The Impact of Public Employment: Evidence from Bonn*

Sascha O. Becker
University of Warwick, CAGE and CEPR

Stephan Heblich
University of Bristol, IZA and SERC

Daniel M. Sturm
London School of Economics and CEPR

September 22, 2015

Preliminary and Incomplete Draft

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 show that after the relocation of the government, Bonn experienced a substantial increase in total employment and population relative to a control group of cities. However, this increase was almost exclusively driven by an increase in public employment, while private employment remained largely unchanged. 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

*We are grateful to the European Research Council (Grant Number 263733), for financial support. Iain Bamford, Horst Bräunlich, and Daniela Glocker provided excellent research assistance. We would like to thank conference and seminar participants for their helpful comments. We are also grateful to Ben Faber, Steve Redding, and Tony Venables for their comments and suggestions. The usual disclaimer applies.

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. Second, the location of public employment should have important impacts on the location of private sector activity through its effects on wages, housing costs and potential productivity and amenity spillovers.

In this paper we use the relocation of the (West) 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 trace 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 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 this model both private sector firms and the public sector demand labour across different cities. Private sector firms produce manufacturing varieties which are tradable across cities at some cost while public sector workers produce a global public good. 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 crowds out private sector employment through increases in house prices and nominal wages. On the other hand, public sector employment can promote private sector activity through productivity and amenity spillovers from public to private sector workers and by dampening the extent of local competition.

Using panel data for Bonn and 40 control cities covering the the period from 1925 to 1987, we examine the predictions of our model. Our basic finding is that total employment and population in

[1] Across the G10 countries in 2005, for example, on average 15.9 percent of employment was in the public sector, while average employment in industry amounted to only 14.6 percent (see OECD (2009) and OECD (2015)). The original G10 countries are Belgium, Canada, France, Germany, Italy, Japan, Netherlands, Sweden, United Kingdom and United States.

Bonn substantially increased in the wake of the arrival of the Federal Government in Bonn relative to the control group of cities. However, this increase in employment is driven by the large increase in public employment, while private sector employment remains essentially unchanged relative to the control group of cities.

We use our model to provide evidence for the different mechanisms through which public and private employment interact to produce this pattern of results. First, we provide some suggestive direct evidence for the amenity and productivity spillovers from public to private employment that the model postulates. We find that while there is considerable evidence for amenity spillovers from public employment in Bonn, there is little evidence for productivity spillovers from public to private employment. Second, we show that long-term changes in wages are in line with the predictions of the model. Third, we provide evidence that house prices in Bonn have also increased in the way predicted by the model. Finally, we calibrate the model to explore for which parameters the model can best fit the reduced form evidence.

Our paper contributes to a number of literatures. First, there is little systematic empirical evidence for the interaction between public and private sector employment. A notable exception is Faggio and Overman (2014) who use UK local authority level data covering the period 1999 to 2007 and find that increases in public sector employment have no identifiable effect on total private sector employment. To control for the endogenous location of public employment they use a shift-share instrument.[2] Our approach differs from Faggio and Overman (2014) 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 and difference-in-differences strategy to identify the impact of public employment on private employment. Finally, we use our model to shed light on the mechanisms through which public and private employment interact.[3]

Second, our paper is related to the literature on local multipliers. This literature assesses how additional manufacturing jobs create additional employment through increased demand for local goods and services. Moretti (2010) 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 compared to unskilled jobs. Black, McKinnish, and Sanders (2005) analyse the effect of the coal

[2]Other recent contributions to this literature are Faggio (2014) who uses a difference-in-differences approach on data covering 2003-2008 to evaluate relocations of public sector employment from London during the 2004-2010 Lyons Review, finding broadly similar results to Faggio and Overman (2014). Schlüter (2014) investigates whether the opening of federal ministries in Berlin from 1999 onwards has resulted in private sector job creation in the immediate vicinity of these ministries.

[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.

boom in the 1970s and the subsequent coal bust in the 1980s on local labour markets in Kentucky, Ohio, Pennsylvania, and West Virginia. They estimate that each additional mining job created 0.17 non-tradable jobs; the loss of a mining job implies a loss of 0.34 non-tradable jobs. Carrington (1996) finds significant increases in the non-tradable sector when the construction of the Trans-Alaskan Pipeline System between 1974 and 1977 temporarily increased construction employment. Our paper looks at employment effects of an exogenous increase in public sector employment which we have good reason to expect to be different from the effect of increases in other sectors.

Third, following Rosen (1979) and Roback (1982) a large literature has examined the implications of amenity and productivity differences across locations. See Albouy (2008) and Albouy and Lue (2015) for recent contributions. 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) and Diamond (2015). We show that public employment is potentially an important channel of local amenity differences across cities. Fourth, we also contribute to the wider literature on local labor market shocks. One strand of this literature investigates the impact of closures of military bases on local economic outcomes (see, for example, aus dem Moore and Spitz-Oener (2012) and Moretti (2011) for a recent survey). Finally, there is a developing literature on evaluating 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

In this section 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).[4] The location choices of private sector firms are determined by the usual agglomeration and dispersion forces. The main agglomeration forces are that firms want to locate production near large markets to economise on transport costs and similarly consumers want to locate close to large markets where they can consume more varieties without incurring transport costs. The main dispersion forces are that firms in larger markets face more competition and that house prices increase until workers are

[4] A more detailed exposition of the model is contained in the technical appendix of this paper.

indifferent across all locations that are populated in equilibrium. The key novel feature of the model is the presence of public employment, which is assumed to produce 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 private sector equilibrium.

2.1 Endowments, 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 labour which is supplied inelastically with zero disutility.[5] Utility is defined over a consumption index of manufacturing varieties, C_c^M, 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:

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

The tradeables consumption index takes the standard CES (Dixit-Stiglitz) form. There are iceberg type transport costs for manufacturing varieties between locations and for one unit of a variety produced in city i to arrive in city c, a quantity $T_{ic} > 1$ must be shipped. The dual tradeables price index is as follows:

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

which uses the fact that all n_i tradeable 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. Note, that the price index in equation (2) depends on the access of consumers in city c to tradeable varieties, as captured by the number of varieties and their free on board prices in each city i, together with the trade costs of shipping the varieties from cities i to c.

There is a fixed cost in terms of labour of producing tradeable varieties, $F > 0$, and a constant variable cost. The total amount of labor, l, required to produce x units of a variety is:

$$l = F + \frac{x}{\varphi_i}, \tag{3}$$

where φ_i captures the productivity of firms in city i. Profit maximisation subject to a downward sloping demand curve for each tradeable variety yields the standard result that the equilibrium free

[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.

on board price of tradeable varieties is a constant mark-up over marginal cost:

$$p_i = \left(\frac{\sigma}{\sigma - 1}\right) \frac{w_i}{\varphi_i}. \tag{4}$$

2.2 Public Sector

The public sector produces a global public good (e.g. defence) that affects all agent in the economy equally independent of their location. Without loss of generality we normalise the utility that the agents derive from this public good to zero. The public sector hires a number of workers L_i^G in city i, which we treat as a policy parameter.[7] 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.[8] 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:

$$\tau \sum_i (w_i L_i) = \sum_i (w_i L_i^G). \tag{5}$$

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

$$\varphi_i = (L_i^G)^\alpha, \tag{6}$$

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

$$B_c = (L_c^G)^\delta, \tag{7}$$

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, 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:

$$\frac{w_c B_c}{(P_c^M)^\mu \, (P_c^H)^{1-\mu}} = k, \qquad \text{for all } c \tag{8}$$

[7] As we only consider shifts in the spatial location of public sector employment but no changes in the aggregate amount of public employment, it is in the model equivalent whether the public sector creates new jobs in a particular city, or whether the public sector has a dedicated workforce of public sector employees who move between cities.

[8] It would not be difficult to modify the model so that public sector jobs offer a higher wage than private sector employment in the same city.

where we implicitly assume that all cities are populated in equilibrium. The intuition behind (8) is that higher wages or higher amenities make a city more attractive, while higher housing costs or a higher price of manufacturing goods make the city less attractive.

