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Railroads and Rural Industrialization: evidence from a Historical Policy Experiment☆,☆☆

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ABSTRACT

This paper studies the impact of railroads on growth and structural transformation in 19th-century Sweden. To establish causality, the analysis exploits that the main state-owned lines of the network traversed rural communities that were not directly targeted by planners. Areas "accidentally" traversed by one of these trunk lines experienced substantially more rapid population growth and structural transformation over the next 50 years. These findings suggest that investments in transportation networks can spur industrial development and that the railroad is an important factor to account for Sweden's rapid catch-up with the leading European industrializers.

1. Introduction

Railroads were the key technology of the 19th-century transportation revolution, which was deeply intertwined with the spread of industrialization. Indeed, contemporary observers widely believed that the building of a railroad was able to "make or break a district or region" (Pollard, 1981, p.129). Yet, it has remained challenging for economic historians to identify the impact of railroads on local economic development, since they often connected already rapidly growing places. Opened in 1830, the world's first railroad between Manchester and Liverpool highlights this central empirical problem: connected places often experienced rapid industrialization, yet such growth may simply reflect factors that attracted these investments there in the first place.[1] Understanding whether investments in transportation infrastructure can ignite local development remains an important empirical question, not least because such investments remain a key policy tool to promote growth in disadvantaged areas today.

This paper uses the rollout of the 19th-century Swedish railroad network as a quasi-natural experiment to examine whether state-led investments in transportation networks can spur growth and structural transformation in rural areas. Importantly, this setting offers a number of useful features to identify the impacts of the railroad. First, the network was designed by state planners that

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[1] In the context of the Antebellum United States, Fishlow (1965, p.203) famously emphasized this empirical challenge when he argued that: "A key issue, however, is whether such railroad influence was primarily exogenous or endogenous, whether railroads first set in motion the forces culminating in […] economic development […] or whether arising in response to profitable situations, they played a more passive role."

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aimed to connect the capital Stockholm with coastal and interior cities along a set of trunk lines. Consequently, many non-targeted rural communities along these lines gained access to the emerging network, which alleviates some endogeneity concerns. Second, the railroads in the Scandinavian periphery were constructed *prior* to the industrial breakthrough, in contrast to Britain and much of Continental Europe. It is therefore plausible that they may have played a causal role in the onset of industrialization. Indeed, influential scholars such as Heckscher (1907) and Rostow (1960, p.55) have argued that the coming of the railroad is an "extremely important" factor in accounting for Sweden's industrial take-off.[2] Yet little empirical evidence exist to support such arguments.

To examine the impact of the state railroads, I construct a new dataset on manufacturing activity and population for rural parishes—the smallest administrative unit in Sweden—before and after railroad construction took place. In the empirical analysis, I use a difference-in-differences approach and estimate changes in industrial employment, population, and structural transformation between 1850 and 1900 among rural parishes that were traversed by the state trunk lines relative to those that remained unconnected to the network. OLS estimates reveal substantially more rapid population growth and structural transformation over a 50-year horizon in areas that were penetrated by a state trunk line. However, these gains are highly localized. Estimated growth impacts attenuate in parishes that are located more than 5 km from a trunk line. Thus, an important question is whether these localized impacts are offset by a reallocation of manufacturing firms and workers from nearby unconnected areas.[3] Yet, there is limited evidence of negative spillovers onto nearby unconnected areas in flexible specifications. Estimates also remain similar when comparing structural transformation across larger regional units, which presumably absorbs potential negative treatment spillovers, as well as when comparing treated areas to more distant unconnected areas that are plausibly unaffected by the rollout of the network. Moreover, the fact that the employment share in industry more than doubled by the outbreak of World War I suggests that a reallocation of manufacturing activity is unlikely to fully explain the results.

Another empirical concern, however, is that the OLS estimates are potentially biased if the state trunk lines traversed rural areas with better or worse growth prospects. To establish causality, I develop an instrumental variable (IV) strategy that exploits the fact that the trunk lines were explicitly to be built along the shortest route between the endpoints of the network. Against that background, I construct bilateral Least Cost Paths (LCPs) that correspond to the lines that state planners would have built if the sole objective had been cost minimization subject to connecting targeted endpoints along bilateral routes.[4] Using this source of exogenous variation reveals large causal increases in employment, population, and structural transformation in areas traversed by the trunk lines. IV estimates are consistently larger in magnitude than the corresponding OLS estimates. A downward bias is consistent with qualitative historical evidence emphasizing that state planners often routed lines through disadvantaged rural areas (e.g., Heckscher, 1907; Heckscher, 1954; Westlund, 1998), as well as the finding in the broader transportation literature that public infrastructure often is allocated to areas with worse growth prospects (e.g., Baum-Snow, 2007; Burgess et al., 2015; Curto-Grau et al., 2012; Duranton and Turner, 2012).

These findings contribute to a growing literature on the myriad economic impacts of the railroads. One strand of this literature documents the historical impact of railroads on agricultural development in terms of agricultural production and land values (Atack and Margo, 2011; Donaldson, 2018; Donaldson and Hornbeck, 2016), or market integration (Keller and Shiue, 2008). Another strand studies the contribution of railroads to city growth and urbanization in both the short and long run (Atack et al., 2010; Berger and Enflo, 2017; Büchel and Kyburz, 2018; Heblich et al., 2018; Hornung, 2015; Jedwab et al., 2017; Jedwab and Moradi, 2016).[5] A key contribution of this paper is new evidence that the diffusion of railroads caused structural transformation, which is only indirectly captured by proxies such as city growth or urbanization (Jedwab and Vollrath, 2015). It thus relates to Atack et al. (2008) showing that the diffusion of railroads in the United States led to increased establishment sizes within manufacturing and thus supported the transition from the artisan shop to the factory, and Tang (2014) examining the link between railroads and firm activity in Japanese prefectures. However, while providing important evidence on how the railroad affected manufacturing firms, these papers do not shed light on whether the diffusion of railroads promoted a local reallocation of labor towards industrial activities—i.e., structural transformation. In a related paper, Bogart et al. (2017) analyzes the contribution of railroads to occupational shifts and population growth in mid-19th century Britain finding that secondary and tertiary employment grew in proximity to railroad stations. However, Britain differs significantly from the Swedish setting since it enjoyed a well-developed transportation system already prior to industrialization (e.g., Bogart, 2005; Bogart, 2012), while the spatial distribution of economic activity was shaped by early industrialization (e.g., Crafts and Mulatu, 2005; Crafts and Mulatu, 2006; Crafts and Wolf, 2014). My results are therefore presumably

[2] Eli Heckscher's doctoral dissertation *On the importance of railways to Sweden's economic development* provides an assessment of the impact of the Swedish railroads on patterns of population growth between the mid-19th and early 20th century (Heckscher, 1907). Heckscher's observation that places along the tracks experienced faster growth does not necessarily merit a causal interpretation, however, since these areas may have grown more rapidly even in the absence of a railroad.

[3] See, for example, Chandra and Thompson (2000) that study the impact of U.S. highways and find that counties traversed by a highway experience increases in economic activity, which is offset by activity being drawn away from unconnected adjacent counties. More broadly, Redding and Turner (2014, p.42) conclude that "much of the estimated effect of transportation costs and infrastructure on the spatial organization of economic activity is probably due to reorganization rather than growth".

[4] Michaels (2008), Banerjee et al. (2012), and Faber (2014) use a similar identification strategy to study the impacts of highways and railroads in the United States and China respectively.

[5] In particular, Berger and Enflo (2017) document that the "first wave" of railroad construction in Sweden—mainly consisting of the first trunk lines—had a long-run impact on the size distribution of cities that persists until the present day. Also see Andersson et al. (2017) that studies the impacts of the Swedish railroads on innovative activity. In contrast to these studies, this paper focuses on the contribution of the railroad to Sweden's largely rural industrialization.

more relevant for countries at a lower stage of economic development such as other European industrializers in the 19th and 20th century, or developing countries today. In particular, this paper provides evidence that the railroads were an important catalyst of industrial development in the poor Scandinavian periphery, which thus arguably contributed to its rapid convergence with the European industrial leaders (e.g., O'Rourke and Williamson, 1995a; O'Rourke and Williamson, 1995b; Schön, 2010).

