

Market access and regional disparities:
Empirical evidence from German history

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Chapter 1

Outline

Regional disparities have numerous consequences ranging from migration, over political tensions, to violent conflicts (Buhaug et al., 2011; Cederman et al., 2011). Therefore, considerable political efforts are made worldwide to reduce regional disparities and mitigate these problems. For example, since the 1980s, a large part of the European Union (EU) budget is attributed to measures that aim to reduce regional disparities within and across EU member states (Van Deuverden, 2018). The largest expenditure category in the 2021–2027 EU budget includes the sub-heading “Economic, social and territorial cohesion”, which accounts for 31 percent of the total budget (Council of European Union, 2020). Despite the enormous investments in infrastructure and direct subsidies to attract firms to less developed regions, the regional disparities in the EU persist to date and might even increase (Camagni et al., 2020).

However, it is essential to understand the causes of regional disparities to implement effective policy measures. The economic literature discusses several proximate and fundamental factors that may affect regional economic development.¹ In recent years, the notion that institutional differences are the main drivers of economic development has gained increasing attention. The interest in institutional differences surged after the seminal contribution of Acemoglu et al. (2001), who analyze the effect of different colonization strategies of European settlers and associated institutions on long-run development in the former colonies. The authors find that a higher risk of expropriation, which they relate to weaker institutions, negatively affects growth. Other authors, however, emphasize the importance of location fundamentals, such as transport costs, the local disease environment, and natural resources, e.g., Gallup et al. (1999) and Sachs and Warner (2001). Another factor discussed in the literature—and closely related to the effect of

¹For a detailed discussion of development theories see Acemoglu (2009) and for regional development theories see Capello and Nijkamp (2009).

transport costs—is the influence of market access (Redding and Turner, 2015; Proost and Thisse, 2019).

The idea that regional market access differences can alter the spatial equilibrium of economic activity is emphasized in the new economic geography (NEG) literature. In his seminal contribution, Paul Krugman (1991) develops a first NEG model that combines increasing returns to scale at the firm level, monopolistic competition, trade costs, and endogenous location of firms and consumers. The term *market access* conceptualizes that economic activity is distributed across space and that transporting goods is costly. Therefore, firms locate in regions with large market access, i.e., regions with a large home market or low transport costs to other markets, or both. The settlement of firms in a given region, on the other hand, increases the market size of the region. Thus, the agglomeration of economic activity involves a circular causal relationship in which firms' spatial concentration creates and follows market access.

This dissertation contributes to the literature on regional disparities with empirical evidence on the effect of market access on regional development and the consequences of regional disparities on health outcomes. The focus is on historical episodes that provide unique settings to identify these effects. More precisely, Chapter 2 illustrates the difficulties to estimate the causal effect of market access on regional development in the context of the German division after World War II. Chapter 2 shows that the effect found in Redding and Sturm (2008) is sensitive to the inclusion of the population share of refugees in 1950 and corrections of boundary changes in sample cities. Thus, there is an alternative explanation for the observed development differences, other than market access. These results demonstrate the difficulties of finding suitable empirical settings to test the predictions of NEG models but should not be mistaken as a falsification of the market access effect.

Chapter 3 analyzes the effect of a decline in regional transport costs and a resulting increase in market access on parish level development. Therefore, we study the construction of the railway network in South-West Germany between 1829 and 1910. The results indicate a significant and positive effect of railway access on population growth, nominal income, house prices, and the transition from agriculture to industrial production. However, the effect sizes depend on the initial regional economic development and have thus increased regional disparities.

These regional disparities in income levels are a main explanatory variable in analyzing the mortality burden during the 1918 influenza pandemic in South-West Germany presented in Chapter 4. In addition, Chapter 4 studies the effect of air pollution on pandemic mortality. The empirical results indicate that poor and highly polluted parishes experienced a significantly

higher increase in mortality rates during the 1918 influenza pandemic. Hence, the results in Chapter 4 illustrate the far-reaching consequences of regional disparities.

A historical perspective is crucial to analyze the effect of market access on regional development and disparities, but not just to identify suitable natural experiments. As Proost and Thisse (2019, p. 592) put it: “geography and history interact in complex ways to determine the location of activities.” For instance, in a basic multi-region NEG model with symmetric regions, as discussed in Fujita et al. (1999), there are no agglomeration tendencies and the symmetric equilibrium is stable. However, multi-region NEG models allow for multiple stable spatial equilibria. Therefore, if some regions’ market access increases, e.g., due to falling transport costs, the economic activity will concentrate in these regions until a new spatial equilibrium is reached.

Bleakley and Lin (2012) and Jedwab and Moradi (2016) show empirically that initial agglomerations persist even after the factors causing the initial agglomeration tendencies are long gone. This is due to the persistent and self-reinforcing nature of the market access effect in NEG models. Chapter 3 provides further empirical evidence that transport cost changes will affect regions differently based on their initial agglomeration level. Thus, historical factors are essential to understand the processes determining the current spatial distribution of economic activity.

However, the circular causation of NEG models poses a challenge for the empirical identification of a causal effect of market access on regional development. In an influential article, Redding and Sturm (2008) address this problem using the German division after World War II and later reunification as a natural experiment. The authors argue that West German cities close to the East-West German border lost relatively more market access than other West German cities. Moreover, the division was unexpected at the time, and the border was drawn for military purposes, irrespective of the existing distribution of economic activity in space. Redding and Sturm (2008) use a difference-in-differences approach to estimate the effect of the division on population growth, comparing 20 cities within 75 kilometers to the East-West German border to 99 other West German cities. The results show that cities close to the border experienced a considerable decline in annual population growth relative to other West German cities. Redding and Sturm (2008) attribute this growth difference to the differential decline in market access.

After World War II, however, West Germany experienced an inflow of about 9.5 million refugees. These refugees initially arrived in regions close to the East-West German border (Wild, 1979). Thus, the refugees’ inflow represents a contemporaneous shock to the cities’ population, which is correlated with the differential decline in market access. Chapter 2 shows that the

results of Redding and Sturm (2008) are likely driven by the internal migration of refugees in the 1950s rather than market access losses.²

To provide evidence for this alternative explanation, I control for the population share of refugees in 1950 in the estimations of Redding and Sturm (2008). Importantly, the distribution in 1950 was mainly driven by the flight and expulsion of ethnic Germans after World War II, and movement restrictions hindered the migration of refugees within West Germany before 1950. Furthermore, I do not control for the actual migration of refugees in the 1950s, which might be an outcome of the market access shock, but for the share in 1950. Additionally, I correct the outcome variable for measurement error that is correlated with the treatment. More precisely, the population of sample cities is affected by incorporations of neighboring municipalities. These boundary changes took place predominantly in control cities and artificially increase the control group's annual population growth.

Including the population share of refugees in 1950 into the model of Redding and Sturm (2008) renders the overall treatment effect insignificant. The estimated decline in annual population growth of cities within 75 kilometers to the border reduces from -0.746 (s.e. 0.182) to -0.387 (s.e. 0.261) if the population share of refugees 1950 is taken into account. If I also use the adjusted population growth, i.e., population growth net of boundary changes, then the absolute treatment effect decreases further to -0.145 (s.e. 0.217). Hence, there is no significant difference in population growth between West German cities close to the East-West German border and other West German cities after the division.

These results, however, should not be mistaken as evidence that market access does not affect regional development. Instead, the results question the suitability of this specific historical episode to provide an appropriate natural experiment for the approach of Redding and Sturm (2008). Similarly, Becker et al. (2020) call for a more cautious interpretation of the extensive literature on the German division's enduring socio-economic effects.

Chapter 3 focuses on a historical episode that has seen a dramatic decline in transport costs due to the construction of railway networks. Because of this unprecedented decline in transport costs, this episode provides a unique setting to analyze the effects of market access changes on regional development and regional disparities.³ For the analysis, we focus on parishes in the Kingdom of Württemberg between 1829 and 1910. The Kingdom of Württemberg was one of the four Kingdoms of the German Empire and the third-largest after Prussia and Bavaria. The South-West German state completed its first wave of railway construction in 1854. We analyze

²An earlier working paper version of this chapter has been published as Franke (2017).

³In an earlier version of this chapter, Braun and Franke (2019), we mainly study the average growth effect of railway access.

the effect of railway access, defined as having a railway station within the parish, on population growth, nominal income, house prices, and industrialization.

These outcome variables are selected based on the predictions of multi-region NEG models, in which workers move to arbitrage away differences in real wages (Redding and Sturm, 2008; Redding and Turner, 2015). In these models, a reduction in transport costs increases local market access, which in turn attracts firms and increases local wages. Consequently, these regions experience an inflow of population and therefore increasing immobile factor prices, e.g., prices for land or housing.

To identify the causal effect of railway access, we employ a difference-in-differences strategy, semi-parametric models, and construct credible counterfactuals for railway parishes using alternative routes for a given railway line. We find a significant increase in all four outcome variables due to railway access. The results are consistent with predictions of NEG models. However, Chapter 3 also analyzes the heterogeneity of these effects. We find that larger and already industrialized parishes benefited more from railway access than rural and less developed parishes. The construction of the railway thus amplified existing regional disparities.

Chapter 3 thus complements the existing literature on railway access and urban population growth with comprehensive evidence from mainly rural parishes. Recent analyses by Bogart et al. (2019) and Büchel and Kyburz (2020) provided first evidence on the population effect of railways at a comparable aggregation level. The analysis in Chapter 3, however, examines whether the induced population growth goes indeed hand in hand with increases in wages and house prices as predicted by NEG models. Moreover, we apply an alternative identification strategy than the widely used “inconsequential units approach”, which we show to be problematic in the given setting. Instead, we compare parishes with railway access to parishes that would have gained railway access if an alternative proposal of a given line had been built. Thus, we construct credible counterfactuals for the outcomes of treated parishes. Furthermore, the chapter contributes to our understanding of the structural transformation that occurred in Europe during the 18th and 19th century. More precisely, we show that railway access fostered the transition from agricultural to industrialized regional economic activity, but at the expense of increasing regional disparities.

In Chapter 4, I study the 1918 influenza pandemic in Württemberg. The chapter first provides a detailed description of mortality statistics during the 1918 influenza pandemic in Württemberg before focusing on the effect of income and air pollution on pandemic mortality. As shown in Chapter 3, there are significant regional disparities within the Kingdom regarding income and industrialization. Chapter 4 answers the question to what extent these regional

disparities affected mortality during the 1918 influenza pandemic. For the analysis, I combine annual all-cause mortality data at the parish level with socio-economic indicators used in Chapter 3. Moreover, I digitized the location of coal-fired power plants, a major source of air pollution at the beginning of the 20th century.

The empirical analysis in Chapter 4 is based on a difference-in-differences approach that compares the average change in mortality rates between 1917 and 1918 of parishes with low, medium, and high income (pollution). The results show that parishes with low income and high pollution faced a significantly higher mortality rate increase than richer and less polluted parishes in the 1918 influenza pandemic.

Chapter 4 makes several contributions to the literature on regional differences in pandemic severity during the 1918 influenza pandemic. First, the analysis complements the existing literature on urban pandemic severity with comprehensive evidence from mostly rural parishes. The focus on rural parishes is of particular interest because previous results indicate considerable differences in pandemic severity by the degree of urbanization (e.g., Clay et al., 2019). Second, Chapter 4 provides the first analysis of the effect of income and pollution on regional pandemic severity for Germany and one of the first for continental Europe. This point is important as existing evidence points to large differences in pandemic severity between Europe and the US (Bootsma and Ferguson, 2007). Finally, the paper provides a detailed description of mortality statistics in Württemberg and comparisons of excess mortality to the German Empire.

In light of the ongoing debates on effective policy measures to address regional disparities, two conclusions can be drawn from the results. First, regional disparities have far-reaching consequences that also affect regional health outcomes during a pandemic and, therefore, are a fundamental problem that must be addressed for inclusive development. Second, investments in transport infrastructure can strengthen existing regional disparities and, therefore, should be implemented only after a critical assessment of economic activity's initial spatial structure.

Chapter 2

The Costs of Remoteness: Comment

BY RICHARD FRANKE

Abstract Redding and Sturm (2008) use the German division as a natural experiment to study the importance of market access for regional development. They show empirically that cities close to the East-West German border experienced a significant decline in population growth due to division. I argue that their results are driven by the internal migration of refugees in the 1950s rather than the loss of market access. In fact, the treatment effect estimated by Redding and Sturm (2008) disappears completely once the refugee share in 1950 and boundary changes of sample cities are taken into account.

Key words: Market Access, Regional Growth, Internal Migration

JEL classification: F15, N94, R12, R23

2.1 Introduction

In a very influential article, Redding and Sturm (2008) use the German division after World War II (WWII) as a natural experiment to show the importance of market access for regional development. The authors find that following division 20 West German cities located within 75 kilometers to the East-West German border (treatment cities) experienced a considerable decline in annual population growth relative to other West German cities (control cities). They attribute this growth difference to the decline in market access caused by the division. The key assumption in Redding and Sturm (2008) is that distance to the East-West German border, which determines treatment and control status, affects population growth only through its effect on lost market access. By exploiting the German division as a natural experiment, Redding and Sturm (2008) argue to provide causal evidence on the importance of market access for regional development.¹ Other papers that also exploit the European division after WWII as a source of exogenous variation in market access, confirm the influence of market access on regional development (Redding et al. (2011), Brülhart et al. (2012), and Ahlfeldt et al. (2015)).

Redding and Sturm (2008), however, neglect the war related migration of refugees in the 1950s, which provides an alternative explanation for their findings. I provide empirical evidence that the estimated treatment effect in the 1950s is due to the contemporaneous shock in the internal migration of refugees, rather than the decline in market access. The internal migration pattern was induced by German refugees that had fled to regions close to the later East-West German border in the aftermath of WWII and moved to more western regions in the 1950s. Thus, the distance to the East-West German border also affected the population share of refugees in 1950. Moreover, population growth in the 1950s and population share of refugees in 1950 are strongly negatively correlated. Hence, there is a contemporaneous shock which differs between treatment and control group. Since Redding and Sturm (2008) only control for the population share of refugees in 1961, when many WWII refugees had already changed their initial residence, they neglect the internal migration in the 1950s.

It is important to notice that I do not control directly for the internal migration of refugees. This internal migration might itself be driven by the loss in market access, and adding internal migration might thus create a bad control problem. Instead, I control for the population share of refugees in 1950, which was predetermined at the time of division.

¹Redding and Sturm (2008) argue that the East-West German border was drawn for military purposes only. Thus, treatment and control status were assigned randomly and did not depend on any prewar characteristics of the cities that might affect population growth. Moreover, the authors argue that alternative explanations for regional growth differences, like changes in natural endowments or different institutions, can be ruled out in their setting. Additionally, Redding and Sturm (2008) provide a new multi-region NEG model based on Helpman (1998). However, this comment does not address this theoretical model.

I show that the treatment effect estimated by Redding and Sturm (2008) is nearly halved when I add the population share of refugees in 1950 to control for the internal migration pattern. Moreover, I correct the annual city-level population growth rates by accounting for all boundary changes between 1950 and 1988. I show that the treatment effect disappears completely when these boundary changes are taken into account.

2.2 Refugees in West Germany

In 1944, when the defeat of Nazi Germany was just a matter of time, the German authorities ordered the evacuation of ethnic Germans that were living in the eastern territories of the German Reich.² However, the rapid advances of the Soviet troops and the fear of acts of revenge led to an unorganized mass flight of German population to the west. After the unconditional surrender of Nazi Germany on May 8, 1945, the eastern territories of the German Reich were ceded to Poland and Russia (see Figure 2.1). However, even after the unconditional surrender the forceful displacement of ethnic Germans, especially from Polish and Czechoslovakian territories, continued. These *wild* expulsions were followed by organized and compulsory transfers that were sanctioned by the Potsdam Agreement in August 1945.

According to the 1950 census, there were about 8 million expellees (*Heimatvertriebene*) mainly from the eastern territories and further 1.5 million *immigrants* (*Zugewanderte*) from the Soviet occupation zone living in West Germany. These refugees (=expellees + *immigrants*) made up a fifth of the West German population in 1950 (Statistisches Bundesamt, 1955). The 9.5 million refugees, however, were not equally distributed over West Germany. Most refugees fled to the nearest western territories not threatened by the Red Army. Moreover, the organized transports according to the Potsdam Agreement brought refugees to reception points in the east of each receiving occupation zone (Douglas, 2012). Finally, the French zone in the south west of Germany refused to admit any refugees. Overall, therefore, refugees were concentrated close to the later East-West German border (Wild, 1979).

In the immediate aftermath of WWII the German population experienced severe food and housing shortages. The situation was even worse in those regions overly burdened with refugees. Several attempts of the authorities to rebalance the regional distribution of refugees between 1947 and 1949 failed. Refugees that tried to relocate on their own were hindered by several restrictions imposed by the Allied Occupation Forces (Müller and Simon, 1959). Although the restrictions were relaxed by 1949, the unorganized movement of refugees remained at a minor

²See Schulze (2011) for an overview on the expulsion and immigration of ethnic Germans during and after WWII. For further details see Bethlehem (1982), Connor (2007), Douglas (2012), and Ziemer (1973).

scale until 1950 (Müller and Simon, 1959; Ziemer, 1973). Thus, the unequal distribution of refugees was highly persistent and could be observed even 5 years after the war.

For 107 sample cities I have information on the population share of expellees in 1946.³ Expellees are defined as all ethnic Germans who on September 1, 1939 lived in the former German territories east of the Oder-Neisse line, the Saarland or abroad but only if their mother tongue was German. The correlation coefficient between the expellee share in 1946 and 1950 for these 107 sample cities is 0.97. The correlation coefficient between the expellee share in 1946 and the population share of refugees in 1950, which includes Germans that lived in the Soviet occupation zone or Berlin on September 1, 1939, is 0.94.⁴

Free movement was fully restored finally on May 23, 1949 when the Federal Republic of Germany was founded. In 1949, however, the housing stock of the cities was not rebuilt and the influx of population into the cities, thus, picked up slowly (Wild, 1979). Mainly male refugees of working age moved to the industrial centers of West Germany, i.e., cities in the Rhine-Ruhr area, the Rhine-Main area, and Baden-Wuerttemberg, looking for a job and better living conditions (Connor, 2007). These workers wanted to get their families to join them as soon as they could get appropriate accommodations (Pfeil, 1959).

Additionally, the government began to organize population transfers from the three federal states with the highest shares of refugees (Schleswig-Holstein, Lower Saxony, and Bavaria) to the other, more western, federal states at the end of 1949. A second program followed in 1951 and a third in 1953. All together, these programs resettled 750,000 refugees. By the end of June 1958 about 918,000 refugees had been relocated by official programs (Müller and Simon, 1959). The total amount of internal migration was considerably larger, since additional refugees moved on their own.

Figure 2.1 depicts the share of refugees in total population in 1950 and 1961 for all 119 West German sample cities. The data on the population share of refugees in 1961 are from Redding and Sturm (2008). Moreover, Figure 2.1 displays East Germany (the former Soviet occupation zone), the Oder-Neisse line, which was set as the new eastern border of Germany, and the eastern territories of the German Reich that became part of Poland and Russia after WWII. The left part of Figure 2.1 shows the population share of refugees in 1950. It ranges from 2.9 percent in Pirmasens, a city far in the west and in the French occupation zone, to 37.6 percent in Lübeck, located at the East-West German border. The cities lying within 75 kilometers of the East-West

³The data are provided in Statistisches Amt des Vereinigten Wirtschaftsgebietes (1950), Statistisches Landesamt Nordrhein-Westfalen (1949), and Statistisches Landesamt Rheinland-Pfalz (1949).

⁴The data on expellees, refugees and native population in each sample city according to the 1950 census are reported in the Statistical Yearbook of German Cities (*Statistisches Jahrbuch Deutscher Gemeinden*) published by the Deutscher Städtetag (1952, 1953) and Statistisches Landesamt Hamburg (1952).

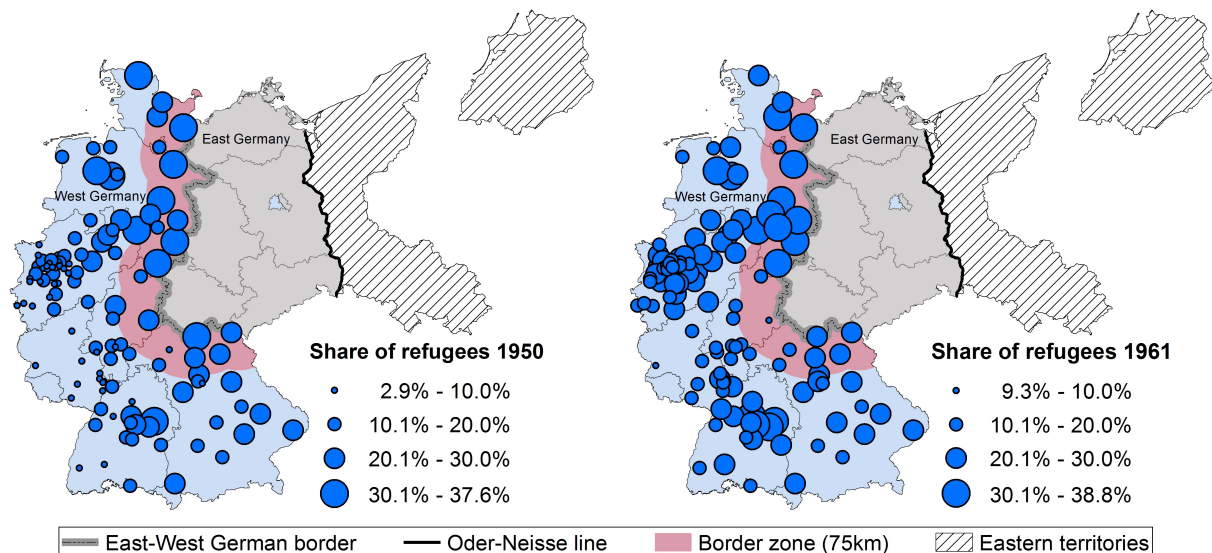


Figure 2.1: The German Reich in its 1937 borders and the distribution of refugees in sample cities in 1950 and 1961.

Sources: Redding and Sturm (2008), MPIDR and CGG (2011), and Deutscher Städtetag (1952, 1953), own calculations. Author's design.

German border had on average higher shares of refugees (25 percent) than more western cities (14 percent) in 1950. The right part of Figure 2.1 shows the population share of refugees in 1961.

The internal migration of refugees changed the structure of city population in the 1950s substantially. First, refugees left the rural areas they had initially arrived at and migrated to cities (Lendl, 1959). Consequently, the overall population share of refugees in the sample cities increased from 16 percent in 1950 to 22 percent in 1961.⁵ Second, their movement to the west reduced differences in the shares of refugees between treatment and control cities, i.e., between cities within 75 kilometers to the East-West German border and other West German cities. In 1961, the treatment cities had an average refugee share of 26 percent while control cities had reached 21 percent. Due to the relocation of the refugees, the difference between treatment and control cities in the population share of refugees more than halved between 1950 and 1961 from 11 to 5 percentage points. Figure 2.1 illustrates these changes.

Figure 2.2 illustrates the negative correlation of annual population growth of the sample cities in the 1950s with the population share of refugees in 1950. The 20 treatment cities are highlighted by circles to show their clustering at the south-east, i.e., having high shares of refugees and low population growth in the 1950s. The unconditional OLS regression line in Figure 2.2 suggests that an increase of the 1950 refugee share of one percentage point is associated with

⁵Between 1950 and 1961 another 3 million refugees from East Germany reached West Germany and located predominantly in cities (Statistisches Bundesamt, 1967). Thus, these new refugees increased the average population share of refugees additionally.

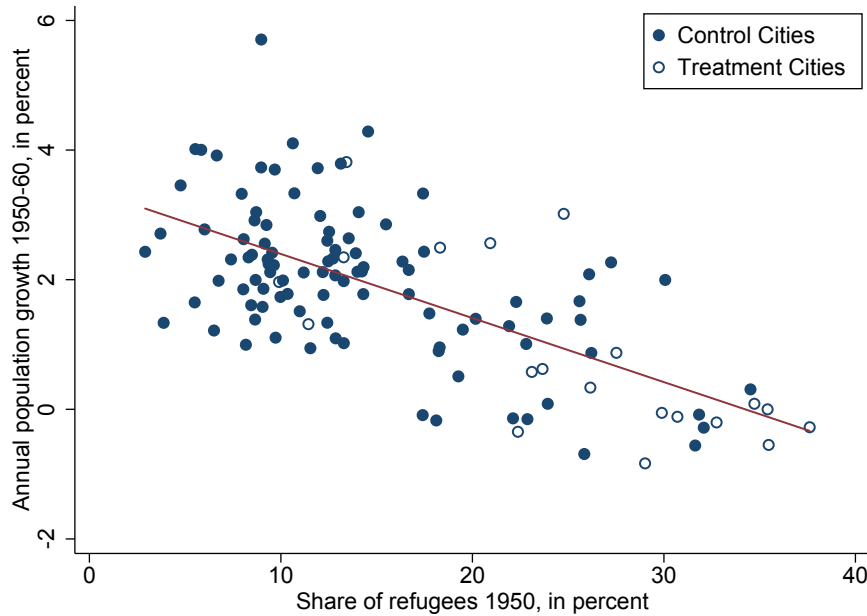


Figure 2.2: Annual population growth 1950–60 and refugee share in 1950.
Sources: Deutscher Städtetag (1952, 1953) and Redding and Sturm (2008), own calculations.

a decrease in the annual population growth between 1950 and 1960 by 0.1 percentage points. However, this result also holds when I restrict the regression to control cities only. Thus, the negative association of population growth in the 1950s and the population share of refugees is not driven by the treatment status. This simple regression provides some preliminary evidence that at least a part of the growth differences between treatment and control cities may be driven by the relocation of refugees.

2.3 Estimations and Discussion

For the empirical analysis I have recourse to the data set provided by Redding and Sturm (2008) and add data on population and refugees in 1950 provided by Deutscher Städtetag (1952, 1953).⁶ Refugees are defined as all ethnic Germans who on September 1, 1939 lived in the former German territories east of the Oder-Neisse line, the Soviet occupation zone, Berlin or the Saarland. In addition, ethnic Germans from abroad with German as their mother tongue are defined as refugees.

⁶To relate the refugee share in 1950 to the same observational units as in Redding and Sturm (2008), I aggregate the cities according to the online appendix of Redding and Sturm (2008) if necessary, i.e., if boundary changes took place in 1950 or later. Moreover, I compare the total population in 1950 of own aggregations with the population in the original data set. The mean absolute difference is about 25, with a maximum of 49 inhabitants, and can be explained by rounding differences, since the values in the original data set are rounded to the nearest full hundreds. The data for Hamburg are from Statistisches Landesamt Hamburg (1952), because erroneously only female refugees were reported in Deutscher Städtetag (1953).

The estimation strategy follows Redding and Sturm (2008) and uses a differences-in-differences model to estimate the treatment effect, i.e., the effect of division on the annual population growth of 20 West German cities within 75 kilometers to the East-West German border compared to 99 more western German cities. The underlying argument is that cities close to the border (treatment cities) lost substantial part of their market access while more western cities (control cities) were less affected. This estimation strategy is prone to contemporaneous shocks that affect treatment and control cities at a different scale. Thus, it is important to control for the inflow of refugees during and after WWII into West German cities. Redding and Sturm (2008) control for the population share of refugees in 1961. On the one hand, this measure allows the authors to include the 3 million refugees which arrived from East Germany between 1950 and 1961. On the other hand, it neglects the migration pattern in the 1950s, because regional distribution of refugees was widely rebalanced in 1961. Thus, I include the population share of refugees in 1950 as a control variable to account for this contemporaneous shock on city population. To be more precise, I do not control for the internal migration flows directly, since these flows might be affected by the changes in market access, but for the refugee share in 1950. An important feature of this control variable is that it was predetermined at the time of division, because most refugees arrived until 1946 and were hindered to relocate in the aftermath of WWII.⁷

2.3.1 Controlling for Internal Migration in the 1950s

In a first step I reproduce the estimation results reported in Table 5 in Redding and Sturm (2008, p. 1790) and additionally conduct the same estimation but with the 1950 population share of refugees as a control. The underlying estimation equation is as follows:

$$\begin{aligned} Popgrowth_{ct} = & \alpha + \beta Border_c + \gamma (Border_c \times Division_t) \\ & + \delta_t (Refugees_{ic} \times d_t) + d_t + \varepsilon_{ct}, \end{aligned} \quad (2.1)$$

where $Popgrowth_{ct}$ is the annualized population growth rate in city c for the periods $t = 1919$ – 1925 , 1925 – 1933 , 1933 – 1939 , 1950 – 1960 , 1960 – 1970 , 1970 – 1980 , and 1980 – 1988 . $Border_c$ is a binary dummy variable that is one for all cities lying within 75 kilometers to the East-West German border and zero otherwise, $Division_t$ is a dummy that is one for the postwar periods 1950 – 1960 , 1960 – 1970 , 1970 – 1980 , and 1980 – 1988 , α is the intercept, d_t is a full set of time fixed effects, and ε_{ct} is an error term, clustered at the city level. Finally, the share of $Refugees_{ic}$

⁷However, some refugees might have endogenously chosen their initial residence or relocated according to city characteristics. I assess this potential endogeneity problem in the Appendix. The robustness checks indicate that endogenous location choice of refugees should be a minor problem, if at all.

in total population in year $i = 1950, 1961$ of each city c is interacted with period dummies to capture the effect of refugees relocation on population growth in each period.

Table 2.1 presents the results in the first two columns. Column 1 contains the original results of Redding and Sturm (2008) with the refugee share in 1961 as a control. The estimated treatment effect γ is -0.678 and is statistically significant at the 1 percent level. This estimate is virtually unchanged compared to their baseline model result that does not control for refugees (-0.746 , s.e. 0.182). Consequently, the authors conclude that the overall treatment effect cannot be explained by refugees.

Column 2 shows the results of the same estimation but includes the 1950 population share of refugees. Now the estimated treatment effect is almost halved (-0.387) and not statistically significant at any standard level. Moreover, the explanatory power of the econometric model increases markedly (R-squared increases from 0.243 in column 1 to 0.281 in column 2). Interaction terms imply that an 1 percentage point increase of the refugee share in 1950 reduces annual population growth in the 1950s by 0.089 percentage points. In 1950, the population share of refugees in treatment cities was on average 11 percentage points above control cities. The average annual population growth rate in the 1950s is 0.88 percent for treatment and 2.00 percent for control cities. Thus, the internal migration of refugees in the 1950s explains a substantial part of the different development of treatment and control cities ($0.089 \times 11 = 0.979$).

In the 1970s, however, the interaction term between the refugee share and the time dummy is 0.046 and significant at the 5 percent level. This result indicates that cities with a high population share of refugees in 1950 grew faster in the 1970s. Vonyó (2012) shows that West German cities suffered from labor shortages right after the war, so these cities may have had a larger reserve of labor force during the *Wirtschaftswunder* and entered a higher growth path in the long run.

A rather surprising result is the positive correlation between refugee share and population growth in the period 1933–1939. A possible explanation is the location of refugees in former army buildings. Between 1933 and 1939, Germany rearmed and increased the number of soldiers substantially to prepare for the war. These soldiers were counted as part of the population in the censuses 1933 and 1939 and thus increased the population of cities with a military base. After the war, refugees occupied vacant army buildings, since housing was scarce. Hence, there is a possible link between population growth in the 1930s and refugees inflow after WWII. I provide empirical evidence for this link in the Appendix.

Table 2.1: Population growth – Controlling for internal migration

	Population growth			
	(1)	(2)	(3)	(4)
$Border_c \times Division_t$	-0.678 (0.211)	-0.387 (0.261)		
$Border_c \times 1950-60$			-1.249 (0.348)	0.019 (0.351)
$Border_c \times 1960-70$			-0.699 (0.283)	-0.259 (0.315)
$Border_c \times 1970-80$			-0.640 (0.355)	-1.122 (0.472)
$Border_c \times 1980-88$			-0.397 (0.147)	-0.187 (0.198)
$Refugees_{ic} \times 1919-25$	0.004 (0.020)	0.000 (0.020)		0.000 (0.020)
$Refugees_{ic} \times 1925-33$	-0.014 (0.014)	-0.018 (0.014)		-0.018 (0.014)
$Refugees_{ic} \times 1933-39$	0.068 (0.022)	0.072 (0.017)		0.072 (0.017)
$Refugees_{ic} \times 1950-60$	-0.052 (0.014)	-0.089 (0.010)		-0.098 (0.011)
$Refugees_{ic} \times 1960-70$	-0.002 (0.016)	-0.019 (0.012)		-0.022 (0.012)
$Refugees_{ic} \times 1970-80$	0.065 (0.024)	0.046 (0.018)		0.062 (0.020)
$Refugees_{ic} \times 1980-88$	0.013 (0.007)	0.003 (0.006)		-0.001 (0.005)
$Border_c$	0.029 (0.167)	-0.067 (0.213)	0.129 (0.139)	-0.067 (0.213)
Year FE	Yes	Yes	Yes	Yes
i	1961	1950	–	1950
Observations	833	833	833	833
R-squared	0.243	0.281	0.214	0.286

Notes: The dependent variable is the annualized population growth rate of each West German city c in percent. The growth periods t are 1919–1925, 1925–1933, 1933–1939, 1950–1960, 1960–1970, 1970–1980, and 1980–1988. The $Border_c$ dummy is one for all cities c within 75 km to the East-West German border and zero else. $Division_t$ is a dummy which is one for each postwar period and zero else. $Refugees_{ic}$ is the population share of refugees in each city in year i , with $i = 1950, 1961$. Thus, column 1 reports the original results of Redding and Sturm (2008) including the population share of refugees in 1961 as a control variable. In column 2, however, the refugee share in 1961 is replaced by the population share of refugees in 1950. Column 3 shows the results of a model with the $Border$ dummy interacted with each postwar period separately but without further control variables. Column 4 adds the population share of refugees in 1950 to the model of column 3. Standard errors in parentheses are adjusted for clustering on cities.

Finally, I analyse the dynamics of growth adjustment. In their *Basic Results* in Table 2, Redding and Sturm (2008, p. 1780) present the treatment effect for each postwar period separately. Column 3 of Table 2.1 shows their results. The authors find the magnitude of the treatment effect to “decline monotonically over time” (Redding and Sturm, 2008, p. 1781). They interpret this finding as a gradual adjustment of the city’s population to the new long run equilibrium after the market access shock. Does the internal migration that took place in the 1950s alter this finding?

For the estimation, I replace the term $(Border_c \times Division_t)$ in equation (2.1) by $(Border_c \times Division_t \times d_t)$. Hence, the border dummy is interacted with each postwar period separately. I present the results in column 4. According to these results, the division had no immediate impact on treatment cities. Hence, the relocation of refugees can fully explain the large growth differences between treatment and control group in the 1950s and 1960s.⁸ The only statistically significant estimate for treatment cities is found for the 1970s with -1.122. This growth difference between treatment and control cities in the 1970s drives the total effect in column 2.

2.3.2 Adjusting for Boundary Changes

The theoretical NEG literature does not provide an explanation for a delayed impact of a market access shock on population growth. Therefore, the strong and significant growth difference for the 1970s is surprising. However, sample cities experienced numerous boundary changes especially in the 1970s. In particular control cities in Baden-Wuerttemberg and North Rhine-Westphalia were affected by these boundary changes. Although Redding and Sturm (2008, Technical Appendix, p. 8) “aggregate any settlement with a population greater than 10,000 in 1919 that merges with one of [their] cities for all years in the sample”, they do not adjust the population growth rate for smaller boundary changes.⁹ Thus, the estimated treatment effect for the 1970s might be a statistical artefact of these smaller boundary changes, where municipalities with less than 10,000 inhabitants in 1919 are incorporated into sample cities. Therefore, I correct the annual population growth rate for all population changes that were induced by boundary changes between 1950 and 1988.

⁸When I estimate the same equation but with the population share of refugees in 1961, the model still estimates a significant treatment effect in all postwar periods because the internal migration in the 1950s is ignored. The estimation results are not reported to save space but can be received upon request.

⁹In the Technical Appendix, Redding and Sturm (2008) exclude cities with smaller boundary changes from the estimation and show their results to be robust in the reduced sample. However, the data set of Redding and Sturm (2008) does neglect some boundary changes. If all boundary changes are taken into account this approach is no longer feasible because most cities experienced boundary changes in the sample period, see Table A.9 in the Appendix.

Data on boundary changes are provided in Statistisches Bundesamt (1958, 1963, 1972, 1983, 1989). These sources report the population of each sample city for the latest census both with and without boundary changes. Thus, I can adjust the annual population growth rate in each postwar growth period for boundary changes. Between 1950 and 1988 there are 155 growth periods with smaller boundary changes in the sample cities, 93 of them in the 1970s. Table A.9 in the Appendix gives an exhaustive overview of all boundary changes.

In Table 2.2 I estimate the baseline differences-in-differences model of Redding and Sturm (2008) with and without adjusting for boundary changes and conduct the same estimations as in the previous section but with the adjusted population growth rate as dependent variable. Column 1 of Table 2.2 shows the original baseline differences-in-differences results of Redding and Sturm (2008), without adjusting for smaller boundary changes. The estimated treatment effect is -0.746. Controlling for all boundary changes in column 2 almost doubles the R-squared and reduces the estimated treatment effect to -0.631.

Columns 3 to 6 present the same results as in Table 2.1 but for the adjusted population growth rate as dependent variable. There are three main differences in the estimation results. First, the explanatory power measured by the R-squared increases in all four specifications compared to the results in Table 2.1. Second, the absolute treatment effect in column 3 (column 4) decreases to -0.483 (-0.145) compared to -0.678 (-0.387) in column 1 (column 2) of Table 2.1. Finally, the interaction term of the treatment dummy with 1970–1980 in columns 5 and 6 is lower in absolute terms. When I take the boundary changes into account the estimated growth difference between treatment and control cities for the 1970s decreases from -1.122 (s.e. 0.472) to -0.246 (s.e. 0.253) in column 6 of Table 2.2.

These results indicate that the boundary changes increase the measurement error in the dependent variable and bias the treatment effect upwards. Furthermore, boundary changes in the 1970s can explain the large growth differences between treatment and control cities in this period. Most importantly, controlling for the refugee share in 1950 and adjusting for boundary changes reduces the baseline treatment effect from -0.746 in column 1 to -0.145 in column 4. Thus, there is no statistically significant growth difference between treatment and control cities once the internal migration of refugees and smaller boundary changes are taken into account.

2.4 Conclusion

The empirical results provide strong evidence that the treatment effect estimated by Redding and Sturm (2008) primarily captures a contemporaneous shock in refugee share, which caused internal migration of refugees in the 1950s, rather than a shock in market access. There is no

Table 2.2: Population growth – Adjusting for boundary changes

	Population growth					
	(1)	(2)	(3)	(4)	(5)	(6)
$Border_c \times Division_t$	-0.746 (0.182)	-0.623 (0.170)	-0.483 (0.192)	-0.145 (0.217)		
$Border_c \times 1950-60$					-1.244 (0.348)	0.058 (0.342)
$Border_c \times 1960-70$					-0.572 (0.188)	-0.206 (0.227)
$Border_c \times 1970-80$					-0.278 (0.192)	-0.246 (0.253)
$Border_c \times 1980-88$					-0.397 (0.147)	-0.186 (0.199)
$Refugees_{ic} \times 1919-25$			0.004 (0.020)	0.000 (0.020)		0.000 (0.020)
$Refugees_{ic} \times 1925-33$			-0.014 (0.014)	-0.018 (0.014)		-0.018 (0.014)
$Refugees_{ic} \times 1933-39$			0.068 (0.022)	0.072 (0.017)		0.072 (0.017)
$Refugees_{ic} \times 1950-60$			-0.059 (0.014)	-0.096 (0.009)		-0.101 (0.011)
$Refugees_{ic} \times 1960-70$			-0.000 (0.010)	-0.017 (0.008)		-0.016 (0.008)
$Refugees_{ic} \times 1970-80$			0.019 (0.008)	0.013 (0.007)		0.015 (0.007)
$Refugees_{ic} \times 1980-88$			0.010 (0.007)	-0.002 (0.006)		-0.001 (0.005)
$Border_c$	0.129 (0.139)	0.129 (0.139)	0.029 (0.167)	-0.067 (0.213)	0.129 (0.139)	-0.067 (0.213)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
i	–	–	1961	1950	–	1950
Boundary changes	No	Yes	Yes	Yes	Yes	Yes
Observations	833	833	833	833	833	833
R-squared	0.211	0.379	0.406	0.455	0.385	0.455

Notes: In column 1 the dependent variable is the annualized population growth rate of each West German city c in percent. In columns 2 to 6, the annualized population growth rate is adjusted for boundary changes between 1950 and 1988. Growth periods t are 1919–1925, 1925–1933, 1933–1939, 1950–1960, 1960–1970, 1970–1980, and 1980–1988. The $Border_c$ dummy is one for all cities c within 75 km to the East-West German border and zero else. $Division_t$ is a dummy which is one for each postwar period and zero else. $Refugees_{ic}$ is the population share of refugees in each city in year i , with $i = 1950, 1961$. Thus, column 1 reports the baseline differences-in-differences results of Redding and Sturm (2008). Column 2 shows the same results but with the adjusted annualized population growth rate as dependent variable. Column 3 includes the population share of refugees in 1961 as a control variable. In column 4, however, the refugee share in 1961 is replaced by the population share of refugees in 1950. Column 5 shows the results of a model with the $Border$ dummy interacted with each postwar period separately but without further control variables. Column 6 adds the population share of refugees in 1950 to the model of column 5. Standard errors in parentheses are adjusted for clustering on cities.

statistically significant treatment effect when I take this contemporaneous shock into account. Moreover, when I adjust the population growth rate for smaller boundary changes the difference between treatment and control cities disappears completely.

Appendix A

The Costs of Remoteness: Comment

A.1 Robustness Checks

Although refugees could hardly choose their preferred destination and were hindered to relocate in the aftermath of WWII, an endogenous location choice of some refugees and relocations can not be ruled out completely. Some refugees might have chosen specific destination cities according to (unobserved) characteristics that in turn affect city growth. Therefore, the previous estimates that include the population share of refugees in 1950 as a control variable might suffer from an endogeneity bias.

In the immediate aftermath of WWII refugees preferred cities with appropriate supply of food and housing. Therefore, they were looking for smaller, more rural cities with less destruction. Moreover, these were cities with relatively small industrial sectors and unfavorable growth perspectives. After this initial phase, refugees tried to move to more urban centers with larger labor markets and better growth perspectives. Whether the previous results are upward or downward biased depends on the influence of these two factors on the distribution of refugees in 1950.

To assess the potential endogeneity problem, I replace the population share of refugees in 1950 with the population share of expellees in 1950 in Section A.1.1 and employ an instrumental variable (IV) approach in Section A.1.2. Both approaches provide evidence that the potential endogeneity bias should be a minor problem, if at all. In Section A.1.3, I show that the rearmament of Germany between 1933 and 1939 can fully explain the correlation between prewar population growth and refugee share in the aftermath of WWII. Finally, I allow the instrumental variable of Section A.1.2 to compete directly against the distance to the East-West German border in Section A.1.4. This regression provides suggestive evidence, that the growth differences

between treatment and control cities are driven by the internal migration of refugees rather than the loss of market access.

A.1.1 Population Share of Expellees

The first approach to take the potential endogeneity bias into account is to restrict the following analysis to expellees. Expellees are defined as ethnic Germans from the former eastern territories of the German Reich and from abroad, but only if their mother tongue was German. Hence, *immigrants* from the Soviet occupation zone or Berlin are excluded because they were less often forcefully displaced and could more often decide on their emigration to West Germany. Therefore, the initial location choice of *immigrants* is more likely driven by destination characteristics that foster postwar growth.

I conduct the same estimations as in Sections 2.3.1 and 2.3.2, but replace the population share of refugees in 1950 by the population share of expellees in 1950. The results are presented in Table A.1. In columns 1 and 2, the dependent variable is the annualized population growth rate, while in columns 3 and 4, the growth rate is adjusted for boundary changes that took place between 1950 and 1988.

The main findings are not affected by focusing on expellees rather than refugees more specifically. The baseline treatment effect reduces from -0.746 to -0.413 (s.e. 0.236) in column 1 and to -0.212 (s.e. 0.197) in column 3. These estimates are very similar to previous results. In columns 2 and 4, I interact the treatment dummy with each post treatment period separately. The results do not differ much from the corresponding results in Tables 2.1 and 2.2. There is no statistically significant treatment effect in the postwar periods, except for the 1970s in column 2. This effect, however, can be explained by boundary changes. Furthermore, in the 1950s, the interaction term between the expellee share and the time dummy is lower compared to previous estimations. This finding is in line with the self selection of East German *immigrants* into cities with favorable growth perspectives.

A.1.2 IV Regression

However, also expellees might have chosen their destination according to unobserved city characteristics. To control for the endogenous location decision of expellees, I follow Braun and Kvasnicka (2014) and use an instrumental variable (IV) approach. During the phase of flight and expulsion expellees fled mainly to the nearest West German region not threatened by the Red Army, i.e., regions close to their old homelands. The IV approach relies on this fact and instruments the population share of expellees in a given sample city by the weighted sum of

Table A.1: Robustness – Population share of expellees in 1950

	Population growth			
	Not Adjusted		Adjusted	
	(1)	(2)	(3)	(4)
$Border_c \times Division_t$	-0.413 (0.236)		-0.212 (0.197)	
$Border_c \times 1950-60$		-0.176 (0.319)		-0.138 (0.307)
$Border_c \times 1960-70$		-0.306 (0.318)		-0.260 (0.216)
$Border_c \times 1970-80$		-0.924 (0.434)		-0.204 (0.238)
$Border_c \times 1980-88$		-0.247 (0.188)		-0.247 (0.188)
$Expellees_{50c} \times 1919-25$	0.007 (0.024)	0.007 (0.024)	0.007 (0.024)	0.007 (0.024)
$Expellees_{50c} \times 1925-33$	-0.024 (0.016)	-0.024 (0.016)	-0.024 (0.016)	-0.024 (0.016)
$Expellees_{50c} \times 1933-39$	0.083 (0.020)	0.083 (0.020)	0.083 (0.020)	0.083 (0.020)
$Expellees_{50c} \times 1950-60$	-0.111 (0.012)	-0.116 (0.014)	-0.119 (0.011)	-0.120 (0.013)
$Expellees_{50c} \times 1960-70$	-0.026 (0.013)	-0.029 (0.013)	-0.019 (0.009)	-0.018 (0.009)
$Expellees_{50c} \times 1970-80$	0.047 (0.021)	0.058 (0.023)	0.013 (0.008)	0.012 (0.008)
$Expellees_{50c} \times 1980-88$	0.006 (0.006)	0.003 (0.006)	0.002 (0.006)	0.003 (0.006)
$Border_c$	-0.041 (0.194)	-0.041 (0.194)	-0.041 (0.194)	-0.041 (0.194)
Year FE	Yes	Yes	Yes	Yes
Observations	833	833	833	833
R-squared	0.281	0.284	0.458	0.458

Notes: The dependent variable is the annualized population growth rate of each West German city c in percent. In columns 3 and 4, the annualized population growth rate is adjusted for boundary changes between 1950 and 1988. Growth periods t are 1919–1925, 1925–1933, 1933–1939, 1950–1960, 1960–1970, 1970–1980, and 1980–1988. The $Border_c$ dummy is one for all cities c within 75 km to the East-West German border and zero else. $Division_t$ is a dummy which is one for each postwar period and zero else. $Expellees_{50c}$ is the population share of expellees in each city in 1950. In columns 1 and 3 the population share of expellees in 1950 is included as a control variable. In columns 2 and 4, however, the $Border$ dummy is interacted with each postwar period separately. Thus, columns 1 and 2 correspond to columns 2 and 4 in Table 2.1. Columns 3 and 4 correspond to columns 4 and 6 in Table 2.2. Standard errors in parentheses are adjusted for clustering on cities.

distances to the origin regions of expellees. The weight is defined as the population of the origin region divided by the total population of all origin regions. Hence, the instrument captures the variation in the population share of expellees driven by the geographical distance to the old homelands. The instrument Z_c is defined as:

$$Z_c = \sum_s \left(Distance_{cs} \times \frac{Population_{39s}}{\sum_s Population_{39s}} \right), \quad (\text{A.1})$$

where $Distance_{cs}$ is the great circle distance between the administrative capital of a origin region of expellees s and a sample city c , and $Population_{39s}$ is the population of a origin region in 1939.¹

The identifying assumption of the IV regression is $Cov(Z_c, \varepsilon_{ct}) = 0$. This assumption requires two things. First, conditional on covariates, there is no unobserved factor that drives Z_c and population growth. Second, the instrument affects the population growth only through its effect on the expellee share.

Due to the structure of the model and because the endogenous regressor as well as the excluded instrument Z_c are time invariant, it is possible to reduce the first stage regression to a single cross section. The estimation equation for the first stage regression is as follows:

$$Expellees_{50c} = \alpha' + \beta' Z_c + \gamma' Border_c + \varepsilon'_c, \quad (\text{A.2})$$

where $Expellees_{50c}$ is the population share of expellees in each sample city c in 1950, Z_c is the instrumental variable defined in equation (A.1), $Border_c$ is a binary dummy variable that is one for all cities located within 75 kilometers to the East-West German border and zero otherwise, and ε'_c is a heteroscedasticity robust error term.

Table A.2 shows the results of the first stage regression. The instrument is statistically significant and the partial F-Statistic of the excluded instrument is above the standard threshold of 10, indicating no weak instrument problem. Moreover, a large fraction of the variance in the expellee share is explained by the first stage (R-squared = 0.45) and the treatment status has no statistically significant influence on the population share of expellees ($\gamma' = -1.158$, s.e. 1.894).

For the IV estimation I use the same differences-in-differences model as before, but instrument the population share of expellees in 1950 with the population weighted distance to origin regions of expellees Z_c . I employ the feasible efficient two step generalized method of moments estimator with errors clustered at the city level implemented by Stata command `ivreg2` (Baum et al., 2007).

¹The origin regions are the districts Königsberg, Gumbinnen and Allenstein in East Prussia, Breslau, Liegnitz and Oppeln in Silesia, Stettin and Köslin in Pommerania, Frankfurt, Danzig, Memel Territory, and the Sudetenland.

Table A.2: IV Regression – First stage

	Z_c	Border $_c$	Observations	R-squared	F-Statistic, excl. instr.
Expellees $_{50c}$	-0.058 (0.006)	-1.158 (1.894)	119	0.453	82.41

Notes: The dependent variable is the population share of expellees in each West German city c in 1950. Z_c is the instrumental variable defined in equation (A.1). The $Border_c$ dummy is one for all cities c within 75 km to the East-West German border and zero else. Heteroscedasticity robust errors in parentheses.

Table A.3 presents the estimation results. In columns 1 and 2, the dependent variable is the annualized population growth rate, while in columns 3 and 4 the growth rate is adjusted for boundary changes. The results in Table A.3 thus correspond to the results in Table A.1.

In all four specifications the treatment effect is not statistically significant and very close to the results in Tables 2.1 and 2.2. The estimation coefficient on the interaction of the instrumented expellee share and the 1950s is lower than in Table A.1. This difference indicates an endogenous relocation of expellees towards cities with better growth perspectives, the difference, however, is not statistically significant.