Labor market clearing implies that labor demand in the tradeables and public sector sums to total employment in the city. Using the fact that free entry in the private sector combined with the CES demand structure implies a constant equilibrium labor input for each variety and the tradeables production technology in equation (3), the labor market clearing condition can be written as:

$$L_i - L_i^G = L_i^M = n_i F \sigma. \tag{9}$$

This relationship therefore pins down the number of tradeable varieties produced in city i as a function of city population, the level of public employment and parameters of the model.

2.4 Simulation

To explore the properties of the model we simulate a simple two-city version of our model. 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. In particular we assume an elasticity of substitution of four which is similar to the values typically used in the international trade literature (see, for example, Feenstra (1994); Ghironi and Melitz (2005)). Furthermore, we set the share of expenditure on housing in total expenditure to 0.25, which is close to the estimates in Davis and Ortalo-Magné (2011). Finally, we assume that economy wide public employment is one quarter of total employment.[9]

Figure 2 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 shifting public employment from city 2 to city 1 actually reduces total employment in city 1. The intuition behind this more than proportional crowd out effect is as follows. If the public sector moves more employment to city 1, this increases local house prices and induces private sector employment to migrate from city 1 to city 2. To see why the crowd-out of private sector employment is more than 100 percent, consider a one to one replacement of private sector employment in city 1 with public sector jobs. This implies that there is less local production of manufacturing varieties in city 1 compared to city 2 and the price index (2) is therefore higher in city 1 than city 2. To bring about spatial equilibrium more private sector worker therefore have to leave city 1 until house prices in city 1 have fallen sufficiently relative to city 2 to make private sector workers indifferent between locating in city 1 and city 2.

[9] We also normalise fixed costs F to one and set the iceberg transport costs between the two cities to 1.5.

If there are either productivity or amenity spillovers from public sector employment to the private sector, this negative impact of increasing public employment on total employment can be overturned. Figure 1 shows that sufficiently strong spillovers can actually increase private employment in city 1 in response to an increase in public employment in this city. The bottom panels show how these changes are reflected in nominal wages and house prices. Given the Cobb-Douglas preferences, house prices are proportional to total nominal income in the city, which is obviously closely related to total employment in the city. Both, productivity and amenity spillovers that are strong enough to increase total employment in city 1 also increase house prices. The impact of productivity and amenity spillovers on wages is slightly more subtle. Both types of spillovers increase nominal wages in city 1 marginally and this is driven by two mechanisms. First, there is the usual new economic geography mechanism that firms with better market access (i.e. the firms located in the larger city) are able to pay higher nominal wages in a zero profit equilibrium. Second, public employment creates demand for private sector firms without any competition effect.[10]

After a brief summary of the historical background, we now examine how public and private sector interact empirically and use the model to provide evidence for the different mechanisms through which the public and private sector interact.

3 Historical Background

In the wake of the Second World War, Germany was divided into four different parts: West and East 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

[10]The prediction that both amenity and productivity spillovers increase wages and house prices differs from the classic Roback (1982) analysis for two main reasons. First, in our model housing is not used as a factor of production. Second, Roback (1982) assumes that there is a freely tradeable homogenous final consumption good, while our model has positive trade costs and love of variety preferences which implies that differences in market access influence nominal wages and the prices consumers face. See Redding and Sturm (2008) and Handbury and Weinstein (2015) for recent evidence for the importance of market access at the city level.

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 preliminary capital. Of the 65 delegates, 29 voted for Frankfurt and 33 for Bonn, with 2 invalid votes.[11] 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 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, and probably more important, 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 promise to retain the majority of federal government employment in Bonn. After extensive building works, the parliament moved from Bonn back to Berlin in September 1999.

[11] The term preliminary was chosen to stress that Bonn was the new capital of West Germany but Berlin would become the capital of a future unified Germany.

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

[13] A popular 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)).

4 Data

Our basic dataset is panel data on employment and population in Bonn and a set of 40 control cities which were also located in West Germany. The pool of 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. Figure 1 shows the spatial distribution of the cities in our sample.[15] 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.[16] To make the cities comparable over time, we follow Redding and Sturm (2008) and control for major changes in city boundaries over the sample period by aggregating all population concentrations that merge at some point in time with one of the sample cities throughout the sample.

Due to changes in employment classifications over time, we are only able to disaggregate total employment into four sectors: agriculture; industry; transportation, wholesale trade, retail trade and communications; other employment. As employment in agriculture is extremely small for the cities that we consider, we abstract from it in the analysis below. The category "other employment" includes public employment but also other private services, including banking and insurance and the non-profit sector. We use this category as our main proxy of public employment in each city. While it is very difficult to construct a consistent time series of public employment spanning several decades at the city level for Germany, we were able to determine the share of public employment in "other employment" for the years 1961 and 1970. These data suggest that public employment accounts for 31 percent of "other employment" in our control cities and for 42 percent in Bonn. In these years, the share of "other employment" in total employment is 52 percent in Bonn and 29 percent in the control cities. So the overall employment share of public employment in Bonn is 22 percent versus 9 percent in the control cities.

In addition to this basic dataset 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)) to compare differences in a variety of consumptive amenities (health, education, culture) between Bonn and the control cities in the late 1980s. 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 es-

[15] Bonn is the 31st largest West German city in 1939 in our data. 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.

[16] We have also experimented with using a larger number of control cities but this has no meaningful impact on the results.

timate individual-level wage regressions. While comparable wage data does 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).

5 Empirical Approach

Our basic empirical approach is a simple difference-in-differences comparison of Bonn and our 40 control cities. 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 analysing 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. Each different vector W produces a different 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 variables for the J possible control cities. For each city, X includes exogenous covariates and a linear combination of pre-treatment outcomes that control for unobserved time-variant common factors that may affect the post-treatment trend. Exogenous covariates include a dummy for proximity to the German-German border to control for market access changes as in Redding and Sturm (2008); data on the number of refugees in the wake of the Second World War; the amount of rubble per capita and percentage of the housing stock destroyed as reported in Kästner (1949) to capture war-time destruction; and the number of students registered at the local university prior to the

Second World War.

Let V be a diagonal matrix with nonnegative components. The values of the diagonal elements of V reflect the relative importance of the variables over which the synthetic control group is constructed. The vector of weights W^* is chosen to minimise $(X_1 - X_0W)'V(X_1 - X_0W)$ subject to $w_j \geq 0$ $(j = 1, 2, ..., J)$ and $w_1 + ... + w_J = 1$. The vector W^* defines the combination of control cities which minimises the mean square prediction error (MSPE) and best resembles the city of Bonn in employment determinants at the outset of German division. We follow Gobillon and Magnac (2015) and set V to be the identity matrix, which gives equal weights to all control 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 MSPE 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.[17]

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:

$$\hat{\alpha}_{1t} = Y_{1t} - Y_{0t}W^*. \tag{10}$$

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 with a simple graphical difference-in-differences comparison of Bonn and the control group and then turn to synthetic controls.

6.1 Difference-in-Differences

Figure 3 shows the level of employment in Bonn and the average level of employment of the 40 control cities over the period 1925 to 1987 and Figure 4 shows the difference between Bonn and the average of the control cities. The graphs show a number of striking patterns. Total employment in Bonn expands substantially relative to the control group in the wake of the Second World War. By 1987, Bonn hosts about 25,000 employees more than the control group. In the pre-period, Bonn and the control group have similar trends though Bonn has between 15,000 and 20,000 employees less than the control group.

[17] Kaul, Klößner, Pfeifer, and Schieler (2015) argue that a potential problem with a data-driven procedure to choose V is that using all outcome lags as separate predictor variables results in very low weights for all other covariates, which can results in significant differences in these other covariates between treatment and synthetic control. As discussed below, using the identity matrix for V, results in well balanced co-variates in our application.

