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Bridges

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Bridges are critical but sparse links in land transport networks. I exploit quasi-experimental variation in bridge construction over major rivers in the United States to measure the causal effects of land transport infrastructure. Bridges are more often built upstream than downstream of tributary confluences—where smaller rivers join larger rivers—generating local differences in connectivity. These local connectivity advantages have negative effects on per capita income. In contrast, major changes in connectivity arising from the opening of major bridges increase per capita economic activity. A narrative explanation that can reconcile both results is that land transport infrastructure creates productivity advantages that drive economic growth, structural transformation, and urbanization over large spatial scales. Local sorting within the cities that form around early transport routes then reverses this gradient over smaller spatial scales.

Keywords: transport infrastructure, growth, economic geography, structural transformation, urbanization

1. Introduction

Transport infrastructure reduces travel costs, creating the potential for welfare gains through increased trade.[1] Anticipating such benefits, governments invest between 1 and 2% of world GDP in transport infrastructure every year (International Energy Agency, 2013) But how efficiently these trillion-dollar investments are allocated remains uncertain. Patterns in cross-sectional data could be misleading. Richer and more rapidly-growing regions typically invest in better quality infrastructure (Akbar, Couture, Duranton, & Storeygard, 2023; Canning, 1998) but transport planners may strategically place infrastructure in poorer or marginalized neighbourhoods to minimize land acquisition costs, avoid well-funded political opposition (Mohl, 2004) or in the often euphemistic name of "urban renewal" (Archer, 2020) The result of these selection effects is that correlations between economic activity and transport infrastructure could reflect underlying characteristics that influenced decisions about where infrastructure is located rather than the causal effects of infrastructure.

[1] See, e.g., Donaldson (2018)

The editor in charge of this paper was Thomas Chaney.

This paper measures the causal effects of land transport infrastructure. I exploit quasi-experimental variation in where and when bridges were built across major rivers in the United States: the Mississippi and the Ohio. Bridges are critical and relatively sparse links in land transport networks. For land near rivers—all of which has comparable water access, historically important for transport, energy, and agriculture—distance to the nearest bridge strongly predicts distance to a major land transport route. I develop two complementary empirical strategies to estimate the causal effects of transport infrastructure over different temporal and spatial scales. First, I exploit variation in the *location* of bridge construction, driven by river geography, to describe the very long-run effects of land transport infrastructure over small spatial scales. I then exploit variation in the *timing* of bridge construction to describe how economic activity evolves in the decades after major changes in connectedness.

To clarify the empirical problem, I first describe how per capita income varies with distance to major land transport infrastructure—rail or road—in fine spatial detail for the contemporary United States. While the areas furthest from transport infrastructure are the poorest, on average, the relationship is hump-shaped: income peaks around 5 km from a major transport route. But how this pattern emerges is uncertain: one could rationalize it at least as well with a story about selection effects as with one of causal economic forces.

To isolate the causal effects of transport infrastructure, my first empirical strategy exploits variation in the location of bridges generated by *tributary confluences*. A tributary confluence is a place where a smaller river joins a larger river, sharply increasing the downstream flow rate in the larger river and, in turn, the cost of bridge construction. The result is that bridges are more often built just upstream of tributary confluences than just downstream. For example, four bridges lie just upstream of the confluence of the Ohio and the Upper Mississippi at Cairo, IL, and there is no bridge for more than 100km downstream.

I compare census tracts located upstream of tributary confluences to their downstream neighbours. Because bridges are more often built upstream, the median upstream tract lies 0.7 km from a bridge, while the median downstream tract is 2.3 km from a bridge. While this difference might seem trivial, population density is highly concentrated near land transport routes: the median census tract in the contiguous United States is 1.3 km from a major land transport route. The marginal effects of proximity to transport infrastructure at this spatial scale are thus relevant to a large share of the population. On average, upstream tracts are 60% closer to a bridge and 27% closer to the nearest major land transport route. The local advantage in access to transport infrastructure dates back more than a century. By 2010, however, the better-connected upstream census tracts have13% *lower* per capita incomes.

Comparing upstream and downstream neighbours at tributary confluences recovers the causal effect of distance to land transport if these places *only* differ in this respect. Tributary confluences might themselves attract human settlements, as natural hubs on water transport routes (Fujita, Krugman, & Venables, 2001) but these advantages should be symmetric upstream and downstream, and I control for distance to a tributary confluence when I compare upstream and downstream census tracts. I also find no evidence for asymmetric development patterns around tributary confluences prior to the era of bridge construction.

This strategy allows me to measure the local long-run effects of access to transport infrastructure but not to evaluate how these effects emerge over time or whether effects vary over larger spatial scales. The second, complementary empirical strategy instead exploits variation in the *timing* of bridge construction to measure the impacts of changes in access to transport infrastructure over the following decades.

When a bridge is built depends on events that evolve slowly over time: technological advancement, and the processes of planning, financing, design, and construction, which for a major bridge typically take several decades. The Wheeling Suspension Bridge illustrates. Technological progress made it possible: it was the first bridge over the Ohio River and the longest suspension bridge in the world at the time. But delays were considerable: a charter to construct the bridge was issued in 1816, but it did not open until 1849. In contrast, when a bridge finally opens, much of its impact on the transport network is realized immediately.

I measure the impacts of bridge construction in 150 years of county-level panel data, a dataset that covers the rise to economic preeminence of the United States, along with the expansion of the railroads, the New Deal program of infrastructure spending, and the creation of the Interstate Highways. The length of the panel allows me to measure the effects of these major changes in access to infrastructure as they play out over several decades.

After a county experiences a 50% reduction in distance to a bridge, land values immediately rise, relative to counties not experiencing a change in distance to a bridge. Over thirty years, the cumulative rise in land values is 9%. Land values are the best consistently-measured proxy for total economic activity in the historical data. Total economic activity decomposes to population times per capita economic activity, and I show that population also grows by an additional 5% over thirty years. The proportionally larger effect on land values than on population suggests that counties experiencing an improvement in connectedness retain higher economic activity in per capita terms after thirty years.

This approach recovers the causal effects of bridge construction if the exact timing of bridge opening is exogenous to short-run deviations from local long-run growth trends, an assumption that I support with historical evidence. I model growth trends using county fixed effects and county-specific quadratic trends that absorb all observable and unobservable differences across counties in initial conditions, trends, and average changes in trends. A natural remaining concern is that bridges might be built in places that have recently experienced higher-than-average growth, which then continues: I might mistakenly attribute this pattern of growth to the causal impact of bridge construction. I show that, in my preferred specifications, outcomes of interest are uncorrelated with future changes in distance to a bridge. A second potential concern is that bridge construction could be correlated with other policies that promote growth. However, to affect the estimates, these policies would need to be timed sufficiently tightly with the construction of bridges to affect growth only after bridge construction was complete. A final concern is that policy-makers might build bridges in *anticipation* of higher-than-average growth, but I show that the results hold for counties whose distance to a bridge is only affected by bridges constructed in other counties.

To reconcile the two sets of results, I trace out effects on a wide range of complementary outcomes. These results suggest a narrative that rationalizes both the effects on economic activity and the patterns in contemporary data: Better connection to transport routes provided a productivity advantage, leading to industrialization and urbanization, and helping shape the relationship between transport infrastructure and economic activity over large spatial scales. City centres formed around historical transport routes and then expanded. Richer households differentially sorted away from historical city centres into lower-density suburban areas, while lower-income households remained in or selectively migrated to the historical city centres. These within-city sorting patterns provide a rationale for how lower incomes could emerge near land transport routes, in addition to or even in the absence of selective targeting of poor neighbourhoods by planners or politicians.[2]

This paper makes three main contributions to the literature on the impacts of transport infrastructure on economic growth. First, comprehensive data on historical transport infrastructure remains scarce. To measure how distance to a bridge evolves over time, I assemble a new dataset that includes every road or rail bridge ever constructed over the Mississippi and Ohio rivers, beginning with the Wheeling Suspension Bridge. Focusing in on a single, critical component of the infrastructure network allows me to construct the longest comprehensive panel of infrastructure data available in the literature.[3]

Second, I propose two novel identification strategies to measure the causal impacts of transport infrastructure improvements. Both may be applied in other contexts where major rivers form substantial obstacles to land transport networks. While a growing literature takes seriously the endogeneity of location choice in transport infrastructure,[4] only three previously-developed strategies can be applied outside of the narrowly-defined context of a specific natural experiment (Redding & Turner, 2015) and while these strategies are widely used, none is without flaws.[5] The identification strategies presented here expand the toolbox of empiricists who wish to understand the effects of transport infrastructure.

Third, I leverage these novel empirical strategies to provide new evidence about how the impacts of transport infrastructure evolve across space and time. Previous literature has paid relatively scant attention to the question of whether effects vary over time and typically considers the inter- and intra-city effects of transport infrastructure separately, with results largely insensitive to spatial scale within each class of effects (Redding & Turner, 2015) Estimating effects over different temporal and spatial scales for the same geographical area allows me to provide a relatively extensive description of the dynamic effects of transport infrastructure over different spatial scales. The results underline the importance of relocation effects. Transport infrastructure can affect both how much economic activity takes place and where it takes place (*economic geography*). Even when purged of selection effects, differences between better and worse-connected areas reflect both the direct economic effects of transport infrastructure and the relocation of economic activity across space (Redding & Turner, 2015) My results illustrate how between- and within-city processes may interact to shape the geography of economic activity with respect to transport infrastructure. Without a full picture of effects over

[2]These results echo findings from a recent literature on the within-city response to construction of the Interstate Highways (Bagagli, 2025; Brinkman & Lin, 2024; Mahajan, 2024; Weiwu, 2025)

[3]I originally extracted data from the National Bridge Inventory and then extensively cross-referenced the data with other contemporary and historical sources, as well as satellite imagery.

[4]Redding and Turner (2015) and Donaldson (2015) provide recent and comprehensive reviews.

[5]Using Redding and Turner's (2015) taxonomy, the "planned route" and "historical route" instrumental variable approaches depend on exogeneity of the planning or historical infrastructure decisions, usually conditional on some observable characteristics such as population. The implicit identifying assumptions can be violated if these decisions instead reflect underlying characteristics that are omitted from the estimated models. The "inconsequential places" approach, exemplified by the "straight line" instrumental variable (Banerjee, Duflo, & Qian, 2020) relies on identifying places that are incidentally connected by transport routes connecting two more distant points. This approach is naturally not applicable to places that are deliberately connected by transport routes.

different spatial and temporal scales, and proper consideration of relocation processes in interpreting the effects, one could reach grossly misleading conclusions about the economic impacts of transport infrastructure.

This study further provides new causal evidence about the role of manmade land transport infrastructure in determining settlement patterns in the historical United States. Earlier literature is divided as to whether early investments in transport infrastructure simply followed pre-existing patterns of population growth (Atack, Bateman, Haines, & Margo, 2010; Fishlow, 1965; Fogel, 1964) or whether they led to meaningful changes in economic geography. My results support the latter view but suggest a moderate level of responsiveness of population density to access to transport infrastructure that takes time to manifest.[6] This pattern of results is consistent with a wider literature on the population response to changes in economic circumstances (e.g., Amior & Manning, 2018)

Other studies consider bridges as critical links in transport infrastructure networks in different settings or with different objectives. Also using data from major rivers in the United States, Armenter, Koren, and Nagy (2014) incorporate bridges as focal points of transport networks in a theory of the economies of density and use correlations between bridges and post offices (a proxy for human settlements) to test the model's predictions. Several studies exploit the construction or closure of a bridge to study the impacts of a change in transport times on trade between two locations.[7] Brooks and Donovan (2020) study footbridges that connect villages to local labour markets in rural Nicaragua.[8]

The remainder of the paper proceeds as follows. Section 2 describes the correlations between distance to land transport infrastructure and economic outcomes in the modern United States. Section 3 recounts the history of bridge construction over the Mississippi and the Ohio, describes the data on bridges, and shows how distance to a bridge correlates with distance to land transport infrastructure. Section 4 explains how I exploit variation in the location of bridges around tributary confluences to estimate the causal effects of transport infrastructure and presents the results, while section 5 does the same for the empirical strategy that exploits variation in the timing of bridge construction. Section 6 broadens the lens to a wider range of outcome variables to provide a narrative account of development that reconciles the two sets of empirical results, and section 7 concludes.

