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The Impact of Public Employment: Evidence from Bonn*

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Abstract

This paper evaluates the impact of public employment on private sector activity using the relocation of the German federal government from Berlin to Bonn in the wake of the Second World War as a source of exogenous variation. To guide our empirical analysis, we develop a simple economic geography model in which public sector employment in a city can crowd out private employment through higher wages and house prices, but also generates potential productivity and amenity spillovers. We find that relative to a control group of cities, Bonn experiences a substantial increase in public employment. However, this results in only modest increases in private sector employment with each additional public sector job destroying around 0.2 jobs in industries and creating just over one additional job in other parts of the private sector. We show how this finding can be explained by our model and provide several pieces of evidence for the mechanisms emphasised by the model.

Keywords: Economic Geography, Public Employment, Place-Based Policies, German Division

JEL: F15, J45, N44, R12

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1 Introduction

Following Krugman (1991), there has been a wave of research investigating the spatial distribution of economic activity. By and large, this literature has concentrated on the location choices of firms and workers in the private sector. However, in most advanced economies, a substantial share of the workforce is employed in the public sector.[1] This is important for at least two reasons. First, the spatial distribution of public employment is unlikely to be determined exclusively by market forces. Indeed, many governments use public employment as a form of regional policy and create public sector jobs in economically lagging regions.[2] Second, the distribution of public employment across locations should have important impacts on the location of private sector activity through its general equilibrium impact on wages and housing costs and also through potential productivity and amenity spillovers from the public to the private sector.

In this paper we use the relocation of the German federal government from Berlin to Bonn in the wake of the Second World War as a source of exogenous variation to evaluate the causal impact of public employment on the spatial distribution of private sector activity. This approach has a number of attractive features. First, the arrival of the federal government in Bonn was a large and plausibly exogenous shock to public employment, which was driven by factors that are unrelated to the local economic performance of Bonn. Second, we are able to follow the impact of this shock over several decades, which allows us to capture the long-run general equilibrium response to this shock. Third, we are able to provide a number of pieces of evidence on the mechanisms through which public and private sector employment interact.

To guide our empirical analysis we develop a simple theoretical model which builds on Helpman (1998) and Redding and Sturm (2008). In the model both private sector firms and the public sector demand labour across different cities. The public sector produces a global public good which enters the utility of all workers in the economy equally. The private sector consists of both a tradable and non-tradable sector. Firms in the tradable sector produce manufacturing varieties which are tradable across cities at some cost while varieties produced by firms in the non-tradable sector can only be consumed locally. In the model the location of public employment affects the location choices of private sector workers and firms through a number of channels. On the one hand, increases in public employment in a location crowd out private sector employment through increases in house prices and nominal wages. On the other hand, public sector employment can potentially promote private sector activity through productivity and amenity spillovers from the public to the private sector and also by dampening the extent of local competition.

[1] OECD (2009) reports that the share of public employment across OECD countries in 2005 ranges from highs close to 30% in Sweden and Norway to lows of less than 10% in Switzerland, Korea and Japan with an OECD average in 2005 of 14.4%. In many countries public employment is higher than employment in industry.

[2] Alesina, Danninger, and Rostagno (2001), for example, argue that in Italy public employment is mostly used as a redistributive device from the richer North to the poorer South.

Using panel data for Bonn and 40 control cities covering the period from 1925 to 1987, we examine the predictions of our model. The 40 control cities are the 20 cities ranked just above and just below Bonn in terms of total 1939 population. We employ both a difference-in-differences comparison between Bonn and the control cities and also construct a synthetic control city for Bonn using the 40 control cities as the donor pool. Both approaches yield similar results and show that the substantial increase in public employment in Bonn has only resulted in more modest increases in private sector employment. In particular, translating the estimated treatment effects from comparing Bonn to its synthetic control into employment multipliers we find that each additional public sector job reduces employment in industry by around 0.2 jobs and creates just over one additional job in other parts of the private sector.

We next show how this finding can be explained by our model and provide several pieces of evidence for the mechanisms emphasised by the model. First, we provide some suggestive direct evidence for the amenity and productivity spillovers from public to private employment that the model postulates. This data suggests that there could be amenity spillovers from public employment in Bonn, but there is little evidence for productivity spillovers. Second, we use data on wages and house prices to show that house prices are positively related to city size as suggested by the model while the expansion of public employment has only had very small effects on nominal wages in Bonn by 1987. Third, we undertake a simple quantitative analysis of the model to explore for which parameters the model can best fit the reduced form evidence. The quantitative analysis suggests that the increase in public employment has generated sizeable amenity spillovers and if anything marginally negative productivity spillovers, which reinforces the reduced form evidence on mechanisms.

Our paper contributes to a number of literatures. The idea that expansions in employment in a tradable sector can have multiplier effects on employment in other sectors in the same location has a long history with early contributions by Daly (1940), Hildebrand and Mace (1950) and Thompson (1959) (see Richardson (1985) for a survey of this early work). The early literature in this area used simple cross-sectional correlations or input-output approaches to estimate multipliers and struggled to isolate plausibly exogenous variation. To overcome this problems Carrington (1996) uses the exogenous shock to employment caused by the construction of the Trans-Alaskan Pipeline between 1974 and 1977 on other parts of the Alaskan labor market. Similarly, Black, McKinnish, and Sanders (2005) analyse the effect of the coal boom in the 1970s and the subsequent coal bust in the 1980s as an exogenous shock and estimate that each additional mining job created 0.17 non-tradable jobs while the loss of a mining job implies a loss of 0.34 non-tradable jobs. Moretti (2010) uses a shift-share instrument to isolate exogenous variation in manufacturing employment across US cities. He estimates that an additional job in the tradable sector of a US city creates 1.6 additional jobs in the city's non-tradable sector, with a larger multiplier effect for additional skilled jobs. Recently, Feyrer, Mansur, and Sacerdote (2017) use the drilling of new wells during the US fracking boom to examine the spatial and sectoral dispersion of this shock.

While governments have some policy instruments to influence the spatial distribution of manufacturing and other tradable employment, they have direct control over the location of public employment and regularly use re-locations of public employment to support lagging regions. Surprisingly, there is little systematic evidence on the spillover effects of changes in the spatial distribution of public employment on the private sector. An important exception is Faggio and Overman (2014) who use a shift-share instrument to isolate exogenous variation in public employment across 325 UK local authorities covering the period 2003 to 2007. They find that increases in public sector employment have small but statistically insignificant positive effects on overall private sector employment that are composed of a decrease in employment in industry and increases in non-tradable sectors.

Other contributions to this literature following Faggio and Overman (2014) are Faggio (2014) who uses a difference-in-differences approach on data covering 2003-2008 to evaluate relocations of public sector employment from London recommended by the Lyons Review, finding broadly similar results to Faggio and Overman (2014). Schlüter (2014) investigates whether the opening of federal ministries in Berlin in 1999 has resulted in faster private sector job creation in areas of Berlin close to the new ministries. Auricchio, Ciani, and de Blasio (2016) use a shift-share instrument similar to Faggio and Overman (2014) to estimate the effect of changes in public employment on private employment across Italians municipalities using data from the 2001 and 2011 census. Jofre-Monseny, Silva, and Vazquez-Grenno (2017) simulate the impact of public on private employment in a search and matching model and also show reduced from evidence using Spanish data for 1980, 1990 and 2001. Our approach differs from Faggio and Overman (2014) and the subsequent literature in three main ways. First, we are able to observe the long-run impact of a large change in public employment. Second, we use a natural experiment to provide plausibly exogenous variation to estimate a difference-in-differences strategy to identify the impact of public employment on private employment. Third, we develop a simple theoretical model which can be calibrated to the reduced form evidence to shed light on the mechanisms through which public and private employment interact.[3,4]

Our paper is also related to the wider literature following Rosen (1979) and Roback (1982) that examines amenity and productivity differences across locations. See, for example, Albouy and Lue (2015) for a recent contribution. While much of this literature has considered exogenous differences in amenities, many urban amenities are plausibly endogenously determined, as in Glaeser, Kolko, and Saiz (2001), Ahlfeldt, Redding, Sturm, and Wolf (2015) and Diamond (2016). We show that public employment is potentially an important channel of local amenity differences across cities. We also contribute to the wider literature on local labor market shocks. One strand of this literature investigates

[3] Boeri, Nicoletti, and Scarpetta (2000) and Algan, Cahuc, and Zylberberg (2002) use panel data across a small set of OECD countries to estimate the impact of higher public employment on private employment at the country level. Although interesting, these estimates do not fully take into account adding-up constraints, or fully resolve the endogeneity problems surrounding public employment.

[4] A further related literature uses spatial variation to provide new estimates of the government spending multiplier. See Acconcia and Simonelli (2014) and Nakamura and Steinsson (2014) for recent contributions.

the impact of closures of military bases on local economic outcomes (see, for example, aus dem Moore and Spitz-Oener (2012) for evidence from Germany and Moretti (2011) for a recent survey). Finally, there is a growing literature that evaluates the effects of place based policies. Recent contributions to this literature include Kline and Busso (2013) and Kline and Moretti (2014) (see, Neumark and Simpson (2015) for a recent survey).

The remainder of this paper is organised as follows. The next section develops a simple theoretical framework to guide our empirical analysis. Section 3 provides some historical background. Section 4 describes our data and Section 5 our empirical strategy. Section 6 presents our main empirical findings. Section 7 provides evidence for the mechanisms through which public and private employment interact and the final section concludes.

2 Theoretical Framework

We develop a simple economic geography model in which both private sector firms and a public sector employ workers. The model builds on Helpman (1998) and Redding and Sturm (2008). The main building blocks of the model are developed in this section and a more detailed exposition of the model is contained in the technical appendix. In the model private sector firms produce varieties of either a tradable or non-tradable good. The two agglomeration forces are, first, that firms with better market access can pay higher nominal wages and, second, consumers with better market access can consume varieties of the tradable good at lower cost and access more non-tradable varieties. The main dispersion force are house prices, which increase as more workers move to a city. In equilibrium workers are indifferent across all locations that are populated in equilibrium. The key novel feature of the model is the presence of a public sector which employs workers and produces a global public good. We treat the location of public sector jobs as a policy parameter and explore how the distribution of public sector employment affects the equilibrium distribution of private sector employment.

2.1 Preferences and Technology

The economy consists of a number of locations, which we refer to as cities. The economy is populated by a mass of representative workers, L, who are mobile across cities and are endowed with a single unit of labor which is supplied inelastically with zero disutility.[5] Utility is defined over a consumption index of tradable varieties, C_c^T, a consumption index of non-tradable varieties, C_c^N, consumption of housing, C_c^H, and the amenity level of the city, B_c.[6] The upper level utility function is assumed to be Cobb-Douglas:

$$U_c = B_c \left(C_c^T\right)^{\mu} \left(C_c^N\right)^{\theta} \left(C_c^H\right)^{1-\mu-\theta}, \qquad 0 < \mu, \theta < 1. \tag{1}$$

[5] It would not be difficult to extend the model to also include non-working dependents for each worker.

[6] We use both c and i to index cities. When the distinction is important, we use c to indicate a city when it is consuming and i to indicate a city when it is producing.

There are iceberg type transport costs for tradable varities between locations and for one unit of a variety produced in city i to arrive in city c, a quantity $\tau_{ic} > 1$ must be shipped. In contrast non-tradable varieties can only be consumed in the city in which they have been produced.

The tradables consumption index takes the standard CES (Dixit-Stiglitz) form. The dual tradables price index is as follows:

$$P_c^T = \left[\sum_i n_i^T (p_i^T \tau_{ic})^{1-\sigma} \right]^{1/(1-\sigma)}, \tag{2}$$

which uses the fact that all n_i tradable varieties produced in city i face the same elasticity of demand and charge the same equilibrium price $p_{ic} = T_{ic} p_i$ to consumers in city c. The price index implies that higher prices in cities i or higher transport costs between cities i and c result in a higher tradables price index in city c. In contrast non-tradable varieties can only be consumed in the city in which they are produced. Hence the non-tradable price index in city c is:

$$P_c^N = \left[n_c^N (p_c^N)^{1-\sigma} \right]^{1/(1-\sigma)} = p_c^N \left(n_c^N \right)^{1/(1-\sigma)}, \tag{3}$$

which depends on the number of non-traded varieties produced in the city (n_c^N) and their equilibrium price (p_c^N) and where we have used the fact that all non-tradable varieties in a city charge the same equilibrium price.

Tradable and non-tradable varieties are produced with the same production technology. There is a fixed cost in terms of labor of producing varieties, $F > 0$, and a constant variable cost. The total amount of labor, l, required to produce x units of a variety is:

$$l = F + \frac{x}{\varphi_i}, \tag{4}$$

where φ_i captures the productivity of firms in city i. Firms maximise profits ignoring their effect on the price index. This yields the standard result that the equilibrium free on board price of varieties is a constant mark-up over marginal cost:

$$p_i = \left(\frac{\sigma}{\sigma - 1} \right) \frac{w_i}{\varphi_i}. \tag{5}$$

where σ is the constant price elasticity of demand, which is the same for tradable and non-tradable varieties.

Instead of modelling a construction sector which provides housing we assume a simple housing supply function that relates house prices to the level of population of a city:

$$P_c^H = (L_c)^\gamma \tag{6}$$

where γ is the elasticity of house prices with respect to city population.[7] We assume that expenditure on housing in city c, which is a constant share of city c income, is redistributed to the population of city c through lump-sum transfers.

