Growth Off the Rails: Aggregate Productivity Growth in Distorted Economies

Richard Hornbeck
University of Chicago

Martin Rotemberg
NYU

January 2024

Abstract

We examine aggregate economic gains in the United States as the railroad network expanded in the 19th century. Using data from the Census of Manufactures, we estimate relative increases in county aggregate productivity from relative increases in county market access. In general equilibrium, we find that the railroads substantially increased national aggregate productivity. By accounting for input distortions, we estimate much larger aggregate economic gains from the railroads than previous estimates. Our estimates highlight how broadly-used infrastructure or technologies can have much larger economic impacts when there are inefficiencies in the economy.

For helpful comments and suggestions, we thank the editor, referees, and many colleagues and seminar participants at: Berkeley, Brown, U.S. Census Bureau, Chicago Booth, Chicago Federal Reserve, Clemson, Columbia, Columbia-NYU, Duke, EHA, Florida State, Harvard, Hunter, Iowa State, Indiana, LSE, Montreal, MSU, NBER DAE, NBER DEV, NBER EG, New Economic School, Northwestern, NYU, OECD, Oxford, PERC, Princeton, Queens, Sciences Po, SED, SMU, Toronto, UCLA, UPF, Wharton, Williams, and Zurich. We are grateful to Matt Jaremski and Chenzi Xu for providing banking data and guidance on the financial institutional context. Andrea Cerrato, William Cockriel, and Julius Luettge provided extensive research assistance. This research was funded in part by the Initiative on Global Markets at the University of Chicago Booth School of Business, the Neubauer Family Faculty Fellowship, NBER Innovation Policy grant program, and PERC. This material is based upon work supported by the National Science Foundation under Grant Number SES-1757050/1757051. Any opinions, findings, and conclusions or recommendations expressed in this material are those of the authors and do not necessarily reflect the views of the National Science Foundation.

We estimate impacts on aggregate productivity from the expansion of the railroad network, which integrated large domestic markets with vast land and commodity resources in the United States over the latter half of the 19th century. The railroads represented a technological improvement in the transportation sector, with modest direct benefits through decreased resources spent on transportation. However, we estimate that the railroads generated substantial indirect benefits through encouraging expansion in manufacturing and other sectors that were below efficient production levels. The railroads thereby generated much larger economic gains than previous estimates (e.g., Fogel, 1964; Donaldson and Hornbeck, 2016), which assume efficient input allocations, highlighting how broadly-used technologies or infrastructure can more-substantially impact aggregate economic growth in distorted economies.

Using newly digitized county-by-industry data from the US Census of Manufactures, we measure counties' manufacturing revenue and costs for materials, labor, and capital. We define "county aggregate productivity" or "county productivity" as the aggregate surplus each county generates (county revenues minus county costs), which sums to national aggregate productivity. In our main estimates, we focus on growth in counties' revenues, costs, and productivity. A key feature of these data is that we can use the detailed industry-level data to measure county-specific production functions, as counties produced different manufactured goods.

The manufacturing data allow us to decompose county productivity growth into two sources: growth in TFPR (total factor productivity) and growth in AE (allocative efficiency). TFPR growth reflects increased revenues from a given set of inputs, while AE growth reflects changes in input levels or their composition. Changes in inputs matter for aggregate productivity when there are market distortions, such as markups (Hall, 1988) or input distortions (Hsieh and Klenow, 2009), because increasing inputs then increases revenues more than costs (Petrin and Levinsohn, 2012; Baqaee and Farhi, 2020). County-level input distortions have a key role in this paper, which were not considered in prior estimates of the railroads' impact (Fogel, 1964; Donaldson and Hornbeck, 2016), generating much larger scope for economic gains from new technologies and infrastructure investment. By contrast, when markets are efficient, TFPR growth is the only source of aggregate productivity growth (Solow, 1957; Jorgenson and Griliches, 1967).

An expanding railroad network substantially decreased some counties' freight transportation costs, increasing manufacturing establishments' access to consumers, workers, and material inputs. The railroads had less benefit for counties on navigable waterways, and even could undercut those counties' access to previously captive consumers and inputs. We develop a general equilibrium model that summarizes these effects through changes in county "market access," building on work by Donaldson and Hornbeck (2016) that focused on the agricultural sector.

We estimate that increases in counties' market access led to substantial increases in county manufacturing activity and, because input-use in these counties was generally inefficiently low, this increase in manufacturing activity generated larger increases in revenues than costs (i.e., growth in county productivity). A one standard deviation greater increase in county market access, from 1860 to 1880, led to a 20% increase in county productivity, with similar percent impacts on county revenue and county expenditures on materials, labor, and capital. We decompose the increase in county productivity, finding impacts mostly driven by allocative efficiency growth (AE growth) rather than changes in county total factor productivity (TFPR growth).

Increases in county market access led to a general expansion of county economic activity, rather than systematic changes in local manufacturing industry concentration or a shift from agriculture to manufacturing. Similarly, we do not find that increases in county market access directly affected county-level input distortions or county-level gaps between the value marginal product of inputs and their marginal costs. Increases in county market access did not make counties more efficient; rather, it encouraged the expansion of economic activity in otherwise distorted counties that, as a consequence, led to increases in county aggregate productivity.

County market access is a function of the entire transportation network, which allows us to explore various sources of reduced form identification. While local railroad construction is potentially endogenous, and otherwise correlated with local growth, the estimated impacts from changes in county market access are robust to controlling flexibly for local railroad construction. The estimated impacts of county market access are thereby identified from more-distant changes in the railroad network, and how the spreading railroad network complemented or substituted for the previous transportation network that relied on navigable waterways for low-cost freight transportation. Places with high initial access to markets through waterways benefit less from expansion of the national railroad network, which we exploit in an instrumental variables approach that yields similar estimates to our baseline approach. We also find that our results are robust to controlling for "expected" changes in market access from potential extensions to the canal network in the absence of the railroad network (Fogel, 1964; Borusyak and Hull, Forthcoming).

Our empirical specifications estimate relative growth in county aggregate productivity from relative increases in county market access, comparing counties that experience differential growth in market access. These estimated relative effects are not sufficient to estimate how the railroads affected *national* aggregate productivity, however, because an expanding railroad network (1) shifted production inputs between counties and (2) increased aggregate production inputs in the United States. Cross-county differences in input distortions matter for (1), but for (2) the average level of input distortions also matters, and this second channel has been particularly under-emphasized in the literature relative to its quantitative importance in our setting.

To quantify impacts of the railroads on national aggregate productivity, we extend a benchmark quantitative spatial model (Eaton and Kortum, 2002; Donaldson and Hornbeck, 2016) to include market distortions that drive a wedge between firms' value marginal product of inputs and their marginal cost. These wedges create a gap between the revenue elasticity of each input and expenditure on that input as a share of overall revenue. We use our data from the manufacturing sector to calculate key parameters of the model, including county-level input wedges. In the model, as in the data, changes in county market access do not affect the wedges.

Holding fixed the total US population in 1890, we estimate that national aggregate productivity would have been lower by 5.5% in 1890 in the absence of the railroads. This reflects an important reallocation of economic activity across counties due to the railroads. This aggregate productivity loss without the railroads does not account for the direct benefits of the railroads themselves. The expansion of the railroad network could be considered a technological improvement in the transportation network, where it became cheaper to ship goods around the country. Fogel 1964 finds that the removal of the railroad would have led to a 2.7% aggregate loss due to increased transportation costs.[1] The total effect on aggregate productivity is the sum of these two estimates, so roughly triple the previous estimate ($\frac{5.5+2.7}{2.7}$). We estimate larger aggregate economic impacts of the railroads because we allow for changes in county input-use to affect county productivity, due to county-level distortions in input-use, rather than assuming that the value marginal product of inputs is equal to their marginal cost in all counties.

As an alternative counterfactual assumption, given the substantial immigration to the United States in the 19th century, we hold fixed worker utility and allow the total US population to be lower in 1890 without the railroad network. For this counterfactual scenario, we estimate a national aggregate productivity loss of 27%. We also consider intermediate cases, with declines in both aggregate population and worker utility, with intermediate declines in national aggregate productivity.

The railroads had a central role in enabling the substantial growth of the US economy,

[1] Much of Fogel's calculation reflects lost land value, in places assumed to be abandoned without the railroads, and Donaldson and Hornbeck 2016 focus on estimating these losses in land value and find a 3.2% aggregate loss. We further compare our approaches and estimates in Section VI.

and would not have been easily replaced. We estimate a 48% annual social rate of return on the \$8 billion of capital invested in the railroads in 1890 (in 1890 dollars), and estimate that the railroads privately captured only 7% of this social return. Additional canals might have been constructed in the absence of the railroads (Fogel, 1964), but we estimate that replacing the railroad network with this extended canal network would have mitigated only a small share of the aggregate losses from removing the railroad network.

Our paper highlights an important limitation underlying a long tradition in economics, back to at least Harberger (1964), of simplifying economic analysis by assuming there are no distortions in secondary sectors or locations. David (1969) critiques Fogel (1964) on related grounds, emphasizing the potential for increasing returns to scale, while Allen and Arkolakis (2022) show how the rationale for Fogel's social savings calculation can break down in the presence of agglomeration economies. There is a persistent appeal to economic analysis, in the style of Fogel's social savings calculation, that assigns value to some technology based on the cost of accommodating its absence. We highlight that social savings calculations are no longer upper bounds on welfare if other activities have positive social returns, and those activities would decline in the absence of the technology. Specifically, in our context, Fogel's social savings calculation is not an upper bound for the welfare effects of the railroad network when the marginal product of inputs exceeds their marginal cost. Measured impacts on land values in the tradition of hedonic analyses, as in Donaldson and Hornbeck (2016), can similarly understate economic impacts dramatically because substantial economic surplus may not be paid out to land (or other factors) when there are market distortions.

Our paper includes market distortions to extend a literature on estimating the impacts of market access (Redding and Venables, 2004; Hanson, 2005; Redding and Sturm, 2007; Head and Mayer, 2011; Duranton, Morrow and Turner, 2014; Donaldson and Hornbeck, 2016; Yang, 2018; Jaworski and Kitchens, 2019; Heblich, Redding and Sturm, 2020; Balboni, 2021). We find that input distortions create a quantitatively important additional channel through which increases in market access can generate economic gains (or losses, in principle). In doing so, our work relates to a literature that considers how the efficiency of resource allocation is affected by policies such as trade liberalization, financial regulations, and taxes (Khandelwal, Schott and Wei, 2013; Święcki, 2017; Singer, 2019; Tombe and Zhu, 2019; Bai, Jin and Lu, 2023; Berthou et al., 2020; Caliendo et al., 2023). In contrast to previous work on resource misallocation, which generally holds aggregate inputs fixed and considers the gains or losses from their reallocation (Asturias, García-Santana and Ramos, 2019; Firth, 2019; Zárate, 2022), an important feature of our analysis is how the railroads encouraged growth in aggregate inputs in the economy. By bringing this research on resource misallocation into a model of economic geography, we can explore both (1) the spatial allocation of economic activity and (2) how production expanded to use additional workers and new resources.

Our paper complements a literature that highlights the presence of resource misallocation in generating income differences (Restuccia and Rogerson, 2008; Hsieh and Klenow, 2009; Midrigan and Xu, 2014; Baqaee and Farhi, 2019*a*; Liu, 2019). We focus on the growth opportunities created by a variety of market distortions, rather than gains from reducing distortions themselves. We draw on a framework that allows for changes in aggregate productivity from increased input-use without changes to the production technology itself or changes in input distortions (Hulten, 1978; Petrin and Levinsohn, 2012; Baqaee and Farhi, 2020).

Understanding the local and aggregate economic impacts of the railroads speaks to the potential for market integration to drive economic growth and, more generally, for single technological advances to generate large economic gains throughout the economy. Market distortions magnify the impacts of technologies or infrastructure that encourage other economic activities that are *marginally* productive and thereby increase the value of output by more than the increased cost of inputs. The resulting economic gains are largest when the economy is most inefficient; that is, with great problems come great possibilities.

I Data Construction

I.A Manufacturing Data and Descriptive Statistics

We use data from the US Census of Manufactures (CMF), which published county-level totals for 1850, 1860, 1870, 1880, 1890, and 1900 (Haines, 2010). These manufacturing data include total annual revenue, total cost of raw materials, total wages paid, and the total value of capital invested (including buildings and land). Revenues and materials costs reflect "factory-gate" prices, based on Census instructions to enumerators: transportation costs were included in establishment expenditures on materials, whereas revenue received by the manufacturing establishment did not include costs of shipping goods to customers. We measure annual capital expenditures by multiplying the total value of capital invested by a state-specific mortgage interest rate that varies between 5.5% and 11.4%, with an average value of 8% (Fogel, 1964). See Appendix A for more discussion of data measurement.

We digitized county-by-industry tabulations published for 1860, 1870, and 1880. For our main analysis, we concorded the reported industries into 31 industry groups, though we also report outcomes using 193 more-detailed industry categories. We assume each industry has its own Cobb-Douglas production function. Our baseline regressions are at the county level, as industry entry and exit within counties makes it difficult to interpret percent growth at the county-industry level. To mitigate this concern for some further county-industry level analysis, we aggregate industries to five more-consistently present categories: clothing, textiles, and leather; food and beverage; lumber and wood products; metals and metal products; and other industries.

For each county-industry-year, we observe factory-gate revenue (R_{cit}) and expenditure on each input k (E^k_{cit}). County input expenditures tend to be smaller than revenue, which suggest the presence of market distortions that cause "wedges" between the value marginal product of inputs and their marginal cost (ψ^k_c). A positive wedge means there is inefficiently too little usage of input k, which could reflect firms' chosen markups or external factors like borrowing constraints. Figure 1 shows the distribution of expenditure shares across counties. Average expenditure, as a share of revenue, is 0.8 with a standard deviation of 0.1, which indicates positive wedges distributed broadly across the country. The distributions are fairly stable over time, though the average expenditure share is slightly higher in 1880.[2]

To measure the wedges, we need production function elasticities that, unlike the revenue shares, are not reported directly in the data. Measuring production functions is a classic setting where simultaneity bias is an issue (Olley and Pakes, 1996; Levinsohn and Petrin, 2003). To overcome this issue, we follow Hsieh and Klenow (2009) and exploit properties of Cobb-Douglas production functions. Given this assumption of Cobb-Douglas production (and given a known returns to scale), inputs' average cost shares reflect their production function elasticities regardless of shocks to productivity or prices.[3] We then use the relationship between producers' cost shares and revenue shares to infer distortions.

The general view is that historical manufacturing firm returns to scale were roughly constant (Atack, 1977; Sokoloff, 1984; Margo, 2014), as in modern manufacturing (Blackwood et al., 2021), so we assume constant returns to scale in our main specifications and later discuss implications of alternative economies of scale. We use national cost shares to measure industry production function elasticities: $\alpha^k_{it} = \frac{\sum_c E^k_{cit}}{\sum_c \sum_k E^k_{cit}}$. We then calculate county production functions, computing the revenue-share weighted average of the cost shares of the industries in the county and averaging across 1860, 1870, and 1880: $\alpha^k_c = \frac{1}{3} \sum_t \sum_i \alpha^k_{it} \frac{R_{cit}}{\sum_i R_{cit}}$. Materials are the most important input, with an average production function elasticity of 0.71 in 1860 (Appendix Table 1), followed by labor (0.25) and capital (0.04).

As in Hsieh and Klenow (2009), we calculate the wedges $\psi^k_c = \frac{\alpha^k_c - s^k_c}{s^k_c}$, where s^k_c is input k's average share of county revenue ($s^k_c = \frac{1}{3} \sum_t \frac{\sum_i E^k_{cit}}{\sum_i R_{cit}}$). Wedges reflect the perspective of firm optimization, since they cause firms to use proportionally too few inputs.

[2] Some counties have expenditure shares above 1, but few: around 1.5% of counties have an expenditure share above one in any given year (though almost all are below 1.1). Only 0.4% of counties are above 1 twice, and it never occurs three times. We suspect that measurement error is the most likely explanation. Our regression analysis uses the average expenditure share, which is never above one.

[3] Under no distortions, the ratio of inputs' cost shares are equal to the ratio of their production function elasticities, and the sum of the inputs' revenue shares is equal to the returns to scale.

In considering how changes in input-use across firms impact aggregate productivity, it is also important to consider the *difference* or "gap" between the value marginal product of inputs and their marginal cost. A wedge on a relatively important input will lead to a large gap, and therefore a large effect on aggregate productivity. Conversely, a large wedge on an input with little expenditure ($s_c^k \approx 0$) would not matter for aggregate productivity.

We can calculate the gap by multiplying each input's wedge by its revenue share: $\alpha_c^k - s_c^k = \psi_c^k s_c^k$. Appendix B discusses alternative methods for calculating county production function elasticities and wedges. The ψ_c^k and α_c^k terms are determined by the manufacturing data and firm cost minimization, which are independent of how we later model consumer demand and trade.

Appendix Figures 1 and 2 show the cross-county variation in average input wedges, which are similar across inputs but moderately smaller for materials. Regional differences in the wedges are largely driven by differences in revenue shares, rather than differences in output elasticities. Average input wedges are roughly one-fifth to one-third, which is similar to measured input wedges for the modern United States (Rotemberg and White, 2021). Average county gaps are largest for materials, followed by labor and then capital. This is because materials expenditures are the largest share of total input expenditures, rather than materials having the highest input wedges. Average input wedges and gaps declined over this period, with the notable exception of a sharp temporary increase in labor wedges in the South in 1870 following the emancipation of enslaved people and a substantial restructuring of labor markets (Appendix Tables 2 – 4). Appendix Tables 5 – 7 report information by industry group instead of region.

Input wedges can reflect a variety of market distortions, including markups and borrowing constraints. Producer cartels and insider lending may have contributed to misallocation in the 19th century United States (Lamoreaux, 1996; Ziebarth, 2013). County-level bank capital is itself endogenous, but we estimate that county input wedges are often lower in counties with more national-chartered banking activity and find more limited effects of state-chartered banks (Appendix Table 8). This is consistent with literature that national-chartered banks were more relevant for local manufacturing activity than state-chartered banks (Pope, 1914; Jaremski, 2014; Jaremski and Fishback, 2018; Carlson, Correia and Luck, 2022; Xu and Yang, 2022).

The correlation of the wedges and production function elasticities ranges from 0.3 (materials) to 0.5 (capital), and Appendix Figure 3 shows their joint distribution. If markups were the only source of wedges between marginal products and costs, then the correlation would be zero. The positive correlation is consistent with additional input-specific distortions (e.g., borrowing constraints) that bind more on industries that use that input more (as in Rajan and Zingales, 1998).

