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Locomotives of Local Growth: The Short- and Long-Term Impact of Railroads in Sweden*

Thor Berger† Kerstin Enflo

Abstract

This paper studies the impact of railroads on town-level growth in Sweden over 150 years. Our analysis builds on the fact that railroads historically were extended quasi-randomly across towns. Towns that gained access to a rail connection grew larger relative to other towns, with large negative spillovers on unconnected nearby towns. Over the 20th century, we find little adjustment to the initial shock in town populations, despite a sharp reversal in relative connectivity. Evidence on historical investments and present-day factors is consistent with this temporary shock giving rise to path dependence in the location of economic activity.

JEL: N73, N93, R12, R40

Keywords: Transport Infrastructure, Urban Growth, Path Dependence

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1 Introduction

Economists and policy makers have long been concerned that high trade costs may constitute a barrier to development (World Bank 1994, 2009). Improving transport infrastructure is a crucial component to lowering trade costs and income differentials between countries, regions and cities. However, credibly identifying the effects of such improvements remains empirically challenging. In particular, because transport infrastructure is not randomly assigned across locations, naïvely comparing locations that receive such investments to those that do not is unlikely to be informative of a causal effect.[1] In the presence of labor market rigidities or durable investments, long-run adjustments to such improvements 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 19th-century railroads—that provides a historical experiment to study the impact of transport infrastructure on urban development in a poor, rural and predominately agricultural setting. We find that towns that gained access to an early rail connection grew substantially larger relative to other towns, with large negative spillovers on nearby unconnected towns. More notably, we document that the transitory advantage of an early rail connection resulted in lasting relative differences in town populations, persisting for well over 150 years.

Our main empirical analysis builds on a "first wave" of railroad expansion, between 1855 and 1870, in which state-financed lines evolved into the backbone of the modern Swedish railroad network. Partly due to military concerns, state planners avoided the coastline and lines were routed through disadvantaged rural regions. As a result, rail connections were in practice extended in a quasi-random fashion across towns. A surge in private railroad construction after 1870 led to a sharp reversal of this early advantage: 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 (see Figure 1).

The quasi-random rollout provides us with an opportunity to identify the short-term impact of rail connectivity on town-level population growth and the reversal in rail connectivity allows us to identify long-run adjustments to the transitory advantage of an early rail connection.

We estimate the relative impact of rail connectivity using a difference-in-differences approach, which shows that towns that gained access to a rail connection grew substantially larger (27%) between 1855 and 1870 relative to other towns. We adress the potential non-random placement

[1] Such comparisons are plagued by the fact that the expected direction of bias may go either way. For instance, political interference in the allocation process that results in infrastructure being supplied to areas with particularly bleak economic prospects would imply a downward bias. On the other hand, if such investments instead target areas that are growing due to other reasons, we would expect an upward bias. Similarly challenging identification problems exist in the evaluation of other forms of infrastructure, such as (hydroelectric) dams and electrification (Duflo and Pande 2007; Dinkelman 2011; Lipscomb, Mobarak and Barham 2013).

of such connections in three ways. First, we compare towns that were observationally similar prior to railroad construction began. Second, we derive three instruments from two major network proposals and low-cost routes between the terminal points of the network. Using such alternative sources of identification yields similar results. Third, we indirectly test the excludability of our instruments by exploiting lines that were proposed, but not built by 1870, and lines only built thereafter. Estimated effects for these connections are close to zero and statistically insignificant, which reduces concerns that our estimates reflect unobserved factors that covary with the early railroads. Taken together, this suggest a sizable short-run impact of transport infrastructure on urban growth.

Economists are generally concerned that gains from geographically targeted policies are partially or completely offset elsewhere through a displacement of economic activity.[2] Indeed, we demonstrate that growth in towns with an early rail connection seems to have come at the cost of nearby unconnected towns. Relative increases in population are largest compared to unconnected towns in proximity to the network, whereas compared to towns more than 90 km from the network, relative increases in population are essentially zero. Such results hint at the distributional consequences of large infrastructure investments, and supports conventional arguments that railroads historically had small effects on aggregate economic activity.[3]

Towns that gained access to the network during the "first wave" continued to grow differentially faster throughout the 19th century, even as rail connectivity converged across towns. Relative differences in population stabilized around 1900 and largely persisted over the 20th century. Today, towns that gained access to the railroad network during the "first wave" are substantially larger than initially similar towns that did not.

Are such persistent differences in town populations evidence of path dependence in the location of economic activity? If there is a multiplicity of steady states, even a small shock—such as the advantage of an early rail connection—could be capable of nudging initially very similar towns into very different long-term growth trajectories.[4] However, such apparent persistence could also reflect slowly depreciating historically sunk investments, in which case we would expect to observe a gradual convergence in town sizes. We provide a variety of historical and contemporary evidence to empirically discern between these two explanations.

Historical evidence from the late 19th century is consistent with railroads increasing the pace of industrialization, promoting scale economies in manufacturing and a relative shift away from

[2]See Gottlieb and Glaeser (2008) and Kline and Moretti (2013*b*) for an overview.

[3]Fogel (1964) is the canonical reference. Economic historians typically have evaluated the contribution of the railroad to the wider economy by estimating the cost difference—the *social savings*—in transporting a fixed amount of goods in a world with the railroad, compared to a counterfactual world where transport is constrained to conform to non-rail transport modes. Such estimates typically are small compared to pre-existing expectations. See Fogel (1979) for a discussion of the early literature and Leunig (2010) for an overview of this literature to date.

[4]Krugman (1991*b*) is the canonical model that features several stable equilbria. Also see David (1985), Arthur (1994) Fujita, Krugman and Venables (1999) and Bleakley and Lin (2012). A related literature discusses the feasability of "big push" policies (Rosenstein-Rodan 1943; Murphy, Shleifer and Vishny 1989; Kline and Moretti 2013*a*).

artisanal production. However, neither an industrial advantage in the early 20th century nor a range of historically sunk investments such as schools, electricity works, communications infrastructure or subsequent railroads explain more than a small fraction of the variation in town size today that we attribute to the population shock induced by the early railroads.

Comparing towns with an early rail connection with similarly large towns today in terms of transport infrastructure, age structure and housing prices we find little evidence that suggest that towns that gained access in the "first wave" persist due to slowly depreciating factors. Our results rather appear to be most consistent with path dependence arising from the population shock induced by the transitory advantage of a rail connection during the "first wave."

Our paper speaks to two broad strands in the literature. First, our results complement a growing body of evidence that examines the causal impact of the railroad on historical development (Atack et al. 2010; Atack, Haines and Margo 2008; Keller and Shiue 2008; Donaldson 2013; Donaldson and Hornbeck 2012), as well as recent efforts to disentangle the impact of modern 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). Most directly related to the spirit of our work is Jedwab and Moradi (2011), Banerjee, Duflo and Qian (2012) and Kline and Moretti (2013*a*), which examine the long-run impact of infrastructure investments. Similar to much of this literature, our results suggest that improvements of transport infrastructure can have substantial short-run effects on urban development, but that such effects to a large extent are likely reflecting the displacement of economic activity from nearby areas. More importantly, our results show that such short-run effects can affect local development paths over a century of substantial economic modernization.

Second, 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. Our paper has much in common with Bleakley and Lin (2012) which documents the persistence of U.S. cities at portage sites, despite this initial advantage being made economically irrelevant more than a century ago. However, one important difference is that our focus is on a man-made advantage (railroads) with more obvious policy implications. In that sense, our paper is related to Redding, Sturm and Wolf (2011) which examines a shift between multiple equilibria in the location of Germany's main air hub following post-war division. Such evidence stands in stark constrast to claims that economic activity is uniquely tied down by fundamentals, such as Davis and Weinstein (2002, 2008) who finds little persistence in city populations exposed to the large-scale Allied bombing campaigns of Japanese cities during World War II.[5] One

[5]See Brakman, Garretsen and Schramm (2004), Bosker et al. (2007) and Miguel and Roland (2011) for a similar approach. Historically sunk investments in infrastructure networks, around which reconstruction efforts could be coordinated, is one potential explanation for the fact that even such extreme shocks do not seem to shift urban economies between steady states, as pointed out by Redding, Sturm and Wolf (2011). Similarly, such persistence could be the result of coordination around the *expectation* that rebuilding will commence after destruction (Krugman 1991*a*).

explanation for such differences emphasises initial conditions: Swedish towns were small in the 19th century and the population was predominately rural, ensuring that a shock of comparably small magnitude was able to permanently shift economic activity. Evidence of path dependence suggests a world in which small and temporary policy interventions are capable of permanently affecting long-term local development trajectories and more generally that geographical patterns of economic activity today are partly determined by small "historical accidents."

The remainder of this paper is structured as follows. In the following section we provide a historical background of railroad construction and describe our data. Section three details our empirical strategies, with results provided in section four. In section five we compare historical and contemporary differences. Section six provides some concluding remarks.

2 Historical Background and Data

This section provides a brief description of the historical context of 19th-century Sweden. We then describe the two major network proposals and document the initial divergence and subsequently sharp reversal in rail connectivity for towns in the "first wave" relative to other towns over the last 150 years. Lastly, we discuss our data and compare a range of pre-rail characteristics for towns that gained access to the early railroads and towns that did not.

2.1 Swedish Economic Development in the 19th Century

Sweden underwent a dramatic economic, political and social transition over the latter half of the 19th century, as railroads permeated the country (Gårdlund 1942; Montgomery 1947; Heckscher 1954). Between 1856, when the first steam railroad line went into commercial operation, and the outbreak of World War I, per capita incomes grew 65 percent faster than in the U.K. and 20 percent faster than in the U.S. (Bolt and van Zanden 2013). Rapid convergence was also manifest in terms of real wages, increasing from about half those paid to British workers to parity over the same period (Williamson 1995).