[7] Saiz (2010) provides a full micro-foundation of this housing supply function.

2.2 Public Sector

The public sector produces a global public good (e.g. defence) that affects all agents in the economy equally independent of their location. Without loss of generality we normalise the utility that agents derive from this public good to zero. We assume for simplicity that the total number of public sector workers in the economy is constant. The key policy decision is the distribution of public employment across cities where L_i^G is number of public sector workers in city i.[8] Public sector workers in city i are paid a wage w_i^G which we assume to be equal to the wage w_i that private sector workers in city i receive.[9] To finance public employment the government levies a flat income tax τ on the wage income of both public and private sector workers to exactly satisfy the government budget constraint:

$$\tau \sum_i (w_i L_i) = \sum_i (w_i L_i^G). \tag{7}$$

We assume that public sector employment in a city generates two potential spillovers to private sector workers in both the tradable and non-tradable sector in the same city. First, productivity of private sector workers in city i is:

$$\varphi_i = (L_i^G)^\alpha, \tag{8}$$

where α is the elasticity of the productivity of private sector firms in city i with respect to the size of the public sector in this city. Similarly, we assume that consumption amenities in city i are determined by:

$$B_c = (L_c^G)^\delta, \tag{9}$$

where δ is the elasticity of city amenities with respect to the size of the public sector.

2.3 Spatial Equilibrium

With free labor mobility across cities and also across sectors within cities, workers' utility has to be the same in all cities. Substituting the equilibrium demand functions into the utility function (1), equal utility across cities implies:

$$\frac{w_c B_c}{(P_c^T)^\mu \, (P_c^N)^\theta \, (P_c^H)^{1-\mu-\theta}} = k, \qquad \text{for all } c \tag{10}$$

where we implicitly assume that all cities are populated in equilibrium. The intuition behind (10) is that higher wages or higher amenities make a city more attractive, while higher housing costs or a higher price of tradable or non-tradable varieties make the city less attractive.

[8] In the model it is equivalent whether the public sector creates job openings in a particular city, or whether the public sector has a dedicated workforce that can be moved between cities.

[9] It would not be difficult to extend the model so that the public sector pays a higher wage than private sector firms and public sector employment is rationed in equilibrium. This would marginally change the quantitative implications of our model but not the qualitative insights.

2.4 Simulation

Due to the non-linearity of the model we simulate a two-city version of our model as is standard in the economic geography literature. In particular we start with two ex-ante identical cities and explore the impact of changes in the location of public employment on private sector activity. For this simulation we assume central values for the key parameters of the model. First, we assume an elasticity of substitution (σ) of five which is similar to the values typically used in the international trade literature (see, for example, Feenstra (1994); Ghironi and Melitz (2005)). Second, we set the share of expenditure on housing in total expenditure ($1-\mu-\theta$) to one-third which is somewhat larger than the housing expenditure share of around 0.25 estimated by Davis and Ortalo-Magné (2011) for the US. Third, we set the share of public employment in total employment to 12 percent which is the average level of pre-treatment public employment in Bonn and its synthetic control and is also not far from the 2005 OECD average reported in OECD (2009) as discussed in the introduction. Fourth, we set the share of expenditure on both tradable output (μ) and non-tradable output (θ) both equal to one-third. Moretti (2010) and Faggio and Overman (2014) assume that only manufacturing is tradable, which ignores that some services are also tradable. In contrast, Lombardo and Ravenna (2012) estimate the share of tradable sectors at the level of countries and report tradable shares in excess of 50 percent of employment. Fifth, we assume that the iceberg trade cost for tradable output between the two cities (τ_{ic}) is equal to 1.25, which is very similar to the average transport costs (excluding border related costs and wholesale distribution costs) of 21% of the value of goods estimated by Anderson and van Wincoop (2004). Finally, Saiz (2010) estimates a population-weighted average elasticity of housing supply in US metropolitan areas of 1.75. We are interested in the inverse of this, i.e. the elasticity of house prices with respect to population (γ), which we set to $1/1.75 = 0.57$.[10]

Figure 1 shows the impact of shifting public employment from city 2 to city 1 for different assumed values of the amenity and productivity spillover parameters δ and α. The figure has a number of striking features. In the absence of productivity or amenity spillovers (i.e. $\delta = 0$ and $\alpha = 0$) shifting public employment from city 2 to city 1 actually reduces total employment in city 1, with particularly large reductions in private employment in the tradable sector. To see the intuition why private sector employment has to fall by more than the additional public sector employment in city 1 consider the following thought experiment. Suppose that one public sector worker migrates from city 2 to city 1 and a private sector worker in the tradables sector migrates at the same time from city 1 to city 2. This change leaves total employment, nominal wages, house prices and total expenditure in the two cities unchanged. However, there is now less private sector tradables production in city 1, which increases the price index of tradables (2) in city 1 relative to city 2 as more varieties are now shipped from city 2 to city 1 involving positive trade costs. To bring about spatial equilibrium more private sector workers therefore have to leave city 1 until house prices in city 1 have fallen sufficiently relative to city 2 to make

[10] We also normalise the fixed costs F, which rescales nominal wages across all cities, to one.

workers again indifferent between locating in city 1 and city 2. The drop in private sector workers is concentrated in the tradable sector of city 1 due to the Cobb-Douglas specification for the upper level utility function that implies that non-tradables demand is proportional to city population. The more than proportional crowd-out of private sector employment depends critically on the assumption of positive trade costs between cities. If trade between cities was frictionless then the reduction in private employment in city 1 would be exactly proportional to the increase in public employment and city size would be unaffected by a shift in public employment.

Figure 1 also shows that if there are productivity and amenity spillovers from public sector employment to the private sector, then the negative impact of shifting public employment to city 1 on total employment in city 1 can be overturned. In particular, Figure 1 shows both a case of intermediate spillovers where we set $\alpha = 0.004$ and $\delta = 0.004$ and a case of stronger spillovers where we set $\alpha = 0.008$ and $\delta = 0.008$. With intermediate spillovers total private sector employment in city 1 stays roughly constant as public employment is relocated to city 1 and total employment therefore increases. The constant private sector employment in city 1 is made up of a drop in tradables employment and an similar increase in non-tradables employment in city 1. With strong spillovers both tradable and non-tradable employment increases in city 1 as public sector employment is added to the city with the increase being particularly pronounced for non-tradable employment.

Productivity or amenity spillovers that are strong enough to increase total employment in city 1 also increase house prices in city 1. The reason is that house prices depend on nominal expenditure in each city, which is primarily a function of total city population. The impact of shifts in public employment on wages in the two cities is somewhat more subtle and quantitatively small. Even in the no-spillover scenario nominal wages in city 1 increase marginally due to two opposing forces. First, there is the usual new economic geography mechanism that firms with better market access (i.e. firms located in the larger city) are able to pay higher nominal wages in a zero profit equilibrium. Second, public employment creates additional demand for private sector firms that in contrast to additional demand from workers in the private sector does not increase local competition. The combination of these two effects results in a small increase in nominal wages in city 1.

We have also simulated the model for the case of only productivity spillovers or only amenity spillovers. Both types of spillovers have a qualitatively similar impact on private sector employment in the tradable and non-tradable sector. Both spillovers also have a positive impact on both nominal wages and house prices in city 1. However, amenity spillovers have a larger positive effect on house prices than wages relative to the impact of productivity spillovers on house prices and wages. This prediction of the model is similar in spirit to the classic Roback (1982) analysis. However in contrast to Roback (1982) in our model both wages and house prices increase if there are amenity spillovers from increased public employment in city 1 while in Roback (1982) high amenities in a location reduce wages. Note that from the spatial equilibrium condition (10) also in this model wages and amenities enter multiplicatively and higher amenities could therefore be offset by lower wages. However, our

model features positive transport costs which imply that an increase in population due to an increase in amenities not only increases local housing prices but also improves market access of a city which in turn enables firms to pay higher wages in a zero profit equilibrium. This market size mechanisms dominates the classic Roback (1982) trade-off between wages and amenities.[11]

In summary, the model illustrates how the interaction between public and private employment is shaped by crowd-out mechanisms through wages and house prices and potential agglomeration forces through amenity and productivity spillovers from public to private employment. The relative strength of these mechanisms is an open empirical question to which we now turn. In section 7.5 we return to the theoretical model and determine for which parameter values it best fits the reduced form evidence.

3 Historical Background

In the wake of the Second World War, Germany was divided into four different parts: East and West Germany, areas that became part of Poland, and an area that became part of the Soviet Union. Berlin, the pre-war capital of Germany, was situated approximately 200 kilometres to the east of the border between East and West Germany. Berlin was jointly occupied by U.S., British, French, and Soviet troops and divided into four sectors of occupation. The origins of Germany's division can be traced back to a wartime protocol that organised Germany into zones for the purposes of the post-war military occupation. With the intensification of the Cold War, cooperation between the Western allies and the Soviet Union deteriorated and West Germany was founded in 1949 on the area of the U.S., British, and French zones, while East Germany was founded in the same year on the Soviet zone (see, for example, Franklin (1963) and Kettenacker (1989)).

As part of the foundation of West Germany, a location for the new West German government had to be found. There were four main contenders for the seat of government, which in order of their 1939 population were: Frankfurt, Stuttgart, Kassel and Bonn. It was widely believed that Frankfurt was the obvious choice for several reasons: It had been the seat of the first German parliament in 1848, it was the largest of the candidate cities, and it was centrally located in West Germany with good transport links. However, on 10 May 1949, the West German Parliamentary Council ("Parlamentarische Rat") voted narrowly for Bonn as the new capital of West Germany. Of the 65 delegates, 29 voted for Frankfurt and 33 for Bonn, with one invalid vote and two abstentions. This decision was confirmed by the newly constituted West German parliament on 3 November 1949 with 200 votes for Bonn versus 176 for Frankfurt (Floehr (1986)).[12] A popular myth is that Konrad Adenauer, Germany's first post-war

[11] Redding and Sturm (2008) and Handbury and Weinstein (2015) for recent evidence for the importance of market access at the city level.

[12] The unexpected choice of a provincial town as Germany's capital was widely ridiculed. The New York Herald Tribune called Bonn in 1959 "one of the strangest capitals of the twentieth century"; the English envoys referred to the British Embassy in Bonn as "Her Majesty's only mission in a cornfield" and a Newsweek correspondent was surprised to see that his news bureau close to the federal parliament "faced a meadow on which a shepherd grazed his flock every Friday

chancellor, single-handedly engineered this outcome as he was of an advanced age and lived on the outskirts of Bonn.[13] The truth is likely to be more mundane. First, the heavy military presence of American troops in Frankfurt was viewed as a disadvantage for an independent West German government. Second, making Bonn the new capital was viewed as a signal that the division of Germany was a temporary arrangement that had to be overcome as soon as possible.

Despite initial hopes that division would be short-lived, it was over time formalised in international treaties and became widely believed to be permanent.[14] However, increasing dissatisfaction among East Germans led to large scale demonstrations in 1989 and culminated in the fall of the Berlin Wall on 9 November 1989. Only eleven months later, on 3 October 1990, East and West Germany were formally reunified. On 20 June 1991 the German parliament voted in a narrow vote of 338 to 320 to relocate the parliament and parts of the federal ministries back to Berlin. In return for the loss of status and economic power, Bonn received generous compensation. This involved financial compensation, the allocation of new institutions of national and international significance, and the agreement that every federal ministry would have offices both in Berlin and Bonn and the majority of federal government employment would remain in Bonn. After extensive building works, the parliament moved from Bonn back to Berlin in September 1999.[15]

4 Data

Our basic dataset is panel data on employment in Bonn and a set of 40 control cities which were also located in West Germany. The control cities are the 20 West German cities with a 1939 population just above Bonn and the 20 cities with a 1939 population just below Bonn. Bonn itself had 152,057 inhabitants in 1939 and was the 31st largest West German city in 1939. The population of the 40 control cities ranges from 83,385 in Bottrop to 458,429 in Stuttgart in 1939. We have also experimented with using a larger number of control cities but this has no meaningful impact on the results. Figure 2 shows the spatial distribution of Bonn and the 40 control cities.[16] The dataset contains observations from the pre-war censuses in 1925, 1933 and 1939 as well as data from the post-war censuses in 1946, 1950, 1961, 1970 and 1987. To make cities as comparable as possible over time, we follow Redding and Sturm (2008) and aggregate settlements that had at least 10,000 inhabitants in 1919 and merged with

afternoon" (as cited in Wise (1998)).

[13] A common German joke at the time was that "If you say 'A' for Adenauer you also have to say 'B' for Bonn".

[14] West German opinion polls in the 1980s show that less than 10 percent of the respondents expected a reunification to occur during their lifetime (Herdegen (1992)).

[15] The extensive compensation and limited relocation of federal ministries suggest that the relocation of the federal government back to Berlin was a much smaller shock for Bonn. Consistent with this there is no evidence for a change in the growth rate of population or employment in Bonn in the years after 1999. In this paper we therefore concentrate on the much larger and cleaner shock of the arrival of the federal government in Bonn in the wake of the Second World War.

[16] We exclude the city of Saarbrücken from the control group, because Saarbrücken was under the rule of the League of Nations from 1919 to 1935 and under French rule from 1945 to 1957.

one of our cities during the sample period in all years in the data. A list of all aggregations is contained in the technical appendix.