A.1.3 Rearmament of Germany

In Table A.3, there is a significant correlation between the instrumented population share of expellees and prewar growth. A possible explanation for this correlation is the location of expellees in former army buildings. Between 1933 and 1939 Nazi Germany increased the number of military personnel substantially to prepare for the war. These soldiers were counted as part of the population in the censuses 1933 and 1939 and thus increased the population of cities with a military base. After the war, refugees occupied vacant army buildings, since housing was scarce.

To test whether the correlation between prewar growth and the expellee inflow is driven by the rearmament of Germany or not, I add the public sector employment share in 1939 as a control variable. In the occupational census in 1939, the military personnel was counted as part of the public sector. Therefore, the employment share of the public sector in total employment provides information on the size of the military personnel. More precisely, I use the variable *Public1939* in the data set of Redding and Sturm (2008), which comprises not the whole public sector but a single occupational group. This occupational group includes only public administration, justice, and military personnel. Thus, large groups in the public sector like teachers or medical workers are excluded. The variable is a good proxy for military personnel, if the personnel in administration and justice is almost proportional to the size of total employment in each city

Table A.3: Robustness – IV Regression Using Distance to Origin of Refugees

	Population growth			
	Not Adjusted		Adjusted	
	(1)	(2)	(3)	(4)
Border _c × Division _t	-0.343 (0.275)		-0.168 (0.226)	
Border _c × 1950–60		-0.009 (0.331)		0.007 (0.329)
Border _c × 1960–70		-0.194 (0.362)		-0.270 (0.249)
Border _c × 1970–80		-1.008 (0.558)		-0.248 (0.259)
Border _c × 1980–88		-0.161 (0.241)		-0.161 (0.241)
Expellees'_{50c} × 1919–25	-0.010 (0.027)	-0.010 (0.027)	-0.010 (0.027)	-0.010 (0.027)
Expellees'_{50c} × 1925–33	-0.051 (0.020)	-0.051 (0.020)	-0.051 (0.020)	-0.051 (0.020)
Expellees'_{50c} × 1933–39	0.110 (0.028)	0.110 (0.028)	0.110 (0.028)	0.110 (0.028)
Expellees'_{50c} × 1950–60	-0.125 (0.020)	-0.143 (0.022)	-0.135 (0.019)	-0.145 (0.022)
Expellees'_{50c} × 1960–70	-0.041 (0.026)	-0.049 (0.030)	-0.028 (0.017)	-0.022 (0.019)
Expellees'_{50c} × 1970–80	0.028 (0.034)	0.064 (0.045)	0.008 (0.012)	0.013 (0.013)
Expellees'_{50c} × 1980–88	-0.004 (0.015)	-0.014 (0.013)	-0.013 (0.012)	-0.014 (0.013)
Border _c	0.001 (0.199)	0.001 (0.199)	0.001 (0.199)	0.001 (0.199)
Year FE	Yes	Yes	Yes	Yes
Observations	833	833	833	833
R-squared	0.273	0.275	0.449	0.449

Notes: The dependent variable is the annualized population growth rate of each West German city c in percent. In columns 3 and 4, the annualized population growth rate is adjusted for boundary changes between 1950 and 1988. Growth periods t are 1919–1925, 1925–1933, 1933–1939, 1950–1960, 1960–1970, 1970–1980, and 1980–1988. The $Border_c$ dummy is one for all cities c within 75 km to the East-West German border and zero else. $Division_t$ is a dummy which is one for each postwar period and zero else. $Expellees'_{50c}$ are the fitted values of the first stage regression for the population share of expellees in each city in 1950. Standard errors in parentheses are adjusted for clustering on cities.

and therefore the variation in the public sector employment share is mostly driven by the size of the military personnel.

The public sector employment share in 1939 ranges from 0.9 to 14.7 percent with a median of 4.9 percent. Among the 10 cities with the largest public sector employment share are three large harbours of the German Navy and the other 7 cities had a military base as well (Stahl, 1954). Among the 10 cities with the lowest share, however, there are 5 cities that have not even a single duty station. The correlation coefficient between the public sector employment share and the expellee share in 1950 is 0.49. Unfortunately there are no data on public sector employment available for two control cities (Bergisch-Gladbach and Villingen-Schwenningen). However, all previous results are robust to the exclusion of these two cities.

For the first stage regression I include the public sector employment share of 1939 in equation (A.2). Table A.4 shows the results of the first stage regression. The estimator of the population weighted distance (-0.049, s.e. 0.007) is slightly below the estimator in Table A.2 but still statistically significant. The partial F-Statistic for the instrument is 45 and indicates no weak instrument problem. The estimator for the public sector employment share (0.534, s.e. 0.193) is statistically significant and has the expected positive sign. The estimator indicates, that cities with a higher public sector employment share in 1939 have a higher population share of expellees in 1950, which is in line with the location of expellees in former army buildings. The treatment status has no statistically significant influence on the population share of expellees ($\gamma' = -0.718$, s.e. 1.825).

Table A.4: IV Regression – First stage – Public Sector Employment Share

	Z_c	Public1939 $_c$	Border $_c$	Observations	R-squared	F-Statistic, excl. instr.
Expellees $_{50c}$	-0.049 (0.007)	0.534 (0.193)	-0.718 (1.825)	117	0.488	45.00

Notes: The dependent variable is the population share of expellees in each West German city c in 1950. Z_c is the instrumental variable defined in equation (A.1). The $Border_c$ dummy is one for all cities c within 75 km to the East-West German border and zero else. $Public_{39c}$ is the employment share of the public sector in each city in 1939. Heteroscedasticity robust errors in parentheses.

In the econometric model for the IV regression I interact the public sector employment share with a full set of time dummies. Table A.5 presents the results of the IV regression. In column 1, the dependent variable is the annualized population growth rate, while the growth rate is adjusted for boundary changes in column 2. The treatment dummies are not statistically significant and almost identical to previous estimations. The main difference is the interaction term of the instrumented expellee share and the 1930s time dummy. Compared to previous results, the estimator reduces from 0.110 (s.e. 0.028) in Table A.3 to 0.022 (s.e. 0.035) and is not

statistically significant. The interaction of the public sector employment share with the prewar period is 0.315 (s.e. 0.068). This estimator implies that an 1 percentage point increase in the public sector employment share increases the annual population growth between 1933 and 1933 by 0.315 percentage points. The average annual population growth rate in the 1930s is 1.74. These results suggest, that the rearmament of Germany can explain the correlation between the postwar inflow of expellees and prewar growth.

Table A.5: Robustness – IV Regression with Public Sector Employment Share

	Population growth			
	Not Adjusted		Adjusted	
	(1)		(2)	
$Border_c \times Division_t$	-0.351	(0.283)	-0.186	(0.236)
$Expellees'_{50c} \times 1919-25$	0.020	(0.036)	0.020	(0.036)
$Expellees'_{50c} \times 1925-33$	-0.049	(0.028)	-0.049	(0.028)
$Expellees'_{50c} \times 1933-39$	0.022	(0.035)	0.022	(0.035)
$Expellees'_{50c} \times 1950-60$	-0.140	(0.030)	-0.150	(0.028)
$Expellees'_{50c} \times 1960-70$	-0.056	(0.028)	-0.033	(0.020)
$Expellees'_{50c} \times 1970-80$	0.004	(0.046)	-0.005	(0.016)
$Expellees'_{50c} \times 1980-88$	-0.007	(0.019)	-0.018	(0.016)
$Public1939_c \times 1919-25$	-0.108	(0.067)	-0.108	(0.067)
$Public1939_c \times 1925-33$	-0.022	(0.049)	-0.022	(0.049)
$Public1939_c \times 1933-39$	0.315	(0.068)	0.315	(0.068)
$Public1939_c \times 1950-60$	0.060	(0.056)	0.063	(0.056)
$Public1939_c \times 1960-70$	0.065	(0.039)	0.034	(0.029)
$Public1939_c \times 1970-80$	0.081	(0.079)	0.049	(0.026)
$Public1939_c \times 1980-88$	0.004	(0.025)	0.012	(0.025)
$Border_c$	0.031	(0.211)	0.031	(0.211)
Year FE	Yes		Yes	
Observations	819		819	
R-squared	0.320		0.508	

Notes: The dependent variable is the annualized population growth rate of each West German city c in percent. In column 2, the annualized population growth rate is adjusted for boundary changes between 1950 and 1988. Growth periods t are 1919–1925, 1925–1933, 1933–1939, 1950–1960, 1960–1970, 1970–1980, and 1980–1988. The $Border_c$ dummy is one for all cities c within 75 km to the East-West German border and zero else. $Division_t$ is a dummy which is one for each postwar period and zero else. $Expellees'_{50c}$ are the fitted values of the first stage regression for the population share of expellees in each city in 1950. $Public1939_c$ is the employment share of the public sector in each city in 1939. Standard errors in parentheses are adjusted for clustering on cities.

However, the assumption of the staff in administration and justice to be almost proportional to total employment might not hold in case of an administrative center. Therefore, I replace the public sector employment share in 1939 by the population share living in collective accommodation in 1939. The data are from the census in 1939 (Statistisches Reichsamtsamt, 1944). Collective accommodation (*Anstaltshaushaltungen*) includes all forms of households where people

live together voluntary and non-voluntary, e.g. military bases, ships, youth centers, dormitories, nursing homes, and also concentration camps. The advantage of the collective accommodation share is that expellees were accommodated in all of these facilities after the war.

The variation in the collective accommodation share, however, is driven by the variation in the military personnel. Hence, the collective accommodation share and the public sector employment share in 1939 are strongly correlated, with a correlation coefficient of 0.85. The correlation coefficient between the collective accommodation share in 1939 and the population share of expellees in 1950 is 0.50. The results of the first stage regression are in Table A.6 and the IV regression results are in Table A.7. However, when I include the collective accommodation as control variable the results are virtually unchanged.

Table A.6: IV Regression – First stage – Collective Accommodation Share

	Z_c	$Collective_{39c}$	$Border_c$	Observations	R-squared	F-Statistic, excl. instr.
Expellees _{50c}	-0.047 (0.007)	0.414 (0.116)	-0.339 (1.843)	119	0.511	40.79

Notes: The dependent variable is the population share of expellees in each West German city c in 1950. Z_c is the instrumental variable defined in equation (A.1). The $Border_c$ dummy is one for all cities c within 75 km to the East-West German border and zero else. $Collective_{39c}$ is the population share living in collective accommodation in 1939. Heteroscedasticity robust errors in parentheses.

A.1.4 Validity of Exclusion Restriction

The small differences between the IV regression results in the Appendix and the results in the main text, provide suggestive evidence that the potential endogeneity bias of the treatment effect should be a minor problem, if at all. However, I cannot directly test the exclusion restriction, i.e., that the instrument affects the population growth only through its effect on the expellee share.

The main potential problem is the correlation between the distance to the East-West German border and the population weighted distance to the origin region of expellees Z_c (correlation coefficient = 0.87). Hence, the instrument might captures a part of the growth effect due to the loss in market access. Therefore, I allow the instrument to compete directly against the distance to the East-West German border. While the instrument captures more of the variation in the expellee share the latter captures lost market access, especially when I assume that the treatment effect is of limited range, for example 75 kilometers. The correlation between the population share of expellees in 1950 and the distance to the East-West German border is -0.50, whereas the correlation with the population weighted distance is -0.67.

Table A.7: Robustness – IV Regression with Collective Accommodation Share

	Population growth			
	Not Adjusted		Adjusted	
	(1)		(2)	
Border _c × Division _t	-0.313	(0.289)	-0.171	(0.242)
Expellees' _{50c} × 1919–25	0.013	(0.034)	0.013	(0.034)
Expellees' _{50c} × 1925–33	-0.058	(0.028)	-0.058	(0.028)
Expellees' _{50c} × 1933–39	0.036	(0.033)	0.036	(0.033)
Expellees' _{50c} × 1950–60	-0.150	(0.030)	-0.157	(0.028)
Expellees' _{50c} × 1960–70	-0.075	(0.030)	-0.045	(0.022)
Expellees' _{50c} × 1970–80	-0.021	(0.046)	-0.015	(0.017)
Expellees' _{50c} × 1980–88	-0.016	(0.019)	-0.025	(0.016)
Collective _{39c} × 1919–25	-0.064	(0.037)	-0.064	(0.037)
Collective _{39c} × 1925–33	0.009	(0.032)	0.009	(0.032)
Collective _{39c} × 1933–39	0.178	(0.040)	0.178	(0.040)
Collective _{39c} × 1950–60	0.049	(0.034)	0.047	(0.033)
Collective _{39c} × 1960–70	0.073	(0.026)	0.036	(0.021)
Collective _{39c} × 1970–80	0.110	(0.049)	0.051	(0.017)
Collective _{39c} × 1980–88	0.017	(0.015)	0.022	(0.015)
Border _c	0.060	(0.214)	0.060	(0.214)
Year FE	Yes		Yes	
Observations	833		833	
R-squared	0.320		0.500	

Notes: The dependent variable is the annualized population growth rate of each West German city c in percent. In column 2, the annualized population growth rate is adjusted for boundary changes between 1950 and 1988. Growth periods t are 1919–1925, 1925–1933, 1933–1939, 1950–1960, 1960–1970, 1970–1980, and 1980–1988. The $Border_c$ dummy is one for all cities c within 75 km to the East-West German border and zero else. $Division_t$ is a dummy which is one for each postwar period and zero else. $Expellees'_{50c}$ are the fitted values of the first stage regression for the population share of expellees in each city in 1950. $Collective_{39c}$ is the population share living in collective accommodation in 1939. Standard errors in parentheses are adjusted for clustering on cities.

The estimation equation is as follows:

$$Popgrowth_{ct} = \alpha^\dagger + \beta_t^\dagger (BorderDistance_c \times d_t) + \gamma_t^\dagger (Z_c \times d_t) + \varepsilon_c^\dagger, \quad (\text{A.3})$$

where $Popgrowth_{ct}$ is the annualized population growth rate in city c in time period t . Z_c is the population weighted distance to the origin regions of expellees and $BorderDistance_c$ is the distance to the East-West German border in kilometers. The variable d_t represents a full set of time fixed effects and ε_c^\dagger is an error term, clustered at the city level.

Table A.8 presents the estimation results. In column 2, the population growth rate is adjusted for boundary changes between 1950 and 1988. However, the results in columns 1 and 2 are virtually identical. The estimator of $\gamma_{1950-60}^\dagger$ is 0.012 and significant at the one percent level. Thus, cities further away from the sending districts experienced a larger population growth in the 1950s. This finding is in line with the internal migration of refugees to the west. However, the estimator of $\beta_{1950-60}^\dagger$ is -0.005 (s.e. 0.002) and indicates a negative association between distance to the East-West German border and the population growth in the 1950s. The estimator of $\beta_{1950-60}^\dagger$ contradicts the supposed negative effect of the market access shock on cities close to the East-West German border. The horse race produces a clear winner and provides suggestive evidence that the treatment effect estimated by Redding and Sturm (2008) is driven by the relocation of refugees after WWII, rather than the loss of market access.

Table A.8: Robustness – Horse Race

	Population growth			
	Not Adjusted		Adjusted	
	(1)		(2)	
Border Distance _c × 1919–25	0.005	(0.004)	0.005	(0.004)
Border Distance _c × 1925–33	-0.002	(0.002)	-0.002	(0.002)
Border Distance _c × 1933–39	-0.005	(0.004)	-0.005	(0.004)
Border Distance _c × 1950–60	-0.005	(0.002)	-0.005	(0.002)
Border Distance _c × 1960–70	-0.000	(0.002)	0.002	(0.002)
Border Distance _c × 1970–80	0.006	(0.005)	0.001	(0.001)
Border Distance _c × 1980–88	0.002	(0.001)	0.002	(0.001)
Z _c × 1919–25	-0.003	(0.003)	-0.003	(0.003)
Z _c × 1925–33	0.004	(0.002)	0.004	(0.002)
Z _c × 1933–39	-0.003	(0.003)	-0.003	(0.003)
Z _c × 1950–60	0.012	(0.002)	0.012	(0.002)
Z _c × 1960–70	0.003	(0.002)	0.000	(0.002)
Z _c × 1970–80	-0.005	(0.004)	-0.001	(0.001)
Z _c × 1980–88	-0.000	(0.001)	-0.000	(0.001)
Year FE	Yes		Yes	
Observations	833		833	
R-squared	0.263		0.440	

Notes: The dependent variable is the annualized population growth rate of each West German city c in percent. In column 2, the annualized population growth rate is adjusted for boundary changes between 1950 and 1988. Growth periods t are 1919–1925, 1925–1933, 1933–1939, 1950–1960, 1960–1970, 1970–1980, and 1980–1988. The $BorderDistance_c$ is the distance of each city c to the East-West German border in kilometers. Z_c is the population weighted distance to the origin regions of expellees. Standard errors in parentheses are adjusted for clustering on cities.

A.2 Boundary Changes 1950–1988

Table A.9: Boundary Changes Between 1950 and 1988

City	Year	Total	Absorbed (parts of) (+) / Exclusion to (-)
Aachen	1958	339	Belgium (+339)
	1972	57011	Eilendorf (+13171), Walheim (+7717), Laurensberg (+9749), Brand (+10714), Broichweiden (+17), Haaren (+8162), Kornelimünster (+2645), Richterich (+4707), Stolberg (+124), Würselen (+5)
Amberg	1972	6755	Ammersricht (+1923), Gailoh (+1386), Karmensölden (+1049), Raigering (+1104), Kümmersbruck (+186), Traßberg (+1107)
Ansbach	1950	721	Bernhardswinden (+237), unincorporated areas (+484)
	1960	49	Neuses (+49)
	1961	109	Hennenbach (+106), Schalkenhausen (+3)
	1962	11	unincorporated areas (+11)
	1963	10	Hennenbach (+10)
	1970	2667	Eyb (+2667)
	1972	7088	Bernhardswinden (+610), Brodswinden (+630), Elpersdorf (+1121), Hennenbach (+2208), Neuses (+957), Schalkhausen (+1214), Claffheim (+348)
Aschaffenburg	1975	1430	Gailbach (+1430)
	1978	3215	Obernau (+3186), Großostheim (+29)
Augsburg	1951	-6	Stadtbergen (-6)
	1960	7	Haunstetten (+7)
	1972	42134	Göggingen (+15980), Haunstetten (+21810), Inningen (+4344)
	1978	523	Stätzling (+523)
	1979	11	Gersthofen (+11)
Bad Kreuznach	1969	4166	Winzenheim (+1196), Bosenheim (+954), Ippenheim (+265), Planing (+1751)
Baden-Baden	1972	8202	Ebersteinburg (+1078), Neuweiher (+2121), Steinbach (+3273), Varnhalt (+1730)
	1974	2814	Haueneberstein (+2814)
	1975	2992	Sandweier (+2992)
Bamberg	1952	6	unincorporated areas (+6)
	1956	35	unincorporated areas (+35)
	1960	42	unincorporated areas (+42)
	1963	91	Memmeldorf (+91)
	1970	258	Hallstadt (+258)
	1972	6868	Bug (+766), Gaustadt (+5507), Wildensorg (+432), Gundelsheim (+119), Strullendorf (+44)
	1980	4	Bischberg (+4)
Bayreuth	1961	6	unincorporated areas (+6)
	1972	2610	Oberkonnersreuth (+541), Laineck (+2050), Thiergarten (+19)
	1976	1933	Aichig (+660), Oberpreuschwitz (+641), Seulbitz (+232), Thiergarten (+400)
	1978	133	Wolfsbach (+133)
Bergisch-Gladbach	1975	3154	Odenthal (+3154)
	1978	-15	Overath (-15)
Bielefeld	1961	875	Babenhhausen (+108), Brake (+767)

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Table A.9 – continued from previous page

City	Year	Total	Absorbed (parts of) (+) / Exclusion to (-)
	1973	145423	Altenhagen (+4072), Babenhausen (+2392), Brackwede (+39856), Brake (+6738), Brönninghausen (+940), Gadderbaum (+8498), Großdornberg (+2216), Heepen (+9351), Hillegossen (+4323), Niederdornberg-Deppendorf (+1232), Oldentrup (+2800), Jöllenbeck (+9319), Kirchdornberg (+1023), Lämershagen-Gräfinhagen (+1085), Milse (+3111), Sennestadt (+20187), Theesen (+2227), Ubbedissen (+3453), Vilsendorf (+2013), Hoberge-Uerentrup (+2413), Senne I (+17140), Häger (+47), Schröttinghausen (+871), Steinhagen (+116)
	1977	5	Leopoldshöhe (+5)
	1982	-105	Spenge (-97), Werther (-8)
Bocholt	1975	15754	Barlo (+1540), Biemenhorst (+2588), Hemden (+904), Holtwick (+961), Liedern (+950), Lowick (+1943), Mussum (+2624), Spork (+904), Stenern (+1705), Suderwick (+1353), Dingden (+282)
Bonn	1969	61905	Buschdorf (+657), Duisdorf(+10215), Ippendorf (+3515), Lengsdorf (+3990), Lessenich (+1522), Röttingen (+2192), Beuel (+31550), Oberkassel (5593), Holzlar (+2282), Stieldorf (+389)
Bottrop	1976	15285	Kirchhellen (15285)
Braunschweig	1959	1001	Querum (+1001)
	1971	86	Querum (+86)
	1974	47639	Harxbüttel (+373), Geitelde (+870), Leiferde (+1587), Stiddien (+288), Bevenrode (+723), Bienrode (+2366), Broitzem (+3594), Dibbesdorf (+961), Hondelage (+1888), Lamme (+1524), Mascherode (+2505), Rautheim (+3936), Rünigen (+4276), Schapen (+1639), Stöckheim (+4519), Thune (+1051), Timerlah (+1540), Völkenrode (+1071), Volkmarode (+3071), Wag-gum (+2902), Watenbüttel (+1838), Wenden (+5075), Buchhorst (+33), Klein Schöppenstedt (+9)
Celle	1961	208	Vorwerk (+208)
	1963	205	Altencelle (+205)
	1968	289	Westercelle (-22), Boye (+311)
	1973	16863	Altencelle (+2788), Altenhagen (+900), Borstel (+308), Garßen (+2217), Groß Hehlen (+2241), Hustedt (+650), Lachtehausen (+540), Scheuen (+1402), Westercelle (+5817)
Coburg	1972	5414	Lützelbuch (+517), Seidmannsdorf (+492), Rögen (+184), Beiersdorf (+938), Creidlitz (+1721), Scheuerfeld (+1562)
	1976	312	Neu u. Neershof (+297), Dörfles-Esbach (+15)
	1977	624	Bertelsdorf (+624)
Darmstadt	1958	-63	Wixhausen (-63)
	1977	2227	Wixhausen (+4185), Griesheim (-1958)
Datteln	1975	2547	Ahsen (+1169), Horneburg (+1378)
Delmenhorst	1974	2453	Hasbergen (+2453)
Dinslaken	1975	1820	Voerde (+826), Walsum (+994)
	1977	26	Voerde (+26)
Dortmund	1975	7498	Holzen (+4429), Lichtendorf (+2438), Westhofen (+634), Hagen (-3)

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Table A.9 – continued from previous page

City	Year	Total	Absorbed (parts of) (+) / Exclusion to (-)
Düren	1972	34237	Arnoldswweiler (+3046), Birgel (+1412), Birkesdorf (+8308), Derichswweiler (+2181), Echtz-Konzendorf (+1373), Gürzenich (+4828), Mariawweiler-Hoven (+3625), Merken (+2724), D'horn (+20), Kreuzau (+25), Lendersdorf (+4696), Merzenich (+23), Niederau (+1976)
	1978	3	Merzenich (+3)
Düsseldorf	1975	48808	Hubbelrath (+972), Angermund (+3906), Wittlaer (+5199), Erkrath (+6714), Hasselbeck-Schwarzbach (+1260), Hilden (+35), Monheim (+30716)
	1979	-3	Ratingen (-3)
Duisburg	1975	19816	Budberg (+216), Moers (+31), Rheinkamp (+4418), Rumeln-Kaldenhausen (+14493), Dinslaken (+760)
Emden	1961	-9	Wybelsum (-9)
	1972	4102	Petkum (+952), Widdelswehr (+969), Logumer Vorwerk (+297), Twixlum (+688), Wybelsum (+1196)
	1980	10	Krummhörn (+10)
Erlangen	1950	679	unincorporated areas (+679)
	1957	-13	Bubenreuth (-13)
	1960	15	Eltersdorf (+15)
	1967	402	Kosbach (+402)
	1972	10854	Großdechendorf (+1950), Eltersdorf (+2544), Frauenaaurach (+3285), Hütendorf (+269), Kriegenbrunn (+566), Tennenlohe (+2240)
	1974	-5	Nürnberg (-5)
	1977	4	Fürth (+4)
Eschweiler	1972	15223	Dürwiß (+5282), Laurenzberg (+167), Lohn (+1189), Weisweiler (+5627), Gressenich (+10), Kinzweiler (+2948)
Essen	1967	-153	Gelsenkirchen (-153)
	1970	6087	Altendorf (+6087)
	1975	16803	Kettwig (+16803)
Esslingen a.N.	1973	10748	Berkheim (+6697), Zell am Neckar (+4051)
Flensburg	1970	1251	Adelby (+1251)
	1971	-17	Harrislee (-17)
	1972	14	Harrislee (+14)
	1974	1703	Adelby (+1703)
Frankenthal (Pfalz)	1969	1783	Eppstein (+1783)
	1974	-30	Bobenheim-Roxheim (-30)
Frankfurt a.M.	1972	14985	Harheim (+3043), Nieder-Erlenbach (+2619), Nieder-Eschbach (+6435), Kalbach (+2851), Oberursel (+45), Friedrichsdorf (-8)
	1974	-45	Oberursel (-45)
	1977	14722	Berg-enkheim (+14722)
Freiburg i.Br.	1966	11	Lehen (+11)
	1971	3258	Lehen (+1971), Opfingen (+1287)
	1972	1140	Waltershofen (+1140)
	1973	3421	Tiengen (+1078), Münzingen (+863), Hochdorf (+1480)
	1974	4267	Ebnet (+1793), Kappel (+2474)
	1978	69	Umkirch (+69)

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Table A.9 – continued from previous page

City	Year	Total	Absorbed (parts of) (+) / Exclusion to (-)
Fürth	1950	53	unincorporated areas (+53)
	1972	10552	Sack (+1782), Stadeln (+6128), Vach (+2424), Boxdorf (+218)
	1977	-4	Erlangen (-4)
Fulda	1972	14726	Bernhards (+339), Besges (+70), Bronnzel (+1240), Dietershan (+492), Edelzell (+1035), Gläserzell (+1108), Haimbach (+1087), Harmerz (+871), Istergiesel (+276), Johannesberg (+388), Kämmerzell (+667), Kohlhaus (+893), Lehnerz (+1660), Lüdermünd (+208), Maberzell (+1501), Malkes (+106), Mittelrode (+208), Niederrode (+113), Niesig (+864), Oberrode (+363), Rodges (+48), Sickels (+552), Zell (+128), Zirkenbach (+509)
Gelsenkirchen	1967	183	Essen (+183)
	1981	-28	Gladbeck (-28)
Giessen	1953	-3	Heuchelheim (-3)
	1971	2969	Allendorf (+1497), Födgen (+1472)
Gladbeck	1981	28	Gelsenkirchen (+28)
Göppingen	1953	31	Schlat (+31)
	1956	1451	Bartenbach (+1451)
	1957	793	Bezgenriet (+793)
	1971	1444	Hohenstaufen (+1444)
	1972	398	Maitis (+398)
	1973	74	Schwäbisch-Gmünd (+74)
	1975	6954	Faurndau (+6954)
Göttingen	1960	4	Grone (+4)
	1963	927	Herberhausen (+927)
	1964	22242	Geismar (+8552), Grone (+5401), Nikolausberg (+606), Weende (+7883)
	1971	6	Bovenden (+6)
	1973	6025	Deppoldshausen (+18), Elliehausen (+1510), Esebeck (+437), Groß Ellershausen (+1109), Hetjershausen (+859), Holtensen (+1131), Knutbühen (+126), Roringen (+835)
Goslar	1972	12538	Hahnenklee-Bockswiese (+1262), Hahndorf (+1167), Jerstedt (+1510), Oker (+8599)
	1974	-5	Bad Harzburg (-5)
Gütersloh	1970	17509	Avenwedde (+8528), Friedrichsdorf (+896), Spexard (+3851), Ebbesloh (+203), Hollen (+646), Nienhorst (+565), Isselhorst (+2440), Ummeln (+46), Nordrheda-Ems (+101), Verl (+230), Varensell (+3)
	1973	281	Senne I (+281)
	1979	6	Rheda-Wiedenbrück (+6)
Hagen	1970	156	Dahl (+91), Ennepetal (+65)
	1975	35291	Berchum (+1553), Hohenlimburg (+26755), Garenfeld (+939), Breckerfeld (+5930), Waldbauer (+114)
Hamburg	1969	81	Cuxhaven (+81)
Hameln	1973	15596	Afferde (+3810), Groß Hilligsfeld (+781), Hastenbeck (+1047), Haverbeck (+669), Holtensen (+1211), Klein Berkel (+4064), Klein Hilligsfeld (+193), Tündern (+1944), Unsen (+526), Wehrbergen (+445), Welliehausen (+231)
	1968	10920	Berge (+3686), Westtünnen (+2344), Wiescherhöfen (+4974), Pelkum (-84)
	1975	84949	Heessen (+17700), Bockum-Hövel (+25143), Pelkum (+25172), Uentrop (+11497), Rhynern (+5437)

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Table A.9 – continued from previous page

City	Year	Total	Absorbed (parts of) (+) / Exclusion to (-)
Hanau	1968	16	Großauheim (+16)
	1969	9	Dörnigheim (+2), Mittelbuchen (+7)
	1971	1860	Mittelbuchen (+1860)
	1974	32589	Großauheim (+15884), Kleinauheim (+6978), Steinheim (+9727)
Hannover	1967	165	Wettbergen (+165)
	1968	200	Wettbergen (+200)
	1974	51707	Laatzen (+251), Langenhagen (+3237), Isernhagen (+2653), Ahlem (+9727), Anderten (+5728), Misburg (+19859), Vinnhorst (+5662), Wettbergen (+3822), Wülferode (+863), Warmbüchen (-95)
	1980	11	Hemmingen (+11)
	1981	-121	Laatzen (-121)
Heidelberg	1975	8674	Ziegelhausen (+8674)
	1976	-41	Neckargemünd (-41)
Heilbronn	1962	-9	Flein (-9)
	1965	6	Frankenbach (+6)
	1970	1094	Klingenberg (+1094)
	1972	2636	Kirchhausen (+2636)
Herford	1974	9429	Biberach (+2545), Frankenbach (+4714), Horkheim (+2170)
	1969	10509	Diebrock (+1533), Eickum (+1389), Elverdissen (+2796), Falkendiek (+799), Laar (+615), Schwarzenmoor (+1321), Stedefreund (+756), Herringhausen (+1300)
Hildesheim	1961	20	Itzum (+20)
	1965	47	Achtum (+47)
	1971	2327	Ochtersum (+2327)
	1973	5	Himmelsthür (+5)
	1974	12174	Achtum-Uppen (+1127), Bavenstedt (+784), Einum (+826), Himmelsthür (+6398), Itzum (+674), Marienburg (+209), Marienrode (+183), Sorsum (+1973)
Hof	1960	175	Leimitz (+175)
	1972	1221	Unterkotzau (+757), Martinsreuth (+196), Wölbattendorf (+268)
	1975	2	Tauperlitz (+2)
	1977	707	Leimitz (+707)
	1978	484	Wölbattendorf (+318), Haidt (+166)
Ingolstadt	1962	5904	Unsernherrn (+5904)
	1968	34	Mailing (+34)
	1969	330	Friedrichshofen (+330)
	1972	15293	Brunnenreuth (+1320), Dünzlau (+219), Etting (+1593), Gerolfing (+1497), Hagau (+255), Irgertsheim (+483), Mailing (+3443), Mühlhausen (+116), Oberhaunstadt (+4020), Pettenhofen (+254), Zuchering (+2093)
	1974	-24	Weichering (-24)
Iserlohn	1956	1100	Oestrich (+1100)
	1971	51	Letmathe (+51)
	1975	37450	Hennen (+6764), Kesbern (+481), Letmathe (+26405), Hemer (+678), Sümern (+3122)
Kaiserslautern	1969	13050	Dansenberg (+1008), Erlenbach (+1610), Erfenbach (+2730), Hohenecken (+2553), Mölschbach (+883), Morlautern (+2155), Siegelbach (+2111)

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Table A.9 – continued from previous page

City	Year	Total	Absorbed (parts of) (+) / Exclusion to (-)
	1974	49	Kindsbach (+43), Mehlingen (+6)
Karlsruhe	1958	45	Neureut (+45)
	1972	3323	Hohenwettersbach (+1415), Stupferich (+1908)
	1973	1762	Wolfartsweiher (+1762)
	1974	6694	Grötzingen (+6694)
	1975	16428	Wettersbach (4426), Neureut (+12002)
Kempton	1972	11753	Sankt Lorenz (+2055), Sankt Mang (+9698)
Kiel	1958	1633	Suchsdorf (+1633)
	1959	1220	Schilksee (+1220)
	1963	606	Melsdorf (+530), Oppendorf (+76)
	1965	94	Kronshagen (+94)
	1966	88	Russee (+88)
	1970	6219	Meimersdorf (+907), Moorsee (+918), Rönne (+300), Wellsee (+1573), Russee (+2521)
	1984	-7	Altenholz (-7)
Koblenz	1960	30	Kesselheim (+30)
	1969	2212	Kapellen-Stolzenfels (+636), Kesselheim (+1576)
	1970	18069	Arenberg-Immendorf (+3999), Arzheim (+2344), Bubenheim (+832), Güls (+5121), Lay (+1578), Rübenach (+3957), Rhens (+238)
Köln	1975	98064	Rodenkirchen (+41755), Brauweiler (+3233), Frechen (+170), Hürth (+45), Lövenich (+20575), Pulheim (+30), Sinnersdorf (+6475), Wesseling (+25913), Bornheim (+21), Dormagen (-135), Leverkusen (-18)
	1976	-27016	Wesseling (-27016)
Konstanz	1971	1794	Litzelstetten (+1794)
	1975	3525	Dingelsdorf (+1061), Dettingen (+2464)
Krefeld	1970	861	Willich (+859), Vorst (+2)
	1975	13021	Kapellen (+313), Rumeln-Kaldenhausen (+102), Kempen (+12606)
	1976	82	Kempton (+82)
Landshut	1961	547	unincorporated areas (+404) and Schönbrunn (+143)
	1963	8	Ergolding (+8)
	1972	3988	Müchnerau (+679), Schönbrunn (+2881), Hoheneggkofen (+241), Kumhausen (+187)
	1974	1982	Frauenberg (+1478), Altdorf (+424), Ergolding (+80)
	1981	-11	Bruckberg (-11)
Ludwigsburg	1956	87	Aldingen (+87)
	1974	5513	Neckarweihingen (+5513)
	1975	3185	Poppenweiler (+3185)
Ludwigshafen a.R.	1974	2267	Ruchheim (+2267)
Lübeck	1969	25	Ratekau (+25)
	1970	970	Ratekau (+20), Stockelsdorf (+950)
Lüdenscheid	1969	-3775	Lüdenscheid-Land (-3775)
Lüneburg	1963	719	Oedeme (+719)
	1974	4652	Häcklingen (+748), Ochtmissen (+828), Oedeme (+1375), Rettmer (+633), Reppenstedt (+272), Wendisch Evern (+73), Adendorf (+723)
Mainz	1957	361	Hechtsheim (+361)

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Table A.9 – continued from previous page

City	Year	Total	Absorbed (parts of) (+) / Exclusion to (-)
	1969	17532	Drais (+921), Ebersheim (+1359), Finthen (+4933), Hechtsheim (+5752), Laubenheim (+3366), Marienborn (+1201)
	1971	5	Ober-Olm (+5)
Marburg	1974	18490	Bauerbach (+673), Cappel (+6309), Cyriaxweimar (+282), Dilschhausen (+126), Einhausen (+592), Ginseldorf (+385), Gisselberg (+495), Hadamshausen (+310), Hermershausen (+241), Marbach (+3411), Schröck (+1266), Wehrda (+3656), Wehrshausen (+746), Lahntal (-2)
Minden	1973	28530	Aminghausen (+398), Böhlorst (+865), Dankersen (+3636), Dützen (+2969), Haddenhausen (+1471), Häverstädt (+2142), Hahlen (+3646), Kutenhausen (+1501), Leteln (+2804), Meißen (+3032), Päpinghausen (+407), Stemmer (+1288), Todtenhausen (+2980), Barkhausen (+746), Hartum (+133), Holzhausen II (+512)
Mönchengladbach	1975	12670	Wickrath (+12508), Jüchen (+11), Wegberg (+151)
Moers	1975	47746	Kapellen (+11266), Budberg (+11), Rheinkamp (+36500), Homberg (-31)
Mülheim a.d.R.	1975	605	Kettwig (+605)
	1981	-117	Ratingen (-117)
München	1952	-1142	Gröbenzell (-1142)
	1957	15	unincorporated areas (+15)
	1958	7	unincorporated areas (+7)
	1959	39	unincorporated areas (+39)
	1973	9	Oberschleißheim (+9)
Münster	1956	592	Sankt Mauritz (+592)
	1975	53429	Albachten (+2559), Amelsbüren (+4632), Angelmodde (+6342), Handorf (+4608), Hiltrup (+14052), Nienberge (+3055), Sankt Mauritz (+8089), Wolbeck (+4817), Albersloh (+66), Rinkerode (+3), Roxel (+4383), Telgte (+823)
Neumünster	1970	9383	Einfeld (+6214), Gadeland (+2883), Tungendorf (+286)
Neuss	1975	20740	Holzheim (+6831), Norf (+6113), Rosellen (+3419), Kaarst (+1495), Neukirchen (+2882)
Nürnberg	1952	2258	unincorporated areas (+2258)
	1961	-29	Katzwang (-29)
	1972	33641	Großgründlach (+2900), Neunhof (+1162), Brunn (+247), Fischbach (+12033), Katzwang (+6423), Kornburg (+1584), Worzeldorf (+2894), Boxdorf (+2493), Wolkersdorf (+3316), Schwaig (+589)
	1974	5	Erlangen (+5)
	1978	19	Kleinschwarzenlohe (+19)
	1985	6	Oberasbach (+6)
Offenbach a.M.	1955	-11	Heusenstamm (-11)
	1968	7	Heusenstamm (+7)
Osnabrück	1970	3197	Holzhausen (+3197)
	1971	16	Nahne (+16)
	1972	21183	Atter (+2704), Darum (+593), Gretesch (+1727), Hellern (+4186), Lüstringen (+3105), Nahne (+2422), Pye (+1786), Voxtrup (+4660)
	1976	-15	Wallenhorst (-15)
Paderborn	1969	3413	Marienloh (+936), Wewer (+2477)

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Table A.9 – continued from previous page

City	Year	Total	Absorbed (parts of) (+) / Exclusion to (-)
	1975	26040	Benhausen (+1107), Dahl (+1072), Elsen (+6314), Sande (+2288), Schloß Neuhaus (+13606), Hövelhof (+84), Dörenhagen (+11), Neuenbeken (+1547), Ostenland (+11)
Passau	1958	3	Heining (+3)
	1972	18097	Grubweg (+5545), Hacklberg (+4038), Hals (+1586), Heining (+6449), Kirchberg (+479)
Pforzheim	1963	8	Büchenbronn (+8)
	1971	1714	Würm (+1714)
	1972	791	Hohenwart (+791)
	1974	4415	Büchenbronn (+4415)
	1975	9152	Huchenfeldt (+2652), Eutingen (+6500)
	1977	-5	Niefern-öschelbronn (-5)
Pirmasens	1956	964	Lemberg (+964)
	1958	-26	Simten (-26)
	1969	5890	Erlenbrunn (+1451), Fehrbach (+1290), Hensberg (+234), Winzeln (+1542), Simten (+1719), Rodalben (-346)
	1972	2081	Gersbach (+1309), Windsberg (+772)
	1974	2	Rodalben (+2)
Regensburg	1971	12	Oberisling (+12)
	1972	307	Burgweinting (+307)
	1977	2727	Burgweinting (+1274), Harting (+535), Oberisling (+918)
	1978	431	Barbing (+431)
Remscheid	1975	1491	Hückeswagen (+1170), Wermelskirchen (+321)
Reutlingen	1969	5	Pfullingen (+5)
	1971	5144	Bronnweiler (+802), Gönningen (+2833), Oferdingen (+1159), Reicheneck (+350)
	1972	3365	Altenburg (+1071), Degerschlacht (+1138), Sickenhausen (+1156)
	1974	1887	Rommelsbach (+1887)
	1975	2482	Mittelstadt (+2482)
Schwäbisch Gmünd	1959	3063	Bettringen (+3063)
	1968	17	Straßdorf (+12), Herlikofen (+5)
	1969	3520	Herlikokofen (+3520)
	1971	5998	Bargau (+2357), Degenfeld (+411), Weiler in den Bergen (+931), Lindach (+2299)
	1972	5273	Großdeinbach (+2191), Straßdorf (+3082)
	1973	-257	Göppingen (-74), Mutlangen (-183)
	1974	-40	Lorch (-40)
	1975	1359	Rechberg (+1364), Waldstetten (-5)
Schweinfurt	1961	-13	Niederwerrn (-13)
	1978	56	Grafenheinfeld (+56)
Siegen	1966	7461	Breitenbach (+97), Bürbach (+936), Kaan-Marienborn (+3492), Seelbach (+1117), Trupach (+1671), Volnsberg (+148)
	1969	224	Feuersbach (+224)
	1975	61907	Eiserfeld (+22346), Hüttental (+39561)
Soest	1969	4892	several villages (+4892)

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Table A.9 – continued from previous page

City	Year	Total	Absorbed (parts of) (+) / Exclusion to (-)
	1980	11	Bad Sassendorf (+11)
Solingen	1975	2259	Burg a.d. Wupper (+2055), Wermelskirchen (+197), Witzhelden (+7)
Straubing	1972	6219	Hornstorf (+414), Kagers (+687), Alburg (+1984), Ittling (+3134)
	1976	104	Unterzeitldorn (+104)
	1978	-7	Mitterharthausen (+4), Aiterhofen (-11)
Trier	1969	18594	Ehrang-Pfalzel (+11314), Eitelsbach (+253), Filsch (+256), Irsch (+607), Kernscheid (+424), Ruwer (+2254), Tarforst (+439), Zewen-Oberkirch (+3037), Kenn (+10)
	1974	7	Newel (+7)
Tuebingen	1971	10943	Bühl (+1326), Hagelloch (+1315), Hirschau (+2131), Kilchberg (+845), Pfrondorf (+2353), Unterjesingen (+1982), Weilheim (+991)
	1974	376	Bebenhausen (+376)
Ulm	1968	4	Einsingen (+4)
	1971	1768	Jungingen (+1768)
	1972	1239	Unterweiler (+513), Mähringen (+726)
	1974	4679	Eggingen (+823), Donaustetten (+708), Einsingen (+1651), Ermingen (+556), Gögglingen (+941)
	1979	13	Blaustein (+13)
Velbert	1975	37741	Langenberg (+16858), Neviges (+19145), Wülfrath (+1738)
Viersen	1970	38156	Dülken (+20992), Süchteln (15126), Boisheim (+1542), Oedt (+344), Neersen (+49), Lobberich (+103)
Villingen-Schwenningen	1970	489	Mühlhausen (+489)
	1971	1033	Oberschach (+1033)
	1972	2810	Tannheim (+1062), Herzogenweiler (+146), Pfaffenweiler (+1029), Rietheim (+573)
	1974	1178	Marbach (+1178)
	1975	1888	Weigheim (+1039), Weilersbach (+849)
Wesel	1969	5958	Obringhoven (+4525), Flüren (+1433)
	1975	9332	Büderich (+4722), Voerde (+400), Hamminkeln (+1539), Bislich (+2404), Diersfordt (+260), Hünxe (+7)
Wiesbaden	1956	-11	Schlangenbad (-11)
	1977	11742	Auringen (+1196), Breckenheim (+1633), Delkenheim (+3648), Medenbach (+1147), Naurod (+2219), Nordenstadt (+1910), Hofheim am Taunus (+3), Hochheim am Main (-14)
Wilhelmshaven	1951	19	Sengwarden (+19)
	1972	2186	Sengwarden (+2186)
Witten	1970	69	Herdecke (+69)
	1975	15117	Herbede (+15117)
Worms	1969	12914	Abenheim (+2441), Heppenheim a.d. Wiese (+2020), Ibersheim (+480) Pfeddersheim (+4443), Rheindürkheim (+2246), Wies-Oppenheim (+1126, Osthofen (+158)
Würzburg	1971	9	Randersacker (+9)
	1973	-62	Unterdürnbach (-62)
	1974	1040	Rottenbauer (+1040)
	1976	2836	Unterdürnbach (+1832), Oberdürnbach (+1004)

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Table A.9 – continued from previous page

City	Year	Total	Absorbed (parts of) (+) / Exclusion to (-)
	1978	7592	Lengfeld (+2860), Versbach (+4408), Höchberg (+324)
	1982	-12	Höchberg (-12)
	1983	-4	Veitshöchheim (-4)

Chapter 3

Railways, Growth, and Industrialization in a Developing German Economy, 1829–1910

BY SEBASTIAN T. BRAUN AND RICHARD FRANKE

Abstract This paper studies the average and heterogeneous effects of railway access on municipal population, income and industrialization in Württemberg during the Industrial Revolution. We show that the growth-enhancing effect of the railway was much greater in municipalities that were larger and more industrial at the outset. Yet, such early industrial municipalities were rare in the relatively poor German state. This might explain why we find small average growth effects, which only increase at the end of the 19th century. Heterogeneity in the impact of the railway thus increased economic disparities within Württemberg and contributed to the state's sluggish growth.

Key words: Railway access, growth, sectoral employment, Industrial Revolution, Württemberg.

JEL classification: R12, R40, O14, N73, N93.

3.1 Introduction

Explanations of Germany's rapid industrialization in the mid- and late-19th century often point to the railway as the single most important initiator of the country's take-off to modern economic growth (Rostow, 1962; Fremdling, 1977; Ziegler, 2012). Importantly, the railway was tightly connected to Germany's heavy industries. Railway construction boosted the demand for iron and steel production, which in turn relied increasingly on coal. At the same time, railways expanded the market for German coal, especially from the Ruhr and Upper Silesia. The railway was thus at the heart of a 'leading sector complex' (Fremdling, 1977), which drove Germany's industrial take-off (Broadberry et al., 2008). Yet, the interplay between the railway and heavy industries might have favored central and coal-mining regions, thereby increasing regional economic disparities within Germany (Gutberlet, 2013b; Ziegler, 2012).

Exploring heterogeneity in the impact of the railway is thus important for understanding its effect on the spatial distribution of economic activity in Germany. This paper uses exceptionally rich population and employment data to document average and heterogeneous effects of railway access in the Kingdom of Württemberg during the Industrial Revolution. There are two reasons why Württemberg, the third largest state in the German Empire, is a particularly interesting case for studying regional heterogeneity in the importance of the railway for Germany's industrial take-off.

First, Württemberg did not industrialize based on heavy industries, as it lacked coal deposits and did not develop an important iron and steel producing sector. Consequently, the railway might have benefited Württemberg less than Germany's centers of heavy industry, which might explain the state's comparably sluggish growth performance in the 19th century. Second, regional economic differences within Württemberg grew markedly in the second half of the 19th century.¹ Our finely grained data on the universe of Württemberg's civil parishes (henceforth, parishes)—the state's smallest administrative unit which include both rural villages and larger towns—allow us to evaluate whether the railway particularly benefited larger and more industrial parishes and thus increased regional disparities within the Kingdom.

We focus on the short- and long-run effects of the first wave of railway expansion in 1845-54, which connected the capital Stuttgart in the middle of the country with major towns in the

¹The coefficient of variation of regional income per capita doubled in Württemberg between 1849 and 1907, whereas it remained constant in Prussia (Frank, 1993). Among the four districts (*Kreise*) in Württemberg, the *Neckarkreis* stood out as the most densely populated, urbanized and industrialized one. Frank (1993) estimates that its income per capita was persistently above the Germany-wide average in 1849–1907. The other three districts had lower average income per capita than Germany as a whole and fell further behind between 1849 and 1907. Section 3.2 discusses Württemberg's economic development in the 19th century in more detail.

east, north, and south. Along the way, 73 of Württemberg's 1,858 parishes gained access to the railway, many of them small and insignificant before the coming of the railway. We consider two sets of outcomes: First, we study population, wages and housing values. In spatial equilibrium, these three variables are linked, as workers move between parishes to arbitrage away differences in real wages.² Second, we consider a wide range of industrialization measures, including the employment share in industry, the local adoption of steam engines, and firm size. Since our employment data distinguish between 320 sectors, we can also study the effect of railway access on specific industries and specialization within industry.

We document two key results. First, railway access had a positive but small average effect on parish level population growth in Württemberg. In particular, we find that railway access increased annual population growth by 0.3–0.4 percentage points in 1843–1871. This is considerably smaller than the 1–2 percentage point increase that Hornung (2015) documents for Prussian towns over the same period. Therefore, Württemberg seems to have benefited less from the railway than Prussia, at least in the first decades of Germany's industrial take-off. The positive effect of railway access on population then increased markedly in Württemberg towards the end of the 19th century. Faster population growth coincided with higher local wages and housing costs as well as a more rapid reallocation of labor towards industrial activities.

Second, we document important heterogeneities in the effect of the railway within Württemberg. The positive economic effects of railway access were much greater in initially larger and more industrial parishes, consistent with new economic geography models.³ We show, for instance, that the positive effect on population growth is three times larger in parishes that already had a factory in 1832 than in parishes without a factory. However, such early industrial parishes were rare in Württemberg, which might explain why we find comparably small *average* growth effects. We also show that the positive effect on industrial employment is largely driven by the textile and machine building industry.

A key challenge for the causal interpretation of our findings is the endogenous location of railway lines. For example, large or growing parishes might have been more likely to gain access to the railway than small or stagnant parishes. Railways may then follow economic development

²Spatial equilibrium implies that positive attributes of a location are offset by negative attributes. For instance, the canonical Rosen-Roback model of spatial equilibrium between cities studies the trade-off between income, amenities and housing costs. Given amenities, cities with high nominal wages also exhibit high costs of housing, so that utility is the same across all locations. If transport infrastructure improvements increase local wages, e.g. by increasing market access and thus demand (as in Redding and Turner, 2015), workers move to the high-wage location. These population inflows then drive up the price of housing. In spatial equilibrium, we would thus expect the railway to simultaneously increase wages, population, and housing prices.

³Krugman (1991) has famously shown how transport infrastructure improvement can disproportionately benefit locations with initially larger non-rural population.

rather than causing it (Fishlow, 1965). We use three empirical strategies to gauge the causal effect of railway access on spatial economic development in Württemberg.