Economic growth was also reflected in a massive inflow of people from rural areas to towns and cities. Formal barriers to rural-urban migration were dismantled through the abolishment of internal passport requirements in 1860, which contrasts historical Sweden to modern developing countries where labor mobility is more restricted (e.g., Banerjee, Duflo and Qian 2012).[6] From the mid-19th century until World War I, the number of urban dwellers increased from less than 400,000 to 1.5 million, corresponding to an increase in the urbanization rate from about 12 percent to more than 30 percent (Statistiska Centralbyrån 1969).

[6]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.

2.1.1 Transport Before and After the Railroad

Railroads gradually complemented and partly displaced an inefficient 19th-century transport network, 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 (Gårdlund 1942; Heckscher 1954). Transport costs were high and distinctly seasonal, since canals, waterways and harbors froze in the wintertime. In addition, goods were typically transported using several modes and therefore required frequent transshipment. Overland transport in excess of 200 km was not viable (Heckscher 1907), and important high weight-to-value goods, such as iron ore, could not profitably be hauled more than 30 km (Sjöberg 1956).

Railroads radically altered the means of transport, offering year-round operation at higher speed, lower cost, at unitary tariff rates (Montgomery 1947). Freight rates were cut by three-fourths, passenger costs decreased by half and travel speeds increased tenfold (Sjöberg 1956). Already by the end of the 1860s, the railroad had overtaken water transport as the primary means for internal transportation (Westlund 1992). While substandard transport had constrained industrialization and town growth prior to the railroad era, the emerging network allowed cheap transportation of basic necessities to urban dwellers and raw materials to manufacturers (Thorburn 2000), effectively reducing the barriers to urban expansion.[7]

2.2 Constructing Railroads: State Planners or Market Forces?

A key question is whether transport infrastructure is most efficiently supplied by the market or local and national governments. Whether railroads in Sweden 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* as early as the 1820s, it would take the better part of another three decades of polarized debates between the peasant and noble estates before the first lines went into commercial operation.[8] Over this period, two proposals for a national railroad network emerged: (i) a market-based proposal; and (ii) a *de facto* state monopoly. We use these proposals as the basis for our empirical strategies, and devote the following two subsections to describing their background.

2.2.1 Adolf von Rosen's 1845 Proposal

In 1845, Count Adolf von Rosen—a major in the Naval Mechanical Corps—announced the first comprehensive proposal for a national railroad network. It was based on privately funded

[7]According to the official railroad statistics (*Bidrag till Sveriges officiella statistik. L, Statens järnvägstrafik*), grain and fuel—coal, wood, and charcoal—constituted more than one-fifth of the tonnage transported via rail in 1870.

[8]Prior to its abolishment in 1866, the *Riksdag of the Estates* (henceforth, the *Riksdag*) was a national diet where the four estates (the nobility, clergy, burghers and peasants) were represented. Rydfors (1906) provides an overview of the early debates in the *Riksdag* concerning railroad construction.

lines, that were to be managed by private companies. Despite lines being privately funded, the proposal encompassed an extensive national network, as shown in Figure 2, meant to address the interference of local political lobbying that had plagued piecemeal railroad construction elsewhere in Europe (Sjöberg 1956).

Several proposed routes were surveyed by von Rosen in cooperation with British engineers, and the *Riksdag* ordered topographical surveys of additional routes in the proposal (Sjöberg 1956). These surveys collected detailed geographical information, and therefore lowered the cost of future railroad construction along these routes (Rydfors 1906). Figure 2 provides suggestive evidence of this, showing that several of the lines constructed by 1870 followed the initial routes proposed by von Rosen. In section 3 we discuss how this "path dependence in planning" motivate the von Rosen's proposal as the basis for an instrumental variable and placebo strategy.

In the end, von Rosen's market-based approach to railroad construction resulted in a spectacular failure (Heckscher 1954). Despite state concessions and interest guarantees amounting to 4 percent of construction costs, von Rosen repeatedly failed to raise sufficient capital.[9] Such failures led to mouting scepticism among politicians and the public against leaving construction of the railroad network in the hands of foreign investors and private enterprise (Rydfors 1906). A speech by the Minister for Finance—Johan August Gripenstedt—addressing the *Riksdag*, captures the changing mood of public opinion in the 1850s:

> "[I]f 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 railroads."[10]

By then it stood clear that, due to an underdeveloped domestic capital market and a lack of domestic demand for transport services, any attempt to bring about railroads by private enterprise were doomed to fail (Nicander 1980). Railroads, if they were to be constructed, would have to be constructed "ahead of demand"—by the state.

2.2.2 Nils Ericson's 1856 Proposal

In the *Riksdag* of 1853/54 it was decided that all 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 by the *Riksdag* to lead construction and was given "dictatorial powers" to route the main trunk lines as he saw fit (Rydfors 1906).[11]

Ericson's proposal, announced publicly in 1856, centered around five main trunk lines. These trunk lines were to be constructed by the state, on which privately funded branch lines would

[9] British investors had initially commited to provide means to finance the main lines. However, following the the speculation and inevitable collapse during the British "Railway Mania" of the 1840s, such attempts were thwarted.

[10] Cited in Kaijser (1999, p.223).

[11] Heckscher (1954, p.241) describes Ericson in colorful terms as a "born leader (...) who worked with immense force and unwavering purpose to realize his plan [of a railroad network]."

then expand.[12] They were to connect the capital Stockholm with the other major ports (Sjöberg 1956), but 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).[13] In comparison to other contemporary European countries, where the early railroad lines were to serve already growing industrial regions, such considerations were unique (Pollard 1981). Additionally, due to military concerns, the trunk lines were to be routed through the interior, avoiding towns located close to the coastline and existing transport routes (Heckscher 1907, p.15).

As a result, many historically important towns were left without a connection, and Ericson's plan was fiercly criticized and ridiculed by contemporaries for its "horror of waterways and towns" (Heckscher 1954, p.241).[14] Because placement of the main trunk lines affected construction costs for all other lines, they therefore 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. Protracted and polarized debates in the *Riksdag* concerning the routing of each remaining line took place throughout the 1860s, as local politicians seized on the capital to ensure that lines were routed through their districts (Westlund 1998). Ensuing political infighting meant that only part of Ericson's plan had been realized by 1870. Importantly, even though Ericson's plan was rejected by the *Riksdag*, Figure 2 documents that "he was able to enforce the realization of his plans with hardly any change" (Heckscher 1954, p.241).

In section 3 we describe how the parts of the proposal that remained unbuilt by 1870 provides a set of lines to use as the basis for a placebo strategy, and Ericson's original proposal as an instrumental variable for the network actually constructed by 1870.

2.3 150 Years of Railroads: From the "First Wave" Until Today

Between 1855 and 1870, the "first wave" of railroad expansion commenced based on Ericson's 1856 proposal. Initially, the *Riksdag* approved construction of the Southern and Western trunk

[12]Private initiatives had to (i) undertake a survey of the proposed route by an experienced railroad technician; (ii) obtain a state concession; and (iii) 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. Construction, traffic, and maintenance were, however, 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, p.15).

[13]Much of the historiographical work on Swedish railroads emphasises their function as a tool for regional policy. For example, Westlund (1998, p.74) argues that railroads "were that epoch's great instrument for regional policy for spreading industrialization and economic development to new regions."

[14]Figure 2 lends support to his contemporary critics, by documenting how Ericson's proposed railroad lines avoided the historically important mining region Bergslagen, northwest of Stockholm—a center for mining and ore smelting for over half a millennia—as well as important 17th-century naval towns, such as Karlskrona, in the southeast.

line, and in November 1862 the Western trunk line—running from Stockholm to 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 "first wave" of railroad expansion had reached its end, leaving a network spanning 1,727 km, two-thirds of which directly owned by the state. This network had connected less than a third of all towns; of the twenty largest towns as of 1855, less than half had a rail connection. Private railroads began to be established with a frenzy in the 1870s (Nicander 1980), and despite initial resistance among private finaciers, 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). Towns left without a connection in the "first wave" were now rapidly becoming connected to the expanding network.

Figure 1 contrasts the average number of rail connections, normalized by population, for towns that gained access in the "first wave" and those that did not. Two features of Figure 1 corroborate our interpretation of the early rollout as generating quasi-random variation in rail connectivity. First, between 1855 and 1870, there was a large divergence in the number of rail connections, corresponding to the "treatment" in our empirical analysis.[15] Second, after 1870, we observe a rapid convergence in rail connections. Already by 1890, towns that did not gain access to a rail connection in the "first wave" had more rail connections on average than those towns that did. Throughout the 20th century, towns that became connected after the "first wave" consistently had more rail connections per inhabitant, suggesting that rail connectivity itself is an unlikely explanation for long-run differences in population growth. Importantly, this suggests that the "first wave" of railroads—primarily constructed by the state—did not reflect underlying differences in local demand for transport services.