Once we split overall employment into three sectors, it turns out that the growth in total employment in Bonn is almost entirely driven by the expansion in "other employment" which is our proxy for public employment. In this category, Bonn and the control group show almost exactly the same development in the pre-period. However, employment in industry is essentially unchanged relative to the control group as is employment in retail and wholesale trade and transportation.

While Bonn and the control cities track each other fairly well before 1949 there are also clear differences. First, the level of employment is not the same in Bonn and the control group, which could affect post-war growth trends. Second, Bonn suffered slightly less war-time destruction than the control cities which is reflected in slightly larger employment growth during the war. We now turn to synthetic control methods (SCM) to reduces these and any other observable differences between Bonn and the control group.

6.2 Synthetic control method

Figure 5 shows Bonn and the synthetic control city. The synthetic control city tracks employment in Bonn prior to 1949 closely both in levels and growth rate. Figure 6 compares the treatment effect estimated using the synthetic control city to that obtained from the simple difference-in-differences comparison above. Results from the synthetic control method and simple difference-in-differences do not differ substantially. We again find that there is a sharp relative increase in employment in Bonn which is driven by our proxy for public employment while manufacturing, trade and transportation are largely unchanged.

Table 1 shows the balance in observables between the synthetic control city and Bonn across the four outcome variables. All covariates are given the same weights in our specification and we observe small differences between Bonn and its synthetic counterpart. The RMSPEs are small, taking on values of 0.341 for other employment, 0.615 for industry employment, 0.686 for employment in trade and transportation, and 2.432 for total employment. This indicates that the synthetic control method is providing a good fit in the pre-treatment period.

Figure 7 investigates the uniqueness of Bonn's employment development. This graph combines 40 placebo estimates for the cities in our donor pool of control cities (grey lines) with the graph for Bonn (black line). Each line in this figure shows the difference between a city and its synthetic control. In addition to the 40 placebo graphs, the shaded area encloses the region between the 5th and the 95th percentile of the distribution of placebo graphs (excluding the largest city Stuttgart whose size cannot be reproduced with any combination of cities in our sample). In terms of total employment, Bonn's post-1949 development is more pronounced than that of most other cities. Once we focus on "other employment", which is out proxy for changes in public sector employment, Bonn's trend

clearly dominates the entire distribution. This provides strong evidence for the uniqueness of Bonn's development.

We can summarise our main results as follows. Looking at a panel of more than 60 years, we find robust evidence that the decision to make Bonn the capital of West Germany had massive effects on the size of the city and its local labor market. Bonn's overall population and employment nearly doubled. However, this exceptional increase is driven by the expansion of the public sector, while employment in industry and trade and transportation is virtually unchanged.[18]

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 and population. However, this expansion was driven by a large expansion in public employment, while private sector employment remained largely unchanged. 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 also responded in the way predicted by the model. Finally, we calibrate the model to see for which spillover parameters the model can best fit 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 consumptive 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

[18] We ran two further alternative specifications that support the validity of our findings. First, we repeated our analysis and generated weights for control variables using a data-driven procedure. Second, one may argue that generating different synthetic control groups for our four outcomes may be misleading since it implies that the size of one sector does not depend on the size of the other sectors. To account for that, we used the same vector of weights W^* from the total employment estimation for all sub-sectors. Both specifications do not affect our results.

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 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. While these regression are simple cross-sectional regressions, they are suggestive that amenities in Bonn differ substantially from other cites of a similar size in Germany.

7.2 Productivity Spillovers

We will now turn to potential productivity spillovers from increased public employment in Bonn. 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 on controls for university town status, state capital status and a dummy variable for Bonn.

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. We cautiously interpret this 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 subsample 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.[19] 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 in-

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

dividuals which are not covered by the social security system.[20]

Table 4 shows the results of the wage regressions. In Column (1), we present a bivariate regression showing that wages are, on average, 7.3% 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 and gender, education and nationality as suggested by Beaudry, Green, and Sand (2012). In Column (3), we add dummies for 88 2-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 variable controls for potential pre-existing differences in income before Bonn became capital. One can think of this regression as a quasi-DID regression. The Bonn dummy gets smaller as we add these further control variables and is marginally positive but statistically insignificant in Column (4).

The remaining columns of the table consider several further robustness checks. Column (5), uses the weights that determine the synthetic control city estimated in Section 6.2 which does not change the results substantially. In Column (6) we impute the truncated wages following Gartner (2005), which has minimal impact on the results. In the final column we add a control for log population. Holding population constant means that we control for the fact that larger cities typically have higher wages. Since the increase in size in Bonn is driven by public sector employment that does not increase local competition among private sector firms, one can interpret this specification as suggestive evidence for the relative importance the competition effect. In line with these considerations, we see the coefficient on the Bonn dummy decrease and taking on a negative sign though the estimate remains statistically insignificant. Taken together, these results suggest that there has been little change in nominal wages in Bonn relative to the control group of cities.

7.4 Rents

[Work in progress]

7.5 Calibrating the Model

[Work in progress]

8 Conclusion

[To be written]

[20] 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 jobs since there is no information on hours worked in the HES.

References

Abadie, A., A. Diamond, and J. Hainmueller (2010): "Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program," *Journal of the American Statistical Association*, 105(490), 493–505.

Abadie, A., and J. Gardeazabal (2003): "The Economic Costs of Conflict: A Case Study of the Basque Country," *American Economic Review*, 93(1), 113–132.

Albouy, D. (2008): "Are Big Cities Bad Places to Live? Estimating Quality of Life across Metropolitan Areas," *NBER Working Paper*, 14472.

Albouy, D., and B. Lue (2015): "Driving to Opportunity: Local Rents, Wages, Commuting Costs and Sub-Metropolitan Quality of Life," *Journal of Urban Economics*, 89, 74–92.

Algan, Y., P. Cahuc, and A. Zylberberg (2002): "Public Employment and Labour Market Performance," *Economic Policy*, 17, 7–65.

aus dem Moore, J. P., and A. Spitz-Oener (2012): "Bye Bye, G.I. - The Impact of the U.S. Military Drawdown on Local German Labor Markets," Humboldt University, mimeo.

Beaudry, P., D. A. Green, and B. Sand (2012): "Does Industrial Composition Matter for Wages? A Test of Search and Bargaining Theory," *Econometrica*, 80(3), 1063–1104.

Bender, S., A. Haas, and C. Klose (2000): "IAB employment subsample 1975-1995," *Schmollers Jahrbuch [Journal of Applied Social Science Studies]*, 120, 649–662.

Black, D., T. McKinnish, and S. Sanders (2005): "The Economic Impact of the Coal Boom and Bust," *The Economic Journal*, 115(503), 449–476.

Boeri, T., G. Nicoletti, and S. Scarpetta (2000): "Regulation and labour market performance," in *Regulatory Reform and Competitiveness in Europe*, ed. by G. Galli, and J. Pelkmans, pp. 324–380. Edward Elgar Publisher, Cheltenham UK.

Carrington, W. J. (1996): "The Alaskan Labor Market During the Pipeline Era," *Journal of Political Economy*, 104(1), 186–218.

Conley, T. G., and C. R. Taber (2011): "Inference with 'Difference in Differences' with a Small Number of Policy Changes," *The Review of Economics and Statistics*, 93(1), 113–125.

Davis, M. A., and F. Ortalo-Magné (2011): "Housing Expenditures, Wages, Rents," *Review of Economic Dynamics*, 14(2), 248–261.

Deutscher Städtetag (1988): *Statistisches Jahrbuch der Deutschen Gemeinden*. Deutscher Städtetag, Berlin.

Diamond, R. (2015): "The Determinants and Welfare Implications of US Workers' Diverging Location Choices by Skill: 1980-2000," Stanford Graduate School of Business, mimeo.

Faggio, G. (2014): "Relocation of Public Sector Workers: Evaluating a Place-based Policy," SERC Discussion Paper No. 155.

Faggio, G., and H. Overman (2014): "The Effect of Public Sector Employment on Local Labour Markets," *Journal of Urban Economics*, 79, 91–107.

