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Interaction of public and private employment: Evidence from a German government move[☆]

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ABSTRACT

We use the German government move from Bonn to Berlin in 1999 to explore the interaction between public and private sector employment within a local labor market. Our findings show a positive effect of public sector expansion on private sector employment, with a local multiplier of 1.32–1.35, mainly driven by the service sector. The policy impact is highly localized, strongest within 300 m of a relocation site, and evident one year after the relocation. Three quarters of new private sector jobs were created by establishments that did not exist before 1998. These newly created jobs disproportionally employ women, younger workers, individuals in managerial and professional roles, and those with lower levels of education.

1. Introduction

Spatial inequality is a prime factor when analyzing economic performance across regions and countries. To explain spatial inequality, the new economic geography literature has focused on the location of private sector activity. In this paper, we shift the focus from looking solely at the private sector to examining how the private sector interacts with the public sector to understand local economic performance. Our approach is important for, at least, three reasons: first, public employment accounts for a substantial share of total employment in most OECD countries, with figures of 23.5%, 19.8% and 15.4% in the UK, France and Germany, respectively (see OECD, 2015). Second, governments have frequently used relocation programs of public sector workers as a tool to address unemployment in declining regions (see Jefferson and Trainor, 1996). Third, in the aftermath of the 2008 recession, some austerity measures were introduced in the form of public sector job cuts, with the expectation that by reducing the size of the public sector, private activity would return and flourish.[2] These conflicting rationales highlight the level of uncertainty about the size and direction of the effects.

This paper examines the relocation of the German government from Bonn to Berlin in the 1990s to understand how public and private sector

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2 During the emergency Budget speech on June 22, 2010, George Osborne, the then Chancellor of the Exchequer, introduced deep austerity measures and forecasted that a surge in private sector employment would offset the cuts in public sector numbers, particularly in regions that had traditionally relied on the public sector for growth (House of Commons, 2010; OBR, 2010). Contrary to the government's expectations, Cribb et al. (2014) document that UK regions with larger cuts to public sector employment during 2010–2013 were those that experienced the lowest growth in private sector activity. Conversely, looking at the experiences of Germany and Italy, Senftleben-König, (2015) and Auricchio et al. (2020a) show that a contraction of public sector employment at the district/municipality level leads to a rise in local private sector activity.

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employment interact in localized labor markets. Proponents of relocation policies argue that such policies trigger local multiplier effects: the arrival of public sector jobs in an area may increase demand for locally produced goods and services. Opponents of this view stress that the newly created jobs may merely crowd out existing ones: possible general equilibrium effects in the form of higher housing rents and wages raise local production costs with negative consequences for businesses. Crowding-out effects may be stronger than multiplier effects (see Alesina et al., 2001; Auricchio et al., 2020a; Senftleben-König, 2015), even though evidence on relocation policies is at present limited (see Becker et al., 2021; Faggio, 2019).

Our study offers two key contributions to the existing literature on public sector expansion and contraction: (*i*) we conduct the analysis at the establishment[3] level instead of the area level and, thus, we estimate the policy impact on the average plant *i* located at distance *d* of a relocation site, allowing effects to vary by distance; (*ii*) we examine the policy impact within a city boundary, as opposed to conducting cross-city or cross-municipality analyses.

Conducting the analysis at the establishment level offers several advantages. It allows us to examine the policy impact in a highly localized manner, specifically on close-by establishments. This approach also helps us to disentangle policy effects from establishment fixed effects, local business cycle effects, and other time-variant unobserved heterogeneity that could affect our estimates. Additionally, it enables us to disaggregate employment effects by worker and plant characteristics, facilitating an exploration of whether certain groups of workers or establishments were more affected by the government relocation. Identifying potential winners and losers of a relocation program would be valuable in designing effective policies.

The flip side of our approach is that it does not allow us to compute city-wide effects of the relocation program, even though we acknowledge that these effects may be important. Additionally, our research design cannot control for spatial substitution effects resulting from inflows of workers into the center of Berlin from peripheral areas or neighboring towns. Furthermore, our analysis focuses solely on potential local multiplier effects surrounding the workers' workplace. The relocation exercise may also have sparked local multiplier effects near the workers' home, which we cannot capture in our analysis.

Based on the UK experience, Swinney (2021) suggests that not only the size of a relocation, but also the exact location of the relocated jobs, matters for local economic development, arguing that placing jobs in the city center can maximize the potential benefits of a relocation project. Swinney's argument is premised on the idea that larger city centers offer better employment opportunities for all residents. In this study, we explore the impact of relocating government jobs to the center of Berlin, with destination sites primarily chosen for historical reasons. As per Swinney's argument, our estimates would provide an upper bound of the potential benefits of such policies.

Previous studies (see, e.g., Jefferson and Trainor, 1996) have shown that relocating public sector workers often aims to boost local employment, which can lead to non-random site selection and negatively affect identification. We argue that this concern holds less weight in our analysis, as the Bonn-to-Berlin relocation was not aimed at improving local economic conditions in specific Berlin areas. Our identification assumption rests on the premise that the selection of government and embassy buildings was largely driven by historical considerations, with a preference for occupying buildings of historical importance whenever possible. We find no evidence indicating that the decision on location was influenced by a desire to stimulate economic activity in areas of Berlin that were poor or declining or to locate near areas expected to bloom rapidly.

To estimate the effects of the German government relocation on private sector employment, we analyze data at the plant level and use three complementary models: a long-difference model, a dynamic specification with distributed leads and lags spanning a seven-year period, and an event study specification with varying treatment effects. Each specification has its strengths and weaknesses, and combining all three helps us to better identify the policy impact. We retrieve information on all Berlin establishments, including incumbents and new entrants, from the Establishment History Panel, an administrative data set assembled by the Institute for Employment Research. For this project, we combine the Establishment History Panel with geo-referenced address data to identify each establishment's location within Berlin and calculate its distance from any relocation site.

We find that the policy had a positive and significant impact on private sector employment within a 300-m radius of a relocation site, while private sector establishments located further away did not seem to benefit as much. Consistent with previous studies on public sector expansion (see, e.g., Jofre-Monseny et al., 2018; Faggio, 2019), this positive impact is largely driven by services with no change in manufacturing. Further decomposition of the main effect into sub-groups reveals that the policy positively affected media, tourism, and cafés & restaurants within the first 300 m. The hotel sector, in contrast, experienced expanding employment in the 300-500-m range, but not within the first 300 m.

We also contribute to the literature on public sector relocation by providing an extensive set of original results. We investigate the policy impact around the actual timing of the relocation episodes and find that the strongest impact is within the first 300 m and one year after the relocation episodes took place. Analyzing establishments by age reveals that new establishments are creating about 75% of the new jobs in services. Compared to the period preceding the move (1994–1997), the newly created jobs are disproportionally filled by women (up 7.1 percentage points), younger workers (21.6 pp), individuals in managerial and professional roles (16.6 pp), and those with lower education (11.9 pp). Conversely, jobs decreased for workers aged 35–49 (down 25.9 pp), in medium-skilled occupations (−9.7 pp), and with vocational training or high-school education (−13.1 pp).

Furthermore, we derive a measure of the local multiplier effect of approximately 1.3. Although this figure is not uncommon in the local multiplier literature (e.g., Moretti and Thulin, 2013), it is somewhat smaller than the estimates reported in previous studies on public sector relocation (e.g., Becker et al., 2021; Faggio, 2019). Becker et al. (2021) examine the relocation of the German government from Berlin to Bonn after WWII, which preceded the government return to Berlin analyzed in this paper and find a local multiplier of 1.86. Faggio (2019) analyzes a more recent relocation program in the UK and finds a local multiplier of 2.1.

Our results are consistent with models that stress demand linkages in local labor markets, e.g. those described in the economic base theory (see Thulin, 2015, for an overview). In these models, local production is split between a basic sector that produces for foreign markets and a non-basic sector that produces for local consumption. While empirical applications of these models typically define export-oriented private sector firms as the economic base and study the impact of changes in employment in these firms on total economic activity, the government move to Berlin can be interpreted as a poster child for an increase in the economic base: federal institutions consume local products, but their provision of government services is valuable nationwide. This study, therefore, circumvents some of the problems in this literature since it reliably delimits the economic base sector and identifies a shock which is largely exogenous to local agents.

The remainder of the paper is organized as follows. Section 2 clarifies the contribution of the paper to the existing literature. Section 3 provides an overview of the historical setting and details the relocation. Section 4 discusses our empirical strategy, while Section 5 describes the data used in the analysis and their sources. Section 6 presents the results and Section 7 concludes.

[3] We use the terms establishment and plant interchangeably throughout the paper.

2. Contribution to the literature

This paper contributes to four strands of literature. First, it contributes to a growing literature examining the interaction between public and private sector employment within a local labor market. A line of papers looks at periods of public sector expansion and finds a positive impact of such expansions on private sector employment, particularly in the non-tradable sector. Using employment data on 352 English local authorities during 2003–2007, Faggio and Overman (2014) find that public sector growth does not affect private employment but it changes the sectoral composition of local jobs towards services (non-tradables) and away from manufacturing (tradables). Jofre-Monseny et al. (2018) 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 0.9 jobs in the non-tradable sector while not affecting the tradable sector.

Another line of studies focuses on periods of public sector contraction and finds that a reduction of public employment stimulates local jobs in the private sector, particularly in the tradable sector. Senftleben-König (2015) explores the interaction of public and private sector employment within 412 German districts between 2003 and 2007, a period during which Germany's public sector employment on average contracted. She finds that reducing public employment by one unit triggers the creation of about 0.7 local jobs in the private sector, particularly tradables. Using municipality-level data, Auricchio et al. (2020a) examine the downsizing of public sector employment in Italy during the 2000s and find that a reduction of one public employee raises private employment by about 0.6–0.8 jobs, with the effect largely driven by manufacturing. When exploring the North-South divide, Auricchio et al. (2020b) find a larger impact for municipalities located in the lagging South showing that a reduction of one public employee crowds in 1.06 private employees, with the effect equally split between tradables and non-tradables. The impact in the North is instead less than half (−0.43) of that in the South and concentrated in tradables. Auricchio et al. (2020b)'s findings corroborate previous work by Alesina et al. (2001), which also documents how public employment discourages the development of the local private sector in the South of Italy.[4]

Studies that look at episodes of public sector relocation are probably the closest to us. Becker et al. (2021) evaluate the impact of public employment on private sector activity using the move of the German government to Bonn after WWII. They document a substantial increase in total employment in Bonn after 1949, comparing the new West German capital to a group of 40 control cities. They also document a positive and sizeable impact of government jobs on the city's private sector employment (local multiplier of 1.86), with the largest effect found in the non-tradable sector. Faggio (2019) analyzes the impact of a UK relocation program (the Lyons Review) using information on 150, 000 UK Census Output Areas. She finds that public employment has a positive impact on total private sector activity (multiplier of 2.1), with results mainly driven by services. She also finds that the program has highly localized effects that disappear quickly over distance.

Second, the paper contributes to the literature on local multipliers. Moretti (2010) quantifies the long-term impact on a city's tradable and non-tradable jobs generated by a permanent increase in tradable sector employment. He finds that, in the US, the creation of 100 jobs in one industry (defined at the 2-digit level) of the tradable sector increases employment in the non-tradable sector by 160 jobs (multiplier of 2.60), whereas it has no effect on other tradable industries. Moretti and Thulin (2013) compares US figures with corresponding ones in Sweden and find a smaller multiplier effect (1.48). Van Dijk (2017) confirms Moretti (2010)'s results, although he argues that estimates of the multiplier effect may vary depending on the choice of the base 2-digit industry relative to which estimates are computed. 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, this line of studies is mainly concerned with multiplier effects between tradable and non-tradable components of the private sector. Another stream of papers looks at the openings of Wal-Mart stores and their impact on local employment and prices (see, e.g., Basker, 2005a, 2005b, 2007; Pope and Pope, 2015). For instance, Basker (2005a) estimates large and positive direct effects of Wal-Mart openings on local retail employment in the first year of entry, which are cut in half after five years. She detects no spillover effects in retail industries in which Wal-Mart does not compete directly.

Third, 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. 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 economic activity within West Berlin following the city's division and reunification. Becker et al. (2021) is another study of this kind.

Fourth, this paper contributes to the growing literature on the spatial decay of agglomeration effects (see Rosenthal and Strange, 2020, for a useful survey). Rosenthal and Strange (2003, 2008) find that agglomeration economies related to business start-ups, new firm employment and wages fade quickly with distance. Similarly, Arzaghi and Henderson (2008) document significant but rapidly declining productivity gains from agency co-location in Manhattan's advertising industry. Andersson et al. (2004, 2009) show that university decentralization in Sweden leads to substantial but localized firm productivity spillovers. Ahlfeldt et al. (2015) report concentrated production and residential externalities using within-Berlin census block data, while Rossi-Hansberg et al. (2010) show that housing externalities in Richmond, Virginia, halve every 1000 feet. Baum-Snow (2020) similarly identifies that the construction of US highways reshapes urban spatial structure, concentrating production externalities in central city locations.

