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The impact of public employment on private sector activity: Evidence from Berlin

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Abstract

We use the move of the seat of the German government from Bonn to Berlin in 1999 to test competing views about the impact of public employment on private sector activity in a local labor market. Using employment data from a 50% sample of establishments across 190 Berlin postcodes, we apply a treatment intensity approach which takes the possibility of spillovers into account. Results indicate that the arrival of 100 public sector jobs into an area generates 55 additional jobs in the private sector. There is evidence of spillovers: relocations up to a distance of 1km from a postcode boundary increases employment in the private sector by 36 jobs. These effects are coming through job gains in the service sector, while manufacturing employment is not influenced by the relocation.

I.   Introduction

Substantial differences in economic performance exist within and across countries. To explain this spatial inequality, the economic geography literature has focused on the location of private sector activity. This emphasis is justified by the fact that decisions in the private sector are largely driven by market forces whereas decisions in the public sector respond to multiple objectives. Nevertheless, shifting the focus from looking exclusively at the private sector to examining the interaction between public and private employment would be important for, at least, three reasons. First, public employment accounts for a substantial share of total employment in most OECD countries (21.3% on average in 2013). According to the latest estimates (see OECD, 2015), the size of public sector employment is 23.5%, 19.8% and 15.4% in the UK, France and Germany, respectively. Second, in the aftermath of the Great Recession, some austerity measures were introduced in the form of public sector job cuts, with the hope that by reducing the size of the public sector in an area, private activity would return and flourish. Scarce empirical evidence exists of these claims. Third, governments have frequently used relocation programs of public sector workers as a tool to address employment problems in declining regions (see Jefferson and Trainor, 1996). Advocates of these policies suggest that job relocation triggers local multiplier effects and therefore boosts regional economic performance. The arrival of public sector jobs in an area may increase demand for locally-produced goods and services (see Moretti, 2010; Faggio and Overman, 2014). Opponents to this view stress that the new jobs merely crowd out existing ones. Higher housing rents increase local production costs which impede new businesses to form or hamper existing businesses' competitiveness. These crowding-out effects may be stronger than the multiplier effects. Evidence on the economic impact of relocation policies is, however, limited (see Faggio, 2015).

This project uses the move of the German government from Bonn to Berlin at the end of the 1990s in order to investigate the interaction between public and private employment within a local labor market. Our objective is to identify which effect (multiplier or crowding-out) prevails when public sector jobs are relocated into small local areas. We conduct the analysis at a fine geographical scale (190 Berlin postcodes) and exploit the spatial and temporal variation in the data. We use information from the German Establishment History Panel that comprises a 50% sample of Berlin establishments with at least 1 employee from 1992 to 2010. Analyses of this kind are complicated by two factors: (1) the geographical spread of the policy is not known *a priori*; and (2) locations are not randomly chosen. To solve these issues, we construct a set of treatment intensity variables adapting from Gibbons *et al.* (2011) and Faggio (2015). We also control for initial (pre-treatment) economic and demographic characteristics at the postcode level. Our empirical strategy is a treatment intensity approach that takes the possibility of local spillovers into account. We compare employment changes in postcodes that have received the relocated jobs relative to postcodes that have not received them; we also compare employment changes in postcodes in proximity to a relocation site relative to postcodes at an increasing distance to a relocation site. In doing so, we evaluate the effects before and after the implementation of (a large part of) the relocation program.

We find that the arrival of 100 public sector jobs into an area generates 55 additional jobs in the private sector. There is evidence of spillovers: the inflow of 100 public sector jobs into an area also triggers job creation (36 new jobs) in areas neighboring the relocation site. These effects are coming through job gains in the service sector, while manufacturing employment is not influenced by the relocation. In addition, we find that agglomeration effects are highly localized within the first few kilometers from a relocation site and disappear sharply over distance. Our results are robust to a series of checks and extensions: we verify that the multiplier effects we detect

are indeed due to the relocation program and not to other transformation processes that were triggered by the reunification. We show, in fact, that the employment dynamics prior to the relocation differ substantially from those observed during the period of the government move, so indicating a structural break. We also rule out the possibility that our estimates are driven by the expansion of the city centre after the fall of the Berlin Wall by looking at the evolution of the CBD (central business district) expansion over time. Moreover, we rule out the possibility that the rise in private sector employment is due to a few large companies that relocated their headquarters near the new seat of the government by focusing on SME employment only. Furthermore, we replicate the analysis by using distance weighting functions instead of our measures of treatment intensity. Results seem to hold.

This paper contributes to an emerging literature examining the interaction between public and private sector employment within a local labor market. Faggio and Overman (2014) made a first attempt to look at the impact of public employment on private sector activity. Analyzing changes in total public sector employment for 352 English Local Authorities during the period 2003-2007, they find that public sector growth does not have any impact on total private sector growth. It changes, however, the local composition of private sector jobs by increasing the share of services. Faggio (2015) analyzes the effect of a public sector relocation program on a very detailed geographical scale using 160,000 UK Census Output Areas. She finds that public employment has a positive impact on private sector employment and it changes the composition of local private sector jobs towards services and away from manufacturing. She also finds that the program has highly localized effects that disappear quickly over distance. Becker *et al.* (2015) evaluate the impact of public employment on private sector activity using the move of the German government to Bonn after World War II (WWII) as a source of exogenous variation. They find that total employment and population in Bonn substantially increased after 1949 relative to a group of

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40 control cities. Because of data limitations, they cannot fully investigate the impact of public employment on the sectoral composition of local jobs. Jofre-Monseny *et al.* (2016) estimate the effects of public job expansions on decennial changes (1980-1990 and 1990-2001) in the employment and population of Spanish cities. They find that one additional public sector job creates about 1.3 jobs in the private sector. They also point out that these new jobs do not translate into a substantial reduction in local unemployment as better labor market conditions attract new workers to the city.[1]

The paper also makes a contribution to the literature on local multipliers. Moretti (2010) quantifies the long-term changes in the number of jobs in a city's tradable and non-tradable sectors generated by a permanent increase in the number of jobs in the tradable sector. He finds that, in the US, 100 additional jobs in the tradable sector increase employment in the non-tradable sector by 160 jobs, whereas it has no effect on employment in other parts of the tradable sector. A similar study compares US figures to those for Sweden, here the multiplier effect amounts to 48 jobs for 100 additional jobs in the tradable sector (Moretti and Thulin, 2013). In contrast to our focus here, Moretti's definition of the non-tradable sector specifically excludes government jobs (along with those in agriculture, mining and the military). Thus, his papers are mainly concerned with multiplier effects between tradable and non-tradable components of the private sector. Looking at the openings of Wal-Mart stores at the US county level, Basker (2005) estimates large and positive

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[1] In the macro-economic literature, a limited number of studies use OECD country data and look at the potential impact of public sector employment on labor market outcomes (e.g., unemployment and private employment), often finding contradictory results. Whereas Edin and Holmlund (1997) show that a rise in public sector employment reduces unemployment, Boeri *et al.* (2000) and Algan *et al.* (2002) find the opposite effect as public sector employment in the long-run destroys private sector jobs.

direct effects on retail employment in the first year of entry, which are cut in half after five years. She detects no spillover effects in retail sectors in which Wal-Mart does not compete directly.

In addition, our work is related to studies that use German division and reunification as historical natural experiments and examine their impact on the spatial distribution of economic activity.[2] Redding and Sturm (2008) exploit the division of Germany after WWII and the reunification of East and West Germany in 1990 to examine the changes in market access for the growth of West German cities. Redding *et al.* (2011) explain the relocation of Germany's air hub from Berlin to Frankfurt in response to the country's division after WWII as a shift between multiple steady-state equilibria. Ahlfeldt *et al.* (2015) develop a quantitative model of internal city structure that accounts for the observed changes in the location of economy activity within West Berlin following the city's division and reunification. As already mentioned, Becker *et al.* (2015) is another study of this kind.

Moreover, the paper contributes to a growing literature documenting the rapid spatial decay of agglomeration effects. 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 gains from the co-location of advertising firms in Manhattan, but these gains attenuate rapidly over space. Turning to Sweden, Andersson *et al.* (2004, 2009) investigate the impact of university decentralization and find

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[2] Several studies examine the impact of historical natural experiments on the location of economic activity: e.g., Hanson (1996, 1997) using Mexican trade liberalization; David and Weinstein (2002, 2008) using bombing of Japan in WWII; and Kline and Moretti (2014) using the creation of the Tennessee Valley Authority. Here, we focus on those using German division and reunification.

substantial but highly localized spillovers on firm productivity over distance. Ahlfeldt *et al*. (2015) find highly localized production and residential externalities using within-Berlin census block data.

Our study is of considerable interest for policy makers. It gives evidence on the suitability of public sector relocation programs to address local employment problems. Despite the fact that governments frequently use these policies, evidence of their impact is scarce. The project also helps to understand uneven spatial effects of changes in public sector employment, which are relevant for both public sector job creation and destruction – another highly debated topic. Furthermore, our project is highly relevant as a case study. The 1994 Bonn-Berlin Act specified that 50% of government employees have to remain in Bonn. Whether the law should be changed is frequently debated. This study helps to shed light on the possible impact for Bonn and Berlin should the German government decide to move the remaining ministries.

The remainder of the paper is organized as follows. Section II gives an overview of the historical setting and provides details on the relocation. Section III discusses our empirical strategy. Section IV describes the data underlying the analysis and their sources. Section V presents the results and Section VI concludes.

II.   Historical setting

A. Overview

When Germany lost WWII, the country was divided into four sectors administered by the Four Powers, namely the US, Russia, France and the UK. Similarly, the city of Berlin, which was the capital of Germany between 1871 and 1945, was jointly occupied. The cooling of the political climate between the Western powers and Russia led to the division of Germany in 1949 into the Federal Republic of Germany (FRG) and the German Democratic Republic (GRD). Berlin was claimed by both states resulting in the situation shown in Figure 2. Though Berlin was located within the GDR boundaries about 130 kilometers away from the West German territory, the West-

Berlin zones occupied by the US, France and the UK became part of the FRG. Conversely, the East-Berlin zone occupied by Russia became part of the GDR. From the West-German perspective the former capital was isolated and therefore unsuitable for government functions. Under the promise that Berlin would be made capital again as soon as the political situation changed, Bonn was chosen as the new capital and seat of the FRG government.

This 'provisional' situation lasted until reunification in 1990. A clause in the Unification Treaty (1990) signed by the GDR and the FGR agreed on making Berlin again capital of united Germany. A year later it was decided to move the seat of the government back from Bonn to Berlin. The decision was unpopular and could only be reached by making large concessions to the city of Bonn, to compensate for the loss of status and economic power. Part of the agreement was a 'fair division of labor' between Berlin and Bonn, implying that core government functions should take their seat in Berlin but the majority of government jobs should remain in Bonn. Additionally, Bonn should receive financial compensation as well as new functions and institutions of national and international significance.[3] The initial plan was to move the government to Berlin within four years and to fully complete the move within at most twelve years (Deutscher Bundestag, 1991a).

Details on the implementation of the move were left open. By 1992 it became clear that moving the core government functions within four years was unfeasible. What followed was a long discussion about the timing of the move and its costs. One motion proposed to stop any further government related investment in Berlin until the financial situation of the FRG had improved. Another proposed to postpone the move until 2010. Furthermore, a mass petition was organized to

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[3] The ability of Bonn to secure large (financial and non-financial) concessions as a form of compensation for the loss of status and economic power makes it difficult to estimate the impact of the move of the seat of the government on Bonn. In addition, there are data limitations. With only 17 postcode areas (the smallest geographical unit available in our data) in Bonn, it is unlikely that our identification strategy for local shocks would work.

suspend a decision about the date of the move until the government had full knowledge of the costs and the financial situation of the state (Bund) and federal states (Länder) had improved (Deutscher Bundestag, 2010). The dispute created uncertainty among private companies that had started to invest in Berlin. In November 1993, 40 national and international companies pointed at the breach of trust should the government cease its effort to press ahead with the move (Hoffman, 1998, p. 213).[4]

The passing of the Berlin-Bonn Act (1994) provided statutory security about the move to Berlin. Though it did not specify a concrete moving date, the act determined important details of the implementation of the move such as the definition of a 'fair division of labor' between Berlin and Bonn and concrete compensatory measures for the former capital. Six ministries should keep their first seat in Bonn[5] and get a second seat in Berlin; nine ministries should take their first seat in Berlin and keep their second seat in Bonn. Additionally, it was decided that the majority of ministerial positions were to remain in Bonn. It was only by November 1997 that the Federal Parliament (Bundestag) announced a moving date. The government was to take up its work in

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[4] Since some company headquarters were relocated to the centre of Berlin after the government decision, we are concerned that their presence could drive our results. As a robustness check, we re-estimate our model focusing on SME employment only. Results do not change (see Section V.D).

