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Locomotives of local growth: The short- and long-term impact of railroads in Sweden[☆]

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ABSTRACT

This paper studies the impact of railroads on 150 years of urban growth in Sweden, identifying the short- and long-term effects of a first wave of railroad construction. Difference-in-differences and instrumental variable estimates show that towns that gained access experienced substantial relative increases in population, though such growth mainly reflected a relocation of economic activity. Over the twentieth century, we find little evidence of convergence in town populations, despite the railroad network expanding further to connect nearly all towns. Evidence on historical investments and present-day factors is consistent with the idea that the transitory shock of the first railroads gave rise to path dependence in the location of economic activity.

1. Introduction

Economists and policy makers have long been concerned that high trade costs may constitute a barrier to economic development. Improving transport infrastructure is thus seen as crucial to spurring growth (World Bank, 1994, 2009). Yet, credibly identifying the causal effects of such improvements remains empirically challenging because transport infrastructure is not randomly assigned across locations. Moreover, in the presence of labor market rigidities or durable investments, long-run adjustments may be slow to materialize. Historical episodes of large-scale transportation improvements provide unique opportunities to analyze such adjustments in both the short and long run.

In this paper, we exploit the rollout of the largest public infrastructure network in Swedish history—the nineteenth-century railroads—to study the impact of transport infrastructure on urban development in a poor, rural and predominately agricultural setting. In a *first wave* of railroad expansion, between 1855 and 1870, state-financed lines evolved into the backbone of the modern Swedish railroad network. An overarching ambition of state planners was to connect the capital Stockholm with the other two major port cities. Yet, due to military concerns and developmental objectives, the main trunk lines were in many instances routed through disadvantaged interior areas, connecting many smaller cities and towns.

In our main empirical analysis, we examine the short-term impact of the first wave on urban growth. Difference-in-differences estimates show that towns that gained access to the network experienced substantial relative increases in population. OLS estimates may, however, be downward biased if state planners targeted places with low potential for growth, though we find few observable differences between connected and non-connected towns prior to the construction of the network. To adress this issue we use an instrumental variable (IV) strategy that exploit low-cost

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routes between major endpoints of the network. Falsification tests show no evidence for more rapid growth along these routes prior to construction and we find no effects where a railroad was not actually built. IV estimates are indeed substantially larger than OLS estimates and are robust to a range of additional controls, in a sample balanced on propensity score, as well as using two historical network proposals as alternative instruments.[1] Additional estimates for several sets of placebo lines, that were proposed but not built and lines that were constructed after 1870, are close to zero and statistically insignificant, suggesting that our estimates reflect the causal impact of rail infrastructure on urban growth.

While our estimates imply large relative increases in population for connected towns, we also demonstrate that growth came at the cost of nearby non-connected towns: relative increases are essentially zero compared to non-connected towns more than 90 km from the network. Much of the growth that we observe therefore likely reflect a reorganization of economic activity across towns, mirroring the general concern that gains from geographically targeted policies may partially or completely be offset elsewhere through a displacement of economic activity (Gottlieb and Glaeser, 2008; Kline and Moretti, 2013; Redding and Turner, 2014).[2]

After 1870, a surge in railroad construction led to a sharp reversal of differences in connectivity. Within two decades, towns that were not assigned a rail connection in the first wave had on average more rail connections per inhabitant than towns with early access to the network. We examine whether the short-term effects persisted as the railroad network was extended to nearly all towns, asking whether the transitory shock of the first railroads permanently shifted the location of economic activity across towns.

Towns that gained access to the network during the first wave continued to grow differentially faster throughout the nineteenth century and over the twentieth century differences in town sizes largely persisted. Are such persistent differences evidence of path dependence in the location of economic activity? We distinguish between two explanations. If there is a multiplicity of steady states, even a small shock may be capable of nudging initially similar towns into very different long-term growth trajectories (David, 1985; Krugman, 1991a; Arthur, 1994).[3] Alternatively, such persistence may reflect slowly depreciating sunk investments, in which case we would expect to observe a gradual convergence in town sizes.

Historical evidence from the late-nineteenth century is consistent with railroads increasing the pace of industrialization, promoting scale economies in manufacturing and a relative shift away from artisanal production. However, neither an industrial advantage in the early twentieth century nor a range of historically sunk investments in schools, electricity works, communications infrastructure or railroads explain the present-day variation in town size that we attribute to the population shock induced by the early railroads. Furthermore, comparing towns with an early rail connection to similarly large towns today—in terms of, for example, roads, railroads and housing prices—we find little to suggest that towns that gained access in the first wave persist due to slowly depreciating factors. Our results thus seem most consistent with the idea that the transitory advantage of an early rail connection permanently shifted the location of economic activity.

Our paper speaks to two strands in the literature. We contribute to a growing body of work that examines the causal impact of the railroad on historical development (Atack et al., 2010; Keller and Shiue, 2008; Donaldson, 2015; Donaldson and Hornbeck, 2015; Hornung, 2015), as well as recent efforts to disentangle the impact of transport infrastructure on regional trade (Michaels, 2008), urban form (Baum-Snow, 2007; Baum-Snow et al., 2012) and urban growth (Duranton and Turner, 2012; Storeygard, 2013). Similar to much of this literature, our results suggest that transport infrastructure can have substantial causal short-term effects on urban development, though we also document that such growth partly reflect a displacement of economic activity. More novel, our results show that such short-run effects can affect local development paths over a century of substantial economic modernization, during which Sweden transformed from one of the poorest countries in Europe to one of the richest in the world. In that sense, our results appear in a historical context very different from Jedwab and Moradi (2015) and Jedwab et al. (2015) documenting that African cities formed along the Colonial railroads and that cities persist at these locations today. We instead show how the staggered roll-out of a major transportation network can lead to persistent differences, despite such a network eventually connecting all locations. Importantly, this allows us to distinguish between historically sunk investments in rail infrastructure and path dependence as competing explanations for persistent differences in urban populations.

Our evidence of path dependence in the location of economic activity contributes to an emerging empirical literature on urban development and the existence of multiple spatial equilibria. Bleakley and Lin (2012), for example, document the formation and persistence of US cities at portage sites, despite this natural advantage being made economically irrelevant more than a century ago.[4] In contrast, we examine a man-made and reproducible advantage with more obvious policy implications. In that sense, our paper is related to Kline and Moretti (2014) that examine the long-run impact of a major US regional policy intervention under the Tennessee Valley Authority and Redding et al. (2011) that examine a shift between multiple equilibria in the location of Germany's main air hub following post-war division. Evidence of path dependence stands in constrast to the finding that economic activity is uniquely tied down by fundamentals even in the face of extreme shocks (Davis and Weinstein, 2002, 2008).[5] A potential explanation for why we observe path dependence emphasizes initial conditions: Swedish towns were small and the population was largely rural in the nineteenth century, ensuring that a shock of comparably small magnitude was able to permanently shift economic activity between locations. Our results thus have implications for debates about the potential impact of investments in major transportation networks in modern developing countries that are less urbanized.

The remainder of this paper is structured as follows. In the next section we provide a historical background, document the initial divergence and subsequent sharp reversal in rail connectivity for towns in the first wave relative to other towns over the last 150 years. Section 3 details our empirical strategies, with estimates of the impact of the first wave on short- and long-term patterns

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[1] Both Baum-Snow (2007) and Duranton and Turner (2012) find that IV estimates of the impact of highways in US cities are larger than OLS estimates, suggesting that the political allocation process assigned road infrastructure to slowly growing places.

[2] However, during the period under study a large reallocation of people from rural to urban areas took place, with the urban population more than tripling, meaning that the railroad could have contributed to urbanization in the aggregate. Though our estimates do not allow us to make inference about the railroads' nationwide effects, evidence on reorganization lends qualitative support to work that downplays the railroads' historical impact on aggregate growth (Fogel, 1964).

[3] A related literature discusses the feasability of "big push" policies (Rosenstein-Rodan, 1943; Murphy et al., 1989; Kline and Moretti, 2014).

[4] A related literature examines intra-city persistence; Brooks and Lutz (2014), for example, examine the persistent impact of Los Angeles' streetcars and Ahlfeldt et al. (2015) use the division and reunification of Berlin to study the role of market access in determining urban form.

[5] Also, see Brakman et al. (2004), Bosker et al. (2007) and Miguel and Roland (2011). As pointed out by Redding et al. (2011), historically sunk investments in infrastructure networks, around which reconstruction efforts could be coordinated, is one potential explanation for the fact that even extreme shocks do not seem to shift urban economies between steady states.

of urban growth provided in Section 4. In Section 5, we compare historical and contemporary factors, showing that persistent differences in town populations likely reflect path dependence. Section 6 provides some concluding remarks.

2. Historical background and data

2.1. Railroads and Swedish economic development in the nineteenth century

Sweden underwent a dramatic economic, political and social transition over the latter half of the nineteenth century as it converged with the industrial leaders (Heckscher, 1954; Schön, 2010). Between the mid-nineteenth century, when the first railroads went into commercial operation, and the outbreak of World War I, real wages increased from about half those paid to British workers to parity (Williamson, 1995). Sweden's economic transition was also reflected in a massive inflow of people from rural areas to towns and cities.[6] Over the same period, the number of urban dwellers increased from about 350,000 to 1.5 million, translating into an increase in the urbanization rate from 10% to 30% (Statistiska Centralbyrån, 1969).

An influential explanation for this remarkable growth spurt rests on Heckscher–Ohlin logic, emphasizing the role of the expanding nineteenth-century commodity trade, capital inflows and mass emigration (O'Rourke and Williamson, 1995a; O'Rourke and Williamson, 1995b). Eli Heckscher (1954, p. 240) himself, however, also underlined the importance of transport improvements, arguing that "[t]here is little doubt that the revolution in transport was far more important than foreign trade policies."