3. Historical setting

A Overview

When Germany lost WWII, the country was divided into four sectors administered by the Four Powers: the US, Russia, France and the UK. Similarly, the city of Berlin, which had been the capital of Germany from 1871 to 1945, was also divided. Cooling relations between the Western powers and Russia led to Germany's division in 1949, which solidified into the Federal Republic of Germany (FRG) and the German Democratic Republic (GDR). Both sides claimed Berlin, resulting in the situation shown in Fig. 1. Although Berlin was located within the GDR boundaries, about 130 km away from 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 become the capital again when the political situation changed, Bonn was chosen as the new capital and seat of the

[4] 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 sector 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.

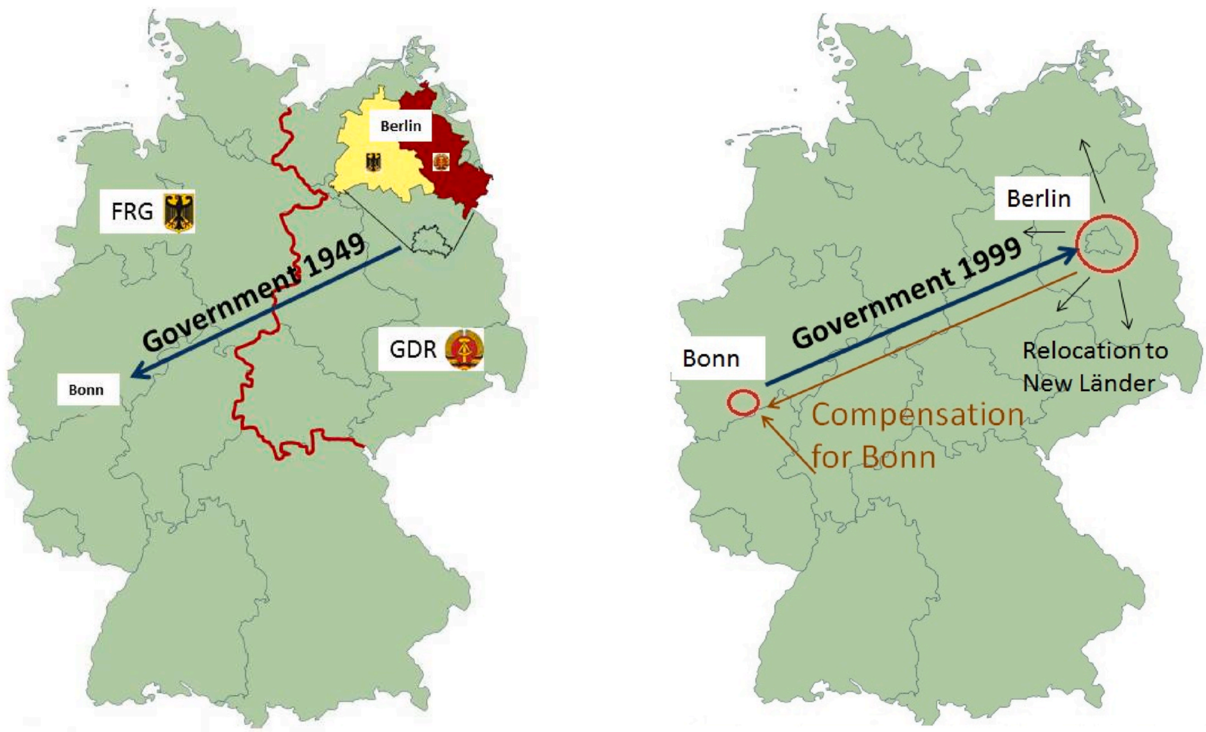

Fig. 1. Historic setting.
Note: Left Picture: Period of division lasting from 1949 to 1990. Right Picture: Implementation of the move of the government from Bonn to Berlin in 1999.
Source: authors' work using ArcGIS software; Layer: © GeoBasis-DE / BKG 2015.

FRG government.

This 'provisional' situation lasted until reunification in 1990, when a clause in the Unification Treaty signed by the GDR and the FGR agreed on Berlin becoming the capital of a united Germany once again. A year later, it was decided to move the seat of the government back from Bonn to Berlin. The decision was unpopular among entrenched vested interests and could only be reached by making large concessions to the city of Bonn to compensate for its loss of status and economic power. Part of the agreement was a 'fair division of labor' between Berlin and Bonn, which meant that core government functions would be relocated to Berlin, while the majority of government jobs would remain in Bonn. Additionally, Bonn would receive financial compensation, as well as new functions and institutions of national and international significance.[5] The ability of Bonn to secure large (financial and non-financial) concessions as a form of compensation makes it difficult to disentangle the impact of the government move on Bonn from that of other factors.

The initial plan was to move the government to Berlin within four years and fully complete the move within a maximum of twelve years (Deutscher Bundestag, 1991a), though details on the implementation of the move were left open. By 1992, it was evident that moving the core government functions within four years was unfeasible. Subsequently, there were prolonged discussions about the timing and cost of the move. One proposal suggested halting any further government-related investment in Berlin until the financial situation of the FRG had improved, while another suggested postponing the move until 2010. Additionally, a mass petition was organized to delay the decision on the move's date 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 begun to invest in Berlin. In November 1993, 40 national and international companies pointed at a breach of trust and the potential contractual obligation if the government ceased its effort to proceed with the move (Hoffman, 1998, p. 213).

The passing of the Berlin-Bonn Act (1994) provided statutory security about the move to Berlin, although 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 were to keep their first seat in Bonn and get a second seat in Berlin; nine ministries were to 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. Despite this, the timing of the move remained heavily debated in the following years. In September 1996, 50 MPs belonging to the SPD and BÜNDNIS 90/DIE GRÜNEN brought in a motion to postpone the move by at least 5 years. It was only in November 1997 that the Federal Parliament (Bundestag) announced a moving date: the government was to take up its work in Berlin in September 1999. A timeline summarizing the core events of the decision-making process is shown in Fig. 2.[6]

B The situation in Berlin during the 1990s

The end of the city's division came with the opening of the wall in November 1989, but Berlin's journey towards reunification was challenging. The city faced numerous difficulties, including high unemployment rates, especially among former East German workers; an outdated building stock that fueled a construction boom, quickly followed by a housing bust; and a declining urban population. During the first three years of reunification, total unemployment in Berlin remained at 10–12%. It rapidly increased from 12.1% in September 1993 to 18.9%

[5] For example, the Federal Competition Authority (Bundeskartellamt) was relocated to Bonn to provide alternative employment to employees of the Ministry of Finance (see Bornhöft et al., 2001).

[6] A more detailed description of the historical events and decision-making process is provided in Online Appendix A.

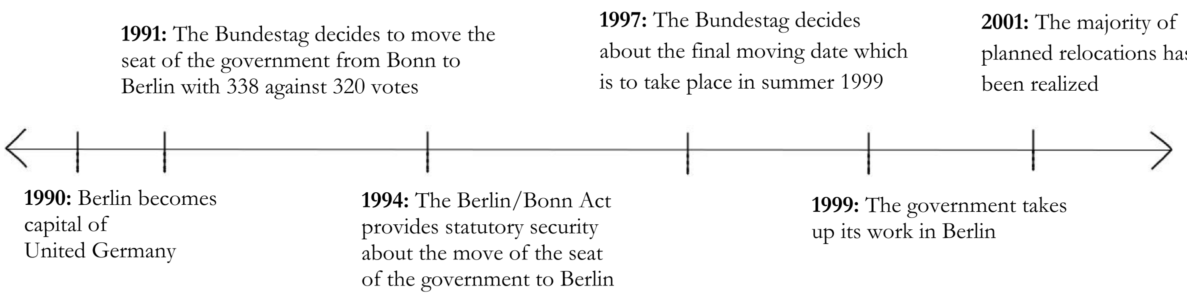

Fig. 2. Timeline of the decision-making process.
Source: Deutscher Bundestag, 1991a, 1991b and 2010, own representation.

in January 1998, remained around 17–18% in 2000 and then started to rise again, reaching 20.9% by February 2003 (Bundesagentur für Arbeit, 2005). Construction activity in Berlin peaked at about 2 million m^2 for non-residential developments and approximately 2.5 million m^2 for residential developments in 1997, two years before the Parliament's inauguration. According to official statistics, the percentage of vacant residential dwellings in Berlin increased from 8.6% in 1998 to 10% in 2002 (Statistisches Bundesamt, 2020), while the price index for non-residential buildings declined from a peak of 105.2 in 1996 to a trough of 98.6 in 2002 (Statistisches Landesamt Berlin, 2005). Moreover, Berlin underwent extensive suburbanization during the 1990s, with a significant proportion of the population moving to the suburbs (Kopske, 2004). Between 1991 and 2000, the city's population dropped from 3.45 million to 3.38 million, a decrease of about 2% or 70,000 residents.

C Location decisions within the city of Berlin

Location decisions for government institutions were heavily debated. While the airport of Tempelhof was suggested as a potential site for parliament due to the availability of unbuilt land, the final site chosen was the 'Spreebogen' in Berlin Mitte, with the Reichstagsbuilding as a focal point.[7] To keep relocation costs as low as possible, many ministries were accommodated in existing housing stock, some of which had historically hosted government functions of the GDR as well as the German Reich.

Several embassies utilized their former military missions, consulate generals, or branch offices until they could rebuild or construct a suitable building for their representation (Gehrcken, 2013). Despite the destruction of nearly all building stock in West Berlin between 1939 and1945,[8] many countries still owned parcels of land in Berlin that they had purchased over a century earlier. The former embassies in East Berlin closed in 1990 and were repurposed as consulates, with some later reopening as representations in a united Germany. By 2015, 163 countries (158 embassies and 5 honorary consulates) had representation in Berlin.

Our identification strategy hinges on a crucial institutional detail. Although relocated jobs were not randomly distributed across space, the choice of government and embassy buildings was largely driven by historical considerations, with a desire to occupy buildings of historical importance whenever possible. We found no evidence suggesting that the location decision was driven by a desire to stimulate economic activity in struggling areas of Berlin or to be near areas expected to flourish.

D The magnitude of the relocation

The Bundestag and the government officially started operating in Berlin on September 1, 1999. Table 1 gives an overview of the number of jobs relocated from Bonn to Berlin. The move involved about 15,000 government-related jobs, and an additional 10,000 positions related to foreign representations, media, political parties, and interest groups followed suit. At the same time, Berlin experienced a significant outflow of public sector jobs, about 7000 in total. Following the recommendations of a commission established to oversee the redistribution of federal offices across the federal states that were part of the GDR ('new Länder'), several Berlin-based institutions left the city and relocated to these new Länder. Additionally, Berlin lost several of its institutions to compensate Bonn for its employment losses (see Fig. 1, right panel). The sum of positive and negative job moves resulted in a net gain of about 18,000 jobs for Berlin. However, those jobs did not correspond to the number of relocated workers, as employees were given the option to: 1) follow their job; 2) take up a position in one of the federal institutions that remained in Bonn; or 3) relocate to Bonn as part of the city's compensation

Table 1
Number of relocated jobs.

Institutions	Number of jobs moved
Positions moved from Bonn to Berlin	
Ministries	9075
Bundestag, -rat,-präsidialamt	5276
Länder representations	626
Government-Related Jobs	**14,977**
Foreign representations	6300
Media, parties and interest groups	3,700[a]
Foreign and Media Related Jobs	**10,000**
Positions moved from Berlin to Bonn	
Federal and other institutions	−4054
Positions moved from Berlin to the New Länder	
Federal institutions	−2,927[b]
Positions Moved Out of Berlin	**−6981**
Total	**24,977–6981 = 17,996**[c]

[a] According to the Deutscher 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.
[b] As a federal country, Germany needs to balance the distribution of federal institutions across all federal states. The initial program involved the move of 4700 jobs out of Berlin to the New Federal States (New Länder), but some reallocations never materialized.
[c] 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.
Source: See Table B.1 in Online Appendix B for details.

[7] In this paper, we do not discuss what the chosen buildings would have been used for, had the government not moved its offices from Bonn to Berlin. The example of Tempelhof Airport, which remains vacant despite its size and central location, illustrates the uncertainty and variability in potential uses for sites and buildings of historical value and/or in disuse.

[8] 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).

measures. According to official documents (Deutscher Bundestag, 1999), roughly 34% of government employees decided to stay in Bonn; most of them were public sector workers of lower or middle grade.

E The timing of the relocation

The relocation period was spread out over several years, though the majority of jobs had moved by the end of 1999, as shown in Fig. 3. Government employees mostly moved between 1999 and 2000 (see Fig. 3, top-left panel). By the end of 2000, over 8000 ministerial employees and about 5300 employees of the administration of the federal parliament, parliamentary groups or deputies and their assistants had relocated to Berlin. All federal states equally established their representation in Berlin. Most embassies chose to be present in Berlin when the government took up its work in 1999, and many more arrived in the following years (see Fig. 3, bottom-left panel). The relocation of federal offices out of Berlin occurred over a slightly longer period, taking place between 1996 and 2003 (see Fig. 3, top-right panel).

