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Abstract

This paper provides a comprehensive assessment of the effect of railways on the spatial economic development of a German economy, the Kingdom of Württemberg, during the Industrial Revolution. Our identification strategy compares the economic development of 'winning' municipalities that were connected to the railway in 1845-54 to the development of 'losing' municipalities that were the runners-up choice for a given railway line between two major towns. Estimates from both differences-in-differences and inverse-probability-weighted models suggest that railway access increased annual population growth by 0.4 percentage points over more than half a century. Railways also increased wages, income and housing values, in line with predictions of economic geography models of transport infrastructure improvements, reduced the gender wage gap, and accelerated the transition away from agriculture. We find little evidence that these effects are driven by localised displacement effects.

Keywords: Railway access, growth, sectoral employment, Industrial Revolution, Württemberg.
JEL Classification: R12, R40, O14, N73, N93.

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1 Introduction

The invention of the railway–and the construction of railway networks–is widely regarded as a major driver of economic growth during the Industrial Revolution. This paper studies the effect of railway access on municipal population, wages, housing values, and industrialisation of a developing German economy in the 19th century. Positive effects of falling transport costs on population, wages and housing values are key predictions of new economic geography models in the spirit of Helpman (1998), Redding and Sturm (2008), and Redding and Turner (2015). Moreover, industrialisation, i.e., the shift from agriculture to industry, played an important role in the transition from stagnation to modern growth in the 19th century (see, e.g., Hansen and Prescott, 2002; Strulik and Weisdorf, 2008). The paper thus provides a comprehensive assessment of the effect of railways on spatial economic activity during the Industrial Revolution.

We focus on the development of the Kingdom of Württemberg, the third largest state in the German Empire (see Appendix Figure A-1), in 1829-1910. Württemberg is a particularly interesting case for studying the economic effects of railways during a country's industrialisation process. It was a poor agrarian state at the beginning of the 19th century, which lacked raw materials and navigable waterways. Württemberg thus had little industry when the construction of the railway began in the 1840s. We use exceptionally rich data at the municipality level to study whether and how the railway altered the location and structure of economic activity in Württemberg–and paved its way to one of Germany's economic powerhouses, which today has the highest density of industrial employment of all German states.

Württemberg founded a public railway company in 1843, and opened its first railway line in 1845. The first stage of the network expansion from 1845 to 1854 connected the capital Stuttgart in the middle of the country with major towns in the east, north, and south of the Kingdom. Along the way, 73 of Württemberg's 1,858 municipalities gained access to the railway until 1854, many of them small and insignificant before the coming of the railway. We evaluate the short- and long-run effects of this first stage of the railway expansion on the organisation of spatial economic activity.

Our choice of outcome variables is motivated by the predictions of multi-region economic geography models, in which workers move to arbitrage away differences in real wages (Redding and Sturm, 2008; Redding and Turner, 2015). Transport infrastructure improvements increase local wages in these models and thus lead to population inflows, which in turn drive up the price of the immobile factor (usually land or housing). The effect of transport infrastructure on the sectoral employment structure is less explored. Sector-specific employment responses are likely to depend on the weight-to-value ratio, the prevalence of agglomeration economies, and the land share of production (Redding and Turner, 2015). Railways might accelerate the shift away from agriculture by increasing land prices and enabling non-agricultural sectors to realise agglomeration economies (Bogart, 2014). Moreover, railways might boost specific industries by forward and backward linkages, increasing, e.g., the demand for iron and steel as inputs (Fremdling, 1985). We thus complement our broader analysis of the effect of railways on the transition from agriculture to industry with a more explorative analysis of employment in different sub-industries–such as the consumer or capital goods industry.

Our analysis is based on novel and highly detailed panel data of regional population and sectoral employment in Württemberg between 1829 and 1910. The data, which we link with geo-referenced information on railway construction, cover all 1,858 municipalities in Württemberg and distinguish between employment in 320 sectors. This allows us to study the effect of railways on economic development at a significantly finer sectoral and spatial level than much of the previous literature. Since the data cover the universe of Württemberg's municipalities, we can also study the effect of railways on smaller and rural municipalities, often neglected in related studies (see, e.g., Berger and Enflo, 2017; Hornung, 2015).

A key challenge for identifying the causal effect of railway access on economic development is the endogenous location of railway lines. For example, large or growing municipalities might have been more likely to gain access to the railway network than small or stagnant municipalities. Railways may then follow economic development rather than causing it (Fishlow, 1965). This paper uses information on unrealised alternative routes to identify valid counterfactuals for what would have happened to railway municipalities in the absence of the railway. Württemberg's government only specified the beginning and end points of a line. For each line, external experts then proposed and discussed alternative routes. Our empirical strategy compares outcomes of 'winning' municipalities, which are located along an actually built route, and 'losing' municipalities, which are located along the alternative unrealised route(s) of the same line.[1]

[1] Greenstone 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 unrealised 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).

We use two different estimation approaches to implement our empirical strategy. First, our baseline approach compares changes in economic outcomes of winning and losing municipalities in a differences-in-differences framework. The key identifying assumption of this approach is that economic outcomes in winning and losing municipalities would have followed the same time trend in the absence of the railway construction. 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. We show that winning and losing municipalities were actually very similar in their pre-railway characteristics and trends. This lends credibility to our identifying assumptions.

Both estimation approaches yield very similar results. We find a positive–and statistically significant–effect of railway access on municipal population which is growing over time. By 1910, railway access in the first stage of the railway expansion had increased population by around 0.23 log points. This corresponds to a 0.4 percentage points increase in annual population growth. Interestingly, much of the population increase occurred in the 1890s and 1900s, i.e., long after the first stage of the railway expansion was completed in 1854. This finding is consistent with significant adjustment costs, which slow down the transition to a new steady state after transport infrastructure improvements.

Repeated cross-sectional estimates of the effect of railway access on population are almost identical to our event study results. This suggest that within-line comparisons of winning and runner-up municipalities allow credible identification also for outcome variables where we lack data for the pre-treatment period, namely wages, income and housing. Applying semi-parametric methods, we find a sizeable positive effect of railway access on wages and income. The annual taxable income per capita increased by about 12 percent in 1907 relative to losing municipalities. Moreover, railways increased the wage of female day labourers by more than that of males and thus reduced the gender wage gap. Railway access also increased housing values by between 37.3 and 43.4 percent. These findings are consistent with the predictions of spatial equilibrium models of transport infrastructure improvements.

Railway access also strongly accelerated the transition away from agriculture, both in cross-sectional and panel estimations. We find that railway access increased winning municipalities' employment share in industry by between 16.3 percent and 18.8 percent until 1895 (relative to losing municipalities). Effects are particularly large for employment in construction (+42.4 percent) and the capital goods industry (+42.3 percent). Railways were thus an important driving force of the shift from agriculture to industry, which is a key feature of the transition to modern growth (see, e.g., Galor, 2005). We find little evidence that the positive effects on industrialisation–as well as on wages, income, and housing values–are driven by localised displacement effects from nearby municipalities.

Related literature. Our paper is closely related to a growing literature that studies the growth effects of railways[2] by comparing areas with access to the railway network to areas without access.[3] Many of these studies–especially those that study historical settings–analyse the effect of railways on urban population growth as a proxy for economic development.[4] Hornung (2015), for instance, shows that railway access increases urban population growth in Prussia between 1840 and 1871. Likewise, Jedwab and Moradi (2016) find that the construction of the colonial railway network in Ghana had a large positive effect on city growth, which persisted even after the later demise of the network. Berger and Enflo (2017) document a positive and persistent effect of railways on urban population growth in Sweden between 1850 and 2010, and Atack et al. (2010) find a positive effect of railway access on the urbanisation of counties in the American Midwest between 1850 and 1860.

Our paper contributes to this literature by studying the effect of railways on population at an unusually disaggregated level: the median land area of municipalities in our sample is just 8.6 square kilometres. Our analysis thus complements the existing literature on urban population growth with comprehensive evidence from often rural municipalities. One additional advantage of our setting is that municipality borders remained largely unchanged over a 100-year period. We are thus able to observe the evolution of smaller villages to towns over time, and the effect that railways had on this process. Town-level studies, in contrast, usually have to exclude towns that were formed after the beginning of the sample period.

[2] Closely related literature strands quantify the economic effects of motorways (see, e.g., Duranton and Turner, 2012; Möller and Zierer, 2018) and regional public transport, such as subways (e.g., Gonzalez-Navarro and Turner, 2018) or commuter rail systems (e.g., Mayer and Trevien, 2017).

[3] An 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.

[4] Important exceptions include Banerjee et al. (2012) who estimate the effect of the Chinese transport network on regional GDP and Donaldson (2018) who estimates the effect of Colonial India's railway network on trade volumes and real agricultural income.

They might thus understate the effect of railways in the long run (Berger and Enflo, 2017). Our paper also contributes by studying wages and income, which trigger population flows in spatial equilibrium, and housing values, which respond to population flows, as additional outcomes.

Only recently have Büchel and Kyburz (2019) provided first evidence on the population effect of railways at a level of aggregation comparable to ours. The authors focus on Swiss municipalities in the 19th century. Using least cost paths as an instrument for railway access, they find that railway access increased a municipality's annual population growth rate by about 0.4 percentage points–an estimate similar to ours. We add to Büchel and Kyburz (2019) by studying whether population flows are indeed associated with increases in local income and housing values, and by analyzing structural change as a key feature of the Industrial Revolution. We also apply an alternative identification strategy. In particular, we compare population changes of winning and losing municipalities, instead of applying the IV strategy used by Büchel and Kyburz (2019) and many others in the literature (Banerjee et al., 2012; Berger and Enflo, 2017; Hornung, 2015). One problem of the IV approach is that geography is likely to correlate with least cost paths, thereby potentially violating the exclusion restriction.

Our paper also relates to the literature on the effect of railways on the organisation of production. 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. Tang (2014) shows for 19th century Japan that rail access increased average firm capitalisation and led to a redistribution of firms from scarcely- to densely-populated prefectures. Gutberlet (2013) shows that transport-induced improvements in market access increased the spatial concentration of manufacturing across districts of the German Empire between 1861 and 1882. All these papers study the effects of railways at a relatively highly aggregated spatial level. Our novel data set allows us to study the spatial concentration of production at the level of municipalities.

Our paper also contributes to our understanding of the structural transformation that occurred in Europe during the 18th and 19th century (see, e.g., Broadberry et al. (2010) for an overview). 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. We contribute to this literature by highlighting the role of the railway for structural transformation, complementing recent work by Bogart et al.

(2018) for parishes in England and Wales. Bogart et al. (2018) study the effect of railways on population over the 19th century and for employment between 1851 and 1881. They show that the railway boosted growth in population and secondary employment. In the short run, population increases close to railway stations came at the expense of parishes further away.

The key differences between Bogart et al. (2018) and our study are the identification strategy and the historical context. While Bogart et al. (2018) use least cost paths as an instrument for railway stations, our strategy relies on alternative unrealised routes of a given line to identify valid counterfactuals for municipalities with railway access. Moreover, Bogart et al. (2018) focus on England, Europe's powerhouse in the 19th century and the front-runner of the Industrial Revolution. England had already seen substantial declines in the share of the workforce in agriculture during the 17th and early 18th century (Chilosi et al., 2018; Shaw-Taylor and Wrigley, 2014). In fact, agriculture was no longer the dominant sector in 1851 when Bogart et al. (2018) begin their analysis of employment growth. This is in sharp contrast to our setting: Württemberg was at the very beginning of the structural transformation process when we measure its pre-railway employment structure in 1829.

