; 84,935 (201 clusters) in construction; and 42,391 (201 clusters) in transport. All dependent variables are described as in Table 4. All columns include 2001 area controls and (1998-2002) pre-trends. All regressions include 202 TTWA fixed effects and standard errors are clustered at the TTWA level.
Sources: See Table 2.

Table 8: The impact of 2003-07 cumulative relocations on Δ(2003-08) central government employment, LSOA level

	Relocation size variables			Relocation dummy variables		
	(1)	(2)	(3)	(4)	(5)	(6)
Receiving areas						
0km	0.897**	0.838**	0.779*	149.364***	128.479***	115.290***
	(0.443)	(0.422)	(0.404)	(32.971)	(27.885)	(26.141)
Spillovers						
0-1km	0.061	0.072	0.054	35.513	25.254	26.828
	(0.096)	(0.094)	(0.102)	(20.343)	(23.559)	(24.824)
0-2km	0.104	0.059	0.069	-11.895	-26.137	-24.955
	(0.071)	(0.075)	(0.068)	(19.560)	(21.191)	(21.261)
0-3km	-0.063	-0.094	-0.093	-11.782	-5.857	-5.614
	(0.089)	(0.097)	(0.092)	(26.077)	(23.400)	(23.642)
0-4km	0.079	0.088	0.094	10.681	7.175	8.229
	(0.105)	(0.090)	(0.087)	(29.240)	(26.472)	(26.467)
0-5km	-0.072	-0.071	-0.069	0.512	8.798	9.797
	(0.054)	(0.054)	(0.054)	(18.885)	(17.973)	(18.921)
0-50km	-0.033	-0.024	-0.022			
	(0.020)	(0.017)	(0.017)			
Area controls		√	√		√	√
Pre-trend			√			√
Observations	2,188	2,188	2,188	2,188	2,188	2,188
# of clusters	191	191	191	191	191	191

Note: Robust standard errors are reported in parentheses; *, **, *** indicate significance at the 10%, 5% and 1% levels, respectively. All regressions include TTWA fixed effects and standard errors are clustered at the TTWA level. All variables are defined as in Table 2, but they are computed at the LSOA level. In Columns (2)-(3) and (5)-(6), LSOA area controls are expressed as unweighted averages of OA control variables within a LSOA.
Sources: See Table 2.

Table 9: The impact on total private sector, manufacturing and service employment, (2003-2008) employment changes, LSOA level

	Total Private Sector			Manufacturing			Services		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Receiving areas									
0km	0.854***	0.529*	0.486*	-0.331**	-0.194*	-0.205*	1.178***	0.796***	0.787***
	(0.317)	(0.307)	(0.291)	(0.133)	(0.112)	(0.116)	(0.312)	(0.253)	(0.241)
Spillovers									
0-1km	0.276	0.011	0.013	-0.149**	-0.040	-0.038	0.512**	0.201	0.199
	(0.190)	(0.143)	(0.145)	(0.066)	(0.047)	(0.046)	(0.197)	(0.126)	(0.124)
0-2km	0.042	-0.117	-0.122	-0.085**	-0.014	-0.025	0.090	-0.097*	-0.097*
	(0.076)	(0.075)	(0.074)	(0.041)	(0.039)	(0.035)	(0.057)	(0.055)	(0.055)
0-3km	-0.092	-0.091	-0.092	-0.020	0.011	0.018	-0.104	-0.119**	-0.120**
	(0.079)	(0.062)	(0.061)	(0.038)	(0.026)	(0.025)	(0.064)	(0.054)	(0.053)
0-4km	0.038	0.050	0.053	0.002	0.030	0.033	-0.004	0.001	0.022
	(0.047)	(0.043)	(0.043)	(0.031)	(0.027)	(0.029)	(0.039)	(0.035)	(0.039)
0-5km	0.018	0.048	0.046	-0.040**	-0.008	-0.005	0.065**	0.090***	0.089***
	(0.036)	(0.040)	(0.041)	(0.018)	(0.019)	(0.017)	(0.031)	(0.030)	(0.032)
0-50km	0.003	0.032	0.032	0.019	0.011	0.012	-0.003	0.036	0.036
	(0.025)	(0.029)	(0.029)	(0.019)	(0.016)	(0.016)	(0.021)	(0.025)	(0.025)
Area controls		√	√		√	√		√	√
Pre-trends			√			√			√
Observations	35,559	35,559	35,559	27,300	27,300	27,300	35,525	35,525	35,525
# of clusters	200	200	200	200	200	200	200	200	200