First, our baseline approach compares changes in economic outcomes of parishes with and without railway access in a differences-in-differences framework. The key identifying assumption of this approach is that economic outcomes in railway and non-railway parishes would have followed the same time trend in the absence of the railway. We also estimate event study regressions that allow the effect of the railway to vary over time. Second, we apply semi-parametric methods of the treatment effects literature. These methods require railway access to be a function of observable characteristics only but do not postulate a specific functional form for the outcome variables. Third, we restrict the control group to ‘losing’ parishes that were the runners-up choice for a given railway line. We show that winning and losing parishes were very similar in their pre-railway characteristics and trends. This lends credibility to our identifying assumptions. Our main findings are robust across all three empirical strategies.

Contribution to the literature. Our paper is closely related to a growing literature that studies the growth effects of railways in the 19th century by comparing areas with access to the railway network to areas without access.⁴ Many of these studies document positive effects of railways on urban population growth.⁵ Important examples include Atack et al. (2010) for the US; Berger and Enflo (2017) for Sweden; Hornung (2015) for Prussia; and Jedwab and Moradi (2016) for Ghana. Recently, Berger (2019), Bogart et al. (2019) and Büchel and Kyburz (2020) have complemented the large literature on urban population growth with evidence from parish level data. Berger (2019) shows that rural parishes in 19th century Sweden experienced more rapid population growth if they were traversed by state-owned railway lines. Bogart et al. (2019) document a positive effect of railway access on population growth for 19th century England and Wales and Büchel and Kyburz (2020) do so for Switzerland.

Our study contributes to this literature in at least two important ways. First, we consider a broader set of outcome variables. In particular, we study income, wages and housing values in addition to population.⁶ Since population growth often serves as a proxy for economic devel-

⁴An influential earlier literature estimates the aggregate social savings of railways (see Fogel (1964) and Fishlow (1965) for seminal works and Leunig (2010) for a critical survey of the social saving method). In an important recent paper, Donaldson and Hornbeck (2016) estimate the aggregate impact of market access on US agricultural land values. They then combine these estimates with the decline in market access without railways to assess the aggregate effect of railways on the agricultural sector in 1890.

⁵Another important finding in this literature is that railway access increased average firm size. Atack et al. (2011), for instance, find that access to the railway increased establishment size across US counties between 1850 and 1870, and hence conclude that the railway was an important factor in the rise of the factory. Hornung (2015) finds similar results for Prussia.

⁶Berger and Enflo (2017) show for Sweden that house prices today are still higher in towns with early access to the railway. Price differentials vanish once the authors account for contemporary differences in town size. Our estimates for Württemberg consider the effect of mid-19th century railways on house prices in the early 20th rather

opment (Hornung, 2015), it is important to verify that increases in population indeed go hand in hand with increases in local income. Furthermore, we consider various indicators for industrialization, which is again only indirectly captured by population growth. Our finely grained data on employment in 320 sectors allow us to study the effect of railways on specific industries, deemed important for Germany’s industrial take-off. In related work, Berger (2019) shows for Sweden that the railway increased industrial employment. However, the study does not differentiate between sectors within industry, as we do. We also add to Berger (2019) by studying the effect of the railway on the adoption of steam as a core technology of the Industrial Revolution.

Second, our disaggregated data on the universe of parishes in Württemberg allow us to uncover new findings on the heterogeneous effects of railways. In particular, we find that the positive effects of the railway on population, income, and industrialization were particularly pronounced in larger and industrial parishes and in the densely populated regions of Württemberg. These findings contribute to a small literature that explores heterogeneity in the impact of railways (Berger, 2019; Okoye et al., 2019; Tang, 2014). Consistent with our results, Tang (2014) and Gutberlet (2013b) find that densely populated regions in 19th century Japan and Germany, respectively, benefited more from the railway.⁷ In contrast, Hornung (2015) and Bogart et al. (2019) show for Prussia and England, respectively, that the railway had smaller effects on population growth in larger towns and densely populated areas. The fact that Württemberg had very few large towns and urban areas might explain why our results differ from those in Hornung (2015) and Bogart et al. (2019).⁸

We also apply a different identification strategy than most papers in the literature. In particular, we compare population changes of winning and losing parishes,⁹ instead of using

than 21st century. Banerjee et al. (2020) find moderate positive effects of proximity to the Chinese transport networks on county-level GDP per capita but no effect on per capita GDP growth in 1986–2006.

⁷Tang (2014) shows for 19th century Japan that rail access led to a redistribution of firms from scarcely- to densely-populated prefectures. Gutberlet (2013b) shows that transport-induced improvements in market access increased the spatial concentration of manufacturing across districts (*Regierungsbezirke*) of the German Empire between 1861 and 1882. Both studies do not only study different outcome variables, they also do so at a more aggregated spatial level.

⁸Hornung (2015) defines towns as large if they had more than 5,000 inhabitants in 1837. Württemberg had only 14 such towns (out of 1,858 parishes). Bogart et al. (2019) show that railways had larger population effects in middle and upper density parishes than in parishes in the top decile. They explain their findings by the limited availability of land close to city centers in the already urbanized English economy. In contrast to England, Württemberg experienced a very slow urbanization process. In 1837, little more than 10 percent of the population lived in towns with 5,000 inhabitants or more.

⁹Greenstone et al. (2010) use a similar approach to quantify agglomeration spillovers. The authors study the productivity of incumbent plants in counties where a large manufacturing plant opened and take incumbent plants in counties that narrowly lost the competition for the new plant as the control group. In the literature on railways and growth, some papers use unrealized lines in placebo regressions, verifying that the treatment effect for placebo lines is zero (Ahlfeldt and Feddersen, 2018; Berger and Enflo, 2017; Donaldson, 2018; Jedwab and Moradi, 2016; Jedwab et al., 2017). Starting with Baum-Snow (2007) and Michaels (2008), some studies also use planned and historical routes to instrument for actual and contemporary transport networks (see Redding and Turner, 2015, for a survey).

proximity to the least cost path or straight line between railway nodes as instrument for actual railway access (as in, e.g., Berger, 2019; Berger and Enflo, 2017; Bogart et al., 2019; Büchel and Kyburz, 2020; Hornung, 2015). One problem of the IV approach is that least cost paths are likely to correlate with geography and pre-existing transport networks, thereby potentially violating the exclusion restriction. We show that this problem indeed arises in our context.

Taken together, our results on the average and heterogeneous effects of the railway in Württemberg also add to and bring together two strands of the literature on Germany's industrial take-off that quantify the contribution of railways to economic growth (Fremdling, 1977, 1985; Hornung, 2015) and study growing regional disparities during the take-off (Frank, 1993; Kiesewetter, 2004; Gutberlet, 2013a). The railway was an important driver of Germany's aggregate economic growth in the 19th century. Yet, its impacts varied strongly across regions, with coal-mining and industrialized regions arguably benefiting most.¹⁰ This might partly explain why Württemberg, despite all the investment in its railway network, remained poorer than most other parts of Germany at the beginning of the 20th century (Frank, 1993; Mann, 2006).

3.2 Background

The Kingdom of Württemberg was formed in 1806, emanating from the Duchy of Württemberg at the instigation of Napoleon Bonaparte. Württemberg was initially part of the Confederation of the Rhine (*Rheinbund*), a confederation of German states under the auspice of the French Empire. As a confederate of Napoleon, Württemberg gained several territories in the Napoleonic era, almost doubling its size between 1803 and 1810. These new territories are referred to as *Neuwürttemberg*, whereas the old territories of the Duchy of Württemberg are referred to as *Altwürttemberg*. After the dissolution of the Rheinbund in 1813, Württemberg first joined the German Confederation (*Deutscher Bund*), created at the Congress of Vienna in 1815, and later became a member of the new German Empire, founded in 1871. It was the third largest state of the German Empire after Prussia and Bavaria (see Appendix Figure B.1).

Württemberg's initial conditions for the industrialization process were poor (Marquardt, 1985; Seybold, 1974). The Kingdom's lack of raw materials, such as coal or ore, impeded the development of heavy industries and made the manufacturing sector's energy production dependent on water or even animal power. Württemberg also lacked navigable waterways, and its hilly topography made overland transports time-consuming and expensive. The poor transport infrastructure prohibited the import of much-needed raw materials and limited the selling market

¹⁰Consistent with this argument, Bogart et al. (2019) show for England that railway-induced population gains were larger in parishes that had coal.

accessible to firms. The division of property among all heirs (*Realteilung*), prevalent especially in Altwürttemberg, led to a fragmentation of property and reduced the mobility of property-owning workers (Flik, 2001). The fragmentation of land ownership also led to a mixture of agricultural and industrial employment: small farmers sought additional income outside agriculture, and traders often possessed some livestock and land.

Württemberg's institutional arrangements contributed to its relative economic backwardness (Ogilvie, 2004; Ogilvie and Carus, 2014). In Württemberg, the parliament exercised strong control over the executive, especially when compared to other German states, such as Prussia. Dominated by bourgeois wealth holders, the parliament pushed for policies that granted far-reaching privileges to guilds and other occupational associations. The rent-seeking activities of these special interest groups stifled economic growth in Württemberg until well into the 19th century. It was only in 1862 that guilds were abolished in Württemberg (von Hippel, 1992; Ogilvie, 2019).

At the dawn of the Industrial Revolution, the textile sector was Württemberg's most important industry. In 1832, official statistics counted 142 manufactories and factories in Württemberg's leather, textile and clothing industry, 58 in the paper and printing industry and 37 in the chemical industry (Feyer, 1973).¹¹ Most of Württemberg's 342 industrial plants were still small: Almost a third had at most five workers, and only 21 had 100 workers or more. A specific characteristic of Württemberg's industrial landscape was the predominance of 'mixed plants' that employed both factory workers and home workers.

The German Customs Union (*Zollverein*), founded in 1834 under Prussian leadership, gave an important impulse for Württemberg's industrialization process (Gysin, 1989). By creating a free-trade area throughout much of Germany, the Union considerably expanded firms' potential selling markets (see Keller and Shiue, 2014; Shiue, 2005, for comprehensive assessments of the economic effects of the *Zollverein*). Increasing trade volumes between German states also reinforced plans for a German railway network, which the economist Friedrich List advocated for Württemberg already in 1824 (Dehlinger, 1949; Mühl and Seidel, 1980).

Yet, it was only in 1843 that Württemberg founded a public railway company, the *Königlich Württembergische Staats-Eisenbahnen*, and began to build a railway network. At this time, railway lines had already been opened in the other larger states of the German Confederation (Bavaria, Saxony, Prussia, Austria, Brunswick, Baden, Hanover). Importantly, Württemberg did not approve and license private railway companies for the construction and operation of

¹¹The terms manufactory (*Manufaktur*) and factory (*Fabrik*) were often used as synonyms at the time, referring to industrial plants that employed a relatively large number of workers and/or produced relatively large quantities (Gysin, 1989).

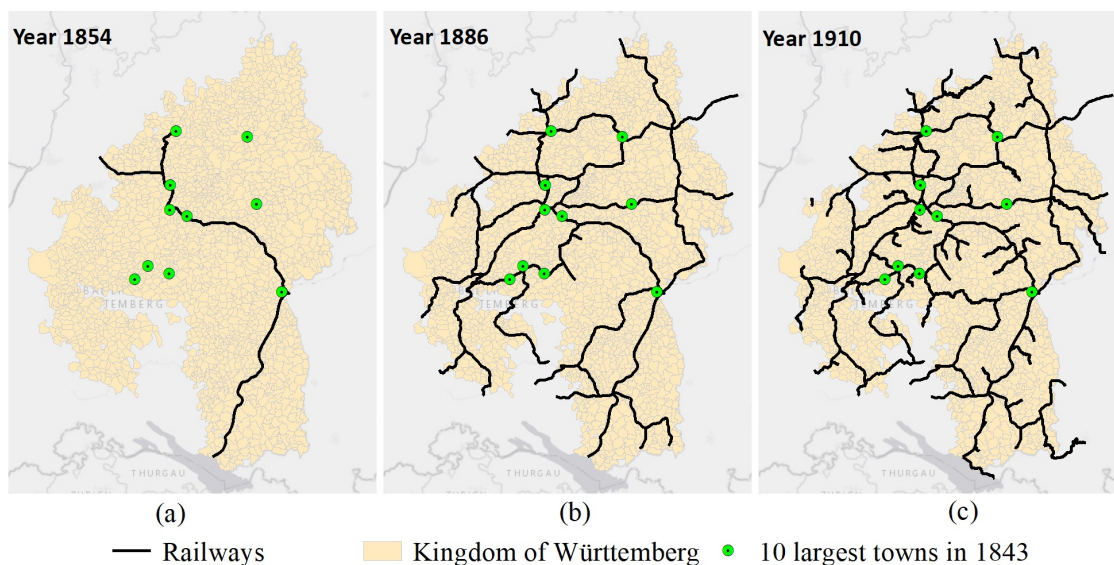


Figure 3.1: Network development stages in the Kingdom of Württemberg 1845–1910

Notes: Panels (a), (b), and (c) show the railway network at the end of the construction phases in 1854, 1886, and 1910, respectively.

Sources: Kunz and Zipf (2008), Dumjahn (1984), Kommission für geschichtliche Landeskunde in Baden-Württemberg and Landesvermessungsamt Baden-Württemberg (1972), and Esri HERE Delorme, MapmyIndia, OpenStreetMap[®] contributors, and the GIS map user community. Authors' design.

its main railways. We might thus expect that railways were not only, as in the case of private networks, built according to the expected profitability of a particular line and therefore less biased towards parishes with favorable growth perspectives.

The construction of Württemberg's railway network. The expansion of the railway network in Württemberg proceeded in three broad stages (Mühl and Seidel, 1980; Supper, 1895), depicted in Figure 3.1. The first stage from 1845 to 1854 (see panel (a)) saw the construction of the country's central line (*Zentralbahn*), connecting Ludwigsburg, the capital Stuttgart, and Esslingen along the river Neckar. The central line was then extended via the eastern line (*Ostbahn*) to Ulm and via the southern line (*Südbahn*) to Friedrichshafen at Lake Constance. The northern line connected Ludwigsburg and Heilbronn, and the western line (*Westbahn*) connected Württemberg with the neighboring state of Baden—and thus to the pan-German railway network. Finally, a bridge over the Danube was completed in 1854, connecting Ulm in Württemberg with Neu-Ulm in Bavaria. The bridge opened a railway corridor from the Dutch harbors to Bavaria.

The second stage, which took place between 1857 and 1886, completed Württemberg's main railway network by connecting all major towns and urban areas to the network (see panel (b) of Figure 3.1). After often lengthy negotiations, Württemberg's railway network was now well connected to those of its neighboring states. The length of Württemberg's railway network

increased from 290 kilometers in 1854 to 1,560 kilometers in 1886 and the number of parishes with railway access increased from 73 to 350. The third stage from 1887 onwards saw the construction of several branch lines that connected the rural area of Württemberg's inland with the main lines (see panel (c)). In contrast to the main lines, the branch lines were frequently constructed and operated by private railway companies.

Württemberg's government determined the main nodes of the railway network but generally not the exact route (Mühl and Seidel, 1980). The first Railway Law (*Eisenbahngesetz*) of April 1843, for instance, stipulated that the main line was to connect Stuttgart and Cannstatt in the middle of the country with Ulm, Biberach, Ravensburg and Friedrichshafen in the east and south, Heilbronn in the north, and with Württemberg's border to Baden in the west. The aim was to construct the *shortest* connection between Lake Constance, the access point to Switzerland, and the end points of the navigable waterways Neckar and Danube (Mühl and Seidel, 1980).

The government then instructed a railway commission and external experts to develop the exact route for each line. The commission compared competing routes mainly under technical aspects, setting thresholds for the permissible curve radius and railway gradient (Mühl and Seidel, 1980). External planners were asked to compare the length, gradient, and cost of alternative routes. Appendix B.5 describes the planning process for the central line in detail.

In addition to technical aspects, Württemberg's geographical location—squeezed between Baden in the north and west, Bavaria in the east, and Prussia in the south—influenced and often delayed the construction of the railway network. Towns and villages close to Württemberg's borders were generally at a disadvantage (Mühl and Seidel, 1980). The shortest route between Horb and Sulz in southwest Württemberg, for instance, crossed the Prussian territory of Sigmaringen. Württemberg first explored potential by-passes, but eventually approached Prussia to get permission for the railway to continue through its territory. It was only the treaty between Prussia and Württemberg of 1865 that solved the issue.

Railway treaties between states often involved painful compromises, as states competed for transit passengers and freight. The treaty between Bavaria and Württemberg of January 1861, for instance, allowed Württemberg to connect its network to the Bavarian town of Nördlingen. In return, the treaty obliged Württemberg to not connect its Cannstatt-Nördlingen railway to the Cannstatt-Ulm railway until 1875. The reason was an economic one: Such an extension would have made the connection between Nördlingen and Lake Constance shorter on Württemberg than on Bavarian territory. It would thus have reduced traffic on the Bavarian railway line.

Towns also sometimes tried to influence the direction and timing of Württemberg's railway extension (Mann, 2006; Mühl and Seidel, 1980). Ulm, for instance, founded a railway society (*Ulmer Eisenbahn-Gesellschaft*) in 1835, which lobbied for a direct railway connection between Cannstatt and Ulm. Ulm argued that only a railway connection would allow it to recover its role as an important commercial center. It had relinquished several districts and lost its status as a local capital when it was incorporated into Württemberg in 1810. Ulm indeed became an important junction of Württemberg's railway network, not least because Württemberg struggled to reduce Ulm's resentments against the Kingdom.

Industrialization until 1907. Württemberg was a late-comer in Germany's industrial take-off. It was not until the late 19th century that the industrialization process in Württemberg accelerated markedly. Between 1882 and 1895, employment in industry increased by 24 percent. Textile and metal processing were two of the main drivers of industrial employment growth in Württemberg in the late 19th century. Employment in textile and metal processing increased by 19 and 38 percent, respectively. The number of industrial firms with at least five employees even increased by 86 percent between 1882 and 1895, and employment in these firms more than doubled.

Nevertheless, Württemberg still lagged behind most other parts of Germany at the turn of the 20th century. By 1895, 44.4 percent of all full-time employees in Württemberg were still in agriculture, compared to just 36.2 percent in the German Empire as a whole (Losch, 1912). Large industrial firms, equipped with engines and work machines, did not yet dominate Württemberg's industrial production. In fact, firms with four workers or less still accounted for half of its industrial employment. The agricultural employment share decreased only slowly and still reached 41.3 percent in 1907. At the dawn of World War I, Württemberg's national income per capita was well below the Germany-wide average (Frank, 1993; Mann, 2006). Factors adduced as causes for Württemberg's late industrialization include the lack of coal and iron ore (Boelcke, 1973), the entrenched power of guilds and local communities (Ogilvie, 1997; Ogilvie and Carus, 2014), and the relatively late construction of the railway network (Naujoks, 1982).

Importantly, industrialization advanced at different speeds across Württemberg. Appendix Table B.1 compares Württemberg's four districts with respect to their estimated national income per capita in 1849 and 1907, agricultural employment share in 1907, and urbanization rate in 1907. The *Neckarkreis* stands out as the most economically developed and dynamic district. Its agricultural employment share was 30.0 percent in 1907, well below the average in Württemberg (41.3) and Germany (32.7). Relative to the German-wide average, estimated income per capita of the *Neckarkreis* increased slightly in the second half of the 19th century, from 111.2 in 1849 to 113.3 in 1907 (Frank, 1993). In contrast, relative income of the other three districts plummeted.

Württemberg's poorest regions thus participated the least in Germany's spectacular growth performance in the second half of the 19th century. As we will see, heterogeneous effects of the railway contributed to these patterns.

3.3 Data

Our panel data cover all civil parishes in the Kingdom of Württemberg. Civil parishes are the lowest political administrative unit in Württemberg and comprise all towns and villages. We aggregate parishes to take boundary changes into account that occur during the observation period.¹² This leaves us with 1,858 parishes with a median area of 8.6 square kilometers.

Outcome variables. Population data come from 21 population censuses (Statistisches Landesamt Baden-Württemberg, 2008), conducted in the Kingdom of Württemberg between 1834 and 1910.¹³ Every census recorded the total population of each parish. Selected censuses also contain information on other demographic characteristics of the parish's population, such as their age structure, place of birth, or marital status.

Total population in Württemberg grew from 1.570 million in 1834 to 1.819 million in 1871 and 2.458 million in 1910. Appendix Figure B.2 shows the average annual growth rate of parishes in 1834–1910, along with Württemberg's railway network in 1855. The figure documents large variation in population growth, with almost a third of Württemberg's parishes experiencing population decline. Württemberg's poor economic conditions made the Kingdom one of the main origin regions for oversea migration from Germany in the 19th century. More than 337,000 people left Württemberg in 1834–1871 alone (von Hippel, 1984). Figure B.2 also indicates that population growth was indeed higher in parishes along the railway network.

We digitized data on the average daily wage of day laborers in 1884, 1898, and 1909, taxable income and building tax revenues in 1907, and the fire insurance value of buildings in 1908 (Königliches Statistisches Landesamt, 1898, 1910). The 'usual local daily wages of ordinary day laborers' (*ortsüblichen Tagelöhne gewöhnlicher Tagarbeiter*) were recorded following the Sickness Insurance Law of 1883. The data distinguish between wages of females and males. Taxable income refers to natural persons and equals income net of tax allowances and other

¹²We digitized parish borders from Kommission für geschichtliche Landeskunde in Baden-Württemberg and Landesvermessungsamt Baden-Württemberg (1972) and use information on border changes from Statistisches Landesamt Baden-Württemberg (2008). Overall, 71 parishes are affected by aggregation.

¹³The census years are 1834, 1837, 1840, 1843, 1846, 1849, 1852, 1855, 1858, 1861, 1864, 1867, 1871, 1875, 1880, 1885, 1890, 1895, 1900, 1905, and 1910. We correct a few obvious data errors and interpolate missing population data for parishes in the county of Böblingen in 1867 and the county of Leonberg in 1858.

deductions. We approximate average housing values from building tax revenues in 1907 and use the average fire insurance value of buildings as an alternative indicator.¹⁴

We further digitized employment data from the occupation censuses of 1895 and 1907 (Königliches Statistisches Landesamt, 1900a, 1910).¹⁵ The occupation censuses comprise parish level information on the number of full-time gainfully employed persons (self-employed and dependent) in agriculture, industry, and trade and transport. We calculate—separately for each of the three sectors—the number of employed persons per 100 individuals in a parish.

In addition, we digitized Württemberg’s *Gewerbestatistik* for 1829 (various volumes of *Gewerbekataster*, Staatsarchiv Ludwigsburg E 258 VI) and 1895 (Königliches Statistisches Landesamt, 1900b).¹⁶ *Gewerbe* includes mining, manufacturing, handicrafts, construction, trade and transport (excluding railways and post). The data provide detailed information on the number of establishments and their total employment, disaggregated by 3-digit sectors.¹⁷ We use the disaggregated data from the *Gewerbestatistik* to distinguish between industrial employment in specific industries, which have been identified as key drivers for Germany’s industrialization process. Moreover, we measure (absolute) specialization in industry by the Herfindahl- Hirschman-Index (HHI): $HHI_{it} = \sum_{l=1}^L (b_{ilt})^2$, where b_{ilt} is parish i ’s employment share of the (3-digit) industrial sector l in total industrial employment at time t (1829, 1895). This measure is bounded between $1/L$ (if all sectors have the same employment) and 1 (if all employment is concentrated in one sector). We also use the *Gewerbestatistik* to calculate establishment size in industry as the average number of persons employed in an establishment (*Hauptbetrieb*).

Finally, we obtain data on the location of steam engines from archival records (Staatsarchiv Ludwigsburg E 170 Bü 272). Data are available from 1845 until 1869, and include the year of installation and the maximum capacity of each steam engine.

Railway access. We link the panel data on population, sectoral employment, and income with geo-referenced information on parishes and railway construction in Württemberg. Dumjahn (1984) and Wolff and Menges (1995) report the starting and end points of each railway line, the length of the line and its opening date. We use this information to define the nodes of the

¹⁴The building tax was 2 percent of a building’s return, and the return was set to 3 percent of a building’s market value (Pistorius, 1904). We thus approximate the overall building value by dividing tax revenues by 0.02×0.03 . We divide the overall building value by the number of buildings to arrive at an indicator of a parish’s average housing value. Fire insurance covers furniture and other possessions in the house in addition to the property value.

¹⁵The 1895 occupation census is—to the best of our knowledge—the first census that provides employment data at the parish level.

¹⁶To the best of our knowledge, Württemberg’s statistical office did not publish the 1907 edition.

¹⁷We match the 3-digit industry groups from the 1829 to the 320 groups of the 1895 edition. Employment data of the *Gewerbestatistik* and the occupation census are not directly comparable. The occupation census records employment at the place of residence of each worker, while the *Gewerbestatistik* focuses on the location of plants.

railway network. We identify parishes as nodes if they serve as a network junction or are named as starting or end points of a railway segment that was constructed without interruption. Data on railway stations in 1911 are published in Königliches Statistisches Landesamt (1911).

Control variables. We take information on the pre-railway share of Protestants from Königliches Statistisches Landesamt (1824). We add data on the location of manufactories in 1832, published in Memminger (1833). Overall, there are 342 distinct manufactories in 128 parishes with an average employment of 28 workers (excluding seasonal and home employment). Data on the average elevation of parishes come from Bundesamt für Kartographie und Geodäsie (2017). We also add dummies for being located at a river navigable in 1845 and being connected to a paved road in 1848 (Kunz and Zipf, 2008).

3.4 Empirical Strategy

3.4.1 Treatment and control group

The main challenge for identifying the causal effect of railway access is that parishes were not randomly chosen to be connected to the railway. Table 3.1 illustrates this selection problem: It compares economic and demographic characteristics of different groups of parishes before the construction of Württemberg’s railway network began. Column (1) shows characteristics for the railway nodes, Column (2) for other parishes who gained access to the railway in the first stage of the railway expansion, and Column (3) for all other parishes. Columns (5) and (6) restrict attention to the sub-sample of winner and runner-up parishes.

Württemberg’s government generally chose the largest and economically most important cities—such as the capital Stuttgart in the middle of the country, Ulm in the east or Heilbronn in the north—as railway nodes. Nodes had much higher population than other parishes and were more likely to be located at rivers and to have a manufactory in 1832 (see Column (1) of Table 3.1). We generally exclude nodes from our analysis, since we cannot construct credible counterfactuals for them. As it is common in the literature (see, e.g., Berger and Enflo, 2017; Hornung, 2015), our analysis thus focuses on the effect of railway access on parishes that gained access to the railway in the first stage of the expansion and were not network nodes. These are the parishes in the treatment group.

One potential control group are all parishes that did not gain access to the railway in the first stage of the expansion. However, the selection problem carries over—albeit in muted form—to a comparison between railway parishes that were not nodes (Column (2)) and non-railway parishes (Column (3)). Treated parishes were generally larger, situated at lower altitude, and

Table 3.1: Group comparison of pre-treatment characteristics

	Full sample				Winners vs. runners-up		
	Railway		Non-railway		Difference		Difference
	Nodes (1)	parishes (2)	parishes (3)	parishes (4)	parishes (5)	parishes (6)	
Pop. 1834 (log)	8.379 (1.464)	6.801 (0.642)	6.390 (0.702)	0.411 [0.091]	6.740 (0.662)	6.927 (0.715)	-0.187 [0.116]
Pop. density 1834 (log)	5.628 (0.753)	4.455 (0.655)	4.278 (0.604)	0.178 [0.079]	4.452 (0.678)	4.571 (0.603)	-0.119 [0.105]
Protestants 1821 (share)	0.730 (0.379)	0.613 (0.484)	0.617 (0.469)	-0.004 [0.061]	0.621 (0.482)	0.673 (0.452)	-0.052 [0.077]
Manufactory dummy 1832	0.583 (0.515)	0.082 (0.277)	0.053 (0.223)	0.029 [0.029]	0.088 (0.285)	0.172 (0.379)	-0.084 [0.058]
Ind. employment per 100 persons 1829	12.953 (4.087)	8.793 (3.611)	7.730 (4.320)	1.063 [0.560]	8.930 (3.651)	9.568 (4.176)	-0.637 [0.664]
Average elevation (in m)	391.685 (120.070)	406.572 (136.975)	496.470 (155.942)	-89.898 [20.229]	418.102 (145.237)	411.113 (138.093)	6.989 [23.399]
River dummy	0.417 (0.515)	0.148 (0.358)	0.075 (0.264)	0.072 [0.035]	0.175 (0.384)	0.131 (0.339)	0.044 [0.059]
Road dummy	0.917 (0.289)	0.820 (0.388)	0.480 (0.500)	0.340 [0.065]	0.807 (0.398)	0.778 (0.418)	0.029 [0.068]
Observations	12	61	1,785		57	99	

Notes: The table shows average values of pre-treatment characteristics for nodes (Column (1)), railway parishes (Column (2)), and non-railway parishes (Column (3)), 'winners' (Column (5)), and 'runners-up' (Column (6)). Column (4) shows the mean difference in pre-treatment characteristic between railway and non-railway parishes and Column (7) the mean difference between winners and runners-up. Nodes are defined as parishes that are either starting or end points of a railway segment or serve as network junctions. Railway parishes obtained railway access in the first construction stage 1845-1854 (but are not nodes). Non-railway parishes did not obtain railway access in the first construction stage. Winners are parishes that obtained railway access in the first construction stage (but are not nodes). Runners-up are parishes that would have obtained railway access in the first construction stage if unrealized alternative routes had been built. See main text and Appendix for data sources and details. Standard deviations are in parentheses (Columns (1)-(3) and (5)-(6)). Standard errors of a two-sided mean difference t-test are in brackets (Columns (4) and (7)).

more likely to have road access than non-railway parishes (Column (4)). Comparisons between the two groups might thus yield biased estimates.

Parts of our analyses thus focus on an alternative control group, consisting of parishes that would have obtained access to the railway if proposed alternative routes had been built. As described in Section 3.2, Württemberg’s government first determined the nodes of the network and then instructed a railway commission and external experts to develop the exact route for a given railway. The experts typically came up with a number of proposals. We use these proposals to identify runner-up parishes, i.e., parishes with designated railway access on an alternative line that was eventually not built in the first stage of the railway expansion.

Figure 3.2 shows the runner-up parishes (crosses), along with the winner parishes that were actually chosen (points) and the railway nodes (stars). Colors mark all parishes that under a specific proposal would have received access to the railway. Henceforth, we refer to all potential routes suggested for one line as ‘cases’. Overall, there are seven such cases in the first construction stage (see Appendix Table B.4 for details). The decision for or against a specific route was mainly based on technical aspects. Political conflicts with neighboring countries also played a role (see Section 3.2 and Appendix B.5). In contrast, local special interest groups had arguably only little influence on the decision process, despite their generally powerful role in Württemberg’s politics and society (Appendix B.7 discusses this point in great length and also presents empirical evidence against an effect of political connections on railway access).

We rely on runner-up or losing parishes to identify a valid counterfactual for what would have happened to railway parishes had they not gained access to the railway in the first stage of the railway construction. The idea is simple: runner-up parishes should share many of the pre-treatment characteristics with parishes in the treatment group, as both groups of parishes were candidates for railway access in the first stage. In fact, many of the proposed lines that were initially not realized were built later.

Columns (5) to (7) of Table 3.1 provide support for our empirical strategy. Differences in pre-treatment characteristics decrease considerably when we compare winner parishes (Column (5)) and runner-up parishes (Column (6)).¹⁸ This holds, in particular, for parishes’ access to roads and rivers, which differed greatly between railway and non-railway parishes but not between winner and runner-up parishes. In fact, none of the mean differences between winners and

¹⁸Nine of the railway parishes in Column (2) are not among the winner parishes in Column (5). These are parishes that would have been connected to the railway under all alternative proposals (Asperg, Baidt, Ölbronn, and Tamm) or parishes on the line Ravensburg-Friedrichshafen (Berg, Eschach, Hirschlatt, Meckenbeuren, and Taldorf), for which no alternative line was proposed. On the other hand, five parishes are counted twice, as they were among the winning parishes in two different cases (namely Obertürkheim, Schweinhausen, Ummendorf, Unteressendorf, and Wolpertswende).

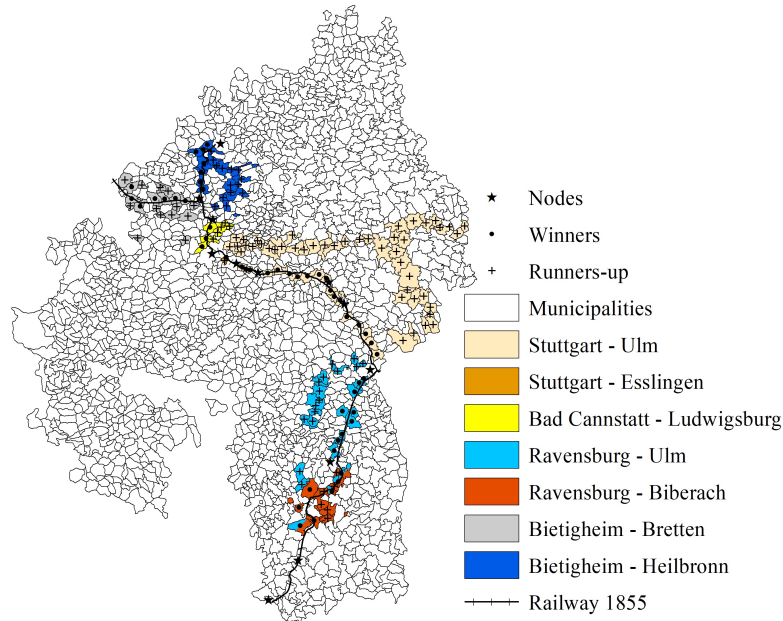


Figure 3.2: Winner and runner-up parishes

Notes: The figure shows railway nodes (stars), ‘winner parishes’ (points) and ‘runner-up parishes’ (crosses). Winners are parishes that obtained railway access in the first construction stage (but are not nodes). Runners-up are parishes with designated railway access on an alternative line that was eventually not built in the first stage of the railway expansion. Colors distinguish between the different cases and mark all potential routes suggested for one railway line. ‘Gaps’ between winners or runners-up along a proposed route arise because not all parishes traversed by a railway line were (meant to be) connected to the railway.

Sources: Dumjahn (1984), Etzel et al. (1985), Kunz and Zipf (2008), Kommission für geschichtliche Landeskunde in Baden-Württemberg and Landesvermessungsamt Baden-Württemberg (1972), and Königliches Statistisches Landesamt (1911). Authors’ design.

runners-up is statistically significantly different from zero (Column (7)) (although we caution that the lack of statistical significance may be a consequence of the relatively small sample size). If anything, winners appear to be smaller and less industrialized than runners-up before the coming of the railway.

Distinguishing, in contrast, between parishes that are and are not located on a straight-line corridor between two railway nodes does not balance pre-treatment characteristics. Appendix Table B.9 shows that parishes on a straight-line corridor (or least cost path) were much more likely to have been connected to pre-railway transport networks. The significant differences in pre-treatment observables suggest that also unobservables may differ between groups. Using location on a straight-line corridor or least-cost path as instrument for railway access, as done in most of the related literature, would then produce biased estimates.

3.4.2 Empirical specification

Differences-in-differences. We begin by estimating the effect of railway access using a differences-in-differences (DiD) strategy, which we describe in the following for the comparison of winner and runner-up parishes. Let $D_{ij,1855}$ be a binary treatment indicator that indicates

whether parish i of case j was connected to the railway by 1855, and let y_{ijt} be an outcome variable of parish i in year t . Our basic DiD specification is:

$$\begin{aligned} y_{ijt} &= \alpha + \lambda_t + \theta_j + \beta D_{ij,1855} + \gamma(D_{ij,1855} \times 1(\tau \geq 0)_{jt}) + \varepsilon_{ijt} \\ &= \alpha + \lambda_t + \theta_j + \beta D_{ij,1855} + \gamma \mathbf{Line}_{ijt} + \varepsilon_{ijt}, \end{aligned} \quad (3.1)$$

where λ_t are year fixed effects and θ_j are case fixed effects. τ denotes years but is normalized so that for each case, the railway line's opening year is $\tau = 0$. $1(\tau \geq 0)_{jt}$ is thus a dummy equal to one for all years after case j 's railway line was opened.¹⁹ The coefficient of interest (γ) captures mean shifts in outcome variables of treatment parishes relative to control parishes after the railway line was opened. For most outcome variables (sectoral employment, specialization, firm size, adoption of steam), we have data for one year before (typically 1829) and one year after the first stage of the construction of the railway network (typically 1895/1907). For population, we use data for 21 census years between 1834 and 1910.

Identification of the impact of the railway in equation (3.1) rests on the assumption that parishes that gained access to the network until 1855 would have developed similarly to other parishes in the absence of railway construction. Formally, OLS estimation of equation (3.1) will yield a consistent estimate of γ if:

Assumption 1 *Strict exogeneity:*

$$\mathbb{E}[\varepsilon_{ijt} | \mathbf{Line}_i, \lambda_t, \theta_j, D_{ij,1855}] = 0 \text{ for all } t \text{ where } \mathbf{Line}_i \equiv [Line_{ij0}, \dots, Line_{ijT}].$$

The similarity of treated and control parishes in their pre-railway outcomes (see Table 3.1) lends credibility to the assumption. We also add parish fixed effects to equation (3.1) to eliminate any unobservable time invariant parish characteristics. In our analysis of the effect of railways on population, we probe the robustness of our estimates to the inclusion of case-by-year fixed effects and test for differences in pre-treatment trends (see below).

Event study analysis. Specification (3.1) tests for a mean shift in our outcome variables. In our population analysis with many periods, the model implicitly assumes that any effect occurs immediately and then remains constant over time. In additional event study regressions, we

¹⁹Our baseline specification abstracts from within-line differences in opening years and takes the first year in which a segment of the line was opened as the opening year of the entire line. This should reduce potential anticipation effects. In a robustness check, we instead use parish-specific opening years and come to similar conclusions. This is to be expected as most segments and stations of a line open within just a short time period.

relax this assumption and allow the effect to vary with the time since treatment by estimating:

$$y_{ijt} = \ddot{\alpha} + \ddot{\lambda}_t + \ddot{\theta}_j + \ddot{\beta}D_{ij,1855} + \sum_{k=-4}^{13} \gamma_k(D_{ij,1855} \times 1(\tau = k)_{jt}) + \sum_{k=-4}^{13} \delta_k 1(\tau = k)_{jt} + \ddot{\varepsilon}_{ijt}. \quad (3.2)$$

Coefficient γ_k for $k \geq 0$ corresponds to the difference in log population between treated and runner-up parishes k periods after the railway line was opened. The difference is expressed relative to four periods before the line was opened (i.e., we normalize γ_{-4} to zero).²⁰ We estimate the specification for $-4 \leq \tau \leq 13$, as the sample is balanced for these periods. Specification (3.2) also tests for differences in trends between treated and control parishes in the periods before the railway line was opened. If our identifying assumption holds, we would expect coefficients γ_k to be statistically indistinguishable from zero for $k < 0$.

Semi-parametric estimates. We can interpret the parameter γ in equation (3.1) as an estimate of the average treatment effect on the treated (ATT). However, this interpretation hinges on the linearity assumption present in equation (3.1). In an alternative strategy, we leave the data-generating process of the outcome variables unspecified and estimate the ATT by inverse probability weighting (IPW) (see Appendix B.9 for technical details). IPW does not specify a model of the outcome of interest but instead focuses on modelling railway access, the treatment. IPW estimates the ATT by comparing *weighted* outcome means of parishes with and without railway access. Intuitively, IPW places more weight on observations in the control group that—given their covariates—had a high probability of being treated in the first place. In a robustness check, we combine regression adjustment and IPW. This so-called inverse probability weighted regression adjustment (IPWRA) approach has the advantage that either the outcome or the treatment model has to be correctly specified, not both (see again Appendix B.9 for details).

The key assumption for IPW (and IPWRA) to yield the causal effect of interest is as follows:

Assumption 2 Unconfoundedness: $(y_{ijt}(1), y_{ijt}(0)) \perp D_{ij,1855} \mid \mathbf{X}_i$.²¹

This assumption states that conditional on a set of observed covariates \mathbf{X}_i , potential outcomes are independent of railway access. Here, $y_{ijt}(1)$ denotes the potential outcome with railway access and $y_{ijt}(0)$ the potential outcome without railway access.

²⁰Historical accounts suggest that railway parishes already experienced a population increase just before the railway line opened, as construction workers gathered in the parishes. That is why we express our estimates relative to the first period in our sample—rather than relative to the period just before the opening of the line.

²¹In addition, we rely on the overlap assumption $\Pr(D_{ij,1855} = 1 \mid \mathbf{X}_i) < 1$ for any value of \mathbf{X}_i . This assumption implies that for all values of the covariates, there is a chance of observing a unit without railway access. Appendix Figure B.6 shows the estimated densities for the propensity of railway access for winners and runners-up. The figure reveals propensity scores well below one and considerable overlap between the two groups.

Assumption 1 specifies a linear model for the outcomes of interest but allows for time-invariant unobserved characteristics. In contrast, Assumption 2 does not specify a model for the outcome variables but requires the set of control variables to be rich enough for potential outcomes to be conditionally independent of treatment status. In contrast to the DiD model, IPW/IPWRA do not necessarily require data on the pre-treatment period, which we lack for agricultural employment, income, wages, and housing values.

Our covariates in \mathbf{X}_i include log population and log population density in 1834, the share of protestants in 1821, a binary variable that indicates a running manufactory or factory in 1832, industrial employment per 100 persons in 1829, the average elevation in meters, and two binary variables that indicate access to a paved road in 1848 and a waterway navigable in 1845 (see Table 3.1 for summary statistics). Case fixed effects ensure that the impact of railway access is identified from within-case comparisons.

3.5 Average Effects of the Railway: Württemberg-wide Results

This section presents our estimation results on the average effects of the railway in Württemberg. We first study the effect on population, consider income, wages, and housing values next, and then turn to industrial development and sectoral employment. Finally, we explore potential localized displacement effects.

3.5.1 Population growth

Differences-in-differences. Table 3.2 presents DiD estimates of the effect of railway access on population. Column (1) reports results from our baseline specification (3.1), restricting the sample to winner and runner-up parishes. The regression suggests that railway access increased the population of winning parishes by 0.091 log points (relative to losing parishes). This effect is statistically significant with a standard error of 0.042.

Specifications (2) and (3) probe the robustness of our results. Specification (2) adds parish fixed effects to control for any unobserved heterogeneity between parishes that is constant over time. Specification (3) further adds year by case fixed effects, which control for case-specific time trends. The estimated treatment effect increases slightly to 0.117 (s.e. of 0.033) and 0.136 (s.e. of 0.032) in specifications (2) and (3), respectively. This increase is consistent with the observation from Table 3.1 that, if anything, winning parishes were slightly smaller and less industrialized than losing parishes—and thus potentially negatively selected. Inference based on Conley standard errors, which account for potential spatial and serial correlation, yields very similar results (see Appendix Table B.7 for details).

Table 3.2: DiD estimates of the effect of railway access on population

	Winners vs. runners-up			Full sample		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment effect	0.091** (0.042)	0.117*** (0.033)	0.136*** (0.032)	0.207*** (0.032)	0.172*** (0.024)	0.139*** (0.023)
Observations	3,276	3,276	3,276	38,766	38,766	38,766
Group FE	Yes	No	No	Yes	No	No
Parish FE	No	Yes	Yes	No	Yes	Yes
Year \times Case/County FE	No	No	Yes	No	No	Yes

Notes: The table shows panel regression estimates of the effect of railway access in 1845–54 on log population. Regressions (1) to (3) are estimated for the winners versus runners-up sample, regressions (4) to (6) for the complete sample excluding railway nodes. All regressions include a full set of year dummies. Regressions (1) and (4) include a dummy for the treatment group; all remaining regressions include a full set of parish dummies. Regression (3) additionally includes case by year fixed effects and regression (6) includes county (*Oberamt*) by year fixed effects. Standard errors clustered at the parish level are in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

Specifications (4) to (6) re-estimate the regressions on the full sample of parishes. At 0.207 (s.e. of 0.032), the baseline DiD estimate for the full sample is more than twice as large as the corresponding estimate for the winners versus runners-up sample (compare Columns (4) and (1) of Table 3.2). This is consistent with the notion that in the full sample, the control group includes many small and remote parishes with unfavorable growth perspectives. The treatment effect in Column (4) is thus likely upward biased. The difference in the treatment effect estimated for the two samples gets smaller when we add parish fixed effects (Column (5)) and vanishes when we also add year by county (*Oberamt*) fixed effects (Column (6)).

Event study and semi-parametric estimates. The event study analysis allows the effect of railway access on population to vary with time since treatment. Figure 3.3 shows the estimated differences in log population between winner and runner-up parishes, relative to the baseline difference four periods before the treatment. Dots indicate point estimates; vertical bands mark 95 percent confidence intervals. We consider three periods before the treatment, the treatment period itself, and thirteen periods after treatment.

Point estimates for the pre-treatment periods are very close to zero and statistically insignificant. Population in winning and losing parishes thus evolve in tandem before the arrival of the railway. This supports our key identifying assumption that in the absence of the railway, population in winning and losing parishes would have grown in parallel.

At the time of the treatment, the point estimate jumps up to 0.036 (s.e. of 0.020). The difference in log population between winner and runner-up parishes then gradually widens with time since treatment. Thirteen periods after treatment (or after about 50 years), population in winner parishes is, on average, 0.248 log points larger than in runner-up parishes (relative to the

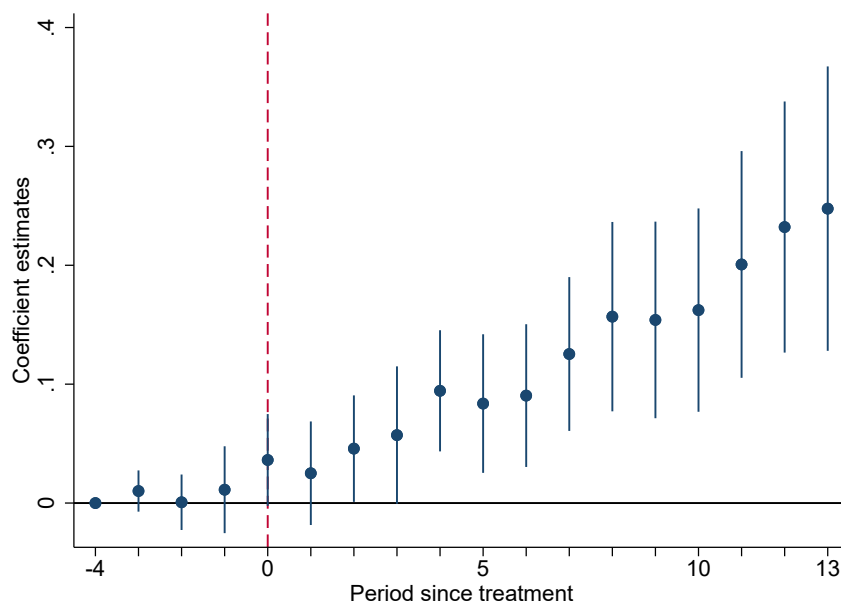


Figure 3.3: Event study estimates of the effect of railway access on log population

Notes: The graph depicts differences in log population between winner and runner-up parishes for pre- and post-treatment periods, as estimated in an event study regression (see Section 3.4.2 for details). Differences are expressed relative to the baseline difference four periods before the treatment. Point estimates are marked by a dot. The vertical bands indicate the 95 percent confidence interval of each estimate. The red dashed vertical line indicates the treatment period.

baseline difference four periods before the treatment). This corresponds to an increase in *annual* population growth of about 0.4 percentage points, which is considerably smaller than existing estimates for Prussian towns (Hornung, 2015).

Appendix B.11 shows that semi-parametric IPW and IPWRA yield results that are almost identical to the event study estimates. This is re-assuring for our subsequent analyses of wages, income, and housing values, which, due to the lack of pre-treatment data, are based on cross-sectional estimates only. Overall, our results for the winner versus runners up sample suggest that getting railway access in the first construction stage had lasting effects on population growth and that much of the cumulative effect of railway access on population occurred decades after the treatment. Appendix B.12 shows that we find similar results for the full sample.

Natural population growth vs. immigration. Appendix B.13 presents suggestive evidence that the positive effect of the railway on parish level population is mainly driven by immigration, in line with spatial equilibrium models. In contrast, we find no statistically significant difference in fertility and mortality rates between winners and runners-up in 1871–1910.

Gaining railway access late. Following much of the literature (see, e.g., Berger and Enflo, 2017; Hornung, 2015), our analysis has focused on the effect of gaining railway access in the first construction stage. Appendix B.14 shows for the full sample that gaining access in the second stage from 1857 to 1886 also boosted population. However, the effect for the second stage is

somewhat smaller than for the first. This is not surprising, as the most important lines, especially for transit passengers and freight, had been built already in the first construction phase.

The positive effect of later railway access on population does not carry over to the winner versus runner-up sample (see again Appendix B.14). Runner-up parishes that gained access in 1857–86 did not grow faster than runner-up parishes that remained without access by 1886. This might be because runner-up parishes are located along alternative routes between major towns that had already been connected to the network in 1845–54. Building initially unrealized ‘alternative routes’ between these towns did not boost population along the way, probably precisely because the winning lines were already in operation.

3.5.2 Income, wages and housing values

We next analyze the effect of railway access on income, wages, and housing values. In spatial equilibrium, we would expect increases in population to go hand in hand with increases in income and housing values. Our analysis in this section relies on cross-sectional models only, as we lack comparable data for the pre-treatment period. We focus on the more comparable parishes in the winners versus runners-up sample and report qualitatively similar findings for the full sample in Appendix B.17.

Day laborer wage. We first consider the effect of railway access on the average daily wage of day laborers in 1884, 1898, and 1909, distinguishing between females and males (see Columns (1) to (6) in Table 3.3). Estimates from IPW (Panel A), IPWRA (Panel B) and OLS (Panel C) models all suggest that railway access had a statistically significant positive wage effect. IPW estimates indicate that access increased female day laborer wages by 7.0 (s.e. of 2.2), 9.7 (s.e. of 2.2), and 8.2 *Pfennig* (s.e. of 2.6) in 1884, 1898, and 1909, respectively. This corresponds to an increase of 6.3, 8.3, and 4.9 percent, respectively, relative to the control group average. The relative effect is somewhat lower for male day laborers, ranging from 3.3 to 5.5 percent. These results suggest that railway-induced industrialization benefited also the working class (see Leonard and Ljungberg, 2010, for a discussion of living standards in Europe in 1870–1914).²²

The higher treatment effect for females translates into a statistically significant lower gender wage gap in winning parishes of 2.0 and 1.7 percentage points in 1884 and 1898 (from a baseline of 32.8 percent, see Appendix Table B.8 for detailed results). The lower gender wage gap

²²An alternative interpretation of this result is that the railway-induced decrease in travel costs fostered oversea emigration, thereby decreasing local labor supply. In fact, Karadja and Prawitz (2019) have recently shown that mass emigration to the US increased wages of low-skilled workers in Sweden. They use the interaction of severe local agricultural shocks and proximity to emigration ports to predict emigration. In our context, however, railway access increased net migration into local parishes (see Appendix B.13), so that a demand-based explanation seems more plausible.

