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On the road: Access to transportation infrastructure and economic growth in China[☆]

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ABSTRACT

This paper estimates the effect of access to transportation networks on regional economic outcomes in China over a twenty-year period of rapid income growth. It addresses the problem of the endogenous placement of networks by exploiting the fact that these networks tend to connect historical cities. Our results show that proximity to transportation networks have a moderately sized positive causal effect on per capita GDP levels across sectors, but no effect on per capita GDP growth. We provide a simple theoretical framework with empirically testable predictions to interpret our results. We argue that our results are consistent with factor mobility playing an important role in determining the economic benefits of infrastructure development.

"A key issue [on whether railroads benefit economic development], however, is whether such railroad influence was primarily exogenous or endogenous, whether railroads first set in motion the forces culminating in the economic development of the decade, or whether arising in response to profitable situations, they played a more passive role." – Albert Fishlow, *American Railroads and the Transformation of the Ante-bellum Economy*, 1965 pp. 203

1. Introduction

Transportation infrastructure is often mentioned as a key to promoting growth and development. The argument relies on the simple logic that one first needs to have access to markets and ideas before one can benefit from them. This belief is supported by the observation that the historical construction of infrastructure, such as railroads, coincided with periods of rapid economic growth in Western Europe, Japan and the United States. Today, it is indisputable that richer countries have dramatically better transportation infrastructure than poorer ones. However, policymakers considering the trade-offs of investing in infrastructure must consider several related questions. First, they must consider the question of causality: is infrastructure development a worthwhile object of policy, or is it better to rely on the natural forces of the market and/or competition between local jurisdictions to endogenously provide the necessary infrastructure in response to demand? For example, Fogel (1962, 1964) famously argues that one of the most frequently mentioned historical innovations in transportation infrastructure, the railroad, was less effective for economic development in the United States than the pre-existing river networks and that this misdirected investment was a result of government policies for promoting railroads.

Second, policymakers are typically concerned about the distributional effects of infrastructure, which are by no means obvious. On the one hand, for fixed factor endowments, the increased access to markets and ideas should benefit all regions. For example, in the historical context of the United States, it has been argued that transportation infrastructure gives rise to more cities, which then turned into "engines" of

☆ This paper updates and supersedes "The Railroad to Success: The Effect of Access to Transportation Infrastructure on Economic Growth in China" (Banerjee et al., 2004), which used the same basic empirical strategy, but substantially less data. We are grateful to Naomi Lamoreaux, Tom Rawski, Thomas Piketty and the participants at the 2004 MacArthur Network for Inequality Conference in Beijing, the China Summer Institute, and the 2011 IGC Conference in London for very helpful comments. We thank Zhichao Wei, Gongwen Xu and the large team they assembled to collect the data; Ricardo Dahis, Zhentao Jiang and Joris Mueller for excellent research assistance; and Giovanni Zambotti and Ceren Baysan for invaluable assistance with GIS. We acknowledge financial support from the IGC.

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growth for the country as a whole.[1] On the other hand, transportation infrastructure increases the access of rural regions to cities, and the well-known agglomeration effects of cities may cause productive capital and skilled labor to move from rural regions to cities over time, with the result that those who remain in rural areas receive very limited benefits from urbanization or even become impoverished. Along similar lines, it has been argued that the expansion of motor road networks in the United States promoted large-scale suburbanization and left many cities without a viable economic model (Glaeser and Gottlieb, 2009).

This paper makes progress in understanding the impact of access to transportation infrastructure by examining the causal effect of access on economic performance in different regions in China during a twenty-year period of rapid growth. We ask the straightforward question: do areas that are "quasi-randomly" assigned to have better access to transportation networks consequently have better economic outcomes in the long run? Specifically, we attempt to empirically examine two closely related questions. First, we ask whether access to better transportation enriches the *average region* that is affected (because it draws in or generates more new economic activities) or impoverishes it (because it becomes easier for human and physical capital to exit). Second, we ask whether areas that have better access to transportation networks benefit much more and serve as engines of growth when new economic opportunities arise and growth becomes possible after 1979.

For our discussion, it is important to keep three points in mind. First, our focus is on long term effects. We are not only interested in the impact on trade and prices that result from greater access, which tend to be relatively quick, but also in the subsequent changes in the patterns of localization of economic activity as people and factories relocate. Second, the emphasis on understanding the effect of infrastructure for the *average* location is crucial to our study since it is entirely possible that some of the largest cities benefit from infrastructure through greater concentration of resources while most other places lose out. Finally, there cannot be one definitive answer to these questions, since the answer will clearly depend on the starting point – i.e., the first road to connect the agricultural hinterland to a port is very different from the fifth such road.

We use county-level economic data from China to try to answer these questions. In many ways, China offers an ideal setting for our work. In the late 19th and early 20th century, the Chinese government and a set of Western colonial powers built railroads connecting the historical cities of China to each other and to the newly constructed "Treaty Ports".[2] We identify our average "treated" areas to be those that were close to the straight line connecting this set of cities. Our analysis excludes the terminal cities, where there are obviously confounding effects. Our strategy is to first compare areas closer to the lines to areas further away and show that they have on average better infrastructure. We then compare various measures of economic outcomes further and closer to the line and interpret any difference in economic outcomes as the overall effect of any transportation infrastructure – the original railroads and any other infrastructure later added – along these historical transportation corridors.

This strategy has a number of advantages. First, it provides us with an exogenous source of variation in access to transportation networks. Second, this variation goes back to at least fifty years before our study begins in 1986, by which time the patterns of economic activity would have had ample chance to relocate. We can therefore ask what the long run level effect of being close to the line (and hence to transportation) was, say around 1986. Third, our study period, 1986–2006, coincides with China's opening up and subsequent growth acceleration. Our treatment areas were plausibly in a good position to take the lead in exploiting these new opportunities, exporting to the rest of the world, using their access, although they could also export their resources to the terminal cities, which would have the opposite effect. We therefore also study growth effects of being close to the line over the period 1986–2003.

The results show that being close to the line had a positive level effect. Per capita GDP was higher in places closer to the line. However, the effect is not large. The elasticity of per capita GDP with respect to distance from historical transportation networks is approximately −0.07. The small level effect is consistent with independent data from a higher-quality household survey for rural areas, the *National Fixed Point Survey* (NFS) collected by the Chinese Ministry of Agriculture, which shows that distance has no significant effect on household income. For the estimates of the effect of proximity on growth, we find a precisely estimated zero effect. The estimated elasticity between distance to the line and annual per capita GDP growth is −0.002 and statistically insignificant (the standard error is 0.003). Places close to the line grew exactly as fast as places further away.

Our finding that better access to transportation networks does not have a large impact on the (relative) economic performance of those areas is consistent with the Fogelian view that transportation infrastructure by itself does not contribute much to growth, excepting perhaps where there was already a demand for it. Based on similar logic, China scholars have criticized the tremendous amount of public investment in domestic transportation infrastructure after 1990 (Huang, 2008).

However, there is an alternative and complementary interpretation under which the *measured* benefits of infrastructure are small even if better transportation causes substantial gains to overall GDP. The basic idea emphasizes the role of factor mobility. Under full labor and capital mobility, wages and incomes would be equalized in our treatment and control areas even if there are large macro effects and we would see no difference in their outcomes. Of course, the assumption of full factor mobility is clearly inconsistent with the institutional conditions in China. In the paper, we present a simple model which shows that we can observe similar patterns with limited factor mobility. Specifically, if labor mobility is very limited, but capital is also relatively immobile compared to goods, and its mobility depends on the distance to transportation infrastructure, more remote areas may actually retain more of their capital compared to better connected areas (where all the resources move to the nearest metropolitan center). For the latter reason, GDP per capita may not dramatically decline in remote areas. Moreover, this effect only tells us about the level of GDP. As far growth is concerned, since even the more remote locations retain a substantial part of their resources, they also retain the possibility of participating in and benefitting from the exposure to global markets that raised the growth rate everywhere in China. Therefore, the impact on the growth rate, starting at a lower level, can be similar in remote and less remote areas. We summarize this intuition in the body of the paper and provide a formal discussion in the Appendix.

In assessing what general lessons one can learn from our results, one should consider whether our results are driven by conditions specific to the Chinese context. For example, one may be concerned that the marginal effect of access to infrastructure is especially low in China due to the massive public investment in infrastructure during recent years.[3] We believe that this is highly unlikely to be driving our results because our data show large variation in access to infrastructure. The distance to the railroad for the counties that are the nearest the line (defined as the nearest decile) are only one-third of the distance of those furthest (defined as the furthest decile) from the line. Similarly, the nearest counties have more than twice the length of highway relative to the furthest counties (despite the fact that the latter are almost eight times as large).

[1] For example, see the World Bank's World Development Report 2009 on *Reshaping Economic Geography* by Aoyama and Horner (2009) for a nuanced statement of this view.

[2] For example, see Pong (1973).

[3] For example, see Huang (2008) for a discussion on infrastructure investment in recent years in China.

Another concern for external validity is that the lack of factor mobility stems from the Chinese government's attempts to control labor mobility and that the empirical findings are not easily generalizable to the context of other developing countries.[4] We acknowledge that the Chinese government may be unique in implementing an explicit policy for controlling migration for so long. However, it is important to note that the actual patterns of low levels of migration are not unique to China. In particular, the main policy effort has focused on unskilled low-wage rural workers (Meng, 2005), a group that has been found to be relatively immobile in other contexts such as in India (Munshi and Rosenzweig, 2009). Moreover, our simple theory predicts similar effects regardless of which factor (e.g., labor, capital) is immobile and the immobility of capital has been documented in several developing country contexts. For example, Chan et al. (2011) documents the immobility of capital in China during the period of our study.[5]

Since we began our study in 2004, a growing number of recent papers have developed compelling identification strategies to evaluate the impact of transportation infrastructure. Most existing studies examine the effect of transportation infrastructure from the point of view of market integration and the focus is on price convergence and changes in the relative price of factors along the lines predicted by trade models. The results suggest that transportation infrastructure favors greater price convergence and that factor prices shift in the direction predicted by trade theory (e.g., Michaels, 2008; Donaldson, Forthcoming; Keller and Shiue, 2008).[6] Recent studies also provide evidence that better transportation can have adverse local effects. For example, Faber (2014) finds that China's new highway system adversely affects productivity in newly connected regions and Baum-Snow (2007) and Baum-Snow et al. (2017) find that better transportation infrastructure shifts populations and economic activities away from city centers in the United States and China.

Our study differs from these studies in its focus on the longer-run and more macro question: do areas that benefit from access to the reduction in trade costs and perhaps other costs become wealthier as a consequence? This is by no means obvious even if there is clear evidence that trade and other flows such as migration increased when infrastructure became available. Our estimates provide a much more reduced form effect, which presumably includes not just the possible gains from more efficient trade but also the effects of greater factor mobility, better access to education, health care and finance, and other, more diffuse effects coming from the diffusion of ideas, technologies, etc. In this sense, our study is more closely related to recent studies that examine the effects on population growth, land values and city size. For example, Atack et al. (2010) finds that access to railroads has a strong positive effect on urbanization but a small effect on population growth in the United States.[7] Other recent studies examining long-run effects using U.S. data include Donaldson and Hornbeck (forthcoming), which finds evidence that increased market access from the historical expansion of U.S. railway networks increases land values; and Sequeira et al. (2017), which finds that access to railroads increased immigration during America's Age of Mass Migration (1850–1920), which had long-run consequences for economic prosperity.[8]

Moreover, our paper provides a potential interpretation of the lack of infrastructure effects which is of some independent interest. The idea that the lack of factor mobility might limit the *measured* impact of better infrastructure is of considerable relevance to many developing countries that are currently investing in improving their infrastructure.

Our findings also add to understanding the long-run effects of European Imperialism on China's economic development. In using Treaty Ports to construct the lines, we are closely related to Jia (2014), which documents that Treaty Ports experienced better long-run economic development than other regions in China. We add to this by showing that through railroads, areas outside of the Treaty Ports were also affected.[9]

The rest of the paper is organized as follows: We start with a brief review of the literature in Section 2. Section 3 presents the theoretical framework that we use to think about our results including a simple model of industrial location choice. Section 4 provides the background and the empirical strategy. Section 5 describes the data. Section 6 presents the results. Section 7 offers concluding remarks.

2. Growth, capital and mobility

This section briefly discusses factor mobility in China in relation to the simple model we present in the next section. We aim to make three points. First, central planning policies caused the endowment of human and physical capital to be higher in urban areas relative to rural areas in the pre-reform era (1949-76). However, to promote rural industrialization, the pre-reform government also invested substantial amounts of capital in rural areas (Unger, 2002). Second, restrictions on migration largely prohibited the mobility of unskilled labor during the post-reform period of our study and limited financial development probably inhibited capital mobility (West and Zhao, 2000). Finally, the post-reform era was characterized by very high growth rates.[10]

Chinese central planners have always focused on economic growth and industrialization. In the early 1950s, this meant moving skilled workers and machines into cities. During this period, the percentage of government revenues used to fund industrial development increased from 32% in 1952 to 57% in 1957 (Eckstein, 1977). Much emphasis was also put into improving human capital in cities. In addition to moving skilled workers into cities, a special emphasis was put on secondary and higher education. All secondary and higher education institutions in China are located in cities which naturally causes human capital to be drawn into cities even if some of the students were born in rural areas.

Rural areas also received investment, albeit less than the cities. An enormous number of primary schools were established so that all rural children could have access to a basic education. Literacy rates in China reportedly improved from less than 20% in 1949 to 68% by 1982, even though almost 80% of the population was still rural (Jowett, 1989). Rural areas also received investments in physical capital: villages were collectivized and physical capital was owned and managed by collectives. When China de-collectivized during the early 1980s, collective assets were inherited by villages, and were often used to form Town and Village Enterprises (TVE).

[4] For example, see West and Zhao (2000) for a review of studies on labor migration.

[5] For example, see Banerjee and Duflo (2005); Duflo (2004) for evidence of limited capital mobility within Indonesia.

[6] Michaels (2008) examines the effect of highway construction in the United States in the 1950s, using both a *difference-in-difference* (DD) approach and an instrumental variables approach, where he exploits the variation in access caused by the fact that highways tended to be built in either a North-South direction or an East-West direction starting from big cities. Donaldson (Forthcoming) studies the effects of railroad construction in 19th century India using a DD approach. Keller and Shiue (2008) uses a similar strategy to examine the opening up of railways between regions within Germany. Also, Chandra and Thompson (2000) use historical U.S. data to find that connections to highways have heterogeneous effects across industries.

[7] While Atack, Bateman, Haines, and Margo (2010) primarily uses a DD approach, it also constructs an instrument for the distance to the railroad based on the straight line between the start and end points of a railway line. The authors generously credit the straight-line instrument to an earlier version of our paper (Banerjee et al., 2004).

