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Railroads and Growth in Prussia

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Railroads and Growth in Prussia *

Abstract

We study the effect of railroad access on urban population growth. Using GIS techniques, we match triennial population data for roughly 1,000 cities in nineteenth-century Prussia to georeferenced maps of the German railroad network. We find positive short- and long-term effects of having a station on urban growth for different periods during 1840–1871. Causal effects of (potentially endogenous) railroad access on city growth are identified using propensity score matching, instrumental variables, and fixed-effects estimation techniques. Our instrument identifies exogenous variation in railroad access by constructing straight-line corridors between nodes. Counterfactual models using pre-railroad growth yield no evidence to support the hypothesis that railroads appeared as a consequence of a previous growth spurt.

JEL Classification: O18, O33, N73

Keywords: Railroads, Technological Diffusion, Economic Growth, Population Growth

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1 Introduction

The statement that technological change is one of the driving forces of economic growth is beyond dispute. Railroads, as one of the most important innovations of the nineteenth century, have been repeatedly discussed as being *the* technology that shaped growth during the Industrial Revolution (seminal work by [Fishlow, 1965](#), [Fogel, 1962](#), and [Rostow, 1962](#)). Using the concept of social savings, the effect of railroads on aggregate growth has been comparatively calculated for many countries that were early adopters of railroad technology and ranges from 4% to 25% of the GNP, depending on the country and the period under consideration (see [O’Brien, 1983](#)).¹

In addition to its macroeconomic effect, technology adoption can be crucial in generating localized comparative advantages and regional economic growth. However, a major challenge in the literature is to establish causality in this relationship. Recent work by [Atack et al. \(2010\)](#) and [Banerjee, Duflo, and Qian \(2012\)](#) attempt to answer the problem raised by [Fishlow \(1965\)](#): Did railroads have a substantial impact on economic growth or did they appear as a consequence of growth? The overall results still seem to be ambiguous.²

This paper sheds light on the direction of causality between railroad adoption and economic growth in a range of approaches using highly detailed city-level data from the historical German state of Prussia. Using an extensive dataset for all 978 Prussian cities, we provide evidence that access to this new technology massively influenced city growth rates—a widely used proxy for regional economic growth.³ Following the notion that “city sizes grow with improvements in technology” ([Henderson, 2005](#), p. 1577), we estimate that railroad technology induced an additional annual growth ranging between 1 and 2 percentage points for adopting cities compared to non-adopting cities. The size

¹For Germany, the relationship between railroads and economic growth has been primarily analyzed by calculating the investment induced by railroad construction and the backward linkages to other industries ([Fremdling, 1977, 1985](#)). [Pierenkemper and Tilly \(2004](#), p. 63) for example, note that the demand for iron and coal induced by railroad construction was the engine of the Industrial Revolution in Germany.

²There is some consensus in the literature on German railroads that the latter is most likely ([Hahn, 2005](#), p. 26; [Fremdling, 1983](#), p. 122). However, the question of whether regions grew comparatively faster after they gained access to the railroad has not been answered conclusively ([Matzerath, 1996](#), p. XI).

³An expanding body of literature examines the effects of the diffusion of historical innovations on growth—proxied by urban population growth. Such studies use the geographic distribution of an important cultural or technological innovation and analyze its effects on local economic growth. These studies analyze the diffusion of banking in the United States ([Bodenhorn and Cuberes, 2010](#)), the diffusion of Protestantism in Germany ([Cantoni, 2010](#)), the diffusion of the printing press in Europe ([Dittmar, 2011](#)), and the diffusion of potato cultivation in Europe ([Numm and Qian, 2011](#)).

of this effect remains very stable across a range of different periods and specifications. Estimating counterfactual models of railroad access on growth prior to access yields no evidence of a reversed causality.

Using a geographic information system (GIS), we geo-reference historical maps of the German railroad system as well as the location of all Prussian cities to obtain information on railroad diffusion over time. This allows us to test the relationship cross-sectionally as well as in a panel setting. The period under consideration covers the beginning of railroad construction in Prussia in 1838 until the main railroad framework was laid out during the mid-1860s.⁴

The paper is structured to gradually build up specifications from a cross-sectional approach using ordinary least squares (OLS), instrumental variables (IV), and propensity score matching (PSM) to a fixed-effects panel approach using OLS and IV, thus reflecting the hierarchy between the different specifications. These approaches successively address issues of endogeneity and unobserved heterogeneity to estimate the causal effect of railroad access on growth.

The IV approach rests on the assumption that until the mid-1860s, Prussian railroads were built to connect important cities.⁵ Since construction costs were high, lines were mostly built linearly. Consequently, cities located on a direct line between these important cities were able to gain access to the railroad by chance, whereas cities whose location deviated from the straight line could gain access only for reasons potentially endogenous to the city's growth. By using a straight line to connect terminal and junction stations (nodes), we can construct a variable indicating the potential for railroad adoption—being located within a straight-line corridor—that we use to instrument actual railroad access.⁶

The instrument developed in this paper proves to be powerful for cross-sectional as well as for fixed-effects panel estimations. Whenever new railroad lines are built, new straight lines can be drawn between nodes, effectively creating exogenous variation across cities as well as over time. Both approaches return significant positive effects of railroad access over a range of different periods. As such, this paper seems to be the first successful

⁴This corresponds to the prevailing periodization of the German railroad system based on [Sombart \(1921, p. 239\)](#).

⁵See Sections 2 and 4.3 below for more information.

⁶The use of straight-line instrumental variables is well established in the literature on transportation infrastructure and was most prominently started in [Michaels \(2008\)](#).

attempt to develop a time-varying straight-line instrument to allow for causal inferences regarding the effects of transportation infrastructure using panel data with fixed effects.

We further apply matching techniques to account for city-level heterogeneity in pre-railroad development. This allows for estimations in samples of cities that are highly comparable and ideally differ only in their access to a railroad line. The previous findings are confirmed when applying our instrumental-variable estimation strategy to these matched samples.

Finally, we use the fixed-effects panel setting to estimate event study specifications. Results from such specifications credibly show the absence of differences in pre-railroad growth trends between railroad and non-railroad cities as well as a sharp upward trend after railroad access has been established.

A recently growing literature analyzes the effects of transport infrastructure on a range of outcomes. Authors address the consequences of establishing railroad systems for a number of countries from very different points of view.⁷ Research analyzing aspects of market integration, increasing trade flows, and price convergence usually finds evidence of large gains from increasing trade due to railroad network expansion (see [Donaldson, 2014](#); [Donaldson and Hornbeck, 2013](#); [Keller and Shiue, 2008, 2013](#)). Focusing on aspects of development and growth such as population growth, urbanization, per-capita GDP growth, income inequality, and firm location yields mixed results in terms of size or significance of the effects (see [Atack et al., 2010](#); [Atack, Haines, and Margo, 2011](#); [Banerjee, Duflo, and Qian, 2012](#)). Many of these works predominantly focus on analyzing the consequences of establishing railroads for the agricultural sector or for agricultural societies with limited factor mobility. As railroads are often strongly connected to the industrial sector, the Prussian environment seems suitable for analyzing the consequences of railroads for industrial development. We will discuss our findings in comparison to the literature toward the end of this paper.

There may be many, non-exclusive channels through which railroads might affect the economy and our data allow us to shed light on some of them. [Atack, Haines, and](#)

⁷This literature is also closely related to research in urban economics which analyzes the effects of interstate highways on outcomes such as suburbanization, the composition of industrial activity, or the demand for skill (see [Baum-Snow, 2007](#); [Duranton and Turner, 2012](#); [Michaels, 2008](#)).

Margo (2011) argue that railroads increase competition among firms due to their role in increasing market size. Consequently, firms attempt to increase productivity through the division of labor which in turn leads to an increase in establishment size. A part of this hypothesis can actually be tested using Prussian city-level factory data. We find that the average firm size is larger in cities that are connected to the railroad network than in unconnected cities. Furthermore, we do not find evidence that railroads increase the number of factories located in a city. Thus railroads seem to have a causal effect on industrial development at the intensive margin in the form of increasing returns to scale rather than at the extensive margin. Additional results suggest that railroads induce population growth by increasing migration to urban centers while fertility remains unchanged.

The remainder of the paper is structured as follows. Section 2 provides the historical background of the railroad network expansion and urbanization patterns in Prussia. Section 3 introduces and describes the data used for the empirical analysis. Section 4 addresses endogeneity issues and presents results from cross-sectional and fixed-effects panel data using OLS, PSM, and IV estimation techniques, reflecting the hierarchy between the different specifications. Section 5 fits our findings into the recent literature and discusses possible mechanisms and remaining issues. Section 6 concludes.

2 Patterns of Railroad Expansion and Urbanization

This section provides relevant historical information and periodization for Prussian railroad network expansion as well as urbanization.

2.1 The Expansion of the Railroad Network

At the beginning of the 19th century, Germany had an inadequate transportation network when compared to other European countries (Pierenkemper and Tilly, 2004). This was noted by German economist Friedrich List, who published his thoughts about the benefits of a national German railroad network as early as in 1833 (List, 1833). List's blueprint for the railroad system connects all major cities throughout Germany. The simultaneous

founding of the Zollverein (German Customs Union) led to increasing trade between the many states and fiefdoms and the pan-German transport network expansion became desirable (Keller and Shiue, 2008, 2013).⁸

Due to constitutional restrictions, the Prussian government was not able to raise the capital necessary to finance a public railroad network. However, Prussia was intrigued by the British example and in 1838 a law was enacted to allow private parties to build railroads. That same year, the first railroad, linking the capital of Berlin with the residency of Potsdam, was opened. The connection was, like most railroad projects prior to the 1870s, privately owned, financed, and operated. Since the railroad joint-stock companies easily raised capital, the network grew rapidly and by 1845 had overtaken the French system in length (Pierenkemper and Tilly, 2004). Table 1 shows the expansion of the Prussian railroad until 1880.

The government's decision, due to a lack of funds, not to directly construct a railroad network, but to approve and license private railroad enterprises, meant that railroad construction in Prussia lacked a central plan (Fremdling and Knieps, 1997, p. 137), but was built according to the expected profitability of the lines. Consequently, the sparsely populated eastern provinces of Prussia remained unconnected until the government started building the so-called 'Ostbahn' in 1848. The state then built and operated railroads similar to those privately owned (Fremdling and Knieps, 1997, p. 138).

Access costs to railroad-knowledge were quite low and the technology could be considered to be a "free-lunch." A much larger obstacle to railroad adoption was its character of a network technology. Benefits from adoption thus increase with the expected size of the network (Hall, 2005). In the presence of such network externalities, benefits from joining the network increase with the number of adopters.⁹ The diffusion often follows

⁸Prussia abolished internal customs barriers and tariffs in 1818 and initiated the Zollverein that covered most parts of Germany by 1834. Consequently, Prussia could trade freely with most of Germany during the period under analysis in the paper. However, the most direct connection between the eastern and western parts of Prussia runs through the Duchy of Brunswick and the Kingdom of Hanover. Brunswick entered into the Zollverein in 1841 while Hanover entered only in 1854. After establishing a railroad line from Minden to Magedburg, Hanover agreed to impose a low transit rate for goods passing through from one part of Prussia to the other. Passengers, mail, and money were able to transit free of duties.

⁹Technological diffusion usually follows an S-shaped curve, and this is also true for network technologies. In the beginning, only those agents whose expected benefits from adoption are larger than the costs of adoption will adopt. With increasing network size, benefits increase, making it feasible for a larger number of agents to adopt the technology.

the trickle-down pattern observed by [Comin and Hobijn \(2004\)](#), starting from the economic leader and ending with the laggards.

Such a pattern can also be observed in a periodization of the German railroad network expansion following [Sombart \(1921, p. 239\)](#):¹⁰ 1. Preliminary stage until 1845—connecting the major cities; 2. Construction of a framework until 1860—uninterrupted connection of most important cities through trunk lines; 3. Full system of standard-gauge railroads until 1880—completion of a coarse network; 4. Ramification until 1913—railroad supply for smaller towns through branch lines.

2.2 Urban Population Growth

The process of Prussian urbanization can be similarly subdivided into four phases following [Matzerath \(1985\)](#): 1. Transitional phase from 1815 to 1840; 2. Start-up phase until 1871; 3. Actual urbanization phase until World War I; 4. Stabilization phase until the end of World War II. Since industrialization and urbanization are closely related, their phases are similar, too. The period we are most interested in is the second phase, which coincides with the start of the railroad diffusion process.

Interestingly the first period (1815–1840) is characterized by population growth in cities and rural areas alike. While the urban population grew from 2.8 to 4 million (43%), the total population grew from 10.3 to 14.9 million (45%). Differences can be found, however, particularly between the east and the west. West Prussian city populations grew faster than the rural population, but the reverse was true for the East. Furthermore, while rural population growth was mainly due to a birth surplus, urban population growth resulted from a net migration gain as well as a birth surplus ([Matzerath, 1985](#), pp. 76-80).

The second phase of urbanization (1840–1871) was characterized by an increasing urban population growth. While the urban population grew from 4 to 6.7 million (68%), the total population grew from 14.9 to 20.2 million (35%).¹¹ West Prussian cities grew much more than those in the middle provinces, which grew much more than cities in the East. [Matzerath \(1985, pp. 117-123\)](#) further detects that population growth is positively correlated with city size. In a side note, [Matzerath \(1985, p. 139\)](#) calculates that the

¹⁰This periodization is still used today, i.e., in [Henning \(1995, p. 162\)](#).

¹¹Calculated within the borders of 1840.

average annual population growth (1849–1858) of nine Prussian cities, that were important locations for railroad engines (but not particularly industrial cities) was 1.6% and thus slightly higher than the average city growth of 1.5%.

3 The Data

The variable of interest in our analysis indicates whether a city was connected to the railroad in a given year (the treatment). We use GIS-software to collect information on railroad diffusion. Using point coordinates of the city centers, we create a map of all cities in Prussia. We then overlay the city map with annual maps of the German railroad system (see [IEG, 2010](#)) to discover which cities had access to the railroad in a given year. The resulting binary variable takes the value 1 if one or more railroad lines intersect the city in a given year.

The GIS approach sometimes returns inaccurate results because cities are represented only by point coordinates, which do not reflect their historical dimensions. Thus, it often appears as if a city had no railroad access. We correct our data using information on actual railroad access from the German handbook of cities ([Keyser, 1939-1974](#)), which specifies the year in which access was established and indicates the corresponding connection. This information is then checked and verified with information from official Prussian sources ([Königlich Preussisches Statistisches Bureau, 1883](#)).

In [Table 2](#) we present information on all railroad lines established by 1848, the relevant year for our cross-sectional analysis (see below). Twenty-one railroad lines were built in Prussia during the period 1838–1848. We provide information on the year of construction, as well as their length, passenger and freight transport statistics for each of the lines during the year 1848. The list of railroad lines also yields one important information—the terminal stations of the lines. These nodes perform a crucial role in a network because they are locations that were chosen to be connected in the first place. Whenever a new railroad line is built, it is planned to connect two or more locations. These locations are obviously not chosen arbitrarily and have higher-level functions than other locations along the line.

This paper assigns the role of a node in the railroad network to two types of cities: terminals and junctions. A city is identified as a node if it is mentioned in the name of the line (see column 1 of Table 2). For example, Berlin and Frankfurt (Oder) are identified as nodes after the Berlin-Frankfurter Eisenbahn (Berlin-Frankfurt railroad) was opened in 1842. The subsequent decision to build the Berlin-Breslau railroad using the existing connection Berlin-Frankfurt leads to the identification of Breslau as a node for this line in 1846, while Frankfurt keeps its original function. Furthermore, the line Berlin-Breslau was built to take a detour to provide a connection to the Görlitz-Dresden railroad line connecting Prussia to the Kingdom of Saxony. The junction to this line was chosen to be at the small village of Kohlfurt, close to the border. Kohlfurt is thus identified as a junction-node.

The subsequent analysis will consider the claim that network expansion primarily focused on the connection of important cities. Table 3 lists the twenty largest Prussian cities according to their population size before the establishment of the first railroad line in 1837. We find that during the first ten years of railroad building in Prussia, fourteen of the largest twenty cities had become nodes. List's railroad plan originally assigned the node function to ten cities on Prussian territory. Seven of List's nodes are among the top twelve major cities, six of which had become the intended node by 1848. This corroborates the previous assumption that railroad lines were built to connect important cities.

Generalizing from urban population growth to economic growth has shown to be an acceptable approximation in cases where data on income are unavailable (Acemoglu, Johnson, and Robinson, 2002). In similar vein, the outcome of interest in our empirical setup is urban population growth, which serves as a proxy for economic growth. This seems an appropriate choice in light of the fact that urban centers were the places where most of the innovation, as well as human and physical capital, was located and accumulated.

Soon after Prussia's new borders were established, from 1816, comprehensive and systematic population accounts were published by the Prussian Statistical Office. The urban population was counted on a triennial basis. Only places that held city rights in

the year of the census were included.¹² The censuses usually provided separate accounts for civilian and military residents.¹³ These data have been digitized and made available by [Matzerath \(1985\)](#). We corrected some digitization mistakes in the data and added missing variables using the original sources.

In contrast to much of the literature mentioned in the introduction, this city-level dataset does not make use of an ad-hoc population threshold, which was recently criticized by [Ploeckl \(2011\)](#). Making use of the legal definition of a town, the dataset consists of all Prussian cities during the nineteenth century. Consequently, the data also include a set of very small cities.¹⁴ The average city size in the dataset increases from 3,804 civilian inhabitants in 1837, to 4,525 in 1849, to 6,703 in 1871. From these data we calculate the dependent variable for the cross-sectional analysis, the annual growth rate of the civilian population for the periods between the censuses.¹⁵

To achieve consistency in the data, we restrict our sample to the 978 cities that held city rights in 1849. Cities that lost or gained city rights before or after 1849 are excluded from the analysis.¹⁶ This restriction is further motivated by the census of 1849 ([Statistisches Bureau zu Berlin, 1851-1855](#)), which is unique in providing a wealth of information at the city level. We will thus analyze the 1849 cross section in depth.