2. Historical background

Sweden's railroad network was a product of state intervention.[6] It was widely believed that placing construction in the hands of private enterprise would result in an inefficient design of the network, which would prohibit the crucial movement of mail and military troops.[7] Furthermore, contemporaries stressed that the state should invest in a national rail network to promote economic development.[8] In the words of Johan August Gripenstedt, Minister of Finance:

> "If one wants to extend a helping hand to our industry […] the State cannot support the improvement of the country in a more efficient, appropriate, impartial and magnificent way, than by a firm action to bring about railways." (Kaijser, 1999, p.223)

In the early 1850s, the *Riksdag* consequently decided that the state would take on the main responsibility to construct, fund, and operate the main network. Nils Ericson, a colonel in the Navy Mechanical Corps, was granted far-reaching powers to design it (Rydfors, 1906). A guiding principle of his network proposal, depicted in Fig. 2(b), was that the state should construct a number of trunk lines. These lines should connect the capital Stockholm with neighboring Norway, as well as important cities in the west and south. Ericson envisioned the trunk lines to be built along the shortest routes between these targeted locations. At the same time, they should be drawn through the interior to make them less vulnerable in the case of foreign invasion, and to avoid the previously constructed canals and existing transport routes to reduce intermodal competition. As a result, these lines traversed many previously isolated rural areas. Heckscher (1954, p.241), emphasizes Ericson's developmental ambitions and argues that a "mainspring of his thinking was that the railroads were to stimulate economic development in those parts of the country which, through the absence of communications, had been left behind".[9]

Sweden's railroad era began in 1856 when the first stretches of the Southern and Western trunk lines were opened. In the 1860s, the major trunk lines—connecting important cities such as Gothenburg, Malmö, and Norrköping with the capital Stockholm—were finished. As the state railroads spread, many economically marginal areas along the routes of the trunk lines also gained access to the emerging network. Contemporary observers believed that the emerging network was "as if by magic, to bring throbbing prosperity even to regions without any prerequisites for economic development" (Heckscher 1954, p.243). Indeed, such sentiment has also been underlined by later economic historians like Westlund (1998, p.74) who argue that "[the railroads] were that epoch's great instrument for regional policy for spreading industrialization and economic development to new regions." As shown in Fig. 1, railroad construction surged again from the 1870s and onwards as state and private railroads proliferated. By the turn of the century, much of the modern Swedish railroad network was in place (see Fig. 2(a)).[10] Notably, even though the length of the private lines surpassed the state lines, goods and passenger traffic remained higher on the latter in the early 20th century (see Table 1).

Historical data on freight rates suggest that the expansion of railroads led to a substantial decrease in transportation costs. Costs decreased by about half for passenger traffic and about three-quarters for goods traffic relative to road transport (Sjöberg, 1956, p.42).[11] Moreover, these estimates constitute lower bounds as the railroad also increased travel speeds manifold (Leunig, 2006): When the Western trunk line opened in 1862, for example, the weeklong trip from Stockholm to Gothenburg by stagecoach was reduced to a 14-hour trip by train. Against the backdrop of sharply decreasing transportation costs, regional price and wage differentials narrowed considerably as local markets were integrated (e.g., Heckscher, 1954; Henning et al., 2011; Lundh et al., 2005).

The expanding railroad network improved access to both domestic and foreign markets for industries located in the countryside, which potentially contributed to the markedly rural character of Swedish industrialization (Söderberg, 1984). Indeed, by the early 20th century more than half of all factories and manufacturing workers were still located in the countryside, and rural areas experienced even more rapid industrial growth than urban locations during Sweden's most intensive phase of industrialization

[6] State involvement in infrastructure provision had long traditions, reflecting the fact that the country is geographically vast and sparsely populated. During the 17th-century Great Power Era, these circumstances motivated the state organization of road provision, transport facilities, inns, and the postal system to centralize economic, military, and political control (Westlund, 1998).

[7] The rest of this section draws heavily on Heckscher (1907, 1954), Rydfors (1906), Sjöberg (1956), and Westlund (1998). Also see Berger and Enflo (2017) for a discussion on the planning of the railroad network.

[8] An additional motivation for state involvement was the relative underdevelopment of the domestic bond market, which offered limited opportunities to finance the major investments that a national railroad network required. Indeed, state investments in railroads ushered in a period of sustained capital imports (Schön, 1989); by the outbreak of World War I, three-quarters of the Swedish foreign debt was due to loans taken to finance the railroad network (Hedin, 1967).

[9] Contemporary views also support this interpretation. As summarized by Gripenstedt, addressing the *Riksdag*: " [The Swedish railroads] should not, as in other countries, have the purpose to reinforce established patterns of economic activity, but rather promote an increase in production and economic activity, where it has been inhibited by long distances and a lack of means of transport". My translation from Rydfors (1906, p.77).

[10] At its peak in the late 1930s, the railroad network spanned almost 17,000 km. Thus, approximately two-thirds of the network had been constructed by 1900.

[11] According to a 1859 survey by *Järnvägsundersökningskomitén* of transportation costs along the country roads, the lowest reported cost (*forlön*) in *kr* of transporting one ton of goods between Södertälje-Stockholm, Uppsala-Gävle, and Jönköping-Gothenburg was 9.4, 26.3, and 41.1 respectively. By the early 20th century, the cost of transporting the same amount of goods along the same routes by rail had dropped to 1.6, 3.6, and 4.9 *kr* respectively based on the most common tariff (*massgodstariff n:o 11*) (Heckscher, 1907, p.88).

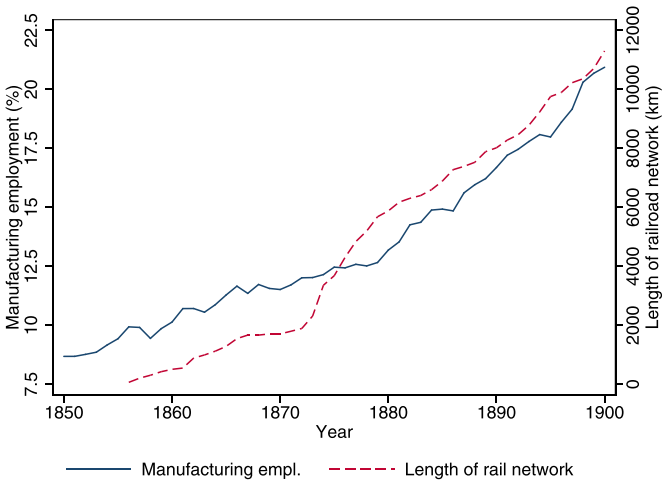
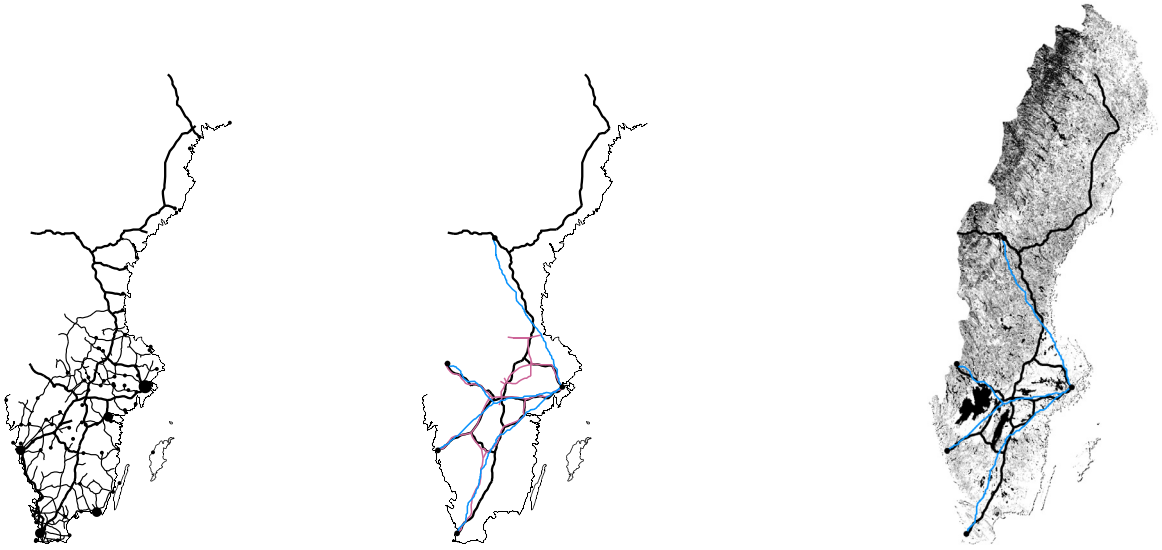


Fig. 1. Expansion of the railroad network and structural transformation, 1850–1900. *Notes:* Employment share in manufacturing calculated from data provided in Krantz and Schön (2007) and the total size of the railroad network drawn from Kongl. Jernvägs-Styrelsen (1901, p.7).



a) Railroad network, 1900 b) Trunk lines, LCPs, and Ericson's 1856 plan c) Trunk lines, LCPs and construction costs

Fig. 2. The Swedish railroad network, 1900. *Notes:* Figure a: extent of the national railroad network in 1900. Narrow lines denote railroads owned by private companies and bold lines state-owned railroads. Also shown are cities scaled by population. Figure b: state-owned trunk lines (black), Ericson's network proposal (pink), and the LCPs (blue) and endpoints. Figure c: the LCPs (blue) and the state-owned trunk lines (black). Also shown is the cost layer used to identify the LCPs, where lighter shades corresponds to lower costs. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

(Heckscher, 1907, p.72). A widened market increased the scope for standardized products particularly benefiting industries producing private consumption goods.[12] Indeed, footloose industries producing cotton fabrics, matches, soap, and tools experienced rapid growth during the period (Schön, 2010, p.143). Yet, the existing evidence on the railroads impact on industrial growth is typ-

[12] Notably, backward linkages from the railroad sector were seemingly unimportant for the development of Swedish industry. Although the state railroad administration promoted the domestic production of fuel, tracks, and wagons, virtually all of the consumed coal and laid rail was imported (Modig, 1971, p.137). Even in the most affected industry—mechanical engineering—sales to the railroad sector constituted a meagre 10 percent of the industry's output. Against that background, Modig (1971, p.139) concludes that the railroad' s significance for Swedish industry in general was limited, due to the irregularity of orders and low level of demand.