[5] These six ministries are as follows: the Federal Ministry of Defense (BMVg); the Federal Ministry of Health (BMG); the Federal Ministry of Food, Agriculture and Consumer Protection (BMELV); the Federal Ministry of Economic Cooperation and Development (BMZ); the Federal Ministry for the Environment, Nature Conservation and Nuclear Safety (BMU); the Federal Ministry of Education and Research (BMBF).

Berlin in September 1999. Until then the timing had remained heavily debated.[6] A timeline summarizing the core events of the decision making process is shown in Figure 1.[7]

B. The magnitude of the relocation

The Bundestag and the government officially started their work in Berlin in September 1999. Table 1 gives an overview of the number of jobs relocated from Bonn to Berlin. In total, the move involved about 15,000 government-related jobs. An additional 10,000 positions related to foreign representations, the media, political parties and interest groups followed suit. At the same time, Berlin experienced significant outflows of public sector jobs. Following the recommendations of a commission established to oversee the redistribution of federal offices across the federal states that used to be part of the GDR ('new Länder'), several Berlin-based institutions left the city and relocated in these new Länder. Berlin also lost several of its institutions to compensate Bonn for its employment losses (see Figure 2, right panel). Adding up positive and negative job moves results in a net gain of about 18,000 jobs for Berlin. Those jobs did not correspond to the number of relocated workers as employees were given the choice whether they would like to follow their job or take up a position in one of the federal institutions that remained in Bonn or were relocated to Bonn as part of the compensation measures.[8]

C. The timing of the relocation

The relocation period was spread across several years, though the majority of jobs had moved by the official relocation date or shortly after as can be seen in Figure 3. Government

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[6] In September 1996, for instance, 50 MPs belonging to the SPD and BÜNDIS 90 / DIE GRÜNEN brought in the motion to postpone the move by at least 5 years.

[7] A more detailed description of events is provided in Appendix A.1.

[8] Approximately 34% of government employees decided to stay in Bonn; most of them were public sector workers of lower or middle grade (Deutscher Bundestag, 1999).

employees largely moved between 1999 and 2000 (see Figure 3, Panel C). By the end of 2000, more than 8,000 ministerial employees as well as about 5,300 employees of the administration of the federal parliament, parliamentary groups or deputies and their assistants were located in Berlin. All federal states equally established a representation in Berlin. The majority of embassies chose to be present in Berlin when the government took up its work in 1999 and many more arrived in the following years (Panel A). Some of them had made short-term arrangements to accommodate their staff such as renting offices or using facilities of their consulate generals and branch offices as well as former military missions until they could rebuild or construct a suitable building for their representations (Gehrcken, 2013). The relocation of institutions out of Berlin is comparatively spread, taking place between 1996 and 2003 (Panel B).

D. Location decisions within the city of Berlin

Location decisions for government institutions were heavily debated. While the airport of Tempelhof was proposed as a possible site for parliament due to the availability of unbuilt land, the final site was chosen for historical reasons. To keep the costs of the move as low as possible, many ministries were accommodated in the existing housing stock. As noted above, embassies used the facilities of their consulate generals, branch offices or former military missions. Although the diplomatic building stock in West-Berlin had been almost entirely destroyed between 1939 and 1945[9], many countries still owned parcels of land in Berlin that they had purchased more than a century ago. The former embassies in the GDR located in Berlin-East had closed in 1990 and were used as consulates. Some reopened later on as representations in the FRG. By 2015, 163 states are

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[9] During the construction works for the capital 'Germania' under the Nazi regime, several embassies had been demolished. For some the planned reconstruction never materialized as diplomatic relations broke off during WWII. In addition, severe bomb attacks destroyed a large number of buildings in the Tiergartenviertel, the neighborhood where embassies were historically located (Fleischmann, 2005).

represented in Berlin by 158 embassies and 5 honorary consulates. It was therefore mainly historical factors that determined the location patterns of the government and related institutions. This institutional detail is very important for our empirical analysis. Although relocated jobs were not randomly distributed across space, the choice of government buildings was not related to pre-existing economic and social conditions of postcode areas within Berlin. Instead, it was largely driven by historical considerations, i.e. by the desire of the government to occupy (where possible) buildings of historical importance (see Section III for more details). Table 2 reports descriptive statistics between the groups of Berlin postcodes receiving and non-receiving relocated jobs during the period 1999 and 2001. The table shows that receiving postcodes (in the number of 38)[10] have a higher number of jobs and characterized by larger shares of female, part-time and highly qualified workers, especially professionals. Conversely, the age structure of the workforce as well as the proportion of managers and skilled workers are similar between the two groups of receiving and non-receiving postcodes.

III.   Empirical Strategy

We are interested in the change in private sector employment after the relocation of public sector jobs to Berlin. A change in private sector jobs can either arise through existing firms hiring or laying off workers or firms entering or leaving an area within the labor market. To capture both channels, we aggregate firm level data up to the postcode level, which is the lowest level of geography available in our data.

Studies that look at the effect of job relocations are complicated by two factors. First, the geographical spread of the policy is unknown *a priori*. Second, locations are not randomly chosen.

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[10] Since we focus on net changes, receiving postcodes include 30 postcodes affected by a positive public sector employment shock and 8 postcodes affected by a negative shock.

To deal with these issues, we construct a measure of treatment intensity that is a non-parametric function of the distance to a relocation site. Adapting from Gibbons *et al.* (2011) and Faggio (2015), we construct treatment intensity variables as the number of relocated jobs within each postcode as well as the number of relocated jobs in subsequent 1km-wide distance bands starting from the postcode boundary. This approach allows the relocation of public sector institutions and public sector jobs into an area to affect neighboring areas and its impact to differ by distance. For instance, local businesses like cafés, restaurants or other local services might benefit from a higher number of customers in the area and thus hire more workers. It might also be the case that firms offering consulting or legal services decide to relocate after the intervention in order to benefit from proximity to the public sector. As a consequence, the number of jobs would increase in areas close to public sector institutions and would decrease in areas further away.

As noted before, to the extent that relocations of public sector workers are used as policies targeted at improving local employment conditions, locations are not randomly chosen. This has consequences in terms of identification. We argue that this concern is much weaker in our analysis than in related studies as the relocation of the government from Bonn to Berlin was not aiming at improving local economic conditions in specific neighborhoods. As discussed in Section II, location decisions were largely determined by historical factors. The seat of the government was chosen to be located next to the former Reichstagsbuilding, embassies moved to their own branch offices and built on their own parcels of land dating back to the time of the German empire or (in the former East) dating back to the 1970s when the GDR was internationally acknowledged. Federal institutions selected to compensate Bonn for its loss of economic status and employment were chosen to allow government employees who did not want to move to Berlin to keep a similar

position in Bonn (Bornhöft *et al.*, 2001).[11] Their specific location in Berlin was not a factor in the decision making process. Still, due to the sheer size of the relocation, one of the necessary conditions was the availability of a sufficiently large number of offices or buildings suitable to be converted into offices and land suitable for the construction of the main government buildings. In our empirical analysis, we partly address these issues by controlling for time-invariant postcode characteristics and for differences in pre-existing employment trends.

We 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, the approach combines (or replaces) a treatment dummy, typical of a standard difference-in-differences approach, with treatment intensity variables. 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:

$$\Delta priv_{p,t} = \beta^0 \Delta pub_{p,t}^0 + \sum_{d=1}^{6} \beta^d \Delta pub_{p,t}^d + \sum_{j=1}^{J} \delta^j X_{p,t-n}^j + \Delta \varepsilon_{p,t} \qquad (1)$$

where Δ denotes a long lag operator, i.e. $\Delta priv_{p,t} = priv_{p,t} - priv_{p,t-n}$. Specifically, $\Delta priv_{p,t}$ refers to the change in private sector employment between time *t* and *t-n* in postcode *p*; $\Delta pub_{p,t}^0$ denotes the number of public sector jobs a postcode *p* received between time *t* and *t-n*; and $\Delta pub_{p,t}^d$ refers to the number of jobs relocated a postcode *p* faces within distance band d, with $d \geq 1$. Since a postcode *p* could be hit by positive and negative flows of public sector workers during the sample period, both $\Delta pub_{p,t}^0$ and $\Delta pub_{p,t}^d$ measure net employment changes. Equation (1) is augmented by

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[11] For example, the Federal Competition Authority (Bundeskartellamt) was relocated to Bonn to provide alternative employment to employees of the Ministry of Finance. All these location decisions were independent of the local employment situation in Berlin.

a set of control variables denoted by the vector $X_{p,t-n}^{j}$, which includes the log of total employment, the shares of female and part-time workers, the age structure of the workforce, the distribution of occupation and education groups. All control variables are measured at the initial level $(t\text{-}n)$. $\Delta\varepsilon_{p,t}$ refers to the error term.

In the main specification, the outcome variable ($\Delta priv_{p,t}$) refers to the raw changes in private sector employment between 1998 and 2002, i.e. $t = 2002$ and $t - n = 1998$. As shown in Figure 3, net public sector job turnover (see Figure 3, Panel D) in Berlin was highest in 1999 and 2000. Thus, the chosen interval corresponds to the years just before and after the most intensive treatment period. A short time span is also of advantage, especially in the case of Berlin which has been going through a deep structural change during the 1990s and 2000s, making it less likely that ongoing trends could confound our estimates.

The first treatment variable ($\Delta pub_{p,t}^{0}$) identifies the treatment effect on postcodes that received public sector jobs during the 1999-2001 period relative to areas that did not receive such jobs. This is equivalent (apart from measuring the size of the relocation rather than the status of being treated) to a typical treatment dummy in a standard difference-in-differences approach. If the analysis focused exclusively on this variable, it would only capture the policy impact on receiving areas. By introducing treatment intensity variables, the analysis allows for potential spillovers effects into neighboring areas to be identified.

Treatment intensity variables ($\sum_{d=1}^{6} \beta^{d}\Delta pub_{p,t}^{d}$) capture indirect or spillover effects by counting the number of relocated jobs within distance bands. Faggio (2015) constructs 1km-wide distance bands starting from an area centroid; we construct 1km-wide distance bands starting from the postcode boundaries. This modification is necessary in order to take into account that postcode areas in Berlin largely vary by size. To avoid collinearity problems, a second modification proves necessary. We treat the two central postcodes (10117 and 10557 in Berlin Mitte) as "seed" locations

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only, i.e. they are affected by the number of jobs they receive (direct effect), but they are not affected by the number of jobs relocated in surrounding postcodes (indirect effect). Given the intensity of the direct treatment these two postcodes received (see Figure 4), spillovers are less likely to explain employment growth within these postcodes.

Using ArcGIS we construct five subsequent 1km-wide rings and an additional ring ranging from 5km to 15km. In doing so, we aim to detect any effect within the first five kilometres from a postcode boundary while covering the whole surface of Berlin. By geocoding the exact location of each institution that received public sector jobs, we can count the number of jobs that fall into each ring. Thus, we make the assumption that the effects are additive. We then measure treatment intensity variables as the interactions between distance and size, where size refers to the number of jobs moved.

Our treatment intensity variables are constructed in a cumulative way: $\Delta pub_{p,t}^1$ refers to all relocations (and the associated number of jobs moved) within a 0-1km distance band, but outside the postcode boundary.[12] The comparison group consists of postcodes that do not face relocations within the 0-1km band, but face relocations at a greater distance. $\Delta pub_{p,t}^2$ refers to all relocations within a 0-2km distance band (which also includes relocations considered in the 0-1km ring) relative to areas that face relocations further away. The comparison group now consists of postcodes that do not face relocations within the 0-2km band, but face relocations at a greater distance. $\Delta pub_{p,t}^3$ refers to all relocations 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. The sixth ring ($\Delta pub_{p,t}^6$) refers to all relocations within a 0-15km distance band from the postcode boundary. We make sure we include all jobs moved such that our rings are exhaustive.