In [Table 4](#) we provide descriptive summary statistics by treatment status for the 1849 cross section. The treatment group (column 2) consists of cities that gained railroad access during the period 1838 to 1848; the control group (column 3) consists of cities that had no access by 1848.¹⁷ In column 4, we compare variable means for railroad cities with non-railroad cities. Panel A reports annual growth rates for a range of periods from 1821 to 1871, Panel B reports descriptives for controls variables that will be included in the

¹²After the establishment of the German Reich in 1871, censuses were conducted only in years ending with 0 and 5.

¹³Unfortunately, some of the censuses between 1819 and 1837 provide only the civilian population or were not published at all.

¹⁴The dataset encompasses 434 cities below the usual ad-hoc threshold of 2,000 inhabitants in 1837, 364 in 1849, and 266 in 1871.

¹⁵By calculating the average annual growth rates we avoid any issues arising due to diverging census intervals.

¹⁶Using this legally defined threshold results in the omission of a couple of city-like locations. According to [Ploeckl \(2011\)](#), in Saxony, these locations were actually some of the fastest growing during the Industrial Revolution. When examining the Prussian census data, we find 39 towns entering the census during the period 1849–1885 and thus gaining legal city rights. These cities had an average size of 4,915 inhabitants in 1871 and an average annual growth of 3% during the period 1871–1885. The Prussian-wide average was 1% in this period. Fifteen of these cities had access to railroads prior to obtaining city rights, indicating that railroads might have induced growth at the extensive margin.

¹⁷The control group however, includes cities that subsequently gained access in the period 1849–1871.

cross-sectional analysis, and Panel C reports descriptives for the pre-railroad adoption period that will be used for a matching approach.

The descriptives presented in Panel A allow a comparison of population growth rates between the treatment and control group as well as pre- and post-treatment trends. Column 5, shows that pre-treatment differences in growth rates between treatment and control group were generally quite small and insignificant for most periods. Furthermore, we find a strong divergence in growth rates between the groups for post-treatment periods starting from the period 1843–1846.

We also present the growth rates of node cities in column 2 of Table 4. These cities will, in most cases, be excluded from our subsequent analysis. Growth rates of node cities behave similarly to those of the treatment group at first, but tend to be slightly higher, on average, after the period 1846–1849.

Panel B of Table 4 presents the various control variables¹⁸ including access to rivaling infrastructure such as (i) main roads and (ii) navigable rivers and ports. Indicators of urbanization include (iii) pre-railroad city growth 1831–1837 and the size of the (iv) civilian and (v) military population in 1849. Indicators of industrial development include (vi) the share of citizens employed in factories and (vii) the occurrence of mining activity at the county level. As geographical endowments are usually among the major determinants of city growth, we control for (viii) the county-level concentration of large landholdings. As shown by [Cinnirella and Hornung \(2013\)](#), the concentration of large landholdings is correlated with soil-quality and can thus be viewed as a proxy for geographical endowments and therefore the supply of food for urban markets. Further controls include (ix) the age composition and (x) the education of the urban population. These controls are aimed at capturing differences in future population growth as well as the city’s progressiveness. We also calculate and control for (xi) the distance to the closest node of railroad lines since nearby cities are more likely to become connected to the network.

The unobserved incorporation of suburbs and smaller municipalities, as well as mergers between cities, sometimes introduces measurement error in the data and in some cases population appears to grow erratically. Our estimates could be biased in cases where

¹⁸See [Appendix A.1](#) for more specific definitions and sources.

cities that had access to railroads systematically had higher growth rates because of incorporations. We can control for such (xii) incorporations using the dataset provided by [Matzerath \(1985\)](#), which also indicates whether a city changed dimensions in a given period.

Unless otherwise specified, all data refer to the base year 1849. At that time, roughly 8% of the cities in the sample were connected to the railroad, 41% had access to a main street, 20% had access to navigable waterways, and 10% were located in a county with mining activity.¹⁹ Comparing cities by treatment status, we find substantial heterogeneity in many of the control variables. However, it is not clear if these differences arise due to the existing railroad access or if they were pre-determined. Our subsequent analysis will gradually build toward eliminating issues arising due to these differences.

Panel C of [4](#) presents pre-treatment variables which will subsequently be used for a matching exercise.²⁰ Comparing the treatment and control group, we find that cities systematically differ in some aspects such as size but were highly comparable in many other aspects such as commercial development previous to any railroad construction. We will discuss these aspects in more detail in [Section 4.4](#).

4 The Effect of Railroad Access on City Growth

This section is structured to gradually build up specifications from a cross-sectional approach using ordinary least squares (OLS), instrumental variables (IV), and propensity score matching (PSM) to a fixed-effects panel approach using OLS and IV, thus reflecting the hierarchy between the different specifications.

In a first step, we estimate the effect of railroad access on urban growth in a standard cross-city growth regression by ordinary least squares (OLS). By doing so, we can draw on a variety of unique city-level control variables provided by the Prussian census of 1849. In addition, we can calculate population growth rates between different censuses in order to analyze the short- and long term effects. This results in a model where the urban

¹⁹The share of factory workers, as well as the school enrollment rate, exceeds 100% in some cases, presumably due to workers and schoolchildren commuting from outside of the city.

²⁰See [Appendix A.2](#) for more specific definitions and sources.

population growth rate PGR^{21} in a variety of periods (t) is a function of railroad access RA in 1848 and other explanatory factors X :

$$PGR_t = \alpha_1 + \beta_1 RA_{1848} + X'_{1849} \gamma_1 + \varepsilon_t. \quad (1)$$

We emphasize here that the explanatory factors X include a lagged dependent variable to account for the dynamic aspects of urban growth.²²

4.1 Cross-Sectional Estimates

We start by presenting the results of a bivariate regression of annual city growth in the period 1849–1871 on railroad access in 1848 in Table 5, column 1. During this period a couple of new lines were built and new cities gained access to a railroad. Therefore, in order to not bias the results due to cities that receive the treatment after 1848 but remain in the control group, we control for cities that gain access during the period under consideration with a dummy for later access (column 2).

Column 3 adds indicators of rivaling infrastructure. Interestingly, neither connection to a main street nor to navigable waterways is correlated with urban population growth after 1849. Controlling for these other modes of transportation also rules out the possibility that railroad lines were just built alongside the main trade routes and reinforced their status. Column 4 adds indicators of urbanization.²³ As expected, we find that the lagged dependent variable pre-railroad city growth significantly determines subsequent growth in many specifications. Column 5 adds indicators of industrial development. To separate railroad access from other indicators of industrialization, we control for the share of factory workers in the city population and the occurrence of mining activity at the county level. Both indicators are significant and positively correlated with urban population growth. Also, cities in counties with a high share of large farms—which proxies

²¹The urban population growth rate is defined as $\frac{\ln(POP_{t2}) - \ln(POP_{t1})}{(t2 - t1)}$.

²²However, specifications not including the lagged dependent variable will not yield substantially different results (see Appendix Table A-16, Panel D).

²³Some cities are dropped from the sample since they did not have city rights in the period 1831–37 or lose city rights during the period 1849–71.

for the supply of agricultural products to urban markets—grow faster than others. The dominance of small family farms might thus have retarded urban population growth.²⁴

Column 6 adds several other controls that might have an effect on urban population growth. Interestingly, growth significantly increases with distance to the next node, implying that major railroad cities attracted most of the regional migration, leaving less migration to nearby cities. In column 7, we exclude all cities from the sample that experienced incorporations during the period under consideration.²⁵ Testing for interaction effects with streets and rivers we find that it is particularly useful to combine all three modes of transportation infrastructure. However, combining only one of them with railroad access does not yield additional growth (not shown).

Across specifications, we find that the coefficient on railroad access remains at a stable level of roughly 1% additional growth due to railroad access.

4.2 Endogeneity Issues

As previously mentioned, the direction of causality between railroad access and urban growth is not straightforward. Railroads might induce population growth in connected cities, but having access itself might not be independent of a city’s importance, wealth, and growth prospects. Thus, there might be an omitted variable that is correlated with both city growth and railroad access. Reverse causality, unobserved heterogeneity, and omitted variable bias could be serious issues in this setting. To address these issues, we take several different econometric approaches lending evidence for causal effects from railroad access on urban population growth: IV estimations, PSM, and fixed-effects panel regression.

At this point, we start by excluding from our sample all cities that are most likely to have gained access to the railroad for reasons endogenous to our dependent variable,

²⁴Additionally controlling for soil texture, a proxy for geographical endowments (see also [Cinnirella and Hornung, 2013](#)), does not change the results (see Appendix Table [A-16](#), Panel E).

²⁵Unfortunately, after excluding these cases we sometimes still observe implausible jumps in the population accounts that might be due to unobserved incorporations or similar artificial changes in the census population. After careful inspection of the data our sanity check finds that growth rates that exceed minus or plus 10% are hardly due to natural changes in the population. Thus we decided to exclude such observations from future estimations in subperiods that surpass this threshold. For results using the full sample of cities including nodes as well as outliers please refer to Appendix Table [A-16](#), Panels A and B. This table also shows results excluding outliers according to a standardized residuals threshold (see Panel C). Point estimates in these models are similar in magnitude to our baseline specification.

namely, the nodes of the railroad network.²⁶ Since, up until the 1860s, railroads were built to connect important cities, nodes are located in those cities that were the reason for the construction of the line and thus do not qualify for the assumption of random assignment (see Section 2.1). For now, we assume that all other cities had access to railroad technology simply because they were located en route between two major cities.

Each column of Table 6 reports OLS estimates of urban population growth on railroad access for different periods between 1831 and 1871—excluding the nodes. We find that being connected to the railroad in 1848 significantly increased the annual population growth by 0.9 percentage points during the period 1849–1871 (column 2). Comparing all periods across columns 3 to 9, we find that the annual population growth generated by railroad access varies between 0.4 and 1.1 percentage points. The coefficient seems to stabilize in the later periods under consideration, which hints at long-term effects from railroad access.

Note that the counterfactual specification in column 1 yields no significant effect when we regress railroad access until 1848 on pre-rail population growth 1831–1837. Prior to the advent of the rail, cities that were connected by 1848 thus had very similar growth patterns compared to those that were not. We find no pre-trend in rail access that favored cities with high growth rates.²⁷ Similarly, we find no effect on previous growth for railroad lines established in the period 1872–85 (for more information on this placebo test see Appendix C).

4.3 Instrumental Variable Estimates

Up to now, we have assumed that railroad lines were built to connect important cities and that cities located along the way were able to gain access to the railroad network by chance. Nevertheless, OLS estimates of the relationship might be biased in cases of omitted variables. Thus, we use an instrumental variable approach to resolve the omitted variable concern. Similar to the approaches taken by [Atack et al. \(2010\)](#) and [Banerjee,](#)

²⁶The additional exclusion of incorporations and outliers explains the varying number of observations over the subperiods.

²⁷We find similar results when extending the period to 1821–1837 in all our specifications (not shown). For better comparability, we show the period 1831–1837 since more observations are missing for 1821.

Duflo, and Qian (2004),²⁸ we predict actual railroad access RA in 1848 with the potential for railroad adoption in 1848—being located within a straight-line corridor SLC :

$$RA_{1848} = \alpha_2 + \beta_2 SLC_{1848} + X'_{1849} \gamma_2 + \eta_t. \quad (2)$$

Until the 1860s, Prussian railroads were built to connect important cities (see also Section 2.1). Under the assumption that lines were exclusively built to establish a fast connection between important cities A and B, cities en route were able to connect to the railroad simply because they were located on this straight line. Thus, all cities on a straight line between A and B were randomly assigned to adopt railroad technology. If it were only these cities that had gained access, our OLS estimates would be unbiased. In reality, we observe that connections sometimes deviate from the straight line. Cities located on such a deviation might have gained access for endogenous reasons.

Our instrument SLC is a binary variable determined by location on a straight line between nodes. We thus use variation in the potential for railroad adoption to instrument actual access. The idea behind this instrument is that deviation from the straight line bears additional costs.²⁹ If the railroad actually deviates from the straight line in order to connect a city, the additional costs of land acquisition, building tracks and stations, and additional operational costs, as well as the extension of travel time between the major cities, would be immense.³⁰ On the other hand, deviation from the straight line might reduce costs in the event of natural geographical obstacles such as lakes and hills. Column 6 of Table 2 shows that large shares of the lines were built linearly, indicating the high costs of deviation from the straight line.³¹

Using GIS techniques, we connect the nodes between which railroads were constructed with straight lines. The straight lines are chosen to follow the routing of an existing railroad line from node to node. Thus, the instrument proxies potential railroad access

²⁸The revised version of this paper (Banerjee, Duflo, and Qian, 2012) uses distance to the straight line as the main explanatory variable.

²⁹For example, the connection Cologne-Duisburg-Minden was originally intended to pass through the city of Lünen, which is located close to the straight line. This routing would have bypassed the city of Dortmund, which was to become a major industrial center. It was only the city's willingness to build the station at its own expense and an additional contribution of 3,000 Thaler that convinced the railroad company to build the costly detour, with extra mileage of roughly 10km, to connect Dortmund (Ziegler, 1996, p. 310).

³⁰The average construction stock for a Prussian mile (7.53km) of railroad was roughly 350,000 Thalers for lines built until 1848.

³¹The share of straight lines measure is adopted from official Prussian records.

along the straight line of existing routes. Furthermore, we create a buffer around these railroad lines.³² All cities within this corridor could potentially connect to the railroad due to the fact that they were accidentally located on a linear line between major cities. The instrument takes the value 1 for all observations within the corridor while all other observations take the value 0. This means that all cities that had access to railroads, despite not being located on a straight line, are taken as endogenous.³³

Obviously, deviation from the straight line did not happen exclusively in order to connect a certain city and geography introduces random measurement error into our instrument. Rivers are one of the main reasons for a deviation from the straight line since bridge building was expensive and orthogonality was required. Thus we allow the buffer to expand the linear line by 1.5 kilometers in each direction.³⁴

Figure 2 provides some helpful examples. The map displays a section of the railroad system centering around Berlin. We observe five railroad lines radiating from the Prussian capital. The hollow circles mark cities that have a railroad station, while black circled cities do not. The hash lines show the actual routing of the railroads, whereas the dark tubes show the straight-line corridor.³⁵

For example, in 1842/43, the connection between Berlin and Stettin (today Szczecin) was built to provide Berlin with fast access to the Baltic Sea. The city of Biesenthal did not become connected even though it was located only 3km from the actual line. However, it is not located within the corridor either, and is assigned the value 0 for the instrument. Interestingly, Biesenthal did not open a station until 1865 after a street was built toward the railroad line. On the other hand, it looks like the Berlin-Stettin line takes a marked deviation from the straight line in order to connect the city of Greiffenberg. Actually,

³²See Figures 1 and 2 for examples.

³³We also consider two alternative instruments in Appendix B. The first approach draws straight lines between nodes selected in List's railroad plan of 1833. The second approach connects nodes using a least-cost path based on terrain slope and rivers. Results from IV estimations using these approaches are presented in Appendix Table A-17 and are qualitatively similar to the *SLC* approach.

³⁴We find that coefficients remain significant using corridors with a width of 2, 4, 6, 20, 30, or 40 kilometers. Although not significantly different from each other, point estimates decrease when increasing the corridor width. Furthermore, increased corridor width will increase the power of the instrument. See Appendix Figure A-4 for a graph that plots beta coefficients against corridor width. Note that a corridor width of 40km might already pick up cities from other corridors. This could explain the increased beta coefficient as compared to the 30km corridor width. Also, note that the average distance to the next nearby city is 10.8km and 17.4km to the next nearby city with more than 3,000 inhabitants.

³⁵Black lines mark the routing of main streets.

Greiffenberg became connected to a different line only in 1863 and therefore the noted deviation seems to be for geographical reasons or because land could not be acquired.

Another very different example is the connection Berlin–Potsdam which was extended to Magdeburg and was completed in 1850. This line deviates markedly from the straight line in order to connect the cities of Brandenburg, Genthin, and Burg. Since these cities are not located within the straight-line corridor, the instrument will take the value 0, assuming these cities gained access to the railroad for endogenous reasons.

Table 7 reports estimates using the straight-line corridor location as an instrument. Panel A shows first-stage results of the IV approach. The instrument *SLC* is significantly correlated with actual railroad access. First-stage F-statistics are high and confirm the power of the instrument. Second-stage results reported in Panel B, show the causal effects of railroad access on urban population growth. We find a significant increase in urban population growth due to railroad access of 2.1 percentage points during the period 1849–1871. Across all subperiods under consideration, the effect varies between 1.1 and 2.2 pp for a city that gained access by 1848. Again, it is reassuring that the counterfactual model for the period 1831–1837 does not yield significant results (column 1 of Table 7). Coefficients estimated by IV are approximately twice as large as coefficients derived from OLS estimations. The OLS coefficients might be biased downward in case of an omitted variable—for example, cities with lower growth prospects might have influenced routing in order to become connected.

Robustness tests that introduce 25 district dummies, that exclude the sparsely populated eastern provinces,³⁶ or that include continuous variables for roads and waterways instead of dummies do not yield qualitatively different results (not shown).

The exclusion restriction would be violated if the instrument was correlated with the error term. This would be the case if location in the *SLC* was associated with urban population growth through a channel other than the railroad; for example, if the corridor coincided with historical trade routes that still fostered growth. The coefficients would be biased if cities in the *SLC* were larger or better integrated in trade than cities outside of the corridor. When estimating the reduced-form relationship of urban growth on location

³⁶These are the predominantly Polish-speaking provinces of Prussia, Poznan, Pomerania and Silesia.

in the *SLC*, we find no correlation with the pre-railroad growth during 1831–1837 (column 1 in Panel C of Table 7).

Although violations of the exclusion restriction can ultimately not be tested, we further address such concerns by showing bivariate regressions of the instrument *SLC* on the full set of control variables in Table 8 (columns 1-2). Indeed, we find that *SLC* location is positively associated with street access as well as mining activity and negatively associated with agricultural endowments in the full sample of cities. Thus, controlling for the full set of control variables, proves to be important for eliminating such channels.

However, we also find that *SLC* location is correlated with a range of city characteristics measured at a time prior to railroad building. Anticipating our subsequent propensity score matching approach (see Section 4.4 below), we also show bivariate regression of the instrument *SLC* on the full set of control and matching variables in Table 8 (columns 3-4). In the matched sample of cities, based on weights resulting from a kernel propensity score matching, we find that cities inside and outside the corridor do not differ significantly regarding the control and matching variables.³⁷ However, we find that cities in the *SLC* are also located closer to a node by 9km—this indicates that it is harder to find good matches in close proximity to nodes.