Table 1
Expansion of the Swedish railroad network, 1856–1900.

	Length of lines (km)		Passenger traffic (mil. pkm)		Goods traffic (mil. tkm)	
Year	State	Private	State	Private	State	Private
1856	32	34	8	–	–	–
1860	303	224	42	–	–	–
1870	1118	609	128	43	123	62
1880	1956	3920	171	110	222	191
1890	2613	5150	223	219	325	366
1900	3850	7032	516	449	961	712

Notes: Length of state-owned and private lines at the end of each indicated year and traffic data for the subsequent five-year period (e.g., 1870 corresponds to the average annual passenger and goods traffic for 1871/1875). Data obtained from Statistiska Centralbyrån (1960, Tables 44 and 47).

ically confined to anecdotal accounts of individual industries or rural communities that experienced a growth spurt as they became connected to the network.[13] Thus, the extent to which the railroads contributed to Sweden's industrialization remains an open empirical question, which requires systematic data on industrial activity and statistical evidence to be settled.

3. Estimating the impact of the state trunk lines

3.1. Data

To analyze the impact of the Swedish state trunk lines on rural economic development, I construct a new dataset for a balanced sample of rural parishes. The dataset combines employment and population data from historical parish books and censuses for 1850 and 1900 with Geographic Information Systems (GIS) data to capture differences in rail access across parishes. Employment and population data for 1850 is based on information reported by the clergymen in each parish to the Tabular Commission in Stockholm.[14] For each parish, this source provides employment counts by sector, which is used to calculate the share of the labor force that is employed in manufacturing, as well as a total population count. To measure the occupational structure and population of parishes in 1900, I use data from the full-count 1900 population census that contain demographic and occupational information for some 5,2 million individuals, which has been made available through the North Atlantic Population Project (NAPP, 2016).[15] I rely on the *Historical International Standard Classification of Occupations* (HISCO) classification system to aggregate the individual-level data to parish-level shares of the labor force employed in industry (Leeuwen et al., 2002).[16] Parishes in the 1900 census are matched to their 1850 equivalents by constructing a crosswalk that corrects for differences in spelling of parish names as well as adjusting for merges and splits of parishes over the period. Although this procedure is straightforward in most cases, the available historical information does not allow me to take into account minor adjustments of parish boundaries.

Throughout the analysis, I restrict the sample to rural parishes. The restriction to rural parishes is motivated in two ways. First, it aligns with the paper's broader question regarding whether infrastructure investments can spur development in *rural* areas, which is important given the prominent role of transport infrastructure as a policy lever to promote growth in peripheral regions. Second, in contrast to larger towns and cities, individual rural parishes are unlikely to have been directly targeted by planners based on their level of development or rate of growth. Below, I document that rural parishes traversed by the trunk lines indeed did not exhibit differential trends in industrialization or population prior to railroad construction, which is consistent with the restriction to rural parishes alleviating some selection concerns.

To define rural areas, I use the definitions of rural and urban parishes in the census, which maintains consistency with the Swedish population statistics. However, the NAPP cautions that the rural/urban classification may not always be precise, since an "urban" area may contain vast rural parts, or that a "rural" parish may constitute a growing suburb to a nearby city. I therefore also aggregate some parishes that later merged in the 20th century and exclude those that contain any urban part, which in particular facilitates the exclusion of suburban parishes that were later absorbed by their urban counterparts. In addition, to examine whether the results may be sensitive to potential misclassifications in the census or arbitrary definitions of rural areas, I also show that the main results are similar in a sample restricted to rural parishes with population densities below that observed among parishes designated as urban (i.e., using a *de facto* rather than *de jure* definition of urbanity) and when restricting the sample to parishes that are located far away

[13] Heckscher (1907, p.107), for example, describes the establishment of matchstick factories along the Eastern trunk line, and Sjöberg (1956, p.45) emphasizes the role of the railroad in accounting for the growth and location of the furniture, paper, and pulp industries.

[14] The Tabular Commission data is obtained from the Tabverk database maintained by Umeå University.

[15] Historically, the censuses were extracted from the continuously updated parish books, maintained by the priest or vicar in each parish, except for the city of Stockholm where data was based on the tax census.

[16] A limitation of the 1900 census is that substantial share of laborers are not easily classified according to their sector (e.g., those simply designated as "laborer" or "worker"). While I exclude these occupational groups in the main analysis, I show in the Appendix that the main results remain very similar when including them (i.e., when allocating all "general" laborers to industry).

from cities to further deal with the potential problem of suburban rural areas. In total, the baseline sample consists of 1503 rural parishes that are observed both in 1850 and 1900, which is limited by the availability of the Tabular Commission data.[17]

Using GIS software I digitize historical maps of the railroad network obtained from contemporary publications by Statistics Sweden that include information on both state and private railways in operation in 1900 (see Fig. 2(a)).[18] I georeference this map to parish boundaries obtained from the Swedish National Archives, which allows me to calculate each parish's distance to the nearest rail line that is connected to the network. Additional control variables are are drawn from a variety of sources. Agricultural suitability is based on data from the FAO's Global Agro-Ecological Zones (FAO-GAEZ) database that is used to calculate each parish's fraction of land that is at least highly suitable for growing barley, potatoes, rye, and wheat (Berger, 2018).[19] Elevation, slope, and terrain ruggedness for each parish is calculated from geospatial layers based on CGIAR SRTM data.[20] Lastly, using the coordinates of cities and towns, and shapefiles of the Swedish coastline allows me to calculate the distance from each parish to urban areas and the coast respectively.

3.2. Empirical strategy

To examine the impact of the state trunk lines, the main empirical analysis is based on a difference-in-differences approach that compares changes in parishes in proximity to the state trunk lines relative to more distant areas:

$$\triangle y_p = \alpha_c + \delta D_p^S + \mathbf{X_p}\boldsymbol{\theta} + \varepsilon_p \tag{1}$$

where $\triangle y_p$ either corresponds to differences in *ln* employment or population, or the share employed in industry in parish p with the difference operator $\triangle$ denoting changes between 1850 and 1900. D_p^S is a distance measure to the state trunk lines in 1900, which in the main specifications corresponds to an indicator taking the value 1 for parishes located within 5 km of the state trunk lines and 0 for other parishes.[21] Alternative specifications instead add additional distance indicators to motivate the use of the 5 km cutoff, or the *ln* distance to the nearest trunk line as a continuous measure of access.[22]

At the same time as the railroad network was rolled out, large domestic migrations were taking place across counties. To take such county-level shocks into account, which may be correlated with the spread of the state railroads, I also include a full set of 24 county fixed effects (α_c).[23] Throughout the analysis, I cluster standard errors at the county-level to account for potentially correlated shocks across parishes within the same county, though I also present alternative inference based on Conley (1999) standard errors that flexibly allow for spatial dependence.

To avoid over controlling bias, I restrict the set of controls in $\mathbf{X_p}$ to arguably exogenous or predetermined characteristics: a parish's area, agricultural suitability, access to the canal network and urban markets, elevation, distance to the coast, terrain ruggedness, and industrial activity and population in the pre-rail era. Specifically, to account for differences in conditions for agricultural production, I control for each parish's suitability for cultivating barley, potatoes, rye, and wheat. To account for other types of transport, I control for distance to the coast, as well as whether a parish was connected to the pre-rail canal network. Similarly, controlling for the distance to the closest city or town accounts for potential benefits of being located closer to urban markets.[24] Moreover, I control for elevation and terrain ruggedness since elevated and rugged terrain posed a veritable engineering challenge for railroad construction, and also may have a direct impact on economic development (Nunn and Puga, 2012). Accounting for the possibility that connected areas differed already prior to railroad construction, the baseline set of controls also include population and the share employed in industry in 1850. Lastly, some specifications also control for access to other state or private railroads constructed between 1850–1900, though it may be argued that these may constitute "bad controls" (Angrist and Pischke, 2008).

While the inclusion of this rich set of controls mitigate some concerns about selection on observables, historical accounts emphasize the regional policy motivations of state planners that may indicate that the state trunk lines traversed rural areas with worse (unobserved) growth prospects. In that case, OLS estimates may be downward biased. Therefore, I proceed to develop an IV strategy that isolates plausibly exogenous variation in access to the state trunk lines.