[6]These findings are qualitatively consistent with those of Donaldson and Hornbeck (2016a) who find that the effects of railroad access on population growth are only a third of those predicted under perfectly elastic population mobility. They also complement Bleakley and Lin's (2012) evaluation of the long-run impacts of an obsolete natural transport advantage on economic geography, and studies of the impact of the Interstate Highways on growth of (Duranton & Turner, 2012) and in cities (Baum-Snow, 2007)

[7]Åkerman (2009) Blankespoor, Emran, Shilpi, and Xu (2022) Jones and Salazar (2021) Volpe Martincus, Carballo, Garcia, and Graziano (2014) See also von Carnap, Christian, Tompsett, and Zwager (2024) who describe patterns of agricultural land use around river crossings in rural Mozambique.

[8]Plausibly exogenous variation in bridge construction arises from whether or not a proposed footbridge passes an engineering feasibility test. My empirical strategies do not rely on the existence of information about technical feasibility or the ability to observe all candidate locations for a bridge.

2. Transport infrastructure and economic activity

To clarify the empirical problem, I first describe the correlations between land transport infrastructure and economic activity in the United States in contemporary census data.[9]

Figure 1a shows how per capita income varies with distance to a land transport route for all census tracts in the contiguous United States. For census tracts more than about 5 km from a land transport route, per capita income decreases with distance from land transport routes. The difference between per capita income 5 km from a land transport route and 30 km away is equivalent to falling 20 percentiles in the income distribution. This relationship resembles the correlation observed in macro-scale data between transport infrastructure and income (Calderón & Servén, 2004; Timilsina, Stern, & Das, 2021) However, near land transport routes, the gradient is reversed, and per capita income increases with distance to land transport. Fitting a piecewise linear function to the relationship between log per capita income and log distance to a land transport route suggests an elasticity of 0.072 (95% CI: 0.067, 0.077) at distances below 4.1 km (95% CI: 3.9, 4.6), and an elasticity of -0.096 (95% CI: -0.112, -0.080) at greater distances.[10]

Distance to a land transport route is the minimum of distance to a railroad or primary road, but the patterns shown in Figure 1a are not specific to the mode of transport. A similar hump-shaped relationship is seen for both distance to a railroad and distance to a primary road (Appendix Figures C1a and C1b).

To contextualize these differences, Figure 1b repeats the exercise for population density. In contrast to per-capita income, population density falls monotonically with distance to a land transport route. Population density varies more than per capita income, so income density remains highest near land transport routes (Appendix Figure C2). An implication of these patterns is that most Americans (85%) live in places where mean income locally increases with distance to a land transport route rather than decreases.

While these relationships are striking, they are purely correlational. They might reflect the causal effects of transport infrastructure on economic activity: these could hypothetically be positive at a macro scale but negative in the immediate vicinity of the transport route, potentially due to congestion or pollution externalities. However, they could equally reflect selection effects driven by decisions about where to locate land transport infrastructure. Given the patterns of population density, a third potential explanation concerns relocation. More educated or wealthier households might be attracted to regions that are better served by transport infrastructure because of better economic opportunities, but then avoid living close to transport routes because of local externalities. The rest of this paper aims to shed light on the extent to which the patterns in the data reflect causal effects, and how and why such patterns might emerge.

[9]Data are from the National Historical Geographical Information System (NHGIS; Schroeder et al., 2025) and U.S. Census Bureau (2010) More detail on all data sources is provided in Appendix A.

[10]Parameters estimated using non-linear least squares. Confidence intervals are Bayesian-bootstrapped, computed using Dirichlet weights (1000 iterations, clustered by state, see Cheng, Yu, & Huang, 2013; Rubin, 1981; Shao & Tu, 2012)

Figure 1a

Distance from a land transport route and per capita income

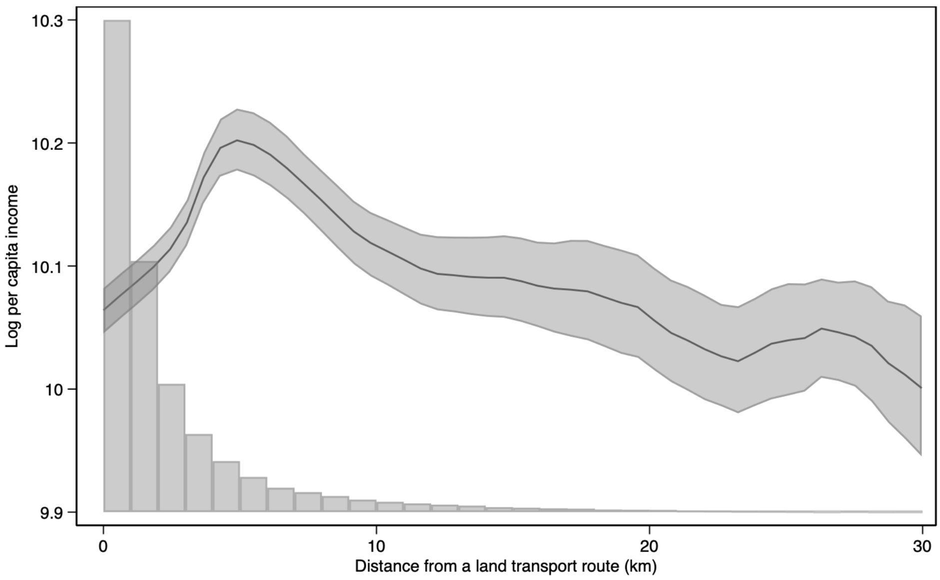
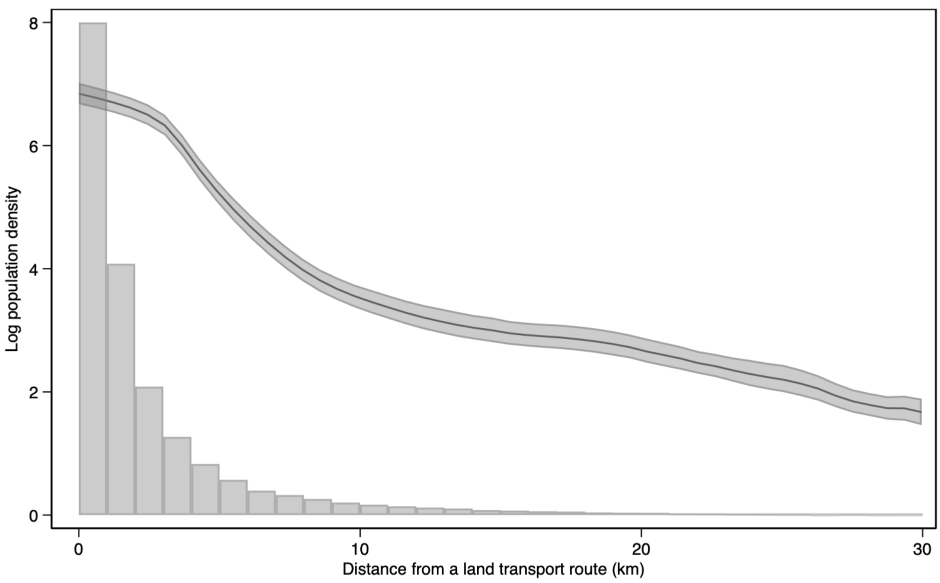


Figure 1b

Distance from a land transport route and population density



Notes Sample comprises all census tracts in the contiguous United States with non-missing data (N = 71,819 for per capita income, N = 72,004 for population density). Data are from 2010 census. Results from a locally linear regression with bandwidth 2.5km and Epanechnikov kernel. Results shown for census tracts less than 30km from a land transport route (>99% of full sample). Bayesian-bootstrapped 95% confidence intervals shaded, computed using Dirichlet weights (1000 iterations, clustered by state, see Cheng et al., 2013; Rubin, 1981; Shao & Tu, 2012) Histograms show density of observations.

3. Bridging the great rivers

The era of land transport in the United States began with the expansion of the railroads in the mid-nineteenth century. Before the railroads, almost all inland transport took place along waterways. A significant obstacle to railroad expansion was the difficulty and expense of constructing bridges to cross rivers and valleys.[11] In particular, the great Ohio and Mississippi Rivers together constituted a major barrier to land transport routes, spanning as they do almost the entire North-South extent of the country (Figure 2a).

Early bridge technology was rudimentary. The available materials were wood and stone. Wooden bridges typically lasted only twenty to thirty years. Stone bridges were prohibitively expensive: by 1850, only four significant stone railroad bridges had been constructed in the United States. Modern bridge construction became possible when the cost of smelting cast iron fell sufficiently to allow its use as a construction material. However, early bridges were designed with little or no formal attempt to calculate loads and stresses. In the mid nineteenth-century, “no more than ten men in America” had the necessary skills and training to scientifically design a bridge (Plowden, 1974)

From the second half of the nineteenth century onwards, advances in bridge technology and design made bridges possible over ever-wider spans. Cast iron was in its turn superseded, first by less-brittle wrought iron, and then by steel. Innovative new truss designs increased strength-to-weight ratios. More resilient wires replaced chains in suspension bridges. Caisson technology (compressed air boxes) allowed workers to construct piers below the water surface, although fully exploiting caisson technology required methods to prevent decompression sickness: dozens of workers were hospitalized and fourteen died after working in caissons during the construction of the Eads Bridge at St Louis (Butler, 2004)

These progressive advances in bridge technology, the fruits of extensive experimentation, gradually overcame the challenge of bridging the great rivers. The first bridge over the Ohio River—the John A. Roebling Bridge at Wheeling, completed in 1849—was then the longest suspension bridge in the world. The first bridge over the Lower Mississippi—the Frisco Bridge at Memphis, completed more than forty years later in 1892—had the longest span of any rail bridge ever built in the United States (American Society of Civil Engineers, n.d.) This history of innovation is preserved in the physical record: the Ohio River is a “virtual outdoor museum of American bridge engineering” (Plowden, 1974)

Bridge data I assemble a dataset that tracks the history of every bridge ever constructed over the Mississippi and Ohio rivers. I originally extracted data from the Federal Highway Administration’s National Bridge Inventory (NBI Federal Highway Administration, 2008) and then extensively cross-checked the data with satellite imagery, and with other contemporary and historical sources. Figure 2b) maps bridges on the Mississippi and Ohio in 1860, the starting year for most analyses in this study, at which time only four bridges had been successfully constructed.[12]

[11] Unless otherwise indicated, the account in this section is based on Plowden (1974)

[12] The Wheeling Bridge, on the Ohio; and the Rock Island Arsenal, Hennepin Avenue, and Wabasha Street Bridges, on the Upper Mississippi. Bridges had also been built in 1857, at Broadway Avenue, Little Falls and Broadway Avenue, Minneapolis, but both were destroyed in 1859. I truncate the sample at Lake Winnibigoshish, in north-central Minnesota, and Pittsburgh, where the Monongahela and the Allegheny merge to form the Ohio. Above Lake Winnibigoshish, the Mississippi River meanders extensively. Results are insensitive to truncating at an alternative, lower point on the Upper Mississippi, defined by an engineer’s informal assessment of where major engineering works become necessary (Weeks, n.d.)