Due to Germany's federal structure in the post-war period, census employment data at the city level is published by the statistical offices of the different West German states ("Länder") while the pre-war data was published by the Statistical Office of Germany ("Statistisches Reichsamt").[17] We are able to disaggregate total employment into 10 sectors ("Wirtschaftsabteilungen"). These 10 sectors have been used unchanged in the 1961, 1970 and 1987 censuses. We use a concordance to aggregate the employment data from the 1950 and 1946 censuses to the same 10 sectors. The 1933 and 1925 censuses have also published employment data at the city level for a large number of sectors. We have developed a concordance to aggregate this data to the same 10 sectors that are used in the post-war data. The 1939 census has only published employment at the city level in four sectors which are aggregates of the 10 sectors. We use the 1933 employment shares in each city to disaggregate employment in 1939 in these four sectors into the same 10 sectors that we use for other years. Dropping the 1939 data from the sample makes no difference to our results.[18]

The 10 sectors into which we are able to disaggregate employment (and their 1987 German name) are agriculture ("Land- und Forstwirtschaft und Fischerei"), mining and energy ("Energie- und Wasserversorgung und Bergbau"), construction ("Baugewerbe"), industry ("Verarbeitendes Gewerbe"), trade ("Handel"), transport and communication ("Verkehr und Nachrichtenübermittlung"), finance and insurance ("Kreditinstitute und Versicherungsgewerbe"), other services ("Dienstleistungen von Unternehmen und freien Berufen"), non-profit sector ("Organisationen ohne Erwerbszweck und private Haushalte"), public sector ("Gebietskörperschaften und Sozialversicherung"). Agricultural employment is very low in our sample of cities and we omit it when we disaggregate total employment into its sectors. Public employment in the 1961 to 1987 censuses consists of a conservative definition of public employment that only includes employment in the public administration and social security administration. It does not include employment in the health sector and education which are both part of "other services", and also does not include employment in state owned enterprises. Across Bonn and the control cities the average share of public employment in total employment in our data is just over 12% in 1987, which is similar to the 2005 average of the 10 countries discussed in the introduction.[19]

In addition to the employment data we have collected a number of datasets to provide evidence for the mechanisms emphasised by our model. First, we use data reported in Deutscher Städtetag (1988),

[17] The technical appendix provides detailed information on the publications of each regional statistical office from which the data has been compiled.

[18] Details of the concordance and how we disaggregate the data reported in 4 sectors for 1939 into 10 sectors are provided in the technical appendix. We also use a similar imputation to disaggregate employment reported in 4 sectors into 10 sectors for a small number of additional observations due to missing 10 sector employment breakdowns for smaller cities.

[19] As discussed in detail in Section 2 of the technical appendix, data limitations in the earlier years of our long time series imply that the definition of public employment changes slightly over time. In particular, in the pre-war censuses of 1925, 1933 and 1939 public employment includes the military and the clergy and in 1950 it includes employment by the occupying forces and foreign embassies.

which contains a number of proxies for consumption amenities across the cities in our sample in 1987. We have also obtain similar data for 1929/1930 for a much smaller number of proxies for amenities from Deutscher Städtetag (1931). Second, we have collected data on gross value added per worker in Bonn and the control cities from Gemeinschaftsveröffentlichung der Statistischen Landesämter (1991). Third, we use data from the Historic Employment and Establishment Statistics (HES) database (see Bender, Haas, and Klose (2000) for a detailed description) for 1987 that allows us to estimate individual-level wage regressions. While comparable wage data do not exist for the pre-war period, we proxy pre-war wage differences across cities with information on payroll tax receipts in 1937 reported in Statistisches Reichsamt (1941). Finally, we have obtained data for 1986 to 1988 on the average price of different types of real estate across a sample of West German cities from the Association of Real Estate Agents ("Ring Deutscher Makler - RDM").

5 Empirical Approach

Our basic empirical approach is a simple difference-in-differences comparison of Bonn and our 40 control cities:

$$Y_{ct} = \gamma + T_c + T_c \times D_t + \lambda_t + \varepsilon_{ct} \tag{11}$$

where Y_{ct} is employment in city c in period t and employment can be total employment or employment in subsectors depending on the specification; T_c is a dummy that is equal to one for Bonn and zero otherwise; D_t is a dummy that is equal to one after 1949 and zero otherwise; λ_t is a full set of time dummies; ε_{ct} is an error term. As we observe Bonn and the control group of cities for several periods prior to treatment, we can assess whether the treatment city and control group of cities move in parallel before the treatment. Whereas difference-in-differences comparisons are one of the classic approaches to analyse natural experiments, there are two potential concerns with this approach in our setting. First, as we only have one treatment unit (Bonn), it is particularly important to compare this city to a control group that is as similar to Bonn as possible. Second, Conley and Taber (2011) point out that clustering standard errors at the level of cities is likely to underestimate standard errors if there is only one treatment unit in a difference-in-differences regression.

To address both of these concerns, we also construct a synthetic control city for Bonn as proposed by Abadie and Gardeazabal (2003) and extended by Abadie, Diamond, and Hainmueller (2010). The main idea of the synthetic control method (SCM) is to acknowledge that a simple average of other cities may not be a good enough control, and instead to let the data speak and search for a weighted average of cities that best mimics the pre-treatment trend (predictors) in Bonn. More formally, let J be the number of available control cities ('donor pool'), where J is equal to 40 in our application. Let $W = (w_1, ..., w_J)'$ be a $(J \times 1)$ vector of nonnegative weights which sum to one. The scalar w_j represents the weight of city j in the synthetic Bonn. The synthetic control method chooses the weights W so that the synthetic Bonn most closely resembles the actual one before 1949.

To understand how the weights are determined, let X_1 be a $(K \times 1)$ vector of pre-1949 values of K employment predictors for Bonn. Similar to X_1, X_0 is defined as a $(K \times J)$ matrix which contains the values of the same employment predictors for the J possible control cities. We include in X_1 and X_0 both pre-treatment values of employment and exogenous covariates. In particular we include in all specifications the pre-treatment values of employment in the non-tradable sector, the tradable sector and the public sector.[20] These employment variables capture differences in sectoral employment patterns that may affect the post-treatment trend. As exogenous covariates that could influence post-war employment growth we include a dummy for proximity (75 kilometers) to the German-German border to control for market access changes as in Redding and Sturm (2008) and two measures of war-related destruction reported in Kästner (1949): the amount of rubble in cubic meters per capita and the percentage of the 1939 housing stock that has been destroyed.

Let V be a diagonal matrix with non-negative components. The values of the diagonal elements of V reflect the relative importance of the K employment predictors in the construction of the synthetic control. The vector of weights W is chosen to minimize the objective function:

$$(X_1 - X_0 W)' V (X_1 - X_0 W) \qquad \text{subject to} \quad w_j \geq 0. \tag{12}$$

The optimal weights W^* that minimises this objective function clearly depend on the choice of weighting matrix V. We follow Gobillon and Magnac (2016) and set V to be the identity matrix, which gives equal weights to all predictor variables. Alternatively, Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010) propose a data-driven procedure to choose V such that employment before 1949 is best reproduced by the synthetic control defined by $W^*(V)$, i.e. the RMSPE is minimised over the pre-treatment period. We have also estimated this alternative specification and find very similar results with this approach and our results are therefore not sensitive to the choice of V.[21]

The treatment effects α_{1t} for Bonn in the post-treatment years $t = 1950, 1961, 1970, 1987$ are then estimated as the difference between Bonn and the synthetic control city:

$$\hat{\alpha}_{1t} = Y_{1t} - Y_{0t} W^* \tag{13}$$

where Y_{1t} is the outcome for Bonn in year t and Y_{0t} is a (row) vector with the same outcome for the J control cities in year t. Since we use employment in the tradable, non-tradable and public sector as our predictor variables no matter whether the outcome variable is total employment or employment in one of our sub-sectors, the weights W^* do not change across outcomes. This is our preferred specification because it ensures that the synthetic control for Bonn does not vary across specifications and the estimated treatment effects for the different sub-sectors add up to the estimated treatment effect for total employment.[22]

[20] We discuss in Section 6 below how we define employment in the tradable and non-tradable sector.

[21] Kaul, Klößner, Pfeifer, and Schieler (2017) argue that using all outcome lags as separate predictors renders the other covariates irrelevant. This concern does not apply to our procedure since using the identity matrix for V assigns equal weights to all covariates.

[22] In unreported specifications we have also just used the pre-treatment values of the respective measure of employment

6 Basic Results

In this section, we use both difference-in-differences and the synthetic control method to establish a counterfactual for Bonn in the absence of the arrival of the federal government. We begin by looking at three outcomes: total employment, public sector employment and private sector employment. We then further disaggregate private employment into its sub-sectors to examine the heterogeneity of the impact of increases in public employment on different parts of the private sector.

6.1 The development of public, private and total employment

Figure 3 shows our basic results for the impact of the arrival of the federal government in Bonn on public employment, private employment and total employment. The three panels on the left hand side of the figure compare Bonn (solid line) to the average of the 40 control cities (dashed line) in a difference-in-differences comparison while the three panels of the right hand side compare Bonn to its synthetic control. The synthetic Bonn is a weighted average of the cities of Heidelberg (16.8%), Kiel (0.2%), Koblenz (34.6%), Mannheim (4.7%), Münster (8.1%), Stuttgart (7.0%), Wiesbaden (25.4%), and Wilhelmshaven (3.2%).[23] Table 1 compares the similarity between Bonn and both the unweighted average of the 40 control cities and the synthetic Bonn before the treatment. The table shows that across the predictor variables, i.e. the pre-treatment values of non-tradable employment, tradable employment and public employment as well as our exogenous growth predictors, Bonn is more similar to the synthetic Bonn than the average of the 40 control cities.

The closer fit between Bonn and its synthetic control compared to the average of the 40 control cities is also visible in Figure 3. Even though we use the same weights to compute the synthetic control for total, public and private employment in Bonn, we find that Bonn and its synthetic control track each other very closely both in terms of levels and growth rates before Bonn becomes the capital of West Germany in 1949. In contrast, Bonn and the average of the 40 control cities are not as close to each other, particularly for private sector and total employment. Bonn has less private sector employment than the average of the 40 control cities and the treatment and control group also have slightly different growth rates. The same difference between Bonn and the average of the 40 control cities is also visible in total employment in years prior to the arrival of the federal government.

Comparing Bonn to either the average of the 40 control cities or the synthetic Bonn after 1949 reveals a number of striking patterns. First, public employment increases rapidly in Bonn relative to either counterfactual.

as a predictor variables (along with our exogenous covariates). Doing so results in different weights W^* across specifications with different measures of employment as the dependent variable. Reassuringly, these alternative specifications lead to very similar results even though the treatment effects estimated for sub-sectors do not exactly add up to the estimated treatment effect for total employment.

[23] As noted before, we will use these weights for all subsequent sub-sector specifications such as, for example, public sector and private sector employment.

Comparing the change in public sector employment in Bonn between 1987 and the pre-treatment years to the same change in the synthetic control we find a treatment effect on public sector employment in Bonn of just over 15,600.[24] When we compare the change in public employment in Bonn to the change in public employment in the un-weighted average of the 40 control cities, we find a marginally larger treatment effect on public employment. Second, this rapid expansion of public employment is not mirrored in an equally rapid expansion of private employment in Bonn. Comparing the change in private sector employment in Bonn to the same change in the synthetic control we find a treatment effect on private sector employment in Bonn of just under 13,400 private sector workers. Comparing Bonn to the un-weighted average of the 40 control cities instead results in a larger treatment effect of just over 21,600 private sector workers, but this estimate seems less reliable due to the much poorer fit between Bonn and the average of the 40 control cities during the pre-treatment years in terms of private sector employment. Third, as total employment is the sum of public and private employment, total employment also rises rapidly in Bonn relative to either counterfactual. The relative increase in public employment in Bonn is reflected in the development of the percentage share of public employment in total employment in Bonn, which increases from an average of 12.1% in the pre-treatment years to 24.7% in 1987. In contrast, in the synthetic control the percentage share of public employment in total employment increases from an average of 11.9% in the pre-treatment years to 14.8% in 1987.

The results suggest that while the average of the 40 control cities and the synthetic control are two quite different counterfactuals, they produce fairly similar treatment effects of the arrival of the federal government in Bonn. The similarity of the results across these two very different counterfactuals is re-assuring as it suggests that the estimated treatment effects do not depend sensitively on the details how one constructs a counterfactual for Bonn. In what follows we concentrate on the results using the synthetic Bonn as the counterfactual, which ensures the closest possible fit between Bonn and its counterfactual prior to treatment, but the results would be qualitatively similar if we used the un-weighted average of the control cities as the counterfactual for Bonn instead.