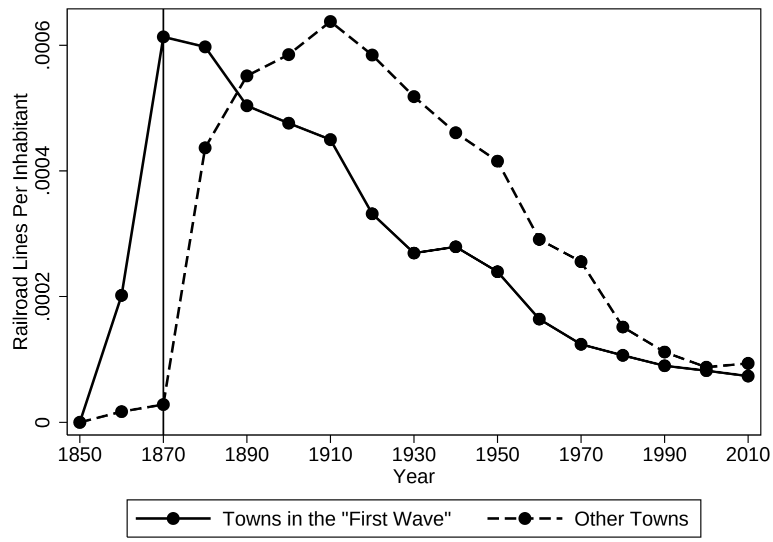
2.4 Data on Towns and Railroads

Our sample consists of all Swedish towns that held town charters in 1840, prior to when railroad construction began.[16] We exclude towns that formed after 1840 because the location of such towns is endogenous to the placement of the railroad network. 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.[17] These restrictions reduce our baseline sample used throughout the rest of the paper to 81 towns.

[15]For clarity, we emphasize here that Figure 1 shows the average number of rail connections irrespective of if these connections linked a town to the railroad network or not, whereas in our empirical analysis, treatment status is assigned based on having a connection to the network.

[16]Because we exclude all towns that gained town charters after the railroads were constructed we will, however, 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 (e.g., Heckscher 1907).

[17]Below reported results are typically stronger in an unrestricted sample of towns.



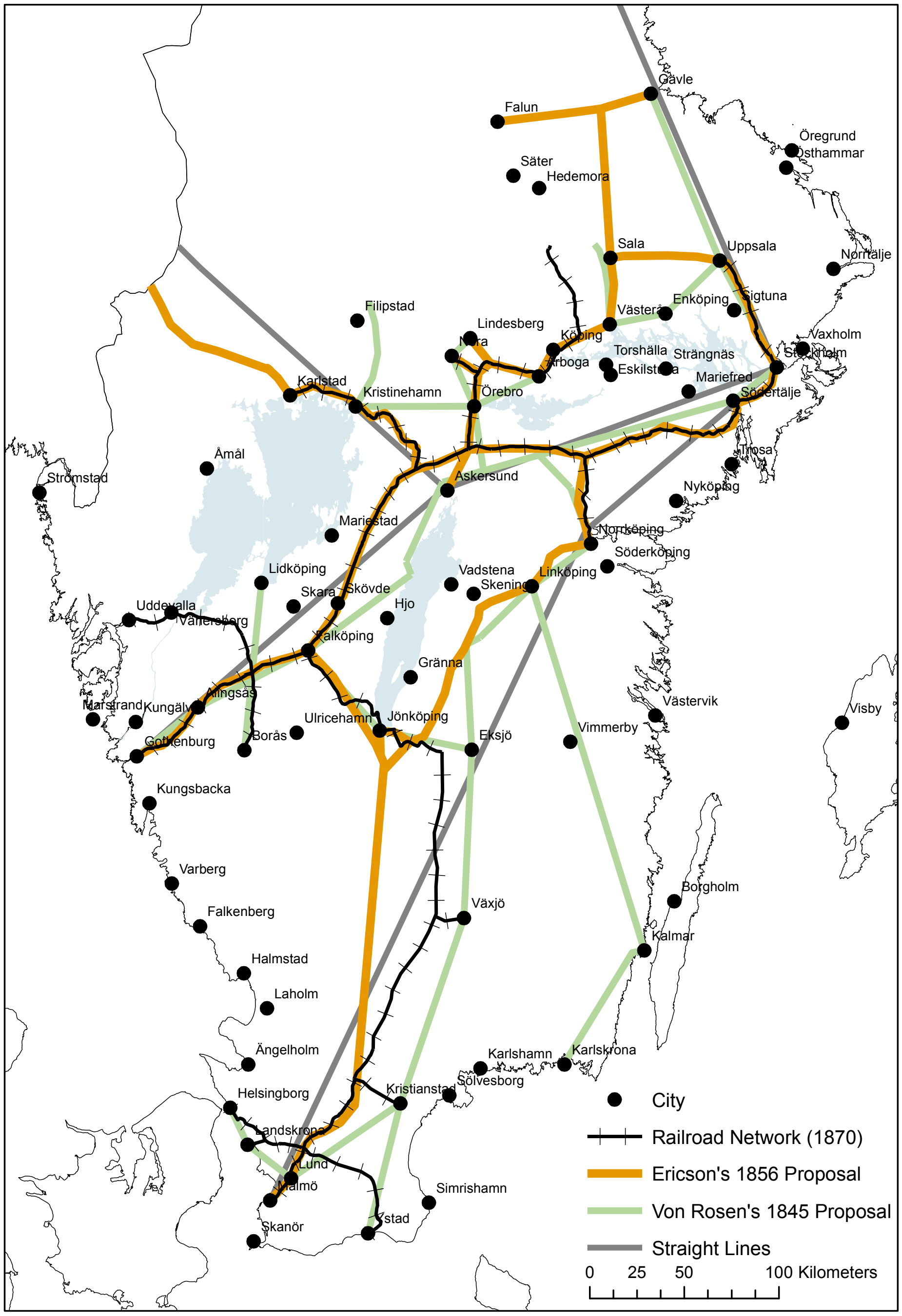
Notes: This figure shows the divergence of rail connections for towns that gained access to the railroad network in the "first wave" and towns that did not between 1855 and 1870, and the subsequently rapid convergence over the 19th century. Solid lines denote the average number of rail connections per inhabitant for each group of towns respectively. Data is drawn from historical map collections of the Swedish railroad network and population censuses, described in more detail in Appendix A.

Figure 1: Rail Connections for Towns in the "First Wave" and Other Towns, 1850-2010.

For each town we collect data on population for each decade between 1800 and 2010, from historical population censuses from Statistics Sweden and Nilsson (1992). Unfortunately, data on consistently defined metropolitan areas does not exist for the more than two centuries that we examine this paper. Our data therefore pertain to towns as defined by evolving administrative boundaries. This could be problematic if administrative boundaries changed differentially for towns that gained access to the railroad network during the "first wave" relative to other towns.[18] However, historically most incorporations were small. An average town had 4,400 inhabitants in 1855, increasing to 6,200 in 1870, and eventually to 53,800 in 2010. We collect additional town-level outcomes from a variety of historical and contemporary sources, described in more detail in Appendix A and when introduced in the paper.

Using GIS software and historical maps of the railroad network, we reconstruct the 1870 railroad network. In addition, we also recreate von Rosen's 1845 and Ericson's 1856 network proposals from maps in Sjöberg (1956). Our baseline measure of access to the network is a dummy taking the value 1 for a town that had direct access to the network through a rail connection by 1870 and 0 for other towns. An alternative measure that we use is the distance from each town to the 1870 network. Additionally, for each 10-year period we also code similar dummies for towns that gained access to the railroad network through segments of the network that opened between 1870 and 1900. To control for alternative modes of transport we code a dummy for all towns located at the coast and towns located by one of the four major lakes

[18]Using data on the (land) area of each town today from Statistics Sweden, we find no evidence that towns that gained access in the "first wave" are larger (in terms of land area) relative to similarly large cities.



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 an instrument as described in section 3.1 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 Appendix A for a description of the underlying data.

Figure 2: The "First Wave" of Railroad Expansion, 1870.

respectively, which indirectly also captures the differential access to the major canals constructed in the early 19th century. Figure 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 early rollout of the railroads did not aim to connect towns with better preconditions for growth, as discussed in section 2, which would imply that towns that gained access in the "first wave" and towns that did not should look similar prior to the initiation of railroad construction. Here we confront such qualitative historical accounts with statistical evidence.

Table 1, Panel A, reports average pre-rail (1855) 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). Towns that gained access to a rail connection were slightly larger (on average 1,500 additional inhabitants in 1855), but they were not growing significantly faster between 1840 and 1855, the period directly preceeding railroad construction. Towns with and without rail access had similar access to domestic urban markets and a similar sectoral structure, measured by employment shares in artisanal, trade, military, manufacturing and service occupations.[19] However, because the early rail lines were routed through the interior, towns with an early rail connection were less likely to be located at the coast and consequently had a smaller share of the population employed in the shipping sector.

These regressions are largely consistent with historical accounts stressing that the early railroads did not single out towns with much different propensities to grow. However, one concern is that even slight observable differences may hint at some form of selection, affecting the estimated impact of a rail connection. We address such concerns by using a range of identification strategies to deal with the potential of selection on observable or unobservable factors.

3 Empirical Strategies

Our main empirical strategy is a difference-in-differences specification, where we compare outcomes for towns that gained access to the railroad network during the "first wave" to a control group of towns that did not. Equation (1) details our baseline specification, in which the outcome always is the population (P_{ijt}) of each town *i*, located in region *j*, observed in year *t*:

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

[19] 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 $MA_{it} = \sum_{j \neq i} P_{jt} D_{ij}^{-1}$, where P is the population of town *j* in year *t*, 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.

	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 & 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 (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)
Sea (=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 a first-order polynomial in the longitude and latitude of each town. See Appendix A 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.

Table 1: Pre-Rail Differences Between Towns With and Without Access in the "First Wave".