Figure 2
The Mississippi and Ohio Rivers

Figure 2
a) Rivers

Figure 2
b) Bridges (1860)

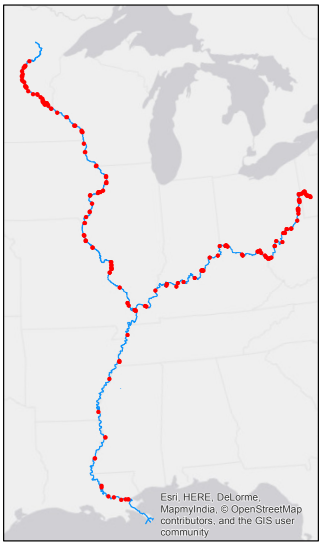


Figure 2
c) Bridges (2010)

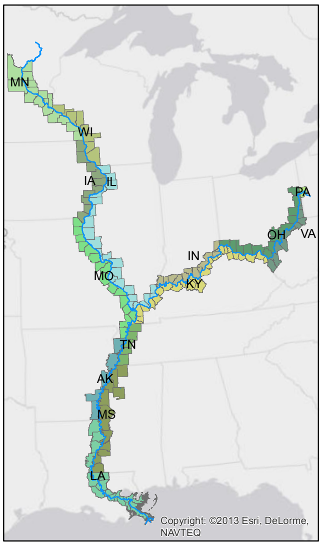


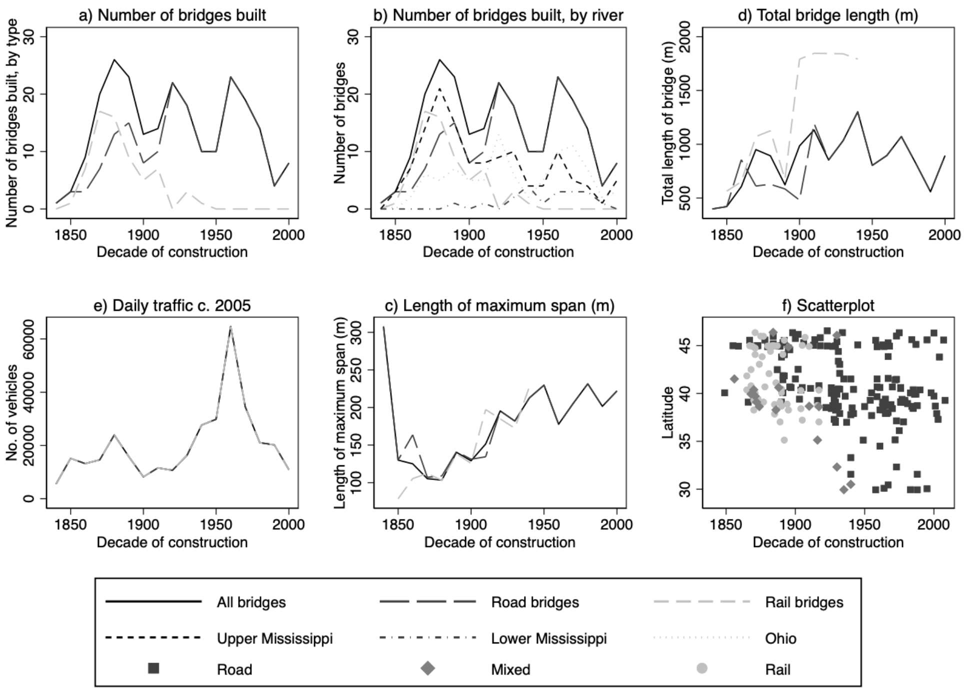
Figure 2
d) County Boundaries (1860)

Notes Rivers shown in blue throughout; bridges in red (b and c); sample counties, shaded by state (d).

Figure 3 describes trends in bridge construction.[13] Early bridges were mostly rail bridges, while since 1950 most new bridges have been road bridges. Few bridges were initially built over the Lower Mississippi, where bridge construction is most technically challenging. Peaks in bridge construction activity correspond to railroad expansion, Roosevelt’s New Deal programs, and the construction of the Interstate Highway System. Figure 2c) maps bridges in 2010, the end year of this study.

Bridges became progressively more complex and ambitious in the wake of technological advances. Bridges were built with increasing spans (the distance between supports) and greater length until at least the middle of the 20th Century.[14] The steady increases reflect the largely incremental nature of innovations in bridge technology. Bridge technology continues to evolve, with the first cable-stayed bridge over the Mississippi River only built in 1993.[15]

FIGURE 3
Bridges: descriptive statistics



Notes Bridges on the Mississippi and Ohio rivers (N = 237). Traffic counts are for road bridges listed in the NBI only, from 2001-2006; 33 bridges have missing data. I dropped traffic data from one bridge from 1993, and from a rail bridge which appeared to have road traffic data listed in error.

[13] Appendix Table C1 provides further summary statistics.

[14] The anomalous value in Figure 3c for bridges constructed before 1860 is entirely driven by the Wheeling Bridge. Rail bridges are longer than road bridges because rail bridges need shallower inclines.

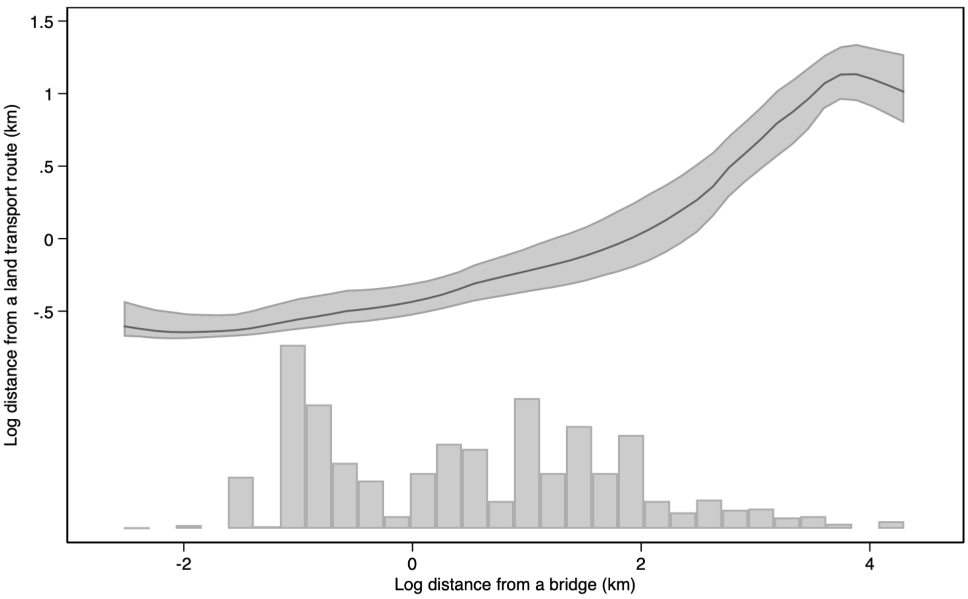
[15] The Hale Boggs Memorial Bridge in St Charles Parish, Louisiana.

Bridges and land transport infrastructure Throughout the rest of this paper, I focus on a geographical area on the banks of the Mississippi and Ohio Rivers within the limits covered by the bridge dataset, as summarized in Table 1 and illustrated in Figure 2d).[16] The unit of analysis is the census tract in Section 4 and the county in Section 5. Within this geographical area, I treat distance to a bridge as a proxy for distance to a land transport route, which all bridges in my dataset carry. As a measure of access to land transport infrastructure, distance to a bridge has the advantage that it can be measured consistently throughout the entire study period.[17]

An important implicit assumption is that distance to a bridge captures meaningful variation in access to land transport infrastructure networks in the study area. When data on land transport networks are available, this assumption can be tested. In modern data, distance to a bridge correlates strongly with distance to land transport infrastructure (Figure 4). Bridges carry either road or rail, or both. The relationship in Figure 4 reflects both distance to rail and distance to a primary road (Appendix Figure C3).

Figure 4

Distance from a land transport route and distance from bridge on the Mississippi and Ohio rivers



Notes Sample comprises census tracts for which any part of the tract lies within 10km of the Mississippi or Ohio rivers and whose centroid is less than 31.4 km from a tributary confluence, N = 1,054. Data from 2010. Results from a locally linear regression with bandwidth 0.75 and Epanechnikov kernel. Excludes 4 outliers with log distance to a bridge less than -3km. Bayesian-bootstrapped 95% confidence intervals shaded, computed using Dirichlet weights (1000 iterations, clustered by tributary confluence, see Cheng, Yu, & Huang, 2013; Rubin, 1981; Shao & Tu, 2012) Histogram shows density of observations.

[16] I define the geographical sample using three river alignment datasets (ESRI n.d.-a, n.d.-b; USGS 2019)

[17] Comprehensive historical data on the full US transport network over the full study period is not available. To my knowledge, the most comprehensive dataset available is Atack (2016) which is comprehensive for railroads and waterways, but not roads, and only until 1911.

TABLE 1
Summary Statistics

a) Bridge data		
Number of bridges	237	
b) Census tract sample. Cross section, 2010.		
Number of census tracts	1,054	
Number of counties represented	74	
Number of states represented	13	
Number of tributary confluences	27	
Mean tract area (km^2)	43.3	
Mean distance from centroid to river (km)	5.0	
Mean distance from a bridge (2010)	7.8	
Fraction upstream of tributary confluence	0.47	
Mean per capita income (US\$, 2010)	25,027	
Fraction with zero population density	0.001	
Mean population density ($/km^2$ 2010)	1,386	
c) County sample. Panel data, 1860-2010.	1860	2010
Number of counties (1860 boundaries)	181	
Number of counties ever building a bridge	124	
Number of states	14	
Mean county area (km^2)	1,309	
Mean distance from centroid to river (km)	14.1	
Mean distance from a bridge (km)	432	24
Mean farm land values (US\$, contemporary)	19	3,723
Mean population density ($/km^2$)	16	79

Note: Statistics refer to main samples used in analysis.

These simple correlations between distance to a bridge and distance to rail or primary road potentially understate the importance of the bridges in my data to land transport networks. The roads and railroads carried by these bridges are mostly major transport routes. Traffic data show that the road bridges carry, on average, tens of thousands of vehicles per day. The most extensively used crossings have daily traffic counts in the hundreds of thousands, especially bridges constructed around 1960, many of which form part of the Interstate Highway System (Figure 3e).

The ideal measure of the importance of bridges to transport networks would arguably be market access, which captures both the state of the wider transport infrastructure network and the size of the markets to which transport infrastructure connects (Donaldson & Hornbeck, 2016a) However, calculating market access is computationally expensive at small spatial scales and requires comprehensive mapping of the full US transport network. Market access data are not available at the census tract level in any year, but historical market access data are available for a subset of sample counties in 1870 and 1890 (Donaldson & Hornbeck, 2016b) In these data, market access correlates well with distance to a bridge.[18]

[18] In the subset of Donaldson and Hornbeck's data that overlaps with my sample, log distance to a bridge explains 46% of the variation in market access in 1890 with an elasticity of around 0.1, implying that halving distance to a bridge increases market access by 7% (Appendix Figure C4). For comparison, at the same time, counties with a railroad had 10% higher access than those that did not.

4. Evidence from where bridges are built

4.1. Empirical strategy

To separate the causal effects of access to land transport infrastructure from selection effects, I employ a quasi-experimental approach. I focus on variation in distance to a bridge driven by discontinuities in bridge construction costs at *tributary confluences*, where tributaries—smaller rivers—join larger rivers. Bridge construction costs increase with the flow rate of a river, the volume of water that passes through a given cross-section in a unit of time.[19] Flow rates largely evolve smoothly over space, rising through surface runoff from rainfall and snowmelt, and falling through evaporation and infiltration. At tributary confluences, however, flow rates increase sharply when the tributary merges with the main stream. This produces a correspondingly sharp local increase in the cost of bridge construction. The result is that bridges are often preferentially located upstream of tributary confluences.

A salient example is the confluence of the Mississippi and the Ohio at Cairo, IL (Figure 5). Four bridges cross the relatively smaller streams of the Upper Mississippi and the Ohio immediately upstream of the confluence. No bridge crosses the Lower Mississippi for more than 100km downstream. As this example suggests, the preference to construct bridges upstream dominates even when this entails the construction of two shorter bridges over two separate streams. Elsewhere in the world, similar examples are easily identified from satellite imagery, suggesting potentially broad applicability (e.g., Appendix Figure C5.)