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. A confederate of Napoleon, Württemberg gained several territories in the Napoleonic era, almost doubling its size between 1803 and 1810. These new territories are generally 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 one of the four Kingdoms of the German Empire, and the third largest state in the Empire after Prussia and Bavaria (see Figure A-1 in the Appendix).

Württemberg's initial conditions for the industrialisation 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 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.

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).[5] 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 industrialisation process (Gysin, 1989). By creating a free-trade area throughout much of Germany, the Union considerably expanded firms' potential selling markets. 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). However, it was only in 1843 that Württemberg founded a public railway company, the *Königlich Württembergische Staats-Eisenbahnen*, and began to build a railway network. Importantly, Württemberg did not approve and license private railway companies for the construction and operation of 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 municipalities with favourable 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 1. The first stage from 1845 to 1854 (see panel (a)) saw the con-

[5]The 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).

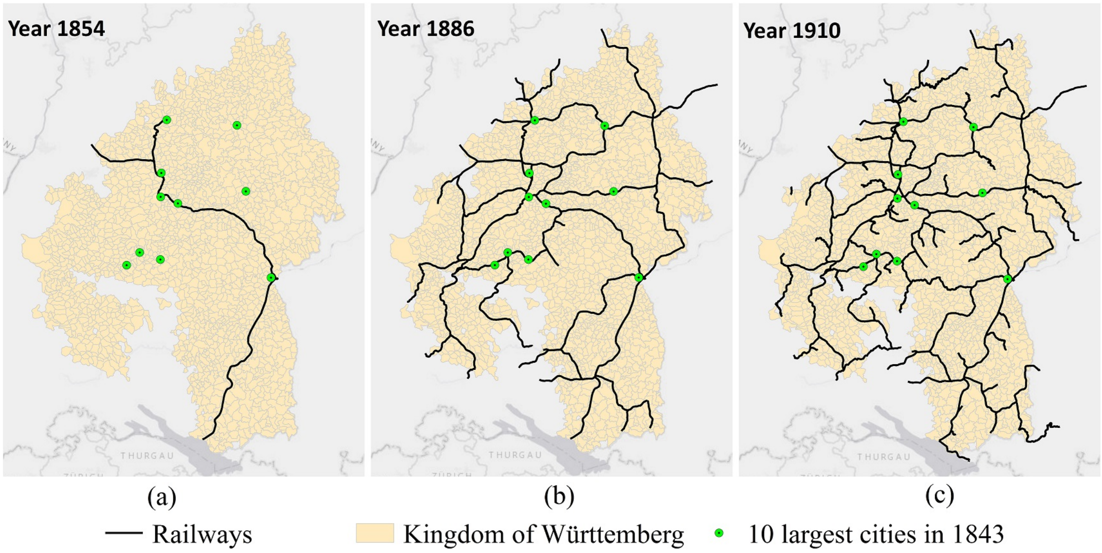

Figure 1: Network development stages in the Kingdom of Württemberg 1845-1910

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.

struction 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 neighbouring 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 harbours 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 1). After often lengthy negotiations, Württemberg's railway network was now also well connected to those of its neighbouring states. The length of Württemberg's railway network increased from 290 kilometres in 1854 to 1,560 kilometres in 1886 and the number of municipalities 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 (*Eisenbahn-Kommission*) and external experts to develop the exact routes for each line.

The railway commission compared competing routes mainly under technical aspects. In particular, the commission set 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 potential alternative routes.[6] Appendix A.3 describes the planing process for the central line in detail.

In addition to technical aspects, Württemberg's geographical location–squeezed between Baden in the 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-passings, 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 territory than on Bavarian territory. It would thus have reduced passenger volume of the Bavarian railway line.

[6] Reports and lectures of the external planners for the first stage of the railway construction are published in Etzel et al. (1985).

Larger towns or counties 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 centre. 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.

Industrialisation until 1895. The railway is widely seen as an important cornerstone for Württemberg's industrial take-off (Flik, 2001; Marquardt, 1985). Württemberg was nevertheless a late-comer among the German states–and it was not until the end of the 19th century that the industrialisation 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 percent and 38 percent, respectively. The number of industrial firms with at least 5 employees even increased by 86 percent between 1882 and 1895, and employment in these firms more than doubled.

Nevertheless, Württemberg's industrialisation process still lagged behind the rest of the German Empire at the dawn of the 19th century. 44.4 percent of all full-time employees in Württemberg were in agriculture in 1895, compared to just 36.2 percent in the whole German Empire (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 industrial employment in 1895. The agricultural employment share decreased only slowly over time and still reached 41.3 percent in 1907.

3 Data

Our panel data cover all municipalities in the Kingdom of Württemberg. We aggregate municipalities to take boundary changes into account that occur during the sample period.[7]

[7] We digitised municipality borders from Kommission für geschichtliche Landeskunde in Baden-Württemberg and Landesvermessungsamt Baden-Württemberg (1972) and use information on boundary changes from Statistisches Landesamt Baden-Württemberg (2008). Overall, 71 municipalities are affected by mergers between 1821 and 1910.

This leaves us with 1,858 municipalities with a median area of 8.6 square kilometres.

Outcome variables. Population data come from 21 population censuses (Statistisches Landesamt Baden-Württemberg, 2008), conducted in the Kingdom of Württemberg between 1834 and 1910.[8] Every census recorded the total population of each municipality. Selected censuses also contain information on other demographic characteristics of the municipal 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 A-2 shows the average annual growth rate of municipalities 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 municipalities experiencing population decline. Württemberg's poor economic conditions made the kingdom one of the main origin regions for oversea migration from Germany over much of the 19th century. More than 337,000 people left Württemberg in 1834-1871 alone (von Hippel, 1984). Figure A-2 also indicates that population growth was indeed higher in municipalities located along the railway network.

We digitised data on the average daily wage of day labourers in 1884, 1898, and 1909, taxable income and building taxe 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 labourers' (*ortsüblichen Tagelöhne gewöhnlicher Tagarbeiter*) were recorded following the Sickness Insurance Law of 1883. The data distinguish between wages of females and males, allowing us to calculate the gender wage gap. Taxable income refers to natural persons and equals income net of tax allowances and other deductions. We approximate average housing values from building tax revenues in 1907[9] and use the average fire insurance value of buildings as an alternative indicator.[10]

We further digitised employment data from the occupation censuses of 1895 and 1907

[8] The 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 municipalities in the county of Böblingen in 1867 and the county of Leonberg in 1858.

[9] The 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 municipality's average housing value.

[10] Though dominated by the property value, the fire insurance also covers furniture and other possessions in the house.

(Königliches Statistisches Landesamt, 1900a, 1910).[11] The occupation censuses comprise municipal information on the number of full-time gainfully employed person (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 municipality.

In addition, we digitalised Württemberg's *Gewerbestatistik* for 1829 (various volumes of *Gewerbekataster*, Staatsarchiv Ludwigsburg E 258 VI) and 1895 (Königliches Statistisches Landesamt, 1900b).[12] *Gewerbe* includes mining, manufacturing, handicrafts, construction, trade and transport (excluding railways and post). The 1829 and 1895 data provide detailed data on the number of industrial plants and total employment in these establishments, disaggregated by 3-digit sectors.[13]

We use the disaggregated data from the *Gewerbestatistik* to distinguish between industrial employment in the basic metal industry (e.g. mining, quarries or paper production), capital goods industry (e.g. engineering or heavy chemical industry), construction, consumer goods industry (e.g. musical instruments, spinning or weaving) and food industry.[14] Moreover, we measure (absolute) specialisation in industry by the Hirschman-Herfindahl (HHI) index:

$$HHI_{it} = \sum_{l=1}^{L} (b_{ilt})^2,$$

where b_{ilt} is municipality 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). Finally, we use the *Gewerbestatistik* to calculate establishment size in industry as the average number of person employed in an establishment (*Hauptbetrieb*).

Railway access. We link the panel data on population, sectoral employment, and income with geo-referenced information on municipalities and railway construction in Württemberg.

[11] The 1895 occupation census is–to the best of our knowledge–the first census that provides employment data at the municipality level.

[12] Unfortunately, the 1907 edition was–to the best of our knowledge–never published by Württemberg's statistical office.

[13] We match the 3-digit industry groups from the 1829 statistic to the 320 groups of the 1895 statistic. 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 employee, whereas the *Gewerbestatistik* focuses on location of plants.

[14] The detailed assignment of the 320 groups to the five categories can be obtained from the authors upon request.

Dumjahn (1984) and Wolff and Menges (1995) provide information on the starting and end point of each railway line, the length of the line and the date it was opened. We use this information to define the nodes of the railway network. We identify municipalities as nodes if they are named as starting or end points of a railway segment or serve as a network junction.[15] 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 also add data on manufactories for the year 1832, published in Memminger (1833). The manufactory data comprise the location, products and employment level of each manufactory in 1832. Overall, there are 342 distinct manufactories in 128 municipalities with an average employment of 28 workers (excluding seasonal and home employment). We generate a dummy indicating whether a municipality had a manufactory in 1832. Data on the average elevation of municipalities 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).

4 Empirical Strategy

4.1 Treatment and control group

The main challenge for identifying the causal effect of railway access is that municipalities were not randomly chosen to be connected to the railway. Table 1 illustrates this selection problem: It compares economic and demographic characteristics of different groups of municipalities before the construction of Württemberg's railway network began.

Column (1) shows characteristics for the railway nodes, Column (2) for other municipalities who gained access to the railway in the first stage of the railway expansion, and Column (3) for all other municipalities. Columns (5) and (6) restrict attention to the sub-sample of winner and runner-up municipalities.

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. The nodes were not only much larger and more likely to be located at rivers than the other municipalities (see Column (1) of Table 1). They were also

[15] Railway segments are railways that were constructed without interruption and opened at the same date.

Table 1: Group comparison of pre-treatment characteristics

	Full sample				Winners vs. runners-up		
	Nodes	Railway municipalities	Non-railway municipalities	Difference (2) - (3)	Winner municipalities	Runner-up municipalities	Difference (5) - (6)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Pop. 1834 (log)	8.244	6.804	6.390	0.414	6.745	6.927	-0.183
	(1.484)	(0.647)	(0.702)	[0.092]	(0.674)	(0.715)	[0.118]
Pop. density 1834 (log)	5.656	4.430	4.278	0.152	4.396	4.571	-0.175
	(0.728)	(0.629)	(0.604)	[0.079]	(0.621)	(0.603)	[0.103]
Protestants 1821 (share)	0.760	0.623	0.618	0.006	0.602	0.675	-0.073
	(0.375)	(0.481)	(0.469)	[0.062]	(0.453)	(0.433)	[0.074]
Manufactory dummy 1832	0.538	0.083	0.051	0.032	0.091	0.172	-0.081
	(0.519)	(0.279)	(0.220)	[0.029]	(0.290)	(0.379)	[0.059]
Ind. employment per 100 persons 1829	12.436	8.689	7.717	0.972	9.029	9.568	-0.539
	(4.334)	(3.801)	(4.334)	[0.567]	(3.680)	(4.176)	[0.674]
Average elevation (in m)	382.469	408.817	496.470	-87.653	423.420	411.113	12.306
	(119.664)	(136.995)	(155.942)	[20.393]	(145.101)	(138.093)	[23.649]
River dummy	0.462	0.133	0.075	0.058	0.145	0.131	0.014
	(0.519)	(0.343)	(0.264)	[0.035]	(0.356)	(0.339)	[0.058]
Road dummy	0.846	0.833	0.480	0.353	0.836	0.778	0.059
	(0.376)	(0.376)	(0.500)	[0.065]	(0.373)	(0.418)	[0.068]
Observations	13	60	1,785		55	99	

Notes: The table shows average values of pre-treatment characteristics for nodes (Column (1)), railway municipalities (Column (2)), and non-railway municipalities (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 municipalities and Column (7) the mean difference between winners and runners-up. Nodes are defined as municipalities that are either starting or end points of a railway segment or serve as network junctions. Railway municipalities obtained railway access in the first construction stage 1845-1854 (but are not nodes). Non-railway municipalities did not obtain railway access in the first construction stage. Winners are municipalities that obtained railway access in the first construction stage (but are not nodes). Runners-up are municipalities 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)).

economically more advanced, with higher industrial employment shares in 1829 and a higher likelihood to have a manufactory in 1832. We generally exclude nodes from our analysis, since we cannot construct credible counterfactuals for them.