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[17] Although all parishes were supposed to report data, it was not uncommon that the pre-printed forms were destroyed in fires or were lost when transported from the parishes to the Tabular Commission in Stockholm. In many cases, the clergy in some parishes simply did not receive the forms. In the Appendix, I show that the sample of parishes that reported data to the Tabular Commission in 1850 do not differ from parishes that did not in terms of industrial employment or population density in 1900 (see Table A.1), thus largely reducing concerns that sample selection issues introduce a bias in the analysis. Figure A.1 displays the parishes included in the sample; note that the population of the parishes included in the sample contains roughly three-quarters of the total rural population in 1900.

[18] Maps are available from *Bidrag till Sveriges officiella statistik (BiSOS) L) Statens Järnvägstrafik* that have been digitized by Statistics Sweden (https://scb.se).

[19] Available at: http://fao.org/nr/gaez/en/

[20] Obtained through the Diva-GIS data portal (http://diva-gis.org).

[21] Although the empirical analysis below is based on distances calculated based on parish centroids, instead measuring distances based on parish borders yields nearly identical results (not reported).

[22] An alternative approach would be to focus on distances to railroad stations. However, focusing on the distance to the nearest trunk line rather than the distance to the nearest station (on a trunk line) provides a more exogenous measure of access since the opening of a station is endogenous to the demand for transportation services. Furthermore, access to a line may have exerted an influence even in the absence of a station, due to mobile loading docks and side tracks, as well as direct line loading (Heckscher, 1907, pp.13–14).

[23] The average (median) county contains 63 (60) of the rural parishes included in the main sample.

[24] I control for distance to the nearest urban area that holds town rights; limiting the sample of urban areas using population cutoffs (e.g., populations of 5000 or 10,000) yields very similar results (not reported).

Table 2
Balancedness of instrument.

	(1)	(2)	(3)	(4)	(5)	(6)
				Panel A		
Outcome:	Ind. share, 1850	Pop., 1850 (*ln*)	Area (*ln*)	Dist. to city (*ln*)	Canal (0/1)	Dist. to coast (*ln*)
ln distance to LCP	−0.001	−0.026	0.015	0.107**	−0.013	−0.120**
	(0.002)	(0.028)	(0.061)	(0.049)	(0.009)	(0.045)
				Panel B		
Outcome:	Elevation (*ln*)	Ruggedness (*ln*)	Wheat suit. (0–1)	Rye suit. (0–1)	Potato suit. (0–1)	Barley suit. (0–1)
ln distance to LCP	−0.034	0.004	−0.025	−0.017	−0.016	−0.025
	(0.034)	(0.031)	(0.034)	(0.037)	(0.020)	(0.034)
County FE?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1503	1503	1503	1503	1503	1503

Notes: OLS estimates from regressing each parish-level outcome on the *ln* distance to the nearest LCP. Standard errors are clustered at the county-level. Statistical significance is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

3.2.1. *IV strategy*

As the basis of the IV strategy, I construct a hypothetical network that connects the major targeted endpoints along approximate LCPs. These LCPs reflect how state planners would design the network if they were minimizing costs when connecting the selected target destinations. While the targeted endpoints were arguably not randomly chosen, the validity of the instrument relies on the argument that areas that were located *along* these routes were more likely to be traversed by a trunk line, solely due to their location along these LCPs.

To construct the instrument, I proceed in three steps. First, I identify the major endpoints of the network emanating from Stockholm: Gothenburg, Malmö, Östersund, and Kongsvinger in Norway (see Fig. 2(b)). Second, to identify the bilateral LCPs that connect Stockholm with these endpoints, I use GIS data on land cover and slope.[25] Construction costs are assumed to increase linearly with slope, while water cover is assigned the highest cost in the cost function to capture the extremely costly endeavor of traversing (e.g., by constructing bridges) major water bodies (see Fig. 2(c)). Third, each bilateral LCP is then sequentially constructed (starting with the Stockholm-Gothenburg line) by applying Dijkstra's algorithm to find the cost-minimizing paths. Fig. 2(b) shows the LCPs, as well as the main trunk lines of the network in 1900.

For the exclusion restriction to hold, it requires that there is no correlation (conditional on county fixed effects) between factors that shape the future growth of a parish and proximity to these LCPs. As proximity to a LCP is mechanically correlated with the distance to the targeted endpoints, however, this condition is likely violated. Throughout the IV analysis, I therefore always control for each parish's *ln* distance to the nearest targeted endpoint.[26] As an indirect test of the exclusion restriction, I next provide evidence showing that there is no correlation between a variety of observable characteristics such as population or industrialization and proximity to the LCPs, and in robustness checks below I show that the "effect" of being located along the LCPs is zero where no railroad was actually built.

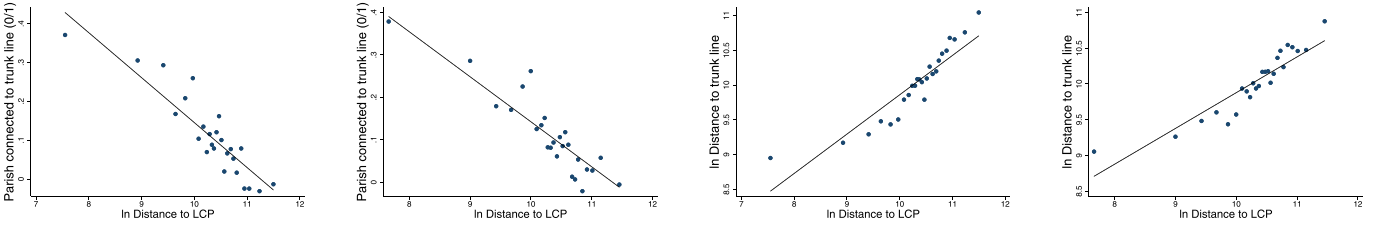
Balance tests

To explore the validity of the instrument, I examine whether areas in proximity to the LCPs differ in terms of observable characteristics at baseline in 1850. Table 2 presents OLS estimates from regressing a variety of exogenous or pre-determined outcomes on the *ln* distance to the nearest LCP, while conditioning on county fixed effects. Parishes in proximity to the LCPs do no differ from those more distant, with two exceptions. First, areas closer to the LCPs were typically located closer to towns that existed in the pre-rail era, which arises due to the fact that proximity to the LCPs is mechanically correlated with distance to the endpoints that includes the three large cities Stockholm, Gothenburg, and Malmö.[27] Second, the LCPs traverse places located further inland, which arises from the fact that the targeted cities are connected through the interior. Yet, the rest of the estimates provide broad indirect support for the exclusion restriction in the IV analysis, namely that location along these hypothetical routes is not correlated with key determinants of growth (e.g., initial levels of industrialization and population). Thus, the instrument is presumably valid conditional on county fixed effects, distance to the nearest targeted endpoint, and the coast.

First stage

To document that rural areas along the LCPs also are more likely to be traversed by a trunk line, Fig. 3 visualizes the first stage using binned scatterplots. To construct each figure, all 1503 parishes are grouped into 25 equal-sized bins based on the *ln* distance to the nearest LCP. Each dot denotes to the share of parishes within 5 km of a trunk line (a and b) or the mean *ln* distance to the nearest trunk line (c and d), and the mean *ln* distance to the nearest LCP respectively within each bin. Also shown are best-fit lines estimated from the underlying (ungrouped) data. Areas in proximity to the LCPs are clearly more likely to be traversed by a trunk line, both when including and excluding the full set of additional controls respectively.

[25] Land cover data is drawn from the Global Land Cover 2000 Project (GLC 2000) available through the Diva-GIS data portal (http://diva-gis.org).
[26] Note that endpoints such as Stockholm and Gothenburg are always excluded due to sample being restricted to rural parishes.
[27] Indeed, controlling for the *ln* distance to the nearest endpoint renders the link between distance to the LCPs and distance to the nearest city statistically insignificant and much smaller in magnitude (the OLS estimate is 0.050 with a standard error of 0.055).



a) Baseline b) Additional controls c) Baseline d) Additional controls

Fig. 3. First stage: State trunk lines and distance to LCPs. *Notes:* Non-parametric relationship between access to a trunk line and distance to the LCPs. To construct each figure, all parishes are grouped into 25 equal-sized bins based on the *ln* distance to the nearest LCP. Each dot corresponds to the share of parishes within 5 km of a trunk line (a and b) or the mean *ln* distance to the nearest trunk line (c and d), and the mean *ln* distance to the nearest LCP within each bin. Figure a and c include county fixed effects and controls for the *ln* distance to the nearest endpoint, while b and d also include controls for: *ln* area, *ln* altitude; an indicator for canal access; the share of land suitable for cultivating barley, potatoes, rye, and wheat; *ln* distance to the coast; *ln* distance to the nearest town; *ln* terrain ruggedness; and the share employed in industry and *ln* population in 1850. Also shown are best-fit lines estimated on the underlying (ungrouped) data.