Feenstra, R. C. (1994): "Zonal Boundaries and Access to Berlin," *American Economic Review*, 84(1), 157–177.

Floehr, R. (1986): *Bonn - die gekaufte Hauptstadt*. Fleur, Krefeld.

Franklin, W. M. (1963): "Zonal Boundaries and Access to Berlin," *World Politics*, 16(1), 1–31.

Gartner, H. (2005): "The imputation of wages above the contribution limit with the German IAB employment sample," FDZ Methodenreport 2-2005, Institute for Employment Research.

Gemeinschaftsveröffentlichung der Statistischen Landesämter (1991): *Volkswirtschaftliche Gesamtrechnung der Länder*. Statistisches Landesamt Baden Württemberg, Stuttgart.

Ghironi, F., and M. J. Melitz (2005): "International Trade and Macroeconomic Dynamics with Heterogeneous Firms," *Quarterly Journal of Economics*, 120(3), 865–915.

Glaeser, E. L., J. Kolko, and A. Saiz (2001): "Consumer City," *Journal of Economic Geography*, 1(1), 27–50.

Gobillon, L., and T. Magnac (2015): "Regional Policy Evaluation: Interactive Fixed Effects and Synthetic Controls," *Review of Economics and Statistics*, forthcoming.

Handbury, J., and D. Weinstein (2015): "Goods Prices and Availability in Cities," *Review of Economic Studies*, 82(1), 258–296.

Helpman, E. (1998): "The Size of Regions," in *Topics in Public Economics: Theoretical and Applied Analysis*, ed. by D. Pines, E. Sadka, and I. Zilcha, pp. 33–54. Cambridge University Press, Cambridge.

Herdegen, G. (1992): "Einstellungen zur Deutschen Einheit," in *Handwörterbuch zur Deutschen Einheit*, ed. by W. Weidenfeld, and K.-R. Korte, vol. 4. Campus Verlag, Frankfurt am Main.

Kästner, F. (1949): "Kriegsschäden: Trümmermengen, Wohnungsverluste, Grundsteuerausfall und Vermögensteuerausfall," in *Statistisches Jahrbuch deutscher Gemeinden*, vol. 37. Deutscher Städtetag, Berlin.

Kaul, A., S. Klössner, G. Pfeifer, and M. Schieler (2015): "Synthetic Control Methods: Never Use All Pre-Intervention Outcomes as Economic Predictors," *mimeo*.

Kettenacker, L. (1989): *Krieg zur Friedensicherung: Die Deutschlandplanung der britischen Regierung während des Zweiten Weltkrieges*. Vandenhoeck and Ruprecht, Göttingen.

Kline, P., and M. Busso (2013): "Assessing the Incidence and Efficiency of a Prominent Place Based Policy," *American Economic Review*, 103(2), 897–947.

Kline, P., and E. Moretti (2014): "Local Economic Development, Agglomeration Economies, and the Big Push: 100 Years of Evidence from the Tennessee Valley Authority," *Quarterly Journal of Economics*, 129(1), 275–331.

Krugman, P. (1991): "Increasing Returns and Economic Geography," *Journal of Political Economy*, 99(3), 483–499.

Moretti, E. (2010): "Local Multipliers," *American Economic Review, Papers and Proceedings*, 100(2), 1–7.

——— (2011): "Local Labor Markets," in *Handbook of Labor Economics*, ed. by D. Card, and O. Ashenfelter, vol. 4b, pp. 1238–1303. Elsevier North Holland, Amsterdam.

Neumark, D., and H. Simpson (2015): "Place-based Policies," in *Handbook of Regional and Urban Economics*, ed. by G. Duranton, V. Henderson, and W. Strange, vol. 5, pp. 1197–1287. Elsevier, Amsterdam.

OECD (2009): *Government at a Glance*. OECD Publishing, Paris.

——— (2015): *STAN: OECD Structural Analysis Statistics*. OECD Publishing (Downloaded September 2015), Paris.

Redding, S. J., and D. M. Sturm (2008): "The Costs of Remoteness: Evidence from German Division and Reunification," *American Economic Review*, 98(5), 1766–1797.

Roback, J. (1982): "Wages, Rents and the Quality of Life," *Journal of Political Economy*, 90(6), 1257–1278.

Rosen, S. (1979): "Wages-based Indexes of Urban Quality of Life," in *Current Issues in Urban Economics*, ed. by P. Mieszkowski, and M. Straszheim. John Hopkins University Press, Baltimore.

Schlüter, T. (2014): "The Spatial Dimension of Labour Markets: An Investigation of Economic Inequalities and a Local Employment Shock," PhD Thesis London School of Economics.

Statistisches Reichsamt (1941): *Die Steuerleistung der Finanzamtsbezirke in den Rechnungsjahren 1926 bis 1938*. P. Schmidt, Berlin.

Wise, M. Z. (1998): *Capital Dilemma: Germany's Search for a New Architecture of Democracy*. Princeton Architectural Press, New York.

Table 1: Balancing Tests

	(1)	(2)	(3)	(4)	(5)	(6)
	Employment			Industry		
	Treatment	Control	T-C	Treatment	Control	T-C
Outcome in 1925	56.252	55.143	1.109	25.226	24.315	0.911
Outcome in 1933	61.229	62.128	-0.899	25.673	26.070	-0.397
Outcome in 1939	65.283	67.864	-2.581	27.628	28.326	-0.698
Outcome in 1946	58.923	55.040	3.883	22.007	21.830	0.177
Border Dummy	0.000	0.063	-0.063	0.000	0.054	-0.054
Refugees	23.200	22.049	1.151	23.200	22.094	1.106
Rubble	7.300	8.571	-1.271	7.300	8.584	-1.284
Flats	37.300	33.751	3.549	37.300	34.084	3.216
Share students 1925-1939	0.013	0.012	0.001	0.013	0.012	0.001
	Trade and Transportation			Other Sectors		
	Treatment	Control	T-C	Treatment	Control	T-C
Outcome in 1925	14.591	14.250	0.341	13.816	13.731	0.085
Outcome in 1933	16.415	16.752	-0.337	15.987	16.128	-0.141
Outcome in 1939	16.166	17.046	-0.880	18.360	18.567	-0.207
Outcome in 1946	13.274	12.329	0.945	19.922	19.325	0.597
Border Dummy	0.000	0.063	-0.063	0.000	0.069	-0.069
Refugees	23.200	22.133	1.067	23.200	22.001	1.200
Rubble	7.300	8.554	-1.254	7.300	8.987	-1.687
Flats	37.300	34.138	3.161	37.300	33.094	4.205
Share students 1925-1939	0.013	0.012	0.001	0.013	0.012	0.001

Notes: The table shows the balance of the covariates between the treatment (Bonn) and its synthetic control group for four different outcome varibales, total employment, industry imployment, trade and transportation employment and other sector employment that includes services and the public sector.