Note: Robust standard errors are reported in parentheses; (*), (**), (***) indicate significance at the 10%, 5% and 1% levels, respectively. All regressions include TTWA fixed effects and standard errors are clustered at the TTWA level. Variables are expressed as employment density, i.e. the number of jobs/employees per squared kilometre. In columns (1)-(3), the dependent variable is defined as the (2003-2008) changes in total private sector employment per squared kilometre (SIC11-SIC93). Columns (4)-(5) refer to manufacturing employment per squared kilometre; Columns (7)-(9) refer to service employment per square kilometre. 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 squared kilometres) 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. LSOA area controls are expressed as unweighted averages of OA control variables within a LSOA.
Sources: See Table 2.

Appendix A: Data construction

The Business Structure Database (BSD)

The analysis uses (2003-2008) 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 1998-2011. The BSD is an annual snapshot (taken in April 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 includes 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. In particular, 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. Secondly, I check the consistency of units' postcodes and sectors of activity over the years, and drop cases with missing or anomalous information.[39] 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 same postcode. Finally,

[39] 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.

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 14 years (1998-2011).

Table A.1: List of OA characteristics retrieved from the 2001 UK Census of Population

Variable Name	Variable Definition	Mean	S.D.
Demographic variables			
pop_density	Population density (in square kilometres)	52.3	62.1
pop_less25	Proportion of population aged 24 or younger	0.299	0.087
pop_25_44	Proportion of population aged 25 to 44	0.296	0.089
pop_45_64	Proportion of population aged 45 to 64	0.239	0.072
pop_65_74	Proportion of population aged 65 to 74	0.087	0.047
pop_over75	Proportion of population aged 75+	0.079	0.066
noqual_wap	Proportion of WAP without qualifications	0.301	0.137
qlev1_wap	Proportion of WAP with qualification level 1	0.173	0.063
qlev2_wap	Proportion of WAP with qualification level 2	0.186	0.054
qlev3_wap	Proportion of WAP with qualification level 3	0.078	0.048
qlev4o5_wap	Proportion of WAP with qualification level 4 o 5	0.203	0.136
qoth_wap	Proportion of WAP with other qualifications	0.060	0.034
pop_female	Proportion of female population	0.515	0.037
Labour Market Variables			
emp_xstud	Employment-to-population ratio - excluding students	0.645	0.123
inact_xstud	Inactivity-to-population ratio - excluding students	0.317	0.111
un_xstud	Unemployment-to-population ratio - excluding students	0.038	0.030
sh_stud	Share of students in the WAP(16-74)	0.068	0.067
sh_ret	Share of retired individuals in the WAP(16-74)	0.140	0.076
work_se_rate	Proportion of self-employed workers	0.133	0.079
work_pt_rate	Proportion of part-time employees	0.226	0.071
home_emp	Proportion of employees working from home	0.088	0.057
Social-economic Variables			
nowhite_sh	Proportion of all non-whites over population	0.080	0.145
lone_hous	Proportion of lone parent households over total households	0.064	0.055
migr_pop	Proportion of non-UK born over total population	0.086	0.108
hous_overcr	Proportion of overcrowded households	0.078	0.088
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	0.085	0.098
hous_wocars	Proportion of households without a car	0.277	0.189
hous_own	Proportion of households living in owned houses	0.681	0.247
hous_privrent	Proportion of private renters on total renters	0.536	0.347
hous_council	Proportion of social renters on total renters	0.464	0.347
pop_estab	Share of total population living in communal establishments	0.014	0.057
Commuting Variables			
travel_undergr	Proportion of employees travelling to work by tube	0.033	0.094
travel_train	Proportion of employees travelling to work by train	0.038	0.062
travel_bus	Proportion of employees travelling to work by bus	0.099	0.091
travel_motocy	Proportion of employees travelling to work by motorcycle	0.011	0.015
travel_car	Proportion of employees travelling to work by car	0.586	0.179
travel_carpas	Proportion of employees travelling to work by common car	0.072	0.043
travel_taxi	Proportion of employees travelling to work by taxi	0.006	0.013
travel_bike	Proportion of employees travelling to work by bike	0.028	0.034
travel_foot	Proportion of employees walking	0.121	0.093
travel_oth	Proportion of employees travelling to work by other means	0.006	0.013
av_commdist	Average distance (km) travelled to fixed place of work	13.0	6.0
pub_trans	People in employment who use public transport	0.157	0.146