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" 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 town-specific factors that potentially are correlated with gaining access to the network and period (λ_t) fixed effects capture the many factors causing urbanization in this period. Extended specifications also include region-by-period fixed effects (θ_{jt}) that take into account time-varying shocks across regions and periods as well as town- and region-specific spatial trends in the vector $\mathbf{Z_{it}}$.[20]

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, our previous comparisons of pre-rail outcomes and trends do not suggest that such an assumption is violated (see Table 1) and below reported empirical estimates provide little evidence of pre-existing differential trends. However, one way to get at concerns that observable pre-rail differences may influence our estimates is compare towns with an early rail connection to a control group of observationally similar towns in terms of pre-rail observables. Table 1, Panel B, reports mean characteristics for a subsample balanced on propensity scores.[21] Statistical differences between towns with and without access to the network attenuate (see column 6), suggesting that this restricted control group constitutes a relevant counterfactual.

Our baseline regressions estimate equation (1) using data for the years 1840, 1855 and 1870. Estimating the short-run impact over a rather long period (15 years) allows firms and migrants adequate time to respond to changes brought about by the railroad, even in the presence of large adjustment costs. It also reduces concerns of railroad construction resulting in a temporary local economic boom—for instance, due to employment of local navvies—affecting our estimates.

Our second set of results are obtained using data on town populations from 1800 until 2010, which allows us to estimate relative differences in population before and after railroad construction was initiated. We modify equation (1) by allowing the effect of a rail connection in the "first wave" to vary by decade:

$$\ln\left(P_{it}\right) = \alpha_i + \lambda_t + \delta_t FW_i + \varepsilon_{it} \tag{2}$$

[20] Region-by-period fixed effects are constructed based on an indicator for each of the eight National Areas (*Riksområden*), aggregated from counties as defined by historical administrative boundaries, interacted with our period dummies (λ_t). Controlling for such regional shocks is partly motivated by a natural resource bonanza in northern Sweden (due to booming timber exports) that attracted large inflows of migrants in the period under study in our paper.

[21] Propensity scores are estimated by probit, where we regress a dummy that equals one for all towns with a rail connection by 1870 on all variables listed in Table 1 and a first-order polynomial in longitude and latitude of the centroid of each town. Following Crump et al. (2009) we trim our sample by excluding all towns with propensity scores outside the interval [0.10, 0.90]. This results in a sample consisting of 46 out of the 81 towns in our baseline sample.

This coefficient (δ_t) returns the average difference in (ln) population between towns with and without access to the network by the end of the "first wave" of railroad expansion, relative to the base year 1855 that we omit. A virtue of this approach is that because virtually all towns gained access to the railroad network in the post-1870 period, estimates of $\triangle\delta_{>1870}$ plausibly would reflect the degree of persistence accruing from an early railroad connection, rather than the direct effect of the railroad itself.[22]

Standard errors are clustered at the town-level in all specifications, allowing for arbitrary patterns of heteroscedasticity and serial dependence (Bertrand, Duflo and Mullainathan 2004).[23] Correcting for spatial dependence using the Conley (1999) estimator, assuming a linear decay and cutoff at 100 km, typically inflates standard errors by less than 20 percent, which in practice does not affect our statistical inference.

OLS estimates of the treatment effect of a rail connection (δ) in equation (1) may be biased if the early rollout out the railroad network was correlated with unobserved time-varying factors at the town-level so that $cov\left(Rail_{ijt}, \varepsilon_{ijt}|\alpha_i, \theta_{jt}, \lambda_t, \mathbf{Z}'_{\mathbf{it}}\right) \neq 0$. This motivates the use of alternative identification strategies discussed below.

3.1 Instrumental Variables Strategy

Despite historical accounts suggesting that the "first wave" of railroads was not extended to connect towns with particularly bright growth prospects, one empirical concern is that non-random placement of rail connections confounds our estimated impact of the railroad. To alleviate such concerns, we complement our basic difference-in-differences estimates with an instrumental variables (IV) strategy. Our instruments draw upon the two existing proposals of the network and approximate low-cost routes between the major cities.

Von Rosen's 1845 and Ericson's 1856 proposals are valid instruments because they were not conceived to promote urban growth, but to connect the three major cities (that we always exclude) and promote land reclamation in rural areas. Estimated pre-rail differences in population growth for towns that were included in each proposal relative to other towns are close to zero and statistically insignificant, which lends support to the qualitative historical evidence on the motivation behind these proposals.[24] Moreover, proposed rail routes are less likely correlated with contemporaneous town-level economic changes between 1855 and 1870, reducing concerns that our estimates will reflect rail connections that were assigned to towns that for unobserved

[22]While estimates of δ_{1870} corresponds to the short-term impact of a rail connection, estimates of $\triangle\delta_{>1870}$ captures the degree to which this short-term effect persists, attenuates or intensifies as the railroad network continued to expand in the 19th century.

[23]Bertrand, Duflo and Mullainathan (2004) discuss problems related to inference in the presence of serial correlation in specifications such as that in equation (1), and propose that clustering standard errors on the cross-sectional unit of observation works well in settings with more than 50 clusters, such as in our case.

[24]Two separate OLS regressions of the average annual percentage population growth between 1840 and 1855 on a dummy taking the value one for towns present in von Rosen's and Ericson's proposals yield a coefficient of -0.08 (s.e. = 0.21) and 0.16 (s.e. = 0.24) respectively.

reasons were growing faster in this period. The relevance of these two instruments rely on the strong first stage relationship between each proposals and actual rail lines constructed: respectively 56 and 67 percent of the towns included in von Rosen's and Ericson's proposals gained access to a rail connection in the "first wave." We construct our instrument as a dummy, taking the value 1 if a town was included in each proposal respectively and 0 otherwise.

Our third instrument follows the logic of Banerjee, Duflo and Qian (2012), where we create an instrument based on low-cost routes between Stockholm and the other major terminal points of the network.[25] This instrument is based on the intuition that when building a railroad to connect, for example, Stockholm and Gothenburg, towns located along the shortest and approximately cheapest route between these cities will exogenously gain access to a railroad.[26] Low-cost routes are approximated by connecting them by straight lines, corresponding to the shortest distance between each set of points (see Figure 2). In practice, because towns are represented as a point in longitude-latitude space we create buffer zones along the straight lines depicted in Figure 2.[27] Our instrument is then constructed as a dummy, taking the value 1 for all towns located in the buffer zone of these straight lines and 0 for all other towns.

These three instruments are used to separately predict rail connections in place by 1870, which yields the following first- and second-stage:

$$FW_{it} = \alpha_i + \lambda_t + \psi \left(PR_i^z \times Post_t \right) + \vartheta_{ijt} \tag{3}$$

$$\ln \left(P_{ijt} \right) = \alpha_i + \lambda_t + \delta \left(\widehat{FW}_{it} \times Post_t \right) + \varepsilon_{ijt} \tag{4}$$

where PR_i^z in turn denotes each of the three instruments (z) described above, and all other notation follows equation (1). We report both 2SLS estimates using each of the three instruments separately and LIML estimates using all three instruments jointly in the first stage, which have better properties in the presence of weak instruments (Stock and Yogo 2005).

Identification of the impact of an early rail connection (δ) in equation (4) requires that the instruments z do not affect urban populations through channels other than rail connections actually constructed. We provide indirect evidence in three placebo tests to support this implicit exclusion restriction.

[25] Based on the existing network proposals and discussions in Rydfors (1906) and Sjöberg (1956) we identify these other terminal points as Gothenburg, Malmö, the mining regions in the north and Norway, as shown in Figure 2. Recall that we always exclude these endpoints from our empirical analysis.

[26] Indeed, railroad historians have claimed that the main trunk lines were to be constructed along the shortest possible route between the terminal points, lending support to this argument (Rydfors 1906).

[27] We report results from using a buffer of 10 km, but results are similar using larger or smaller buffers.

3.2 Placebo Tests

One particular concern in the empirical analysis is that the rollout of the first railroads covaried with some unobserved and time-varying factor that also affected urban growth. To get at such concerns we conduct three placebo tests, where we examine the effect for routes that were proposed but for largely exogenous reasons not built by 1870 and lines only constructed after that point in time.

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 (see Figure 2). Topographical surveys were conducted for several of these routes and most were constructed after 1870. Moreover, lines that were not built prior to 1870 were often held up due to political infighting in the *Riksdag*, plausibly exogenous to relative differences in local development (Rydfors 1906; Sjöberg 1956).[28]

A third set of placebo lines are constructed based on all rail connections that opened between 1870 and 1880. These lines constituted the "second wave" of railroad construction, in which four-fifths of the mileage constructed was private (Nicander 1980). Because such connections plausibly were constructed with clear profit motives, with budding rail entrepreneurs seeking out locations with bright economic prospects, they would reflect local differences in demand for transport services. If such demand reflected growth prospects, we would expect to find "effects" for these unbuilt connections.

Taken together, these three sets of placebo lines are likely to reflect any bias stemming from unobserved time-varying factors correlated with the rollout of the "first wave." Conversely, if our estimates are picking up 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 equation (1), where they enter individually in the vector $\mathbf{Z_{it}}$.

4 Main Results

4.1 The Short-Run Impact of the "First Wave" of Railroads

Table 2 presents OLS estimates of equations (1) and (4) documenting that towns that gained access to the railroad network prior to 1870 grew significantly larger between 1855 and 1870 relative to other towns.