I focus on a sample of 27 major tributary confluences on the Mississippi and Ohio Rivers, identifying and defining major confluences using data on flow rates from hydrological models and spatially matching census tracts to these tributary confluences.[20] Although the resulting sample of census tracts is naturally a relatively small and non-random subsample, per capita income and population density display qualitatively similar relationships with distance to land transport infrastructure in this subsample as they do in the contiguous United States as a whole (Appendix Figure C7).

Panels a) and b) of Figure 6 show flow rates and bridge construction around tributary confluences on the Mississippi and the Ohio rivers. In each panel of Figure 6, outcome data are residualized with respect to local means and pooled across tributary confluences. Panel a) shows the sharp changes in flow rates. Panel b) shows that more bridges are built upstream than downstream. Bridge construction rates are elevated for about 10km upstream of the tributary confluence, while the downstream rate of bridge construction

[19] Higher flow rates increase either the width of a river, its depth, or its velocity. Each in turn increases the maximum required span: A wider river requires a longer crossing, and it is more difficult and more expensive to construct piers (bridge supports) in deep or fast rivers. The maximum required span increases bridge construction costs convexly as a function of basic structural mechanics: the maximum bending moment in a beam, a measure of the force it needs to withstand, is a function of the beam’s length squared.

[20] I classify river nodes as major tributary confluences if the flow increment at the node exceeds 7.5% of the upstream flow rate or 10,000 cubic feet per second. The flow rate is estimated based on topography and rainfall, thus approximating flow in the absence of human intervention (see the National Hydrography Dataset, USGS 2019) While hydrogeomorphologists primarily classify tributaries by *stream order*—topological distance from source or river mouth—stream order does not cleanly map to volumetric flow. I include census tracts i) if any part of the tract lies within 10km of the Mississippi or Ohio, between the limits defined previously, or ii) if the tract is completely enclosed by tracts meeting criteria i), yielding a geographically contiguous final sample (Appendix Figure C6). Details are in Appendix A.

Figure 5

Cairo, IL, confluence of the Mississippi and Ohio

Notes Bridges outlined in red. Imagery ©2020 Landsat/Copernicus, Maxar Technologies, USDA Farm Service Agency; Map data ©2020 Google.

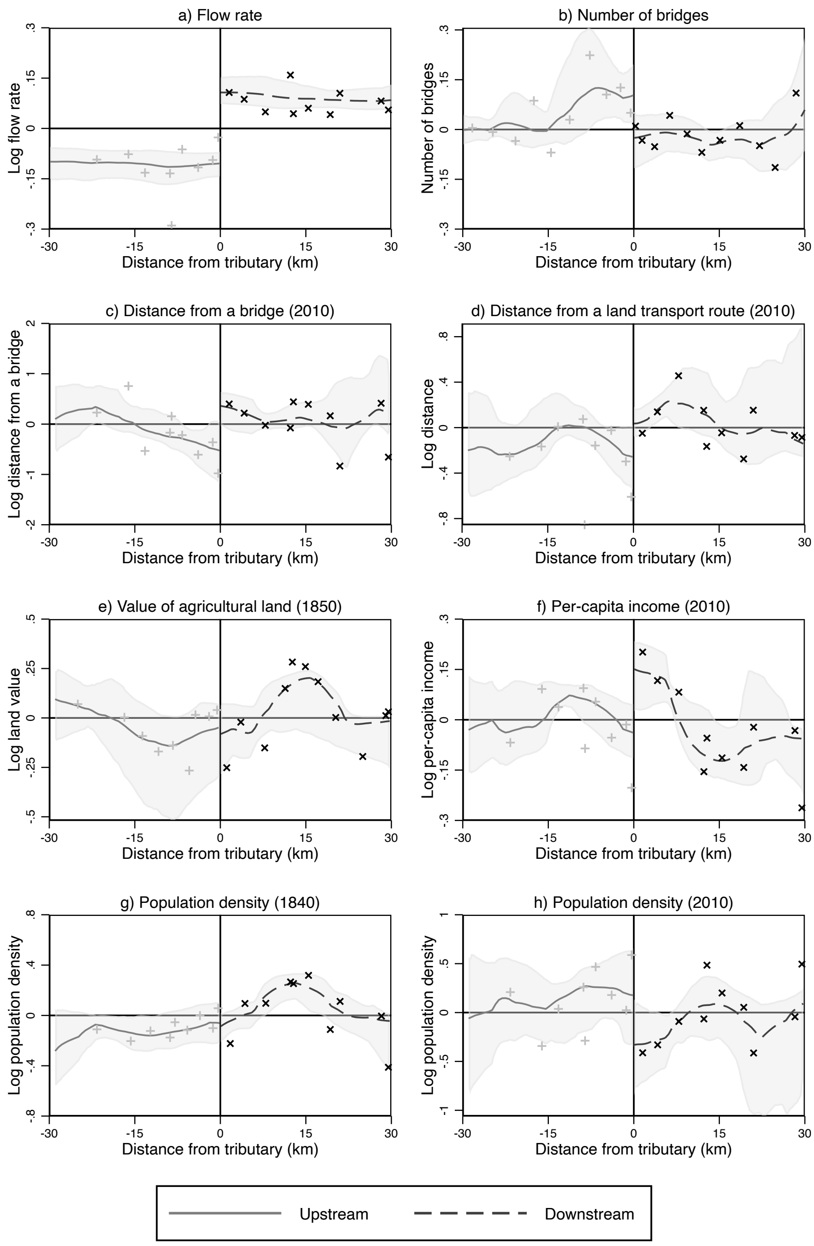
is, if anything, locally suppressed. A rationale for local suppression is that bridges are to some extent local substitutes: engineers locate new bridges to "fill in gaps".

The discontinuity in the costs and likelihood of bridge construction results in a distinctive, asymmetric pattern of distance to transport infrastructure around tributary confluences. Census tracts that lie upstream of tributary confluences are, on average, closer to bridges than those downstream (panel c) of Figure 6).[21] The differences are small in absolute magnitude, as shown in Figure 7, which plots the distribution of distance to a bridge in upstream and downstream tracts. We can think of tributary confluences as producing differences in whether bridges are built a few kilometers upstream rather than a few kilometers downstream. An apparent discontinuity appears in panel c) as a result of averaging distance to a bridge across census tracts. Distance to a bridge is naturally continuous across space, but upstream census tracts are more likely to contain bridges than their downstream neighbours and are thus on average closer to a bridge. Whether these patterns are detected around tributary confluences is therefore sensitive to the unit of observation and is, for example, attenuated at the scale of the county.[22] The asymmetry in distance to a bridge in turn maps to asymmetry in distance to a land transport route (panel d), driven approximately equally by distance to rail and to a primary road (Appendix Figure C9).

[21] I calculate distance to a bridge by river reach and assign census tracts the properties of the nearest river reach to the tract centroid, isolating the component of distance that is parallel to river flow.

[22] Appendix Figure C8 uses simulations to illustrate how a discontinuous change in the probability of bridge construction generates a continuous and asymmetric pattern of distance to a bridge, which nonetheless appears discontinuous in aggregate data, and how the extent of the apparent discontinuity depends on the aggregation scale.

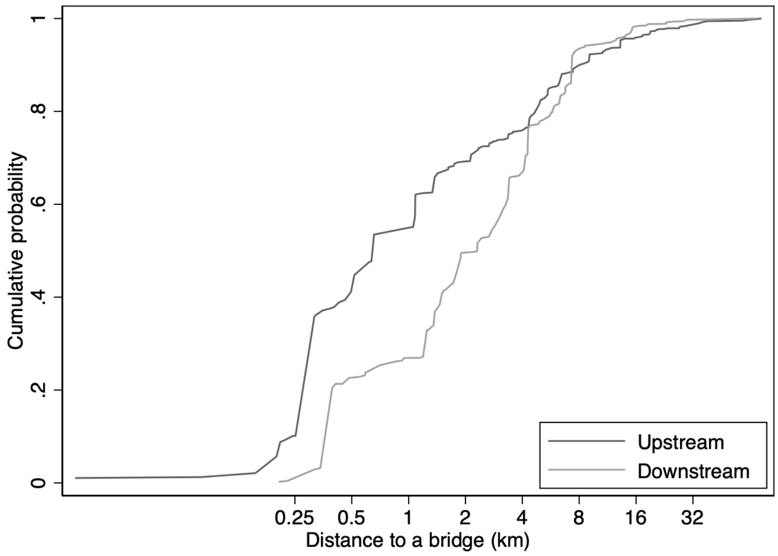
Figure 6
Patterns around tributary confluences



Notes Graphs show patterns of listed outcomes with respect to position relative to the nearest tributary confluence. Lines with 95% confidence intervals reflect non-parametric regressions (locally linear, bandwidth 3km), bootstrapped with clustering at the nearest tributary confluence (1000 replications). Outcomes residualized relative to nearest-tributary means. Markers show binned data. Sample includes river reaches (a and b) and census tracts (c to h) on the Mississippi and Ohio less than 30km from the nearest confluence.

Figure 7

Empirical distribution of distance to a bridge around tributary confluences



Notes Empirical cumulative density function for distance to a bridge, measured in 2010, upstream and downstream of tributary confluences. Sample contains year 2010 census tracts for which i) any part of the tract is within 10km of the Mississippi or Ohio rivers and ii) the centroid lies within 31.4km of a tributary confluence. Observations weighted using a triangular kernel in river-distance to nearest tributary confluence.

To quantify the differences between upstream and downstream census tracts, I estimate the following reduced form equation:

$$w_{ij,2010} = \theta_j + \pi \text{downstream}_{ij} + \beta_1 \text{d}_{ij} + \beta_2 \left(\text{d}_{ij} \times \text{downstream}_{ij}\right) + \nu_{i,2010} \tag{4.1}$$

Outcome variables $w_{ij,2010}$ are measured in census tract i in 2010. The variable $downstream_{ij}$ is an indicator taking the value one if census tract i lies downstream of the nearest tributary confluence j and zero if it lies upstream. The coefficient of interest π captures mean differences in outcomes upstream and downstream of confluences. Nearest-tributary fixed effects θ_j absorb broad spatial trends that are largely orthogonal to the patterns around tributary confluences and improve precision in the estimates.

The regression additionally controls for position relative to the nearest tributary d_{ij}, measured in river-kilometers, where distances downstream take positive values and distances upstream take negative values, as well as the interaction between d_{ij} and $downstream_{ij}$. These controls address two potential issues. Downstream census tracts are closer to the river mouth in Louisiana, to the south, and further from the river sources, to the north. Census tracts also vary in their absolute distance from the tributary confluence.[23] Proximity to the tributary confluence might affect outcomes if tributary confluences attract human settlements as natural hubs on water transport routes (see Fujita et al., 2001) Controlling for d_{ij} and its interaction with $downstream_{ij}$ ensures that the estimated upstream-downstream differences are neither affected by north-south trends nor by distance to the nearest tributary.

As Figure 6 shows, position with respect to a tributary confluence only affects bridge construction in the immediate vicinity of the confluence. I weight observations using a triangular kernel that places greatest weight on observations nearest to confluences, and I exclude census tracts that are distant from tributary confluences. As a data-driven way to determine the threshold distance for exclusion, I employ the optimal bandwidth selection approach from the regression discontinuity literature (Calonico, Cattaneo, & Titiunik, 2014) selecting an threshold of 31.4km.[24] I also test robustness to varying this exclusion threshold.

Outcomes and distance to a bridge are both strongly spatially correlated in census tract level data. To conduct inference, I bootstrap the estimates, resampling at the level of the nearest tributary. Intuitively, I treat each tributary confluence as a separate observation.

This regression recovers the causal impacts of improved access to land transport infrastructure if the position of a census tract with respect to the nearest tributary confluence is only correlated with the outcome variables via distance to a land transport route, conditional on the control variables included in Equation 4.1. In formal terms:

$$E\big(\nu_{i,2010}|\theta_j,\mathrm{downstream}_{ij},\mathrm{d}_{ij},\mathrm{d}_{ij}\times\mathrm{downstream}_{ij}\big)=0$$

Violations of this condition are hypothetically possible. Although neither the Mississippi nor the Ohio is well-suited to water power or energy generation in my study area, faster-flowing water provides more energy, and stream velocities also increase slightly downstream of tributary confluences (Appendix Figure C11). This might cause industry to selectively locate downstream. Ports might also form downstream of confluences, where river traffic traveling along either the main river or its tributary must pass. Tributary confluences are often Y-shaped, so upstream areas might have better access to the tributary river or a higher share of land covered by surface water. Rivers also tend to flood upstream of confluences, as the river backs up and overflows.