In supplemental analysis, we use data from the Census of Manufactures on the number of manufacturing establishments and workers. We also use data from the Census of Agriculture and Census of Population, which include county-level data on the total value of home manufactures, agricultural land value, and population.

I.B Market Access Data and County-level Changes

An expanding railroad network lowered county-to-county freight transportation costs. Figure 2, panel A, shows the network of waterway routes that includes canals, navigable rivers, lakes, and oceans. Panel B shows the railroad network constructed by 1860, which then expanded by 1870 (panel C) and 1880 (panel D). Appendix Figure 4 shows the railroad network in 1890 and 1900.

Railroads and waterways both provided low-cost freight transportation routes, but the comparatively sparse waterway network required more wagon transportation that was much more expensive per ton mile. We calculate freight transportation costs between each pair of counties using the available transportation routes in each decade.[4] We also calculate transportation costs under counterfactual scenarios that remove the railroad network or replace the railroad network with an expanded canal network proposed by Fogel (1964).

We approximate the "market access" of origin county o, summing over that county's cost of transporting goods (τ) to or from each other county d with population L:

$$MA_o = \sum_{d \neq o} (\tau_{od})^{-\theta} L_d. \tag{1}$$

County o has greater market access when it is cheaper to trade with other counties d that have greater population. Changes in counties' market access summarize how changes in transportation costs affect counties through interacting goods markets and factor markets across all counties. In Section V we derive this approximation for county market access in a general equilibrium trade model with input distortions. This same approximation for market access arises in a more-restricted model without input distortions (Donaldson and Hornbeck, 2016).

[4] Following Donaldson and Hornbeck (2016), our main specifications set railroad rates at 0.63 cents per ton mile and waterway rates at 0.49 cents per ton mile. Transshipment costs 50 cents per ton, incurred whenever transferring goods to/from a railroad car, river boat, canal barge, or ocean liner. Wagon transportation costs 23.1 cents per ton mile, defined as the straight line distance between two points. Due to the wide dispersion in travel costs by transportation method, the key features of the transportation network in this setting concern the required length of wagon transportation and the number of transshipment points. These assumptions abstract from price variation within transportation method, for instance due to competition. See Atkin and Donaldson (2015) for discussion of a setting where markups in the transportation sector affect the incidence of decreasing trade barriers.

For measuring county market access, as defined in Equation 1, we need estimates of θ and τ_{od}. The parameter θ reflects the "trade elasticity," which varies across empirical contexts. The parameters τ_{od} represent "iceberg trade costs," which normalize the measured per ton county-to-county transportation costs t_{od} by the average price per ton of transported goods ($\tau_{od} = 1 + t_{od}/\overline{P}$).

In Section V.D, we jointly estimate values for θ (3.05) and $\overline{P}$ (38.7). The estimated value of 38.7 for $\overline{P}$ is close to the value of 35 assumed by Donaldson and Hornbeck (2016) based on commodity price data from Fogel (1964). The estimated value of 3.05 for θ is smaller than the estimated value of 8.22 in Donaldson and Hornbeck (2016), due to differences in the model such as allowing for traded inputs, though the estimated relative effects of market access and aggregate counterfactual impacts from removing the railroad network are not sensitive to the value of θ.

Figure 3 shows in darker shades those counties that have relatively greater increases in market access from 1860 to 1870 and from 1870 to 1880 (see Appendix Figure 5 for 1880 to 1890 and 1890 to 1900). Our empirical specifications compare changes in darker shaded counties to changes in lighter shaded counties. Comparing counties within nearby areas, there is substantial variation in changes in county market access. Further, across decades, it is often different counties that are experiencing relatively larger or smaller changes in market access; which means that the estimated impacts of county market access do not only reflect particular counties growing relatively over the entire sample period.

Figure 3 also shows our main regression sample of 1,802 counties, which includes all counties that report manufacturing revenues and input expenditures in 1860, 1870, and 1880. We calculate county market access to all other counties with reported population, including other counties that are excluded from the regression sample because they do not report manufacturing data in each decade. We adjust the data in each decade to maintain consistent geographic units, as in Hornbeck (2010), which reflect county boundaries in 1890 and match the network database from Donaldson and Hornbeck (2016).

II Defining and Decomposing County Productivity

Our main outcome variable for county c is "county aggregate productivity" or "county productivity" in the manufacturing sector: total revenue minus total input costs, $Pr_c \equiv R_c - \sum_k E_c^k$. We focus on dollar revenue and expenditures in the regression analysis, though we use the model in Section V to discuss how market access affects both real and nominal outcomes, since market access can affect input prices.

For considering why county productivity increases with county market access, it is useful to re-write the impact of log market access on log productivity as a function of the impacts of log market access on log revenue (R_c) and log expenditures on k inputs (E_c^k):[5]

$$
\begin{aligned}
(2)\qquad \frac{\partial \ln Pr_c}{\partial \ln MA_c} &= \frac{R_c}{Pr_c}\left[\frac{\partial \ln R_c}{\partial \ln MA_c} - \sum_k s_c^k \frac{\partial \ln E_c^k}{\partial \ln MA_c}\right] \\
(3)\qquad &= \nu_c\left[\frac{\partial \ln R_c}{\partial \ln MA_c} - \sum_k \alpha_c^k \frac{\partial \ln E_c^k}{\partial \ln MA_c}\right] \qquad \text{(TFPR)} \\
&\quad +\nu_c\left[\sum_k (\alpha_c^k - s_c^k)\frac{\partial \ln E_c^k}{\partial \ln MA_c}\right], \qquad \text{(AE)}
\end{aligned}
$$

where $s_c^k = \frac{E_c^k}{R_c}$ and is the revenue share of input k (see Appendix C.2). In Equation 2, the term in brackets represents the percent impact of market access on revenue minus the (revenue share weighted) percent impact of market access on input expenditures. This term in brackets is scaled up by the ratio of county revenue to county productivity, $\nu_c = \frac{R_c}{Pr_c}$, which re-scales percent growth in county revenue into percent growth in county productivity.[6] In Equation 2, we measure the relationship between county productivity and market access regardless of the production technology.

Equation 3 decomposes the impact of market access on productivity, adding and subtracting $\sum_k \alpha_c^k \frac{\partial \ln E_c^k}{\partial \ln MA_c}$. Assuming production function elasticities for α_c^k gives a useful meaning to the decomposition (Petrin and Levinsohn, 2012), which becomes split into the expected growth in revenue from increased inputs (TFPR) and further increases in revenue due to market distortions (AE).

The first row reflects the difference between how much revenue actually changes with market access, and how much revenue would be expected to change given how much inputs actually change with market access. This difference would be positive if increases in market access improved counties' ability to turn inputs into revenue. This row reflects growth in county productivity through impacts of market access on county revenue total factor productivity (TFPR), defined conventionally as log revenue minus production-function-elasticity-weighted log input expenditures. TFPR is the only source of county productivity growth if markets are efficient, such that value marginal product of inputs is equal to their marginal cost.

The second row reflects further increases in county productivity from changes in inputs

[5]Revenue is equal to physical output of county c (Q_c) times price P_c. Expenditure on each input is equal to physical inputs (X_c^k) times its price W_c^k.

[6]This scaling factor approaches infinity as productivity approaches zero; in practice, we use the average county scaling factor across 1860-1880 (5.1) and discuss robustness to alternative calculations in Appendix B. This scaling factor is similar to that in Hulten (1978) and Petrin and Levinsohn (2012), though it is not a Domar weight (Domar, 1961) because we are reporting the percent impact on productivity rather than the percent impact on value-added.

when there are market distortions. If market access increases county inputs, that will increase county productivity if the value marginal product of those inputs is greater than their marginal cost (i.e., if $\alpha_c^k > s_c^k$ due to $\psi_c^k > 0$). Market access increases county productivity through growth in county allocative efficiency (AE) when market access increases the use of inputs with positive gaps, thereby increasing county revenues more than county costs. At the national level, total AE can increase by reallocating inputs from counties with relatively lower gaps to counties with relatively higher gaps, an important GE force in the counterfactual analysis, along with new inputs becoming used in counties with positive gaps.

We use data from the Census of Manufactures to define several outcome variables for county c in year t. We start by showing effects on log revenue ($\ln(R_{ct})$), log materials expenditures ($\ln(E_{ct}^M)$), log labor expenditures ($\ln(E_{ct}^L)$), and log capital expenditures ($\ln(E_{ct}^K)$), defined as the total values for county c in year t. Our main outcome variable is log productivity, in county c and year t, which we define as $\nu_c \left[\ln R_{ct} - \sum_k s_c^k \ln E_{ct}^k\right]$. We then define two additional outcome variables that decompose the impacts of market access on county productivity into the impacts through county TFPR ($\nu_c \left[\ln R_{ct} - \sum_k \alpha_c^k \ln E_{ct}^k\right]$) and the impacts through county AE ($\nu_c \left[\sum_k (\alpha_c^k - s_c^k) \ln E_{ct}^k\right]$). County revenue and county input expenditure vary by year, whereas fixed over time are the county revenue shares, county production function elasticities, and the scaling factor. Appendix A provides a reference for these formulas, along with further information on the underlying data from the Census of Manufactures.

III Estimating Equation

We regress outcome Y in county c and year t on log market access (MA_{ct}), county fixed effects (γ_c), state-by-year fixed effects (γ_{st}), and a cubic polynomial in county latitude and longitude interacted with year effects ($\gamma_t f(y_c)$ and $\gamma_t f(x_c)$).[7] Following the specification in Donaldson and Hornbeck (2016):

$$Y_{ct} = \beta \ln(MA_{ct}) + \gamma_c + \gamma_{st} + \gamma_t f(y_c) + \gamma_t f(x_c) + \varepsilon_{ct}. \tag{4}$$

The coefficient β reports the impact of county market access on outcome Y, comparing changes in counties with relative increases in market access to other counties within the same state and adjusting for changes associated flexibly with county latitude and longitude. We report standard errors that are clustered by state to adjust for correlation in ε_{ct} over time and within states.

[7] We assign county "latitude" (y_c) and county "longitude" (x_c) using the y-coordinate and x-coordinate of the county centroid, based on an Albers equal-area projection of the United States, whose coordinates reflect consistent distances North-South and East-West.

The identification assumption is that counties with relative increases in market access would otherwise have changed similarly to nearby counties. In Appendix B, we show that our estimates are robust to a variety of controls for alternative sources of differential county growth, and that counties had similar growth prior to relative increases in market access.

When estimating impacts of railroads, the main identification concern is generally that railroad construction may have been directed toward counties that would have otherwise grown more for reasons other than the new railroads, as discussed by Atack et al. (2010). We estimate impacts of county market access, which depends on changes in the entire railroad network and its interaction with the existing transportation network. We report estimates that control flexibly for changes in railroads within a county and within nearby areas. We also exploit the interaction between railroads and pre-existing low-cost waterway transportation, whereby some counties inherently benefited less from the national railroad network, to instrument for growth in county market access.

An additional potential concern, discussed by Allen and Arkolakis (2023), comes from the recursive nature of market access: growing counties induce their neighbors to grow, which in turn shows up as an increase in measured market access, so growth can lead to market access rather only than the reverse. This issue resembles the "reflection" problem for estimates of peer effects (Manski, 1993). We show that our estimates are similar when calculating market access in each period holding county populations fixed at initial levels, and only leveraging changes in the transportation network, which avoids this feedback effect.

IV Estimated Impacts of County Market Access

IV.A Estimated Impacts on Productivity

Table 1 presents results from estimating Equation 4. We estimate that county market access has a substantial and statistically significant impact on county manufacturing revenue and input expenditures. Column 1 reports that a one standard deviation greater increase in market access from 1860 to 1880 leads to a 19.2% increase in revenue (panel A), 18.3% increase in materials expenditure, 19.6% increase in labor expenditure, and 15.8% increase in capital expenditure. This one standard deviation greater increase in market access corresponds to a 24% greater increase in market access from 1860 to 1880 for our baseline definition of market access.

We estimate substantial increases in county productivity from increases in county market access, as Panel E reports a 20.4% increase in log productivity. As county market access increases, there is an increase in total county revenue that substantively exceeds the increase in total county input expenditures; that is, there is increasingly more revenue produced in excess of the value of inputs used. Column 2 reports similar estimates when calculating market access in each period holding county populations fixed at 1860 levels, such that changes in county market access are only due to changes in county-to-county transportation costs.[8] Column 3 reports moderately larger estimates when extending the sample period to 1900, using county-aggregate manufacturing data. Column 4 reports similar impacts on county revenue and capital expenditures, when extending the sample with the available county-level data for 1850.

Table 2 shows that most of the estimated impact of market access on log productivity (panel A) is driven by growth in county AE (panel C) with an insignificant contribution from county TFPR growth (panel B). This decomposition depends on the county production function elasticities, described in Section I.A, but column 2 reports similar estimates when using a more-detailed industry classification for this calculation and column 3 reports similar estimates when assuming one production function for all manufacturing industries (from 1860 to 1880). Because market access has similar percent impacts on revenue and each input (Table 1), this implies little effect on county TFPR when production exhibits constant returns to scale. Column 4 reports larger impacts on county productivity and county AE when extending the sample through 1900 using the available aggregated county-level data.

There would be more contribution from county TFPR growth under decreasing returns to scale, and less contribution under increasing returns to scale, but the impacts on county productivity continue to be driven more by county AE growth under moderate decreasing returns to scale. We also find that our estimates are generally not sensitive to controlling for other sources of differential growth, using alternative methods of measuring productivity, considering measurement error, or using alternative measures of market access (Appendix B).

IV.B Sources of Growth in County Allocative Efficiency (AE)

We estimate that county AE growth is driven by increases in input expenditures, in places where distortions lead to gaps between the value marginal product of inputs and their marginal cost, but those gaps do not themselves decrease with county market access. We estimate little systematic change in the structure of the county economy itself; rather, there was a general expansion of county economic activity from increases in counties' market access.

Table 3, column 1, reports that the estimated increase in county AE (from Table 2) is largely driven by increases in materials (panel C), followed by labor (panel B), with little

[8] In our setting, actual market access is highly correlated with population-fixed market access, and so our estimates in Table 1 are effectively unchanged whether we use actual market access or market access with fixed populations. Our preferred specifications use actual market access because, to be consistent with the model, we are interested in the effects of the railroads also through changes in the population distribution. We could also instrument for market access with fixed-population market access, which we describe below, but the first-stage is precisely 1 and so we report the reduced-form effect in Table 4.

change from capital (panel A). Columns 2 and 3 report that county input gaps and wedges do not themselves change systematically from increases in county market access. Constraints on firm behavior, such as those in borrowing markets, need not decrease with market access; and this result is also consistent with firm markups contributing to the measured county-level distortions, as markups would not vary with market access under CES demand and Cobb-Douglas production with constant returns to scale. There is some indication of market access decreasing labor wedges in the South and increasing input gaps in Western areas, but we do not estimate systematic impacts of market access on gaps or wedges in "frontier areas" (Bazzi, Fiszbein and Gebresilasse, 2020) and, overall, there is little systematic impact of market access on wedges and gaps within region (Appendix Table 9).

Table 3, column 4, reports that increased market access did not shift county production toward industries that are more capital-intensive, labor-intensive, or materials-intensive. Column 5 reports little change in the standard deviation of wedges across industries within a county, which suggests that inputs did not shift from more-distorted industries to less-distorted industries within counties. These estimates suggest that within-county reallocation of inputs, from marginally less-productive industries to marginally more-productive industries, is not increasing county TFPR.

To further explore the within-industry impacts of market access, we run a county-industry analysis that extends our baseline specification to include county-industry fixed effects and state-year-industry fixed effects. Column 1 of Table 4 reports estimated average impacts of market access on county-industry productivity, county-industry AE, and county-industry TFPR that are similar to our county-level estimates from Table 2, which suggests that across-industry within-county reallocation of inputs is not driving our baseline county-level estimates. Column 2 reports similar average impacts when weighting county-industries by their 1860 share of county revenue. There is some variation in industry-specific effects of market access, in columns 3 – 6, but no industry-specific effect is statistically different than the average of the other industries. Market access has little impact on the food sector, which was indeed a more local industry during our sample period: the refrigerated railcar was not used widely until the 1880s, and George Smith's patent for grinding less-perishable flour was filed in 1882 (Cronon, 2009). Appendix Table 10 reports little systematic effect of market access on gaps and wedges within these industry groups.

Increases in county manufacturing activity appear to be a general expansion of existing economic activity in the county. Column 1 of Table 5 reports little impact of county market access on the number of industries in a county. Table 5 also reports little impact of market access on the average size of establishments, measured as average revenue per establishment (column 2) or average number of workers per establishment (column 3). Instead, increases in county market access lead to a substantial increase in the number of manufacturing establishments (column 4), which is driving the overall increases in revenue and expenditures.

While county manufacturing activity increased substantially with increases in market access, we do not estimate that increases in market access prompted an economic shift from the agricultural sector toward the manufacturing sector within counties. Table 5, column 5, reports little impact of market access on county manufacturing revenue as a share of total manufacturing and agricultural revenue in the county. Similarly, columns 6, 7, and 8 report little impact on a county's manufacturing share of value-added, surplus, or employment. We also do not estimate that increases in market access encouraged economic activity in counties to become specialized in either manufacturing or agriculture or to become specialized within manufacturing (Appendix Table 11).

IV.C Endogeneity of Railroad Construction

A main empirical concern when estimating the impacts of transportation infrastructure is that infrastructure investment is generally directed toward areas that might otherwise change differently over time. Local railroad construction might also directly impact local manufacturing activity through increases in the demand for manufactured construction materials (Fishlow, 1965). One of the advantages of analyzing changes in county market access, rather than directly estimating impacts of local railroad construction, is that much variation in counties' market access is due to changes in the entire railroad network and how the railroad network interacts with other components of the transportation network (Donaldson and Hornbeck, 2016).

Table 6 reports similar impacts of county market access on county productivity when controlling flexibly for local railroad construction. Local railroad construction predicts increases in county market access, but the estimated impacts of county market access are similar when identified from more-distant changes in the railroad network and how railroad construction complemented or substituted for the previously established waterway network of rivers, canals, lakes, and oceans.

As alternative empirical approaches, we focus on changes in county market access that are driven by how the waterway network interacts with changes in the railroad network. First, note that as the railroad network expanded throughout the country, counties with pre-existing cheap access to markets through waterways generally experienced less increase in market access (because they already had low-cost access to many markets). We define county "water market access" in 1860, which reflects its measured market access when excluding all railroads from the transportation network.