Our baseline difference-in-differences estimate in column 1 suggest that access to the railroad network in the "first wave" led to a relative population increase of 27 percent (0.23 log points) on average. Rail connectivity can therefore account for the whole difference in relative population

[28]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 qualitative evidence on similar such episodes.

growth between towns with and without access to the railroad network in the "first wave."[29] Taking into account time-varying region-specific shocks suggests a slightly larger effect of the railroad (column 2), which implies that towns that gained access to a rail connection where located in regions that on average experienced slower town-level population growth. Column 3 shows that balancing the sample on pre-rail characteristics does not affect the magnitude or precision of our estimate in a meaningful way. Our findings are therefore unlikely to be driven by state planners selecting towns that differed in terms of pre-rail observables. These estimates are all highly statistically significant.

Columns 4-6 report 2SLS estimates, using the two plans of the network and our low-cost route instrument to predict actual railroad lines in place by 1870, and column 7 presents LIML estimates, jointly using all three instruments in the first stage.[30] IV estimates are consistently larger than our baseline estimates, though differences are not statistically significant, which suggests that OLS estimates, if anything, tend to underestimate the impact of a rail connection on urban expansion. A large literature emphasizes that IV estimates can be severely biased in the presence of weak instruments (Bound, Jaeger and Baker 1995; Staiger and Stock 1997; Stock, Wright and Yogo 2002). However, rather strong first stages largely reduce such concerns.[31]

For the two sets of railroad lines that were included in von Rosen's 1845 and Ericson's 1856 proposal, but not constructed by 1870, and the set of lines constructed in the 1870s, point estimates are close to zero and statistically insignificant (columns 7-9). This would reduce concerns of unobserved time-varying factors that that potentially could covary both with the rollout of the "first wave" and town-level population growth. Moreover, the fact that the effect for assigned but unbuilt connections are precisely estimated to zero provides indirect evidence that the implicit exclusion restriction in our IV analysis holds, supporting a causal interpretation of the effect of access to the network on urban growth.

Taken together, our empirical analysis suggests a sizeable impact of the "first wave" of railroad expansion on urban growth. In the following two subsections we address potential threats to the validity of these estimates and estimate the extent of local spillovers on towns that remained unconnected to the railroad network by 1870.

[29] 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 acccess 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).

[30] Since our LIML estimates are overidentified, we can use standard tests to assess instrument validity. Based on Hansen's J statistic, we cannot reject the joint null of instruments being valid (p-value = 0.43).

[31] The corresponding Stock and Yogo (2005) critical value for a maximum 10 percent IV bias is 16.38 and 6.46 for one and three instruments and one endogenous regressor respectively. In three out of our four IV specifications we can therefore clearly reject the null of weak instruments in the first stage.

	Panel A. Baseline (OLS)			Panel B. IV (2SLS/LIML)				Panel C. Placebo Lines		
	Baseline	Baseline	Matched	1845 Proposal	1856 Proposal	Straight Lines	All Three	1845 Plan	1856 Plan	Built After 1870
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
First Wave (=1)	0.234***	0.278***	0.233***	0.242***	0.341***	0.321**	0.292***	0.240***	0.238***	0.261***
	(0.048)	(0.047)	(0.057)	(0.082)	(0.078)	(0.144)	(0.071)	(0.053)	(0.050)	(0.058)
Placebo Line (=1)								0.025	0.035	0.040
								(0.049)	(0.046)	(0.057)
Town FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Period FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Region × Period FE	No	Yes	No	No	No	No	No	No	No	No
Observations	243	243	138	243	243	243	243	243	243	243
1^{st} stage F-statistic	-	-	-	32.82	29.76	6.11	15.73	-	-	-
R-squared	0.74	0.82	0.84	0.74	0.73	0.73	0.74	0.70	0.74	0.71

Notes: This table presents estimates of equation (1) where the left-hand side variable is (ln) town population. Columns 1-3 report OLS estimates in our baseline sample, including region-by-year FE and in a sample balanced on pre-rail characteristics respectively. Columns 4-6 report 2SLS estimates using instruments derived from the two network proposals and from a straight-line instruments, as described in the text (see section 3.1). Column 7 report LIML estimates using all three instruments jointly in the first stage. We report Kleibergen-Paap (rk Wald) F-statistics for each respective IV estimation at the bottom. Columns 8-10 report similar estimates as in column 1, but where we also include a separate treatment for towns that were assigned a rail connection in the existing network proposals but that remained unbuilt by 1870, and for rail lines that were only constructed after 1870. Statistical significance based on standard errors clustered at the town-level is denoted by: *** p<0.01, ** p<0.05, * p<0.10.

Table 2: The Short-Run Impact of the "First Wave" on Town Populations, 1840-1870.

4.1.1 Robustness

Since our analysis is constrained by the fact that relatively few towns existed in the pre-rail era, one concern is that large estimated effects of a rail connection in the "first wave" is due to an outlier town or other specifics of our sample. Here we adress a number of potential sources of such biases. For brevity, we discuss these sensitivity tests here, but confine reporting of our results to the appendix (Appendix B, Table 7).

We begin by excluding all large and small towns (above and below the 75th and 25th percentile of 1855 population) and all towns that were a terminal point for a railroad line respectively (columns 1-3). All three estimates are close to our baseline estimate in Table 2, column 1.

Standard concerns in a difference-in-differences analysis is that estimated effects reflect pre-existing differential trends, rather than a true treatment effect. Column 4 and 5 include a full set of town- and region-specific linear trends respectively, to more flexibly allow for the possibility that towns with an early rail connection and those without were trending differently already before construction took place. However, relative differences in population remain largely within the range of previous estimates.

Related concerns are that the first railroads predominately connected towns in the interior, which could mean that our estimates partly are picking up differential trends of population growth across towns with and without access to waterborne transport. Column 6 and 7 include linear trends in longitude/latitude and access to waterborne transport respectively. Column 8 allows for convergence in town size by including trends in 1840 town population. Overall, our estimated effects remain largely similar in magnitude and precision.

Empirical estimates of the impact of rail connectivity reflects the effect for an average town, which could mask considerable heterogeneity. To consider if the "first wave" had a heterogenous effect along initial town characteristics columns 9-11 adds triple interactions of $FW_i \times Post_t \times X_i$ to equation (1), where X_i denotes initial (1855) population, market potential and access to waterborne transport respectively.[32] Estimates for these triple interactions are always small and statistically insignificant, leaving our estimated impact of the "first wave" largely unaffected.

4.1.2 Estimating Local Spillovers: Town Growth and Proximity to the Network

Our primary interest is to identify the causal impact on relative town populations of gaining access to the railroad network. However, it is also of interest to understand if the increases in population documented in the previous section reflect a reallocation of economic activity from towns without a rail connection to towns that became connected in the "first wave." In the presence of such general equilibrium effects, our estimates would overstate the aggregate impact

[32]For simplicity, we treat location on the coast or along one of the four major lakes as equivalent. Allowing for heterogeneity to the sea and access to the major lakes yields identical results.

of rail connectivity on urban growth.

We would expect (positive or negative) spillovers from the railroad to be confined to areas in close proximity to it. Hence, one way to get at such effects is to compare outcomes for towns with a rail connection to towns without, at various distances from the network. In the case of positive spillovers we would expect relative differences in urban growth to be larger when we compare towns in the "first wave" to other towns in proximity to the network, whereas in the presence of negative spillovers the opposite would be true.

Table 3 presents difference-in-differences estimates from equation (1) where we gradually alter the control group to consist of towns located further from the railroad network. For example, column 2 compares towns that gained access to the railroad network during the "first wave" to towns that were located at least 20 km from the network. Each column then shifts this cutoff outward in 10 km increments. Clearly, the relative impact of the railroad decreases as we shift the control group to consist of towns further from the network, which suggests large and negative spillovers on unconnected towns in proximity to the network. Estimated gains are essentially zero relative to towns more than 90 km away from the network.[33] Empirical estimates are similar in 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 natural access to waterborne transportation may have experienced different growth trends (Panel C).[34]

Overall, these results strongly suggest that population growth in towns that gained access to a rail connection in the "first wave" came at the cost of population growth in other towns in proximity to the emerging railroad network, consistent with the work by Fogel (1964).

4.1.3 Did Rail Connections Other than the "First Wave" Matter?

From 1870 to 1900, the railroad network expanded from 1,727 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 such rail connections have similar effects on town populations as those documented for the early railroad lines, or were settlement patterns induced by the "first wave" persistent in the face of further railroad expansion?

Table 4 presents results from OLS estimates of equation (1) adding an additional treatment indicator taking the value 1 as a town became connected to the railroad network and 0 in all pre-rail periods. Estimates are obtained from expanding our main sample to encompass decadal data on town-level populations from 1840 through 1900.

[33]Some readers might prefer a more direct way of estimating treatment spillovers by incorporating additional treatment indicators in our baseline difference-in-differences specification. Doing so yields qualitatively the same results (i.e., sharply negative relative effects as distance to the network decreases) as those that are reported in Table 3 and are available from the authors on request.

[34]The motivation for allowing for differential trends for towns with potential waterborne transport is primarily that coastal locations are correlated with the distance to the railroad network, as evident in Figure 2.

Control Group	>10 km	>20 km	>30 km	>40 km	>50 km	>60 km	>70 km	>80 km	>90 km
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Baseline Sample									
First Wave (=1)	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 (=1)	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. Allowing for Differential Trends for Towns With Waterborne Transport									
First Wave (=1)	0.239***	0.228***	0.208***	0.188***	0.167**	0.145*	-0.010	-0.028	-0.055
	(0.048)	(0.052)	(0.059)	(0.067)	(0.071)	(0.076)	(0.088)	(0.094)	(0.094)
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 equation (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 linear trends for towns located at the coast and the major lakes respectively. 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: Did Towns in the "First Wave" Grow at the Expense of Other Towns?