[8] Outside of the U.S. context, see Alder (2015), which uses a model-based approach to demonstrate the benefits of China's current transportation system relative to India's system; and Storeygard (2016), which finds that higher transportation costs reduce city size in sub-Saharan Africa.

[9] Note that the exclusion of termini cities means that Treaty Ports are excluded from our analysis.

[10] See, for example, Hu et al. (1997) for an overview of Chinese growth.

For our study, it is important to note the following facts. First, a significant proportion of industrial output in China during our study period came from TVEs. As a percentage of national industrial output, output from TVEs grew from 9% in 1978 to 36% in 1993.[11] Second, TVE assets are jointly owned by all community residents, which were approximately 400 households in an average village and 3500 households in an average township. Households owned equal shares in TVEs and it was illegal to sell or transfer their shares to non-community members. Third, the law required that at least 60% of the profits be retained in the village.[12] The data show that over half of the profits were reinvested.[13] These three facts together suggest that a significant amount of productive capital was in rural areas, and policy both prevented their mobility to cities and promoted further capital accumulation in rural areas.

Labor mobility was also restricted. If a worker moved without official permission, she lost access to all public goods. For urban residents, this meant losing access to schools, healthcare, and during the 1980s and early 90s, it also meant the loss of food rations and housing. For rural residents, this meant the loss of farmland. Government permission was easier to obtain for skilled workers such as college graduates who could obtain jobs that assisted them in getting the permission to relocate or workers with skills that were needed in specific industries such as construction during the mid- and late- 1990s. But for the rest of the population, permission was extremely difficult to obtain (e.g., Meng, 2005; Meng and Kidd, 1997). Therefore, while the number of migrant workers increased greatly during this period, most of them were temporary migrants who maintained their original residences.[14]

Finally, it is important to point out the differences in growth rates between cities and rural areas and how they changed over time in China during the post-Mao reform era, when income increased rapidly for the country. During the first years of the period, 1978-84, the real income of rural residents grew at 17.7% per year while it was only 7.9% for urban residents. This pattern was reversed in the mid-1980s and the urban advantage increased steadily for the remainder of the reform era. On average, rural real income growth rates declined to only 4.1% while urban real income growth was approximately 6.6% (Cai, 2010).

3. Conceptual framework

In the Appendix, we present a simple model where labor is immobile and capital is less mobile than goods, though even goods are costly to move. As a result, even remote areas continue to hold onto a part of their capital and produce exportables and benefit from globalization. Moreover, if the mobility of capital is more limited out of relatively remote areas, the effect on GDP per capita of being well connected will be the result of two contending forces. On the one hand, distance is costly and makes exports less profitable. On the other hand, if remote areas retain more capital per head than better connected areas, this would boost the productivity of labor in those areas. As a result, the variation in per capita GDP between near and far places may be relatively small and both be involved in the production of exports. As a result, the boost to TFP and growth resulting from the opening of the economy for global trade may be the same in proportional terms in all locations. Therefore, even though better transportation helps China as a whole to gain more from trade, GDP level differences between well and poorly connected areas can be small and there may be no differences in growth rates between the two areas. One could make the same argument if the relatively immobile factor was human capital instead of physical capital.

The premise of this model is that goods move more easily than capital. Unfortunately, we cannot directly observe the relative mobility of goods and capital. However, our model tells us that relatively low mobility of capital is likely to be associated with a situation where there is higher inequality in better connected areas, as long as the direction of capital movement is from less connected areas towards better connected ones. Using regional income inequality data computed from the *National Fixed Point Survey* collected by China's Ministry of Agriculture (1987–2005), we do find that inequality is higher in better connected areas.

4. Historical background and empirical strategy

4.1. The birth of modern infrastructure

As explained above, the basic idea behind our empirical strategy is to examine the correlation between the distance to the nearest straight line connecting two historical cities and the outcomes of interest. Throughout the paper, we assert that these lines capture major transportation networks during the 1980s because they capture the first modern infrastructure (e.g. railroads) built in China and much of the infrastructure development afterwards began by initially building along these routes. Later in Section 6.4.1, we will provide evidence for our assertion.

To draw the lines, we start with the set of important historical cities in China *circa* 1860: Beijing, Chengdu, Guiyang, Kunming, Lanzhou, Nanchang, Taiyuan and Xian. These were urban centers that were politically and economically important and which never became Treaty Ports at any time (Murphey, 1970).[15] To these we add the four Treaty Ports that were set up by the League of Eight Nations after they defeated the Qing government in the First Opium War in 1842 (Shanghai, Ningbo, Fuzhou and Guangzhou). These four cities were chosen for their strategic locations. The "unequal treaties" that were signed between China and the League of Eight Nations after the Opium Wars allowed the Western countries to house their military in the Treaty Ports but not beyond. Therefore, these ports were chosen to be easily accessible by European ships and also to be strategically advantageous for reaching Chinese cities in case of an uprising or war. Later waves of Treaty Ports were chosen more for economic reasons, and therefore are more likely to be correlated to factors that can affect our outcomes of interest.

The four Treaty Ports in our sample are all along the coast or a major navigable river. Shanghai and Ningbo are on the northern and southern mouth of the Yangtze River, Fuzhou was on the southern coast of the Yellow Sea, and Guangzhou was on the Xi River, near its mouth on the South China Sea. All of these ports were easily accessible by the naval gunships of the Western countries and therefore allowed them to both impose their military presence as well as control international trade with China.[16] With the exception of Guangzhou, these locations were villages and not prominent historical urban centers prior to becoming Treaty Ports (such as Nanchang or Xian). Therefore, the lines that we draw between these Treaty Ports and the historical Chinese cities have no reason to go through regions of particular importance to the Chinese.

[11] See the *Statistical Material of Township and Enterprises*, 1992.

[12] See Articles 18 and 32 in *The Regulation on Township and Village Collective Enterprises of the People's Republic of China* (1990).

[13] See *Statistical Survey of China*, 1992: pp. 67.

[14] There have been numerous studies on migration in China. Zhao (1999) provides a survey of recent evidence.

[15] See Appendix Fig. A1 for a map of all Treaty Ports and historically important cities taken from Murphey (1970) page 35.

[16] The Treaty Ports were established in *Article 2* of The Treaty of Nanjing, which was signed between the British and the Qing government. Article 2 requested the four cities we mention and Xiamen to be established as Treaty Ports. But in practice, Xiamen did not receive significant investment from the West and only became a Treaty Port during the second wave of Treaty Port Relinquishment by the Qing in 1865. Therefore, in our line construction, we omit Xiamen. The other Treaty Ports of the second wave were Tianjin, Niuzhang, Yantai, Zhenjiang, Hankou, Shantou, Taibei and Tainan (e.g., Pong, 1973; Spence, 1990).

Moreover, it is important to point out that the Chinese were significantly behind the Europeans in terms of naval technology in 1842, and did not possess a fleet of similar ocean-going naval gunships for which the Treaty Ports were chosen. More generally, China had conducted a very limited amount of international trade since the 16th Century during the Ming and Qing Dynasties. It did not have an outgoing navy for several centuries leading up to the Opium Wars.[17] Therefore, places such as Shanghai, Ningbo and Fuzhou, while not entirely uninhabited prior to 1842, were rural areas with small stations for domestic naval patrol boats. Their insignificance before 1842 is shown by the fact that none of the four cities were connected to the Grand Canal, which was a north-south canal built to connect Beijing to the important Southern cities.[18] It follows that when we draw lines to connect the Treaty Ports and historical Chinese cities, we are unlikely to be systematically capturing important routes from before 1842. Instead, the lines will capture modern transportation networks built afterwards.

The first and perhaps most important transportation infrastructure are railroads. They were mostly built during the early 20th Century by the Qing government and Western countries. The latter provided much of the financing and had substantial influence over the placement of the railroads. They were largely built to promote Western economic and military interests in China and connected Treaty Ports to historical cities, and also connected historical cities to Colonial cities outside of China. For example, the British planned and financed railways to connect the Yangtze River valley as well as a north-south railway to connect Wuhan to Guangzhou against the protest of the Qing government, who feared that this would facilitate fast British troop deployment from Shanghai and Ningbo to important Chinese cities. The French planned and financed a railway to connect Kunming to Hanoi, an important city in French Indochina. The Russians planned a railway that was almost a straight line from Beijing to Vladivostok through Liaoning, Jilin and Heilongjiang provinces (Spence, 1990, pp. 249-56).

4.2. *Straight lines*

We construct our independent variable using a simple algorithm. We draw a straight line from each historically important city to the nearest Treaty Port and/or to the nearest other historically important city. If there are two cities (or ports) where the difference in distances is less than 100 km, we draw a line to both. The line is continued past the city until it hits a natural barrier (e.g. Tibetan Plateau, coast line), or a border to another country. The lines are shown in Fig. 1.[19]

As expected, the lines drawn this way coincide well with railroads constructed during the early 20th century.[20] The three places where they do not match well are North-Western China (Xinjiang, Tibet, parts of Inner Mongolia), where construction occurred under the Communist government after the 1970s, partly as an attempt to politically integrate these areas into China; and North Eastern China (Manchuria), where most of the construction was done by a *de facto* colonial Japanese government during the 1920–30s (Fig. 1 shows that in Manchuria, most counties have a railroad). For this reason, our estimating sample will exclude Xinjiang, Tibet, Inner Mongolia and the provinces in Manchuria.

Our main source of plausibly exogenous variation for access to infrastructure is the nearest distance from the center of each county to this straight line. Both the centroids and the nearest distance are computed by ArcGIS using the *Asia Conical Projection*. We use geographic distance rather than travel distance measured as kilometers. This line is also our proxy for transportation infrastructure. We deliberately make no use of information about *changes* in infrastructure.

To check that the lines do indeed proxy for transportation infrastructure, we estimate the correlation between distance to the line and various measures of infrastructure using the following equation:

$$I_{cpt} = \delta lnL_{cp} + \rho_p + \gamma_t + \varepsilon_{cpt}. \tag{1}$$

Transportation infrastructure in county c in province p and year t, I_{cpt}, is a function of: the natural logarithm of the distance to the nearest line connecting Treaty Ports and historical cities illustrated in Fig. 1, L_{cp}; province fixed effects, ρ_p; and year fixed effects γ_t. Note that the fact that the line is likely to be connected with many different types of transportation infrastructure means that L_{cp} is not an excludable instrument for any given infrastructure.

Our main estimating equation is the following:

$$y_{cpt} = \beta lnL_{cp} + \Gamma Z_{cp} + \rho_p + \gamma_t + \varepsilon_{cpt}. \tag{2}$$

The outcome for county c, province p and year t, y_{cpt}, is a function of: the natural logarithm of the shortest distance to the line for county c in province p, lnL_{cp}; a vector of county-specific controls, Z_{cp}; province fixed effects, ρ_p; and, year fixed effects, γ_t. The standard errors are clustered at the county level. If proximity to the line is beneficial, then $\widehat{\beta} < 0$.

Interpreting β as the causal effect of proximity to the line assumes that the only difference between places near the line and places further away is the distance to the line. This obviously relies on the termini cities not being chosen so that the straight line between them would run through economically important regions. This is the reason why we focus on the ancient cities of China and the Treaty Ports – i.e., the historical cities are both sufficiently far from each other and clearly more important than any place between them in the historical era that it is easier to be comfortable with the identification assumption in this context. Similarly, the Treaty Ports were chosen for their suitability for European gunships rather than what laid between them and the historical cities. Note that we restrict our attention to the first four Treaty Ports to avoid the potentially endogenous influences of later Treaty Ports, which may have been chosen for economic reasons (e.g., proximity to economically viable or prosperous regions).

There are two caveats. First, being closer to the line will, by construction, mean that a county is also closer to the terminal cities. Therefore, our baseline specification will control for distance to the terminal cities. Second, the line from some historically important cities to a Treaty Port might follow a river, an important traditional means for transportation as well as an important input for agriculture (e.g., river beds provide fertile soils). In this case, distance from our line will also capture the distance from the river, which presumably captures many other effects. To address this, our baseline specifications always control for distance to the nearest navigable river.

The baseline estimation also controls for other potentially influential factors, which we will discuss and motivate later in the paper.

Note that it is not clear that we can expand the set of cities being connected (and therefore use more of the data) without running into

[17] See (Spence, 1990, Ch. 2) for a detailed discussion of China during the 19th Century.

[18] A large flood in 1855 permanently changed the course of the Yellow River, causing the canal to significantly decline in importance.

[19] The goal of the lines is to provide a measure of proximity to infrastructure that is exogenous to local potential for economic growth (conditional on the baseline controls). For our purposes, the ideal identification would be provided by randomly assigning infrastructure in 1840 and estimating the effect of proximity to that infrastructure on outcomes in the 20th century. In practice, since infrastructure was not randomly constructed, we exploit variation in the proximity to our constructed lines, controlling for things such as distance to the terminus city. The more randomly assigned is the line, the better for causal inference. Stopping the lines in the cities where the historical railways end in practice would introduce endogeneity relative to extending the lines to the border. The concern is that the historical railways ended at point X because the regions beyond it had less potential to grow. Thus, we precisely want to extend the lines to the border to avoid endogeneity.

[20] While the railroads suffered much damage during World War II, after the war, the Guomingtang (KMT) and then the Communist (post −1949) governments undertook extensive repairs and construction focused on upgrading the physical structure. A comparison of maps from the 1930s to maps from the 1950s indicate that they mostly did not alter the course of the railroads.