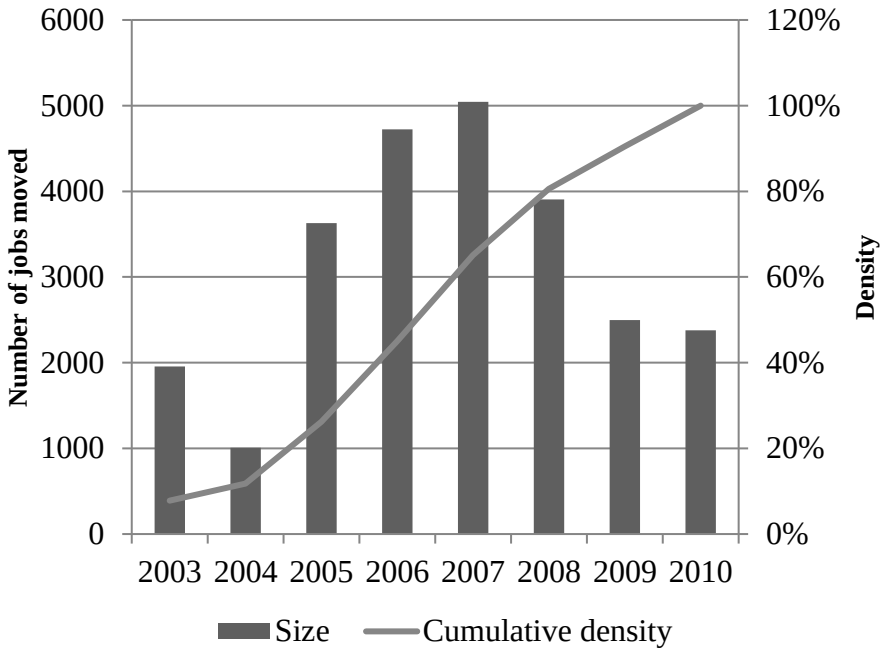
Note: WAP stands for working age population. Total number of observations: 168,625 OAs. **Source**: UK 2001 Census.

Table A.2: List of OA characteristics retrieved from the 1991 UK Census of Population

Variable Name	Variable Definition	Mean	S.D.
Demographic Variables			
pop_density	Population density (in square kilometres)	48.2	53.8
pop_less25	Proportion of population aged 24 or younger	0.319	0.068
pop_25_44	Proportion of population aged 25 to 44	0.295	0.070
pop_45_64	Proportion of population aged 45 to 64	0.220	0.057
pop_65_74	Proportion of population aged 65 to 75	0.091	0.043
pop_over75	Proportion of population aged over 75	0.073	0.050
qoth_wap	Proportion of WAP without higher degree, degree or diploma	0.853	0.127
qleva_wap	Proportion of WAP with higher degree	0.011	0.025
qlevb_wap	Proportion of WAP with degree	0.069	0.080
qlevc_wap	Proportion of WAP with diploma	0.067	0.061
pop_female	Proportion of females over tot pop	0.516	0.031
Labour Market Variables			
emp_xstud	emp_to_pop ratio - excluding students	0.586	0.117
inact_xstud	inact_to_pop ratio - excluding students	0.361	0.103
un_xstud	un_to_pop ratio - excluding students	0.058	0.040
sh_stud	share of students	0.045	0.028
sh_ret	share of retired individuals	0.189	0.083
work_se_rate	Proportion of self-employed workers	0.129	0.073
work_pt_rate	Proportion of part-time employee workers	0.206	0.056
home_emp	Proportion of employees working from home	0.054	0.087
Social-economic Variables			
nowhite_sh	Proportion of all non-whites over population	0.054	0.112
hous_lone	Proportion of lone parent households over total households	0.034	0.035
migr_pop	Proportion of non-UK born over total population	0.070	0.091
hous_own	Proportion of households living in owned houses	0.684	0.246
hous_council	Proportion of social renters on total renters	0.495	0.350
hous_privrent	Proportion of private renters on total renters	0.488	0.349
hous_overcr	Proportion of overcrowded households	0.022	0.028
hous_avsize	Average household size	1.6	0.7
hous_nbrooms	Average number of rooms per household	3.3	1.5
hous_noheat	Proportion of households without heating	0.187	0.164
hous_wocars	Proportion of households without a car	0.318	0.193
pop_in_est	Share of total population living in communal establishments	0.018	0.085
Commuting Variables			
travel_train	Proportion of employees travelling to work by train	0.060	0.118
travel_bus	Proportion of employees travelling to work by bus	0.112	0.134
travel_motocy	Proportion of employees travelling to work by motorcycle	0.015	0.031
travel_car	Proportion of employees travelling to work by car	0.559	0.207
travel_carpas	Proportion of employees travelling to work by common car	0.081	0.080
travel_bike	Proportion of employees travelling to work by bike	0.030	0.052
travel_foot	Proportion of employees travelling to work on foot	0.131	0.131
travel_oth	Proportion of employees travelling to work by other	0.005	0.025