Column 1 shows a 42 percent (0.35 log points) increase in population for towns that gained access in the "first wave" relative to other towns. More interestingly, the coefficient on rail connections that opened after 1870 is small and statistically insignificant, which implies that rail connections that opened after 1870 had virtually no measurable impact on town populations. Empirical estimates remain similar when restricting the sample to the periods 1840-1880 and 1840-1890 respectively (columns 2 and 3), including region-by-period fixed effects (column 3), balancing the sample of towns on pre-rail characteristics (column 5), or using a LIML estimator (column 6), instrumenting for the "first wave" using the two network proposals and low-cost routes, as above.[35] Some specifications even imply that towns that gained access later lost population relative to other towns.

These results strenghten our argument of the "first wave" of railroads having a large and potentially long-lasting impact on relative town populations. The fact that later rail connections had little measurable impact on town populations is consistent with the historical importance of the first railroads to understand patterns of urban growth over the long run. In the next section we proceed to examine the effects of the "first wave" from the 19th century until today.

4.2 The Persistent Impact of the "First Wave" of Railroads

Figure 3 graphs estimated differences in population for towns with an early rail connection relative to other towns, over the last 200 years. Solid lines denote OLS estimates of δ_t in equation (2) and dashed lines correspond to a 95 percent confidence interval.[36]

From 1855 through 1900, population increased by 60 percent (0.42 log points) in towns with early access to the railroad network relative to other towns. Relative differences in population further intensified through the 1930s and 1940s. However, following World War II through the 1970s, in tandem with highway expansion and the breakthrough of motoring, population in towns with early access to the railroad network decreased relative to other towns, eventually stabilizing around the same level attained in the early 20th century. Albeit precision of the estimates attuenates over time, relative differences in population remains statistically significant at a 5 percent level in each decade from 1860 through 2010. Figure 3(b) 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. Importantly, prior to the railroads were constructed, relative differences in population are consistently estimated close to zero, suggesting that the identifying assumption is not violated.

Figure 3(c) reports estimated relative changes in the "urban hierarchy", simply defined

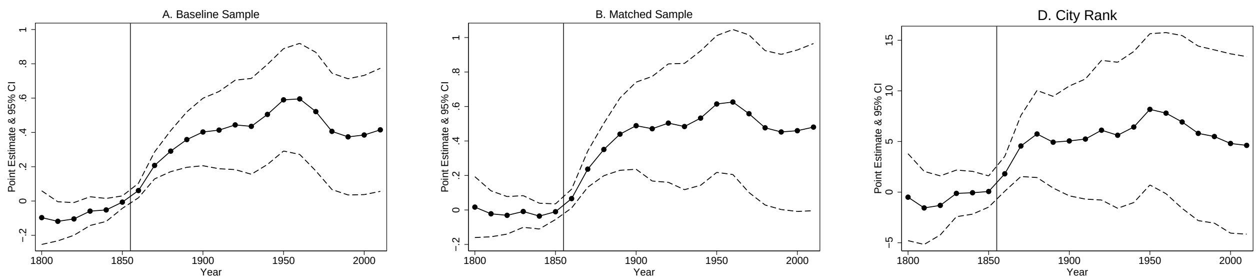
[35]We can easily reject that instruments are jointly weak in the first stage. The Kleibergen-Paap (rk Wald) F-statistic is 42.30, compared to a Stock and Yogo (2005) critical value for a maximum 10 percent bias of 6.46. Furthermore, based on Hansen's J-statistic, we cannot reject the null that instruments are valid (p-value = 0.36).

[36]For brevity, regression results underlying Figure 3 are confined to Appendix B, Table 8.

	Baseline (1)	Baseline (2)	Baseline (3)	Baseline (4)	Matched (5)	LIML (6)
First Wave (=1)	0.356***	0.250***	0.304***	0.451***	0.363***	0.433***
	(0.068)	(0.060)	(0.064)	(0.065)	(0.081)	(0.079)
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
1^{st} stage F-statistic	-	-	-	-	-	18.90
Observations	486	324	405	486	252	486
R-squared	0.77	0.78	0.77	0.83	0.83	0.77
Sample	1840-1900	1840-1880	1840-1890	1840-1900	1840-1900	1840-1900

Notes: This table presents estimates of equation (1) where we include additional treatment indicators for rail connections that opened after 1870. The left-hand side variable is (ln) town population. Columns 1-3 report OLS estimates in our baseline sample, including region-by-year FE and linear trends for each region respectively. Column 4 reports estimates in our sample that is balanced on pre-rail characteristics. Column 5 uses the three instruments described in the text (see section 3.1) in the first stage to predict rail connections in the "first wave". We report the Kleibergen-Paap (rk Wald) F-statistic at the bottom. Statistical significance based on standard errors clustered at the town-level is denoted by: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table 4: Comparing the "First Wave" to Later Rail Connections, 1840-1900.



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

Notes: These figures plot the δ_t-coefficients from equation (2). Connected solid lines correspond to point estimates and dashed lines to a 95 percent 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 relative to which all coefficients are measured against.

Figure 3: The Long-Run Impact of the "First Wave" on Town Populations, 1800-2010.

as the ranking of towns by their size.[37] We sort towns by their size S^r_{it} in year t, such that $S^{81}_{it} > S^{80}_{it} > ... > S^1_{it}$, and assign each town a rank ($r = 81, 80, ..., 1$) 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 20th century such relative increases are largely stable, although a slight but imprecisely estimated decline from the 1950s and onwards is visible.[38]

In sum, these results show that the first railroad lines led to a divergence of relative town populations in the 19th century. Towns with an early rail connection seems to have reached their long-run equilibrium level in the early 1900s, as reflected in largely stable relative differences in town populations over the 20th century. It is not obvious whether declining relative differences since the mid-20th century corresponds to a slow reversion to an unique town size equilibria, though below estimated differences seems more consistent with an explanation of path dependence.

5 Channels of Persistence

Our analysis this far has documented that towns that gained access to the railroad network in the "first wave" grew substantially larger over the latter half of the 19th century, and that these relative differences in population remained largely stable until today, despite the fact that virtually all towns eventually gained access to a rail connection. Does such persistence reflect path dependence in the location of economic activity?

[37]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

[38]See Heckscher (1907) for a discussion on historical changes in the urban hierarchy.

In this section we provide historical and present-day evidence of a variety of outcomes for towns that gained access to the network during the "first wave" and towns that did not. As outlined in Bleakley and Lin (2012), there are two main theoretical explanations for the observed persistent relative differences in town populations. First, historically sunk investments—such as housing or subsequent transport infrastructure—may slow down convergence along the transition path to long-run equilibria. Second, if there is a multiplicity of steady states, changes in settlement patterns across towns induced by the early railroads, may have solved a coordination problem of where, across several identical locations, households (and firms) should locate. Such sorting of initially similar towns into very different long-term equilibrias would correspond to path dependence in the location of economic activity.

5.1 Historical Differences Around 1900

Table 5 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 differences in historical factors can explain changes in urban population between 1855 and 2010 (Panel C).

We begin by comparing rail connectivity around 1900. Column 1 shows that towns with early access to the railroad network already by 1900 had worse rail connectivity than other cities, consistent with the sharp reversal of relative rail connectivity over the latter half of the 19th century documented in Figure 1. However, compared to towns of similar size differences in rail connectivity are close to zero and statistically insignificant (Panel B). These estimates are inconsistent with an explanation of persistent relative differences in towns populations arising from a historical oversupply of rail infrastructure.

Sweden underwent a rapid but uneven industrialization over the latter half of the 19th century. Did the "first wave" of railroads contribute to this process? In Appendix B (Table 9), we estimate relative differences using data from the 1870 manufacturing census that show that manufacturing employment in the towns of the "first wave" was disproportionately higher, that establishments were substantially larger, more likely to be incorporated and used more steam engines relative to towns that remained unconnected.

Table 5, columns 3-6 similarly use data from the 1900 population census to estimate relative differences in sectoral employment. Relative differences in manufacturing employment seemingly persisted over the 19th century. Employment in transport-related sectors was surprisingly 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 (HHI), that is increasing in the degree of specialization.[39] There is little evidence, however, that the sectoral distribution of employment differed between towns with an early rail connection and other towns.

Columns 8-10 compare relative investments in other forms of infrastructure and public utilities. Towns with early access experienced higher levels of historical investments grammar schools, telephones and electricity production.[40] However, such differences largely evaporate when we condition on town populations in 1900.

Could an industrial advantage in the early 1900s be an explanation for long-run persistence of these towns over the 20th century? We run a series of regressions of town populations today (2010) on our "first wave" dummy, conditioning on each historical factor and pre-rail town populations, from which results are reported in Panel C.

As a benchmark, column 1 reports that towns in that gained access to an early rail connection are on average 82 percent (0.60 log points) larger than other towns today. Each column then conditions on one of the factors in the top row. Conditioning on any of these factors have little effect on the size and precision of the estimated long-run effect of being assigned 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 percent, when we condition on the share of the local labor force employed in manufacturing around 1900 (column 2).

In sum, while we cannot rule out that some unobserved historical factor can account for long-run relative differences in population, there is little evidence to support explanations that put much weight on any single factor. Although early access to the railroad network seems to have had a differential impact on urban development in the 19th century—particularly within manufacturing—any such indirect effect of the early railroads is therefore an unlikely candidate to explain why towns with an early rail connection maintained their relatively larger populations over the 20th century.