4.4 Matching and Other Sample Restrictions

As observed in Table 4 column 5, differences in means between railroad and non-railroad cities are significant for some of the post-treatment as well as the pre-treatment variables. This casts doubts on the suitability of using the entire sample of non-railroad cities as a control group. After having established our baseline specifications using the full population of cities in Prussia, this section presents specifications introducing sample restrictions regarding geography, size, and pre-railroad development to provide for a high comparability between the treatment and the control group.

Inspecting the map in Figure 1, we notice that many cities are located in areas not even in close proximity to a railroad line. In addition to controlling for the distance to a node, we test whether the results are affected when restricting our sample to geographically

³⁷A radius matching approach yields similar results (available from the author upon request).

matched cities, effectively comparing only cities within close proximity to the railroad. The advantage of such an approach is that cities in geographical proximity might be very similar in many other aspects too, reducing the omitted variable bias. The resulting estimates show the effect of having a railroad station compared to merely being located in proximity to one. We limit our sample to cities that had access to railroads in 1848 and their two next unconnected neighbors.³⁸ Instrumental variable estimations in this matched sample confirm previous results and show that cities with a railroad station grow significantly faster than their nearby neighbors (see Appendix Table A-16, Panel F). In a similar approach, we reduce our sample to cities within a corridor around the straight-line corridor. This sample includes only cities within a 15km distance to the straight line. IV estimation in these models yield qualitatively similar results (see Appendix Table A-16, Panel G).

In addition to controlling for the size of a city, we test whether restricting our sample to a set of small and rather unimportant cities will affect our results. A sample of cities with a population of only 3,000 inhabitants in 1837 (before the first railroad was built in Prussia), guarantees that none of these were important enough to become connected because of sheer size.³⁹ Although the instrument loses some of its power, Panel A of Table 10 shows that this sample yields results qualitatively similar to those of the full sample. Nevertheless, point estimates in this sample are somewhat lower than in the baseline specification and for some of the periods the point estimates become insignificant.

Finally, we employ propensity score matching techniques including indicators of pre-railroad development, size, and geography. The aim of propensity score matching is to compare the outcome for cities that are as similar as possible and—ideally—differ only in their assignment to the treatment. Propensity score matching is particularly useful in cases where assignment to the treatment group is not explicitly random. In our case, the worry might be that even though cities are located on a straight line between terminal or junction stations, they did not gain access just because of this fact.

³⁸Unconnected cities that were matched to two or more connected cities are weighted correspondingly. The average distance is 11.8km to the first nearby city and 15.2km to the second nearby city.

³⁹Cities with fewer than 5,000 inhabitants were considered as small by the Prussian administration. Forty of the cities below 3,000 inhabitants had access to railroads by 1848; 627 did not. Restricting the sample to cities with a population between 1,500 and 5,000 inhabitants, thus excluding a large number of very small and very big cities, yields similar results (not shown).

To obtain a highly comparable sample, we match treated and untreated observations using the set of pre-railroad variables presented in Table 4, Panel C.⁴⁰ Since the first Prussian railroad was built in 1838, we match cities by (i) their size in 1837, (ii) their population growth during the period 1821–1837, and normalized numbers of (iii) merchants in 1819, (iv) looms in 1819, (v) Protestants in 1816, (vi) private dwellings in 1821, (vii) commercial buildings in 1821, and (viii) the insurance value of buildings against fire in 1821. These variables are targeted at matching cities regarding their size and commercial development prior to railroad construction. Propensity score matching is done using radius and kernel matching techniques. To reduce the inclusion of poor matches, we make use of the common support condition.⁴¹

Table 9 presents descriptive statistics by treatment status for a radius and a kernel matching approach. Radius matching finds all untreated observations that are within distance of a specified caliper (0.001) to a treated observation according to the propensity score. Kernel matching compares the outcome of treated observations to a weighted average of outcomes of untreated observations. Observations that are more similar receive more weight than others. Column 3 shows differences in means between the matched samples. Both approaches yield highly comparable sets of cities. We find that otherwise similar cities in this sample that gained access to a railroad line before 1848 achieve significantly higher population growth in the period 1849-71.

As matching can only resolve observed heterogeneity, the endogeneity of railroad access due to unobserved heterogeneity might still be an issue. Thus we combine the outcomes of the propensity score matching with our instrumental variable approach. The weights obtained from matching are included in equation 2 to estimate the effect of railroad access on growth using the instrumental variable *SLC* in the matched sample.

Results of the IV estimation in a radius-matched sample are shown in Panel B of Table 10. We find a significant positive increase of 1.1 percentage points in annual population growth over the period 1849-71. Since the number of matched observations is small, standard errors are higher and coefficients become insignificant in four subperiods. However,

⁴⁰For this purpose, we exclude all cities that gained access to railroads during the period under consideration from the matching. As the number of cities with railroad access increases, finding suitable matches becomes more difficult. This is the reason why the sample size decreases over the subperiods when using a radius matching.

⁴¹Appendix Figure A-5 shows the frequency distribution of the propensity score by treatment status.

in terms of magnitude, the point estimates range between earlier OLS and IV estimates using the full sample. Since one of the matching variables is the population growth rate for the period 1821-37, it does not come as a surprise that the counterfactual model for the period 1831–1837 does not yield significant results in the matched specifications (column 1 in each panel). Results of the IV estimation in a sample matched using a nonparametric kernel approach are presented in Panel C of Table 10. Results obtained in this sample are qualitatively similar to the radius matched sample. We find that railroad access significantly increases annual population growth by 1.7% over the period 1849-71 using a kernel-matched sample.

In Panel D, we expand the matching variables to include the geographic location of a city, namely longitude and latitude. In doing so, we aim at finding pairs of cities that are similar in terms of location as well as in size and commercial development previous to railroad construction. Estimates using weights from this matching approach behave very similar to prior matching approaches—railroad access significantly increases annual population growth by 1.7% over the period 1849–71.⁴²

Comparing the overall results from our various sampling restrictions to the baseline estimates, we find qualitatively similar results. However, point estimates are usually somewhat smaller, indicating that unobserved heterogeneity between cities might account for part of the railroad effect.

4.5 Panel Data Estimates

In our preferred approach to estimate the effect of railroad access on urban growth, we use panel techniques. The advantage of the panel approach is the possibility to overcome time-invariant unobserved heterogeneity by including fixed effects. As such, the city-level heterogeneity in pre-railroad development observed in our matching variables in Table 9 can be excluded here by including city fixed effects. This allows us to exclusively exploit within-city variation. To eliminate concerns of reverse causality, we regress city size,

⁴²Again the propensity score matching successfully reduces the differences in means between railroad and non-railroad cities. Results are qualitatively similar but coefficients seem less stable when using radius matching techniques. Furthermore, results show similar patterns when using the matching variables as control variables instead of using them for the matching approach. (Results available upon request from the author.)

measured as the natural logarithm of the total civilian population $\ln(POP)$ in city i in year t , on a dummy variable indicating railroad access RA in the previous year:

$$\ln POP_{it} = \alpha_i + \tau_t + \beta_3 RA_{it-1} + X'_{it} \gamma_3 + \nu_{it}. \quad (3)$$

We can further include city fixed effects α_i as well as time fixed effects τ_t , capturing national trends in population growth in our regressions. In such a panel setting, the estimated coefficient of interest β_3 returns the additional growth in population levels for cities that had access to railroads, compared to those that did not, after gaining access. The covered period ranges from 1840, just after the first railroad was built in Prussia, to 1861, just at the end of Phase 2 of the railroad network expansion—connecting major cities. Since the censuses provide triennial data, we derive a panel consisting of eight repeated cross sections. The only information published in this frequency at the city level is population counts. Thus, only a few control variables X' from the original model in 1 are available in the panel setting. Available city-level controls include the military population, distance to the next node, and a dummy that controls for the incorporation of municipalities as provided by [Matzerath \(1985\)](#).

We present panel estimates in Table 11. The first specification reports estimates in a pooled sample, including time-fixed effects (column 1). Column 2 introduces city-fixed effects and thus shows the within-city effect of gaining railroad access in one year on subsequent additional population growth, similar to a difference-in-differences approach. The dummy variable indicating railroad access switches from 0 to 1 when a city is connected to the railroad network. Interestingly, the coefficient estimated in the fixed-effects model is also close to the pooled sample, indicating low levels of unobserved heterogeneity at the city level.

By excluding node-cities, we again try to minimize issues of endogeneity in column 3. The results indicate that railroad access additionally increases urban population levels by 5.6% over a period of three years. This translates to an annual rate of 1.9%.

One drawback of our panel estimation is the lack of time-variant control variables. Thus, we are not able to account for trends in, for example, industrialization occurring

during the period, which might have influenced both railroad access and city growth. To address this issue, the specification in column 4 adds 324 county-fixed effects interacted with time-fixed effects. Such a specification captures county-wide shocks during one period that affected all cities within the same county. An obvious example could be the discovery of mineral resources that introduces a shock to a county's economy. Other examples include shocks to the food supply or epidemics. The point estimate increases in magnitude to the previous specification implying that there might indeed have been negative shocks at the county level. The difference between coefficients is, however, not significant.

It is also interesting to test for heterogeneous treatment effects since railroad access might be something that does not affect every city in the same vein. Columns 5 and 6 report estimates in separate samples for the six eastern provinces and the two western provinces. We find that the effects are larger in the West, confirming expectations due to strong industrialization in this region. In column 7 we restrict our sample to cities within close proximity of 15km to the *SLC* in 1861. Compared to the full-sample estimates the coefficient is lower. Further estimates suggest that the coefficient increases with distance to the straight line (not shown). This finding indicates the existence of spatial spillovers. Non-railroad cities located in proximity to a railroad line might benefit from positive spillovers and have higher growth rates; cities far away from any railroad line might suffer from remoteness and have lower growth rates.

Furthermore, we report estimates in separate samples for small cities and large cities in columns 8 and 9.⁴³ Interestingly, we find that effects are larger for smaller cities, indicating that the estimated effect is not driven by cities that were already large.

We also test for heterogeneity in the provision of railroad lines. From 1850 the Prussian state became more involved in railroads and started building and running railroad lines on its own account.⁴⁴ In column 10, we include two dummies each taking the value of 1 just after a railroad became state owned or state administered. The coefficient on state administered railroads is within the range of our previous findings and is significant at

⁴³A city is identified as small if its size was below 3,000 inhabitants in 1837, before the first railroad was built; a large city had more than 10,000 inhabitants in 1837. The coefficient found in a sample of medium cities is very similar to the small cities sample.

⁴⁴Railroad building occurred to increase profits, adding to the budget, as well as for military reasons.

the 11% level. However, the point estimate on state owned railroads is somewhat smaller and insignificant suggesting that these lines did not induce similar growth.

We can further address endogeneity issues in the panel setting using the straight-line corridor approach discussed in Section 4.3. A strong advantage of our setting is that the instrument actually exhibits time variation. Whenever new lines were built, new straight-line corridors are established, providing over-time variation in the instrument. We construct straight-line corridors on a triennial basis and use them to generate an instrument that varies over time.

Results of the first-stage relationship between *SLC* location and actual access are presented in column 11. The effect is similar in size and magnitude as the one found in the cross-sectional setting. The second-stage results presented in column 12 show a causal effect of 7.7%. This means a city that was connected to the railroad subsequently experienced an increase in growth of roughly 2.6% per year. This result is actually close to the 2.1% annual growth found in the baseline IV specification presented in Table 7 for the period 1849–1871.

The exclusion restriction might be violated if the establishment of railroad lines over time was correlated with, for example, their expected profitability. We test for this possibility by running bivariate regressions of profitability measured by return on investment (*ROI*) on the year of establishment. For each railroad line, the *ROI* is normalized to years after establishment. The results presented in Table 12 show that after an initial stage of roughly four years there is no systematic difference in profitability for lines that had been established earlier.⁴⁵

4.6 Event Study Analysis

A dummy that switches from 0 to 1 after gaining access only allows us to identify changes in growth rates in the subsequent period—similar to a difference-in-differences approach. To determine the timing of a long-term shift in growth rates we estimate an event-study specification. Since the event of gaining railroad access occurs at different times for different locations, the panel analysis with city- and time-fixed effects will prove extremely

⁴⁵The initial stage is characterized by a high number of lines effectively realizing losses with a *ROI* of zero.

helpful. Such an identification strategy can test whether results are driven by underlying pre-event trends since it traces out the trend in growth rates for the periods leading up to and following railroad access. The presence of a pre-trend would raise concerns about our identification strategy.

To be able to show the dynamics of the effects from gaining access, we code separate dummies for periods before and after adoption.⁴⁶ Each dummy only takes the value 1 in a single period prior to or after a city becomes connected to the railroad and is 0 for all other periods. Since the occurrence of a railroad varies over time, this approach allows us to pool the statistical information for each separate stage of railroad access. Effectively, we can now compare cities in the same stages of railroad access across periods.

Table 13 shows significant positive effects of railroad access over all periods after gaining access. All coefficients are measured relative to the omitted coefficient which is the period prior to access. It is important to emphasize that we find no differences in growth rates prior to the event, which excludes the possibility of pre-trends. This finding becomes visible in Figure 3 which plots the beta coefficients over time allowing an inspection of the pre- and post-event growth for cities that gained railroad access during the period 1838–1861. The figure shows an absence of pre-railroad trends in growth rates and a sharp upward trend after railroad access is established. In combination with city-fixed effects, the absence of pre-trends confirms the perception that the results do not suffer from unobserved heterogeneity issues.

5 Discussion of the Results

The above analysis focuses on establishing causality between railroad access and population growth. Such reduced form estimates provide a very general lesson on the relative impact of gaining railroad access which can be attributed to a range of different mechanisms. This section provides evidence for one of the possible mechanism through which railroads affect growth, integrates our findings into the recent literature on railroads, and further discusses issues regarding economic spillovers, heterogeneous treatment effects, and the generalizability of the results.

⁴⁶Each period spans a triennium and is thus of the same length.

5.1 Possible Mechanisms

The literature identifies the immediate effects of transport infrastructure expansion in terms of reductions in the cost of trading, increases in trade volumes, increases in market access, and reductions in price gaps (see [Donaldson, 2014](#); [Donaldson and Hornbeck, 2013](#); [Keller and Shiue, 2008, 2013](#); [Michaels, 2008](#)). Other works show that such advantages might further translate into increases in firm size, firm profit, the number of firms, the female labor force participation rate, and employment (see [Atack et al., 2010](#); [Atack, Haines, and Margo, 2011](#); [Banerjee, Duflo, and Qian, 2012](#); [Duranton and Turner, 2012](#)). Regarding urban expansion this implies that cities can support a larger number of individuals due to decreases in the cost of living, that is, in form of a reduction in food prices or due to higher incomes, i.e., higher wages due to productivity gains.

[Atack, Haines, and Margo \(2011\)](#) argue that railroads increase competition among firms due to an increase in the market size. Consequently, firms attempt to increase productivity through the division of labor which in turn leads to an increase in establishment size. As industrial productivity increases, so do wages, attracting an inflow of workers from rural areas to urban centers ([Malanima, 2010](#)).⁴⁷

Similar to [Atack, Haines, and Margo \(2011\)](#), we test whether railroad access increased firm size, which might have translated into urban population growth in nineteenth-century Prussia. The 1849 census ([Statistisches Bureau zu Berlin, 1851-1855](#)) includes a factory census allowing the effects of railroad access on firm size to be tested at the city level. The census reports the number of factories and workers in 119 different product categories.⁴⁸ We calculate the average size of factories at the city level and use it as an alternative dependent variable in the cross-sectional set-up using OLS, IV, and matching techniques. Since such data are exclusively available for the 1849 cross section, we can only estimate the level effects of railroad access on firm size.

⁴⁷Job opportunities created by factories in cities with railroad access attracted a massive inflow of rural workers ([Boelcke, 1996](#)). In fact, since railroads were usually built so that they passed a city tangentially, the development of cities itself changed such that they grew toward the station. The road leading toward the station usually developed into an important commercial street, attracting industry, and working-class quarters were built to surround the factories ([Matzerath, 1985](#), p. 156).

⁴⁸For further information see [Becker, Hornung, and Woessmann \(2011\)](#).

The results of this approach are presented in Table 14, starting with an OLS estimation in column 1.⁴⁹ We proceed to estimate the relationship using the *SLC* instrumental-variable approach of Section 4.3. Column 2 shows that firms located in a city with a railroad station were 74% larger than in cities without a station. This effect increases to 177% in the kernel-matched sample, implying that establishments in treated cities are more than twice as large. Reassuringly, we find no counterfactual effect of railroads built after 1848 on firm size in 1849.

Do firms in railroad cities just grow bigger or are these cities also able to attract a larger number firms? Columns 4 to 6 in Table 14 show that the latter was not the case. The estimated effect of railroad access on the number of firms is insignificant in the IV and PSM specifications. Our results might thus indicate that firms indeed increased the division of labor as a response to railroad access. However, railroad access seems to have affected industry location only at the intensive margin rather than the extensive margin.⁵⁰

The additional employment opportunities generated in railroad cities can induce population growth either by attracting immigration or by increasing fertility. The data allow us to distinguish between sources of population growth in our cross-sectional setting. Columns 7-9 present results when using fertility as an outcome. Throughout specifications we find no significant effect of railroad access on the child-woman ratio in 1849.⁵¹ Columns 10-12 present results when using migration as an outcome. IV estimates show that the share of urban dwellers that were born outside of the city was 12.8% higher in railroad cities in 1871. The coefficient becomes insignificant in the matched estimates due to the strongly inflated standard errors.

Furthermore, we can use differences in market prices to test whether market integration is the only channel through which railroads affect growth. Using a cross section of county-level data on the average market prices for different crops for the period 1837–1860, we include the price of the most important crops in our cross-sectional IV specification. By including the price for wheat, rye, and potatoes as control variables, we exclude the market integration channel. Prices enter the baseline model with significant coefficients

⁴⁹Please note that these regressions do not control for the share of factory workers in the city population. However, if they do, the coefficient on railroad access 1838-48 is hardly affected.

