Interaction of public and private employment:
Evidence from a German government move
Online appendices

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Appendix A: Historical setting – details

When the Federal Republic of Germany (FRG) and the German Democratic Republic (GDR) were founded in 1949, it was uncertain how long this separation would endure. Berlin, which had been the German capital from 1871 to 1945, was claimed by both states. In the GDR's constitution, Berlin was declared its capital, while in the basic law of the FRG, (Great-) Berlin was considered the 11th federal state. Consequently, the city was divided into East Berlin, governed by the USSR, and West Berlin, composed of the three sectors administered by the US, France, and the UK. Geographically, the entire city fell within the Soviet occupation zone.

The two German states did not immediately recognize each other's sovereignty. The FRG viewed the division as temporary, with both political leaders and the general public advocating for a reunified Germany with Berlin as its capital (Süß, 1999). The widely held belief that the situation was transitional was also evident in discussions about the location of the FRG government. The new capital was intended to have a provisional character and moderate influence (Dreher, 1979), so as not to impede the government's return to Berlin once the two German states reunified.

The perception that the division would be of short duration changed when construction of the Berlin Wall began in August 1961. In December 1972, the FRG and the GDR accepted the status quo and mutually recognized each other as sovereign states by signing the Basic Treaty. Shortly before, the Allies had resolved their dispute over their rights and responsibilities in Berlin in the Quadripartite Agreement. Berlin would continue not to be a constituent part of the FRG.[1] In his government declaration in 1973, Willy Brandt referred to Bonn as the federal capital of Germany for the first time. Although the ultimate aim of the West German government was still reunification, the political discussion about Berlin as the capital subsided.

Reunification and the Bonn/Berlin question

Political protests against the East German government began in September 1989 with the so-called Monday demonstrations in Leipzig. The fall of the Berlin Wall in November 1989 once again allowed free movement within both the Eastern and Western parts of the city. Berlin became the capital of unified Germany in 1990 when the Unification Treaty (1990) was signed between the newly elected governments of the GDR and FRG. However, the decision on the location of the

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

government seat was postponed until after the election of the first assembly (Bundestag) of reunified Germany.

The crucial debate regarding the government relocation to Berlin took place on June 20th, 1991. The assembly was divided, with deputies from the ten western federal states already indicating their preference for Bonn during the negotiation of the Reunification Treaty (Süß, 1999). Polls among the total of 662 members of parliament showed Bonn as the clear favorite (Tschirch, 1999). Advocates for Bonn pointed to its successful democratic and federalist tradition. Bonn's proximity to the Western allies and the EU headquarters in Brussels had facilitated European integration. They argued that integration would slow down if Berlin became the new capital (Salz, 2006). Furthermore, substantial infrastructure investments had transformed Bonn into a highly efficient administration center. These investments would be lost, and the funds allocated for establishing the government in Berlin could be better spent on construction projects in the new federal states (Tschirch, 1999).

The main argument of the pro-Berlin faction was that of credibility. Since 1949, when the FRG was founded, politicians had consistently reiterated that Berlin was the true capital of Germany. Bonn had become the capital with a provisional mandate that would return to Berlin once East and West Germany were united. Other significant arguments included the symbolic importance of the move as a sign of solidarity between the old and the new federal states and Berlin's potential as a bridge to Eastern Europe. Economically, the government relocation was expected to strengthen Berlin's weak local economic position and boost the underdeveloped east. Conversely, the city of Bonn feared that 'the small Bonn' would lose both its political significance and economic power (Deutscher Bundestag, 1991b, pp. 2736-2738).

The most significant arguments in the discussion revolved around rather abstract concepts. Credibility and the future of a reunited Germany were central points for the pro-Berlin faction, while Bonn was viewed as a symbol of a successful democratic tradition (Tschirch, 1999). In the final vote, the assembly decided to relocate the government seat from Bonn to Berlin, with 338 votes in favor and 320 votes against (Deutscher Bundestag, 1991b). The narrow majority was only achieved by making substantial concessions to the city of Bonn. Negotiations were planned for a fair division of labor between Berlin and Bonn, where core government functions would be situated in Berlin, but the majority of government jobs would remain in Bonn. Bonn was also slated to receive financial compensation, as well as new functions and institutions of national and

international significance. A commission was to be appointed to propose the distribution of national and international agencies across the new federal states, as per the constitution of Germany, which grants each federal state the right to retain some national power. The national parliament (Bundestag) was expected to commence its functions in Berlin within 4 years, with all government functions to be relocated to Berlin within 10-12 years (Deutscher Bundestag, 1991a). Two weeks later, on July 3rd, 1991, the Federal Assembly decided by a vote of 38 against 30 to remain in Bonn (Deutscher Bundestag, 2010).

Realization of the move

The decision made in 1991 left the details of the move open. By 1992, it became evident that relocating the core government functions within four years was not feasible. What followed was a protracted discussion regarding the timing of the move and its associated costs. One proposal suggested ceasing any further government-related investment in Berlin until the financial situation of the FRG had improved. Another suggested postponing the move until 2010. Additionally, a mass petition was organized to defer a decision about the move's date until the government had a comprehensive understanding of the costs and the financial situation of both the state (Bund) and federal states (Länder) had improved (Deutscher Bundestag, 2010).