Prior to the railroad era, transport was principally confined to pack animals and horse-drawn carts on small unpaved roads, sleigh haulage on "winter roads" and shipment along the coast and canals (Heckscher, 1954). Transport costs were high and distinctly seasonal as canals, waterways and harbors froze in the wintertime. In addition, goods were typically transported using several modes, requiring frequent transshipment. Overland transport in excess of 200 km was not viable and important high weight-to-value goods, such as iron ore, could not profitably be hauled more than 30 km (Heckscher, 1907).

Against this background, railroads radically altered the means of transport, offering year-round operation at higher speed and lower cost: freight rates were cut by three-fourths, passenger costs decreased by half and travel speeds increased tenfold (Sjöberg, 1956). As a result, the railroad overtook water transport as the primary means for internal transportation already by the end of the 1860s (Westlund, 1992). Importantly, whereas substandard transport had constrained industrialization and town growth, the emerging network allowed cheap transportation of basic necessities to urban dwellers and raw materials to manufacturers, effectively reducing the barriers to urban expansion (Thorburn, 2000).

2.2. Constructing railroads: market forces or state planners?

A key question is whether transport infrastructure is most efficiently supplied by the market or local and national governments. Accordingly, whether the Swedish railroads should be constructed and managed by private companies or the state became a politically divisive issue. Although the prospects of a railroad network was debated in the Riksdag of the Estates (henceforth, the *Riksdag*) as early as the 1820s, it would take the better part of another three decades of polarized debate before the first lines went into commercial operation.[7] Over this period, two proposals for a national railroad network emerged: (1) a market-based proposal; and (2) a *de facto* state monopoly.

2.2.1. Adolf von Rosen's 1845 proposal

In 1845, the first proposal for a national railroad network was announced by Count Adolf von Rosen, a major in the Naval Mechanical Corps. In *The Times*, he offered British investors to buy stocks in the Swedish General Railroad Company, with the purpose of building "a good trunk line of railroad from Gottenburg to Stockholm, with important branches" (Nicander, 1980, p. 2). It was based on privately funded lines, that were to be managed and operated by private companies. Yet, the proposal encompassed an extensive national network, in order to address the interference of local political lobbying that had plagued piecemeal railroad construction elsewhere in Europe.

Several proposed routes were surveyed by von Rosen, aided by British engineers, and the Riksdag ordered topographical surveys of additional routes in the proposal. Collection of detailed geographical information lowered the cost of future railroad construction along surveyed routes, thus shaping the future rollout of the network (Rydfors, 1906): Fig. 2 shows that several of the lines constructed by 1870 indeed followed the initial routes proposed by von Rosen. In Section 3, we motivate the use of von Rosen's proposal as the basis for an IV and placebo strategy.

In the end, however, von Rosen's market-based approach to railroad construction resulted in a spectacular failure. Despite state concessions and interest guarantees, he failed to raise sufficient capital, leading to mounting public scepticism against leaving railroad construction in the hands of foreign investors and private enterprise (Rydfors, 1906).

2.2.2. Nils Ericson's 1856 proposal

In the Riksdag of 1853/54 it was decided that the major trunk lines of the network were to be planned, financed and constructed by the state. In 1855, Nils Ericson, a colonel in the Navy Mechanical Corps, was appointed to lead construction and was granted "dictatorial powers" to route the main trunk lines as he saw fit (Rydfors, 1906).

Ericson's proposal, announced publicly in 1856, centered around five main trunk lines, on which privately funded branch lines would then expand.[8] Although the main objective was to connect the capital Stockholm with the other major ports, a "mainspring [of Ericson's] 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" (Heckscher, 1954, p. 241). Furthermore, due to military concerns, these trunk lines were to be routed through the interior, avoiding towns located close to the coastline and existing transport routes (Heckscher, 1907). As a result, many historically important towns were left without a connection, prompting widespread criticism of Ericson's "horror of waterways and towns" (Heckscher, 1954, p. 241). Moreover, because placement of the main trunk lines

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[6] In 1860, formal barriers to rural–urban migration were dismantled through the abolishment of internal passport requirements. Additionally, two subsequent reforms in 1846 and 1864 abolished the guild system and introduced freedom to establish private enterprises respectively. Such reforms undermined the pervasive control of urban trades by guilds and burghers, reducing barriers to urban growth.

[7] Prior to its abolishment in 1866, the Riksdag was a national diet where the four estates (the nobility, clergy, burghers and peasants) were represented. See Rydfors (1906) for an overview of the early rail debates in the Riksdag.

[8] Private initiatives had to: (1) undertake a survey of the proposed route by an experienced railroad technician; (2) obtain a state concession; and (3) undergo a review by the technical authorities. If a proposal was approved, a joint stock company had to be formed. Financial support from the State could be granted if the company found other buyers of at least half of the offered stock. However, construction, traffic, and maintenance were to remain under direct state supervision. In that sense, the state retained a *de facto* control of the rollout of the entire network (Nicander, 1980).

affected construction costs for subsequent lines, they indirectly influenced the rollout of the entire network (Rydfors, 1906).

In the Riksdag of 1857, Ericson's proposal was rejected due to conflicts between the estates. In the wake of this decision, local political groupings gained the clout to block and influence the construction of remaining lines. Throughout the 1860s, protracted and polarized debates took place in the Riksdag as local politicians seized on the capital to ensure that lines were routed through their districts (Westlund, 1998). Political infighting meant that only a fraction of Ericson's proposal had been realized by 1870. Even though Ericson's plan was rejected by the Riksdag, however, the emerging network closely resembled his original intentions (see Fig. 2).[9] In Section 3, we describe how Ericson's proposal can be used as an instrument for the railroad network actually constructed and how the parts that remained unbuilt by 1870 provide a set of lines to use as the basis for a placebo strategy.

2.3. 150 years of railroads: from the first wave to the present day

Fig. 1 contrasts the average number of rail connections for towns that gained access in the first wave to those that did not.[10] Between 1855 and 1870, the first wave of railroad expansion commenced around Ericson's 1856 proposal. In 1862, the Western trunk line, directly connecting Stockholm and Gothenburg, was inaugurated. Three years later the Southern trunk line opened, connecting the three major cities and additional smaller towns along these routes. By 1870, the network spanned 1727 km, two-thirds of which was state-owned. Importantly, this network connected less than a third of all towns; among the twenty largest towns, less than half had a rail connection.

After 1870, differences in rail connectivity was sharply reversed as both state and private construction surged.[11] Railroads were now "often expected, as if by magic, to bring throbbing prosperity even to regions without any prerequisites for economic development" (Heckscher, 1954, p. 243). Already by 1890, towns connected *after* the first wave had more rail connections per inhabitant, with this advantage persisting over the twentieth century.[12] Rail connectivity itself is therefore an unlikely explanation for long-run differences in town populations.

2.4. Data on towns and railroads

Our sample consists of all Swedish towns that held town charters in 1840, prior to when railroad construction began. We exclude towns that formed after 1840, because their location is endogenous to the placement of the railroad network.[13] In addition, we exclude the three major cities (Stockholm, Gothenburg and Malmö) that constituted the terminal points of all network proposals and two insular towns, that by our definition could not gain access to the network. These restrictions reduce our baseline sample to 81 towns.

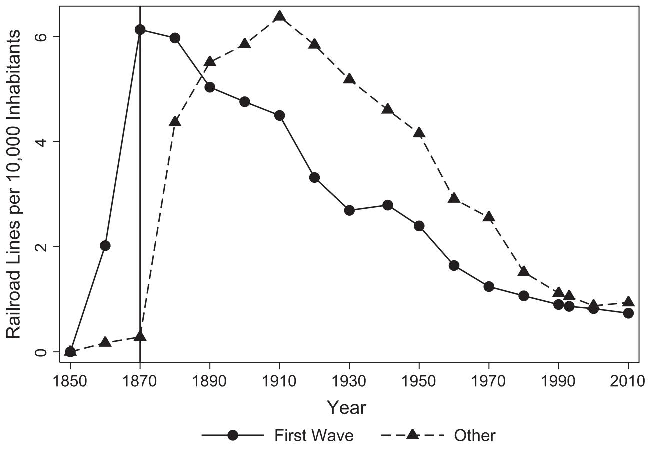

Fig. 1. Rail Connections for Towns in the First Wave and Other Towns, 1850–2010. *Notes:* This figure shows the average number of railroad lines per 10,000 inhabitants for towns that gained access to the network in the first wave and other towns. A solid vertical line denotes the end of the first wave (1870). Data is drawn from historical map collections of the Swedish railroad network and population censuses (see the Appendix for details).

For each town we collect data on population for each decade between 1800 and 2010 from historical population censuses obtained from Statistics Sweden and Nilsson (1992). Unfortunately, data on consistently defined urban areas does not exist for the more than two centuries that we examine in this paper. Our data therefore pertain to towns as defined by evolving administrative boundaries.[14] Additional town-level outcomes were collected from a variety of historical and contemporary sources, described in detail in the Appendix.

We reconstruct the 1870 railroad network using GIS software and historical maps of the railroad network obtained from Statistics Sweden. Similarly, we digitize von Rosen's and Ericson's network proposals from maps in Sjöberg (1956). Fig. 2 shows the extent of the railroad network as of 1870, proposed lines in the two major network proposals and the location of all towns in our unrestricted sample.

2.4.1. Summary statistics and balancing tests

Historical accounts suggest that the first railroads were not routed to connect towns with brighter growth prospects, which would imply minor observable differences in terms of pre-rail outcomes for towns that gained access to the network relative to nonconnected towns. If anything, the developmental objectives of state planners would suggest that towns that became connected may have had lower growth potential.