4. Empirical strategy

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. To address the first concern, 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. (2017, 2021) and Faggio (2019), we construct treatment intensity variables as the number of relocated jobs in subsequent distance bands of 300, 500, 1000 and 3000 m starting from each establishment *i*'s geocoded location. The novelty of this study relative to the previous literature is that we conduct the analysis at the establishment level instead of the area level and, thus, we estimate the policy impact on the average plant *i* located at distance *d* from a relocation site, allowing effects to vary by distance. Conducting the analysis at the establishment level has three main advantages: (*i*) it allows us to investigate the policy impact very locally for close-by establishments; (*ii*) it allows us to better disentangle policy effects from establishment fixed effects, local business cycle effects and other time-variant unobserved heterogeneity that may confound our estimates, thus ensuring that we compare like with like; (*iii*) it allows us to detect whether the policy impact varies by worker and plant characteristics.

Regarding the second concern mentioned above, previous studies (see, e.g., Jefferson and Trainor, 1996) have shown that relocating public sector workers is often used as a tool for improving local employment conditions. This, in turn, implies that treated locations are not randomly chosen, but disadvantaged areas are more likely to be targeted, with obvious undesirable consequences in terms of identification. We argue that this concern is weaker in our analysis than in other studies as the original purpose of the Bonn-to-Berlin relocation was not to improve local economic conditions in specific Berlin areas. As documented in Section 3, the destination of relocated jobs in the center of Berlin was largely driven by historical factors.

Still, due to the sheer size of the relocation exercise, one of the necessary conditions was the availability of a sufficiently large number of offices or buildings suitable to be converted into office space and land area suitable for the construction of the main government buildings. In our empirical analysis, we partly address these concerns by controlling for time-invariant unobserved heterogeneity across Berlin areas. After dividing Berlin into 479 2-km-side grids, we include grid-specific constants (grid fixed effects) in our plant-level regressions.[9] In addition, we cluster standard errors at the grid level to allow for intra-grid correlation.

To estimate the effects of the German government move on private sector employment, we use three complementary models: (*i*) a long-difference specification, which uses a parsimonious regression model to provide a cumulative estimate of the policy impact; (*ii*) a dynamic specification with distributed leads and lags spanning seven years, borrowed from the minimum wage literature (see, e.g., Dube et al., 2010), which allows us to estimate changes in outcome around the actual time of the policy and provide evidence on medium-term effects; and (*iii*) an event study difference-in-differences specification with varying treatment, also borrowed from the minimum wage literature (see, e.g., Card, 1992; Dolton et al. 2012, 2015) and recently used in other contexts (see Fetzer, 2019; Braakmann and McDonald, 2020; Bray et al., 2022). The event study model enables us to directly control for preexisting trends and allows the estimated impact of the policy to vary over time, similar to the dynamic specification. Each of the specifications has strengths and weaknesses. Utilizing all three helps identify robust results.

For all specifications, we use a balanced panel of establishments located in Berlin between 1993 and 2005 that are within a 3 km radius of public sector relocations, measured from the geo-referenced address of the establishment. Our sample includes both incumbent establishments and new entrants during the sample period, totaling 142,875 establishments each year. By using a fully rectangularized version of the data set with the same number of annual observations, our estimates capture both the intensive (linked to incumbent establishments) and extensive (linked to new entrants) margins of the relocation policy. For new entrants, we replace missing employment data with zeros for the years preceding their entry.

We start with the long-difference specification:

$$\Delta emp_{i,1998-2002} = \sum_d \beta^d R^d_{i,1996-2001} + emp_{i,1998} + \Delta emp_{i,1994-1997} + \alpha_g + \Delta\varepsilon_{i,t} \tag{1}$$

where Δ denotes a long difference operator, i.e. $\Delta emp_{i,t} = emp_{i,t} - emp_{i,t-n}$. Specifically, $\Delta emp_{i,1998-2002}$ refers to the change in plant i employment between 1998 and 2002, while $R^d_{i,1996-2001}$ refers to the net number of relocated jobs faced by plant i within distance band d, with $d \in (300, 500, 1000, 3000\ meters)$. Since we consider both positive and negative flows of public sector workers, $R^d_{i,1996-2001}$ measures net changes over the period 1996–2001. Equation (1) also includes initial plant level employment ($emp_{i,1998}$) and a measure of pre-trends in the outcome variable ($\Delta emp_{i,1994-1997}$). Pre-trends are defined as the raw changes in plant employment between 1994 and 1997. Grid fixed effects (α_g) are added to control for time-invariant unobservables that are common to establishments located within the same grid area. The error term ($\Delta\varepsilon_{i,t}$) captures the impact of unobservable factors that vary over time and space. As mentioned before, standard errors are clustered at the grid level.

In equation (1), the outcome variable ($\Delta emp_{i,1998-2002}$) focuses on the period 1998–2002. As shown in Fig. 3 (bottom-right panel), net public sector job turnover in Berlin peaked 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 advantageous, especially in the case of Berlin, which underwent significant transformation during the 1990s and 2000s, reducing the likelihood that ongoing trends could confound our estimates.

Treatment intensity variables ($\sum_d \beta^d R^d_{i,t}$) capture the impact on average plant i employment of relocations occurring within distance

[9] In the analysis, we use a smaller number of grids than 479 since we only include establishments located within a 3-km distance from a relocation site.

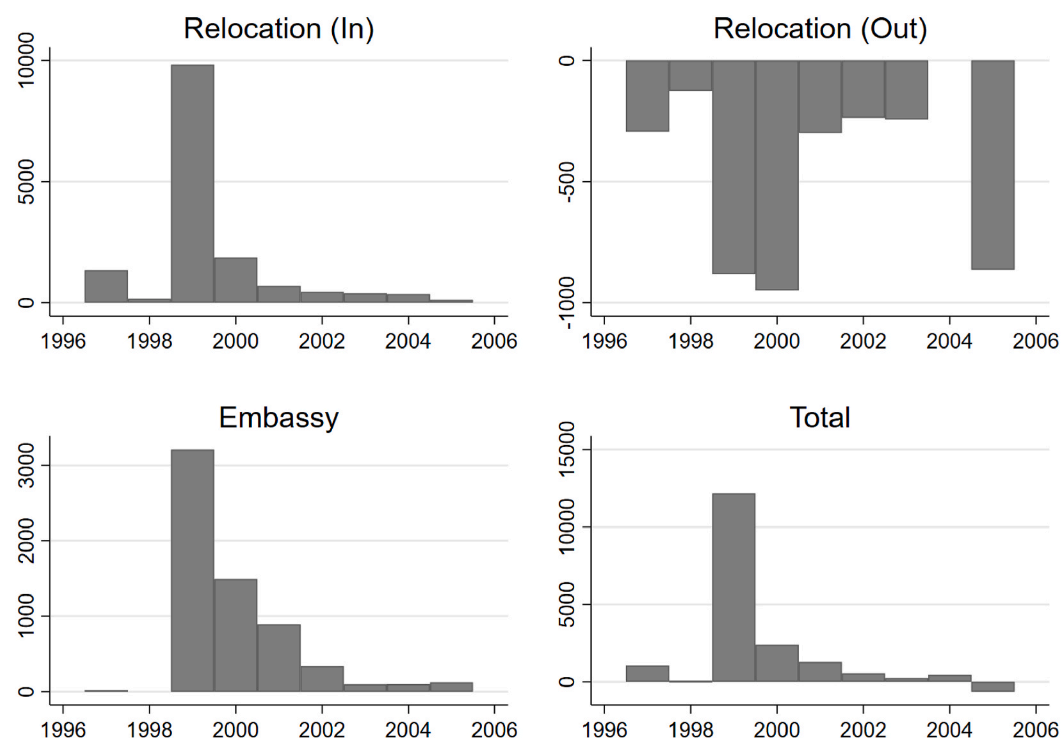

Fig. 3. Timing of the relocation program.
Note: Top-left panel shows government-related jobs that moved into Berlin; top-right panel shows jobs in federal institutions that moved out of Berlin; bottom-left panel shows embassy jobs that moved into Berlin; bottom-right panel shows net changes in total jobs.
Source: Data compiled by the authors; see Table B.1 in Online Appendix B for details.

bands d. We construct four subsequent distance bands of 300, 500, 1000 and 3000 m, starting from each establishment i location.[10] By geocoding the exact sites of institutions receiving (or losing) public-sector jobs, we can count the number of jobs falling within each ring. Thus, we assume 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.

The model specification indicated in Equation (1) has two features worth noting. First, it has no explicit control group in terms of distance bands. This is because treatment variables are measured in terms of relocation size. If these variables were defined in terms of any relocation occurring (e.g., using dummy variables that take 0/1 values) rather than the number of jobs moved, the 0–3 km band would effectively provide the baseline group. Second, Equation (1) includes treatment variables constructed in a cumulative way: $R^{300}_{i,t}$ refers to all relocations (and the associated job movements) within a 0–300 m distance band from plant i location; $R^{500}_{i,t}$ refers to all relocations within a 0–500 m distance band (including those in the 0–300 m ring) from plant i location; and so forth.

The main advantage of using a cumulative definition of treatment intensity variables is that it aligns with the basic notion that the local multiplier effect should become stronger as one gets closer to a relocation site. To interpret the β^d coefficients in this context, it is helpful to start with the outermost ring, $R^{3000}_{i,t}$. While $R^{3000}_{i,t}$ refers to all relocations within a 0–3 km distance range from plant i location, its coefficient, β^{3000}, effectively captures the marginal effect of an increase in the net number of relocated jobs between 1 and 3 km. Coefficients for closer bands (1000, 500, 300 m) then capture the marginal effects of relocations within each band relative to the band one step further out (e.g., β^{1000} relative to β^{3000}; β^{500} relative to β^{1000}; β^{300} relative to β^{500}). Using cumulative treatment intensity variables also enables us to directly test whether the multiplier effect indeed strengthens with proximity to a relocation site without requiring additional t-tests between β^d and β^{d+1} to verify whether the impacts of two treatment intensity variables are significantly different. In Appendix 1, we demonstrate that results from a cumulative specification are equivalent to those from a separate bin-by-bin specification and that coefficients can be easily transformed between the two approaches, if necessary.

The long-difference specification of Equation (1) provides a cumulative estimate of the policy impact over the sample period. Even though this compact presentation is useful, a dynamic specification would allow us to estimate changes in outcomes around the actual time of the relocations as well as provide evidence on medium-term effects. Moreover, it will add to the credibility of our research design by evaluating trends prior to the government move.

Borrowed from the minimum wage literature (see, e.g., Dube et al., 2010), our second specification considers a change in the number of relocated jobs, indicated as $R^d_{i,t-j}$, for a given establishment i within distance band d at time $(t-j)$ as a new relocation 'event', similarly to how minimum wage increases have been treated as new events affecting a given geographic area at a specific time. Consistent with this interpretation, we can identify numerous and overlapping relocation events in our sample. Therefore, we do not employ a pure event study approach using specific relocation episodes, but we follow Dube et al. (2010) and use a dynamic specification with distributed leads and lags spanning a seven-year period:

$$emp_{i,t} = \sum_{j\in[-3;+3]} \sum_{d} \beta^d_j R^d_{i,t-j} + \gamma_i + \delta_t + \varepsilon_{i,t} \tag{2}$$

[10] To assess the robustness of our results with respect to the 3 km threshold, we repeat this exercise using a threshold value of 5 km instead of 3 km. For the long-difference model outlined in Equation (1), where computational considerations play less of a role, we further conduct an estimation including all private sector establishments located in Berlin. Results are qualitatively equivalent and available upon request. They are part of the replication package supplementing the article.

where the outcome variable, $emp_{i,t}$, is plant i employment at time t; $t \in [1994;\ 2002]$ is time measured in years; $j \in [-3; +3]$ is an indicator for leads/lags; $R^d_{i,t-j}$ refers to the cumulative number of relocated jobs an establishment i faces within distance band d at time $(t-j)$. Obviously, this cumulative sum is null for years before the government move ($t-j < 1996$) and turns positive as jobs start being relocated ($t-\ j \geq 1996$). In estimating Equation (2), we use data in panel form and include both establishment fixed effects (γ_i) and year fixed effects (δ_t). In more demanding versions of Equation (2), we also include grid-specific year trends and grid-specific year fixed effects. As in Equation (1), we allow for spatial autocorrelation by clustering standard errors at the grid level. $\varepsilon_{i,t}$ captures the error term.

The main advantage of using Equation (2) relative to the first specification is that it allows us to estimate the policy impact in a dynamic way. It helps us understand the timing of the effects around the occurrence of each relocation episode. Ideally, we would like to find no impact for years before a relocation. As argued by Deryugina (2017), pre-event coefficients in specifications similar to Equation (2) help assess the presence of pre-trends, although they do not control for them.[11] Equation (2) also helps us evaluate any delay or persistence of the effects by estimating post-event coefficients up to 3 lags. Moreover, the framework flexibly handles the fact that the timing of relocations is somewhat spread out (see Fig. 3) and relocations might have occurred in consecutive steps at a given site.