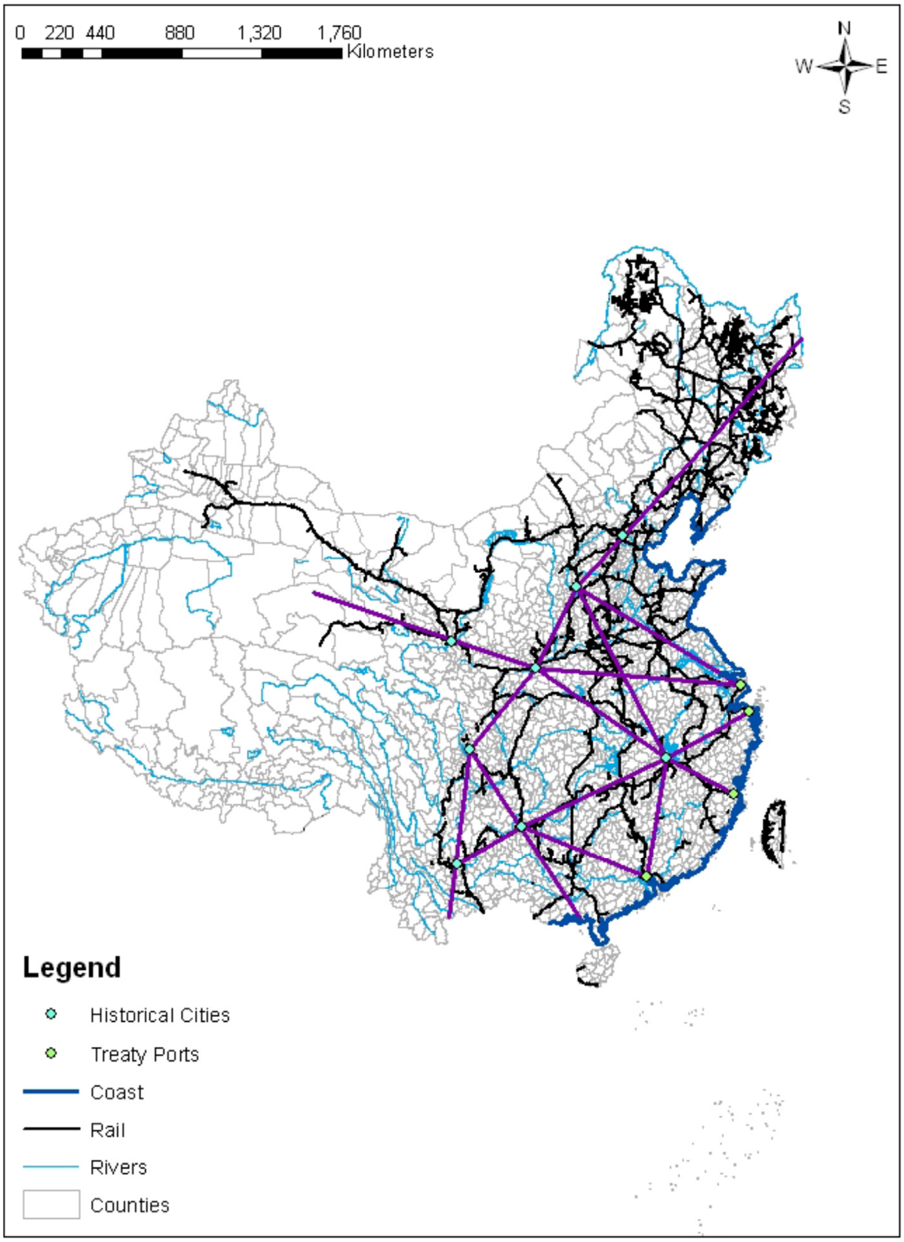


Fig. 1. Lines and transportation infrastructure.

potential problems. One issue is that of endogeneity raised earlier. Another equally important issue comes from the very nature of the construction of lines. We compare places that are close to a line with those that are further away. The implicit assumption is that moving further away from one line does not bring us closer to a different line, a problem that occurs when there are too many lines. We ensure this by having relatively few lines and using a sample of counties that are not too distant from any line. The maximum distance of any county in our sample from the nearest line will be 366 km. Fig. 1 shows that there are only ten lines. We will return to discuss this further in Section 6.4.1.

5. Data

This paper uses data from multiple sources. All raw maps are obtained in digital format from the Michigan China Data Center. Geographic measures are constructed using ArcGIS software, assuming a Conical Projection. We define centroids of cities and counties. The lines are constructed to connect the centroids of segment cities (Treaty Ports and historically important cities) using the algorithm described earlier. We compute the nearest distance from each centroid to the straight line, railroads, navigable rivers, the coastline, the country border and segment termini.[21] Fig. 1 displays a map of county boundaries, our constructed lines, railways and major navigable rivers.

The first outcome measure we examine is county-level per capita GDP. These data are from the *Provincial Statistical Yearbooks* from China from 1986 to 2003 stored in the Archives National Library in Beijing, China. We manually collected and digitized data from all published yearbooks that reported county-level statistics on GDP. These data are interesting because they measure production whereas previous studies have mainly focused on prices. However, there are several problems with these data. First, GDP may have been measured using different techniques across provinces and over time. To the extent that these changes are documented or obvious (e.g., changes in the units of mea-

[21] The county boundaries are based on a 1990 map of China.

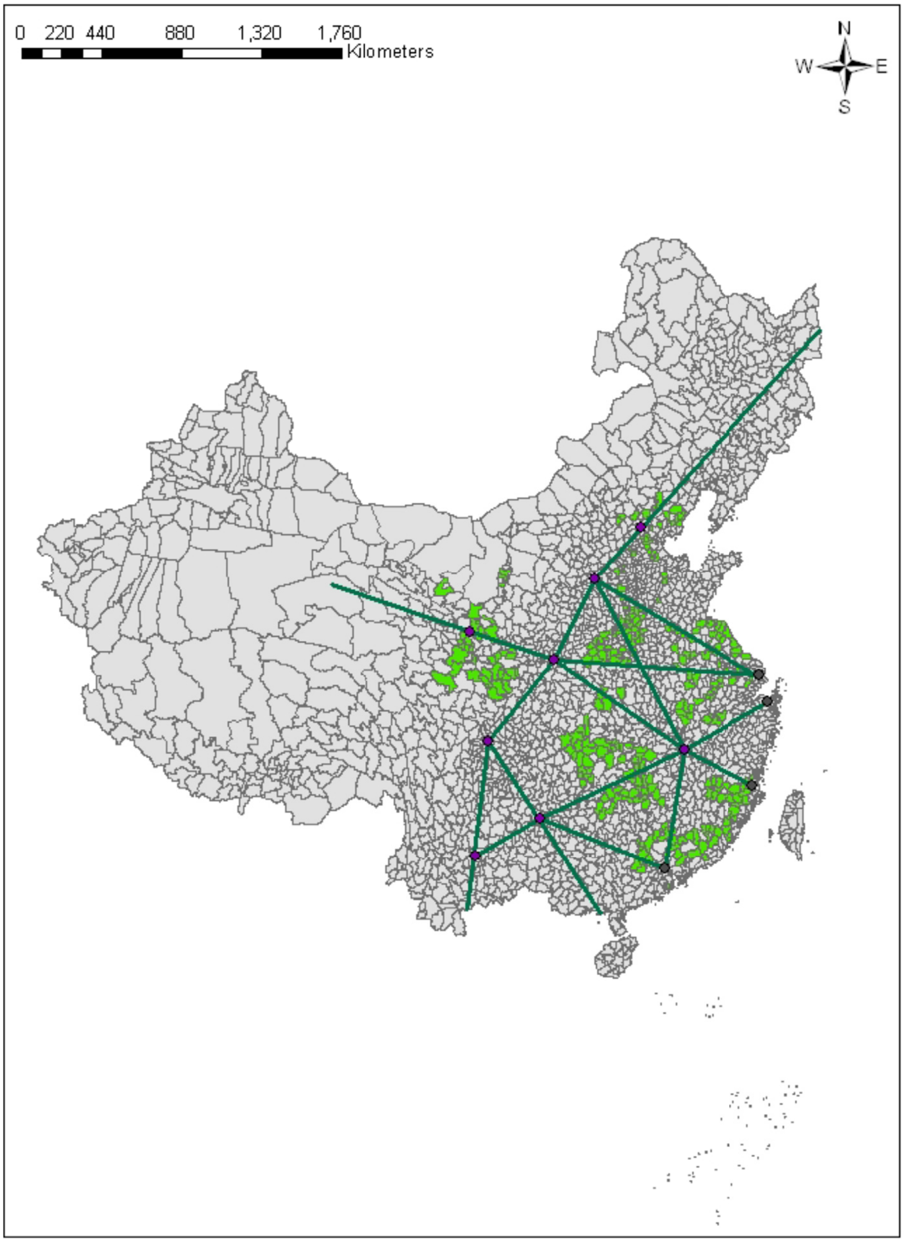


Fig. 2. Counties with GDP data from the *Provincial Statistical Yearbooks*.

surement), we have corrected for them. But this is clearly still imperfect. Second, not all counties report GDP and those that are reported are not a random sample of Chinese counties. Third, many counties do not consistently report over time, which means that we have an unbalanced panel where attrition is non-random. There is little documentation on the logic behind the decision of which counties report GDP and we can do little to correct for it. Our final sample is an unbalanced panel of 295 counties within sixteen provinces.[22] In addition to the data on GDP, we collected data on county population so that we can calculate per capita GDP. Fig. 2 maps the counties for which we have GDP data.

To address these measurement difficulties, we supplement the analysis with two additional data sets of higher quality. While they cannot allow us to directly correct for the county-level GDP data, they do allow us to check that the estimated effects in these two alternative data sets are consistent with our theory. The first of these are firm-level data from the *Census of Industrial Plants in 1993 and the Census of Manufacturing Firms* during 2004–2006. We are able to geocode these data to the county level.[23] The first survey includes all industrial plants. The second survey samples all state-owned manufacturing firms and all privately owned manufacturing firms with revenues of five million RMBs or more. We will examine two outcomes, the number of firms and their profits. The data are aggregated to the county and year level and form an unbalanced panel of counties. Fig. 3 maps the counties for which we have firm data.

The second additional data are village-level data for rural household incomes from the *National Fixed Point Survey* (NFS) for the years 1987–1991, 1993, 1995–2005. There were no surveys in 1992 and 1994

[22] Beijing, Hebei, Jiangsu, Anhui, Fujian, Jiangxi, Shandong, Henan, Hubei, Hunan, Guangdong, Guangxi, Guizhou, Gansu, Qinghai, and Ningxia.

[23] These data are in principle available for other years. However, we only use the four years for which we could geographically identify the location of the firm at the county level. This data has been used by many studies, the most well-known of which is probably Hsieh and Klenow (2009).

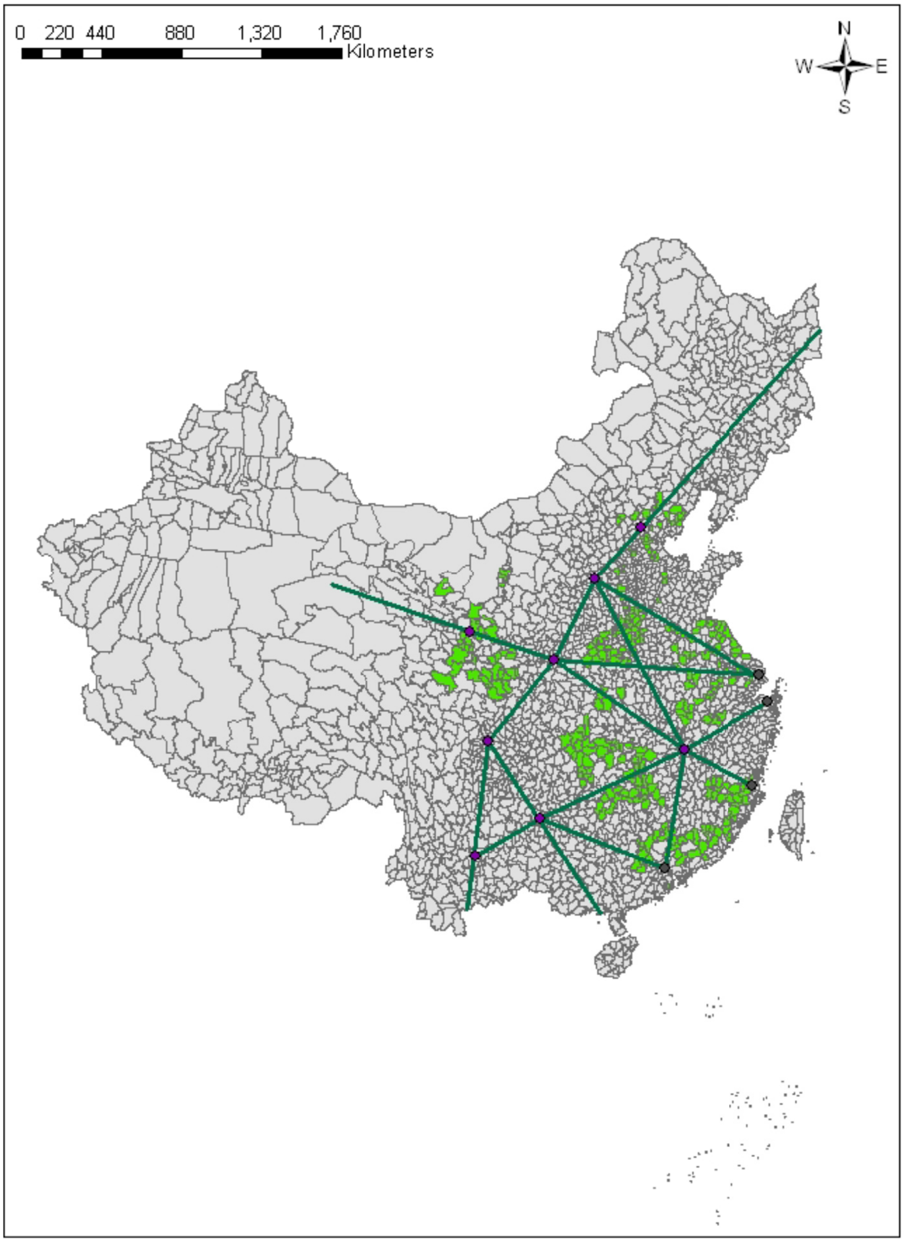


Fig. 2. Counties with GDP data from the *Provincial Statistical Yearbooks*.

for administrative reasons. The NFS is a longitudinal survey of about 320 villages and 24,000 households distributed across all continental Chinese provinces conducted by the research arm of the Ministry of Agriculture (RCRE). The villages were chosen in 1987 to be nationally representative. There is very little attrition. To maintain its representativeness, villages and households are added over time. Therefore, the panel of villages is not perfectly balanced. For this study, we use household-level data on income. Each village contains on average 400 households and approximately one-third of them are surveyed by the NFS. The large number of households surveyed in each village means that we can examine the within village income distribution.[24]

Our income variable measures total net income – i.e., the sum of household income (e.g., home production, agricultural production, wages) minus the sum of production costs, excluding labor costs for home production and agriculture. The data are aggregated to the county and year level. The RCRE provided us with income for each decile of the village income distribution and the Gini coefficient for the within village income distribution each year and did not provide us with average income across all households. Therefore, in the analysis, we will focus on income of the 10th, 50th, 90th percentiles and the Gini coefficient. Fig. 4 maps the counties for which we have NFS data. Note that the exact location of these villages are confidential. Therefore, our distance variables measure the distance from the centroid of the county that contains the village to the object of interest. This introduces measurement error to the right-hand-side of our estimates for household income that is most likely classical in nature.

For all samples, we exclude the autonomous regions of Tibet, Xinjiang and Inner Mongolia both because these provinces are predominantly non-Han ethnic minorities, and therefore faced different policies,

[24] Villages and households are surveyed every year. The survey uses a stratified sampling approach. For each province, it first randomly selects a number of counties, and then randomly selects a number of villages within each county. Households are then randomly selected from each village. See Martinez-Bravo et al. (2017) for a description of these data.