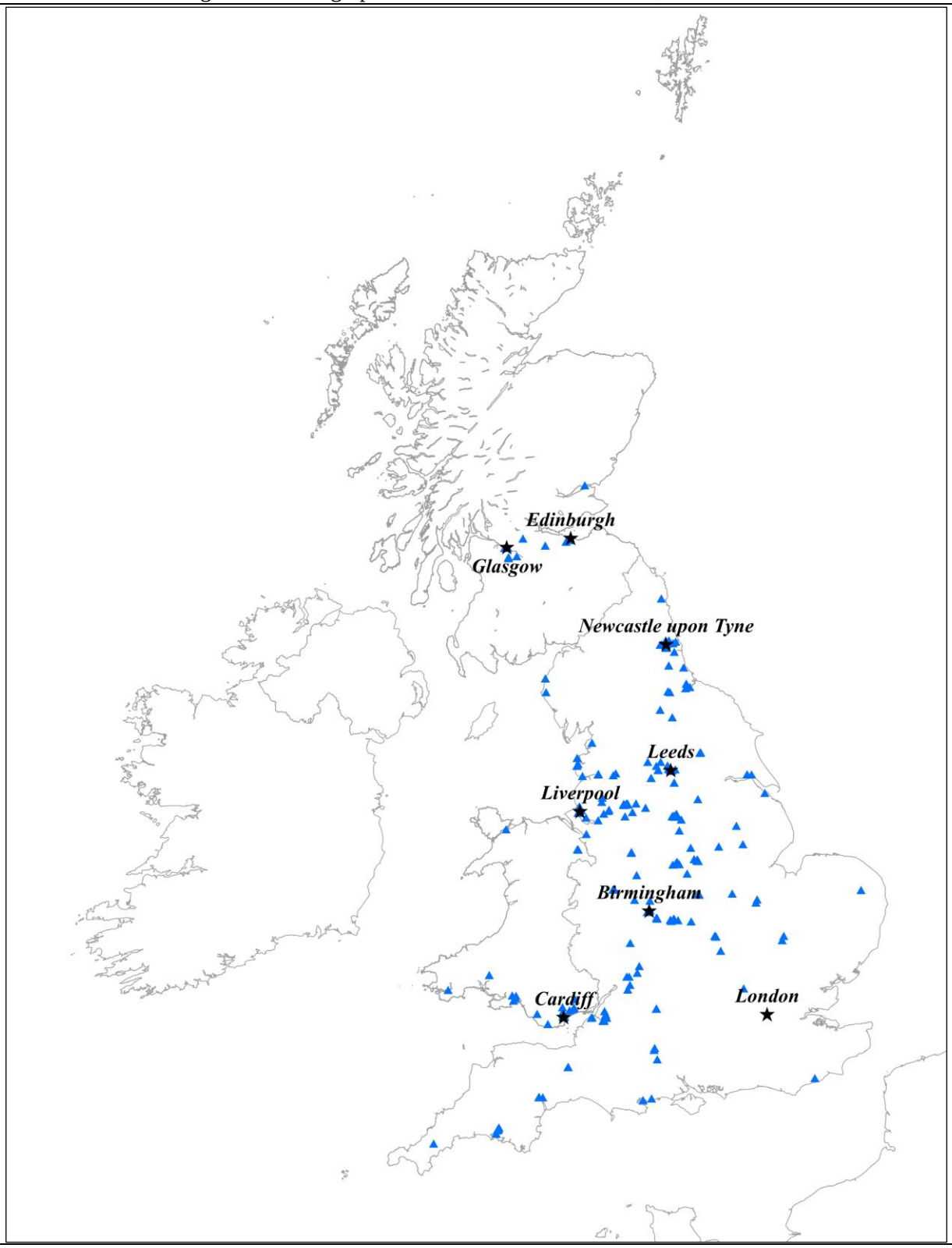
Note: WAP stands for working age population. Total number of observations: 167,749 OAs.
Source: UK Census of Population, 1991.