Our third approach uses an *event study* difference-in-differences estimator with a varying treatment variable, similar to those traditionally used in the evaluation of minimum wages (see the seminal work by Card, 1992; and subsequent applications, e.g., Dolton et al., 2012, 2015) and recently used, e.g., in evaluating the impact of UK austerity measures on Brexit (Fetzer, 2019) or hate crime (Bray et al., 2022). Model (3) can be described as follows:

$$emp_{i,t} = \sum_d \beta^d R^d_{i,1996-2001} + \sum_t \sum_d \mu^d_t \left(R^d_{i,1996-2001} \times \delta_t \right) + \gamma_i + \delta_t + \varepsilon_{i,t} \qquad (3)$$

where $emp_{i,t}$ refers to plant i employment at time t, with $t \in [1994;\ 2002]$; $R^d_{i,1996-2001}$ is the net number of jobs moved between 1996 and 2001 faced by establishment i within distance band d (defined exactly as in Equation (1)); γ_i and δ_t refer to plant and year fixed effects, respectively; and $\varepsilon_{i,t}$ is the error term. Equation (3) also includes an interaction term between $R^d_{i,1996-2001}$ and year fixed effects. As in previous specifications, standard errors are clustered at the grid level. In augmented versions of Equation (3), we experiment with grid-specific year trends and grid-specific year fixed effects.

The main feature of Equation (3) consists in the use of a treatment variable defined as $R^d_{i,1996-2001}$ in a panel data estimation model. $R^d_{i,1996-2001}$ varies by establishment i and distance band d, but it does not vary over time for any combination of establishment and distance band.[12] By including the interaction term between this variable and year fixed effects, Equation (3) estimates the policy impact (for the average plant i at distance band d) for all years preceding and following its implementation. Obviously, for the internal validity of our estimates, we would like to find no policy impact for the years preceding the government move. The main advantage of this specification is that it allows us to verify the existence of pre-trends in the outcome variable and to control for them. It also provides a quick alternative estimate of the medium run effect of public sector job relocations on local private employment.

5. Data

A Sources

Information on employment is retrieved from the weakly anonymous Establishment History Panel (BHP)[13] (see Schmucker et al., 2016 for a detailed data description). The dataset is assembled by the Institute for Employment Research (IAB) and holds information on all German establishments employing at least one worker on social security records on June 30 of any given year.[14] The time span of the panel ranges from 1975 to 2014 for former West Germany and from 1991 to 2014 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, employment type, occupation (1 digit), highest educational achievement and nationality. Additional variables include a time-consistent industry classification code (3 digits) as well as dates of market entry and exit. Given the availability of data on establishment entry and exit, the BHP fully tracks incumbent establishments, exits, and entrants each year.

For this project, we restrict the BHP data to establishments located in Berlin between 1993 and 2008. For this selection, we use a separate database with establishment-specific geo-referenced address information to group establishments into anonymized 2 km-side grids and calculate, for each establishment, the number of relocated public sector jobs within several distance bands (300m, 500m, 1 km and 3 km). The available address data allows us to merge this additional geographic information for more than 98% of BHP establishments representing more than 97% of the BHP workforce, starting in 1999.

The address data are not available before 1999. This reduced time span creates a potential obstacle to our estimation strategy. Since the government move mainly occurred between 1999 and 2001, the combined data set does not initially seem to cover the period before the policy implementation. To overcome this obstacle, we proceed as follows: 1) we assume that establishments do not change their address[15] and focus on existing establishments in 1999, tracing them back to the year they entered the BHP panel.[16] This leaves us with the problem of plant exit before 1999, as we cannot attribute a geo-referenced address to an establishment that left the panel before 1999; 2) in Section 6.E, we provide additional evidence of firm openings and closings before 1999 using data at the level of Berlins ‘Bezirke’ (23 city districts) retrieved from the Statistical Office of Berlin-Brandenburg. Evidence suggests that plant exit played a limited role in Berlin before 1999.

As mentioned in Section 4, the final sample consists of 142,875 establishments annually between 1993 and 2005, located within a 3 km

[11] Deryugina, T. (2017) also argues that the presence of pre-trends does not invalidate the idea that relocations are exogenous. One can still estimate the causal effect of a relocation as long as nothing is changing differentially for the treated and control groups following a relocation that is not caused by the relocation itself. In other words, one can relax the parallel trends assumption and still estimate the treatment effect.

[12] Hence, the first term in Equation (3) drops out in the estimation.

[13] IAB Establishment History Panel (BHP) 1975–2014 version 1, total population.

[14] This sentence states the condition for an establishment to be included in the BHP. The BHP is influenced by establishment dynamics, with new establishments being added and existing ones exiting each year.

[15] Using our final data set, we calculate a percentage estimate for likely address changes of 0.5% for the two-year period 1998–1999 and 4% for the three-year period 1997–1999. Online Appendix C.1 provides an extended discussion on the likely direction and magnitude of any bias linked to addresses potentially misclassified.

[16] We found that 42% of 1993 establishments (representing 23% of FTE jobs) have no geocode because they were not in the panel in 1999 or after. This proportion falls quickly and reaches 10% (4% of FTE jobs) for 1998 establishments.

radius of public sector relocations in Berlin.[17] Due to missing address data before 1999, we exclude plants that both enter and exit the BHP panel any time between 1994 and 1998, as well as 1993 incumbents exiting before 1999. By using a balanced sample, our estimates account for both intensive (incumbents) and extensive (new entrants) margins. Missing employment data for new entrants is replaced with zeros for pre-entry years, and zeros are assigned post-exit for establishments exiting the data set between 1999 and 2005.

We conduct the analysis at the plant level with establishment size as our main variable of interest. We measure establishment size by the number of jobs in terms of full-time equivalents (FTEs), considering both jobs that are subject to social security contributions and 'marginal jobs' that are not. Marginal jobs are jobs with monthly earnings below a government-chosen threshold, which is adjusted from time to time.[18] The BHP does not have information on actual hours worked. Still, to build our FTE measure, we define a part-time job to be equivalent to 23/38 h of a full-time job and a marginal job to be equivalent to 9/38 h of a full-time job.[19] As a result, our FTE employment measure is computed as the weighted sum of full-time, part-time, and marginal jobs at the establishment level. In the analysis that follows, when looking at worker characteristics, we will also define total employment as headcount.

A potential drawback of using the BHP is that address and worker information are not available separately for every branch of an establishment located in Berlin. This is because the German social security notification system assigns one establishment ID number and one address to firms that have several sites or branches (*i*) in the same municipality and (*ii*) that operate in the same Economic Class according to the 1993 Standard Classification of Economic Activities.[20] For example, multiple branches of the same supermarket chain within Berlin appear as just one establishment with one address in our data, presumably that of the head office. If head offices were mostly located in the city center while branches were spread across peripheral areas, we would overestimate employment in the center. While we do not have a good estimate of how large this measurement error is, we acknowledge that it could be non-negligible and affect our results. However, in Online Appendix C.2, we explain why our main results likely represent lower bounds of the true effect and provide evidence that suggests this is true.

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 was 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 embassy workers covering administrative or technical support positions, we assumed that their number is proportional to the number of diplomatic staff and derived an estimate of 6300 workers.[21]

Fig. 4 shows the spatial distribution of the relocation program across Berlins 'Bezirke' (23 city regions). The map shows aggregate numbers of jobs moved over the sample period by the institutions receiving or losing the relocated jobs within 500 m-side grids. Net employment changes range from −813 employees in an area that lost an important federal institution to about 5200 employees in an area in 'Berlin Mitte', a centrally located district where most historical buildings are found. Shaded in grey are the city regions that received the largest number of relocated jobs, i.e., 'Berlin Mitte' in the former East and 'Tiergarten' in the former West of the city.

B Plausibility check

In this section, we conduct a plausibility check to verify the accuracy of our employment data. Specifically, we compare the employment data we collected from independent sources with the BHP data on public sector employment. This serves as a form of first-stage estimation or an evaluation of the 'bite' in a difference-in-differences framework. Our research design begins by asserting that public sector employment increased in certain Berlin locations, and we aim to verify this claim.

Using a specification similar to Equation (1), we regress the changes in public sector and special interest group employment at plant i (from 1998 to 2002) on the number of relocated jobs associated with the government move and collected through independent sources. We define public sector establishments as those operating under industry codes SIC75 (Public administration and defence) and SIC99 (Extra-territorial organizations and bodies) of the 1993 Standard Industry Classification of Economic Activities. Similarly, we define special interest group establishments as those operating under code SIC91 (Activities of membership organizations) of the same classification.[22]

Our expectations are as follows: if the BHP data comprised all public sector jobs and our data collection were exhaustive, we would expect a 1:1 correspondence between the two data sets. In practice, this is not the case because senior civil servants and foreign embassy personnel are not subject to German social security regulations and, thus, they do not

[17] To assess the robustness of our results with respect to the 3 km threshold, we repeat this exercise using a threshold value of 5 km instead of 3 km. For the long-difference model outlined in equation (1), where computational considerations play less of a role, we further carry out a regression including all private sector establishments located in Berlin. Results are qualitatively equivalent and available upon request. They can also be found in the replication package supplementing the article.

[18] The threshold was Euro 325 between 1999 and 2002 and Euro 400 between 2003 and 2012.

[19] Looking at official statistics, average weekly hours of work are 38, 23, and 9 for full-time, part-time, and marginal workers, respectively. To account for any possible measurement error in the calculation of FTEs, we check whether our results are qualitatively robust to specifications that measure establishment size by full-time employment instead of full-time-equivalent employment.

[20] If a firm, instead, operates across municipalities, each of its branches will receive a different ID. Moreover, if a firm has several branches with different sector affiliations, each will receive a different ID, even though they are located in the same municipality.

[21] Online Appendix B provides an overview of the data sources used and detailed information on the estimation procedure for the embassy personnel.

[22] We also experiment with using the 1973 Standard Industry Classification of Economic Activities. In doing so, we define public sector and special interest group establishments as those operating in 3-digit codes: 910–912 (public administration), 920 (defense), 930 (social insurance), 940 (extra-territorial organizations and bodies), 870–872 (business and professional organizations), 881–882 (political parties; scientific and cultural organizations), 890 (churches and fraternities). Results are qualitatively equivalent and available upon request.

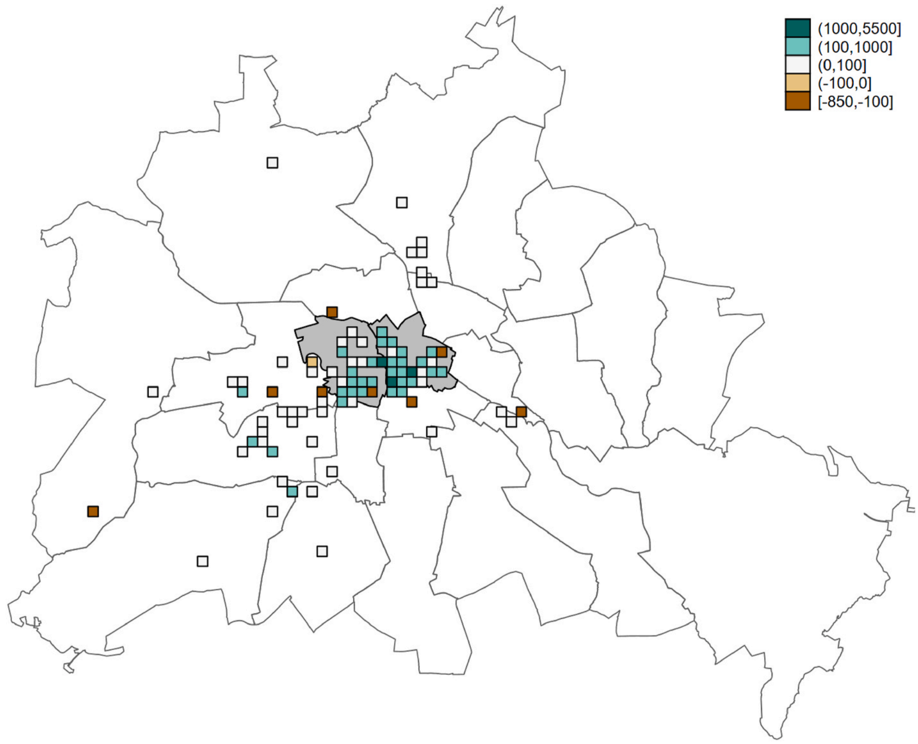

Fig. 4. Distribution of relocated jobs by Berlin 'Bezirke' (23 city districts).
Note: The light-grey borders indicate the borders of 23 Berlin 'Bezirke' (city districts) before the 2001 administrative reform which reduced the number of Bezirke to 12.
Source: Own representation; Data compiled by the authors, see Table B.1 in Online Appendix B for details; Layers: RBS-Ortsteile © Amt für Statistik Berlin Brandenburg 2016, CC BY 3.0 DE, © GeoBasis-DE / BKG 2015.

appear in the BHP.[23] This also limits the use of BHP data to effectively measure changes in public sector employment brought about by the relocation. Consequently, we anticipate a less than 1:1 numerical correspondence. Instead, we expect a strong geographic correspondence, with both data sets capturing the same locations that received relocated jobs. To verify this, we adjust Equation (1) by including distance bands of 100, 300, 500 and 3000 m, expecting significant effects mainly within the first distance band. Results are presented in Table 2.