Table 3.3: The effect of railway access on day laborer wages, taxable income, and building values

	Day laborer wage (<i>Pfennig</i>)						Taxable income (<i>Mark</i>) 1907 (7)	Building value (<i>Mark</i>) 1907 (8)	Fire insurance value (<i>Mark</i>) 1908 (9)
	Female		Male						
	1884 (1)	1898 (2)	1909 (3)	1884 (4)	1898 (5)	1909 (6)			
Panel A: IPW									
Treatment effect	7.013*** (2.197)	9.741*** (2.174)	8.171*** (2.592)	5.463** (2.707)	9.669*** (3.159)	9.694** (4.545)	48.305*** (14.724)	1,388.4*** (282.9)	1,749.0*** (379.2)
Panel B: IPWRA									
Treatment effect	7.496*** (2.381)	9.962*** (2.268)	8.225*** (2.542)	5.714** (2.829)	9.855*** (3.146)	9.679** (4.521)	49.946*** (14.204)	1,388.2*** (283.5)	1,718.9*** (385.9)
Panel C: OLS									
Treatment effect	8.055*** (2.007)	11.637*** (2.165)	9.831*** (2.517)	6.480** (2.512)	11.939*** (3.040)	12.239*** (4.212)	57.598*** (14.111)	1,573.5*** (293.3)	1,881.0*** (392.6)
Observations	155	156	156	156	156	156	156	156	156
Control mean	111.43	116.72	167.22	165.96	174.34	254.14	366.11	3,150.4	4,391.6

Notes: The table shows regression estimates of the effect of railway access in 1845–54 on the average daily wage of female (Columns (1) to (3)) and male (Columns (4) to (6)) day laborers in 1884, 1898, and 1909, on taxable income per capita in 1907 (Column (7)), the average value of buildings in 1907 (Column (8)), and the average fire insurance value per building in 1908 (Column (9)). Values in Columns (1) to (6) are in *Pfennig* and values in Columns (7) to (9) are in *Mark*, with 1 *Mark* = 100 *Pfennig*. Regressions in Panel A are estimated by IPW, regressions in Panel B by IPWRA and regressions in Panel C by OLS. All regressions include as control variables log population and log population density in 1834, the share of protestants in 1821, a dummy for having a factory in 1832, industry employment per 100 persons in 1829, elevation, dummies for access to a navigable river in 1845 and to a road in 1848, and case dummies. Robust standard errors are in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

is consistent with the idea that falling transport costs induced mechanization, which in turn increased the relative productivity of women by reducing the importance of human strength in production (Goldin, 1990; Galor and Weil, 1996; Juhn et al., 2014).

Taxable income. We next consider taxable income per capita in 1907. IPW and IPWRA estimates suggests that railway access increased the annual taxable income in winning parishes by 48.3 and 49.9 *Mark*, respectively, or by 13.2 and 13.6 percent relative to the control mean (see Column (7) of Table 3.3). The relative increase in taxable income is thus comparable to the increase in day laborer wages. Overall, our findings suggest that railway access boosted regional nominal income, which might then have triggered population inflows.

Housing values. Finally, we study the effect of railway access on housing values. In spatial equilibrium, higher nominal wages are offset by higher living costs, of which housing is an important part. Columns (8) and (9) of Table 3.3 show the treatment effect on the average market value of buildings in 1907 and the fire insurance value in 1908, respectively. Railway access increased the average building value by 1388.4 *Mark* or 44.1 percent (IPW estimate in Panel A). The increase in insurance value per building is of comparable size.

3.5.3 Industrial development and sectoral employment

We next study the effect of railway access on structural change from agriculture to industry—a core characteristics of the Industrial Revolution and source of agglomeration economies in new economic geography models (Helpman, 1998). We also provide evidence on specific sectors identified in the literature as drivers of Germany’s industrialization process. We again restrict the discussion to the winners versus runners-up sample and report the results for the full sample in Appendix B.17.²³

Industrial development. We first study the effect of railway access on various measures of industrial development. Panels A and B of Table 3.4 present results from IPW and IPWRA estimations and Panel C from DiD regressions (with one pre- and one post-treatment period).

Results in Columns (1) to (4) show that railway access accelerated the transition from agriculture to industry.²⁴ The IPW estimates in Column (1) of Panel A imply that railway access increased industry employment in winning parishes by 2.8 employees per 100 persons (s.e. of

²³The latter are qualitatively similar but typically larger (in absolute magnitude) and more precisely estimated than the results for the winners versus runners-up sample. An exception is specialization: We find a positive and statistically significant effect of railway access on specialization in the full sample but not in the winners versus runners-up sample.

²⁴In addition to the railway, recent econometric work has highlighted the importance of education (Becker et al., 2011) and banking (Heblich and Trew, 2019) for industrial employment growth in Prussia and England, respectively.

Table 3.4: The effect of railway access on industrial development

	Employment				Estab- lishment size (logs)	Steam engine	
	Industry		Agriculture			(0/1)	HP pc
	1895 (1)	1907 (2)	1895 (3)	1907 (4)	1895 (5)	1869 (6)	1869 (7)
Panel A: IPW							
Treatment effect	2.794** (1.426)	2.789* (1.465)	-3.741** (1.513)	-6.740*** (2.037)	0.227** (0.094)	0.206*** (0.065)	14.48** (5.719)
Panel B: IPWRA							
Treatment effect	2.889** (1.287)	2.827** (1.343)	-3.748*** (1.393)	-6.546*** (1.879)	0.228** (0.093)	0.202*** (0.064)	14.52** (5.679)
Observations	156	156	156	156	156	156	156
Panel C: Panel estimates							
Treatment effect	2.794* (1.430)	2.847* (1.590)	–	–	0.189 (0.119)	0.126 (0.082)	13.70* (7.366)
Observations	312	312			311	312	312
Control mean	14.81	17.47	23.71	26.43	0.836	0.253	2.207

Notes: The table shows estimates of the effect of railway access in 1845–54 on the number of full-time employees in industry (Columns (1) and (2)) and agriculture (Columns (3) and (4)) per 100 persons in 1895 and 1907, establishment size in industry in 1895 (Column (5)), the probability of having installed at least one steam engine by 1869 (Column (6)), and steam horsepower per 1,000 persons in 1869 (Column (7)). Establishment size is the average number of persons employed in a main plant (*Hauptbetrieb*). Panels A and B display IPW and IPWRA estimates, respectively. Regressions in Panels A and B include as control variables log population and log population density in 1834, the share of protestants in 1821, a dummy for having a manufactory in 1832, elevation, dummies for access to a navigable river in 1845 and to a road in 1848, and case dummies. Panel C displays estimates from panel fixed effects regression that include parish and year fixed effects. The pre-treatment period is 1829 in Columns (1) to (5) and 1846 in Columns (6) and (7). We cannot run panel fixed effects regression for agricultural employment, as we lack data on agricultural employment in the pre-treatment period. The control mean gives the mean value of the outcome for the control group in 1895 (Columns (1), (3), (5)) 1907 (Columns (2) and (4)) and 1869 (Columns (6) and (7)). Robust standard errors are in parentheses. Standard errors in Panel C are clustered at the parish level. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

1.4) or 18.9 percent relative to the 1895 average in losing parishes. The percent increase in industrial employment in 1907 (Column (2)) is very similar to that in 1895. Increased industrial employment came at the expense of agricultural employment (Columns (3) and (4)). The IPW estimate in Column (2), for instance, implies that railway access decreased the number of full-time employees in agriculture by 3.7 employees per 100 persons (s.e. of 1.5) or 15.8 percent relative to the control group average in 1895. This negative effect increases to 6.7 employees or 25.5 percent in 1907.²⁵ The results of the IPWRA (Panel B) and DiD regressions (Panel C) are almost identical to the IPW estimates.

In the last three columns of Table 3.5, we analyze the effect of railway access on plant size and the local use of steam power. Falling transport costs are widely believed to have increased

²⁵The IPW/IPWRA estimates seem to suggest that railway access decreased the total number of full-time employees per 100 person, as the employment decline in agriculture is larger than the increase in industry. Unreported IPW/IPWRA estimations show, however, that the total employment effect of railway access is statistically indistinguishable from zero, as access also increased employment in trade and the public sector.

optimal establishment size by integrating markets and expanding market size.²⁶ The ensuing competitive pressures, so the argument, forced firms to increase productivity through the division of labor and mechanization—and thus promoted the rise of factories (Atack et al., 2011). In line with this argument, we find that railway access increased establishment size by between 0.189 and 0.228 log points compared to losing parishes (Column (5) of Table 3.5).

We also find strong evidence that the railway accelerated local technological change. IPW/IPWRA estimates suggest that railway access increased the likelihood of having at least one steam engine in operation in 1867 by more than 20 percentage points (from a baseline of 25.3 percent in losing parishes) and the total steam power installed by 14.5 horsepower per 1,000 persons (from a baseline of 2.2). Railway access might have lowered the costs of coal shipments sufficiently for steam powered industrial growth to happen outside the coal mining regions (Gutberlet, 2014).

Employment in key industries. Our unusually disaggregated data from the *Gewerbestatistik* allow us to further study the effect of railways on employment in specific industries. We focus on the sectors that prior literature has identified as central in the different phases of Germany’s industrialization process, namely the textile industry; coal, iron and steel; machine building; the electrical industry; and the chemical industry (see, e.g., Fremdling, 1977; Ziegler, 2012). Table 3.5 shows results from IPW (Panel A), IPWRA (Panel B) and DiD (Panel C) models.

Column (1) suggests that railway access boosted the local textile industry, Württemberg’s most important industry at the dawn of the Industrial Revolution and a key sector in Germany’s early industrialization process. Average 1895 employment in the textile sector (excluding fiber production) was 4.3 employees in winning parishes but only 2.0 employees in losing parishes, whereas textile employment was virtually identical in the pre-treatment period 1829 (1.9 and 2.0 in winning and losing parishes, respectively). The railway expanded the market for textile exports and enabled coal to be transported to power steam engines. In fact, the textile industry used 37.8 percent of the steam power installed in Württemberg in 1875 (Kaiserliches Statistisches Amt, 1879).

In contrast, the railway had no effect on the coal, iron, and steel industry, which played a core role in Germany’s growth take-off during the third quarter of the 19th century (Broadberry et al., 2008; Ziegler, 2012). If anything, the effect is negative (though not statistically significant, see Column (2)). This result might seem surprising, as railways boosted the German coal, iron

²⁶Keller and Shiue (2008) show that the effect of railways on market integration, as measured by price convergence between 68 European markets, was indeed positive—and substantially larger than the effect of trade liberalization and currency agreements.

and steel production (Fremdling, 1985). However, Württemberg lacked coal deposits. After the railway markedly decreased transport costs, Württemberg's steel and iron producers were no longer able to compete with the cheaper producers located in the resource-rich Ruhr and Saar regions. Consequently, Württemberg's share in the German pig iron and steel production plummeted from 2.6 percent in 1850 to 0.2 percent around 1895 (von Hippel, 1992).

Table 3.5: The effect of railway access on employment in key industrial sectors and specialization

	Employment in key industrial sectors					
	Textile (1)	Coal, iron & steel (2)	Machines & instruments all (3)	electrical (4)	Chem- ical (5)	Spec- ialization (6)
Panel A: IPW						
Treatment effect	2.491* (1.333)	-0.051 (0.107)	0.530** (0.240)	0.008 (0.009)	0.092 (0.091)	-0.020 (0.018)
Panel B: IPWRA						
Treatment effect	2.549* (1.308)	-0.071 (0.128)	0.527** (0.239)	0.008 (0.009)	0.091 (0.091)	-0.018 (0.018)
Observations	156	156	156	156	156	156
Panel C: Panel estimates						
Treatment effect	2.435 (1.558)	-0.120 (0.203)	0.475 (0.326)	0.007 (0.009)	0.077 (0.098)	-0.010 (0.019)
Observations	312	312	312	312	312	311
Control mean	1.959	0.251	0.112	0.002	0.048	0.161

Notes: The table shows estimates of the effect of railway access in 1845–54 on the number of full-time employees per 100 persons in different industries (Columns (1)-(5)) and specialization within industry (Column (6)) in 1895. We distinguish between employment in the textile industry (Column (1)), coal, iron, and steel industry (Column (2)), building of machines and instruments (Column (3)), building of electrical machines and instruments (Column (4)), and the chemical industry (Column (5)). Specialization is measured by the Hirschman-Herfindahl-Index (with $\alpha = 2$). Panels A and B display IPW and IPWRA estimates, respectively, using employment in 1895 as outcome variable. Regressions in Panels A and B include as control variables log population and log population density in 1834, industry employment per 100 persons in 1829, a dummy for having a manufactory in 1832, the share of protestants in 1821, elevation, dummies for access to a navigable river in 1845 and to a road in 1848, and case dummies. Panel C displays estimates from panel fixed effects regression that include parish and year fixed effects. The pre-treatment period is 1829. The control mean gives the mean value of the outcome for the control group in 1895. Standard errors in Panel C are clustered at the parish level. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

Column (3) shows that railway access strongly increased employment in the local machine and instrument building industry (by about 0.5 employees per 100 persons from a baseline of 0.1 employees), which was an important driver of economic growth in Germany both during the earlier and later stages of the Industrial Revolution. The drive towards mechanization and the expansion of the railway were pivotal for the rise of Württemberg's machine and instrument industry since the mid-19th century (von Hippel, 1992). In fact, Württemberg's largest industrial company at the time, the *Maschinenfabrik Esslingen*, was founded in 1846 to produce locomo-

tives for Württemberg’s public railway company.²⁷ Since Württemberg’s machine industry was export-oriented, it also benefited from the falling transport costs brought about by the railway.

The late 19th century saw the rise of the electronic and chemical industry in Germany, which gradually replaced the heavy industry as the leading sector in Germany’s industrialization process. Yet, both industries were still small in Württemberg in 1895, counting 821 (electronic) and 2232 (chemical, excluding pharmacies) employees in the entire state. Columns (4) and (5) show that railway access is positively associated with employment in the two industries, but the estimates are not statistically significantly different from zero and small in absolute magnitude.

Finally, Column (6) in Table 3.5 reports evidence that the degree of specialization within industry is lower in winning than in losing parishes. However, the estimates are imprecise and not statistically significant at conventional levels.

3.5.4 Localized displacement

One important topic, which has received comparably little attention in the literature, are potential displacement effects of railways. In particular, railway access might induce growth in economic activity or reorganize existing economic activities across space. Distinguishing between growth and reorganization requires additional identifying assumption in reduced-form analyses. One potential assumption is that railway access has no effect on far-away parishes (Redding and Turner, 2015). Positive effects in the immediate vicinity of the railway will then come at the expense of locations in middle distances if railways indeed cause reorganization. Berger and Enflo (2017), Bogart et al. (2019), and Büchel and Kyburz (2020) apply this idea to test for reorganization of population. We extend their analysis by studying also potential displacement effects in income, building values, and industrial employment shares. Estimates from local polynomial regressions yield little evidence for localized displacement effects in our finely grained spatial data. Appendix Section B.15 discusses these results in more detail.

3.6 Heterogeneous Effects of the Railway

This section analyzes heterogeneities in the economic effects of the railway within Württemberg. Given that the analyses are demanding on the data, we focus on the full sample of parishes (results based on the winners vs. runners-up sample are qualitatively similar). Specifically, we test whether the effects of railway access were larger for parishes that already had a manufactory in 1832 and for parishes with above-median population²⁸ in 1843, in line with arguments in Gutberlet (2013b) and Ziegler (2012). In particular, we might expect that agglomeration forces drew

²⁷Esslingen is, however, a railway node and thus excluded from the analysis.

²⁸We focus on population in the main locality in case a parish has several localities (*Wohnplätze*).

economic activity to the established industrial centers, as transport costs decreased (Krugman, 1991).

In addition, we test for effect heterogeneity by railway line, distinguishing between the northern line (Stuttgart-Bietigheim-Heilbronn/Bretten), the eastern line (Stuttgart-Esslingen-Ulm) and the southern line (Ulm-Friedrichshafen) (see Figure B.3 in the Appendix). The eastern line formed arguably the core of the network. It connected the densely populated Neckar basin, where much of the important textile industry was concentrated already before the railway era (Feyer, 1973), with Ulm, Württemberg's second largest town. It is thus not surprising that the eastern line was the busiest section of Württemberg's railway network, benefiting also from the transit traffic between Baden and Bavaria. The southern line from Ulm to Friedrichshafen, in contrast, was much less frequented as it served the sparsely populated and hardly industrialized south-east of Württemberg.²⁹ We thus expect that parishes along the eastern line benefited more from the railway than those along the southern line.

Population. Table 3.6 studies heterogeneity in the effect of railway access on population. Column (1) reproduces the results from our full-fledged DiD specification with parish and county by year fixed effects (from Column (6) of Table 3.2). The remaining specifications add interactions between the treatment effect dummy and dummies for having a manufactory in 1832 (Column (2)), above-median population in 1843 (Column (3)) and being located along the northern and southern lines (Column (4)). Parish fixed effects absorb the corresponding main effects.

Column (2) suggests that the positive effect of railway access on population is almost three times larger for parishes that already had a manufactory in 1832 (0.343 log points relative to a baseline effect of 0.121 log points for parishes without a manufactory). This is consistent with the idea that existing industrial centers particularly benefited from the railway-induced increase in market access. Moreover, the effect of railway access on population is 0.081 log points larger in bigger than in smaller parishes (Column (3)) although the difference is only statistically significant at the 10 percent level.

Finally, Column (4) shows that the railway's growth-enhancing effect that we have documented for Württemberg as a whole is mostly driven by the eastern and, to a lesser degree, the northern line. While railway access increased population of parishes along the eastern line by 0.228 log points (s.e. of 0.048), the increase is much smaller for parishes along the southern

²⁹Transport statistics from 1868/69 illustrate the difference between the lines (Königliches Statistisches Landesamt, 1874). Looking at internal freight with origin and destination in Württemberg, railways transported 4,487,810 centners per mile per year on the eastern line but only 2,106,534 centners on the northern, and 1,903,806 centners on the southern line. Differences are even more striking if we consider freight with origin and/or destination outside Württemberg. Such freight amounted to 4,277,782 and 4,765,137 centners on the eastern and northern line, respectively, but to only 480,872 centners on the southern line.

Table 3.6: Heterogeneous effects of railway access on population, DiD estimates

	(1)	(2)	(3)	(4)
Treatment dummy	0.139*** (0.023)	0.121*** (0.023)	0.099*** (0.025)	0.228*** (0.048)
Treatment dummy × Manufactory 1832		0.233*** (0.070)		
Population 1843 > Treatment group median			0.081* (0.046)	
Northern line				-0.089 (0.065)
Southern line				-0.159*** (0.052)

Notes: The table shows panel regression estimates of the effect of railway access in 1845–54 on log population, based on the full sample excluding railway nodes (38,766 observations). All regressions include a full set of parish dummies and county by year fixed effects. Regression (1) reproduces our baseline results from Column (6) of Table 3.2. Regressions (2)–(4) add interaction terms between the treatment effect dummy and (pre-railway or time-invariant) parish characteristics, namely the existence of a manufactory in 1832 (regression (2)), above-median population in 1843 (regression (3)), and location along the northern and southern line (regression (4)). Standard errors clustered at the parish level are in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

line (0.069 log points, s.e. of 0.023). The railway thus aggravated regional imbalances between Württemberg’s densely populated Neckar basin and the sparsely populated southwest.

Income, wages and housing values. Table 3.7 tests for heterogeneity in the effect of railway access on wages, income and housing values. Panel A reports average effects for Württemberg as a whole, Panels B, C and D consider heterogeneity by manufactory location in 1832, population in 1843, and railway line, respectively. We interact the treatment dummy with the relevant parish characteristics and include these characteristics as additional controls in our (cross-sectional) regressions.

The results mirror our findings for population. We find that the positive effects of railway access on wages, income, and housing values are 2-3 times larger for parishes that already had a manufactory in 1832 (Panel B of Table 3.7), although differences in effect size are relatively imprecisely estimated, especially for income and building values. Effect heterogeneity by population (Panel C) is also sizable but tends to be smaller than by manufactory location. Finally, we find strong and precisely estimated differences between parishes located along the eastern and southern line (Panel D). As a point in case, we find no statistically significant effect of railway access on wages of day laborers for parishes along the southern line. In contrast, access increased female and male day laborer wages along the eastern line by 14.96 and 26.24 *Pfennig*, respectively (or 9.0 and 10.6 percent relative to the control mean).

Table 3.7: Heterogeneous effects of railway access on day laborer wages, taxable income, and building values

	Day laborer wage (<i>Pfennig</i>)		Taxable income	Building value	Fire insur- ance value
	Female	Male	(<i>Mark</i>)	(<i>Mark</i>)	(<i>Mark</i>)
	1909	1909	1907	1907	1908
	(1)	(2)	(3)	(4)	(5)
Panel A: Average effects					
Treatment dummy	9.850*** (2.154)	16.087*** (3.670)	61.462*** (12.474)	1,226.9*** (250.2)	1,497.0*** (313.1)
Panel B: Heterogeneity by manufactory 1832					
Treatment dummy	8.411*** (2.119)	13.913*** (3.728)	55.835*** (12.628)	1,106.8*** (249.5)	1,291.0*** (307.0)
Treatment dummy × Manufactory 1832	17.748** (8.504)	26.797** (11.549)	69.362 (50.952)	1,480.3 (1,132.3)	2,539.4* (1,322.8)
Panel C: Heterogeneity by population 1843					
Treatment dummy	5.702* (2.965)	11.727** (5.186)	32.766** (13.154)	795.1*** (159.3)	1,210.9*** (332.8)
Treatment dummy × Population 1843 > Treatment group median	8.470** (4.077)	8.902 (7.062)	58.591** (23.489)	881.6* (480.1)	584.1 (599.5)
Panel D: Heterogeneity by railway line					
Treatment dummy	14.959*** (2.973)	26.236*** (3.836)	117.623*** (21.927)	2,207.0*** (482.5)	2,887.5*** (669.2)
Treatment dummy × Northern line	2.522 (4.704)	-5.130 (8.272)	-58.645* (29.985)	-1,169.1* (695.9)	-1,811.9** (835.4)
Southern line	-16.598*** (4.025)	-23.651*** (6.718)	-103.051*** (26.715)	-1,664.3*** (498.9)	-2,220.6*** (709.0)
Observations	1,843	1,843	1,843	1,843	1,843
Control mean	165.76	246.96	320.96	2,763.9	3,794.0

Notes: The table shows OLS regression estimates of the effect of railway access in 1845–54 on the average daily wage of female (Column (1)) and male (Column (2)) day laborers in 1909, on taxable income per capita in 1907 (Column (3)), the average value of buildings in 1907 (Column (4)), and the average fire insurance value per building in 1908 (Column (5)). Values in Columns (1) and (2) are in *Pfennig* and values in Columns (3) to (5) are in *Mark*, with 1 *Mark* = 100 *Pfennig*. All regressions are estimated by OLS and include as control variables log population and log population density in 1834, the share of protestants in 1821, a dummy for having a manufactory in 1832, industry employment per 100 persons in 1829, elevation, dummies for access to a navigable river in 1845 and to a road in 1848, a dummy indicating whether population in the main location of residence in 1843 was above the treatment group median, and dummies for geographic location within Württemberg. Regressions in Panel (A) report average effects of railway access. Regressions in Panels (B)–(D) add interaction terms between the treatment effect dummy and (pre-railway or time-invariant) parish characteristics, namely the existence of a manufactory in 1832 (Panel (B)), above-median population in 1843 (Panel (C)), and location along the northern and southern line (Panel (D)). Robust standard errors are in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

Industrial development. We conclude this section with studying heterogeneity in the effect of railway access on industrial development. All estimates in Table 3.8 are from panel fixed effects regression that include parish and year by county fixed effects. Panel A reports average, Panels B to D heterogeneous effects.

Effect heterogeneity is particularly striking between parishes with and without a manufactory (Panel B). In particular, railway access boosted steam powered industrial growth much more strongly in parishes that already had a manufactory in 1832. In fact, all manufactory parishes that gained railway access by 1855 had a steam engine installed by 1869 (while none of them had in 1846). This is consistent with earlier evidence that larger establishments were more

Table 3.8: Heterogeneous effects of railway access on industrial development

	Industry employment		Spec- ialization	Estab- lishment size (logs)	Steam engine	
	1895	1907			(0/1)	HP pc
	(1)	(2)	1895	1895	1869	1869
Panel A: Average effects						
Treatment dummy	5.699*** (0.909)	6.738*** (0.834)	0.031** (0.015)	0.460*** (0.083)	0.215*** (0.062)	6.230** (2.930)
Panel B: Heterogeneity by manufactory 1832						
Treatment dummy	5.156*** (0.922)	6.251*** (0.821)	0.025 (0.016)	0.396*** (0.081)	0.160*** (0.061)	4.418 (2.892)
Treatment dummy × Manufactory 1832	7.063*** (2.230)	6.327** (3.120)	0.076** (0.035)	0.836*** (0.305)	0.791*** (0.071)	26.074*** (10.389)
Panel C: Heterogeneity by population 1843						
Treatment dummy	3.677*** (1.103)	5.103*** (1.116)	0.007 (0.016)	0.244** (0.109)	0.152** (0.077)	5.527* (2.852)
Treatment dummy × Population 1843 > Treatment group median	4.126** (1.703)	3.339** (1.560)	0.049* (0.029)	0.440*** (0.162)	0.137 (0.128)	1.523 (6.260)
Panel D: Heterogeneity by railway line						
Treatment dummy	9.745*** (1.822)	10.752*** (1.724)	0.085*** (0.028)	0.842*** (0.180)	0.419*** (0.132)	20.916** (9.785)
Treatment dummy × Northern line	-3.926 (2.439)	-4.586** (2.186)	-0.055 (0.043)	-0.382* (0.221)	-0.210 (0.174)	-19.789** (9.811)
Southern line	-7.492*** (2.039)	-6.834*** (1.963)	-0.098*** (0.031)	-0.697*** (0.197)	-0.333** (0.151)	-20.468** (9.726)
Observations	3,692	3,686	3,318	3,318	3,692	3,692
Control mean	9.629	11.058	0.171	0.478	0.073	1.002

Notes: The table shows regression estimates of the effect of railway access in 1845–54 on the number of full-time employees in industry per 100 persons in 1895 and 1907 (Columns (1) and (2)), specialization within industry in 1895 (Column (3)), establishment size in industry in 1895 (Column (4)), the probability of having installed at least one steam engine by 1869 (Column (5)), and steam horsepower per 1,000 persons in 1869 (Column (6)). All estimates are from panel fixed effects regression that include parish and year by county fixed effects. The pre-treatment period is 1829 in Columns (1) to (4) and 1846 in Columns (5) and (6). The control mean gives the mean value of the outcome for the control group in 1895 (Columns (1), (3) and (4)), 1907 (Column (2)) and 1869 in Columns (5) and (6). Regressions in Panel (A) report average effects of railway access. Regressions in Panels (B)–(D) add interaction terms between the treatment effect dummy and (pre-railway or time-invariant) parish characteristics, namely the existence of a manufactory in 1832 (Panel (B)), above-median population in 1843 (Panel (C)), and location along the northern and southern line (Panel (D)). Robust standard errors clustered at the parish level are in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

likely to adopt steam power (Atack et al., 2008). We also find that the effect of railway access on establishment size is three times larger in manufactory parishes (1.232 log points vs. 0.396 points). Overall, the railway boosted industrial development especially in existing industrial centers. This might be because the large, export-oriented manufactories in these centers benefited disproportionately from larger market access and could better withstand increasing competitive pressures as transport costs fell. Table 3.8 also shows that the railway increased specialization in the relatively developed industrial centers but not elsewhere. This is broadly consistent with the literature that finds a U-shape in specialization in the development process (Cadot et al., 2011; Imbs and Wacziarg, 2003).

Railway access also had stronger positive effects on industrial development in larger than in smaller parishes (Panel C). However, differences are less pronounced than between parishes with

and without a manufactory, especially regarding the adoption of steam. Panel D highlights once more the large differences in the effect that railway access had on parishes along the eastern and southern line. For instance, we find that by 1895, railway access had increased industry employment by 9.7 employees per 100 persons along the eastern but by only 2.3 employees along the southern line. Likewise, access increased the probability of adopting a steam engine by almost 42 percentage points along the eastern line but by only 8.6 points along the southern line. Overall, the results confirm that the railway increased regional disparities between the more and less industrialized regions of Württemberg.

3.7 Discussion and concluding remarks

This paper has provided a comprehensive analysis of the average and heterogeneous effect of railway access on local growth and industrialization in Württemberg during the Industrial Revolution. Figure 3.4 summarizes our two key findings and puts them into perspective. The graph depicts differences in log population between railway and non-railway parishes in 1834–1910 (as estimated in panel regression with parish and county by year fixed effects on our full sample). Blue dots mark the differences estimated for the average railway parish, red diamonds those estimated for railway parishes that had a manufactory in 1832.

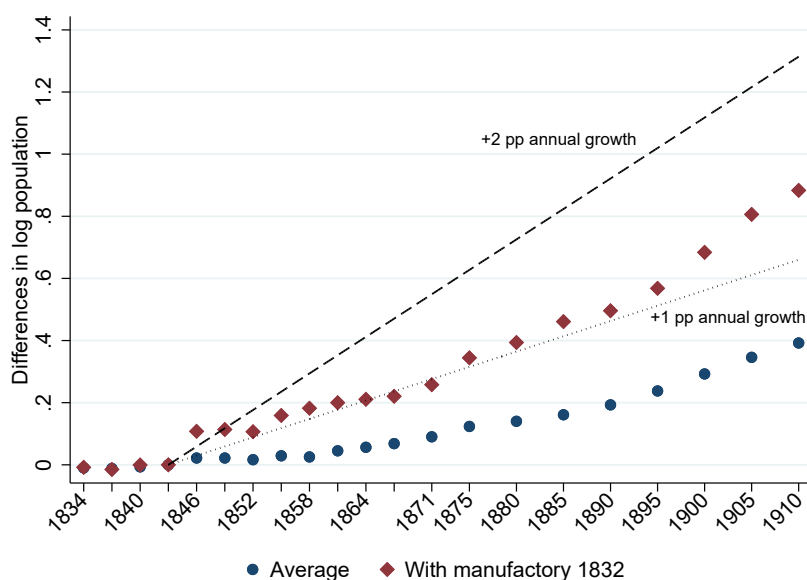


Figure 3.4: Differences in log population between railway and non-railway parishes, 1834–1910

Notes: The graph depicts differences in log population between railway and non-railway parishes in 1834–1910, as estimated in panel regression with parish and county by year fixed effects. Blue dots mark the differences estimated for the average railway parish, red diamonds those estimated for railway parish that had a manufactory in 1832. Estimates are based on the full sample excluding railway nodes. 1843 serves as baseline period. The black dotted and dashed lines show how differences in log population would have evolved over time if railway access had increased annual population growth by 1 and 2 percentage points, respectively.

Blue dots show that parishes that gained access to the railway in the first construction stage grew faster than parishes that did not. However, the growth effect was relatively modest in the first decades after the first railway line opened in 1845. It then accelerated in the last quarter of the 19th century. By 1910, early railway access had increased parish level population growth by, on average, 0.39 log points. This corresponds to an increase in annual population growth of 0.6 percentage points between 1843 and 1910.

To put our finding in perspective, the dotted and dashed lines depict the hypothetical differences in log population that would have occurred had railway access increased annual population growth by 1.0 and 2.0 percentage points, respectively. These values reflect the range of earlier estimates of the effect of railway access on urban population growth in Prussia (Hornung, 2015).

The first main take-away from Figure 3.4 is that our estimates for population growth in Württemberg are considerably smaller than earlier estimates for Prussian towns. If we consider only the period until 1871, as Hornung (2015) does, railway access increased annual parish level population growth in Württemberg by just 0.3 percentage points, compared to Prussia's 1–2 percentage points. In light of the core role ascribed to the railway in Germany's industrialization process (Rostow, 1962; Fremdling, 1977; Ziegler, 2012), the comparably small growth effects in Württemberg might partly explain why the Kingdom was still relatively poor by German standards in the early 20th century. The growth-enhancing effect of the railway was simply larger in other parts of Germany.

Why did Württemberg benefited less from the railway than Prussia? A first explanation is the belated construction of Württemberg's railway network and its limited length. In Prussia, 21 railway lines were built in 1838–1848, connecting major cities such as Berlin and Hamburg or Magdeburg and Leipzig. In contrast, Württemberg's railway network was limited to short sections around Stuttgart in 1848. This delayed construction of the railway network has been put forward as a potential cause for Württemberg's late industrialization (Naujoks, 1982).

Württemberg's railway network then gradually expanded in the second half of the 19th century. By 1868, the density of the railway network was actually higher in Württemberg than in Prussia (see Appendix Figure B.5 for a comparison of network density, i.e., length per square km of land area, in Bavaria, Prussia, and Württemberg in 1848–1903). This might explain why the growth effects of early railway access increased over time, as shown in Figure 3.4. Parishes along the main railway lines might have benefited more and more from the positive externalities of a growing network.

However, such positive network externalities were presumably smaller in Württemberg than in Prussia, as the latter had the much larger network. Although the railway networks of the different German states were connected, railway administrations were not unified before 1920. This caused significant frictions in the railway traffic between German states, as time tables lacked co-ordination and price systems differed even after the foundation of the German Empire in 1871 (Weichlein, 2004; Ziegler, 2012). Without a comprehensive integration of the German railway network, positive externalities were presumably largest in the dominant Prussian network.

Our empirical finding of strong heterogeneity in the effect of railway access provides a second explanation for why we observe small average growth effects in Württemberg. Figure 3.4 illustrates that the effect of railway access on population growth was much stronger in parishes that had a manufactory in 1832. For these industrial parishes, we estimate growth effects that are well within the 1–2 percentage points range reported for Prussia. Yet, only very few parishes in Württemberg had a manufactory already in the pre-railway era. This relative backwardness might have limited the overall growth effect of the railway. Württemberg simply lacked the coal-mining and industrial regions that benefited most from the tight interplay between the railway and heavy industries in Germany (Gutberlet, 2013b; Ziegler, 2012). Our empirical analysis also suggests that larger parishes benefited more from the railway than smaller ones, which might be another reason why Hornung (2015) finds considerably larger effects in his sample of Prussian towns.³⁰

Our finding of significant effect heterogeneity is also important in its own right. The coming of the railway was not only an important driver of Germany’s industrial take-off. It also increased regional economic disparities both between and within German states, an aspect that remains under-explored in the empirical literature on the effect of the railway on economic growth in 19th century Germany. More generally, our finding shows that even within a country, investment in transportation infrastructure can have markedly different effects on economic growth and development, depending on the specific local conditions. A better understanding of this heterogeneity has the potential to increase the effectiveness and efficiency of transport infrastructure investments also in contemporary contexts.

³⁰The average 1837 log population of Prussian railway towns in (Hornung, 2015) is 8.168 (excluding nodes, see his Table 4). This compares to a log population of just 6.824 for railway parishes in our analysis. Consistent with this explanation, Büchel and Kyburz (2020) also find relatively modest growth effects in their analysis of mostly small, rural Swiss parishes in the 19th century. Just like Württemberg, Switzerland lacked coal and iron ore and did not industrialize based on heavy industries. Indirect evidence for the importance of the (size of the) unit of observation also comes from a comparison of existing results for Sweden. While Berger and Enflo (2017) show that early access to the railroad increased population size of Swedish towns by 69 percent between 1855 and 1900, Berger (2019) ‘only’ reports an increase of nearly 25 percent for rural parishes between 1850 and 1900. The results are, however, not strictly comparably, as the treatment differs. While Berger and Enflo (2017) consider the effect of railway access, Berger (2019) studies the effect of being located within 5 kilometers of the Swedish trunk lines.

Appendix B

Railways, Growth, and Industrialization in a Developing German Economy, 1829–1910

B.1 Württemberg in the German Empire

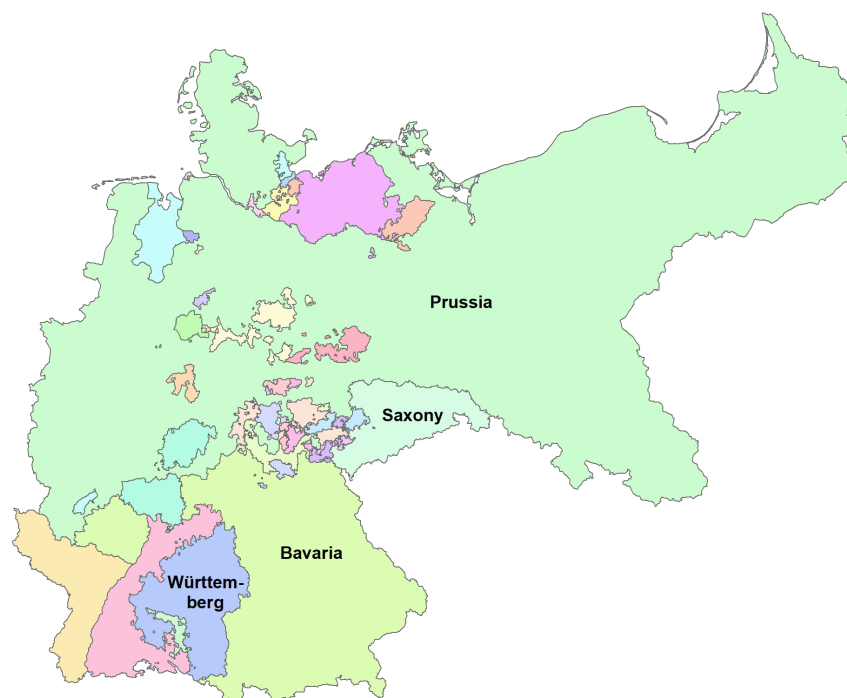


Figure B.1: The German Empire in 1871

Notes: The figure shows the German Empire in its 1871 borders. Labels mark the four Kingdoms that were part of the German Empire (namely, the Kingdoms of Bavaria, Prussia, Saxony and Württemberg).

Source: Max Planck Institute for Demographic Research (MPIDR) and Chair for Geodesy and Geoinformatics, University of Rostock (CGG) (2011). Authors' design.

B.2 Regional economic development in Württemberg

Table B.1: Development indicators of Württemberg's districts

	National income per capita (in % of average)		Agricultural employment share (in %)	Urbanization rate (in %)
	1849	1907/13	1907	1910
Neckarkreis	111.2	113.3	30.0	53.9
Schwarzwaldkreis	96.8	85.5	44.8	32.4
Jagstkreis	88.0	72.6	52.9	23.1
Donaukreis	96.6	85.7	45.3	32.6
Württemberg			41.3	38.7
German Empire	100	100	32.7	44.3

Notes: The urbanization rate measures the percent of the total population living in urban municipalities of 2,000 or more inhabitants.

Source: Data on national income per capita is taken from Frank (1993), data on agricultural employment is from Kaiserliches Statistisches Amt (1910, 1913), and data on urbanization from Kaiserliches Statistisches Amt (1915).

B.3 Parish level population growth in 1834–1910

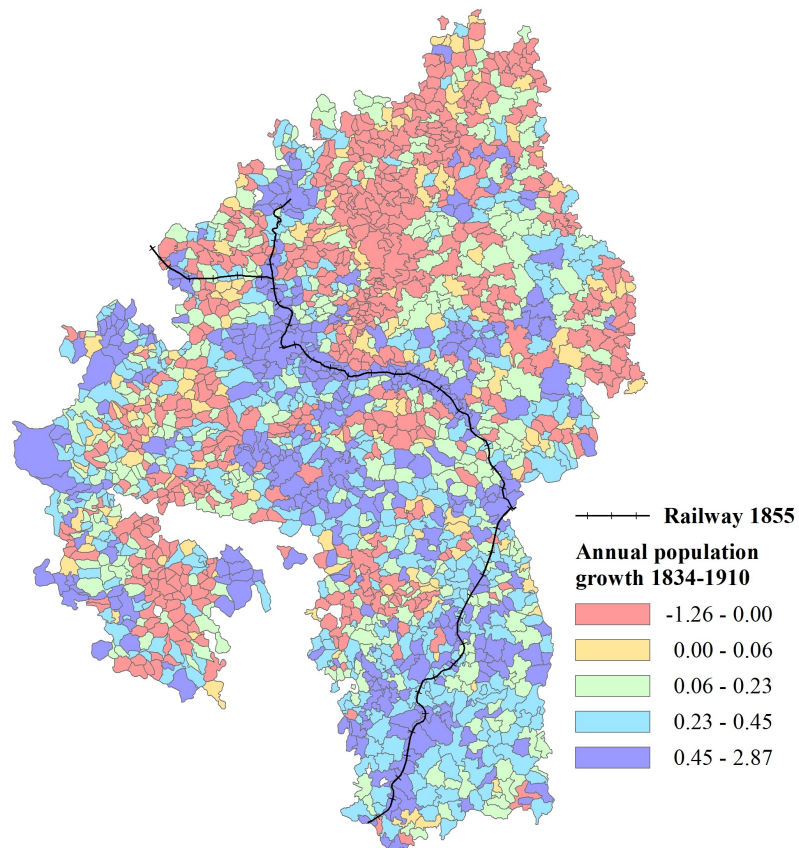


Figure B.2: Average annual population growth in 1834–1910

Notes: The figure shows the average annual population growth in parishes in Württemberg between 1834 and 1910. The solid black line depicts the railway network in 1855.

Sources: Kunz and Zipf (2008), Dumjahn (1984), Kommission für geschichtliche Landeskunde in Baden-Württemberg and Landesvermessungsamt Baden-Württemberg (1972), Statistisches Landesamt Baden-Württemberg (2008). Authors' design.

B.4 Northern, eastern and southern line of the railway network in Württemberg

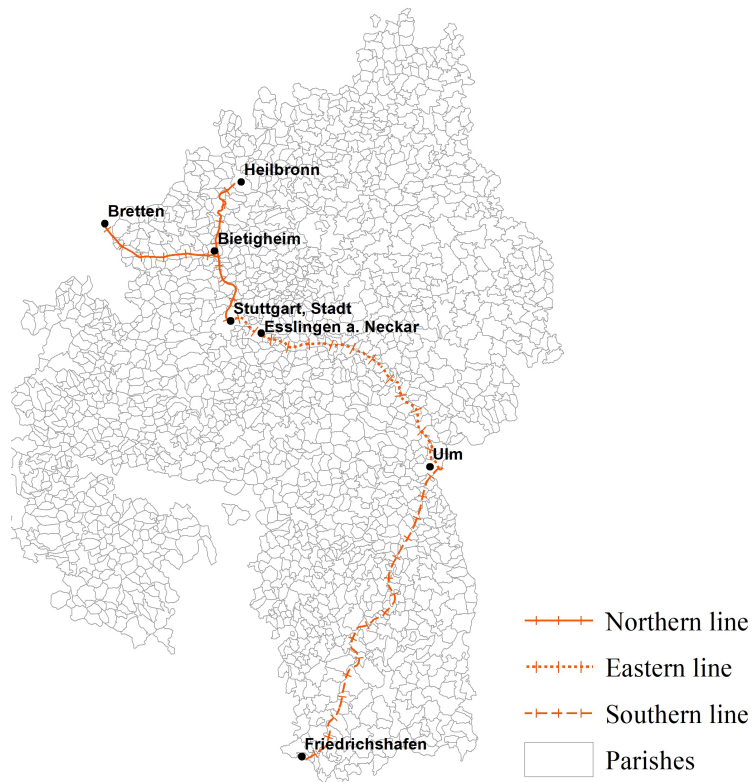


Figure B.3: Northern, eastern and southern line of the railway network in Württemberg 1855

Notes: The figure shows the northern (solid line), eastern (dotted line) and southern railway line (dashed line) of the railway network in Württemberg in 1855.

Sources: Kunz and Zipf (2008), Dumjahn (1984), Kommission für geschichtliche Landeskunde in Baden-Württemberg and Landesvermessungsamt Baden-Württemberg (1972), Statistisches Landesamt Baden-Württemberg (2008). Authors' design.

B.5 The planning process for the central line

This section outlines the planning process for the central line (*Zentralbahn*), the first railway line constructed in Württemberg. The central line was destined to connect the capital Stuttgart with Ludwigsburg in the north and with Cannstatt and Esslingen in the east.

On behalf of the government, building officer Georg von Bühler and engineer Carl von Seeger worked out the first detailed plan of the central line in 1836–39 (Mühl and Seidel, 1980). Figure B.4 sketches their proposed route (thin red dashed line), along with three later proposals that we discuss below. Von Bühler and von Seeger’s route mostly follows the river Neckar. Beginning in Ludwigsburg, the route heads east and then follows the western shore of the river. The route from Cannstatt to Stuttgart branches off the main line. By following the flat shore of the river, von Bühler and von Seeger’s proposal reduced height differences and kept the railway gradient below a threshold of 1:100 (Etzel et al., 1985). The expected construction costs for the central line amounted to 3,390,430 *Gulden* (von Reden, 1846).

In 1839, Württemberg’s parliament asked for another expert to inspect the existing railway plans. Alois Negrelli, a chief engineer at the Emperor Ferdinand Northern Railway in Vienna (*Kaiser Ferdinands-Nordbahn*), approved the plans of von Bühler and von Seeger in 1843 and recommended only minor changes (Mühl and Seidel, 1980). His proposal is delineated by the thin red dash-dotted line in Figure B.4.

After Negrelli’s report, the parliament was largely convinced of the feasibility of a railway network and asked the government to appoint a railway commission to elaborate on the technical aspects. The commission entrusted engineers Charles Vignoles (eponym of the Vignoles rail), Ludwig Klein, Karl Etzel, and Michael Knoll with examining various railway lines (Mühl and Seidel, 1980).

Figure B.4 illustrates the routes proposed for the central line by Vignoles in 1843 (bold red dashed line) and Etzel in 1844 (bold red solid line). Both proposals significantly changed the initial plans by recommending two separate lines that both start in Stuttgart. The first line connects Stuttgart to Ludwigsburg on a shorter route, which does not follow the Neckar but requires a tunnel near Feuerbach. The second line crosses the Neckar near Cannstatt and then follows the eastern shore of the river to Esslingen. The additional tunnel (and inflation) increased the estimated construction costs for Etzel’s proposal to 3,732,380 *Gulden* (von Reden, 1846).

The railway commission finally asked engineer Ludwig Klein to re-evaluate all existing proposals. Klein argued in his report that expected traffic—and thus the catchment area of a line—

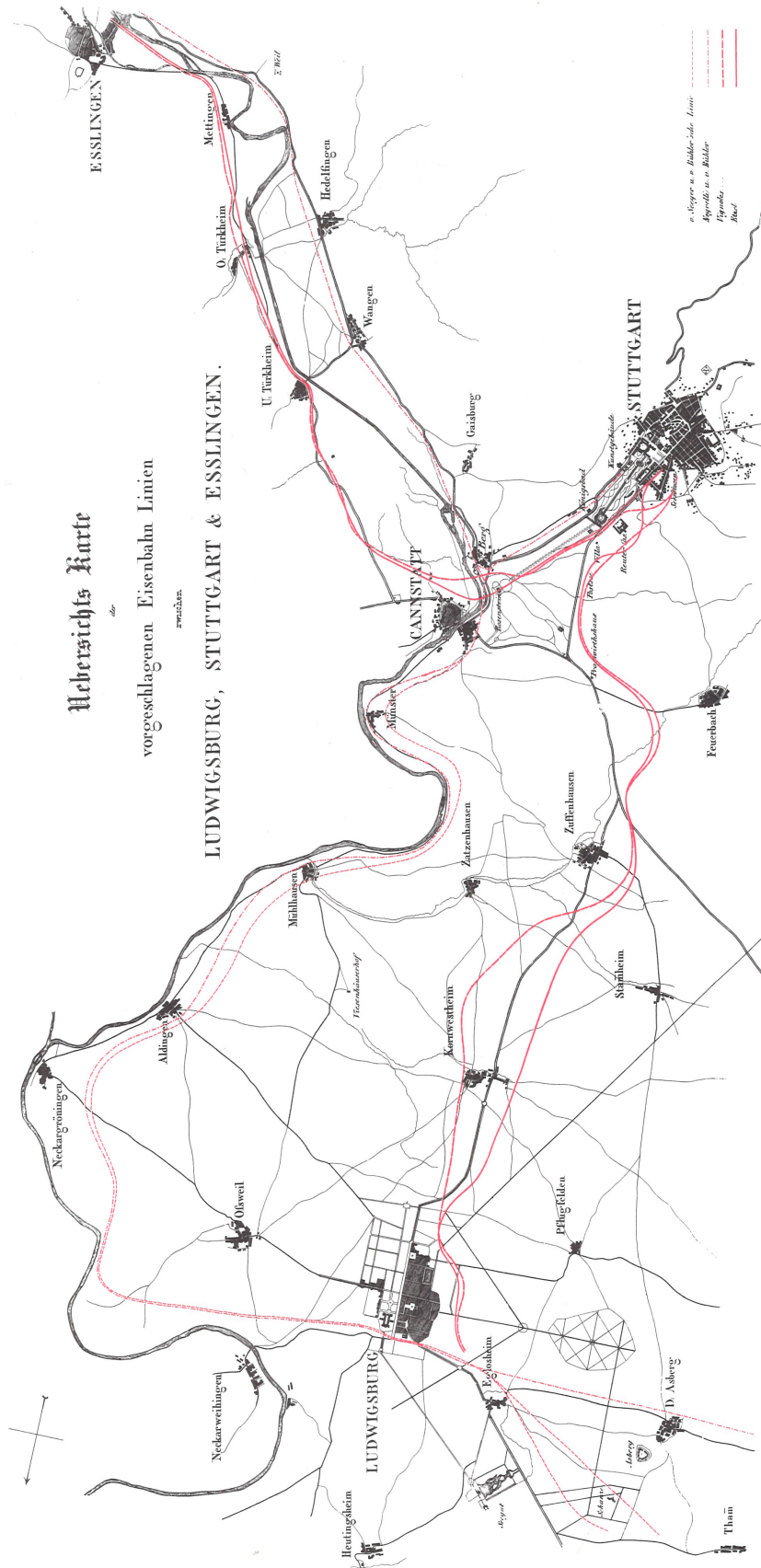


Figure B.4: Route proposals for the central line (*Zentralbahn*) in 1844

Notes: The figure shows the four different routes of the central line proposed during the planning process by von Büthler and von Seeger in 1839 (thin red dashed line), Negrlli in 1843 (thin red dashed-dotted line), Vignoles in 1843 (bold red dashed line), and Etzel in 1844 (bold red solid line).

Source: Etzel et al. (1985).

determines the turnover of a railway but that costs—and thus technical aspects of the line—drive profits (Etzel et al., 1985). Consequently, Klein’s report compares the proposals mainly under technical aspects.

In particular, Klein compared proposals I. by von Bühler and von Seeger (including Negrelli’s refinement), II. by Vignoles, and III. by Etzel based on their overall length, curvature, height difference, gradient, and weighted length (which accounts for curvature and gradient). Panel A of Table B.2 shows the results of this comparison for the line between Stuttgart and Esslingen. Route I. has the shortest length, both unweighted (44,600 feet) and weighted (49,100 feet). However, it also has the highest maximum gradient (1:100) and the lowest minimum curve radius (800 feet). Klein thus recommended route III., which dominates route II. in all aspects (Etzel et al., 1985).