To shed light on whether differences between upstream and downstream locations could be driven by these—or other—factors that are unrelated to bridges, I search for asymmetries around tributary confluences that predate bridge construction. Any such asymmetries would lend support to the possibility of an alternative explanation. The test is imperfect, because historical data are not available with the same spatial resolution as modern data. However, matching census tracts to historical county-level data, I show

[23] Appendix Figure C10 shows the distribution of census tract observations relative to tributary confluences. The density of observations is correlated with population density, because census tracts are constructed to have an average population of 4000 individuals. As a result, a McCrary-style density test at tributary confluences would fail.

[24] Specifically, I run optimal bandwidth selection on the full sample of census tracts matched to tributary confluences, for all tributary confluences with at least two matched census tracts. I calculate optimal bandwidths for three key outcomes—log distance to a bridge, log population density, and log per capita income, all measured in 2010—after demeaning with respect to nearest-tributary average values. For consistency, I then fix the exclusion threshold throughout as the minimum of the three optimal bandwidths selected.

that the asymmetric patterns around tributary confluences emerge only after bridge construction begins. In data from before bridges were built, land values—the proxy for total economic activity I will use in the historical data—and population density—which should reveal any particular advantage for human settlement—are very similar upstream and downstream of tributary confluences (Figure 6, panel c and e). The absence of asymmetries before the era of bridge construction lends support to the identifying assumption.

The primary alternative way to cross a river is by ferry.[25] In contrast to bridges, ferries do not seem to have selectively located upstream or downstream of tributary confluences.[26] While data on ferry crossing locations are very sparse, their traces are preserved in place names (USGS 2022) such as Ferryville, WI, or Trotter Landing, MS. These place names appear equally frequently upstream and downstream of tributary confluences (Appendix Figure C12).

I focus on the reduced form effects of geographical location relative to a tributary confluence because, as I will show, location relative to a tributary confluence predicts distance to both road and rail, and it predicts distance to transport infrastructure from 1880 onwards. This makes it impossible, without restrictive assumptions, to separate the effects of access to road from the effects of access to rail, or the effects of historical and contemporary access. The coefficient π in equation 6 should thus be interpreted as the net effect of lying further from a land transport route since 1880.

The primary outcome variable is per capita income. I also report effects on population density, allowing me to infer effects on income density, which corresponds to total economic activity. Population density is also of independent interest in understanding the factors that shape the geography of human settlements.

4.2. Results

Census tracts located downstream of a tributary confluence are significantly further from a bridge than those located upstream (column 1 of Table 2). Converting the point estimate into a percentage change, the results imply that upstream tracts are around 60% closer to bridges than downstream tracts. The upstream tracts are consequently 27% closer to the nearest major land transport route (column 2 of Table 2), driven in approximately equal magnitude by distance to railroads and primary roads (Appendix Table C2). Figure 8 shows that the asymmetry in distance to a bridge is established by 1880 and persists thereafter.[27] Upstream tracts have thus had a local advantage with respect to proximity to a land transport route for more than a century.

[25] Or, historically, to cross on winter ice. Railroad tracks were even laid down on ice sometimes.

[26] Ferry crossings require shallow approach slopes to facilitate loading, while bridges are cheaper where the river is narrower and the ground material more stable, implying steep, rocky banks. To illustrate, Clark's Ferry in Iowa, with "good banks just opposite to an opening in the islands" was "the most convenient place to cross the Mississippi" between the Balize, in Louisiana, and Prairie du Chien, in Wisconsin (Lea, 1836) Long defunct, the site lies almost exactly equidistant from the two nearest bridges.

[27] I also re-estimate Equation 4.1 for log distance to a bridge in the year 2010, controlling in sequential regressions for log distance to a bridge in years from 1850 onwards. The independent estimated effect on distance to a bridge is insensitive to controlling for distance to a bridge before 1880 but attenuates progressively as I control for more recent values of distance to a bridge (Appendix Figure C13).

TABLE 2

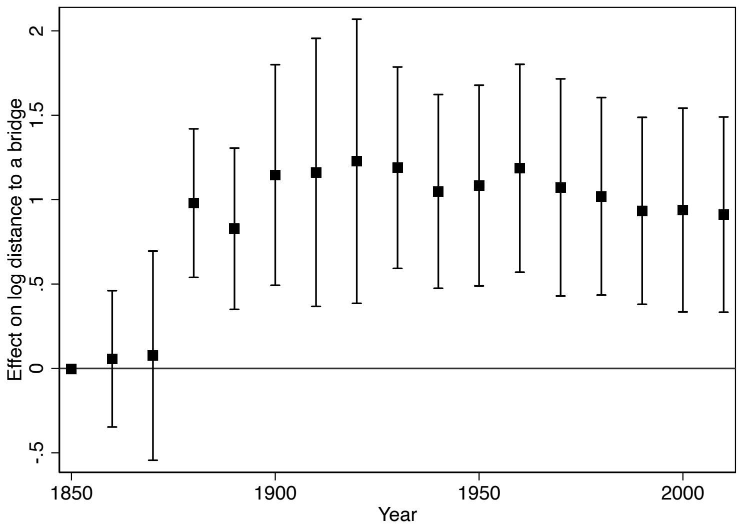
Transport infrastructure, population density, and per capita income around tributary confluences (2010)

	Log distance to a bridge	Log distance to land transport	Log per capita income	Log population density	Log income density
	(1)	(2)	(3)	(4)	(5)
Downstream	0.91***	0.32*	0.14**	−0.49**	−0.30
	(0.30)	(0.18)	(0.06)	(0.23)	(0.22)
N (census tracts)	1052	1052	1050	1051	1050
N (tributaries)	27	27	27	27	27

Notes: Estimates from regressions of listed outcome variable on downstream indicator, controlling for nearest-tributary fixed effects, river-distance from nearest tributary confluence, and its interaction with the downstream indicator. All outcomes measured in 2010. Sample contains year 2010 census tracts for which i) any part of the tract is within 10km of the Mississippi or Ohio rivers and ii) the centroid lies within 31.4km of a tributary confluence, excluding tributary confluences only matched to one census tract. Observations weighted using a triangular kernel in river-distance to nearest tributary confluence. Standard errors are cluster-bootstrapped by nearest tributary confluence (1000 replications). *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

FIGURE 8

Distance to a bridge around tributary confluences over time



Notes Point estimates from regressions of log distance to a bridge in the indicated year on downstream indicator, controlling for nearest-tributary fixed effects, river-distance from nearest tributary confluence, and its interaction with the downstream indicator. Sample contains year 2010 census tracts for which i) any part of the tract is within 10km of the Mississippi or Ohio rivers and ii) the centroid lies within 31.4km of a tributary confluence, excluding tributary confluences only matched to one census tract. Observations weighted using a triangular kernel in river-distance to nearest tributary confluence. Standard errors are cluster-bootstrapped by nearest tributary confluence (1000 replications).

Columns 3 and 4 of Table 2 show differences between upstream and downstream tracts in per capita income and population density in 2010. Converted to percentage changes, the better-connected upstream tracts have13% lower incomes and 63% higher population densities. Panels d and f of Figure 6 illustrate, showing corresponding asymmetries in the outcome variables at tributary confluences. Scaling the effect on income by the effect on distance to land transport suggests an elasticity of 0.44, considerably larger than the elasticity in the raw data near land transport routes. The effect on per capita income is more than offset by the effect on population density so that income density is higher in upstream census tracts, although upstream-downstream differences are not statistically different from zero (column 5 of Table 2 and Appendix Figure C14).

Table 3 confirms that estimating effects on the historical placebo outcomes yields null results, consistent with the graphical patterns in Figure 6. Only a subset of modern census tracts can be matched to county-level outcomes in 1840 and 1850, and the subset varies depending on the outcome. Estimating the main effects in the subsamples that can be matched to historical outcomes (column 2) yields similar coefficients to those from the full sample (column 1). Column 3 shows the estimated effects on the historical outcomes. Measurement error introduced by assigning county-level variables to census tracts might partly explain the absence of asymmetries in the 1840 and 1850 data, but the reduction in magnitude of the coefficients is still quite compelling. The main results are also robust to including historical outcomes as control variables (column 4).

TABLE 3
Tributary confluences: placebo tests and controls for historical conditions

	(1)	(2)	(3)	(4)
a) Per capita income				
Downstream	0.14**	0.17*	0.08	0.19*
	(0.06)	(0.10)	(0.09)	(0.09)
N (census tracts)	1050	544	545	544
N (tributaries)	27	20	20	20
b) Population density				
Downstream	−0.49**	−0.49**	0.10	−0.52**
	(0.23)	(0.23)	(0.09)	(0.22)
N (census tracts)	1051	1045	1046	1045
N (tributaries)	27	26	26	26
Outcome	2010	2010	Placebo	2010
Sample	Near	Historical	Historical	Historical
Historical controls	No	No	-	Yes

Notes: Estimates from regressions of listed outcome variable on downstream indicator, controlling for nearest-tributary fixed effects, river-distance from nearest tributary confluence and its interaction with the downstream indicator, and historical (or placebo) outcomes where indicated. Placebo outcomes are population density in 1840 and value of agricultural land in 1850. "Near" sample contains year 2010 census tracts for which i) any part of the tract is within 10km of the Mississippi or Ohio rivers and ii) the centroid lies within 31.4km of a tributary confluence, excluding tributary confluences only matched to one census tract. "Historical" sample comprises subsample that can be matched to historical county boundaries. Observations weighted using a triangular kernel in river-distance to nearest tributary confluence. Standard errors are cluster-bootstrapped by nearest tributary confluence (1000 replications). *** p<0.01, ** p<0.05, * p<0.1.

In principle, we could shed light on dynamics by extending this analysis further back in time. Unfortunately, however, geographical coverage of census-tract level data drops off rapidly before 1990 to a small, non-representative group of metropolitan areas (S Lee & Lin, 2018) Estimating effects in census-tract level data from 1990 or 2000 yields very similar point estimates to the 2010 data (Appendix Figure C15), showing that differences in both per capita income and population density are well-established. Matching historical county-level data on outcomes to current census tracts suffers from the same potential problem with measurement error as do the historical placebo tests. With that caveat, however, I detect no statistically significant differences in agricultural land values at any time, consistent with the results on income density, while differences in population density around tributary confluences begin to emerge in 1880, coincident with the differences in distance to a bridge, and peak twenty or thirty years later (Appendix Figure C16).

Most of the tributary confluences in the data are on the Ohio and Upper Mississippi, and point estimates are broadly similar for subsamples on both these rivers. The results are strongest for census tracts to the West of the Ohio, where access to bridges is plausibly more important for access to land transport routes (Appendix Table C3).

4.3. Robustness

Permutation tests There are 27 tributary confluences in the data. Although I bootstrap standard errors, resampling by tributary confluence, one might still be concerned about influential outliers (see, e.g., Young, 2022) To allay this concern, I randomly reshuffle the local orientation of half the tributary confluences in the sample (i.e., flip upstream to downstream and vice versa), over 1000 replications. These simulations impose the null of no consistent difference between upstream and downstream census tracts. I estimate effects in each simulated dataset. The distributions of simulated effects are centered at zero, and the observed effects lie in the tails of the distributions, confirming that they are unlikely to have arisen by chance (Appendix Figure C17).[28]

Varying exclusion distance The estimated effects on distance to a bridge and population density are largely stable across a range of cutoff distances for exclusion from the analysis, while the effect on per capita income decays with higher cutoff distances. I interpret this pattern of results as confirming that the negative relationship between proximity to land transport infrastructure and per capita income is primarily local (Appendix Figure C18).