This dispute created uncertainty among private companies that had begun investing in Berlin. In November 1993, 40 national and international companies pointed at a breach of trust and potential contractual obligations should the government cease its efforts to proceed with the move (Hoffman, 1998, p. 213).

The passage of the Berlin/Bonn Act in 1994 provided statutory security regarding the relocation to Berlin. Although it did not specify a concrete moving date, the act determined essential details for implementing the move, including the definition of a 'fair division of labor' between Berlin and Bonn and specific compensatory measures for the former capital. Under the act, six ministries were to maintain their primary headquarters in Bonn[2] and establish a secondary

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

seat in Berlin, while nine ministries would have their primary headquarters in Berlin[3] and a secondary seat in Bonn. Furthermore, it was decided that the majority of ministerial positions would remain in Bonn.

The subsequent years were primarily focused on the practical implementation of the move. Construction of new buildings, such as the Jakob-Kaiser-Haus and Paul-Löbe-Haus, began in spring 1997, but it wasn't until November 1997 that the Federal Parliament announced a moving date. The government was scheduled to begin its work in Berlin in September 1999. Until then, the timing had been a subject of intense debate. Parliament and the government officially commenced their work in Berlin in September 1999. The majority of employees relocated in 1999 and 2000, resulting in more than 8,000 ministerial employees being located in Berlin by the end of 2000. In the subsequent years, additional positions were gradually moved from Bonn to Berlin. By 2010, approximately 10,000 positions had been established in Berlin.

Since the Federal Assembly had revised its decision to remain in Bonn in September 1996, the Federal States also established their representations in Berlin. In total, around 600 employees and approximately 70 members of the Federal Assembly relocated between 1998 and 2003.

The majority of foreign embassies chose to relocate their headquarters from Bonn to Berlin, aligning their presence with the German government move in 1999. Some of these embassies initially made short-term arrangements to accommodate their staff, such as renting offices or utilizing facilities from their former military missions, consulate generals, or branch offices until they could construct suitable buildings for their representations (Gehrcken, 2013). Despite the widespread destruction of the building stock in West Berlin between 1939 and 1945, many countries still owned parcels of land in Berlin that they had acquired over a century earlier. The former embassies in East Berlin had closed in 1990 and were repurposed as consulates, with some later reopening as representations in a unified Germany. By 2015, 163 countries (158 embassies and 5 honorary consulates) had established representation in Berlin.

To compensate Bonn for its loss of employment, several federal offices relocated from Berlin to Bonn in 1999 and 2000. Berlin also saw a transfer of some of its prior functions to the

[3] These nine ministries included: the Federal Press Office; the Foreign Office (AA); the Federal Ministry of the Interior (BMI); the Federal Ministry of Finance (BMF); the Federal Ministry of Justice (BMJ); the Federal Ministry of Economics and Technology (now Federal Ministry for Economic Affairs and Climate Action, BMWK); the Federal Ministry of Labor and Social Affairs (BMAS); the Federal Ministry of Transport, Building and Urban Development (now Federal Ministry for Digital and Transport, BMDV); the Federal Ministry of Family Affairs, Senior Citizens, Women and Youth (BMFSFJ). Additionally, the Chancellor's Office (*Bundeskanzleramt*) moved to Berlin.

New Länder. The recommendations of the federal commission affected various Berlin-based institutions that moved in the following years. Before reunification, approximately 28,000 employees had worked for federal offices in Berlin (Guerra, 1999). The two relocation programs impacted about 8,700 positions.

Appendix B: Data collection through independent sources

Due to the lack of official sources on public sector employment, we initiated an extensive data collection exercise, gathering information on three main variables. First, we collected data on the number of jobs in each relocating institution before and after the move. Second, we recorded the year in which each institution moved in or out of Berlin. Third, we documented the new address of the institution in Berlin or the former address in Berlin for those institutions that relocated to Bonn and the New Länder.

We also obtained information on the number of government employees working in Berlin in 1997, 1999, 2001, and 2004. These figures were sourced from official documents (BT-Drucksache) and an issue of the *Spiegel*, a weekly nationwide newspaper (Bornhöft et al., 2001). The number of relocated jobs in federal institutions was also obtained from official documents (BT-Drucksache). Additionally, the *Berliner Zeitung*, a daily newspaper based in Berlin, provided information on the number of employees in the federal administration and parliamentary groups, as well as the number of deputies and their employees in 1999. For 1998 employment levels in the Länder representations, we relied on data published in the *Generalanzeiger*, a local newspaper in the Bonn region.

Collecting embassy personnel data proved to be more challenging and required us to make several assumptions. The Ministry of Foreign Affairs (Auswärtiges Amt) annually publishes a list of diplomatic staff in foreign embassies with representations in Germany. From these documents, we retrieved the number of diplomatic staff located in Germany in 1996 as our pre-treatment level. However, as these documents do not contain information about embassy workers in administrative or technical support positions, we assumed that their number is proportional to the number of diplomatic staff and estimated the total number of workers in each embassy based on the total number of individuals registered at the Ministry of Foreign Affairs.