Table 3
First stage: State trunk lines and distance to LCPs.

	(1)	(2)	(3)	(4)	(5)
	Panel A. Outcome: Trunk line (=1)				
ln distance to LCP	−0.116***	−0.106***	−0.105***	−0.103***	−0.103***
	(0.026)	(0.022)	(0.021)	(0.022)	(0.023)
	Panel B. Outcome: *ln* distance to trunk line				
ln distance to LCP	0.566***	0.501***	0.501***	0.496***	0.495***
	(0.081)	(0.069)	(0.068)	(0.069)	(0.071)
County FE?	Yes	Yes	Yes	Yes	Yes
ln Distance to endpoint?	Yes	Yes	Yes	Yes	Yes
Additional controls?	No	Yes	Yes	Yes	Yes
Additional railroads?	No	Yes	Yes	Yes	Yes
Eriksgata?	No	No	Yes	No	No
17th-century postal routes?	No	No	No	Yes	No
Medieval roads?	No	No	No	No	Yes
Kleibergen-Paap F-stat (panel A)	20.34	22.89	24.29	22.44	20.82
Kleibergen-Paap F-stat (panel B)	48.58	53.49	54.31	52.23	49.27
Observations	1503	1503	1503	1503	1503

Notes: OLS estimates. Additional controls include: *ln* area, *ln* altitude; an indicator for canal access; the share of land suitable for cultivating barley, potatoes, rye, and wheat; *ln* distance to the coast; *ln* distance to the nearest town; *ln* terrain ruggedness; and the share employed in industry and *ln* population in 1850. Additional railroads includes two indicators for whether a parish is connected to a state (i.e., non-trunk line) or privately owned railroad. Standard errors are clustered at the county-level. Statistical significance is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 3, panels A and B, presents the first-stage OLS regressions. Distance to the nearest LCP is a statistically significant predictor of the actual trunk lines constructed, also when conditioning on the full set of controls, county fixed effects, as well as distance to the nearest targeted endpoint (column 2). A concern is that historical trade routes or infrastructure may have existed along the LCPs, which in turn may shape later growth patterns. To empirically assess this issue, I digitize maps of the historical *Eriksgatan*, the medieval road network, and the location of 17th-century postal routes.[28] As shown in columns 3–5, the distance to the nearest LCP remains a strong instrument also when conditioning on these alternative proxies for historical trade routes. Indeed, while a large literature emphasizes that weak instruments may lead to biased IV estimates, the Kleibergen–Paap F-statistics reported at the bottom of Table 3 are always sufficiently large to allow me to reject a 10 percent maximum IV bias (Stock and Yogo, 2005).

4. Analysis and results

4.1. Main results

4.1.1. OLS estimates

To identify the areas affected by the trunk lines, Fig. 4 displays estimated relative changes in the share employed in industry between 1850 and 1900 among parishes located at different distances to the trunk lines. The underlying OLS regression is based on Eq. (1) where I add a set of indicators for 5 km bins (0–5 km, 5–10 km, etc.) of distance to the trunk lines. Each individual point estimate and its confidence interval is plotted in Fig. 4. As evident from these estimates, the impact of the trunk lines attenuate beyond 5 km, particularly when including the full set of controls (Fig. 4(b)), with no statistically significant effects beyond this distance.[29] In the subsequent analysis, I use this 5 km cutoff to define a single access indicator that (conservatively) assumes no impact on areas further away, but also report results using the *ln* distance to the nearest trunk line to avoid imposing a distance cutoff when defining access.

Table 4 presents OLS estimates of Eq. (1) where the connectivity measure is an indicator variable taking the value 1 if a parish is located within 5 km of the trunk lines, or the *ln* distance to the nearest trunk line. Column 1 presents the baseline OLS estimate suggesting that parishes located within 5 km of the state trunk lines experienced a relative increase in their employment share in industry of 1.6 percentage points between 1850 and 1900, an estimate that is highly statistically significant. To put this increase in

[28] In medieval times, *Eriksgatan* was the route that a newly-elected king had to travel in order to be confirmed by local assemblies, which remained an important travel route connecting key economic areas in the pre-industrial period. Additional data is digitized from historical map collections, as well as maps held by the Swedish National Archives. Access to these networks is measured using an indicator taking the value 1 if a parish is within 5 km of, for example, a postal route, and 0 otherwise.

[29] To more flexibly identify the distance cutoff, I also present OLS estimates where the treatment area is sequentially enlarged in 1 km increments (i.e., within 0–1 km, 0–2 km, etc. of a trunk line) in Figure A.2 for all three main outcomes in the analysis below. The estimated impact on all three outcomes declines after the treatment area includes parishes further than about 5–8 km away from the trunk lines, which suggests that 5 km is a suitable cutoff. Moreover, I prefer to use the wider 5 km buffer to identify treated areas in the baseline analysis given the potential measurement error in using smaller bins even though, as noted in the text, this is a somewhat conservative definition of access.

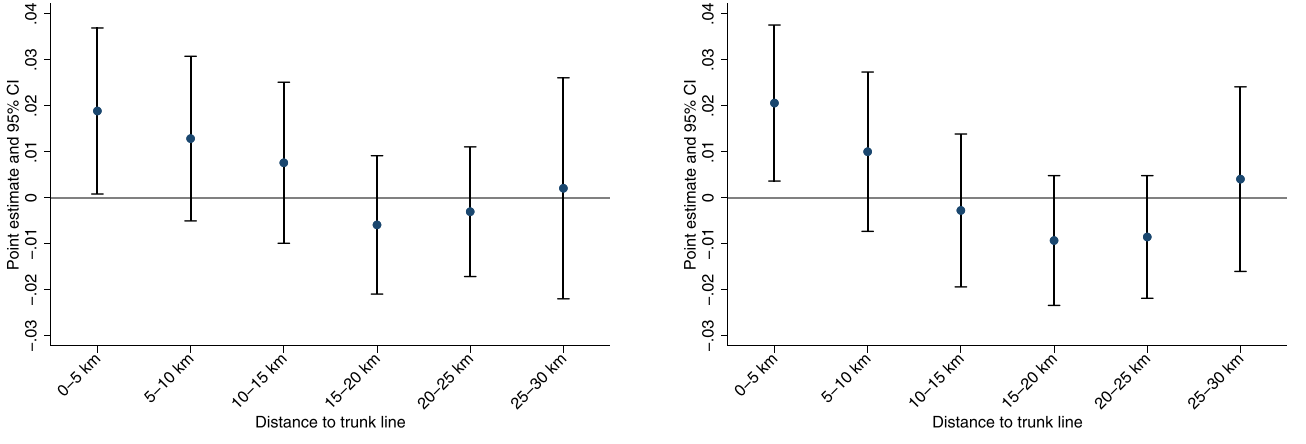
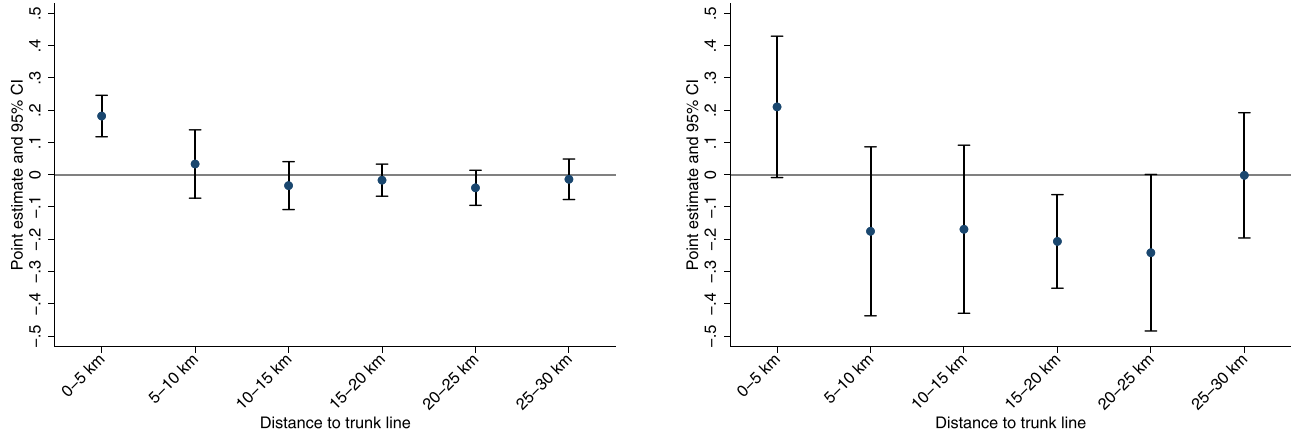