To illustrate the magnitude of the effects that we find and to compare the results to the existing literature we convert our treatment effects into simple employment multipliers. Dividing the treatment effect of the arrival of the federal government in Bonn on private sector employment (using our synthetic control for Bonn as the counterfactual) by the analogous treatment effect on public sector employment we find that each additional public sector job in Bonn has created approximately 0.86 additional jobs in the private sector. This effect is substantially smaller than the results in Moretti (2010) who finds that an additional job in the tradable sector of a US city creates 1.6 additional jobs in the city's non-tradable sector, with even larger multiplier effects for additional skilled jobs. Our multiplier

[24] This treatment effect is computed as the change in public sector employment in Bonn between the average of the pre-treatment observations (1925, 1933, 1939 and 1946) and 1987 of 21,428 compared with the analogous change in the synthetic control for Bonn of 5,791 which results in a treatment effect of 21,428 - 5,791 = 15,637. Other treatment effects in this section are computed analogously.

is somewhat larger than the multiplier of 0.2 estimated by Faggio and Overman (2014) in their preferred specification, but their multipliers range from 0.07 to 0.86 across alternative specifications. There are a number of reasons why the employment spillovers from additional public employment could be substantially smaller than that of additional manufacturing employment. For example, input-output linkages between the manufacturing sector and other sectors may attract additional employment to a location while the public sector likely has much fewer input-output linkages. We return to this point in Section 7 when we examine the potential mechanisms that link changes in public employment to changes in private employment.

6.2 Heterogeneity across different parts of the private sector

The results so far show that public employment in Bonn has increased substantially after the arrival of the federal government in Bonn while the increase in private-sector employment has been more modest. In this section we use our data on the breakdown of overall employment into ten sectors, discussed in Section 4 above, to examine the heterogeneity of the changes in employment across sectors. The ten sub-sectors include agricultural employment, which is so small in all years of our data for Bonn and the 40 control cities that we omit this sector. This leaves nine sectors, one of which is public employment.

The simulations of our model in Figure 1 show that changes in public sector employment should have a differential impact on private employment in the tradable and non-tradable sector regardless of the strength of amenity or productivity spillovers. In particular, as discussed in more detail in Section 2.4, the impact of an increase in public employment should have a more positive effect on employment in the non-tradable sector compared to employment in the tradable sector. The intuition is that additional public employment creates additional demand for non-tradables, which can only be produced locally even if local housing costs and wages increase. In contrast higher rents and housing costs reduce employment in the local tradable sector, where the increase in local demand can also be met by firms from other cities.

In Figure 4 we show the development of Bonn and its synthetic control across all nine sectors without taking a stance on the tradability of the output of each of these sectors. The synthetic Bonn is computed using the same fixed weights as before which ensures that the treatment effects across the different sub-sectors add up to the treatment effect on total employment. The figure shows that for a number of the nine sectors there is hardly any visible treatment effect of the arrival of the federal government in Bonn.[25] However, there are also notable exceptions to this pattern. First, the final panel shows the large increase in public employment in Bonn relative to its synthetic control, which is just reproduced here from Figure 3. Second, the largest positive treatment effect on private sector employment is in the sector "other service employment". This sector contains a broad set of service

[25] We examine the statistical significance of the treatment effects across the different sub-sectors formally below.

employment categories including employment in restaurants, hotels and theaters, business services, personal services and hair dressers. Third, also employment in the non-profit sector experiences a clearly visible increase relative to the synthetic Bonn.

To link the reduced form evidence closer to the model Figure 5 aggregates private employment across the eight categories shown in Figure 4 into a tradable and non-tradable sector. The tradable sector is simply industry employment while non-tradable employment is the sum across employment in all service sectors (excluding public employment). While this classification is not perfect it should provide a first-order approximation to the tradability of different sectors. The figure shows that in line with the predictions of our theoretical model the arrival of the federal government in Bonn has had a positive impact on non-tradable employment while the impact on the tradable sector is much smaller and actually negative in our point estimate. Converted into employment multipliers, we find that each additional job in public employment reduces employment in the tradable sector by 0.19 jobs while it creates 1.05 additional jobs in the non-tradable sector.

Figure 6 investigates the statistical significance of the treatment effects across the different sectors shown in Figure 4. This graph combines 39 placebo estimates for the cities in our donor pool (grey lines) with the graph for Bonn (black line). Note that we plot 39 instead of 40 placebo graphs because the city with the largest employment (Stuttgart) cannot be reproduced with any combination of cities in our sample.[26] Each line in this figure shows the difference between a city and its synthetic control. In addition to the 39 placebo graphs, the shaded area encloses the region between the 5th and the 95th percentile of the distribution of placebo graphs. In terms of public employment, Bonn's post-1949 development is more pronounced than that of all other cities. This provides strong evidence for the uniqueness of Bonn's development. Bonn's treatment effect in terms of the non-profit sector employment growth is also clearly statistically significant. The increase in other service employment, which shows the quantitatively largest increase in private sector employment is only marginally statistically significant due to a set of more noisy placebo treatment effects. Finally the small decline in industry employment in Bonn relative to its synthetic control is entirely statistically insignificant with rather noisy placebo treatment effects.

In summary, in this section we have shown that comparing Bonn to its synthetic control over the period from 1925 to 1987 reveals that the arrival of the federal government in Bonn after 1949 has resulted in a substantial increase in public employment. At the same time the increase in private employment has been more modest and has been concentrated in the non-tradable sector, while industrial employment in Bonn has experienced a small but statistically insignificant decline. We now analyze the possible mechanisms suggested by our theoretical model that link the development of public and private sector employment in a location.

[26] This is a well-known limitation of placebo exercises, or rather the synthetic control method when the treated city (in the context of the placebo exercise this is Stuttgart) is at the top or bottom of the distribution.

7 Mechanisms

The previous section has shown that in the wake of the arrival of the federal government in Bonn, the city has experienced a dramatic expansion in total employment. However, this expansion was driven by a large expansion in public employment, while private sector employment has increased more modestly. In this section, we interpret this finding in the light of our theoretical model and provide a number of pieces of evidence for the mechanisms emphasised by the model. First, we provide some suggestive direct evidence for the amenity and productivity spillovers from public to private employment that the model postulates. Second, we show that proxies for long-term changes in wages are in line with the predictions of the model. Third, we provide evidence that house prices in Bonn responded in the way predicted by the model. Finally, we undertake a simple quantitative analysis of the model to see for which spillover parameters the model best fits the reduced form evidence.

7.1 Amenity Spillovers

The two key spillovers emphasised by the model are amenity and productivity spillovers from public to private sector employment. In this section we provide some suggestive evidence that there are likely to be substantial amenity spillovers from public employment in Bonn. Glaeser, Kolko, and Saiz (2001) discuss four consumption amenities in cities that they consider to be key to urban success. First, the size and diversity of local services and consumer goods including restaurants and live performance theatres. Second, aesthetics and the physical setting which involves the built environment as well as the natural environment. Third, the quality of public services including good schools and low crime rates. Finally, the speed of public transportation to facilitate commuting. While some of the these amenities such as the natural environment are difficult to change, most can be changed through local interventions.

Table 2 shows a set of simple cross-sectional regressions which compare Bonn in 1987 to the control cities for a number of proxies for local amenities in the spirit of Glaeser, Kolko, and Saiz (2001). The regressions control for a university town dummy, state capital dummy and the logarithm of 1987 population. Across many of these regressions Bonn has substantially higher values of amenities than the control group of cities. Column (1), for example, suggests that cultural spending by the city of Bonn is around 50% higher than in the control group of cities. Column (2) shows that total expenditure in Bonn is less than 10% higher than in the control group, so the substantially higher expenditure on culture implies a higher share of spending on cultural amenities in Bonn. Columns (3) to (7) shed more light on the cultural sector. Columns (3), (4) and (5) together suggest that Bonn has substantially larger expenses on theaters, which are not driven by a larger employment of actors, but by a higher wage bill. If higher wages of actors in Bonn reflected higher quality of actors, Bonn would stage better productions than comparable cities. Column (6), in turn, suggests that these high-quality productions are not attended by more visitors, though. It is also instructive to look at the number of artists more

broadly. While theatres are generally publicly funded, the number of artists comprises both those employed in public institutions and those who are self-employed or dependent employees in the private sector. A larger share of artists in Bonn in Column (7) suggests that Bonn offers a richer (or more high-quality) cultural environment not only in the public sector, but overall. Going beyond the cultural sector, Columns (8) and (9) look at medical provision. Bonn has a larger number of doctors and a larger number of hospital beds than the control cities. Finally, turning to schooling quality, as proxied by class size (student-teacher ratios), Bonn does not seem to stand out in this dimension as we find no statistically significant difference between Bonn and the control cities. While these regressions are simple cross-sectional regressions, they provide suggestive evidence for Bonn offering a substantially different set of amenities than other German cites of a similar size.

It is unfortunately not possible to compile comparable pre-war data for the same set of amenity proxies that we observe in 1987 and we therefore cannot compare the change in these proxies in Bonn to the change in the synthetic control for Bonn. We have, however, been able to find data for 1929/1930 for a smaller number of similar proxies for amenities before the Second World War. This data is shown in Figure 7 which scatters the pre-war data for the number of classrooms in primary schools, the student teacher ratio, the number of theater seats and the number of theater shows against the 1939 population of each city in our sample together with a linear regression. The figure shows that across all of these proxies for pre-war amenities Bonn was if anything marginally below the average of the control cities. In particular it has fewer classrooms and somewhat larger class sizes than the average city in our sample conditional on population. The number of theatre seats and theatre shows lies almost exactly on the regression line. This evidence suggests that Bonn did not have better amenities before the Second World War, consistent with the idea that the higher values of the amenities proxies that we observe in Bonn in 1987 are indeed caused by the arrival of the federal government in Bonn and are not due to a pre-existing difference between Bonn and the control cities.

7.2 Productivity Spillovers

We will now turn to potential productivity spillovers from increased public employment in Bonn. Ideally, this exercise would involve firm-level productivity estimations for firms in Bonn and the control group. Unfortunately, such data are not available for the time under investigation, neither from national statistics nor from AMADEUS (a European subset of the ORBIS firm database by Bureau van Dijk).[27] In the absence of data that would allow us to estimate firm-level productivity regressions, we present regressions of city-level gross value added in the industry sector and the trade and transportation sector (Gemeinschaftsveröffentlichung der Statistischen Landesämter 1991) on controls for

[27] To the best of our knowledge, there is only one study by Bernard and Wagner (1997) that uses plant-level data in this time. However, the study is restricted to one federal state, Lower Saxony, and comparable data for other states are not available before 1995. After that, the research data center of the federal states' statistical offices provides access to some official firm data (see Malchin and Voshage 2009).

university town status, state capital status, log population and a dummy variable for Bonn in 1988.

The regressions in Table 3 show a clear picture. There is no evidence that gross value added per worker in Bonn is higher than in the control group. Instead, we find negative and statistically significant differences between gross value added per worker in Bonn and the control group of cities in the industry sector and an insignificant effect that is close to zero for the trade and transportation sector. With the data limitations in mind, Columns (1) to (3) suggest that industry gross value added in Bonn is between 27-39 percent lower than in the control group. For the trade and transportation sector, the unconditional specification in Column (4) suggests a 17 percent higher gross value added in Bonn than in the control group but the size of this effect drops to close to zero and becomes statistically insignificant once we add controls. Finally, when we combine both sectors in Columns (7) to (9), the estimated difference in gross value added suggests a 12-25 percent lower value in Bonn than in the control group. Given the crude data and the lack of information about the cities' productivity before the Second World War, we do not attempt to interpret the magnitude of the effects. Instead, we cautiously interpret these regressions as suggestive evidence that public employment is unlikely to produce productive spillovers that benefit private sector firms.

7.3 Nominal Wages

To investigate how the large increase in employment affected nominal wages in Bonn, we use a 5% random sub-sample of wages from the Historic Employment and Establishment Statistics (HES) database for 1987 and estimate whether wages in Bonn are higher compared to the control cities. The HES data report workers' daily wages, their industry and occupation, and socio-demographic characteristics such as educational attainment, gender, age, and place of work. The wage information is very reliable, since it is used to determine social insurance contributions. One drawback of the data is that wages are censored due to the limit for compulsory social insurance payments.[28] To control for this, we include a dummy variable for observations where wages are top-coded. Alternatively, we have also imputed the truncated wages following Gartner (2005). The data exclude civil servants and self-employed individuals which are not covered by the social security system.[29]

Table 4 shows the results of the wage regressions. In Column (1), we present a bivariate regression showing that wages are, on average, 2% lower in Bonn than in the set of 40 control cities. In Columns (2) to (4), we add a number of control variables. In Column (2), we add experience and experience squared, education, gender and a foreigner dummy. Additionally, we include interactions between experience, experience squared, gender, education and nationality as suggested by Beaudry, Green, and Sand (2012). In Column (3), we add dummies for 88 two-digit industries. In Column (4) we add a control for the logarithm of income tax revenue per capita for 1937 in each city in the sample. This

[28] This affects only 9.6 percent of the observations.

[29] We also drop from the sample workers younger than 18 or older than 65 and we also exclude all individuals in training and in part-time employment since there is no information on hours worked in the HES.

variable controls for potential pre-existing differences in income before Bonn became capital. One can think of this regression as a quasi-difference-in-differences regression. In Column (4) the Bonn dummy is positive and statistically significant suggesting that wages in Bonn were about 2.4% higher than in the control cities.[30]

The remaining columns of the table consider several further robustness checks. In Column (5) we impute the truncated wages following Gartner (2005), which has minimal impact on the results. Finally, in Column (6) we add a control for log population. In line with the existing literature the coefficient estimate for total population suggests that larger cities pay somewhat higher nominal wages. The Bonn dummy becomes slightly smaller but remains statistically significant.[31] Taken together, these results suggest that there has been a modest increase in nominal wages in Bonn relative to the control group of cities.