5.2 Evidence on Path Dependence from Today

Table 6 compares a variety of current factors for towns that gained access to a rail connection in the "first wave" relative to other towns. Similar to the previous section we report both simple

[39]We calculate the Herfindahl–Hirschman index as $HHI_i = \sum \mathrm{e}_{\mathrm{si}}^2$ where *e* is the share of total employment in sector *s* in town *i*, across five sectors (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.

[40]We think of these variables as proxies for the broader provision of public infrastructure. Despite the fact that we limit our analysis to a relatively small set of factors in this section, other potentially observable factors are likely partially—although not perfectly—correlated with the factors that we condition on.

Historical Factor	Baseline	Railroads	Manufacturing	Trade	Transport	Services	HHI	Schools	Telephones	Electricity
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Access in the "first wave" and historical outcomes (LHS: 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? (LHS: 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? (LHS: 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 a first-order polynomial in longitude and latitude. In column 2 the historical factor is the number of rail connection normalized by town size (scaled by a factor of 1,000); 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 5: Comparing Towns in the "First Wave" and Other Towns Around 1900.

mean differences (Panel A) and estimates were we condition on present-day town populations (Panel B).

We begin by comparing the supply of transport infrastructure today. On average, there are no difference in the number of rail connections, adjusted by population, for towns with early access to the railroad network relative to other towns. This holds true both when compare simple differences-in-means and when we compare towns with an early rail connection to towns that are similarly large today. Moreover, this is consistent with the evolution of relative rail connectivity over the last 150 years (see Figure 1), and results in the previous section that found little difference in rail connectivity already by 1900. Columns 2 and 3 compare access to highways and trunk roads today. Towns with an early rail connection have, if anything, worse access to road infrastructure, although differences are quantitatively small and imprecisely estimated. Overall, there is little evidence for claims that relative differences in town populations persisted due to a historical oversupply of transport infrastructure.

Housing is a durable investment that depreciates slowly. Using data on the composition of the composition of the housing stock today, column 4 shows that the share of the the housing stock that was constructed prior to 1921 was 2 percentage points lower in towns with early access to the network relative to other towns. Towns of the "first wave", however, have a slightly higher share of old housing units in their housing stock today (Panel B). If housing was historically oversupplied in towns with an early rail connection, we would expect housing to be underpriced in those towns today. However, 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).[41]

A substantial body of work documents a positive relationship between population density and productivity and constraints imposed by historical building patterns.[42] If early railroads resulted in contemporary differences in density, due to differences in historical building patterns or evolving zoning laws, this could be one reason for observed relative differences in town sizes. Indeed, towns of the "first wave" are average almost 30 percent denser in terms of people per square kilometer today (column 6). However, compared to towns of similar size, they are indistinguishable in density (Panel B).

Old people, rather than old housing, is another intriguing channel of persistence. As towns with early access to the railroad increased their population relative to other towns in the early 20th century, these migration inflows could have resulted in an adverse demographic composition today—these towns could slowly be dying with their inhabitants. To the contrary, 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, this difference is precisely estimated to zero (Panel B).

[41] In addition, because the average pre-1921 share of the housing stock for the towns in our sample is 7 percent it is doubtful to what extent historical housing investments explain location decisions in the aggregate today.

[42] See Ciccone and Hall 1996; Rosenthal and Strange 2004; Combes et al. 2010 and Hornbeck and Keniston 2013.

Historical specialization patterns may be sunk in the sense of interlinked networks of suppliers and subcontractors. Although we do not directly observe such network, we get at such explanations by indirect comparisons of sectoral specilization using two indices. First, measuring sectoral diversity using an entropy index across 16 industry (SNI) classifications, shows no evidence of relative differences in diversity (column 8).[43] Secondly, column 9 shows that in towns with early access the number of industries, measured at the (SNI) 5-digit level, were on average 61 percent 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 inhabitant of towns with an early rail connection are more likely to work in the local labor market, as opposed to commuting. Column 11 shows that establishments are on average larger. However, both differences are entirely accounted for by 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. There is little to suggest that persistent relative differences in population reflect an oversupply of historical factors. These results seemingly suggest that the transitory advantage of an early rail connection in the "first wave" of railroad expansion in the mid-19th century gave rise to path dependence in the location of economic activity.

6 Conclusions

This paper analyzed the historical rollout of the Swedish railroad network, which provides historical circumstances that allows us to examine the plausibly causal impact of rail connectivity on town-level growth. We find large short-run increases in population for towns that gained access to the network, but also large negative spillovers on unconnected nearby towns. Such findings are consistent both with views that ascribe transportation infrastructure a central role in city growth and the argument that historical impact of the railroad was small in the aggregate due to displacement effects.

The advantage of an early rail connection was sharply reversed as the network continued to expand over the 19th century, which allows to study long-run adjustments in population to this transitory shock. We find little such adjustment over 150 years, as relative differences in town populations remained largely stable over the 20th century. Such evidence is seemingly consistent with economic geography models that features a multiplicity of steady states and

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

	Sunk Investments in Infrastructure and Housing, 2005/2010						Local Labor Markets, 2005/2010				
	Railroads	Highways	Trunk Roads	Old Housing	House Prices	Density	Old People	Ind. Diversity	No. of Ind.	Commuters	Est. Size
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(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 a first-order polynomial in longitude and latitude. 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: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table 6: Comparing Towns in the "First Wave" and Other Towns Today.

suggests the feasibility of transitory policy interventions permanently altering local development paths. Further research should aim to clarify whether such findings are generalizable to other settings by analyzing how the timing in the rollout of major infrastructure networks, that affect the locational decisions of households and firms, matter for long-run local development. More generally, how technological shocks in transport technology interact with initial conditions in shaping long-run patterns of local economic development seems to us to be an important question for future research to answer.

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A Data Appendix

Data Appendix

This appendix describes the construction of our dataset and provides detailed information on sources used. Our sample consists of all towns that held town charters prior to 1840. We merge two towns (Skanör and Falsterbo) that had been a under joint political rule since 1754 and formed a single municipality from 1863 and onwards. We also exclude the three major towns Gothenburg, Malmö, and Stockholm and the two insular towns Borgholm and Visby. This brings the sample size down to 81 towns, that constitute the baseline sample used in the paper.[44]

Population For each town in our sample we collect population data at decadal intervals 1800-2010, as well as for the year 1855. We obtained our data for the period 1800-1950 from Nilsson (1992) and Stads och Kommunhistoriska Institutet (2012). For the period 1960-2010 our data was obtained from Statistics Sweden.[45] For a small number of towns that did not hold town charters in the early 1800s and therefore were not reported in the official statistics we have assumed that their growth equalled the average growth of all other towns.[46]

Sectoral Employment 1855/1900 For the year 1855 our data on sectoral employment is based on census materials (*Tabellverkets Folkmängstabeller*), obtained from Stads och Kommunhistoriska Institutet (2012). As female employment is only sporadically reported our data only include male employees. We calculate employment as a share of the town-level labor force. Artisanal workers is the sum of *"hantverkare"* and *"arbetare*."

We also obtained the sectoral composition of employment in each town in 1900 from the population census (*Folkräkningen*) obtained from Stads och Kommunhistoriska Institutet (2012). Based on this source we calculate a Hirschmann-Herfindahl of sectoral specialization (see main text for calculation) and the share of manufacturing, trade, transport, agricultural and services sector as a share of the town-level workforce.

For 1870, data on manufacturing and artisanal employment was obtained from the official industrial statistics (*Bidrag till Sveriges officiella statistik D: Fabriker och Manufakturer*) available from Statistics Sweden. From this source we calculate the total number of manufacturing workers

[44] Towns included in our baseline sample are: Alingsås, Arboga, Askersund, Borås, Eksjö, Enköping, Eskilstuna, Falkenberg, Falköping, Falun, Filipstad, Gränna, Gävle, Halmstad, Haparanda, Hedemora, Helsingborg, Hjo, Hudiksvall, Härnösand, Jönköping, Kalmar, Karlshamn, Karlskrona, Karlstad, Kristianstad, Kristinehamn, Kungsbacka, Kungälv, Köping, Laholm, Landskrona, Lidköping, Lindesberg, Linköping, Luleå, Lund, Mariefred, Mariestad, Marstrand, Nora, Norrköping, Norrtälje, Nyköping, Piteå, Sala, Sigtuna, Simrishamn, Skanör (Falsterbo), Skara, Skänninge, Skövde, Strängnäs, Strömstad, Sundsvall, Säter, Söderhamn, Söderköping, Södertälje, Sölvesborg, Torshälla, Trosa, Uddevalla, Ulricehamn, Umeå, Uppsala, Vadstena, Varberg, Vaxholm, Vimmerby, Vänersborg, Västervik, Västerås, Växjö, Ystad, Åmål, Ängelholm, Örebro, Öregrund, Östersund, and Östhammar.

[45] All data that is obtained from Statistics Sweden is directly available at their website (www.scb.se).

[46] Results reported in the paper are nearly identical when instead using an unbalanced panel.

and artisans in each town. We also obtain data on the share of manufacturing establishments that belonged to incorporated firms, number of active establishments, gross output of the manufacturing industry and the number of steam engines used in each town.

Railroads, Highways and Trunk Roads Historical maps of the railroad network that include all lines built in each year were obtained from Statistics Sweden (*Bidrag till Sveriges officiella statistik L: Statens järnvägstrafik* 1862-1910). This is combined with modern GIS maps of the Swedish railroad network from Digital Chart of the World (http://www.diva-gis.org). Using ArcGIS, these two sources were combined to recreate the national railroad network as of 1870. We exclude all minor railroad lines that did not link up to the network. All towns were linked to this spatial layer based on the longitude and latitude of the centroid of each town.[47] In addition, we digitized the two alternative plans of the railroad network based on maps provided by Kungl. Järnvägsstyrelsen (1956, Map 1).