We thus restrict attention to the effect of railway access on municipalities that gained access to the railway in the first stage of the expansion and were not network nodes. These are the municipalities in the treatment group. One potential control group is the universe of municipalities 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 municipalities that were not nodes (Column (2)) and non-railway municipalities (Column (3)). Treated municipalities were generally larger, situated at lower altitude, and more likely to have road access than non-railway municipalities (Column (4)). Naive comparisons between the two groups are thus likely to yield biased estimates.

Much of our analyses thus focuses on an alternative control group, consisting of municipalities that would have obtained access to the railway if proposed alternative routes had been built. As described in Section 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 municipalities, i.e., municipalities with designated railway access on an alternative line that was eventually not built in the first stage of the railway expansion.

Figure 2 shows the runner-up municipalities (crosses), along with the winner municipalities that were actually chosen (points). Colours indicate all routes–and associated municipalities–that have been suggested to connect two nodes. 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 Table A-2 in the Appendix for details). The decision for or against a specific route was mainly based on technical aspects. Political conflicts with neighbouring countries also played a role (see Section 2).

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

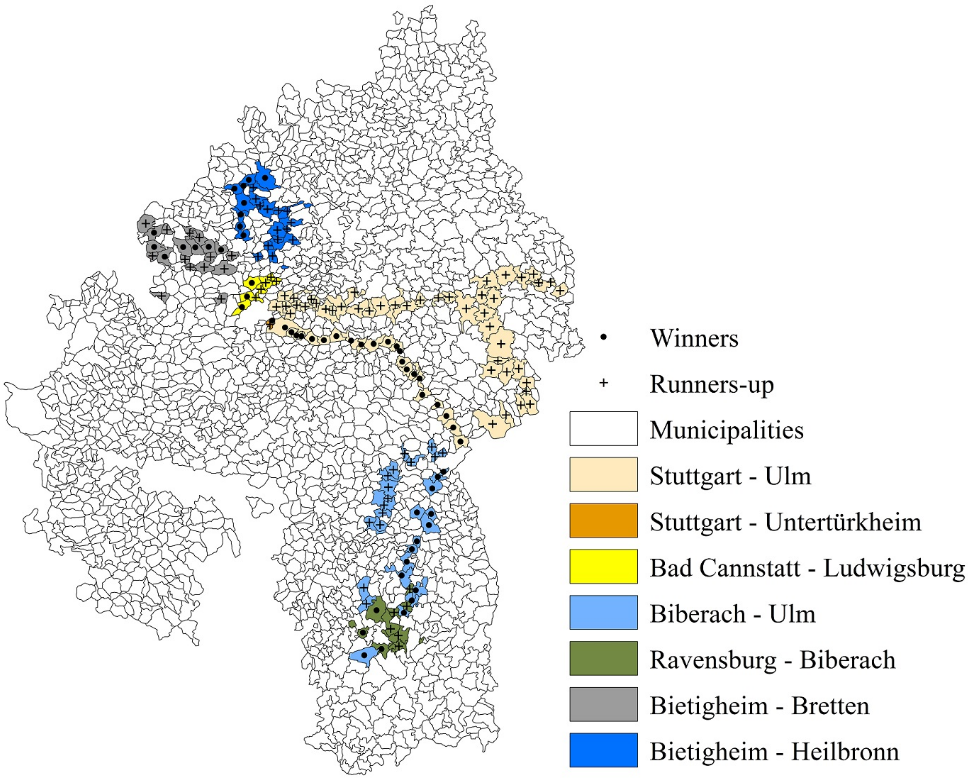

Figure 2: Winner and runner-up municipalities

Notes: The figure shows 'winner municipalities' (points) and 'runner-up municipalities' (crosses). Winners are municipalities that obtained railway access in the first construction stage (but are not nodes). Runners-up are municipalities with designated railway access on an alternative line that was eventually not built in the first stage of the railway expansion. Colours distinguish between the different cases and mark all potential routes suggested for one railway line.
Sources: Dumjahn (1984), Etzel et al. (1985), Kunz and Zipf (2008), Kommission für geschichtliche Landeskunde in Baden-Württemberg and Landesvermessungsamt Baden-Württemberg (1972), Königliches Statistisches Landesamt (1911), and Esri HERE Delorme, MapmyIndia, OpenStreetMap© contributors, and the GIS map user community. Authors' design.

many of the proposed lines that were initially not realised were built later.

Columns (5) to (7) of Table 1 provide support for our empirical strategy. Differences in pre-treatment characteristics decrease considerably when we compare winner municipalities (Column (5)) and runner-up municipalities (Column (6)).[16] This holds, in particular, for municipalities' access to roads and rivers, which differed greatly between railway and non-

[16]Nine of the railway municipalities in Column (2) are not among the winner municipalities in Column (5). These are municipalities that would have been connected to the railway under all alternative proposals (Asperg, Baindt, Ölbronn, and Tamm) or municipalities on the line Ravensburg - Friedrichshafen (Berg, Eschach, Hirschlatt, Meckenbeuren, and Taldorf), for which no alternative line was proposed. On the other hand, four municipalities are counted twice, as they were among the winning municipalities in two different cases (namely Schweinhausen, Ummendorf, Unteressendorf, and Wolpertswende). This is because Case 5 concerns a segment of Case 4.

railway municipalities but not between winner and runner-up municipalities. In fact, none of the mean differences between winners and runners-up is statistically significantly different from zero (Column (7)). If anything, winners appear to be somewhat smaller and less industrialised than runners-up before the coming of the railway.

4.2 Empirical specification

Differences-in-differences. We begin our analysis by comparing economic outcomes of municipalities in the treatment group (winners) and control group (runners-up) using a differences-in-differences strategy. Let $D_{ij,1855}$ be a binary treatment indicator that indicates whether municipality i of case j was connected to the railway by 1855, and let y_{ijt} be an outcome variable of municipality i in year t. The outcome variables are log population, income and wages, housing values, sectoral employment, the degree of sectoral specialisation, and average firm size.

Our basic differences-in-differences specification is:

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

where λ_t are year fixed effects and θ_j are case fixed effects. τ denotes years but is normalised 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.[17] The coefficient of interest (γ) captures mean shifts in outcome variables of treatment municipalities relative to control municipalities after the railway line was opened.

For sectoral employment, specialisation and firm size, we use data for 1829 and 1895/1907–and hence for one period before and one period after the first stage of the construction of the railway network. This renders equation (1) a standard differences-in-differences equation where all municipalities are treated at the same time. For population, we use data for 21 census years between 1834 and 1910.

Identification of the impact of the railway in equation (1) rests on the assumption that

[17] Our 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 municipality-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.

municipalities that gained access to the network until 1855 would have developed similarly to other municipalities in the absence of railway construction. Formally, OLS estimation of equation (1) will yield a consistent estimate of γ if:

Assumption 1 ***Strict exogeneity:***
$\mathbb{E}\left[\varepsilon_{ijt}|\boldsymbol{Line}_i, \lambda_t, \theta_j, D_{ij,1855}\right] = 0$ *for all* t *where* $\boldsymbol{Line}_i \equiv [Line_{ij0}, ..., Line_{ijT}]$.

Although this assumption is not directly testable, the similarity of treated and control municipalities in their pre-railway outcomes (see Table 1) lends credibility to the assumption. We also add municipality fixed effects to equation (1) to eliminate any unobservable time invariant municipality characteristics. In our analysis of the effect of railways on population, we also 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 (1) tests for a mean shift in our outcome variables. In our population analysis with many time periods, the model implicitly assumes that any effect occurs immediately and then remains constant over time. In additional event study regressions, we relax this assumption and allow the effect to vary with the time since treatment.

In particular, we estimate the following OLS regression:

$$y_{ijt} = \ddot{\alpha} + \ddot{\lambda}_t + \ddot{\theta}_j + \ddot{\beta} D_{ij,1855} + \sum_{k=-4}^{13} \gamma_k (D_{ij,1855} \times 1(\tau = k)_{jt}) + \sum_{k=-4}^{13} \delta_k 1(\tau = k)_{jt} + \varepsilon_{ijt}. \quad (2)$$

Coefficient γ_k for $k \geq 0$ corresponds to the difference in log population between treated and runner-up municipalities k periods after the railway line was opened. The difference is expressed relative to four periods before the line was opened (i.e., we normalise γ_{-4} to zero).[18] We estimate the specification for $-4 \leq \tau \leq 13$, as the sample is balanced for these periods. Specification (2) also tests for differences in trends between treated and control municipalities 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 $k < 0$.

[18] Historical accounts suggest that railway municipalities already experienced a population increase just before the railway line opened, as construction workers gathered in the municipalities. 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 railway line.

Semi-parametric estimates. We can interpret the parameter γ in equation (1) as an estimate of the average treatment effect on the treated (ATT).[19] However, this interpretation hinges on the linearity assumption present in equation (1). In an alternative strategy, we leave the data-generating process of the outcome variables unspecified and estimate the effect of interest by inverse probability weighting (IPW).[20]

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 municipalities 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.[21]

In an additional 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.[22] It thus provides some robustness to misspecification of parametric models–and is thus referred

[19]Let $y^{\tau}_{ijt}(d)$ denote the potential outcome at time $t+\tau$ of municipality i of case j whose winning line was opened in t. Here, $d \in \{0,1\}$ indicates railway access, so that $y^{\tau}_{ijt}(1)$ denotes the potential outcome with railway access and $y^{\tau}_{ijt}(0)$ the potential outcome without railway access. We furthermore define the potential outcome growth between periods $t-4$ and $t+\tau$ as $\Delta y^{\tau}_{ijt}(d) = y^{\tau}_{ijt}(d) - y_{ijt-4}$. The causal effect of railway access at time t on the outcome of interest after τ periods is $\gamma_{att,\tau} \equiv \mathbb{E}\left[\Delta y^{\tau}_{ijt}(1) - \Delta y^{\tau}_{ijt}(0) \mid D_{ij,1855}=1\right]$. As in the event study analysis, we again express population relative to a baseline four periods before the treatment.

[20]We use STATA 14.2 command `teffects`. Wooldridge (2010) provides a detailed description of the IPW estimator as well as the IPWRA estimator described further below.