⁵⁰Similarly, [Gutberlet \(2013\)](#) finds that railroads increased manufacturing employment at the district level in Germany.

⁵¹The child-woman ratio is calculated as the ratio of children under 5 to women aged 15-45.

(see Appendix Table A-16, Panel H). The coefficient on railroad access is hardly affected. Keeping in mind that this analysis can only estimate a level effect and might be flawed due to the unavailability of city level market prices, we find no evidence that differences in market integration account for the entire effect of railroads on growth.

We can also think of various other channels through which railroads might affect growth. Railroad lines were accompanied by a series of other improvements such as new forms of information exchange by telegraph lines⁵² or by improved postal service.⁵³ Railroads may thus foster technological diffusion through knowledge exchange. According to Mokyr (2002, p. 30), the ‘technology of knowledge transmission’ is important to the diffusion of knowledge and technology itself. Benefits from such improvements due to railroads are captured in our access measure and cannot be separated within this dataset.

5.2 Discussion of the Results in Context of the Literature

Recently the literature has seen an increasing amount of research on the effects of transport infrastructure expansion, which can be roughly divided into two groups: research focussing on aspects of market integration due to the expansion of infrastructure transport network and research focusing on more macro aspects of development and growth. Here, we will focus on discussing findings by the latter group that is more closely related to our paper.

Besides reduced price gaps and increased trade flows across Indian districts, Donaldson (2014) also finds increases in real income levels due to railroad access. He estimates that railroads increased agricultural income by 16% and that only 14% of the effect cannot be attributed to increases in trade. This study on historical India fundamentally differs from the Prussian setting in that labor was less mobile, railroads did not foster growth in the negligible industrial sector and lines were built mainly for military purposes. Banerjee, Duflo, and Qian (2012) focus on the long-rung effects of railroad building in contemporary

⁵²Railroad construction was often accompanied by the development of telegraph lines, which were built along the railroad line and in a number of cases even incorporated into the railroad embankment. Thus, in many cases, railroad adoption also meant the adoption of telegraphy which might have advanced the speed of communication.

⁵³From the early days of the Prussian railroad network, railroads took over the function formerly performed by stage coaches—the transport of passengers and mail. Borchardt (1972) even describes the coming of the railroad as a communication revolution. The increasing possibility for knowledge exchange through direct personal contact and the acceleration of the mail traffic led to all sorts of new possibilities for technological diffusion and knowledge spillovers. For an assessment of the effect of postal services on the spatial structure of the population distribution in the German Empire see Ploeckl (2012).

China. Estimates yield only small effects on a range of development indicators, a result that might also be attributed to a lack of factor mobility in this particular Chinese setting.

The Indian and Chinese examples thus provide benchmarks for the impact of railroads in rural societies that are characterized by low factor mobility. However, mid-nineteenth-century Prussia was characterized by an increasing factor mobility and strong industrial development. Thus, our results might be more suitable to be extended to countries in periods of industrial development.

For example, [Atack et al. \(2010\)](#) find that railroads explain 58.3% of urbanization in the midwestern United States in the 1850s at the county level. The somewhat smaller effects found for population density might be caused by the special U.S. case with abundant land. Furthermore, [Atack, Haines, and Margo \(2011\)](#) find that railroads induced industrial development by increasing the likelihood that an establishment was a factory by roughly 16%. These findings are much more in line with our findings on the industrial development in Prussia presented above.

[Donaldson and Hornbeck \(2013\)](#) estimate the effect of changes in market access due to railroads on changes in agricultural land values in a panel of U.S. counties. The authors calculate that removing all railroads built by 1890 would translate into a reduction in agricultural land values equal to an annual loss of 3.4% of GNP.⁵⁴ However, this setting can only provide evidence for the non-industrial sector of the economy.

The works presented by [Keller and Shiue \(2008, 2013\)](#) differ from our approach in analyzing bilateral trade flows between major German cities using nineteenth-century city-level wheat prices, suggesting that trade is one of the most important channels through which railroads affect growth. Such a setting is particularly useful in assessing whether a particular connection between two cities changes their relative economic environment. Here, railroads as determinants of trade function as a proximate factor through which a fundamental factor, the institution of the German Customs Union, determines development.

⁵⁴As changes in market access represent a cumulative measure of all changes in the transportation infrastructure network, the results of such estimations show an aggregated effect on the economy.

There are institutions which might be qualified as necessary preconditions for railroads to be effectively fostering growth. Prussian railroads probably would have been less effective without the German Customs Union which created free trade (see [Keller and Shiue, 2013](#)). Similarly, the free movement of the factors of production might be a necessary precondition as indicated by [Banerjee, Duflo, and Qian \(2012\)](#) for China. These necessary institutions allowing for the free movement of labor were introduced with the agricultural reforms in Prussia at the beginning of the nineteenth century.⁵⁵

In sum, the recent literature has predominantly focused on analyzing the consequences of establishing railroads for the agricultural sector or for agricultural societies with limited factor mobility. As railroads are often strongly connected to the industrial sector, the Prussian environment seems a natural laboratory to assess the consequence of railroads on industrial development.

5.3 Spillovers and Heterogeneous Treatment Effects

Some issues arise when assessing the effects of place-based network policies. Growth due to railroad access might either induce positive spillovers or happen at the expense of other regions.⁵⁶ Furthermore, heterogeneous-treatment effects become an issue in networks where parts of the network are of a higher local value. More specifically, railroad access might be something that is not of fixed importance: different lines might have different effects on otherwise similar cities. Even cities on the same line might be affected in very different ways depending on their local endowments.

Within our framework, which uses a binary measure for local network access, it is however not possible to be definite about these issues. [Donaldson \(2014\)](#) and [Donaldson and Hornbeck \(2013\)](#) show that using measures for transportation infrastructure which capture the effect of the entire railroad network on a place leads to estimated effects which are larger than when using measures of local railroad access. These findings suggest that

⁵⁵[Landes \(1969, p. 154\)](#) notes that faster transportation meant that labor became more mobile and natural obstacles to the movement of the factors of production were eliminated.

⁵⁶City growth usually results in large parts from migration. We can think of a scenario where the positive growth effect for treated cities is entirely due to urban-urban immigration from untreated cities, leading to an aggregate effect of zero. In mid nineteenth-century Prussia, where factor mobility was already very high, it is however much more plausible to think of growth due to rural-urban migration. [Ziegler \(1996, p. 304\)](#) notes that we know today that railroad adoption did not end in a zero sum game at the expense of other regions. In such a setting railroads might have worked as a pull factor for rural-urban migration.

our results might be interpreted as underestimating the effect of railroads in Prussia. From our various robustness tests we can further infer that there seem to be heterogeneous effects over time, depending on geography, public provision of the good, or depending on the size of a city.

Concerns that the overall effect in equilibrium might be much smaller are associated with the choice of the city level as a unit of observation. We aim to attenuate such concerns by aggregating our data to the county level. When escalating to this higher level of aggregation, we use urbanization as an outcome variable which we regress on county level indicators of railroad access. County-level data on urbanization and many other control variables are available for two cross sections in 1849 and 1864 which allows us to estimate fixed-effects models using panel data. Table 15 presents OLS and IV results from our regression. Here, county-level indicators of railroad access are the number of cities with railroad access and the share of a county’s population living in cities with access.⁵⁷ The instrument is again based on the established *SLC* and counts the number of cities in a county located within 1.5km range of the *SLC* or the share of a county’s population that lives in a city located within this range, respectively. We find significant positive effects of increasing the number of stations as well as increasing the share of population with railroad access. The average change in the number of stations per county during the period 1849–1864 is 0.37 which we estimate to cause a change in urbanization of 0.44%—or roughly one third of the overall increase in urbanization during the period. Similarly, a 6% change in the population with access to a railroad causes an estimated change in urbanization of 0.52%. It thus seems that we can exclude concerns of small or non-existing general equilibrium effects—at least for the county level.

One limitation of using IV estimation approaches lies in the fact that we can only estimate the local average treatment effect (*LATE*) of railroad access for cities in the *SLC*. Since the IV approach is not informative about the non-compliers, that is, cities in the corridor that do not gain access or cities outside of the corridor that gain access regardless, we cannot be specific about average treatment effects (*ATE*). During the course of the paper, we have also successively reduced the sample by excluding nodes,

⁵⁷Unfortunately, this measure does not capture the few places that had railroad access but did not have city rights.

excluding large cities, or applying a matching methodology. Judging from such reduced samples, it is difficult to assess the effect of railroad access for the entire population of Prussian cities. Due to these limitations, the results might not be generalizable to the full population without reservations. However, throughout all specifications we have consistently found effects ranging between 1 and 2 percentage points increases in annual population growth. This very stable result indicates that the heterogeneity of the effect is somewhat limited to a narrow range. This finding might also help to generalize from local average treatment effects to a statement about the entire population.

6 Conclusion

This paper tests the hypothesis that railroads induced economic growth at the city level for the historical German state of Prussia during a period of rapid railroad network expansion. We find that railroads had a significant causal effect on urban population growth over the period 1838–1871. Cities that gained railroad access during this period experienced additional *annual* growth of roughly 1% to 2%—a substantial amount. Across a range of different specifications, the effect is presumably best identified in the propensity score matching and fixed-effects panel estimations using instrumental variables.

The paper adds to the literature by successfully establishing a time-variant instrumental variable to estimate the causal effect of transport infrastructure on growth. As such, we can plausibly introduce exogenous within-city variation in railroad access into a panel using city-fixed effects. Excluding time-invariant differences between cities, this allows for a convincing assessment of the local consequences of railroad access.

This paper further provides evidence for one of the channels through which railroads affect growth, namely by increasing firm size. We further show that railroad access induced immigration, leading to city growth. The results show that cities with railroad access hosted factories that were more than twice as large, presumably triggering population growth through the demand for workers from the industrial sector.

We contribute additional evidence to the debate over whether railroads induced or followed economic growth. By estimating counterfactual models, in a series of different

specifications, where we regress pre-railroad growth on subsequent railroad adoption, we find no evidence that railroads appeared as a consequence of a previous growth spurt. The further inspection of non-linearities in within-city variation of growth show that railroad access changed the pattern of growth for adopting cities.

Our analysis is, however, limited in providing evidence for the effects of the entire railroad network and cannot take spillovers into account. Furthermore, we suspect the existence of heterogeneous treatment effects which can ultimately not be estimated using a binary variable for railroad access. This leaves room for improvements in future research regarding the relative effect of railroads on growth.

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Appendix A Data Appendix

Appendix A.1 Definition and Sources of Control Variables

For additional information on our Prussian census data see [Becker et al. \(2014\)](#).

(i) *Street access*: Dummy variable indicating whether a city was connected to a main road. Similar to the maps on railroad access, we geo-reference the corresponding map for paved and unpaved main roads (*Hauptstraßen*) in 1848 and match it with the location of Prussian cities.

(ii) *Waterway access*: Dummy variable indicating whether a city had at least one cargo ship for river navigation or one seagoing vessel in 1849.

(iii) *Annual city growth 1831-37*: the average annual growth of the civilian population as counted in the censuses of 1831 and 1837.

(iv) *Civilian population (log)*: the natural logarithm of the resident civilian population in 1849.

(v) *Military population (log)*: the natural logarithm of the military population in 1849.

(vi) *Factory workers (share)*: the share of total population employed in factories of all kinds in 1849.

(vii) *Mining (county level)*: Dummy variable indicating whether the city is located in a county that has a least one steam engine in mining.

(viii) *Large farming (county level)*: Measured as the county-level share of land holdings larger than 300 Prussian Morgen (roughly 75 hectare) over the total number of land holdings in 1849.

(ix) *Age composition*: Measured as the share of the population younger than 15 years over the total population in 1849.

(x) *School enrollment rate*: Measured as the share of children at compulsory school age (6-14) that attended school in 1849.

(xi) *Distance to next railroad start*: Measured as the linear distance to the closest node in 100 kilometers in 1848.

(xii) *incorporations*: Dummy variable indicating whether a city changed its dimension

through incorporation of surrounding parishes.

Control variable (i) is coded using maps provided by [IEG \(2010\)](#), variables (ii) and (iv) to (viii) are digitized data from the 1849 census ([Statistisches Bureau zu Berlin, 1851-1855](#)), and variables (iii) and (xii) are from data provided by [Matzerath \(1985\)](#).

Appendix A.2 Definition and Sources of Matching Variables

(i) *City size 1837*: the natural logarithm of the total number of civilian inhabitants in 1837.

(ii) *Annual city growth 1821-37*: the average annual growth of the civilian population as counted in the censuses of 1821 and 1837.

(iii) *Merchants*: the share of merchants, hawkers and victual mongers in the total population in 1819.

(iv) *Looms*: the number of looms on different fabrics over the total population in 1819.

(v) *Protestants*: the share of the Protestant population in 1816.

(vi) *Private dwellings*: the number of private dwellings over the total population in 1821.

(vii) *Commercial buildings*: the number of manufactories, mills and warehouses over the total population in 1821.

(viii) *Insurance-value of buildings against fire*: the natural logarithm of the average insurance value of buildings insured by the local fire insurance company (*Feuersocietät*) in 1821.

Matching variables (i) and (ii) are calculated using the data provided by [Matzerath \(1985\)](#), variables (iii) to (viii) are digitized data from the 1816–1821 censuses ([Mützell, 1823-1825](#)).

Appendix A.3 Definition and Sources of Outcome Variables

(i) *Return on investment - ROI*: the ratio of profit to capital invested in the (operating) railroad line

(ii) *Number of firms 1849 (log)*: the natural logarithm of the total number of factories located in the city in 1849

(iii) *Firm size 1849 (log)*: the natural logarithm of the ratio of the number of workers to the number of factories located in the city in 1849

(iv) *Child-woman ratio 1849*: the ratio of the number of children under 5 to the number of women aged 15-45.

(v) *Born in city 1871 (share)*: the ratio of the number of city dwellers born outside of the city to the total number of inhabitants in 1871.

Variable (i) is digitized from [Technisches Eisenbahn-Büreau \(1855\)](#), variables (ii) to (iv) are digitized data from the 1849 census [Statistisches Bureau zu Berlin \(1851-1855\)](#), and variable (v) is digitized from [Königliches Statistisches Bureau \(1874\)](#).

Appendix B Alternative Instruments

This section introduces two instrumental variables that are based on a similar principle as the original *SLC* instrument and might serve as a robustness check toward its validity.

Appendix B.1 List's Concept of a German Railroad Network

In 1833 Friedrich List proposed and published a concept for a pan-German railroad network ([List, 1833](#)). In this concept, a system of trunk lines was thought to extend the line Leipzig-Dresden to connect the Kingdom of Saxony with all major cities in the German Reich. List sketched this plan to connect the major German cities to Saxony without considering local conditions and presumably chose nodes without an eye on what lay between them. As can be observed in [Figure A-6](#) the actual network developed in a fairly similar fashion as compared to the proposed system.

We instrument actual railroad access in 1848 with location in a corridor around the straight lines established by List. Such an approach, although similar to our *SLC* approach, might be less exposed to skepticism regarding the choice of our straight-line corridors since nodes were chosen based only on their own merits. Similar to [Atack et al. \(2010\)](#) who use straight lines between nodes proposed in congressional surveys, we draw straight lines between nodes proposed by List for this alternative instrumental variable approach.

We present results using a 3km buffer around the straight lines proposed by List in [Table A-17](#), Panel A. These results are very much in line with our previous findings. Also, when using both instruments together, the results remain within the same ballpark of our baseline specification (not shown).⁵⁸

The drawback of using List’s railroad plan for an IV approach is that it does not include any variation over time making it unfeasible in the panel setting.

Appendix B.2 Least Cost Paths

Least cost paths (*LCP*) constitute another alternative to straight lines between nodes. Using a weighted combination of a 1km x 1km hillshade raster data and a polyline waterway map, we calculate the magnitude of obstacles against railroad building in the terrain profile. The least cost path thus constitutes the most direct route between nodes associated with the lowest obstacles. The hillshade raster data are categorized into eight value bins increasing with the slope profile. As rivers create strong obstacles and require bridge building, they are assigned the value 8 in the combined cost profile.

The weighted cost matrix is then used to calculate the least cost path between nodes. Similar to our *SLC* approach, we create a buffer that expands 1.5 kilometers in each direction around the *LCP*. All cities within this corridor are assigned the value one, whereas cities located outside are assigned the value zero. Thus, we use exogenous variation from *LCP* location to estimate the effect of railroad access on city growth.

⁵⁸Consulting a second instrument allows for a testing of overidentifying restrictions. Using a Sargan-Hansen test, we cannot reject the null hypothesis that the instruments are uncorrelated with the error term across all specifications. Thus, the instruments seem to be valid. However, since both instruments are based on a somewhat similar approach, we cannot conclude with certainty that this test yields valid results.

The notion behind this approach is that cities located on this path are able to gain access to the railroad without any additional cost of railroad building, whereas cities that deviate from the *LCP* might have gained access for endogenous reasons. *LCPs* as well as the combined terrain profile are presented in Figure A-7.