Using historical map collections housed at the University Library in Lund we have identified the number of railroad lines that emanated from each town, on a decade-by-decade basis from 1870 through 2010 (the following maps were used: *Liber Kartor AB, Stockholm 1993; Kartcentrum, Lantmäteriet, Vällingby, 2003; Esselte kartor, Generalstabens Litografiska anstalt, Stockholm 1980; Sverige på spår, Oskar Fröidh, Stenvalls förlag - Karta: Järnvägar i Sverige den 1 Januari 2000*). From such maps we also calculated the number of highways (*Europavägar*) and trunk roads (*läns- och riksvägar*) that emanated from each city in 2010.

Elevation The elevation of (the centroid of) each city, measured in feet above the sea-level, was obtained from an electronic atlas (http://www.veloroutes.org).

Schools 1900 Based on the educational statistics (*Bidrag till Sveriges officiella statistik P: Undervisningsväsendet 1899-1900*) we have coded a dummy for whether or not a grammar school (*Allmänna Högre Läroverk*) existed in a town in 1900.

Telephones 1900 From the official statistics on the telegraph network (*Bidrag till Sveriges officiella statistik. I: Telegrafväsendet 1900*) we have calculated the number of telephones per inhabitant in 1900.

Electricity Works 1900 Data on output per electricity work in 1900 is obtained from Berger et al. (2012), based on the official industrial statistics.

[47]Longitude and latitude was obtained from: http://www.findlatitudeandlongitude.com/batch-geocode/

Housing Stocks and Prices 2010 Mean house prices in 2010 was collected from a broker site (www.mäklarstatistik.se). We use prices for "*villor*". We impute data for one town (Marstrand) based on regressions of mean house prices on town populations (dropping this town from the sample yields identical results). The composition of the housing stock by age was obtained from Statistics Sweden (*Tätorter 2010, bostadsbebyggelsens ålder i tioårsintervall, antal och andelar*).

Local Labor Markets 2005 From Statistics Sweden we collected data on population density, the number of 5-digit industries present in each town, an entropy index of industrial composition, the share of the population that works outside of the local labor market, the number of workers and the number of establishments (*Table 1. Number of persons occupied (day-time population) in localities 2005*).

B Additional Results (Not for publication)

B.1 Heterogenous Impact of the "First Wave"

Table 7 reports estimates of equation (1) in subsamples excluding all large, small and terminal cities respectively (columns 1-3); including linear trends (columns 4-8); and allowing for heterogeneity in the treatment effect (columns 8-10).

B.2 Regressions Underlying Figure 3

Table 8 presents the regression coefficients underlying Figure 3, estimated from equation (2). For brevity, we only report the coefficient on the time-varying coefficients, that are shown in the figures.

B.3 Manufacturing Outcomes, 1870

Table 9 uses data from the 1870 manufacturing census, showing that a rail connection in the "first wave" was associated with accelerated industrialization and an overall modernization of industrial establishments.

As reported in column 1, the share of population employed in manufacturing was on average 2.8 percentage points higher in towns with access to the railroad network. Manufacturing workers were not only more plentiful, but also displaced artisanal workers in relative terms (column 2), which is consistent with existing evidence suggesting that the railroad promoted the shift from artisanal production to the factory system (Atack, Haines and Margo 2008).

Remaining columns explore how manufacturing establishments differed across towns with and without access to the network. While there were no more manufacturing establishments

	Subsamples			Linear Trends					Treatment Heterogeneity		
	No Large	No Small	No Terminal	Towns	Regions	Long/Lat	Water	1840 Pop.	Initial Pop	Initial MA	Sea/Lake
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
First Wave (=1)	0.224***	0.186***	0.289***	0.156***	0.263***	0.159***	0.233***	0.159***	0.256***	0.192**	0.238***
	(0.071)	(0.052)	(0.061)	(0.047)	(0.042)	(0.036)	(0.044)	(0.036)	(0.072)	(0.091)	(0.053)
First Wave (=1) ×											
Town size, 1855									-0.000		
									(0.000)		
Market Access, 1855										0.000	
										(0.000)	
Waternborne Transport (=1)											-0.019
											(0.030)
Town FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	183	183	213	243	243	243	243	243	243	243	243
R-squared	0.68	0.77	0.72	0.97	0.81	0.97	0.74	0.97	0.74	0.74	0.74

Notes: This table presents robustness checks of the main results provided in Table 2, based on estimations of equation (1). The dependent variable is ln town population. Statistical significance based on standard errors clustered at the town-level is denoted by: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table 7: Additional Robustness Checks, 1840-1870.

	Baseline		Matched		Rank	
	(1)	(2)	(3)	(4)	(5)	(6)
Year	δ_t	S.E.	δ_t	S.E.	δ_t	S.E.
1800	-0.097	(0.079)	-0.012	(0.087)	-0.499	(2.157)
1810	-0.118**	(0.057)	-0.045	(0.065)	-1.560	(1.813)
1820	-0.104**	(0.048)	-0.048	(0.053)	-1.310	(1.465)
1830	-0.059	(0.042)	-0.019	(0.044)	-0.125	(1.157)
1840	-0.052	(0.034)	-0.046	(0.036)	-0.062	(1.062)
1850	-0.006	(0.018)	-0.005	(0.022)	0.062	(0.775)
1860	0.061***	(0.021)	0.071**	(0.026)	1.810**	(0.865)
1870	0.208***	(0.040)	0.235***	(0.050)	4.555***	(1.515)
1880	0.291***	(0.060)	0.336***	(0.072)	5.741***	(2.161)
1890	0.358***	(0.081)	0.425***	(0.098)	4.930**	(2.275)
1900	0.403***	(0.099)	0.466***	(0.118)	5.055*	(2.725)
1910	0.414***	(0.113)	0.454***	(0.141)	5.242*	(2.981)
1920	0.444***	(0.131)	0.471***	(0.163)	6.116*	(3.465)
1930	0.435***	(0.140)	0.457**	(0.174)	5.616	(3.624)
1940	0.505***	(0.147)	0.510***	(0.184)	6.428*	(3.748)
1950	0.590***	(0.150)	0.600***	(0.187)	8.175**	(3.753)
1960	0.596***	(0.163)	0.630***	(0.200)	7.800*	(3.998)
1970	0.521***	(0.174)	0.574**	(0.218)	6.927	(4.289)
1980	0.406**	(0.170)	0.488**	(0.212)	5.804	(4.330)
1990	0.374**	(0.170)	0.461**	(0.212)	5.492	(4.299)
2000	0.385**	(0.175)	0.461**	(0.219)	4.805	(4.450)
2010	0.415**	(0.180)	0.487**	(0.227)	4.618	(4.406)
Town FE	Yes		Yes		Yes	
Decade FE	Yes		Yes		Yes	
Observations	1,863		1,058		1,863	
R-squared	0.929		0.936		0.879	

Notes: This table presents regression coefficients underlying Figure 3, estimated from equation (2). For brevity, we do not report any of the included (decade and town) fixed 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 8: The Long-Run Impact of the "First Wave", 1800-2010.

in towns with a rail connection (column 3), existing establishments were considerably larger in terms of employment and output (columns 4 and 5), and more commonly belonged to incorporated firms as opposed to sole proprietors (column 3).[48] Railroads also lowered the cost of transporting imported coal, further fueling an increase in the size of establishments by promoting the use of steam engines (column 6).

Overall, these results suggest a sizable impact of improvement of transport infrastructure on urban industrial development. Moreover, this evidence suggests that the impact of the railroad affected local industries along the intensive rather than extensive margin and, more generally, that railroads contributed to the increase in the average size of manufacturing establishments during this period.[49]

[48]The fact that the average difference in the size of establishments is substantially larger when measured as average gross output (i.e., including intermediate goods) per establishment (column 5) than when measured as the number of workers per establishment (column 4) suggest that towns with access to the railroad network specialized in production of goods where intermediates, that likely had to be transported, constituted a large share of the gross value of output.

[49]Manufacturing establishments, however, remained characteristically small despite this increase in average size: an establishment in a town with access to the railroad network employed on average 28 workers in 1870.

	Work/Pop.	Work/Art.	Est.	Workers/Est.	Output/Est.	Inc.	Steam/Est.
	(1)	(2)	(3)	(5)	(6)	(4)	(7)
First Wave (=1)	2.846***	0.853***	2.955	8.614*	0.767**	1.211***	0.131**
	(0.864)	(0.278)	(1.996)	(5.101)	(0.351)	(0.434)	(0.058)
Region FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	81	81	81	71	71	71	71
R-squared	0.32	0.28	0.47	0.29	0.45	0.42	0.20

Notes: This table compares manufacturing industries in towns that gained access to the network in the "first wave" to other towns. Dependent variables are defined as follows: the percentage share of the population employed in manufacturing (column 1), the ratio of manufacturing workers to artisanal workers (column 2), the percentage share of establishments that are owned by an incorporated firm (column 3), the log workers per manufacturing establishment (column 4), the log output per manufacturing establishment (column 5) and the number of steam engines used per manufacturing establishment (column 6). All specifications include controls for: (ln) 1870 population, log 1870 market potential, dummy that equal one if a town is located on the coast or has direct access to one of the four major lakes, and the percentage of the population employed in manufacturing in 1855. See the main text and Appendix A for a description of the data. Statistical significance based on standard errors clustered at the region-level is denoted by: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.

Table 9: Industrialization and the "First Wave" of Railroads, 1870.