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[12] In other words, Δpub^0 is not included in Δpub^1 (and subsequent Δpub^d).

Using separate 1km-wide (rather than cumulative) rings in Equation (1) would lead to the same findings. The actual estimates in the two specifications would be different, but it would be possible to derive estimates of one specification from those of the other. The main advantage of using a cumulative definition of treatment intensity variables is that it simplifies the comparison across coefficients and thus the interpretation of the results. Each coefficient (β^d) captures the marginal effect of each additional treatment intensity d included in the estimation. No *t*-test between β^d and β^{d-1} would be necessary in order to verify whether the impact of the two treatment intensity variables are significantly different from each other.[13]

In Equation (1), we estimate the effects of changes in the number of public sector jobs on changes in the number of private sector jobs, when those changes occur simultaneously. In the robustness section, we explore the possibility of long-term effects by looking at differences over a longer time period.

While our method is capable of identifying a localized public employment multiplier and its spatial reach, it cannot distinguish between jobs moving within Berlin and jobs being filled with

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[13] Consider cumulative distance bands defined as:

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

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

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

Because:

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

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

The cumulative specification gives coefficients which capture the direct impact of each band (b1, b2 and b3) on the variable of interest. Conversely, the non-cumulative specification of distance bands needs to be followed by *t*-tests on the equality of the coefficients for the different distance bands.

workers from outside Berlin or from the formerly unemployed. Thus, strictly speaking, we cannot test whether overall employment in Berlin is higher with the government move than in an alternative scenario without. We experimented with the synthetic control group approach in order to find the effect on total private sector employment. This method requires a long time series, however, which is only available for Western Berlin and not for the former East. Also, as the years after 1990 were strongly influenced by reunification, the construction of a reasonable control group from other German cities is unfortunately not possible for that time.

IV.   Data

A. Sources

Information on employment is retrieved from the weakly anonymous Establishment History Panel (EHP) (see Gruhl *et al.*, 2012). The dataset is assembled by the Institute for Employment Research (IAB) and holds information on a 50% random sample of all German establishments employing at least one worker on social security records. The time span of the panel ranges from 1975 to 2010 for former West-Germany and from 1991 to 2010 for the New Länder. The data include information on the total number of employees for each establishment and the number of employees in each of the following categories: age band, gender, qualification class, occupation (1 digit) and nationality. Additional variables include date of market entry and exit as well as a time consistent industry classification code (3 digits).

For this project, we focus on Berlin and enhance the data set by additional postcode information.[14] All Berlin establishments were linked to one of 190 Berlin postcodes, which represent our unit of analysis. Observations for two postcodes had to be dropped because of

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[14] The enhanced dataset is not part of the standard data portfolio of the Research Data Centre. Access for replication studies is possible after successful application. Contact the RDC at iab.fdz@iab.de for further advice.

confidentiality reasons as the specific postcodes contained less than 20 establishments. Although we have establishment level data, worker and address information is not available for every branch. More specifically, the procedure for the allocation of establishment IDs in the German Social Security System demands that firms pool together all branches (i) located in the same municipality and that (ii) operate in the same Economic Class according to the Standard Classification of Economic Activities. As an example, multiple branches of the same supermarket chain within Berlin show up as just one establishment with one address in our data, presumably the one of the head office. This may cause measurement error in the spatial distribution of jobs across postcodes. If the head offices were largely located in the city centre but branches were spread across peripheral areas, we would overestimate employment in the centre. While we do not have a good estimate of how large this measurement error is, we acknowledge that it might be non-negligible.[15] We note, however, that it should be relatively stable over time and arguably more closely linked to centrality *per se* than the specific distribution of public sector jobs in Berlin. Section V.E on robustness checks provides some evidence to support these claims.

The official start of government activities in Berlin was September 1, 1999. Estimates of the total number of jobs that were destined to relocate were frequently cited in the media as well in the general discussion in Parliament. For our analysis, this information is indicative, but of little concrete use as the relocation of the government and related institutions along their employees was

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[15] According to the IAB Establishment Panel, an annual representative survey of establishments in Germany, about 3 percent of establishments in Berlin, representing roughly 20 percent of the workforce, are head offices or middle level authorities with subordinate branches. Unfortunately, there is no distinction into branches within and outside of Berlin (branches in other cities or countries do not bias our distribution of jobs). Keeping in mind that it is attractive for firms operating nationally or internationally to have their headquarters located in Berlin, we find it most likely that the share of jobs in our data that are potentially incorrectly located is much smaller than 20 percent.

spread over a much longer time span and information on the spatial distribution of these jobs within Berlin was not provided. Due to lack of official sources on public sector employment, we embarked in an extensive data collection exercise, gathering information on three main variables: first, the number of jobs of each relocating institution before and after the move; second, the year the institution moved in or out of Berlin; and third, the new address of the institution in Berlin or the former address in Berlin of those institutions that were relocated to Bonn and the New Länder. We also gathered information on the number of government employees working in Berlin in 1997, 1999, 2001 and 2004. This demanding data collection exercise involved the use of official documents (e.g. BT-Drucksachen); nationwide newspapers (e.g. the *Spiegel*); and local newspapers (e.g. the *Berliner Zeitung*, the *Generalanzeiger*).

We used lists of diplomatic staff published by the Ministry of Foreign Affairs (Auswärtiges Amt) to estimate the number of embassy personnel. From these documents, we retrieved the number of diplomatic staff in Germany in 1996 and use it as the pre-treatment level. As the documents do not contain any information on members of the embassies covering administrative or technical support positions, we had to make some assumptions to estimate the total number of embassy personnel. We attribute the number of relocated jobs to a Berlin postcode by checking the address (in official registers) of the institution receiving employment in a specific year. In the case of an institution having multiple sites we attribute all employment to the main address.[16] Section A.2 in the appendix provides an overview of the data sources used and more detailed information on the estimation procedure for the embassy personnel.

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[16] Latitude and longitude are added to the data using the online georeference tool provided at: http://www.gpsvisualizer.com/geocoder/

Figure 4 shows the distribution of the number of jobs relocated between 1999 and 2001 across 190 postcodes in Berlin. The map shows aggregate numbers of relocated jobs for each postcode. Postcodes differ in size, the median postcode measures about 3 km^2, the smallest postcode is about 0.4 km^2, the largest is about 35 km^2. 30 out of 190 postcode received a positive public sector employment shock, eight postcodes received a negative shock. Relocated employment per postcode ranges from -813 employees in the postcode that lost an important federal institution to about 5,200 employees in a postcode in 'Berlin Mitte', the most centrally located district in Berlin, where several ministries and embassies are clustered. The number of treated postcode in West-Berlin is higher than in the East.

The number of relocated jobs on either side of the former line of the Berlin Wall is balanced though, as the central postcode with the highest number of received public sector workers lies in the East. All federal institutions that were moved out of Berlin were geographically spread across West-Berlin as they fulfilled administrative tasks for the FRG during the time of division. The eight postcodes that lost public sector workers are therefore spread across the western postcodes of the city.

B. Plausibility check

We first undergo a plausibility check to verify the consistency between the addresses and employment data we collected through independent sources and EHP data on public sector employment. Instead of using the change in private sector employment as the outcome variable in a specification similar to Equation (1), we regress the (1998-2002) changes in public sector and

special interest group employment on the number of relocated jobs related to the government move and collected through independent sources.[17] Results are presented in Table 3.

Table 3 is organized as follows: Columns (1) and (6) report baseline results without controls; Columns (2) and (7) include pre-treatment postcode characteristics; Columns (3) and (8) include both area characteristics and pre-trends. Pre-trends are defined as the (1995-1997) changes in public sector (Column 3) and special interest group (Column 8) employment. Columns (4) and (9) augment the specification with a variable measuring the closest distance of the postcode centroid to the Berlin Wall; Columns (5) and (10) add a location dummy for West Berlin. Looking at Table 3 (Columns 1-5), results indicate that postcodes to which we attribute relocated jobs experienced a significant increase in employment in public administration (SIC75) and foreign representations (SIC99). If the EHP data comprised all public sector jobs, we would expect a coefficient equal to one, i.e. each relocated job in postcode p would increase public sector employment in that postcode by one. This cannot be the case because civil servants and foreign embassy personnel are not subject to German social security regulations and, thus, they do not appear in the EHP.[18] It is therefore reasonable to find a coefficient smaller than one (0.802 in Column 5). The plausibility test confirms that public sector employment as measured by employees in SIC75 and SIC99 registered in social security rolls increased in those areas that received government jobs. As expected, there is no evidence of spillover effects. Receiving public sector workers in surrounding buffers has no impact.

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[17] Special interest group employment is identified as employment recorded in codes 870, 871, 872 and 881 of the 1973 standard industry code (SIC) classification of economic activities (3-digit). Public sector employment is identified as employment recorded in codes 75 and 99 of the 1993 SIC classification (2-digit).

[18] While some public sector jobs (Angestellte) are subject to German security regulations, others (Beamte) are not.

We were concerned about attributing all relocated jobs to an institution's primary address because of the potential measurement error that could derive. This plausibility check seems to confirm that no major spatial measurement error has been introduced. One possible explanation is that the majority of jobs might have targeted the institution's primary location with a small fraction moving to secondary locations. Despite this, our data might still suffer from other forms of measurement error. For instance, the estimated number of jobs in each embassy is based on a number of assumptions. This error will tend to bias our estimates towards zero. The estimates in the results section therefore represent lower bounds of the actual effect.

We also use Equation (1) to analyze the effect of government relocations on private organisations that are directly linked to government activities such as lobbying organisations and trade unions. These organisations can be identified looking at the industry codes SIC870-SIC872 and SIC881 (1973 SIC) in the EHP.[19] The media has widely documented that most interest groups moved from Bonn to Berlin along with the government. Looking at Table 3 (Column 10), a coefficient of 0.040 (s.e. 0.020) suggests that the arrival of 100 public sector jobs in a postcode triggers the creation of about 4 jobs in lobbying organisations within the same area. It also seems that the inflow of public sector jobs in an area triggers the creation of about 5-6 jobs in lobbying organisations in areas within 1km distance from the boundary of the receiving postcode. Nevertheless, these spillovers effects are not statistically significant. These results indicate that interest groups tend to locate in relatively close proximity to government institutions. Given the aim of lobbying organizations to influence the political process, our findings are as expected.

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[19] This classification encompasses activities of business, employers' and professional organizations; activities of trade unions; and activities of other membership organizations.

To summarize, evidence presented so far seems to confirm the validity of our data collection exercise. There is good correspondence between the postcodes we have identified through independent sources and postcodes that registered a surge in public sector employment and lobbying organisations during the government move. In addition, we do not find evidence of spillovers in public sector employment. We are thus confident that the data we collected and the addresses we attributed to each institution give a realistic description of the spatial distribution of the actual employment shock.

V.   Empirical Analysis

A. Main results

Table 4 shows estimation results of Equation (1) for the change in total private sector employment between 1998 and 2002. Table 4 is organized in the same way as Table 3, with Column (1) reporting the baseline specification and Column (5) a specification with the full set of controls. Results indicate that there is a large direct effect, which is about 0.52-0.56 across all specifications. That is, the arrival of 100 public sector jobs in an area creates about 52-56 additional jobs in the private sector within that area. Looking at the point estimates, the coefficient is not statistically significant in Column (1), whereas it gains significance as we include initial postcode characteristics in Column (2). This confirms the importance of controlling for initial employment size and other pre-treatment characteristics in our estimation. We also find evidence of spillover effects. Focusing on the 0-1km coefficient in Column (5), our estimation allows two interpretations. First, the inflow of 100 public sector jobs into an area triggers the creation of 36 jobs on average in each of the postcodes that are located within the 1km-wide ring surrounding the postcode boundary. Thus, we measure spillover effects going from the receiving postcode to neighboring areas. Second, the arrival of 100 public sector jobs into an area within the 1km distance ring triggers job creation in neighboring postcodes. One of them will be the postcode relative to which we have

constructed the ring. In this second interpretation, spillover effects go from the ring to the central postcode. Job relocations more than 1km away have no additional effect.