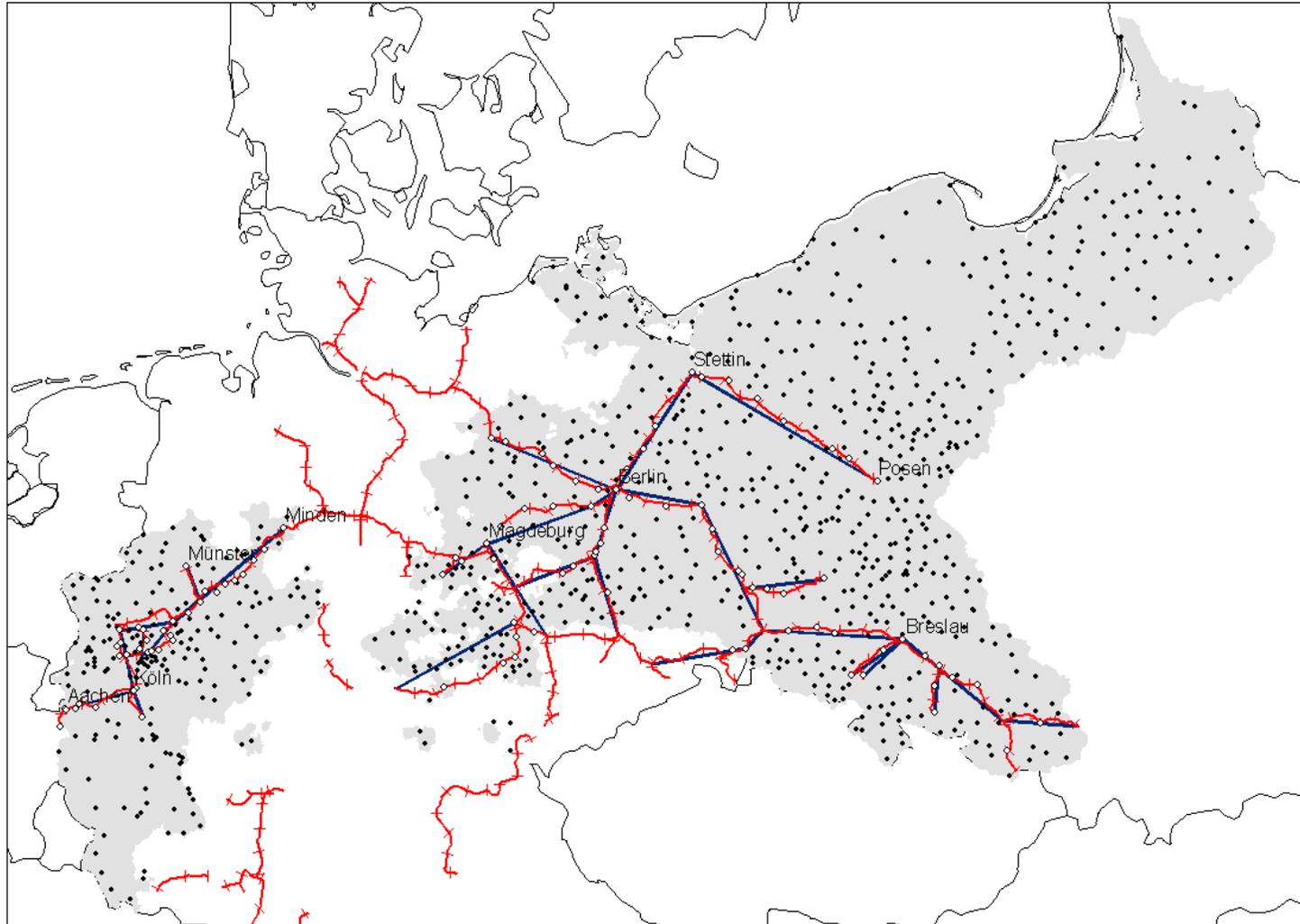
We present estimates using the *LCP* instrument in Table A-17, Panel B. Results are qualitatively similar to our baseline strategy. However, the first stage F-statistic indicates that the instrument is weaker as compared to the original *SLC* instrument.

The *LCP* might be prone to criticism since the location-specific advantages here are much more definite than for *SLC* locations. The worry might be that geographical obstacles might have been very obstructive prior to railroad building and historical trade routes would pick exactly the least cost paths, leading to an omitted variable bias. Thus, we should interpret results from this specification with care.

Appendix C Placebo Lines

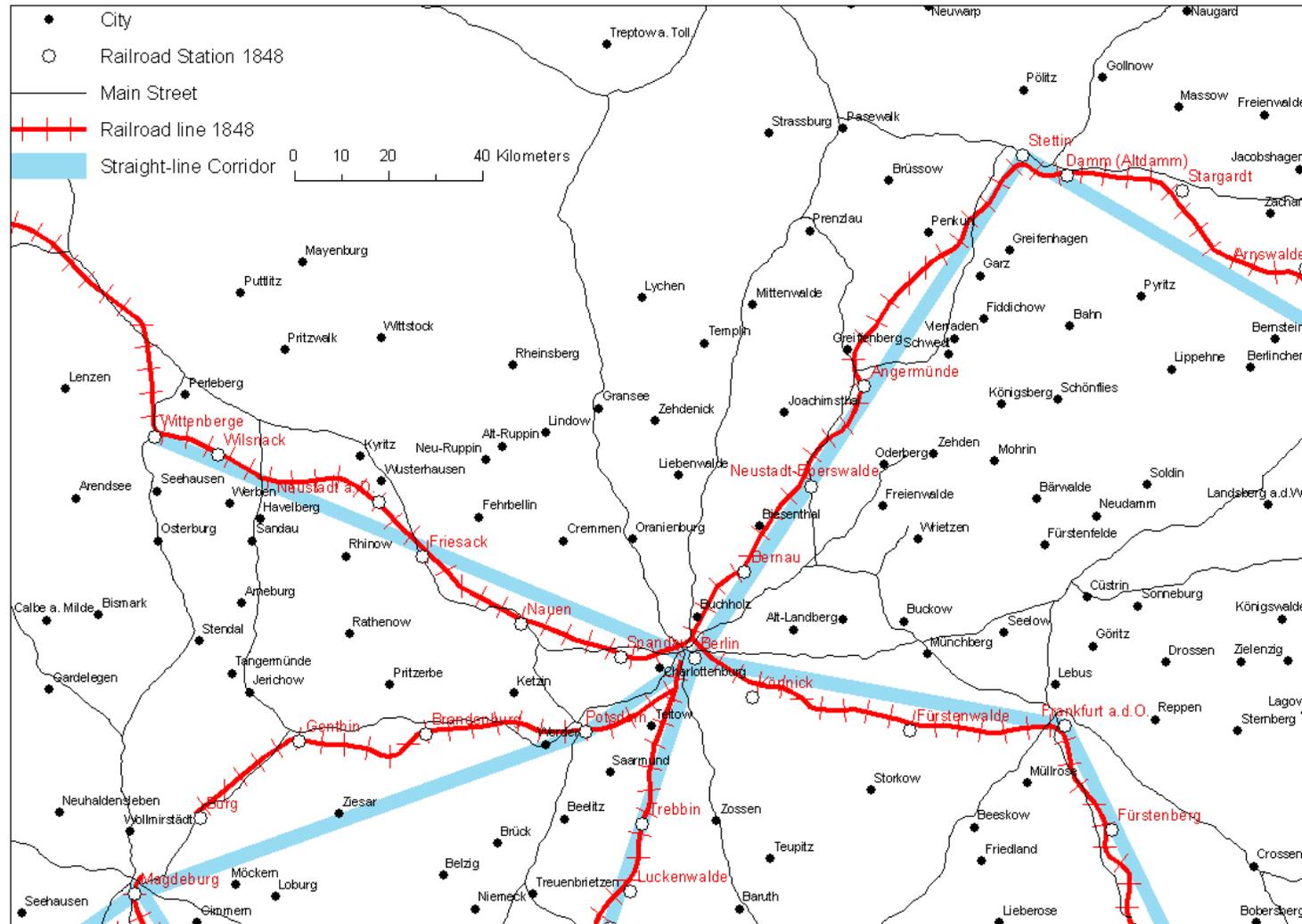
Here, we use unbuilt railroad lines to test whether these “placebo lines” have an effect on our outcome. This test for a counterfactual effect from unbuilt lines uses railroads that were built in the period 1872–1885. Such lines should have no effect on growth prior to their establishment. Table A-18 shows that this is actually the case. The point estimates for all subperiods prior to 1871 are insignificant and are often close to zero. Only in the periods 1867–1871 and 1871–1880 do we find significant positive effects from lines opened between 1872 and 1885. This result confirms our previous findings that railroads were not built in response to previous growth.

Figure 1: German-Prussian Railroad Network, 1848.



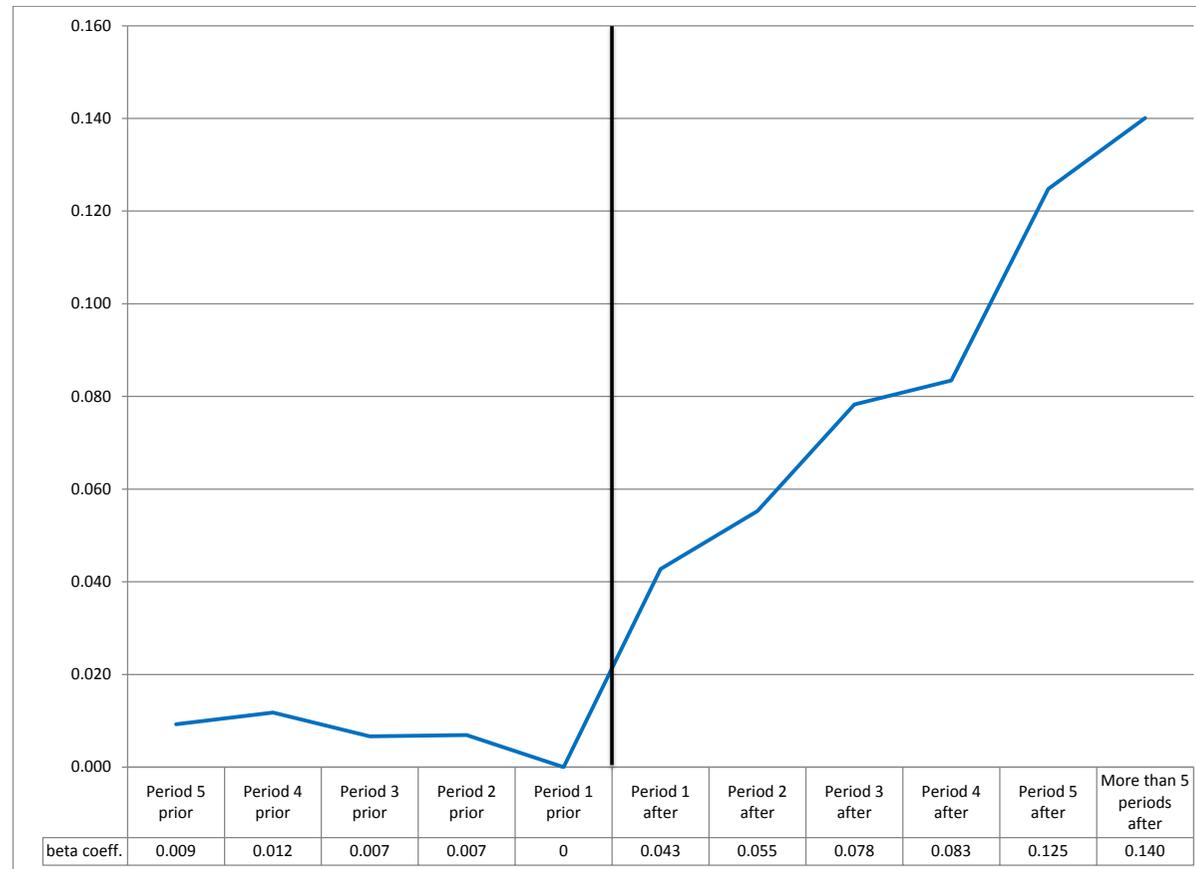
Note: Gray area indicates Prussian territory in 1848. Hash lines indicate railroad routings in the German Reich. Tubes indicate the straight-line corridor using a 1.5km buffer. Hollow circles indicate cities that had a railroad station by 1848. Black circles indicate cities that did not have access by 1848. Source: Own illustration; see main text for details.

Figure 2: Straight-Line Corridors and Actual Railroad Access Around Berlin.



Note: Hash lines indicate railroad routings from Berlin to Stettin, Frankfurt (Oder), Halle, Magdeburg, and Hamburg. Black lines indicate main streets. Tubes indicate the straight-line corridor using a 1.5km buffer. Hollow circles indicate cities that had railroad access by 1848. Black circles indicate cities that did not have access by 1848. Source: Own illustration; see main text for details.

Figure 3: Event Study - City Had Railroad Access Last Year.



Note: The figure plots coefficients from the event study analysis presented in Table 13 in the sample excluding nodes. Coefficients are defined as periods relative to the period before railroad access was established in the city. The specification includes controls for military population (log), distance to next node, and a dummy for incorporations, as well as city and year fixed effects.

Table 1: Prussian Railroad Network Expansion

Year	State owned State administration	Private owned State administration	Private owned Private administration	Total
1838	-	-	34.7	34.7
1840	-	-	232.2	232.2
1845	70.0	-	1308.6	1378.6
1850	657.8	480.9	2729.9	3868.6
1855	1859.3	510.7	2719.4	5089.4
1860	2550.4	1278.0	3340.9	7169.3
1865	2986.5	1430.2	4237.5	8654.2
1870	3505.7	1820.5	6196.8	11523.0
1875	4390.9	2735.5	9750.7	16877.1
1880	11455.3	3649.5	5243.6	20348.4

Note: Length of the railroad network in kilometers at the end of the specified year. Source: [Königlich Preussisches Statistisches Bureau \(1883, p. 161\)](#)

Table 2: Railroad Lines Built by 1848

Connection (1)	Built (2)	Length in km (3)	Passengers (4)	Freight in cwt (5)	Share of straight lines (6)
Berlin-Stettin	1843	134	279,768	1,302,519	81.7%
Stettin-Posen	1847	205	172,234	727,245	80.0%
Berlin-Frankfurt-Breslau	1843/45	389	632,899	1,730,987	79.3%
Hansdorf-Glogau	1847	72	108,697	204,899	67.2%
Breslau-Schweidnitz-Freiburg	1844	67	193,996	1,314,144	80.9%
Breslau-Myslowitz	1843	198	376,910	2,109,013	79.0%
Brieg-Neisse	1847	44	85,533	211,993	73.7%
Kosel-Oderberg	1846	54	76,098	338,726	82.8%
Berlin-Hamburg	1846	286	523,145	1,831,190	83.0%
Magdeburg-Leipzig	1840	119	725,495	2,294,189	77.5%
Berlin-Potsdam-Magdeburg	1838/46	147	739,608	869,727	81.3%
Magdeburg-Halberstadt-Thale	1843	58	320,215	1,627,154	69.9%
Berlin-Jüterbog-Halle	1841/48	232	330,024	1,098,306	78.6%
Halle-Gerstungen	1846	165	632,943	1,052,009	62.3%
Köln-Minden	1846	267	1,451,703	3,292,257	83.0%
Münster-Hamm	1848	35	134,990	120,095	88.4%
Steele-Vohwinkel	1831/47	33	116,834	1,190,570	40.1%
Elberfeld-Dortmund	1848	58	553,027	2,023,728	53.5%
Düsseldorf-Elberfeld	1842	26	331,112	1,960,077	60.1%
Köln-Bonn	1844	29	608,937	71,509	71.3%
Köln-Aachen	1841	86	514,430	6,033,504	72.4%

Note: Presented data cover the year 1848. Freight is measured in Prussian hundredweights. Source: [Technisches Eisenbahn-Büreau \(1855\)](#)

Table 3: Major Prussian Cities and Nodes

#	City	Population 1837	Population 1849	Node 1848	List's Plan
1	Berlin	265394	401154	1	1
2	Breslau	88869	104222	1	1
3	Köln	66179	88356	1	1
4	Königsberg	64200	70198	-	-
5	Danzig	56257	58012	-	1
6	Magdeburg	51344	65295	1	1
7	Aachen	38878	48687	1	-
8	Posen	32456	37964	1	-
9	Stettin	31093	42930	1	1
10	Barmen	28975	35984	-	-
11	Elberfeld	26770	38361	1	-
12	Halle a.d.S.	26447	32493	1	1
13	Potsdam	25560	31394	1	-
14	Erfurt	24308	26663	-	-
15	Frankfurt a.d.O.	23378	28460	1	-
16	Krefeld	23008	36111	-	-
17	Düsseldorf	21858	23860	1	-
18	Münster	19763	21275	1	-
19	Elbing	18725	21386	-	-
20	Halberstadt	17227	18770	1	-

Note: The Table shows the 20 largest Prussian cities according to their population size in 1837, before the building of the first railroad line in Prussia. The column 'List's Plan' indicates if a city had been assigned the role of a node in List's original railroad plan of 1833. The column 'Node' indicates if a city was an actual terminal or junction station of a railroad line in 1848.