[21] More specifically, we first use 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 $\boldsymbol{X}_i$. 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):

$$\hat{w}_i = \begin{cases} 1/\hat{\mathbb{E}}\left[D_{ij,1855}\right] \text{ if } D_{ij,1855}=1, \\ -\hat{P}_i/\left[\hat{\mathbb{E}}[D_{ij,1855}](1-\hat{P}_i)\right] \text{ if } D_{ij,1855}=0, \end{cases} \tag{3}$$

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 (3), for instance, is simply the fraction of municipalities 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: $\hat{\gamma}_{att,\tau,IPW} = \hat{\mathbb{E}}[\hat{w}_i \cdot \Delta y^{\tau}_{ijt}]$. Here, $\Delta y^{\tau}_{ijt} = y^{t+\tau}_{ijt} - y^{t-4}_{ijt}$ is the change in outcome y between period $t-4$ and $t+\tau$ for a municipality 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 $\tau = -3, ..., 13$ with $\tau = 0$ corresponding to the year of railway access.

[22]The IPWRA model uses $\hat{w}_i$ from equation (3) in footnote 21 to run weighted regressions of Δy^{τ}_{ijt} on our set of covariates. These regressions are estimated separately for treated and control municipalities. Specifically, we estimate parameters (α_0, ω_0) and (α_1, ω_1) by solving the following weighted least squares problems:

$$\min_{\alpha_0,\omega_0} (1-D_{ij,1855})\hat{w}_i(\Delta y^{\tau}_{ijt} - \alpha_0 - \omega_0 \boldsymbol{X}_i)^2,$$
$$\min_{\alpha_1,\omega_1} D_{ij,1855}\hat{w}_i(\Delta y^{\tau}_{ijt} - \alpha_1 - \omega_1 \boldsymbol{X}_i)^2.$$

The IPWRA estimate is then given by the average of the difference in predicted values, as evaluated for the sub-population of municipalities with railway access: $\hat{\tau}_{att,\tau,IPWRA} = \hat{\mathbb{E}}\left[(\hat{\alpha}_1 - \hat{\omega}_1 \boldsymbol{X}_i) - (\hat{\alpha}_0 - \hat{\omega}_0 \boldsymbol{X}_i) \mid D_{ij,1855}=1\right]$.

to as a *doubly-robust* estimator (see Imbens and Wooldridge (2009) for a discussion).

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

Assumption 2 ***Unconfoundedness:*** $(\Delta y_{ijt}^{\tau}(1), \Delta y_{ijt}^{\tau}(0)) \perp D_{ij,1855} \mid \boldsymbol{X}_i$.

This assumption states that conditional on a set of observed covariates $\boldsymbol{X}_i$, potential outcomes are independent of railway access. In addition, we rely on:

Assumption 3 ***Overlap:*** $\Pr(D_{ij,1855} = 1 \mid \boldsymbol{X}_i) < 1$ *for any value of* $\boldsymbol{X}_i$.

This assumption implies that for all values of the covariates, there is a chance of observing a unit without railway access.

Assumption 1 specifies a linear model for the outcomes of interest but allows for time-invariant unobserved characteristics at the group or municipality level. In contrast, Assumption 2 does not specify a model for the outcome variables but requires the information on background variables to be rich enough for potential outcomes to be conditionally independent of treatment status. In contrast to the differences-in-differences model, IPW/IPWRA do not necessarily require data on the pre-treatment period, which we lack for agricultural employment, income, wages, and housing values. Assumption 2 then applies to the level rather than the difference in potential outcomes.

Our covariates in $\boldsymbol{X}_i$ 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 metres, and two binary variables that indicate access to a paved road in 1848 and a waterway navigable in 1845 (see Table 1 for summary statistics). Case fixed effects ensure that the impact of railway access is identified from within-case comparisons.

5 Results

This section presents and discusses our estimation results. Section 5.1 reports the results on population, Section 5.2 the results on income, wages, and housing values, and Section 5.3 the results on sectoral employment, specialisation and establishment size, and Section 5.4 explores potential localised displacement effects.

5.1 Population growth

Differences-in-differences. Table 2 presents differences-in-differences estimates of the effect of railway access on population. Column (1) reports results from our baseline specification (1), restricting the sample to winner and runner-up municipalities. The regression suggests that railway access increased the population of winning municipalities by 0.079 log points (relative to losing municipalities). This effect is statistically significant with a standard error of 0.041.

Table 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	0.079*	0.105***	0.125***	0.200***	0.165***	0.133***
	(0.041)	(0.031)	(0.031)	(0.032)	(0.024)	(0.023)
Observations	3,234	3,234	3,234	38,745	38,745	38,745
Regional FE	Group	Parish	Parish	Group	Parish	Parish
Year × 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 municipality dummies. Regression (3) additionally include case by year fixed effects and regression (6) include county by year fixed effects. Standard errors clustered at the municipality level are in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

Specifications (2) and (3) probe the robustness of our results. Specification (2) adds municipality fixed effects to control for any unobserved heterogeneity between municipalities 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 somewhat to 0.105 (s.e. of 0.031) and 0.125 (s.e. of 0.031) in specifications (2) and (3), respectively. This increase is in line with the observation from Table 1 that, if anything, winning municipalities were slightly smaller and less industrialised than losing municipalities–and thus potentially negatively selected.

Specifications (4) to (6) re-estimate the regressions on the full sample of municipalities. At 0.200 (s.e. of 0.032), the baseline differences-in-differences 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 2). This is consistent with the notion that in the full sample, the control group includes many small and remote municipalities with unfavourable growth perspectives. The treatment effect in Column (4) is thus likely upward biased. The differences in the treatment effect estimated for the two samples become much smaller once we add municipality fixed effects (Column (5)) and year by county fixed effects (Column (6)). The coefficient estimates from the full-fledged specifications (3) and (6) are very similar (0.125 versus 0.133).

Event study. The event study analysis allows the effect of railway access on population to vary with the time since treatment. Figure 3 shows the estimated differences in log population between winner and runner-up municipalities, relative to the baseline difference four periods before the treatment.[23] 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 municipalities 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 municipalities would have grown in parallel.

At the time of the treatment, the point estimate jumps up to 0.035 (s.e. of 0.020). The difference in log population between winner and runner-up municipalities then gradually widens with time since treatment. Thirteen periods after treatment, population in winner municipalities is, on average, 0.231 log points larger than in runner-up municipalities (relative to the baseline difference four periods before the treatment). The event study analysis thus suggests that getting railway access in the first construction stage has lasting effects on population growth. In fact, much of the cumulative effect of railway access on population occurs decades after the treatment (recall that a period is 3-5 years long).

Semi-parametric estimates. Figure 4 shows the results from semi-parametric IPW and IPWRA 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 normalised

[23] Figure A-6 in the Appendix additionally compares over time differences in population between railway and non-railway municipalities for the full sample. The 'period since treatment' is not defined for non-railway municipalities that were not runners-up for a railway line. We thus instead compare differences 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. They nevertheless show a similar picture, with population differences gradually growing over time.

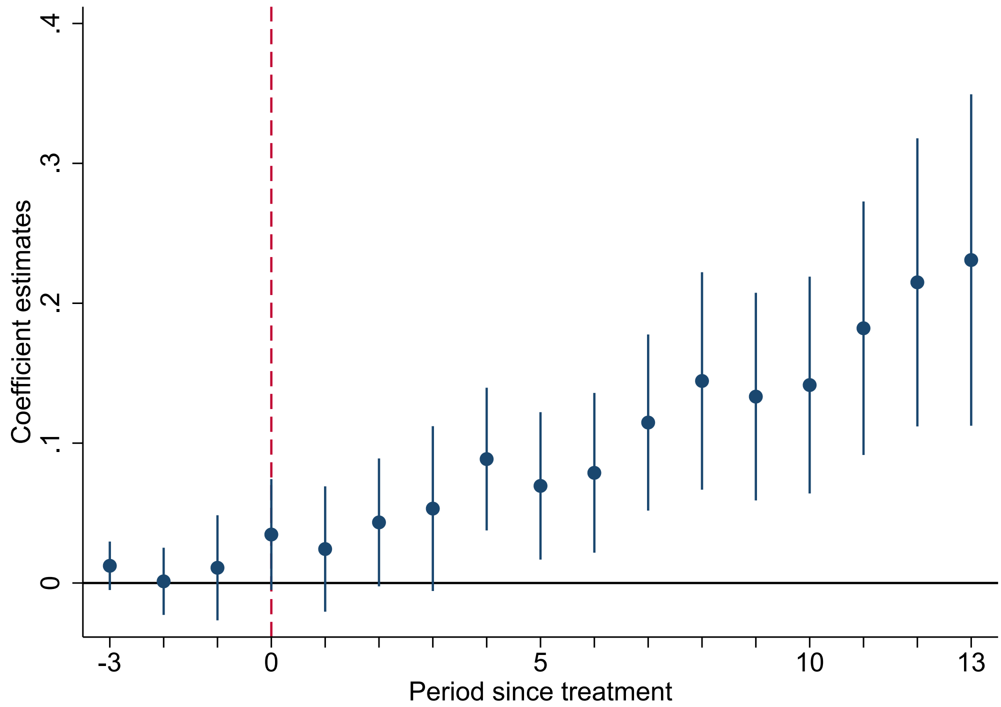

Figure 3: Event study estimates of the effect of railway access on log population

Notes: The graph depicts differences in log population between winner and runner-up municipalities for pre- and post-treatment periods, as estimated in an event study regression (see Section 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.

to zero for $\tau = -4$.[24]

The figure depicts estimates for $\tau = -4, -3, ..., 13$. Dots again mark the point estimates, vertical bands the corresponding 95 percent confidence intervals. Reassuringly, we again see no differential population trends between winner and runner-up municipalities before the arrival of the railway. Thereafter, log population gradually increases in winner municipalities in both IPW and IPWRA estimations. After thirteen periods, the cumulative effect of railway access on population reaches 0.220 and 0.223 log points in the IPW and IPWRA estimation, respectively. Semi-parametric estimates are thus almost identical to our event study results. 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 semi-parametric cross-sectional[25] estimates

[24] Figure A-7 in the Appendix shows semi-parametric results for the full sample, using the change in log population since 1834 as dependent variable. Results qualitatively resemble those for the winner vs. runners-up sample.

[25] Strictly speaking, IPW/IPWRA models in Figure 4 use the *change in population* relative to the baseline period as outcome variable. However, estimates are almost identical when we use the *level* instead. This is to be expected as treatment and control municipalities have similar population sizes in the baseline period.