Table 1, panel A, reports average pre-rail characteristics for towns with (column 1) and without (column 2) access to the railroad network by 1870, and the difference-in-means and corresponding standard errors (column 3). Connected towns were slightly larger, but were not growing significantly faster between 1840 and 1855. Towns with and without rail access had similar

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[9] Even though the proposal was formally rejected, Heckscher (1954, p. 241) argues that Ericson "was able to enforce the realization of his plans with hardly any change."

[10] To be clear, Fig. 1 shows the average number of rail connections for towns irrespective of if these connections linked a town to the national network or not. In our empirical analysis, however, treatment status is based on having a direct connection to the network.

[11] Over time the distinction between state-owned and private railroads was blurred as the network was gradually nationalized beginning in the late nineteenth century. Following a decision in the Riksdag in 1938, the vast majority of the railroad network was nationalized in the 1940s and 1950s.

[12] Furthermore, as pointed out by a referee, other technical and organizational improvements—in terms of, for example, rail quality, organizational capacity and reliability—make the reversal in rail connectivity even more striking.

[13] Excluding towns that were incorporated after the railroads were constructed will, however, lead us to understate the long-run impact of the railroads, as there are many smaller urban agglomerations that we *ex post* know formed towns due to their location on railroad junctions (Heckscher, 1907). For example, all 34 urban agglomerations that were awarded town charters between 1910 and 1950 had access to a railroad line and several of these towns owed their existence exclusively to the railroad (Westlund, 1998, p. 84).

[14] If administrative boundaries changed differentially for towns that gained access to the railroad network during the first wave relative to other towns our estimates may be biased. However, in the Appendix we compare the present-day geographical area of towns which provides little evidence to suggest that towns in the first wave are larger than towns with a similar initial population. Table 8 further shows that there are no differences in terms of population density today. Lastly, most historical incorporations were also small (see Nilsson (1992)), reducing concerns that changing administrative boundaries are affecting our results.

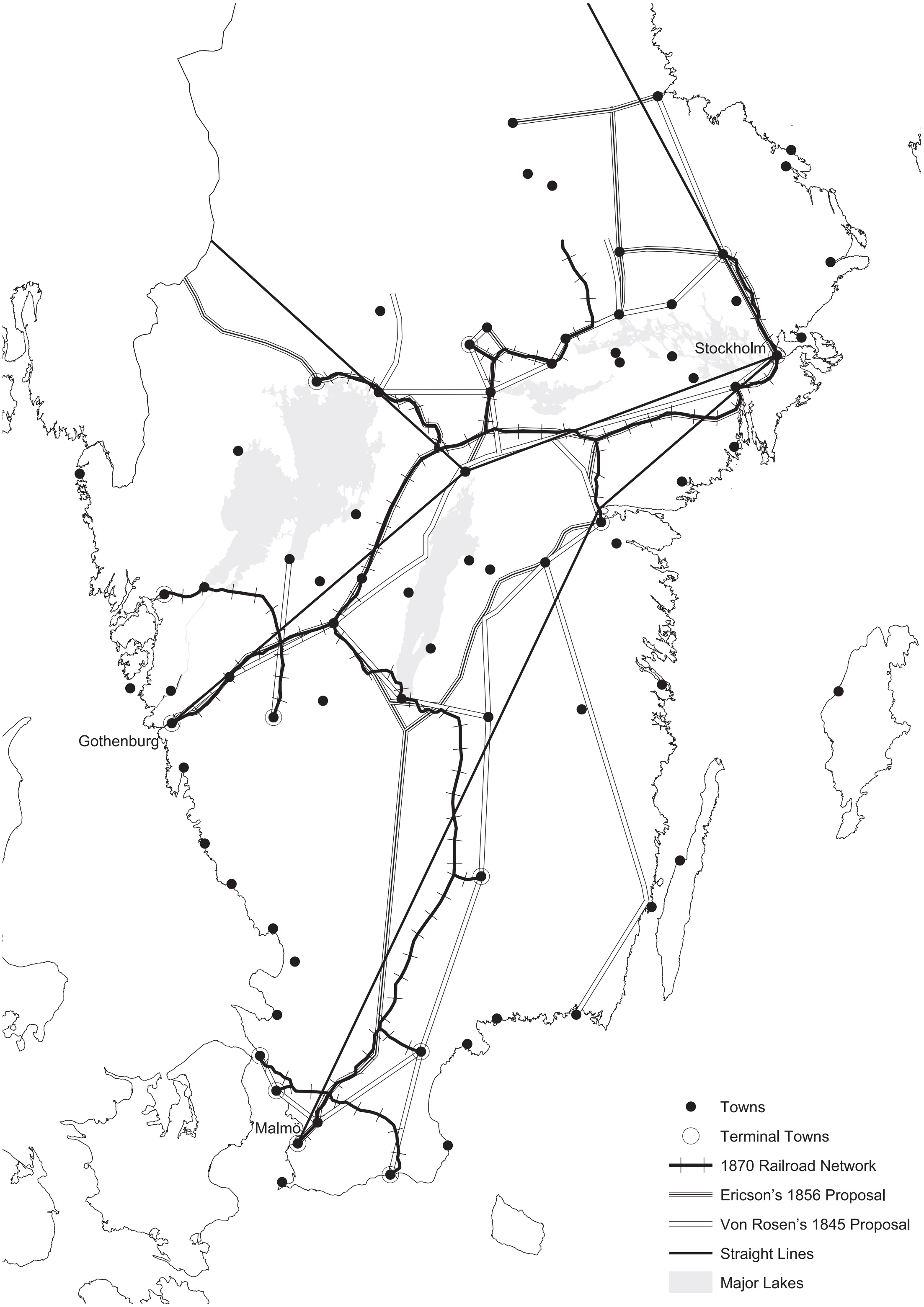

Fig. 2. The First Wave of Railroad Expansion, 1870. *Notes:* This map shows the actual railroad network as of 1870, the network proposals by Adolf von Rosen in 1845 and Nils Ericson in 1856 (see Section 2.2), the four major lakes, a set of "straight lines" that form the basis of our IV strategy described in Section 3.2 and all towns that held town charters in 1840, prior to when railroad construction began. For clarity, we exclude minor railroad lines that were built to exclusively connect two locations and do not show towns in northern Sweden. See the main text and the Appendix for a description of the underlying data.

Table 1
Pre-rail differences between towns with and without access in the first wave.

	Panel A. Baseline sample			Panel B. Balanced sample		
	First wave (1)	Other (2)	Diff. (1) – (2) (3)	First wave (4)	Other (5)	Diff. (4) – (5) (6)
Town populations and market access						
Town size, 1855 (ln)	8.072	7.412	0.660***	7.932	7.590	0.342
	(0.778)	(0.772)	(0.193)	(0.679)	(0.703)	(0.208)
Pop. growth, 1840–55 (%)	1.670	1.321	0.348	1.662	1.352	0.310
	(0.803)	(1.104)	(0.223)	(0.812)	(0.748)	(0.237)
Market access, 1855 (ln)	7.706	7.574	0.132	7.692	7.758	−0.066
	(0.226)	(0.508)	(0.082)	(0.242)	(0.303)	(0.081)
Geography						
Elevation (m)	61.459	33.616	27.843*	64.702	58.913	5.789
	(60.765)	(48.991)	(14.327)	(64.800)	(57.674)	(18.711)
Coast (=1)	0.273	0.492	−0.219*	0.278	0.250	0.028
	(0.456)	(0.504)	(0.117)	(0.461)	(0.441)	(0.137)
Major lakes (=1)	0.273	0.186	0.086	0.278	0.321	−0.044
	(0.456)	(0.393)	(0.109)	(0.461)	(0.476)	(0.141)
Employment structure, 1855 (% of local labor force)						
Industry	8.495	5.327	3.168	6.389	6.600	−0.211
	(10.777)	(7.394)	(2.470)	(6.042)	(9.252)	(2.255)
Artisans	53.200	48.893	4.307	53.000	52.736	0.264
	(10.799)	(13.395)	(2.873)	(9.613)	(13.878)	(3.465)
Trade	9.259	9.412	−0.153	9.928	10.739	−0.812
	(4.429)	(7.325)	(1.338)	(4.647)	(10.077)	(2.200)
Services	12.218	11.205	1.013	13.044	12.779	0.266
	(9.128)	(9.873)	(2.318)	(9.833)	(10.394)	(3.032)
Shipping	2.118	8.503	−6.385***	2.017	1.943	0.074
	(2.556)	(12.900)	(1.770)	(2.402)	(2.482)	(0.734)
Military	4.855	4.019	0.836	5.717	5.486	0.231
	(8.951)	(9.860)	(2.286)	(9.723)	(11.150)	(3.108)
No. of towns	22	59	81	18	28	46

Notes: This table compares average pre-rail (1855) characteristics for towns that gained access to the railroad network in the first wave to towns that did not. Columns 1, 2, 4, and 5 report means and standard deviations (in parentheses) and columns 3 and 6 report difference-in-means, from an OLS regression of each characteristic on a dummy variable taking the value 1 for towns that gained access in the first wave and corresponding Huber–White standard errors (in parentheses). Panel A reports characteristics for our baseline sample and panel B reports characteristics for a sample balanced on propensity scores, estimated based on all pre-rail characteristics reported in this table and each town's longitude, latitude and their interaction. See Appendix for a description of the data. Statistical significance based on Huber–White standard errors is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

access to domestic urban markets and a similar sectoral structure, measured by employment shares in artisanal, trade, military, manufacturing and service occupations.[15] However, because the early rail lines were routed through the interior, towns with an early rail connection were naturally less likely to be located at the coast and had a smaller share of the labor force employed in the shipping sector. Overall, these results are largely consistent with historical accounts stressing that the early railroads did not connect towns with better observable growth prospects, suggesting that a comparison of connected and non-connected towns may be informative about the impact of the railroad. However, ultimately the extent and direction of any bias remains an empirical question which we resolve by using alternative identification strategies.