Table 2: Proxys for Amenity Spillovers

	(1) Total Cultural Expenditure	(2) Total City Expenditure	(3) Total Budget Theaters	(4) Employment Actors	(5) Wage Bill Actors	(6) Theater Visitors	(7) Artists per 1,000 Empl	(8) Number of Doctors	(9) Hospital Beds	(10) Class Size
Bonn	0.553***	0.086*	0.665***	-0.071	0.807***	-0.026	0.235***	0.233***	0.281***	0.022
	(0.085)	(0.044)	(0.087)	(0.118)	(0.109)	(0.096)	(0.082)	(0.061)	(0.048)	(0.023)
log (Population)	1.048***	1.006***	0.732***	0.487**	0.612***	0.536***	1.008***	0.735***	0.526***	0.005
	(0.115)	(0.094)	(0.136)	(0.220)	(0.180)	(0.169)	(0.172)	(0.075)	(0.086)	(0.035)
University Town	0.560***	0.154	0.330**	0.144	0.392**	0.302*	0.863***	0.394***	0.440***	-0.094***
	(0.117)	(0.095)	(0.127)	(0.195)	(0.162)	(0.160)	(0.205)	(0.080)	(0.086)	(0.027)
State Capital	0.005	0.102	0.188	0.116	0.225	0.182	0.240	0.222	0.081	0.073
	(0.094)	(0.157)	(0.178)	(0.181)	(0.185)	(0.163)	(0.238)	(0.134)	(0.094)	(0.047)
Observations	41	39	33	33	32	33	41	41	41	41
R-squared	0.793	0.800	0.676	0.310	0.588	0.491	0.698	0.836	0.761	0.238

Notes: All columns report OLS regressions of city specific amenity measures (in log) 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 numbers result from missing observations for the city state Bremen with its two cities Bremen and Bremerhaven (Column 2) or missings information for theater outcomes in Solingen, Remscheid, Recklinghausen, Offenbach, Muelheim, Ludwigshafen, Herne, Bottrop (Columns 2, 4, 6) and missings wage information for artists in Heidelberg (Column 5). Robust standard errors reported in paranthesis. Artists in Column (7) include musicians, performing artists and graphical artists. *** denotes statistical significant at the 1 percent level. ** denotes statistical significant at the 5 percent level. * denotes statistical significant at the 10 percent level.

Table 3: Gross Value Added Across Cities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Industry Gross Value Added 1988			Trade/Transportation Gross Value Added			Combined Gross Value Added Industry and Trade/Transportation		
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

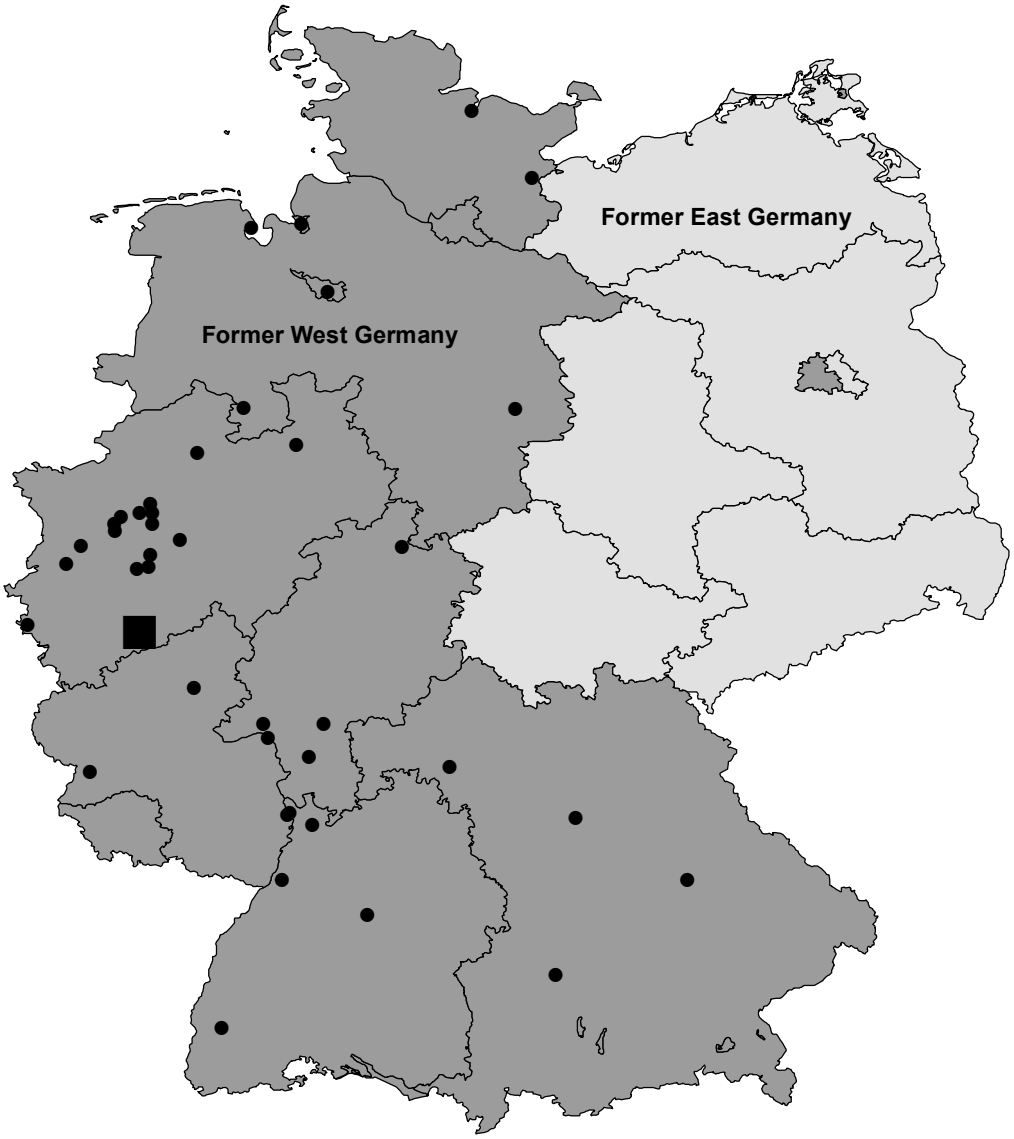
Notes: All columns report OLS regressions of city specific gross value added per worker at market prices (in log) on a dummy for Bonn and controls for a university town dummy, a state capital dummy, and the log of population. Robust standard errors reported in paranthesis. *** denotes statistical significant at the 1 percent level. ** denotes statistical significant at the 5 percent level. * denotes statistical significant at the 10 percent level.

Table 4: Wages

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Baseline				Imputed Wage	Synth Weights	Pop Control
	log (wage)	log (wage)	log (wage)	log (wage)	log (wage)	log (wage)	log (wage)
Bonn	-0.073***	-0.037***	-0.016*	0.004	0.005	0.026	-0.003
	(0.010)	(0.011)	(0.008)	(0.005)	(0.005)	(0.023)	(0.005)
Experience		0.036***	0.031***	0.032***	0.032***	0.033***	0.031***
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Expericence squared		-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Female		-0.184***	-0.140***	-0.140***	-0.129***	-0.114**	-0.140***
		(0.017)	(0.012)	(0.011)	(0.011)	(0.036)	(0.012)
Foreigner		-0.047***	-0.055***	-0.051***	-0.054***	-0.070***	-0.052***
		(0.013)	(0.011)	(0.009)	(0.009)	(0.015)	(0.010)
log Income Tax pc 1937				0.047***	0.049***	0.098**	-0.003
				(0.017)	(0.017)	(0.031)	(0.005)
log (Population)							0.019**
							(0.010)
Education Dummies	No	Yes	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	No	No	Yes	Yes	Yes	Yes	Yes
Weighted	No	No	No	No	No	Yes	No
Observations	134,910	126,801	126,747	126,747	126,747	29,408	126,747
R-squared	0.200	0.443	0.546	0.548	0.621	0.545	0.549