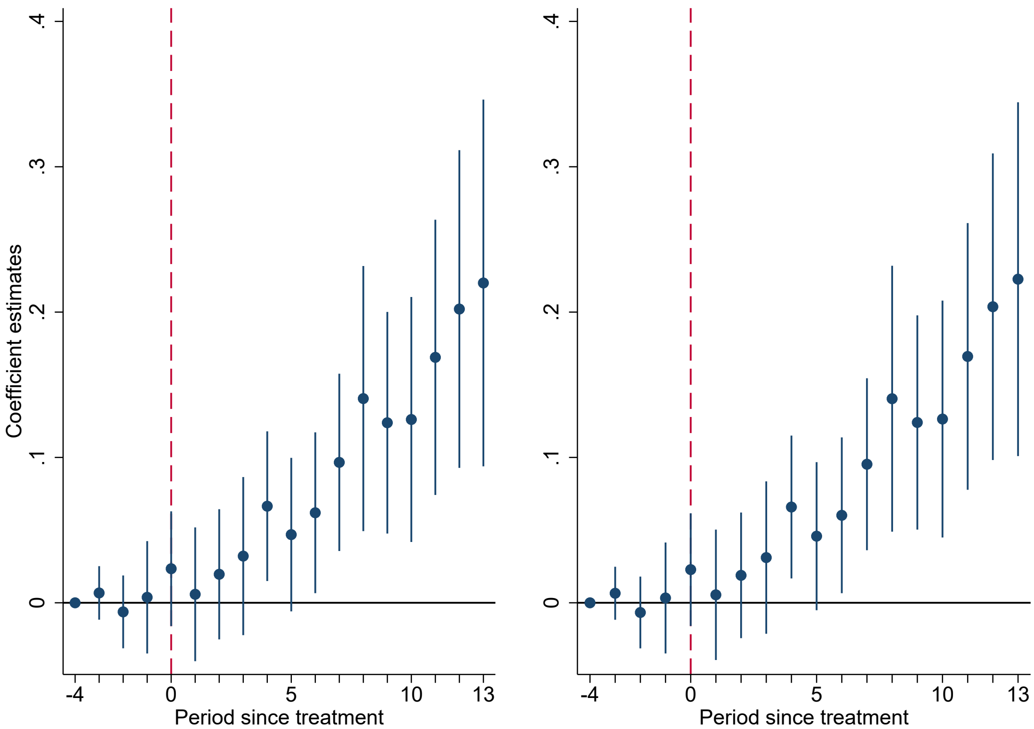

Figure 4: Semi-parametric estimates of the effect of railway access on log population

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 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.

only.

Natural population growth vs. immigration. We have established that railway access had a sizeable and lasting positive effect on municipal 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 municipalities. For all three years, we regress the share of inhabitants born outside of a municipality (hereafter: foreign-born) on the treatment group

Appendix Figure A-5 compares the event study results from Figure 3 with cross-sectional IPWRA estimates, which use the population level as outcome variable. Both models yield very similar results for the over-time effect of railway access on population.

dummy and our usual set of control variables (we cannot run panel regression 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 5.1 percentage points in 1871 (from a baseline of 24.5 percent), by 5.8 percentage points in 1895 (from a baseline of 28.9 percent), and by 5.2 percentage points in 1900 (from a baseline of 30.9 percent). OLS regressions yield virtually identical results (see Table A-3 in the Appendix for detailed results).

Data for 1871 additionally distinguishes between foreign-born who were born a) in a different municipality in Württemberg, b) in a member state of the German Customs Union (except Württemberg), and c) abroad. Much of the differences in the share of the foreign-born between winner and runner-up municipalities is driven by migration within Württemberg: the (unconditional) population share of foreign-born who are originally from another municipality in Württemberg is 27.8 percent in winning municipalities but only 23.0 percent in losing municipalities. Population growth in winner municipalities was thus–at least in part–due to relocation within Württemberg. Migration across state borders was much less important: Only 0.4 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 more than twice as high in winning municipalities (0.5 percent) than in losing municipalities (0.2 percent)–and such differences might have become more important over time.[26]

We also use annual data on the number of birth and deceased between 1871 and 1910 to estimate OLS and semi-parametric models with the average annual birth rate, death rate and 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 A-3 in the Appendix for detailed results). We thus conclude that the positive effect of railways on population growth in winner municipalities is mainly driven by immigration, in line with spatial equilibrium models.

5.2 Income, wages and housing values

We next analyse the effect of railway access on income, wages, and housing values. In spatial equilibrium, population changes–as studied in the previous sub-section–mirror regional wage

[26]Data 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 1.5 percent in winning municipalities and 0.8 percent in losing municipalities.

and income differences and trigger adjustments in the housing market. Our analysis assesses these predictions. We rely on cross-sectional models only, as we lack comparable data for the pre-treatment period. However, the results reported in Section 5.1 suggest that comparing winner and runner-up municipality might well yield consistent estimates even in cross-sectional models.

Day labourer wage. We first consider the average daily wage of day labourers in 1884, 1898, and 1909, distinguishing between females and males. The cross-sectional results in Table 3 show a sizeable and statistically significant effect of railway access on female and male wages. IPWRA estimates indicate that railway access increased female day labourer wages by 7.594 (s.e. of 2.368), 10.055 (s.e. of 2.329), and 8.143 *Pfennig*[27] (s.e. of 2.552) in 1884, 1898, and 1909, respectively. This corresponds to an increase of 6.8, 8.6, and 4.9 percent, respectively, relative to the control group average. The (relative) effect is somewhat lower for male day labourers, ranging from 3.2 to 5.2 percent. These results suggest that railway-induced industrialisation benefited also the working class (see Leonard and Ljungberg, 2010, for a discussion of living standards in Europe in 1870-1914).

The higher treatment effect for females translates into a statistically significant lower gender wage gap in winning municipalities of 2.3 percentage points in 1884 and 1898 (from a baseline of 32.8 percent). The effect is less visible for 1909 (see Table A-4 in the Appendix for detailed results). The lower gender wage gap in winning municipalities is consistent with the idea that falling transport costs induced mechanisation, 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 municipalities by 41.2 and 43.8 *Mark* or by 11.3 and 12.0 percent relative to the control mean (see Column (7) of Table 3). The relative increase in taxable income is thus comparable to the increase in day labourer wages. The OLS results are only slightly larger. Overall, our findings suggest that railway access boosted regional nominal income, which might then have triggered population inflows.

[27] *Pfennig* is the subunit of the *German Goldmark*, or just *Mark*, with 1 *Mark* = 100 *Pfennig*.

Table 3: The effect of railway access on day labourer wages, taxable income, and building values

	Day labourer wage (*Pfennig*)						Taxable income (*Mark*)	Building value (*Mark*)	Fire insurance value (*Mark*)
	Female			Male					
	1884	1898	1909	1884	1898	1909	1907	1907	1908
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: IPW									
Treatment effect	6.992***	9.754***	8.062***	5.246*	8.893***	9.035**	41.223***	1174.4***	1414.3***
	(2.200)	(2.265)	(2.600)	(2.779)	(3.056)	(4.475)	(14.409)	(246.1)	(317.4)
Panel B: IPWRA									
Treatment effect	7.594***	10.055***	8.143***	5.545*	9.122***	9.039**	43.819***	1189.0***	1408.8***
	(2.368)	(2.329)	(2.552)	(2.841)	(3.069)	(4.466)	(13.952)	(250.8)	(324.3)
Panel C: OLS									
Treatment effect	7.792***	11.557***	9.776***	5.986**	11.128***	11.649***	50.376***	1367.7***	1560.8***
	(1.985)	(2.250)	(2.591)	(2.537)	(3.038)	(4.303)	(13.807)	(277.0)	(360.9)
Observations	153	154	154	154	154	154	154	154	154
Control mean	111.43	116.72	167.22	165.96	174.34	254.14	366.11	3150.4	4391.6

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 labourers 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, and case dummies. Robust standard errors are in parentheses. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

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 show the treatment effect on the average market value of buildings in 1907 and the fire insurance value in 1908, respectively. Railway access increases the average building value by 1189.0 *Mark* (s.e. of 250.8) or 37.7 percent (IPWRA estimate in Panel B, Column (8)). The increase in insurance value per building in 1908 is 32.2 percent and thus of comparable size (Column (9)).

5.3 Sectoral employment, specialisation and establishment size

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 this development.

Sectoral employment. We first study the effect of railway access on sectoral employment, distinguishing between full-time employees in industry, trade and transport, and agriculture (per 100 persons of the population). Panels A and B of Table 4 presents results from semi-parametric IPW and IPWRA estimations and Panel C from panel regressions.[28] The post-treatment period is 1895 in Columns (1) to (3) and 1907 in Columns (4) to (6).

The results of the semi-parametric IPW estimations in Panel A suggest that railway access had a sizeable positive effect on employment in industry (Columns (1) and (4)) and trade and transport (Columns (2) and (5)). Railway access increased industry employment in winning municipalities by 2.787 employees per 100 persons (s.e. of 1.354) or 18.8 percent relative to the 1895 average in losing municipalities. The corresponding increase in trade and transport is 0.946 employees (s.e. of 0.256) or 40.6 percent. The point estimates remain almost unchanged if we consider sectoral employment in 1907. Obtaining railway access in the first stage of the construction period thus had a long lasting positive effect on employment in industry and trade.

[28]Since we only have employment data for one pre-treatment and one post-treatment period, we cannot test for differences in pre-treatment trends between treatment and control group or add case/county by period fixed effect to our panel regressions in Panel C of Table 4. We thus restrict our subsequent discussion to the comparable set of municipalities in the winners versus runners-up sample. We nevertheless re-do all our analyses for the full sample and report the results in Appendix A.7. The results for the two samples are qualitatively comparable. However, the results for the full sample are typically larger (in absolute magnitude) and more precisely estimated. This is to be expected if railway municipalities were positively selected.

Table 4: The effect of railway access on sectoral employment in industry, trade and agriculture

	1895			1907		
	Industry	Trade	Agriculture	Industry	Trade	Agriculture
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: IPW						
Treatment effect	2.787**	0.946***	-3.359**	2.948**	1.035***	-5.986***
	(1.354)	(0.256)	(1.477)	(1.420)	(0.267)	(1.956)
Panel B: IPWRA						
Treatment effect	2.783**	0.946***	-3.319**	2.926**	1.027***	-5.940***
	(1.317)	(0.262)	(1.436)	(1.393)	(0.273)	(1.887)
Observations	154	154	154	154	154	154
Panel C: Panel estimates						
Treatment effect	2.415*	0.818***	–	2.376	0.841***	–
	(1.413)	(0.243)		(1.553)	(0.277)	
Observations	308	308		308	308	
Control mean	14.81	2.33	23.71	17.47	2.68	26.43

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 (4)), trade and transport (Columns (2) and (5)) and agriculture (Columns (3) and (6)) per 100 persons. Columns (1) to (3) focus on sectoral employment in 1895, columns (4) to (6) on employment in 1907. Panels A and B display IPW and IPWRA estimates, respectively, using employment in 1895/1907 as outcome variable. 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 municipality and year fixed effects. The pre-treatment period is 1829. 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) to (3)) and 1907 (Columns (4) to (6)). Robust standard errors are in parentheses. Standard errors in Panel C are clustered at the municipality level. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

The increase in employment in industry and trade came at the expense of agricultural employment (Columns (3) and (6)). The IPW estimate in Column (3), for instance, implies that railway access decreased the number of full time employees in agriculture by 3.359 employees per 100 persons (s.e. of 1.477) or 14.2 percent relative to the control group average in 1895. This negative effect increases to 5.986 employees or 22.6 percent in 1907.[29] The results of the IPWRA estimations in Panel B are almost identical.

Panel C shows the corresponding results from differences-in-differences regressions with municipality fixed effects. The panel regressions for industry and trade generally confirm

[29]The IPW/IPWRA estimates for 1907 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 combined increase in industry and trade. However, the regressions abstract from public sector employment. Unreported IPW/IPWRA estimations show that the total employment effect of railway access is statistically indistinguishable from zero.

the cross-sectional estimates (but are slightly smaller). We cannot run panel regressions for agriculture, as we lack data on agricultural employment in the pre-treatment period.

Industry sectors. Our unusually disaggregated data from the *Gewerbestatistik* allow us to further study the effect of railways on different sectors within industry. In particular, we distinguish between employment in mining and the basic metal industry, capital goods industry, construction, consumer goods industry and food industry (per 100 persons).