Table B.2: Comparison of alternatives for the central line by Klein in 1844

Route (1)	Length (feet) (2)	Length of		Smallest curve radius (feet) (5)	Height difference (feet) (6)	Maximum gradient (7)	Weighted length (feet) (8)
		Straight lines (feet) (3)	Curves (feet) (4)				
Panel A: Stuttgart to Esslingen							
I.	44,600	33,100	11,500	800	119.3	1:100	49,100
II.	50,200	26,000	24,200	1,000	161.0	1:115	60,960
III.	49,260	30,875	18,385	1,200	144.0	1:125	56,600
Panel B: Stuttgart to Ludwigsburg							
I.	79,000	46,690	32,310	800	273.2	1:100	96,865
II.	54,105	26,085	28,020	1,500	228.7	1:125	64,619
III.	51,988	22,840	29,148	1,600	234.3	1:125	63,261

Notes: The table compares different routes for the line from Stuttgart to Esslingen (Panel A) and Stuttgart to Ludwigsburg (Panel B) proposed by von Bühler and von Seeger (I.), Vignoles (II.), and Etzel (III.) based on the length in total (Column(2)), of straight lines (Column (3)) and of curves (Column(4)). The table also shows the smallest curve radius (Column (5)), the height difference (Column (6)), the maximum gradient (Column (7)), and the weighted length (Column (8)), i.e. total length plus a penalty for curves and gradient. Distances in Württemberg feet, with 1,000 feet = 286.49 meters.

Source: Based on Tables XVI and XVIII from the report of Klein (1844) (Etzel et al., 1985, pages 71 and 76).

Panel B of Table B.2 shows the corresponding values for the three alternative routes of the line Stuttgart-Ludwigsburg. Again, Klein recommended route III. to the government. Route III. is the shortest of all three alternatives, both in terms of unweighted and weighted length. It also has the largest minimum curve radius and the lowest maximum gradient. The government followed Klein’s recommendations and choose proposal III. for both lines. Construction works began in June 1844. The first segment between Stuttgart and Esslingen was finished in November 1845, and the central line was completed in October 1846.

Our empirical analysis defines Obertürkheim as winning parish on the line Stuttgart-Esslingen and Feuerbach, Kornwestheim, Zuffenhausen as winning parishes on the line Stuttgart-Ludwigs-

burg. These parishes were only connected to the railway because the eventually built route followed Etzel's proposal and not von Bühler and von Seeger's earlier plans. Losing parishes are those that would have been connected to the railway under Bühler and von Seeger's plans but not under Etzel's (see Table B.4 for a list of winning and losing parishes by railway line).

B.6 Density of Württemberg's railway network in comparison

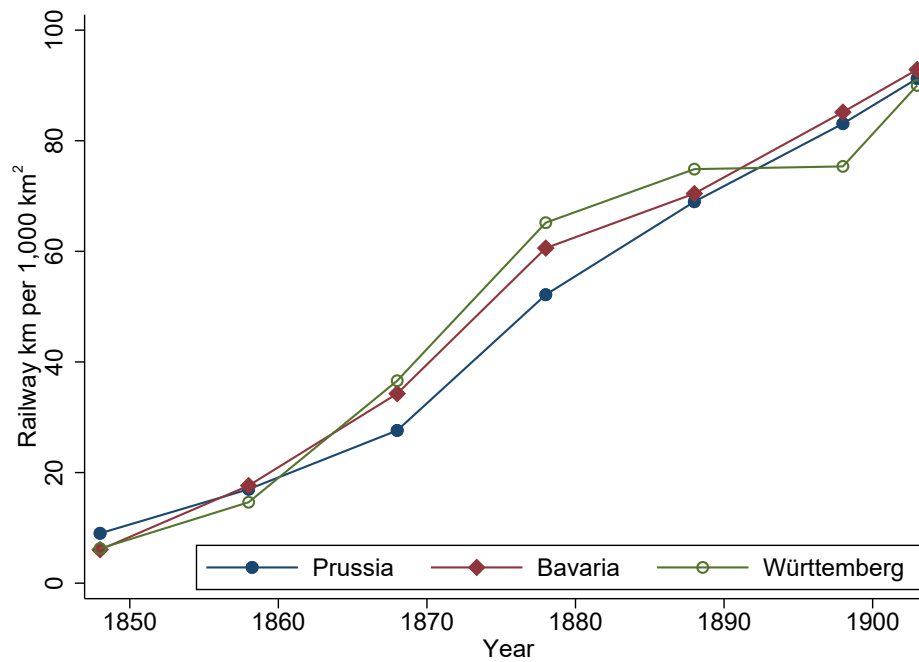


Figure B.5: Density of railway network in Bavaria, Prussia, and Württemberg, 1848–1903

Notes: The figure shows the density of the railway network in Bavaria, Prussia, and Württemberg from 1848 to 1903. Density is measured as the total length of the railway network (in km) over the land area of a state (in 1000 km²).

Sources: Length of the railway network from Lenschau (1906) and area from Kaiserliches Statistisches Amt (1903). Authors' design.

B.7 Political representation, lobbying, and railway access

By contemporary standards, Württemberg's parliament exercised unusually strong control over the executive. Manned by business interest, it supported and maintained the power of local communities, guilds, and cartels (Ogilvie et al., 2009; Ogilvie and Carus, 2014). We might thus expect that lobbying and political pressure had profound effects also on the routing of the railway. If so, winning and losing parishes might systematically differ, insofar as the former were more successful in their lobbying efforts. This might also explain why winners tended to be smaller and less industrialized than runners-up (see Table 3.1). As less dynamic places, they might have been better for political connections.

It is beyond the scope of this paper to comprehensively assess the influence that local special interest group—through lobbying and political pressure—had on the routing of the railway. We nevertheless think that any such influence was presumably small, at least in the first stage of the railway expansion. There are several reasons for this assessment.

First, the decision as to where to build rested primarily with the government, not the parliament (Mann, 2006). This was because local communities naturally had conflicting interests, which made decision-making in the parliament difficult. Parliamentary representatives did not agree on whether to build a railway in the first place (Mühl and Seidel, 1980; Supper, 1895). Some argued that Württemberg, as an agrarian country, did not need a railway. Others pointed to the disastrous consequences of the railway for carters and other trades, and to the burden imposed by state railways on taxpayers in remote areas without railway access. After long-lasting debates, the majority nevertheless agreed to the railway bill of 1843, which legislated the first stage of Württemberg's railway expansion (see Section 3.2 for details).

Naturally, conflicts also arose over the exact routes of the lines, stated in the railway bill. A key debate in the first construction phase was over the route of the *Ostbahn* from Cannstatt to Ulm. The first, and eventually realized, route ran via Göppingen through the Fills Valley (so-called *Filstalbahn*). The alternative route ran via Aalen through the valleys of Neckar, Rems and Brenz (so-called *Remsbahn*). While the *Remsbahn* bypassed the Swabian Alb, it was considerably longer than the *Filstalbahn* and ran partly over Bavarian territory (Figure 3.2 in the main text maps the proposed alternatives). Therefore, Alois Negrelli, the first external expert commissioned to inspect Württemberg's railway plans, strongly advocated the *Filstalbahn*. His strong rejection of the *Remsbahn* prompted local interest groups to commission a counter assessment, which, however, was eventually refuted by the second external expert, Ludwig Klein.

Second, the important role played by external experts arguably limited the influence of local special interest groups (see, e.g., Mühl and Seidel, 1980; von Morlok, 1890; Supper, 1895, for detailed descriptions of the reports written by these experts and their influence on the decision process). Importantly, Alois Negrelli and Ludwig Klein both came from outside Württemberg. Negrelli oversaw the construction of railways in the Austrian Empire and Switzerland, and also advised the Kingdom of Saxony. Klein came from Vienna to Württemberg, and had previously worked in Russia and the US. His influential report of 1843, approved by the railway commission and the ministry, explicitly states that “circumstances of local nature” must not be considered in his scientific evaluation of existing proposals (von Morlok, 1890, p. 24). Instead, Klein argues that the expected traffic and costs of a line should be the only two decision criteria. His report is also explicitly written on the premise that no economic or political obstacles stand in the way of any of the proposed lines.

Third, the railway bill of 1843 limited the scope of towns to lobby for a railway access in the first construction phase, as it determined both the general direction and destination of Württemberg’s main lines.¹ An exception was the western line, for which the bill did not specify a destination due to the pending negotiations with Baden. However, the route of the western line was mainly determined in direct negotiations between Baden and Württemberg.²

Fourth, we find no empirical evidence that parishes with direct connections to representatives in either the parliament or the advisory privy council (*Geheimer Rat*) were more likely to gain access to the railway in the first construction phase.³ We construct two measures for political connections. The first indicates whether at least one representative of the nobility in the privy council or parliamentary estates owned land in a parish. The second indicates whether at least one of the elected representatives of the parliament had his place of work in a parish. Information

¹In contrast, Mühl and Seidel (1980) discuss a number of examples where towns tried to influence the direction of railway lines in the second and third expansion stage.

²While Württemberg had approached Baden already in 1838 for negotiations about the connection of their networks, Baden initially focused on the connection of its railway with Switzerland (in an attempt to exclude Württemberg from this trade route). However, Württemberg finished its main line, and thus the connection to Lake Constance and Switzerland, already in June 1850, and thus earlier than Baden. Only then was Baden interested in connecting its network to Württemberg’s, also to redirect trade flows between the Netherlands and Austria-Hungary from more northern trade routes. Württemberg preferred a connection in the north between Heidelberg and Heilbronn, while Baden preferred a line between Durlach and Bietigheim in the south. Both countries favored the connection that kept the trains as long on their territories as possible. On December 4, 1850, they agreed to connect both networks between Bruchsal in Baden and Bietigheim in Württemberg, which is in the middle of their initial proposals.

³The constitution of September 1819 turned Württemberg into Germany’s first constitutional monarchy. The legislature was organized into two chambers. Members of the first chamber (*Ständekammer*) were the princes of the House of Württemberg, representatives of the nobility, and nominees of the King. Member of the second chamber (*Abgeordneten-kammer*) were 70 elected representatives of the administrative districts (*Oberämter*) and largest towns as well as 23 ‘privileged representatives’ (namely, representatives of the knightly nobility, the churches, and the chancellor of the University of Tübingen). The re-established privy council acted as a link between the parliamentary estates and the King. As the highest state bureaucracy, the council was directly subordinate to the King. It consisted of the ministers and additional members appointed by the King.

on the names of representatives and their workplaces as well as the landholdings of the nobility refer to 1843 and come from Königliches Statistisches Landesamt (1843).

Table B.3: OLS estimates of the effect of political connections on railway access

	Dependent variable:			
	Railway access 1845–1854 (0/1)			
	(1)	(2)	(3)	(4)
Panel A: Winners vs. runners-up				
Noble landholder (0/1)	0.062 (0.084)	0.021 (0.093)		
Place of work (0/1)			-0.152 (0.145)	-0.086 (0.135)
Control variables	No	Yes	No	Yes
Observations	156	156	156	156
Panel B: Full sample				
Noble landholder (0/1)	-0.004 (0.009)	-0.006 (0.009)		
Place of work (0/1)			0.017 (0.035)	-0.021 (0.040)
Control variables	No	Yes	No	Yes
Observations	1,846	1,846	1,846	1,846

Notes: The table shows OLS regression estimates of the effect of political connections on the probability of gaining railway access in 1845–54. Regressions in Panel A are estimated for the winners versus runners-up sample, regressions in Panel B for the complete sample excluding railway nodes. Regressions in Columns (1) and (2) measure political connections with a dummy indicating whether at least one representative of the nobility in the privy council or parliamentary estates owned land in a parish. Regressions in Columns (3) and (4) measure political connections with a dummy indicating whether at least one of the elected representatives of the parliament had his place of work in a parish. Regressions in Columns (2) and (4) include as control variables log population and log population density in 1834, the share of protestants in 1821, a dummy for having a manufactory in 1832, industry employment per 100 persons in 1829, elevation, dummies for access to a navigable river in 1845 and to a road in 1848, and case dummies. Robust standard errors are in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

Table B.3 report OLS regression estimates of the effect of political connections on the probability of gaining railway access in 1845–54, both for the winners versus runners-up sample (Panel A) and the full sample (Panel B). Columns (1) and (3) show unconditional estimates, whereas Columns (2) and (4) condition on our usual control variable. None of the specifications indicates a statistically significant effect of political connections on railway access. Of course, our measures of political connections are at best imperfect proxies. The empirical results in Table B.3 should thus not be taken as definite evidence against the importance of lobbying and political pressure for the routing of the railway. Nevertheless, they are consistent with our general assessment that any such influence was presumably small.

B.8 List of winning and losing parishes

Table B.4 shows the list of winning and losing parishes by case and railway line. We exclude railway nodes and parishes that would have been connected to the railway under all alternative proposals from the list.

Table B.4: Winning and losing parishes by case

Case (1)	Line (2)	Winning parishes (3)	Losing parishes (4)
1	Stuttgart - Ulm	Altbach, Altenstadt, Amstetten, Beimerstetten, Ebersbach an der Fils, Faurndau, Gingen an der Fils, Göppingen, Großeisingen, Jungingen, Kuchen, Lonsee, Oberesslingen, Obertürkheim, Reichenbach an der Fils, Salach, UHINGEN, Westerstetten, Zell am Neckar	Aalen, Aufhausen, Beinstein, Bergenweiler, Beutelsbach, Bolheim, Bopfingen, Endersbach, Essingen, Fellbach, Geradstetten, Giengen an der Brenz, Großdeinbach, Grunbach, Heidenheim a.d. Brenz, Herbrechtingen, Herlikofen, Hermaringen, Königsbronn, Langenau, Lauchheim, Lorch, Mergelstetten, Möggingen, Niederstotzingen, Oberkochen, Oberurbach, Pflaumloch, Plüderhausen, Rammingen, Rommelshausen, Röttingen, Schorndorf, Schwäbisch Gmünd, Sontheim an der Brenz, Stetten im Remstal, Trochtelfingen, Unterböbingen, Unterkochen, Waiblingen, Waldhausen, Wasseralfingen, Weiler (Rems), Westhausen, Winterbach
2	Stuttgart - Esslingen	Obertürkheim	Hedelfingen
3	Bad Cannstatt - Ludwigsburg	Feuerbach, Kornwestheim, Zuffenhausen	Aldingen am Neckar, Mühlhausen, Münster, Neckargröningen, Neckar-rem

Continued on next page

Table B.4 – *Continued from previous page*

Case	Line	Winning parishes	Losing parishes
(1)	(2)	(3)	(4)
4	Biberach - Ulm	Achstetten, Einsingen, Erbach, Grimmelfingen, Langenschemmern, Laupheim, Rißtissen, Schemmerberg, Schweinhausen, Ummendorf, Unteressendorf, Warthausen, Wolpertswende	Allmendingen, Bad Buchau, Berkach, Blaubeuren, Dettingen, Ehingen (Donau), Ehrenstein, Gerhausen, Herrlingen, Klingenstein, Munderkingen, Reichenbach bei Schussenried, Rottenacker, Schelklingen, Schmiechen
5	Ravensburg - Biberach	Aulendorf, Bad Schussenried, Reute, Schweinhausen, Ummendorf, Unteressendorf, Wolpertswende	Bad Waldsee, Hochdorf, Michelwinnaden, Rißegg, Steinach, Winterstettenstadt
6	Bietigheim - Bretten	Dürrmenz/Mühlacker, Ensingen, Großsachsenheim, Illingen, Maulbronn, Ötisheim, Sersheim	Aurich, Bissingen an der Enz, Ditzingen, Enzberg, Enzweihingen, Friolzheim, Gündelbach, Horrheim, Knittlingen, Markgröningen, Oberriexingen, Roßwag, Zaisersweiher
7	Bietigheim - Heilbronn	Besigheim, Böckingen, Kirchheim am Neckar, Klingenberg, Lauffen am Neckar, Nordheim, Walheim	Auenstein, Beihingen am Neckar, Beilstein, Großbottwar, Hof und Lembach, Ilsfeld, Kleinbottwar, Marbach am Neckar, Murr, Oberstenfeld, Schozach, Sontheim, Steinheim an der Murr, Talheim

B.9 Semi-parametric models

This section provides technical details on IPW and IPWRA models (see Imbens and Wooldridge (2009) and Wooldridge (2010) for a thorough discussion), which we estimate using Stata’s 16.1 command `teffects`.

Average treatment effect on the treated (ATT). The parameter γ in models (3.1) and (3.2) can be interpreted as the ATT, provided that the linearity assumption inherent in these models holds. Let $y_{ijt}^\tau(d)$ denote the potential outcome at time $t + \tau$ of parish i of case j whose winning line was opened in t . Here, $d \in \{0, 1\}$ indicates railway access, so that $y_{ijt}^\tau(1)$ denotes the potential outcome with railway access and $y_{ijt}^\tau(0)$ the potential outcome without railway access. We furthermore define the potential outcome growth between periods $t - 4$ and $t + \tau$ as $\Delta y_{ijt}^\tau(d) = y_{ijt}^\tau(d) - y_{ijt-4}$. The causal effect of railway access at time t on the outcome of interest after τ periods is

$$\gamma_{att,\tau} \equiv \mathbb{E} [\Delta y_{ijt}^\tau(1) - \Delta y_{ijt}^\tau(0) \mid D_{ij,1855} = 1]. \quad (\text{B.1})$$

As in the event study analysis, we again express population relative to a baseline four periods before the treatment. Assumption 2 then applies to the difference rather than the level in potential outcomes, i.e., $(\Delta y_{ijt}^\tau(1), \Delta y_{ijt}^\tau(0)) \perp D_{ij,1855} \mid \mathbf{X}_i$.

Inverse probability weighting. IPW estimates the ATT by comparing *weighted* outcome means of parishes with and without railway access, placing more weight on observations in the control group that—given their covariates—had a high probability of being treated in the first place. More specifically, IPW first uses a probit model to estimate the propensity score—or probability—of being in the treatment group (i.e., of $D_{ij,1855} = 1$) conditional on covariates \mathbf{X}_i . We then use the predicted propensity score \hat{P}_i to re-weight the outcome variable, applying the efficient weights \hat{w}_i of Hirano et al. (2003):

$$\hat{w}_i = \begin{cases} 1/\hat{\mathbb{E}}[D_{ij,1855}] & \text{if } D_{ij,1855} = 1, \\ -\hat{P}_i / \left[\hat{\mathbb{E}}[D_{ij,1855}](1 - \hat{P}_i) \right] & \text{if } D_{ij,1855} = 0, \end{cases} \quad (\text{B.2})$$

where $\hat{\mathbb{E}}[X \mid S]$ denotes the sample average of X for all observations in a set S . $\hat{\mathbb{E}}[D_{ij,1855}]$ in equation (B.2), for instance, is simply the fraction of parishes in the sample that are part of the treatment group. Finally, we obtain the IPW estimate of the effect of railway access on the change in outcome from four periods before the line opened to τ periods thereafter by

comparing means of the re-weighted data: $\hat{\gamma}_{att,\tau,IPW} = \hat{\mathbb{E}}[\hat{w}_i \cdot \Delta y_{ijt}^\tau]$. Here, $\Delta y_{ijt}^\tau = y_{ijt}^{t+\tau} - y_{ijt}^{t-4}$ is the change in outcome y between period $t-4$ and $t+\tau$ for a parish i . As before, τ are the time periods since the (case-specific) railway line's opening year t . We compute estimates $\hat{\gamma}_{att,\tau,IPW}$ for $\tau = -3, \dots, 13$ with $\tau = 0$ corresponding to the year of railway access.

Inverse probability weighting regression adjustment. The IPWRA model uses \hat{w}_i from equation (B.2) to run weighted regressions of Δy_{ijt}^τ on our set of covariates. These regressions are estimated separately for treated and control parishes. Specifically, we estimate parameters (α_0, ω_0) and (α_1, ω_1) by solving the following weighted least squares problems:

$$\begin{aligned} \min_{\alpha_0, \omega_0} (1 - D_{ij,1855}) \hat{w}_i (\Delta y_{ijt}^\tau - \alpha_0 - \omega_0 \mathbf{X}_i)^2, \\ \min_{\alpha_1, \omega_1} D_{ij,1855} \hat{w}_i (\Delta y_{ijt}^\tau - \alpha_1 - \omega_1 \mathbf{X}_i)^2. \end{aligned}$$

The IPWRA estimate is then given by the average of the difference in predicted values, as evaluated for the sub-population of parishes with railway access:

$$\hat{\tau}_{att,\tau,IPWRA} = \hat{\mathbb{E}} [(\hat{\alpha}_1 - \hat{\omega}_1 \mathbf{X}_i) - \hat{\alpha}_0 - \hat{\omega}_0 \mathbf{X}_i \mid D_{ij,1855} = 1].$$

B.10 Overlap assumption

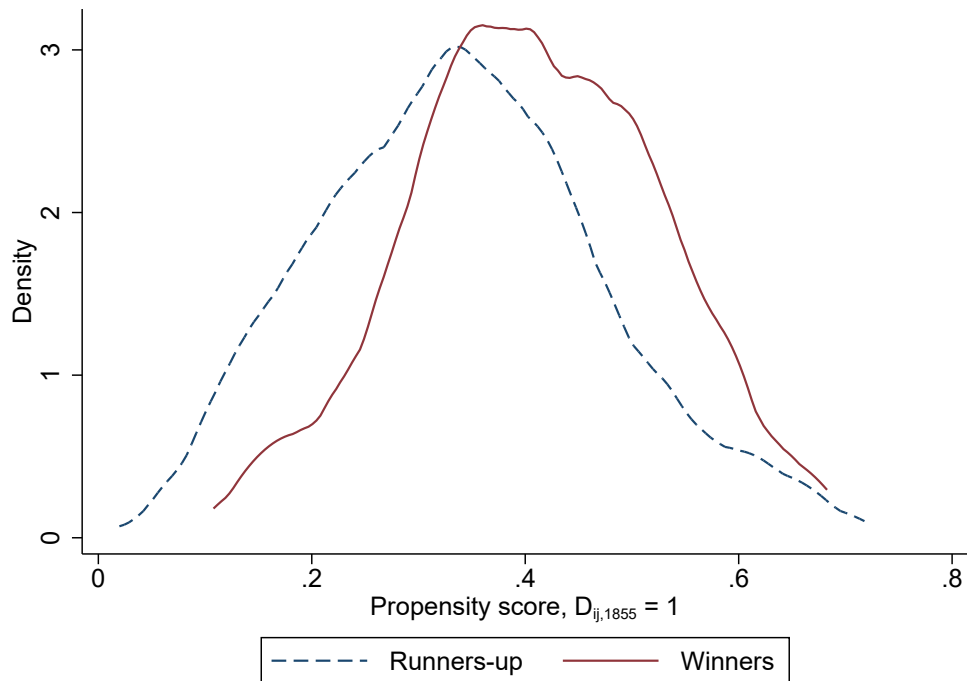


Figure B.6: Smoothed density for the estimated propensity for railway access in the first construction stage

Notes: The figure shows smoothed densities of the estimated propensities for railway access in the first construction stage both for winners (solid line) and runners-up (dashed line). The explanatory variables are log population and log population density in 1834, the share of protestants in 1821, a dummy for having a manufactory in 1832, industry employment per 100 persons in 1829, elevation, dummies for access to a navigable river in 1845 and to a road in 1848, and case dummies. We smooth the densities using an Epanechnikov kernel.

B.11 Semi-parametric estimates for population growth, winner versus runners-up sample

Figure B.7 shows the results from semi-parametric IPW and IPWRA of the effect of railway access on population growth for the winner versus runners-up sample. The dependent variable is the change in log population between period $t - 4$ and period $t + \tau$ where t is the time when a case's winning line was opened. The effect of railway access on the change in log population is thus normalized to zero for $\tau = -4$.

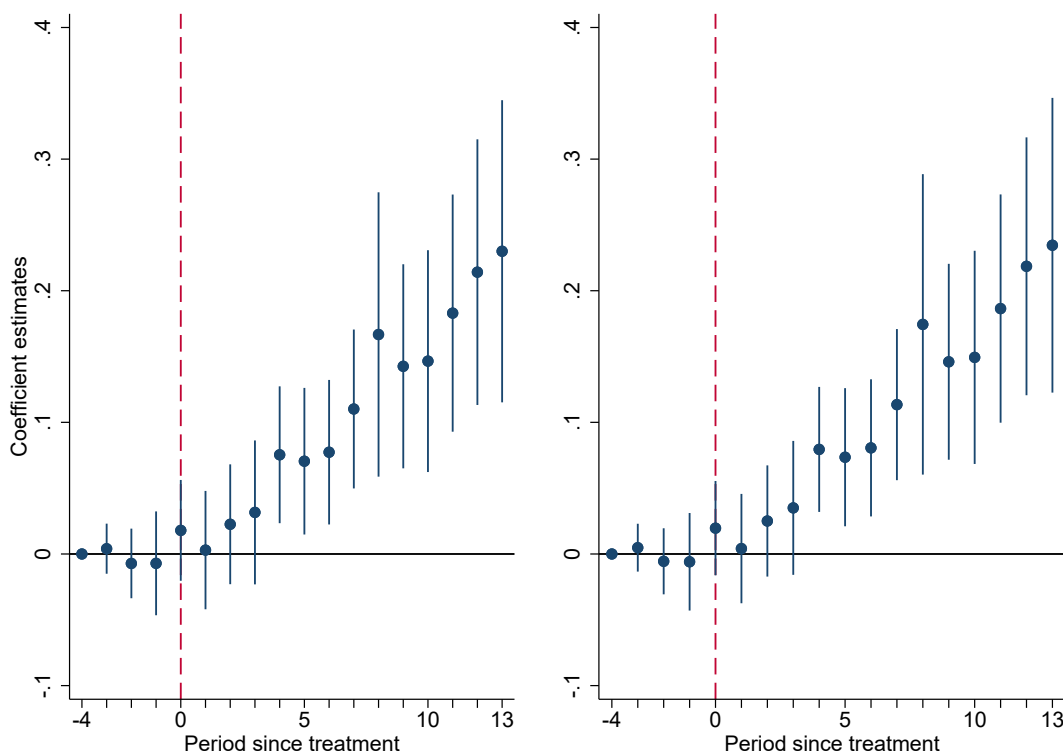


Figure B.7: Semi-parametric estimates of the effect of railway access on log population

Notes: This figure plots semi-parametric estimates of the effect of railway access in 1845–54 on log population. The dependent variable is the change in log population since the fourth period before the treatment. Section 3.4.2 describes the estimation in detail. The left panel shows estimates from inverse probability weighting (IPW), the right panel from inverse probability weighting regression adjustment (IPWRA). Point estimates are marked by a dot. The vertical bands indicate the 95 percent confidence interval of each estimate. The red dashed vertical line indicates the treatment period.

The figure depicts estimates for $\tau = -4, -3, \dots, 13$. Dots mark the point estimates, vertical bands the corresponding 95 percent confidence intervals. Reassuringly, we see no differential population trends between winner and runner-up parishes before the arrival of the railway. Thereafter, log population gradually increases in winner parishes in both IPW and IPWRA estimations. After thirteen periods, the cumulative effect of railway access on population reaches 0.229 and 0.234 log points in the IPW and IPWRA estimation, respectively. Semi-parametric

estimates are thus very similar to our event study results in Section 3.5.1. Figure B.8 additionally compares the event study results from Figure 3.3 in the main text to cross-sectional IPWRA estimates, which use the population *level* rather than the *change in population* relative to the baseline period as outcome variable. Both models again yield very similar results for the over-time effect of railway access on population.

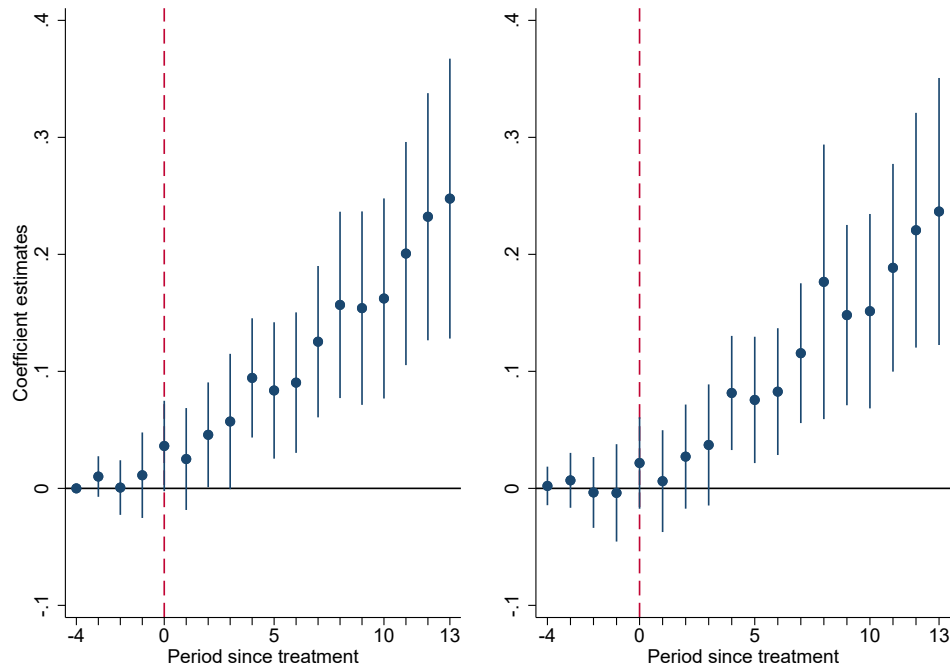


Figure B.8: Event study and cross sectional IPWRA estimates

Notes: The graph depicts differences in log population between winner and runner-up parishes. The left panel shows Figure 3.3, i.e., the difference in log population for pre- and post-treatment periods, as estimated in an event study regression (see Section 3.4.2 for details). Differences are expressed relative to the baseline differences four periods before the treatment. The right panel shows cross sectional estimates from inverse probability weighting regression adjustment (IPWRA) with log population as dependent variable. Each point estimate shows the difference in log population for a cross section in pre- and post-treatment periods $\tau = -4, -3, \dots, 13$. Point estimates are marked by a dot. The vertical bands indicate the 95 percent confidence interval of each estimate. The red dashed vertical line indicates the treatment period.

B.12 Event study and semi-parametric estimates for population growth, full sample

Event-study and semi-parametric estimates for the winners versus runners-up sample in Section 3.5.1 shows that much of the positive effect of railway access on population growth materializes decades after the treatment. This Appendix shows that we reach similar conclusion for the full sample as well.

Appendix Figure B.9 compares over time differences in population between railway and non-railway parishes for the full sample, based on panel fixed effects regression. The ‘period since treatment’ is not defined for non-railway parishes that were not runners-up for a railway line. We thus instead compare differences between railway and non-railway parishes over time, taking 1834 as the baseline year. Consequently, results for the full sample are not directly comparable to those for the winner versus runners-up sample reported in Section 3.5.1. They nevertheless show a similar picture, with population differences gradually growing over time. In fact, Figure B.9 shows that the widening of the population gap between railway and non-railway parishes accelerated in the 1890s and 1900s, long after parishes first got access to the railway. Semi-parametric IPW and IPWRA estimates yield very similar conclusions (see Figure B.10).

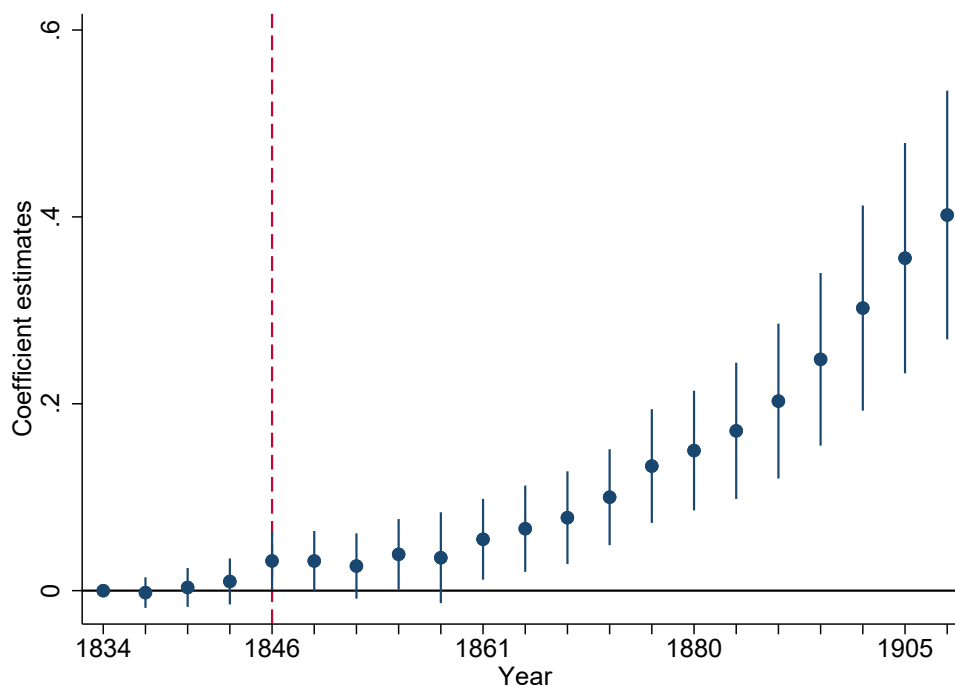


Figure B.9: Event study estimates – Full sample

Notes: The graph depicts differences in log population between railway and non-railway parishes in 1837–1910, as estimated in a panel regression with parish fixed effects. 1834 serves as baseline period. Point estimates are marked by a dot. The vertical bands indicate the 95 percent confidence interval of each estimate. The red dashed vertical line indicates the treatment period.

B.13 Natural population growth versus immigration

We have established in the main text that railway access had a sizable and lasting positive effect on parish level population. Such population increase could be driven by immigration and/or changes in the rate of natural population increase (i.e., an increasing birth rate and/or a decreasing death rate). Census data for 1871, 1895 and 1900 tentatively suggest that railway access indeed induced immigration to winner parishes. For all three years, we regress the share of inhabitants born outside of a parish (hereafter: foreign-born) on the treatment group dummy and our usual set of control variables (we cannot run panel regressions as we do not have pre-treatment information on the share of foreign-born). IPW and IPWRA estimations suggest that railway access increased the population share of foreign-born inhabitants by 5.8 percentage points in 1871 (from a baseline of 24.5 percent), by 6.3 percentage points in 1895 (from a baseline of 28.9 percent), and by 6.0 percentage points in 1900 (from a baseline of 30.9 percent). OLS regressions yield virtually identical results (see again Table B.5 for details).

Data for 1871 additionally distinguishes between foreign-born who were born a) in a different parish in Württemberg, b) in a member state of the German Customs Union (except

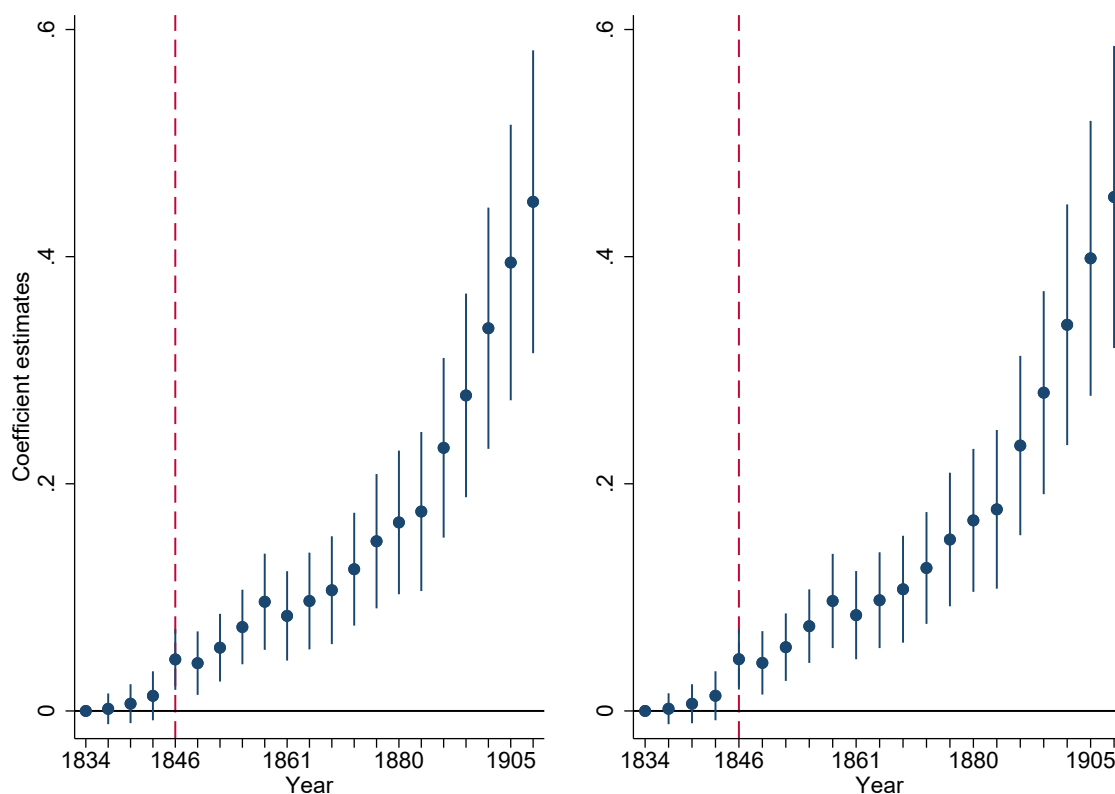


Figure B.10: Semi-parametric estimates of the over time effect of railway access on log population, full sample

Notes: This figure plots semi-parametric estimates of the effect of railway access in 1845–54 on log population. The dependent variable is the change in log population since 1834. The left panel shows estimates from inverse probability weighting (IPW), the right panel from inverse probability weighting regression adjustment (IPWRA). Section 3.4.2 describes the estimation in detail. Point estimates are marked by a dot. The vertical bands indicate the 95 percent confidence interval of each estimate. The red dashed vertical line indicates the treatment period.

Württemberg), and c) abroad. Much of the differences in the share of the foreign-born between winner and runner-up parishes is driven by migration within Württemberg: the (unconditional) population share of foreign-born who are originally from another parish in Württemberg is 28.1 percent in winning parishes but only 23.0 percent in losing parishes. Population growth in winner parishes was thus—at least in part—due to relocation within Württemberg. Migration across state borders was much less important: Only 0.5 percent of individuals in our winners versus runners-up sample were born abroad in 1871. Yet, the average population share of migrants born abroad is three times higher in winning parishes (0.6 percent) compared to losing parishes (0.2 percent)—and such differences might have become more important over time.⁴

We also use annual data on the number of births and deaths between 1871 and 1910 to estimate OLS and semi-parametric models with the average annual birth rate, death rate and

⁴Data on immigration is not consistent over time, as the different censuses use very different definitions. The last census in our sample from 1910 recorded the number of individuals without German citizenship. The average share is 1.4 percent in winning parishes and 0.8 percent in losing parishes.

Table B.5: The effect of railway access on the share of foreign-born and the rate of natural population increase

	Share of foreign born			Fertility	Mortality	Rate of
	1871	1895	1900	rate	rate	nat. increase
	(1)	(2)	(3)	1871–1910	1871–1910	1871–1910
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: IPW						
Treatment effect	0.058*** (0.016)	0.063*** (0.015)	0.060*** (0.018)	-0.303 (0.983)	-0.174 (0.788)	-0.129 (0.899)
Panel B: IPWRA						
Treatment effect	0.057*** (0.016)	0.062*** (0.015)	0.059*** (0.017)	-0.172 (0.942)	-0.180 (0.792)	0.008 (0.892)
Panel C: OLS						
Treatment effect	0.059*** (0.015)	0.066*** (0.015)	0.065*** (0.017)	-0.072 (0.976)	-0.548 (0.673)	0.475 (0.805)
Observations	156	156	156	152	152	152
Control mean	0.245	0.289	0.309	37.65	26.26	11.39

Notes: The table shows regression estimates of the effect of railway access in 1845–54 on the share of inhabitants born outside a parish in 1871 (Column (1)), 1895 (Column (2)) and 1900 (Column (3)), and on the annual number of birth (Column (4)), death (Column (5)) and natural population increase (Column (6)) per 1,000 inhabitants, averaged for 1871–1910. The regressions in Panel A are estimated by IPW, regressions in Panel B by IPWRA and regressions in Panel C by OLS. All regressions include as control variables log population and log population density in 1834, the share of protestants in 1821, a dummy for having a manufactory in 1832, industry employment per 100 persons in 1829, elevation, dummies for access to a navigable river in 1845 and to a road in 1848, and case dummies. Robust standard errors are in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

rate of natural population increase as dependent variables. The results indicate that there is no statistically significant difference in fertility and mortality rates between winners and runners-up in 1871–1910 (see Table B.5 for details). Finally, we also use the vital statistics to calculate the hypothetical average annual parish level population growth in 1871–1910 had population only changed through net migration (i.e., average annual population growth net of natural population increase). Appendix Figure B.11 shows this hypothetical growth rate, along with Württemberg’s railway network in 1855. The figure indicates that migration-induced population growth was indeed higher in parishes along the railway network.⁵ We thus conclude that the positive effect of the railway on population growth in winner parishes is mainly driven by immigration, in line with spatial equilibrium models.

⁵The figure suggests that net emigration rates were highest in the northwest of the country. Most historical accounts explain this pattern with the poor soil quality in the northwest of Württemberg (von Hippel, 1984). Moreover, the cultivation of potatoes and wine in the north-west of Württemberg was more susceptible to crop failures than the traditional grain cultivation in the east. This led to higher emigration during hunger crises.

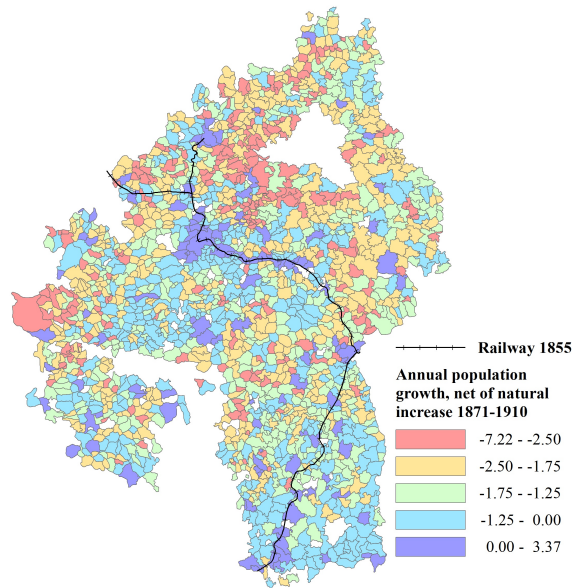


Figure B.11: Average annual population growth in 1871–1910, net of natural population increases

Notes: The figure shows the hypothetical average annual parish level population growth in 1871–1910 had population only changed through net migration (i.e., average annual population growth net of natural population increase). The solid black line depicts the railway network in 1855. Data on vital statistics are missing for the district of Hall and a few other parishes.

Sources: Kunz and Zipf (2008), Dumjahn (1984), Kommission für geschichtliche Landeskunde in Baden-Württemberg and Landesvermessungsamt Baden-Württemberg (1972). Authors' design.

B.14 The effect of gaining railway access in later construction stages

As it is common in the literature (see, e.g., Berger and Enflo, 2017; Hornung, 2015), our analysis focuses on *early* railway connections. In particular, we focus on the effect of gaining access to the railway in the first construction stage from 1845 to 1854. However, a significant number of parishes that did not get access to the railway in the first construction stage did get access in the second stage from 1857 to 1886. In fact, the second stage was of major importance for Württemberg's railway network: It expanded its length from 290 to 1,560 kilometers and increased the number of parishes with railway access from 73 to 350. Figure B.12 shows the share of winners, runners-up, and non-railway parishes with access to the railway over time. This subsection studies whether gaining railway access in later construction stages had similar effects on population as those that we document for the first stage.

Table B.6 replicates the results from our baseline DiD specifications (from Table 3.2 in the main text), adding a separate treatment indicator for the second construction stage to equation (3.1).⁶ Columns (1) to (3) restrict the sample to winner and runner-up parishes. Of

⁶Let $D_{i,1886}$ be a binary treatment indicator that indicates whether parish i was connected to the railway between 1857 and 1886 (and not earlier, later or never), and let $1(\kappa \geq 0)_{it}$ be a dummy that indicates whether

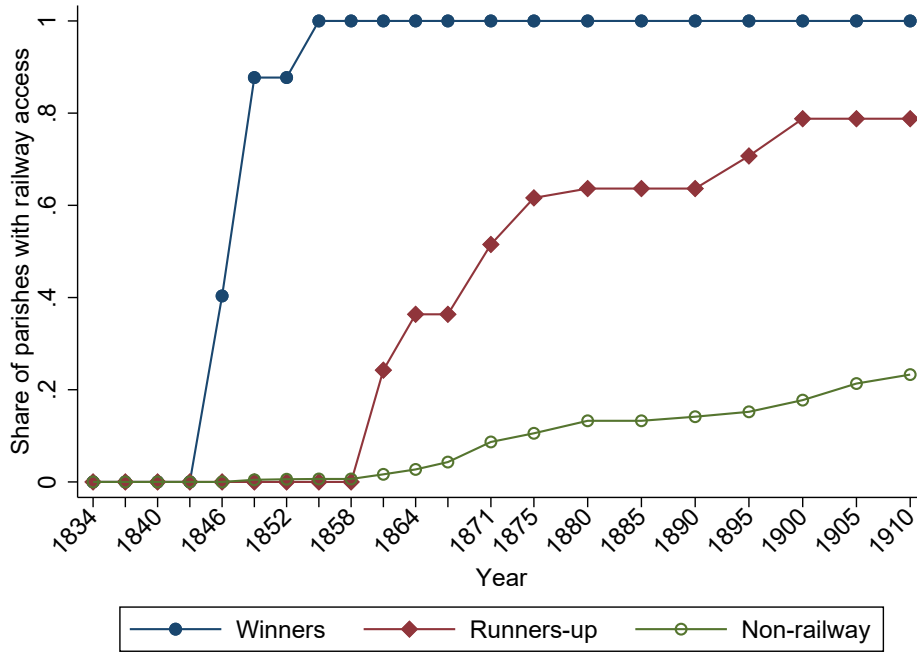


Figure B.12: Share of parishes with railway access by winners, runners-up, and non-railway parishes, 1834–1910

Notes: The figure shows the share of parishes with railway access for winners, runners-up, and non-railway parishes by year.

the 99 runner-up parishes, 61 parishes—or 62 percent—gained access to the railway in the second construction phase (see Appendix Figure B.12). Our estimates suggest, however, that these parishes did not grow faster than parishes that remained without access by 1886.⁷ This also implies that runner-up parishes did not catch up to the winner parishes even if they later gained access to the railway themselves.

The fact that later railway access did not boost population in the winner vs. runner-up sample may seem surprising. After all, the second construction stage connected major towns to the network, such as Reutlingen or Tübingen, which had remained without access after the first stage. However, our winners versus runners-up sample does not capture the most important new lines that were added to the network in 1857–86 (such as the railway line connecting Plochingen with Reutlingen and Tübingen). This is because the runner-up parishes are located along alternative routes between major towns, which had already been connected in the first

parish i had railway access at time t . The treatment indicator for the second construction stage is $D_{i,1886} \times 1(\kappa \geq 0)_{it}$.

⁷An important shortcoming of these regressions is that runner-up parishes that did not gain railway access by 1886 are likely to be a selected group of all runner-up parishes. In fact, runner-up parishes that did not get access in the second stage were statistically significantly smaller in 1855 than runner-up parishes that gained access in 1857–86 (difference in log population is 0.28 with a s.e. of 0.14). This selection should, however, bias the estimated impact of later rail connections upward and can thus not explain our ‘no effect finding’.

Table B.6: DiD estimates of the effect of early and late railway access on population

	Winners vs. runners-up			Full sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Railway access 1845–54	0.133*** (0.038)	0.106*** (0.030)	0.116*** (0.028)	0.219*** (0.032)	0.180*** (0.024)	0.142*** (0.023)	0.143*** (0.023)
Railway access 1857–86	0.045 (0.058)	-0.029 (0.034)	-0.059* (0.035)	0.132*** (0.023)	0.095*** (0.011)	0.112*** (0.010)	0.113*** (0.010)
Railway access 1887–1910							0.054*** (0.020)
Observations	3,276	3,276	3,276	38,766	38,766	38,766	38,766
Group FE	Yes	No	No	Yes	No	No	No
Parish FE	No	Yes	Yes	No	Yes	Yes	Yes
Year \times Case/County FE	No	No	Yes	No	No	Yes	Yes

Notes: The table shows panel regression estimates of the effect of railway access in the first, second, and third construction phase (1845–54, 1857–86 and 1887–1910) on log population. Regressions (1) to (3) are estimated for the winners versus runners-up sample, regressions (4) to (7) for the complete sample excluding railway nodes. All regressions include a full set of year dummies. Regressions (1) and (4) include dummies for the treatment groups that gained railway access in the first and second construction phase; all remaining regressions include a full set of parish dummies. Regression (3) additionally includes case by year fixed effects and regressions (6) and (7) include county by year fixed effects. Standard errors clustered at the parish level are in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

construction stage. Building these initially unrealized ‘alternative routes’ later did not boost population along the way, probably precisely because the winning lines were already in operation.

This interpretation is strengthened by our findings for the full sample, reported in Columns (4) to (6) of Table B.6. Of the 1,786 parishes that did not get railway access in the first construction stage, 277 did so in 1857–86. The estimates suggest that in the full sample, late railway access indeed increased population. The effect is sizable but somewhat smaller than for early railway access (0.095–0.132 compared to 0.142–0.219 log points). This is not surprising since the lines built in the first construction phase were arguably the most important ones, especially for transit passengers and freight. The estimated effect of early railway access on population is slightly larger in Table B.6 than in our baseline regressions in Table 3.2. This is because in later years, parishes that gained access in 1857–1886 are no longer in the control group in Table B.6 (and these parishes grew faster themselves). Yet, differences are small, as the majority of parishes remained without railway access by 1886.

In the full sample, we can also study the effect of the third construction stage from 1887 onwards, which connected mostly rural parishes via branch lines to the main network. Of the 1,509 parishes that did not get railway access until 1886, 173 did so in 1887–1910. Column (7) adds separate treatment indicators for the second and third construction stage to our full-fledged specification with parish and year times county fixed effects. The estimates suggest that the third construction stage still had a positive effect on population, but that the effect was considerably smaller than for the first two stages. The decreasing effect size presumably reflect the lower importance, in terms of passengers and freight, of the lines that were built later.

B.15 Localized displacement

This sub-section describes our results on localized displacement effects in greater detail. Following Büchel and Kyburz (2020), we estimate local polynomial regressions of residual outcomes on log distance (in meters) to the nearest railway parish in 1855. Under the assumption that railways had no effect on distant parishes, the resulting spatial pattern should be hump-shaped if railways indeed cause reorganization (see also Berger and Enflo, 2017; Bogart et al., 2019). We use the full sample for this analysis since the winners vs. runners-up sample exhibits too little variation in the distance to railway parishes for the analysis to be meaningful.