Fig. 4. State trunk lines and rural industrialization, 1850–1900: Flexible OLS estimates. *Notes:* OLS estimates from estimating Eq. (1) where I regress changes in the share of the labor force employed in industry between 1850 and 1900 on indicators for 5 km bins of distance to the state trunk lines in 1900. Figure a includes a full set of county fixed effects, while b also controls for *ln* area, *ln* altitude; an indicator for canal access; the share of land suitable for cultivating barley, potatoes, rye, and wheat; *ln* distance to the coast; *ln* distance to the nearest town; *ln* terrain ruggedness, the share employed in industry and *ln* population in 1850, as well as two indicators for whether a parish is connected to a state (i.e., non-trunk line) or privately owned railroad. Also shown are 95 percent confidence intervals. Standard errors are clustered at the county-level.



a) Outcome: △ *ln* Population, 1850–1900

b) Outcome: △ *ln* Ind. employment, 1850–1900

Fig. 5. State trunk lines, employment, and population growth, 1850–1900: Flexible OLS estimates. *Notes:* OLS estimates from estimating Eq. (1) where I regress changes in *ln* population (a) and employment in industry (b) between 1850 and 1900 on indicators for 5 km bins of distance to the state trunk lines in 1900, while controlling for a full set of county fixed effects and *ln* area, *ln* altitude; an indicator for canal access; the share of land suitable for cultivating barley, potatoes, rye, and wheat; *ln* distance to the coast; *ln* distance to the nearest town; *ln* terrain ruggedness, the share employed in industry and *ln* population in 1850, as well as two indicators for whether a parish is connected to a state (i.e., non-trunk line) or privately owned railroad. Also shown are 95 percent confidence intervals. Standard errors are clustered at the county-level.

Table 4
State trunk lines and rural industrialization, 1850–1900: OLS estimates.

	(1)	(2)	(3)	(4)	(5)	(6)
	Outcome: △ Industry share, 1850–1900					
Trunk line (=1)	0.016**	0.018**	0.021***			
	(0.006)	(0.007)	(0.007)			
ln distance to trunk line				−0.006**	−0.006**	−0.006**
				(0.003)	(0.002)	(0.002)
State railroad (=1)			−0.004			−0.004
			(0.006)			(0.006)
Private railroad (=1)			0.021***			0.021***
			(0.005)			(0.005)
County FE?	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls?	No	Yes	Yes	No	Yes	Yes
Observations	1503	1503	1503	1503	1503	1503

Notes: OLS estimates of Eq. (1). Additional controls include: *ln* area, *ln* altitude; an indicator for canal access; the share of land suitable for cultivating barley, potatoes, rye, and wheat; *ln* distance to the coast; *ln* distance to the nearest town; *ln* terrain ruggedness; and the share employed in industry and *ln* population in 1850. Standard errors are clustered at the county-level. Statistical significance is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

perspective, it is informative to note that the mean increase in the sample is 6 percentage points with a standard deviation (SD) of 0.08, which suggests that the gains due to the state trunk lines were sizable. A sizable impact of the trunk lines on industrialization is also evident in column 4, which instead measures connectivity by the *ln* distance to the nearest trunk line. The negative and statistically significant link suggests that areas in proximity to the trunk lines saw more rapid structural transformation relative to more distant locations: A 1 SD increase in the distance to the nearest trunk line is associated with a 0.09 SD slower relative increase in the share employed in industry 1850–1900. In columns 2 and 5, I add the full set of baseline controls. Adding controls inflates the estimated impact of the trunk lines on structural transformation, which suggests that the trunk lines traversed areas with worse (observable) growth fundamentals. In columns 3 and 6, I further add controls for the presence of other state and private railroads. Although there is a large positive association between access to a private railroad and structural transformation, such effects are likely to be highly endogenous since these railroads were built were they were deemed to be profitable, or in response to a growing transport demand potentially due to industrial expansion. Notably, there is a significant positive link between access to the trunk lines and structural transformation also when controlling for the distribution of other railroads.

Basic robustness

In the Appendix, I provide three sets of basic robustness checks. First, I show that basing the statistical inference on the approach in Conley (1999), allowing for different degrees of spatial dependence, produces standard errors that generally are smaller than when clustering standard errors at the county-level (see Table A.5). Second, the outcome in the main specifications exclude unclassified laborers, as discussed above. I replicate the results allocating all unclassified laborers to industry showing that the results are similar (see Table A.6).[30] Third, I show that the results are similar in three different subsamples: (*i*) When restricting the sample to parishes within 75, 50, and 25 km of Ericson's 1856 plan for the state lines, which explores the potential importance of underlying differences between targeted and non-targeted areas (see Table A.2); (*ii*) when restricting the sample to *de facto* rural areas, rather than using *de jure* definitions of rurality. To do this, I "endogenously" define rural parishes based on population densities observed across parishes designated as urban in 1900 and limit the sample of rural parishes to those that fall below the 10th, 25th, and 50th percentile of population density among urban parishes (see Table A.3); and (*iii*) when excluding parishes that fall below the 1st, 5th, and 10th percentile of the cross-parish distribution of distance to the nearest city, which further mitigates concerns of rural/urban definitions driving the results (see Table A.4). Reassuringly, the estimates remain similar in magnitude and statistical precision in all three subsamples.

Selection on unobservables

Although these results are suggestive of large positive impacts of the state trunk lines on structural transformation, they do not fully account for a potential bias from unobservables. To assess how large the selection on unobservable factors has to be in order to explain away the estimated impact of the railroad, I rely on the insights of Altonji et al. (2005).[31] It involves estimating two separate versions of Eq. (1): One that includes a restricted set of controls and one that includes a full set of controls in $\mathbf{X_p}$. Letting δ^R and δ^F denote the estimates in the restricted and full model respectively, the ratio $\delta^F/(\delta^R - \delta^F)$ corresponds to the magnitude that the selection on unobservables has to take *relative* to the selection on observables to make the estimated impact of the trunk lines economically insignificant. Columns 1 and 3 of Table 4 above report OLS estimates from a restricted and full version of Eq. (1), which

[30] A larger magnitude of the estimated impact of the trunk lines is mainly driven by the fact that the mean change in the share employed is industry when including all unclassified laborers is more than 14 percentage points, which can be compared with 6 percentage points using the more restrictive definition above.

[31] See Altonji et al. (2005) for the development of the test and a discussion of the underlying assumptions, as well as Bellows and Miguel (2009) and Nunn and Wantchekon (2011) for extensions and applications.

Table 5
State trunk lines and rural industrialization, 1850–1900: 2SLS estimates.

	Outcome: △ Industry share, 1850–1900				
	(1)	(2)	(3)	(4)	(5)
	Panel A. Second stage (2SLS)				
Trunk line (=1)	0.069**	0.065**	0.065**	0.063*	0.066*
	(0.028)	(0.032)	(0.032)	(0.034)	(0.034)
	Panel B. Second stage (2SLS)				
ln distance to trunk line	−0.014***	−0.014**	−0.014**	−0.013**	−0.014**
	(0.005)	(0.006)	(0.006)	(0.007)	(0.006)
	Panel C. Reduced form (OLS)				
ln distance to LCP	−0.008***	−0.007**	−0.007**	−0.006**	−0.007**
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
County FE?	Yes	Yes	Yes	Yes	Yes
ln Distance to endpoint?	Yes	Yes	Yes	Yes	Yes
Additional controls?	No	Yes	Yes	Yes	Yes
Additional railroads?	No	Yes	Yes	Yes	Yes
Eriksgata?	No	No	Yes	No	No
17th-century postal routes?	No	No	No	Yes	No
Medieval roads?	No	No	No	No	Yes
Kleibergen-Paap F-stat (panel A)	20.34	22.89	24.29	22.44	20.82
Kleibergen-Paap F-stat (panel B)	48.58	53.49	54.31	52.23	49.27
Observations	1503	1503	1503	1503	1503

Notes: 2SLS (panels A and B) estimates of Eq. (1) and OLS estimates (panel C) of the reduced form. Additional controls include: *ln* area, *ln* altitude; an indicator for canal access; the share of land suitable for cultivating barley, potatoes, rye, and wheat; *ln* distance to the coast; *ln* distance to the nearest town; *ln* terrain ruggedness; and the share employed in industry and *ln* population in 1850. Additional railroads includes two indicators for whether a parish is connected to a state (i.e., non-trunk line) or privately owned railroad. Standard errors are clustered at the county-level. Statistical significance is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

results in a ratio of about –4.9 (see Table A.10). In other words, for the results to become economically insignificant, the selection on unobservable factors would have to be almost five times as large as the selection on the included observables and work in the opposite direction. Notably, similar results are obtained if a richer set of controls are added to the full model.[32] Unobserved or omitted factors are thus unlikely to explain away the estimated impacts of the trunk lines. Yet, the fact that the OLS estimates increase in magnitude when adding controls suggest that the trunk lines may have been routed through areas with worse (unobserved) growth prospects. In that case, the baseline OLS estimates may be downward biased. Therefore, I proceed by using the LCPs as a source of plausibly exogenous variation in access to the trunk lines.