3. Empirical strategies

3.1. Difference-in-differences

To examine the short-term impact of rail access on town populations, we compare towns that gained access to the network during the first wave to a control group of towns that did not using a difference-in-differences strategy. Using data for the years 1840, 1855 and 1870, we estimate Eq. (1) in which the outcome is the population (P_{ijt}) of a town i, located in region j, observed in year t:

$$\ln\left(P_{ijt}\right) = \alpha_i + \theta_{jt} + \lambda_t + \delta\left(FW_i \times Post_t\right) + \mathbf{Z}'_{\mathbf{it}}\beta_i + \varepsilon_{ijt} \qquad (1)$$

Our treatment indicator consists of an interaction between the dummy FW_i that takes the value 1 for towns that gained access to the railroad network in the first wave (1855–1870) and 0 for all other towns and $Post_t$ that takes the value 1 for the year 1870 and 0 for all other periods. Town fixed effects (α_i) capture time-invariant and town-specific factors, potentially correlated with gaining access to the network. Period fixed effects (λ_t) capture the many factors causing urbanization in this period. To take into account the possibility that towns located along the coast or the major lakes experienced differential relative changes in population, we include dummies for being located at the coast and one of the major lakes as well as each city's longitude, latitude and their interaction, all interacted with a full set of period effects.[16] Extended specifications also include region-by-period fixed effects (θ_{jt}) as well as town- and region-specific spatial trends in the vector $\mathbf{Z}_{\mathbf{it}}$.[17]

To examine the long-run impact of the first railroads, we use decadal data on town populations from 1800 through 2010. Importantly, including years prior to 1855 allows us to empirically assess

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[15] To measure each town's access to domestic urban markets, we construct a town-level measure of market access (MA) in the spirit of Harris (1954). For each town i we calculate $MA_i = \sum_{j \neq i} P_j D_{ij}^{-1}$, where P is the population of town j, and D is the geodesic distance between town i and j. Following the standard approach in the literature, we exclude each town's own population in the calculation.

[16] Controlling for access to the major lakes also indirectly captures the differential access to the canals constructed in the early nineteenth century.

[17] Region-by-period fixed effects are constructed based on a dummy for each of the eight National Areas (*Riksområden*), aggregated from counties as defined by historical administrative boundaries, interacted with period dummies (λ_t). Controlling for regional shocks is partly motivated by a natural resource bonanza in northern Sweden, due to booming timber exports, that attracted large inflows of migrant workers and regionally confined harvest failures that ignited mass emigration (see Schön (2010) and Enflo et al. (2014)).

whether towns that became connected in the first wave developed differently from other towns before construction began. We modify Eq. (1) by allowing the effect of a rail connection in the first wave to vary by decade:

$$\ln(P_{it}) = \alpha_i + \lambda_t + \delta_t FW_i + \mathbf{Z_i'}\beta_{it}\varepsilon_{it} \tag{2}$$

where the coefficient δ_t returns the average difference in population between towns with and without access to the network by the end of the first wave, relative to the omitted base year 1855. A virtue of this approach is that because nearly all towns gained access to the railroad network after 1870, estimates of $\triangle\,\delta_{t>1870}$ reflect the degree of persistence accruing from an early rail connection, rather than the direct effect of the railroad itself.

To examine whether observable pre-rail differences may bias our estimates, alternative specifications compare towns with an early rail connection to a control group of observationally similar non-connected towns. Table 1, panel B, reports mean characteristics for a subsample balanced on propensity scores, showing that statistical differences between towns with and without access to the network attenuate, suggesting that this restricted control group constitutes a relevant counterfactual. If towns of the first wave were selected based on these observable characteristics, estimates in this balanced sample would correct for this source of bias.

Identification of the impact of the railroad (δ) rests on the assumption that towns that gained access to the network would have developed similar to other towns in the absence of railroad construction. Although this assumption is not directly testable, comparisons of pre-rail outcomes and trends do not suggest that such an assumption is violated (see Table 1). Yet, if the first wave of railroads were correlated with unobserved time-varying factors so that $cov(Rail_{ijt},\varepsilon_{ijt}|\alpha_i,\theta_{jt},\lambda_t,\mathbf{Z'_{it}}) \neq 0$, OLS estimates of δ in Eq. (1) may be biased. In particular, since Ericson's plan had explicit developmental objectives, one concern is that OLS estimates may be downward biased if towns with worse growth prospects were being targeted by planners. To further alleviate concerns that a non-random placement confounds our OLS estimates we develop an IV strategy, drawing upon approximate low-cost routes between the major endpoints and the two network proposals as sources of identification.

3.2. IV strategies

Our main instrument exploits the fact that the main trunk lines were constructed to connect certain important endpoints along the shortest route (Rydfors, 1906), which meant that several towns by virtue of their location along these routes exogenously gained access to the network.[18] Low-cost routes are approximated by connecting each of the endpoints by straight lines, avoiding major natural obstacles, corresponding to the shortest distance between each set of endpoints (see Fig. 2). Based on the network proposals and discussions in Rydfors (1906) and Sjöberg (1956) we identify these other endpoints as Gothenburg, Malmö, the mining regions in the north, and Norway.[19] In practice, since towns are represented as a point in longitude–latitude space, we create buffer zones along the straight lines and define our instrument as a dummy, taking the value 1 for all towns located in the buffer zone and 0 for all other towns. In our analysis, we report results from using a buffer of 10 and 20 km respectively, though results are similar using larger or smaller buffers.

Location in a straight line buffer is a potent predictor of being connected: more than a third of the towns located within 10 km of the straight lines also gained access to the network in the first wave. For the exclusion to hold it requires that location in these straight line buffer zones is not correlated with other determinants of growth. Although we cannot test this directly, we provide evidence from a falsification test in the Appendix, showing that towns located along these straight lines were not larger and did not grow faster prior to the rollout of the network and in the next section we discuss additional placebo tests to further support the exclusion restriction.

As two complementary instruments, we use Von Rosen's 1845 and Ericson's 1856 proposals. Although the former proposal may be upward biased due to its market-orientation and the latter may be downward biased due to its developmental objectives, neither proposal was conceived to directly promote urban growth.[20] Importantly, proposed rail routes are also less likely correlated with contemporaneous town-level economic changes between 1855 and 1870, thus correcting for the breakdown of the planning process in the 1860s that may have led to changes in how railroads were allocated. The relevance of these two instruments rely on the strong first stage relationship between each proposal and actual rail lines constructed; more than half of the towns included in von Rosen's and Ericson's proposals gained access to the network during the first wave. We construct our instruments as a dummy, taking the value 1 if a town was included in each proposal respectively and 0 otherwise.

In practice, we use these instruments to predict rail connections in place by 1870, corresponding to the following first- and second stage:

$$R_{it} = \zeta_i + \phi_t + \psi\left(L_i^z \times Post_t\right) + \mathbf{Z'_{it}}\beta_i + \vartheta_{ijt} \tag{3}$$

$$\ln\left(P_{ijt}\right) = \alpha_i + \lambda_t + \sigma\widehat{R_{it}} + \mathbf{Z'_{it}}\beta_i + \varepsilon_{ijt} \tag{4}$$

where L_i^z denotes one of the instruments z that are interacted with a dummy taking the value 1 for the year 1870 and 0 for other periods, used to predict rail connections ($\widehat{R_{it}}$) in the second stage; ζ_i and α_i are town- and ϕ_t and λ_t are period fixed effects respectively; and $\mathbf{Z_{it}}$ is a vector of control variables.

Identification of the impact of an early rail connection (δ) in Eq. (4) requires that the instruments $z$do not affect urban populations through channels other than rail connections actually constructed. To further support this exclusion restriction, we provide indirect evidence from four placebo tests.

3.3. Placebo tests

To assess the validity of our IV strategies, we conduct four placebo tests where we examine the "effects" of location in a straight line buffer zone but where no rail construction took place, for routes that were proposed but for plausibly exogenous reasons not built by the end of the first wave, and lines only constructed *after* 1870.

In practice, we create a set of placebo lines from our straight line buffer zones that include towns that were located along the straight lines, but where no railroad was actually constructed. If

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[18] In a similar way, Banerjee et al. (2012) exploits the fact that historical railroads in China were constructed between cities in the interior and the Treaty Ports, established after the First Opium War, to identify the effects of transportation infrastructure on regional growth. See Redding and Turner (2014) for an overview of related papers using this 'inconsequential units approach' for identification.

[19] In practice, we begin to create the low-cost route between Stockholm and Gothenburg. Next, with this line in place we create the shortest route to the Norwegian border, corresponding to the line emanating from Askersund. As the endpoint for the northern straight line we use the town of Östersund, the only major urban agglomeration in the northern interior. Recall that we exclude the major endpoints from our analysis.

[20] Estimated pre-rail differences in population growth for towns that were included in each proposal relative to excluded towns are close to zero and statistically insignificant. Two separate OLS regressions of the average annual percentage population growth between 1840 and 1855 on a dummy taking the value 1 for towns present in von Rosen's and Ericson's proposals and 0 for other towns yield a coefficient of -0.08 (s.e. $= 0.21$) and 0.16 (s.e. $= 0.24$) respectively.

the straight lines are correlated with other growth determinants, we would expect to find that towns located in their buffer zones experienced more rapid growth than non-connected towns even if they actually were not connected. Similarly, from von Rosen's 1845 and Ericson's 1856 proposals we create two sets of placebo lines that include all lines that were proposed, but not built by 1870. Most of these routes were indeed constructed after 1870. Moreover, proposed lines that were not built prior to 1870 were typically held up due to political infighting in the Riksdag, plausibly unrelated to relative differences in local growth.[21] A fourth set of placebo lines are based on rail connections that opened between 1870 and 1880, a period in which four-fifths of the mileage constructed was private (Nicander, 1980). Because most of these connections were constructed by private financiers, seeking out locations with bright economic prospects, they likely reflect underlying differences in growth potential.