State fixed effects and local topography Rivers are often jurisdictional boundaries, raising the concern that effects of state policies could confound the estimates. Further, the flow of rivers is determined by local topography, and local topography could also influence outcomes. However, results are similar when I add controls for state fixed effects, slope, or elevation (using data from USGS 2020, see Appendix Table C3)

5. Evidence from when bridges are built

5.1. Empirical strategy

The analysis in the preceding section measures the long-run effects of proximity to land transport infrastructure at a very local scale, but it does not tell us how these effects emerge over time or about effects over larger spatial scales. To shed light on both these

[28] The percentages of the simulated effects that exceed the observed effect in absolute magnitude are: 0.1%, for distance to a bridge; 1.8%, for per capita income; and 2.4%, for population density.

questions, I exploit variation in *when* bridges are built, leveraging two key facts about bridges.

First, planning, financing, designing, and building major bridges takes a very long time, with many potential obstacles. Years before construction or even design begins, stakeholders must solve the complex collective action problem of determining who would fund a bridge and how to raise the required capital. Negotiations are often complicated by the involvement of several fiscal and political jurisdictions. Further, every bridge is unique, designed specifically for local conditions. Design and construction each take several years or more, with frequent delays, not least because bridges under construction are vulnerable to extreme weather events. Decades often pass between the first proposal for a bridge and the day it opens, as the following examples illustrate:[29] A charter to construct the Wheeling Suspension Bridge was issued in 1816 but the bridge was only completed in 1849 (Plowden, 1974) The need for a bridge at St Louis was established by 1836 but construction only began in 1867 and was completed in 1874 (Plowden, 1974) The Memphis and Arkansas Bridge, completed in 1949, was nicknamed the "Eleven-Year Bridge" because of how long it took to build (Cordell, 2011) A committee formed in 1946 to plan a bridge linking West Tennessee to Missouri, but approval for what would be the Caruthersville bridge was only obtained in 1964. Construction began in 1969 and was completed in 1976 (Cordell, 2011) Planning for the Stan Musial Veterans Memorial Bridge at St Louis began in or before 1991; construction began in 2010, completed in 2014. The result of these idiosyncratic delays is that the exact time that bridges finally open is plausibly exogenous within a window of decades.

Second, when bridges open, they create sharp changes in feasible journeys and travel times that are largely realized in a single day. While this impact may evolve over time with the construction of complementary infrastructure, bridges were often the crucial final link that made a journey possible.[30] This property is captured evocatively in the names of bridges, such as the Short Line Bridge or the Short Route Bridge.[31] The result is that bridge openings create abrupt and substantial changes in land transport networks whose effects can be distinguished from long-run trends.

To measure the impact of bridge openings, I construct a panel dataset at the county level between 1860 and 2010. The county is the most disaggregated level for which data are available over the full historical period of interest. Setting the baseline year to 1860 represents a compromise between maximizing spatial and temporal coverage.[32] I include counties in the sample if they are adjacent to the Mississippi or Ohio Rivers within the limits covered by the bridge dataset (Figure 2b). To account for changes in administrative boundaries, I remap all data back to 1860 boundaries.[33] The final sample is a balanced

[29] It is difficult to systematically document this lag time. Bridges are often only named after construction. Identifying the earliest reference to a bridge is thus virtually impossible without extensive detective work.

[30] E.g., the Canton Viaduct: completed in 1835, the first Boston-Providence train ran 24 days later.

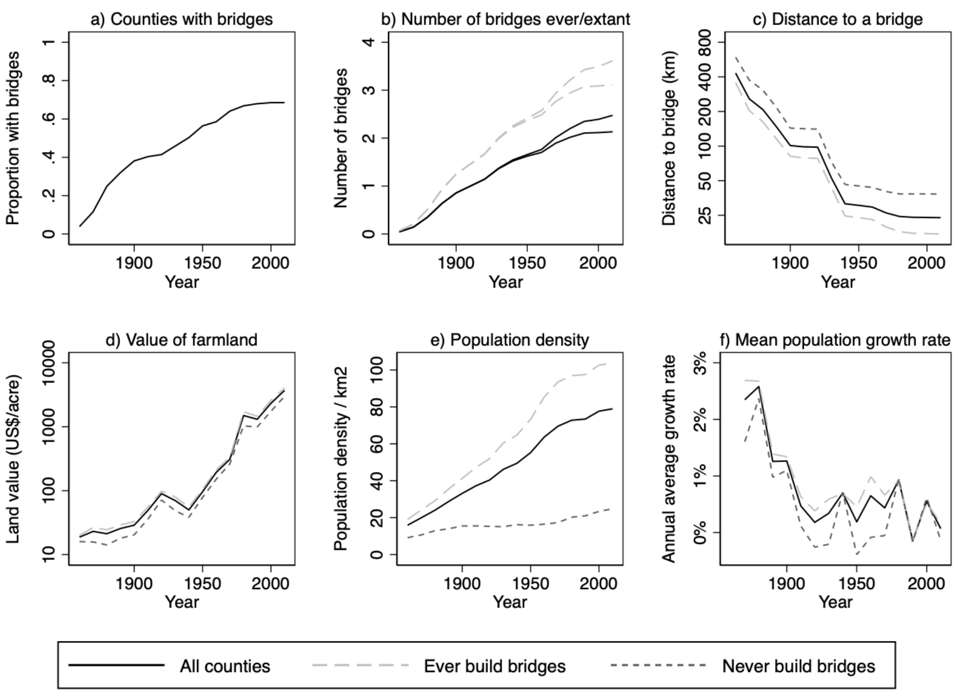
[31] The Short Line Bridge connects St Paul and Minnesota. The Clarksburg-Columbus Short Route Bridge no longer exists: it was first renamed and then replaced.

[32] The United States has collected census data since 1790 with varying coverage. Before 1860, coverage is considerably more restricted. The results remain consistent with a start date of 1840 (before any bridge construction) or 1880 (excluding the Civil War and the last decade of slavery), or an end date of 1960.

[33] Specifically, if counties have separated, I aggregate information back to the baseline boundaries by summing totals or taking spatially-weighted mean values. If counties have merged, I assign outcomes to the original counties according to the proportion of the merged county area corresponding to the original county.

panel of 181 counties in 14 states over 150 years. Figure 9 plots the share of counties that build bridges, the mean number of bridges, and distance to a bridge over the study period.[34]

FIGURE 9
County sample: descriptive statistics 1860-2010



Notes Data shown for main sample of counties (N = 181), as well as for subsamples of counties in which bridges have ever been constructed (N = 124) and in which bridges are never constructed (N = 57).

In this panel dataset, I estimate the following distributed lag model:

$$w_{i,t} = \gamma_t + \alpha_{0i} + \alpha_{1i} t + \alpha_{2i} t^2 + \sum_{j=0}^{k} \beta_j \Delta dist_{i,t-j} + \epsilon_{i,t} \tag{5.2}$$

where $w_{i,t}$ is an outcome variable in county i at a time t; γ_t is a year fixed effect that flexibly captures global trends; α_{0i}, α_{1i}, and α_{2i} are county-specific parameters that capture local long-run trends; $\Delta dist_{t-j}$ is the change in log distance to a bridge j time periods before time t; and the coefficients of interest β_j capture the cumulative effect on the outcome variable at time t of a change in distance to a bridge j periods ago.

The identifying assumption is that the exact *timing* of changes in distance to a bridge is exogenous to short-run deviations from local long-run trends in outcome variables. In

[34]Distance to a bridge measures the distance between a county's centroid and the nearest bridge. Appendix Table C4 provides additional summary statistics.

formal terms, the identifying assumption is that:

$$\begin{aligned} E(\epsilon_{i,t}|\Delta dist_{i,t+k},\alpha_{0i},\alpha_{1i},\alpha_{2i},\Gamma) &= 0 \\ t &= 1860,1870,...,2010 \\ k &= -30,-20,...,20,30 \end{aligned} \tag{5.3}$$

where Γ is the vector of time fixed effects. This is locally equivalent to assuming that $E(y_{it}|\alpha_{0i},\alpha_{1i},\alpha_{2i},\Gamma)=\gamma_t+\alpha_{0i}+\alpha_{1i}t+\alpha_{2i}t^2$ for $t=1860,1870,...,2010$.

The main specifications use county-specific fixed effects and quadratic trends to approximate the long-run counterfactual trends. As Figure 9 shows, counties in which bridges are built have higher population densities and higher land values in 1860, and their populations grow at faster rates throughout most of the study period.[35] The county-specific trend controls account for all unobservable and observable differences in initial conditions, growth rates, and linear trends in growth rates over time. In robustness tests, I show that the choice of quadratic trends is not critical; the results are similar if I use an equivalently conservative piecewise linear spline instead.

This empirical strategy only allows me to consistently estimate the first few β_j terms following a change in distance to a bridge, for two reasons. First, fitting the long-term quadratic trends absorbs any persistent effects. Second, the identifying assumption is only plausible within a window of about plus or minus three decades. These constraints imply that longer-run effects cannot be estimated using this approach.

Because the $\Delta dist_{t-j}$ terms are serially correlated, I include additional lags of $\Delta dist_{t-j}$ in the specification beyond those for which the identifying assumption holds. I select the lag length k for each outcome variable by varying the number of lags included and selecting the lag length for which the lead coefficients are closest to zero, to ensure that effects are measured relative to pre-bridge construction trends.[36]

Changes in distance to a bridge are primarily driven by new bridges opening, but bridges may also close or be destroyed, either unexpectedly or according to plan. Unexpected bridge closures or destructions are typically driven by plausibly exogenous factors, such as extreme weather conditions[37] or safety concerns.[38] When bridge closures are planned, the bridges are usually replaced nearby, resulting in minimal changes in distance to a bridge.

The identifying assumption would be violated if the opening of a bridge at a given time were correlated with short-run deviations from county-specific trends in outcome variables. For example, the estimates would be biased upward if policy-makers decide to build bridges as stimuli after periods of relatively low growth or following the start of a "boom". If so, I might mistake a regression to mean, in the first case, or the dynamics of the "boom", in the second, for an impact of bridge construction. On the other hand, if policy-makers build bridges in response to several decades of growth, regression to mean

[35] Appendix Table C5 provides further details.

[36] Serial correlation in the $\Delta dist$ terms is negative, primarily because distance to a bridge is bounded at zero. Counties that previously experience a large reduction in distance to a bridge can later only experience small reductions in distance to a bridge unless a bridge is destroyed. See Appendix Figure C19 and Appendix Table C6.

[37] E.g., the Pink Bridge at Fort Ripley, Minnesota, destroyed in 1947 by high water and an ice jam.

[38] E.g., after the Silver Bridge between Point Pleasant, West Virginia and Gallipolis, Ohio collapsed in 1967—because a single eyebar in a suspension chain failed—the nearby Clarksburg-Columbus Short Route Bridge was closed. With a similar design, it was considered vulnerable to similar failures.

would tend to bias my estimates downwards. However, in my preferred specifications, future changes in distance to a bridge are not systematically correlated with current values of the outcome variable, suggesting that the estimates are, on average, unlikely to be affected by these biases.

Although sites that are well-suited to ferry crossings are not necessarily well-suited to bridge crossings, changes in distance to a bridge could still interact with distance to a ferry crossing in other ways. First, bridges often immediately replaced or superseded ferry crossings.[39] Second, ferry boats could be and were sold onwards when a ferry crossing ceased operation in a particular location (see, e.g., Thomas, 1980) Both these interactions bias the estimated effects downwards. If the original ferry crossings led to growth, then the fitted trends will reflect these impacts, obscuring the impact of the replacement bridge, while ferries that were sold onwards likely created compensatory improvements in transport access in locations that are distant from the opened bridge.