Table 7, column 1, reports that counties with greater water market access in 1860 experienced less increase in market access from 1860 to 1870 and from 1870 to 1880. Under the identification assumption that counties with greater water market access would have otherwise experienced similar changes in manufacturing productivity, we can instrument for changes in county market access using county water market access in 1860. Columns 2, 3, and 4 report the 2SLS estimated effects of market access on county productivity, county TFPR, and county AE, which are less precise but similar in magnitude to the OLS estimates.

Borusyak and Hull (Forthcoming) suggest controlling for the "expected" change in market access when analyzing the actual change, given other potential changes in the transportation network, so identification comes from the unexpected residual change in market access. For this, we use Fogel (1964)'s proposal for canals that *might* have been built in the absence of the railroads.[9] Places along these proposed canals would plausibly have experienced greater increases in market access, even without the railroads. We calculate market access using Fogel (1964)'s proposed transportation network, and then include it as a control in our regressions. The estimated effects of market access (Table 7, Columns 5, 6, and 7) are similar to those in our baseline specifications.

V Aggregate Counterfactual Analysis

We now examine how national aggregate productivity was affected by changes in county market access due to the railroads. The regressions estimate that relative increases in county productivity were driven by relative increases in input-use, in counties with positive "gaps" where the value marginal product of inputs exceeded their marginal cost, but some of this relative increase reflects shifting inputs from other counties that also have positive gaps. An expanding railroad network may also increase aggregate inputs in the US economy. We use a quantitative spatial model to estimate the national aggregate economic impacts from the expansion of the railroad network and national aggregate economic losses under counterfactual transportation networks.

We add input distortions to a baseline model of economic geography (Eaton and Kortum, 2002; Donaldson and Hornbeck, 2016). Workers maximize utility and, in long-run equilibrium, are indifferent between counties. Firms maximize profits. Goods markets clear when the total value of production in a county equals total expenditure in that county. Each county is impacted by changes in transportation costs across the country, through linked goods markets and factor markets, and the effects on each county are summarized by changes in a county's market access.

We show how the presence of input distortions causes there to be national aggregate productivity gains from reductions in transportation costs that are not captured by changes

[9] The specific approach of Borusyak and Hull (Forthcoming) would require effectively random construction of particular railroad lines, which we do not think is plausible in our setting.

in land values (as in Donaldson and Hornbeck, 2016) or transportation cost savings (as in Fogel, 1964). We estimate the national aggregate productivity losses from counterfactual transportation networks, such as removing the railroad network or replacing the railroad network with an extended network of canals, along with impacts on worker welfare under alternative assumptions about international labor mobility.

V.A Model Setup

Following Eaton and Kortum (2002), each origin county o has an exogenous Hicks-neutral technical efficiency level $z_o(j)$, for each variety j, drawn from a Fréchet distribution with CDF given by: $F_o(z) = 1 - e^{-A_o z^{-\theta}}$, with $\theta > 1$. The parameter A_o captures average technical efficiency in county o (absolute advantage), while the parameter θ captures the standard deviation of technical efficiency across varieties (scope for comparative advantage). A smaller θ is associated with more-dispersed technical efficiency across varieties, larger incentives to trade across counties, and a less elastic response of cross-county trade flows to trade costs. As a consequence, this θ corresponds to the trade elasticity θ in Equation 1 (as in Donaldson and Hornbeck, 2016).

Firms in each county have a Cobb-Douglas production function for good variety j, $z_o(j) \prod_{k \in \{T,L,K,M\}} X_o^k(j)^{\alpha_o^k}$, using land T, labor L, capital K, and materials M. Firms use a continuum of good varieties as materials, with a constant elasticity of substitution across varieties, and so $X_o^M(j)$ is the CES quantity index for materials in county o, with an elasticity of substitution σ.

The main addition in our model, compared to Donaldson and Hornbeck (2016), is that our focus on manufacturing leads us to allow for firms to face input-specific distortions. These positive distortions are exogenous and represent market inefficiencies that discourage further use of labor (ψ^L), capital (ψ^K), land (ψ^T), or materials (ψ^M).

County input prices reflect factor mobility. We assume capital is mobile, such that interest rates are fixed exogenously. As in Donaldson and Hornbeck (2016), we assume that the United States faces a perfectly elastic supply of capital and that the nominal price of capital relative to the price index in New York City is fixed. County land prices are endogenous, and we assume the total quantity of physical land is fixed in each county.

Labor is supplied by workers, who consume good varieties j in the same manner that firms use these varieties in roundabout production; the CES price index workers pay for their consumption basket, P_o, is the same price index W_o^M paid by local firms for their inputs (Redding and Venables, 2004; Caliendo and Parro, 2015). Workers spend labor income in their home county. Workers' indirect utility in county o is $V(P_o, W_o^L) = W_o^L / P_o$. We assume workers are mobile across counties, focusing on a long-run equilibrium in which workers can arbitrage real wage differences, such that worker utility is constant across counties ($\bar{U}$).

As with labor income, we assume that factor payments to land are earned by (immobile) local landlords and the income associated with input distortions ($\Pi_d = \sum_k \psi_d^k W_d^k X_d^k$) accrues to local rentiers. In each county, landlords and rentiers (and capital owners) face the same price index as workers. We allow capital ownership to be geographically flexible, though we do not directly model forward-looking investment in capital since our model is static. We derive our baseline model assuming that the geographic distribution of capital ownership is equal to the geographic distribution of capital expenditures (as in Donaldson and Hornbeck 2016 and Kleinman, Liu and Redding 2023). Because we do not have data on where capital is owned, this is a convenient assumption whereby there are no cross-county capital flows in equilibrium when the returns on capital are equal across counties. Section V.H discusses alternative assumptions for the geographic distribution of capital ownership, following the approach of Caliendo et al. (2018). Our estimates are not sensitive to alternative assumptions because capital payments are a small share of total expenditures.

There is costly trade of good varieties across counties, for both final goods and intermediate goods (materials). Transporting goods from county o (origin) to county d (destination) incurs a proportional "iceberg" trade cost $\tau_{od} > 1$.

V.B Solving for Market Access

The price of variety j produced and sold in county o is:

$$p_o(j) = \frac{\prod\limits_{k \in \{T,L,K,M\}} \left(\left(1+\psi_o^k\right) W_o^k\right)^{\alpha_o^k}}{z_o(j)}, \tag{5}$$

where W_o^k and α_o^k are the price and production function elasticities, respectively, for input k in county o, and X_o^k is the corresponding quantity used in production.[10] In county d, the purchase price of good variety j is $p_d(j) = \min\limits_o \tau_{od} p_o(j)$. The price of a unit of materials (X_o^M) is the CES aggregator over the prices of each variety:

$$W_d^M \equiv \left[\int \left(p_d(j)\right)^{1-\sigma} dj\right]^{1/(1-\sigma)}. \tag{6}$$

Following Eaton and Kortum (2002), the value of total exports from county o is given

[10] It will also be useful to define the price of goods in o net of the productivity draw: $c_o = \prod\limits_k \left(\left(1+\psi_o^k\right) W_o^k\right)^{\alpha_o^k}$.

by:[11]

$$\text{Exports}_{od} = \kappa_1 A_o \left(\tau_{od} c_o\right)^{-\theta} Y_d P_d^{\theta}, \tag{7}$$

where $c_o = \prod_k \left(\left(1+\psi_o^k\right) W_o^k\right)^{\alpha_o^k}$. Equation 7 captures key forces governing trade flows in the model. First, county o sends more goods to county d when county o has higher technical efficiency (A_o) or lower "effective costs" (c_o), where "effective costs" reflect input prices and distortions.[12] Second, county o also sends more goods to county d when bilateral transportation costs are lower (τ_{od}), and when county d has higher revenue (Y_d).

Third, county o exports more goods to destinations with a high price index (P_d^{θ}). Firms produce more when they can sell to destinations with a high price index ("firm market access"), whereas consumers and firms purchase more goods and materials when they can buy from origins with a low price index ("consumer market access"). We show in Appendix C.1 that firm market access and consumer market access are proportional, so we define "market access" in county o as $MA_o \propto P_o^{-\theta}$. Changes in market access summarize the effect on county o from changes in the transportation network.

We can then use Equation 7 to express market access in county o as a function of the endogenous number of workers in each other county d:

$$MA_o = \left(\bar{U}\rho^{\frac{1+\theta}{\theta}}\right) \sum_d \tau_{od}^{-\theta} L_d MA_d^{\frac{-(1+\theta)}{\theta}} \frac{\left(1+\psi_d^L\right)}{\alpha_d^L} \qquad \forall o, \tag{8}$$

where ρ is a constant described in Appendix C. Market access is higher in county o when it is cheaper to trade with more-populated counties that have less access to other markets, a lower labor cost share, and a higher labor input wedge. This equation for market access simplifies to the corresponding Equation 9 in Donaldson and Hornbeck (2016) when there is no labor input wedge and a homogeneous labor production function elasticity. Our reduced-form analysis approximates Equation 8 with Equation 1, using the same expression from Donaldson and Hornbeck (2016), which considers only the "first-order" cost-weighted access to populations ($\tau_{od}^{-\theta} L_d$) and not higher-order changes in those populations' market access ($MA_d^{\frac{-(1+\theta)}{\theta}}$) or other variation in destination county characteristics ($\frac{\left(1+\psi_d^L\right)}{\alpha_d^L}$).

[11] Here, $\kappa_1 = \left[\Gamma\left(\frac{\theta+1-\sigma}{\theta}\right)\right]^{-\frac{\theta}{1-\sigma}}$, where $\Gamma(\cdot)$ is the Γ function defined by $\Gamma(t) = \int_0^{\infty} x^{t-1} e^{-x} dx$.

[12] Differences in these "effective costs" reflect sources of variation in marginal costs other than from technical efficiency.

V.C Equilibrium Conditions

The equilibrium conditions consist of market clearing, producer and worker optimization, and trade flows. Profit maximization and market clearing for capital, labor, land, and materials require that:

$$(1+\psi_o^k)W_o^k X_o^k = \alpha_o^k Y_o \qquad \forall o. \tag{9}$$

The aggregate quantity of land is fixed for each county:

$$X_o^T = \bar{X}_o^T \qquad \forall o. \tag{10}$$

The aggregate quantity of labor is fixed at the national level:

$$\sum_o X_o^L = \bar{X}^L. \tag{11}$$

Free mobility implies:

$$W_o^L = \bar{U} P_o \qquad \forall o. \tag{12}$$

The nominal price of capital is

$$W_o^K = \bar{W}^K \qquad \forall o. \tag{13}$$

Total revenue in an origin is the sum of its exports to all destinations including itself, $Y_o = \sum_d \text{Exports}_{od}$. Summing Equation 7 over all destinations:

$$Y_o = \sum_d \frac{A_o \left(\tau_{od} c_o\right)^{-\theta}}{\sum_{o'} A_{o'} \left(\tau_{o'd} c_{o'}\right)^{-\theta}} Y_d \qquad \forall o. \tag{14}$$

Equation 14 looks similar to its equivalent in a model without distortions, but distortions matter for equilibrium trade flows. The income that accrues to the local rentiers comes from wedges that discourage input use below efficient levels, which rationalize why firm revenues exceed firm input expenditures, but there is no "tax" on consumption. No income is destroyed and total expenditure by county d is the sum of spending from owners of capital, owners of land, worker wages, firm spending on materials, and rentiers:

$$Y_o = \sum_d \frac{A_o \left(\tau_{od} c_o\right)^{-\theta}}{\sum_{o'} A_{o'} \left(\tau_{o'd} c_{o'}\right)^{-\theta}} \left(W_d^K X_d^K + W_d^T X_d^T + W_d^L X_d^L + W_d^M X_d^M + \Pi_d\right) \qquad \forall o. \tag{15}$$

We can also write Equations 14 and 15 as a function of expenditure shares ($\alpha^k Y$) instead of factor payments ($W^k X^k$):

(16)

$$Y_o = \sum_d \frac{A_o \left(\tau_{od} c_o\right)^{-\theta}}{\sum_{o'} A_{o'} \left(\tau_{o'd} c_{o'}\right)^{-\theta}} \left(\frac{\alpha_d^K Y_d}{(1+\psi_d^K)} + \frac{\alpha_d^T Y_d}{(1+\psi_d^T)} + \frac{\alpha_d^L Y_d}{(1+\psi_d^L)} + \frac{\alpha_d^M Y_d}{(1+\psi_d^M)} + \Pi_d \right) \qquad \forall o.$$

Input-specific production distortions lower expenditure on each input below efficient levels, but total expenditure by county d is still equal to total revenue of county d.

The equilibrium is $\{p_o(j), P_o, Y_o, W_o^L, W_o^M, W_o^K, W_o^T, X_o^L, X_o^K, X_o^T, X_o^M\}$ such that Equations 5, 6, 7, 9, 10, 11, 12, 13, and 14 hold.

We solve for market access in each county and each decade (and counterfactual scenario). The equilibrium values of market access are the solutions to the N-by-N system of equations (Equation 8) with N unknowns (market access in each county n). County population (L_d) comes from the Census of Population in each decade, and we discuss below how we measure the additional components of Equation 8 and related parameters for estimating counterfactual changes in county population.

V.D Estimating Parameters

For measuring the origin county input wedges, ψ_o^k, we use the input wedges described in Section I.A from the manufacturing sector in each county for materials, labor, and capital.[13] Our baseline approach assumes that county input wedges in the agricultural sector and other sectors are the same as in the manufacturing sector in that county, due to the absence of detailed data on input expenditures outside the manufacturing sector.[14]

For the county production function elasticities, α_o^k, we use a weighted average of national elasticities in the agricultural sector and county elasticities for the non-agricultural sector.[15]

[13] We set land wedges ψ_o^T to zero. Since land's quantity does not change, the level of ψ_o^T does not affect productivity changes in the counterfactuals.

[14] The counterfactual sample is 2,722 counties with positive population and positive manufacturing or agricultural revenue in 1890, of which 309 sparsely-populated counties do not report manufacturing data in 1880 (which we assume reflects zero manufacturing revenue). We use the 1880 county-by-industry manufacturing data to measure ψ_o^k, as in Section I.A, when these data are available. We use 1890 county-level manufacturing data for 176 counties with no manufacturing data in 1880 (4% of US population in 1890), then use 1900 county-level manufacturing data for 69 counties with no manufacturing data in 1890 (1.5% of US population in 1890), and then use state median values of ψ_o^k in 1880 for the remaining 64 counties (0.2% of US population in 1890).

[15] For agriculture, following Donaldson and Hornbeck (2016), we use national value-added elasticities from Caselli and Coleman (2001) and the materials input share from Towne and Rasmussen (1960), giving us production function elasticities of 0.552 for labor, 0.1932 for capital, 0.1748 for land, and 0.08 for materials. Without data on the importance of fixed factors outside of agriculture, where it is likely important for sectors like housing, we use the land elasticity from agriculture as the fixed factor share outside of agriculture as well. For the remaining share of expenditure on labor, capital, and materials in counties' non-agricultural sector, we use values from counties' manufacturing sector in 1880. The manufacturing data includes land in

Given county production function elasticities α_o^k, the county input wedges ψ_o^k then imply county input revenue shares s_o^k.

For the origin-to-destination trade costs, τ_{od}, we use the calculated transportation costs from Section I.B. The network database calculates transportation cost per ton (t_{od}), whereas trade costs in the model have a proportional "iceberg" form (τ_{od}). We reconcile these by estimating an average price per ton of goods shipped in the economy ($\overline{P}$), such that: $\tau_{od} = 1 + t_{od}/\overline{P}$.

For the county-level fundamentals — average productivity (A_o) and the quantity of fixed factors (X_o^T) — we solve for the values that rationalize the observed distribution of population in 1890 (see Appendix C.3).

Appendix C.3 describes how we jointly estimate $\overline{P}$ and the trade elasticity θ, using data on total railroad shipments, aggregate revenue, and county land values. Broadly, we iterate to find the best values: first find the value of θ that minimizes the residual sum of squares between the model-predicted relationship between land values and market access and the corresponding relationship in the data (conditional on $\overline{P}$), as shown in Appendix Figure 6. Given θ, we then find the $\overline{P}$ that minimizes the difference between actual and model-implied total railroad shipments, as shown in Appendix Figure 7. This procedure allows us to estimate $\overline{P}$ in each county up to a proportional constant γ, which we estimate by minimizing the distance between nominal output in the model and in the data. We repeat the process until the values converge on estimates $\overline{P} = 38.7$ and $\theta = 3.05$. For these values of $\overline{P}$ and θ, the estimated impact of market access on county land value is 0.286 (0.037) from estimating Equation 4.

V.E Predicted Impacts of Market Access

We now describe how market access affects county productivity, and how we aggregate from impacts on county productivity to national aggregate productivity. The effect of market access on productivity in county o is given by:

$$\frac{d \ln Pr_o}{d \ln MA_o} = \nu_o \sum_k \left(\alpha_o^k - s_o^k\right) \frac{d \ln X_o^k}{d \ln MA_o}. \tag{17}$$

Market access increases county productivity by increasing real input usage ($\frac{d \ln X_o^k}{d \ln MA_o}$), when the value marginal product of that input exceeds its marginal cost (when $\alpha_o^k > s_o^k$ or $\psi_o^k > 0$).

Appendix C derives the log-linear impact of market access on each input ($\frac{d \ln X_o^k}{d \ln MA_o}$). Mar-

reported capital, along with buildings and machinery, but we cannot separate these components and we do not have other measures of counties' non-agricultural factor shares. We calculate county production function elasticities in 1890 as the weighted average of these agricultural and non-agricultural sector elasticities, where the respective weights are agricultural revenue and manufacturing revenue as a share of their summed revenue.

ket access increases capital usage by $\frac{\alpha_o^M+\alpha_o^L+1}{1+\theta\alpha_o^T}$ percent. Market access has a larger impact on labor and materials usage ($\frac{\alpha_o^M+\alpha_o^L+1}{1+\theta\alpha_o^T}+\frac{1}{\theta}$ percent), where there is an additional term $\left(\frac{1}{\theta}\right)$ because market access also decreases nominal wages and materials costs in county o.

To measure national aggregate productivity growth, expressed in percent terms relative to national value-added (GDP), we sum the growth in county productivity:[16]

$$APG = \sum_o D_o \sum_k \left(\alpha_o^k - s_o^k\right) d\ln X_o^k. \tag{18}$$

D_o is the Domar (1961) weight for county o (county revenue divided by national value-added). Our counterfactual analysis assigns each county the average of its factual and counterfactual Domar weight, which in both scenarios sum to 1.6. These Domar weights sum to more than 1, and are the appropriate way to aggregate county-level changes in settings with intermediate goods (Hulten, 1978) and distortions that generate a gap between the value marginal product of inputs and their marginal cost (Petrin and Levinsohn, 2012; Baqaee and Farhi, 2019*b*).