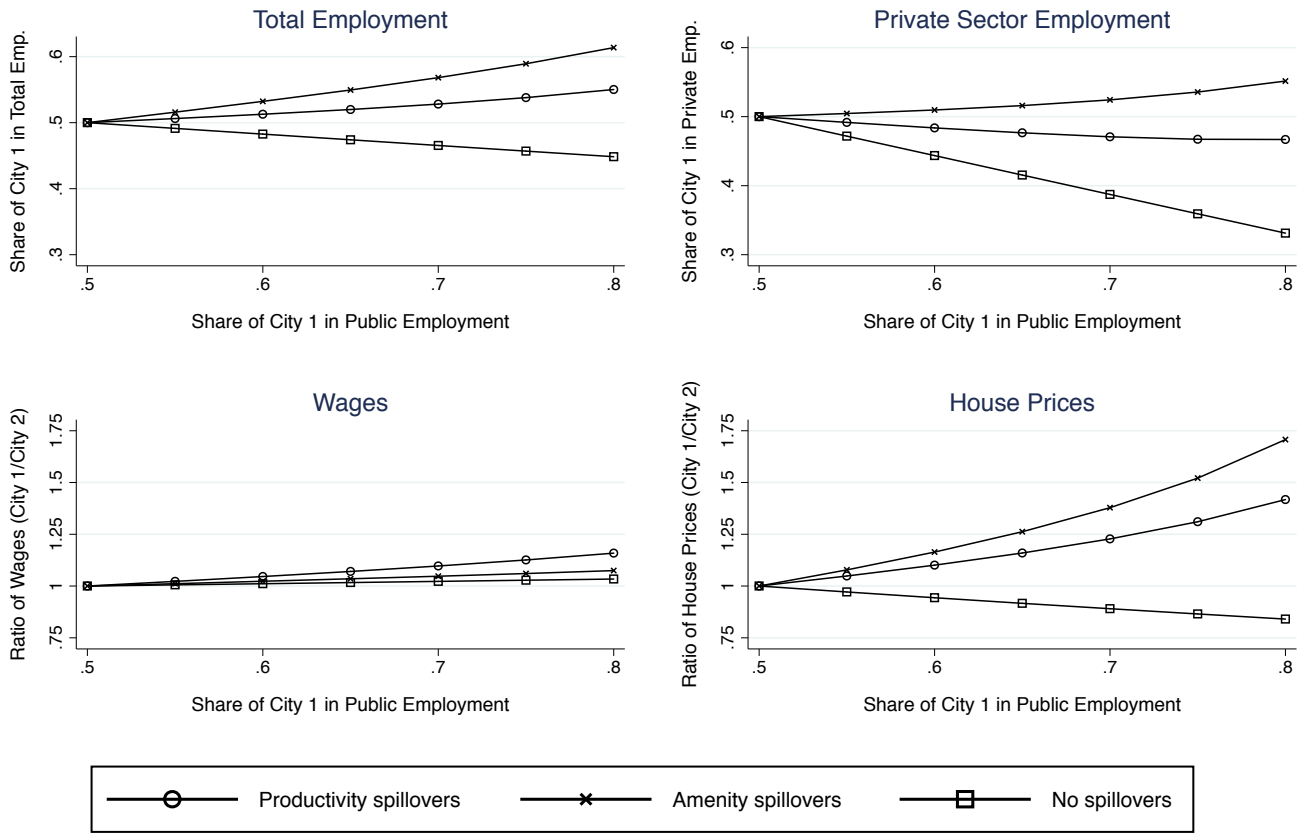
Notes: All estimations acount for top coding and allow for different experience and education effects for females and foreigners and experiecne to vary with education. Coefficients on these interactions are not reported. In column (5) we allow all observations above the cut-off to have different personal characteristcis. Again, these interactions are not shown. Education dummies include the categories unskilled (base category), skilled and high skilled and industry fixed effects are included for 2-digit industries. In Column (6) we weight cities with the W-matrix weights from synthetic control group estimations where we use total employment as outcome. In Column (7) we add a control for log population to hold. Standard Errors are clustered at the level of the city. *** denotes statistical significance at the 1 percent level. ** denotes statistical significance at the 5 percent level. * denotes statistical significance at the 10 percent level

Figure 1: Bonn and the Control Cities



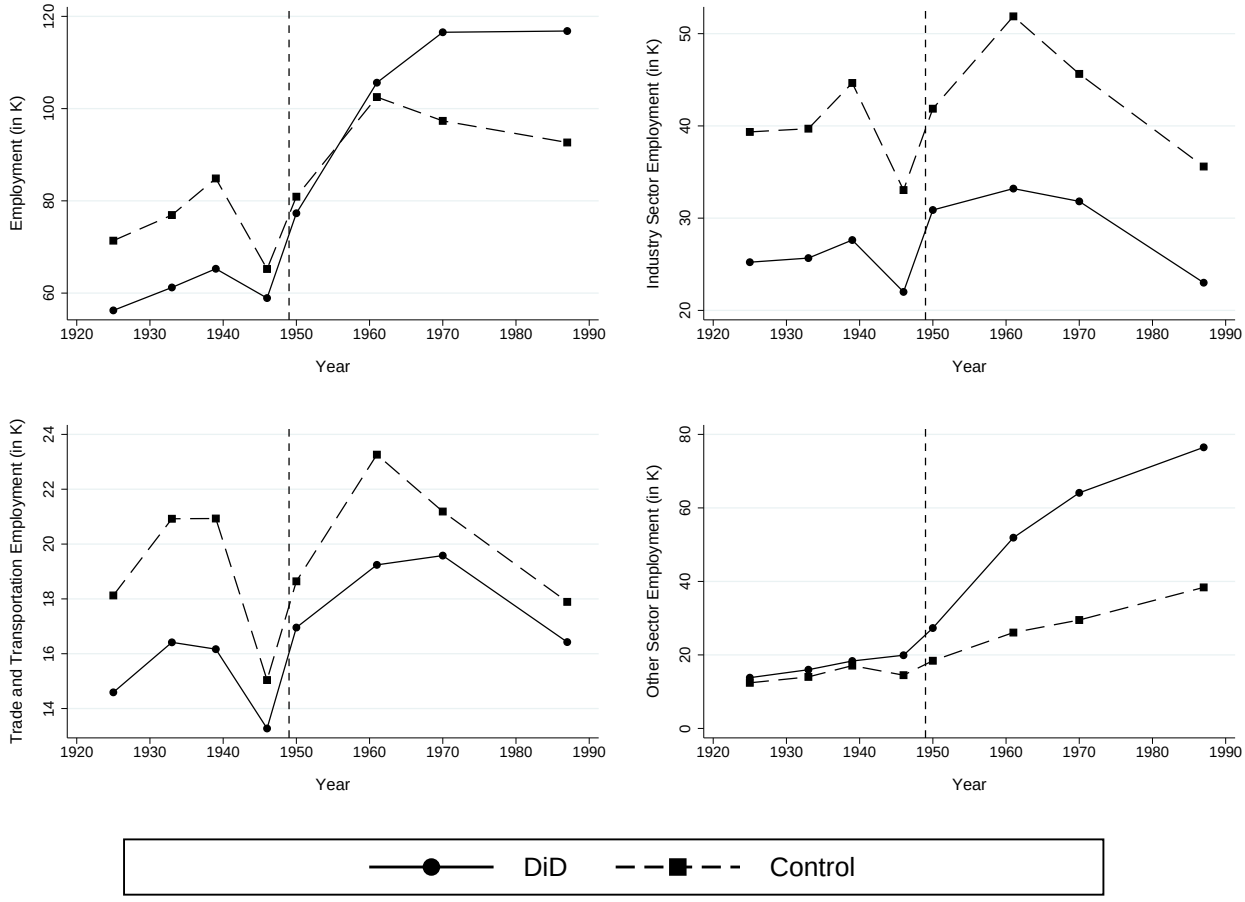
Note: The map shows the location of the city of Bonn (square) and the 40 control cities that comprise the control group in the DID comparisons and the donor pool for the synthetic control approach.