Since the Ministry of Foreign Affairs also registers family members, we had to make additional assumptions to determine the number of actual embassy personnel. We acknowledge the difficulty in making reasonable assumptions about the family composition of embassy members. Nevertheless, we assumed that the average household size of an embassy member is approximately 2.5, which yielded an estimate of a total of 6,300 embassy personnel. To validate this estimate, we cross-checked it against the total of 10,000 embassy workers, media representatives, and employees of lobbying organizations that relocated from Bonn to Berlin

during the relocation program. We concluded that a number of this magnitude appeared reasonable and utilized it in our analysis. We attributed the number of relocated jobs to a Berlin location by verifying the address (through official registers) of the institution receiving employment in a specific year. In cases where an institution had multiple sites, we attributed all employment to the main address.[4] Table B.1 provides details on data sources.

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

Table B.1: Details on data sources for the job relocation treatment

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

Appendix C.1: Data limitation 1 – incomplete address information

In describing the data set used in the main analysis (see section V.A.), we point out that establishment address information is not available before 1999. To address this data limitation, we focus on existing establishments in 1999 and trace them back to the year they entered the BHP panel, assuming that establishments do not change addresses. This assumption of stable establishments over our sample period may be incorrect in some cases, thereby creating a potential for bias.

In this appendix, we provide a theoretical discussion of the likely direction of any bias introduced by attributing erroneous addresses to establishments, distinguishing between establishments that do not change addresses and establishments that do. The latter case carries the most significant implications for our analysis. Focusing on the latter, we then attempt to quantify the magnitude of the bias. Our conclusion is that the extent of the bias is relatively small and does not render our empirical exercise invalid.

Before delving into the discussion of the direction and extent of address biases, it is worth noting that our analysis takes an establishment perspective rather than an address perspective. In other words, it focuses on employment changes at the establishment level rather than the address level. As a result, mere relocations of establishments closer to or further away from relocated government jobs do not alter the measured policy impact if the number of employees at the establishment level remains constant. This would be different if our focus were on addresses instead.

Theoretical considerations

C.1.a.: Attributing erroneous addresses to stable establishments

We first consider biases arising from attributing erroneous addresses to stable establishments. In this scenario, an address incorrectly attributed to a stable establishment has the following implications:

(i) If an establishment is truly located close to relocated government jobs (but the incorrect address places it far away), it is going to be ‘treated’ less intensively than it should (treatment measure biased downward).

(ii) If an establishment is truly located far away from relocated government jobs (but the incorrect address places it nearby), it is going to be 'treated' more intensively than it should (treatment measure biased upward).

In case i), we tend to underestimate the policy impact on local employment. This underestimation stems from both a smaller average establishment effect and a smaller total policy effect (the latter being measured as the average effect multiplied by the number of establishments within a given distance ring). In case ii), we still face an attenuation bias, primarily driven by a smaller average effect, while we expect no variation in the total policy effect. The incorrect attribution of establishments with no expected employment gains near a relocation site reduces the average effect. At the same time, the smaller average effect is likely offset by a larger number of establishments treated, resulting in no change in the total policy effect.

Therefore, when attributing an incorrect address to stable establishments, we introduce an attenuation bias. Our estimated coefficients would be lower than the true values, providing a lower bound. Although not ideal, we are not overly concerned about erroneous addresses assigned to stable establishments. Furthermore, these cases are likely to be quite rare.

C.1.b.: Attributing erroneous addresses to establishments that move

We now consider biases stemming from attributing erroneous addresses to establishments that relocate. There are two noteworthy cases to describe:

First, consider an establishment originally located far from government buildings receiving relocated jobs. In anticipation of the arrival of public sector workers, this hypothetical establishment moves closer and hires additional workers. Since we do not observe its previous address (i.e., the address change occurred before 1999), but only its new address, we place the establishment in proximity to relocated government jobs, and its employment expansion contributes to a positive policy impact. We do not consider this case to be a significant concern, although it does blur the lines between anticipation effects and actual policy effects.

Second, consider an establishment that was initially located close to government buildings receiving relocated jobs before the government move but was negatively affected as follows: the arrival of public sector workers increased commercial rents, potentially exerting negative pressure on businesses. Consequently, the hypothetical establishment decides to move away and reduce its workforce. Since we do not have information about its previous address, we treat the new one as

the correct location. Consequently, the establishment is classified as receiving treatment less intensively or not at all. In this scenario, we overestimate any positive policy impact in the inner distance rings by overlooking this negative counter-effect. Simultaneously, we might underestimate positive policy impacts in the outer distance rings if the establishment does not relocate too far, and the negative employment effect is erroneously attributed to one of the outer rings (within the 3km threshold used in the main analysis).

Empirical considerations

How serious is the problem of attributing erroneous addresses to establishments that move – and, particularly, move away from relocated jobs? It depends on (i) how strong the potential negative effects are and (ii) how many addresses are misclassified.