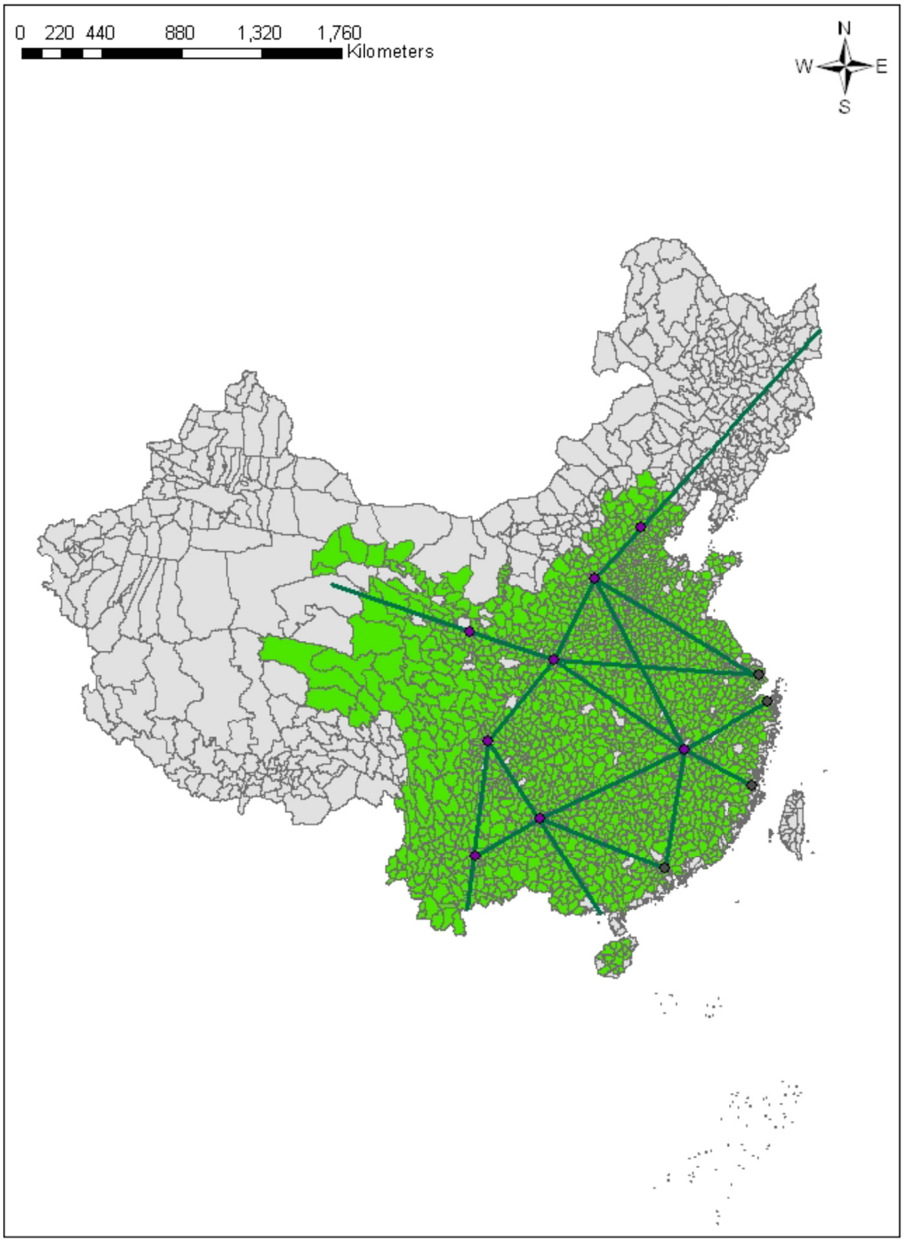


Fig. 3. Counties with Firm data from the *Censuses for Manufacturing Firms*.

and because the railroads constructed in these regions were the results of very different imperatives. For the latter reason, we also excluded the three Manchurian provinces of Heilongjiang, Liaoning and Jilin. The large cities that are on the segment termini are also excluded to avoid the results being driven by the end-points, which are on the line and were chosen because they were important to begin with. It is important to note that other cities on the line (that are not the termini of line segments) are included in our sample so that our estimates will capture any effects that transportation infrastructure may have on the formation or growth of cities.

5.1. Descriptive statistics

Table 1 describes the data. Panel A describes the sample with GDP data. On average, these counties are approximately 71 km from the line and 39 km from railroads. The fact that the average distance to railroads is less than the average distance to the line reflects the fact that we constructed many fewer lines than there are railroads to only capture the distance to major transportation networks and to avoid the problem of having too many lines that we discussed earlier. During our study period, there are very few highways with median dividers in China. On average, a county has only approximately 6 km of divided highways. Most motor traffic occurred on paved motor roads without dividers. An average county has approximately 84 km of such undivided paved roads.[25] The average county is far from a navigable river, the coastline and the country border.

Note that the data show significant variation in access to transportation infrastructure. This alleviates any concerns that our study cannot detect significant marginal effects of access because high levels of infrastructure investment by the Chinese government causes there to be too little variation in access.

The average population of a county is approximately 201,347. Per capita GDP is 6834 RMB. The nominal GDP reported in the statistical

[25] The GIS data on roads are produced by Harvard CHGIS and presumed to reflect 1990 conditions.

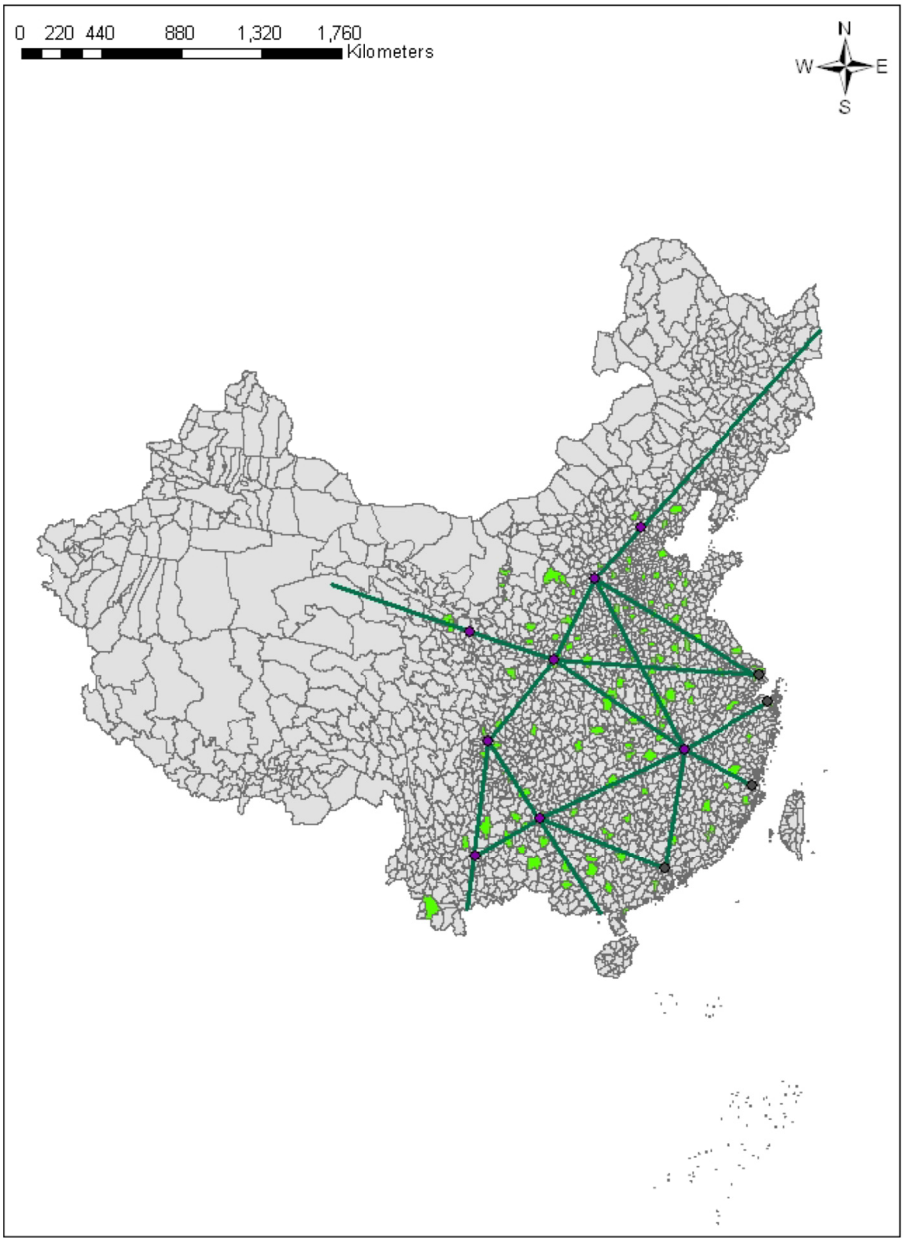


Fig. 4. Counties with Income data from the *National Fixed Point Survey*.

yearbooks are adjusted by the national CPI. GDP from primary, secondary and tertiary sectors are roughly similar in size. Average per capita GDP growth is 8% in this sample, which is similar to the national average during the study period. Most of the income growth comes from the secondary and tertiary sectors. Note that the number of observations differ across the GDP variables because not every county is engaged in economic activity in every sector.

Panel B displays the descriptive statistics for the sample of household income data. For the sake of brevity, we focus our discussion on the economic variables. The average within-village Gini coefficient is 0.28. The net household income for the median household is on average 5460 RMB (constant), which is almost twice as much as the income of the 10th percentile household and approximately half of the income of the 90th percentile. Inequality is growing over time. The Gini coefficient increases by 0.001 per year on average. This is driven by a higher income growth rate for richer households, although the level of income increases across all parts of the income distribution.

Since there are approximately three people per household in these data, the household income here implies a slightly lower income than the per capita GDP from the sample in panel A. This is not surprising since the earlier sample includes urban and rural areas, while the household income data in panel B only includes rural households, which are on average poorer than urban households. Similarly, income growth is slower in rural areas. Recall that there are no data for 1992 and 94. Therefore, we interpolate the annual growth rates between 1991 and 93, and 1993 and 95 as the growth rates for each two-year interval divided by two.

Panel C describes the firm data. Again, for the sake of brevity, we focus on the economic variables. On average, there are 82 manufacturing firms in a county. We can divide these firms into three ownership types: firms owned by the state, firms of mixed ownership, and firms owned by private individuals. State-owned firms are directly controlled by the state. Mixed-ownership firms are typically privatized state firms for which the state owns most of the equity. Individually owned firms

Table 1
Descriptive statistics.

Variable	Obs	Mean	Std.Dev.
A. Sample 1: County Level GDP (1986–2003)			
Distance to Historical Line (km)	2744	71.31	64.16
Distance to Railroad (km)	2744	38.81	39.20
Length of Highway (km)	2744	6.05	12.77
Length of Paved Roads (km)	2744	83.54	47.16
Distance to River (km)	2744	183.23	137.38
Distance to Coastline (km)	2744	424.98	372.41
Distance to Country Border (km)	2744	865.14	205.50
Distance to Segment City (km)	2744	144.17	81.27
County Area (Sqkm)	2744	1918	1134
County Population (Individuals)	2744	201,347	211,227
Per Capita GDP (Constant RMB)	2744	6834	8076
Primary	2266	1968	2424
Secondary	2266	2192	3579
Tertiary	2199	1672	2295
Per Capita GDP Growth	1879	0.08	0.14
Primary	1644	0.04	0.17
Secondary	1644	0.10	0.19
Tertiary	1551	0.11	0.17
B. Sample 2: Household Income (1987-91, 93, 95–2005)			
Distance to Historical Line (km)	1897	64.79	67.66
Distance to Railroad (km)	1897	37.82	42.36
Length of Highway (km)	1897	7.69	14.79
Length of Paved Roads (km)	1897	89.10	50.94
Distance to River (km)	1897	129.93	104.50
Distance to Coastline (km)	1897	475.19	318.46
Distance to Country Border (km)	1897	739.17	299.00
Distance to Segment City (km)	1897	133.01	78.90
County Area (Sqkm)	1897	2058	1261
Gini	1897	0.28	0.07
10th Percentile HH Income (Constant RMB)	1897	2729	1322
50th Percentile HH Income (Constant RMB)	1897	5460	2837
90th Percentile HH Income (Constant RMB)	1897	11,681	11,407
Gini Growth	1782	0.001	0.049
10th Percentile HH Income Growth	1782	0.018	0.221
50th Percentile HH Income Growth	1782	0.028	0.162
90th Percentile HH Income Growth	1782	0.031	0.199
C. Sample 3: Firms (1993, 2004-06)			
Distance to Historical Line (km)	3663	81.63	87.83
Distance to Railroad (km)	3663	49.70	65.26
Length of Highway (km)	3663	5.72	13.80
Length of Paved Roads (km)	3663	92.57	78.33
Distance to River (km)	3663	148.43	111.98
Distance to Coastline (km)	3663	544.66	405.04
Distance to Country Border (km)	3663	705.24	309.89
Distance to Segment City (km)	3663	156.13	102.49
County Area (Sqkm)	3663	2405	3716
Number of Firms	3663	81.68	145.83
Public Ownership	3663	39.75	79.39
Mixed Ownership	3663	7.24	16.22
Individual Ownership	3663	24.76	80.28
Aggregate (Sum) Profits (Constant 10,000 RMB)	3321	227,411	1,176,004
Average Profits	3321	2889	28,211
Public Ownership	3233	1997	18,066
Mixed Ownership	1909	12,753	88,431
Individual Ownership	1785	1629	4441

Notes: Variables are observed at the county and year level. Sample 1 in panel A uses data from Provincial Statistical Yearbooks. Sample 2 in panel B uses data from the National Fixed Point Survey. Sample 3 in panel C uses data from the Censuses of Manufacturing Firms. The geographic data in all samples are computed by the authors using ArcGIS.

are truly private enterprises that have little connection to the state. The data show that most firms are owned by the state and individuals. There are only a few firms that are owned by a mix of state and private parties. Next, we describe the data on firm profits. These only report profits on counties with at least one firm. Therefore, the number of observations will differ across variables because not every county has a manufacturing firm of a particular type. The high level of reported profits is consistent with the fact that these data sample large firms (more than five million in revenues).

Table 2 shows the outcome variables of interest for different distances to the line. These data show that most of the economic measures of interest decline with distance from the line. Most importantly, we do not observe systematic upticks in these measures as we approach the furthest deciles, which is reassuring for the concern that distance from our line bring us closer towards another transportation network.

Table 2
Distance from the historical line and economic indicators.