V.F Model Interpretation and Discussion of Assumptions

Our model highlights the impact of market access on county productivity and national aggregate productivity, which arises due to input distortions and generates economic gains from the railroads that are in addition to impacts on land values considered by Donaldson and Hornbeck (2016). In that model without distortions, all economic gains from increased market access are captured by the increase in land values (i.e., capitalized in the price of the fixed factor when there is frictionless international migration and worker utility is fixed). Increased land values reflect greater factor input payments, but increased productivity reflects gains in output that are not paid to inputs. Thus, in our model, the aggregate economic gains from the railroads are given by the increase in national aggregate productivity in addition to the increase in land values.

We make some important assumptions to maintain tractability in this general equilibrium setting, while extending the model to include input wedges. First, we assume that county input wedges are exogenous, which is consistent with our estimates that measured wedges and gaps were not impacted by changes in market access (Table 3). Second, we assume that county production function elasticities are exogenous, which is consistent with our estimates that county market access growth did not change county manufacturing cost shares (Table 3) or the county manufacturing share (Table 5). Third, we assume that county technical

[16] To go from Equation 17 to Equation 18, we multiply by county productivity (Pr_o), sum the level increase across counties, and express that sum relative to national value-added (GDP). Petrin and Levinsohn (2012) and Baqaee and Farhi (2019*b*) provide alternative derivations that simplify to Equation 18 for settings, like ours, in which technical efficiency is constant.

efficiency is exogenous, which is consistent with estimates from Table 2 that showed little impact of market access on county TFPR. Impacts of market access on TFPR may understate impacts on county technical efficiency (TFPQ) due to lower output prices, though TFPR is often correlated with technical efficiency in settings when both are measured (Foster, Haltiwanger and Syverson, 2008; Haltiwanger, Kulick and Syverson, 2018). Our counterfactual analysis considers impacts on aggregate productivity only through changes in allocative efficiency, under the assumption that technical efficiency does not also decline in the absence of the railroads. We use the model to compare impacts on real and nominal productivity, and report counterfactual impacts on real productivity.

V.G Model Validation Exercises

We undertake a few exercises to validate implications of the model. First, we use the model to generate predicted county-level outcomes in 1860, 1870, and 1880, where the only primitive allowed to vary over "time" is the railroad network and the model parameters are estimated in 1890. Table 8, Column 2, reports similar impacts of market access on nominal outcomes in this simulated data as in our reduced-form results. The model replicates the reduced-form results despite our not disciplining model parameters with the estimated relationship between market access and manufacturing revenues or expenditures. This similarity reflects two countervailing forces, though: (1) the model reflects a somewhat larger response of real inputs to market access, which pushes up the effect on productivity; but (2) in the model market access has no effect on TFPR, while in the data market access leads to a small increase in TFPR (though statistically insignificant).

We can measure quantities and prices separately in the model-generated data, and Column 3 of Table 8 reports the model-generated relationship between market access and real values. The prices of labor and materials decrease with market access, which decreases output prices, so the effect of market access on real productivity is slightly larger than its impact on nominal productivity. This implies that the previous regression estimates understate the impact of market access on real county AE because measured increases in input expenditures understate the increase in real input usage. Nevertheless, the real and nominal values of productivity are highly correlated: for each counterfactual scenario, their cross-county correlation is above 0.99.

One reason the model generates a larger productivity response than in the data is that workers are instantaneously mobile across counties in the model, whereas in the data workers might respond over time (Allen and Donaldson, 2022). Appendix Table 15 shows the estimated impact of market access and lagged market access on log manufacturing employment. Over a 20 year period, a one standard deviation increase in market access leads to a very similar increase in manufacturing employment as the model value (Table 8, Column 2). Of this total response, three-fourths of the migration response comes in the first decade.

The model predicts that the effect of market access on revenue is similar within a county, across industries, regardless of the industries' initial gaps. This contrasts with the model-predicted effect on productivity, where industries with higher initial gaps should see larger productivity growth from the same change in revenue. Appendix Table 16 shows estimates consistent with this theoretical result: in the county-industry data, the impact on revenue from market access does not vary with the counties' average wedge, but this interaction effect does predict a greater increase in productivity.

The model predicts log-linear impacts of market access on county productivity and, indeed, Appendix Figure 8 shows approximately log-linear impacts of market access on county productivity that are driven by county AE growth with little change in county TFPR. This pattern holds for model-derived market access (Equation 8) and the first-order approximation of market access (Equation 1).

The model-derived changes in county market access, from 1860 to 1880, have a correlation coefficient above 0.99 with the first-order approximated changes in county market access used in the regression analysis. These measures need not be so highly correlated in other empirical settings, for instance if large destinations experience very different changes in market access, but this appears to be rare and applications of this method generally use the first-order approximated formula. While the reduced-form analyses can rely on the approximation in Equation 1, the full expression for market access is required for aggregate counterfactual analysis because we need to determine not only relative changes in county market access but the absolute changes in counties' market access under counterfactual scenarios.

V.H Estimated Counterfactual Impacts

We now estimate the decline in national aggregate productivity from removing the railroad network or from other counterfactual transportation networks. To benchmark the magnitudes, US aggregate productivity growth in manufacturing was about 2.2% annually from 1860 to 1900 in our data, with roughly a quarter coming from technical efficiency growth. Similarly, for the whole economy in this era, estimates indicate around 0.5% annual growth in technical efficiency (Abramovitz and David, 1973).

Figures 4 and 5 map the county-level declines in market access and productivity when removing the railroad network, such that county-to-county freight transportation must rely on the existing waterway network and high-cost wagon transportation. Darker-shaded counties represent larger counterfactual declines in market access and productivity in the absence of the railroads, as economic activity shifts toward the waterway network. The declines in county productivity reflect counterfactual declines in market access and production inputs, interacted with county-level "gaps" between the value marginal product of inputs and their marginal cost (Appendix Figure 9), as in Equation 17. National aggregate productivity declines as aggregate inputs decline, given positive average gaps, and additionally as inputs shift from counties with larger gaps to counties with smaller gaps.

We estimate that national aggregate productivity would have been 26.7% lower in the United States in 1890, if there were no railroad network (Table 9, panel A, column 1). This 26.7% decline in national aggregate productivity reflects only decreases in allocative efficiency, with no decline in county technical efficiency. This is equivalent to roughly 12 years of aggregate productivity growth at the rate of growth in manufacturing. For inference on our estimated productivity losses, Appendix B.4 discusses bootstrapping over county-industry observations. For each realization of the bootstrap, we sample county-industries (with replacement) to create alternative measures of county wedges, and then estimate the counterfactual losses from removing the railroads. The resulting 99% confidence interval for our bootstrapped estimates is an aggregate productivity decline between 24.67% and 26.87%.

The 26.7% decrease in national aggregate productivity is worth 26.7% of GDP annually, or \$3.2 billion in 1890 dollars. As a comparison, the estimated cost of the railroad network in 1890 was \$8 billion (Adams, 1895). We estimate that this investment in the railroads generated an annual social return of 48%, and that the railroad sector privately captured only 7% of its social return in 1890.[17]

Rather than removing the entire railroad network, we can also consider the counterfactual economic losses if the railroad network had stopped expanding. For example, in 1890, we estimate that productivity would have been 2.6% lower using only railroads that existed in 1880, 9.9% lower using railroads from 1870, 15.6% lower using railroads from 1860, or 22.3% lower using railroads from 1850 (Table 9, panel A, columns 2 – 5). Expansion of the railroad network after 1860 contributed roughly one-fifth of total manufacturing productivity growth, given the observed annual growth rate of 2.2%.

Additional canals might have been constructed to mitigate national productivity losses,

[17] We estimate that the railroads generated an annual private return of 3.5% in 1890. For this calculation, based on numbers from Adams (1895), we sum the railroads' reported net income (\$145 million), debt interest payments (\$217 million), net capital expenditure (\$5 million), and subtract losses not otherwise reflected from some companies (\$30 million) along with subtracting income from other sources (\$52 million). We then divide \$285 million by the cost of the railroads including equipment (\$8.041 billion) and value of land (\$80 million). Much of the railroads' reported transportation expenses were maintenance costs (39% or \$271 million), and we interpret the reported "permanent improvements" of \$5 million as total capital expenditure minus depreciation. To calculate the annual social return, we sum the annual private return (\$285 million), our estimated annualized increase in agricultural land value (\$414 million), and our estimated increase in annual productivity (\$3.204 billion), divided by the cost of the railroads including equipment and land (\$8.121 billion).

in the absence of the railroad network, but we find that these canals would have been an ineffective substitute for the railroad network. We evaluate the system of feasible canals proposed by Fogel (1964), estimating that productivity would have been lower by 23.5% in 1890 when replacing the railroad network with these additional canals (Table 9, panel A, column 6). That is, the additional canals would have mitigated only 12% of the national aggregate productivity loss from removing the railroad network.

By contrast, the railroads would have been "cheap at twice the price." We estimate that productivity would have been lower by 8.7% in 1890 if railroad rates were double (Table 9, panel A, column 7). Compared to losing access to the railroad network entirely, using these more-expensive railroads would mitigate 67% of the national productivity decline.

In estimating the decline in aggregate productivity, we consider several scenarios for counterfactual changes in US total population. Our baseline estimates reflect the counterfactual decline in total population that holds fixed worker utility (real wages). We also consider a scenario that holds fixed total population, and calculate the associated decline in worker utility, along with scenarios that reflect intermediate declines in total population.

When allowing for aggregate declines in population, the model predicts a substantial decline in population in the United States. For worker utility to be unchanged in the counterfactual, the model predicts that the US population would need to be 66% lower in 1890. By comparison, the US population was 39% lower in 1870 and 73% lower in 1840 than in 1890 (United States Census Bureau, 1975). If we replace the 1890 railroad network with the 1860 railroad network, the model explains 84% of total population growth between 1860 and 1890.

When holding fixed total population, we solve for counties' population shares in the absence of the railroad network. We then calculate corresponding changes in other production inputs, revenue, and the resulting change in aggregate productivity in the United States. Panel B of Table 9 reports that in the absence of the railroad network, forcing total US population to remain unchanged, national aggregate productivity is estimated to fall by 5.5% in 1890. The bootstrapped 99% confidence interval is declines between 4.96% and 5.61%. Population and other production inputs become condensed into limited geographic areas, decreasing labor productivity due to an increase in the land-labor ratio and increasing goods prices, such that worker utility falls by 32.7%. Intuitively, the incidence of economic gains from the railroads falls more on workers when their mobility is restricted.

These two counterfactual scenarios highlight the relative contributions from the average level of distortions in the economy, as compared to variation in the distortions across counties. We can decompose the the 26.7% aggregate productivity decline, without the railroads, into an average component from county inputs changing given positive average national gaps

$(\overline{\alpha_o^k - s_o^k})$ and a residual term that reflects idiosyncratic county gaps:

$$
\begin{aligned}
(19) \qquad APG \; &= \; \sum_o D_o \sum_k \overline{(\alpha_o^k - s_o^k)} d \ln X_o^k \\
&+ \sum_o D_o \sum_k \left((\alpha_o^k - s_o^k) - \overline{(\alpha_o^k - s_o^k)} \right) d \ln X_o^k .
\end{aligned}
$$

We calculate a 20.7% decline in aggregate productivity from the average component, with one-fifth of the total effect driven by the residual component that itself is similar to the estimated loss in overall aggregate productivity when holding aggregate population fixed. The contribution from the average component is consistent with multiplying the sum of the revenue-weighted average input gaps for the whole economy (0.132) by the average counterfactual decline in county inputs (1.11 log points) and by the sum of the county Domar weights (1.6) to get a national aggregate productivity decline of 0.234 log points or 21%. The sum of the average input gaps (0.131) largely reflects the gap for materials (0.073), whereas the gap for capital is 0.012, so the aggregate productivity decline is not sensitive to capital distortions that are more subject to measurement error.

The railroads and an expanding US economy encouraged immigration and aggregate population growth, but the true counterfactual response in aggregate population is likely somewhere between our extreme scenarios of a complete migration response (holding utility fixed) and no response (holding aggregate population fixed). We cannot directly estimate the impact of county market access on immigration and aggregate population growth in the US, but can provide a benchmark using relative worker movement within the US. The within-US response of workers to market access is 77% of the model-predicted full migration response within the first decade (Appendix Table 15). Appendix Table 17 shows a range of counterfactual estimates, for alternative assumptions on aggregate population declines. If we assume the aggregate population decline is 77% of the population decline predicted from a full migration response, which corresponds to a 51% decline in total population, then counterfactual aggregate productivity falls by 20% (and utility falls by 13%). If we instead assume that total US population would be lower by 33% in the absence of the railroads, which excludes the foreign-born population in 1890 and white native-born children of foreign-born parents, then we estimate a 14% decline in productivity and a 22% decline in worker utility.

National aggregate productivity falls in the counterfactual scenarios because of gaps between the value marginal product of inputs and their marginal cost, but manufacturing gaps in our data are not large in comparison to other eras. For the United States, in 1997, the manufacturing gap is around 0.3 using the NBER-CES database (Becker, Gray and Marvakov, 2013; Jaumandreu, 2022). Thus, the substantial impacts of the railroads on national aggregate productivity are not driven by especially large measured gaps in the historical data; rather, the effects are driven by moderately-sized gaps and the substantial impacts of the railroads on both the relative allocation of inputs across counties and aggregate inputs in the United States.

We would estimate zero impact of the railroads on national aggregate productivity, mechanically, if we assumed zero gaps between inputs' value marginal product and marginal cost. Our baseline counterfactual assumes the measured wedges in the manufacturing sector also reflect wedges in the agricultural sector. If we assume no distortions outside of the manufacturing sector, we estimate an aggregate productivity loss of 16.5% in 1890 without the railroad network (Table 10, column 2). The estimated counterfactual impacts also become moderately smaller if we adjust counties' measured input expenditures using counties' measured materials wedges as a proxy for capital wedges or labor wedges (Table 10, columns 3 and 4). The measurement of capital expenditures is particularly subject to measurement error, but capital expenditures are a small share of total input expenditures and so assuming zero misallocation in capital only moderately reduces the aggregate productivity impact to 20.2% (column 5). Capital may also be at a statically inefficient level due to adjustment costs (Asker, Collard-Wexler and De Loecker, 2014), or face risk such that ex-post capital use is seemingly distorted, which are further motivations for showing the sensitivity of our results to alternative distortions for capital. If we decrease the dispersion in capital wedges (or all input wedges) wedges by 5, 10, or 25 percent, the counterfactual estimates are within one percentage point of our baseline estimate.

Our estimated counterfactual impacts on national aggregate productivity vary moderately with the estimated value of $\overline{P}$ (average price per ton of traded goods) and are not sensitive to the estimated value of θ (the trade elasticity) in columns 6 – 10 of Table 10. The results are stable across values of θ, as in Donaldson and Hornbeck (2016), which reflects two countervailing forces: (1) for a higher θ, changes in market access matter less for economic outcomes; but (2) for a higher θ, market access declines more in the counterfactual without railroads. In our model, as in the trade literature generally, a higher trade elasticity implies lower gains from given trade flows. However, to fit the data on the spatial distribution of economic activity, we estimate more trade flows for a higher θ. These two forces largely cancel out, so counterfactual impacts from shocks to market access are not sensitive to θ. The estimated counterfactual impacts are more sensitive to the estimated value of $\overline{P}$ because higher values of $\overline{P}$ effectively re-scale the baseline transportation cost parameters and diminish differences between the factual and counterfactual scenarios.

Our main estimates assume that the ownership of capital assets is in the same location where they are used in the factual and counterfactual equilibria (as in Donaldson and Hornbeck 2016 and Kleinman, Liu and Redding 2023). An implication is that total revenue then equals total expenditure in every county, so trade is balanced (and balanced trade is a standard set-up in quantitative geography models (Helpman, 1995; Redding and Sturm, 2008; Ramondo, Rodríguez-Clare and Saborío-Rodríguez, 2016; Sotelo, 2020; Allen and Arkolakis, 2023). Capital ownership may not equal capital use (as in Caliendo et al. (2018)), which means intranational trade would be unbalanced (as for international trade, in Eaton et al. 2016). Following Caliendo et al. (2018), we can instead allow for net capital flows by assuming that all capital is held in a national portfolio whose ownership is allocated to various counties. Each county's share of the national portfolio is then held fixed, across factual and counterfactual equilibria, which leads to endogenously changing trade balances. We report estimates for alternative assumptions on counties' fixed share of capital ownership. Formally, this means rewriting Equation 15 such that expenditure in every destination county is:

$$Y_d = \left(R_d^K + W_d^T X_d^T + W_d^L X_d^L + W_d^M X_d^M + \Pi_d\right), \tag{20}$$

where R_d^K is the capital income in county d such that $\sum_{d'} R_{d'}^K \equiv R^K = \sum_{d'} W_{d'}^K X_{d'}^K$ and $R_{d'}^K = \mu_{d'} R^K$, where μ_d is exogenously given.

Appendix Table 18 shows that our counterfactual estimates are similar under different assumptions for the geographic distribution of capital ownership: assuming all capital is owned in New York City; assuming fixed capital ownership shares in the 1890 factual equilibrium or our baseline counterfactual equilibrium without railroads; or assuming capital ownership shares equal to personal property shares recorded in the 1870 Census of Population.[18] Our results are not sensitive to assumptions on the geographic distribution of capital ownership because capital expenditures are a small share of total expenditure.

VI Interpretation

We estimate substantially larger economic gains from the railroads, as a share of GDP, than previous estimates of 3.2% (Donaldson and Hornbeck, 2016) or 2.7% (Fogel, 1964). Our estimated impacts on national aggregate productivity supplement those previous estimates: we would estimate no impact on national aggregate productivity if there were no differences between counties' value marginal product of inputs and their marginal cost (as assumed by Fogel (1964) and Donaldson and Hornbeck (2016)), whereas the economy would still benefit from the railroads decreasing resources spent on transportation (as in Fogel 1964) or economic gains capitalized in land values (as in Donaldson and Hornbeck, 2016).

[18] We aggregate values at the county level using the digitized complete-count data (Ruggles et al., 2021), and then use each county's share of reported personal wealth as its (fixed) share of capital ownership.