Regarding the potential negative effects (i), it is worth noting that our analysis of commercial registration statistics (refer to Figure 7 in the main text) does not provide evidence of a significant negative extensive margin adjustment. In fact, the number of business closures remains relatively constant in the two central districts of Berlin compared to the other 21 districts during the period 1995-2000.

To estimate the share of potentially misclassified addresses (ii), we conducted the following back-of-the-envelope calculation. First, we selected establishments with a recorded address in 2003 and checked how many of them had a different address in each of the four preceding years (if they were already part of the data set). The percentages were as follows: 3 percent for a one-year lag, 7 percent for a two-year lag, 10 percent for a three-year lag, and 13 percent for a four-year lag. Assuming that these address change patterns are relatively stable over time, we used these numbers as a rough approximation for the pre-1999 period.

Second, we observed that while the address data officially begin in 1999 (as stated in the main text), the 'start date' - 'end date' structure for address entries extends back before 1999 for a significant proportion of existing establishments. When looking at establishments existing in 1999, we found that 82 percent had an address entry reaching back to 1998 or earlier. For 1997 and earlier, this percentage remained at 48 percent, dropping considerably only when examining records before 1997.

Third, combining the information from the last two paragraphs, we can estimate likely address changes between 1998-1999 as $(1-0.82)\times0.03=0.005$ or 0.5 percent, and between 1997-

1999 as (1-0.48)×0.07=0.04 or 4 percent. These numbers are relatively small. Looking at the years before 1997, estimates for the probability of address changes increase. At the same time, it also becomes increasingly unlikely that address changes before 1997 are linked to establishments relocating in anticipation of the government move (i.e., our paper documents that, after years of debate, the Bundestag announced a moving date for the government in November 1997).

To summarize the discussion on incomplete addresses, we acknowledge that our inability to observe all addresses over the study period potentially introduces bias into our estimates, which might either attenuate or amplify the true effect, depending on the nature of the missing information. Given the evidence provided in this appendix, we believe, however, that the extent of this bias is quite small and does not render our empirical exercise invalid.

Appendix C2: Data limitation 2 – multi-branch and single-site companies

As stated in section V.A. in the main text, one potential drawback of using the BHP for our analysis is that address and worker information are not available separately for every branch of an establishment located in Berlin. The data assigns one establishment ID number and one address to companies that have several sites or branches (i) located in the same municipality and that (ii) operate in the same Economic Class (3-digit) according to the 1993 Standard Classification of Economic Activities. As an example, multiple branches of the same supermarket chain within Berlin show up as just one establishment with one address in our data, presumably the one of the head offices. If head offices were largely located in the city center but branches were spread across peripheral areas, we would overestimate employment in the center.

In this appendix, we provide a theoretical discussion of the likely direction of any bias introduced by this data structure and leverage additional data sources to conduct an exercise shedding some light on the extent of these concerns. Ultimately, we argue that the estimates we find in the main data exercise are most likely to be lower bounds of the true effect. In our view, this makes our results quite valuable despite the potential for bias.

Theoretical considerations

Assume that a company has two branches in Berlin, one master branch that carries out all social security notifications and whose address we observe and one junior branch that we cannot observe. We distinguish two cases. In case A, the master branch is located close to a government relocation site and the junior branch is located far away. In case B, the master branch is located far away and the junior branch is located (or opens up) close to a government relocation site.

First, consider case A, with the master branch located close to a government relocation site. Here, any employment changes in the master branch during the sample period will be picked up by our methodology and correctly attributed to the policy impact. Employment changes in the junior branch, however, are potentially problematic, as they do not really happen close to the relocation site and therefore should not be attributed to the policy. However, given that the junior branch is located far away, there is no reason for the government move to affect it in a way that would be picked up in our event study design. Rather, we would expect those employment fluctuations to increase the overall variance of our outcome variable (employment), thereby making it harder for us to precisely estimate the policy effects.

Second, consider case B where the master branch is located far away from a government relocation site. In this case, a true positive multiplier effect will be wrongly attributed to an outside address. It might be picked up by one of the outside distance bands or lost entirely due to our outer threshold that excludes establishments more than 3km away from a relocation site. This case would lead to attenuation bias in estimating the policy impact.

Considering these two cases, we can conclude that junior branches located close to government relocations sites are a bigger problem than those located further away, because the former situation leads to an attenuation bias while the latter only makes our estimates less precise. Therefore, our estimates of the policy impact presented in the main text are likely to represent a lower bound of the true effect considering attenuation bias caused by the lack of branch addresses.

Empirical inspection

We start our empirical exercise on narrowing down the potential extent of any attenuation bias for our main results by observing that attenuation bias should naturally vary by industry. In industries characterised by a larger proportion of multi-branch companies (i.e., a multi-branch company is a company that does business at multiple physical locations), attenuation bias should be strongest. In industries characterized by single-site companies, however, there should be no attenuation bias at all. Since the BHP does not allow us to make that distinction, we conduct an additional data exercise to this end.