To draw the correct inference about statistical significance, I cluster standard errors by county, allowing for arbitrary residual correlation over time between observations from a county (Angrist & Pischke, 2009; Bertrand, Duflo, & Mullainathan, 2004) a conservative approach given the 150-year time horizon. Additionally, I account for spatial correlation over 200km using Conley (1999) standard errors, adapting code from Hsiang (2010) The results are also robust to an alternative approach to inference based on the distribution of plausible counterfactual changes in distance to a bridge (see Borusyak & Hull, 2023)

The estimated effects are a weighted average of effects that are potentially heterogeneous across space and time. The implicit weighting function places most weight on groups with the highest residual variance in distance to a bridge (Deaton, 1997; Gibbons, Serrato, & Urbancic, 2019; Solon, Haider, & Wooldridge, 2015) These groups comprise times and places that experienced large changes in distance to a bridge (Appendix Figure C20).

Few variables are recorded consistently over the full 150-year study period. To measure economic activity, I use land values, an imperfect but commonly-used proxy for total economic activity. In simple theoretical models, land values correlate with total economic activity.[40] Specifically, I use the average value of agricultural land, which is recorded consistently across the full study period.[41] Beyond simple static models, we can more generally interpret values of agricultural land as capturing the productive value of land

[39]See, for example, the Lutcher-Crescent Ferry in Louisiana, closed when the Veterans Memorial Bridge opened in 1990 (Times-Picayune, 2012) Cooks Ferry in Pennsylvania, closed when the Shippingport Bridge opened in 1964 (The Historical Marker Database, 2022) and the McGregor Ferry, replaced by the Prairie du Chien suspension bridge one mile upstream in 1932 (City of McGregor, n.d.)

[40]Rent for a unit of land equals αY, where α is land's factor share and Y is total production per unit land area. Rent is capitalized in land value V_z as $\frac{1}{r}\alpha Y$, where r is the interest rate. If α and r do not vary with distance from a bridge d then $\frac{\partial \ln V_z}{\partial \ln d} = \frac{\partial \ln Y}{\partial \ln d}$, and the elasticity of land values with respect to distance from a bridge is equal to the elasticity of production with respect to distance from a bridge.

[41]I use data from the Inter-university Consortium for Political and Social Research (ICPSR; M R Haines & ICPSR 2010; M Haines, Fishback, & Rhode, 2018) Donaldson and Hornbeck (2016a) use the total value of agricultural land, in preference to the average value of agricultural land, to avoid missing effects on total agricultural production that arise from bringing unproductive land into cultivation. Over the much longer period of this study, during which there is extensive urbanization and structural transformation, it seems preferable to focus on the average value of agricultural land. Prices of agricultural land also internalize land improvements such as fencing, irrigation and buildings. While Donaldson and Hornbeck (2016a) apply corrections for these investments, I cannot do the same over the much longer time period of this study.

on the margin. To infer effects on per capita economic activity, I also track effects on population growth.

5.2. Results

Table 4 shows the results. Each point estimate can be interpreted as the cumulative effect on the outcome variable at time t of a change in distance to a bridge j years ago. Over thirty to forty years after a change in distance to a bridge, the value of agricultural land, which proxies for total economic activity, gradually increases (column 1), as does the population (column 3).[42] Point estimates are negative because reducing distance to a bridge is correlated with growth in population and land values. Converting the estimated elasticities into percentage changes, a 50% reduction in distance to a bridge leads cumulatively to approximately 9% higher land values and 5% greater population after thirty to forty years.

I also estimate Equation 5.2 including lead (future) changes in bridge distance (columns 2 and 4). The coefficients on future changes in distance to a bridge are all close to zero, confirming that I estimate impacts relative to ongoing trends. Figure 10 plots the point estimates and confidence intervals from these regressions. I reverse the y-axis so that the graphs are intuitively easier to interpet; a rise in the outcome variable after a reduction in distance to a bridge is shown as a rise on the figure. The point estimates on the coefficients of interest do not change when I include the lead variables. The main effect of introducing these irrelevant variables is to inflate the standard errors on the coefficients of interest. For this reason, I exclude the lead variables from the main analyses.

Using the value of agricultural land as a proxy for total economic activity may potentially over- or underestimate the true effects. Three attenuating factors increase the likelihood that the effects are underestimates: 1) measurement error; 2) the fact that, while the value of agricultural land may equal the value of urban land at the urban-rural frontier (as in a monocentric city model, e.g., Alonso, 1964; Mills, 1967; Muth, 1969) it almost certainly understates the average value of land in urban areas; and 3) a downward bias that arises when controlling for long-run trends, as long as longer- and shorter-run effects have the same sign.[43] With the caveat that they apply over a smaller spatial scale, the effects in section 4 take the same sign as the corresponding effects in this section, suggesting that the most likely effect of any persistent longer-run effects is to bias the estimated effects towards zero.

That population grows as land values increase suggests that the effects on economic activity should be smaller in per capita terms than those suggested by the effects on land values. In simple static models, the same assumptions that rationalize land values as a proxy for total economic activity suggest a proxy for per capita economic activity, the *difference between the log value of agricultural land and log population.*[44] Estimating effects on this proxy for per capita economic activity indeed yields positive effects that are smaller than those on total economic activity (Appendix Figure C22 and Appendix Table C7).

[42] With county fixed effects, effects on log population or log population density are identical.

[43] If shorter- and longer-run effects are both positive, the longer-run positive effects lead to a steeper fitted county-specific trend, which absorbs part of the true shorter-run effects, resulting in smaller estimated shorter-run effects. Appendix Figure C21 illustrates.

[44] Since per capita production $y = \frac{Y}{L}$, where Y is production per unit land area and L is population per unit land area, then $\frac{\partial \ln y}{\partial \ln d} = \frac{\partial \ln Y - \ln L}{\partial \ln d} = \frac{\partial \ln Y}{\partial \ln d} - \frac{\partial \ln L}{\partial \ln d} = \frac{\partial \ln V_z}{\partial \ln d} - \frac{\partial \ln L}{\partial \ln d} = \frac{\partial \ln V_z - \ln L}{\partial \ln d}$.

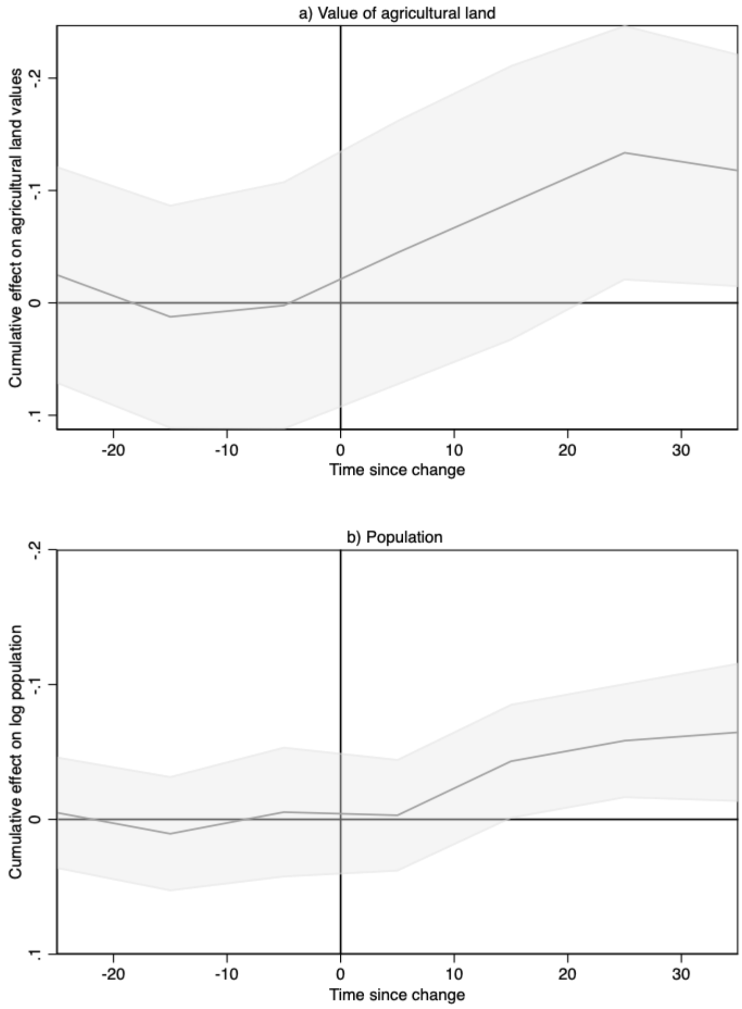
TABLE 4
Cumulative effects on population and land values following change in distance to a bridge

Effect of change in log distance to a bridge between:	On log value agricultural land at time t		On log population at time t	
	(1)	(2)	(3)	(4)
t + 20 and t + 30		−0.025		−0.005
		(0.049)		(0.021)
t + 10 and t + 20		0.012		0.011
		(0.051)		(0.022)
t and t + 10		0.002		−0.005
		(0.056)		(0.025)
t - 10 and t	−0.045	−0.045	−0.002	−0.003
	(0.031)	(0.060)	(0.017)	(0.021)
t - 20 and t - 10	−0.090**	−0.089	−0.043***	−0.043**
	(0.040)	(0.063)	(0.016)	(0.022)
t - 30 and t - 20	−0.134***	−0.134**	−0.058***	−0.058***
	(0.037)	(0.058)	(0.018)	(0.022)
t - 40 and t - 30	−0.118***	−0.118**	−0.064***	−0.065**
	(0.038)	(0.053)	(0.023)	(0.026)
t - 50 and t - 40	−0.097***	−0.097**	−0.051**	−0.051**
	(0.031)	(0.044)	(0.020)	(0.022)
t - 60 and t - 50	−0.109***	−0.109***	−0.037**	−0.037**
	(0.031)	(0.040)	(0.014)	(0.015)
t - 70 and t - 60	−0.104***	−0.102***		
	(0.029)	(0.034)		
Joint p value lead coefficients:		0.762		0.848
Joint p value lag coefficients:	0.003	0.005	0.013	0.027
N	2891	2891	2896	2896

Notes: Coefficients from regressions of listed outcome variables on lead and lag changes in log distance to a bridge, year fixed effects, county fixed effects, and county-specific quadratic trends. Sample consists of counties on the rivers Mississippi and Ohio, unweighted, using constant 1860 boundaries. Standard errors clustered by county and robust to spatial correlation within 200km. Joint test of lead coefficients tests joint significance of lead effects; joint test of lag coefficients tests joint significance of contemporaneous and first three lagged effects. *** $p<0.01$, ** $p<0.05$.

Figure 10

Population density and values of agricultural land before and after a change in distance to a bridge



Notes Coefficients from a regression of log outcome variable on lead and lagged changes in log bridge distance, year fixed effects, and county-specific quadratic trends. Time zero is defined as the beginning of the decade in which the change in distance takes place. Coefficients on future changes in bridge distance are shown to the left of the black line, and coefficients on contemporaneous and lagged changes in bridge distance are shown to the right of the black line. Sample consists of counties on the rivers Mississippi and Ohio, unweighted, using constant 1860 boundaries. Standard errors are clustered at the county level and robust to spatial correlation within a 200km radius. Y-axis is reversed.

A natural question is whether effects are directly attributable to the presence of land transport infrastructure or whether they are shaped by agglomeration feedback effects. I re-estimate the effects using an equivalently conservative specification in first differences with county-specific linear trends (Appendix Table C8). This specification allows me to control flexibly for lagged population density (Michaels, Rauch, & Redding, 2012) effectively narrowing the empirical comparison to places with a similar density at a given moment in time. Controlling for lagged population density does not change the estimated effects of changes in distance to a bridge on either growth in population density or land values (Appendix Table C9). These results suggests that the estimated effects are most likely driven directly by access to land transport infrastructure rather than agglomeration forces.

The estimates nest both effects of distance to a rail bridge and distance to a road bridge. When estimated separately or jointly, the effects of distance to a rail bridge are slightly stronger than the effects of distance to a road bridge, but they not statistically distinguishable (Appendix Table C10). Effects are similar for the first and second half of the study period (Appendix Table C11). The overall effects are primarily driven by counties on the Lower Mississippi, where bridges are most sparse, and specifically by counties on the Western banks of the Mississippi, where access to land transport routes may be most constrained by bridges (Appendix Table C12). Effects on land values do not depend on initial distance from a bridge, but effects on population density are larger for counties initially at higher distances from a bridge, for which proportional (log) changes in distance to a bridge translate into greater absolute changes in distance (Appendix Figure C23).