Table 2 is organized as follows: Column (1) reports baseline results without controls; Column (2) adds initial employment and pre-trends; Columns (3)–(5) include initial employment, pre-trends and grid fixed effects. Pre-trends are defined as changes in the dependent variable from 1994 to 1997. The dependent variables used are SIC75 and SIC99 employment in Columns (1)–(3); SIC75, SIC91 and SIC99 employment in Column (4); and SIC91 employment in Column (5). Standard errors are clustered at the grid level.

Looking at Table 2 (Columns 1–3), we observe a statistically significant increase in public sector employment (SIC75 and SIC99) within 100 m of a relocation site. A coefficient of 115.06–121.99 (statistically significant at the 1% level) indicates a strong correspondence between our independently collected relocation data and the BHP public sector employment data. The estimated coefficient captures the average impact (multiplied by 1000)[24] on establishments operating in SIC75 or SIC99 located within the first 100 m of a relocation site, compared to corresponding public sector establishments located within the 100–300 m range. In addition, we observe a negative and statistically significant coefficient for the 300–500 m ring. This suggests that public sector agencies located between 300 and 500 m from a relocation site experienced a slight reduction in employment compared to agencies located further away (500–3000 m). This finding is consistent with the evidence presented in Fig. 4 and Table 1, which shows the relocation of federal institutions out of Berlin as government officials moved to the capital. It also reflects the broader reorganization of public sector and embassy employment within Berlin at that time.

Using the correspondence described in Appendix 1, we can derive the estimated number of added public sector jobs in the direct vicinity of a relocation site implied by the BHP data. Multiplying 104.92/1000 by the number of public sector establishments located within 100 m of a relocation site (397) and the average number of relocated jobs within the first distance band (192.3), we obtain a total effect of about 8010 employees.[25] This number is substantial but lower than the total number of relocated jobs derived using independent sources (about 18,000; see Table 1). This discrepancy is expected due to the exclusion of certain government officials and embassy personnel from the BHP data.

For special interest groups (see Table 2, Column 5), we find that the policy impact is still positive, statistically significant, and highly localized. Nonetheless, the estimated coefficient for SIC91 employment at 15 (se 0.822) is eight times smaller than the estimate for SIC75 and SIC99 employment. This is not surprising since SIC91 includes organizations like political parties, trade unions, industry lobbying groups, consumer interest groups, which are involved in government activities but are not formally part of it. While the relocation program affected these groups, the impact was more subdued.

Table 2 illustrates the cumulative policy impact during the sample period. To better understand the timing of these effects, we use Equation

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

[24] All estimates have been multiplied by 1000 to increase clarity and readability.

[25] The raw (not relative) marginal effect is 121.99 - 5.40 - 8.38–3.29 = 104.92. We then have $0.105 \times 397 \times 192.3 = 8016.03$. Both 397 and 192.3 are 2001 figures.

Table 2
Plausibility check - the impact of 1996–2001 cumulative relocations on (1998–2002) changes in public sector and special interest group employment.

Dep. Variable	SIC75,99	SIC75,99	SIC75,99	SIC75,91,99	SIC91
	(1)	(2)	(3)	(4)	(5)
0–100 m	119.242***	115.061***	121.987***	62.183***	14.997***
	(22.592)	(24.258)	(19.316)	(5.751)	(0.822)
0–300	−2.909	−0.838	−5.395	−0.156	−2.661
	(5.829)	(8.401)	(7.894)	(3.389)	(3.585)
0–500	−8.385**	−7.567*	−8.384***	−1.545	3.957
	(3.465)	(3.995)	(2.874)	(1.365)	(3.974)
0–3000	2.819*	0.575	−3.286	−0.186	0.609
	(1.429)	(1.003)	(3.704)	(1.009)	(0.384)
Constant	−22.579	15.733	40.705	10.144	−1.23
	(15.642)	(13.200)	(28.444)	(8.019)	(2.168)
$emp_{i,1998}$		✓	✓	✓	✓
Pre-trends		✓	✓	✓	✓
Grid fixed effects			✓	✓	✓
Observations	1015	1015	1015	3068	2043
# of clusters	69	69	69	93	93
R^2	0.025	0.391	0.421	0.383	0.100

Note: Robust standard errors are reported in parentheses; (*), (**), (***) indicate significance at the 10%, 5% and 1% levels, respectively. The dependent variable used in Columns (1)–(3) is SIC75 (Public administration and defense) and SIC99 (Extra-territorial organizations and bodies) employment; the dependent variable used in Column (4) pulls together employment in SIC75, SIC99 and SIC91 (Activities of membership organizations); the dependent variable used in Column (5) is SIC91 employment. Employment is defined as full-time equivalent (FTE). Column (2) includes initial (1998) plant employment and pre-trends, which are defined as (1994–1997) changes in the dependent variable. Column (3) adds grid fixed effects. Columns (4) and (5) use the full set of controls. In all specifications, standard errors are clustered at the grid level. All estimates have been multiplied by 1000 to improve readability.
Source: BHP 1975–2014 version 1, total population; relocation data are collected from several sources (see Table B.1 in Online Appendix B for details).

(2). Results from this dynamic specification are shown in Fig. 5, Panel A. The chart shows that the main policy impact occurs immediately, with some lagged effects at time $t+1$ and $t+2$ (i.e., at time L and L_2 in Fig. 5, Panel A) within a 100-m distance ring. There is also a smaller negative effect at time $t+1$ within 300–500 m. Consistent with Table 2, Fig. 5 (Panel A) indicates significant effects primarily within 100 m and 300–500 m of a relocation site.

The dynamic specification just described helps assess pre-existing trends in the outcome variable, but it does not directly control for them. For this, we use an event study difference-in-differences specification with varying treatment effects, as presented in Equation (3). This specification estimates the policy impact for each year following the government move and conducts a placebo test for pre-trends in each preceding year.

Results of the third specification, reported in Fig. 5, Panel B, confirm our previous findings. There is a strong correspondence between the independently collected relocation data and public sector employment recorded in social security rolls. Effects are highly localized, with significant impacts within 100 m and, to a lesser extent, within 300–500 m of relocation sites. The timing of the relocation episodes is credible, with positive and statistically significant effects from 1999 onwards, increasing over time and peaking in 2002 when the relocation program was nearing completion.

Two points about our third specification are worth noting: (*i*) yearly estimates are expressed relative to 1995, the benchmark year; (*ii*) the estimation uses a plant-distance varying treatment (fixed over time for plant *i* and distance *d*), which consists of the total number of jobs relocated between 1996 and 2001 that plant *i* faces at distance *d*. In terms of interpretation, these choices imply that an estimated coefficient of 125.34 (se 6.724) for year 2002 and 100-m distance band (see Fig. 5, Panel B) reflects the impact on the change in public sector employment from 1995 to 2002 of all relocations between 1996 and 2001 (with the effect computed relative to the next outer ring, 100–300 m, in 2002). Similarly, an estimated coefficient of 89.12 (se 20.266) for year 2000 and 100-m distance band captures the impact on the change in employment from 1995 to 2000 of all relocated jobs from 1996 to 2001 (relative to the 100–300 m ring in 2000). Unsurprisingly, the latter estimate is smaller than the former, as the 2000 coefficient does not yet incorporate the effects of job moves that occurred in 2001 (and in the second half of 2000), which are expected to be positive.

We were concerned about attributing all relocated jobs to an institution's primary address due to potential measurement error. However, this plausibility check shows no severe spatial measurement error was introduced. One possible explanation is that most jobs likely targeted the institution's primary location, with a small fraction moving to secondary locations. Our data may still suffer from other measurement errors, such as inaccuracies in computing embassy jobs (see Online Appendix B for details). These errors tend to bias our estimates towards zero, making the estimates in Table 2 a lower bound of the actual effect.

To summarize, the evidence presented so far confirms the validity of our data collection. Using three alternative specifications, we show a robust geographic correspondence between our relocation data and public sector employment recorded in social security rolls. We are thus confident that the data we collected, and the addresses attributed to each institution accurately reflect the spatial distribution of the actual employment shock.

6. Empirical analysis

A Main results

Table 3 shows estimation results of Equation (1) for the change in total private sector employment between 1998 and 2002. The table is organized similarly to Table 2, with Column (1) reporting the baseline specification and Column (3) including the full set of controls. Results indicate a positive impact of relocations on private sector employment within 300 m of a relocation site. Columns (1) and (2) also show some statistically significant effects within 1 and 3 km, although these effects are much smaller and disappear when the full set of controls is included (see Column 3).

Looking at Column (3) of Table 3, a coefficient of 2.534 (se 0.644) indicates that the arrival of 1000 relocated public sector jobs in Berlin increased average employment at a typical private sector establishment (plant *i*) located within 300 m of a relocation site by about 2.5 jobs, compared to establishments located slightly further away (300–500 m). The local spillover effect therefore dies off very quickly. To understand the magnitude of this effect, we can use the raw marginal effect ($2.53 + 0.15 + 0.28 + 0.05 = 3.01$) and compute the total impact for private sector establishments located within the first distance band. With an average of 150.2 jobs relocated within 0–300 m, employment increased

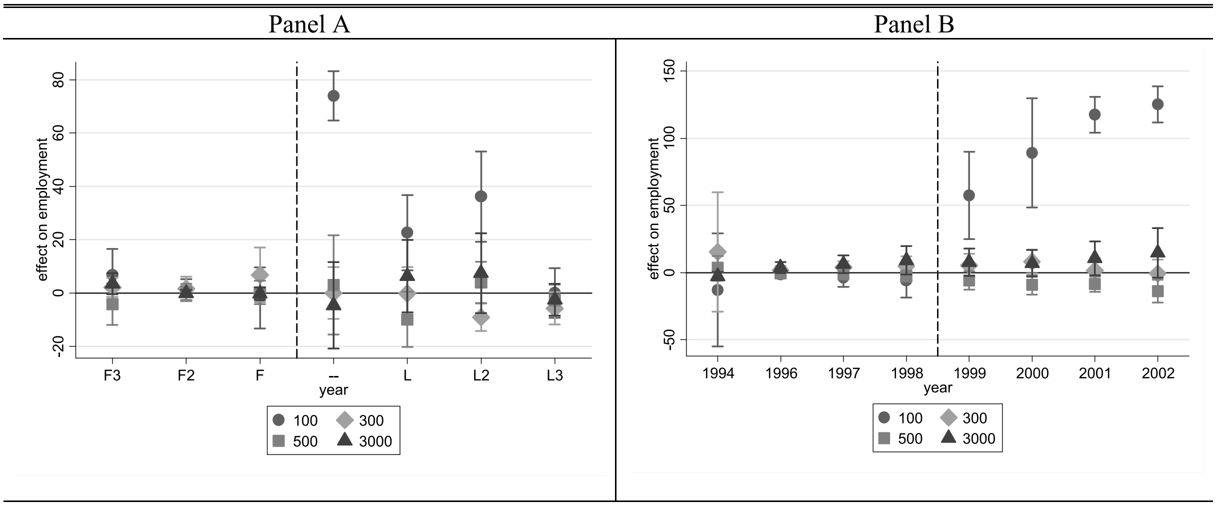

Fig. 5. Plausibility check – the impact of relocated jobs on public sector employment using a dynamic specification (Panel A) and event study specification with varying treatment effects (Panel B).

Note: Panels A and B are confidence bar charts, with marks indicating estimates and bars showing 95% confidence intervals. Panel A reports results of a dynamic specification with leads and lags spanning a seven-year period (see Equation 2 in the main text); Panel B reports results of an event study specification with varying treatment (see Equation 3 in the main text). All specifications include plant fixed effects and (grid × year) fixed effects. Standard errors are clustered at the grid level. The dependent variable is public sector employment recorded in codes SIC75 (Public administration and defense) and SIC99 (Extra-territorial organizations and bodies) of the 1993 Standard Industrial Classification of Economic Activities. Number of observations: 9130 in both Panels A and B. All estimates are multiplied by 1000.

Source: see Table 2 for details.

Table 3
The impact of 1996–2001 cumulative relocations on (1998–2002) changes in private sector employment.