Figure 2: Simulating the Impact of Public Employment



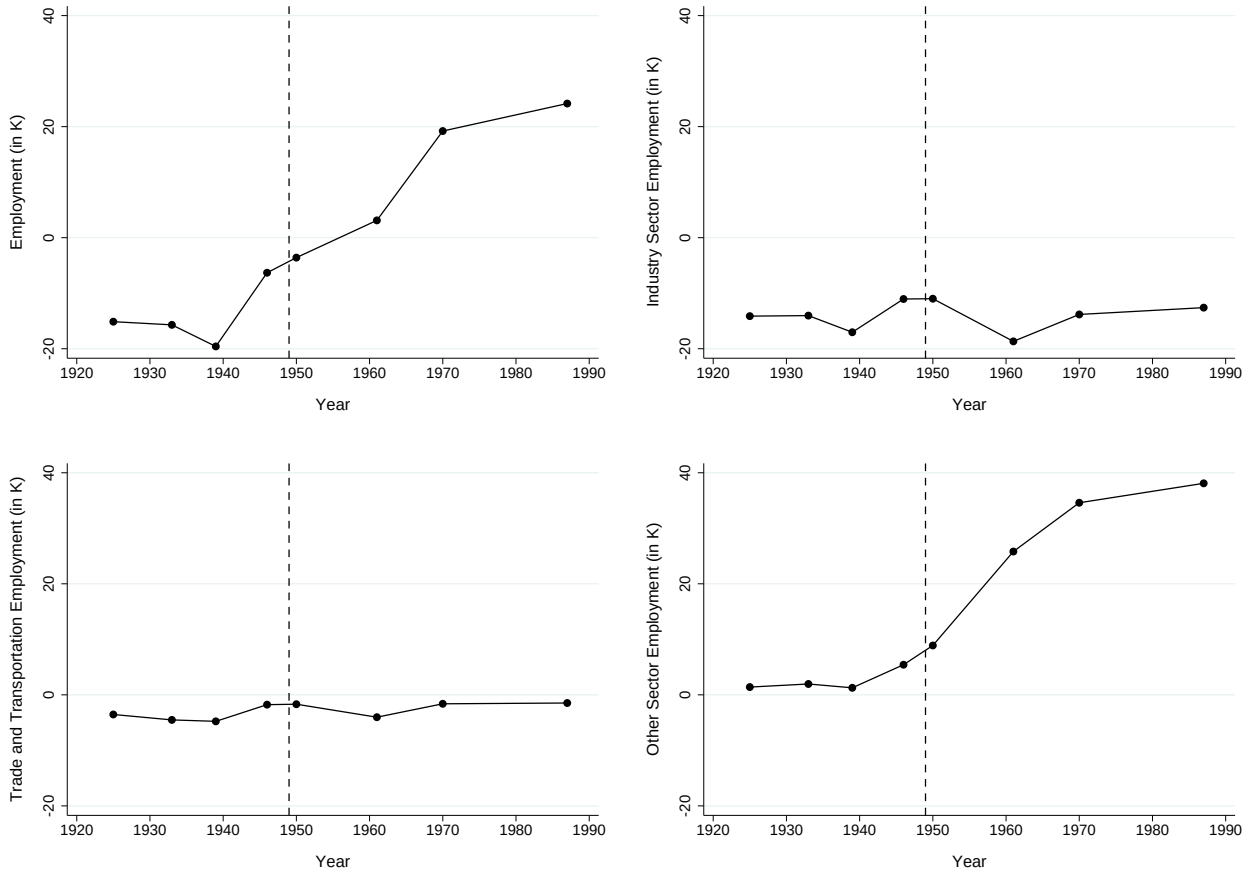
Note: The graphs show a simulation of the model for two ex-ante symmetric cities. In the simulations with productivity spillovers we assume an elasticity of productivity with respect to the level of public employment of 8 percent ($\alpha = 0.08$) and no amenity spillovers. In the simulations with amenity spillovers we assume an elasticity of amenities with respect to the level of public employment of 8 percent ($\delta = 0.08$) and no productivity spillovers.

Figure 3: Difference-in-Differences



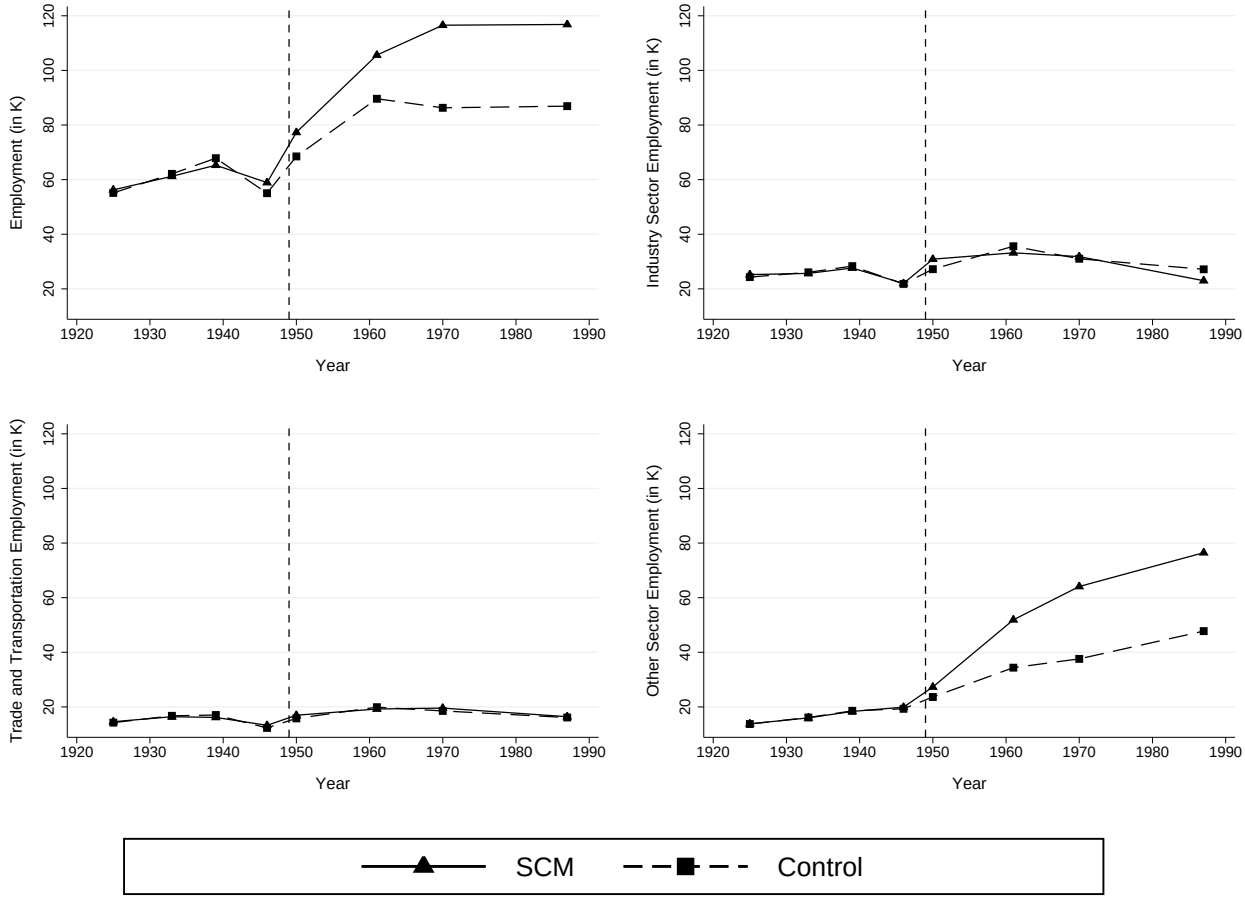
Note: The graphs show employment in Bonn (solid line) and the average for the 40 control cities (dotted line) in thousands. Total employment is broken down into three sectors: Industry; Retail and Wholesale Trade, Transportation and Communication; Other Services which includes public employment.