Figure A.3: 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 on the right-hand side. Even though the Lyons Review was published in 2004, the first job moves were recorded in June 2003. Between June 2003 and December 2004, about 3,000 jobs (12% of all jobs) were relocated.
Source: Government relocation data (2003-2010).

Figure A.4: Geographical distribution of relocations, 2003-2007



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

Appendix B: Additional results – Not for publication

Table B.1: The impact on total private sector, manufacturing and service employment, (2003-2008) employment changes, IV results

	Total Private Sector			Manufacturing			Services		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Receiving areas									
0km	1.145	1.051	1.070	-0.265	-0.222	-0.207	1.342	1.243	1.251
	(0.819)	(0.783)	(0.778)	(0.187)	(0.175)	(0.178)	(0.841)	(0.800)	(0.800)
Spillovers									
0-1km	0.113*	0.095*	0.095*	-0.005	0.002	0.004	0.139*	0.118*	0.118*
	(0.052)	(0.048)	(0.048)	(0.014)	(0.013)	(0.013)	(0.060)	(0.052)	(0.052)
0-2km	-0.023	-0.028*	-0.029*	-0.008	-0.005	-0.007	-0.018	-0.025*	-0.025*
	(0.012)	(0.012)	(0.012)	(0.011)	(0.011)	(0.011)	(0.012)	(0.011)	(0.011)
0-3km	0.000	-0.002	-0.001	-0.005	-0.005	-0.004	0.001	-0.001	-0.001
	(0.004)	(0.004)	(0.004)	(0.009)	(0.008)	(0.008)	(0.002)	(0.003)	(0.003)
0-4km	0.001	0.001	0.001	-0.005	-0.003	-0.003	0.002	0.001	0.001
	(0.004)	(0.004)	(0.003)	(0.008)	(0.008)	(0.008)	(0.002)	(0.002)	(0.002)
0-5km	-0.001	0.002	0.002	0.003	0.003	0.003	-0.002*	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.004)	(0.004)	(0.004)	(0.001)	(0.001)	(0.001)
0-50km	-0.000	0.000*	0.000	-0.000**	-0.000	-0.000	-0.000	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Controls		√	√		√	√		√	√
Pre-trends			√			√			√
Obs	161,864	161,864	161,864	54,040	54,040	54,040	158,223	158,223	158,223
# of clusters	202	202	202	201	201	201	202	202	202
Angrist-Pischke F-test of excluded instruments									
0km	2.75	2.75	2.75	2.73	2.74	2.74	2.75	2.75	2.75
	[0.0988]	[0.0988]	[0.0987]	[0.1001]	[0.0996]	[0.0994]	[0.0988]	[0.0988]	[0.0986]
0-1km	8.34	8.21	8.21	17.55	17.13	17.16	8.48	8.34	8.35
	[0.0043]	[0.0046]	[0.0046]	[0.0000]	[0.0001]	[0.0001]	[0.0040]	[0.0043]	[0.0043]
0-2km	29.54	29.57	29.57	34.41	35.18	35.24	29.07	29.11	29.12
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]
0-3km	23.68	23.42	23.42	23.42	21.68	21.68	24.99	24.63	24.63
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]
0-4km	41.00	43.40	43.40	49.44	51.32	51.32	40.45	42.84	42.84
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]
0-5km	39.65	35.76	35.76	38.17	31.62	31.62	39.44	35.34	35.34
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]
0-50km	234.95	173.81	173.82	271.57	193.91	193.99	235.03	173.53	173.53
	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]	[0.0000]

Note: Robust standard errors are reported in parentheses; (*), (**), (***) indicate significance at the 10%, 5% and 1% levels, respectively. The main regressors are the (2003-2008) change in central government employment in each ring *c*, with *c* = 0km, 0-1km, 0-2km,..., 0-50km, while the corresponding instrumental variables are the number of jobs moved between 2003-2007 in each ring *c*. Angrist-Pischke F-test statistics and p-values are also reported. All regressions include TTWA fixed effects and standard errors are clustered at the TTWA level. All other controls are measured as of Table 2.
Sources: see Table 2.