	Private sector			Manufacturing	Services
	(1)	(2)	(3)	(4)	(5)
0–300 m	2.363***	2.415***	2.534***	0.048	2.486***
	(0.673)	(0.691)	(0.644)	(0.098)	(0.642)
0-500	−0.083	0.122	0.152	0.08	0.072
	(0.378)	(0.335)	(0.315)	(0.073)	(0.329)
0-1000	0.281***	0.228*	0.277	−0.017	0.293*
	(0.099)	(0.119)	(0.178)	(0.023)	(0.169)
0-3000	0.049**	0.083***	0.048	0.016	0.032
	(0.023)	(0.021)	(0.039)	(0.017)	(0.047)
Constant	−0.058	0.849***	0.930***	0.152	0.778***
	(0.094)	(0.151)	(0.176)	(0.219)	(0.220)
$emp_{i,1998}$		✓	✓	✓	✓
Pre-trends		✓	✓	✓	✓
Grid fixed effects			✓	✓	✓
Observations	88,144	88,144	88,144	88,144	88,144
# of clusters	211	211	211	211	211
R^2	0.001	0.300	0.301	0.158	0.177

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 private sector employment, including codes SIC15-SIC37 (Manufacturing), SIC45 (Construction), SIC50-SIC74 (Services), SIC92 (Recreational, culture and sporting activities) and SIC93 (Other service activities). The dependent variable in Column (4) is SIC15-SIC37 employment; the dependent variable in Column (5) is SIC45, SIC50-SIC74, and SIC92-SIC93 employment. Column (2) includes initial (1998) plant-level employment and pre-trends, which are defined as (1994–1997) changes in the dependent variable. Column (3) adds grid fixed effects. Columns (4) and (5) use the full set of controls. In all specifications, standard errors are clustered at the grid level. All estimates have been multiplied by 1000 to improve readability.
Source: see Table 2 for details.

by about 0.45 jobs per establishment ($[3.01/1000] \times 150.2 = 0.45$). Multiplying this by the total number of private sector establishments within 300 m (12,954), we estimate a total effect of 5829 jobs. This indicates that the German government move from Bonn to Berlin of the late 1990s (involving about 18,000 jobs) triggered the creation of approximately 5800 jobs in nearby private sector establishments. These figures correspond to a local multiplier of 1.32.

The policy impact comes entirely through job creation in the service sector. Columns (4) and (5) of Table 3 show the results for manufacturing and services separately. Column (4) shows no effect of public employment on manufacturing, which is not surprising given Berlin's smaller and declining share of manufacturing during the period studied[26] as well as the weak input-output linkages between the government and the manufacturing sector. Conversely, job changes in services (see Table 3, Column 5) closely mirror those reported for total private employment.[27] Column (5) also implies a positive impact on private sector establishments located within 300–1000 m of a relocation site, though the estimated coefficient is smaller and statistically significant only at the 10% level.

To better understand the policy impact around the actual time of the relocation episodes, we turn to the dynamic model of Equation (2). Our preferred specification, which includes establishment, year, grid and grid × year fixed effects, is shown in Fig. 6, Panel A. Results show a positive but not statistically significant coefficient of 0.506 (se 0.626) for the first 300 m at time $t = 0$, i.e. the time at which the relocation episodes occurred. Significant effects are observed with a one-year lag (indicated as time L in Fig. 6, Panel A), with the strongest impact within the first distance band. One year after the relocation episodes took place, average employment at plant i remained largely unaffected for relocations beyond 1000 m, increased slightly by 0.179 jobs (se 0.059) within 500–1000 m, and decreased by about 0.351 jobs (se 0.120) relative to that within 300–500 m. The main effect, with an increase by about 2.205 jobs (se 0.565) relative to the 300–500 m band, is again

[26] Manufacturing went from 22.5% of the size of the service sector in 1998 to 20% in 2002.

[27] We also experiment with applying the 1973 Standard Industry Classification of Economic Activities. However, this classification does not clearly differentiate between manufacturing and service activities. Therefore, we decided to use the 1993 SIC classification as our preferred classification throughout the paper.

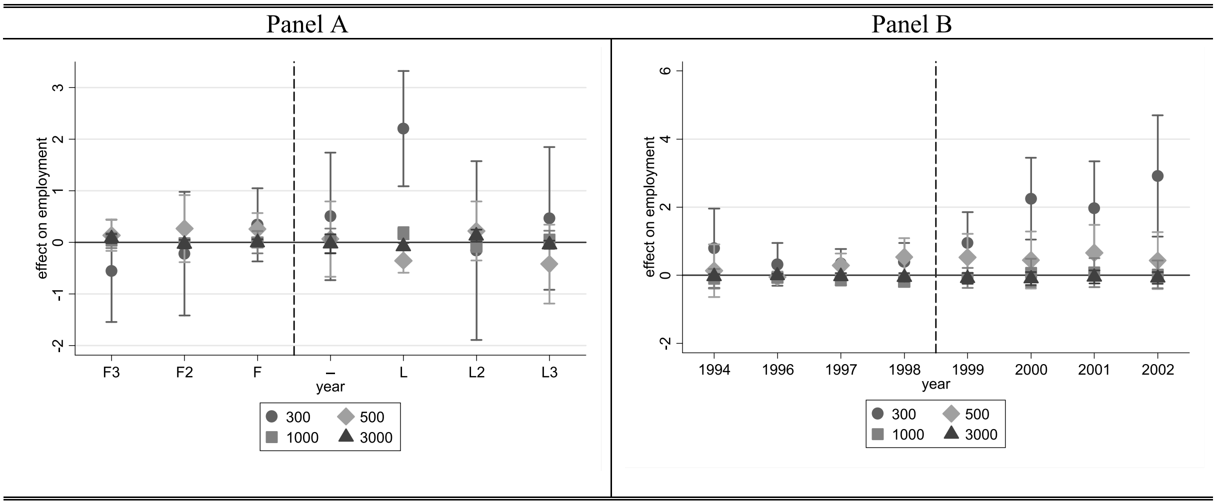

Fig. 6. The impact of relocated jobs on private sector employment using a dynamic specification (Panel A) and event study specification with varying treatment effects (Panel B).

Note: Panels A and B are confidence bar charts, with marks indicating estimates and bars showing 95% confidence intervals. Panel A reports results of a dynamic specification with leads and lags spanning a seven-year period (see Equation 2 in the main text); Panel B reports results of an event study specification with varying treatment (see Equation 3 in the main text). All specifications include plant fixed effects and (grid × year) fixed effects. Standard errors are clustered at the grid level. The dependent variable is private sector employment recorded as employment in codes SIC15-SIC37 (Manufacturing), SIC45 (Construction), SIC50-SIC74 (Services), SIC92 (Recreational, culture and sporting activities) and SIC93 (Other service activities). Number of observations: 793,401 in Panel A and 793,397 in Panel B. All estimates are multiplied by 1000.

Source: see Table 2 for details.

observed within the first 300 m.

By focusing on time $t+1$, the average effect for an establishment within 300 m can be derived by multiplying 2.205–0.351 + 0.179 + 0.0 = 2.003 by the average number of jobs relocated within that distance (150.2). This results in an impact of 0.301 jobs per establishment ([2.003/1000] × 150.2). Multiplying this by the total number of private sector plants within 300 m (12,954), we estimate a total effect of 3897 jobs (67% of the previously estimated 5829 jobs). These results suggest the impact of the relocation program is highly localized and mostly felt within the first two years.

Fig. 6, Panel A, reassuringly shows no significant effects leading up to a relocation episode. To formally control for pre-existing trends, we turn to the event study model as expressed by Equation (3), with results reported in Fig. 6, Panel B. We find no pre-trends between 1994 and 1996 across all distance bands. Small but significant effects are observed in 1997 and 1998 for the 1000-m ring, with coefficients of −0.152 (se 0.069) and −0.195 (se 0.059), and in 1998 for the 500-m ring (0.539; se 0.281). These findings are consistent with Fig. 3, showing some job movements in and out of Berlin in both 1997 and 1998. Larger and statistically significant estimates are shown from 1999 onwards for establishments within the first distance band. Establishments located more than 300 m from a relocation site are not affected by the program.

Using this third set of estimates, we can also derive the total policy impact within 300 m by focusing on the coefficients for year 2002 (2.911 + 0.429 + 0.026–0.079 = 3.287). Multiplying 3.287/1000 by the average number of relocated jobs within such distance (150.2) and by the total number of private sector plants located within the first distance ring (12,954), we obtain a total effect of 6340 jobs. This slightly higher estimate is consistent with previous values and implies a local multiplier of 1.35.

B Splitting by employment type and worker characteristics

The novelty of this paper lies in our use of establishment level data rather than area level data, compared to previous studies on public sector relocations. The establishment level data we use (BHP data) provide rich information on various plant and worker characteristics, such as employment type (full-time, part-time and marginal worker), age, gender, occupation (1 digit) and highest educational achievement. This data availability allows us to explore the policy impact on a more refined definition of workers, distinguishing between men and women, full-time and part-time, and workers of different ages, etc. Previous studies, which used geographic areas as their unit of analysis, lacked this level of detail.

In this section, we first compare the main results on private sector employment, as described in Section 6.A, with corresponding results based on a more traditional definition of plant size, namely headcount employment. Then, we examine whether the policy impact varied by worker characteristic, highlighting whether certain types of private sector workers were affected by the relocation program more than others.

So far, we have measured establishment size by the number of jobs in terms of full-time equivalents (FTEs). This measure is calculated as the weighted sum of full-time, part-time, and marginal jobs at the establishment level (refer to Section 5 for details). Table 4, Column (1) summarizes the results using this FTE definition, showing the cumulative policy effects by distance band for each of the three model specifications: the long-difference specification in the top panel, the dynamic specification in the middle panel, and the event study specification in the bottom panel.

It is important to note that each specification covers a slightly different time period for calculating its cumulative policy impact: 1998–2002 for the long-difference model, 1995–2002 for the event study model, and the sum of the contemporaneous and the three lag effects for the dynamic model. These varying time periods largely explain the differences in effect sizes observed in Column (1).

We explore a more traditional definition of employment based on headcount because detailed worker characteristics are not available by employment type. For example, while we can determine the number of male or female employees in a given establishment, we cannot ascertain how many of these female employees work full-time, part-time, or as marginal workers. Although we cannot compute FTE measures by worker characteristic, we can compute and compare headcount measures.

Table 4
The impact of 1996–2001 cumulative relocations on (1998–2002) changes in private sector employment: splitting by employment type.

Dep. Variable	FTEs	Headcount	Full-time	Part-time
	(1)	(2)	(3)	(4)
Long-difference specification				
0–300 m	2.534***	2.874***	2.008***	0.880***
	(0.644)	(0.764)	(0.518)	(0.234)
0–500	0.152	0.179	0.211	−0.063
	(0.315)	(0.338)	(0.310)	(0.064)
0–1000	0.277	0.309	0.269	−0.008
	(0.178)	(0.187)	(0.169)	(0.052)
0–3000	0.048	0.047	0.043	0.013
	(0.039)	(0.044)	(0.036)	(0.012)
Dynamic specification				
0–300 m	3.026**	3.386**	2.414**	1.069**
	(1.313)	(1.494)	(1.083)	(0.515)
0–500	−0.480	−0.436	−0.373	−0.196
	(0.764)	(0.779)	(0.725)	(0.128)
0–1000	0.201	0.218	0.204	−0.022
	(0.148)	(0.132)	(0.163)	(0.061)
0–3000	−0.011	0.009	−0.026	0.018
	(0.118)	(0.135)	(0.103)	(0.037)
Event study specification				
0–300 m	2.911***	3.403***	2.122***	1.328***
	(0.902)	(1.050)	(0.689)	(0.431)
0–500	0.429	0.477	0.585*	−0.26
	(0.423)	(0.495)	(0.349)	(0.222)
0–1000	0.026	0.038	0.029	−0.023
	(0.204)	(0.216)	(0.192)	(0.062)
0–3000	−0.079	−0.082	−0.082	0.006
	(0.087)	(0.097)	(0.083)	(0.017)

Note: Robust standard errors are reported in parentheses; (*), (**), (***) indicate significance at the 10%, 5% and 1% levels, respectively. The dependent variable is private sector employment, distinguishing between full-time equivalent (FTE) employment (Column 1), headcount (Column 2), full-time employees (Column 3) and part-time employees (Column 4). The top panel reports results obtained by using a long-difference specification like that reported in Table 3, Column (3). The middle panel reports results obtained by computing cumulative estimates from a dynamic specification with leads and lags spanning 7 years as shown in Fig. 6, Panel A. The bottom panel reports results obtained by using an event study specification with varying treatment effects like that shown in Fig. 6, Panel B. Observations used are: 88,144 (long-difference), 793,401 (dynamic) and 793,397 (event study). In all specifications, standard errors are clustered at the grid level. All estimates have been multiplied by 1000 to improve readability.
Source: see Table 2 for details.

Results using the headcount definition of employment are presented in Table 4, Column (2). As expected, estimates are slightly larger when using headcount compared to FTEs. Consistent with the findings in Column (1), Column (2) estimates for the top panel specification are slightly smaller than those for other panels. Overall, there is a strong correspondence between FTE and headcount estimates, with both showing statistically significant effects within the first distance band only. When we split headcount employment into full-time and part-time workers (see Table 4, Columns 3 and 4), we find that the relocation program created more full-time than part-time jobs. Approximately 65–71% of all jobs created were full-time, while 26–35% were part-time.[28]

Table 5 presents estimates obtained by splitting the sample based on worker characteristics. Specifically, we focus on four worker characteristics: gender, age (15–34; 35–49; 50–64 years old), occupational level (routine and manual occupations; medium-skilled occupations; managerial and professional occupations) and highest educational attainment (primary to lower secondary education; vocational training and high-school education; college graduates). We only report results for the event study specification with varying treatment.[29]

Evidence suggests that the relocation program led to the creation of new local jobs in the private sector, which were mostly filled by male workers (58% compared to 42% female workers); younger and prime-age workers (51% and 46%, respectively); and individuals working in medium-skilled occupations (48% of all jobs), with a smaller impact on manual (28%) and professional workers (23%). In terms of educational attainment, workers with vocational training or high-school education (55%) were most affected, followed by college graduates (23%) and low-education workers (18%). Effects were mainly felt within a 300 m radius of a relocation site, but significant impacts were also observed within the 300–500 m distance ring, particularly for individuals working in professional occupations and with higher qualifications.