Taken together, if our IV strategies are valid and our estimates are reflecting the causal effect of a rail connection we would expect the estimated effects for these lines to be close to zero. We estimate the effects for these placebo lines from Eq. (1), where they enter individually in the vector $\mathbf{Z_{it}}$.

4. Main results

4.1. The short-term impact of the first wave of railroads, 1840–1870

Table 2, panel A, presents OLS estimates of Eq. (1), showing that towns that gained access to the railroad network prior to 1870 experienced substantial and statistically significant relative increases in population between 1855 and 1870.[22] Our baseline estimate in column 1 suggests that access to the railroad network in the first wave led to an average relative population increase of 27% (0.23 log points). Taken at face value, rail connectivity can therefore account for the entire difference in growth between towns with and without access to the railroad network in the first wave.[23]

OLS estimates are slightly larger when we add additional controls or restrict the sample to towns with similar propensity scores in columns 2–4, which is consistent with connected towns, if anything, having worse growth prospects. Allowing for differential changes for towns located along the coast or the major lakes and in longitude/latitude (column 2), further reduces concerns that our estimates are merely reflecting population increases in interior towns relative to coastal towns. Including a full set of region-by-year fixed effects suggests that towns that gained access to a rail connection where located in regions that on average experienced slower population growth (column 3), consistent with the developmental objectives of state planners.

Table 2, panel B reports results from the four different placebo tests. Column 5 examines whether towns that were located in one of the straight line buffer zones but that were not connected by 1870 experienced more rapid growth relative to non-connected towns. Importantly, the fact that the estimated effect of being located in a straight line buffer is close to zero and not statistically significant provides indirect evidence in favor of the exclusion restriction of our main IV analysis. Estimates are also close to zero and statistically insignificant for the two sets of railroad lines that were included in von Rosen's 1845 and Ericson's 1856 proposal but that were not actually constructed by 1870, and the set of lines constructed first in the 1870s (columns 6–8).

Table 3, columns 1–4, report 2SLS estimates obtained using the straight line instrument to predict actual railroad lines in place by 1870. Panel A reports the first stage results, showing that the straight line instrument is a strong predictor of being connected in the first wave. Although a large literature emphasizes that IV estimates may be biased in the presence of weak instruments (Bound et al., 1995; Staiger and Stock, 1997; Stock et al., 2002), rather strong *F*-statistics in the first stage largely reduce such concerns and we can typically reject a 15% maximum IV bias (Stock and Yogo, 2005). IV estimates reported in panel B are consistently larger than the corresponding OLS estimates in Table 2, consistent with qualitative historical evidence that state planners were targeting areas with worse growth prospects: our most conservative IV estimate suggest that a connection to the network led to a 54% (0.43 log points) increase in population, which is about twice the size of our baseline OLS estimate. More precisely, since there is little to suggest that towns were selected based on observable characteristics (see Table 1), this suggests that towns were likely negatively selected based on unobservable characteristics in the first wave. Negative selection is also consistent with the fact that IV estimates increase in magnitude when we include additional controls or balance the sample on pre-rail observables (columns 2–4), though such differences are not statistically significant. Using a wider buffer (20 km) to define the straight line instrument in the first stage or using the two plans of the network as alternative instruments yields very similar estimates (columns 5–7), despite that they rely on a slightly different source of identifying variation.[24]

In sum, these results suggest a sizeable causal impact of the first wave of railroad expansion on urban growth. In the Appendix we examine the robustness of these results in several ways. First, we show that our estimates are very similar in subsamples that exlude large, small, terminal, and coastal cities respectively. Second, allowing towns to be on different growth trends based on whether they are located on the coast, their longitude/latitude, the region in which they are situated in, or their initial (1840) population does not affect our estimates. Lastly, there is little evidence of heterogenous treatment effects: initially larger towns, towns with better market access, or those located along the coast or the lakes all experienced similar increases in population due to the coming of the railroad. In the following two subsections we examine the extent to which relative population increases during the first wave reflect growth or reorganization and the whether rail connections that opened later in the nineteenth century had similar impacts.

4.1.1. Growth vs. reorganization

A central issue is to understand whether transport infrastructure leads to growth or mainly causes economic activity to be reallocated across locations (Redding et al., 2011). While estimates presented above suggest substantial relative gains for connected towns, growth may be driven by a relocation from nearby non-connected towns. In the presence of such general equilibrium

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[21] One such example is the debate over the Eastern trunk line, intended to directly connect Stockholm and Malmö through eastern Sweden. Rydfors (1906, p. 127) recounts how representatives from the northern regions blocked further construction of the Eastern trunk line for as long as construction of the northern line did not begin. Sjöberg (1956) provides additional evidence on similar episodes.

[22] Standard errors are clustered at the town-level in all specifications as suggested by Bertrand et al. (2004). Correcting for spatial dependence following Conley (1999), assuming a linear decay and cutoff at 100 km inflates standard errors marginally. In practice, this does not affect our statistical inference.

[23] Towns with access to the railroad network saw their population increase by 0.41 log points on average between 1855 and 1870, whereas towns without access experienced an average increase of 0.20 log points. In the absence of railroad construction the implied growth rate for towns with access in the first wave is $0.41 - 0.23 = 0.17$ log points, which in turn would imply a lower population increase relative to towns without access (-0.03 log points).

[24] A further robustness check is to drop the "western" straight line, emanating from Askersund, leaving only the three straight lines that emanate from Stockholm. Excluding the towns located along this route yields an estimate of 0.52 (s.e. $= 0.22$) and the Cragg–Donald Wald *F*-statistic remains above 10, reducing concerns about weak instruments.

Table 2
The short-run impact of the first wave on town populations, 1840–1870: OLS estimates.

	Panel A. Baseline results				Panel B. Placebo tests			
	Baseline (1)	Controls (2)	Region FE (3)	Matched (4)	SL (10 km) (5)	1845 Plan (6)	1856 Plan (7)	Built After 1870 (8)
$First\ wave_i \times Post_t$	0.234***	0.272***	0.281***	0.259***	0.227***	0.255***	0.250***	0.285***
	(0.048)	(0.048)	(0.052)	(0.062)	(0.050)	(0.054)	(0.051)	(0.057)
$Placebo\ line_i \times Post_t$					0.084	0.044	0.055	0.058
					(0.069)	(0.050)	(0.047)	(0.058)
Town FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region × period FE	No	No	Yes	No	No	No	No	No
Additional controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	243	243	243	138	243	243	243	243

Notes: This table present OLS estimates of Eq. (1) where the left-hand side variable is (ln) town population. Columns 1–3 report estimates in our baseline sample and column 4 reports estimates in a subsample balanced on pre-rail characteristics. Columns 5–8 report estimates where we also include separate treatment indicators for towns that were located in a straight line (SL) buffer zone but did not have access to the network by 1870, towns that were assigned a rail connection in one of the two existing network proposals that remained unbuilt by 1870, and for rail lines that were constructed after 1870 respectively. Additional controls include dummies for location on the coast or one of the major lakes and the longitude, latitude and their interaction, all interacted with period effects. Statistical significance based on standard errors clustered at the town-level is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 3
The short-run impact of the first wave on town populations, 1840–1870: IV estimates.

	Instrument: straight line (10 km)				Alternative IVs		
	Baseline (1)	Controls (2)	Region FE (3)	Matched (4)	SL (20 km) (5)	1845 Plan (6)	1856 Plan (7)
	Panel A. First stage (outcome: connected in first wave?)						
$L_i \times Post_t$	0.318**	0.300**	0.298**	0.326*	0.276**	0.479***	0.593***
	(0.129)	(0.139)	(0.151)	(0.181)	(0.135)	(0.092)	(0.089)
	Panel B. Second stage (outcome: ln population)						
$\widehat{R_{it}}$	0.440**	0.523**	0.528***	0.481**	0.584***	0.433***	0.432***
	(0.209)	(0.213)	(0.190)	(0.215)	(0.195)	(0.085)	(0.083)
Town FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region × period FE	No	No	Yes	No	Yes	Yes	Yes
Additional controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations	243	243	243	138	243	243	243
Cragg–Donald wald *F*-statistic (first stage)	11.75	10.18	8.97	5.37	9.32	44.10	59.52

Notes: This table presents 2SLS estimates of Eq. (1). In panel A, the outcome is a time-varying dummy variable taking the value 1 when a town became connected to the network in the first wave and in panel B the outcome variable is ln population of each town. Columns 1–3 and 5–7 report estimates in our baseline sample and column 4 reports estimates in a subsample balanced on pre-rail characteristics. Additional controls include dummies for location on the coast or one of the major lakes and the longitude, latitude and their interaction, all interacted with period effects. Statistical significance based on standard errors clustered at the town-level is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

effects, estimated relative changes in population would overstate the impact of rail connectivity on aggregate urban growth.

To distinguish between growth and reorganization, we compare outcomes for towns with a rail connection to non-connected towns at varying distances to the network. For this approach to be informative, it requires that more distant towns are less affected by the railroad. If reorganization is important, treatment effects should decrease as the control group is shifted to consist of non-connected towns located further away from the network.