The policy impact comes entirely through job creation in the service sector. Table 5 shows results for the split of total private employment into manufacturing and services.[20] Looking at Column (1), there is a small negative effect of public employment on manufacturing, but the impact turns insignificant when we include initial area controls. Columns (2)-(5) report no significant direct or spillover effects apart from a small negative impact in postcodes located on the outskirts of the city. Conversely, job changes in services (see Table 5, Columns 6-10) closely mirror those reported for total private employment. They are strong in (direct effects) and around (indirect effects) the receiving postcodes.

To understand which service sub-sectors benefit from the relocation of public sector employment, the partition is refined in Table 6. For each sub-group, the specification with the full set of controls (equivalent to Column 10 in Table 5) is reported. The arrival of public sector workers into an area might spur demand for local activities through a multiplier effect, both in terms of intermediate demand for consultancy and legal work and/or in terms of consumer demand for catering and personal services. We find evidence for both channels: the relocation of public sector institutions increases employment in business (including consultancy, legal and professional services) and real estate as well as employment in trade and personal services (comprising hairdressers, dry cleaners, etc.). The most important channel for the employment multiplier is business-to-business activities. Out of the total 56 jobs that are created in the immediate proximity to a relocation site (see Table 5, Column 10), 27 jobs are created in the business sector as shown

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[20] The manufacturing sector in Berlin was much smaller than services and also relatively declining in the period under study. It went from 22.5 percent of the size of the service sector in 1998 to only 20 percent in 2002.

in Column (7); 3.2 come from the real estate sector (Column 6); 4.4 through trade (Column 2); and 1.6 through personal services (Column 9).

B. Comparison with existing estimates of the agglomeration literature

Our findings are consistent with those found in the literature. Focusing on short-run effects (2003-2008), Faggio (2015) finds that the arrival of 100 public sector workers into a UK Census Output area triggers the creation of 55 new jobs in the private sector, with service activities benefiting the most from the relocation program. Similar to what we observe here, she also finds that the positive impact on services is largely driven by increases in business & real estate as well as trade & catering activities. Looking at the impact of public sector expansions in England over the period (2003-2007), Faggio and Overman (2014) find no short-term effects of public employment on private sector growth in a local labor market. However, they detect a differential impact on tradable versus non-tradable activities. Each additional public sector job creates 0.5 jobs in the non-tradable sector (construction and services) while crowding out 0.4 jobs in the tradable sector (manufacturing). Analyzing the long-run effects of public sector expansions on Spanish city growth, Jofre-Monseny *et al*. (2016) find no impact on tradables, a multiplier effect of 1.1 on non-tradables and a resulting impact on total employment of 1.3. A multiplier effect of 1.6 was also found by Moretti (2010) when exploring the long-run impact of manufacturing employment on services across US cities. Moretti (2010) also estimates multipliers of 2 when analyzing the impact of skilled manufacturing employment on skilled service jobs.

Consistent with a growing literature (e.g., Ahlfeldt *et al.*, 2015; Andersson *et al.*, 2004, 2009; Arzaghi and Henderson, 2008; Rosenthal and Strange, 2003) documenting the rapid spatial decay of agglomeration effects, our results also show that multiplier effects are highly localized: they are concentrated within the postcode area receiving the relocated jobs and the 1km-wide buffer surrounding the postcode boundary. There are no effects beyond the 1km-wide distance band. In

the case of New York City, Arzaghi and Henderson (2008) show that advertising agencies enjoy substantial knowledge spillovers from close proximity to other advertising agencies, but such effects attenuate sharply beyond a 500 metres straight-line distance. Looking at the case of historic Berlin, Ahlfeldt *et al.* (2015) estimate the impact on the productivity of a West Berlin block on spillovers from East Berlin, both for the division and reunification experiments. They find evidence of substantial production and residential externalities that are highly localized and dissipate rapidly with distance. After 10 minutes of travel time (corresponding to about 0.83 kilometers on foot at the average speed of 5km per hour), production and residential externalities fall to a value close to zero.

C. Falsification test

Berlin has experienced several dramatic changes during the last decades. The fall of the Berlin Wall in 1989 and the subsequent reunification of East- and West-Germany had a strong effect on the Berlin economy. The move of the government took place less than 10 years after these events during which Berlin went through a phase of economic and social transformation. We therefore undertake additional checks to show that the multiplier effect we find is due to the relocation of the government and not to the ongoing transformation processes that were triggered by the reunification. To rule out the possibility that the effect we find is due to spatially differentiated employment growth that happened to coincide with the sites of the government, we apply Equation (1) to the time period before the move of the government took place. If reunification and the subsequent redevelopment of the central areas close to the Berlin wall were the drivers of the employment dynamics we identify in our specification, the effect should already be present in the period before the government relocation. In the falsification test we show that this is not the case. Ideally we would like to use a time period before the decision of the move of the government

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was made. However, as shown in Figure 1, this date coincides with the reunification and before 1991 there are no data available for East-Berlin.[21]

As discussed in the historical section, the timing of the move was very uncertain until the very last moment due to the strong opposition against the relocation project by both politicians and the general public. It was only in 1997 that a final moving data was announced. We, therefore, use the period before the decision on the final moving date for our falsification test, i.e. we use private sector employment growth between 1995 and 1997 as the dependent variable in our specification. This specification still allows us to control for pre-existing trends in employment change and we include 1993-1994 differences as a control.

Results for private sector employment, manufacturing and services are shown in Table 7 (Columns 1-3). We do not find evidence of stronger employment changes in postcodes that were to receive public sector institutions in later years. Before the move of the government to Berlin, the treatment intensity variables had no effect on changes in the private, manufacturing or service sector. As for the main multiplier effect we split the service sector according to different industries (see Table 7, Columns 4-7). It is notable that the positive effects on employment changes in business, real estate and personal services in the postcodes that would receive the relocated jobs are not present in the years prior to the implementation of the program. Overall, the coefficients we report in Table 7 are small in size, mostly statistically insignificant and do not seem to follow a meaningful pattern (differently from what shown in Tables 4-6).

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[21] We exclude years 1991 and 1992 from the analysis because of data problems relating to the introduction of the employment notification procedure in the federal states of Eastern Germany.

D. Distance to the centre

An important concern about our analysis is the issue of centrality. After reunification several companies opened subsidiaries in Berlin during the 1990s and the 2000s, and within Berlin, chose the central locations as their preferred destination area. As documented in Section II and Figure 4, the largest employment shock due to the government move also occurred in the postcodes located in the centre of Berlin. Because of these parallel developments, our estimates could confound the impact of the government move with the expansion of the city centre.

To shed some light on these issues, we conduct two robustness checks about the role of centrality. First, we replicate our analysis as reported in Tables 4 and 5 substituting our measure of distance to the Wall with measures of distance to the CBD of Berlin. We use four different definitions of the CBD because Berlin is a polycentric city: (i) distance to the Bundestag, which is the new political centre; (ii) distance to Alexanderplatz, the main centre of the former East; (iii) distance to Postdamer Platz, a newly emerged commercial centre; and (iv) distance to Kudamm, the main centre of the former West. When analyzing the impact of Berlin's division (and reunification) on house price gradients in Western blocks, Ahlfeldt *et al.* (2015) point out that it is not proximity to the Berlin Wall *per se* that matters, but the loss of access (and regained access) to the pre-war CBD. In fact, they find little evidence of a negative treatment effect of division along segments of the Berlin Wall far from the pre-war CBD. In our case, the adoption of different measures of distance to the CBD does not alter our main results (see Table 8). The impact of public employment remains large, positive and statistically significant for total private sector employment and services in areas receiving and in close proximity to a relocation site. There is essentially no impact on manufacturing employment. In addition, none of the distance-to-the-centre dummies enters the estimation significantly, as was the case with the distance-to-the-Wall dummy.

Second, we quantify the importance of centrality over time by regressing 5-year growth rates (1992-1997; 1997-2002; 2002-2007) in total private sector employment on one of our distance-to-the-centre measures, i.e. distance to the Bundestag.[22] A negative correlation between employment growth and distance to the CBD would indicate that employment growth is higher in postcodes closer to the city centre than in periphery areas. A stronger (weaker) correlation for later rather than earlier periods would suggest that the importance of Berlin city-centre increases (decreases) over time. Looking at Table 9, results show that there is indeed a negative correlation between private employment and distance to the CBD confirming the presumption that, during the 15-year period considered, economic activity has expanded at higher rates in central postcodes than peripheral areas. Furthermore, results show that the importance of centrality for private job expansion decreases over time. Looking at the weighted regression with initial controls (Table 9, Column 6), the coefficient is largest and statistically significant (coeff. -0.016; s.e. 0.005) in the initial period (1992-1997), shrinks by about 40% in the middle period (1997-2002) and becomes 12.5% of what it was initially (turning statistically insignificant) in the later period (2002-2007).

Taken together, the robustness checks presented in this section indicate that the risk that our estimates could confound the impact of the government move on private activity with the expansion of the Berlin city-centre is low. Measures of the distance to the CBD do not affect our analysis. Moreover, the importance of centrality in explaining the city expansion decreases over time, being indeed weaker during the government relocation program.

E. **Further robustness checks and extensions**

As mentioned in the data section, another concern that might impact our findings is that we attribute addresses of multi-site firms to the address of the headquarters. There might be a tendency

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[22] Results (available upon request) do not change if other distance-to-the-centre measures are used.

that this address is in the centre of Berlin, where the public employment shock is largest. As a result, private sector employment changes in proximity to public sector buildings might seem larger than they actually are, given that the true change is spread across multiple sites. In order to isolate the impact of larger firms from smaller ones, we replicate our analysis restricting our sample to private sector firms with either less than 250 employees or less than 50 employees in 1998. Qualitatively, the main results still hold (see Table 10), but effects are smaller in size, particularly for the sample of firms with less than 50 employees. We find that the arrival of 100 public sector jobs into a postcode area in Berlin spurs the creation of 16 additional jobs in small (<50 workers) service firms located within the same area. When using both large and small firms, the impact was 56 new jobs (see Table 5, Column 10). The public job inflow also spurs the creation of 4 new jobs among small (<50 workers) service firms in areas in close proximity to a relocation site (see Table 10, Column 6). The corresponding effect for the whole sample was 34 jobs. As multi-side firms are most likely to have a high number of employees (e.g., the total number of employees working for a supermarket chain), the possibility of attributing all employment to centrally located head offices does not seem to be driving our findings.

A second robustness check consists in comparing short- and long-run effects (see Table 11). It is noteworthy that the impact of public employment on private activity consistently increases over time. Focusing on the short-term, we regress (1998-2000) changes in private employment on the spatial distribution of job moves occurring in 1999 only. We find large and statistically significant direct effects, but no statistically significant spillovers effects. As we move from the short- to the long-term (1998-2005) so that more cumulative relocations (those occurring between 1998 and 2004) can be included in our set of treatment intensity variables, we find larger direct and indirect effects (both statistically significant). It is interesting to note that effects are also larger

than the ones reported in our preferred specification, i.e. (1998-2002) changes as reported in Tables 4 and 5.

As an additional robustness check we assess the role of the bandwidth for our results. *A priori* we do not know the geographical spread of the policy. Using different bandwidth we can check at which distance the policy effect attenuates. Consistent with an emerging literature on the spatial range of agglomeration effects, we also find that effects are highly localized. They occur in and around the receiving postcodes. There are no spillovers beyond the 1km ring. In this exercise, we want to enlarge the width of the first ring from 1km to 1.5km, and then to 2km. In doing so, the average number of postcodes included in the first ring expands, as postcodes at an increasing distance to a relocation site and, thus, potentially less affected by the government move will be included. As the first ring expands, we expect to estimate a decreasing spillover effect for the average postcode included in the now larger first ring. Results confirm these expectations (see Table 12). The drop is largest when we expand the first ring from 0-1km to 0-1.5km: the corresponding estimated coefficients are 0.333 (s.e. 0.104) and 0.136 (s.e. 0.081), respectively. As we further expand the ring to cover 0-2km, the estimated coefficient decreases to 0.120 (s.e. 0.068).