We collect data on commercial registrations ("Gewerbean- und -abmeldungen") from various statistical reports available at the Statistical Office of Berlin-Brandenburg (i.e., the same statistical office from which we retrieved data on total business openings and closings shown in Figure 7). These data focus on openings and closings of both main units and branches located in Berlin for selected 2-digit industries as well as aggregated economic sectors. These data have a few limitations. First, such detailed statistics are not available for our main sample period (1996-2002), only for a later period (2011-2015). Still, we believe that the organisation of an industry (with its ratio of multi-branch companies and single-branch companies) would not dramatically change over a 15-year period. Second, the data show the number of newly registered main units and branches in Berlin in a given year (i.e., flow data). Ideally, we would like to access the stock of all registered main units and branches in Berlin reported at the end of each year. Despite this, aggregating flow data on commercial registrations over several years (2011-2015) to even out

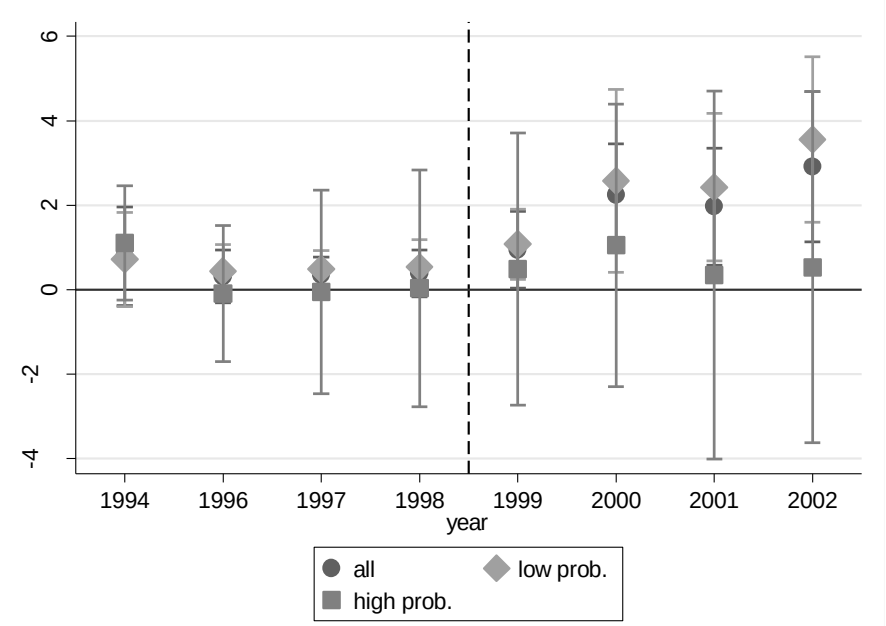
yearly idiosyncrasies, is a good proxy to identify the industries with the highest share of multi-branch companies.

To measure the prevalence of multi-branch companies by industry, we create an indicator defined as the ratio of openings plus closings in branches over openings plus closings in main units for each 2-digit industry.[5] The result of this exercise leads to plausible outcomes. We find that industries with some of the lowest multi-branch ratios are construction, real estate activities and industrial cleaning with a minimum ratio of 0.09. On the other end of the spectrum, we find that industries with some of the highest multi-branch ratios are pharmacies, retail, gas stations and banks, with a maximum ratio of 0.93.

Having identified the industries with the highest ratio of branches to main units, our next step is to verify whether the attenuation bias in the policy impact that we discussed above is indeed stronger for those industries characterised by a higher multi-branch indicator. We test this hypothesis by running separate regressions for industries representing the 20 percent of the workforce with the highest multi-branch indicator versus the bottom 80 percent. We, indeed, find that the attenuation bias in the policy impact is stronger for the 20-percent sample group. Figure C.2.1 shows results for the pooled sample and the two groups of private sector establishments: establishments with a low multi-branch indicator (low prob.) and establishments with a high multi-branch indicator (high prob.). The figure focuses on the first distance band (300m) only. Looking at figure C.2.1, we observe that the estimated policy impact for the bottom 80-percent group is broadly in line with (or slightly larger than) the impact for the pooled sample. For the 20 percent group, we find a much smaller effect, not statistically significant and estimated quite imprecisely.

[5] We adjusted the indicator computed from commercial registration data because they rely on a more recent industry classification (SIC 2008) than the one we use in our main analysis (SIC 2003). To this end, we built a crosswalk between SIC 1993 3-digit codes used in the main analysis and SIC 2008 (more aggregated) 2-digit codes used in the commercial registration statistics. Considering that a single SIC 1993 3-digit code might point to several 2-digit ratios due to a non-unique mapping, our final 3-digit multi-branch indicator is derived as an employment-weighted average of 2-digit ratios.

Figure C.2.1: The impact of relocated jobs on private sector employment: comparing all establishments; establishments with low multi-branch indicator; establishments with high multi-branch indicator



Note: Legend labels are as follows: 'all' refers to all establishments – these results are identical to those reported in Figure 5, Panel B, for the 300m distance band; 'low prob.' refers to establishments with low multi-branch indicator (80 percent of the workforce); 'high prob.' refers to establishments with high multi-branch indicator (20 percent of the workforce). Estimates refer to the first distance (300m) band only, taken from a model specification that includes 300m, 500m, 1000m and 3000m distance bands. All estimates are multiplied by 1000.
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); Statistical office Berlin-Brandenburg; own calculations.