Table 4 presents estimates of Eq. (1) where we gradually alter the control group to consist of towns located further from the network.[25] For example, column 2 compares towns that gained access to the railroad network during the first wave to non-connected towns located at least 20 km from the network. In each column, we then shift this cutoff outward in 10 km increments. Clearly, the treatment effect decreases as we shift the control group to consist of towns further from the network: relative population increases are essentially zero relative to towns more than 90 km away from the network. Empirical estimates are similar in our baseline sample of towns (panel A), when comparing only towns that were observationally similar prior to when railroad construction began (panel B) and when we allow for the fact that towns with access to waterborne transportation may have experienced differential changes in each period (panel C).[26]

In sum, these results suggest that population growth in towns that gained access to a rail connection in the first wave came at the cost of other towns in proximity to the emerging railroad network. Reorganization was seemingly more important than growth.[27] However, considerable rural to urban migration means that railroads may have contributed to urbanization in the aggregate, which is not reflected in our estimates.[28]

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[25] As the distance cutoff is shifted outward the number of towns in the control group decreases mechanically, which is why we do not report results for distances >90 km. However, we find no significant differences in population growth when continuing to shift the control group in 10 km increments up to 200 km.

[26] Allowing for differential changes for towns with waterborne transport is potentially important since coastal location is correlated with distance to the railroad network. Thus, when the control group is shifted to consist of towns further from the network it also increasingly consists of coastal towns.

[27] Reorganization may be even more important than suggested by the estimates discussed in this section, since OLS estimates likely understates the population increases due to the railroad.

[28] In 1855, about 6% of the Swedish population lived in the towns included in our baseline sample (in 2010, the share is 25%). Thus, while our estimates suggest that reorganization between towns is important, railroads may well have promoted rural–urban migration, leading to growth in towns everywhere.

Table 4
Did towns in the first wave grow at the expense of other towns?

Control group	>10 km (1)	>20 km (2)	>30 km (3)	>40 km (4)	>50 km (5)	>60 km (6)	>70 km (7)	>80 km (8)	>90 km (9)
Panel A. Baseline sample									
First wave$_i$ × Post$_t$	0.231^{***}	0.215^{***}	0.182^{***}	0.161^{***}	0.138^{**}	0.127^{*}	0.032	0.041	0.010
	(0.048)	(0.050)	(0.055)	(0.059)	(0.063)	(0.064)	(0.069)	(0.072)	(0.076)
Panel B. Balanced sample									
First wave$_i$ × Post$_t$	0.234^{***}	0.217^{***}	0.184^{***}	0.163^{**}	0.140^{**}	0.129^{*}	0.035	0.043	0.012
	(0.054)	(0.056)	(0.060)	(0.064)	(0.068)	(0.069)	(0.074)	(0.077)	(0.080)
Panel C. Controlling for differential changes for towns along the coast and major lakes									
First wave$_i$ × Post$_t$	0.271^{***}	0.263^{***}	0.256^{***}	0.251^{***}	0.237^{***}	0.210^{***}	0.073	0.102	0.053
	(0.048)	(0.052)	(0.056)	(0.063)	(0.064)	(0.066)	(0.093)	(0.078)	(0.087)
Town FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents estimates of Eq. (1) where we gradually shift the control group to consist of towns further away from the 1870 railroad network. The left-hand side variable is (ln) town population. Column 1 presents estimates comparing towns in the first wave to towns located at least 10 km from the railroad network, each column then shift this cutoff 10 km outward. Panel A reports estimates in our baseline sample of towns, panel B reports estimates from our sample that is balanced on pre-rail characteristics and panel C includes dummies for towns located along the coast and the major lakes, as well as the longitude, latitude and their interaction, all interacted with period effects respectively. Statistical significance based on standard errors clustered at the town-level is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

4.1.2. Did later rail connections matter?

Between 1870 and 1900, the railroad network expanded from 1727 km to more than 11,000 km, eventually connecting more than four-fifths of the towns without a rail connection in the first wave to the network. Did rail connections that opened *after* 1870 have similar effects on town populations as those documented for the early railroad lines, or were relative population shifts induced by the first wave persistent in the face of further railroad expansion in the nineteenth century?

Table 5 presents results from estimating of Eq. (1) adding an additional treatment indicator taking the value 1 as a town became connected to the railroad network. We expand our main sample to encompass decadal data on town populations from 1840 through 1900. Column 1 shows a 43% (0.36 log points) increase in population for towns that gained access in the first wave relative to other towns. This estimate is substantially larger than that obtained when comparing relative population changes between 1855 and 1870 (see Table 2, column 1), suggesting that towns that gained access in the first wave experienced relative increases in population also after 1870.

Estimated changes in population for towns that gained access after 1870 are small and statistically insignificant, however, which implies that these connections had no measurable relative impact on town populations.[29] In columns 2 and 3, we restrict the sample to rail connections that opened between 1870–1880 and 1870–1890 respectively, which yields similar results. Results are also similar when introducing region-by-period fixed effects (column 3), balancing the sample on pre-rail characteristics (column 5), or instrumenting for the first wave using the two network proposals and low-cost routes jointly in the first stage (column 6).

Taken together, the fact that later rail connections had little impact on town populations strengthen our interpretation of the first wave of railroads having a large and potentially long-lasting impact on patterns of urban growth. We next turn to examine the long-term impact of the first railroads until the present day.

4.2. The long-term impact of the first wave of railroads, 1800–2010

Fig. 3 graphs estimated differences in population for towns with an early rail connection relative to other towns, from estimating Eq. (2). Table 6 presents the underlying regression coefficients, where columns 1–3 correspond to Fig. 3a-c respectively. Importantly, prior to the railroads were constructed, relative differences in population are consistently estimated close to zero.

Between 1855 and 1900, towns with early access to the railroad network experienced a relative increase in population of 69% (0.53 log points). Relative differences in population further intensified through the 1930s and 1940s. Following World War II through the 1970s, however, the population of towns with early access to the railroad network decreased relative to other towns, after which relative differences stabilized around the same level attained in the early twentieth century. Albeit precision of the estimates attuenates over time, relative differences in population remains statistically significant at a 5% level in each decade from 1860 through 2010. Fig. 3b shows that estimated relative differences in population are very similar when we examine estimates from our sample that is balanced on observable pre-rail characteristics.

Fig. 3c reports estimated relative changes in the urban hierarchy, simply defined as the ranking of towns by their size.[30] We sort towns by their size S_{it}^{r} in year t, such that $S_{it}^{81} > S_{it}^{80} > \cdots > S_{it}^{1}$, and assign each town a rank ($r = 81, 80, \ldots, 1$) that is increasing in town size. We observe little relative changes in the urban hierarchy in the pre-rail era, but an average increase from 1855 through 1900 corresponding to an increase of six steps in the ranking for towns that gained access to a rail connection in the first wave relative to other towns. Over the twentieth century such relative increases remain largely stable, though a slight but imprecisely estimated decline from the 1950s and onwards is visible.

In sum, these results show that the first railroad lines led to a divergence of relative town populations in the nineteenth century. Towns with an early rail connection seem to have reached their long-run equilibrium level in the early 1900s, as reflected in largely stable relative differences in town populations over the twentieth century.

5. Channels of persistence

In previous sections, we have documented that towns that gained access to the railroad network in the first wave experienced substantial relative increases in population over the latter

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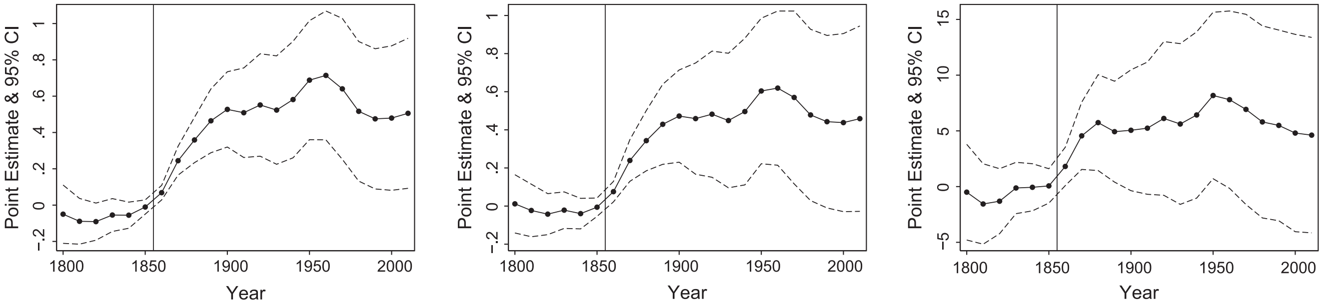
[29] A potential shortcoming of this analysis is that the control group increasingly consists of marginal towns as additional towns became connected, or that the quality of rail connections improved over time. Such compositional changes, however, would bias the estimated impact of later rail connections upward.

[30] Estimating the impact on the urban hierarchy is arguably less susceptible to measurement error in historical population data, idiosyncratic changes in urban administrative boundaries and influential outliers.

Table 5
Comparing the first wave to later rail connections, 1840–1900.

	Baseline (1)	Baseline (2)	Baseline (3)	Baseline (4)	Matched (5)	2SLS (6)
First wave (=1)	0.356***	0.250***	0.304***	0.451***	0.363***	0.432***
	(0.068)	(0.060)	(0.064)	(0.065)	(0.081)	(0.078)
Later rail connection (=1)	0.031	−0.100*	−0.020	0.051	−0.015	0.055
	(0.048)	(0.051)	(0.045)	(0.047)	(0.068)	(0.040)
Town FE	Yes	Yes	Yes	Yes	Yes	Yes
Period FE	Yes	Yes	Yes	Yes	Yes	Yes
Region × Period FE	No	No	No	Yes	No	No
Cragg–Donald wald *F*-statistic (first stage)	–	–	–	–	–	73.57
Observations	486	324	405	486	276	486
Later rail connections built	1870–1900	1870–1880	1870–1890	1870–1900	1870–1900	1870–1900

Notes: This table presents estimates of Eq. (1) where we include additional treatment indicators for rail connections that opened after 1870. The left-hand side variable is (ln) town population. Columns 1–4 report OLS estimates in our baseline sample. Column 5 reports OLS estimates in our sample that is balanced on pre-rail characteristics. Column 6 uses the three instruments described in the text (see Section 3.2) in the first stage to predict rail connections in the first wave. We report the first stage *F*-statistic at the bottom. Statistical significance based on standard errors clustered at the town-level is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.