Distance to Historical Line by Decile	Per Capita GDP		Annual Per Capita GDP Growth		Median Village Household Income		Gini		Number of Firms		Aggregate Firm Profits	
	Mean	.Std. Dev.	Mean	Std. Dev.	Mean	.Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
0	7035	10,613	0.07	0.16	6732	4243	0.30	0.08	111.85	184.24	2451	5202
1	6684	7742	0.08	0.14	5472	1995	0.28	0.06	97.40	171.65	2700	7681
2	7119	9968	0.10	0.12	5010	2428	0.27	0.08	115.95	214.98	1169	6235
3	7079	8365	0.07	0.13	6736	3734	0.29	0.05	96.31	149.72	2571	6170
4	6241	6823	0.08	0.14	4995	1505	0.28	0.06	92.99	135.23	1555	5613
5	7507	8334	0.06	0.14	4475	1819	0.26	0.06	73.43	99.54	2014	4549
6	6958	6789	0.09	0.16	4349	1726	0.26	0.06	93.32	166.48	2639	6407
7	5972	6771	0.08	0.12	5756	3561	0.26	0.07	75.57	101.47	1833	5943
8	5734	6775	0.07	0.13	5383	2429	0.27	0.07	44.37	76.66	9150	85,967
9	7994	7505	0.05	0.19	5774	2587	0.28	0.07	16.07	26.42	2800	16,706

Notes: Columns (1)–(4) use data from the Provincial Statistical Yearbooks. Columns (5)–(8) use data from the National Fixed Point Survey. Columns (9)–(12) use data from the Census of Manufacturing Firms. Distance is calculated by the authors using ArcGIS.

5.2. Correlation between pre-treatment era measures and the distance to the line

Before the main analysis, we can investigate the validity of our identification strategy by examining the relationship between the distance to the historical line and three variables for which we have data in the pre-treatment period that are likely to be correlated with the potential for economic development. We describe them below.[26]

The first is a measure of population from 1850.[27] Table 3 column (1) shows that there is no relationship between log population in 1850 and log distance to the historical lines, conditional on the same baseline controls that we use for the main analysis. The only difference to the baseline specification is that we exclude year dummies in this estimate since the data are one cross section.[28] We recognize that 1850 is after the establishment of the first Treaty Ports. However, it has the advantage of being prior to the Taiping Rebellion (1851-64), which caused tremendous population losses and displacement.[29]

The second measure is an official political-economic rating of importance given by the Qing Dynasty, *Chong Fan Pi Nan*. These four indicators were used to determine policies like military presence, conscription, tax rates, tax enforcement, etc. The rating system was established in 1731 (Zhang, 2017). Western historians, such as, McMahon (2014), have translated *Chong Fan Pi Nan* as "frequented, troublesome, difficult, and fatiguing" administrative posts for Qing bureaucrats. Bai and Jia (2016) uses these measures at the higher prefecture level, and interprets the designations in the following way: "chong (important in transportation/communication), fan (important in business), pi (difficult to gather taxes) and nan (high in crimes)".[30]

Chong, Fan, Pi and Nan provide four indicators that are not mutually exclusive, i.e., a county can be rated as any one or all four – Chong, Fan, Pi and Nan. While crude, these measures are interesting because they are potentially correlated to factors that could affect later economic development (e.g., local cultural norms, administrative capacity, connectedness to the central government, local political stability). However, since it is hard to predict whether the relationship between development and any one of these factors would be positive or negative, and because these four variables are highly correlated, we will examine the principal component in our analysis.

Table 3 column (2) shows that the first principal component is uncorrelated with the distance to the historical line, conditional on the baseline controls.

Finally, we obtain data on whether a county had a Buddhist temple in 1820.[31] The presence of a Buddhist temple was likely to be correlated to population density and the location being politically and economically important. Bai and Jia (2016) also argues that it is associated with social capital.

978 counties have at least one Buddhist temple. Amongst these counties, the average is two temples. The maximum number of temples is 45 in a county. We merge them with our infrastructure data. Referee Table 3 column (3) shows that the presence of any temple is uncorrelated with distance to the historical lines. In column (4) we examine the number of temples with a Poisson regression. Similarly, we find no relationship.

These results are consistent with our identification assumption, which implies that there should be no correlation (conditional on the baseline controls).

[26] See the Appendix for more discussion of the data.
[27] Appendix Fig. A.2 displays these data.
[28] We present robust standard errors (instead of clustering them at the county level).
[29] See Section 6.4 for more discussion.
[30] Appendix Figs. A.3a - A.3d display these data.
[31] Appendix Fig. A.4 displays these data.

Table 3
Correlates of baseline characteristics and distance to the historical lines.

	Dependent Variable			
	(1) Ln Pop 1850	(2) Qing Rating PCA	(3) Buddhist Temple Dummy	(4) # of Buddhist Temples
Ln Dist Historical Line	−0.0226	0.0166	−0.00929	−0.0184
	(0.0273)	(0.0348)	(0.0102)	(0.0368)
Observations	588	1117	2220	2220
R-squared	0.434	0.194	0.236	

Notes: All regressions are cross-sectional estimates. They control for the full set of baseline controls in Table 5 column (6), except for the year fixed effects. Columns (1)–(3) presents OLS estimates. Column (4) presents Poisson estimates. Robust standard errors are presented. Data for 1850 population are reported by Ge (2000). Qing Dynasty ratings are reported by Zhao (1976). The number of Buddhist temples are reported by Harvard CHGIS.

6. Results

6.1. Lines, railroads and transportation networks

Table 4 shows the estimates of the correlation between the distance to the nearest transportation infrastructure and the distance to our constructed lines based on equation (1). Distance is measured in terms of kilometers. Panel A shows that distance from the historical lines is positively correlated with distances from railroads, the coastline and segment cities; negatively correlated with the distances to the country border and the length of highways within a county; and uncorrelated with whether a county is on the coastline or near a navigable river, and the length of paved roads. The correlations shown in Panels B and C will be discussed later in this section.

6.2. The effect of distance from the line on GDP

To illustrate the effects of our baseline controls, we first estimate the effects of distance to the line on the log of GDP per capita. In Table 5, we begin with a specification that only controls for province and year fixed effects (see column (1)). In columns (2)–(6), we gradually introduce the baseline controls. The distances to the segment city control for the effect of proximity to a large urban terminus. The distances to the nearest navigable river and coastline control for access to traditional methods of transportation that existed before the lines of interest were constructed. Controlling for the distance to the country border addresses the possible influences of a “border” effect.[32] Finally, the control for the distance to the coastline also addresses the fact that during the period of our study, economic conditions diverged greatly between the coastal areas and the interior areas. Without this control, one could be concerned that a positive correlation between economic outcomes and distance to our lines is an outcome of faster growth in the coastal areas, which may also be coincidentally closer to our lines on average. In addition to controlling for the log of the linear measure of these distance measures, we also control for the quadratic terms to capture the idea that the costs of distance from transportation may be diminishing over distance (e.g., there maybe increasing returns to profit).

The estimates show that the coefficient for the log distance to the historical line and its standard error is reasonably stable across specifications. The full baseline specification is shown in column (6). It is statistically significant at the 5% level. It shows that the elasticity between the distance to the line and per capita GDP is-0.0681. Note that because the data indicate that the relationship between the distance from historical lines and per capita GDP is log-linear, our main specification in column (6) does not control for the quadratic of the distance from the line.

One way to assess the magnitude of our results is to benchmark our estimates of the effect of distance on GDP across space to the total increase of GDP over time in our sample. In our sample, the 75th-percentile county in terms of distance is 3.8 times further away from the line than the 25th-percentile county. Our estimates imply that distance will cause the 75th-percentile county to have almost 26 percent ($-0.0681 \times 3.8 = -0.258$) lower per capita GDP. During the eighteen years covered by our data, per capita GDP growth in our sample grew from approximately 2744 to 9916 RMB (e.g., the annual growth rate was approximately 7.5 percent), which is approximately a 242% increase. Therefore, a comparison of the effect of distance across space to the increase in GDP over time suggests that the spatial difference attributable to distance from the line is relatively moderate in size.[33]

For the remaining results, we will show only the baseline specification for the sake of brevity. All regressions will control for the full set of baseline controls shown in column (6) of Table 5: the distances to segment cities, the nearest navigable river, the coastline and the country border; the total area of the county; the squared terms of each of the aforementioned variables; and province and year fixed effects. As with the results in Table 5, the estimated coefficients for the distance from the line are very similar with different combinations of controls.[34]

In Table 6, we examine per capita GDP and annual growth in per capita GDP by sector. We estimate the reduced form effect of the distance to the line from equation (1). The estimates for the full sample are shown in Panel A. Columns (1)–(4) show that distance to the line is negatively correlated with GDP levels across sectors. The estimates are statistically significant at the 5% level for per capita GDP in the secondary and tertiary sectors.

Columns (5)–(8) show the estimates of the effect of distance from the line on per capita GDP growth. We calculate per capita GDP growth as the difference between log per capita GDP growth next year and this year for each county, $ln(pcgdp_{c,t+1}) - ln(pcgdp_{c,t})$. To control for the possibility that poorer regions may experience different rates of growth relative to rich regions for reasons that are independent of access to infrastructure (i.e. income may be mean-reverting), we control for two lags of the level measures of the dependent variable: $ln(pcgdp_{c,t-1})$ and

[32] For example, see Feenstra (2002) and the studies referenced there within.

[33] For another benchmark, consider the fact that the 25th and 75th percentile counties grew at 3% and 13% per year, resulting in around 70% and 800% growth in per capita income levels over eighteen years.

[34] These results are available upon request.

Table 4
Distance from the line and transportation infrastructure.

	Dependent Variables: Distance to Infrastructure						
	(1) Ln Distance to RR	(2) Ln Distance to Segment City	(3) Length of Highway	(4) Length of Paved Roads	(5) Ln Distance to Navagable River	(6) Ln Distance to Coastline	(7) Ln Distance to Country Border
A. Historical Lines							
Ln Dist Historical Line	0.202(0.0656)	0.178 (0.0360)	−0.180(0.0814)	0.00104(0.0460)	−0.0491(0.0601)	0.180(0.0658)	−0.0267(0.0151)
Observations R-squared	27440.084	2744 0.189	27440.049	27440.329	27440.082	27440.099	27440.043
B. Expanded Lines							
Ln Distance to Expanded Lines	0.369(0.0708)	0.327 (0.0429)	−0.453(0.0809)	−0.0618(0.0327)	0.0411(0.0550)	0.105(0.0742)	−0.00434(0.0151)
Observations R-squared	26050.163	2605 0.390	26050.158	26050.336	26050.081	26050.086	26050.032
C. Historical and Expanded Lines							
Ln Dist Historical Line	0.169(0.0670)	0.136(0.0349)	−0.0895(0.0752)	0.0107(0.0485)	−0.0484(0.0642)	0.151(0.0696)	−0.0281(0.0165)
Ln Distance to Expanded Lines	0.348(0.0699)	0.310(0.0398)	−0.441(0.0829)	−0.0632(0.0315)	0.0472(0.0563)	0.0860(0.0753)	−0.000817(0.0158)
Observations R-squared	26050.182	26050.432	26050.162	26050.336	26050.083	26050.102	26050.045

Notes: All regressions control for the logarithm of the area of the county, year and province fixed effects. Standard errors are clustered at the county level. These estimates use an unbalanced county-year level panel. Distances to historical lines are computed by the authors. Distance to the expanded set of lines is taken from Faber (2009).

$ln(pcgdp_{c,t-2})$.[35] The estimates are statistically insignificant for all sectors. They are also very small in magnitude, especially when we consider that the mean growth rate in our sample is 4–8% percent per year, depending on the sector. Therefore, we conclude that we find a precisely estimated zero effect of the distance from the line on GDP per capita growth.[36]

Another way to assess the magnitude of the estimates is to make the extreme assumption that being near the line benefits production only through a region's access to railroads. Under this assumption, we can estimate the upper-bound of the effect of the distance from railroads by dividing our main estimates by the estimated correlation between distance to the line and distance to the railroad (e.g., equation (2) with the dependent variable being the log of distance to railroads). This estimate is 0.133 with a standard error is 0.0628 (not shown in tables), which means that conditional on all of the baseline controls, doubling a county's distance from the line increases the distance to the nearest railroad by approximately thirteen percent. Dividing the estimate in Table 6 panel A column (1) by 0.133, we calculate that the maximum elasticity of per capita GDP with respect to the distance to the railroad is 0.5 for all GDP. Dividing the estimate in column (5) by 0.133, we calculate that the maximum elasticity of growth with respect to distance is −0.0156. As we pointed out earlier, the distance to the line is not an excludable instrument for the distance to the railroads because it is also correlated with other forms of transportation infrastructure. However, by interpreting these two-stage calculations as the upper-bound effects of railroads, we can starkly illustrate the small magnitude of the effect of access to transportation on per capita growth relative to the effect on the level of per capita GDP.

One potential issue for interpreting our finding that per capita GDP levels are higher in regions near the line is the possibility of displacement. For example, the placement of transportation may cause a "crowding-in" effect such that firms relocate to be near the line. This could cause proximity to the line to be positively correlated with production even if the investment in having a line does not increase aggregate (provincial or national) production from when there is no line. To investigate this issue, we repeat the estimation on a sample where the 10% nearest counties are excluded, and then again on samples where the 20% are excluded. If the full sample results are caused by productive firms relocating to be very near the railroad, then the estimated effect should decrease in magnitude when we omit those groups (since one would expect firms that choose to relocate to be close to the line to relocate as close as possible to the line).

Table 6 panels B and C provide little support for the crowding-in hypothesizes. For example, a comparison of the estimates in columns (1)–(4) between the full sample estimates in panel A to panel C, where the 20% nearest counties are omitted shows that per capita GDP, if anything, slightly larger in magnitude as we move further away from

[35] To check that our results are not driven by the particular lag structure of the controls, we alternatively control for 3, 4 or 5 year moving averages of lag per capita GDP. Our results are robust and we find no effect of distance to the line on growth. The estimated coefficients are similarly small in magnitude and statistically insignificant. The sample size becomes smaller as we introduce longer lags and the estimates become more imprecise. These estimates are not shown for the sake of brevity, but are available upon request. Note that one could alternatively control for per capita GDP in the first year of the panel. We do not do this because the unbalanced nature of our panel means that we would lose too many observations.

[36] Note that the estimates above avoid the Nickell (1981) bias as we do not control for lag growth. To check that our results are not driven by this choice of specification, we also estimate the growth regression using the more traditional method of the *Arrellano-Bond System Dynamic Panel Estimation*, where we control for the lag of per capita growth rate (Arellano and Bond, 1991). The estimates are presented in Appendix Table A.1 panel A. The estimates are small in magnitude and statistically insignificant. Thus, they are consistent with our main estimates that distance from the lines have little effect on growth.

Table 5
The effect of distance to the line on production levels.