4.1.2. IV estimates

Table 5 displays the 2SLS second-stage estimates of Eq. (1) and OLS estimates of the reduced form. First, panel C documents the significant negative reduced-form link between *ln* distance to the nearest LCP and changes in the share employed in industry between 1850 and 1900. A negative and statistically significant relationship shows that rural parishes in proximity to the LCPs experienced relatively faster rates of structural transformation. Second, panels A and B report the second-stage 2SLS estimates using the 5 km indicator (panel A) or *ln* distance (panel B) to measure access to the trunk lines. IV estimates are consistently larger than the corresponding OLS estimates reported in Table 4, and they are typically statistically significant at least at the 10-percent level.[33] As reported in panel A, column 1, these estimates suggest that a parish that was traversed by a trunk line experienced an average increase in the share employed in industry of 6.9 percentage points between 1850 and 1900, which corresponds to about nine-tenths of a SD. Similarly, the corresponding estimate in panel B, column 1, suggests that a 1 SD increase in the distance to the nearest trunk line leads to a 0.22 SD relative decrease in the share employed in industry. In columns 3–5 of Table 5, I individually add additional proxies for historical trade routes described above. Estimates remain similar in magnitude and statistical precision, which largely reduces concerns that the correlation between the LCPs and historical transportation routes biases the IV estimates. Moreover, to document that the 2SLS estimates are robust to restricting the sample to *de facto* rural parishes or those located far away from the existing cities, as above, Table A.8 and A.9 shows that estimates are of a broadly similar magnitude and significance also in these subsamples.

[32] Table A.10 also presents additional calculations when including pre-rail growth, mining activity, and proximity to targeted areas. Consistently, ratios calculated based on a richer set of controls are of a similar magnitude (absolute values range between 3.8–4.8) thus suggesting that selection on unobservables must be substantial also relative to these additional observables to explain away the impact of the trunk lines.

[33] For purposes of comparison, Table A.7 reproduces the 2SLS estimates using OLS when including the identical set of additional controls (i.e., the *ln* distance to the nearest endpoint and proxies for historical trade routes) as in the 2SLS estimation reported in Table 5.

Table 6
State trunk lines, employment, and population growth, 1850–1900: OLS and 2SLS estimates.

	ln Population		*ln* Ind. employment	
	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)
	Panel A. Outcome: △ 1850–1900			
Trunk line (=1)	0.188***	0.207**	0.328***	0.687*
	(0.021)	(0.097)	(0.089)	(0.402)
	Panel B. Outcome: *ln* density per km^2, 1900			
ln distance to trunk line	−0.048***	−0.044**	−0.108***	−0.159***
	(0.010)	(0.020)	(0.022)	(0.051)
County FE?	Yes	Yes	Yes	Yes
Additional controls?	Yes	Yes	Yes	Yes
Additional railroads?	Yes	Yes	Yes	Yes
Kleibergen-Paap F-stat (panel A)	–	22.89	–	22.89
Kleibergen-Paap F-stat (panel B)	–	53.49	–	53.49
Observations	1503	1503	1503	1503

Notes: Panel A presents OLS and 2SLS estimates of Eq. (1). Panel B presents OLS and 2SLS estimates from regressing *ln* employment or population density per km^2 in 1900 on the *ln* distance to the trunk lines. Additional controls include: *ln* area, *ln* altitude; an indicator for canal access; the share of land suitable for cultivating barley, potatoes, rye, and wheat; *ln* distance to the coast; *ln* distance to the nearest town; *ln* terrain ruggedness; and the share employed in industry and *ln* population in 1850. (All 2SLS regressions also include *ln* distance to the nearest endpoint.) Additional railroads includes two indicators for whether a parish is connected to a state (i.e., non-trunk line) or privately owned railroad. Standard errors are clustered at the county-level. Statistical significance is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

In sum, these estimates show that the spread of the state trunk lines had a substantial causal effect on structural transformation in rural areas over the latter half of the 19th century. A larger magnitude of the IV relative to the OLS estimates suggest that the trunk lines may have traversed areas with worse growth prospects, which results in a downward bias in the OLS estimates. Yet, an alternative explanation is that the instrument picks up a local average treatment effect (LATE). While I cannot provide direct evidence against a LATE interpretation, a negative bias is seemingly more consistent with the fact that OLS estimate increase in magnitude when adding controls (see Table 4) and historical evidence that state planners in many cases routed lines through disadvantaged rural areas (see Section 2), which echoes the general finding in the literature that transport infrastructure tends to be allocated to areas with worse growth prospects (e.g., Baum-Snow, 2007; Duranton and Turner, 2012; Jedwab and Storeygard, 2019).

4.2. Additional estimates and robustness checks

This section presents additional results to further support the main result documented above: that rural areas traversed by the state trunk lines experienced substantially more rapid structural transformation between 1850 and 1900. First, I document similar relative changes in terms of industrial employment and population. Second, I provide a battery of falsification and placebo checks to further support a causal interpretation of these links. Lastly, I then examine potential heterogeneities in the estimated impacts of the trunk lines showing that there is no evidence that the impacts differed in, for example, initially more populous areas or those with worse alternative transport opportunities.

4.2.1. Employment and population growth

A process of local structural transformation is likely to be accompanied by an inflow of migrants leading to overall increases in both employment and population levels. Although the previous regressions have documented a relative structural reallocation towards industrial activities, they remain silent regarding such absolute increases in manufacturing employment and population.[34] To that end, Fig. 5 provides flexible estimates from Eq. (1) examining changes in *ln* population and manufacturing employment between 1850 and 1900 across 5 km bins of distance to the trunk lines.[35] Areas located within 5 km of the trunk lines saw more rapid growth in both employment and population relative to more distant locations. Similarly, Table 6, panel A, presents OLS and 2SLS estimates from Eq. (1) where the outcome variable again is changes in *ln* population or manufacturing employment between 1850 and 1900 respectively. IV estimates are larger in magnitude relative to the OLS estimates both when considering changes in employment and population: estimates in columns 2 and 4 suggest a relative increase of almost 25 and 100 percent (or 0.207 and 0.687 log points)

[34] An alternative explanation for the observed relative shift towards industry may thus, for example, be that agricultural workers simply exited rural areas as they became connected to the network, which mechanically inflates the relative share employed in industry through a reduction in the agricultural population.

[35] As some parishes report zero employment in industry in 1850, I add one worker to each parish before takings logs.

in population and manufacturing employment respectively, although the latter is imprecisely estimated. Yet, these estimates suggest that large increases in industrial employment and population can causally be attributed to the coming of the railroad.

An alternative empirical approach, which facilitates comparisons with existing estimates in the literature, is to estimate the cross-sectional elasticity of industrial employment and population density with respect to the distance to transportation networks. Such OLS and 2SLS estimates are reported in Table 6, panel B, obtained from regressing the *ln* employment or population density per km^2 in 1900 on the *ln* distance to the trunk lines, while conditioning on the full set of controls. 2SLS estimates reported in columns 2 and 4 suggest an elasticity of –0.044 and –0.159 for population and employment respectively. Interestingly, these estimates are comparable to estimates reported in the modern literature surveyed in Redding and Turner (2014, p.42) where doubling the distance to a highway or railroad typically leads to a 6–15 percent decline in employment or population density. Thus, the impacts of the Swedish trunk lines on the spatial distribution of economic activity are seemingly similar compared to transportation networks in developing and developed countries today.

4.2.2. Falsification and placebo tests

To further support a causal interpretation of the main results, I provide evidence from three falsification and placebo checks showing that (*i*) there are no differences in population growth or structural transformation during the decades leading up to the construction of the railroad network; (*ii*) that there are no effects for lines that were planned but ultimately not built; and (*iii*) that the estimated treatment effects constitute clear outliers relative to the distribution of placebo estimates, obtained by randomly reshuffling the treatment indicator across parishes.

Table A.11 presents estimates of Eq. (1) using changes in *ln* manufacturing employment, *ln* population, and the share of the labor force employed in industry between 1830 and 1850 (i.e., over the two decades that preceded railroad construction) as the outcome, using either the 5 km indicator or *ln* distance to measure access to the trunk lines. Reassuringly, there are no pre-existing trends in terms of industrialization or population growth in areas that later would be traversed by the trunk lines, at least among the parishes for which data is available.[36]

Table A.12 presents evidence from four placebo checks that examine the effect of being located along routes of Ericson's proposed network, additional line proposals put forth in the early 1870s by a state committee and municipalities respectively, or along the LCPs but where no railroad was actually built by 1900. If the planning process assigned routes to areas with brighter (worse) growth prospects, one would expect to find faster (slower) growth there even in the absence of rail construction. Reassuringly, however, none of these estimates are statistically significant (columns 1–3). Similarly, column 4 shows that areas traversed by the LCPs, but where no railroad was built, do not exhibit higher rates of industrialization or population growth. These findings lend support to the validity of the estimates above, as well as the exclusion restriction of the IV analysis.