7.4 House Prices

We now investigate to what extent house prices have likely responded to a change in employment and hence population in Bonn. Recent work by Combes, Duranton, and Gobillon (2016) exploits micro data on individual land transactions in French cities to isolate the causal effect of an increase in population on the price of land and through this on the price of housing. They show that in line with the predictions of standard economic geography models of the kind developed in this paper, an inflow of population increases housing prices in a city. Saiz (2010) provides similar estimates for US metropolitan areas.

Comparable micro data on house or land prices is currently unfortunately not available for Germany as information on real estate transactions is held locally and not reported to a national database. However, we have been able to obtain data from the Association of Real Estate Agents that reports the average price of transactions across four different types of real estate for a number of German cities. Specifically, the data report the average rental price of new-build flats along with the average purchase price of new-build flats, new-build semi-detached houses and new-build detached houses. This data is far from perfect as it covers a subset of German cities that only partially overlaps with the cities in our sample, only reports average prices and is unlikely to capture the universe of transactions.

Figure 8 correlates the logarithm of this data against the logarithm of population in 1987 for each city for which the Association of Real Estate Agents reports data (which is a somewhat different sample of cities than our control cities) and also shows a linear regression. The figure shows that there is a clear positive correlation between the different measures of house prices and the logarithm of population. Bonn is close to the regression line for most measures of housing prices reported by the RDM. This suggests that also in Germany there is likely to be a causal relationship between city size and house

[30] Using income tax revenue per capita for 1928 yields very similar results.

[31] Note that the population of Bonn is by construction of the sample close to the average population of the cities in the sample and the Bonn dummy should therefore not be substantially correlated with the population control variable.

prices and that Bonn is no exception to this relationship. Taken together this provides suggestive evidence that the dispersion mechanism of higher real estate prices emphasised by our model seems to be operating also in Germany.

7.5 Quantitative Analysis of the Model

We now undertake a simple quantitative analysis of the theoretical model to see whether the model can match the key features of our reduced from results. In particular we are going to search for values of the amenity and productivity spillover (δ and α respectively) and the share of total public employment located in Bonn for which the model can best match the reduced form evidence. In doing so, we maintain other parameters of the model, such as the elasticity of substitution, at central values from the existing literature. The simulations in Figure 1 show that conditional on the other parameters of the model, the strength of amenity and productivity spillovers and the share of total public employment located in each of the two cities in the model uniquely determine the distribution of public and private employment in our model. Intuitively, the distribution of public employment produces spillovers to the private sector in the model and the strength of these spillovers depends on the values of δ and α.

These three parameters are identified using three moments in the data. The first moment is the increase in the share of public employment in total employment in Bonn relative to the synthetic control city. As discussed in Section 6.1, the share of public employment in Bonn increases by approximately 12.6 percentage points between the pre-treatment years and 1987 while the same change in the synthetic control is just under 3 percentage points resulting in a treatment effect of 9.6 percentage points, which we target in the quantitative analysis. The second moment is the increase in private employment in Bonn relative to its synthetic control. As also discussed in 6.1 this treatment effect on private employment in Bonn is just under 13,400 workers. For the quantitative analysis we convert this treatment effect into a percentage change to abstract from changes in the overall level of employment in West Germany between the pre-treatment years and 1987. The increase in private employment in Bonn between the pre-treatment years and 1987 is 63.9% while the same change in the synthetic control is 38.5%, resulting in a treatment effect of 63.9% - 38.5% = 25.4%. We target this 25.4 percentage point difference in the model. The third moment is the difference in wages between Bonn and the control cities estimated in Section 7.3 above. The results in Column (4) of Table 4 show that wages in Bonn are 2.4% higher than in the control cities after controlling for observable worker characteristics and also 1937 differences in income tax per capita receipts across cities. We therefore target a wage difference of 2.4% in the quantitative analysis.

The intuition how these moments are able to identify the three parameters is as follows. The change in private employment in Bonn helps to identify the magnitude of a combination of amenity and productivity spillovers. If both of these spillovers are zero, then private employment in Bonn declines in the model due its negative impact on house prices and the price index for tradables. Similarly, a

high level of these spillovers would make the city with more public employment also more attractive for private employment resulting in an increase in private employment. Second, the difference in the share in public employment in total employment between Bonn and the synthetic control city pins down the share of total public employment that is located in Bonn, i.e the inequality in the distribution of public employment across cities. Third, the difference in wages across Bonn and the control cities identifies the relative strength of amenity and productivity spillovers. While both higher productivity and amenity spillovers from public employment increase wages and house prices in the city with more public employment, productivity spillovers have a comparatively stronger effect on wages than amenity spillovers.

To determine the values of these three parameters we perform a grid-search over values for α and δ between -0.08 and 0.08 in steps of 0.002 and the share of public employment in Bonn between 0.7 and 0.8 in steps of 0.01 which involves evaluating 72,171 parameter combinations.[32] Table 5 shows the ten best fit values of amenity and productivity spillovers and the share of total public employment located in each of the two cities in the model. For the best fit parameters the estimated productivity spillover of public employment is negative but close to zero, while the estimated amenity spillover of public employment is positive. The point estimates imply that that doubling public employment in a city reduces productivity in the private sector by about 0.4% while amenities in the city increase by about 1.8%. The parameter estimates are fairly similar across the 10 best fit parameter values shown in Table 5 which suggests that the objective function defined by the three moments is well-behaved and has a unique global minimum. To further examine how well the moments identify the magnitude of the productivity and amenity spillover Figure 9 shows the objective function for different values of amenity and productivity spillovers holding the share of public employment constant at the value that minimises the objective function. The graph shows a clearly defined valley around the best fit parameter combination for the amenity and productivity spillover. The shape of the objective function shows that the sum of amenity and productivity spillovers are tightly identified. Distinguishing between the relative strength of amenity and productivity spillovers is harder but the objective function also has a clearly visible curvature in this dimension.

Overall, the results of the quantitative analysis are consistent with and complement the reduced form evidence on the mechanisms. Both the quantitative analysis and the reduced from evidence suggest that the increase in public employment has increased amenities in Bonn but has at best left productivity in the private sector unchanged.

[32] We first evaluated a coarse grid with values of α and δ between -0.08 and 0.08 and public employment shares between 0.5 and 0.9 to confirm that the global minimum of the objective function must involve public employment shares between 0.7 and 0.8.

8 Conclusion

This paper has used the relocation of the German government from Berlin to Bonn in the wake of the Second World War as a natural experiment to provide evidence for the effects of public employment on private sector employment. Comparing Bonn to a control group of ex-ante similar cities and a synthetic control city, we find that the substantial increase in public employment in Bonn has only resulted in a more modest increase in private sector employment. Expressed as employment multipliers, the estimated treatment effects suggest that each additional public sector job reduces employment in industry by around 0.2 jobs while it creates just over one additional job in other parts of the private sector. We interpret our results in the context of a simple theoretical model which allows for productivity and amenity spillovers from public to private employment. Reduced form evidence suggests that the mechanism behind our results is an increase in endogenous consumption amenities in Bonn while productivity in the private sector has at best been unchanged. A simple quantitative analysis of our theoretical model finds complementary results. While the best fit parameter for the estimated productivity spillover of public employment is negative and close to zero, the best fit parameter for the estimated amenity spillover of public employment is positive and much larger.

More broadly our results contribute to the debate whether relocations of public employment are a viable policy instrument to support economically lagging regions. As our natural experiment involves the relocation of the bureaucracy of a federal government our estimated effects should be upper bounds for the positive spillover effects that additional public sector employment can generate in the private sector. Some of what we capture as an amenity spillover could, for example, be driven by political economy forces that diverted additional government expenditure on cultural amenities to Bonn. Similarly, associations and other organisation that try to influence policy are likely attracted by this type of public employment. This implies that relocations of more mundane types of public employment to lagging regions could have even smaller spillover effects on private sector activity in the targeted regions. A full cost-benefit analysis of such policies would clearly also need to take other parameters into account apart from the spillover effects of public employment on private employment.

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Table 1: Balancing Tests

	(1)	(2)	(3)	(4)	(5)
		DiD		Synthetic Control	
	Bonn	Control	B-C	Control	B-C
Non-tradable empl. in 1925 (in 1000s)	30.910	35.885	-4.975	29.194	1.716
Non-tradable empl. in 1933 (in 1000s)	33.854	40.578	-6.724	35.617	-1.763
Non-tradable empl. in 1939 (in 1000s)	36.424	43.978	-7.554	39.645	-3.221
Non-tradable empl. in 1946 (in 1000s)	30.111	33.317	-3.206	28.618	1.493
Tradable empl. in 1925 (in 1000s)	20.502	30.084	-9.582	18.965	1.537
Tradable empl. in 1933 (in 1000s)	20.375	30.287	-9.912	20.529	-0.154
Tradable empl. in 1939 (in 1000s)	21.927	34.298	-12.371	22.055	-0.128
Tradable empl. in 1946 (in 1000s)	20.621	30.680	-10.059	22.321	-1.700
Public empl. in 1925 (in 1000s)	4.840	5.407	-0.567	5.509	-0.669
Public empl. in 1933 (in 1000s)	7.000	6.060	0.940	6.165	0.835
Public empl. in 1939 (in 1000s)	6.932	6.588	0.344	7.013	-0.081
Public empl. in 1946 (in 1000s)	10.929	7.361	3.568	10.523	0.406
Border Dummy (within 75km)	0.000	0.125	-0.125	0.002	-0.002
Rubble (in cubic meters per capita)	7.300	12.240	-4.940	7.611	-0.311
Destroyed Housing (as % of 1939 stock)	37.300	39.950	-2.650	36.388	0.912

Notes: The table shows the balance of a set of predictor variables between Bonn and (i) the unweighted average of the 40 control cities (columns 2 and 3) and (ii) the synthetic control group (columns 4 and 5). These predictor varibales are used to determine the synthetic control group and they include the level of non-tradable employment, tradeable employment and public employment in all four pre-treatment years, a dummy for locations in proximity (within 75 kilometers) to the German-German border and two measures of Second World War destruction (rubble and destroyed housing stock).

Table 2: Proxies for Amenity Spillovers

	(1) Total Cultural Expenditure	(2) Total City Expenditure	(3) Total Budget Theaters	(4) Employment Actors	(5) Wage Bill Actors
Bonn	0.553***	0.086*	0.665***	-0.071	0.807***
	(0.085)	(0.044)	(0.087)	(0.118)	(0.109)
log (Population)	1.048***	1.006***	0.732***	0.487**	0.612***
	(0.115)	(0.094)	(0.136)	(0.220)	(0.180)
University Town	0.560***	0.154	0.330**	0.144	0.392**
	(0.117)	(0.095)	(0.127)	(0.195)	(0.162)
State Capital	0.005	0.102	0.188	0.116	0.225
	(0.094)	(0.157)	(0.178)	(0.181)	(0.185)
Observations	41	39	33	33	32
R-squared	0.793	0.800	0.676	0.310	0.588

	(6) Theater Visitors	(7) Artists per 1,000 Empl	(8) Number of Doctors	(9) Hospital Beds	(10) Class Size
Bonn	-0.026	0.235***	0.233***	0.281***	0.022
	(0.096)	(0.082)	(0.061)	(0.048)	(0.023)
log (Population)	0.536***	1.008***	0.735***	0.526***	0.005
	(0.169)	(0.172)	(0.075)	(0.086)	(0.035)
University Town	0.302*	0.863***	0.394***	0.440***	-0.094***
	(0.160)	(0.205)	(0.080)	(0.086)	(0.027)
State Capital	0.182	0.240	0.222	0.081	0.073
	(0.163)	(0.238)	(0.134)	(0.094)	(0.047)
Observations	33	41	41	41	41
R-squared	0.491	0.698	0.836	0.761	0.238

Notes: All columns report OLS regressions of the logarithm of a city specific amenity measure on a dummy for Bonn and controls for log population, a university town dummy and a state capital dummy. Differences in the number of observations result from missing observations for the city state Bremen with its two cities Bremen and Bremerhaven (Column 2), missing information for theater outcomes in Solingen, Remscheid, Recklinghausen, Offenbach, Mülheim, Ludwigshafen, Herne and Bottrop (Columns 2, 4, 6), and missing wage information for artists in Heidelberg (Column 5). Artists in Column (7) include musicians, performing artists and graphical artists. Robust standard errors reported in parenthesis. *** $p<0.01$, ** $p<0.05$, * $p<0.10$

Table 3: Gross Value Added per Worker Across Cities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Industry Gross Value Added, 1988			Trade/Transportation Gross Value Added, 1988			Industry & Trade/Transport. Gross Value Added, 1988		
Bonn	-0.272***	-0.394***	-0.374***	0.168***	-0.024	0.013	-0.117**	-0.251***	-0.231***
	(0.062)	(0.077)	(0.082)	(0.052)	(0.050)	(0.052)	(0.056)	(0.060)	(0.069)
University Town		0.313**	0.330**		0.489***	0.518***		0.343***	0.359***
		(0.125)	(0.133)		(0.073)	(0.072)		(0.108)	(0.110)
State Capital		0.028	0.060		0.029	0.088		0.021	0.054
		(0.108)	(0.127)		(0.069)	(0.097)		(0.085)	(0.105)
log (Population)			-0.067			-0.122			-0.068
			(0.135)			(0.084)			(0.099)
Observations	41	41	41	41	41	41	41	41	41
R-squared	0.007	0.572	0.597	0.012	0.173	0.178	0.003	0.246	0.252