	Dependent Variable Ln Per Capita GDP					
	(1)	(2)	(3)	(4)	(5)	(6)
Ln Distance to Historical Lines	−0.0617−0.0434(0.0286)		−0.0491	−0.0581	−0.0699	−0.0681
		(0.0281)	(0.0277)	(0.0265)	(0.0270)	(0.0272)
Ln Distance to Segment City		0.065	−0.061	−0.063	−0.178	−0.208
		(0.232)	(0.266)	(0.282)	(0.325)	(0.298)
Ln Distance to Segment City^2		−0.0276	−0.0089	−0.0071	0.0072	0.0101
		(0.0277)	(0.0308)	(0.0324)	(0.0372)	(0.0344)
Ln Distance to Navigable River			0.318	0.321	0.366	0.385
			(0.153)	(0.141)	(0.140)	(0.138)
Ln Distance to Navigable River^2			−0.0517	−0.0481	−0.0550	−0.0554
			(0.0200)	(0.0186)	(0.0188)	(0.0183)
Ln Area				−1.572	−1.442	−1.441
				(0.681)	(0.635)	(0.642)
Ln Area^2				0.0983	0.0911	0.0894
				(0.0486)	(0.0459)	(0.0464)
Ln Distance to Coastline					−0.243	−0.207
					(0.224)	(0.219)
Ln Distance to Coastline^2					0.0168	0.0141
					(0.0265)	(0.0259)
Ln Distance to Country Border						−16.49
						(6.097)
Ln Distance to Country Border^2						1.241
						(0.459)
Observations R-squared	2744	2744	2744	2744	2744	2744
	0.818	0.826	0.833	0.845	0.849	0.852

Notes: All regressions control for year and province fixed effects. Standard errors are clustered at the county level. These estimates use an unbalanced county-year level panel. GDP data are from Provincial Statistical Yearbooks. All geographic variables are computed by the authors.

Table 6
The effect of distance to the line on production levels and growth.

	Dependent Variables: Per Capita GDP							
	Ln(Per Capita GDP)				Annual Growth in Ln(Per Capita GDP)			
	All	Primary	Secondary	Tertiary	All	Primary	Secondary	Tertiary
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
A. Full Sample								
Ln Dist Historical Line	−0.0681 (0.0272)	−0.0353 (0.0216)	−0.0944 (0.0458)	−0.0773 (0.0324)	−0.00229 (0.00339)	−0.00025 (0.00523)	−0.00787 (0.00521)	−0.00104 (0.00357)
Observations	2744	2266	2266	2199	1110	1018	1018	899
R-squared	0.852	0.870	0.722	0.842	0.204	0.225	0.182	0.181
B. Omit 10% Nearest								
Ln Dist Historical Line	−0.0677 (0.0396)	−0.0492 (0.0408)	−0.0634 (0.0643)	Dependent Variables: Per Capita GDP	−0.00180 (0.00541)	0.00942 (0.00754)	−0.00763 (0.00889)	−0.00437 (0.00709)
Observations	2480	2031	2031	Ln(Per	1004	914	914	808
R-squared	0.854	0.870	0.722	Capita GDP)	0.226	0.285	0.170	0.175
C. Omit 20% Nearest								
Ln Dist Historical Line	−0.104 (0.050)	−0.117 (0.049)	−0.109 (0.082)	−0.077 (0.054)	−0.00102 (0.00556)	0.00998 (0.01130)	−0.00325 (0.00998)	−0.00374 (0.00610)
Observations	2200	1789	1789	1728	882	798	798	692
R-squared	0.860	0.879	0.721	0.849	0.256	0.306	0.163	0.173

Notes: All regressions control for the full set of baseline controls in Table 5 column (6). The growth regressions in columns (5)–(8) also control for the one and two year lags of sector-specific per capita GDP levels. Standard errors are clustered at the county level. The sample is an unbalanced county-year level panel. The GDP data are from Provincial Statistical Yearbooks. All geographic variables are computed by the authors.

the line. As with the full sample, we find no effect on per capita GDP growth.

6.3. *The effect on firm placement and household income*

Table 7 shows the estimated effects of the distance from the line on the number and average profits of manufacturing firms. Panel A shows the estimates for the full sample. Columns (1)–(4) show that distance from the line results in fewer firms. The estimates are statistically significant at the 1% level for all firm ownership types. The coefficient in column (1) indicates that increasing the distance by 1% will result in a 0.09% reduction in the number of firms. In columns (5)–(8), we exam-

Table 7
The effect of distance to the line on firm location and profits.

	Dependent Variables Per Capita GDP							
	Ln Number of Firms				Average Firm Profit			
	(1) All	(2) Public	(3) Mixed	(4) Individual	(5) All	(6) Public	(7) Mixed	(8) Individual
A. Full Sample								
Ln Distance to Historical Lines	−0.091	−0.062	−0.089	−0.122	−0.105	−0.100	−0.072	−0.028
	(0.022)	(0.025)	(0.033)	(0.032)	(0.040)	(0.048)	(0.067)	(0.043)
Observations	3321	3321	3321	3321	2763	2416	1642	1503
R-squared	0.639	0.680	0.700	0.785	0.449	0.319	0.203	0.342
B. Omit Nearest 10%								
Ln Distance to Historical Lines	−0.127	−0.095	−0.101	−0.146	−0.150	−0.167	−0.095	−0.046
	(0.035)	(0.036)	(0.054)	(0.054)	(0.065)	(0.076)	(0.095)	(0.070)
Observations	2994	2994	2994	2994	2477	2151	1460	1343
R-squared	0.647	0.693	0.691	0.776	0.443	0.322	0.211	0.348
C. Omit Nearest 20%								
Ln Distance to Historical Lines	−0.152	−0.076	−0.138	−0.158	−0.174	−0.063	−0.011	−0.018
	(0.046)	(0.046)	(0.068)	(0.073)	(0.085)	(0.090)	(0.122)	(0.099)
Observations	2663	2663	2663	2663	2196	1897	1288	1182
R-squared	0.657	0.695	0.701	0.772	0.449	0.332	0.223	0.337

Notes: All regressions control for the full set of baseline controls in Table 5 column (6). Standard errors are clustered at the county level. The sample is an unbalanced county-year level panel. The data for the dependent variables are from the Censuses of Manufacturing Firms. All geographic variables are computed by the authors.

ine log average firm profits. The estimates show that amongst counties that have at least one firm (of the relevant type), distance results in lower profits. The estimates are statistically significant for all firms and publicly owned firms at the 1% and 5% levels. Column (5) shows that a 1% increase in distance results in a 0.1% reduction in average firm profits. The estimates for mix ownership and individually owned firms are negative but imprecisely estimated.

As with our earlier exercise, we assess the magnitude of our estimates by comparing our estimates of the effect of distance on the number of firms across space to the total increase in the number of firms over time. Since the 75th-percentile county in terms of distance from the line is approximately 4.68 times further away than the 25th-percentile county, our estimate in column (1) implies that it should have approximately 42.6 percent fewer firms ($-0.091 \times 4.68 = -0.426$).[37] During the three years for which our data use a consistent sampling frame (2004-06), the average number of firms per county grew by twelve percent from approximately 83 to 93 firms per county.[38] Relative to the change over time, our estimate of the distance therefore implies a large effect. However, this is mostly an artifact of the short time horizon of the firm panel data. For example, if the number of firms had grown at the same rate (approximately five percent per year) for eighteen years (which is the sample length of our GDP data), then the number of firms would have grown by approximately 130% from approximately 40 to 91 firms per county. Relative to the cumulative growth over the longer time horizon, the implied effect of distance on the number of firms appear to be higher.

Repeating the same calculation for average firm profits, the estimate in column (5) implies that firms in the 75th-percentile county in terms of distance from the line should have approximately 39% lower profits than firms in the 25th-percentile county on average ($-0.105 \times 3.68 = -0.386$). In contrast, average firm profits grew at approximately sixty percent per year during 2004-6. If this was sustained for eighteen years, the cumulative growth in firm profits would be 2950%. While this crude estimate of cumulative profit growth is likely to be significantly higher than actual firm profit growth over the eighteen year period, it nevertheless illustrates the fact that the implied elasticities between distance from the line and firm profits is relatively small in size.

In panels B and C, we repeat the estimates on samples where the nearest 10% and 20% counties to the line are excluded. The estimates do not decline monotonically as we examine more distant firms. Thus, our finding that more firms locate nearer the line is unlikely to reflect a crowding-in effect.

We also examined the effect of distance on the growth of the number of firms, the growth of average firm profits and the returns to capital as measured by profits divided by the value of total capital. These estimates were negative, small in magnitude and statistically insignificant. We do not report them in the paper for the sake of brevity and because of concerns over the quality of the data for returns to capital. Specifically, it is unclear how capital is valued by these firms. Much of the capital is inherited from the state or collectives and one would only know the market value if she observed the market transaction of another similar piece of capital. If further away regions have fewer market transactions such that firms there are more likely to under-value the capital, then our estimate of the returns to capital will systematically over-state the effect of the line as we move further away from the line. This measurement issue is a generic problem in the Chinese data on firm assets.

Table 8 shows the estimated effects of distance on average household income for agricultural households at the village level. Panel A column (1) shows that distance from the line is negatively correlated with the Gini coefficient for village household incomes. The estimate is statistically significant at the 5% level. In Column (5), we estimate the effect of distance on the annual change of the Gini coefficient. It shows that distance from the line is correlated with slower growth in inequality. The estimate is statistically significant at the 1% level. The estimated effects on income growth are statistically indistinguishable

[37] Note that the geographic coverage varies across the samples in Tables 6–8, which means that the distance from the line for the 75th and 25th percentile counties will also differ.

[38] Recall that the 1993 firm data is from a census of all industrial plants and has a different sampling frame relative to the *Census of Manufacturing* firms which includes the former and, in addition, privately owned manufacturing firms with five million RMB or more in revenues.

Table 8
The effect of distance to the line on income inequality.

	Dependent Variables: Household Income Distribution							
	Ln (HH Income) by Percentile				Annual Growth Rate			
	(1) Gini	(2) 10th	(3) 50th	(4) 90th	(5) Gini	(6) 10th	(7) 50th	(8) 90th
A. Full Sample								
Ln Distance to Historical Lines	−0.0071	−0.0285	−0.0120	−0.0158	−0.00195	−0.00018	−0.00182	−0.00257
	(0.0031)	(0.0255)	(0.0236)	(0.0276)	(0.00058)	(0.00330)	0.00193)	(0.00270)
Observations	1897	1897	1897	1897	1533	1533	1533	1533
R-squared	0.272	0.454	0.590	0.582	0.036	0.124	0.171	0.095
B. Omit Nearest 10%								
Ln Distance to Historical Lines	−0.0050	0.0176	0.0291	0.0198	−0.00184	0.00003	0.00191	−0.00287
	(0.0054)	(0.0393)	(0.0328)	(0.0418)	(0.00095)	(0.00454)	(0.00342)	(0.00463)
Observations	1722	1722	1722	1722	1392	1392	1392	1392
R-squared	0.253	0.478	0.596	0.583	0.041	0.144	0.200	0.098
C. Omit Nearest 20%								
Ln Distance o Historical Lines	−0.0025	0.0582	0.0696	0.0625	−0.00079	0.00340	0.00523	0.00394
	(0.0075)	(0.0477)	(0.0403)	(0.0556)	(0.00113)	(0.00574)	(0.00445)	(0.00508)
Observations	1531	1531	1531	1531	1238	1238	1238	1238
R-squared	0.262	0.499	0.619	0.606	0.043	0.134	0.185	0.088

Notes: All regressions control for the full set of baseline controls in Table 5 column (6). The regression in column (5) also controls for the 1 and 2 year lag of the gini, and the regressions in columns (6)–(8) control for the 1 and 2 year lags of the relevant income levels. Standard errors are clustered at the county level. The sample is an unbalanced panel of villages. Income data are from the National Fixed Point Survey. All geographic data are computed by the authors.

Table 9
Historical lines and expanded lines.

	Dependent Variables					
	Ln PC GDP		Gini		Ln Total # Firms	
	(1)	(2)	(3)	(4)	(5)	(6)
Ln Distance to Hist Line	−0.0823	−0.0826	−0.00715	−0.00795	−0.102	−0.101
	(0.0276)	(0.0279)	(0.00333)	(0.00383)	(0.025)	(0.025)
Ln Distance to Expanded Lines		−0.0224		−0.00363		−0.029
		(0.0326)		(0.00589)		(0.028)
Observations	2605	2605	1070	1070	2704	2704
R-squared	0.860	0.860	0.319	0.320	0.665	0.666

Notes: All regressions control for the full set of baseline controls in Table 5 column (6). Standard errors are clustered at the county level. The sample is an unbalanced county-year level panel. The GDP data used in column (1)–(2) are from Provincial Statistical Yearbooks. The income data used in columns (3)–(4) are from the National Fixed Point Surveys. The firm data used in columns (5)–(6) are from the Censuses of Manufacturing Firms. The distance to the historical line is calculated by the authors. The distance to the expanded set of lines is taken from Faber (2009).

from zero.[39]

In panels B and C, we present the results for restricted samples where we omit the 10% and 20% counties nearest the line. The estimates show that the effect on household income inequality is mainly driven by the nearest counties. This might reflect that these areas are the ones that both gain the most in terms of trade opportunities, but also lose the most from capital (and, though it is not in our model, human capital) mobility.

6.4. Robustness

6.4.1. Additional lines

One obvious concern with our strategy regards the relevance of our historical lines. Earlier in this section, we showed that proximity to our lines is positively correlated with proximity to transportation infrastructure such as railroads and coastal routes. However, our estimates also suggested that our lines are uncorrelated with the more recently constructed paved motorways, which have been found by Faber (2014) to also affect production and growth. In this section, we test that our main results are robust to controlling for access to such recent transportation infrastructure. Specifically, we directly control for distance to the expanded set of lines constructed by an earlier working paper version of Faber (2014), Faber (2009), which the author kindly shared with us. This expanded set of lines includes our historical lines of transportation and adds many additional lines to capture recently constructed road networks.

First, we investigate the difference between the historical and expanded lines in terms of how each correlates to transportation networks. Table 4 Panel B shows that on average, the distance from the expanded set of lines is positively correlated to distance from railroads and the distance from the segment city; negatively correlated with the length of highways and roads; and uncorrelated with distance from rivers, coastline and country borders. Therefore the key difference

[39] Arellano-Bond estimates are presented in Appendix Table A1 panel B. They are consistent with the main results in showing that distance from the line has little effect on income growth. The estimates are small in magnitude and statistically insignificant.

Table 10
Correlation between distance to the historical lines and geo-climatic determinants of growth.