Would it be possible to quantify how strong this attenuation bias is and to correct for it? Lacking convincing instruments, we need some rather strong assumptions about the relationship between the attenuation bias and our multiple branch indicator. Assuming that this relationship is linear, we can add the multi-branch indicator to our regression model and interact it with our treatment intensity variables. This approach effectively filters out the attenuation bias – given that the assumed relationship holds. Comparing these new results with those reported in Table 3 in the main text, the 'adjusted' policy impact for the smallest distance bin (0-300m) is now 2.539 (see Table C.2.1, column 4) instead of 1.754 (see Table C.2.1, column 1), an increase of nearly 45 percent. There are no discernible differences for the outer distance bands.

Table C.2.1: Quantifying the attenuation bias

	Original model specification	Controlling for industry fixed effects	Adding indicator and interaction terms	Further controlling for industry fixed effects
	(1)	(2)	(3)	(4)
0-300 meters	2.534***	2.019***	3.968**	3.756**
	(0.644)	(0.668)	(1.654)	(1.547)
0-500	0.152	0.083	-0.042	-0.152
	(0.315)	(0.301)	(0.318)	(0.347)
0-1000	0.277	0.26	0.238	0.265
	(0.178)	(0.163)	(0.172)	(0.179)
0-3000	0.048	0.03	0.072	0.046
	(0.039)	(0.039)	(0.053)	(0.053)
Constant	0.930***	1.014***	0.724***	1.016***
	(0.176)	(0.179)	(0.189)	(0.178)
Multi-branch indicator			√	
Interaction terms			√	√
Industry FX		√		√
Obs	88,144	88,135	88,140	88,133
R^2	0.301	0.308	0.301	0.308

Note: Robust standard errors are reported in parentheses; (*), (**), (***) indicate significance at the 10%, 5% and 1% levels, respectively. Column (1) reproduces the results reported in the manuscript (see Table 3, column 3). Column (2) add industry fixed effects to the model specification of Column (1). Column (3) adds the multi-branch indicator and interaction terms between the treatment intensity variables and the multi-branch indicator. Column (4) adds industry fixed effects to the model specification of Column (3). All specifications include initial (1998) plant-level employment; pre-trends, defined as (1994-1997) changes in the dependent variable; and grid-specific fixed effects. In addition, standard errors are clustered at the grid level. All estimates are multiplied by 1000.

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); Statistical office Berlin-Brandenburg; own calculations.

While instructive and intriguing, these results need to be taken with caution. There is no particular reason why the attenuation bias should depend on the prevalence of branches to main units in a linear fashion. Therefore, this adjustment might very well over- or under-correct the bias. We note, however, that both our theoretical discussion and the empirical exercise in this section point to the policy impact found in our manuscript to be real, and suggesting that the estimates we found are likely to be lower bounds of the true effect.

Appendix D: Potential winners and losers from the relocation

To assess whether the relocation created winners or losers, we compare the distribution of newly created jobs by worker and plant characteristics with the distribution that prevailed within the same spatial areas in Berlin (within 0-300m of a relocation site) between 1994-1997, a period preceding the government move. The 1994-1997 figures are constructed using the BHP data, while the 1995-2002 figures are derived from our estimations of Model (3) as reported in Tables 5 and 6 of the main text. Results are reported in Tables D.1 and D.2.

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 pp), those in intermediate occupations (-9.7 pp), and middle-skilled workers (-13.1 pp).

Compared to 1994-1997, the relocation program also increased the proportion of jobs in the hospitality sector (cafés & restaurants show a rise of 25 pp), tourism, sport & recreational activities (27.6 pp) and other services (28.6 pp). The 'other services' category includes investigation and security activities, industrial cleaning, photographic services, packaging, secretarial and translation activities, and other business activities not elsewhere classified. The service industries that were negatively affected were hotels (a drop of 18.7 pp), finance (-18.3 pp), media (-14 pp), retail (-15.5 pp) and construction (- 10.7 pp).

We did not make similar comparisons by plant size and establishment age because of the difficulty in separating the group of newly established plants before and after the relocation program.

Table D.1: Distribution of newly created jobs by worker characteristic, comparison between 1994-1997 and 1995-2002

			1994-1997		*1995-2002*	*difference*
		Berlin	***3000 m***	***300 m***	***300 m***	***(4)-(3)***
		(1)	(2)	(3)	(4)	(5)
Gender	**Female**	43.3%	42.6%	34.8%	41.9%	0.071
	Male	56.7%	57.4%	65.2%	58.1%	-0.071
Age	**15-34**	3.2%	15.4%	29.5%	51.1%	0.216
	35-49	113.1%	93.5%	71.6%	45.7%	-0.259
	50-64	-13.9%	-6.8%	2.5%	3.0%	0.005
Occupational level	**Routine & Manual**	17.6%	7.1%	28.1%	27.7%	-0.004
	Intermediate	67.2%	76.4%	57.7%	48.0%	-0.097
	Managerial & Professional	9.7%	11.5%	6.2%	22.8%	0.166
Education qualification	**Low-qualified**	-5.3%	-1.5%	6.4%	18.3%	0.119
	Middle	79.0%	67.5%	68.5%	55.4%	-0.131
	High	22.7%	30.6%	22.1%	22.9%	0.008

Note: Column (5) refers to the difference between the job composition in 1995-2002 and that in 1994-1997, both within 300 meters of a relocation site. Column (4) refers to the results reported in Table 5 in the main text.
Source: BHP 1975-2014 version 1, total population.