(a) Baseline Sample (b) Matched Sample (c) Urban Rank

Fig. 3. The Long-Run Impact of the First Wave on Town Populations, 1800–2010. *Notes:* These figures plot the δ_t-coefficients from Eq. (2). Connected solid lines correspond to point estimates and dashed lines to a 95% confidence interval based on standard errors clustered at the town-level. Panel A reports estimates obtained from our baseline sample and panel B reports estimates from our sample balanced on pre-rail characteristics (see Section 3). Panel C reports estimates of relative differences in urban ranks where we switch sign on the ranking, so that larger cities have a higher rank (number). A solid vertical line denotes the year 1855 when railroad construction began, which also constitutes the base year against which all coefficients are measured. Table 6 provides the underlying regression results.

half of the nineteenth century and that these increases largely persisted over the twentieth century, despite further expansion of the network. How do we interpret such persistence? To make sense of these empirical results, we here discuss an informal framework building on Bleakley and Lin (2012) and provide empirical evidence to discern between two competing explanations.

As the advantage of an early rail connection was lost over the nineteenth century, one may expect that these towns became relatively less attractive for individuals and firms to locate in.[31] Yet, there is little to suggest that towns that gained access in the first wave have experienced relative declines over the twentieth century. Principally, there are two ways to interpret such persistent differences in the geographical distribution of economic activity. In a neoclassical model with locally decreasing returns to scale, where the size of towns is tied down by locational fundamentals, town populations should adjust back to their initial relative levels once the advantage of an early rail connection was lost.[32] Transitory shocks would therefore not affect the long-term distribution of population. In such a model, persistence is possible only to the extent that an adjustment back to a unique equilibria is confounded by historically sunk investments that depreciate slowly. For example, if towns with an early rail connection experienced higher investments in housing, an oversupply of housing may keep individuals in these towns until these investments have fully depreciated. In contrast, models with increasing returns to scale creates a tipping-point dynamic (David, 1985; Krugman, 1991b; Redding et al., 2011; Bleakley and Lin, 2012; Kline and Moretti, 2014). In such a case, changes in settlement patterns induced by the early railroads may helped to select a high-population equilibria for some towns. Even after the transitory advantage of an early railroad dissipates, these towns may remain more attractive locations, if their larger concentrations of firms and people result in productive advantages due to agglomeration economies.[33]

This imply two contrasting observational implications: (1) In a model with congestion costs and locational fundamentals as the main determinant of relative town sizes, persistent differences in populations should be reflected in higher stocks of durable capital, not yet fully depreciated; or (2) To interpret long-term differences in population as path dependence, we would expect to find that towns with an early railroad are statistically indistinguishable from other towns in terms of durable capital stocks.

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[31] Although railroads are not obsolete today, we think of the early rail connections as reflecting a temporary relative advantage that was lost as the railroad network continued to expand over the nineteenth century.

[32] To be clear, by locational fundamentals we here refer to a range of natural advantages such as rivers, mountains, natural harbors or the productivity of agricultural land. From Fig. 2 it is evident that such factors were important determinants of town locations since a majority of Swedish towns were located along the coast or in proximity to one of the major lakes in the interior. To compare towns with broadly similar locational fundamentals, we control for several such factors in our regressions.

[33] Duranton and Puga (2004) provides a discussion of the micro-foundations for such agglomeration economies, categorizing alternative mechanisms as coming from three broad groups: matching, sharing and learning. See Rosenthal and Strange (2004) for an overview of the empirical literature.

Table 6
The long-run impact of the first wave, 1800–2010.

Year	Baseline		Matched		Rank	
	(1) δ_t	(2) S.E.	(3) δ_t	(4) S.E.	(5) δ_t	(6) S.E.
1800	−0.050	(0.081)	0.012	(0.076)	−0.499	(2.157)
1810	−0.089	(0.063)	−0.023	(0.068)	−1.560	(1.813)
1820	−0.091*	(0.051)	−0.042	(0.053)	−1.310	(1.465)
1830	−0.055	(0.045)	−0.021	(0.048)	−0.125	(1.157)
1840	−0.055	(0.036)	−0.040	(0.040)	−0.062	(1.062)
1850	−0.010	(0.020)	−0.006	(0.024)	0.062	(0.775)
1860	0.069***	(0.020)	0.076***	(0.027)	1.810**	(0.865)
1870	0.245***	(0.040)	0.239***	(0.054)	4.555***	(1.515)
1880	0.359***	(0.063)	0.343***	(0.079)	5.741***	(2.161)
1890	0.464***	(0.088)	0.429***	(0.104)	4.930**	(2.275)
1900	0.527***	(0.104)	0.472***	(0.120)	5.055*	(2.725)
1910	0.509***	(0.124)	0.459***	(0.145)	5.242*	(2.981)
1920	0.552***	(0.142)	0.482***	(0.164)	6.116*	(3.465)
1930	0.524***	(0.150)	0.449**	(0.175)	5.616	(3.624)
1940	0.581***	(0.161)	0.496**	(0.191)	6.428*	(3.748)
1950	0.688***	(0.165)	0.604***	(0.190)	8.175**	(3.753)
1960	0.714***	(0.178)	0.619***	(0.201)	7.800*	(3.998)
1970	0.640***	(0.195)	0.570**	(0.225)	6.927	(4.289)
1980	0.517***	(0.193)	0.478**	(0.223)	5.804	(4.330)
1990	0.475**	(0.194)	0.443*	(0.225)	5.492	(4.299)
2000	0.479**	(0.200)	0.438*	(0.232)	4.805	(4.450)
2010	0.506**	(0.208)	0.459*	(0.241)	4.618	(4.406)
Town FE	Yes		Yes		Yes	
Decade FE	Yes		Yes		Yes	
Additional controls	Yes		Yes		Yes	
Observations	1,863		1,058		1,863	

Notes: This table presents the δ_t-coefficients from Eq. (2). For brevity, we do not report the decade and town fixed effects or the additional controls (dummies for towns located along the coast and the major lakes, as well as the longitude, latitude and their interaction, all interacted with decade fixed effects respectively). Statistical significance based on standard errors clustered at the town-level is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

In the following two subsections, we estimate differences in terms of a variety of historical and present-day factors. Overall, we find few such observable differences, consistent with persistent differences in town populations reflecting path dependence induced by the transitory advantage of an early rail connection.

5.1. Historical differences around 1900

Table 7 compares towns that gained access to a rail connection during the first wave to other towns in terms of a variety of outcomes observed around 1900. We report both a simple difference-in-means comparison (panel A) and comparisons conditioned on contemporary population, so that we effectively are comparing towns with an early rail connection to similarly large towns (panel B). Lastly, we examine the extent to which historical factors can explain changes in town populations between 1855 and 2010 (panel C). Throughout all regressions we condition on location along the coast or one of the major lakes in the interior, as well as each city's longitude, latitude and their interaction, to capture differences in broad geographic conditions.

Already by 1900, towns with early access to the railroad network had worse rail connectivity (column 2), consistent with the sharp reversal of relative rail connectivity over the latter half of the nineteenth century (see Fig. 1). However, compared to towns of similar size differences in rail connectivity are close to zero and statistically insignificant (panel B). Overall, these estimates are not consistent with an explanation of persistent relative differences in town populations arising from a historical oversupply of rail infrastructure.

Access to the network may have induced specialization in tradable sectors, which in turn may have provided the basis for sustained growth. Columns 3–6 use data from the 1900 population census to estimate relative differences in sectoral employment. Relative differences in manufacturing employment seemingly persisted over the nineteenth century.[34] Employment in transport-related sectors was lower, whereas employment in trade and service professions were similar. However, relative differences attenuate when we condition on contemporary town population in panel B. Column 7, compares patterns of sectoral specialization using a Herfindahl–Hirschman index, that is increasing in the degree of specialization.[35] There is little evidence, however, that the sectoral concentration of employment differed between towns with an early rail connection and other towns.

An early rail connection may have induced complementary investments in other forms of infrastructure and public utilities. Indeed, towns with early access experienced higher levels of investments in grammar schools, telephones and electricity works (columns 8–10). Yet, such differences largely evaporate when we condition on town populations in 1900.[36]

Could an industrial advantage or higher investments in public utilities in the early twentieth century explain relative differences in towns populations that we observe today? To answer such questions, panel C reports results from a series of regressions of town

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[34] In the Appendix we report estimated relative differences using data from the 1870 manufacturing census, showing that by the end of the first wave manufacturing employment in towns with an early rail connection was disproportionately higher and that establishments were substantially larger, more likely to be incorporated and used more steam engines relative to establishments in non-connected towns.

[35] We calculate the Herfindahl–Hirschman index (*HHI*) as $HHI_i = \sum e_{si}^2$ where e is the share of total employment in town i, across five sectors s (agriculture, industry, trade, transport, and services). If all employees work in one sector—that is, if a town is completely specialized—the index takes the value one.

[36] Despite the fact that we limit our analysis to a relatively small set of factors, other potentially observable factors are likely partially correlated with the factors that we condition on thus serving as a proxy for the broader provision of public infrastructure.

Table 7
Comparing towns in the first wave and other towns around 1900.