Table 5 shows results from IPW (Panel A) and IPWRA (Panel B), and differences-in-differences (Panel C) models. Column (1) suggests that railway access had, if anything, only small positive effects on employment in mining and the basic metals industry. This results might be surprising, as railways are widely believed to have significantly increased the German coal, iron 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-wide pig iron and steel production plummeted from 2.6 percent in 1850 to 0.2 percent around 1895 (von Hippel, 1992).

Railway access had a sizeable positive effect on employment in the capital goods industry (Column (2) of Table 5). However, the effect is only statistically significant in the IPW and IPWRA estimations. The IPW estimate implies that railway access increased employment by 0.885 employees per 100 persons (s.e. of 0.416) or 42.3 percent compared to losing municipalities. The capital good industry includes, for instance, the machine industry, an important driver of Germany's industrialisation. Machine industry employment was indeed considerably larger in winning municipalities (0.279) than in losing municipalities (0.112) in 1895.[30]

We also find a positive–and statistically significant–effect of railway access on employment in construction (see Column (3)). Point estimates are very similar across estimations. The IPW estimates in Panel A suggests that railway access increased construction employment in winning municipalities by 0.624 employees per 100 person or 42.4 percent relative to losing municipalities. This effect is driven by construction and construction maintenance firms that were engaged in the construction of buildings, railways, roads and waterways.

[30]The by far biggest manufacturer of locomotives and railway wagons in Württemberg was the *Maschinenfabrik Esslingen*, founded in 1846 in Esslingen. However, Esslingen is a railway node and thus excluded from the sample.

Table 5: The effect of railway access on industrial employment, establishment size and specialisation

	Employment						
	Mining & basic metals	Capital goods	Con-struction	Consumer goods	Food	Estab-lishment size (logs)	Spec-ialisation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: IPW							
Treatment effect	0.331	0.885**	0.624*	0.789	-0.127	0.154	-0.032
	(0.511)	(0.416)	(0.327)	(1.358)	(0.218)	(0.095)	(0.021)
Panel B: IPWRA							
Treatment effect	0.325	0.872**	0.625*	0.858	-0.153	0.155*	-0.032
	(0.506)	(0.419)	(0.325)	(1.307)	(0.210)	(0.092)	(0.021)
Observations	154	154	154	154	154	154	154
Panel C: Panel estimates							
Treatment effect	0.218	0.539	0.663*	0.740	-0.358	0.125	-0.013
	(0.517)	(0.481)	(0.341)	(1.258)	(0.264)	(0.103)	(0.020)
Observations	308	308	308	308	308	307	307
Control mean	1.208	2.091	1.473	6.639	2.108	0.836	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 person in industry (Columns (1)-(5)), establishment size in industry (Column (6)), and specialisation in industry (Column (7)). We distinguish between employment in the basic metal industry (Column (1)), capital goods industry (Column (2)), construction (Column (3)), consumer goods industry (Column (4)), and food processing industry (Column (5)) per 100 persons. Specialisation is measured by the Hirschman-Herfindahl-Index (with $\alpha = 2$) and establishment size is the average number of persons employed in a main plant (*Hauptbetrieb*). 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 municipality 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 municipality level. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

We do not find statistically significant effects of railway access on employment in the consumer goods industry (Column 4) and the food industry (Column 5). The positive coefficient on the consumer good industry is driven by the textile industry, Württemberg's most important industry at the dawn of the Industrial Revolution. Average 1895 employment in the textile sector (excluding fibre production) was 3.173 employees in winning municipalities but only 1.913 employees in losing municipalities, whereas textile employment was virtually identical in the pre-treatment period (1.913 and 1.966 in winning and losing municipalities, respectively). The negative coefficient estimate on food industry employment is driven by slower employment growth in vegetable food production among winning municipalities.[31]

[31] In losing municipalities, employment in the production of vegetable foods increased from 0.674 employees per 100 person in 1829 to 1.240 employees in 1895. The increase was much modest in winning municipalities where employment in vegetable food production increased from 0.642 to 0.894 employees.

Establishment size and specialisation. In the last two columns of Table 5, we analyse the effect of railway access on plant size and specialisation (within industry). Falling transport costs are widely believed to have increased optimal establishment size by integrating markets and expanding market size. The ensuing competitive pressures, so the argument, forced firms to increase productivity through the division of labour and mechanisation–and thus promoted the rise of factories (Atack et al., 2011). In line with this argument, our estimates suggest that railway access increased establishment size by between 0.125 and 0.155 log points compared to losing municipalities (Column (6) of Table 5). Estimates are, however, relatively imprecise. We also find evidence that the degree of specialisation is lower in winning than in losing municipalities (Column (7)). This is in line with the literature that finds a U-shape in specialisation in the development process (Cadot et al., 2011; Imbs and Wacziarg, 2003). However, the estimates are imprecise and not statistically significant at conventional levels.

5.4 Localised 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 reorganise existing economic activities across space. Distinguishing between growth and re-organisation requires additional identifying assumption in reduced-form analyses. One potential assumption is that railway access has no effect on far-away municipalities (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 reorganisation. Berger and Enflo (2017) and Büchel and Kyburz (2019) apply this idea to test for re-organisation 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 localised displacement effects in our finely grained spatial data. Appendix Section A.5 discusses these results in more detail.

6 Conclusion

This paper of the effect of railways on growth and industrialisation in Württemberg has established four main findings. First, railway access fosters agglomeration and increased municipal population growth by 0.23 log points in 1855-1910, corresponding to an increase in *annual* population growth of 0.4 percentage points. Our estimate is remarkably similar to recent findings for rural Swiss municipalities in the 19th century (Büchel and Kyburz, 2019), but considerably smaller than existing estimates for Prussian towns in 1838-1871 (Hornung, 2015). Second, railway access boosted day labourer wages by up to 10 percent and reduced the gender wage gap. It also increased annual taxable income per capita in 1907 by between 11.3 and 12.0 percent. Third, railway access increases housing values by about one third. These findings are consistent with the predictions of economic geography models, in which wage increases following infrastructure improvements are arbitraged away by population inflows.

Fourth, railways accelerated the transition from agriculture to industry, increasing industrial employment in municipalities that gained access to the railway in 1845-54 by between 16.3 percent and 18.8 percent until 1895. Effects are particularly large for employment in construction (+42.4 percent) and the capital goods industry (+42.3 percent). Finally, railway access increased establishment size and may have had a small negative effect on the degree of specialisation in industry.

We reach these findings by comparing winning municipalities that were connected to the railway in 1845-1854 to losing municipalities that were the runners-up choice for a given railway line between two major towns. We show that winning and losing municipalities were comparable before the arrival of the railway, supporting a causal interpretation of our estimates from differences-in-differences and inverse-probability-weighted models. Estimates from local polynopmial regressions tentatively suggest that the positive effects on income, housing values and industrial employment were not driven by localised displacement effects from nearby municipalities.

Overall, we conclude that the construction of a railway network played an important role in Württemberg's transition from a poor agrarian state to an industrial economy. The economic effects of the railway were long-lasting but also took time to materialise. Our results are consistent with the view that rapid improvements in transport infrastructure were a key driver of 19th century industrialisation–and can still be crucial for the development of low- and middle-income countries today.

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A Online Appendix

A.1 Württemberg in the German Empire

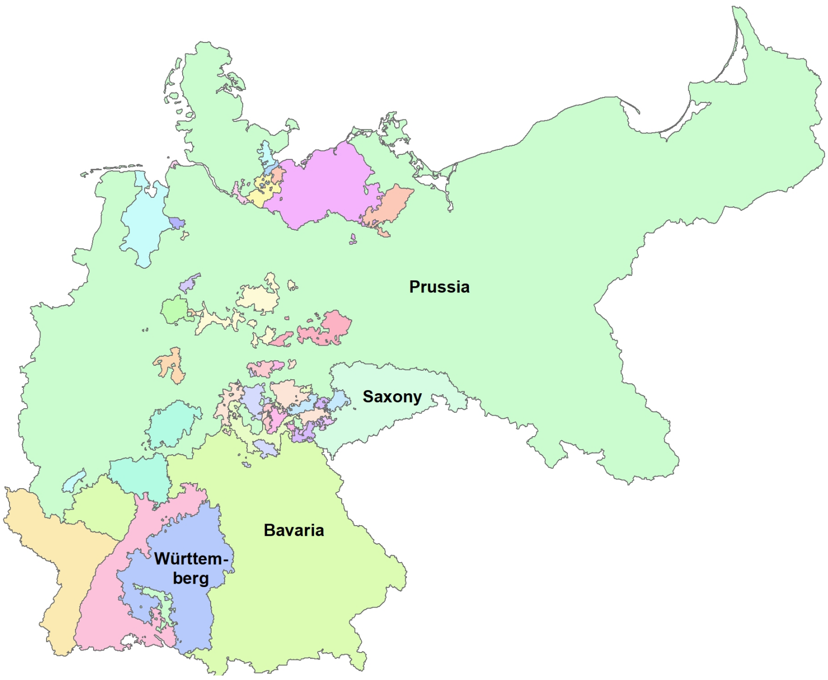

Figure A-1: The German Empire in 1871

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.

A.2 Municipal population growth in 1834-1910

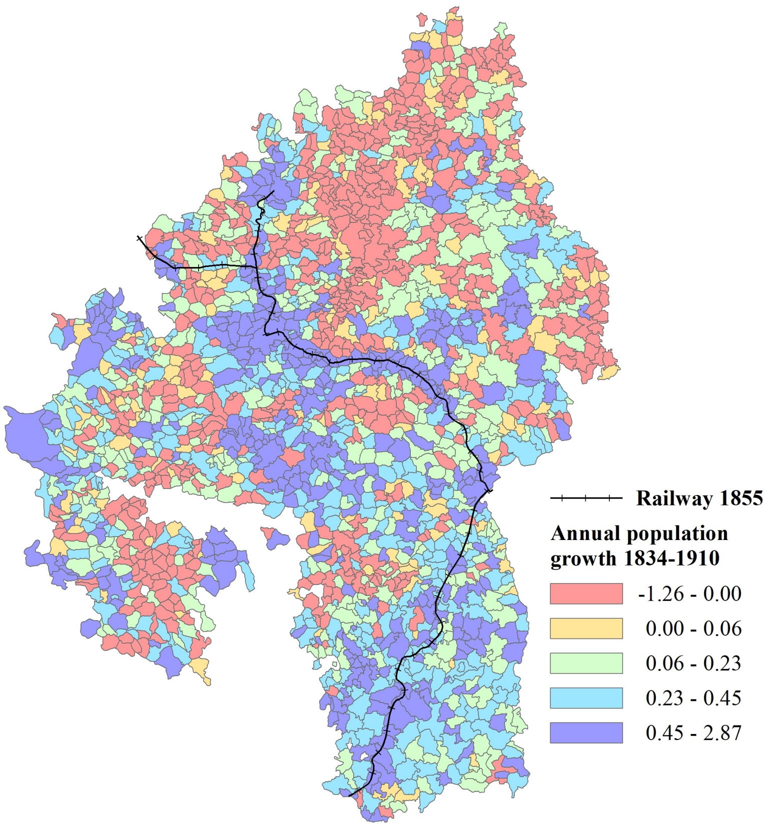

Figure A-2: Average annual population growth in 1834-1910

Notes: The figure shows the average annual population growth in municipalities 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.