Table 4: Descriptive Statistics by Railroad Access 1848

	Nodes		Railroad Cities excluding Nodes		Non-Railroad Cities		Difference in means between (2) and (3)
	Obs.	Mean	Obs.	Mean	Obs.	Mean	(2) and (3)
	(1)		(2)		(3)		(4)
Annual Population Growth Rates							
1816-21	29	0.016 (0.013)	67	0.020 (0.018)	824	0.019 (0.021)	0.001 (0.003)
1821-31	28	0.015 (0.010)	68	0.014 (0.010)	840	0.011 (0.013)	0.002 (0.002)
1831-37	28	0.016 (0.011)	76	0.015 (0.013)	854	0.013 (0.013)	0.002 (0.002)
1837-40	28	0.021 (0.017)	76	0.018 (0.019)	860	0.016 (0.018)	0.002 (0.002)
1840-43	28	0.018 (0.010)	75	0.017 (0.014)	861	0.014 (0.016)	0.003 (0.002)
1843-46	27	0.024 (0.013)	75	0.023 (0.013)	867	0.013 (0.015)	0.010*** (0.002)
1846-49	29	0.011 (0.016)	75	0.007 (0.015)	870	0.003 (0.017)	0.004** (0.002)
1849-52	28	0.026 (0.016)	76	0.019 (0.016)	867	0.013 (0.016)	0.006*** (0.002)
1852-55	29	0.014 (0.020)	75	0.012 (0.021)	862	0.003 (0.014)	0.009*** (0.002)
1855-58	26	0.020 (0.024)	72	0.015 (0.015)	856	0.009 (0.017)	0.006*** (0.002)
1858-61	29	0.019 (0.014)	74	0.015 (0.016)	867	0.012 (0.014)	0.003* (0.002)
1861-64	28	0.025 (0.017)	74	0.021 (0.021)	863	0.011 (0.015)	0.010*** (0.002)
1864-67	28	0.021 (0.017)	75	0.014 (0.020)	857	0.003 (0.017)	0.012*** (0.002)
1867-71	29	0.022 (0.016)	75	0.014 (0.018)	858	0.005 (0.016)	0.010*** (0.002)

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Table 4 – Continued

	Nodes		Railroad Cities excluding Nodes		Non-Railroad Cities		Difference in means between (2) and (3) (4)
	Obs.	Mean	Obs.	Mean	Obs.	Mean	
	(1)		(2)		(3)		
Control Variables (post Railroad access)							
Straight-line corridor = 1	29	1.000 (0.000)	76	0.250 (0.436)	871	0.011 (0.107)	0.239*** (0.019)
Street access = 1	29	0.931 (0.258)	76	0.592 (0.495)	871	0.393 (0.489)	0.199*** (0.059)
Waterway access = 1	29	0.552 (0.506)	76	0.276 (0.450)	871	0.191 (0.393)	0.086* (0.048)
Civilian population (log)	29	9.853 (1.104)	76	8.357 (0.777)	871	7.793 (0.692)	0.564*** (0.084)
Military population (log)	29	6.345 (2.742)	76	3.159 (2.400)	871	2.265 (1.944)	0.894*** (0.237)
Factory workers (share)	29	0.072 (0.097)	76	0.060 (0.088)	871	0.038 (0.120)	0.022 (0.014)
Mining (county level)	29	0.310 (0.471)	76	0.197 (0.401)	871	0.095 (0.294)	0.102*** (0.036)
Large farming (county level)	29	0.015 (0.013)	76	0.018 (0.013)	871	0.027 (0.026)	-0.009*** (0.003)
Age composition	29	0.292 (0.041)	76	0.334 (0.033)	871	0.343 (0.037)	-0.009** (0.004)
School enrolment rate	29	0.796 (0.150)	76	0.928 (0.177)	871	0.916 (0.294)	0.013 (0.034)
Distance to node	29	0.000 (0.000)	76	0.281 (0.198)	871	0.803 (0.887)	-0.523*** (0.102)

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Table 4 – Continued

	Nodes		Railroad Cities excluding Nodes		Non-Railroad Cities		Difference in means between (2) and (3) (4)
	Obs.	Mean	Obs.	Mean	Obs.	Mean	
	(1)		(2)		(3)		
Matching Variables (pre Railroad access)							
City size 1837 (log)	28	9.694 (1.058)	76	8.168 (0.769)	861	7.665 (0.683)	0.502*** (0.083)
Annual city growth 1821-37	28	0.015 (0.009)	68	0.015 (0.006)	836	0.012 (0.010)	0.003** (0.001)
Merchants 1819 (p.c.)	27	0.016 (0.005)	62	0.016 (0.011)	793	0.015 (0.010)	0.002 (0.001)
Looms 1819 (p.c.)	27	0.014 (0.020)	62	0.017 (0.019)	797	0.019 (0.024)	-0.002 (0.003)
Protestants 1816 (share)	27	0.623 (0.374)	65	0.786 (0.326)	828	0.626 (0.400)	0.160*** (0.051)
Private dwellings 1821 (p.c.)	28	0.104 (0.043)	67	0.130 (0.027)	845	0.138 (0.033)	-0.009** (0.004)
Commercial buildings 1821 (p.c.)	28	0.005 (0.006)	67	0.005 (0.005)	844	0.005 (0.006)	-0.000 (0.001)
Insurance-value of buildings against fire 1821 (log)	28	6.380 (0.829)	67	5.806 (0.640)	819	5.385 (0.710)	0.421*** (0.090)

Note: Summary statistics for the 1849 cross section by treatment status. The number of observations changes due to missing information in the period before 1849 or due to the exclusion of outliers in the annual population growth rates. The ‘difference between means’ is calculated using a two-sided test. Standard deviations in parentheses. Significance: *** p<0.01, ** p<0.05, * p<0.1. Source: See main text and [Appendix A](#) for data sources and details.

Table 5: Railroad Access and Growth in a Cross Section of Cities

DepVar: Population growth rate 1849-71	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Rail access 1838-48 = 1	0.010*** (0.002)	0.012*** (0.002)	0.012*** (0.002)	0.011*** (0.002)	0.010*** (0.002)	0.011*** (0.002)	0.011*** (0.002)
Later access = 1		0.007*** (0.001)	0.007*** (0.001)	0.006*** (0.001)	0.006*** (0.001)	0.006*** (0.001)	0.005*** (0.001)
Street access = 1			0.001 (0.001)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)	0.000 (0.001)
Waterway access = 1			-0.002 (0.001)	-0.002* (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)
Annual city growth 1831-37				0.097*** (0.034)	0.082*** (0.027)	0.092*** (0.024)	0.99*** (0.026)
Civilian population (log)				0.001 (0.001)	-0.000 (0.001)	0.001 (0.001)	0.000 (0.001)
Military population (log)				-0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Factory workers (share)					0.029** (0.011)	0.029** (0.011)	0.025** (0.011)
Mining (county level)					0.006*** (0.002)	0.006*** (0.002)	0.007*** (0.002)
Large farming (county level)					0.048** (0.020)	0.038** (0.016)	0.045** (0.015)
Age composition						0.005 (0.015)	-0.002 (0.014)
School enrolment rate						0.006 (0.005)	-0.001 (0.001)
Distance to next node						0.002*** (0.001)	0.001*** (0.000)
Constant	0.009*** (0.001)	0.007*** (0.001)	0.007*** (0.001)	-0.001 (0.007)	0.004 (0.007)	-0.009 (0.010)	-0.000 (0.008)
Observations	970	970	970	956	956	956	945
R-squared	0.06	0.10	0.10	0.12	0.19	0.21	0.25

Note: The table shows city-level OLS estimates regressing annual population growth for the period 1849–1871 on railroad access in 1848. Column 7 excludes outliers that exceed a growth rate of +/- 10%. If not specified otherwise, all control variables refer to the year 1849. Standard errors, clustered at the county level, in parentheses. Significance: *** p<0.01, ** p<0.05, * p<0.1. Source: See main text and [Appendix A](#) for data sources and details.

Table 6: Railroad Access and Growth in Different Periods

DepVar: Population growth rate	Main periods		Subperiods						
	1831-37 (1)	1849-71 (2)	1849-52 (3)	1852-55 (4)	1855-58 (5)	1858-61 (6)	1861-64 (7)	1864-67 (8)	1867-71 (9)
Rail access 1838-48	0.002 (0.002)	0.009*** (0.002)	0.006*** (0.002)	0.008*** (0.003)	0.006*** (0.002)	0.004** (0.002)	0.011*** (0.003)	0.010*** (0.002)	0.010*** (0.002)
Later access = 1		0.005*** (0.001)	0.009** (0.004)	0.008** (0.003)	0.002 (0.003)	0.002 (0.002)	0.003* (0.002)	0.005*** (0.002)	0.006*** (0.001)
Street access = 1	0.001 (0.001)	0.001 (0.001)	-0.000 (0.001)	0.000 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	0.002 (0.001)	-0.000 (0.001)
Waterway access = 1	-0.002* (0.001)	-0.001 (0.001)	0.002* (0.001)	0.001 (0.001)	0.001 (0.002)	0.001 (0.001)	-0.001 (0.001)	-0.003* (0.002)	-0.004*** (0.001)
Annual city growth 1816-31	-0.080* (0.045)								
Annual city growth 1831-37		0.083*** (0.022)	0.083** (0.036)	0.051 (0.040)	0.076* (0.043)	0.034 (0.029)	0.115*** (0.033)	0.089** (0.036)	0.044 (0.030)
Civilian population (log)	0.001 (0.001)	0.000 (0.001)	0.001 (0.002)	0.002* (0.001)	0.003** (0.001)	0.001 (0.001)	0.002* (0.001)	0.001 (0.002)	-0.000 (0.001)
Military population (log)	0.000 (0.000)	0.000* (0.000)	0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)	0.001** (0.000)	0.001*** (0.000)
Factory workers (share)	0.008** (0.004)	0.011** (0.005)	0.010* (0.006)	0.012*** (0.004)	0.016** (0.007)	0.009 (0.007)	0.005 (0.004)	0.015*** (0.004)	0.015*** (0.003)
Mining (county level)	-0.002 (0.001)	0.007*** (0.002)	0.006*** (0.002)	0.009*** (0.003)	0.011*** (0.002)	0.000 (0.002)	0.008*** (0.002)	0.005** (0.002)	0.005** (0.002)
Large farming (county level)	-0.003 (0.018)	0.043*** (0.014)	0.125*** (0.028)	0.065** (0.026)	0.057** (0.029)	0.054** (0.021)	0.049** (0.023)	-0.010 (0.025)	-0.003 (0.022)
Age composition	0.058*** (0.015)	0.014 (0.010)	-0.051** (0.021)	0.014 (0.021)	0.037* (0.019)	0.029* (0.016)	-0.016 (0.016)	-0.025 (0.017)	0.048*** (0.017)
School enrolment rate	-0.002 (0.002)	-0.001 (0.001)	0.001 (0.002)	0.002 (0.002)	-0.001 (0.002)	-0.001 (0.002)	-0.002 (0.002)	-0.003 (0.003)	-0.004 (0.003)
Distance to next node	-0.001 (0.001)	0.001*** (0.000)	0.001 (0.001)	0.000 (0.001)	0.003*** (0.001)	0.001 (0.001)	0.003*** (0.001)	0.001 (0.001)	-0.000 (0.001)
Constant	-0.012 (0.010)	-0.005 (0.008)	0.017 (0.014)	-0.021* (0.012)	-0.030** (0.012)	-0.008 (0.010)	-0.002 (0.011)	-0.003 (0.015)	-0.011 (0.014)
Observations	898	906	929	924	914	926	924	919	919
R-squared	0.04	0.22	0.09	0.09	0.10	0.04	0.13	0.14	0.14

Note: The table shows city-level OLS estimates regressing annual population growth for different periods on railroad access in 1848. Standard errors, clustered at the county level, in parentheses. Significance: *** p<0.01, ** p<0.05, * p<0.1. Source: See main text and [Appendix A](#) for data sources and details.

Table 7: Instrumenting Railroad Access with Straight-Line Corridors

DepVar: Population growth rate	Main periods		Subperiods						
	1831-37 (1)	1849-71 (2)	1849-52 (3)	1852-55 (4)	1855-58 (5)	1858-61 (6)	1861-64 (7)	1864-67 (8)	1867-71 (9)
Panel A: First stage - Actual railroad access and location within straight-line corridor.									
Straight-line corridor = 1	0.531*** (0.096)	0.553*** (0.090)	0.547*** (0.097)	0.567*** (0.093)	0.578*** (0.094)	0.525*** (0.096)	0.508*** (0.099)	0.507*** (0.097)	0.491*** (0.097)
Controls	yes								
Observations	898	906	929	924	914	926	924	919	919
R-squared	0.20	0.27	0.20	0.21	0.22	0.22	0.21	0.23	0.25
Panel B: Second stage - Population growth rate and actual railroad access.									
Rail access 1838-48	-0.000 (0.006)	0.021*** (0.006)	0.015** (0.006)	0.017*** (0.006)	0.020** (0.009)	0.011** (0.005)	0.021*** (0.007)	0.021** (0.010)	0.022*** (0.007)
Controls	yes								
Observations	898	906	929	924	914	926	924	919	919
Kleibergen-Paap F statistic	30.39	38.18	31.57	37.41	37.78	29.74	26.46	27.46	25.71
Panel C: Reduced form - Population growth rate and location within straight-line corridor.									
Straight-line corridor = 1	-0.000 (0.003)	0.012*** (0.004)	0.008*** (0.003)	0.010*** (0.004)	0.012** (0.005)	0.006** (0.002)	0.011*** (0.004)	0.011** (0.005)	0.011*** (0.004)
Controls	yes								
Observations	898	906	929	924	914	926	924	919	919
R-squared	0.04	0.20	0.09	0.08	0.10	0.04	0.11	0.13	0.12

Note: The table shows city-level IV-estimates regressing annual population growth for different periods on railroad access in 1848. Railroad access is instrumented by *SLC* location. Standard errors, clustered at the county level, in parentheses. Controls include: additional access dummy, street access dummy, waterway access dummy, annual city growth 1816–1831 (column 1), annual city growth 1831–1837 (columns 2-9), civilian population (log), military population (log), factory workers (share), mining (county level), large farming (county level), age composition, school enrollment rate, distance to next node (keyser), and a constant. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Source: See main text and [Appendix A](#) for data sources and details.

Table 8: Testing for Exogeneity of the Instrument

Dependent variable: Straight-line corridor = 1	Full sample		Matched sample	
	β (1)	SE (2)	β (3)	SE (4)
Control Variables				
Street access = 1	0.022*	(0.012)	0.058	(0.065)
Waterway access = 1	0.015	(0.016)	0.086	(0.078)
Annual city growth 1831-37	-0.209	(0.415)	-1.908	(2.596)
Civilian population (log)	0.011	(0.008)	-0.008	(0.053)
Military population (log)	0.001	(0.003)	-0.009	(0.016)
Factory workers (share)	0.028	(0.038)	0.412	(0.424)
Mining (county level)	0.068**	(0.035)	0.116	(0.152)
Large farming (county level)	-0.485***	(0.148)	-1.707	(1.041)
Age composition	-0.100	(0.160)	-0.044	(1.073)
School enrolment rate	0.049	(0.031)	0.104	(0.128)
Distance to Node	-0.023***	(0.005)	-0.090***	(0.026)
Matching Variables				
Annual city growth 1821-37	0.010	(0.008)	-0.027	(0.050)
City size 1837 (log)	1.021*	(0.524)	1.345	(4.486)
Merchants 1819 (p.c.)	0.465	(0.679)	0.685	(3.612)
Looms 1819 (p.c.)	-0.175	(0.206)	0.057	(1.790)
Protestants 1816 (share)	0.034***	(0.011)	0.095	(0.068)
Private dwellings 1821 (p.c.)	-0.022	(0.162)	0.228	(0.976)
Commercial buildings 1821 (p.c.)	-1.292***	(0.417)	-3.961	(4.037)
Insurance Value of Buildings 1821 (log)	0.010*	(0.006)	-0.010	(0.040)
Number of Observations	947/859		623 total, 106 weighted	

Note: The table shows bivariate regression of straight-line corridor location with all control and matching variables introduced in Table 4. Columns 1 to 2 present results in the full sample of cities excluding nodes. Columns 3 to 4 present results in a matched sample of cities (excluding nodes), based on weights resulting from the kernel propensity score matching approach applied in Table 10, Panel C, column 2. Standard errors, clustered at the county level, in parentheses. Constant omitted. Significance: *** p<0.01, ** p<0.05, * p<0.1. Source: See main text and [Appendix A](#) for data sources and details.

Table 9: Descriptive Statistics after Propensity Score Matching

	(1)	(2)	(3)
	Railroad Cities excluding Nodes Mean	Non-Railroad Cities Mean	Difference in means between (3) and (4)
Panel A: Radius matched sample			
Annual city growth 1849-71	0.012 (0.008)	0.006 (0.006)	0.006*** (0.002)
City size 1837 (log)	7.823 (0.583)	7.843 (0.491)	-0.020 (0.127)
Annual city growth 1821-37	0.015 (0.007)	0.014 (0.007)	0.001 (0.002)
Merchants 1819 (p.c.)	0.014 (0.010)	0.016 (0.011)	-0.002 (0.002)
Looms 1819 (p.c.)	0.021 (0.022)	0.021 (0.021)	0.000 (0.005)
Protestants 1816 (share)	0.788 (0.351)	0.755 (0.354)	0.033 (0.083)
Private dwellings 1821 (p.c.)	0.138 (0.026)	0.139 (0.031)	-0.001 (0.007)
Commercial buildings 1821 (p.c.)	0.005 (0.004)	0.004 (0.005)	0.001 (0.001)
Insurance-value of buildings against fire 1821 (log)	5.557 (0.587)	5.572 (0.586)	-0.015 (0.138)
Observations	36	208	244
Weighted Observations	36	36	72
Panel B: Kernel matched sample			
Annual city growth 1849-71	0.014 (0.012)	0.007 (0.009)	0.007*** (0.002)
City size 1837 (log)	8.086 (0.661)	7.945 (0.649)	0.141 (0.127)
Annual city growth 1821-37	0.015 (0.006)	0.014 (0.009)	0.001 (0.002)
Merchants 1819 (p.c.)	0.016 (0.010)	0.015 (0.010)	0.000 (0.002)
Looms 1819 (p.c.)	0.017 (0.020)	0.018 (0.021)	-0.001 (0.004)
Protestants 1816 (share)	0.780 (0.342)	0.744 (0.358)	0.036 (0.068)
Private dwellings 1821 (p.c.)	0.132 (0.027)	0.134 (0.035)	-0.002 (0.006)
Commercial buildings 1821 (p.c.)	0.004 (0.004)	0.004 (0.006)	0.000 (0.001)
Insurance-value of buildings against fire 1821 (log)	5.659 (0.586)	5.585 (0.732)	0.074 (0.129)
Observations	53	570	623
Weighted Observations	53	53	106

Note: The table presents descriptive statistics for matching variables by treatment status. The difference in means is calculated using a two-sided test. Panel A presents results in a matched sample based on weights resulting from the radius propensity score matching approach applied in Table 10, Panel B, column 2. Panel B presents results in a matched sample based on weights resulting from the kernel propensity score matching approach applied in Table 10, Panel C, column 2. Standard deviations in parentheses. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Source: See main text and [Appendix A](#) for data sources and details.