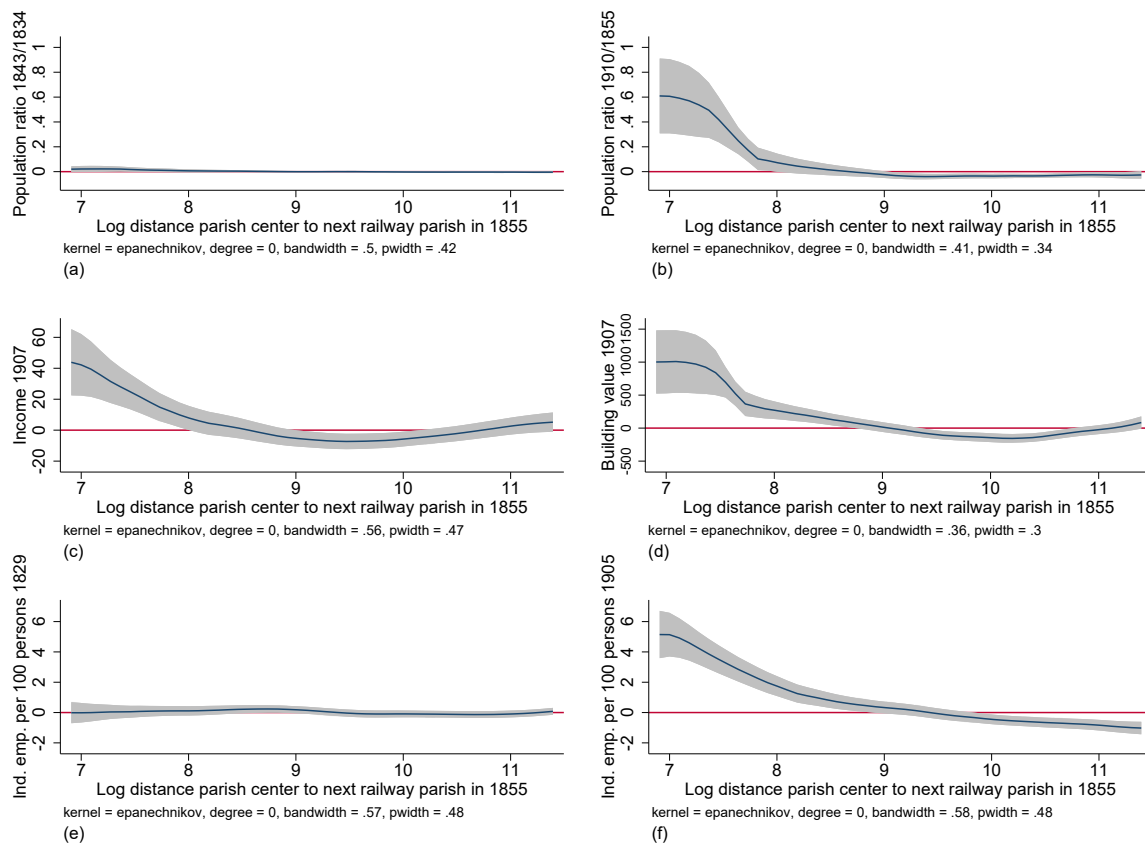


Figure B.13: Polynomial estimates, full sample

Notes: Each graph shows smooth values with 95 percent confidence band from kernel-weighted local polynomial regression of outcome residuals on log distance of parish centroids to the nearest railway parish in 1855. We add 1000 meters to all distances to avoid zero distances and smooth values close to zero. The outcome variables are the population ratio 1843 to 1834 (Graph (a)), the population ratio 1910 to 1855 (Graph (b)), the average annual income in 1907 in *Mark* (Graph (c)), the building value in 1907 in *Mark* (Graph (d)), and the number of full-time employees in industry per 100 persons in 1829 and in 1907 (Graphs (e) and (f)). We take the residuals from OLS regressions of outcome variables on log population and log population density in 1834, the share of protestants in 1821, a dummy for having a manufactory in 1832, industry employment per 100 persons in 1829 (except Graph (e)), elevation, dummies for access to a navigable river in 1845 and to a road in 1848, and district dummies as explanatory variables.

Figure B.13 shows the results for six different outcome variables, namely for the population ratio 1843 to 1834 (Graph (a)), the population ratio 1910 to 1855 (Graph (b)), the average annual income in 1907 in *Mark* (Graph (c)), the building value in 1907 in *Mark* (Graph (d)), and the number of full-time employees in industry per 100 persons in 1829 and 1907 (Graphs (e) and (f)). Residuals come from OLS regressions of the outcome variables on log population and log population density in 1834, the share of protestants in 1821, a dummy for having a manufactory in 1832, industry employment per 100 persons in 1829 (except Graph (e)), elevation, dummies for access to a navigable river in 1845 and to a road in 1848, and district dummies. Reassuringly, the residuals for pre-treatment outcomes, i.e., the population ratio 1843 to 1834 and industry employment in 1829, are uncorrelated with the distance to railway parishes in 1855.

In line with our empirical analysis, Graph (b) of Figure B.13 shows that population growth in 1855–1910 was considerably stronger in parishes close to the railway than in those further away. However, population growth in parishes with railway access did not come only—or even predominantly—at the expense of nearby parishes. This finding is in line with Bogart et al. (2019) who find no evidence for localized displacement effects on population in their study on England and Wales. Büchel and Kyburz (2020), in contrast, document a pronounced hump-shaped pattern in their analysis of population growth in Swiss parishes in the 19th century. Our results for income, housing values, and industrial employment are broadly consistent with our results for population growth. While income and housing values show a small trough at medium distances, industry employment falls monotonically with distance to railway parishes. Overall, we find little evidence for localized displacement effects.

We caution that the cross-sectional regressions in Figure B.13 will only be informative about the “pure” growth effects of railway infrastructure if far-away regions—or some “residual regions” more generally—are unaffected by the treatment (Redding and Turner, 2015). For instance, Section 3.5.1 suggests that immigration from within Württemberg was important for the positive effect of railways on population growth in winner parishes. In this context, Graph (b) of Figure B.13 only clarifies that the relocation of population within Württemberg did not come solely at the expense of parishes in the immediate vicinity of the railway.

B.16 Additional results for the winner versus runners-up sample

Table B.7: DiD estimates of the effect of railway access on population with Conley standard errors

	Winners vs. runners-up		
	(1)	(2)	(3)
Panel A: Distance cut off 10 km			
Treatment effect	0.091 (0.061)	0.117*** (0.018)	0.136*** (0.017)
Panel B: Distance cut off 20 km			
Treatment effect	0.091 (0.062)	0.117*** (0.020)	0.136*** (0.019)
Panel C: Distance cut off 50 km			
Treatment effect	0.091 (0.062)	0.117*** (0.024)	0.136*** (0.022)
Observations	3,276	3,276	3,276
Group FE	Yes	No	No
Parish FE	No	Yes	Yes
Year \times Case FE	No	No	Yes

Notes: The table shows panel regression estimates of the effect of railway access in 1845–54 on log population estimated for the winners versus runners-up sample. All regressions include a full set of year dummies. Regression (1) includes a dummy for the treatment group; all remaining regressions include a full set of parish dummies. Regression (3) additionally includes case by year fixed effects. Conley standard errors are in parentheses. We use Stata command `reg2hdfespatial` to calculate the Conley standard errors (Conley, 1999; Hsiang, 2010). The distance cut off is 10 kilometers in Panel A, 20 kilometers in Panel B, and 50 kilometers in Panel C. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

Table B.8: The effect of railway access on the gender wage gap

	Gender wage gap		
	1884 (1)	1898 (2)	1909 (3)
Panel A: IPW			
Treatment effect	-2.007** (1.020)	-1.736* (0.940)	-0.698 (0.576)
Panel B: IPWRA			
Treatment effect	-2.163** (1.086)	-1.791* (0.952)	-0.727 (0.597)
Panel C: OLS			
Treatment effect	-2.146** (1.032)	-1.918** (0.944)	-0.672 (0.552)
Observations	155	156	156
Control mean	32.78	32.85	33.94

Notes: The table shows regression estimates of the effect of railway access in 1845–54 on the gender wage gap of day laborers in 1884 (Column (1)), 1898 (Column (2)) and 1909 (Column (3)). The regressions in Panel A are estimated by IPW, regressions in Panel B by IPWRA and regressions in Panel C by OLS. All regressions include as control variables log population and log population density in 1834, the share of protestants in 1821, a dummy for having a manufactory in 1832, industry employment per 100 persons in 1829, elevation, dummies for access to a navigable river in 1845 and to a road in 1848, and case dummies. Robust standard errors are in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

B.17 Additional results for the full sample

Table B.9: Comparison of pre-treatment characteristics for parishes on and off straight-line corridors and least cost paths

	Full sample						
	Nodes (1)	On least cost path (500 m) (2)	Off least cost path (500 m) (3)	Difference (2) - (3) (4)	On straight line (500 m) (5)	Off straight line (500 m) (6)	Difference (5) - (6) (7)
Pop. 1834 (log)	8.379 (1.464)	6.548 (0.679)	6.394 (0.704)	0.155 [0.066]	6.577 (0.691)	6.393 (0.703)	0.184 [0.070]
Pop. density 1834 (log)	5.628 (0.753)	4.323 (0.650)	4.281 (0.603)	0.042 [0.057]	4.314 (0.678)	4.282 (0.602)	0.032 [0.060]
Protestants 1821 (share)	0.730 (0.379)	0.558 (0.491)	0.621 (0.468)	-0.063 [0.044]	0.569 (0.486)	0.620 (0.469)	-0.050 [0.047]
Manufactory dummy 1832	0.583 (0.515)	0.049 (0.216)	0.054 (0.226)	-0.005 [0.021]	0.074 (0.263)	0.052 (0.223)	0.022 [0.022]
Ind. employment per 100 persons 1829	12.953 (4.087)	8.330 (3.069)	7.725 (4.374)	0.605 [0.401]	8.396 (3.271)	7.726 (4.355)	0.671 [0.426]
Average elevation (in m)	391.685 (120.070)	417.285 (140.489)	498.940 (155.824)	-81.656 [14.453]	422.137 (137.356)	497.934 (156.205)	-75.797 [15.389]
River dummy	0.417 (0.515)	0.154 (0.363)	0.072 (0.259)	0.083 [0.025]	0.157 (0.366)	0.072 (0.259)	0.085 [0.026]
Road dummy	0.917 (0.289)	0.699 (0.460)	0.476 (0.500)	0.223 [0.046]	0.722 (0.450)	0.477 (0.500)	0.245 [0.049]
Observations	12	123	1,723		108	1,738	

Notes: The table shows average values of pre-treatment characteristics for nodes (Column (1)), parishes that are (Column (2)) and are not (Column (3)) within 500 meters of a least cost path between the nodes, and parishes that are (Column (5)) and are not (Column (6)) within 500 meters of a straight-line between the nodes (Column (5)). Column (4) shows the mean difference in pre-treatment characteristic between parishes on and off the least cost path and Column (7) the mean difference between parishes on and off the straight-line corridor. Nodes are defined as parishes that are either starting or end points of a railway segment or serve as network junctions. In the calculation of the least cost path we follow Keller and Shue (2016) and approximate the gradient related track costs by the foregone freight hauling capacity of trains. In addition, we incorporate political costs and penalize the crossing of foreign territory. We set the cost of crossing foreign territory to equal a complete loss of freight. We calculate a weighted sum of the different costs and create a cost raster, which is used in the least-cost path calculations using Esri ArcGIS's least cost path module. Standard deviations are in parentheses (Columns (1)-(3) and (5)-(6)). Standard errors of a two-sided mean difference t-test are in brackets (Columns (4) and (7)).

Table B.10: The effect of railway access on day laborer wages, taxable income, and building values, full sample

	Day laborer wage (<i>Pfennig</i>)						Taxable income (<i>Mark</i>) 1907 (7)	Building value (<i>Mark</i>) 1907 (8)	Fire insurance value (<i>Mark</i>) 1908 (9)
	Female		Male						
	1884 (1)	1898 (2)	1909 (3)	1884 (4)	1898 (5)	1909 (6)			
Panel A: IPW									
Treatment effect	11.409*** (1.917)	13.586*** (1.989)	11.654*** (2.212)	8.488*** (2.183)	10.219*** (2.393)	16.899*** (3.899)	68.774*** (12.169)	1,364.0*** (248.9)	1,565.7*** (317.5)
Panel B: IPWRA									
Treatment effect	11.456*** (1.980)	13.404*** (1.986)	11.593*** (2.205)	8.718*** (2.210)	10.271*** (2.373)	17.039*** (3.851)	69.810*** (12.136)	1,372.5*** (247.2)	1,584.4*** (316.4)
Panel C: OLS									
Treatment effect	11.542*** (1.931)	12.876*** (1.985)	12.098*** (2.201)	9.377*** (2.126)	10.995*** (2.387)	18.829*** (3.826)	71.443*** (12.508)	1,435.691*** (253.712)	1,687.768*** (314.626)
Observations	1,830	1,846	1,843	1,832	1,846	1,843	1,843	1,843	1,843
Control mean	106.53	116.21	165.76	159.34	173.01	246.96	320.96	2,763.9	3,794.0

Notes: The table shows regression estimates of the effect of railway access in 1845-54 on the average daily wage of female (Columns (1) to (3)) and male (Columns (4) to (6)) day laborers in 1884, 1898, and 1909, on taxable income per capita in 1907 (Column (7)), the average value of buildings in 1907 (Column (8)), and the average fire insurance value per building in 1908 (Column (9)). Values in Columns (1) to (6) are in *Pfennig* and values in Columns (7) to (9) are in *Mark*, with 1 *Mark* = 100 *Pfennig*. Regressions in Panel A are estimated by IPW, regressions in Panel B by IPWRA and regressions in Panel C by OLS. All regressions include as control variables log population and log population density in 1834, the share of protestants in 1821, a dummy for having a manufactory in 1832, industry employment per 100 persons in 1829, elevation, dummies for access to a navigable river in 1845 and to a road in 1848. Robust standard errors are in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

Table B.11: The effect of railway access on industrial development, full sample

	Employment				Estab- lishment size (logs)	Steam engine	
	Industry		Agriculture			(0/1)	HP pc
	1895	1907	1895	1907		1869	1869
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: IPW							
Treatment effect	4.771*** (0.926)	6.122*** (0.905)	-5.653*** (0.947)	-8.575*** (1.348)	0.382*** (0.076)	0.183*** (0.056)	9.12** (4.519)
Panel B: IPWRA							
Treatment effect	4.864*** (0.939)	6.208*** (0.927)	-5.785*** (0.975)	-8.787*** (1.322)	0.388*** (0.076)	0.186*** (0.056)	9.29** (4.516)
Observations	1,846	1,843	1,846	1,843	1,839	1,846	1,846
Panel C: Panel estimates							
Treatment effect	5.795*** (1.086)	7.314*** (1.104)	–	–	0.439*** (0.085)	0.258*** (0.060)	10.50** (4.465)
Observations	3,692	3,689			3,504	3,692	3,692
Control mean	9.629	11.06	31.50	36.84	0.478	0.073	1.002

Notes: The table shows estimates of the effect of railway access in 1845–54 on the number of full-time employees in industry (Columns (1) and (2)) and agriculture (Columns (3) and (4)) per 100 persons in 1895 and 1907, establishment size in industry in 1895 (Column (5)), the probability of having installed at least one steam engine by 1869 (Column (6)), and steam horsepower per 1,000 persons in 1869 (Column (7)). Establishment size is the average number of persons employed in a main plant (*Hauptbetrieb*). Panels A and B display IPW and IPWRA estimates, respectively. Regressions in Panels A and B include as control variables log population and log population density in 1834, the share of protestants in 1821, a dummy for having a manufactory in 1832, elevation, dummies for access to a navigable river in 1845 and to a road in 1848. Panel C displays estimates from panel fixed effects regression that include parish and year fixed effects. The pre-treatment period is 1829 in Columns (1) to (5) and 1846 in Columns (6) and (7). We cannot run panel fixed effects regression for agricultural employment, as we lack data on agricultural employment in the pre-treatment period. The control mean gives the mean value of the outcome for the control group in 1895 (Columns (1), (3), (5)) 1907 (Columns (2) and (4)) and 1869 (Columns (6) and (7)). Robust standard errors are in parentheses. Standard errors in Panel C are clustered at the parish level. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

Table B.12: The effect of railway access on employment in key industrial sectors and specialization, full sample

	Employment in key industrial sectors					
	Textile (1)	Coal, iron & steel (2)	Machines & instruments all (3)	electrical (4)	Chem- ical (5)	Spec- ialization (6)
Panel A: IPW						
Treatment effect	2.381*** (1.069)	-0.004 (0.056)	0.301* (0.171)	0.007 (0.008)	0.076 (0.088)	0.005 (0.014)
Panel B: IPWRA						
Treatment effect	2.398** (1.069)	-0.003 (0.055)	0.303* (0.171)	0.007 (0.008)	0.075 (0.088)	0.005 (0.014)
Observations	1,846	1,846	1,846	1,846	1,846	1,839
Panel C: Panel estimates						
Treatment effect	2.879*** (1.019)	0.038 (0.040)	0.369** (0.175)	0.007 (0.008)	0.111 (0.089)	0.035** (0.014)
Observations	3,692	3,692	3,692	3,692	3,692	3,504
Control mean	0.848	0.015	0.046	0.001	0.027	0.171

Notes: The table shows estimates of the effect of railway access in 1845–54 on the number of full-time employees per 100 persons in different industries (Columns (1)–(5)) and specialization within industry (Column (6)) in 1895. We distinguish between employment in the textile industry (Column (1)), coal, iron, and steel industry (Column (2)), building of machines and instruments (Column (3)), building of electrical machines and instruments (Column (4)), and the chemical industry (Column (5)). Specialization is measured by the Hirschman-Herfindahl-Index (with $\alpha = 2$). Panels A and B display IPW and IPWRA estimates, respectively, using employment in 1895 as outcome variable. Regressions in Panels A and B include as control variables log population and log population density in 1834, industry employment per 100 persons in 1829, a dummy for having a manufactory in 1832, the share of protestants in 1821, elevation, dummies for access to a navigable river in 1845 and to a road in 1848. Panel C displays estimates from panel fixed effects regression that include parish and year fixed effects. The pre-treatment period is 1829. The control mean gives the mean value of the outcome for the control group in 1895. Standard errors in Panel C are clustered at the parish level. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

Chapter 4

Poverty, Pollution, and Mortality: The 1918 Influenza Pandemic in a Developing German Economy

BY RICHARD FRANKE

Abstract The paper provides a detailed analysis of excess mortality during the “Spanish Flu” in a developing German economy and the effect of poverty and air pollution on pandemic mortality. The empirical analysis is based on a difference-in-differences approach using annual all-cause mortality statistics at the parish level in the Kingdom of Württemberg. The paper complements the existing literature on urban pandemic severity with comprehensive evidence from mostly rural parishes. The results show that middle and high-income parishes had a significantly lower increase in mortality rates than low-income parishes. Moreover, the mortality rate during the 1918 influenza pandemic was significantly higher in highly polluted parishes compared to least polluted parishes.

Key words: Pandemics, Spanish Flu, Income, Air Pollution, Mortality.

JEL classification: I14, I15, N34, Q53.

4.1 Introduction

The 1918 “Spanish Flu” was the most deadly influenza pandemic in modern history, likely causing 50–100 million deaths (Johnson and Mueller, 2002). Besides the large range in the global mortality estimates, the local mortality rate estimates show enormous variation across and within countries.¹ To date, the factors influencing the varying regional severity of the pandemic have not been fully explored. This paper studies the local determinants of pandemic mortality in South-West Germany, using exceptionally detailed vital statistics for 1,763 parishes in the German Kingdom of Württemberg. The analysis focuses on the effect of poverty and air pollution on mortality. Both factors have received much attention also in current debates on regional differences in pandemic mortality (Beach et al., 2020; Wu et al., 2020).

The paper makes three contributions to the literature on regional differences in pandemic severity during the 1918 influenza pandemic. First, I study the determinants of pandemic severity at an unusually disaggregated level: the median land area of parishes in the sample is just 8.6 square kilometers, and the median population is 649. The analysis thus complements the existing literature on urban pandemic severity with comprehensive evidence from mostly rural parishes. The focus on rural parishes is essential because previous results indicate considerable differences in pandemic severity by the degree of urbanization (e.g. Clay et al., 2019). Second, I provide the first analysis on the effect of income on regional pandemic severity for Germany and one of the first for continental Europe. This point is important as existing evidence points to significant differences in pandemic severity between Europe and the US (Bootsma and Ferguson, 2007). Moreover, in contrast to the analysis for the US (Bootsma and Ferguson, 2007; Clay et al., 2018, 2019; Grantz et al., 2016), recent studies focusing on Europe provide mixed evidence on the effect of income on influenza mortality (Karlsson et al., 2014; Dahl et al., 2020; Carillo and Jappelli, 2020). Third, I assess the link between air pollution and regional pandemic mortality, motivated by considerable evidence that air pollutants can increase susceptibility to influenza infection (e.g. Jaspers et al., 2005).

Furthermore, the paper provides a detailed description of mortality statistics in Württemberg. The official statistics are disaggregated by cause of death, age, and sex. For instance, the reported influenza mortality did increase by about 3,000 percent in 1918 compared to previous years and showed a distinctive W-shape in the age-specific mortality pattern. To the best of my knowledge, data with a comparable level of detail have not been discussed for any major German

¹For example, Johnson and Mueller (2002) estimate country specific mortality rates between 1.2 and 445.0 per 1,000 persons, for Argentina and Cameroon, respectively, and Bootsma and Ferguson (2007) estimate 4–12 excess deaths per 1,000 persons in a sample of 47 US cities.

state before.² The paper also relates the excess mortality rate estimates for Württemberg to newly estimated excess mortality rates of Germany and its states. Based on these estimates, the paper shows an association of income and air pollution at the national level. Moreover, the paper briefly describes the socio-economic conditions in Württemberg at the beginning of the 20th century and the adverse effects of World War I (WWI) on food supply and pre-pandemic health. The suffering caused by WWI may also explain why the 1918 influenza pandemic did not leave a lasting impression on the collective memory of the German population.

For the empirical analysis, I use annual data on vital statistics (all-age and infant mortality, births) for the universe of parishes in Württemberg in 1914–1925. I combine this data with rich socio-economic data from various population and occupation censuses. For each parish, I observe the amount of total taxable income in 1907 and calculate the average income per capita. In addition, I link the data with available information on the location of coal-fired power plants, a major source of air pollution in the early 20th century (Clay et al., 2018).³ Before WWI, about two-thirds of the installed power plant capacity in Württemberg and neighboring Hohenzollern was based on coal (Ott et al., 1981). The pollution was spatially dispersed from the power plants and affected the pollution levels of parishes in a wider radius. I exploit this fact to calculate the exposure of each parish to pollution from coal-fired power plants.

The empirical analysis is based on a difference-in-differences approach to estimate the effect of poverty and air pollution on pandemic mortality. The approach compares mortality rate changes in poor (least polluted) parishes to mortality rate changes in rich (highly polluted) parishes. The average all-cause mortality rate across parishes in Württemberg was 15.8 deaths per 1,000 persons during the pandemic year 1918, corresponding to a mortality rate increase of 2.9 deaths or 23 percent relative to the baseline in 1917.

The results show that middle and high-income parishes (classified by taxable income per capita) recorded a significantly lower increase in mortality rates than low-income parishes. The respective increase in the mortality rate in medium and high-income parishes was lower by 1.4 and 1.0 deaths per 1,000 population. Moreover, the mortality rate increase from 1917 to 1918 was significantly higher in highly polluted parishes compared to least polluted parishes. The estimates indicate an additional increase in the mortality rate by 1.0 death per 1,000 population. In other words, the spike in 1918 mortality was particularly large in poor and highly polluted parishes.

²Previous studies of the 1918 influenza pandemic in Germany often focus on case studies for smaller areas or the medical debates of the time (Michels, 2010).

³There are no data on the actual air pollution levels in Württemberg available for the time period. However, the level of pollution in the late 19th and early 20th century in industrialized countries is considered to be much higher than today and mainly caused by the usage of coal (Bailey et al., 2018; Beach and Hanlon, 2017).

Two recent articles summarize the extensive literature on the effects of the 1918–1919 influenza pandemic. Beach et al. (2020) focus on literature about economic and health outcomes, and Taubenberger et al. (2019) focus on the medical and biological insights on the 1918 influenza pandemic. The review articles demonstrate that although an extensive body of work has emerged over the last century, numerous questions remain unanswered to date, not least on the origin of the virus and how many deaths it caused. Widely cited estimates on the global mortality burden range between 50 and 100 million deaths (Johnson and Mueller, 2002). This large range in estimates is partially driven by sparse data for developing countries in Africa and Asia, especially China.⁴ However, even for industrialized countries that generally have detailed statistics in the early 20th century, the estimates vary strongly. For example, the excess all-cause mortality rate is estimated between 3.9 and 6.5 deaths per 1,000 persons for the USA, between 2 and 4.1 for Denmark, and between 3.8 and 7.8 for Germany (Beach et al., 2020). Using data on all influenza-related deaths, I show that Württemberg might have experienced up to 4.1 excess deaths per 1,000 persons due to the “Spanish Flu”.

The empirical analysis focuses on the impact of poverty and air pollution on mortality. Both factors have received a lot of attention also in debates on regional differences in mortality during the COVID-19 pandemic. Several studies have analyzed the effect of socio-economic differences on mortality during the 1918 influenza pandemic. However, some of these studies do not study income, but other potentially correlated measures like apartment size (Mamelund, 2006, 2018), social status based on occupation (Bengtsson et al., 2018), and housing conditions (Sydenstricker, 1931; Chowell et al., 2007), or illiteracy rates, homeownership, and unemployment (Clay et al., 2019; Grantz et al., 2016). These papers provide evidence for a socio-economic gradient in mortality rates during the 1918 influenza pandemic. However, the measures used in these studies focus on a specific channel of the income effect. This limitation is not trivial because income differences can affect pandemic mortality through multiple channels, and the studies mentioned above potentially neglect parts of the effect.⁵

Focusing on cross-country differences in income, Murray et al. (2006) and Barro et al. (2020) find a significant negative income effect on pandemic mortality in a sample of 27 and 42 countries,

⁴The actual range of mortality estimates is even larger, e.g., Patterson and Pyle (1991) estimate a global mortality burden of 25–40 million deaths.

⁵Differences in income can affect multiple factors that influence (pandemic) mortality, like the nutritional situation (Blum, 2013), access to sanitary infrastructure (Gallardo-Albarrán, 2020), or access to healthcare (Bauernschuster et al., 2020). For instance, it is not clear to what extent these factors are captured by social status differences of individuals. See Deaton (2003) and Weil (2014) for a detailed discussion on the association between income differences and health.

respectively.⁶ Other studies, however, do not find a significant correlation between income levels and pandemic mortality 1918, e.g., Brainerd and Siegler (2003) for US states, Karlsson et al. (2014) for Swedish counties, and Carillo and Jappelli (2020) for Italian regions. Using Danish municipality level data, Dahl et al. (2020) show that the epidemic intensity in 1918 does not depend on differences in income levels four or more years before the pandemic, but differences in income levels 1917 and income growth from 1916 to 1917.

The literature provides strong evidence on the effect of socio-economic factors, but the effect of income level differences on pandemic mortality is less well understood. In this paper, I show that the income level in 1907 has a statistically and epidemiologically significant effect on the relative change in all-cause mortality rates between 1917 and 1918. I provide evidence that parishes with lower income levels in 1907 experienced a significantly stronger increase in mortality rates from 1917 to 1918. The level of analysis is of particular interest because there is empirical evidence that the influenza pandemic had different impacts in smaller and more rural parishes (Acuna-Soto et al., 2011; Chowell et al., 2007).

The effect of pollution on mortality during the 1918 influenza pandemic has received less attention.⁷ Only recently, an article by Clay et al. (2018) shows the effect of installed coal-fired electricity generating capacity on mortality in a sample of 180 US cities. The authors find the mortality rate to increase by an additional 9.6 percent in high-capacity cities and by 5.4 percent in medium capacity cities, relative to changes in low-capacity cities. Furthermore, Clay et al. (2018) find a significant effect of pollution on infant mortality. In an extended data set of 438 US cities with at least 10,000 inhabitants, Clay et al. (2019) test the effect of multiple factors on excess mortality in 1918, including coal-fired capacity. Their results confirm the significant effect of pollution on pandemic mortality, but only for high-capacity cities.

The empirical strategy of the paper follows mainly Clay et al. (2018). The main difference, however, is the unit of analysis. Clay et al. (2018) use a sample of US cities with at least 20,000 inhabitants, while the median parish in Württemberg has a population of 649 inhabitants. The paper also contributes to a broader literature that analyzes the effect of pollution on (infant) mortality, e.g., Chay and Greenstone (2003) and Currie and Neidell (2005), who study the effect of pollution on infant mortality in the modern-day US, and Beach and Hanlon (2017), who analyzes the effect of pollution on mortality during the Industrialization in England and Wales.

⁶Basco et al. (2021) use Spanish occupation level data and associated income to show a negative association between income and pandemic mortality rates. Furthermore, Clay et al. (2018) control for the manufacturing payroll per worker in 1900, but the results for this estimator are not reported.

⁷The detrimental health effects of pollution on influenza in general, however, have been studied more extensively, see for example Jaspers et al. (2005), Wong et al. (2009), and Wu et al. (2020).

The paper is structured as follows. Section 4.2 describes the socio-economic conditions in Württemberg and the influence of World War I on living conditions, as well as the statistics on influenza and excess mortality during the 1918–1919 influenza pandemic in Württemberg. Furthermore, Section 4.2 compares the excess mortality estimates with new estimates for Germany and provides first evidence on the effect of income and air pollution on regional pandemic severity at the national level. Section 4.3 describes the data used in the empirical analysis on the effect of income and pollution on pandemic mortality rates. Section 4.4 discusses the identification strategy and Section 4.5 reports the main results together with several robustness checks before Section 4.6 concludes.

4.2 World War I and the “Spanish Flu” in Württemberg

On October 19, 1918, the military physician assistant (*Feldhilfsarzt*) Erich Steinthal from Stuttgart published an article about the new “Spanish Disease”. Steinthal (1918) closes his article with a warning that although the press in Württemberg has taken the influenza outbreak lightly, the pandemic’s overall consequences cannot be foreseen. The article, published at the peak of the 1918 influenza pandemic in Stuttgart (the capital of the Kingdom of Württemberg), illustrates the perception of the pandemic as a minor problem in local media. The low level of media and public interest in the pandemic was due to the (self-)censorship of the press and the hardships of everyday survival imposed by the First World War (Witte, 2003). In the following, I describe the socio-economic conditions before and during the Great War and the course of the 1918–1919 influenza pandemic in Württemberg.

4.2.1 The state of Württemberg’s economic development

The Kingdom of Württemberg was one of the four Kingdoms of the German Empire and the third largest after Prussia and Bavaria (see Figure C.1 in the Appendix). According to the 1910 census, Württemberg had a population of about 2.4 million. Württemberg was a latecomer in the Industrial Revolution and its industrialization process still lagged behind other German states at the beginning of the 20th century (Marquardt, 1985; Flik, 2001). The share of agricultural employment in Württemberg was 41.3 percent in 1907, while on average in the German Empire, it was 32.7 percent (Losch, 1912). In addition, Frank (1993) estimates the GDP per capita in 1913 to be 672.26 *Mark* in Württemberg, 745.52 *Mark* in Prussia, and 710.28 *Mark* in neighboring Baden. The aggregate figures, however, mask considerable heterogeneity within Württemberg. The Kingdom comprised four districts (*Kreise*), and the most developed district,

the *Neckarkreis*, had a GDP per capita of 766.8 *Mark* and an agricultural employment share of 33.1 percent in 1907.

Economic historians have identified multiple factors that contributed to the economic backwardness of the Kingdom. One factor is the institutional setting of Württemberg, especially the relatively late abolition of guilds in 1862 (Acemoglu et al., 2011; Ogilvie, 2004, 2019) and the division of property among all heirs (*Realteilung*), which caused fragmentation of property and reduced the mobility of labor (Flik, 2001). Before the construction of railways, Württemberg's economy also faced high transport costs due to the lack of navigable waterways and the hilly topography (Braun and Franke, 2019). In addition, the Kingdom lacked raw materials, such as coal or ore, that have been key drivers in early industrialization (Fernihough and O'Rourke, 2019). In 1913, for example, about 1,000 kilograms of coal were consumed per capita in Württemberg, compared with an average of 3,870 kilograms per capita in the whole German Empire (Statistisches Landesamt, 1923; Statistisches Reichsamt, 1925).

Despite the lack of coal deposits, the public electricity supply in Württemberg before WWI was considered very advanced compared to other German states. An official report states that out of the 1,907 parishes and localities, 1,705 had a sufficient supply of electricity in March 1915 (Ott, 1971). The parishes were served by 273 power plants in 1916, and about two-thirds of the installed power plant capacity in Württemberg and neighboring Hohenzollern was based on coal (Ott, 1971; Ott et al., 1981). Already in 1903, more than 15 percent of the installed steam capacity in Württemberg was used in electric power plants.⁸ With additional power plants completed until 1914, the share of steam engines used for electric power generation increased even further (Ott, 1971). Moreover, WWI did not affect energy production strongly. Statistics show that the usage of coal in Württemberg was 1,000 kilograms per capita in 1913 but 1,037 kilograms of coal per capita in 1918 (Statistisches Landesamt, 1923).⁹

4.2.2 The First World War and food shortages

When the German Empire declared war on Russia on August 1, 1914, there was hardly any publicly noticeable criticism (Herwig, 2014). This changed soon, however, with missing success in military campaigns and an ever prolonging war. In the German Empire, about 2 million

⁸In 1903, there was about 11 MW of installed coal-fired power plant capacity, or about 15,000 horsepower in Württemberg (Ott et al., 1981). Based on a workplace census in 1902, the installed total steam capacity was 102,391 horsepower, including the installed capacity of electric power plants (Königliches Statistisches Landesamt, 1905). The share of steam engines used in electric power generation was likely above 15 percent, because the 11 MW of installed capacity in coal-fired power plants (*Dampfkraftwerke*) excludes steam power in electric power plants that used multiple sources for energy generation in combination with steam (*gemischter Antrieb*).

⁹See Section C.2 in the Appendix for a more detailed discussion of energy consumption and production during WWI.

soldiers lost their lives, and more than 4 million were wounded during the First World War (Statistisches Reichsamt, 1925). In Württemberg, more than 72,000 soldiers died during the war, with over 5,000 deaths due to diseases (Statistisches Landesamt, 1922).¹⁰ Thus, the military losses account for about 3 percent of Württemberg's pre-war population and 14 percent of males of military age (17–45 years old).

The suffering due to WWI, however, was not limited to the soldiers and their families but reached the whole population. Cox (2015) uses data of school-age children in Germany during WWI, and Blum (2011, 2013) uses anthropomorphic data from German World War II soldiers to show severe malnutrition in Germany during WWI. There are multiple reasons for the crisis of food supply. First, the demand of the military for soldiers and draft animals lowered agricultural productivity. For instance, the number of horses in Württemberg decreased by 32 percent between December 1913 and December 1914 (Statistisches Landesamt, 1923). Second, the Allied blockade hindered much-needed agricultural imports to Germany (Howard, 1993). Before WWI, Germany was the largest importer of agricultural products in the world (Blum, 2013). However, Germany not only imported agricultural products for immediate consumption but also fertilizers. Thus, the blockade had an additional negative impact on agricultural productivity. Third, the increasing demand for military products diverted further labor, including women, from agriculture to manufacturing. Finally, crop failures increased food shortages. Most prominently, the potato crop failure of 1916, in combination with the harsh winter of 1916–1917, caused many civilian deaths due to starvation and related diseases. Grebler and Winkler (1940) estimate that 424,000 German civilians died due to starvation during WWI.¹¹

German authorities made several attempts to cope with the food crisis. In 1916 the *Kriegsernährungsamt* was founded. This new state agency should coordinate the efforts of a secure food supply. Measures included the introduction of price ceilings and food rationing. The measures by the government, however, could not increase the food supply and might have even caused the opposite. Blum (2013) describes that the prices for meat relative to other staple products were distorted, such that farmers started to feed staple products to livestock. At the same time, a black market arose, allowing more wealthy people to stock food and thus increase food shortages, especially for poor households (Howard, 1993).

The increasingly hopeless military situation and food shortages facilitated the Kiel mutiny on November 3, 1918. It was the starting point of the German Revolution that ended the German Monarchy within a few days and intensified the calls for peace. On November 11, the

¹⁰Later official statistics report an even higher death toll of 74,026 (Statistisches Landesamt, 1928).

¹¹Other contemporaneous sources estimate an even higher death toll of over 700,000 deaths due to starvation (Cox, 2015). However, this figure might have been exaggerated for political reasons.

Armistice of Compiègne ended the First World War's battles. However, the conditions during the revolutionary period did not improve immediately, partly because the blockade ended only in July 1919.

4.2.3 The mortality burden of the influenza pandemic

In this situation of war and food shortages, the 1918–1919 influenza pandemic reached Württemberg. Figure 4.1 illustrates the monthly number of all-cause deaths for the Kingdom of Württemberg and the capital Stuttgart (Statistisches Landesamt, 1922). The virus that caused the influenza pandemic might have already spread before 1918, but the excess mortality only exceeded detection thresholds worldwide in 3 waves in 1918 and 1919 (Johnson, 2001; Taubenberger et al., 2019). The first wave in northern spring and summer 1918, the second wave in autumn 1918, and the third wave in spring 1919. In line with this pattern, the first wave of the influenza pandemic in Württemberg peaked in July 1918.¹² The overall number of monthly deaths, excluding military personnel and stillbirths, increased from 2,688 in June 1918 to 3,133 in July 1918. The average number of monthly deceased in July for the years 1914 to 1917 is 2,798. Thus, Württemberg has seen an increase in monthly deaths of 17 percent (relative to June 1918) or 12 percent (relative to July 1914–1917). The spike in July 1918 is even more pronounced in Stuttgart, where the number of monthly death increased by 28 or 37 percent, respectively. The big difference in both measures for Stuttgart might indicate that the influenza pandemic hit the city already in June 1918.

The second, more deadly wave of the influenza pandemic spread in Württemberg in October 1918, marked by the vertical dashed line in Figure 4.1. The second wave peaked in November 1918, when the total number of deaths increased to 8,969, an increase of 208 percent relative to the average November 1914–1917. The peak in Stuttgart, however, was already reached in October 1918. Thus, the peak of the second wave of the pandemic was earlier in the capital. This observation fits the well-documented pattern of the spread of the 1918 influenza pandemic, i.e., a spread from more central urban hubs to the rural hinterlands (Clay et al., 2019). Finally, Figure 4.1 reveals the effects of a third wave that spread in Württemberg in April and May 1919, with a peak in overall mortality in May. The magnitude of the third wave is comparable to the first wave, but it was the second wave that was the most severe.

A similar picture is drawn by Figure 4.2, where the solid line depicts the number of deceased infants under age one per 1,000 births in a given month within Württemberg and the dashed line the total number of births per month. Although the infant mortality rate shows higher volatility

¹²Bogusat (1923) notes that the first influenza infections in Württemberg were recorded already in March 1918. These are the earliest records of influenza infections in the German Empire.

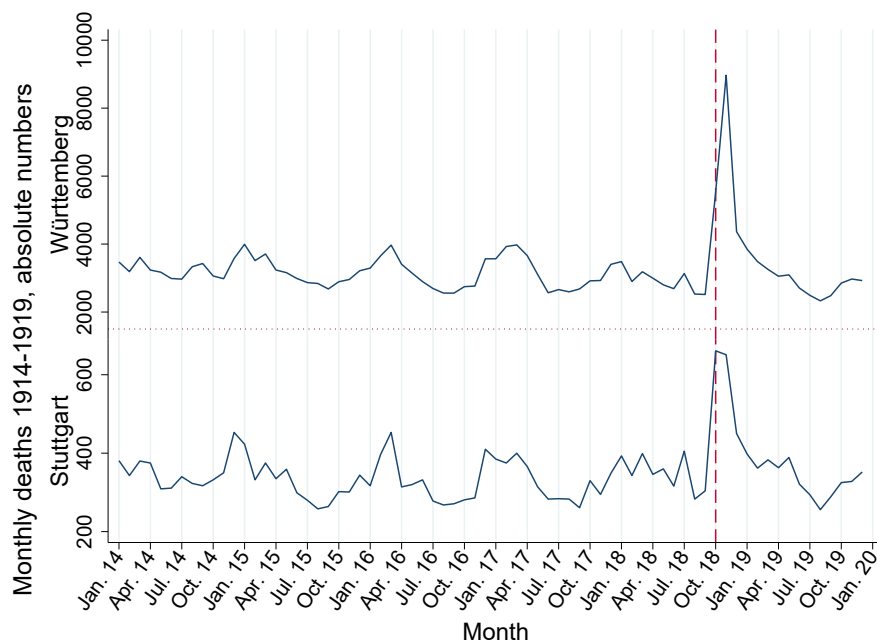


Figure 4.1: Absolute number of monthly deceased 1914–1919

Notes: The graph depicts monthly total deceased in the Kingdom of Württemberg (upper graph) and the capital Stuttgart (bottom graph) for the years 1914–1919. The number of deceased does not include military personnel and stillbirths. *Source:* Statistisches Landesamt (1922). Author's design.

than the total number of deaths in Figure 4.1, there are two distinct peaks in July and November 1918. These peaks in the infant mortality rate coincide with the first and second waves of the influenza pandemic. Additional data indicate an increase in the number of stillborn during the second wave in October and November 1918 relative to the average number of stillborn in the given months in 1914–1917 by about 9.2 and 6.8 percent, respectively. The statistics thus provide some evidence that the 1918 influenza increased the risk of miscarriage (Bloom-Feshbach et al., 2011; Reid, 2005).

Figure 4.2 reveals two additional findings. First, a decline in the number of births in May 1915, i.e., ten months after the German Empire entered the war, the number of births decreased by 26.5 percent compared to the previous month and 34.3 percent compared to May 1914. During the war, the number of births remained at a low level and only reached pre-war levels in September 1919. Second, there is a peak in the infant mortality rate in August and September 1914. On average, 812 infants below the age of one died per month in 1914. In August and September 1914, however, 1,162 and 1,291 infants died, respectively. Such strong increases in infant mortality during the summer months were not uncommon and often caused by heatwaves. This illustrates that the relatively high volatility in the infant mortality rate is partially driven by shocks that are uncorrelated to the 1918 influenza pandemic, like heat waves, local epidemics,

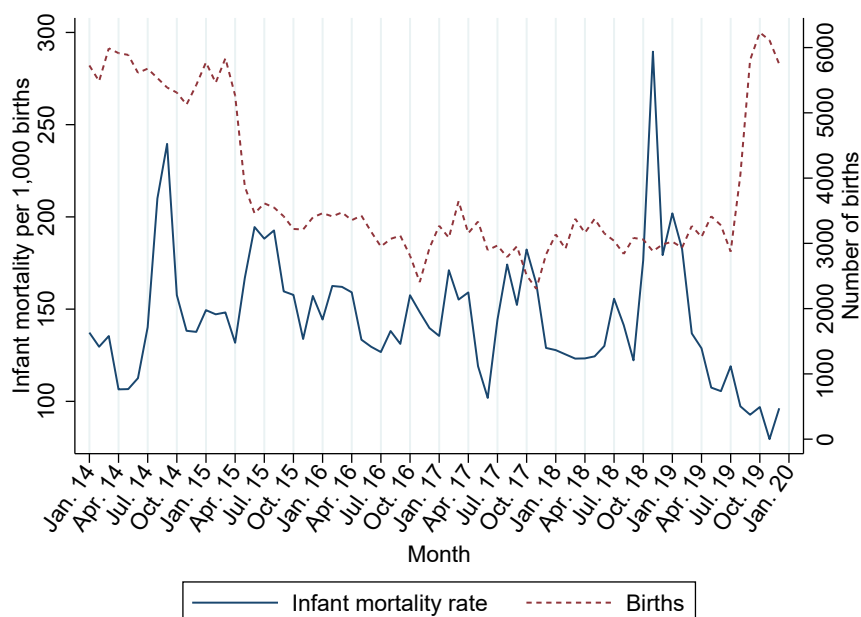


Figure 4.2: Monthly infant mortality rate and absolute number of births 1914–1919

Notes: The graph depicts monthly total births (solid line) and infant mortality rate (dashed line) in the Kingdom of Württemberg for the years 1914–1919. The infant mortality rate is calculated as the number of deceased infants less than one year old per 1,000 births. Stillbirths are not included. *Source:* Statistisches Landesamt (1922). Author’s design.

or changes in fertility. The more noise measure might complicate the estimation of the effect of income and pollution on the infant mortality rate by inflating standard errors (see Section 4.5).

The monthly data on all-cause mortality allow us to identify the onset of the 1918–1919 influenza pandemic and to distinguish the severity of the different waves in line with previous studies. Unfortunately, these data do not allow a further breakdown by cause of death, age, or sex. Therefore, I revert to annual data published in Statistisches Landesamt (1922). Figure 4.3 shows several annual mortality statistics by sex and age groups for the Kingdom of Württemberg, excluding military personnel. Figure 4.3 (a) shows the total number of all age influenza deaths per year and distinguishes between male (blue bars) and female (red bars) deaths. In the years 1914 to 1917, the statistics report on average 103 male and 134 female deaths per year due to influenza in Württemberg. This number increases sharply in 1918 to 2,941 male and 4,322 female deaths. Thus, the statistics indicate 7,026 excess influenza deaths in 1918, i.e., relative to the average of 1914–1917. In 1919, when the third wave of the 1918–1919 influenza pandemic hit Württemberg, the official statistics record 525 male and 692 female influenza deaths.

The influenza mortality statistics might suffer from under-reporting, especially due to cases of influenza that were wrongly assigned to pneumonia.¹³ To evaluate the magnitude of this

¹³Additionally, pneumonia was often caused by an initial influenza infection. Thus, although pneumonia might have been correctly diagnosed, some cases were caused by the influenza pandemic.

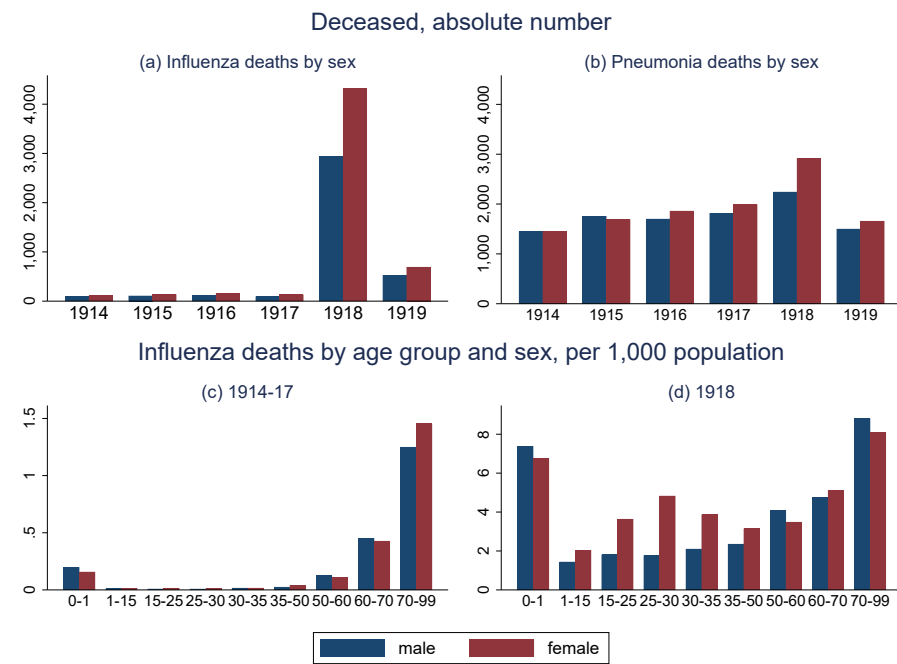


Figure 4.3: Aggregated influenza and pneumonia statistics for Württemberg

Notes: Figures 4.3 (a) and (b) show annual influenza and pneumonia deaths by sex for the years 1914 to 1919. Figure 4.3 (c) depicts the annual influenza mortality rate by age group averaged over the years 1914–1917 and Figure 4.3 (d) shows the influenza mortality rate by age group in 1918. The number of deceased does not include military personnel and stillbirths. *Source:* Statistisches Landesamt (1922). Author’s design.

misreporting, Figure 4.3 (b) shows the annual number of pneumonia deaths by sex for the years 1914 to 1919. Indeed, there is an increase in reported pneumonia deaths in 1918 relative to the previous years. In 1918 there are 2,242 male and 2,914 female deaths assigned to pneumonia, while the average for 1914–1917 is 1,679 and 1,747, respectively. This provides suggestive evidence that a considerable amount of influenza deaths have been assigned to pneumonia.¹⁴ Therefore, most studies focus on all-cause mortality to prevent measurement error.

The reported differences in male and female influenza and pneumonia deaths should not be mistaken as differences in influenza mortality rates by sex. They are mainly driven by the exclusion of military personnel. When influenza and pneumonia deaths of military personnel are included, the differences between the sexes decrease significantly (see Figures C.3 (a) and (b) in the Appendix).

Figures 4.3 (c) and (d) report the influenza mortality rate per 1,000 population by nine age groups and sex. Figure 4.3 (c) shows the average mortality rates for the years 1914–1917, while Figure 4.3 (d) shows the influenza mortality rates for 1918. Unfortunately, there is no age distribution of the whole population available for each year. Thus, I use the age distribution of

¹⁴Figures C.3 (c) and (d) in the Appendix show pneumonia mortality rates per 1,000 population by age groups and sex, for the years 1914–1917 and 1918, respectively. The increase in mortality rates specifically among young adults in 1918, in line with the W-shaped age-specific mortality pattern of the “Spanish Flu”, provides further evidence for the false assignment of cases.

the latest census in 1910 to calculate the age-group-specific influenza mortality rates for each year. Given the decline in birth rates during WWI, the mortality rate of infants and the youngest age group are therefore downward biased.

The average influenza mortality rate per 1,000 population for the years 1914–1917 is 0.2 for infants, almost zero for the age groups between 1 and 50 years old, and increases significantly for people above age 50. The highest mortality rate is observed for the population above age 70, with 1.4 influenza deaths per 1,000 population. The average influenza mortality rate per 1,000 population in 1918 is about 3.0 and thus much higher than in the previous years.¹⁵ The highest influenza mortality rates in Figure 4.3 (d) are observed for infants (7.4 for males and 6.7 for females) and people above age 70 (8.8 for males and 8.1 for females). The true infant mortality rate in 1918 is even higher because Figure 4.3 (d) neglects the decline in birth rates during WWI. Dividing the total number of influenza deaths of infants below the age of one by the number of births in 1917 increases the influenza mortality rate to 11.4. The age-specific distribution of influenza mortality in 1918 is commonly described as W-shaped, i.e., high mortality rates among the youngest and oldest population groups, but also relatively high mortality rates among young adults, peaking at about age 27 (Taubenberger et al., 2019).¹⁶ Figure 4.3 (d) matches the W-shaped mortality pattern, but only for females. Again, this is due to the exclusion of influenza deaths of military personnel.