A.3 The planning process for the central line

This section outlines the planing 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 A-3 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 of 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 von 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 A-3.

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 A-3 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–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.

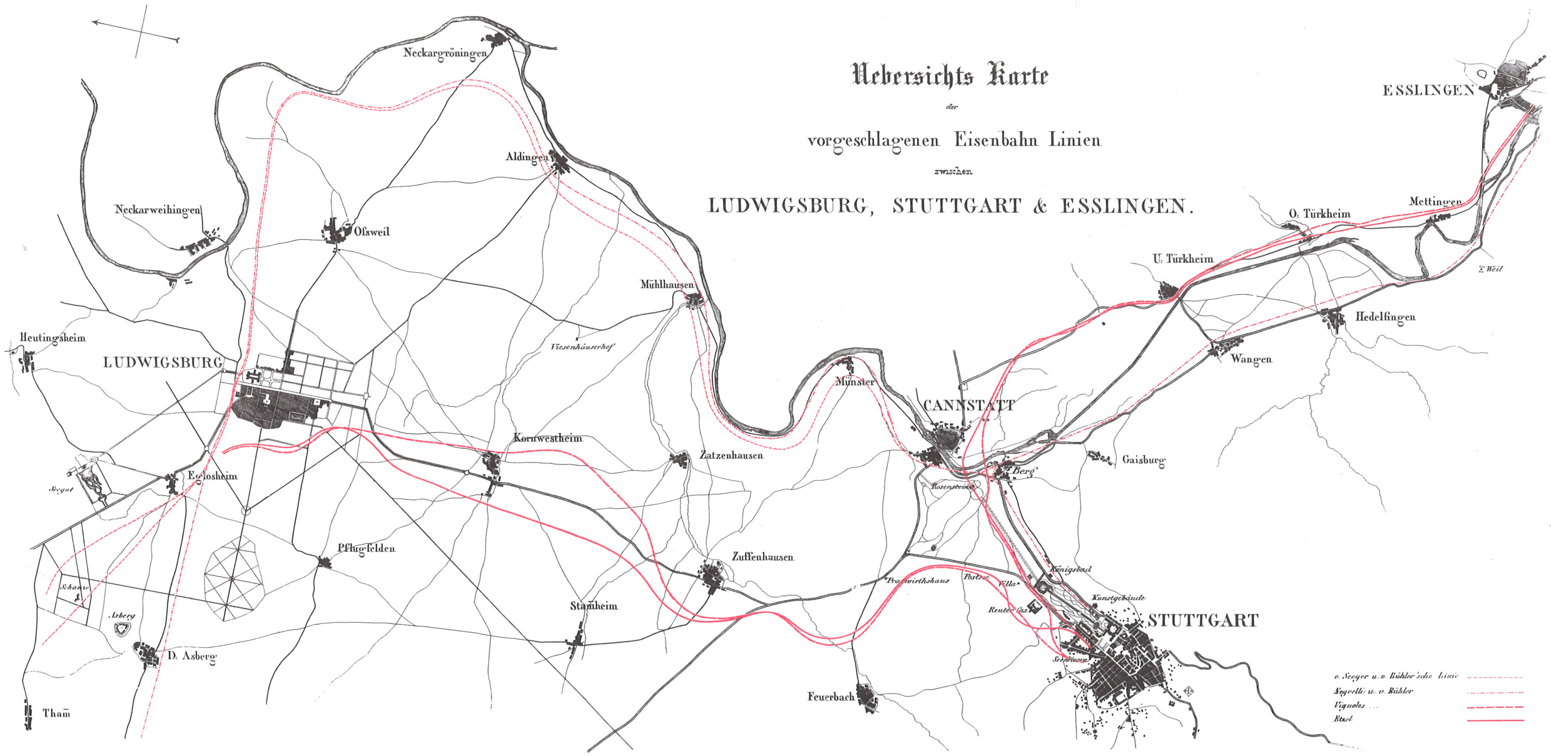

Figure A-3: Route proposals for the central line (*Zentralbahn*) in 1844

Notes: The figure shows the four different routes of the central line proposed during the planning process by von Bühler and von Seeger in 1839 (thin red dashed line), Negrelli 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).

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 A-1 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 A-1: Comparison of alternatives for the central line by Klein in 1844

Route	Length	Length of		Smallest	Height	Maximum	Weighted
		Straight lines	Curves	curve radius	difference	gradient	length
	(feet)	(feet)	(feet)	(feet)	(feet)		(feet)
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
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 penality for curves and gradient. Distances in Württemberg feet, with 1,000 feet = 286.49 metres.
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 A-1 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 Cannstatt and Untertürkheim opened in October 1845, and the central line was completed in October 1846.

Our empirical analysis defines Obertürkheim and Untertürkheim as winning municipalities on the line Stuttgart-Esslingen and Feuerbach, Kornwestheim, Zuffenhausen as winning municipalities on the line Stuttgart-Ludwigsburg. These municipalities 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 municipalities are those that would have been connected to the railway under Bühler and von Seeger's plans but not under Etzel's (see Table A-2 for a list of winning and losing municipalities by railway line).

A.4 List of winning and losing municipalities

Table A-2 shows the list of winning and losing municipalities by case and railway line. We exclude railway nodes and municipalities that would have been connected to the railway under all alternative proposals from the list.

Table A-2: Winning and losing municipalities by case

Case (1)	Line (2)	Winning municipalities (3)	Losing municipalities (4)
1	Stuttgart - Ulm	Altbach, Amstetten, Beimerstetten, Ebersbach an der Fils, Faurndau, Göppingen, Großeislingen, Jungingen, Kleinsüßen, Lonsee, Oberesslingen, Plochingen, 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, Mögglingen, 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, Untertürkheim	Hedelfingen, Wangen
3	Bad Cannstatt - Ludwigsburg	Feuerbach, Kornwestheim, Zuffenhausen	Aldingen am Neckar, Mühlhausen, Münster, Neckargröningen, Neckarrems

Continued on next page

Table A-2 – *Continued from previous page*

Case (1)	Line (2)	Winning municipalities (3)	Losing municipalities (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

A.5 Localised displacement

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

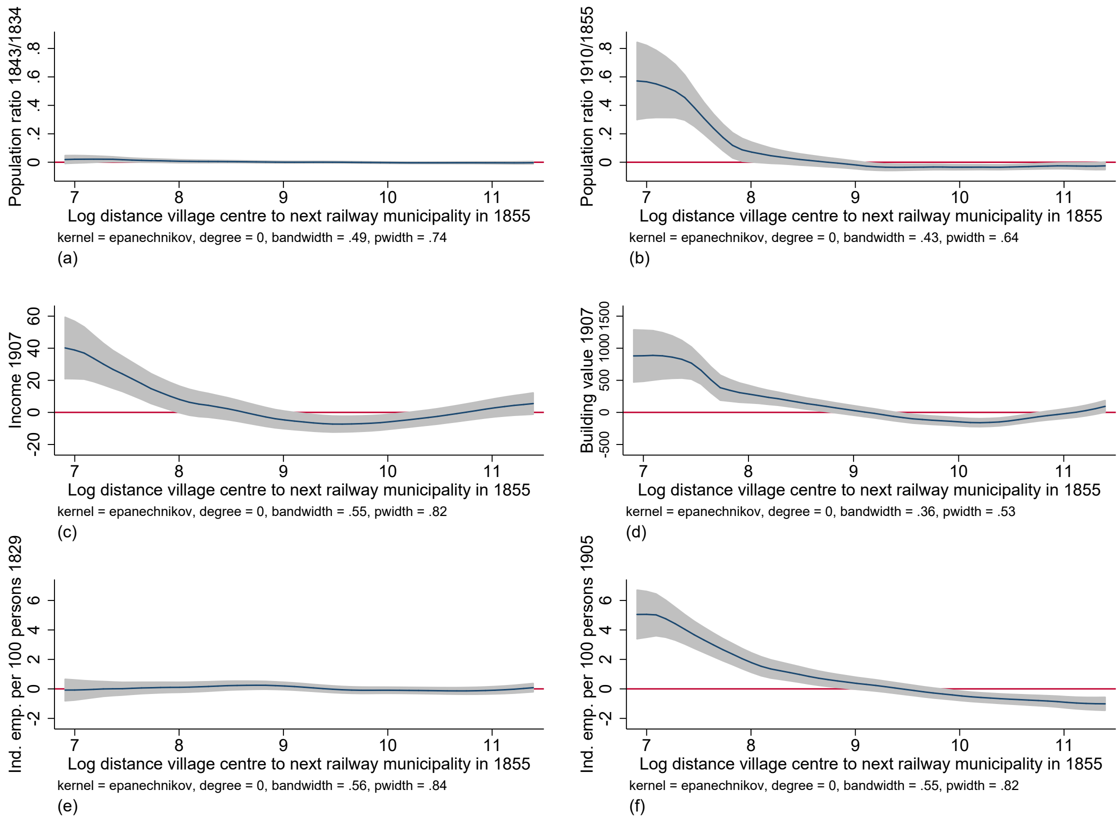

Figure A-4: Polynomial estimates, full sample

Notes: Each graph show smooth values with 95 percent confidence band from kernel-weighted local polynomial regression of outcome residuals on log distance of municipality centroids to the nearest railway municipality in 1855. We add 1000 metres to all distance 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, elevation, dummies for access to a navigable river in 1845 and to a road in 1848, and district dummies as explanatory variables.

Figure A-4 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, 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 municipalities in 1855.

In line with our empirical analysis, Graph (b) of Figure A-4 shows that population growth in 1855-1910 was considerably stronger in municipalities close to the railway than in those further away. However, population growth in municipalities with railway access did not come only–or even predominantly–at the expense of nearby municipalities. This finding is in contrast to Büchel and Kyburz (2019) who 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 municipalities. Overall, we find little evidence for localised displacement effects.

We caution that the cross-sectional regressions in Figure A-4 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 5.1 suggests that immigration from within Württemberg was important for the positive effect of railways on population growth in winner municipalities. In this context, Graph (b) of Figure A-4 only clarifies that the relocation of population within Würrtemberg did not come solely at the expense of municipalities in the immediate vicinity of the railway.

A.6 Additional results for the winner vs. runners-up sample

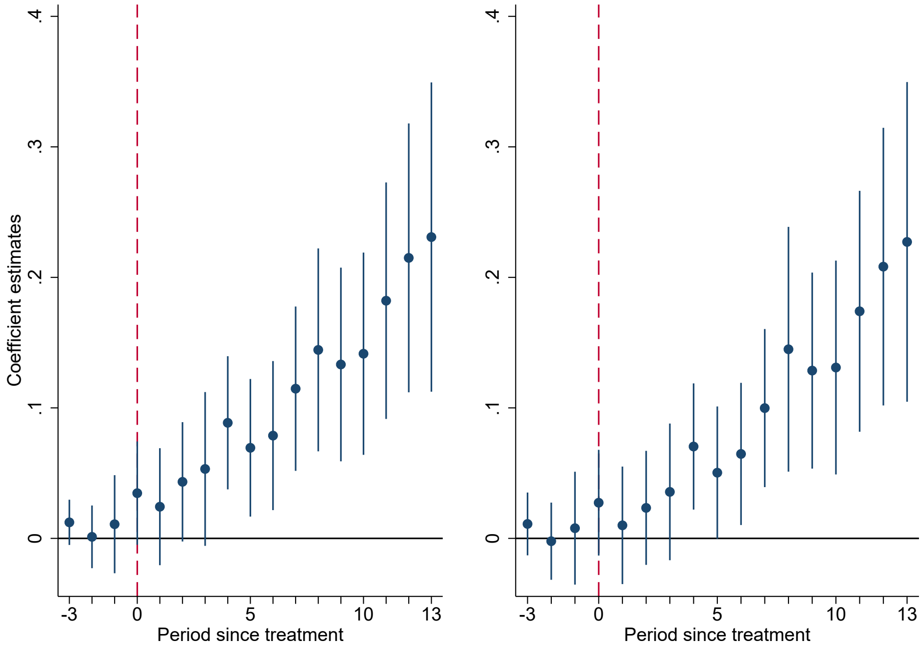

Figure A-5: Event study and cross sectional IPWRA estimates

Notes: The graph depicts differences in log population between winner and runner-up municipalities. The left panel shows Figure 3, i.e., the difference in log population for pre- and post-treatment periods, as estimated in an event study regression (see Section 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 τ = -3,-2,...,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 indicate the treatment period.