Table 10: IV-Estimations in Matched Samples

DepVar: Population growth rate	Main periods		Subperiods						
	1831-37 (1)	1849-71 (2)	1849-52 (3)	1852-55 (4)	1855-58 (5)	1858-61 (6)	1861-64 (7)	1864-67 (8)	1867-71 (9)
Panel A: IV estimates - Sample of cities smaller than 3000 inhabitants in 1837.									
Rail access 1838-48	-0.010 (0.007)	0.011** (0.004)	0.009 (0.007)	0.009 (0.006)	0.011* (0.006)	0.013** (0.006)	0.026** (0.010)	0.016 (0.011)	0.014* (0.007)
Observations	631	640	658	656	644	656	654	648	648
Kleibergen-Paap F statistic	16.56	21.27	15.76	19.15	22.54	13.98	13.51	13.28	12.93
Panel B: IV estimates - Sample and weights obtained from PSM on pre-railroad variables (radius: caliper=0.001).									
Rail access 1838-48	-0.003 (0.010)	0.011** (0.004)	0.011 (0.009)	0.005 (0.011)	0.018*** (0.007)	0.007 (0.008)	0.031* (0.018)	0.028 (0.020)	0.039** (0.015)
Observations	323	244	328	285	342	278	262	232	191
Weighted Obs.	92	72	88	80	84	82	68	74	62
Kleibergen-Paap F statistic	41.75	14.91	36.26	40.14	38.27	44.04	35.15	23.67	41.00
Panel C: IV estimates - Sample and weights obtained from PSM on pre-railroad variables (kernel).									
Rail access 1838-48	-0.004 (0.009)	0.017** (0.008)	0.014* (0.008)	0.010 (0.010)	0.022** (0.011)	0.007 (0.008)	0.020* (0.011)	0.020 (0.017)	0.024*** (0.009)
Observations	810	623	803	786	759	724	705	676	619
Weighted Obs.	118	106	118	112	112	110	108	104	104
Kleibergen-Paap F statistic	62.76	78.38	74.99	83.58	80.90	71.35	75.67	72.03	61.44
Panel D: IV estimates - Sample and weights obtained from PSM on pre-railroad variables and geography (kernel).									
Rail access 1838-48	-0.004 (0.010)	0.017** (0.008)	0.012 (0.008)	0.010 (0.010)	0.022** (0.011)	0.008 (0.008)	0.020* (0.011)	0.021 (0.017)	0.025*** (0.009)
Observations	808	620	800	786	759	724	705	678	616
Weighted Obs.	114	100	112	112	112	110	108	108	98
Kleibergen-Paap F statistic	61.77	41.71	80.35	80.30	76.08	57.67	56.09	55.26	32.32

Note: The table shows city-level IV-estimates regressing annual population growth for different periods on railroad access in 1848 in matched and weighted samples. Railroad access is instrumented by straight-line corridor location. Standard errors, clustered at the county level, in parentheses. All regressions include the full set of baseline controls: additional access dummy, street access dummy, waterway access dummy, annual city growth 1816–1831 (column 1), annual city growth 1831–1837 (columns 2-9), civilian population (log), military population (log), factory workers (share), mining (county level), large farming (county level), age composition, school enrollment rate, distance to next node, and a constant. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Source: See main text and [Appendix A](#) for data sources and details.

Table 11: The Impact of Railroad Access: Panel Estimates

DepVar: (ln) Population	OLS					
	Pooled (1)	FE (2)	Nodes (3)	CFE (4)	East (5)	West (6)
Railroad access = 1	0.088*** (0.013)	0.077*** (0.013)	0.056*** (0.012)	0.064*** (0.018)	0.049*** (0.010)	0.064** (0.030)
Nodes excluded	N	N	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
City fixed effects	N	Y	Y	Y	Y	Y
County × year fixed effects	N	N	N	Y	N	N
Observations	7,737	7,737	7,472	7,472	5,842	1,630
Number of cities	978	978	962	962	746	216
R-squared		0.40	0.38	0.72	0.54	0.27
DepVar: (ln) Population	OLS				IV	
	Geography (7)	Small (8)	Large (9)	State (10)	FS (11)	SS (12)
Railroad access = 1	0.033** (0.014)	0.059*** (0.017)	0.033* (0.017)	0.043*** (0.012)		0.077* (0.041)
State owned = 1				0.009 (0.020)		
State administered = 1				0.057 (0.035)		
Straight-line corridor = 1					0.513*** (0.068)	
Nodes excluded	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
City fixed effects	Y	Y	Y	Y	Y	Y
County × year fixed effects	N	N	N	N	N	N
Observations	2,883	5,261	980	7,472	7,472	7,472
Number of cities	420	667	139	962	962	962
Kleibergen-Paap F statistic						57.09
R-squared	0.32	0.33	0.56	0.38	0.19	0.38

Note: Panel estimates at the city-year level 1840–1861 using triennial data. Railroad access indicates if a city had access to the railroad network in a previous year. Column ‘FE’ introduces county fixed effects, column ‘Nodes’ excludes the nodes of the network, column ‘CFE’ introduces a full set of interactions of county fixed effects with time period fixed effects, columns ‘East’ and ‘West’ restrict to regional samples, ‘Geography’ restricts to cities close to the *SLC*, columns ‘Small’ and ‘Big’ distinguish by city size prior to 1837, ‘State’ introduces dummies for state involvement. Columns ‘FS’ and ‘SS’ indicate first-stage and second-stage estimates, instrumenting actual railroad access with straight-line corridors. Further controls: military population (log), distance to next node, and a dummy for incorporations. Standard errors, clustered at the county level, in parentheses. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Source: See main text and [Appendix A](#) for data sources and details.

Table 12: Year of Establishment and the Profitabilty of Railroad Lines

DepVar: ROI in	Year 1 (1)	Year 2 (2)	Year 3 (3)	Year 4 (4)	Year 5 (5)	Year 6 (6)	Year 7 (7)	Year 8 (8)	Year 9 (9)
Year built	-0.061 (0.067)	-0.219*** (0.075)	-0.225*** (0.067)	-0.140* (0.069)	-0.081 (0.074)	-0.083 (0.068)	-0.074 (0.066)	-0.055 (0.071)	-0.092 (0.066)
Observations	34	34	33	34	34	34	34	34	34
R-squared	0.04	0.23	0.28	0.13	0.04	0.05	0.03	0.02	0.04

Note: The Table shows bivariate regressions of return on investment (*ROI*), normalized to years after establishment, on year of establishment of railroad lines for all lines built until 1860. Standard errors, clustered at the county level, in parentheses. Constant omitted. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Source: See main text and [Appendix A](#) for data sources and details.

Table 13: Non-linear Panel Estimates

DepVar: (ln) Population	Full (1)	Nodes (2)	Small (3)
5 periods prior to access	0.007 (0.006)	0.009 (0.006)	0.011 (0.008)
4 periods prior to access	0.007 (0.008)	0.012 (0.008)	0.015 (0.015)
3 periods prior to access	0.002 (0.009)	0.007 (0.009)	0.014 (0.017)
2 periods prior to access	0.003 (0.008)	0.007 (0.008)	0.009 (0.015)
Access for 1 period	0.051*** (0.012)	0.043*** (0.012)	0.052** (0.021)
Access for 2 periods	0.069*** (0.015)	0.055*** (0.015)	0.063** (0.025)
Access for 3 periods	0.098*** (0.018)	0.078*** (0.017)	0.083*** (0.028)
Access for 4 periods	0.113*** (0.026)	0.083*** (0.019)	0.078*** (0.019)
Access for 5 periods	0.146*** (0.022)	0.125*** (0.024)	0.123*** (0.036)
Access for more than 5 periods	0.162*** (0.026)	0.140*** (0.039)	0.088*** (0.029)
Year fixed effects	Y	Y	Y
City fixed effects	Y	Y	Y
Observations	7,737	7,472	5,261
Number of cities	978	962	667
R-squared	0.41	0.39	0.33

Note: Panel estimates at the city-year level using triennial data for the period 1840–1861. Column ‘Full’ denotes the full sample, column ‘Nodes’ excludes the nodes of the network, column ‘Small’ denotes a sample of cities smaller than 3,000 inhabitants in 1837. Further controls: military population (log), distance to next node, and a dummy for incorporations. Standard errors, clustered at the county level, in parentheses. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Source: See main text and [Appendix A](#) for data sources and details.

Table 14: Railroads, Industrial Development and Sources of Population Growth

DepVar: Sample:	Firm size 1849 (log)			Number of firms 1849 (log)		
	Full		Matched	Full		Matched
	OLS (1)	IV (2)	IV (3)	OLS (4)	IV (5)	IV (6)
Rail access 1838-48	0.312** (0.121)	0.743** (0.338)	1.768** (0.809)	-0.161** (0.075)	-0.203 (0.144)	-0.134 (0.167)
Rail access 1849-71	0.006 (0.079)	0.086 (0.097)		0.072 (0.062)	0.064 (0.063)	
Controls	yes	yes	yes	yes	yes	yes
Observations	922	922	559	924	924	561
Weighted Obs.			72			72
Kleibergen-Paap F statistic		28.29	40.78		28.29	40.69
DepVar: Sample:	Child-woman ratio 1849			Born in city 1871 (share)		
	Full		Matched	Full		Matched
	OLS (7)	IV (8)	IV (9)	OLS (10)	IV (11)	IV (12)
Rail access 1838-48	-0.001 (0.013)	0.005 (0.041)	-0.034 (0.051)	0.067*** (0.013)	0.129*** (0.038)	0.106 (0.075)
Rail access 1849-71	-0.007 (0.009)	-0.006 (0.011)		0.030*** (0.008)	0.042*** (0.011)	
Controls	yes	yes	yes	yes	yes	yes
Observations	934	934	571	926	926	564
Weighted Obs.			72			72
Kleibergen-Paap F statistic		28.56	47.64		28.58	45.83

Note: Estimations at the city level for different outcomes. Columns 1, 4, 7, and 10 show OLS estimates in the full sample excluding nodes. Columns 2, 5, 8, and 11 show second-stage results, instrumenting Rail access 1838-48 with *SLC*. Columns 3, 6, 9, and 12 show IV estimates in kernel-matched samples. Standard errors, clustered at the county level, in parentheses. Controls include: street access dummy, waterway access dummy, annual city growth 1831–1837, civilian population (log), military population (log), mining (county level), factory workers (share - only columns 7-12), large farming (county level), age composition, school enrollment rate, distance to node, and a constant. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Source: See main text and [Appendix A](#) for data sources and details.

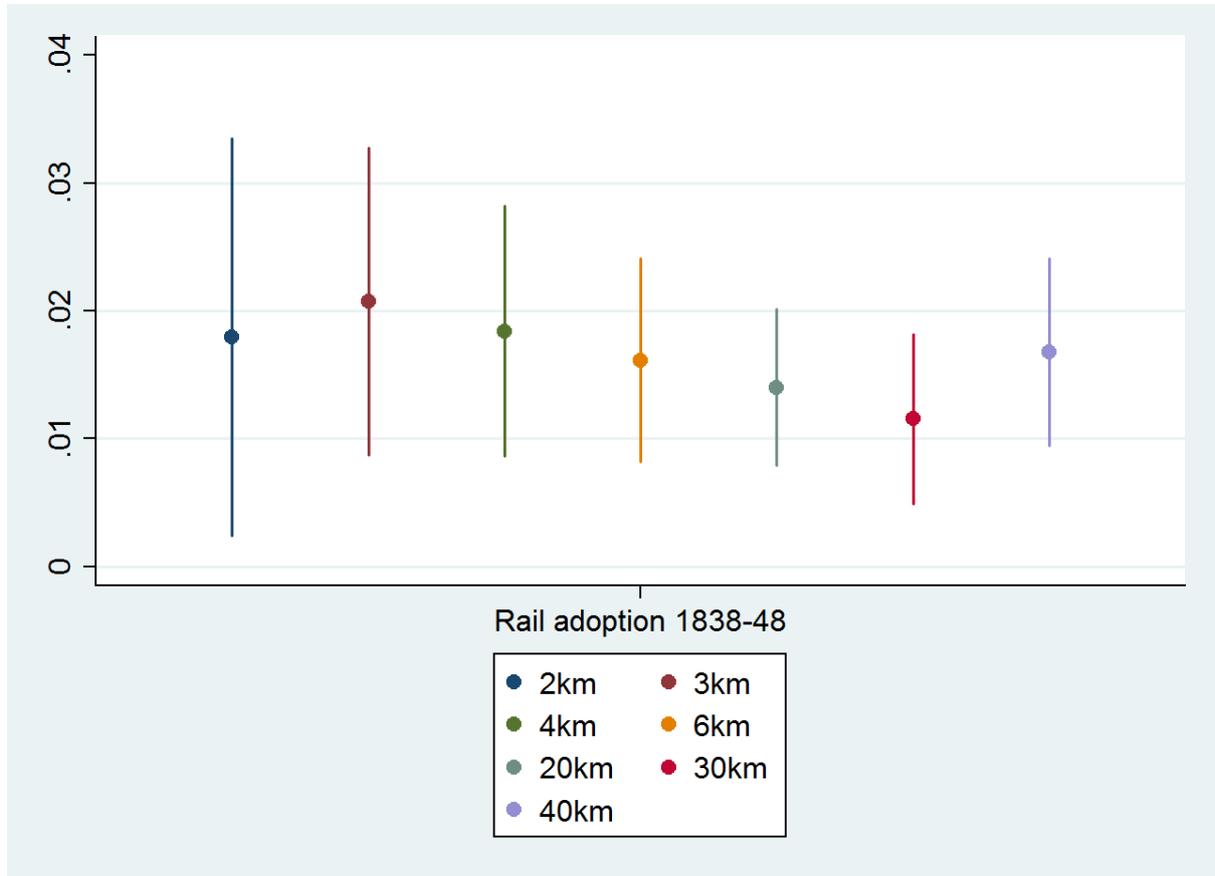
Table 15: Railroads and Urbanization in a Panel of Counties

DepVar: Urbanization	Railroad-stations		Railroad-population	
	OLS (1)	IV (2)	OLS (3)	IV (4)
Number of stations	0.007* (0.004)	0.012* (0.007)		
Share of population with access			0.095*** (0.029)	0.087** (0.035)
Year fixed effects	Y	Y	Y	Y
County fixed effects	Y	Y	Y	Y
Controls	Y	Y	Y	Y
Observations	668	668	668	668
Number of counties	334	334	334	334
Kleibergen-Paap F statistic		46.37		23.15
R-squared	0.19	0.19	0.25	0.25

Note: Panel estimates at the county level with a full set of county and year dummies using two cross-sections for 1849 and 1864. The dependent variable is urbanization—measured as the share of city dwellers in the total county-population. Columns 1 and 2 show OLS and IV estimates using the number of cities in a county that have a station as the explanatory variable. In column 2 this endogenous variable is instrumented with the number of cities in a county located within the 1.5km *SLC*. Columns 3 and 4 show OLS and IV estimates using the share of a county’s population that lives in a city with a station as the explanatory variable. In column 2 this endogenous variable is instrumented with the share of a county’s population that lives in a city located within the 1.5km *SLC*. Standard errors, clustered at the district level, in parentheses. County-level controls include: population density, factory workers (share), agriculture (share), large farming (share), age composition, school enrollment rate, distance to node county, and a constant. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Source: See main text and [Appendix A](#) for data sources and details.