Figure A.3 displays the distribution of the estimated impacts of the trunk lines and randomized placebo estimates to examine whether the former are likely to be spurious. Placebo estimates are obtained by randomly reshuffling the observed distribution of the treatment indicator (i.e., being located within 5 km of a trunk line) across parishes and estimating the baseline specification in Eq. (1) using the three main outcomes. I repeat this procedure 1000 times for each outcome. Figure A.3 displays the distribution of these placebo estimates and kernel density plots when excluding and including the full set of controls respectively, as well as the actual estimated treatment effects. These figures demonstrate that the baseline estimates constitute clear outliers relative to the distribution of placebo estimates, which further reduces concerns that the results are spurious.

4.2.3. Spatial reallocation and structural transformation

Although the previous sections have documented both absolute and relative increases in industrial employment in areas traversed by the state trunk lines, these increases could reflect a local reallocation of manufacturing activity between connected and unconnected parishes as in Chandra and Thompson (2000). While the manufacturing share of employment more than doubled over the latter half of the 19th century, reducing concerns that the results solely reflect a reallocation of economic activity, areas in proximity to the railroad are likely to also have attracted firms and workers from unconnected nearby areas. Thus, an empirical concern is that local structural transformation partly reflects a displacement of economic activity, which would suggest that the aggregate impact of the railroad is substantially smaller than my estimates would suggest.

First, to get a sense of the extent of such redistribution, Figure A.4 presents OLS and 2SLS estimates of Eq. (1) when sequentially excluding unconnected parishes located between 5 and more than 100 km from the network. If a relocation of industrial activity from areas in proximity to the tracks is economically important, we would expect that the estimates decrease in magnitude when nearby areas are excluded from the control group (see Redding and Turner 2014, pp.21–23). Yet, relative increases in industrial activity remain stable in magnitude and statistical precision, suggesting a limited role for reallocation.

Second, I examine changes across larger regional units. If reallocation is an important driver, the estimated effects would attenuate when studying larger regional units to the extent that relative increases in industrial employment are offset by decreases elsewhere within the same region. To examine larger regional units, I collapse the 1503 rural parishes to 255 municipalities, which constitute aggregates of parishes that roughly approximate local labour markets. For simplicity, I define the municipality-level treatment indicator to take the value 1 if more than half of all parishes (weighted by area) within a municipality are within 5 km of a trunk line in

[36] Heckscher (1907, p.21) estimates that among parishes with a station located along the four oldest trunk lines, 33 percent experienced relative *increases* in population, while 43 percent experienced relative *decreases* prior to construction. A larger share of station-parishes experiencing slower growth prior to construction, if anything, suggests that the trunk lines connected areas with worse growth prospects. Note that this comparison differs from that above because I focus on all parishes traversed by the trunk lines, rather than those where a station was later established.

1900. These estimates are reported in the upper half of Table A.13, while the bottom half presents similar estimates when using the area-weighted average *ln* distance to the nearest trunk line as the access measure. Municipalities with better access to the trunk lines saw more rapid growth in industrial employment and population, as well as structural transformation. Moreover, estimates are of a broadly similar magnitude as the baseline estimates, or larger, again suggesting that reallocation is of limited empirical relevance. In sum, this suggests that the expansion of industrial activity in areas traversed by the state trunk lines likely reflects genuine growth rather than a local reallocation of manufacturing activity.

4.2.4. Heterogenous impacts of the state trunk lines

A consistent finding in previous sections is that areas traversed by the trunk lines saw large increases in industrial activity and population over the next half century. In this section, I lastly examine whether these estimates mask heterogeneities. I augment the baseline specification in Eq. (1) with a set of interactions that consist of an indicator variable capturing whether a parish is located within 5 km of one of the state trunk lines and a variety of pre-determined parish characteristics. Table A.14 reports estimates that examine the heterogeneity of the treatment effect across a variety of characteristics using long differences in the share employed in industry (panel A), *ln* industrial employment (panel B), and *ln* population (panel C) as the outcome. As shown in column 1, there is little evidence that endowments of agricultural land slowed down structural transformation as trade integration improved due to the diffusion of railroads (e.g., Matsuyama, 1992).[37] Similarly, there is no evidence that the impact of the railroad was generally larger in areas with initially poorer transport opportunities in columns 2–4 (e.g., Jedwab and Storeygard, 2019).[38] Columns 5 and 6 show that initially more industrially developed or populous parishes did not see differential effects due to the arrival of a trunk line.[39] A related hypothesis is that proximity to urban agglomerations may have led to a shifting of manufacturing activity from rural to urban areas (e.g., Faber, 2014), or that the improved connectivity of rural areas promoted a decentralization of industrial activity from urban to rural areas (e.g., Baum-Snow et al., 2017). Yet, there is no heterogenous effect based on a parish's proximity to urban agglomerations (column 7), which suggests that neither of these explanations are empirically relevant. Lastly, I examine interaction effects with other state and private railroads, but find no clear evidence that parishes traversed by a trunk line *and* another state or private line experienced differentially more rapid growth or structural transformation (see Table A.15).

5. Conclusions

Swedish manufacturing experienced a remarkable growth in terms of employment and output between the mid-19th century and the outbreak of World War I, contributing to rapid income and wage convergence with the European industrial leaders. Notably, much of this growth took place in the countryside. Around mid-century, the Swedish state constructed the backbone of the modern railroad network that partly aimed to promote development in rural areas. While contemporaries and a long line of historians have attributed the accelerated pace of industrialization and its rural character to the diffusion of the railroads, there has been limited quantitative evidence to support this argument.

This paper examined the long-run impact of the state trunk lines among rural parishes that they "accidentally" traversed. Rural areas penetrated by one of these trunk lines experienced sharp increases in manufacturing activity, as well as population, leading to substantially more rapid structural transformation over the next 50 years. Thus, the coming of the railroad was an important causal factor behind the accelerated pace of industrial development in the latter half of the 19th century, as envisaged by Heckscher (1907) and Rostow (1960). Moreover, evidence that the spread of the state trunk lines led to structural transformation in previously isolated rural areas speaks directly to the literature noting the peculiarly rural nature of Sweden's industrialization (e.g., Heckscher, 1907; Schön, 2010; Söderberg, 1984).

An important caveat, however, is that these estimates do not identify the aggregate contribution of the railroad to the economy as in the vast "social savings" literature (e.g., Fogel, 1964; Hawke, 1970; Herranz-Loncán, 2006; Leunig, 2010). In the case of the U.S., Fogel (1964) famously suggested that extending the canal network could have offset a large portion of the losses that removing the railroad network would incur on the economy, though later work modify this conclusion (Donaldson and Hornbeck, 2016). Yet, it is unlikely that other types of transportation infrastructure could have spurred industrialization in Sweden the same way that the railroad did. Canals constituted the bulk of state investment in infrastructure in the first half of the 19th century (Westlund, 1998), yet had by mid-century failed to make a dent in industrialization or urbanization.[40] Similarly, the patchwork of unpaved roads offered little opportunities for mass transportation of goods or people due to seasonality and prohibitively high costs. In that light, the coming

[37] Agricultural suitability is measured by averaging the share of each parish's land that is highly suitable for cultivating barley, rye, potatoes, and wheat; the additional indicator in the interaction takes the value 1 for parishes with an above-median share of highly suitable land.

[38] Alternative transportation opportunities are measured by whether or not a parish is landlocked (defined as being located more than 30 km inland), had access to the main pre-rail transportation network (an indicator taking the value 1 for parishes within 5 km of the pre-existing canals), and based on the *ln* ruggedness of the terrain (defined as an indicator taking the value 1 for parishes with above-median terrain ruggedness) that raised the cost of overland transport. Although there is some evidence that areas connected to the canal network, which were also traversed by a trunk line, experienced relatively slower industrialization (column 2), this is driven by a small number of rural parishes.

[39] Again, I use the median share of employment in industry and population in 1850 as the cutoffs to define the additional indicator in the interaction term.

[40] An important constraint was Sweden's northern latitude: Canal traffic was only in operation for about 8 months per year, as waterways froze in the winter (Heckscher, 1907, p.133). Notably, even over the latter half of the 19th century, there is no evidence of a significant positive link between proximity to canals and structural transformation in the regressions presented above.

of the railroad with its higher speed, lower costs, and year-round operation was seemingly crucial to connect a predominately rural population, widely dispersed across the country.

Although final judgments about the contribution of the Swedish railroads to aggregate growth must await further work, the railroad thus seems to have been "indispensable" to Sweden's rapid and mostly rural industrialization in the latter half of the 19th century. In that sense, my conclusions bear similarities to studies that have analyzed the impacts of the railroad in other backward countries with limited alternative transport opportunities finding substantial aggregate impacts (e.g., Coatsworth, 1979; Summerhill, 2003; Summerhill, 2005). Such a potential for infrastructure to ignite economic development is an encouraging conclusion, not least in light of the fact that governments around the globe continue to invest in transportation infrastructure to spur growth in disadvantaged areas today.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.eeh.2019.06.002.

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