As an alternative to treatment intensity variables, we also experiment with parametric weighting functions, which allocate a weight based on the inverse distance measured from each postcode centroid to the centroid of the areas that received relocated jobs. We thus replace our treatment intensity variables in Equation (1) with a parametric weighting function defined either as $treat_{p1}$ or $treat_{p2}$. In formulas:

$$treat_{p1} = \sum_{i=1}^{188} \frac{1}{|dist_{ij}|+1} \times \Delta emp_j^{reloc} \tag{2}$$

$$treat_{p2} = \sum_{i=1}^{188} \frac{1}{\left(|dist_{ij}|+1\right)^2} \times \Delta emp_j^{reloc} \tag{3}$$

Results for total private sector employment and the industry split are presented in Table 13. For each subsample, we contrast the effects estimated using both $treat_{p1}$ and $treat_{p2}$ on contemporaneous (1998-2002) and pre-treatment (1995-1997) changes in private sector employment. Estimates seem to confirm the results obtained so far. The same industries that react to public sector relocations using the non-parametric specification show significant employment growth when using distance weighting functions. For each 100 public sector employees weighted by the inverse of the absolute distance to the postcode, private sector employment increases by 32 workers. The corresponding effect is 52 workers if the weight is chosen as the inverse of the absolute distance squared. Similarly to what was found before, significant effects are obtained for real estate, business and professional services, and personal services. Using weighting functions, we also find a significant effect on finance whereas we fail to replicate the effect for trade. Looking at pre-treatment employment changes, i.e. a falsification test, all significant effects disappear.

VI. Conclusions

In this paper, we have shown that policies concerning public sector employment cannot be thought of as independent from their potential impact on the private sector. Using the move of the German government from Bonn to Berlin as a natural experiment, we found a significant positive effect of public employment on private sector activity for relocations occurring within the same postcode. The arrival of 100 public sector workers into a postcode area results in an employment gain of about 56 private sector jobs. We also found evidence of spatial spillovers. The inflow of public sector workers into an area add on average another 36 jobs in areas neighboring the relocation site. These effects comes through job creation in the service sector with the largest job gains in professional services such as legal activities and consultancies. Equally, employment in personal services (e.g., hairdressers, beauty salons and dry cleaners) increases indicating a higher

demand for locally-produced goods and services. For manufacturing jobs, we found neither evidence of a multiplier nor of a crowding-out effect.

Our findings are robust to a series of checks and extensions. Most importantly, we found no effect on private sector employment during the years leading up to the relocation program. We verified that the multiplier effect we obtain is indeed due to the relocation program and not to other transformation processes that were triggered by the reunification of West- and East-Germany. We ruled out the possibility that our estimates are driven by the expansion of Berlin CBD.

Our study has limitations. Since our methodology does not allow us to derive general equilibrium effects of the government move, we cannot infer how much higher overall employment in Berlin actually is compared to a scenario without the relocation. Additionally, the employment multipliers we identified in the analysis necessarily ignore one important aspect of the relocation program: in which parts of Berlin the incoming government employees choose to reside. Since we do not have information on residential choices, we cannot study localized effects on the housing market or changes in consumption patterns.

Summing up, we found that a large relocation program of government offices in Germany had a decisive impact on local private employment. In line with the literature on external economies of agglomeration, we also found that employment effects are highly localized and sharply attenuate with distance. These findings suggest that job relocation programs can be used as a tool for local economic development to create employment opportunities in targeted areas. Unlikely, the policy would spread beyond the areas of interest. Though we focus on an overall positive employment shock, it seems likely that public sector job cuts generate comparable (negative) local multiplier effects. This possibility should be taken into account in policy decision making and, in contesting cases, compensatory measures could be considered. Our results suggest that such compensations, if implemented, might have to be precisely targeted to actually help the disadvantaged area.

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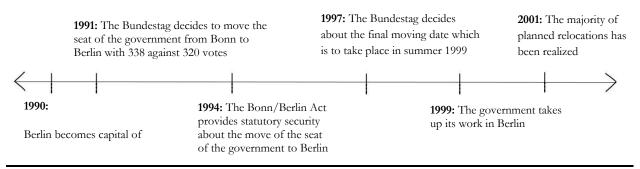
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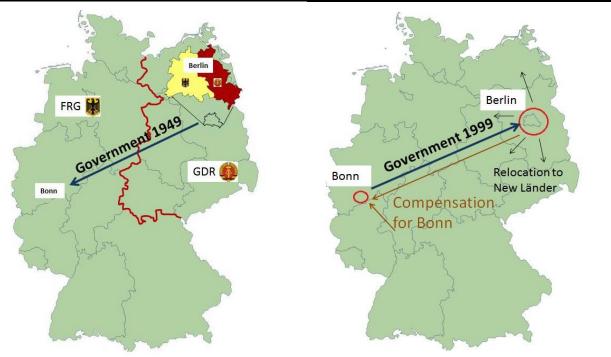
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Figure 1: Timeline of the decision making process

1991: The Bundestag decides to move the seat of the government from Bonn to Berlin with 338 against 320 votes

1997: The Bundestag decides about the final moving date which is to take place in summer 1999

2001: The majority of planned relocations has been realized

1990:

Berlin becomes capital of

1994: The Bonn/Berlin Act provides statutory security about the move of the seat of the government to Berlin

1999: The government takes up its work in Berlin

Sources: Deutscher Bundestag, 1991a, 1991b and 2010

Figure 2: Historic setting

Notes: Left Picture: Period of division lasting from 1949 to 1990. Right Picture: Implementation of the move of the government from Bonn to Berlin in 1999.
Sources: See Figure 1.
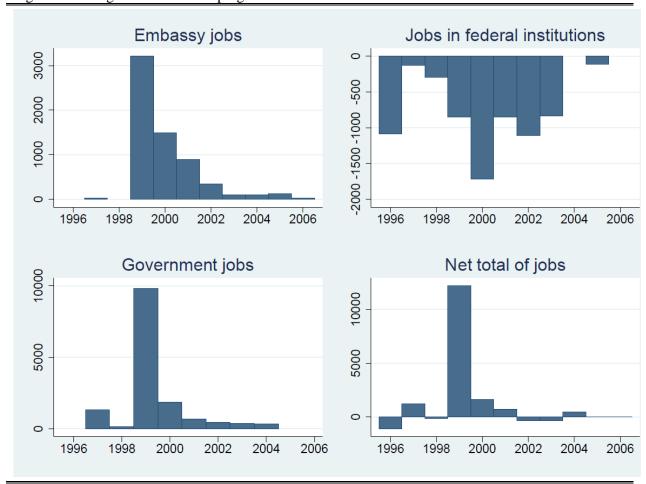
Figure 3: Timing of the relocation program

Note: Panel A shows embassy jobs that moved into Berlin; Panel B shows jobs in federal institutions that moved out of Berlin; Panel C shows government-related jobs that moved into Berlin; Panel D shows net changes in total jobs.

Sources: See Table A.2 for details.
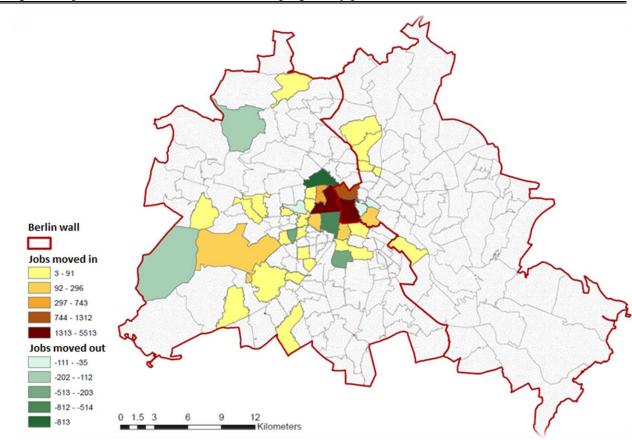
Figure 4: Spatial distribution of the relocation program by postcode

Sources: See Table A.2 for details.

Table 1: Number of relocated jobs

Institutions	Number of jobs moved
POSITIONS MOVED FROM BONN TO BERLIN	
Ministries	9,075
Bundestag, -rat,-präsidialamt	5,276
Länder representations	626
GOVERNMENT RELATED JOBS	**14,977**
Foreign representations	6,300
Media, parties and interest groups	3,700[1]
FOREIGN AND MEDIA RELATED JOBS	**10,000**
POSITIONS MOVED FROM BERLIN TO BONN	
Federal and other institutions	-4,054
POSITIONS MOVED FROM BERLIN TO THE NEW LÄNDER	
Federal institutions	-2,927[2]
POSITIONS MOVED OUT OF BERLIN	**-6,981**
TOTAL	**24,977 - 6,981 = 17,996[3]**

Notes: [1]According to the Deutscher Bundestag (1992), 10,000 jobs in foreign representations, media companies, political parties and interest groups would move from Bonn to Berlin in the aftermath of the relocation.
[2] As a federal country, Germany needs to balance the distribution of federal institutions across all federal states. The initial program involved the move of 4,700 jobs out of Berlin to the New Federal States (New Länder), but some reallocations never materialized.
[3] The DIW estimated a net gain of 18,159 job positions for the city of Berlin (see Geppert and Vesper, 2006) whereas the Prognos AG (2003) estimated a net gain of 14,500 positions. Our estimate is in between.
Sources: See Table A.2 for details.

Table 2: Descriptive statistics for receiving and non-receiving postcodes

Variable	Postcodes not receiving jobs		Postcodes receiving jobs		Difference in means
	Mean	SD	Mean	SD	
log total employment	7.46	0.823	8.12	0.874	-0.654***
% full time employment	77.9%	0.081	74.8%	0.086	3.1%**
% female employment	46.0%	0.130	53.7%	0.080	-7.6%***
% low qualified employment	13.0%	0.063	11.5%	0.052	1.5%
% medium qualified employment	53.7%	0.095	53.6%	0.086	0.1%
% high qualified employment	9.1%	0.060	13.5%	0.093	-4.4%***
% other qualified employment	24.1%	0.107	21.6%	0.104	2.5%
% workers in unskilled occ.	33.6%	0.094	28.9%	0.096	4.7%
% workers in skilled occ.	42.7%	0.087	42.4%	0.125	0.2%
% technicians	5.0%	0.031	4.3%	0.028	0.7%
% semi-professionals	9.1%	0.095	11.7%	0.110	-2.6%
% engineers	3.4%	0.038	3.6%	0.040	-0.2%
% professionals	1.5%	0.016	3.9%	0.064	-2.4%***
% managers	3.3%	0.021	3.8%	0.022	-0.5%
% employees aged 16 - 24	11.6%	0.048	10.6%	0.033	1.0%
% employees aged 25 - 39	42.5%	0.045	42.9%	0.039	-0.5%
% employees aged 40 - 54	33.9%	0.040	34.4%	0.038	-0.5%
% employees aged 55 - 65	11.9%	0.028	12.2%	0.028	-0.3%
Observations	150		38		

Note: All figures refer to 1998. There are 188 Berlin postcodes, with 30 postcodes receiving a positive and 8 postcodes receiving a negative public sector employment shock. 150 postcodes did not receive (or lost) any relocated job.

Sources: Establishment History Panel 1975-2010 (weakly anonymous) with added postcodes for Berlin.