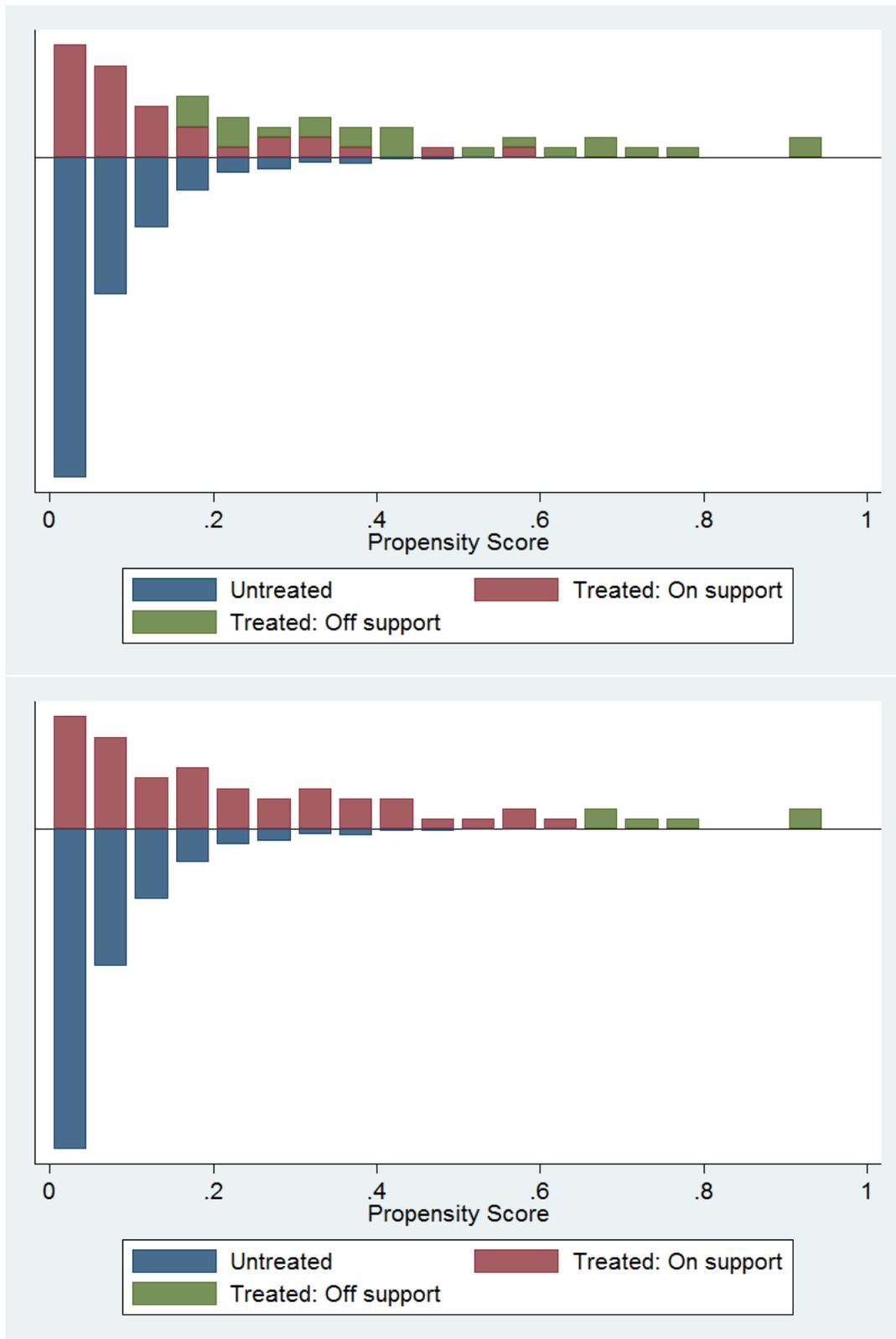
Appendix Figures and Tables

Figure A-4: Results from IV Estimates Using Different Corridor Sizes



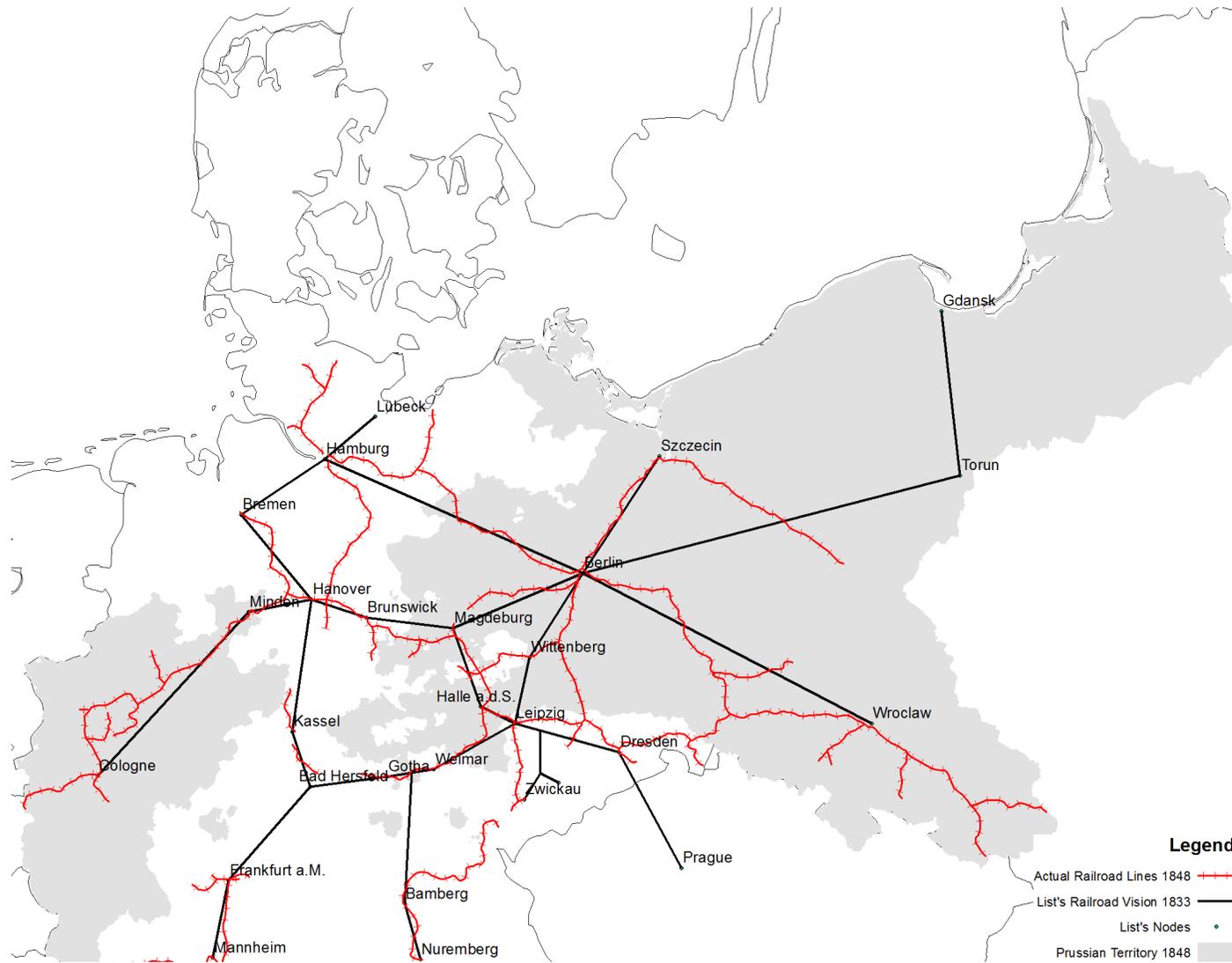
Note: The graph shows the estimated beta coefficients of the *Population growth rate 1849-71* resulting from IV regressions using different straight-line-corridor widths for the instrumental variable. Horizontal spikes indicate confidence intervals.

Figure A-5: Distribution of the Propensity Score Using Radius and Kernel Matching



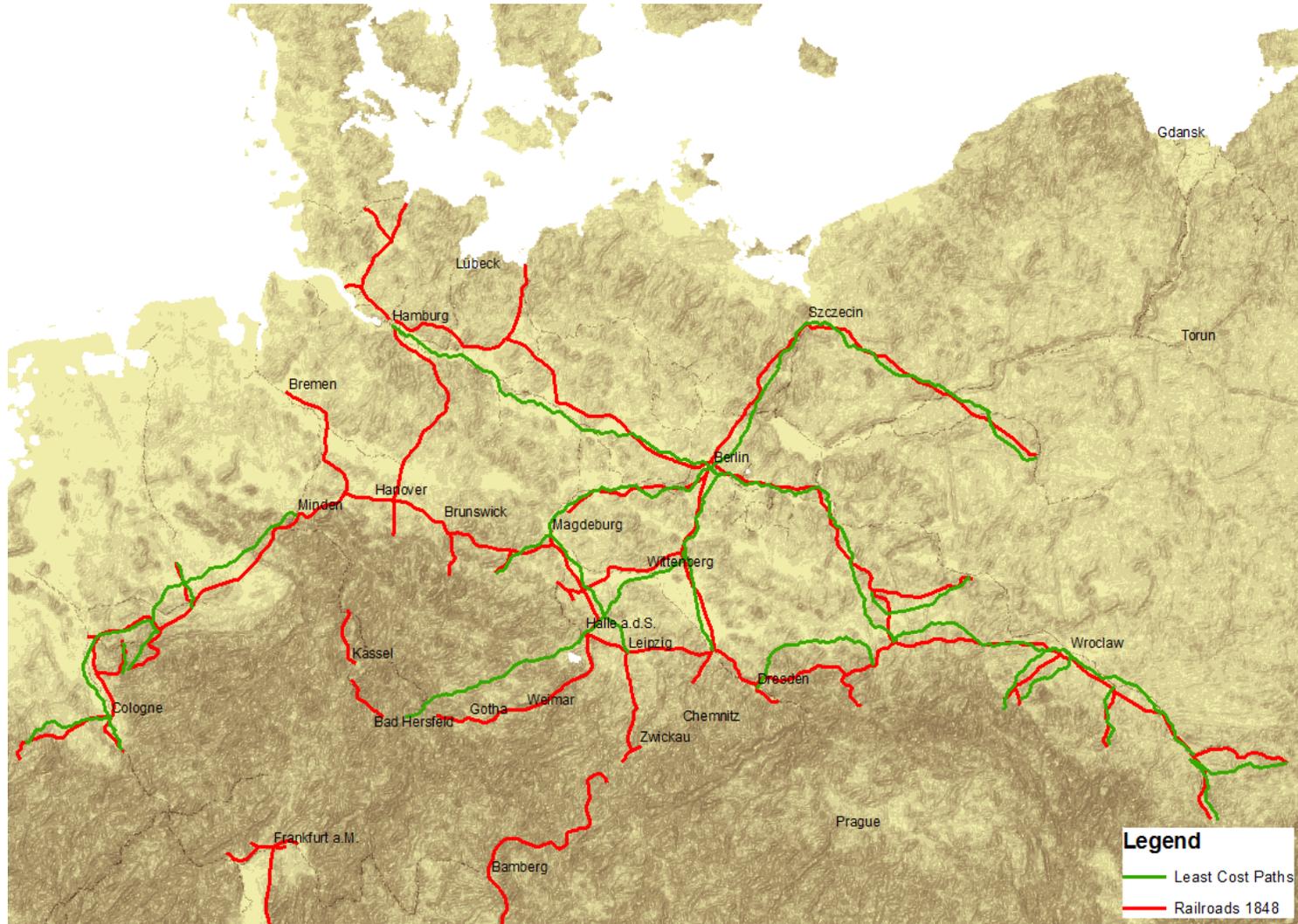
Note: The upper graph shows the frequency distribution of the propensity score using a radius matching (caliper 0.001), based on the approach applied in Table 10, Panel B, column 2. The lower graph shows the frequency distribution of the propensity score using a nonparametric kernel matching, based on the approach applied in Table 10, Panel C, column 2.

Figure A-6: List's Proposed German Railroad System and the Actual Railroad Network by 1848.



Note: Hash lines indicate railroad routings in the German Reich in 1848. Tubes indicate the straight-lines drawn by List to sketch his proposed railroad system. Circles indicate nodes as identified in List's system. Source: Own illustration following List (1833); see main text for details.

Figure A-7: Least Cost Paths Between Nodes in 1848.



Note: Red lines indicate railroad routings in the German Reich in 1848. Green lines indicate the least cost paths between nodes relevant for Prussian cities. Circles indicate nodes as identified in List's system. Source: Own illustration; see main text for details.

Table A-16: Further Robustness Checks in the Cross Section

DepVar: Population growth rate	Main periods					Subperiods			
	1831-37 (1)	1849-71 (2)	1849-52 (3)	1852-55 (4)	1855-58 (5)	1858-61 (6)	1861-64 (7)	1864-67 (8)	1867-71 (9)
Panel A: IV estimates - Full sample including nodes and outliers.									
Rail access 1838-48	0.000 (0.003)	0.019*** (0.005)	0.006 (0.008)	0.017*** (0.005)	0.036*** (0.012)	0.011*** (0.004)	0.023*** (0.008)	0.017** (0.008)	0.019*** (0.005)
Observations	930	934	962	962	951	958	957	958	953
Kleibergen-Paap F statistic	86.56	86.58	84.58	83.35	105.0	76.99	73.38	69.52	61.46
Panel B: IV estimates - Sample including nodes but excluding implausible outliers (growth rates is $>+/- 10\%$).									
Rail access 1838-48	0.000 (0.003)	0.019*** (0.005)	0.012*** (0.004)	0.014*** (0.004)	0.015*** (0.006)	0.008** (0.003)	0.017*** (0.004)	0.016** (0.007)	0.019*** (0.005)
Observations	928	934	958	954	941	956	952	948	948
Kleibergen-Paap F statistic	86.54	86.58	82.91	102.7	95.17	78.69	69.16	67.17	61.18
Panel C: IV estimates - Sample excluding nodes and outliers (standardized residual is $>+/- 2.58$).									
Rail access 1838-48	0.007 (0.005)	0.021*** (0.006)	0.017*** (0.007)	0.018*** (0.007)	0.020** (0.008)	0.011** (0.005)	0.023*** (0.007)	0.022** (0.009)	0.017*** (0.005)
Observations	879	906	918	912	909	912	910	915	909
Kleibergen-Paap F statistic	29.21	41.85	31.73	37.86	36.12	32.99	30.27	30.48	29.77
Panel D: IV estimates - Baseline estimates without lagged dependent variable									
Rail access 1838-48	0.001 (0.005)	0.021*** (0.006)	0.016** (0.006)	0.017*** (0.006)	0.019** (0.008)	0.011** (0.005)	0.022*** (0.007)	0.022** (0.009)	0.021*** (0.007)
Observations	923	919	942	937	926	939	937	931	932
Kleibergen-Paap F statistic	33.77	42.04	34.64	41.22	41.82	32.74	29.29	30.29	28.45

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Table A-16 – Continued

DepVar: Population growth rate	Main periods					Subperiods			
	1831-37 (1)	1849-71 (2)	1849-52 (3)	1852-55 (4)	1855-58 (5)	1858-61 (6)	1861-64 (7)	1864-67 (8)	1867-71 (9)
Panel E: IV estimates - Baseline estimates including soil texture as proxy for geographical endowments									
Rail access 1838-48	0.002 (0.006)	0.021*** (0.006)	0.017*** (0.006)	0.017*** (0.006)	0.019** (0.008)	0.012** (0.005)	0.022*** (0.007)	0.022** (0.009)	0.021*** (0.007)
Observations	898	906	929	924	914	926	924	919	919
Kleibergen-Paap F statistic	34.21	42.50	35.46	41.86	42.36	33.29	29.76	30.65	28.64
Panel F: IV estimates using a sample and weights obtained from geographical matching.									
Rail access 1838-48	-0.000 (0.008)	0.022*** (0.008)	0.016* (0.009)	0.017** (0.008)	0.023** (0.012)	0.016** (0.007)	0.029*** (0.010)	0.013 (0.015)	0.020** (0.009)
Observations	267	267	281	277	271	278	277	271	270
Kleibergen-Paap F statistic	19.65	21.77	16.76	20.04	19.94	15.88	14.30	18.20	13.07
Panel G: IV estimates in a sample of cities within 15 km distance to the straight line corridor.									
Rail access 1838-48	0.001 (0.008)	0.024*** (0.009)	0.014 (0.010)	0.017* (0.009)	0.025** (0.013)	0.013* (0.008)	0.026*** (0.010)	0.024 (0.015)	0.024** (0.010)
Observations	234	236	244	243	237	244	242	241	242
Kleibergen-Paap F statistic	17.49	17.25	14.93	15.51	16.46	14.75	12.98	13.61	11.25
Panel H: Differences in Market Access - Controlling for Average County-level Crop Prices 1838-1860.									
Rail access 1838-48	-0.0003 (0.0054)	0.0200*** (0.0063)	0.0151** (0.0067)	0.0162*** (0.0061)	0.0187** (0.0084)	0.0102** (0.0051)	0.0207*** (0.0067)	0.0192* (0.0099)	0.0207*** (0.0070)
Avg. wheat price 1837-60	0.0001 (0.0001)	0.0003*** (0.0001)	0.0003** (0.0001)	0.0005*** (0.0001)	0.0004* (0.0002)	0.0001 (0.0001)	0.0001 (0.0002)	0.0004*** (0.0001)	0.0002 (0.0002)
Avg. rye price 1837-60	-0.0004*** (0.0001)	-0.0005*** (0.0001)	-0.0004** (0.0002)	-0.0003 (0.0002)	-0.0006*** (0.0002)	-0.0007*** (0.0002)	-0.0006*** (0.0002)	-0.0005*** (0.0002)	-0.0007*** (0.0002)
Avg. potato price 1837-60	0.0002* (0.0001)	0.0003** (0.0001)	0.0001 (0.0001)	0.0003** (0.0001)	0.0002 (0.0002)	0.0003*** (0.0001)	0.0003* (0.0001)	0.0005*** (0.0002)	0.0004** (0.0002)
Observations	768	777	799	796	784	796	794	790	791
Kleibergen-Paap F statistic	30.29	38.26	29.89	36.90	38.86	28.24	24.65	25.36	23.38

Note: The table shows city-level IV-estimates regressing annual population growth for different periods on railroad access in 1848. All regressions include the full set of baseline controls: additional access dummy, street access dummy, waterway access dummy, annual city growth 1816–1831 (column 1), annual city growth 1831–1837 (columns 2-9), civilian population (log), military population (log), factory workers (share), mining (county level), large farming (county level), age composition, school enrollment rate, distance to next node, and a constant. Significance: *** p<0.01, ** p<0.05, * p<0.1. Source: See main text and [Appendix A](#) for data sources and details.

Table A-17: Alternative Instruments

DepVar: Population growth rate	Main periods		Subperiods						
	1831-37 (1)	1849-71 (2)	1849-52 (3)	1852-55 (4)	1855-58 (5)	1858-61 (6)	1861-64 (7)	1864-67 (8)	1867-71 (9)
Panel A: Second stage estimates - Actual railroad access instrumented by <i>SLC</i> location (according to List's railroad plan).									
Rail access 1838-48	0.003 (0.006)	0.015** (0.007)	0.014** (0.007)	0.006 (0.006)	0.016* (0.008)	0.012** (0.006)	0.021** (0.009)	0.010 (0.010)	0.014 (0.014)
Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	898	906	929	924	914	926	924	919	919
Kleibergen-Paap F statistic	28.26	22.17	24.09	26.27	24.61	23.54	24.29	22.66	20.50
Panel B: Second stage estimates - Actual railroad access instrumented by Least Cost Path location.									
Rail access 1838-48	0.004 (0.006)	0.020*** (0.008)	0.018** (0.007)	0.006 (0.011)	0.017* (0.009)	0.012 (0.008)	0.034*** (0.012)	0.024** (0.011)	0.021 (0.016)
Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes
Observations	898	906	929	924	914	926	924	919	919
Kleibergen-Paap F statistic	11.38	10.80	10.20	10.95	11.66	12.67	12.44	11.99	10.69

Note: The table shows city-level IV-estimates regressing annual population growth for different periods on railroad access in 1848. In Panel A, railroad access is instrumented by *SLC* location according to List's railroad plan. In Panel B, railroad access is instrumented by location on the least cost path between nodes. Standard errors, clustered at the county level, in parentheses. Controls include: additional access dummy, street access dummy, waterway access dummy, annual city growth (Column 1-2: 1816-21, Column 3: 1816-31, Columns 4-11: 1831-37), civilian population (log), military population (log), factory workers (share), mining (county level), large farming (county level), age composition, school enrollment rate, distance to next node (Panel A: List, Panel B: Keyser), and a constant. Significance: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Source: See main text and [Appendix A](#) for data sources and details.

Table A-18: Placebo Lines

DepVar: Population growth rate	1831-37 (1)	1849-52 (2)	1852-55 (3)	1855-58 (4)	1858-61 (5)	1861-64 (6)	1864-67 (7)	1867-71 (8)	1871-80 (9)
Rail access 1872-85	0.001 (0.001)	0.000 (0.001)	-0.000 (0.001)	-0.000 (0.001)	0.002 (0.001)	0.001 (0.001)	-0.000 (0.001)	0.003** (0.001)	0.005*** (0.001)
Observations	898	929	924	914	926	924	919	919	921
R-squared	0.04	0.09	0.09	0.10	0.04	0.13	0.14	0.14	0.22

Note: The table shows city-level OLS-estimates regressing annual population growth for different periods on railroad access in the period 1872-85. Standard errors, clustered at the county level, in parentheses. Controls include: earlier access dummy, street access dummy, waterway access dummy, annual city growth 1816-1831 (column 1), annual city growth 1831-1837 (columns 2-9), civilian population (log), military population (log), factory workers (share), mining (county level), large farming (county level), age composition, school enrollment rate, distance to next node, and a constant. Significance: *** p<0.01, ** p<0.05, * p<0.1. Source: See main text and [Appendix A](#) for data sources and details.