Our analysis starts with the manufacturing sector and extends this analysis to the broader economy, whereas Fogel (1964) and Donaldson and Hornbeck (2016) start with the agricultural sector and extend their analyses to the broader economy. In considering impacts on the broader economy, the key difference in our approaches is where those economic gains will appear: for Fogel (1964), the benefits from railroads are confined to the transportation sector through savings in transportation costs; for Donaldson and Hornbeck (2016), the aggregate impacts are capitalized in land values. In our model that allows for market distortions, the difference between output value and input costs is not capitalized in land values and so there can also be impacts of the railroads on national aggregate productivity that are not captured by changes in total land value.

The differences in our estimates to those in Donaldson and Hornbeck (2016) are not primarily driven by our use of manufacturing data to estimate different production function elasticities or the inclusion of traded intermediate inputs. For Donaldson and Hornbeck (2016), the aggregate economic losses from removing the railroad network are capitalized in lower aggregate land values that generate annual economic losses equal to 3.2% of GNP. We estimate that removing the railroads would generate similar declines in land values, generating annual losses equal to 3.5% of GNP. Our estimated population loss from holding utility fixed (65.8%) is also comparable to Donaldson and Hornbeck (2016)'s estimate (58.4%). This aggregate population decline, and the reallocation of economic activity across counties, has much greater economic impact in our analysis, however, because we allow the marginal product of inputs to be greater than their marginal cost differentially across counties. Changes in input-use, without the railroads, then generate substantial aggregate productivity losses that are not capitalized in lower land values.

One general implication for measuring the economic incidence of new infrastructure or new technologies is that increased payments to land (or labor or capital) do not include all economic gains when there are market distortions. We show that these additional economic gains can be substantively large, particularly when new infrastructure or new technologies are broadly used and encourage substantial expansion of economic activity. As in Baqaee and Farhi (2020), TFP growth in one sector (transportation) can increase production in other sectors that were inefficiently small and thereby generate larger aggregate productivity gains than implied by the Domar-weighted increase in transportation sector TFP.

The railroads decreased transportation costs, effectively subsidizing the expansion of economic activities throughout the economy that had a positive social return (i.e., activities whose value marginal product exceeded their marginal cost). The more that economic activity expands in response to decreased transportation costs, the greater the aggregate economic gains, which is opposite to the intuition of Fogel (1964, 1979) in which the railroads' impacts were supposed to be bounded above by assuming an inelastic demand for transportation.

We do not find that railroads reduced market distortions, whether due to firm markups, borrowing constraints, or other inefficiencies, but the railroads generated substantial national aggregate productivity gains by encouraging the expansion of an economy with market distortions. There would also be large potential gains from reducing distortions (as in Restuccia and Rogerson, 2008; Hsieh and Klenow, 2009): estimating a counterfactual that removes all input distortions, and maintaining the railroad network in 1890, we estimate that national aggregate productivity would increase by 110% (holding worker utility fixed) or by 30% (holding total population fixed). But market integration need not decrease market distortions; indeed, estimated distortions in modern US data are similar to this historical era, such that there continue to be large potential aggregate productivity gains from new infrastructure or technologies that would further increase input-use.

The same aggregate productivity gains accrue whether market distortions are due to market power or other inefficiencies. Aggregate productivity increases as inputs are reallocated to firms with more market power, even if that increases the average markup in the economy. Even if high-markup firms are not especially productive, in a physical sense, their high price means that consumers on the margin would value more of that good than the goods being produced with those inputs by lower-markup firms. Further, aggregate productivity increases when inputs move to higher marginal productivity firms even if those firms are less productive on average.

We do not find that the railroads increased county TFPR, and we hold counties' technical efficiency fixed in our counterfactual estimates. Some specifications indicate a larger impact of market access on county TFPR, and our estimated impacts on county TFPR may also understate impacts on physical productivity, and future research can explore impacts of market access on firm-level production decisions and physical productivity. Our estimated increases in county-level production are associated with substantial increases in the number of establishments, with little change in average establishment size, which relates to a literature highlighting the role of entry in aggregate productivity growth (Foster, Haltiwanger and Krizan, 2001; Foster, Haltiwanger and Syverson, 2008). The railroads were also associated with increased patenting activity, though in part through encouraging the filing of lower-quality patents (Perlman, 2017).

Increases in national aggregate productivity are not synonymous with increases in welfare, given any social welfare function, but increases in the difference between total output value and total input costs (aggregate productivity) represent additional resources that society may consume and so are closely associated with increases in welfare (Solow, 1957; Weitzman, 1976; Basu and Fernald, 2002). There is additional surplus in society when the value of output increases by more than the cost of inputs, but we do not consider the distribution of that surplus across people and how that might be weighted. We report substantial losses in national aggregate productivity without the railroads, holding fixed worker utility (real wages), but we also report substantial losses in worker welfare when total population is held fixed or partially restricted in the counterfactual.

VII Conclusion

We estimate that the railroads drove substantial aggregate economic gains in the United States, playing a central role in the economy's growth through the latter half of the 19th century. The railroads integrated domestic markets within the United States, shifting economic activity across counties and increasing aggregate economic activity. We estimate that increases in county aggregate productivity were mostly driven by increases in county AE (allocative efficiency): input-use increased substantially in counties where the value marginal product of inputs was greater than their marginal cost, increasing the value of output more than the value of inputs even if holding fixed county TFPR (revenue total factor productivity).

We emphasize that new technologies or new infrastructure can be particularly impactful when there are market distortions in the economy, such that economic activity increases in places where the value marginal product of inputs is greater than their marginal cost. These potential economic gains are largest when the economy is most inefficient; that is, with great problems come great possibilities.

We find that the railroads generated large indirect economic gains, outside the transportation sector, by increasing marginally productive activities in other sectors. These indirect economic gains were substantially larger than the direct gains from decreased resources spent on transportation itself (i.e., the "social savings" proposed by Fogel 1964) or gains capitalized in land values (as in Donaldson and Hornbeck 2016). The railroads generated large indirect gains because they encouraged a substantial expansion of economic activity in the United States, and this same mechanism would apply to a variety of new technologies or infrastructure investments that encourage the substantial expansion of other activities that have value marginal product greater than marginal cost.

Our counterfactual analysis does not include impacts of the railroads on physical productivity (technical efficiency), or ways in which local or aggregate technological innovation might respond to increases in market access. Further research could use more-detailed firm-level data to explore impacts of market access on technical efficiency, firm-level specialization, technology adoption, and other ways in which market integration could further increase local and aggregate productivity.

We also do not consider a variety of other mechanisms through which railroads may have impacted the US economy. Our analysis does not consider how the construction and operation of the railroads may have directly affected the economy, such as through the development of improved management practices (Chandler, 1965). We also do not consider how the railroads may have impacted worker mobility, both across counties and within urban areas. The railroads encouraged certain economic activities to agglomerate in major urban centers, with potential benefits from urbanization (Haines and Margo, 2008) and particular gains in major cities (Cronon, 2009). Our empirical analysis complements city histories, examining how a broad range of counties were induced to grow by the railroads and increases in market access. The railroads shifted economic activity from some counties to others, along with increasing aggregate economic activity in the United States, and these effects combined to generate both local productivity gains and substantial national aggregate productivity gains.

References

Abramovitz, Moses, and Paul A David. 1973. "Reinterpreting economic growth: parables and realities." *American Economic Review*, 63(2): 428–439.

Adams, Henry C. 1895. *Report on transportation business in the United States at the eleventh census: 1890.* Vol. 1, US GPO.

Allen, Treb, and Costas Arkolakis. 2022. "The welfare effects of transportation infrastructure improvements." *The Review of Economic Studies*, 89(6): 2911–2957.

Allen, Treb, and Costas Arkolakis. 2023. "Economic Activity across Space: A Supply and Demand Approach." *Journal of Economic Perspectives*, 37(2): 3–28.

Allen, Treb, and Dave Donaldson. 2022. "Persistence and path dependence in the spatial economy." *Working Paper.*

Andrews, Isaiah. 2018. "Valid two-step identification-robust confidence sets for GMM." *Review of Economics and Statistics*, 100(2): 337–348.

Asker, John, Allan Collard-Wexler, and Jan De Loecker. 2014. "Dynamic inputs and resource (mis) allocation." *Journal of Political Economy*, 122(5): 1013–1063.

Asturias, Jose, Manuel García-Santana, and Roberto Ramos. 2019. "Competition and the welfare gains from transportation infrastructure: Evidence from the Golden Quadrilateral of India." *Journal of the European Economic Association*, 17(6): 1881–1940.

Atack, Jeremy. 1977. "Returns to scale in antebellum United States manufacturing." *Explorations in Economic History*, 14(4): 337.

Atack, Jeremy, Fred Bateman, Michael Haines, and Robert A Margo. 2010. "Did railroads induce or follow economic growth? Urbanization and population growth in the American Midwest, 1850–1860." *Social Science History*, 34(2): 171–197.

Atkin, David, and Dave Donaldson. 2015. "Who's Getting Globalized? The Size and Implications of Intra-national Trade Costs."

Bai, Yan, Keyu Jin, and Dan Lu. 2023. "Misallocation under trade liberalization." *Working Paper.*

Balboni, Clare. 2021. "In Harm's Way? Infrastructure Investments and the Persistence of Coastal Cities." *Working Paper.*

Baqaee, David, and Emmanuel Farhi. 2019*a*. "The macroeconomic impact of microeconomic shocks: beyond Hulten's Theorem." *Econometrica*, 87(4): 1155–1203.

Baqaee, David, and Emmanuel Farhi. 2019*b*. "A Short Note on Aggregating Productivity."

Baqaee, David, and Emmanuel Farhi. 2020. "Productivity and misallocation in general equilibrium." *Quarterly Journal of Economics*, 135(1): 105–163.

Basu, Susanto, and John G Fernald. 2002. "Aggregate productivity and aggregate technology." *European Economic Review*, 46(6): 963–991.

Bazzi, Samuel, Martin Fiszbein, and Mesay Gebresilasse. 2020. "Frontier culture:

The roots and persistence of "rugged individualism" in the United States." *Econometrica*, 88(6): 2329–2368.

Becker, Randy, Wayne Gray, and Jordan Marvakov. 2013. "NBER-CES manufacturing industry database: Technical notes." *NBER Working Paper*, 5809.

Berthou, Antoine, Jong Hyun Chung, Kalina Manova, and Charlotte Sandoz Dit Bragard. 2020. "Trade, Productivity, and (Mis)allocation."

Blackwood, G Jacob, Lucia S Foster, Cheryl A Grim, John Haltiwanger, and Zoltan Wolf. 2021. "Macro and Micro Dynamics of Productivity: From Devilish Details to Insights." *American Economic Journal: Macroeconomics*, 13(3): 142–72.

Borusyak, Kirill, and Peter Hull. Forthcoming. "Non-Random Exposure to Exogenous Shocks: Theory and Applications." *Econometrica*.

Caliendo, Lorenzo, and Fernando Parro. 2015. "Estimates of the Trade and Welfare Effects of NAFTA." *Review of Economic Studies*, 82(1): 1–44.

Caliendo, Lorenzo, Fernando Parro, Esteban Rossi-Hansberg, and Pierre-Daniel Sarte. 2018. "The impact of regional and sectoral productivity changes on the US economy." *Review of Economic Studies*, 85(4): 2042–2096.

Caliendo, Lorenzo, Robert C Feenstra, John Romalis, and Alan M Taylor. 2023. "Tariff Reductions, Heterogeneous Firms, and Welfare: Theory and Evidence for 1990–2010." *IMF Economic Review*, 1–35.

Carlson, Mark, Sergio Correia, and Stephan Luck. 2022. "The effects of banking competition on growth and financial stability: Evidence from the national banking era." *Journal of Political Economy*, 130(2): 462–520.

Caselli, Francesco, and Wilbur John Coleman. 2001. "The U.S. Structural Transformation and Regional Convergence: A Reinterpretation." *Journal of Political Economy*, 109(3): 584–616.

Chandler, Alfred D. 1965. "The railroads: pioneers in modern corporate management." *Business History Review*, 39(1): 16–40.

Cronon, William. 2009. *Nature's metropolis: Chicago and the Great West.* WW Norton & Company.

David, Paul A. 1969. "Transport Innovation and Economic Growth: Professor Fogel on and off the Rails." *Economic History Review*, 22(3): 506–524.

Domar, Evsey D. 1961. "On the measurement of technological change." *Economic Journal*, 71(284): 709–729.

Donaldson, Dave, and Richard Hornbeck. 2016. "Railroads and American economic growth: A "market access" approach." *Quarterly Journal of Economics*, 131(2): 799–858.

Duranton, Gilles, Peter M Morrow, and Matthew A Turner. 2014. "Roads and Trade: Evidence from the US." *Review of Economic Studies*, 81(2): 681–724.

Eaton, Jonathan, and Samuel Kortum. 2002. "Technology, Geography and Trade." *Econometrica*, 70(5): 1741–1779.

Eaton, Jonathan, Samuel Kortum, Brent Neiman, and John Romalis. 2016. "Trade and the global recession." *American Economic Review*, 106(11): 3401–3438.

Firth, John. 2019. "I've Been Waiting on the Railroad: The Effects of Congestion on Firm Production." *Working Paper.*

Fishlow, Albert. 1965. *American Railroads and the Transformation of the Ante-bellum Economy.* Vol. 127, Harvard University Press.

Fogel, Robert W. 1964. *Railroads and American Economic Growth: Essays in Econometric History.* Johns Hopkins University Press.

Fogel, Robert W. 1979. "Notes on the Social Saving Controversy." *Journal of Economic History*, 39: 1–54.

Foster, Lucia, John C Haltiwanger, and Cornell John Krizan. 2001. "Aggregate productivity growth: lessons from microeconomic evidence." In *New developments in productivity analysis.* 303–372. University of Chicago Press.

Foster, Lucia, John Haltiwanger, and Chad Syverson. 2008. "Reallocation, firm turnover, and efficiency: selection on productivity or profitability?" *American Economic Review*, 98(1): 394–425.

Haines, Michael R. 2010. "Historical, Demographic, Economic, and Social Data: The United States, 1790–2002." *ICPSR*, 2896.

Haines, Michael R, and Robert A Margo. 2008. "Railroads and local economic development." *Quantitative Economic History: The good of counting.*

Hall, Robert. 1988. "The relation between price and marginal cost in US industry." *Journal of Political Economy*, 96(5): 921–947.

Haltiwanger, John, Robert Kulick, and Chad Syverson. 2018. "Misallocation measures: The distortion that ate the residual."

Hanson, Gordon H. 2005. "Market potential, increasing returns and geographic concentration." *Journal of International Economics*, 67(1): 1–24.

Harberger, Arnold. 1964. "Taxation, resource allocation, and welfare." In *The role of direct and indirect taxes in the Federal Reserve System.* 25–80. Princeton University Press.

Head, Keith, and Thierry Mayer. 2011. "Gravity, market potential and economic development." *Journal of Economic Geography*, 11(2): 281–294.

Heblich, Stephan, Stephen J Redding, and Daniel M Sturm. 2020. "The making of the modern metropolis: evidence from London." *Quarterly Journal of Economics*, 135(4): 2059–2133.

Helpman, Elhanan. 1995. "The Size of Regions." *The Foerder Institute for Economic Research*, 14(95).

Hornbeck, Richard. 2010. "Barbed wire: Property rights and agricultural development." *Quarterly Journal of Economics*, 125(2): 767–810.

Hsieh, Chang-Tai, and Peter J Klenow. 2009. "Misallocation and manufacturing TFP in China and India." *Quarterly Journal of Economics*, 124(4): 1403–1448.

Hulten, Charles R. 1978. “Growth accounting with intermediate inputs.” *Review of Economic Studies*, 45(3): 511–518.

Jaremski, Matthew. 2014. “National Banking’s role in US industrialization, 1850–1900.” *Journal of Economic History*, 74(1): 109–140.

Jaremski, Matthew, and Price V Fishback. 2018. “Did inequality in farm sizes lead to suppression of banking and credit in the late nineteenth century?” *Journal of Economic History*, 78(1): 155–195.

Jaumandreu, Jordi. 2022. “The Remarkable Stability of the US Manufacturing Markups.” *Working Paper.*

Jaworski, Taylor, and Carl T. Kitchens. 2019. “National Policy for Regional Development: Historical Evidence from Appalachian Highways.” *Review of Economics and Statistics*, 101(5): 777–790.

Jorgenson, Dale Weldeau, and Zvi Griliches. 1967. “The Explanation of Productivity Change.” *Review of Economic Studies*, 34(3): 249–283.

Khandelwal, Amit K, Peter K Schott, and Shang-Jin Wei. 2013. “Trade liberalization and embedded institutional reform: Evidence from Chinese exporters.” *American Economic Review*, 103(6): 2169–95.

Kleinman, Benny, Ernest Liu, and Stephen J Redding. 2023. “Dynamic spatial general equilibrium.” *Econometrica*, 91(2): 385–424.

Lamoreaux, Naomi R. 1996. *Insider lending: Banks, personal connections, and economic development in industrial New England.* Cambridge University Press.

Levinsohn, James, and Amil Petrin. 2003. “Estimating production functions using inputs to control for unobservables.” *Review of Economic Studies*, 70(2): 317–341.

Liu, Ernest. 2019. “Industrial policies in production networks.” *Quarterly Journal of Economics*, 134(4): 1883–1948.

Manski, Charles F. 1993. “Identification of endogenous social effects: The reflection problem.” *The review of economic studies*, 60(3): 531–542.

Margo, Robert A. 2014. “Economies of Scale in Nineteenth-Century American Manufacturing Revisited: A Resolution of the Entrepreneurial Labor Input Problem.” In *Enterprising America: Businesses, Banks, and Credit Markets in Historical Perspective*. 215–244. University of Chicago Press.

Midrigan, Virgiliu, and Daniel Yi Xu. 2014. “Finance and misallocation: Evidence from plant-level data.” *American Economic Review*, 104(2): 422–458.

Olley, G. Steven, and Ariel Pakes. 1996. “The Dynamics of Productivity in the Telecommunications Equipment Industry.” *Econometrica*, 64(6): 1263–1297.

Perlman, Elisabeth. 2017. “Dense enough to be brilliant: Patents, Urbanization, and Transportation in Nineteenth Century America.” *Working Paper.*

Petrin, Amil, and James Levinsohn. 2012. “Measuring aggregate productivity growth using plant-level data.” *RAND Journal of Economics*, 43(4): 705–725.

Pope, Jesse E. 1914. “Agricultural credit in the United States.” *Quarterly Journal of Economics*, 28(4): 701–746.

Rajan, Raghuram G, and Luigi Zingales. 1998. “Financial Dependence and Growth.” *American Economic Review*, 88(3): 559–586.

Ramondo, Natalia, Andrés Rodríguez-Clare, and Milagro Saborío-Rodríguez. 2016. “Trade, domestic frictions, and scale effects.” *American Economic Review*, 106(10): 3159–3184.