Including only county fixed effects or only county-specific linear trends in the same regressions results in larger estimated lead coefficients that differ systematically from zero (Appendix Figure C24), suggesting that the exclusion restriction might not hold with less conservative controls for local counterfactual trends.

5.3. Robustness

Spillover effects only The estimated effects could be biased upwards if policy-makers construct bridges in anticipation of higher-than-average future growth. To evaluate whether this drives the results, I evaluate effects in counties in which no bridges are ever built, which are only affected by bridges built elsewhere. The results are very similar to those for counties in which bridges are built, suggesting that county-specific anticipated growth is unlikely to explain the results (Appendix Table C13).

More flexible time trends The estimated effects would also be biased upwards if policy-makers construct bridges in anticipation of regional growth or simultaneously implement other regional policies that also promote growth. To generate the observed results, the timing of these changes would need to be sufficiently tightly correlated with the bridge openings to create simultaneous discontinuities in trends. An example would be if states offered firms subsidies when bridges opened.

To evaluate the extent to which such trends could influence the estimated effects, I allow global time trends to vary geographically with increasing flexibility—by river basin, smoothly over space, or by state—in addition to the baseline county-specific quadratic trends. Adding these more conservative sets of controls progressively attenuates the estimated coefficients, though they remain consistent in sign and timing. This pattern of attenuation could support an alternative explanation whereby the estimated effects are affected by state- or geographically-varying shocks that are correlated in time with bridge construction. However, including additional fixed effects also exacerbates attenuation bias through measurement error (see, e.g., Deryugina & Hsiang, 2014; Fisher, Hanemann, Roberts, & Schlenker, 2012)

To help understand why the effects attenuate, I calculate the extent of measurement error that is consistent with the observed attenuation, under the stark but useful assumptions that measurement error is classical and local (Appendix B).[45] Across different specifications for global time trends and different outcome variables, the estimated fraction of measurement error lies consistently between 17 and 21% (Appendix Table C13). Given that distance to a bridge is an imperfectly measured proxy, I conclude that measurement error may be an important explanation for the observed attenuation of the estimated effects.

Non-random exposure to exogenous shocks Even if bridge openings and closures are exogenously timed, the resulting changes in distance to a bridge have a non-random component that depends on the location of other bridges and geography more generally. This creates the potential for omitted variables bias and non-standard inference problems (Borusyak & Hull, 2023) While concerns about omitted variables bias should be allayed by the absence of correlations between future changes in distance to a bridge and present values of the outcome variables, non-standard inference problems could still present a concern.

To address this concern, I follow Borusyak and Hull (2023) and simulate the distribution of counterfactual shocks. I randomly vary the construction date of each bridge within a window of ±35 years, 500 times, and recalculate the full history of distance to a bridge for each county under each counterfactual. I then compare the realized point estimates to the distribution of point estimates obtained from substituting each simulated counterfactual history into the right hand side of Equation 5.2. The realized point estimates lie in the extreme tails of these distributions, confirming that they are unlikely to have occurred by chance (Appendix Figure C25). The point estimates are also very similar if I re-estimate the main effects controlling for the expected value of changes in log distance to a bridge obtained from the simulations, as also proposed by Borusyak and Hull (2023) (Appendix Table C13).

Additional robustness tests The estimated effects are larger if estimated in first differences with county-specific linear trends (Appendix Table C8) but these specifications deal less effectively with differential pre-trends (Appendix Figure C26). Results are insensitive to changing the start and end dates of the study period (Appendix Table C14).

6. Reconciling the results

Per capita income increases with distance from land transport routes over smaller spatial scales and decreases with distance over large spatial scales (section 2). Both patterns can be causally linked to land transport infrastructure (sections 4 and 5). However, many potential mechanisms could contribute to explaining these effects, which emerge over decades, or longer. That population density also responds strongly to land

[45]Specifically, I run an auxiliary regression which regresses the change in distance to a bridge on the controls for each specification. I report the R^2 from these regressions, a measure of how much of the variation in distance to a bridge is absorbed by the regression controls. Then, using the residuals from these regressions, I calculate the percentage of the variance in distance to a bridge which the attenuation of the coefficient implies to be measurement error.

transport infrastructure already suggests that infrastructure affects not only the level of economic activity but also the spatial structure of the economy.

To shed light on how effects emerge, I exploit the richness of US census data to extend the analyses in sections 4 and 5 to a wide range of secondary outcome variables, harmonizing variables that are measured inconsistently across time in the panel data.[46] The secondary outcome variables include urbanization, the industrial composition of the workforce, housing and commuting, education, occupations, and demographics. In the interests of brevity, I summarize these results here and provide detailed results in the appendices.

The results are consistent with transport infrastructure providing economic advantages that led to city formation and structural transformation, shaping the pattern of decreasing income with distance from the advantages of land transport routes over large spatial scales. As the cities that formed expanded, however, wealthier households differentially sorted away from city centres towards the suburbs, creating the pattern of lower incomes immediately adjacent to land transport routes and the peak in incomes several kilometers away.

Cities form around transport routes After counties experience a change in distance to a bridge, growth in population is concentrated in towns and cities. Employment in agriculture gives way to urban occupations: primarily retail and wholesale, and to some extent manufacturing and services. A boom in construction employment suggests physical expansion of the built-up area (Appendix Figure C27 and Appendix Table C15).

Cities expand outwards Census tracts that lie upstream of tributary confluences—which are closer to land transport routes—today have a larger share of the oldest housing stock, suggesting that they were established earlier. Their downstream neighbours have a larger share of newer housing stock, dating, in particular, from the era of suburbanization (Redding, 2022) and suggesting later expansion (Appendix Figure C28).

Suburbanization Households living upstream and downstream of tributary confluences differ in ways that are consistent with differences between city centres and their suburban or peri-urban surrounds. In the more-densely populated upstream census tracts, households are more likely to work in retail or services, rather than manufacturing, and to live in smaller housing units and apartment blocks; they spend less time commuting and commute more often by public transport; and they are exposed to slightly higher levels of air pollution.[47] Households downstream live in larger, more expensive single-household structures, where the higher rents and property values are explained by the characteristics of the housing stock rather than underlying land values, and workers are more likely to commute by car. Few households either up- or downstream live in areas classed as rural or work in agriculture (Appendix Figures C29 to C30 and Appendix Tables C16 to Table C19).

Demographics The reorganization of the population across space also raises the possibility that part of the observed relationship between per capita income and land transport

[46]In addition to data from the NHGIS and ICPSR, I also construct some county-level variables using individual data from the Integrated Public Use Microdata Series (IPUMS; Ruggles et al., 2025; Ruggles et al., 2024) If counties have merged, I assign households from the merged county to both of the original counties, with a weight corresponding to the proportion of the merged county area corresponding to the original county, before aggregating data at the baseline county level.

[47]Pollution data are from Colmer (2020), see Colmer, Hardman, Shimshack, and Voorheis (2020).

infrastructure reflects differences in the population. In modern census data, education levels in the population peak around the same distance from land transport routes as per capita income while the share of the population that is foreign-born or non-white peaks near land transport routes, declines sharply until about 10km away from transport routes, and then flattens out (Appendix Figures C33 and C34).

Causal estimates reflect broadly similar patterns. When counties experience a change in distance to a bridge, the potentially more geographically mobile foreign-born population grows more rapidly than the native-born population. The non-white population grows more rapidly than the white population, possibly reflecting greater mobility of the non-white population during the Great Migration (Black, Sanders, Taylor, & Taylor, 2015) Education levels and the share of workers in white-collar occupations also rise, suggesting that higher-skilled workers migrate inwards, or that skill acquisition is easier or more strongly incentivized (van Maarseveen, 2021) Consistent with these possibilities, the poorer households in the city-centre-like upstream census tracts are less educated and work in lower-skilled occupations than their downstream, more suburban neighbours, although they are if anything more educated than predicted by household income. They are also more likely to be foreign-born (Appendix Figures C35 to C38 and Appendix Tables C20 to C23).

7. Discussion and conclusions

Bridges over major rivers create sharp and distinctive spatial patterns in access to land transport infrastructure. Exploiting quasi-experimental variation in the timing and location of bridge construction, I estimate the causal effects of access to land transport infrastructure over different temporal and spatial scales. Reflecting patterns in cross-sectional data, land transport infrastructure increases economic activity over large spatial scales but has negative effects on per capita income over small spatial scales. A narrative explanation that reconciles these results suggests that land transport infrastructure provided economic advantages that led to urbanization and structural transformation, cities formed around historical transport routes, and then within-city patterns of sorting between city centres and suburbs locally reversed the effects on per capita income.

This paper opened with the problem of how to efficiently target transport infrastructure investments. A policymaker looking for guidance on where and how much to invest in land transport infrastructure may find the answers here unsatisfying. The results illustrate how transport infrastructure acts not only to increase economic growth but also to reorganize the spatial structure of the economy. If relocation effects are important, and what we observe is that better-connected areas grow relative to worse-connected areas—as in the analyses in section 5 here, for example—we cannot be certain whether the worse-connected areas would have declined, remained stable, or grown but to a lesser extent, in the counterfactual scenario in which the infrastructure investments were not made.

To estimate the aggregate effects of land transport infrastructure investments on growth requires a structural economic model, a formalization of a set of assumptions about the underlying forces shaping the economic effects of transport infrastructure, which can then be calibrated to patterns in the data. The results in this paper may help inform future structural analyses. The variation in effects across space confirms that models at a granular scale may ultimately need to account for both between-city and within-city processes in a unified framework to correctly reproduce patterns in the data, as in Monte, Redding, and Rossi-Hansberg (2018) The gradual evolution of effects seen in section 5 also suggests that models may need to account for intermediate levels of mobility that take time to play out, as do Bryan and Morten (2019)

The results do provide guidance on how to interpret patterns of development around transport routes. Urban poverty and suburban wealth may be peculiarly American phenomena, and differences between better- and worse-connected areas may evolve differently over space and time in other contexts. A plausible scenario in modern developing countries is for informal settlements to emerge around new land transport infrastructure. Poorer households may be attracted by the connectivity advantages and willing to tolerate the disutility of living near transport routes, given the alternatives available to them, while land clearance to build infrastructure may leave pockets of uninhabited land that are ripe for informal settlement. Such settlements may indeed be a direct consequence of the economic success of the transport infrastructure development. Anticipating and planning for such a response, however—for example, by pre-emptively installing service infrastructure such as water and sewerage lines—might attenuate the negative welfare consequences of rapid unplanned development.

While this paper sheds light on *how* patterns of economic activity around land transport infrastructure emerge, the results do not fully explain *why* the effects emerge. For example, although the results suggest economic advantages of transport infrastructure, they do not identify which advantages are most attractive to households and firms: lower prices for imported consumption goods or inputs, lower commuting or mobility costs, higher prices for locally produced goods for export, or all of these. The results also do not speak to whether within-city sorting emerges through pro-active migration of the pre-existing population or through differential location choices of new migrants (S K Lee, 2020) nor to the role played by pollution disamenities (Brinkman & Lin, 2024) or discriminatory policies, such as redlining, that other literature emphasises as critical in shaping the history of segregation in US cities (Weiwu, 2025) These are questions that this paper leaves to ongoing and future research.

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Data availability statement. Data and code to replicate results from the paper and supplementary appendices are publicly available on Zenodo at https://doi.org/10.5281/zenodo.17085642 (Tompsett, 2025) The paper uses data from two ICPSR datasets that assemble and harmonize census data (M R Haines & ICPSR 2010; M Haines et al., 2018) ICPSR licensing does not permit distribution of these datasets, but downloading these data is free to affiliates of ICPSR member institutions. I also use full count census data from IPUMS USA (Ruggles et al., 2024) to construct county-level average values for some census variables that were not otherwise available. County-level averages are included in the replication package, along with instructions to recreate these from the full count data, which are freely available upon creating an account. All other sources of data are included in the replication package.

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