Notes: All columns report OLS regressions of the logarithm of city specific gross value added per worker at market prices on a dummy for Bonn, a university town dummy, a state capital dummy, and the logarithm of population. Robust standard errors reported in parenthesis. *** p<0.01, ** p<0.05, * p<0.10

Table 4: Wages

	(1)	(2)	(3)	(4)	(5)	(6)
	Baseline				Imputed Wage	Pop Control
	log (wage)	log (wage)	log (wage)	log (wage)	log (wage)	log (wage)
Bonn	-0.020**	-0.003	0.009	0.024***	0.025***	0.017***
	(0.008)	(0.009)	(0.006)	(0.005)	(0.005)	(0.004)
Experience		0.036***	0.032***	0.032***	0.032***	0.032***
		(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Expericence squared		-0.001***	-0.001***	-0.001***	-0.001***	-0.001***
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Female		-0.179***	-0.152***	-0.152***	-0.140***	-0.152***
		(0.014)	(0.009)	(0.009)	(0.009)	(0.009)
Foreigner		-0.040***	-0.052***	-0.048***	-0.050***	-0.048***
		(0.013)	(0.011)	(0.009)	(0.009)	(0.009)
log Income Tax pc 1937				0.038**	0.039**	0.025**
				(0.015)	(0.015)	(0.011)
log (Population)						0.018*
						(0.009)
Education Dummies	No	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	No	No	Yes	Yes	Yes	Yes
Observations	166,282	157,161	157,107	157,107	157,107	157,107
R-squared	0.198	0.437	0.533	0.535	0.606	0.535

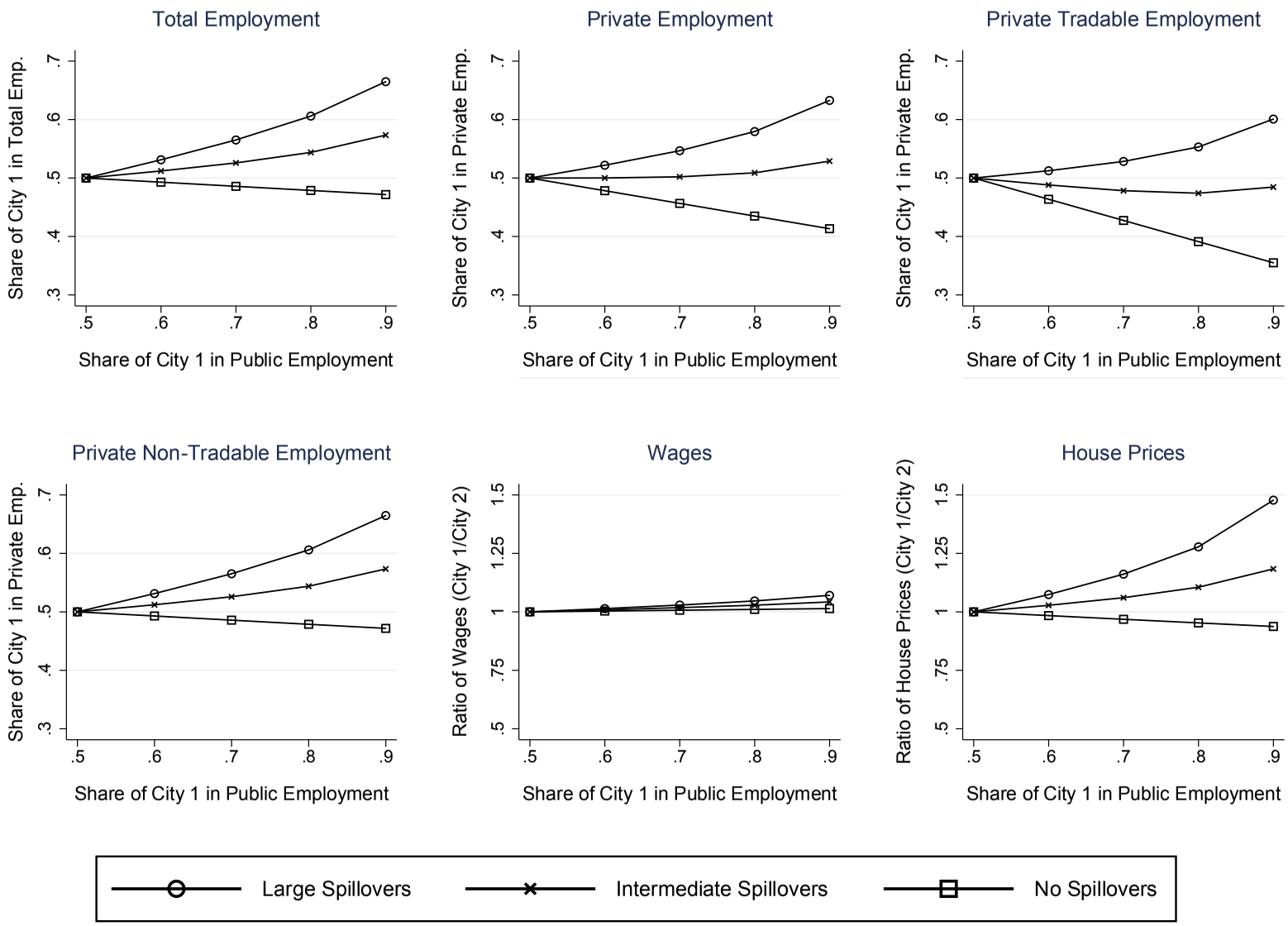
Notes: Column (1) captures the raw wage difference between Bonn and the control cities. In Column (2) we control for various observable worker characteristics including dummies for three education levels and interactions between these variables (which are not shown). Column (3) also controls for 88 industry fixed effects. Column (4) controls for pre-war income tax receipts. In Column (5) we do not use a dummy for top coded income observations but impute these instead. in Column (6) we also control for the logarithm of population. See the main text for more detail. Standard Errors are clustered at the level of each city. *** $p<0.01$, ** $p<0.05$, * $p<0.10$

Table 5: Best Fit Parameters Quantitative Analysis

(1) Productivity Spillover (α)	(2) Amenity Spillover (δ)	(3) Share of Total Public Empl. in City 1	(4) Treatm. Eff. Private Empl. (in p.p.)	(5) Treatm. Eff. Public Empl. (in p.p.)	(6) Wage Difference (in %)	(7) Sum of Squared Differences
-0.004	0.018	0.780	25.377	9.450	2.792	0.188
-0.006	0.020	0.780	25.914	9.399	2.597	0.408
-0.008	0.022	0.770	25.114	9.030	2.305	0.431
-0.010	0.024	0.770	25.627	8.982	2.119	0.580
-0.002	0.016	0.780	24.841	9.500	2.988	0.626
-0.006	0.020	0.770	24.600	9.079	2.491	0.883
0.000	0.014	0.790	25.609	9.873	3.320	0.969
-0.002	0.016	0.790	26.168	9.821	3.114	1.210
-0.008	0.022	0.780	26.448	9.350	2.401	1.279
-0.012	0.026	0.770	26.139	8.933	1.934	1.326

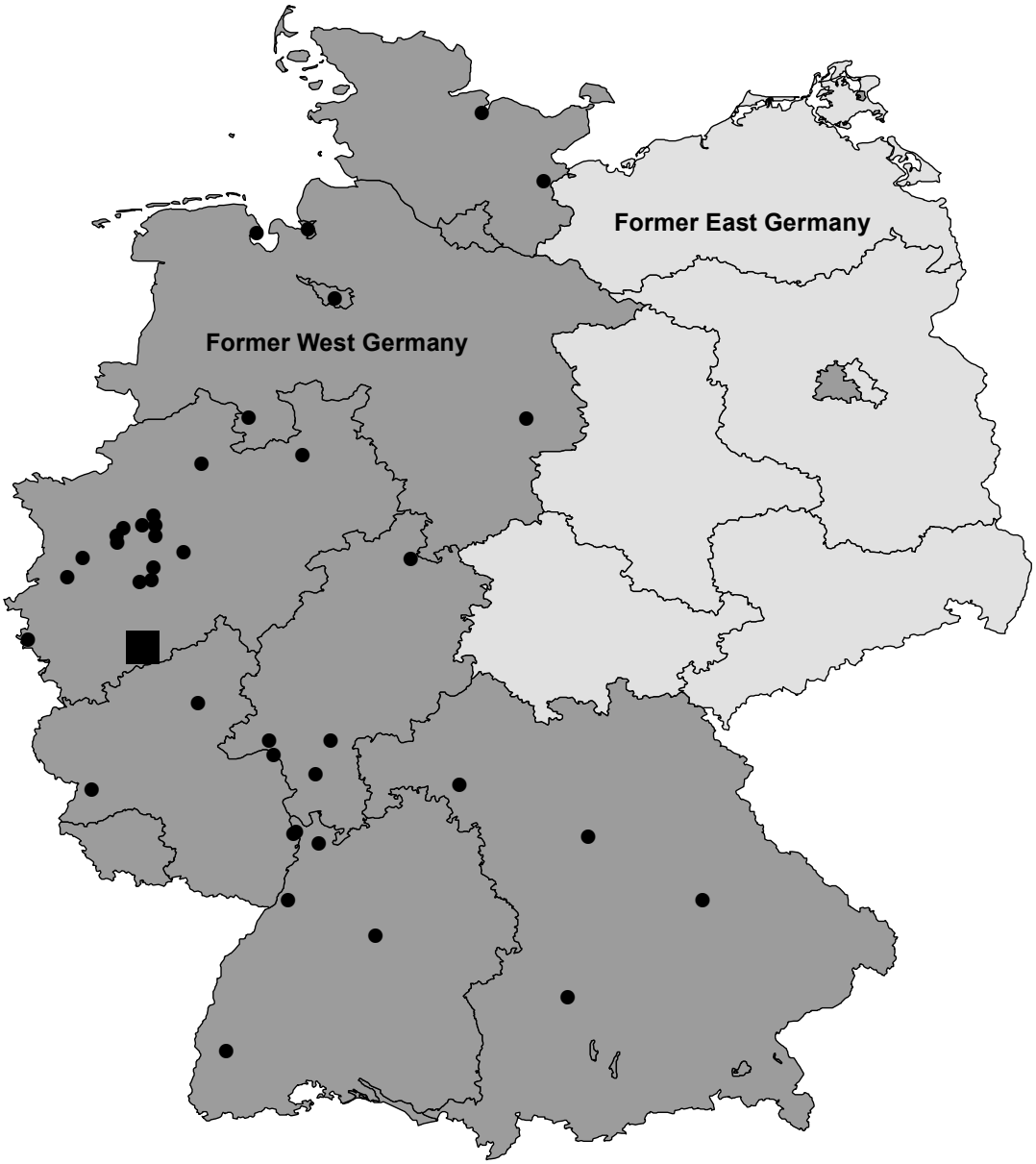
Notes: The table shows the 10 best fit parameter combinations for the productivity spillover, amenity spillover and share of public employment in city 1 and the implied values of the three moments in the model. The final column shows the value of the objective function. The empirical moments that we target are 25.4 percentage points in Column (4), 9.6 percentage points in Column (5) and 2.4 percentage points in Column (6). See Section 7.5 in the main text for more detail.