Redding, Stephen, and Anthony Venables. 2004. “Economic geography and international inequality.” *Journal of International Economics*, 62(1): 53–82.

Redding, Stephen, and Daniel M. Sturm. 2007. “The Cost of Remoteness: Evidence from German Division and Reunification.” *American Economic Review*, 98(5): 1766–1797.

Redding, Stephen J, and Daniel M Sturm. 2008. “The costs of remoteness: Evidence from German division and reunification.” *American Economic Review*, 98(5): 1766–1797.

Restuccia, Diego, and Richard Rogerson. 2008. “Policy distortions and aggregate productivity with heterogeneous establishments.” *Review of Economic dynamics*, 11(4): 707–720.

Rotemberg, Martin, and T Kirk White. 2021. “Plant-to-Table(s and Figures): Processed Manufacturing Data and Measured Misallocation.” *Working Paper.*

Ruggles, Steven, Ronald Goeken, David J. Hacker, Evan Roberts, Megan Schouweiler, and Matthew Sobek. 2021. “IPUMS Ancestry Full Count Data: Version 3.0.” *Minneapolis, MN: IPUMS.*

Singer, Gregor. 2019. “Endogenous markups, input misallocation and geographical supplier access.”

Sokoloff, Kenneth L. 1984. “Was the transition from the artisanal shop to the nonmechanized factory associated with gains in efficiency? Evidence from the US Manufacturing censuses of 1820 and 1850.” *Explorations in Economic History*, 21(4): 351–382.

Solow, Robert M. 1957. “Technical change and the aggregate production function.” *Review of Economics and Statistics*, 312–320.

Sotelo, Sebastian. 2020. “Domestic trade frictions and agriculture.” *Journal of Political Economy*, 128(7): 2690–2738.

Sun, Liyang. 2018. “Implementing valid two-step identification-robust confidence sets for linear instrumental-variables models.” *The Stata Journal*, 18(4): 803–825.

Święcki, Tomasz. 2017. “Intersectoral distortions and the welfare gains from trade.” *Journal of International Economics*, 104: 138–156.

Tombe, Trevor, and Xiaodong Zhu. 2019. “Trade, migration, and productivity: A quantitative analysis of china.” *American Economic Review*, 109(5): 1843–72.

Towne, Marvin, and Wayne Rasmussen. 1960. “Farm Gross Product and Gross Investment in the Nineteenth Century.” In *Trends in the American Economy in the Nineteenth Century*. 255–316. NBER.

United States Census Bureau. 1975. *Historical Statistics of the United States, Colonial Times to 1970.* US Department of Commerce, Bureau of the Census.

Weitzman, Martin L. 1976. "On the welfare significance of national product in a dynamic economy." *Quarterly Journal of Economics*, 90(1): 156–162.

Xu, Chenzi, and He Yang. 2022. "Real effects of stabilizing private money creation." *CEPR Discussion Papers*, 2(17164).

Yang, Yang. 2018. "Transport infrastructure, city productivity growth and sectoral reallocation: Evidence from China." *Working Paper*, 18(276).

Zárate, Román David. 2022. "Factor Allocation, Informality and Transit Improvements: Evidence from Mexico City." *Working Paper.*

Ziebarth, Nicolas L. 2013. "Are China and India backward? Evidence from the 19th century US Census of Manufactures." *Review of Economic Dynamics*, 16(1): 86–99.

A Data Appendix

A.1 County-Industry Manufacturing Data

We have digitized manufacturing data, by county and industry, for 1860, 1870, and 1880 from the original published tabulations of the Census of Manufactures (United States Census Bureau, 1860*b*, 1870, 1880). In 1860, the Census of Manufactures also collected information for enterprises outside of manufacturing (fisheries and mining) that we drop from our analysis for consistency.

The county-industry data report many industries in each decade, with some small variations, which we concord for our analysis. We homogenized industry names from each county to the list of industry names from US-industry tabulations in each decade: 331 names in 1880, 412 names in 1870, and 639 names in 1860 (for a total of over 1100 distinct names). We then grouped these industries into 193 categories that were more consistent across decades, and further grouped these industries into 31 categories. Our estimates are not sensitive to these industry groupings (Table 2), but our goal was to balance industry-level details against statistical noise and to maintain comparability across decades and geographic areas.

Starting in 1870, the county-by-industry data do not list some "neighborhood industries" such as blacksmithing (Atack and Margo, 2019) or additional industries with less than \$10,000 of revenue in total. We define a residual industry to capture the difference between county-level data and the summed county-by-industry data, and include this residual industry in our analysis. This residual "industry" includes less than 5% of manufacturing revenue in 1870 and 1880. For our county-industry results, the most relevant reason for a "residual" industry was that small producers of local products, such as many grist mills, were not included in the county-industry tabulations. We also created an "other" industry, representing less than 1% of revenue, reflecting named but small industries not otherwise classified.

These manufacturing data were collected by Census enumerators, who visited each manufacturing establishment to solicit responses. The Census then published aggregated statistics, including county-by-industry cells that contain only one manufacturing establishment (in 1860, 1870, 1880). For multi-industry establishments, such as grist & lumber mills, the Census would "[separate] the two parts of the business and [assign] each to its appropriate place in the Statistics of Industries" (United States Census Bureau, 1860*b*). We often refer to "firms" for convenience, though note that the Census enumeration is at the establishment level and activity is recorded where it takes place, not at headquarters, so this refers to single-establishment "firms."

The 1860 Census instructions to enumerators discuss the data collection guidelines in useful detail, which we quote below, and there is similar language in the instructions for other decades. Prior to 1850, there are greater concerns about the comprehensiveness of the data collection and the Census data collection was professionalized in 1850 (Atack and Bateman, 1999).

Our main variables of interest, from the manufacturing data, are:

Manufacturing Revenue (R). Total value of products, by county and industry from 1860, 1870, and 1880. These products were valued at the factory gate, excluding transportation costs to customers: "In stating the value of the products, the value of the articles *at the place of manufacture* is to be given, exclusive of the cost of transportation to any market" (emphasis original, United States Census Bureau, 1860*a*).

Manufacturing Materials Expenditure (E^M). Total value of materials, by county and industry from 1860, 1870, and 1880. These materials were valued at the factory gate, including transportation costs from suppliers: "this value is always to represent the cost of the article *at the place where it is used*" (emphasis original, United States Census Bureau, 1860*a*). Materials included fuel and "the articles used for the production of a manufacture," which the instructions noted might be manufactured by another establishment. Unused materials (on June 1) were to be excluded.

Manufacturing Labor Expenditure (E^L). Total amount paid in wages during the year, by county and industry from 1860, 1870, and 1880. Reported wages were intended to reflect total labor costs, including boarding costs paid in kind and the proprietor's own labor. From the Census instructions: "In all cases when the employer boards the hands, the usual charge of board is to be added to the wages, so that *cost of labor* is always to mean the amount paid, whether in money or partly in money and partly in board..." (emphasis original) and to be included was "the individual labor of a producer, working on his own account" (United States Census Bureau, 1860*a*). The measurement of labor costs raises some challenges, particularly in the treatment of owner-operator labor (Weeks, 1886), and Appendix B shows the robustness of our results to inflating measured labor costs to account for potential under-measurement of owners' labor.

Manufacturing Capital Expenditure (E^K). We impute annual capital expenditure by multiplying the reported total value of capital invested, in each county and industry (1860, 1870, 1880), by a state-specific mortgage interest rate that varies between 5.5% and 11.4%, with an average value of 8% (Fogel, 1964).[19] The establishment's capital value was directed to include "capital invested in real and personal estate in the business" (United States Census Bureau, 1860*a*). The measurement of capital is challenging, particularly in distinguishing

[19]The mortgage interest rates are similar to the antebellum returns to equity collected by Bodenhorn and Rockoff (1992). They are also around the implied interest rate currently used by the BLS to convert capital stocks to the flow value of capital services, when only considering assets that existed in the 19th century such as buildings, land, and steam equipment (Cunningham et al., 2021).

between nominal and resale values and potential non-reporting of rented land and equipment.[20] Appendix B reports that our estimates are not sensitive to alternative approaches to adjusting for measurement error in capital, in part because the annual cost of capital is substantially smaller than labor and materials expenditures and because the estimated percent impacts on capital expenditures are similar to the estimated percent impacts on labor and material expenditures.

Manufacturing Establishment Counts. The number of establishments in each county and industry (1860, 1870, 1880) with at least $500 in annual sales. The Census enumerators were instructed to survey every manufacturing establishment, except "household manufactures or small mechanical operations where the annual productions do not exceed five hundred dollars" (United States Census Bureau, 1860*a*). When multiple establishments were owned by the same party, and operated jointly, then Census enumerators were instructed to obtain separate details on the operations of each establishment. If this were impossible, particularly when one establishment manufactured the materials for the other establishment, then enumerators were instructed to "return the last manufacture, giving the raw materials for the first, and capital, fuel, and cost of labor, with the number of hands, in both" (United States Census Bureau, 1860*a*).

Civil War Related Industries. We coded two sets of industries as being "Civil War related." Our strict classification includes: artificial limbs and surgical appliances; awnings and tents; coffins; cutlery, edge tools, and axes; drugs; chemicals and medicines; explosives and fireworks; flags and banners; gun- and lock-smithing; gunpowder; lead; military goods; and ship and boat building. Our broad classification adds: bronze; canning and preserving; carriage and wagon materials; carriages and wagons; clothing (general); cooperage; gloves and mittens; and hats and caps.

A.2 Main Outcomes

The table below is a reference for the formulas used in calculating county productivity and its components. We use an upper bar to denote averages over the sample period. County-level values of revenue and input expenditures in each year reflect a sum of county-industry values in that year.

[20] Rented-in capital has only been irregularly collected in the modern Annual Survey of Manufactures, but Cunningham et al. 2021 report that it is a small share of total capital in years when measured.

Component	Formula	Notes
Revenue	R_{ct}	Gate value of revenue in the Census.
Capital	E_{ct}^K	Book value of capital in the Census, multiplied by interest rate.
Labor	E_{ct}^L	Wage bill in the Census.
Materials	E_{ct}^M	Gate value of materials in the Census.
s_{ct}^k	$\frac{E_{ct}^k}{R_{ct}}$	Revenue share of input k in county c in year t, with s_c^k representing the average across years.
α_{ct}^k	$\sum\limits_i \frac{R_{cit}}{\sum\limits_j R_{cjt}} \frac{\sum\limits_c E_{cit}^k}{\sum\limits_c \sum\limits_\ell E_{cit}^\ell}$	County-level revenue share weighted sum of input's national industry cost share
ν_c	$\frac{1}{1-\left(\frac{1}{C}\sum\limits_c \sum\limits_k s_c^k\right)}$	Used for re-scaling percent growth in county revenue into percent growth in county productivity.
Productivity	$\nu_c \left[R_{ct} - \sum\limits_k s_c^k \ln E_{ct}^k\right]$	
TFPR	$\nu_c \left[R_{ct} - \sum\limits_k \alpha_c^k \ln E_{ct}^k\right]$	
Allocative Efficiency (AE)	$\nu_c \left[\left(\alpha_c^k - s_c^k\right) \ln E_{ct}^k\right]$	
Productivity Robustness: County Scalar	$\nu_c \left[P_{ct}Q_{ct} - \sum\limits_k s_c^k \ln W_{ct}^k X_{ct}^k\right]$	$\nu_c = \frac{1}{1-\left(\sum\limits_k s_c^k\right)}$ Drop counties with negative scalar values and top 1% of values
Productivity Robustness: Median Scalar	$\nu_c \left[P_{ct}Q_{ct} - \sum\limits_k \widetilde{s_c^k} \ln W_{ct}^k X_{ct}^k\right]$	$\nu_c = \frac{1}{1-\left(\frac{1}{C}\sum\limits_c \sum\limits_k \widetilde{s^k}\right)}$ $\widetilde{s_c^k}$ is the median revenue share for k in county c, $\widetilde{s^k}$ is its national median
Productivity Robustness: 1860 Scalar	$\nu_c \left[P_{ct}Q_{ct} - \sum\limits_k s_{c1860}^k \ln W_{ct}^k X_{ct}^k\right]$	$\nu_c = \frac{1}{1-\left(\frac{1}{C}\sum\limits_c \sum\limits_k s_{c1860}^k\right)}$ We also use α_{c1860}^k for decomposing into TFPR and AE

A.3 Other County-Level Data

For some specifications using manufacturing data from 1890 and 1900, when county-industry tabulations are unavailable, we use the corresponding county-level data (Haines, 2010). For 1850, the only values aggregated and published at the county level were manufacturing revenue and capital. Other county-level data are from the United States Census of Population and Census of Agriculture (Haines, 2010).

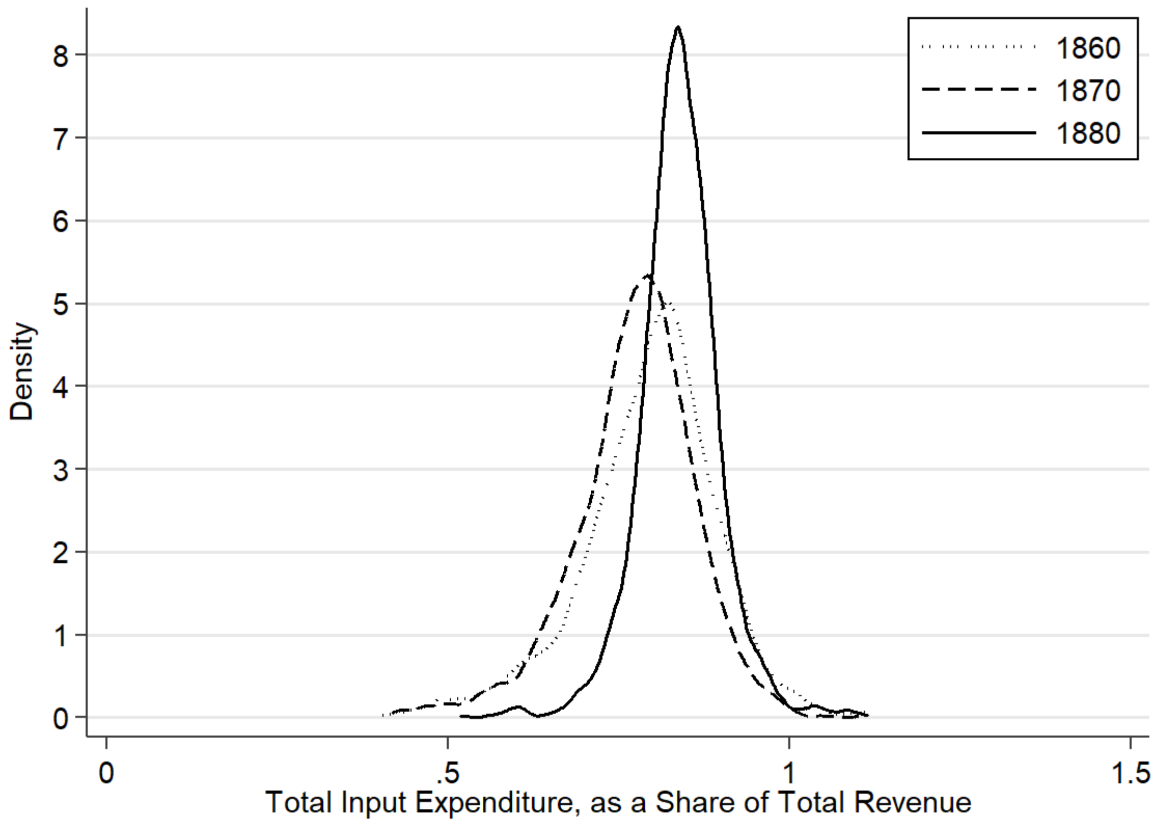
Population is defined as the reported total population in each county. In Appendix B, we inflate these population data due to potential undercounting in the Census that is estimated to vary by region and year: undercounting in the South by 7.6% in 1860, 8.8% in 1870, and 5.2% in 1880, and undercounting in the North by 5.6% in 1860, 6.0% in 1870, and 4.4% in 1880 (Hacker, 2013).

Agricultural land value is defined as the total value of land in farms, including the value of farm buildings and improvements. We follow Donaldson and Hornbeck (2016) in deflating these reported data, using Fogel's state-level estimates of the value of agricultural land only (Fogel, 1964, pp. 82-83).

We adjust county-level data to maintain consistent county definitions in each decade. We adjust data from each decade to reflect county boundaries in 1890 following the procedure outlined by Hornbeck (2010). Using historical United States county boundary files (from NHGIS), county borders in each decade are intersected with county borders in 1890. When counties in another decade fall within more than one 1890 county, data for each piece are calculated by multiplying that decade's county data by the share of its area in the 1890 county. For each other decade, each 1890 county is then assigned the sum of all pieces falling within its area. This procedure assumes that data are evenly distributed across county area, though for most counties in each decade there is little overlap with a second 1890 county. In three instances, we combine separately reported cities into a neighboring county for consistency: Baltimore City is combined into Baltimore County; St. Louis City is combined into St. Louis County; and Washington DC is combined into Montgomery County.