Table D.2: Distribution of newly created jobs by industry, comparison between 1994-1997 and 1995-2002

			1994-1997		*1995-2002*	*difference*
		Berlin	***3000 m***	***300 m***	***300 m***	***(4)-(3)***
		(1)	(2)	(3)	(4)	(5)
Total private sector	***Manufacturing***	-34.3%	-17.4%	0.3%	0.2%	-0.001
	Services	134.3%	117.4%	99.7%	99.3%	-0.004
Services	***Construction***	12.5%	5.6%	13.0%	2.3%	-0.107
	Wholesale	4.7%	-0.3%	0.7%	-4.6%	-0.053
	Retail	22.8%	14.4%	26.5%	11.0%	-0.155
	Hotels	14.2%	20.6%	4.3%	-14.4%	-0.187
	Cafes & restaurants	2.7%	6.4%	-12.3%	12.7%	0.250
	Transport, post and communication	2.3%	1.2%	1.5%	3.6%	0.021
	Finance, banking & insurance	14.3%	12.6%	15.7%	-2.6%	-0.183
	Business & consultancy	2.2%	2.9%	5.6%	5.9%	0.003
	Media & publishing and printing	31.5%	33.1%	62.2%	48.2%	-0.140
	Tourism & recreational services	0.6%	-0.8%	-6.6%	21.0%	0.276
	Personal services	2.2%	0.5%	0.5%	0.0%	-0.005
	Other services	24.2%	21.3%	-11.4%	17.2%	0.286

Note: Column (5) refers to the difference between the job composition in 1995-2002 and that in 1994-1997, both within 300 meters of a relocation site. Column (4) refers to the results reported in Table 6 in the main text. Industries include 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.
Source: BHP 1975-2014 version 1, total population.

Appendix E: Permutation exercise

As an additional robustness check, we perform a permutation exercise to verify the statistical significance of our results. We conduct this test by randomly reshuffling establishments within their own grid[6] to obtain a new set of estimates, which we compared to the original ones. Randomly reshuffling plants within their own grid effectively breaks their pattern of distances to close relocation sites (up to 2km), while keeping the greater structure of economic activity within Berlin intact. We believe that this form of permutation test is preferable to a 'Dartboard-Approach' that builds a contrafactual situation by randomly locating relocations on the Berlin map. The latter approach ignores the city structure of Berlin and assumes that relocated institutions could have been placed anywhere, which is unlikely. Our approach is stricter in rejecting the null hypothesis of no effect of public sector job relocations on private sector employment. By using 1000 random permutations of the data, we can construct a 95% confidence interval (CI) from the 1000 sets of new results. Figure E.1 shows that, by applying this procedure, we can construct a CI for each sub-group of data that we analyzed in Tables 3, 6 and 7 in the main text.

The permutation test is then conducted as follows: if our original estimates fall within a CI, we cannot reject the null hypothesis that the original and the new sets of estimates are equal. If our original estimates lie outside a CI, we can reject the null hypothesis of equality, confirming that the results presented in the main text are statistically different from those obtained by randomly reshuffling plants within their grid. More importantly, this implies that the policy impact documented in our paper is genuine. A permutation test that rejects the null hypothesis would provide additional evidence that distance to a relocation site matters and that our approach has been successful at capturing such effect.