Historical factor	Baseline (1)	Railroads (2)	Manufacturing (3)	Trade (4)	Transport (5)	Services (6)	HHI (7)	Schools (8)	Telephones (9)	Electricity (10)
Panel A. Access in the first wave and historical outcomes (Outcome: Historical factors in top row)										
First wave (=1)	–	−0.234**	5.739**	−0.088	−2.820*	−0.475	0.012	0.314**	0.013**	0.203
	–	(0.108)	(2.288)	(0.418)	(1.500)	(1.367)	(0.015)	(0.129)	(0.006)	(0.185)
Panel B. Did towns in the first wave differ relative to other similarly large towns? (Outcome: Historical factors in top row)										
First wave (=1)	–	−0.002	2.660	−0.156	−0.757	−0.722	0.002	−0.075	−0.001	−0.131
	–	(0.084)	(2.823)	(0.441)	(1.616)	(2.437)	(0.020)	(0.113)	(0.008)	(0.221)
Panel C. Can historical factors explain relative differences in town size today? (Outcome: ln Town size in 2010)										
First wave (=1)	0.602***	0.604***	0.555**	0.610***	0.553**	0.595***	0.601***	0.589**	0.588**	0.593**
	(0.222)	(0.226)	(0.219)	(0.226)	(0.213)	(0.220)	(0.223)	(0.226)	(0.227)	(0.230)
Historical factor (top row)	–	0.027	0.011	0.073	−0.031**	−0.006	0.224	0.615***	3.310	0.236**
	–	(0.220)	(0.008)	(0.046)	(0.015)	(0.009)	(1.362)	(0.226)	(3.552)	(0.113)

Notes: This table compares towns in the first wave to other towns, in terms of a number of historical outcomes, around 1900. Each cell represents a separate OLS regression for the 81 towns included in the sample. Panel A reports coefficients from regressions of each historical outcome in the top row on a dummy taking the value 1 for cities in the first wave, and panel B presents similar estimates conditioned on contemporary town size. Panel C represents regressions of town size in 2010 on a dummy taking the value 1 for all towns in the first wave, conditioned on initial (1855) town size and each historical factor respectively. All regressions include controls for: location at the coast and the major lakes respectively and the longitude, latitude and their interaction. In column 2 the historical factor is the number of rail connection normalized by town size (scaled by a factor of 1000); in columns 3–6 we use the percentage of the labor force that is employed in industry, trade, transport and services respectively; column 7 reports results for a Hirschman–Herfindahl index of sectoral employment; in column 8 the presence of a grammar school is measure by a dummy taking the value 1 if a town housed a grammar school and 0 otherwise; column 9 reports results for the number of telephones per inhabitant in 1900; and column 10 presents results electricity production per inhabitant in 1900. Statistical significance based on Huber–White standard errors is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Table 8
Comparing towns in the first wave and other towns today.

	Sunk investments in Infrastructure and Housing, 2005/2010						Local labor markets, 2005/2010				
	Railroads (1)	Highways (2)	Trunk roads (3)	Old housing (4)	House prices (5)	Density (6)	Aged >65 (7)	Ind. diversity (8)	No. of ind. (9)	Commuters (10)	Est. size (11)
Panel A. Do towns of the first wave differ from other towns today?											
First wave (=1)	−0.027	−0.026	−0.109***	−1.973**	0.167*	0.271***	−0.031***	0.022	0.477***	−6.244**	0.240***
	(0.022)	(0.029)	(0.033)	(0.972)	(0.095)	(0.066)	(0.009)	(0.016)	(0.098)	(2.947)	(0.069)
Panel B. Do towns in the first wave differ from other, similarly large towns today?											
First Wave (=1)	−0.011	0.004	−0.036	1.570*	0.005	0.070	0.001	−0.057*	−0.006	3.158	−0.028
	(0.021)	(0.024)	(0.033)	(0.801)	(0.088)	(0.059)	(0.005)	(0.032)	(0.016)	(2.544)	(0.056)

Notes: This table compares towns in the first wave to other towns, in terms of a number of measures of current (2005/2010) outcomes. Each cell represents a separate OLS regression. Panel A presents results of regressions of each outcome in the top row on a dummy taking the value 1 for towns in the first wave and 0 for all other cities. Panel B reports results for similar regressions, where we also condition on contemporary (2005/2010) town size. All regressions include controls for: location at the coast and the major lakes respectively and the longitude, latitude and their interaction. Column 1 reports results for the number of rail connections per inhabitant in 2010; column 2 and 3 the number of highways and trunk roads that emanate from each town in 2010, normalized by contemporary town populations; column 4 presents results for the percentage share of the housing stock in 2010 that was constructed pre-1921; column 5 uses the mean housing price in 2010; column 6 the (ln) population density; column 7 the share of the population that is aged above 65; column 8 and 9 presents an entropy index of industrial diversity and the (ln) number of 5-digit industries that is present in each town; column 10 uses the percentage share of the population that works outside the local labor market; and column 11 the (ln) average establishment size. Statistical significance based on Huber–White standard errors is denoted by: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

populations, measured in 2010, on our first wave dummy, conditioning on each historical factor and pre-rail (1855) town populations. For comparison, column 1 reports that towns that gained access to an early rail connection on average are 82% (0.60 log points) larger than other towns today. Conditioning on each of the historical factors in the top row have little effect on the magnitude and precision of the estimated long-run effect of a rail connection in the first wave. For instance, conditioning on the number of rail connections in 1900 leaves the estimated long-run difference in population virtually unchanged and historical railroads themselves seem to be a poor predictor of town size today (column 2). At most, the long-run difference in population attributed to early access decreases by around 8%, when we condition on the share of the local labor force employed in manufacturing around 1900 (column 3).

In sum, although we cannot rule out that some unobserved historical factor can account for long-run relative differences in population, there is little evidence that any of the factors examined here can explain why towns with an early rail connection maintained their relatively larger populations over the twentieth century.

5.2. Evidence on path dependence from today

Table 8 compares a variety of present-day factors for towns that gained access to a rail connection in the first wave relative to other towns. As in the previous section we report both simple mean differences (panel A) and estimates where we condition on contemporary town populations (panel B). All regressions include controls for location along the coast or one of the major lakes in the interior, as well as each city's longitude, latitude and their interaction.

There is little to suggest that relative differences in town populations persist due to an oversupply of transport infrastructure: there is no difference in the number of rail connections for towns with early access to the railroad network relative to other towns (column 1), which is consistent with the evolution of relative rail connectivity over the last 150 years (see Fig. 1), and small differences in rail connectivity around 1900 (see Table 7). Columns 2 and 3 compare access to highways and trunk roads. Towns with an early rail connection have, if anything, worse access to road infrastructure, although differences are small and imprecisely estimated.

Housing is a durable investment that depreciates slowly, which may slow down population adjustments. Using data on the composition of the housing stock today, column 4 shows that the share of the housing stock that was constructed prior to 1921 was on average 2 percentage points lower in towns with early access to the network relative to other towns, although they have a slightly higher share of old housing units relative to similarly large towns (panel B, column 2). If housing was historically oversupplied in towns with an early rail connection, we would expect housing to be underpriced in those towns today. Yet, there is little evidence that such price differentials exists, when we control for the fact that housing prices are higher in larger towns (column 5).[37] If early railroads resulted in contemporary differences in density, due to differences in historical building patterns, this could be one reason for observed relative differences in town sizes. Indeed, towns of the first wave are on average almost 30 percent denser today (column 6). Yet, compared to towns of similar size, they are indistinguishable in density (panel B).

An adverse age structure is another intriguing channel of persistence: as towns with early access to the network grew in the late-nineteenth century, these migration inflows could have resulted in differences in age compositions today. Column 7 uses data on the share of the population aged above 65, showing that the population of towns with early access is on average slightly younger today. Compared to towns that are of similar size today, however, this difference is precisely estimated to zero.

Historical specialization patterns may be sunk in the form of interlinked networks of suppliers and subcontractors. Although we do not directly observe such networks, we get at such explanations by indirect comparisons of sectoral specialization using two indices. First, measuring sectoral diversity using an entropy index across 16 industry groups, shows no evidence of relative differences in diversity (column 8).[38] Secondly, column 9 shows that in towns with early access the number of 5-digit industries were on average 61% higher. However, the sign changes and the magnitude is close to zero when we compare towns of similar size today, meaning that differences are primarily driven by the fact that towns with early access to the railroad network are relatively larger than the average town today. Column 10 shows that inhabitants of towns with an early rail connection are more likely to work in the local labor market, as opposed to commuting and column 11 shows that establishments are on average larger. These differences are, however, also entirely accounted for by differences in town size (panel B).

Taken together, we find little meaningful differences between towns that grew larger due to their early access to the railroad network and towns that are similarly large today, which suggest that the transitory advantage of an early rail connection gave rise to path dependence in the location of economic activity.

6. Conclusions

This paper analyzed the rollout of the Swedish railroad network, which provides historical circumstances that allows us to examine the impact of rail connectivity on short- and long-term patterns of urban growth. We document large short-term relative increases in population for towns that gained access to the network in its first wave of expansion. Town growth, however, mainly reflected a relocation of economic activity from non-connected nearby towns. Such findings are consistent both with views that ascribe transportation infrastructure a central role in urban growth and arguments that the impact of transport infrastructure is smaller in the aggregate due to displacement effects.

In the late nineteenth century, differences in connectivity was sharply reversed as the network continued to expand, which allows us to study long-run adjustments in population to this transitory shock. Over the twentieth century, relative differences in town populations remained largely stable, suggesting little adjustment in the long run. Evidence of path dependence lends qualitative support to the idea that temporary policy interventions can permanently alter the geographical distribution of economic activity.

Appendix A. Supplementary material

Supplementary data associated with this article can be found, in the online version, at http://dx.doi.org/10.1016/j.jue.2015.09.001.

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[37] In addition, because the average pre-1921 share of the housing stock for the towns in our sample is a meager 7%, historical housing investments are unlikely to explain locational decisions in the aggregate today.

[38] The 16-industry entropy index is calculated relative to a national average, with higher values corresponding to a more diverse industrial structure and a value of 0 corresponding to complete specialization in one industry. Industry classifications follows the Swedish Standard Industrial Classification (SNI) system, which in turn is based on the standard European NACE classification scheme.

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