To assess whether the relocation created winners or losers, we compared the distribution of newly created jobs by worker characteristic described in the previous paragraph with the distribution that prevailed within the same spatial areas in Berlin (within 0–300m of a relocation site) between 1994 and 1997, a period preceding the government move. Findings indicate that the relocation program increased the proportion of jobs filled by female workers (a rise of 7.1 percentage points), younger workers (21.6 pp), workers in managerial and professional occupations (16.6 pp), and individuals with lower educational attainment (11.9 pp). Conversely, groups that experienced a reduction in available jobs included workers aged 35–49 years (a drop of 25.9 percentage points), those in medium-skilled occupations (−9.7 pp), and workers with vocational training or high-school education (−13.1 pp).[30]

C Splitting by industry and plant size

In this section, we investigate the policy impact by plant characteristics, focusing on sector of activity and initial plant size (measured as the 1994–1997 average). As shown in Table 3, the policy triggered the creation of private sector jobs in services but had no impact on manufacturing. Table 6 further splits services into twelve sub-sectors, including construction, wholesale trade, retail, hotels, cafés & restaurants, transport & communication, finance, business & consultancy, media, tourisms, and personal service activities, with 'other' being the residual category.[31]

Our main objective is to decompose the overall effect on services presented in Table 3 into its sub-sector components. To achieve this, we first estimate Equation (3) including all private sector establishments and controlling for individual fixed effects and grid-specific year fixed effects. From this set of results, we retrieve the partialled-out estimates of the dependent variable ($\widetilde{emp}_{i,t}$) and the treatment intensity variables ($\widetilde{R}^{d}_{1996-2001}$) linked to the government move.

For the sub-sector analysis, we define the transformed dependent variable for sub-sector s as $emp^{s}_{i,t} = 1[sector = s] \times \widetilde{emp}_{i,t}$. We then run a

[28] Marginal workers were added to the BHP data starting in 1999, making up approximately 10–11% of all German employees between 1999 and 2002. To mitigate potential bias from their inclusion, we conducted our main analysis using an FTE definition of employment. By comparing FTE estimates with headcount figures (as we do in Table 4), we can assess the extent of any potential bias. The results reported in Table 4 confirm that marginal workers accounted for 5% or less of total jobs during the government relocation, suggesting their addition to the BHP data in 1999 is unlikely to have significantly inflated our estimates.

[29] Results for the other two model specifications are qualitatively similar and available upon request.

[30] See Online Appendix D for detailed results.

[31] Sub-groups are defined as follows: construction (SIC45), wholesale trade (SIC51), retail (SIC52), hotels (SIC551-SIC552), cafés & restaurants (SIC553-SIC555), transport & communication (SIC60-SIC64 except SIC633), finance, banking & insurance (SIC65-SIC67), business & consultancy (SIC741-SIC744), media, printing & publishing (SIC22, SIC922, SIC924), tourism, sport & recreational activities (SIC633, SIC921, SIC923, SIC925-SIC926), and personal service activities (SIC93), with *other* being the residual category.

Table 5
Splitting by worker characteristics.

		Gender		Age		
	Headcount	Female	Male	15–34	35–49	50–64
	(1)	(2)	(3)	(4)	(5)	(6)
0–300 m	3.403***	1.426***	1.976***	1.740**	1.555***	0.103
	(1.050)	(0.380)	(0.720)	(0.732)	(0.343)	(0.175)
0-500	0.477	0.154	0.322	0.257	0.169	0.047
	(0.495)	(0.288)	(0.231)	(0.357)	(0.196)	(0.118)
0-1000	0.038	0.085	−0.047	0.033	−0.021	0.015
	(0.216)	(0.098)	(0.121)	(0.168)	(0.091)	(0.031)
0-3000	−0.082	−0.031	−0.051	−0.04	−0.009	−0.031
	(0.097)	(0.053)	(0.050)	(0.051)	(0.035)	(0.021)
	Occupational level			Educational qualification		
	Routine & Manual	Medium-skilled	Managerial & Professional	Primary to Lower Secondary	Vocational training and High school	College graduates
	(7)	(8)	(9)	(10)	(11)	(12)
0–300 m	0.942	1.635***	0.775***	0.624**	1.886***	0.778**
	(0.644)	(0.411)	(0.294)	(0.269)	(0.649)	(0.350)
0-500	0.066	0.073	0.216**	0.033	0.224	0.256**
	(0.261)	(0.195)	(0.107)	(0.173)	(0.297)	(0.127)
0-1000	0.089	−0.026	−0.013	−0.003	0.058	−0.031
	(0.061)	(0.124)	(0.061)	(0.034)	(0.105)	(0.081)
0-3000	−0.054	−0.04	0.011	−0.015	−0.093	0.024
	(0.043)	(0.054)	(0.022)	(0.018)	(0.071)	(0.024)

Note: Robust standard errors are reported in parentheses; (*), (**), (***) indicate significance at the 10%, 5% and 1% levels, respectively. The dependent variable is private sector employment, splitting by gender (Columns 2–3), age group (Columns 4–6), occupational level (Columns 7–9) and highest educational attainment (Columns 10–12). All figures refer to cumulative estimates derived from an event study specification with varying treatment effects as shown in Fig. 6, Panel B. Observations: 793,397. In all specifications, standard errors are clustered at the grid level. All estimates have been multiplied by 1000 to improve readability.
Source: see Table 2 for details.

Table 6
Splitting by industry.

	Construction	Wholesale	Retail	Hotels	Cafés & restaurants	Transport
	(1)	(2)	(3)	(4)	(5)	(6)
0–300 m	0.067	−0.133	0.321	−0.418***	0.371***	0.104
	(0.114)	(0.202)	(0.224)	(0.083)	(0.125)	(0.453)
0-500	0.122	0.202	−0.009	0.375***	−0.099	−0.153
	(0.151)	(0.166)	(0.044)	(0.115)	(0.112)	(0.158)
0-1000	−0.074	−0.044	0.003	−0.021	0.016	0.030
	(0.095)	(0.029)	(0.037)	(0.018)	(0.027)	(0.103)
0-3000	0.010	0.019	−0.008	−0.002	0.004	−0.038
	(0.011)	(0.015)	(0.012)	(0.003)	(0.006)	(0.048)
	Finance	Business & consultancy	Media	Tourism	Personal services	Other
	(7)	(8)	(9)	(10)	(11)	(12)
0–300 m	−0.077	0.172	1.404**	0.611*	−0.001	0.500
	(0.144)	(0.207)	(0.575)	(0.344)	(0.037)	(0.560)
0-500	−0.010	−0.097	0.140	0.106*	0.016	−0.171
	(0.046)	(0.106)	(0.115)	(0.055)	(0.011)	(0.192)
0-1000	0.052	0.099**	−0.062	−0.058	0.013*	0.047
	(0.045)	(0.049)	(0.040)	(0.041)	(0.007)	(0.067)
0-3000	−0.046	0.010	−0.012	0.008	0.000	−0.016
	(0.030)	(0.011)	(0.008)	(0.007)	(0.002)	(0.025)

Note: Robust standard errors are reported in parentheses; (*), (**), (***) indicate significance at the 10%, 5% and 1% levels, respectively. The dependent variable is private sector employment in the following sectors: construction (SIC45), wholesale trade (SIC51), retail (SIC52), hotels (SIC551-SIC552), cafés & restaurants (SIC553-SIC555), transport & communication (SIC60-SIC64 except SIC633), finance, banking & insurance (SIC65-SIC67), business & consultancy (SIC741-SIC744), media, printing & publishing (SIC22, SIC922, SIC924), tourism, sport & recreational activities (SIC633, SIC921, SIC923, SIC925-SIC926), and personal service activities (SIC93), with *other* being the residual category. The other category includes investigation & security activities, photographic services, packaging, secretarial & translation activities, and other business activities not elsewhere classified. All figures refer to cumulative estimates derived from an event study specification with varying treatment effects which applies a 2-step procedure as explained in Section 6.C. Observations: 793,397. In all specifications, standard errors are clustered at the grid level. All estimates have been multiplied by 1000 to improve readability.
Source: see Table 2 for details.

variant of Equation (3), replacing the dependent variable with $emp^s_{i,t}$; replacing the set of treatment intensity variables with $\widetilde{R}^d_{1996-2001}$; dropping individual fixed effects (γ_i), as they are already partialled out; and expressing time fixed effects as $1[sector = s] \times \delta_t$. We apply this 2-step approach to our three model specifications, even though only results for the event study model are shown in Tables 6 and 7.[32]

We find that the positive impact within the first 300 m (see Table 6,

[32] Results for the other specifications are qualitatively similar and available upon request.

Table 7
Splitting by initial plant size and age.

Incumbent size				
	New entrants (1998–2002)	1-9 workers	10–49	50+
	(1)	(2)	(3)	(4)
0–300 m	2.234**	−0.293	0.029	1.302
	(0.910)	(0.210)	(0.416)	(0.913)
0–500	0.166	0.113**	0.397***	−0.022
	(0.224)	(0.050)	(0.100)	(0.289)
0–1000	0.038	0.023	−0.111***	0.096
	(0.175)	(0.024)	(0.032)	(0.068)
0–3000	0.026	−0.005	0.012	−0.089
	(0.051)	(0.012)	(0.017)	(0.069)
Incumbent age				
	New entrants (1998–2002)	Recent (1995–1997)	Existing in 1994	
	(5)	(6)	(7)	
0–300 m	2.234**	−0.495*	1.394	
	(0.910)	(0.259)	(1.488)	
0–500	0.166	0.706*	−0.417	
	(0.224)	(0.363)	(0.369)	
0–1000	0.038	−0.171*	0.166*	
	(0.175)	(0.094)	(0.099)	
0–3000	0.026	0.032	−0.146*	
	(0.051)	(0.032)	(0.081)	

Note: Robust standard errors are reported in parentheses; (*), (**), (***) indicate significance at the 10%, 5% and 1% levels, respectively. In Columns (1) and (5), the dependent variable is private sector employment in new establishments created between 1998 and 2002. In Columns (2)–(4), the dependent variable is private sector employment by initial (measured as 1994–1997 average) plant size. In Columns (6) and (7), the dependent variable is private sector employment in establishments created between 1995 and 1997 (*recent*) and those existing in 1994, respectively. All figures refer to cumulative estimates derived from an event study specification with varying treatment effects which applies a 2-step procedure as explained in Section 6.C. Observations: 793,397. In all specifications, standard errors are clustered at the grid level. All estimates have been multiplied by 1000 to improve readability.
Source: see Table 2 for details.

Row 0–300 m) is largely driven by new jobs created in media (1.404; se 0.575), tourism (0.611; se 0.344), and cafés & restaurants (0.371; se 0.125). The estimate for retail trade is positive but insignificant, and the positive effect on the tourism sector is somewhat more spread with a positive impact already measured within 500 m. Conversely, hotels and business plants do not expand near government sites, but they create jobs at a further distance. Table 6, Column (8), shows no policy impact for the business sector within the first 500 m, but a small, positive, and statistically significant coefficient (0.099; se 0.049) within the 500–1000 m range. For hotels (see Table 6, Column (4)), we observe significant local spillover effects in areas within 300–500 m (0.375; se 0.115) of a relocation site, but the effect reverses to zero at closer distance.

Lastly, Table 7 considers establishment age and size. For initial plant size, we find that 75% of all private sector jobs, located within the first 300 m, were created by newly established plants, i.e., plants that did not exist before 1998 (see Table 7, Column 1). For incumbent plants with less than 10 employees (see Table 7, Column 2), the policy seems to have sparked job creation within the 300–500 m distance range. Plants with 10–49 employees (see Table 7, Column 3) show evidence of jobs movement towards relocation sites, with jobs shifting out of areas within 500–1000 m and into areas within 300–500 m. Splitting establishments by age confirms that new entrants are driving the results. There seems to be a positive relationship between distance to a relocation site and plant age. The younger the establishment, the closer it tends to expand near a relocation site. New establishments are expanding within the first 300 m; recently established plants are expanding within 300–500 m, but they are contracting within 500–1000 m; older establishments are contracting within 1000–3000 m (see Table 7, Columns 5–7).[33]

D Interpretation of our findings

Considering our results so far, evidence suggests that the relocation of the German government from Bonn to Berlin in the late 1990s sparked a local multiplier effect, but we find no evidence of crowding out or displacement. However, crowding out may still exist, though our analysis may not fully capture it.

Typically, the literature frames crowding out from relocations in terms of second-order effects, such as the displacement of local businesses due to higher housing costs and wages. In Berlin's case, these effects were likely muted by the city's economic conditions in the late 1990s – an abundance of vacant space, available labor, and a shrinking population, as discussed in Section 3.B. These conditions likely mitigated the general equilibrium effects of the relocation program, reducing any potential crowding-out.