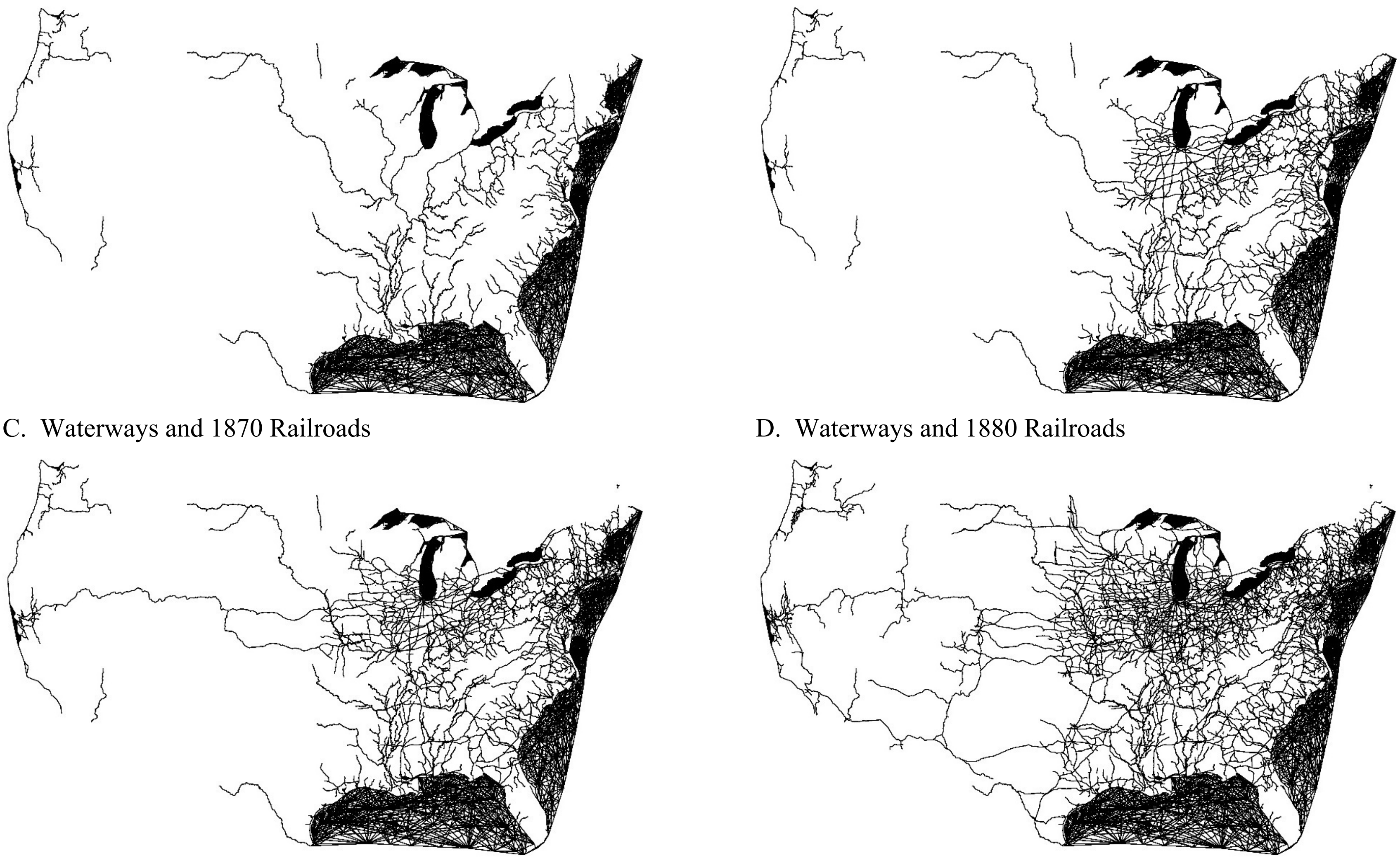
The regression sample is 1,802 counties that report county-industry manufacturing data in 1860, 1870, and 1880 (see Figure 3). The counterfactual sample is 2,722 counties with positive population and positive agricultural or manufacturing revenue in 1890 (see Figure 4).

Figure 1. Cross-County Dispersion in Expenditure as a Share of Total Revenue, by Decade



Notes: This figure plots the cross-county dispersion in county total input expenditure as a share of county total revenue ($\sum_k s_c^k$), by decade. Each observation is a county-decade.

Figure 2. Waterways and Railroads, by Decade



A. Waterways

B. Waterways and 1860 Railroads

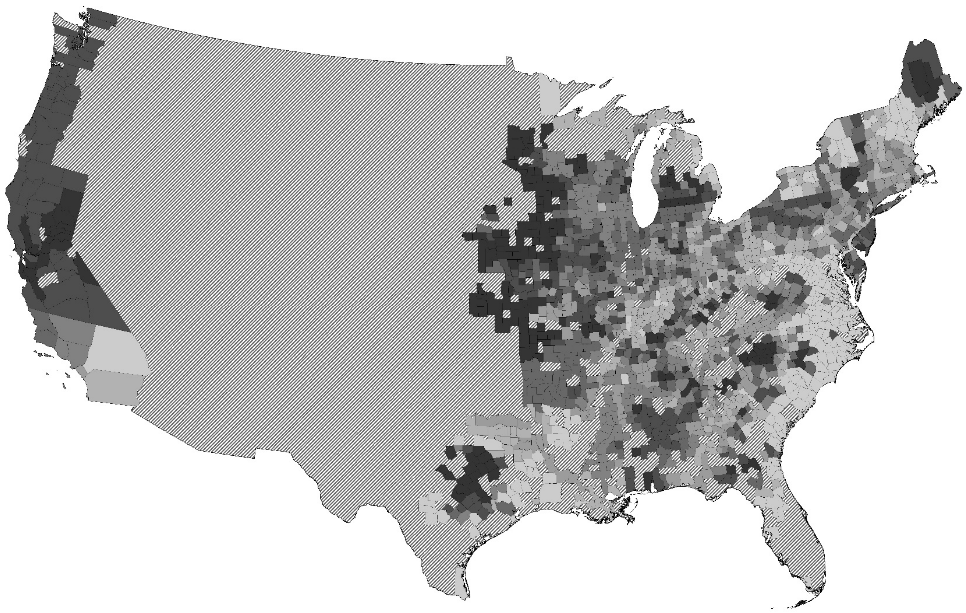
C. Waterways and 1870 Railroads

D. Waterways and 1880 Railroads

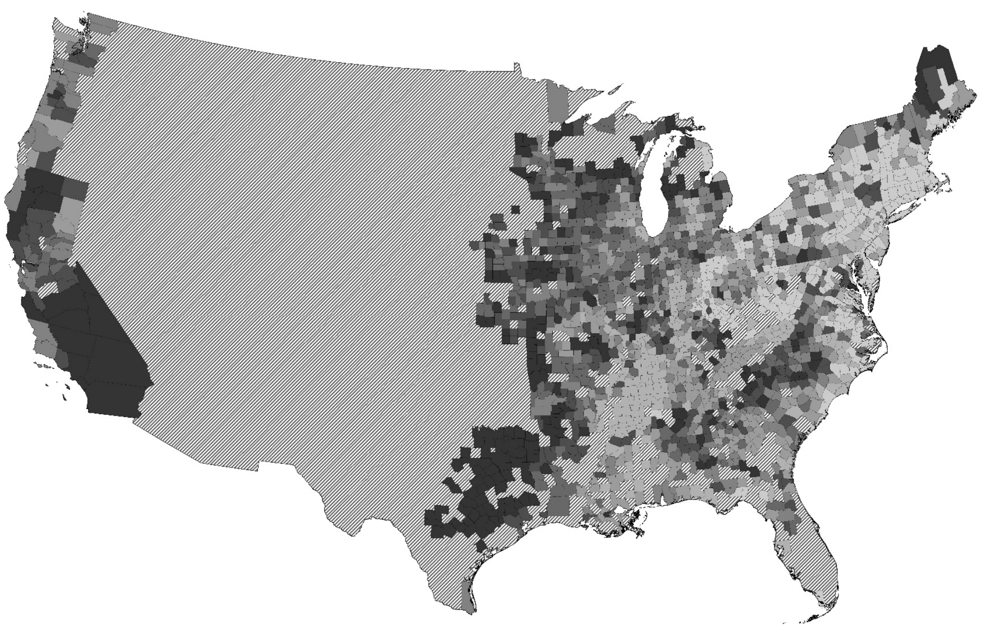
Notes: Panel A shows the waterway network: natural waterways (including navigable rivers, lakes, and oceans) and constructed canals. Panel B adds railroads constructed by 1860, Panel C adds railroads constructed between 1860 and 1870, and Panel D adds railroads constructed between 1870 and 1880.

Figure 3. Calculated Changes in Log Market Access, by County

A. From 1860 to 1870

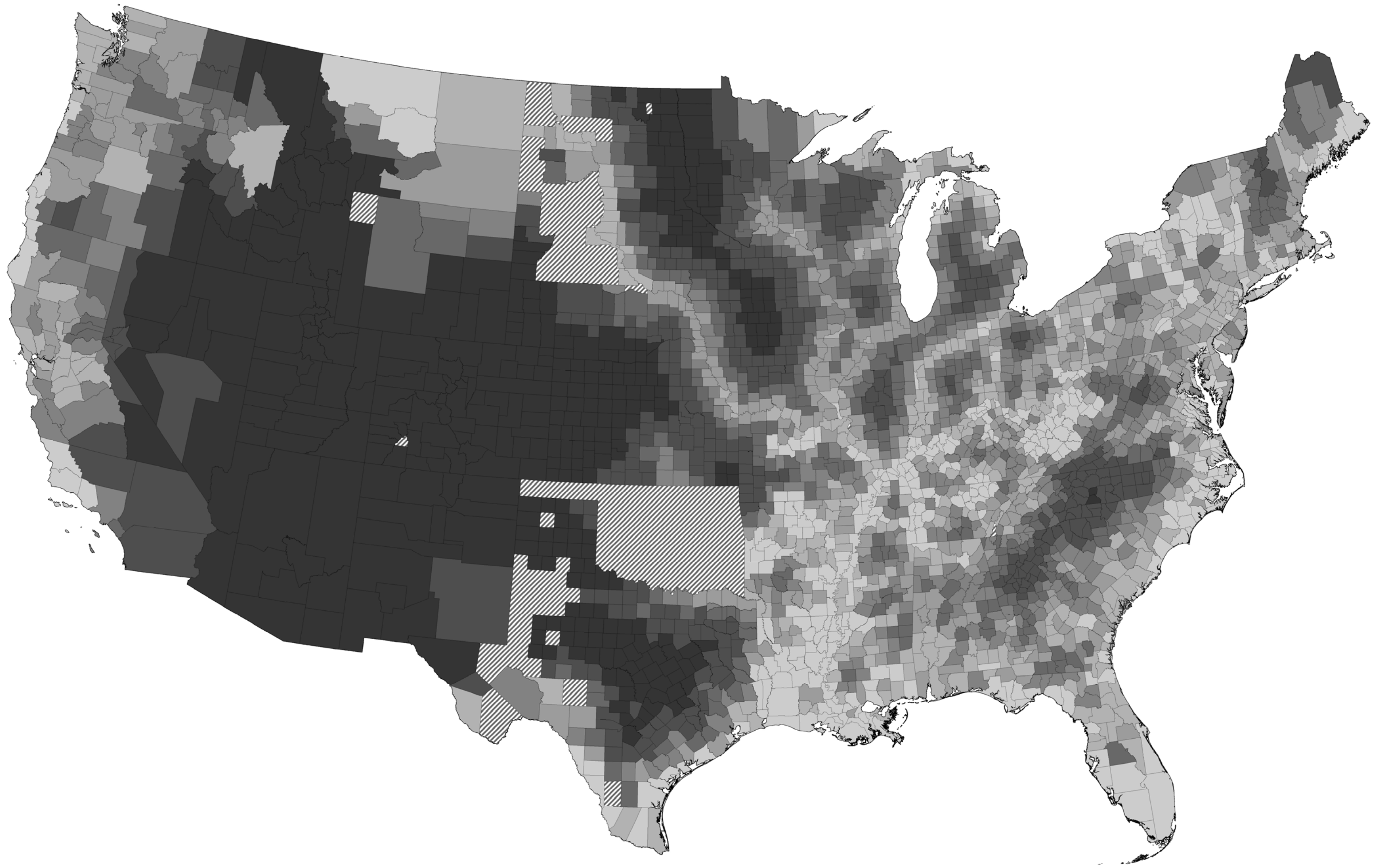


B. From 1870 to 1880



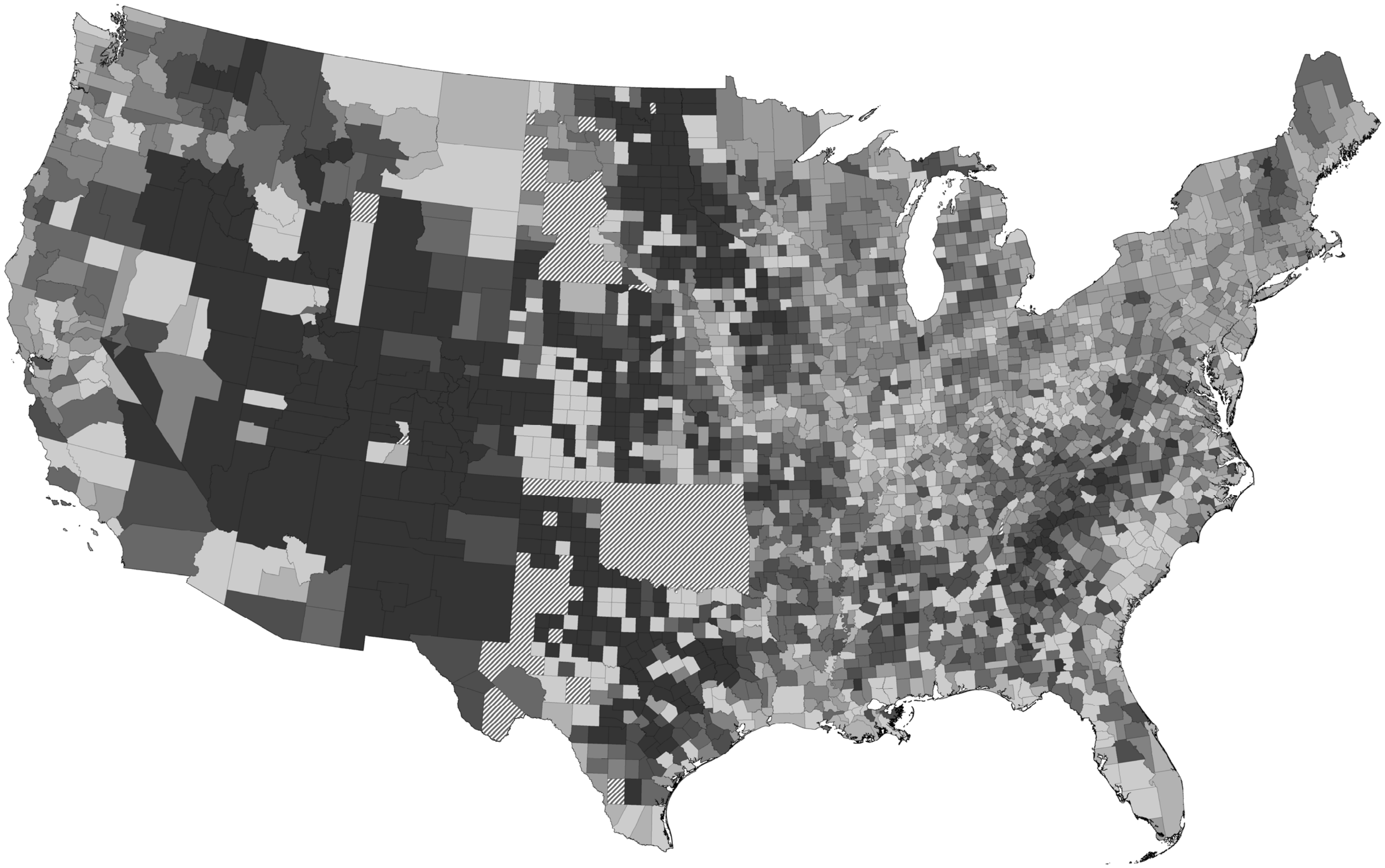
Notes: In each Panel, counties are shaded according to their calculated change in market access from 1860 to 1870 (Panel A) and from 1870 to 1880 (Panel B). Counties are divided into seven groups (with an equal number of counties per group), and darker shades denote larger increases in market access. These maps include the 1,802 sample counties in the regression analysis, which are all counties that report non-zero manufacturing activity from 1860, 1870, and 1880. The excluded geographic areas are cross-hashed. County boundaries correspond to county boundaries in 1890.

Figure 4. Counterfactual Changes in Market Access, by County



Notes: This map shows counties shaded according to their change in market access from 1890 to the baseline counterfactual scenario without railroads and where population is allowed to decline: darker shades denote larger declines in market access, and counties are divided into seven equal groups. This counterfactual sample includes all 2,722 counties that in 1890 report positive population and positive revenue (agriculture and/or manufacturing). The excluded geographic areas are cross-hashed. County boundaries correspond to county boundaries in 1890.

Figure 5. Counterfactual Changes in Productivity, by County



Notes: This map shows counties shaded according to their change in productivity from 1890 to the baseline counterfactual scenario without railroads and where population is allowed to decline: darker shades denote larger declines in productivity, and counties are divided into seven equal groups. This counterfactual sample includes all 2,722 counties that in 1890 report positive population and positive revenue (agriculture and/or manufacturing). The excluded geographic areas are cross-hashed. County boundaries correspond to county boundaries in 1890.

Table 1. Impacts of Market Access on County Revenue, Input Expenditure, and Productivity

	Baseline Specification	Holding Population Fixed at 1860 Levels	County-level Data Only: 1860 to 1900	County-level Data Only: 1850 to 1900
	(1)	(2)	(3)	(4)
Panel A. Log Revenue				
Log Market Access	0.192	0.185	0.257	0.235
	(0.049)	(0.047)	(0.061)	(0.056)
Panel B. Log Capital Expenditure				
Log Market Access	0.158	0.152	0.225	0.208
	(0.053)	(0.051)	(0.060)	(0.055)
Panel C. Log Labor Expenditure				
Log Market Access	0.196	0.187	0.292	
	(0.061)	(0.059)	(0.068)	
Panel D. Log Materials Expenditure				
Log Market Access	0.183	0.176	0.242	
	(0.050)	(0.048)	(0.062)	
Panel E. Log Productivity				
Log Market Access	0.204	0.196	0.279	
	(0.051)	(0.049)	(0.057)	
Number of Counties	1,802	1,802	1,802	1,437
County-Year Obs.	5,406	5,406	9,010	8,622

Notes: Column 1 reports estimates from equation 4: for the indicated outcome variable in each panel, we report the estimated impacts of log market access (as defined in equation 1), controlling for county fixed effects, state-by-year fixed effects, and year-interacted cubic polymials in county latitude and longitude. The sample is a balanced panel of 1,802 counties in columns 1, 2, and 3, and a balanced panel of 1,437 counties in column 4.

The outcome variables are: the log of total county manufacturing annual revenue (Panel A); the log of total county manufacturing annual expenditures on capital, labor, and materials (Panels B, C, D); and the log of total county manufacturing revenue minus the weighted logs of total county manufacturing expenditures on capital, labor, and materials (where those weights are the county's average revenue share for that input, and the variable is scaled by the ratio of average county revenue to average county productivity, as defined in equation 2).

In each column, we report the estimated impact of a one standard deviation greater change in market access from 1860 to 1880 (e.g., the coefficient in column 1, panel E, can be interpreted as a relative productivity increase of 20.4% for counties with a one standard deviation greater change in market access from 1860 to 1880). This one standard deviation greater increase in market access corresponds to a 24% greater increase in market access from 1860 to 1880 for our baseline definition of market access.