Figure 1: Simulating the Impact of Public Employment



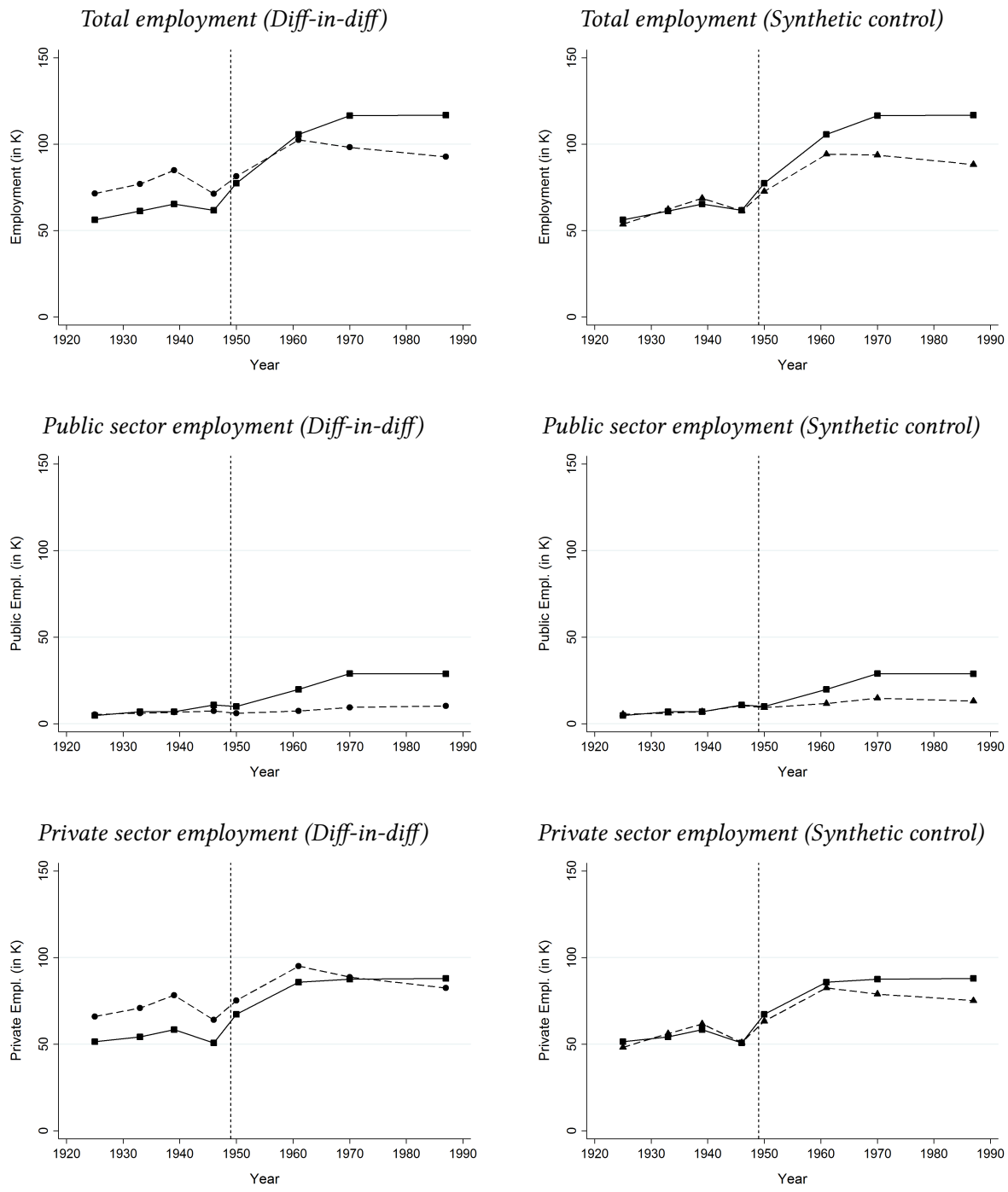
Notes: The graphs show a simulation of the model for two ex-ante symmetric cities. In the simulations we show three scenarios. (i) no spillovers; (ii) intermediate spillovers with $\alpha = \delta = 0.004$ and (iii) higher spillovers with $\alpha = \delta = 0.008$.

Figure 2: Bonn and the Control Cities



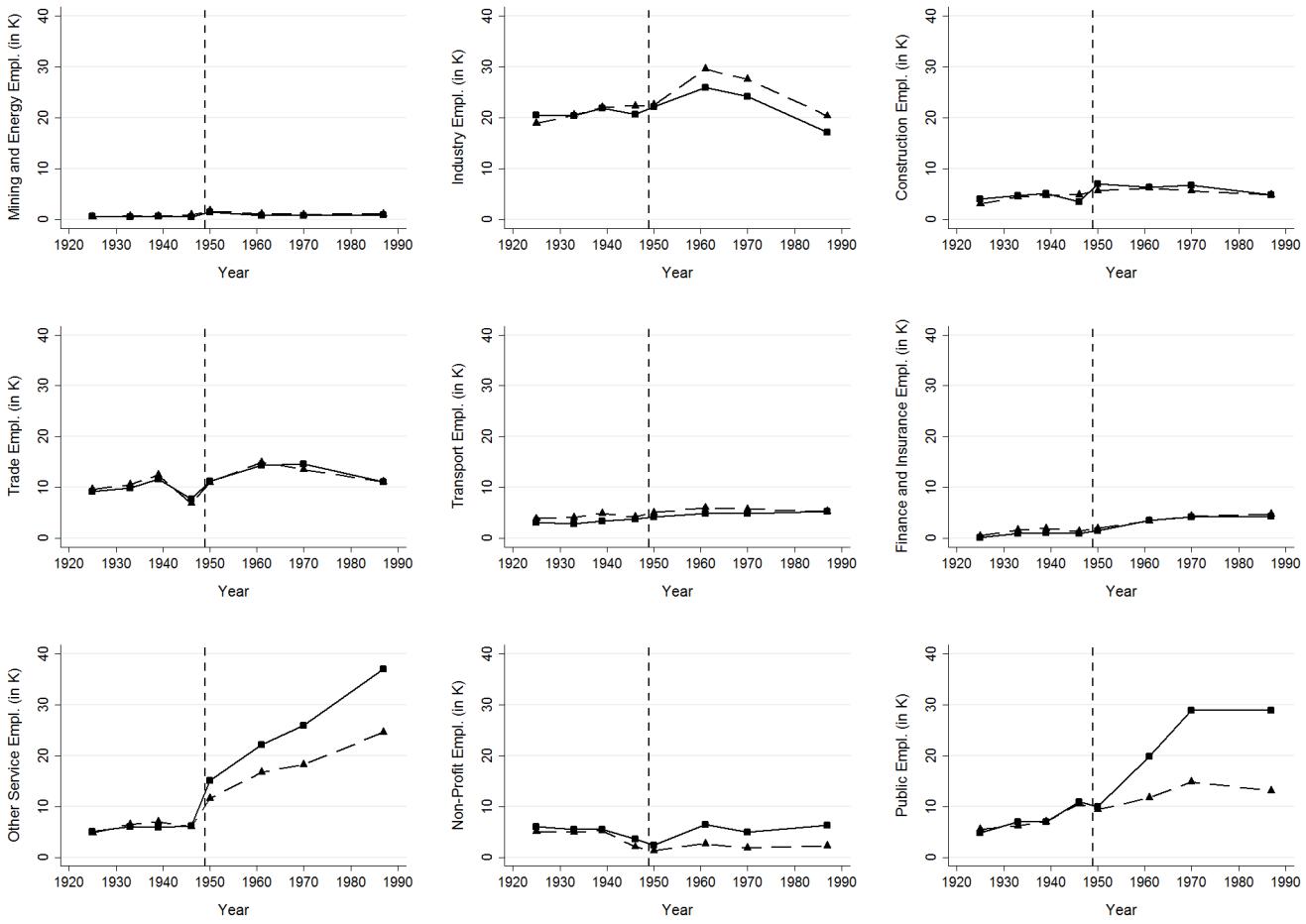
Note: The map shows the location of the city of Bonn (square) and the 40 control cities that comprise the control group in the difference-in-differences comparisons and the donor pool for the synthetic control approach.

Figure 3: Total Employment and Private- and Public-Sector Employment: Bonn vs Control



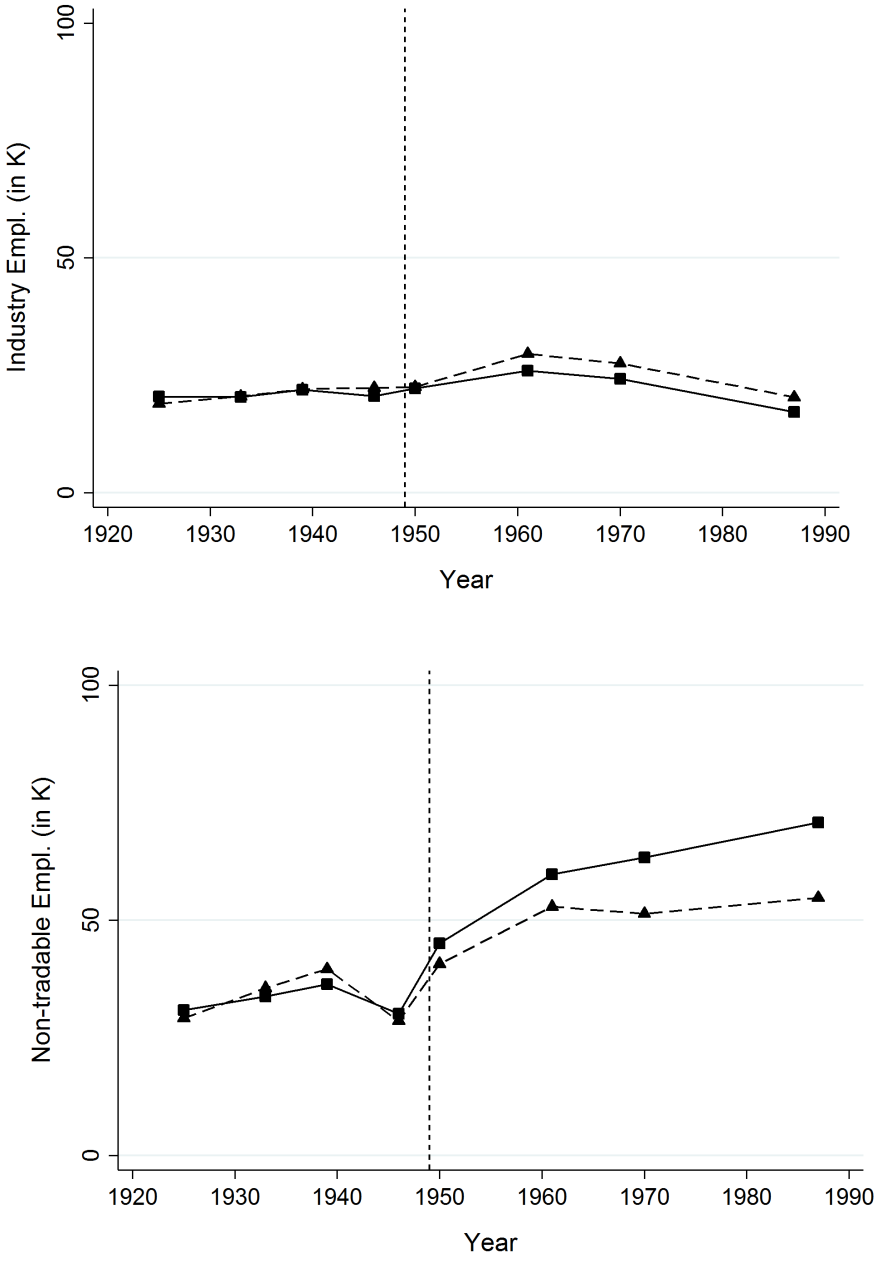
Note: The graphs shows employment in Bonn (solid line), compared to the average for the 40 control cities (dotted line with circles) in thousands in the left-hand panel and compared to a synthetic control city (dotted line with triangles) on the right hand side. The upper graphs are for total employment, the middle graphs for private-sector employment and the bottom graphs are for public sector employment. In constructing Bonn's synthetic control we use the same weights across different specifications which are: Heidelberg (16.8%), Kiel (0.2%), Koblenz (34.6%), Mannheim (4.7%), Münster (8.1%), Stuttgart (7.0%), Wiesbaden (25.4%), and Wilhelmshaven (3.2%). See main text for details.

Figure 4: Employment in Nine Sectors: Bonn vs Control



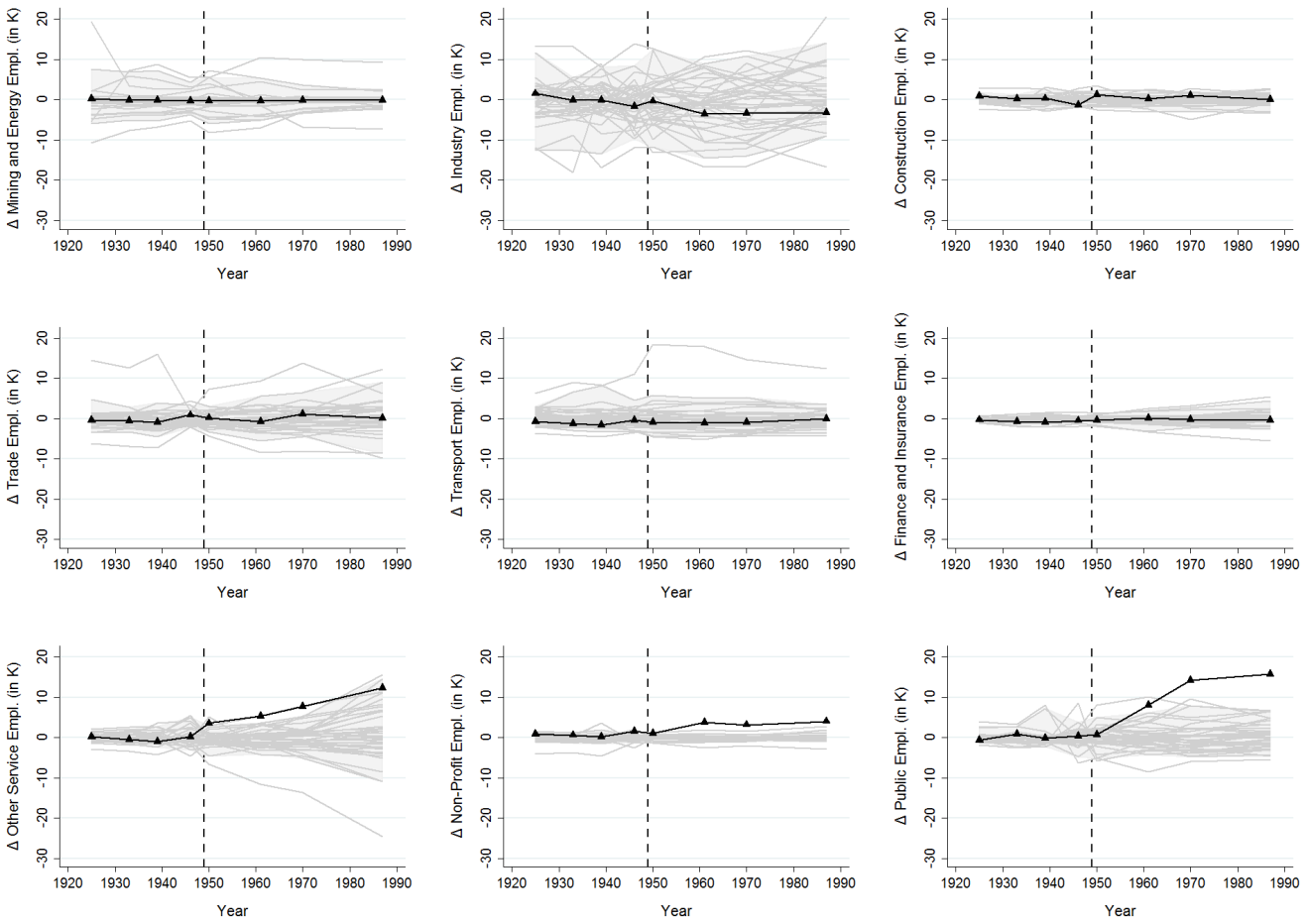
Note: The graphs show employment in Bonn (solid line), compared to the synthetic Bonn (dotted line with triangles) for nine sectors: mining; industry; construction; wholesale trade; finance and insurance; transportation; services; the non-profit sector; and the public sector. We do not display employment in agriculture as it is very small in the cities in our sample.