Table 3: Plausibility check - the impact of 1999-2001 cumulative relocations on (1998-2002) changes in public sector and special interest group employment

	Public sector employment (SIC75 and SIC99)					Special interest group employment				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Receiving areas										
0km	0.673***	0.753***	0.819***	0.805***	0.802***	0.043*	0.044**	0.045**	0.042**	0.040*
	[0.077]	[0.054]	[0.062]	[0.061]	[0.068]	[0.023]	[0.019]	[0.018]	[0.019]	[0.020]
Spillovers										
0-1km	0.001	-0.009	-0.016	-0.014	-0.015	0.048	0.055	0.056	0.057	0.056
	[0.051]	[0.045]	[0.042]	[0.042]	[0.042]	[0.040]	[0.038]	[0.038]	[0.038]	[0.038]
0-2km	-0.056	-0.016	-0.004	-0.005	-0.005	0.006	0.003	-0.002	-0.002	-0.002
	[0.053]	[0.048]	[0.041]	[0.041]	[0.042]	[0.009]	[0.008]	[0.007]	[0.007]	[0.007]
0-3km	0.062	0.040	0.040	0.040	0.040	-0.005	-0.002	0.000	0.000	0.000
	[0.053]	[0.040]	[0.038]	[0.038]	[0.038]	[0.005]	[0.004]	[0.003]	[0.003]	[0.003]
0-4km	-0.028	-0.021	-0.03	-0.03	-0.0301	0.001	-0.001	-0.001	0.000	-0.001
	[0.068]	[0.048]	[0.046]	[0.046]	[0.046]	[0.002]	[0.003]	[0.003]	[0.003]	[0.003]
0-5km	-0.007	0.006	0.008	0.005	0.005	0.000	0.000	0.000	-0.001	-0.001
	[0.033]	[0.026]	[0.025]	[0.025]	[0.024]	[0.001]	[0.002]	[0.001]	[0.001]	[0.001]
0-15km	0.006	0.005	0.004	0.001	0.001	-0.001	-0.001	-0.001	-0.002	-0.002
	[0.006]	[0.014]	[0.013]	[0.015]	[0.017]	[0.001]	[0.001]	[0.001]	[0.002]	[0.002]
Area controls		√	√	√	√		√	√	√	√
Pre-trends			√	√	√			√	√	√
Distance to the Wall				√	√				√	√
Dummy for West Berlin					√					√
Observations	188	188	188	188	188	188	188	188	188	188

Note: Robust standard errors are reported in parentheses; (*), (**), (***) indicate significance at the 10%, 5% and 1% levels, respectively. Columns (2) and (7) include initial (1998) postcode level controls, i.e. log of total employment, shares of female and part-time workers; age, education and occupation structures. Columns (3) and (8) include area controls and pre-trends. Pre-trends are defined as the (1995-1997) changes in public sector employment (in Column 3) and special interest group employment (in Column 8). Columns (4) and (9) include a variable measuring the closest distance from the postcode centroid to the Berlin Wall. Columns (5) and (10) also include a location dummy for West Berlin. Special interest group employment is identified as employment recorded in codes 870, 871, 872 and 881 of the 1973 standard industry code (SIC) classification (3-digit). Public sector employment is identified as employment recorded in codes 75 and 99 of the 1993 SIC classification (2-digit).
Sources: Establishment History Panel 1975-2010 (weakly anonymous) with added postcodes for Berlin; government relocation data collected through independent sources (see Table A.2).

Table 4: The impact of (1999-2001) cumulative relocations on (1998-2002) changes in private sector employment

	(1)	(2)	(3)	(4)	(5)
Receiving areas					
0km	0.519	0.558**	0.555*	0.545*	0.547*
	[0.331]	[0.281]	[0.283]	[0.283]	[0.284]
Spillovers					
0-1 km	0.386***	0.352***	0.355***	0.356***	0.357***
	[0.115]	[0.121]	[0.121]	[0.122]	[0.122]
0-2 km	-0.025	-0.022	-0.021	-0.022	-0.022
	[0.045]	[0.039]	[0.040]	[0.040]	[0.040]
0-3 km	-0.012	-0.008	-0.008	-0.008	-0.009
	[0.043]	[0.036]	[0.036]	[0.036]	[0.036]
0-4 km	0.014	0.008	0.006	0.006	0.006
	[0.034]	[0.033]	[0.033]	[0.033]	[0.033]
0-5 km	-0.008	-0.016	-0.015	-0.017	-0.017
	[0.021]	[0.022]	[0.023]	[0.023]	[0.023]
0-15 km	-0.010	-0.000	-0.001	-0.002	-0.002
	[0.007]	[0.011]	[0.012]	[0.012]	[0.013]
Area controls		√	√	√	√
Pre trends			√	√	√
Distance to the wall				√	√
Dummy for West Berlin					√
Observations	188	188	188	188	188

Note: Robust standard errors are reported in parentheses; (*), (**), (***) indicate significance at the 10%, 5% and 1% levels, respectively. Columns (2) includes initial (1998) postcode level controls, i.e. log of total employment, shares of female and part-time workers; age, education and occupation structures. Columns (3) includes area controls and pre-trends, which are defined as (1995-1997) changes in private sector employment. Columns (4) includes a variable measuring the closest distance from the postcode centroid to the Berlin Wall. Columns (5) includes a West Berlin dummy.
Sources: see Table 3 for details.

Table 5: The impact of (1999-2001) cumulative relocations on (1998-2002) changes in manufacturing and service employment

	Manufacturing					Services				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Receiving areas										
0 km	-0.013***	-0.007	-0.005	-0.009	-0.007	0.535	0.559**	0.556**	0.554**	0.559**
	[0.005]	[0.012]	[0.012]	[0.014]	[0.015]	[0.326]	[0.264]	[0.266]	[0.266]	[0.266]
Spillovers										
0-1 km	0.005	0.005	0.005	0.005	0.006	0.369***	0.337***	0.340***	0.341***	0.343***
	[0.004]	[0.006]	[0.006]	[0.006]	[0.006]	[0.117]	[0.126]	[0.127]	[0.127]	[0.127]
0-2 km	-0.003	0.002	0.002	0.002	0.002	-0.010	-0.012	-0.008	-0.008	-0.009
	[0.004]	[0.005]	[0.005]	[0.005]	[0.005]	[0.044]	[0.041]	[0.042]	[0.043]	[0.043]
0-3 km	0.007	0.004	0.004	0.004	0.004	-0.025	-0.020	-0.022	-0.022	-0.022
	[0.006]	[0.006]	[0.006]	[0.006]	[0.006]	[0.041]	[0.034]	[0.035]	[0.035]	[0.035]
0-4 km	-0.005	-0.005	-0.005	-0.005	-0.005	0.019	0.012	0.009	0.009	0.010
	[0.007]	[0.006]	[0.006]	[0.006]	[0.006]	[0.028]	[0.030]	[0.029]	[0.029]	[0.029]
0-5 km	0.000	0.002	0.002	0.002	0.002	-0.008	-0.017	-0.016	-0.016	-0.015
	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.017]	[0.020]	[0.020]	[0.021]	[0.020]
0-15 km	-0.006***	-0.007***	-0.007***	-0.008***	-0.007**	-0.003	0.004	0.004	0.003	0.005
	[0.002]	[0.002]	[0.002]	[0.003]	[0.003]	[0.006]	[0.011]	[0.011]	[0.011]	[0.012]
Area controls		√	√	√	√		√	√	√	√
Pre-trends			√	√	√			√	√	√
Distance to the wall				√	√				√	√
Dummy for West Berlin					√					√
Observations	188	188	188	188	188	188	188	188	188	188

Note: Robust standard errors are reported in parentheses; (*), (**), (***) indicate significance at the 10%, 5% and 1% levels, respectively. Columns are organized as in Table 4. Pre-trends are defined as the (1995-1997) changes in manufacturing employment (in Column 3) and service employment (in Column 8).
Sources: see Table 3 for details.

Table 6: Splitting services by sub-group

	(1) Transport	(2) Trade	(3) Hotels	(4) Restaurants	(5) Finance	(6) Real Estate	(7) Business	(8) Media	(9) Personal Services	(10) Tourism
Receiving areas										
0 km	0.076	0.044*	-0.003	0.005	0.039	0.032***	0.268*	0.079	0.016***	0.020
	[0.051]	[0.024]	[0.004]	[0.017]	[0.044]	[0.011]	[0.153]	[0.051]	[0.005]	[0.022]
Spillovers										
0-1 km	0.091	0.002	0.014*	0.007	0.118	-0.002	0.089	-0.000	0.007***	0.017
	[0.088]	[0.014]	[0.008]	[0.009]	[0.088]	[0.009]	[0.065]	[0.018]	[0.002]	[0.018]
0-2 km	0.010	-0.012	-0.004	-0.017	-0.009	0.006	-0.002	0.009	-0.003	-0.022**
	[0.021]	[0.010]	[0.002]	[0.013]	[0.018]	[0.011]	[0.019]	[0.01]	[0.003]	[0.010]
0-3 km	-0.021	0.005	0.001	0.006	0.015	-0.008	0.008	-0.003	-0.001	0.010
	[0.019]	[0.008]	[0.002]	[0.006]	[0.012]	[0.005]	[0.014]	[0.007]	[0.005]	[0.006]
0-4 km	0.018	0.008	-0.000	0.003	-0.020*	0.008*	-0.016	-0.002	0.001	-0.00
	[0.013]	[0.008]	[0.002]	[0.005]	[0.011]	[0.004]	[0.013]	[0.004]	[0.005]	[0.006]
0-5 km	-0.010	-0.012	0.000	-0.001	0.007	-0.002	0.004	0.000	0.001	0.002
	[0.009]	[0.008]	[0.001]	[0.002]	[0.007]	[0.002]	[0.008]	[0.002]	[0.002]	[0.003]
0-15 km	0.013	0.002	-0.001	0.001	-0.003	0.001	-0.007	-0.000	0.002	-0.003
Observations	188	188	188	188	188	188	188	188	188	188

Note: Robust standard errors are reported in parentheses; (*), (**), (***) indicate significance at the 10%, 5% and 1% levels, respectively. All columns include initial (1998) postcode level controls, pre-trends, the distance to the Berlin Wall and a West Berlin dummy. Pre-trends are defined as the (1995-1997) changes in each sub-group own employment.
Sources: see Table 3 for details.

Table 7: Falsification test – the impact of (1999-2001) cumulative relocations on (1995-1997) changes in total private sector, manufacturing and service (including selected sub-groups) employment

	(1) Private	(2) Manufacturing	(3) Services	(4) Trade	(5) Real Estate	(6) Business	(7) Personal Services
Receiving areas							
0 km	-0.023	0.007	0.018	-0.008	0.013	0.019	0.001
	[0.042]	[0.012]	[0.033]	[0.014]	[0.012]	[0.013]	[0.002]
Spillovers							
0-1 km	0.044	-0.001	0.033	0.011*	0.004	0.012	0.000
	[0.040]	[0.006]	[0.035]	[0.006]	[0.006]	[0.016]	[0.001]
0-2 km	0.007	0.006	0.023	0.001	-0.001	-0.003	-0.001
	[0.036]	[0.005]	[0.031]	[0.005]	[0.004]	[0.012]	[0.001]
0-3 km	-0.004	0.000	-0.011	-0.004	-0.003	0.011	0.001
	[0.025]	[0.006]	[0.026]	[0.005]	[0.003]	[0.012]	[0.002]
0-4 km	-0.028	-0.004	-0.026	0.008	-0.001	-0.025*	0.000
	[0.020]	[0.007]	[0.022]	[0.010]	[0.003]	[0.013]	[0.001]
0-5 km	0.012	0.004	0.011	-0.006	0.002	0.015*	0.000
	[0.013]	[0.005]	[0.015]	[0.009]	[0.002]	[0.008]	[0.001]
0-15 km	0.005	0.000	0.008	0.004	-0.001	0.000	0.000
	[0.010]	[0.003]	[0.008]	[0.003]	[0.001]	[0.003]	[0.000]
Observations	188	188	188	188	188	188	188

Note: Robust standard errors are reported in parentheses; (*), (**), (***) indicate significance at the 10%, 5% and 1% levels, respectively. All columns include initial (1995) postcode level controls, pre-trends, the distance of each postcode centroid to the Berlin Wall and a West Berlin dummy. Pre-trends are defined as the (1993-1994) changes in total private (Column 1), manufacturing (Column 2) and services (Column 3) employment. Among sub-groups in services, pre-trends are defined as the (1993-1994) changes in trade (Column 4), real estate (Column 5), business (Column 6) and personal services (Column 7) employment.
Sources: see Table 3 for details.