Table A-3: The effect of railway access on the share of foreign-born and the rate of natural population increase

	Share of foreign born			Fertility rate	Mortality rate	Rate of nat. increase
	1871	1895	1900	1871-1910	1871-1910	1871-1910
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: IPW						
Treatment effect	0.051***	0.058***	0.052***	0.485	0.221	0.264
	(0.015)	(0.015)	(0.017)	(0.835)	(0.803)	(0.823)
Panel B: IPWRA						
Treatment effect	0.050***	0.058***	0.052***	0.732	0.299	0.433
	(0.015)	(0.015)	(0.017)	(0.826)	(0.820)	(0.819)
Panel C: OLS						
Treatment effect	0.050***	0.060***	0.057***	0.969	-0.028	0.997
	(0.014)	(0.014)	(0.016)	(0.798)	(0.713)	(0.691)
Observations	154	154	154	149	149	149
Control mean	0.245	0.289	0.309	37.03	25.93	11.10

Notes: The table shows regression estimates of the effect of railway access in 1845-54 on the share of inhabitants born outside a municipality 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.

Table A-4: The effect of railway access on the gender wage gap

	Gender wage gap		
	1884	1898	1909
	(1)	(2)	(3)
Panel A: IPW			
Treatment effect	-2.106**	-2.017**	-0.820
	(1.038)	(0.963)	(0.581)
Panel B: IPWRA			
Treatment effect	-2.316**	-2.099**	-0.853
	(1.114)	(0.972)	(0.602)
Panel C: OLS			
Treatment effect	-2.218**	-2.156**	-0.794
	(1.051)	(0.965)	(0.560)
Observations	153	154	154
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 labourers 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.

A.7 Additional results for the full sample

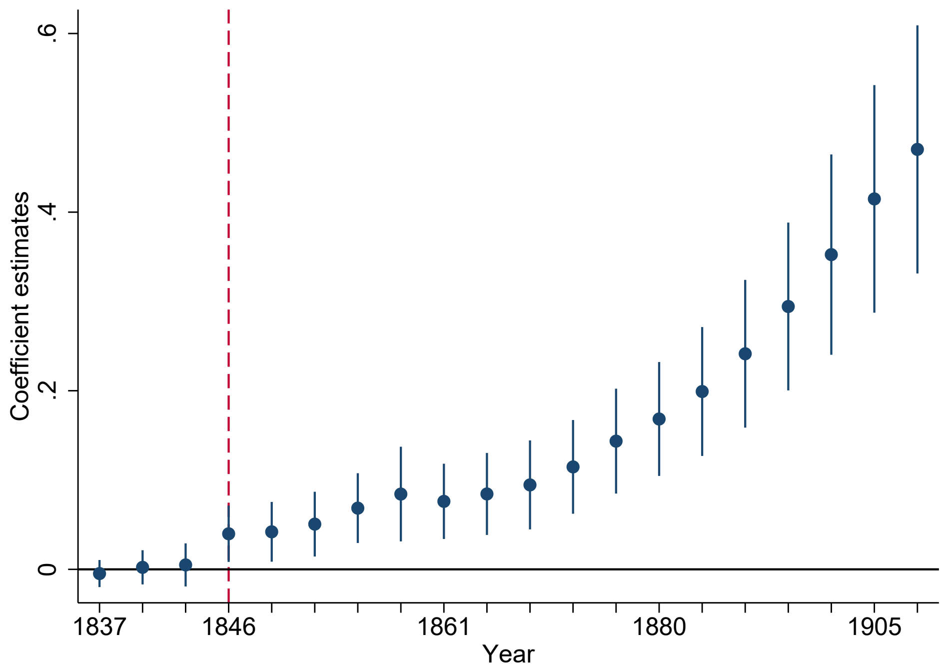

Figure A-6: Event study estimates, full sample

Notes: The graph depicts differences in log population between railway and non-railway municipalities in 1837-1910, as estimated in an event study regression (see Section 4.2 and Footnote 23 for details). 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 indicate the treatment period.

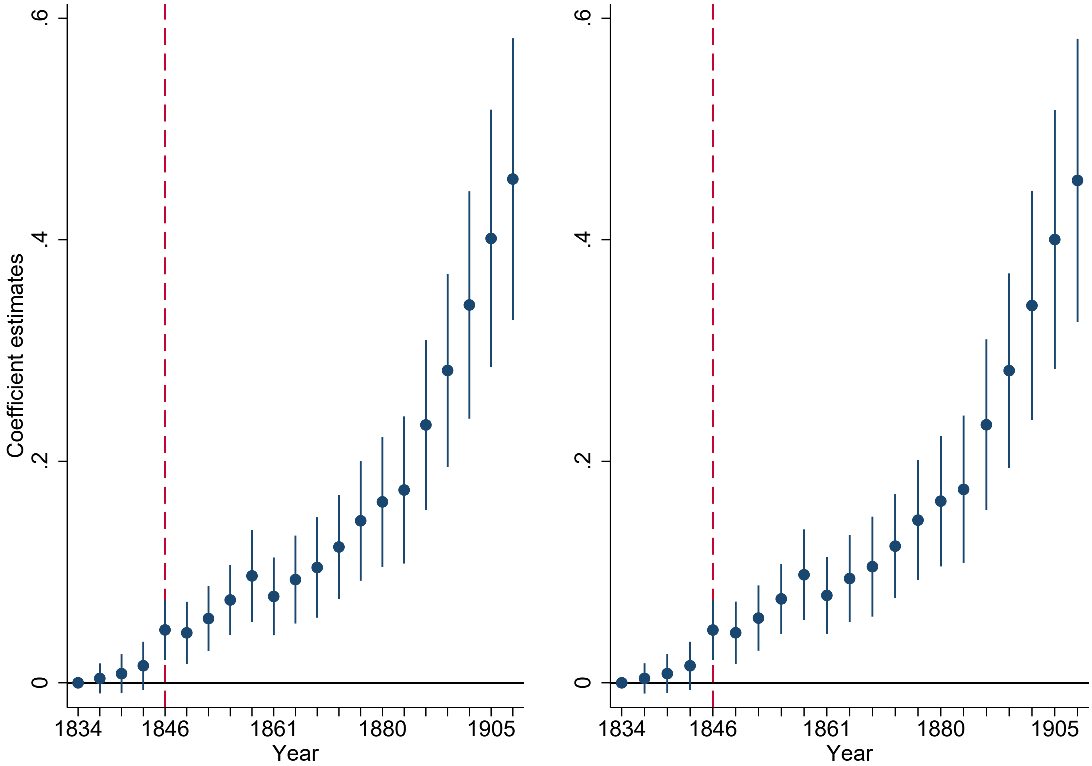

Figure A-7: Semi-parametric estimates of the over time effect of railway access on log population, full sample

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 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.

Table A-5: The effect of railway access on sectoral employment in industry, trade and agriculture, full sample

	1895			1907		
	Industry	Trade	Agriculture	Industry	Trade	Agriculture
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: IPW						
Treatment effect	5.362***	1.048***	-6.404***	6.728***	1.132***	-9.647***
	(0.951)	(0.235)	(0.995)	(0.944)	(0.252)	(1.326)
Panel B: IPWRA						
Treatment effect	5.269***	1.034***	-6.279***	6.615***	1.115***	-9.476***
	(0.971)	(0.229)	(1.029)	(0.973)	(0.247)	(1.337)
Observations	1,845	1,845	1,845	1,842	1,842	1,842
Panel C: Panel estimates						
Treatment effect	5.749***	1.253***	–	7.230***	1.415***	–
	(1.076)	(0.197)		(1.100)	(0.228)	
Observations	3,690	3,690		3,687	3,687	
Control mean	9.629	1.524	31.498	11.058	1.756	36.841

Notes: The table shows regression estimates of the effect of railway access in 1845-54 on the number of full time employees in industry (Columns (1) and (4)), trade and transport (Columns (2) and (5)) and agriculture (Columns (3) and (6)) per 100 persons. Columns (1) to (3) focus on sectoral employment in 1895, Columns (4) to (6) on employment in 1907. Panels A and B display IPW and IPWRA estimates, respectively, using employment in 1895/1907 as outcome variable. Regressions in Panels A and B include as control variables log population and log population density in 1834, and 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 municipality and year fixed effects. The pre-treatment period is 1829. 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) to (3)) and 1907 (Columns (4) to (6)). Robust standard errors are in parentheses. Standard errors in Panel C are clustered at the municipality level. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.

Table A-6: The effect of railway access on industrial employment, establishment size and specialisation, full sample

	Employment						
	Mining & basic metals (1)	Capital goods (2)	Con-struction (3)	Consumer goods (4)	Food (5)	Estab-lishment size (logs) (6)	Spec-ialisation (7)
Panel A: IPW							
Treatment effect	0.896**	1.030***	0.748***	2.211**	0.243	0.372***	0.004
	(0.407)	(0.353)	(0.270)	(1.044)	(0.173)	(0.070)	(0.014)
Panel B: IPWRA							
Treatment effect	0.890**	1.026***	0.744***	2.159**	0.237	0.369***	0.004
	(0.406)	(0.353)	(0.270)	(1.046)	(0.171)	(0.070)	(0.014)
Observations	1,845	1,845	1,845	1,845	1,845	1,838	1,838
Panel C: Panel estimates							
Treatment effect	1.090***	1.120***	0.785***	2.572***	0.242	0.411***	0.035**
	(0.404)	(0.392)	(0.290)	(0.973)	(0.173)	(0.082)	(0.014)
Observations	3,690	3,690	3,690	3,690	3,690	3,497	3,497
Control mean	0.346	1.311	1.103	3.886	1.097	0.478	0.171

Notes: The table shows regression estimates of the effect of railway access in 1845-54 on the number of full time employees per 100 person in industry (Columns (1)-(5)), establishment size in industry (Column (6)), and specialisation in industry (Column (7)). We distinguish between employment in the basic metal industry (Column (1)), capital goods industry (Column (2)), construction (Column (3)), consumer goods industry (Column (4)), and food processing industry (Column (5)) per 100 persons. Specialisation is measured by the Hirschman-Herfindahl-Index (with $\alpha = 2$) and establishment size is the average number of persons employed in a main plant (*Hauptbetrieb*). 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 in 1834, 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 municipality 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 municipality level. ***, **, and * denote statistical significance at the 1, 5, and 10 percent level, respectively.