In general, the cause-specific mortality statistics describe a pattern of the “Spanish Flu” in Württemberg that is in line with the findings for other countries and regions. Yet, the data also demonstrate the difficulties to account for changes in birth rates, influenza mortality of military personnel, and insufficient diagnostics at the time. Therefore, most scholars use all-cause mortality to calculate excess mortality during the 1918–1919 influenza pandemic. However, the estimates for the excess mortality in Germany vary considerably. Johnson and Mueller (2002) estimate the death toll of the influenza pandemic in Germany to be about 225,000 and the excess mortality per 1,000 population to be 3.8. Patterson and Pyle (1991) estimate a range of 4.2 to 5. Ansart et al. (2009) use monthly all-cause mortality statistics and estimate the cumulative excess mortality rate to be 7.3. Murray et al. (2006) and Barro et al. (2020) compare the annual all-cause and influenza-related mortality in 1918–1920 with the average in the three-year periods

¹⁵This is a lower bound of the true influenza mortality rate in 1918 because it neglects the influenza deaths among military personnel and does not account for wrongly assigned pneumonia deaths. On the other hand, it uses the population of the 1910 census as denominator, which causes an upward bias in the mortality rate. When I use the average total population of 1918 as denominator (Statistisches Landesamt, 1928), include influenza deaths among military personnel and excess pneumonia deaths in 1918 (Statistisches Landesamt, 1922), then the adjusted influenza mortality rate per 1,000 population increases to 4.2.

¹⁶Figure C.4 in the Appendix documents a similar age-specific mortality pattern for Germany and its states. The W-shaped curve of age-specific influenza mortality, however, is not always observed (Cilek et al., 2018).

before and after 1918–1920. The resulting cumulative excess mortality rates are 7.6 and 7.8, respectively. Thus, the estimates for Germany range from 3.8 to 7.8, i.e., they vary by a factor of about two. The variation in the estimates can be explained by different data sets, estimation methods, and definitions.

Applying the different approaches to the data available for Württemberg, the excess mortality rate per 1,000 population is 3.8 based on the estimation method in Ansart et al. (2009), 2.3 using all-cause mortality, and 3.9 using all influenza-related deaths based on the definitions by Murray et al. (2006) and Barro et al. (2020), respectively.¹⁷ In addition, I re-estimate excess mortality rates for Germany and its states using mortality and population statistics published in Statistisches Reichsamt (1920, 1921b, 1922, 1924, 1925), see Appendix C.3 for further details. The estimates for Württemberg are slightly higher, with 2.5 and 4.0 excess deaths per 1,000 population based on the definitions by Murray et al. (2006) and Barro et al. (2020). For Germany, the respective estimates are 5.4 and 5.9 excess deaths per 1,000 population, with a range of 2.5–8.0 and 4.0–7.3 across the German states, see Table C.2 in the Appendix. Thus, the estimates for Württemberg are at the lower end of the estimates for Germany.

The variation in regional excess mortality rates raises the question of underlying factors. In the contemporary public perception, there was a link between influenza mortality, food shortages, and the poor health situation caused by the war (Michels, 2010). On the other hand, the relatively high mortality rate observed among healthy young adults seemingly contradicts this explanation (Bogusat, 1923; Taubenberger et al., 2019). Bootsma and Ferguson (2007) show the effectiveness of non-pharmaceutical interventions (NPIs) on pandemic mortality in US cities. These measures included the closure of schools and churches or mandated mask-wearing. However, NPIs can not explain regional differences in Germany because stringent measures were rarely introduced or were short-lived (Witte, 2003; Michels, 2010).

Furthermore, previous studies have discussed the effect of income and air pollution on regional differences in pandemic severity. Figure 4.4 shows that both factors correlate with excess mortality rates of German states and Prussian provinces.¹⁸ Univariate linear regressions indicate a positive and statistically significant association between influenza-related excess mortality rates and GDP per capita in 1907 (0.004, s.e. 0.002) and installed coal-fired capacity in 1913 (0.069, s.e. 0.020). However, in a regression with both factors, the association between income and excess mortality becomes negative (-0.005, s.e. 0.003), while the pollution effect remains

¹⁷Further details on the estimation strategies and results are presented in the Appendix C.3. Unfortunately, I could not determine the exact definitions used by (Johnson and Mueller, 2002) and (Patterson and Pyle, 1991).

¹⁸Data for the states Mecklenburg-Schwerin and Mecklenburg-Strelitz are missing. Furthermore, Berlin is excluded from the analysis because it was the only metropolis of Germany and had significantly different characteristics. However, the results hold qualitatively if Berlin is included (see Figure C.5 in the Appendix).

positive and statistically significant (0.111, s.e. 0.035). These results motivate a further analysis of the two factors.

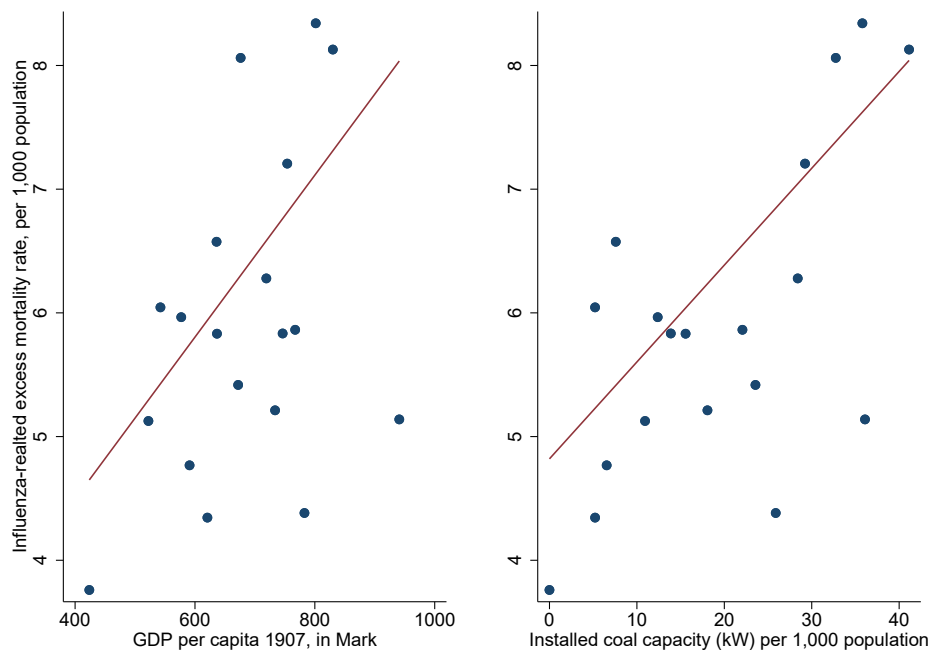


Figure 4.4: Association of income and pollution with influenza-related excess mortality 1918–20 for German states and Prussian provinces

Notes: The graph shows the association of income (left panel) and pollution (right panel) with influenza-related excess mortality 1918–20 for German states and Prussian provinces. The influenza related mortality rate per 1,000 population, includes all deaths from influenza, pneumonia, other diseases of the respiratory organs, tuberculosis, and whooping cough. The excess mortality rate is the sum of deviations in 1918–20 from the average in 1921–23. The income level is measured by GDP per capita in 1907. Pollution is calculated as the installed coal-fired capacity 1913 in kW per 1,000 population. *Sources:* Herzig et al. (1986), Frank (1993), and Statistisches Reichsamt (1920, 1921b, 1922, 1924, 1925), see Appendix C.3 for further details. Author’s design.

4.3 Data

The remainder of the paper focuses on the effect of income and air pollution on pandemic mortality rates during the 1918 influenza pandemic. For the estimations, I use digitized annual data on vital statistics (all-age and infant all-cause mortality, number of births) for the universe of parishes in Württemberg in 1914–1925 (Staatsarchiv Ludwigsburg E 258 VII Bü 120 and 122). I aggregate parishes to take border changes during the sample period into account.¹⁹ Unfortunately, the vital statistics on the parish level are missing for the county *Hall* and two parishes. Thus, there are 1,763 parishes in the resulting data set. The median parish has a land area of 8.7 square kilometers and 649 inhabitants in 1910.

¹⁹Figure C.6 in the Appendix shows parish borders. We digitized parish borders from Kommission für geschichtliche Landeskunde in Baden-Württemberg and Landesvermessungsamt Baden-Württemberg (1972) and used information on border changes from Statistisches Landesamt Baden-Württemberg (2008).

I combine these data with rich socio-economic data from various population and occupation censuses, digitized by Braun and Franke (2019). In particular, the population data are based on censuses in 1910, 1919, and 1925 (Statistisches Landesamt Baden-Württemberg, 2008). To get annual population figures for each parish, I linearly interpolate the population between census years.²⁰ Based on the annual vital statistics and interpolated population data, I calculate the annual all-cause mortality rate per 1,000 population and the annual infant mortality rate per 1,000 births (*IMR*). To prevent biased estimates due to the large changes in fertility during WWI, I subtract the number of infants deceased at age one and below from the total number of deaths. Thus, the main dependent variable is defined as the number of deaths above the age of one per 1,000 population, excluding deaths among military personnel and stillbirths.²¹ Henceforth, I will refer to this variable as mortality rate (*MR*) if not otherwise stated.

The two explanatory variables of interest are the parish income and air pollution levels. Income per capita is measured as the total taxable income of natural persons in 1907, i.e., income net of tax allowances and other deductions, divided by total population (Königliches Statistisches Landesamt, 1910). To measure pollution, I link the data with available information on the location of coal-fired power plants, a major source of air pollution in the early 20th century (Clay et al., 2018). The location of power plants in 1914 is taken from a map by Ott et al. (1981). The map includes all 594 power plants in Württemberg and neighboring Baden and Hohenzollern. I geo-referenced the map using geographic information software to get the location of each power plant. In addition, the map provides information on the type of power generation and the installed maximum capacity. For each parish, I calculate the installed steam-powered capacity (*Dampfkraft*) in MW within 50 kilometers.²²

For the later estimation, I generate dummies for the terciles of income 1907. Hence, these dummies indicate parishes in the sample with low, medium, and high average income. Independently, I generate dummies for the terciles of installed coal-fired power plant capacity within 50 kilometers. The average income per capita is 320.7 *Mark* (see Table 4.1). However, low-income parishes have an average income of 226.0 *Mark* per capita. The average income increases to 312.6 and 423.6 *Mark* per capita for medium and high-income parishes. The average installed

²⁰Alternatively, I project the population for each parish based on the annual number of births and deaths and only interpolate the residual that is due to migration. The estimation results using this alternative population measure are virtually identical. The results are not reported for the sake of brevity but can be received on request.

²¹The main results are, however, robust to the inclusion of infant mortality, see Section 4.5.2.

²²Clay et al. (2018, 2019) choose a similar radius of 30 miles (approximately 48.3 kilometers). The radius is chosen because power plant emissions disperse locally and Levy et al. (2002) show that about 40 percent of primary fine particulate matter (PM_{2.5}) exposure is located within 50 kilometers of modern coal-fired power plants. The dispersion radius, however, depends on the height of the smokestack, which was likely lower in early 20th century Württemberg. Thus, an even higher share of total exposure would have occurred within 50 kilometers.

coal-fired power plant capacity within 50 kilometers is 2.3 MW, 19.2 MW, and 53.7 MW for the low, medium, and high tercile. The average over all parishes is 24.9 MW.

Column (1) of Table 4.1 summarizes the mean and standard deviation of the main variables in the data set. The average mortality rate in 1914–1925 is 11.8 deaths per 1,000 population, with a standard deviation of 5.8. The average infant mortality rate in Württemberg is 135.4 per 1,000 births, with a standard deviation of 139.9.²³ Columns (2) and (3) show the mean difference in mortality rates between low-income parishes and medium and high-income parishes, respectively. In parishes with a medium-income, there are on average 0.5 fewer deaths per 1,000 population compared to parishes in the low-income tercile. The difference is statistically significant at the one percent level (s.e. 0.098). Also, parishes in the high-income tercile have a significantly lower mortality rate. However, the mortality rates of the three groups evolve largely in parallel over time, as shown in Figure 4.5. The average infant mortality rate is significantly higher in medium and high-income parishes.²⁴

Columns (4) and (5) of Table 4.1 show the average difference in (infant) mortality rates between parishes with low and medium and low and higher coal-fired capacity, respectively. There is no statistically significant difference in average mortality rates by coal-fired capacity. Moreover, Figure 4.6 shows that the average mortality rate of all three groups moves in parallel over the period 1914 to 1925 but in 1918. The infant mortality rate, however, is significantly lower in parishes with medium and high coal-fired capacity.²⁵

Table 4.1 also shows the excess mortality rate in 1918. The excess mortality rate for parish i is the difference between the observed mortality rate in 1918 and the predicted mortality rate. The prediction for 1918 is based on a model with parish fixed effects and parish-specific linear trends, estimated for the sample period 1914–1917, 1919–1925 as in Clay et al. (2019). Figure C.6 in the Appendix illustrates the variation of excess mortality rates across parishes. The Figure also shows that there is no clear spatial pattern of excess mortality in Württemberg. The excess infant mortality rate is calculated analogously. The average excess mortality rate in 1918 is 4.1 deaths per 1,000 population. The magnitude of this estimate is thus comparable to the estimates presented in Section 4.2.

The differences in Columns (2) to (5) provide first unconditional evidence of the effect of income and pollution on excess mortality during the 1918 influenza pandemic. Medium and high-income parishes had significantly lower excess mortality rates in 1918. Compared to low-

²³The average infant mortality rate in Württemberg was relatively high compared to other Western European countries. For instance, the average infant mortality rate in the same period was 87.6 in England and Wales, 112.1 in France and 76.8 in the Netherlands (Rothenbacher, 2002).

²⁴Figure C.7 in the Appendix shows the average infant mortality rate over 1914–1925 by income tercile.

²⁵Figure C.8 in the Appendix shows the average infant mortality rate over 1914–1925 by coal capacity tercile.

Table 4.1: Descriptive statistics

	All	Income		Coal	
	(1)	Medium	High	Medium	High
<i>Outcomes</i>					
MR per 1,000 pop. 1914-25	11.766 (5.774)	-0.536 [0.098]	-0.690 [0.100]	-0.123 [0.101]	-0.015 [0.098]
IMR per 1,000 births 1914-25	135.420 (139.857)	7.210 [2.359]	6.739 [2.405]	-17.933 [2.505]	-36.638 [2.225]
Excess MR per 1,000 pop. 1918	4.094 (6.455)	-1.729 [0.384]	-1.790 [0.388]	0.242 [0.387]	1.432 [0.367]
Excess IMR per 1,000 births 1918	20.022 (174.892)	-10.234 [10.597]	8.162 [10.898]	-10.456 [11.359]	-26.015 [8.849]
<i>Explanatory variables</i>					
Tax income per capita 1907 (<i>Mark</i>)	320.683 (97.176)	86.794 [1.842]	198.074 [3.759]	-44.427 [5.163]	-59.682 [5.582]
Coal capacity within 50 km (MW)	24.911 (23.130)	-8.345 [1.326]	-15.071 [1.286]	16.869 [0.576]	51.341 [0.219]
<i>Controls</i>					
Pop. 1910 (log)	6.556 (0.853)	0.179 [0.038]	0.343 [0.053]	-0.006 [0.047]	0.374 [0.049]
Pop. density 1910 (log)	4.412 (0.663)	-0.091 [0.032]	-0.059 [0.041]	0.160 [0.033]	0.727 [0.034]
Industry employment share 1905	11.071 (7.529)	2.462 [0.365]	3.743 [0.443]	1.839 [0.400]	4.735 [0.411]
Establishment size 1895 (log)	1.731 (1.065)	0.161 [0.043]	0.603 [0.068]	-0.028 [0.049]	0.168 [0.065]
Hydro capacity within 50 km (MW)	22.441 (15.044)	-6.514 [0.909]	-11.275 [0.830]	14.181 [0.716]	16.874 [0.639]
Birth non local 1900 (%)	0.262 (0.116)	0.028 [0.006]	0.124 [0.006]	-0.080 [0.007]	-0.091 [0.006]
Railway station 1910, dummy	0.279 (0.448)	0.105 [0.024]	0.247 [0.025]	0.004 [0.025]	0.078 [0.026]
Road access 1848, dummy	0.483 (0.500)	0.087 [0.029]	0.199 [0.029]	-0.117 [0.029]	-0.040 [0.029]
River access, dummy	0.078 (0.268)	0.067 [0.013]	0.095 [0.015]	-0.025 [0.015]	-0.007 [0.016]
Dist. to military base 1918 (km)	5.607 (3.412)	-0.086 [0.182]	-0.566 [0.201]	-1.069 [0.209]	-2.629 [0.188]

Notes: The table shows average values and associated standard deviations in parenthesis below for all 1,763 parishes in the data set (Column (1)). In Columns (2) to (5), the table shows mean differences between parishes with lowest income per capita in 1907 (Columns (2) and (3)) and lowest installed coal-fired power plant capacity within 50 kilometers (Columns (4) and (5)), relative to medium (Columns (2) and (4)) and high levels (Columns (3) and (5)). The standard errors of a two-sided mean difference t-test are in brackets below.

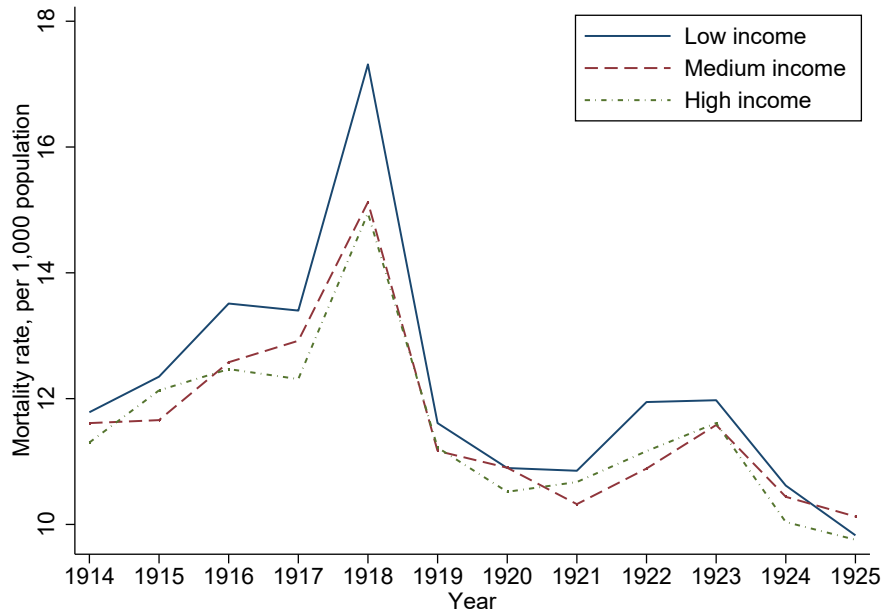


Figure 4.5: Mortality rate 1914–1925 by income tercile

Notes: The figure shows the annual average mortality rate per 1,000 population in parishes for the years 1914 to 1925 by terciles of income 1907. *Sources:* Königliches Statistisches Landesamt (1910), Statistisches Landesamt Baden-Württemberg (2008), and Staatsarchiv Ludwigsburg E 258 VII Bü 120. Author's design.

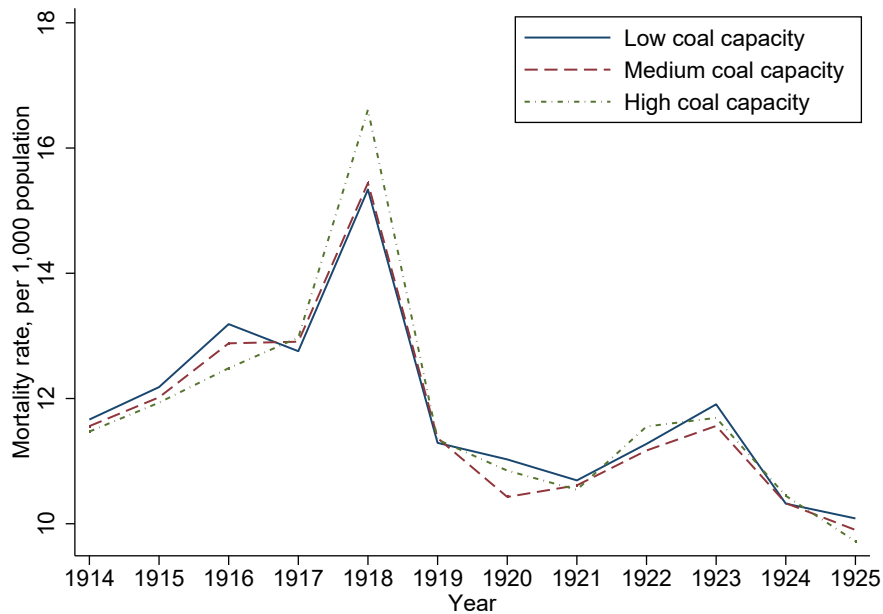


Figure 4.6: Mortality rate 1914–1925 by coal capacity tercile

Notes: The figure shows the annual average mortality rate per 1,000 population in parishes for the years 1914 to 1925 by terciles of installed coal-fired power plant capacity within 50 kilometers. *Source:* Ott et al. (1981), Statistisches Landesamt Baden-Württemberg (2008), and Staatsarchiv Ludwigsburg E 258 VII Bü 120. Author's design.

income parishes, the excess mortality rate decreases by 1.7 and 1.8 excess deaths per 1,000 population. Parishes with the highest coal-fired capacity have 1.4 additional excess deaths per 1,000 population. The difference is statistically significant at the one percent level (s.e. 0.367). For the excess infant mortality rate, the differences are not statistically significant except for highly polluted parishes. The unconditional difference is negative, which would imply that the excess infant mortality rate is significantly lower in parishes with high coal-fired capacity compared to parishes with low coal-fired capacity.

The control variables are log population, log population density in 1910, and the ratio of industrial employment over 100 population in 1905, based on the occupation census 1907 (Königliches Statistisches Landesamt, 1910). The occupation census comprises parish-level information on the number of full-time gainfully employed persons (self-employed and dependent) in agriculture, industry, and trade and transport. Furthermore, I use data from the *Gewerbestatistik* to calculate the establishment size in industry as the average number of persons employed in an establishment (*Hauptbetrieb*) in 1895 (Königliches Statistisches Landesamt, 1900a), the installed capacity of hydroelectric power plants within 50 kilometers (Ott et al., 1981), and the population share that is born in another parish in the year 1900 (Statistisches Landesamt Baden-Württemberg, 2008). To control for access to transport infrastructure, I include binary dummy variables that indicate a railway station in the parish in 1910 (Königliches Statistisches Landesamt, 1911), access to a river navigable in 1845, and connection to a paved road in 1848 (Kunz and Zipf, 2008). Finally, I digitized the location of each military base in Württemberg in 1918 and calculated the distance to the nearest military base in kilometers (von Moser, 1927).

Table 4.1 shows descriptive statistics for the control variables. Parishes with higher income have a higher population size, are more industrialized with, on average larger establishments, and have better access to transport infrastructure. At the same time, parishes with high coal-fired capacity are larger, more densely populated, and more industrialized. Moreover, they have more hydroelectric capacity installed within 50 kilometers and are further away from the nearest military base. Overall, the control variables capture significant differences between parishes by income and pollution level.

4.4 Estimation strategy

The empirical analysis is based on a difference-in-differences approach that compares the average change in mortality rates during the influenza pandemic across parishes with high and medium-income (coal-fired capacity) relative to parishes with low-income (coal-fired capacity).²⁶ The

²⁶The empirical strategy is similar to the approach used in Hornbeck (2012) and Clay et al. (2018).

baseline empirical model is specified by the following equation

$$\begin{aligned}
 MR_{it} = & \sum_{t=1914}^{1925} \beta_{1t} MI_i \cdot d_t + \sum_{t=1914}^{1925} \beta_{2t} HI_i \cdot d_t + \sum_{t=1914}^{1925} \beta_{3t} MP_i \cdot d_t + \sum_{t=1914}^{1925} \beta_{4t} HP_i \cdot d_t \\
 & + \sum_{t=1914}^{1925} \gamma_t \overline{MR}_{i,1910-13} \cdot d_t + \delta X_i \cdot I_{\{1918\}} + d_i + d_t + d_{kt} + \varepsilon_{it}, \quad (4.1)
 \end{aligned}$$

where MR_{it} is the mortality rate in parish i and year t , defined as the number of deaths, excluding infants less than one year old, stillborn children, and military personnel. The mortality rate is regressed on binary dummy variables that indicate parishes with medium income MI_i and coal-fired capacity MP_i , and high-income HI_i and coal-fired capacity HP_i , each interacted with a set of time fixed effects d_t . The coefficients β_{jt} are normalized, such that $\beta_{j,1917} = 0$. Thus, the estimator β_{1t} captures the differential change in the mortality rate from 1917 to year t in medium-income parishes relative to the change in low-income parishes, conditional on pre-pandemic characteristics.²⁷

To control for pre-pandemic parish characteristics, equation (4.1) includes several control variables. Among these control variables are the average mortality rate of the years 1910–1913 in parish i ($\overline{MR}_{i,1910-13}$), interacted with a set of time fixed effects d_t and a set of time-invariant parish specific controls variables X_i that are interacted with an indicator variable $I_{\{1918\}}$. The indicator variable $I_{\{1918\}}$ is one for the pandemic year 1918 and zero otherwise. Furthermore, equation (4.1) includes parish fixed effects d_i that control for any time-invariant parish characteristics, e.g., geographic factors. The time fixed effects d_t and district times year fixed effects d_{kt} control for influences on mortality that vary by time and district, like weather shocks or the overall effect of WWI. The standard errors ε_{it} are clustered at the county level.

The control variables in X_i are as specified in Table 4.1 and can broadly be grouped into two categories. The first category comprises variables that control for socio-economic development and related pre-pandemic health differences between parishes. Population size and density, industry employment share, firm size, installed hydroelectric capacity, and the share of non-local born inhabitants can be group into this first category. Population size and density are included because there is empirical evidence that larger cities in the US might have been able to implement more effective non-pharmaceutical interventions or had a higher immunity in the second wave due to an earlier exposure to the virus (Bootsma and Ferguson, 2007; Acuna-Soto et al., 2011; Clay et al., 2019). On the other hand, densely populated areas could have enhanced the spread (Mills et al., 2004; Chowell et al., 2007). The transmission of the virus might have

²⁷The results are robust to changes in the baseline year, see also Section 4.5.2.

been also higher if people had more contact in their workplace. Therefore I control for the average firm size.

Several studies document the effect of pre-pandemic health on pandemic mortality (Bootsma and Ferguson, 2007; Clay et al., 2019). Here, I include the average mortality rate in 1910–1913 and the share of population born outside of the parish to control for this effect (Clay et al., 2018, 2019). A higher average mortality rate in 1910–1913 indicates a poorer local health environment or a different age structure of the population or both. The share of the non-local-born population takes into account that during the industrialization, rural flight brought many workers into the economic centers. These (internal) migrants are probably younger, poorer, and have worse health than the average local population.²⁸ The inclusion of the industrial employment share and firm size controls for adverse health outcomes of industrial employment caused by the relatively low level of occupational safety.

The second category of control variables captures the potential difference in the exposure to the virus and the timing of onset. Parishes with better access to transport infrastructure might have been more exposed to the virus and might have had an earlier onset (Hogbin, 1985). However, the direction of this effect on pandemic mortality is unclear. An earlier onset of the pandemic might have increased mortality because the virulence may have changed over time (Clay et al., 2019).²⁹ At the same time, more central parishes might have seen a stronger first wave and thus had a higher immunity in the second, more deadly wave (Acuna-Soto et al., 2011; Clay et al., 2019). On the other hand, very remote parishes might have even escaped the pandemic (Erkoreka, 2020). Therefore, I include binary indicators that control for access to the railway, central roads, and navigable waterways. I also control for the distance to the nearest military base because the spread of the virus was likely accelerated by the movement of troops (Patterson and Pyle, 1991).

For a causal interpretation of the effect of income and coal capacity, it must hold that, conditional on control variables, the expected change in pandemic mortality rates would have been the same across parishes with low, medium, and high-income (coal capacity) in the absence of the difference in income (coal capacity). In terms of the difference-in-differences model, this assumption is referred to as common trend assumption. Therefore, it must also hold that there

²⁸Table 4.1 shows that the average mortality rate is lower in parishes with higher income, but there is no significant difference in average mortality rates between parishes by pollution tercile. Thus, one channel of the income effect could be the effect on pre-pandemic health differences, if the model does not capture the pre-pandemic health sufficiently. Likewise, the estimator of the pollution effect would be upward biased, if less healthy individuals sorted into highly polluted parishes and the pre-pandemic health differences are not captured by the model in equation (4.1). The insignificant differences in the average mortality rate, however, do not indicate such a selective migration pattern.

²⁹However, the higher case fatality rates in the second wave could have been due to an increased frequency of secondary bacterial pneumonia (Taubenberger et al., 2019).

is no unobservable factor that influences mortality and correlates with income and coal capacity. Since I control for parish fixed effects, time fixed effects, and time times district fixed effects, these unobservable factors would also need to vary over time at the sub-district level to bias the estimates.

4.5 Results

This section presents the paper's main results on the effect of income and pollution on mortality rates during the 1918 influenza pandemic in Württemberg. In addition to the parish level estimates, the section discusses several robustness checks and presents estimates with data aggregated at the county level.

4.5.1 Parish level estimates

Figure 4.7 shows the differential changes in average mortality rates between middle and low-income parishes (left panel) and high and low-income parishes (right panel) from 1914 to 1925, relative to the baseline year 1917. The results are based on a reduced version of equation (4.1), excluding the indicators for pollution, the control variables in X_i , and county times year fixed effects.³⁰ The vertical bars in Figure 4.7 indicate 95 percent confidence intervals. The pandemic year 1918 is indicated by a vertical dashed line.

The change in the mortality rate 1918 in middle-income parishes relative to low-income parishes is significantly lower by -1.7 deaths per 1,000 population. The same holds for high-income parishes with a point estimate of -1.3. Or in other words, the spike in 1918 mortality was particularly large in poor parishes.

Similarly, Figure 4.8 compares the changes in mortality between parishes in the low tercile of installed coal-fired capacity and parishes in the medium (left panel) and high tercile (right panel) from 1914 to 1925, relative to the baseline year 1917. The estimation is based on a reduced model as in Figure 4.7, but with indicators for medium and high coal capacity (MP_i and HP_i) instead of income indicators. There is no significant difference in the change in mortality rates in parishes with medium coal capacity compared to parishes with low coal capacity. Parishes with high levels of installed coal-fired capacity, on the other hand, have a significantly stronger increase in mortality rates. In these parishes, the mortality rate increases by an additional 1.1 deaths per 1,000 population. The effect is statistically significant at the five percent level.

Thus, Figures 4.7 and 4.8 indicate a lower mortality rate increase for parishes with higher incomes and less pollution between 1917 and 1918. Additionally, the figures show that the

³⁰Formally, this renders equation (4.1) to: $MR_{it} = \beta'_{1t}MI_i \cdot d_t + \beta'_{2t}HI_i \cdot d_t + \gamma'_t\overline{MR}_{i,1910-13} \cdot d_t + d_i + d_t + \varepsilon_{it}$.

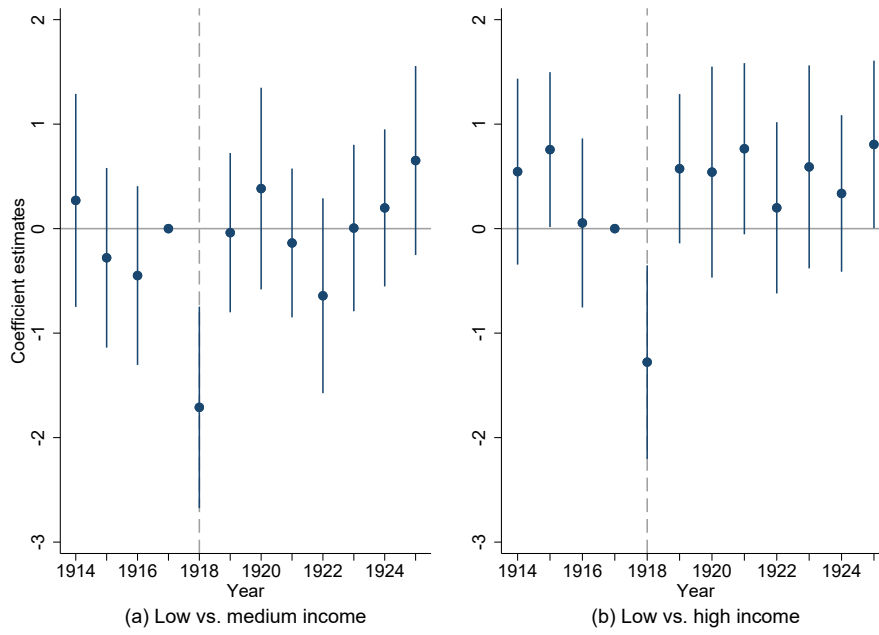


Figure 4.7: Estimated difference in mortality rate changes by income

Notes: The graph depicts differences in mortality rates between parishes with middle and low-income per capita levels (left panel) and the high and low-income per capita levels (right panel), as estimated in an event study regression. Differences are expressed relative to the baseline difference in 1917. Point estimates are marked by a dot. The vertical bands indicate the 95 percent confidence interval of each estimate. The red dashed vertical line indicates the pandemic year 1918. Author's design.

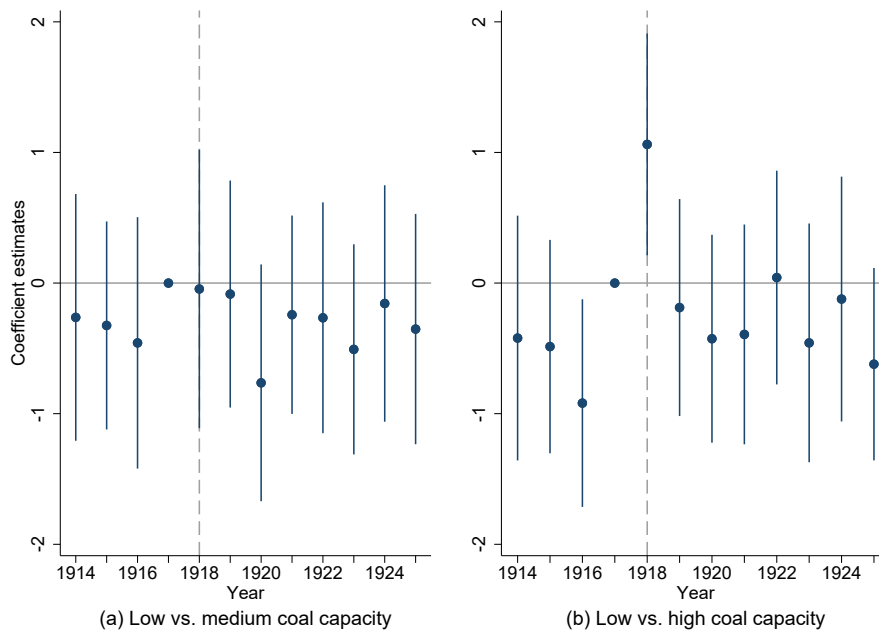


Figure 4.8: Estimated difference in mortality rate changes by coal capacity

Notes: The graph depicts differences in mortality rates between parishes with middle and low pollution levels (left panel) and high and low pollution levels (right panel), as estimated in an event study regression. Differences are expressed relative to the baseline difference in 1917. Point estimates are marked by a dot. The vertical bands indicate the 95 percent confidence interval of each estimate. The red dashed vertical line indicates the pandemic year 1918. Author's design.

estimates for the pandemic year 1918 deviate strongly from all other years. There is no general difference in mortality rate changes that distinguish the different parish groups, other than in 1918. Indeed, only three out of the 44 reported estimates in Figures 4.7 and 4.8 are statistically significant at the five percent level (not counting the 1918 estimates), and all show the opposite sign.³¹ Out of the 12 estimates for the pre-pandemic period, only two are statistically significant. These estimates provide suggestive evidence for the common trend assumption to hold, i.e., there is no systematic difference in mortality rate changes in non-pandemic years.

Table 4.2, Panel A, Columns (1) and (3) show the point estimates and standard errors for the interactions of medium and high-income and coal capacity indicators with the indicator for 1918. The results correspond to the results in Figures 4.7 and 4.8. In low-income parishes, the mortality rate increased from 13.4 in 1917 to 17.3 in 1918, an increase of 3.9 deaths per 1,000 inhabitants or 29 percent. The point estimates in column (1) indicate that the respective increase in medium and high-income parishes was lower by 1.7 and 1.3 deaths per 1,000 population. Both estimates are statistically significant at the one percent level. In column (2), I include the full set of control variables as well as district times year fixed effects. The estimates for the income effect in column (2) are slightly lower but remain statistically and epidemiologically significant.

Table 4.2, column (3) shows the results for coal-fired capacity. In parishes with the highest installed coal capacity within 50 kilometers, the mortality rate increases by an additional 1.1 deaths per 1,000 population. The estimate is statistically significant at the five percent level, with a standard error of 0.425. The point estimate is virtually identical in column (4), where I include the full set of controls and district times year fixed effects. The change in mortality rates is almost identical for parishes with low and medium coal capacity, an average increase of about 2.6 deaths per 1,000 population between 1917 and 1918.

Column (5) includes the measures for income and coal capacity simultaneously, and column (6) adds the full set of control variables. Thus, the estimates in column (6) are based on the full model as specified in equation (4.1).³² Conditional on pre-pandemic socio-economic characteristics and the installed coal capacity, the change in the all-cause mortality is 1.4 less in medium relative to low-income parishes. Likewise, the average change in the mortality rate is lower by one death per 1,000 population for parishes in the high-income tercile compared to the low tercile. On the other hand, parishes with the highest level of installed coal-fired capacity

³¹These estimates could indicate a negative effect of poverty and pollution during the harsh winter of 1916–1917. If the mortality rate in the reference year 1917 is higher in poor and highly polluted parishes this would downward bias the 1918 estimates.

³²Table C.3 in the Appendix shows the full set of results including control variables.

Table 4.2: Baseline results – DiD Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A:</i> Dependent variable: Mortality rate 1914–1925						
Income medium × 1918	-1.710*** (0.482)	-1.378*** (0.466)			-1.638*** (0.494)	-1.379*** (0.473)
Income high × 1918	-1.277*** (0.463)	-0.993* (0.526)			-1.147** (0.494)	-0.969* (0.526)
Coal medium × 1918			-0.045 (0.533)	-0.101 (0.597)	-0.320 (0.528)	-0.202 (0.590)
Coal high × 1918			1.062** (0.425)	1.056* (0.564)	0.715 (0.445)	0.958* (0.569)
Observations	21,156	21,156	21,156	21,156	21,156	21,156
R-squared	0.096	0.100	0.095	0.098	0.098	0.101
<i>Panel B:</i> Dependent variable: Infant mortality rate 1914–1925						
Income medium × 1918	-18.254 (16.102)	-24.947 (17.079)			-19.678 (16.060)	-23.870 (16.930)
Income high × 1918	11.715 (15.283)	-5.478 (19.504)			8.881 (15.122)	-3.902 (19.028)
Coal medium × 1918			2.873 (16.273)	14.450 (17.268)	4.802 (16.398)	13.683 (17.468)
Coal high × 1918			-18.201 (14.607)	-9.434 (21.148)	-15.860 (14.944)	-9.741 (21.563)
Observations	21,097	21,097	21,097	21,097	21,097	21,097
R-squared	0.046	0.050	0.047	0.051	0.048	0.052
FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Year × District FE	No	Yes	No	Yes	No	Yes

Notes: The table shows panel regression estimates of the effect of taxable income 1907 and installed coal-fired power plant capacity within 50 kilometers on the differential change in mortality rates (Panel A) and infant mortality rates (Panel B). Regressions (1) and (2) show the change in medium and high-income parishes between 1917 and 1918 relative to the change in low-income parishes. Regressions (3) and (4) show the change in medium and high coal capacity parishes between 1917 and 1918 relative to the change in low coal capacity parishes. Regressions (5) and (6) include income and coal capacity measures. All regressions include a full set of year and parish fixed effects. Regressions in Panel A include the average mortality rate 1910–1913 interacted with year fixed effects. Regressions in Panel B include the average infant mortality rate 1910–1913 interacted with year fixed effects. Columns (2), (4), and (6) include the full set of pre-treatment control variables X_i each interacted with an indicator variable for the year 1918 and year times county fixed effects. Standard errors clustered at the county level are in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

had, on average, an additional increase by one death per 1,000 population. These effects are large compared to an average increase in the mortality rate between 1917 and 1918 of 2.9.

Table 4.2, Panel B, reports the results with the infant mortality rate, i.e., the number of infants deceased age one or below per 1,000 births, as the dependent variable. In contrast to the previous model, I now control for the average infant mortality rate 1910–1913 ($\overline{IMR}_{i,1910-13}$) of parish i , instead of the average mortality rate 1910–1913 ($\overline{MR}_{i,1910-13}$). All estimates are statistically insignificant. The results illustrate the difficulties in estimating the effect on infant mortality in the given sample. As discussed above, the infant mortality rate shows high variation and multiple shocks that are orthogonal to the influenza pandemic, e.g., heat waves or the response in fertility to WWI. Moreover, the relatively small unit of analysis, parishes with a median population of 649 inhabitants, causes random noise in the dependent variable and thus increases standard errors.

4.5.2 Robustness checks

In the previous estimations, I control for a variety of pre-pandemic factors. However, to address potential concerns that the results are driven by model specifications, characteristics of the sample, or the construction of variables, I perform several robustness checks in this section.

The power plant data also include information on power plants that use a mixture of coal, other fuels, and water power (*gemischter Antrieb*), and internal combustion engines (*Explosionsmotoren*). Thus, focusing on coal-fired power plants underestimates the pollution due to the generation of electric energy. This measurement error might attenuate the estimates of the pollution effect. Table 4.3 shows the results when these additional power plants are included. In Panel A, I measure the installed capacity as the sum of coal-fired capacity and the capacity of power plants that use a combination of coal and other sources of energy generation. The estimated effects are higher and significant at the one and five percent level. The results of the full model in column (4) indicate that parishes in the high-capacity tercile had an additional increase of 1.7 deaths per 1,000 population compared to parishes in the low tercile. Panel B adds the capacity of power plants using internal combustion engines. Again, the results are higher than in the baseline specification and highly significant. These results provide suggestive evidence that the previous results on the effect of pollution on pandemic mortality are a lower bound of the true effect.

The estimates in Table 4.2 show the differential change in mortality rates relative to the baseline year 1917. However, the virus that caused the influenza pandemic might have already spread before 1918 (Johnson, 2001; Taubenberger et al., 2019). Moreover, the mortality rate in

Table 4.3: Robustness checks – Alternative pollution measures

	Mortality rate 1914–1925			
	(1)	(2)	(3)	(4)
<i>Panel A:</i> Capacity measure includes coal and power plants using a mix				
Income medium \times 1918			-1.571*** (0.500)	-1.315*** (0.476)
Income high \times 1918			-1.017** (0.500)	-0.870 (0.536)
Capacity medium \times 1918	0.279 (0.552)	0.487 (0.546)	0.084 (0.553)	0.408 (0.556)
Capacity high \times 1918	1.423*** (0.442)	1.785*** (0.539)	1.150** (0.470)	1.709*** (0.562)
Observations	21,156	21,156	21,156	21,156
R-squared	0.095	0.099	0.098	0.101
<i>Panel B:</i> Capacity measure includes coal, mix, and combustion engines				
Income medium \times 1918			-1.605*** (0.502)	-1.339*** (0.478)
Income high \times 1918			-1.087** (0.476)	-0.922* (0.522)
Capacity medium \times 1918	0.064 (0.520)	0.203 (0.494)	-0.179 (0.494)	0.103 (0.499)
Capacity high \times 1918	1.301*** (0.428)	1.584*** (0.507)	0.999** (0.439)	1.500*** (0.527)
Observations	21,156	21,156	21,156	21,156
R-squared	0.095	0.099	0.098	0.101
FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
Year \times District FE	No	Yes	No	Yes

Notes: The table shows panel regression estimates of the effect of taxable income 1907 and installed power plant capacity within 50 kilometers on the differential change in mortality rates. In Panel A the power plants include coal-fired power plants and power plants that use a combination of coal and other means of power generation. Panel B adds the capacity of power plants that use combustion engines. Regressions (1) and (2) show the change in medium and high coal capacity parishes between 1917 and 1918 relative to the change in low coal capacity parishes. Regressions (3) and (4) include income and coal capacity measures. All regressions include a full set of year fixed effects, parish fixed effects, and the average mortality rate 1910–1913 interacted with year fixed effects. Columns (2) and (4) include the full set of pre-treatment control variables X_i each interacted with an indicator variable for the year 1918 and year times county fixed effects. Standard errors clustered at the county level are in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

1917 was relatively high due to starvation and the harsh winter 1916–1917. If the mortality rate in 1917 is negatively correlated with income and positively correlated with coal-fired capacity, the estimates in Table 4.2 would represent a lower bound of the income and pollution effect on pandemic mortality.

Table C.4 in the Appendix presents results using 1914 as the baseline year instead of 1917. Therefore, the estimates in Table C.4 show the differential change in the mortality rate from 1914 to 1918 in medium and high-income (pollution) parishes relative to the change in low-income (pollution) parishes, conditional on pre-pandemic characteristics. The results of the full model in column (6) indicate that highly polluted parishes faced an additional increase of 1.3 deaths per 1,000 population (s.e. 0.623) relative to medium and low polluted parishes and that the increase in mortality rates was lower in medium and high-income parishes by 1.7 (s.e. 0.530) and 1.6 (s.e. 0.657) deaths per 1,000 population, relative to low-income parishes. In general, all point estimates (not including the medium coal capacity estimates) indicate a stronger effect and higher statistical significance levels than in Table 4.2. Thus, using 1917 as reference year might bias the estimates towards zero.

The results in Table 4.2 hold qualitatively if I include infant deaths (see Appendix Table C.5), include World War I years only to account for any war-related parish specific fixed effects (Table C.6, Panel A), control for the annual number of deaths of military personnel at the parish level (Table C.6, Panel B)³³, or allow the effect of each cross-sectional unit characteristic to vary by year as in Hornbeck (2012) (Table C.7).

As a further robustness check, I replace the main explanatory variables with indicators based on income and coal-fired capacity quintiles (Table C.8). This addresses concerns that the results are driven by the specific division of the sample. The partition in quintiles shows the effect to be statistically insignificant for parishes with medium-high income compared to low-income parishes. However, the general results still indicate a negative estimate for the effect of income and a positive estimate for the effect of pollution on pandemic mortality. I also truncate the sample based on population size in 1910, i.e., I exclude the largest and smallest one (five) percent of parishes from the sample (Table C.9). The truncation reduces the average population size in the sample from 1,354.4 to 972.9 (809.4). The results of these robustness checks are in line with the baseline results. Thus, the effects are not driven by a significantly different health environment in the few urban centers or outliers in small villages' mortality rates. To account

³³I do not control for the number of military personnel deaths in the main specification because this variable could be considered a bad control. The variable includes deaths caused by the influenza pandemic, see Section 4.2, and therefore the conditions soldiers experienced at home, e.g., the exposure to pollution or low-income, could influence the mortality.

for spatial autocorrelation, I use Conley standard errors with cut-off distances of 10, 20, and 50 kilometers (Table C.10). Overall, the results remain statistically significant.

These alternative specifications confirm the positive effect of higher income levels and the negative effect of higher levels of coal-fired capacity on pandemic mortality rates.³⁴ Moreover, the results in Table 4.3 suggest that the pollution effect reported in Table 4.2 provides a lower bound for the actual effect of pollution on the pandemic mortality increase.

4.5.3 County-level estimates

An advantage of the data set is that it allows to exploit heterogeneity at the parish level. However, such detailed data are not always available. Often studies use more aggregated data at the county or even district level. Hence, existing heterogeneity within these larger areas could be averaged out and estimates biased towards zero. This problem is discussed as the modifiable areal unit problem (MAUP) in the literature (Fotheringham and Wong, 1991; Gotway and Young, 2002).

Figures 4.9 and 4.10 show the estimation results based on a reduced model as in Figures 4.7 and 4.8, respectively, but for data aggregated at the county level. The aggregated data set comprises 63 counties. In both figures, the estimated effect of income and coal-fired capacity shows the expected sign, i.e., a negative estimate for the relative change in mortality rates in 1918 compared to 1917 for medium and high-income counties compared to low-income counties and a positive estimate for counties with high compared to low levels of installed coal-fired capacity. However, none of these estimates is statistically significant. The statistical insignificance might just be a result of the loss in statistical power due to the smaller sample size but also due to the aggregation itself. Therefore, the county-level estimates emphasize the importance of complementing existing studies at the aggregate level with analyses using more finely grained data.

4.6 Conclusion

This paper analyzes mortality in the 1918 influenza pandemic in the Kingdom of Württemberg and the effect of income and pollution on pandemic severity. The “Spanish Flu” reached the South-West German state during the hardships of the First World War. The suffering due to starvation and the war, causing more than 72.000 deaths military personnel in Württemberg, could explain that the pandemic received little public attention and did not leave a lasting impression in the collective memory of the population. To put this in perspective, the 72,000

³⁴The robustness checks have been also performed with the infant mortality rate as dependent variable. The results remain statistically insignificant and are not reported here. The results can be received upon request.

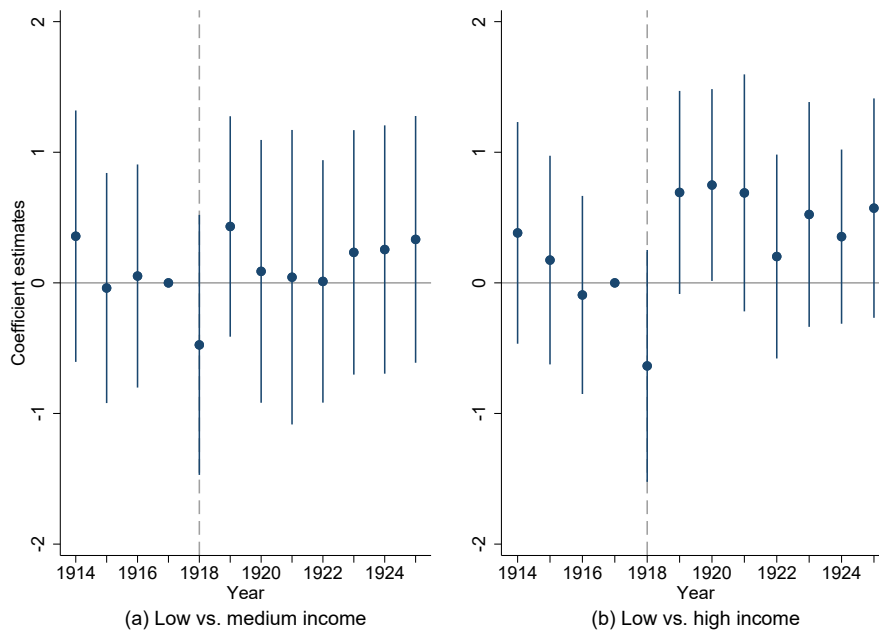


Figure 4.9: County event study results – Income

Notes: The graph depicts differences in mortality rates between counties in the middle and low-income per capita tercile (left panel) and the high and low-income per capita tercile (right panel), as estimated in an event study regression. Differences are expressed relative to the baseline difference in 1917. Point estimates are marked by a dot. The vertical bands indicate the 95 percent confidence interval of each estimate. The red dashed vertical line indicates the pandemic year 1918. Author’s design.