Table 8: The impact of different distance measures

	(1) Private	(2) Manuf	(3) Services	(4) Private	(5) Manf	(6) Services	(7) Private	(8) Manuf	(9) Services	(10) Private	(11) Manuf	(12) Services
Receiving areas												
0 km	0.598**	-0.014	0.594**	0.622**	-0.006	0.612**	0.579**	-0.017	0.579**	0.543*	-0.015	0.553**
	[0.284]	[0.020]	[0.268]	[0.281]	[0.021]	[0.264]	[0.283]	[0.019]	[0.267]	[0.288]	[0.014]	[0.272]
Spillovers												
0-1 km	0.354***	0.007	0.341***	0.350***	0.006	0.337**	0.355***	0.007	0.342***	0.357***	0.007	0.343***
	[0.124]	[0.006]	[0.129]	[0.126]	[0.006]	[0.131]	[0.123]	[0.006]	[0.129]	[0.121]	[0.006]	[0.126]
0-2 km	-0.019	0.002	-0.008	-0.016	0.002	-0.005	-0.020	0.001	-0.008	-0.021	0.002	-0.010
	[0.041]	[0.005]	[0.043]	[0.041]	[0.005]	[0.043]	[0.041]	[0.005]	[0.043]	[0.040]	[0.005]	[0.042]
0-3 km	-0.008	0.003	-0.022	-0.010	0.004	-0.023	-0.008	0.003	-0.022	-0.009	0.003	-0.023
	[0.036]	[0.006]	[0.035]	[0.036]	[0.006]	[0.035]	[0.036]	[0.006]	[0.035]	[0.036]	[0.006]	[0.035]
0-4 km	0.005	-0.005	0.008	0.005	-0.005	0.009	0.005	-0.005	0.009	0.007	-0.004	0.010
	[0.034]	[0.006]	[0.029]	[0.033]	[0.006]	[0.029]	[0.033]	[0.006]	[0.029]	[0.034]	[0.006]	[0.030]
0-5 km	-0.008	0.001	-0.009	-0.005	0.002	-0.007	-0.012	0.000	-0.012	-0.018	0.000	-0.017
	[0.026]	[0.005]	[0.023]	[0.024]	[0.004]	[0.021]	[0.027]	[0.004]	[0.024]	[0.028]	[0.004]	[0.026]
0-15 km	0.009	-0.009**	0.013	0.016	-0.007	0.018	0.005	-0.010**	0.009	-0.004	-0.009***	0.003
	[0.021]	[0.004]	[0.020]	[0.021]	[0.005]	[0.019]	[0.020]	[0.004]	[0.019]	[0.018]	[0.003]	[0.017]
Distance to Bundestag	12.42	-3.186	10.79									
	[22.55]	[5.214]	[21.00]									
Distance to Alexanderplatz				21.24	-0.911	17.45						
				[20.16]	[5.495]	[18.90]						
Distance to Potsdamer Platz							7.137	-4.397	6.706			
							[21.37]	[4.984]	[20.65]			
Distance to Kudamm										-5.805	-4.799	-2.418
										[21.48]	[4.157]	[19.95]
Obs	188	188	188	188	188	188	188	188	188	188	188	188

Note: Robust standard errors are reported in parentheses; (*), (**), (***) indicate significance at the 10%, 5% and 1% levels, respectively. All columns include initial area controls, pre-trends and a West Berlin dummy. The variable distance to the Wall is replaced by distance to the Bundestag (Columns 1-3), distance to Alexanderplatz (Columns 4-6), distance to Potsdamer Platz (Columns 7-9) and distance to Kudamm (Columns 10-12).
Sources: see Table 3 for details.

Table 9: Centrality test – the impact of distance to the CBD on 5-year growth in private sector employment

	(1) Unweighted	(2) Weighted	(3) Unweighted	(4) Weighted	(5) Unweighted	(6) Weighted
distance to the CBD	-0.002 (0.003)	-0.010** (0.005)				
(1992-1997) × distance to the CBD			-0.006* [0.004]	-0.018*** [0.005]	-0.013 [0.005]	-0.016*** [0.005]
(1997-2002) × distance to the CBD			-0.002 [0.004]	-0.008 [0.005]	-0.010** [0.005]	-0.010* [0.005]
(2002-2007) × distance to the CBD			-0.000 [0.004]	-0.002 [0.006]	-0.003 [0.005]	-0.002 [0.005]
Controls					√	√
Observations	564	564	564	564	564	564

Note: Robust standard errors are reported in parentheses; (*), (**), (***) indicate significance at the 10%, 5% and 1% levels, respectively. The dependent variable refers to 5-year growth rates (1992-1997; 1997-2002: 2002-2007) in total private employment for each of the 188 postcodes. In Columns (2), (4) and (6), regressions are weighted by postcode size. Columns (5) and (6) include initial (1992) postcode controls.
Source: see Table 3 for details.

Table 10: The impact of public employment on SME private sector enterprises

	Sample with employment < 250			Sample with employment < 50		
	(1)	(2)	(3)	(4)	(5)	(6)
	Private	Manufacturing	Services	Private	Manufacturing	Services
Receiving areas						
0 km	0.286*	-0.001	0.290*	0.157*	-0.001	0.159*
	[0.163]	[0.003]	[0.152]	[0.090]	[0.001]	[0.087]
Spillovers						
0-1 km	0.075*	0.000	0.070	0.041**	-0.000	0.038*
	[0.042]	[0.001]	[0.046]	[0.019]	[0.001]	[0.021]
0-2 km	-0.011	-0.000	-0.008	0.003	-0.001	0.006
	[0.020]	[0.001]	[0.018]	[0.010]	[0.001]	[0.010]
0-3 km	0.001	0.001	0.005	-0.001	0.001	-0.003
	[0.014]	[0.002]	[0.012]	[0.007]	[0.001]	[0.006]
0-4 km	-0.003	0.001	-0.010	0.007	-0.000	0.008
	[0.015]	[0.002]	[0.016]	[0.007]	[0.001]	[0.007]
0-5 km	0.002	-0.001	0.005	-0.005	-0.001	-0.004
	[0.010]	[0.001]	[0.010]	[0.005]	[0.001]	[0.004]
0-15 km	-0.001	-0.000	0.000	0.001	-0.000	0.001
	[0.007]	[0.001]	[0.005]	[0.003]	[0.000]	[0.003]
Observations	188	188	188	188	188	188

Note: Robust standard errors are reported in parentheses; (*), (**), (***) indicate significance at the 10%, 5% and 1% levels, respectively. The dependent variable in Columns (1)-(3) is defined as the (1998-2002) changes in private sector employment where employment is computed restricting the sample to firms with less than 250 employees in 1998. Correspondently, the dependent variable in Columns (4)-(6) is defined as the (1998-2002) changes in private sector employment where employment is computed restricting the sample to firms with less than 50 employees in 1998. All columns include initial (1998) postcode level controls, pre-trends, the distance of each postcode centroid to the Berlin Wall and a West Berlin dummy.
Sources: see Table 3 for details.

Table 11: The impact of public employment on private employment: short-and long-run effects

	(1998-2000) changes			(1998-2005) changes		
	(1)	(2)	(3)	(4)	(5)	(6)
	Private	Manufacturing	Services	Private	Manufacturing	Services
Receiving areas						
0 km	0.422**	-0.001	0.420**	0.650*	-0.010	0.629*
	[0.203]	[0.016]	[0.191]	[0.390]	[0.023]	[0.360]
Spillovers						
0-1 km	0.264	0.006	0.252	0.392**	0.013	0.348*
	[0.191]	[0.008]	[0.191]	[0.194]	[0.010]	[0.204]
0-2 km	-0.006	0.005	-0.008	0.008	-0.006	0.037
	[0.066]	[0.005]	[0.064]	[0.057]	[0.009]	[0.062]
0-3 km	-0.004	-0.003	-0.007	-0.008	0.013	-0.031
	[0.035]	[0.005]	[0.033]	[0.047]	[0.009]	[0.049]
0-4 km	0.001	-0.001	0.005	0.027	-0.005	0.018
	[0.031]	[0.006]	[0.026]	[0.050]	[0.015]	[0.043]
0-5 km	-0.010	0.001	-0.011	-0.062*	-0.001	-0.054*
	[0.023]	[0.004]	[0.019]	[0.036]	[0.012]	[0.030]
0-15 km	0.000	-0.006*	0.002	-0.013	-0.010***	-0.006
	[0.013]	[0.003]	[0.011]	[0.022]	[0.003]	[0.020]
Observations	188	188	188	188	188	188

Note: Robust standard errors are reported in parentheses; (*), (**), (***) indicate significance at the 10%, 5% and 1% levels, respectively. The dependent variables is defined as the (1998-2000), (1998-2005), and (1998-2008) changes in private sector employment in Columns (1)-(3), (4)-(6), and (7-9), respectively. The treatment intensity variables are computed using relocated jobs between 1999 and 2001 in Columns (1)-(3); they are computed using relocated jobs between 1999 and 2004 in Columns (4)-(9). All columns include initial (1998) postcode level controls, (1995-1997) pre-trends, the minimum distance to the Berlin Wall and a West Berlin dummy.
Sources: see Table 3 for details.

Table 12: Enlarging the first distance band

	(1)	(2)	(3)
Receiving areas			
0 km	0.546*	0.553*	0.554*
	[0.283]	[0.288]	[0.286]
Spillovers			
0-1 km	0.333***		
	[0.109]		
0-1.5 km		0.136*	
		[0.081]	
0-2 km			0.120*
			[0.068]
0-3 km	-0.014	-0.007	-0.027
	[0.020]	[0.021]	[0.029]
0-5 km	-0.012	-0.016	-0.015
	[0.015]	[0.015]	[0.016]
0-15 km	-0.003	0.000	-0.001
	[0.013]	[0.015]	[0.015]
Observations	188	188	188

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 the (1998-2000) changes in private sector employment. All columns include initial (1998) postcode level controls, (1995-1997) pre-trends, the minimum distance to the Berlin Wall and a West Berlin dummy.
Sources: see Table 3 for details.

Table 13: Using distance weighting functions

	Private		Manufacturing		Services	
	(1)	(2)	(3)	(4)	(5)	(6)
	Δ(1998-2002)	Δ(1995-1997)	Δ(1998-2002)	Δ(1995-1997)	Δ(1998-2002)	Δ(1995-1997)
Fct 1	0.316**	-0.031	0.012	0.010	0.308**	-0.018
	[0.157]	[0.033]	[0.010]	[0.007]	[0.150]	[0.031]
Fct 2	0.517**	-0.032	0.015	0.009	0.505**	-0.008
	[0.226]	[0.036]	[0.010]	[0.008]	[0.213]	[0.032]

	Trade		Hotel		Cafes	
	(7)	(8)	(9)	(10)	(11)	(12)
	Δ(1998-2002)	Δ(1995-1997)	Δ(1998-2002)	Δ(1995-1997)	Δ(1998-2002)	Δ(1995-1997)
Fct 1	0.009	0.004	0.002	0.003	-0.000	0.007
	[0.019]	[0.008]	[0.003]	[0.003]	[0.013]	[0.005]
Fct 2	0.031	-0.004	0.003	0.015	-0.001	0.012
	[0.023]	[0.010]	[0.005]	[0.009]	[0.016]	[0.008]

	Finance		Real Estate		Business	
	(13)	(14)	(15)	(16)	(17)	(18)
	Δ(1998-2002)	Δ(1995-1997)	Δ(1998-2002)	Δ(1995-1997)	Δ(1998-2002)	Δ(1995-1997)
Fct 1	0.053*	-0.014	0.018*	0.002	0.144*	0.005
	[0.028]	[0.011]	[0.009]	[0.006]	[0.081]	[0.010]
Fct 2	0.070**	-0.024	0.024**	0.007	0.242**	0.009
	[0.029]	[0.018]	[0.010]	[0.009]	[0.121]	[0.013]

	Media		Personal services		Tourism	
	(19)	(20)	(21)	(22)	(23)	(24)
	Δ(1998-2002)	Δ(1995-1997)	Δ(1998-2002)	Δ(1995-1997)	Δ(1998-2002)	Δ(1995-1997)
Fct 1	0.041	-0.001	0.007**	0.000	0.009	0.008
	[0.027]	[0.002]	[0.003]	[0.001]	[0.015]	[0.009]
Fct 2	0.067	0.000	0.008**	0.000	0.017	0.015
	[0.041]	[0.003]	[0.003]	[0.001]	[0.020]	[0.014]

Note: Robust standard errors are reported in parentheses; (*), (**), (***) indicate significance at the 10%, 5% and 1% levels, respectively. Industry (sub-)sectors are indicated at the top of each panel. Dependent variables correspond to either the (1998-2002) or the (1995-1997) changes in employment for a given (sub-)sector. In all panels, estimates refer to coefficients of two distance weighting functions: Function 1: $treat_{p1} = \sum_{i=1}^{188} \frac{1}{|dist_{ij}|+1} \times \Delta emp_j^{reloc}$ and Function 2: $treat_{p2} = \sum_{i=1}^{188} \frac{1}{(|dist_{ij}|+1)^2} \times \Delta emp_j^{reloc}$. Δemp_j^{reloc} refers to (1999-2001) public sector relocations. As explained in the text, these replace treatment intensity variables in specification similar to Equation (1). Columns referring to (1998-2002) changes include initial (1998) postcode level controls, (1995-1997) pre-trends, the minimum distance to the Berlin Wall and a West Berlin dummy. Columns referring to (1995-1997) changes include initial (1995) postcode level controls, (1993-1994) pre-trends, the minimum distance to the Berlin Wall and a West Berlin dummy.
Sources: see Table 3 for details.