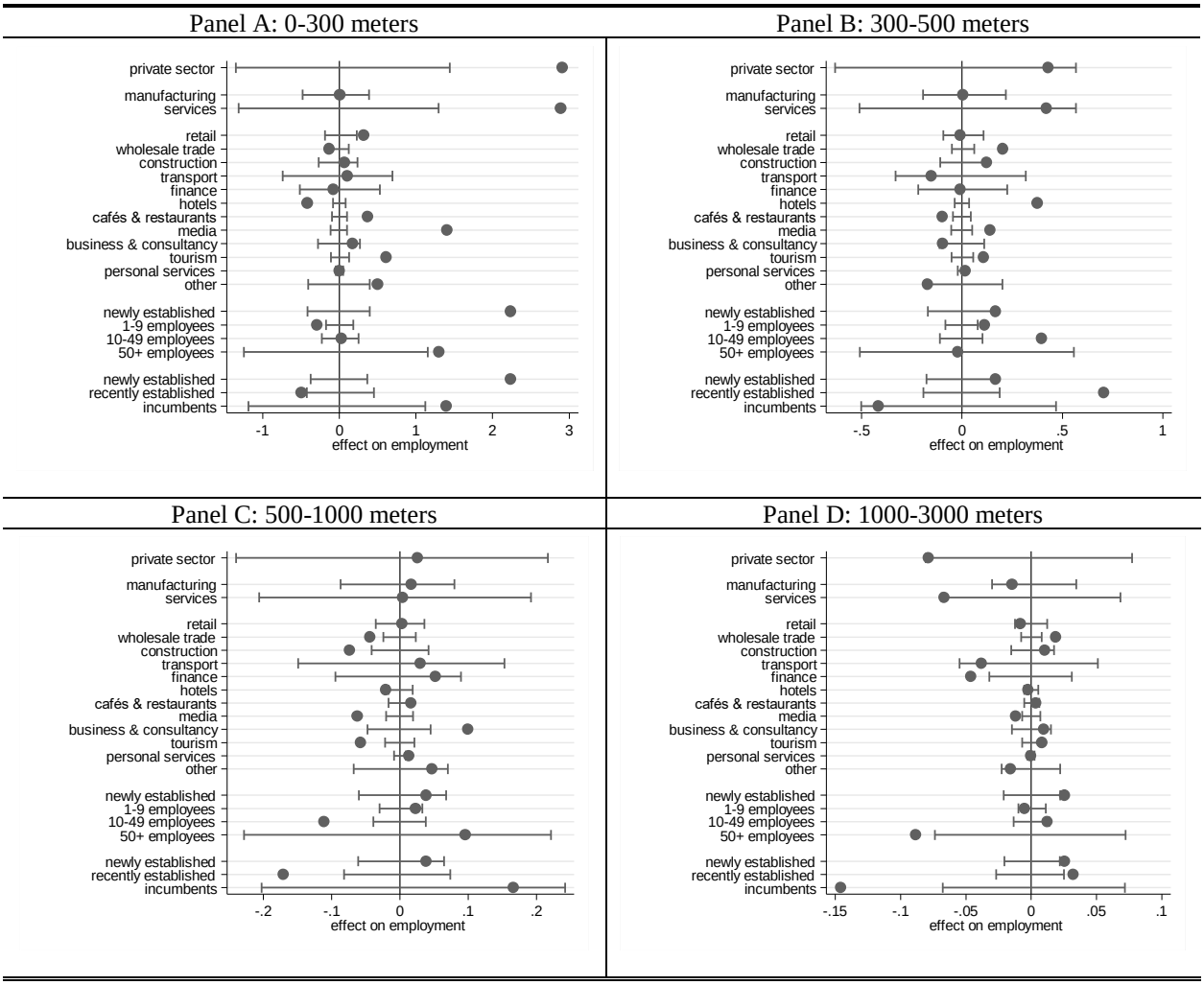
Figure E.1 shows permutation test results for the event studies specification with varying treatment effects. Qualitatively similar results are obtained using the other two specifications and are available upon request. Figure E.1 is organised as follows: each panel reports results for a specific distance band d, with $d \in (300, 500, 1000, 3000\ meters)$; each line represents a sub-group of establishments; for each sub-group, the rounded mark is the original estimated coefficient whereas the 95% CI represents the new set of estimates derived through the 1000 data permutations. Looking at Figure E.1, Panel A, test results confirm that private sector

[6] As mentioned in Section V in the main text, we have divided Berlin in 479 2km-side grids.

establishments located within 300 meters of a relocation site behave differently than corresponding establishments randomly located within a grid. The same reasoning holds for plants operating in services and, more specifically, in media, tourism, cafés & restaurants, retail and hotels. A plant's proximity to a relocation site clearly matters for such sub-groups. Looking at differences by initial plant size, the test clearly shows that new entrants in proximity to a relocation site behave differently from new entrants randomly located within a grid. This is also true for medium-sized plants (with 50-499 employees) within the first 300 meters.

Moving to Figure E.1, Panel B, we observe that the most interesting results are related to plants operating in the hospitality industry, wholesale trade, and media, as well as small-sized plants (with 10-49 employees). For all these sub-groups, the distance to a relocation site matters. As we move to Panels C and D and consider greater distances, the test results become weaker. It becomes increasingly more difficult to identify sub-groups located within 500-1000m and 1000-3000m distances that behave differently from corresponding plants randomly located within a grid. This is unsurprising and consistent with our main findings. It is worth noting the relationship between our original estimates (the rounded marks) and the new sets of estimates (the 95% confidence intervals) as we move from Panel A to Panel D of Figure E.1. As we increase the distance to a relocation site, our estimated policy effects (rounded marks) slowly shift from positive to negative, and their size gets smaller (see Figure E.1, Panels A-D, *x-axis* value range).

Figure E.1: Permutation exercise



Note: Panels A-D show the results of permutation tests (based on 1000 data permutations) for different distance band d, $d \in (300, 500, 1000, 3000)$. The dependent variable is private sector employment recorded as employment in the private sector; manufacturing; services; retail; wholesale trade; construction; transport; finance; hotels; cafés & restaurants; media; business & consultancy; tourisms; personal services; other; newly established plants in 1997; plants with 1-9 employees; plants with 10-49 employees; plants with 50-499 employees; plants with 500+ employees. Results are obtained using an event studies specification with varying treatment effects (see Equation 3 in the main text). All estimates are multiplied by 1000.

Sources: BHP 1975-2014 version 1, total population; relocation data are collected from several sources (see Table B.1 in Online Appendix B for details).

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