Figure 4: Difference-in-Differences: Treatment minus Control



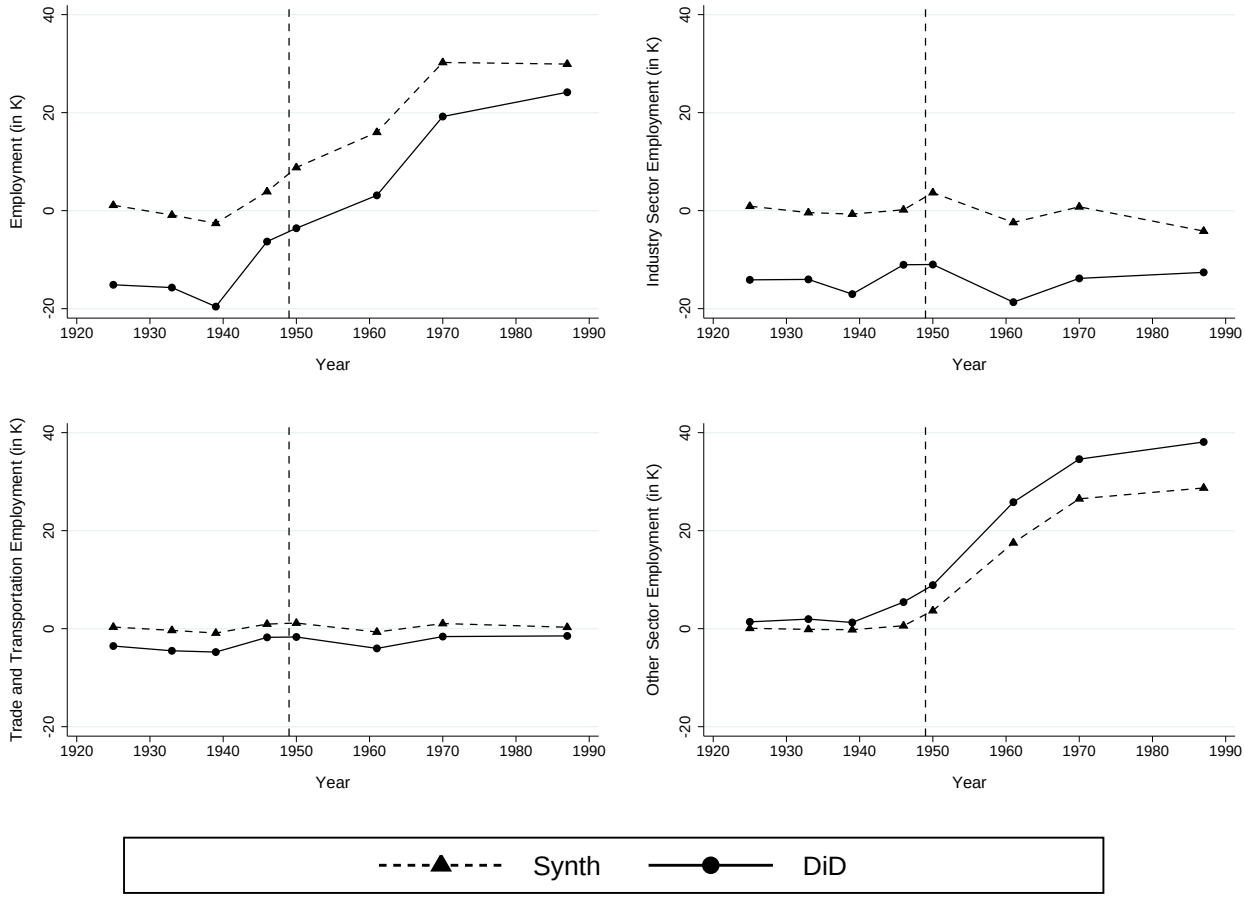
Note: The graphs show the difference between employment in thousands both for Bonn (treatment city) and the average for the 40 control cities in thousands.

Figure 5: Bonn and the Synthetic Control City



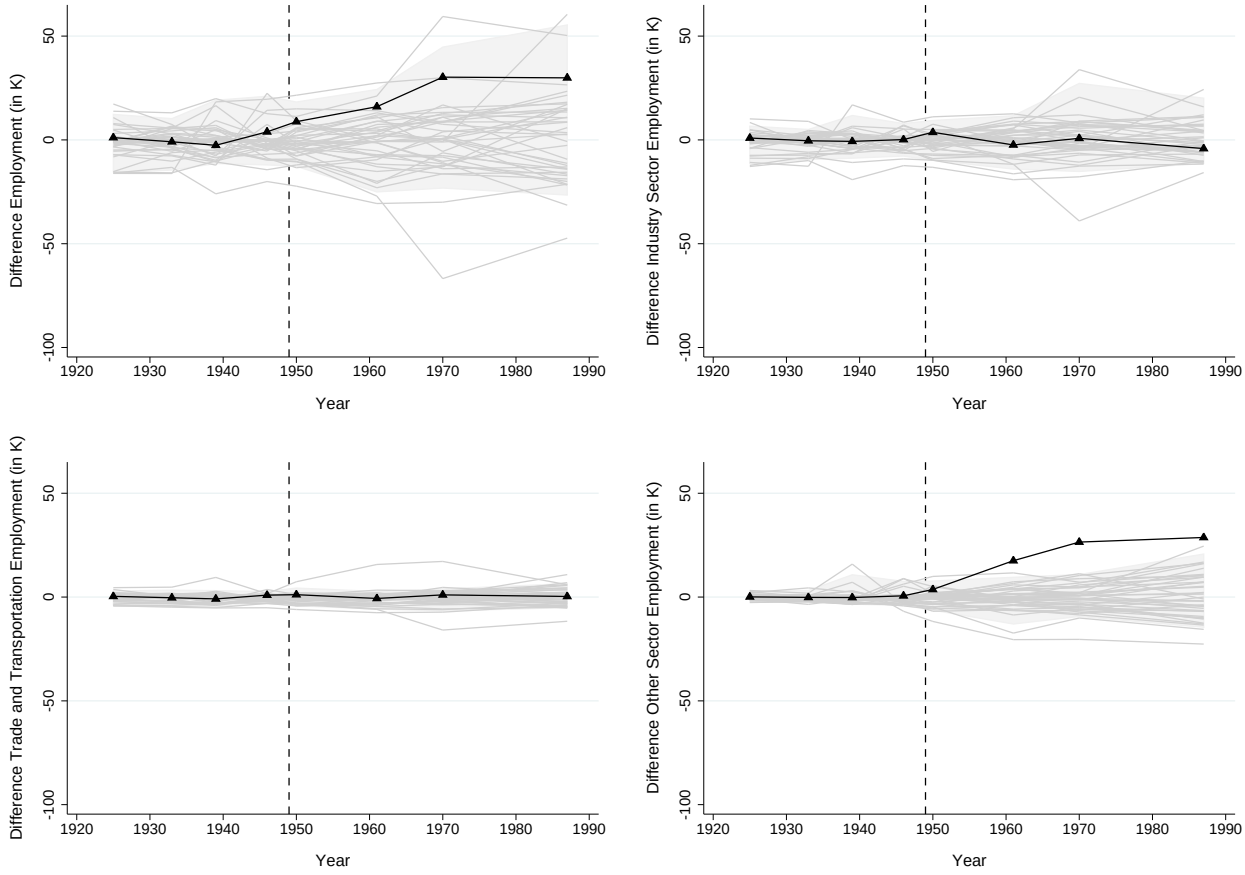
Note: The graphs shows employment in thousands both for Bonn (solid line) and the synthetic control city (dotted line). In the top-left panel with total employment as outcome, control cities and their synthetic weights are: Braunschweig (6.3%), Freiburg (20%), Hagen (31.5%), Heidelberg (18.3%), Münster (6.4%), Osnabrück (12%), and Stuttgart (5.4%). In the top-right panel with industry sector employment as outcome, control cities and their synthetic weights are: Braunschweig (5.4%), Freiburg (13.3%), Hagen (42.9%), Heidelberg (17.1%), Münster (17.6%), and Wilhelmshaven (3.6%). In the bottom-left panel with trade and transportation employment as outcome, control cities and their synthetic weights are: Braunschweig (6.3%), Freiburg (19.9%), Hagen (36%), Heidelberg (17.7%), Münster (8.1%), Osnabrück (9.8%), and Stuttgart (2.4%). In the bottom-right panel with other employment as outcome, control cities and their synthetic weights are: Braunschweig (6.3%), Bremen (4.8%), Freiburg (20.8%), Hagen (13.9%), Heidelberg (21.5%), Osnabrück (18.4%), and Stuttgart (13.9%).

Figure 6: Bonn and the Synthetic Control City



Note: The graphs compares the treatment effects on employment in Bonn estimated using the simple DID approach (solid line) and using the synthetic control approach (dotted line).

Figure 7: Synthetic Placebos



Note: The graphs shows placebo treatment effects for all 40 control cities 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.