Additionally, first-order crowding out occurred when government workers moved into specific Berlin buildings, displacing previous tenants, or because chosen building land was no longer available for alternative development. Due to data limitations, we cannot accurately estimate these moves or predict alternate uses for those buildings or areas. We note, however, that crowding out is not a given. A striking example is Tempelhof Airport, considered as a site for the new parliament but ultimately rejected in favor of Spreebogen in Berlin Mitte. Despite its size and central location, Tempelhof remains largely unused today, highlighting the uncertainty surrounding the repurposing of historically significant sites.

Our findings suggest that local multiplier effects were primarily driven by consumption demand, reflected in the growth of cafés, restaurants, and hotels, rather than by intermediate or production demand, such as business and consultancy services. One possible explanation lies in the nature of the public-sector jobs involved in the relocation. As outlined in Section 3.C, the move included the Federal Parliament, nine ministries, 163 foreign embassies and consulates, and representations of the Federal States. These non-tradable public services are typically concentrated in a capital city, subject to media scrutiny but with limited direct public interaction. It is likely that many of the tasks performed in these offices could not be easily outsourced to the business or consultancy sector, thereby reducing the incentive for private companies to relocate nearby.

An alternative explanation is linked to our empirical approach. As discussed in Section 4 of the paper, our strategy allows us to identify highly localized policy effects, studying impacts within 300, 500, 1000 and 3000 m. Indeed, Table 6 shows a small and positive impact on consultancy firms within the 500–1000 m range. Any effect beyond 3 km is not captured by our analysis. Therefore, it is possible that consultancy firms did relocate to Berlin following the government move but chose locations beyond our 3 km threshold or did not prioritize physical proximity to government sites when selecting office locations.

E Business closings and openings

As noted in Section 5, address data is not available before 1999 and, thus, it does not cover the period before the government move. In the previous analysis, we overcome this obstacle by focusing on existing establishments in 1999 and tracing them back to the year they entered the BHP panel. This leaves us with the problem of plant exits before 1999, as we cannot attribute a geo-referenced address to an establishment that has left the panel before 1999. In this section, we provide evidence of business openings and closings before 1999 using data at the

[33] As a robustness check, we conduct a permutation test to validate our estimates and find that distance to a relocation site matters. Results are available in Online Appendix E.

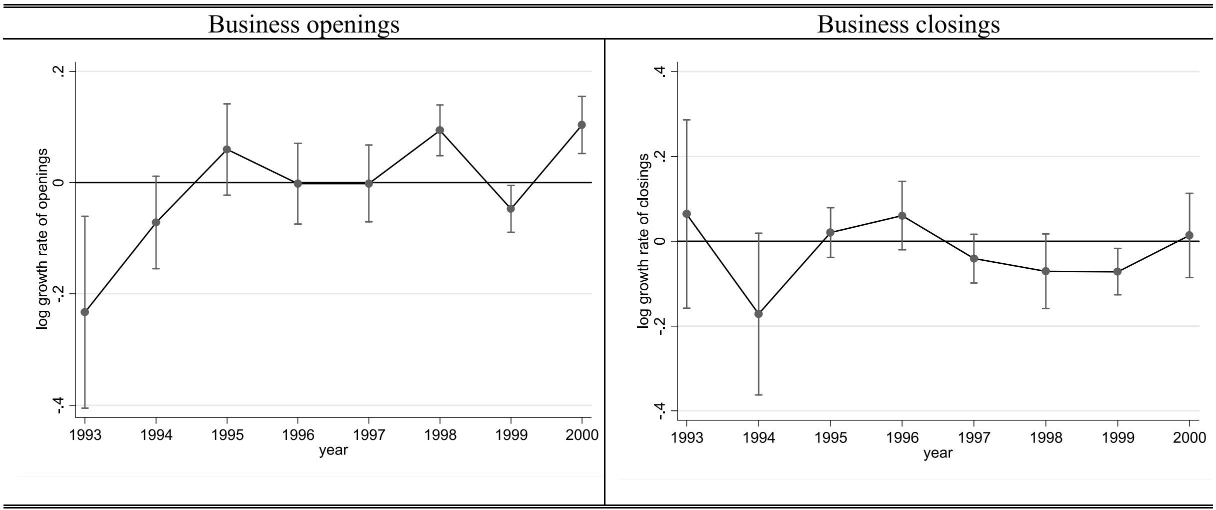

Fig. 7. Business closings and openings in the two central Berlin 'Bezirke' (Berlin Mitte and Tiergarten) versus the other 21 districts.
Note: Panels A and B are confidence bar charts, with marks indicating estimates and bars showing 95% confidence intervals. Each estimate refers to the difference in the log growth rate of openings (closings) in the two central 'Bezirke' of Berlin Mitte and Tiergarten relative to the remaining 21 districts. We use the pre-2001 administrative classification of Berlin 'Bezirke'.
Source: Statistical office Berlin-Brandenburg, own calculations.

level of Berlins 'Bezirke' (23 city districts) retrieved from the Statistical Office of Berlin-Brandenburg.

Our main concern is a possible outflow of establishments near relocation sites occurring before the government move (perhaps linked to large reconstruction projects in Berlin city center) and subsequent openings of new businesses to fill up the vacant commercial space there. If this were the case, the results we presented in Sections 6.A-C would be incorrectly attributed to the relocation policy instead of business dynamics preceding the move. To test this hypothesis, we use yearly statistics on business openings and closings available at the level of the 23 Berlin Bezirke.[34] For simplicity, we define the two central districts of Berlin Mitte and Tiergarten as the treated areas since they received most relocated jobs (see Fig. 4). We then estimate a simple model interacting the treatment dummy with years and plot the differences in openings/closing of the two central districts (Berlin Mitte and Tiergarten) versus the remaining 21 districts (the control group).

Looking at the time profile of business openings (see Fig. 7, Panel A), we observe fewer business openings in Berlin Mitte and Tiergarten relative to other Berlin districts in 1993 and 1994. This is followed by a three-year period (1995–1997) of essentially no difference in business openings across Berlin districts. A surge in business openings is recorded in the two central districts in 1998, followed by a drop in 1999 and then a rise again (but not statistically significant) in 2000. The rise in business openings in 1998 and 2000 is consistent with our main results. The effects in Fig. 7 are recorded a little earlier than we would expect. It is worth noticing that business openings recorded by the Statistical office of Berlin-Brandenburg refer to the whole year 1998, whereas German social security data in the BHP are as of June 30. Moreover, business owners are more likely to first file an opening and then start hiring employees and filling in related social security paperwork. Therefore, a time discrepancy between the two data sets is expected.

Looking at the time profile of business closings (see Fig. 7, Panel B), we observe no statistical difference across Berlin districts for all years preceding 1999. In 1999, closings are lower (not higher) in Berlin Mitte and Tiergarten relative to other Berlin districts. The drop in 1999 is likely linked to the surge in business openings recorded in the previous year. Considering the evidence shown in Fig. 7, we can rule out the possibility that an outflow of businesses from the city center preceding the government move might confound our estimates.

7. Conclusions

In this paper, we have shown that policies involving public sector employment cannot be considered as independent from their impact on the private sector. By 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. Specifically, we found that the policy impact is highly localized and mostly set in within the first two years of the government relocation. We estimated a local multiplier of about 1.32–1.35, indicating that the arrival of 10 government-related jobs in the center of Berlin prompted the creation of approximately 3 jobs in private sector establishments situated nearby. These effects came through job creation in the service sector with the largest job gains found in media, tourism, and cafés & restaurants. About 75% of these new jobs were created by establishments that did not exist before 1998. We found no evidence of a multiplier or crowding-out effect for manufacturing jobs.

This study made a few novel contributions to the literature on public sector expansion and contraction: (*i*) we used data at the establishment level instead of the area level and, thus, we estimated the policy impact on the average plant *i* located at distance *d* of a relocation site, allowing effects to vary by distance; (*ii*) we examined the policy impact within a city boundary instead of conducting the analysis across cities or municipalities; and (*iii*) we adopted three alternative model specifications to ensure robust results.

Still, our study has limitations: it is a partial analysis; it estimates highly localized effects; it does not capture first-order crowding out; and it does not allow us to compare Berlin affected by the program with Berlin under a non-relocation scenario at the entire city level. Our analysis also fails to capture a further important aspect of the relocation program: the effects of relocated government employee residential choices. Thus, we cannot study localized effects on the housing market or changes in private consumption patterns. In the context of China, Qu et al. (2021) made a first step in this direction. They analyze the relocation of Beijing Municipal Government to a subcenter of the city in 2019. They document that the relocation program led to higher staff turnover, largely attributed to commute dissatisfaction and family

[34] Statistisches Landesamt Berlin, Statistische Berichte D I 2 j92-j05: Gewerbeanzeigen im Land Berlin. We use the pre-2001 definition of Berlins Bezirke (23 city districts). Since the 2001 administrative reform, Berlins has 12 Bezirke.

dilemmas. Their findings support the inclusion of both direct and indirect costs in policy evaluations of relocation programs.

Overall, our study is of considerable interest for policy makers for several reasons. First, it provides evidence on the efficacy of public sector relocation programs to address local employment problems. Despite the frequent use of such policies, evidence of their impact is limited. Second, our study helps to comprehend the uneven spatial effects of changes in public sector employment, which are relevant for both public sector job creation and destruction – another highly debated topic. Third, our project serves as a relevant case study. The 1994 Bonn-Berlin Act mandates that 50% of government employees remain in Bonn, and the issue of whether the law should be changed is frequently discussed. Lastly, our study sheds some light on the potential impact on Bonn and Berlin should the German government decide to relocate the remaining ministries.

CRediT authorship contribution statement

Giulia Faggio: Writing – review & editing, Writing – original draft, Methodology, Investigation, Funding acquisition, Formal analysis, Data curation, Conceptualization. **Teresa Schlüter:** Methodology, Investigation, Funding acquisition, Data curation, Conceptualization. **Philipp vom Berge:** Writing – review & editing, Resources, Methodology, Investigation, Formal analysis, Data curation, Conceptualization.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.regsciurbeco.2025.104084.

Appendix 1. Equivalence between the cumulative and the non-cumulative specifications

In our paper, we use a cumulative specification with distance bands R^{0-300}, R^{0-500}, R^{0-1000} and R^{0-3000}. In this Appendix, we demonstrate that this specification is equivalent to a separate bin-by-bin specification using bands R^{0-300}, $R^{300-500}$, $R^{500-1000}$ and $R^{1000-3000}$, and that coefficients can be easily converted between the two specifications.

Starting with an empirical model with separate bins (ignoring time and establishment indices for brevity), we have:

$$y = \alpha_1 R^{0-300} + \alpha_2 R^{300-500} + \alpha_3 R^{500-1000} + \alpha_4 R^{1000-3000} + \varepsilon$$

This can be rewritten as:

$$y = \alpha_1 R^{0-300} + \alpha_2 \left(R^{0-500} - R^{0-300}\right) + \alpha_3 \left(R^{0-1000} - R^{0-500}\right) + \alpha_4 \left(R^{0-3000} - R^{0-1000}\right) + \varepsilon$$

or, after some reorganization:

$$y = (\alpha_1 - \alpha_2) R^{0-300} + (\alpha_2 - \alpha_3) R^{0-500} + (\alpha_3 - \alpha_4) R^{0-1000} + \alpha_4 R^{0-3000} + \varepsilon$$

As a result, the coefficients from the cumulative specification:

$$y = \beta_1 R^{0-300} + \beta_2 R^{0-500} + \beta_3 R^{0-1000} + \beta_4 R^{0-3000} + \varepsilon$$

directly test the marginal effect from an additionally relocated public sector job in each distance band d relative to the effect in the next outer band (e.g., $\beta_1 = \alpha_1 - \alpha_2$).

Reversing this calculation, one can easily show that the non-cumulative marginal effects can be computed from the cumulative model as:

$$\begin{aligned} \alpha_1 &= \beta_1 + \beta_2 + \beta_3 + \beta_4, \\ \alpha_2 &= \beta_2 + \beta_3 + \beta_4, \\ \alpha_3 &= \beta_3 + \beta_4, \\ \alpha_4 &= \beta_4. \end{aligned}$$

Data availability

For our analyses, we used administrative data of the Institute for Employment Research (IAB) [Establishment History Panel 1975-2014 (BHP 7514), full population version]. The data are social security data with administrative origin which are processed and kept by IAB, Regensburger Str. 104, D-90478 Nürnberg, iab@iab.de, phone: +49 911 1790, according to the German Social Code III. There are certain legal restrictions due to the protection of data privacy. The data contain sensitive information and therefore are subject to the confidentiality regulations of the German Social Code (Book I, Section 35, Paragraph 1). The raw data, computer programs, and results have been archived by IAB in accordance with good scientific practice. Computer programs and results can be found in the reproduction package in the supplementary files. If you wish to access the full data for replication purposes, please contact Philipp vom Berge (philipp.vom-berge@iab.de). Please visit https://www.iab.de/en/daten/replikationen.aspx.

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