Web Appendices

A.1. Historical setting – details

When the Federal Republic of Germany (FRG) and the German Democratic Republic (GDR) were founded in 1949, it was unclear how long this separation would last. Berlin, German capital between 1871 and 1945, was claimed to be part of both states. In the constitution of the GDR, Berlin was declared its capital whereas, in the basic law of the FRG, (Great-) Berlin was considered as the 11th federal state. As a result, the city was divided into East-Berlin – the sector governed by the USSR – and West-Berlin – made up of the three sectors governed by the US, France and the UK. Overall, the city geographically fell unto the Soviet occupation zone.

The two German states did not recognize each other's sovereignty straight away. The FRG saw the division as a temporary state and both the political decision makers as well as the general public advocated a reunified Germany with Berlin as its capital (Süß, 1999). That the situation was widely seen a transitional was also reflected in the discussion about the seat of the FRG government. The new capital should have a provisional character and be only of medium power (Dreher, 1979) not to hamper the move of the government back to Berlin once the two German states reunified.

The perception that the division would only be of short duration changed when in August 1961 construction works of the Berlin Wall began. In December 1972, the FRG and the GDR came to terms with the status quo and mutually recognized each other as sovereign states by signing the Basic Treaty. Shortly before, the allies had resolved their dispute on their rights and responsibilities in Berlin in the Quadripartite Agreement. Berlin would continue not to be a constituent part of the FRG.[23] In his government declaration in 1973 Willy Brandt referred for the first time to Bonn as the

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[23] This rule was frequently a source of conflict between the FRG and the GDR, if for example federal offices of the FRG were established in West-Berlin.

federal capital of Germany. Though the ultimate aim of the West German government was still reunification, the political discussion about Berlin as capital was muted substantially.

Reunification and the Bonn/Berlin question

Political protest against the East German government began in September 1989 with the so called Monday-demonstrations in Leipzig. With the fall of the Berlin Wall in November 1989 it was once again possible to freely move within the Eastern and Western parts of the city. Berlin became capital of United Germany in 1990 when the Unification Treaty (1990) was signed between the newly elected government of the GDR and FRG. However, the decision on the seat of the government was postponed until after the election of the first assembly (Bundestag) of reunified Germany.

The crucial debate about the move of the government to Berlin took place on June 20th, 1991. The assembly was divided. The deputies of the ten western federal states had already shown their preference for Bonn during the negotiation of the Reunification Treaty (Süß, 1999). Polls among the total of 662 members of parliament saw Bonn as the clear favorite (Tschirch, 1999). Bonn advocates pointed towards the successful democratic as well as the federalist tradition. European integration had been facilitated by Bonn's proximity to the western allies and the EU seat in Brussels. Integration would slow down if Berlin became the new capital (Salz, 2006). Additionally, large infrastructure investments had turned Bonn into a highly efficient administration. These investments would be lost and the means for setting up the government in Berlin would be better spend on construction projects in the new federal states (Tschirch, 1999).

The main argument of the pro-Berlin faction was that of credibility. Since 1949 when the FRG was founded politicians had repeated again and again that Berlin was the actual capital of Germany. Bonn had become capital with a provisional mandate which would move back to Berlin once East and West Germany were united. Further important arguments were the importance of the move as a sign of solidarity between the old and the new federal states and Berlin's potential as a bridge of

Eastern Europe. Economically the move of the government was expected to strengthen the weak local economic position of Berlin and the economically underdeveloped east. The city of Bonn on the other hand feared that "the small Bonn" would lose its political significance as well as economic power (Deutscher Bundestag, 1991b, p. 2736-2738).

The most important arguments in the discussion were linked to rather abstract concepts. Credibility and the future of the reunited Germany stood out for the pro-Berlin side whereas Bonn was seen as a symbol of successful democratic tradition (Tschirch, 1999). In the final ballot the assembly decided to move the seat of the government from Bonn to Berlin with 338 against 320 votes (Deutscher Bundestag, 1991b). The feeble majority could only be reached by making large concessions to the city of Bonn. A fair division of labor between Berlin and Bonn should be negotiated where core government functions should take their seat in Berlin but the majority of government jobs should remain in Bonn. Bonn should receive financial compensation as well as new functions and institutions of national and international significance. A commission should be appointed to work on proposals for the distribution of national and international agencies across the new federal states, as the constitution of Germany states that each federal state should have some national power. The national parliament (Bundestag) was supposed to take up its function in Berlin within 4 years. Within 10-12 years all government functions should be located in Berlin (Deutscher Bundestag, 1991a). The Federal Assembly decided two weeks later with 38 against 30 votes to remain in Bonn (Deutscher Bundestag, 2010).

Realization of the move

The decision made in 1991 left the details of the move open. While working on a practical concept it became clear by 1991 that a move within four years was unfeasible. What followed was a long discussion about the timing of the move and its costs. One motion proposed to stop any further government related investments in Berlin until the financial situation of the FRG had improved.

Another proposed to postpone the move until 2010. Furthermore, a mass petition was organized to suspend a decision about the date of the move until the government had full knowledge of the costs and the financial situation of the state had improved (Deutscher Bundestag, 2010). This dispute created uncertainty among the private companies that had started to invest in Berlin. In November 1993, 40 national and international companies pointed at the breach of trust should the government cease its effort to press ahead with the move (Hoffman 1998, p. 213).

The uncertainty about the move diminished in 1994 when the Berlin/Bonn Act (1994) passed in March 1994. Though it did not specify a concrete moving date, the act provided statutory security that the move was to take place. It also detailed the implementation of the move such a definition of a fair division of labor between Berlin and Bonn and concrete compensatory measures for Bonn. Six ministries should keep their first seat in Bonn and get a second seat in Berlin; nine ministries should take their first seat in Berlin and keep their second seat in Bonn. Additionally, it was decided that the majority of ministerial positions should remain in Bonn. The following years were mostly concerned with the practical implementation of the move. Construction of the new buildings (Jakob-Kaiser-Haus, Paul-Löbe-Haus) began in spring 1997 but it was only by November 1997 that the Federal Parliament announced a moving date. The government was to take up its work in Berlin in September 1999. Until then the timing had remained heavily debated. Parliament and government officially started their work in Berlin in September 1999. The majority of employees moved in 1999 and 2000 so that by the end of 2000 more than 8,000 ministerial employees were located in Berlin. In the subsequent years more jobs were gradually moved from Bonn to Berlin. In 2010, about 10,000 positions were established in Berlin. Since the Federal Assembly had revised its decision to stay in Bonn in September 1996 also the Federal States established their representations in Berlin. In total, about 600 employees moved between 1998 and 2003, plus about 70 members of the Federal Assembly.

The majority of foreign representations decided to relocate their seats from Bonn to Berlin. Most of embassies chose to be present in Berlin when the German government took up its work in 1999 and made short-term arrangements to accommodate their staff. Besides renting offices and apartments the embassies use the facilities of their consulate generals and branch offices as well as former military missions until they could rebuild or construct a suitable building for their representation (Gehrcken, 2013). Though the diplomatic building stock in Berlin-West had been almost entirely destroyed between 1939 and 1945 many countries still owned a parcel of land in West-Berlin. The former embassies in Berlin-East had closed in 1990 when the GDR became part of the FRG. Some reopened later on as representations in the FRG. Today 163 states are represented in Berlin by 158 embassies and five honorary consulates.

To compensate Bonn for its loss of employment several federal offices moved from Berlin to Bonn in 1999 and 2000. Berlin also lost several of its prior functions to the New Länder. The recommendations of the federal commission concerned several Berlin-based institutions that moved in the subsequent years. Before reunification, 28,000 employees had worked for federal offices in Berlin (Guerra, 1999). The two relocation programs concerned about 8,700 positions.

A.2. Data Collection through Independent Sources

Due to lack of official sources on public sector employment, we embarked in an extensive data collection exercise, gathering information on three main variables: the number of jobs of each relocating institution before and after the move; the year the institution moved in or out of Berlin; and the new address of the institution in Berlin or the former address in Berlin of those institutions that were relocated to Bonn and the New Länder. We could also obtain information on the number of government employees working in Berlin in 1997, 1999, 2001 and 2004. Those figures were retrieved from official documents (BT-Drucksache) and an issue of the *Spiegel*, a weekly nationwide newspaper (Bornhöft *et al.*, 2001). The number of relocated jobs of federal institutions was equally

retrieved from official documents (BT-Drucksache). The *Berliner Zeitung*, a daily newspaper based in Berlin published information on the number of employees in the federal administration and parliamentary groups, as well as the number of deputies and their employees in 1999. We attributed 1998 employment levels to the Länder representations which were published in the *Generalanzeiger*, a local newspaper in the Bonn region.

The collection of embassy personnel data proved more difficult and required us to make a few assumptions. The Ministry of Foreign Affairs (Auswärtiges Amt) publishes each year a list of diplomatic staff in foreign embassies that have a representation in Germany. From these documents, we retrieved the number of diplomatic staff located in Germany in 1996 and use it as the pre-treatment level. As the documents do not contain any information on embassy workers covering administrative or technical support positions, we assume that their number is proportional to the number of diplomatic staff and estimate the total number of workers in each embassy based on the total of people registered at the Ministry of Foreign Affairs. Since the Ministry of Foreign Affairs registers also family members, we have to make additional assumptions to derive the number of actual embassy personnel. We recognize that it is difficult to make reasonable assumptions about the family composition of embassy members. Nevertheless, we assume that the average household size of an embassy member is approximately 2.5 and get an estimate of a total of 6,300 embassy personnel. By double checking this number against a total of 10,000 embassy workers, media representatives and employees of lobbying organizations that moved from Bonn to Berlin during the relocation program, we conclude that a number of this magnitude seems reasonable and use it in our analysis.

Table A.2: Details on data sources

Relocated institution	Year	Source of employment data	Source of address in Berlin
Federal Ministries	1997	BT-Drucksache 13/9537	OECKL Taschenbuch des öffentlichen Lebens
	1999	BT-Drucksache 14/1601	OECKL Taschenbuch des öffentlichen Lebens
	2001	Spiegel 18/2001 "Die Wacht am Rhein"	OECKL Taschenbuch des öffentlichen Lebens
	2004	BT-Drucksache 16/158	OECKL Taschenbuch des öffentlichen Lebens
Embassies	1996	Liste der diplomatischen Missionen in der Bundesrepublik Deutschland, 1998	Senatsverwaltung Berlin, Liste der diplomatischen Vertretungen
Federal Institutes (New Länder)	1999 2003	BT-Drucksache 12/2853	Drucksache 15/875, OECKL Taschenbuch des öffentlichen Lebens
Federal Institutes (Bonn compensation)		Teilungskostenbericht fuer das Jahr 2009	Drucksache 15/875, OECKL Taschenbuch des öffentlichen Lebens
Representations of the Länder	1998	Bonner General-Anzeiger, 09.02.1998, "Verkaufen, vermieten, verwerten: Abschied der Länder"	Presse und Informationsamt der Bundesregierung, OECKL Taschenbuch des öffentlichen Lebens
Deputies and deputee employees, Factions, Federal Parliament Bundestag) and administration	1999	Berliner Zeitung 05.07.1999, "Die Bonner kommen: Bundestag startet offiziellen Umzug"	Anschriftenverzeichnis des Bundes
Office of the Federal President	1998	Berliner Zeitung 24.11.1998, "Bauminister übergibt Schlüssel für das neue Bundespräsidialamt"	Anschriftenverzeichnis des Bundes
Federal Assembly (Bundesrat)	2000	Handelsblatt 31.07.2000, "Bundesrats-Umzug nach Berlin fast abgeschlossen"	Anschriftenverzeichnis des Bundes