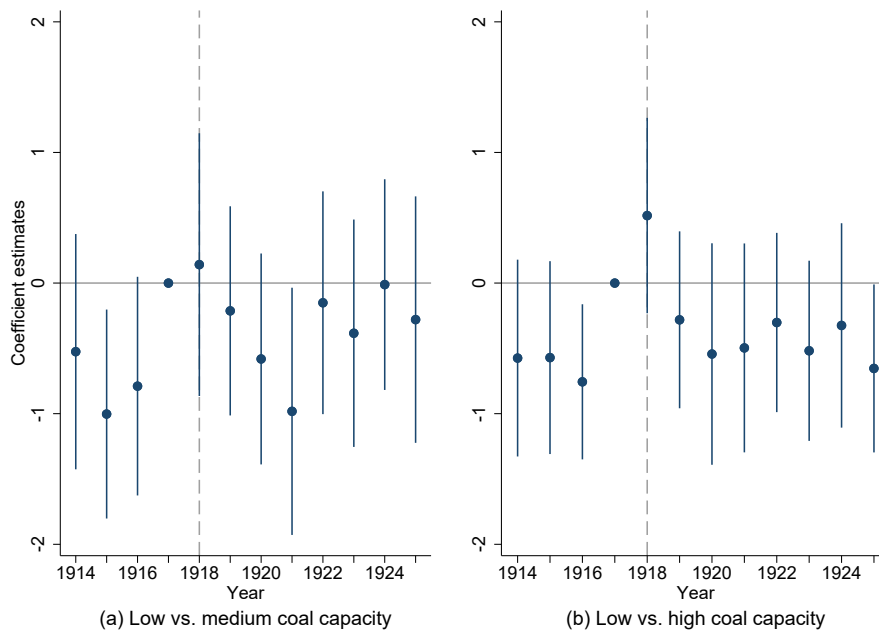


Figure 4.10: County event study results – Pollution

Notes: The graph depicts differences in mortality rates between counties in the middle and low pollution tercile (left panel) and the high and low pollution tercile (right panel), as estimated in an event study regression. Differences are expressed relative to the baseline difference in 1917. Point estimates are marked by a dot. The vertical bands indicate the 95 percent confidence interval of each estimate. The red dashed vertical line indicates the pandemic year 1918. Author’s design.

deaths military personnel account for about 30 deaths per 1,000 inhabitants in 1910, whereas the estimates of the pandemic mortality rate in Württemberg presented in the paper range between 2.3 and 4.1 excess deaths per 1,000 persons (see Section 4.2).

However, the 1918 influenza pandemic led to a significant increase in all-cause mortality rates in Württemberg of 23 percent relative to 1917. The paper shows that this increase was larger in poor and highly polluted parishes. Parishes with high levels of coal-fired power plant capacity within 50 kilometers faced an additional increase of 1.0 death per 1,000 population relative to medium and low polluted parishes. Moreover, the relative increase in mortality rates was lower in medium and high-income parishes by 1.4 and 1.0 deaths per 1,000 population, relative to low-income parishes. These results are robust to changes in model specifications, variable definitions, and the sample. However, the focus on coal-fired power plant capacity might render the pollution effects at the lower bound because it is an imperfect measure of the actual local pollution levels.

The data show that the mortality burden of the 1918 influenza pandemic was lower in Württemberg compared to other German states, even though Württemberg was relatively poor by German standards. The newly calculated estimates for the German excess mortality rate are between 5.4 and 5.9 excess deaths per 1,000 population. One reason might be the relative backwardness of Württemberg's economy, resulting in low usage of coal compared to other German states and foreign countries. For instance, in the sample of US cities in Clay et al. (2018), the average installed coal-fired capacity within 30 miles (48.3 kilometers) is 182.8 MW, while it is only 24.9 MW (within 50 kilometers) in Württemberg. In Clay et al. (2018), medium coal capacity cities have on average an installed capacity of 50.7 MW, while in Württemberg, only parishes in the highest tercile reach this level. In line with this argument, the effect for medium coal capacity parishes is insignificant in Württemberg, whereas Clay et al. (2018) find an effect also for medium coal capacity cities. Thus, the lack of coal that contributed to Württemberg's economic backwardness might have been beneficial in reducing the pandemic's death toll.

Furthermore, Württemberg had a relatively high share of agricultural employment at the beginning of WWI. Possibly, this made the Kingdom more resistant to the Allied blockade and resulting food shortages.

Appendix C

Poverty, Pollution, and Mortality: The 1918 Influenza Pandemic in a Developing German Economy

C.1 Württemberg in the German Empire

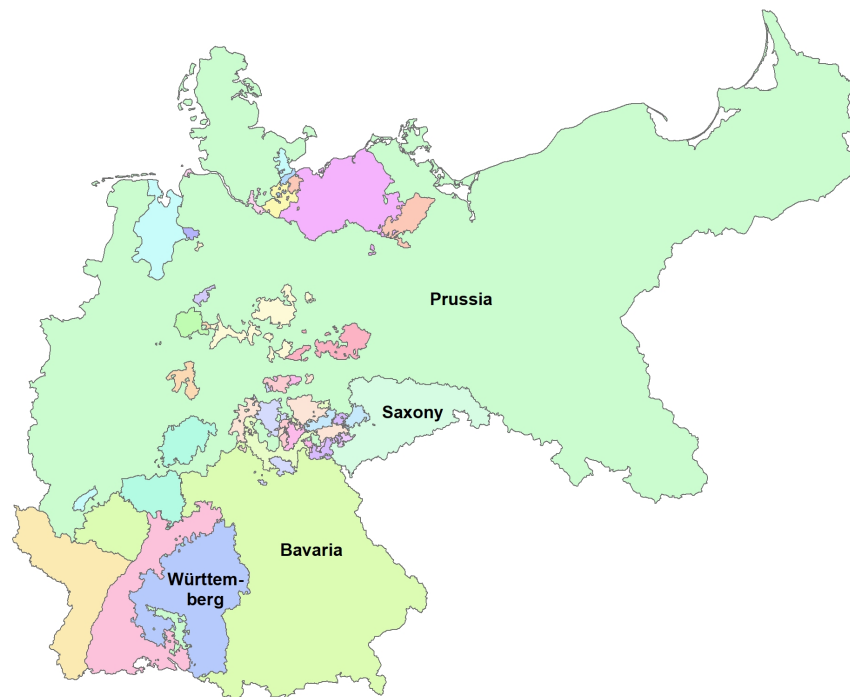


Figure C.1: The German Empire in 1871

Notes: The figure shows the German Empire in its 1871 borders. Labels mark the four Kingdoms that were part of the German Empire (namely, the Kingdoms of Bavaria, Prussia, Saxony and Württemberg).

Source: Max Planck Institute for Demographic Research (MPIDR) and Chair for Geodesy and Geoinformatics, University of Rostock (CGG) (2011). Author's design.

C.2 Energy supply during WWI

If the First World War caused energy production and coal usage to drop dramatically, it could bias the pollution effect estimates because the measure is based on installed capacity. However, already before WWI electric energy was widely used in manufacturing industries. Hence, it was important for the war efforts of the German Empire to secure electric energy supply.

In Section 4.2.1, the paper refers to coal usage statistics for Württemberg. It is shown that in 1918 the average annual coal consumption per capita was 1,013 kilograms and in 1913 it was 1,000 kilograms per capita (Statistisches Landesamt, 1923). Thus, there is no significant change in coal usage in Württemberg. The same holds for the German Empire. In 1913 the annual average coal usage per capita was 3,870 kilograms of coal, while in 1917 it was 3,732 kilograms (Statistisches Reichsamt, 1925).

Often, the total output of coal in the German Empire is used to illustrate a decline in energy supply during WWI. Table C.1 shows the total coal output of the German Empire from 1913 to 1920. The total black coal output decreases from 190,109 thousand tons in 1913 to 158,254 thousand tons in 1918 (Statistisches Reichsamt, 1921a). This decline seemingly contradicts the previous point of a relatively stable coal consumption per capita during WWI. However, the total output does not account for boundary changes of the German Empire. Moreover, the decline in black coal output could be largely offset by other measures. The output of lignite increased in this period from 87,233 to 100,599 thousand tons. In addition, black coal exports halved from 34,598 thousand tons in 1913 to 15,230 thousand tons in 1917 and also lignite exports were reduced (Statistisches Reichsamt, 1925). These measures could also offset the decline in coal imports due to the sea blockade.

Table C.1: Coal output 1913–1920 in thousand tons

Year	Black coal				Lignite
	Empire	Ruhr area	Upper Silesia	Saar Basin	Empire
1913	190,109	114,487	43,435	13,217	87,233
1914	161,385	98,285	36,996	10,032	83,694
1915	146,868	86,778	38,107	8,384	87,694
1916	159,170	94,563	41,723	8,903	94,180
1917	167,747	99,365	42,752	10,265	95,543
1918	158,254 [†]	96,016	39,648	9,989	100,599
1919	116,681 [†]	67,926	25,697	8,990	93,843
1920	140,757 [†]	84,986	31,686	9,410	111,634

Notes: The table shows coal output in the German Empire between 1913 and 1920 in thousand metric tons. [†] Excluding Lorraine. *Source:* Statistisches Reichsamt (1921a).

Another measure to illustrate energy consumption is the actual energy production of power plants in Württemberg. In 1917 the 31 largest power plants in Württemberg, accounting for five-sixths of the total amount of generated power, produced 176,515.4 MWh of energy and reached an average capacity factor of 25.2 percent (Ott, 1971; Statistisches Landesamt, 1923, 1928).¹ Thus, the utilization of installed power during WWI was even higher than in 1926 when the largest power plants in Württemberg had a capacity factor of 23.0 percent (Statistisches Landesamt, 1928).

These statistics show that the energy production was relatively stable during WWI. Therefore, it is likely that also pollution from coal-fired power plants was largely unaffected by the war.

C.3 Excess mortality estimations

In this section, I present the definitions and calculations of the excess mortality statistics presented in Section 4.2.3.

Similar to Ansart et al. (2009), I use the monthly all-cause mortality statistics for Württemberg (Statistisches Landesamt, 1922). However, their data set covers the period 1906–1922 for Germany, while my data only cover the years 1914–1919. Yet, the shorter time period might be advantageous because it could prevent the downward bias in baseline mortality rates due to the inclusion of pre-war years.² For the calculations I normalize monthly all-cause deaths by population (annually average population from Statistisches Landesamt (1928)) to get monthly mortality rates by 1,000 population. The monthly baseline mortality rate B_t is based on a linear model

$$B_t = \alpha_0 + \alpha_t t + \gamma_1 \cos(2\pi t/n) + \delta_1 \sin(2\pi t/n) + \gamma_2 \cos(4\pi t/n) + \delta_2 \sin(4\pi t/n) + \epsilon_t, \quad (\text{C.1})$$

with $n = 12$ and $t = \{1, 2, \dots, 12\}$. The model parameters are estimated using the monthly mortality rate in 1914–1917, excluding in each August to July period the trimester with the highest mortality rates. Based on the estimated model parameters, the baseline mortality is

¹The capacity factor is the ratio of energy output over one year to the maximum possible energy output (installed capacity times 365 days times 24 hours). The energy production includes also hydroelectric power plants and therefore might overestimate the capacity factor of coal-fired power plants in 1917. However, the power plants in Ellwangen and Heilbronn, for instance, had a capacity factor of 20.8 and 23.9 percent in 1917, while 99 and 100 percent of the installed capacity was based on steam engines, respectively. Moreover, the purely hydroelectric power plant in Ludwigsburg achieved a comparable capacity factor of 26.3 percent in 1917 (Statistisches Landesamt, 1923).

²Indeed, Ansart et al. (2009) identify the excess mortality period for Germany to start in March 1918. This could be due to an under estimation of baseline mortality rates during the First World War. Thus, if we assume that the excess mortality in March 1918 was not caused by the influenza virus, the cumulative excess mortality rate attributed to the influenza pandemic is likely upward biased.

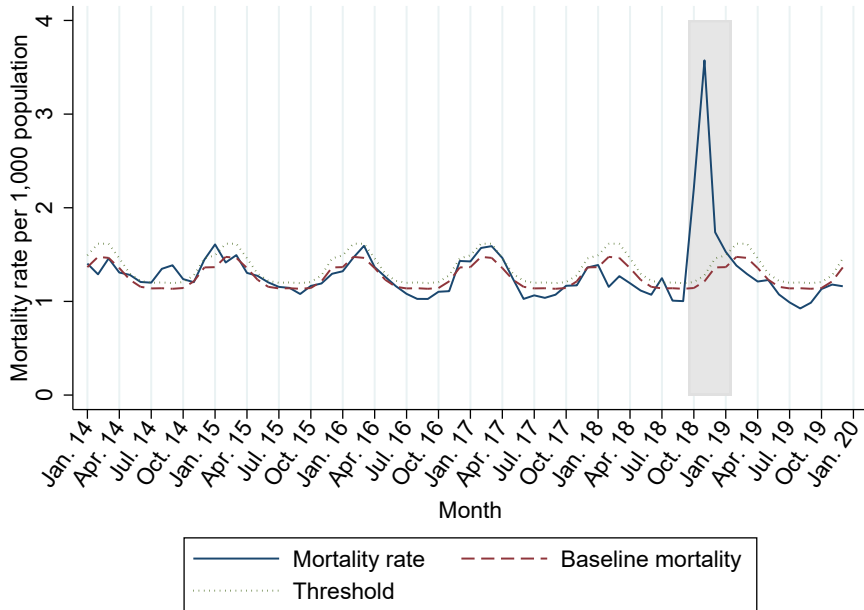


Figure C.2: Monthly excess mortality

Notes: The graph depicts monthly all-cause mortality rate in the Kingdom of Württemberg (solid line), the monthly baseline mortality rate B_t (dashed line), and the threshold, $B_t + 1.96 \cdot \text{sd}(\epsilon_t)$ (dotted line) for the years 1914 to 1919. The mortality rate does not account for the number of death of military personnel and stillbirths. *Source:* Statistisches Landesamt (1922). Author's design.

predicted for 1918 and 1919. An excess mortality period is identified if the mortality rate is above the threshold of $B_t + 1.96\text{sd}(\epsilon_t)$ for at least two consecutive months.

Figure C.2 shows the monthly all-cause mortality rate per 1,000 population (solid line), the estimated baseline mortality rate B_t (dashed line) and the threshold value (dotted line). The excess mortality period in 1918–1919 is identified for the months October 1918 to January 1919, highlighted by gray shading in Figure C.2. The cumulative excess mortality rate is 3.8 deaths per 1,000 population.

Murray et al. (2006) calculate the excess mortality during the “Spanish Flu” as the sum over the deviations in mortality in 1918–1920 from the average in 1915–1917 and 1921–1923. Formally, the excess mortality EM is

$$EM_{1918-20} = \sum_{1918}^{1920} \left(M_t - \frac{M_{1915} + M_{1916} + M_{1917} + M_{1921} + M_{1922} + M_{1923}}{6} \right), \quad (\text{C.2})$$

with M_t defined as the all-cause mortality in year t . To calculate the excess mortality rate, the resulting value of $EM_{1918-20}$ is divided by 1,000 population. Thus, the excess mortality rate for Württemberg is 2.3 (Statistisches Landesamt, 1928).

Barro et al. (2020) apply the method from Murray et al. (2006), but use all influenza-related deaths if available. For the calculation I include all deaths from influenza (*Influenza*), pneumonia (*Lungenentzündung*), other diseases of the respiratory organs (*Krankheiten der Atmungsorgane*), tuberculosis (*Tuberkulose der Lungen (Lungenschwindsucht)*), and whooping cough (*Keuchhusten*). A similar list of diseases is underlying the excess mortality calculations for England and Wales in Johnson and Mueller (2002). The cause-specific mortality data for 1915–1923 are from archival material (Staatsarchiv Ludwigsburg E 258 VII Bü 662). The excess mortality rate per 1,000 population for the years 1918–1920 is 3.9.

Table C.2 shows excess mortality rate estimates for Germany, its states, and Prussian provinces. I digitized the case-specific mortality and population data from various volumes of the *Statistisches Jahrbuch für das Deutsche Reich* (Statistisches Reichsamt, 1920, 1921b, 1922, 1924, 1925). The calculations generally follow the approach of Murray et al. (2006) for all-cause mortality (Columns (1) and (2)) and influenza-related mortality (Columns (3) and (4)). However, in Columns (2) and (4) the excess mortality rate is defined as the aggregate deviation from the average in 1921–23 because for 1915–17 the sources do not report mortality statistics for Prussian provinces.

Moreover, I calculate the aggregate deviation of the mortality rates in 1918–20 from the average mortality rate in non-pandemic years, while Murray et al. (2006) use the number of deaths. This alternative specifications allows to take changes in the total number of deceased due to boundary changes into account. After WWI, Germany lost considerable parts of its territory. The boundary changes affected especially the Prussian provinces of Silesia and Posen-West Prussia. In addition, I correct total population for boundary changes within Germany, namely the transition of Pymont from Waldeck to Hanover in 1921, and the incorporation of municipalities from Brandenburg into Berlin in 1920. Neglecting these boundary changes would severely bias the excess mortality rate estimates upwards in case of territorial losses.

The estimated excess mortality rates for Germany are 5.4 in Column (1) and 5.9 in Column (3) using all-cause mortality and influenza-related mortality respectively. Thus, the estimates for Germany provide further evidence that the true excess mortality rate of the influenza pandemic is within the range of 3.8 to 7.8, which is the range of previous estimates. For Württemberg the respective estimates are slightly higher than above with 2.5 and 4.0 excess deaths per 1,000 population. The difference is due to slightly higher mortality statistics for the years 1919 and 1920 reported in Statistisches Reichsamt (1924). Nevertheless, Table C.2 shows that Württemberg had one of the lowest excess mortality rates in Germany.

The excess mortality rates in Columns (2) and (4) are on average higher than in Columns (1) and (3) but highly correlated. The on average higher excess mortality is driven by the exclusion of the higher average mortality rates observed during WWI. Thus, these estimates are not directly comparable to previous results.

It is beyond the scope of this paper to evaluate the various excess mortality rate estimates for Germany and its regions. However, the estimates presented here might inform future research on the mortality burden during the 1918–1919 influenza pandemic in Germany.

Table C.2: Excess mortality rates in Germany

Region	Excess mortality rate 1918–20, deviation from:			
	1915–17		1915–17	
	1921–23	1921–23	1921–23	1921–23
	All-cause		Influenza related	
	(1)	(2)	(3)	(4)
Germany	5.4	6.1	5.9	6.0
Prussia	6.6	7.8	6.8	7.0
<i>East Prussia</i>		6.0		5.1
<i>Berlin</i>		6.7		6.2
<i>Brandenburg</i>		4.1		5.2
<i>Pomerania</i>		6.0		6.0
<i>Silesia</i>		6.2		8.1
<i>Saxony</i>		6.6		6.3
<i>Schleswig-Holstein</i>		5.5		4.4
<i>Hanover</i>		6.7		5.8
<i>Westphalia</i>		8.1		8.3
<i>Hesse-Nassau</i>		8.8		7.2
<i>Rhineland</i>		10.1		8.1
<i>Hohenzollern</i>		3.5		3.8
<i>Posen-West Prussia</i>		8.2		6.0
Bavaria	3.1	3.8	4.2	4.8
Württemberg	2.5	3.7	4.0	4.3
Saxony	4.2	6.1	4.7	5.1
Baden	4.2	5.2	4.8	5.4
Hesse	5.3	7.0	5.2	5.9
Thuringia	7.1	8.2	5.7	5.8
Oldenburg	5.6	6.9	6.2	6.6
Brunswick	8.0	9.4	7.0	7.8
Anhalt	7.0	7.9	6.5	6.6
Lippe	4.0	6.1	5.9	6.6
Waldeck	5.8	5.8	7.3	7.3
Schaumburg-Lippe	4.1	3.6	5.4	6.0
Hamburg	4.3	4.4	5.0	4.8
Bremen	7.6	8.1	7.0	7.3
Lübeck	2.6	3.4	4.6	5.2

Notes: The table shows excess mortality rate estimates for Germany and its states. Columns (1) and (2) report all-cause excess mortality rates per 1,000 population. Columns (3) and (4) report influenza related excess mortality rates per 1,000 population, including all deaths from influenza (*Influenza*), pneumonia (*Lungenentzündung*), other diseases of the respiratory organs (*Krankheiten der Atmungsorgane*), tuberculosis (*Tuberkulose der Lungen (Lungenschwindsucht)*), and whooping cough (*Keuchhusten*). Estimates in Columns (1) and (3) are based on equation (C.2). Estimates in Columns (2) and (4) are based on equation (C.2) but using only the deviation from average mortality 1921–23.

C.4 Figures

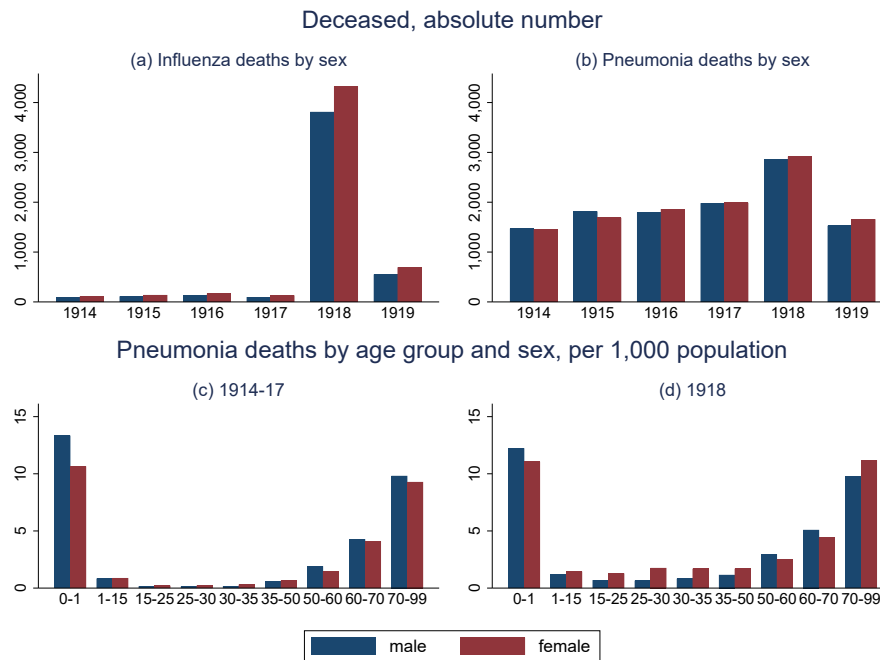


Figure C.3: Influenza and pneumonia mortality statistics for Württemberg

Notes: Figures C.3 (a) and (b) show annual influenza and pneumonia deaths by sex for the years 1914–1919, including the number of deceased among military personnel. Figure C.3 (c) depicts the annual pneumonia mortality rate by age group averaged over the years 1914–1917 and Figure C.3 (d) shows the pneumonia mortality rate by age group in 1918. *Source:* Statistisches Landesamt (1922). Author's design.

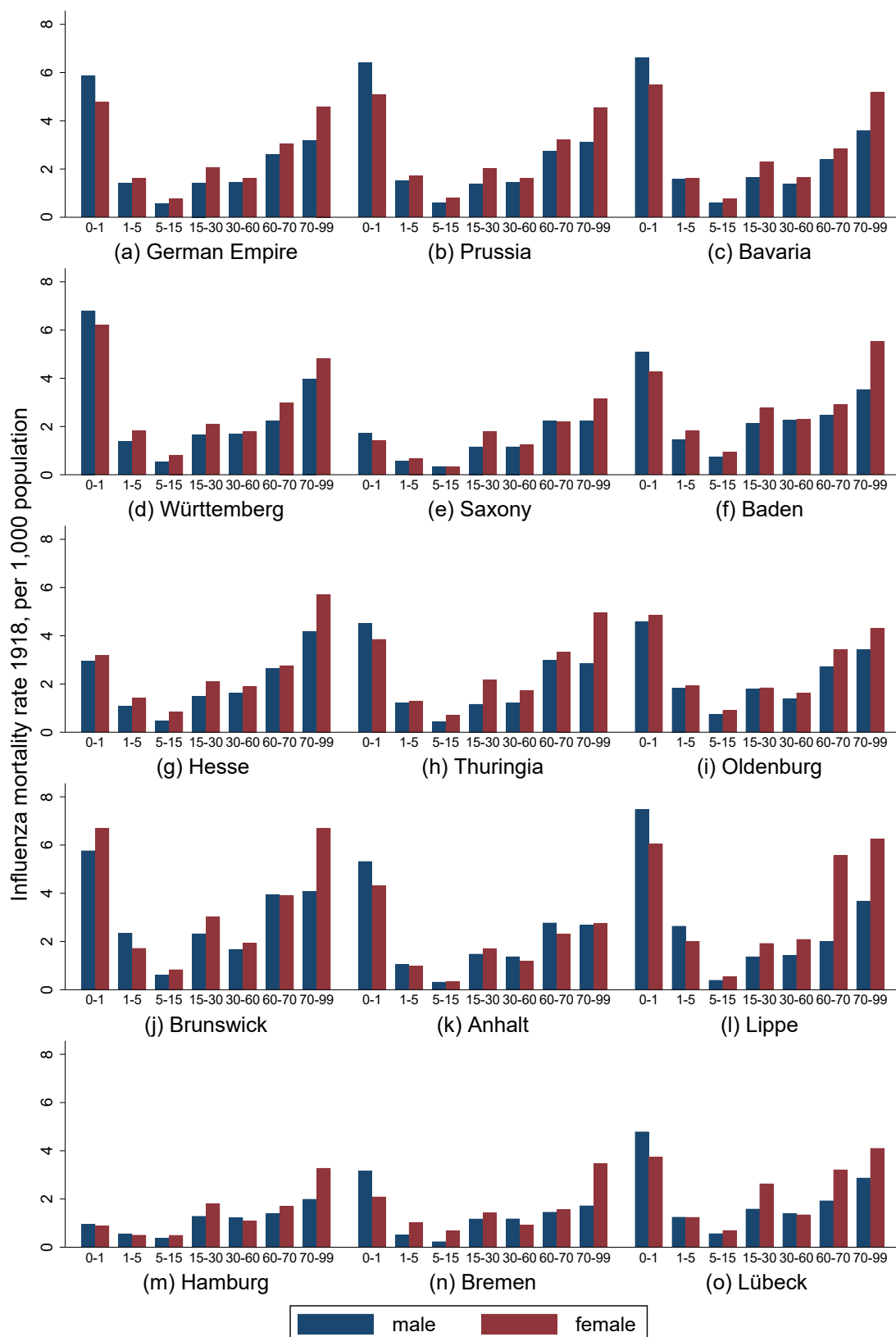


Figure C.4: Influenza mortality rates 1918 for the German Empire and its states, by age and sex, per 1,000 population 1910

Notes: Figures C.4 (a)–(o) show the influenza mortality rate by age group and sex in 1918, including the number of influenza deaths among military personnel. *Sources:* Bogusat (1923) and Kaiserliches Statistisches Amt (1915). Author’s design.

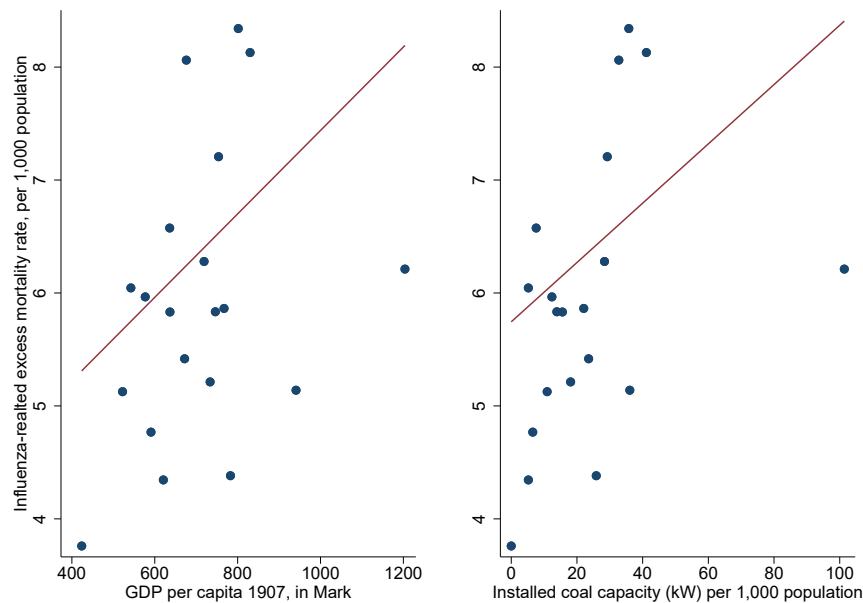


Figure C.5: Association of income and pollution with influenza-related excess mortality 1918–20 for German states and Prussian provinces, including Berlin

Notes: The graph shows the association of income (left panel) and pollution (right panel) with influenza-related excess mortality 1918–20 for German states and Prussian provinces, including Berlin. The influenza related mortality rate per 1,000 population, includes all deaths from influenza, pneumonia, other diseases of the respiratory organs, tuberculosis, and whooping cough. The excess mortality rate is the sum of deviations in 1918–20 from the average in 1921–23. The income level is measured by GDP per capita in 1907. Pollution is calculated as the installed coal-fired capacity 1913 in kW per 1,000 population. *Sources:* Herzig et al. (1986), Frank (1993), and Statistisches Reichsamt (1920, 1921b, 1922, 1924, 1925), see Appendix C.3 for further details. Author's design.



Figure C.6: Excess mortality rate per 1,000 population 1918

Notes: The map shows the excess mortality rate per 1,000 population in 1918 for all 1,763 parishes in the sample. Sources: Statistisches Landesamt Baden-Württemberg (2008), Staatsarchiv Ludwigsburg E 258 VII Bü 120. Author's design.

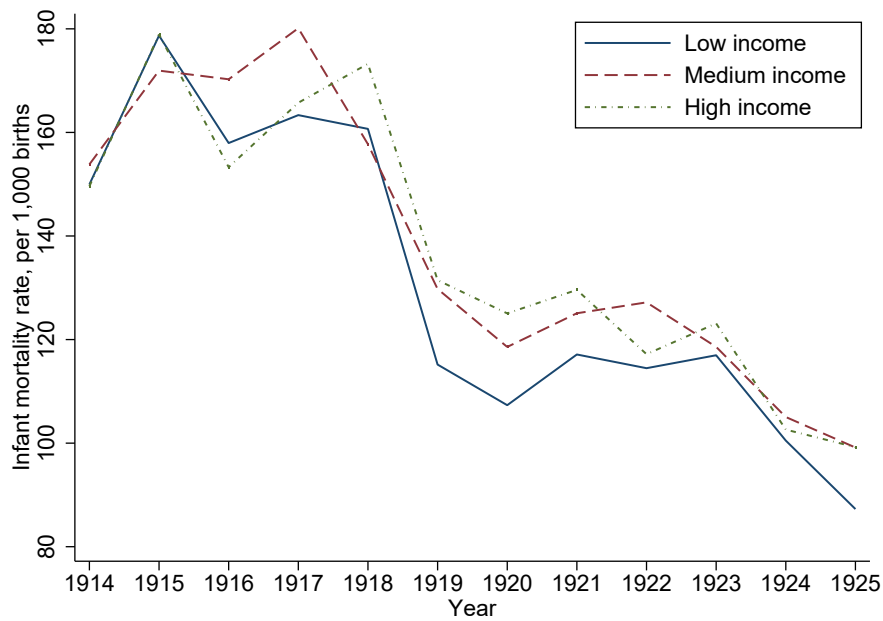


Figure C.7: Infant mortality rate 1914–1925 by income tercile

Notes: The figure shows the annual average infant mortality rate per 1,000 births in parishes for the years 1914 to 1925 by terciles of income 1907. Sources: Königliches Statistisches Landesamt (1910), Statistisches Landesamt Baden-Württemberg (2008), Staatsarchiv Ludwigsburg E 258 VII Bü 120 and 122. Author's design.

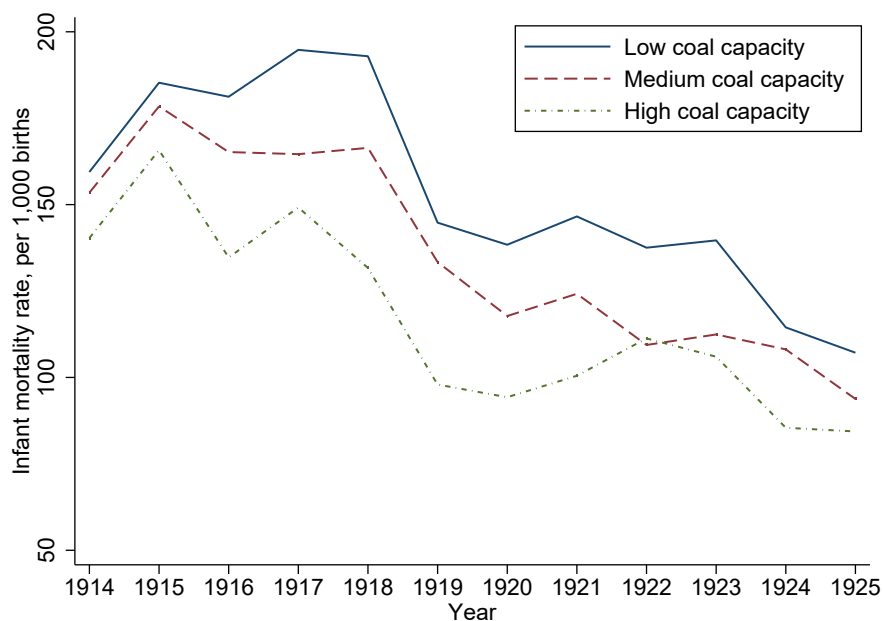


Figure C.8: Infant mortality rate 1914–1925 by coal capacity tercile

Notes: The figure shows the annual average infant mortality rate per 1,000 births in parishes for the years 1914 to 1925 by terciles of installed coal-fired power plant capacity within 50 kilometers. Sources: Ott et al. (1981), Statistisches Landesamt Baden-Württemberg (2008), Staatsarchiv Ludwigsburg E 258 VII Bü 120 and 122. Author's design.

C.5 Tables

Table C.3: Baseline results – DiD Estimates – Full

... × 1918	Mortality rate 1914–1925					
	(1)	(2)	(3)	(4)	(5)	(6)
Income medium	-1.710*** (0.482)	-1.378*** (0.466)			-1.638*** (0.494)	-1.379*** (0.473)
Income high	-1.277*** (0.463)	-0.993* (0.526)			-1.147** (0.494)	-0.969* (0.526)
Coal medium			-0.045 (0.533)	-0.101 (0.597)	-0.320 (0.528)	-0.202 (0.590)
Coal high			1.062** (0.425)	1.056* (0.564)	0.715 (0.445)	0.958* (0.569)
Pop. 1910, log		0.127 (0.254)		0.093 (0.253)		0.154 (0.247)
Pop. density 1910		0.432 (0.384)		0.473 (0.384)		0.271 (0.388)
Ind. empl. 1905, share		-0.009 (0.028)		-0.033 (0.026)		-0.013 (0.027)
Average firm size 1895		0.005 (0.108)		-0.006 (0.103)		-0.003 (0.104)
Hydro capacity, MW		0.024 (0.016)		0.020 (0.016)		0.017 (0.016)
Birth non local 1900, share		3.745** (1.535)		3.548** (1.533)		4.224*** (1.508)
Railway station 1910, dummy		-0.981*** (0.347)		-1.025*** (0.350)		-0.952*** (0.348)
Road access 1848, dummy		-0.228 (0.323)		-0.259 (0.332)		-0.239 (0.321)
River access, dummy		-0.633 (0.489)		-0.803* (0.466)		-0.564 (0.497)
Dist. WWI base, km		-0.017 (0.057)		0.004 (0.056)		-0.004 (0.057)
Observations	21,156	21,156	21,156	21,156	21,156	21,156
R-squared	0.096	0.100	0.095	0.098	0.098	0.101
FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × District FE	No	Yes	No	Yes	No	Yes

Notes: The table shows panel regression estimates of the effect of taxable income 1907 and installed coal-fired power plant capacity within 50 kilometers on the differential change in mortality rates. Regressions (1) and (2) show the change in medium and high-income parishes between 1917 and 1918 relative to the change in low-income parishes. Regressions (3) and (4) show the change in medium and high coal capacity parishes between 1917 and 1918 relative to the change in low coal capacity parishes. Regressions (5) and (6) include income and coal capacity measures. All regressions include a full set of year, parish fixed effects, and the average mortality rate 1910–1913 interacted with year fixed effects. Columns (2), (4), and (6) include the full set of pre-treatment control variables X_i each interacted with an indicator variable for the year 1918 and year times county fixed effects. Standard errors clustered at the county level are in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

Table C.4: Robustness – Baseline year 1914

	Mortality rate 1914–1925					
	(1)	(2)	(3)	(4)	(5)	(6)
Income medium \times 1918	-1.980*** (0.476)	-1.705*** (0.524)			-1.862*** (0.487)	-1.683*** (0.530)
Income high \times 1918	-1.823*** (0.557)	-1.669** (0.659)			-1.605*** (0.555)	-1.603** (0.657)
Coal medium \times 1918			0.218 (0.662)	0.239 (0.630)	-0.165 (0.640)	0.057 (0.622)
Coal high \times 1918			1.483** (0.564)	1.487** (0.628)	1.003* (0.555)	1.298** (0.623)
Observations	21,156	21,156	21,156	21,156	21,156	21,156
R-squared	0.096	0.100	0.095	0.098	0.098	0.101
FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Year \times District FE	No	Yes	No	Yes	No	Yes

Notes: The table shows panel regression estimates of the effect of taxable income 1907 and installed coal-fired power plant capacity within 50 kilometers on the differential change in mortality rates. Regressions (1) and (2) show the change in medium and high-income parishes between 1914 and 1918 relative to the change in low-income parishes. Regressions (3) and (4) show the change in medium and high coal capacity parishes between 1914 and 1918 relative to the change in low coal capacity parishes. Regressions (5) and (6) include income and coal capacity measures. All regressions include a full set of year fixed effects, parish fixed effects, and the average mortality rate 1910–1913 interacted with year fixed effects. Columns (2), (4), and (6) include the full set of pre-treatment control variables X_i each interacted with an indicator variable for the year 1918 and year times county fixed effects. Standard errors clustered at the county level are in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

Table C.5: Robustness checks – Include infant mortality

	All-age mortality rate 1914–1925					
	(1)	(2)	(3)	(4)	(5)	(6)
Income medium \times 1918	-1.739*** (0.576)	-1.555*** (0.555)			-1.702*** (0.578)	-1.545*** (0.561)
Income high \times 1918	-1.053** (0.518)	-1.141** (0.560)			-0.987* (0.530)	-1.095* (0.565)
Coal medium \times 1918			-0.125 (0.548)	0.062 (0.594)	-0.363 (0.541)	-0.048 (0.590)
Coal high \times 1918			0.769* (0.460)	1.013* (0.601)	0.467 (0.470)	0.905 (0.603)
Observations	21,156	21,156	21,156	21,156	21,156	21,156
R-squared	0.156	0.159	0.154	0.158	0.157	0.160
FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Year \times District FE	No	Yes	No	Yes	No	Yes

Notes: The table shows panel regression estimates of the effect of taxable income 1907 and installed coal-fired power plant capacity within 50 kilometers on the differential change in all-age mortality rates, i.e., including infant mortality. Regressions (1) and (2) show the change in medium and high-income parishes between 1917 and 1918 relative to the change in low-income parishes. Regressions (3) and (4) show the change in medium and high coal capacity parishes between 1917 and 1918 relative to the change in low coal capacity parishes. Regressions (5) and (6) include income and coal capacity measures. All regressions include a full set of year fixed effects, parish fixed effects, and the average mortality rate 1910–1913 interacted with year fixed effects. Columns (2), (4), and (6) include the full set of pre-treatment control variables X_i each interacted with an indicator variable for the year 1918 and year times county fixed effects. Standard errors clustered at the county level are in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

Table C.6: Robustness checks – Control for WWI

	Mortality rate					
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A: WWI years only</i>						
Income medium × 1918	-1.710*** (0.482)	-1.368*** (0.472)			-1.638*** (0.493)	-1.370*** (0.478)
Income high × 1918	-1.277*** (0.463)	-1.119** (0.557)			-1.147** (0.494)	-1.098* (0.557)
Coal medium × 1918			-0.045 (0.533)	-0.034 (0.599)	-0.320 (0.528)	-0.148 (0.594)
Coal high × 1918			1.062** (0.425)	1.120* (0.566)	0.715 (0.445)	1.004* (0.573)
Observations	8,815	8,815	8,815	8,815	8,815	8,815
R-squared	0.094	0.100	0.092	0.099	0.096	0.101
<i>Panel B: Control for deaths of military personnel</i>						
Income medium × 1918	-1.712*** (0.480)	-1.382*** (0.464)			-1.638*** (0.491)	-1.384*** (0.470)
Income high × 1918	-1.272*** (0.460)	-0.991* (0.523)			-1.139** (0.490)	-0.967* (0.522)
Coal medium × 1918			-0.058 (0.529)	-0.119 (0.592)	-0.331 (0.524)	-0.219 (0.584)
Coal high × 1918			1.074** (0.423)	1.059* (0.561)	0.728 (0.443)	0.960* (0.566)
Observations	21,156	21,156	21,156	21,156	21,156	21,156
R-squared	0.097	0.100	0.095	0.099	0.098	0.101
FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Year × District FE	No	Yes	No	Yes	No	Yes

Notes: The table shows panel regression estimates of the effect of taxable income 1907 and installed coal-fired power plant capacity within 50 kilometers on the differential change in mortality rates. In Panel A the sample is reduced to include WWI years 1914–1918 only. Panel B includes the annual number of deaths of military personnel at the parish level as control variable. Regressions (1) and (2) show the change in medium and high-income parishes between 1917 and 1918 relative to the change in low-income parishes. Regressions (3) and (4) show the change in medium and high coal capacity parishes between 1917 and 1918 relative to the change in low coal capacity parishes. Regressions (5) and (6) include income and coal capacity measures. All regressions include a full set of year fixed effects, parish fixed effects, and the average mortality rate 1910–1913 interacted with year fixed effects. Columns (2), (4), and (6) include the full set of pre-treatment control variables X_i each interacted with an indicator variable for the year 1918 and year times county fixed effects. Standard errors clustered at the county level are in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

Table C.7: Robustness checks – Controls interacted with year FEs

	Mortality rate 1914–1925		
	(1)	(2)	(3)
Income medium \times 1918	-1.292*** (0.480)		-1.296*** (0.483)
Income high \times 1918	-0.761 (0.574)		-0.739 (0.572)
Coal medium \times 1918		-0.115 (0.601)	-0.214 (0.598)
Coal high \times 1918		1.060* (0.586)	0.970 (0.591)
Observations	21,156	21,156	21,156
R-squared	0.101	0.099	0.102
FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Year \times District FE	Yes	Yes	Yes

Notes: The table shows panel regression estimates of the effect of taxable income 1907 and installed coal-fired power plant capacity within 50 kilometers on the differential change in mortality rates. Regression (1) shows the change in medium and high-income parishes between 1917 and 1918 relative to the change in low-income parishes. Regression (2) shows the change in medium and high coal capacity parishes between 1917 and 1918 relative to the change in low coal capacity parishes. Regression (3) includes income and coal capacity measures. All regressions include a full set of year fixed effects, parish fixed effects, the average mortality rate 1910–1913 interacted with year fixed effects, the full set of pre-treatment control variables X_i each interacted with year fixed effects, and year times county fixed effects. Standard errors clustered at the county level are in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

Table C.8: Robustness checks – Quintiles

	Mortality rate 1914–1925					
	(1)	(2)	(3)	(4)	(5)	(6)
Income medium-low \times 1918	-1.077*	-0.888			-0.967	-0.836
	(0.638)	(0.617)			(0.650)	(0.632)
Income medium \times 1918	-2.316***	-1.968***			-2.164***	-1.933**
	(0.730)	(0.738)			(0.775)	(0.774)
Income medium-high \times 1918	-1.234*	-0.816			-0.936	-0.712
	(0.629)	(0.699)			(0.669)	(0.715)
Income high \times 1918	-1.906**	-1.794**			-1.631**	-1.728**
	(0.717)	(0.831)			(0.780)	(0.861)
Coal medium-low \times 1918			0.400	0.281	0.328	0.421
			(0.689)	(0.704)	(0.677)	(0.697)
Coal medium \times 1918			0.238	0.045	-0.048	0.016
			(0.695)	(0.814)	(0.677)	(0.789)
Coal medium-high \times 1918			1.591***	1.734**	1.325**	1.737**
			(0.572)	(0.688)	(0.575)	(0.679)
Coal high \times 1918			1.289***	1.280	0.894	1.205
			(0.483)	(0.794)	(0.538)	(0.826)
Observations	21,156	21,156	21,156	21,156	21,156	21,156
R-squared	0.098	0.101	0.096	0.100	0.100	0.104
FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Year \times District FE	No	Yes	No	Yes	No	Yes

Notes: The table shows panel regression estimates of the effect of taxable income 1907 and installed coal-fired power plant capacity within 50 kilometers on the differential change in mortality rates. Regressions (1) and (2) show the change in medium-low, medium, medium-high, and high-income parishes between 1917 and 1918 relative to the change in low-income parishes. Regressions (3) and (4) show the change in medium-low, medium, medium-high, and high coal capacity parishes between 1917 and 1918 relative to the change in low coal capacity parishes. Regressions (5) and (6) include income and coal capacity measures. All regressions include a full set of year fixed effects, parish fixed effects, and the average mortality rate 1910–1913 interacted with year fixed effects. Columns (2), (4), and (6) include the full set of pre-treatment control variables X_i each interacted with an indicator variable for the year 1918 and year times county fixed effects. Standard errors clustered at the county level are in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

Table C.9: Robustness checks – Exclude large and small parishes by population 1910

	Mortality rate 1914–1925					
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Panel A:</i> Exclude largest and smallest one percent of parishes						
Income medium \times 1918	-1.748*** (0.475)	-1.469*** (0.453)			-1.675*** (0.485)	-1.459*** (0.461)
Income high \times 1918	-1.360*** (0.437)	-1.162** (0.512)			-1.227** (0.469)	-1.126** (0.511)
Coal medium \times 1918			-0.085 (0.550)	-0.021 (0.615)	-0.385 (0.545)	-0.137 (0.605)
Coal high \times 1918			1.108** (0.435)	1.190** (0.590)	0.727 (0.456)	1.065* (0.592)
Observations	20,748	20,748	20,748	20,748	20,748	20,748
R-squared	0.098	0.101	0.097	0.101	0.100	0.103
<i>Panel B:</i> Exclude largest and smallest 5 percent of parishes						
Income medium \times 1918	-1.591*** (0.459)	-1.266*** (0.447)			-1.496*** (0.472)	-1.222*** (0.455)
Income high \times 1918	-1.096** (0.478)	-0.846 (0.550)			-0.905* (0.528)	-0.765 (0.555)
Coal medium \times 1918			0.043 (0.538)	0.296 (0.613)	-0.187 (0.551)	0.203 (0.605)
Coal high \times 1918			1.074** (0.454)	1.385** (0.638)	0.772 (0.494)	1.281* (0.644)
Observations	19,044	19,044	19,044	19,044	19,044	19,044
R-squared	0.102	0.106	0.102	0.106	0.104	0.108
FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Year \times District FE	No	Yes	No	Yes	No	Yes

Notes: The table shows panel regression estimates of the effect of taxable income 1907 and installed coal-fired power plant capacity within 50 kilometers on the differential change in mortality rates. Panel A (Panel B) shows results based on a sample excluding the smallest and largest one (five) percent of parishes based on population size 1910. Regressions (1) and (2) show the change in medium and high-income parishes between 1917 and 1918 relative to the change in low-income parishes. Regressions (3) and (4) show the change in medium and high coal capacity parishes between 1917 and 1918 relative to the change in low coal capacity parishes. Regressions (5) and (6) include income and coal capacity measures. All regressions include a full set of year fixed effects, parish fixed effects, and the average mortality rate 1910–1913 interacted with year fixed effects. Columns (2), (4), and (6) include the full set of pre-treatment control variables X_i each interacted with an indicator variable for the year 1918 and year times county fixed effects. Standard errors clustered at the county level are in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

Table C.10: Robustness checks – Conley S.E.

	Mortality rate 1914–1925					
	(1)	(2)	(3)	(4)	(5)	(6)
Income medium × 1918	-1.770*** (0.484) [0.436] {0.384}	-1.449*** (0.498) [0.436] {0.381}			-1.695*** (0.485) [0.439] {0.388}	-1.447*** (0.499) [0.438] {0.389}
Income high × 1918	-1.211** (0.475) [0.480] {0.502}	-0.936* (0.559) [0.543] {0.524}			-1.076** (0.481) [0.490] {0.455}	-0.911 (0.558) [0.543] {0.516}
Coal medium × 1918			-0.049 (0.491) [0.494] {0.527}	-0.101 (0.551) [0.524] {0.490}	-0.306 (0.493) [0.476] {0.447}	-0.191 (0.550) [0.518] {0.466}
Coal high × 1918			1.058** (0.452) [0.452] {0.499}	1.088* (0.606) [0.569] {0.526}	0.733* (0.443) [0.439] {0.446}	0.999* (0.601) [0.567] {0.521}
Observations	21,097	21,097	21,097	21,097	21,097	21,097
FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
Year × District FE	No	Yes	No	Yes	No	Yes

Notes: The table shows panel regression estimates of the effect of taxable income 1907 and installed coal-fired power plant capacity within 50 kilometers on the differential change in mortality rates. Regressions (1) and (2) show the change in medium and high-income parishes between 1917 and 1918 relative to the change in low-income parishes. Regressions (3) and (4) show the change in medium and high coal capacity parishes between 1917 and 1918 relative to the change in low coal capacity parishes. Regressions (5) and (6) include income and coal capacity measures. All regressions include a full set of year fixed effects, parish fixed effects, and the average mortality rate 1910–1913 interacted with year fixed effects. Columns (2), (4), and (6) include the full set of pre-treatment control variables X_i each interacted with an indicator variable for the year 1918 and year times county fixed effects. Conley standard errors with cut-off distances at 10 kilometers (in parentheses), 20 kilometers [in brackets], and 50 kilometers {in curly brackets} are below the point estimates. ***, **, and * denote the lowest statistical significance of all three specifications at the 1, 5, and 10 percent level, respectively.

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Declaration to confirm that the dissertation has been produced independently:

I hereby declare that I have produced my doctoral thesis "*Market access and regional disparities: Empirical evidence from German history*" independently and without external assistance, and that I have made a significant contribution as co-author to other scientific articles. I have identified all word-for-word quotations of other authors, as well as comments based closely on other authors' ideas, and I have cited the sources according to the guidelines I received.

Bayreuth, April 8, 2021

Richard Franke