Figure 5: Private sector employment: tradables vs non-tradables



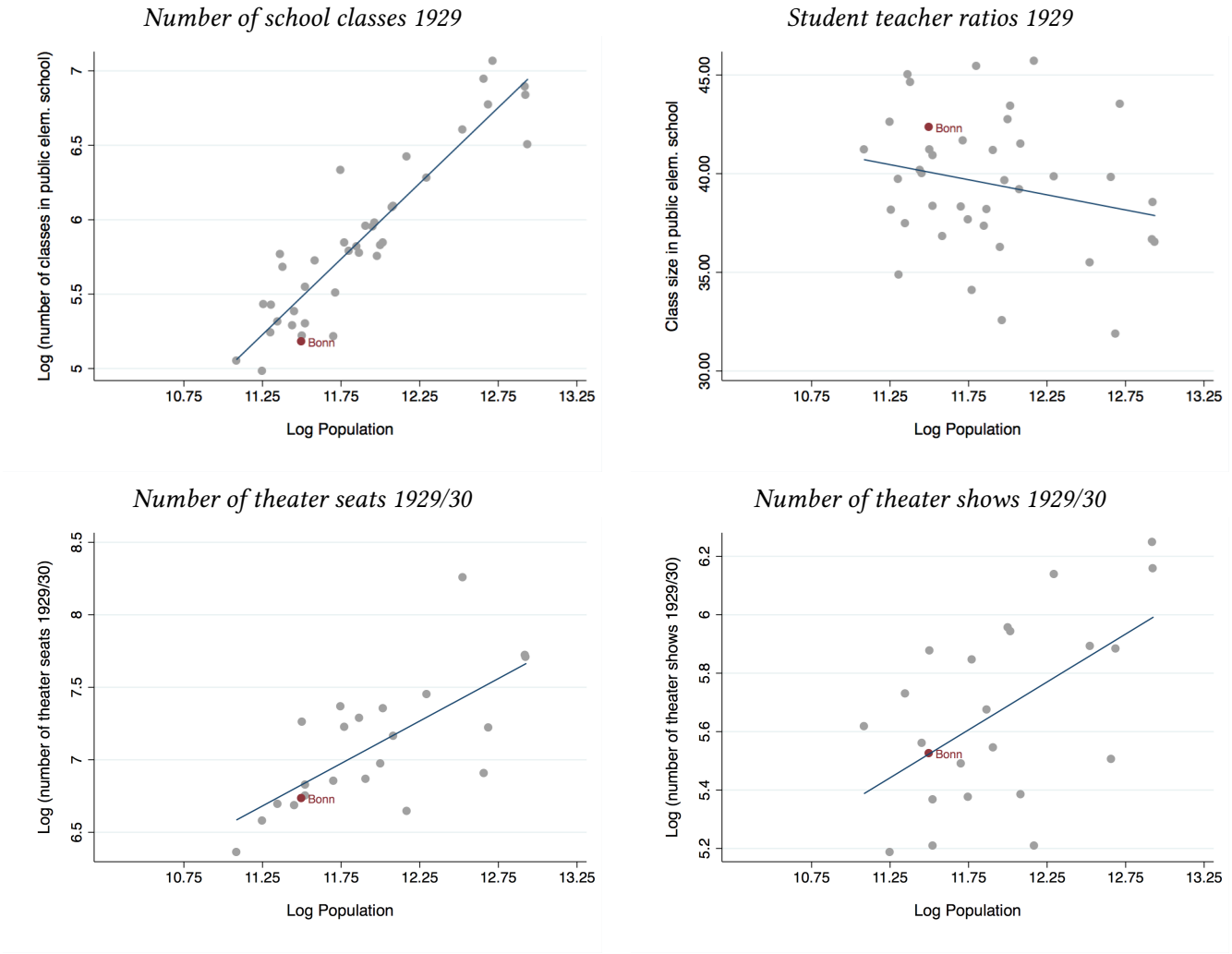
Note: The graph shows the development of tradable and non-tradable employment in Bonn (bold line) and its synthetic control (dotted line). Tradable employment is employment in industry, while non-tradable employment is the sum of all service sector employment (excluding the public sector).

Figure 6: Synthetic Placebos



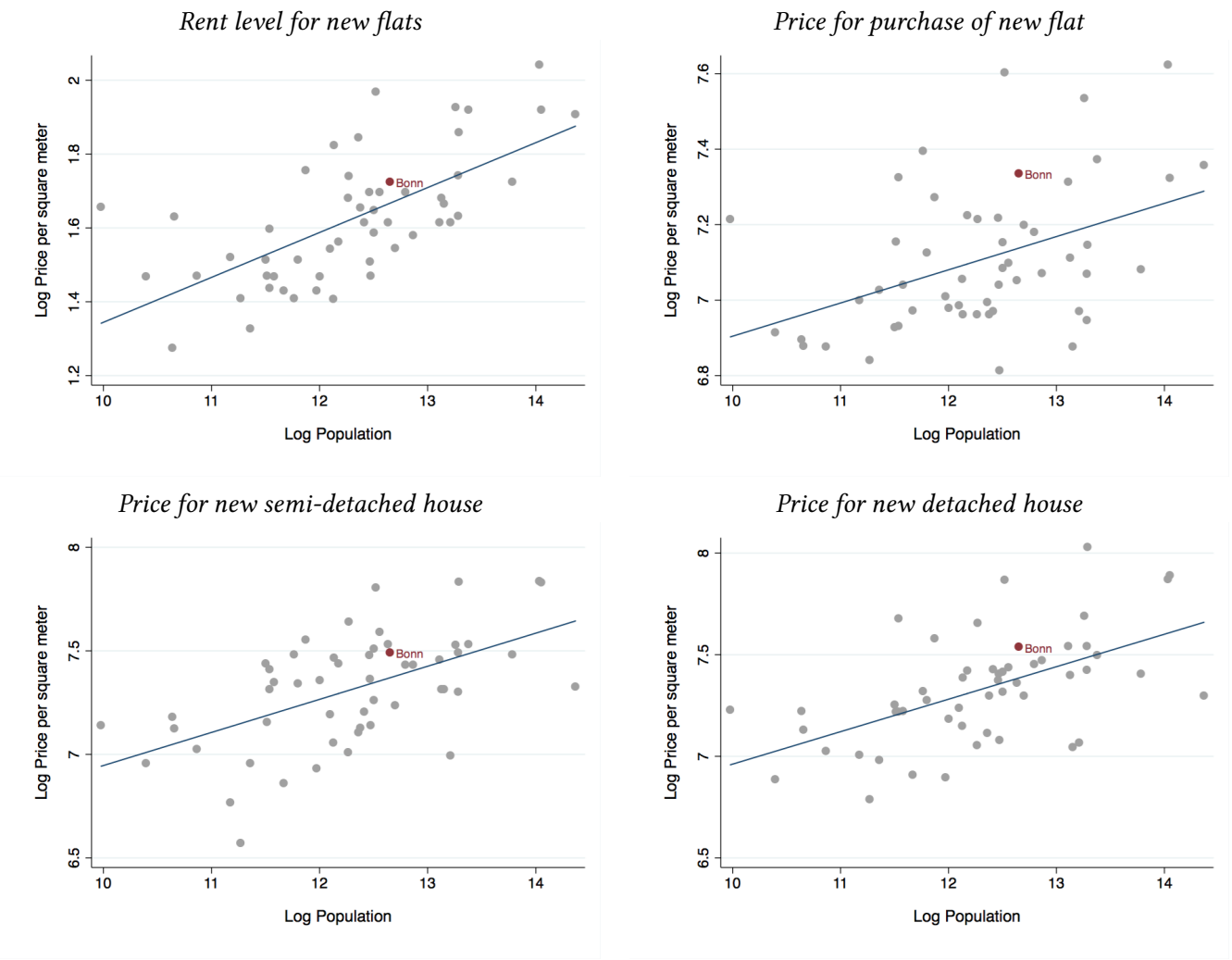
Note: The graphs shows placebo treatment effects for 39 of our control cities (excluding Stuttgart as discussed in the main text) and also the estimated treatment effect for Bonn (bold line). The placebo treatments are estimated by constructing a synthetic control city for each of the control cities in the donor pool. There are nine placebo graphs corresponding to the following nine sectors: mining; industry; construction; wholesale trade; finance and insurance; transportation; services; the non-profit sector; and the public sector. We do not display agriculture which is very small as discussed in the main text.

Figure 7: Amenities before the Second World War



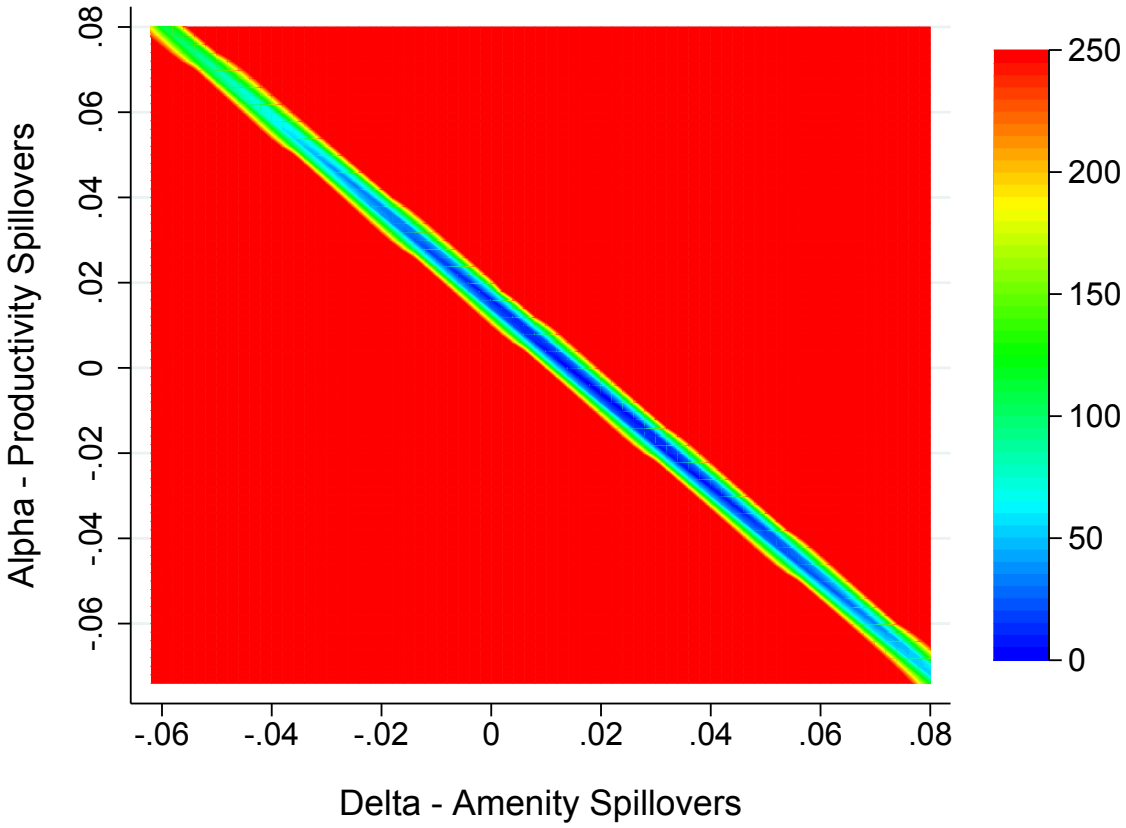
Notes: The graphs shows proxies for amenities before Second World War against city size in 1939.

Figure 8: Real Estate Prices and Population



Notes: The graphs show the logarithm of the average per square meter rent for new-build flats and the logarithm of the average per square meter purchase price of different types of new-build dwellings scattered against the logarithm of 1987 population. See the main text for more detail.

Figure 9: Examining the Objective Function



Note: The graphs shows the objective function that we minimise in the quantitative analysis of the model for different values of amenity and productivity spillovers holding the share of public employment constant at value that minimises the objective function. Different colours represent different values of the objective function, which has a value of 0.188 for the best fit parameter set. Values of the objective function of more than 250 are top coded and shown in red to improve the colour contrasts closer to the minimum of the objective function.