	Dependent Variable											
	Natural Conditions										Man-made Factors	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Buckwheat	Maize	Rice (Wet)	Sorghum	Soy	Sweet Potato	Wheat	White Potato	Log Spring Temp	Log Spring Rain	Log Dist to Grand Canal	Exposure to Taiping Tianguo
A. County GDP Sample												
Ln Dist	−11.59	−83.36	−48.89	−87.26	−50.35	107.8	−87.41	−118.3	−0.00013	−0.0148	−0.0334	−0.0260
Historical Line	(11.11)	(57.67)	(39.50)	(46.55)	(20.25)	(48.24)	(41.98)	(60.53)	(0.00152	(0.0073	(0.0389)	(0.0259)
Observations	295	295	295	295	295	295	295	295	177	177	295	295
R-squared	0.638	0.800	0.907	0.865	0.752	0.949	0.627	0.334	0.828	0.928	0.791	0.222
B. Firms Sample												
Ln Dist	−18.05	5.929	28.95	−23.53	−12.80	68.18	−114.0	−105.3	−0.00119	−0.00548	−0.0416	−0.0099
Historical Line	(5.648)	(26.15)	(24.87)	(20.97)	(1037).	(30.35)	(17.71)	(22.80)	(0.00069)	(0.00312)	(0.0141)	(0.0085)
ObservationsR-	1817	1817	1817	1817	1817	1817	1817	1817	1098	1098	1817	1817
squared	0.706	0.693	0.647	0.808	0.647	0.806	0.720	0.578	0.784	0.905	0.822	0.347
C. Household Income Sample												
Ln Dist	−5.565	60.48	23.21	−16.56	−9.501	59.92	−47.01	−82.65	−0.00183	0.0119	−0.0471	−0.0304
Historical Line	(12.81)	(64.92)	(59.89)	(68.39)	(16.98)	(142.3)	(37.19)	(74.34)	(0.00211)	(0.0175)	(0.0635)	(0.0331)
Observations	122	122	122	122	122	122	122	122	86	86	122	122
R-squared	0.784	0.885	0.848	0.925	0.934	0.874	0.906	0.812	0.854	0.912	0.827	0.484

Notes: All regressions are cross-sectional estimates. They control for the full set of baseline controls in Table 5 column (6) in the manuscript, except for the year fixed effects. Robust standard errors are presented. Suitability data are computed from the FAO's GAEZ database. Weather data are computed from The Terrestrial Air Temperature and Precipitation: Monthly and Annual Time Series (1950–1996). In column (11), the distance to the Grand Canal is computed from the ArcGIS database provided by the Harvard Yenching Insitute (2016), CHGIS, Version 6, Cambridge. In column (12), exposure is a dummy variable that equals one if a county was part of the Taiping Kingdom or ever experienced a Taiping-related battle. The data are coded by the authors based on Hua (1991).

Table 11
Robustness to additional controls – agro-climatic suitability for the cultivation of staple crops.

	Dependent Variables: Per Capita GDP							
	Ln(Per Capita GDP)				Annual Growth in Ln(Per Capita GDP)			
	All (1)	Primary (2)	Secondary (3)	Tertiary (4)	All (5)	Primary (6)	Secondary (7)	Tertiary (8)
Ln Dist Historical Line	−0.0681 (0.0272)	−0.0353 (0.0216)	−0.0944 (0.0458)	−0.0773 (0.0324)	−0.00229 (0.00339)	−0.00025 (0.00523)	−0.00787 (0.00521)	−0.00104 (0.00357)
A. Baseline								
Observations	2744	2266	2266	2199	1110	1018	1018	899
R-squared	0.852	0.870	0.722	0.842	0.204	0.225	0.182	0.181
Observations	2744	2266	2266	2199	1110	1018	1018	899
R-squared	0.852	0.870	0.722	0.842	0.204	0.225	0.182	0.181
B. Control for Agro-climatic Suitability*								
Ln Dist Historical Line	−0.0537 (0.0268)	−0.0363 (0.0211)	−0.0728 (0.0457)	−0.0587 (0.0319)	−0.00041 (0.00333)	0.00156 (0.00496)	−0.00568 (0.00507)	−0.00099 (0.00344)
Observations	2744	2266	2266	2199	1110	1018	1018	899
R-squared	0.876	0.887	0.761	0.868	0.212	0.233	0.188	0.187
Ln Number of Firms					Average Firm Profits			
	All	Public	Mixed	Individual	All	Public	Mixed	Individual
C.Baseline								
Ln Dist Historical Line	−0.091 (0.022)	−0.062 (0.025)	−0.089 (0.033)	−0.122 (0.032)	−0.105 (0.040)	−0.100 (0.048)	−0.072 (0.067)	−0.028 (0.043)
Observations	3321	3321	3321	3321	2763	2416	1642	1503
R-squared	0.639	0.680	0.700	0.785	0.449	0.319	0.203	0.342
D. Control for Agro-climatic Suitability*								
Ln Dist Historical Line	−0.0816 (0.0210)	−0.0649 (0.0260)	−0.0614 (0.0324)	−0.1010 (0.0312)	−0.0913 (0.0391)	−0.0808 (0.0475)	−0.0822 (0.0665)	−0.0238 (0.0428)
Observations	3321	3321	3321	3321	2763	2416	1642	1503
R-squared	0.691	0.690	0.725	0.813	0.455	0.335	0.219	0.359
Ln(HH Income) by Percentile					Annual Growth Rate			
	Gini	10th	50th	90th	Gini	10th	50th	90th
E Baseline								
Ln Dist Historical Line	−0.0071 (0.0031)	−0.0285 (0.0255)	−0.0120 (0.0236)	−0.0158 (0.0276)	−0.00195 (0.00058)	−0.00018 (0.00330)	−0.00182 (0.00193)	−0.00257 (0.00270)
Observations	1897	1897	1897	1897	1533	1533	1533	1533
R-squared	0.272	0.454	0.590	0.582	0.036	0.124	0.171	0.095
F. Control for Agro-climatic Suitability*								
Ln Dist Historical Line	−0.0046 (0.0032)	−0.0448 (0.0242)	−0.0233 (0.0224)	−0.0198 (0.0274)	−0.00153 (0.00061)	−0.00286 (0.00326)	−0.00262 (0.00195)	−0.00280 (0.00281)
Observations	1897	1897	1897	1897	1533	1533	1533	1533
R-squared	0.310	0.481	0.613	0.595	0.038	0.126	0.172	0.097

Notes: All regressions control for agro-climatic suitability for the cultivation of buckwheat, maize, wet rice, sorghum, soy, sweet potato, wheat, white potato, and the full set of baseline controls in Table 5 column (6) in the manuscript. Suitability data are computed from the FAO's GAEZ database. All standard errors are clustered at the county level. In Panels A–D, the samples are unbalanced county-year level panels. In Panels A and B, the data for the dependent variables are from from the Provincial Statistical Yearbooks. In Panels C and D, the data for the dependent variables are from the Censuses of Manufacturing Firms. In Panels E and F, the regression in column (5) also controls for the 1 and 2 year lag of the gini, and the regressions in columns (6)–(8) control for the 1 and 2 year lags of the relevant income levels. The sample is an unbalanced panel of villages. Income data are from the National Fixed Point Survey.

between the expanded set of lines and our historical lines is that the distance to the former are negatively correlated with road density (recall that we control for area of the county), while the distance to the latter are uncorrelated with road density. This is consistent with the fact that the new lines capture new road networks built away from the railroads. In Panel C, we examine the correlations of our historical and expanded set of lines with transportation infrastructure in one regression. The correlation between our historical lines and transportation infrastructure are robust to controlling for the additional lines.

In Table 9, we test the robustness of our main estimates by running a "horse race" between the historical and expanded sets of lines. For brevity, we focus on the main outcomes of interest.[40] Note that the sample size is smaller than the one for our main estimates because the data in (Faber, 2009) do not exactly match to ours. Nevertheless, our baseline estimates from using this restricted sample are similar to those

[40] Results using other outcomes are consistent in showing that our main specification is very robust. They are available upon request.

Table 12
Robustness to additional controls – weather conditions.

	Dependent Variables: Per Capita GDP							
	Ln(Per Capita GDP)				Annual Growth in Ln(Per Capita (GDP)			
	All (1)	Primary (2)	Secondary (3)	Tertiary (4)	All (5)	Primary (6)	Secondary (7)	Tertiary (8)
A. Baseline								
Ln Dist Historical Line	−0.0681 (0.0272)	−0.0353 (0.0216)	−0.0944 (0.0458)	−0.0773 (0.0324)	−0.00229 (0.00339)	−0.00025 (0.00523)	−0.00787 (0.00521)	−0.00104 (0.00357)
Observations R-squared	2744 0.852	2266 0.870	2266 0.722	2199 0.842	1110 0.204	1018 0.225	1018 0.182	899 0.181
B. Control for Spring Temperature and Precipitation								
Ln Dist Historical Line	−0.0657 (0.0376)	−0.0040 (0.0313)	−0.1070 (0.0638)	−0.0821 (0.0446)	−0.00041 (0.00333)	0.00156 (0.00496)	−0.00568 (0.00507)	−0.00099 (0.00344)
Observations	1745	1450	1450	1389	1110	1018	1018	899
R-squared	0.870	0.885	0.752	0.861	0.212	0.233	0.188	0.187
Ln Number of Firms					Average Firm Profits			
	All	Public	Mixed	Individual	All	Public	Mixed	Individual
C.Baseline								
Ln Dist Historical Line	−0.091 (0.022)	−0.062(0.025)	−0.089 (0.033)	−0.122 (0.032)	−0.105 (0.040)	−0.100 (0.048)	−0.072 (0.067)	−0.028 (0.043)
Observations	3321	33210.680	3321	3321	2763	2416	1642	1503
R-squared	0.639		0.700	0.785	0.449	0.319	0.203	0.342
D. Control for Spring Temperature and Precipitation								
Ln Dist Historical Line	−0.0638 (0.0291)	−0.0618 (0.0320)	−0.0892 (0.0450)	−0.0823 (0.0431)	−0.0891 (0.0528)	−0.0843 (0.0636)	−0.0325 (0.0912)	−0.0247 (0.0663)
Observations	2128	2128	2128	2128	1731	1522	1002	905
R-squared	0.692	0.698	0.712	0.775	0.407	0.294	0.236	0.313
Ln(HH Income) by Percentile					Annual Growth Rate			
	Gini	10th	50th	90th	Gini	10th	50th	90th
E.Baseline								
Ln Dist Historical Line	−0.0071 (0.0031)	−0.0285 (0.0255)	−0.0120 (0.0236)	−0.0158 (0.0276)	−0.00195 (0.00058)	−0.00018 (0.00330)	−0.00182 (0.00193)	−0.00257 (0.00270)
Observations	1897	1897	1897	1897	1533	1533	1533	1533
R-squared	0.272	0.454	0.590	0.582	0.036	0.124	0.171	0.095
F. Control for Spring Temperature and Precipitation								
Ln Dist Historical Line	−0.0099 (0.0041)	−0.0032 (0.0363)	0.0056 (0.0291)	−0.0003 (0.0294)	−0.00405 (0.00093)	0.00191 (0.00470)	−0.00178 (0.00230)	−0.00565 (0.00376)
Observations	1344	1344	1344	1344	1087	1087	1087	1087
R-squared	0.336	0.504	0.634	0.649	0.048	0.134	0.181	0.130

Notes: All regressions control for log average spring temperature and log average spring precipitation, and the full set of baseline controls in Table 5 column (6) in the manuscript. Weather data is reported by the Terrestrial Air Temperature and Precipitation: Monthly and Annual Time Series (1950–1996) data set. All standard errors are clustered at the county level. In Panels A–D, the samples are unbalanced county-year level panels. In Panels A and B, the data for the dependent variables are from from the Provincial Statistical Yearbooks. In Panels C and D, the data for the dependent variables are from the Censuses of Manufacturing Firms. In Panels E and F, the regression in column (5) also controls for the 1 and 2 year lag of the gini, and the regressions in columns (6)–(8) control for the 1 and 2 year lags of the relevant income levels. The sample is an unbalanced panel of villages. Income data are from the National Fixed Point Survey.

from using our full sample. The estimates show that our baseline estimates of the effect of historical lines are very robust to controlling for the additional lines and suggest that the historical lines are indeed the relevant lines to study in our context.

Faber (2014) finds that Chinese counties that are along the way between two cities connected by the modern trunk network have lower GDP growth than unconnected cities that are not along the way. For comparison purposes, note two important differences between his analysis and ours. The first is the sample. As we note above, the geographic coverage is different. Also, his study focuses on 1997–2006, whereas our data coverage begins in 1986. The second is that his study uses many more lines to capture the modern trunk work laid out in 1997 than we use to capture the historical network (see Faber (2014) Fig. 3). There are tradeoffs to increasing the number of lines. One the one hand, it follows the modern network more closely, which increases the strength of the first stage if used as an instrumental variable as in Faber (2014). On the other hand, it introduces the concern that increased distance from one line could reflect proximity to another line. Because of the latter concern, we choose to have a more parsimonious set of lines.

6.4.2. Additional controls

Table 10 examines the relationship between the distance to the line and potential drivers of growth. We categorize these factors into

Table 13
Robustness to additional controls – log distance to the grand canal.

	Dependent Variables: Per Capita GDP							
	Ln(Per Capita GDP)				Annual Growth in Ln(Per Capita GDP)			
	All	Primary	Secondary	Tertiary	All	Primary	Secondary	Tertiary
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A.Baseline								
Ln Dist Historical Line	−0.0681	−0.0353	−0.0944	−0.0773	−0.00229	−0.00025	−0.00787	−0.00104
	(0.0272)	(0.0216)	(0.0458)	(0.0324)	(0.00339)	(0.00523)	(0.00521)	(0.00357)
Observations R-squared	2744	2266	2266	2199	1110	1018	1018	899
	0.852	0.870	0.722	0.842	0.204	0.225	0.182	0.181
B. Control for Ln Dist to Grand Canal								
Ln Dist Historical Line	−0.0671	−0.0345	−0.0904	−0.0755	−0.00284	−0.00165	−0.00846	−0.00145
	(0.0267)	(0.0215)	(0.0452)	(0.0319)	(0.00332)	(0.00504)	(0.00510)	(0.00352)
Observations	2744	2266	2266	2199	1110	1018	1018	899
R-squared	0.854	0.870	0.725	0.843	0.206	0.229	0.183	0.182
	Ln Number of Firms				Average Firm Profits			
	All	Public	Mixed	Individual	All	Public	Mixed	Individual
C Baseline								
Ln Dist Historical Line	−0.091	−0.062	−0.089	−0.122	−0.105	−0.100	−0.072	−0.028
	(0.022)	(0.025)	(0.033)	(0.032)	(0.040)	(0.048)	(0.067)	(0.043)
Observations R-squared	3321	3321	3321	3321	2763	2416	1642	1503
	0.639	0.680	0.700	0.785	0.449	0.319	0.203	0.342
D. Control for Ln Dist to Grand Canal								
Ln Dist Historical Line	−0.092	−0.065	−0.083	−0.121	−0.106	−0.104	−0.073	−0.034
	(0.022)	(0.025)	(0.033)	(0.032)	(0.040)	(0.048)	(0.067)	(0.043)
Observations R-squared	3321	3321	3321	3321	2763	2416	1642	1503
	0.639	0.680	0.701	0.785	0.449	0.319	0.203	0.343
	Ln(HH Income) by Percentile				Annual Growth Rate			
	Gini	10th	50th	90th	Gini	10th	50th	90th
E.Baseline								
Ln Dist Historical Line	−0.0071	−0.0285	−0.0120	−0.0158	−0.00195	−0.00018	−0.00182	−0.00257
	(0.0031)	(0.0255)	(0.0236)	(0.0276)	(0.00058)	(0.00330)	(0.00193)	(0.00270)
Observations R-squared	1897	1897	1897	1897	1533	1533	1533	1533
	0.272	0.454	0.590	0.582	0.036	0.124	0.171	0.095
F. Control for Ln Dist to Grand Canal								
Ln Dist Historical Line	−0.0072	−0.0299	−0.0124	−0.0157	−0.00196	−0.00014	−0.00177	−0.00239
	(0.0032)	(0.0257)	(0.0237)	(0.0277)	(0.00058)	(0.00334)	(0.00194)	(0.00270)
Observations R-squared	1897	1897	1897	1897	1533	1533	1533	1533
	0.272	0.455	0.590	0.582	0.036	0.124	0.171	0.095