In Column 2, we calculate market access holding county populations fixed at their 1860 levels, so the only changes in market access comes from changes in the transportation network. Columns 3 and 4 use county-level data only, rather than county-by-industry data, which only affects the definition of Log Productivity in Panel E. Column 3 reports estimates for the 1860 to 1900 period, and Column 4 reports estimates for the 1850 to 1900 period using available data on county revenue and county capital expenditures in 1850.

Robust standard errors clustered by state are reported in parentheses.

Table 2. Impacts on County Productivity, Decomposed into TFPR and AE

	Baseline Specification	Detailed Industry Groups	County-level Data Only	
			1860 to 1880	1860 to 1900
	(1)	(2)	(3)	(4)
Panel A. Log County Productivity				
Log Market Access	0.204	0.204	0.204	0.279
	(0.051)	(0.051)	(0.051)	(0.057)
Panel B. County TFPR (Revenue Total Factor Productivity)				
Log Market Access	0.036	0.038	0.038	0.020
	(0.025)	(0.025)	(0.026)	(0.026)
Panel C. County AE (Allocative Efficiency)				
Log Market Access	0.168	0.166	0.166	0.258
	(0.051)	(0.052)	(0.054)	(0.067)
Number of Counties	1,802	1,802	1,802	1,802
County-Year Obs.	5,406	5,406	5,406	9,010

Notes: Column 1, panel A, corresponds to the estimate reported in Panel E of Column 1 in Table 1. Column 1 reports estimates from equation 4: for the indicated outcome variable in each panel, we report the estimated impacts of log market access (as defined in equation 1), controlling for county fixed effects, state-by-year fixed effects, and year-interacted cubic polymials in county latitude and longitude. The sample is a balanced panel of 1,802 counties (1860, 1870, 1880).

Panel A reports the estimated impacts on log county productivity (as in Panel E of Table 1), and Panels B and C report the impacts on productivity through changes in county TFPR (revenue total factor productivity) and through changes in county AE (allocative efficiency) as defined in equation 3.

In each column, we report the estimated impact of a one standard deviation greater change in market access from 1860 to 1880 (e.g., the coefficient in column 1, panel A, can be interpreted as a relative productivity increase of 20.4% for counties with a one standard deviation greater change in market access from 1860 to 1880). The coefficients in panel B and panel C imply 3.6% county productivity growth through increases in county TFPR and 16.8% county productivity growth through increases in county AE.

Column 2 calculates the outcome variables using county-by-industry data based on 193 industry categories, rather than the 31 industry categories used in column 1. Columns 3 and 4 calculate the outcome variables using county-level data, rather than county-by-industry data, for the same period from 1860 to 1880 (in column 3) and an extended period from 1860 through 1900 (in column 4).

Robust standard errors clustered by state are reported in parentheses.

Table 3. Sources of Growth in County Allocative Efficiency (AE)

	Allocative Efficiency by Input	County Input Gap	County Input Wedge	County Input Cost Share	County Std. Dev. of Wedges
	(1)	(2)	(3)	(4)	(5)
Panel A. Capital					
Log Market Access	-0.004	0.001	0.022	-0.0004	-0.015
	(0.009)	(0.002)	(0.036)	(0.0006)	(0.044)
Panel B. Labor					
Log Market Access	0.066	-0.001	-0.048	-0.0012	-0.022
	(0.015)	(0.005)	(0.068)	(0.0034)	(0.044)
Panel C. Materials					
Log Market Access	0.107	0.012	-0.028	0.0016	0.032
	(0.049)	(0.006)	(0.040)	(0.0038)	(0.054)
Number of Counties	1,802	1,802	1,802	1,802	1,802
County/Year Obs.	5,406	5,406	5,406	5,406	5,406

Notes: For the indicated outcome variable, each column and panel reports the estimated impact of log market access from our baseline specification (as in column 1 of Table 1). Column 1 reports impacts on county productivity through changes in county allocative efficiency (as in Table 2, panel C, column 1) through changes in capital (panel A), labor (panel B), and materials (panel C). Column 2 reports impacts on county-level input gaps (defined as the input's cost share minus its revenue share in that decade), Column 3 reports impacts on county-level input wedges (defined as the input's cost share divided by its revenue share, minus one, in that decade), and Column 4 reports impacts on county-level cost shares (defined as the national industry-level cost shares in each decade multiplied by the share of county revenue in each industry in that decade). Column 5 reports impacts on counties' standard deviation of input wedges across industries in that county and decade.

All regressions include county fixed effects, state-by-year fixed effects, and year-specific cubic polynomials in county latitude and longitude. We continue to report the estimated impact of a one standard deviation greater change in market access from 1860 to 1880. The sample is a balanced panel of 1,802 counties (1860, 1870, 1880). Robust standard errors clustered by state are reported in parentheses.

Table 4. Impacts of Market Access, County-by-Industry Level Regressions

			By Industry Group:			
	Pooled Specification	Weighted by 1860 Revenue Share	Clothing, Textiles, Leather	Food and Beverage	Lumber and Wood Products	Metals and Metal Products
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Log County-Industry Productivity						
Log Market Access	0.189	0.160	0.186	0.007	0.220	0.303
	(0.058)	(0.052)	(0.096)	(0.056)	(0.149)	(0.159)
Panel B. County-Industry TFPR (Revenue Total Factor Productivity)						
Log Market Access	0.056	0.044	0.059	-0.012	0.075	0.054
	(0.023)	(0.025)	(0.073)	(0.023)	(0.038)	(0.123)
Panel C. County-Industry AE (Allocative Efficiency)						
Log Market Access	0.133	0.116	0.127	0.019	0.145	0.249
	(0.058)	(0.043)	(0.089)	(0.063)	(0.139)	(0.119)
Number of Counties	1,800	1,800	994	1,338	1,480	709
County-Year Obs.	5,400	5,400	2,640	3,665	3,984	1,860

Notes: this table reports estimates from regressions at the county-by-industry level, after aggregating the more-detailed industries to five industry groups: clothing, textiles, leather; food and beverage; lumber and wood products; metals and metal products; and other industries. We extend our baseline estimating equation 4 to include county-industry fixed effects and state-year-industry fixed effects. The sample is drawn from our main balanced panel of 1,802 counties in 1860, 1870, and 1880, though each industry group is not reported in each county and decade. We omit county-industries that appear only once, but do not restrict the sample to county-industries that appear all three years. We continue to report the estimated impact of a one standard deviation greater change in market access from 1860 to 1880. Robust standard errors clustered by state are reported in parentheses.

Column 1 reports estimated average impacts of market access on county-industry productivity, county-industry TFPR, and county-industry AE. Column 2 reports estimates when weighting county-industries by their 1860 share of county revenue. Columns 3 to 6 report industry-specific effects of market access from separate regressions for each consistent aggregated industry group.

Table 5. Impacts of Market Access on County Industries, Establishments, and Sector Shares

	Log Number of Industries	Log Average Estab. Size: Revenue / Estab.	Log Average Estab. Size: Workers / Estab.	Log Number of Establishments	County Manufacturing Share of: Revenue	County Manufacturing Share of: Value-Added	County Manufacturing Share of: Surplus	County Manufacturing Share of: Employment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Log Market Access	0.028	0.021	0.024	0.172	0.0076	0.0001	-0.0019	0.0045
	(0.021)	(0.030)	(0.043)	(0.034)	(0.0078)	(0.0059)	(0.0090)	(0.0048)
Number of Counties	1,802	1,802	1,802	1,802	1,774	1,774	1,713	1,687
County/Year Obs.	5,406	5,406	5,406	5,406	5,322	5,322	5,139	5,061

Notes: For the indicated outcome variable, each column reports the estimated impact of log market access from our baseline specification (as in column 1 of Table 1). In column 1, the outcome variable is the log number of manufacturing industries reporting positive output in the county. In columns 2 and 3, the outcome variables are log average manufacturing establishment size in the county, based on revenue per establishment (column 2) or workers per establishment (column 3). In column 4, the outcome variable is the log number of manufacturing establishments in the county. In columns 5 to 8, the outcome variables are the county's manufacturing share of total values for manufacturing and agriculture: revenue (column 5); value-added (column 6), which for manufacturing is defined as revenue minus materials expenditures and for agriculture is defined as 92% of revenue; surplus (column 7), which for manufacturing is defined as revenue minus all input expenditures and for agriculture is defined as the value of land multiplied by the state mortgage interest rate; and employment (column 8).

All regressions include county fixed effects, state-by-year fixed effects, and year-interacted cubic polymials in county latitude and longitude. The samples are drawn from our main balanced panel of 1,802 counties in 1860, 1870, and 1880, which for columns 5 to 8 is smaller due to missing data for some counties in some years. We continue to report the estimated impact of a one standard deviation greater change in market access from 1860 to 1880 in the full sample of 1,802 counties. Robust standard errors clustered by state are reported in parentheses.

Table 6. Impacts of Market Access, Controlling Flexibly for Local Railroad Construction

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Log County Productivity						
Log Market Access	0.204	0.218	0.198	0.197	0.175	0.142
	(0.051)	(0.056)	(0.057)	(0.058)	(0.058)	(0.056)
Panel B. County TFPR (Revenue Total Factor Productivity)						
Log Market Access	0.036	0.049	0.047	0.041	0.032	0.013
	(0.025)	(0.030)	(0.030)	(0.030)	(0.032)	(0.035)
Panel C. County AE (Allocative Efficiency)						
Log Market Access	0.168	0.169	0.152	0.156	0.143	0.129
	(0.051)	(0.055)	(0.056)	(0.055)	(0.055)	(0.054)
Additional Controls for:						
Any Railroad	No	Yes	Yes	Yes	Yes	Yes
Railroad Length	No	No	Yes	Yes	Yes	Yes
Railroad Length Polynomial	No	No	No	Yes	Yes	Yes
Railroads in Nearby Buffer	No	No	No	No	Yes	Yes
Railroads in Further Buffers	No	No	No	No	No	Yes
Number of Counties	1,802	1,802	1,802	1,802	1,802	1,802
County-Year Obs.	5,406	5,406	5,406	5,406	5,406	5,406

Notes: Column 1 reports the estimated impact of market access from the baseline specification (as in column 1 of Table 2). Column 2 includes an additional control for whether a county contains any railroad track. Column 3 also controls for the length of railroad track in the county, and column 4 controls for a cubic polynomial function of the railroad track mileage in a county. Column 5 includes additional controls for whether a county contains any railroad track within 10 miles of the county boundary, and a cubic polynomial function of the railroad track mileage within 10 miles of the county boundary. Column 6 adds controls for separate cubic polymial functions of railroad track within 20 miles, within 30 miles, and within 40 miles of the county.

All regressions include county fixed effects, state-by-year fixed effects, and year-specific cubic polynomials in county latitude and longitude. We continue to report the estimated impact of a one standard deviation greater change in market access from 1860 to 1880. The sample is a balanced panel of 1,802 counties (1860, 1870, 1880). Robust standard errors clustered by state are reported in parentheses.

Table 7. Impacts of Market Access, Using Variation in Water Market Access

	Log Market Access	2SLS			Controlling for Proposed Canals		
		County Productivity	Revenue Total Factor Productivity	Allocative Efficency	County Productivity	Revenue Total Factor Productivity	Allocative Efficency
	OLS						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Log Market Access		0.221	0.040	0.193	0.203	0.036	0.167
		(0.029,0.435)	(-0.111,0.315)	(0.001,0.367)	(0.051)	(0.026)	(0.051)
Instruments:							
Log Water Market Access in 1860 X year=1870	-1.003						
	(0.212)						
Log Water Market Access in 1860 X year=1880	-1.780						
	(0.245)						
Kleibergen-Paap F statistic		27.2	27.2	27.2			
Number of Counties	1,802	1,802	1,802	1,802	1,802	1,802	1,802
County-Year Obs.	5,406	5,406	5,406	5,406	5,406	5,406	5,406

Notes: Column 1 reports the impact of log water market access in 1860 on changes in log market access from 1860 to 1870 and changes in log market access from 1870 to 1880: log market access is regressed on log water market access in 1860, interacted with year fixed effects for 1870 and 1880. Column 2 reports the estimated impact of log market access on county productivity, instrumenting for log market access using the first-stage relationships reported in column 1. Columns 3 and 4 report corresponding 2SLS estimates for county TFPR and county AE. For the 2SLS specifications, we report the Andrews (2018) and Sun (2018) two-step weak-instrument-robust confidence sets, as well as the first stage F statistic.

For Columns 4, 5, and 6, we include as an additional control to our baseline OLS specification the market access that counties would have had if the railroad network had never been built but instead Fogel (1964)'s proposed counterfactual canal network had been constructed. Robust standard errors clustered by state are reported in parentheses.

All regressions include county fixed effects, state-by-year fixed effects, and year-interacted cubic polymials in county latitude and longitude. In columns 2 to 7, we continue to report the estimated impact of a one standard deviation greater change in market access from 1860 to 1880. The sample is a balanced panel of 1,802 counties (1860, 1870, 1880).

Table 8. Impacts of Market Access on Model-Implied Values

	Baseline	Model-Implied Values:	
	Specification	Nominal	Real
	(1)	(2)	(3)
Panel A. Log Revenue			
Log Market Access	0.192	0.259	0.333
	(0.049)		
Panel B. Log Capital Expenditure			
Log Market Access	0.158	0.259	0.259
	(0.053)		
Panel C. Log Labor Expenditure			
Log Market Access	0.196	0.259	0.337
	(0.061)		
Panel D. Log Materials Expenditure			
Log Market Access	0.183	0.259	0.337
	(0.050)		
Panel E. Log Productivity			
Log Market Access	0.204	0.197	0.257
	(0.051)		
Number of Counties	1,802	1,802	1,802
County-Year Obs.	5,406	5,406	5,406

Notes: Column 1 reports estimates from column 1 of Table 1: for the indicated outcome variable in each panel, we report the estimated impacts of log market access (as defined in equation 1), controlling for county fixed effects, state-by-year fixed effects, and year-interacted cubic polymials in county latitude and longitude. The sample is a balanced panel of 1,802 counties (1860, 1870, 1880). We report the estimated impact of a one standard deviation greater change in market access from 1860 to 1880. Robust standard errors clustered by state are reported in parentheses.

Columns 2 and 3 show the relationship between log market access and model-predicted values in 1860, 1870, and 1880, where the only primitive allowed to vary over "time" is the railroad network and the model parameters are estimated in 1890. Column 2 reports impacts on nominal values for the outcome variables, and column 3 reports impacts on real values for the outcome variables.

Table 9. Counterfactual Impacts on National Aggregate Productivity

	Baseline:	Restricted Railroad Networks:				No Railroads,	All Railroads,
	No Railroads	Only 1850 RRs	Only 1860 RRs	Only 1870 RRs	Only 1880 RRs	Extended Canals	Twice the Cost
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Counterfactual scenario, holding worker utility constant							
Change in Aggregate Productivity	-26.7%	-22.3%	-15.6%	-9.9%	-2.6%	-23.5%	-8.7%
Panel B. Counterfactual scenario, holding total population constant							
Change in Aggregate Productivity	-5.5%	-5.0%	-4.1%	-2.7%	-0.7%	-4.4%	-1.4%
Change in Utility	-32.7%	-27.2%	-18.3%	-11.4%	-2.9%	-29.0%	-11.1%

Notes: Each column reports the estimated change in national aggregate productivity from counterfactual changes in the transportation network. Panel A reports estimates from our baseline scenario, which holds worker utility constant in the counterfactual and allows for declines in total population. Panel B reports estimates from an alternative scenario, which holds total population fixed, and so we also report the associated decline in worker utility. In all scenarios, population is allowed to relocate endogenously within the country. The sample includes all 2,722 counties that in 1890 report positive population and positive revenue (agriculture and/or manufacturing).

Column 1 reports impacts under our baseline counterfactual scenario, which removes all railroads in 1890. Columns 2 to 5 report impacts under more moderate counterfactual scenarios, which restrict the railroad network to those railroads that had been constructed by 1850 (column 2), by 1860 (column 3), by 1870 (column 4), or by 1880 (column 5). Column 6 reports impacts from replacing the railroads with feasible extensions to the canal network, as proposed by Fogel (1964). Column 7 reports impacts from maintaining the 1890 railroad network, but doubling the cost of transportation along all railroads.

Table 10. Counterfactual Impacts on National Aggregate Productivity, Robustness

			Use Materials Wedge for:			Alternative Trade Elasticities:			Alternative Average Prices:	
	Baseline	Efficient Agriculture	Capital	Capital and Labor	Efficient Capital	Θ = 2.0	Θ = 3.9	Θ = 8.2	$\overline{P}$ = 20	$\overline{P}$ = 50
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Fixed Worker Utility										
Change in Aggregate Productivity	-26.7%	-16.5%	-24.3%	-20.2%	-23.4%	-27.6%	-26.1%	-23.6%	-36.1%	-23.2%
Panel B. Fixed Total Population										
Change in Aggregate Productivity	-5.5%	-4.6%	-4.6%	-6.1%	-4.1%	-5.7%	-5.4%	-4.0%	-7.6%	-4.7%
Change in Utility	-32.7%	-32.9%	-32.7%	-33.0%	-32.7%	-34.1%	-31.9%	-29.0%	-43.5%	-28.6%

Notes: Each column reports impacts under our baseline counterfactual scenario that removes all railroads in 1890, as in column 1 of Table 9. Panel A reports estimates from our baseline scenario, which holds worker utility constant in the counterfactual and allows for declines in total population. Panel B reports estimates from an alternative scenario, which holds total population fixed, and so we also report the associated decline in worker utility. In all scenarios, population is allows to relocate endogenously within the country. The sample includes all 2,722 counties that in 1890 report positive population and positive revenue (agriculture and/or manufacturing).

Columns 2 to 10 report robustness of our baseline estimates (in column 1) under alternative parameters. Our baseline estimates use our estimated value for Θ, the trade elasticity, of 3.05 with a 95% confidence interval between 1.95 and 3.90. In Column 2, we reduce the estimated degree of input distortions in each county by assuming that the agricultural sector is efficient, and only apply our estimated manufacturing wedges to the county's manufacturing share of combined output across manufacturing and agriculture. Columns 3 and 4 use the estimated materials wedge in each county to assign counties' capital wedge (Column 3) or capital wedge and labor wedge (Column 4). Column 5 assumes that capital-use is efficient, such that there is zero gap for capital (or a wedge of 0). In Columns 6 and 7, we alternatively impose values for Θ of 1.95 or 3.90; in Column 8, we impose a value of 8.22 from Donaldson and Hornbeck (2016). Our baseline estimates also use our estimated value for $\overline{P}$ of 38.7, the average price of transported goods, which scales the assumed transportation costs into proportional costs. In Columns 9 and 10, we alternatively impose values for $\overline{P}$ of 20 or 50.