Notes: All regressions control for log distance to the Grand Canal, and the full set of baseline controls in Table 5 column (6) in the manuscript. The distance to the Grand Canal is computed from the ArcGIS database provided by the Harvard Yenching Insitute (2016), CHGIS, Version 6, Cambridge. In Panels A and B, the data for the dependent variables are from from the Provincial Statistical Yearbooks. In Panels C and D, the data for the dependent variables are from the Censuses of Manufacturing Firms. In Panels E and F, the regression in column (5) also controls for the 1 and 2 year lag of the gini, and the regressions in columns (6)–(8) control for the 1 and 2 year lags of the relevant income levels. The sample is an unbalanced panel of villages. Income data are from the National Fixed Point Survey.

two groups: geo-climatic factors and man-made factors. The first group includes agro-climatic suitability to Chinese staple crops (buckwheat, maize, wet rice, sorghum, soy, sweet potato, wheat, white potato) and weather. For the latter, we examine the log of spring temperature and the log of spring rainfall, because higher rainfall and temperature during the spring months is the most important predictor of agricultural production in China on average (Meng et al., 2015).[41]

To examine whether our baseline specification suffers from omitted variable bias, we regress each suitability and weather variable on the distance to the line while including the full set of baseline controls. The only difference is that because there is only one cross-section of data, we do not control for year fixed effects.[42] Table 10 Panels A, B and C columns (1)–(10) present the correlations for the three samples used in our paper. We see that distance to the line is correlated with some of the measures. However, there is no obvious pattern – some signs are positive, while others are negative.

To investigate whether our results are confounded by omitted variables bias, we include all of the suitability measures into the baseline. Table 11 shows that the results are very similar. A few of the coefficients become slightly less precisely estimated when we add the large number of controls, but are statistically similar in magnitude to the baseline.

In Table 12, we include the two weather measures in the baseline. We examine weather separately from suitability for two reasons. First, the sample size for the weather examination is slightly smaller. Second,

[41] The data for suitability are computed from the FAO's GAEZ database. We choose irrigation as an input. The results are nearly identical if we choose rain-fed irrigation as an input. The weather data is reported by the *Terrestrial Air Temperature and Precipitation: Monthly and Annual Time Series (1950–1996)* data set. The latter does not cover all counties in China. Thus, the sample size will be slightly smaller when we include whether. Appendix F a display the data.

[42] We present robust standard errors (instead of clustering them at the county level).

weather conditions are already internalized by the suitability calculations and having them both in the same regression would lead to over controlling. Again, the results are very similar to the baseline.

The second group of omitted variables that we consider is man-made: the distance to the Grand Canal and exposure to the Taiping Rebellion. The Grand Canal was an important transportation route historically as it connected the major rivers. In 1855, the course of Yellow River, one of the most important in China, changed due to natural causes, which led to the closing of important sections of the Canal. Together with the decline in Qing government administrative capacity, the Grand Canal became obsolete (until a major revival effort in the 1990s). Our distance measure is computed from the ArcGIS database provided by the Harvard Yenching Insitute (2016).[43]

Taiping Tianguo (Taiping Rebellion) was a Christian anti-Qing state which formed in parts of Southern China during 1851–1864. The rebel kingdom was laid under siege by the Qing government for most of its existence. The rebellion was eventually crushed by the Qing with the help of Western Imperial powers. The fighting continued for seven years after the fall of the kingdom in 1871. Because the first wave of Treaty Ports that we use to construct the lines were established in 1842-44, the intensity of the Taiping Rebellion should arguably be an outcome variable rather than a control. One can make this case given the historical evidence (Spence, 1997).

For data, we rely on a Chinese language source, Hua (1991). We hand-code exposure as a dummy variable that equals one if a county was part of the Taiping rebel kingdom or if a Taiping-connected battle ever took place in that county. Appendix Figure A.9 maps the data. The darker regions indicate areas where at least one Taiping-connected battle took place. The lighter regions indicated parts of the Taiping Kingdom.

Table 10 columns (11)–(12) examines the correlations between these two variables and log distance to the historical lines, controlling for the same baseline controls as we did for the natural condition variables. The only statistically significant correlation is between the distance to the canal and the distance to the line for the firm sample in Panel B column (11).

To ensure that we do not have an omitted variables problem, we re-estimate the baseline for all of the main results including log distance to the Grand Canal. Table 13 shows that our results are entirely robust to its inclusion. We do not attempt to control for exposure to the Taiping Rebellion since it may be an outcome of the European invasions. However, note that the our finding no correlation between the Taiping Rebellion and the distance to the line means that including this additional control will not affect our results.

6.4.3. Omit Border Regions

In the last robustness exercise, we omit the border provinces: Heilongjiang, Liaoning, Jilin, Inner Mongolia, Qinghai, Xinjiang, Tibet, Guangxi, Gansu and Yunnan. This effectively excludes all of the counties that lie beyond the last historical city in the network.[44] Appendix Table A2 presents the results with the full sample from the paper and the restricted sample. They are very similar.

7. Conclusion

In this paper, we investigate the effects of access to transportation infrastructure on economic development during the two decades after China opened up to trade and market reforms, when it experienced rapid GDP growth. We find that regions closer to historical transportation networks have higher levels of GDP per capita, higher income inequality, a higher number of firms and higher average firm profits. However, these level differences are relatively small in magnitude and we find no evidence that distance affected income growth during this period.

Our results do not contradict the Fogelian (Fogel, 1962, 1964) interpretation or the view of (Huang, 2008) that during this period of fast growth, the Chinese government should not have focused so much on building transportation infrastructure. However, they are also consistent with an alternative explanation where the infrastructure might have brought sizable benefits for the economy as a whole, but the localization of the gains (and the overall level of the gains) was limited by the lack of factor mobility. The fact that we do not see a strong divergence between well and poorly connected areas does not rule out the possibility that infrastructure had benefits for all of them, but the lack of factor mobility prevented the gains from being concentrated in relatively better connected areas.

These results should not discourage those who believe that investment in transportation infrastructure can promote economic development. Rather, they highlight the importance of other factors which determine the economic impact of infrastructure. Moreover, as we noted in the introduction of this paper, without knowing the returns of such investment, one cannot say whether investments in transportation infrastructure ought to be made. Finding credible ways to estimate or even bound the social returns remains a very important next step in this research agenda.

Appendix.

A. Model

There are a number of reasons why good transportation infrastructure can be advantageous for economic development. First, it plausibly reduces trade costs and promotes market integration. This should lead to a convergence in prices, reduce price volatility and reallocate resources along the lines of comparative advantage. It also increases market size, which allows firms to capture gains from specialization, increasing returns, and promotes more intense competitio. Second, it promotes factor mobility – e.g., it is easier to migrate to the city if one can return easily whenever needed; easier to lend to a borrower whose project one can visit; and easier to deposit one's savings in a bank if the bank is more accessible. Third, it is easier to take advantage of opportunities for investment in human capital – e.g., one can send her child to a better school or take her to a better doctor. Finally, there are intangible benefits. For example, freer movement of people and goods may bring with it new aspirations, new ideas and information about new technologies.

B. A Simple Model of Trade and Factor Mobility

The goal of the model is to look at the effects of distance in a setting where distance affects both the mobility of goods and that of factors of production. The model will illustrate how access to infrastructure can produce very different results depending on which of the two is more affected by distance. In order to get at these issues in the most direct possible ways, we shut down many of the standard dynamic effects coming from capital accumulation and population growth. We recognize that excluding capital accumulation would be an especially bad assumption if we were trying

[43] CHGIS, Version 6, Cambridge, MA. Appendix Fig. A.5 shows the map of the Grand Canal.

[44] Appendix Fig. A.8 shows a map of these regions.

to quantitatively match the performance of the Chinese economy. That is not our goal here. Instead, we simply aim to qualitatively understand the consequences of there being multiple types of mobility, and bringing in the accumulation of factors is unlikely to add important new insights.

B.1. Building Blocks

There are $M + N + 1$ regions in this economy: M distant regions, N connected regions and 1 metropolis. Each region produces one good exclusively for export which could be the same as or different from the goods that it imports (e.g. food), and another good which it consumes. These goods could be either identical or differentiated. The key assumption is that the relative price of the exportable good in terms of the importable good in the "world market" is the same, p. However, distance to the market adds to the cost of exporting. We model this by assuming that this transportation cost is increasing in distance from the market such that the price received by the exporters is p in the metropolis, $p(1 - d_1)$ in the connected region and $p(1 - d_2)$ in the distant regions, where $d_2 > d_1$.

Production is carried out by a population of firms of identical size in each region. Production requires two inputs which we will call labor and capital, but could also be labor and human capital with small adjustments in the arguments. Output of the exportable is given by $AK^{\alpha}L^{1-\alpha}(\overline{K})^{\beta}$ everywhere, where $\overline{K}$ is the average level of K in firms in that region.[45] In other words, in the urban economics tradition, we allow for spillovers from co-location. However, we assume that the spillovers are not so large as to swamp diminishing returns entirely: $\alpha + \beta < 1$.[46] Assume for the time being that there is no other technology of production.

The key assumption is with respect to factor mobility. We assume that labor does not move: The metropolis has an endowment of labor of L^* while all other regions have an endowment of L'. Capital, on the other hand, does move, but moving is costly. We assume that in equilibrium, the direction of movement that would be needed is from the various regions to the metropolis. This is consistent with the view that in the initial years of Chinese growth after 1978, much of the growth and capital accumulation occurred in rural areas, and it was only later that economic freedoms were extended to urban areas and the urban growth rate crossed its rural counterpart. Therefore, when the rental rate for capital in the metropolis is r, we assume that the opportunity cost of capital in the connected regions is $r(1 - \rho d_1)$ and that in the distant regions is $r(1 - \rho d_2)$. In other words, the further one is, the more it costs her to send capital to the metropolis. Therefore, she is willing to accept a lower return on capital if it is invested in her own region (e.g., because she can monitor the borrower more easily).[47] We assume that there are no other constraints on mobility (e.g., no within-region credit constraints).

B.2. Analysis of the Basic Model

Analysis of this model is straightforward. Profit maximization with respect to the inputs yields the generic conditions:

$$w = p(1-d)A(1-\alpha)(\frac{K}{L})^{\alpha}(\overline{K})^{\beta} \text{ and} \tag{3}$$

$$r(1-\rho d) = p(1-d)A\alpha(\frac{L}{K})^{1-\alpha}(\overline{K})^{\beta},$$

where w is the wage rate in that type of region, L is the labor endowment, K is the equilibrium amount of capital invested in a firm in that region and d is the corresponding distance variable ($d = 0$ for the metropolis, $d = d_1$ for the connected regions and $d = d_2$ for the distant regions). In addition, there is the capital market clearing condition:

$$MK_D + NK_C + K_M = K, \tag{4}$$

where K_D is the average amount of capital used in the distant region (per firm), K_C is the same thing in a connected region and K_M is that in the metropolis. K is the total supply of capital in the economy.

Manipulating the capital demand condition and using the fact $\overline{K} = K$ and $L = L'$ outside the metropolis yields

$$K^{1-\alpha-\beta} = \frac{p(1-d)}{r(1-\rho d)}A\alpha(L')^{1-\alpha}, \tag{5}$$

which tells us that whether the distant regions or the connected ones have more capital per firm depends on whether the ratio $\frac{(1-d)}{(1-\rho d)}$ is increasing or decreasing in d. If $\rho > 1$, which is the case where capital is less mobile than goods, then the distant region will actually have more capital per worker. Using the wage-rental ratio as the measure of inequality, as is conventional in trade models, we see that

$$\frac{w}{r(1-\rho d)} = \frac{(1-\alpha)(\frac{K}{L'})}{A\alpha}. \tag{6}$$

It follows directly that inequality is higher wherever K is lower. In other words, if capital is less mobile than goods, then the more distant region would have less inequality because it is able to retain more of its capital. A similar result would hold if we replaced capital by human capital and used the skill premium to measure inequality.

Finally, we compare outputs per worker/capita,

$$y = p(1-d)A(\frac{1}{L'})^{\alpha}(K)^{\alpha+\beta}, \tag{7}$$

which can be written as

$$y = p(1-d)A(\frac{1}{L'})^{\alpha}(\frac{p(1-d)}{r(1-\rho d)}A\alpha(L')^{1-\alpha})^{\frac{\alpha+\beta}{1-\alpha-\beta}}. \tag{8}$$

[45] We could easily let A vary across the locations to capture differences in the flow of ideas.

[46] See Duranton and Puga (2004) for a review of this literature.

[47] The equivalent assumption for human capital would be that there is a cost to relocating from one's home region to the city, but the cost is lower if she is better connected (e.g., because it is easier to travel to and from).

In the case where $\rho < 1$, this expression is clearly decreasing in d since both the $p(1-d)$ term and the $\frac{p(1-d)}{r(1-\rho d)}$ term decline with d. But when $\rho > 1$, we might actually observe the reverse, especially when spillovers are large ($1-\alpha-\beta$ is close to zero) and therefore $\frac{\alpha+\beta}{1-\alpha-\beta}$ is large. Once again this is because the better connected region loses more of its capital.

Result 1: In the basic model, output per capita will always be higher and inequality lower in the better connected region as long as capital is more mobile than goods. However, when capital is less mobile than goods, the more distant area will have less inequality. The difference in per capita output between the regions will tend to be small and per capita output may even be higher in the more distant region.

What is the effect of trade opening in this economy? If we model it as an increase in p, the price of the exportable, it increases incomes everywhere at the same rate. The rate of growth will not depend on the location.

Result 2: In the basic model, the effect of trade opening would be to raise income levels everywhere in proportion and hence have no differential growth effect.

B.3. A Simple Extension

The growth result here is somewhat of an artifact of the way the model is set up. Suppose we add an alternative production technology that uses only labor and produces a perfect substitute for the importable good (e.g. agriculture) using the technology $x = BL$, where L is the labor input. The good is consumed at the location and does not need transporting. The point is that now the wage in the exporting sector, w, needs to be bigger than B for there to be production of exportable goods. In this model, there can be three types of equilibria: type A, where both close and distant locations export; type B, where one of the locations exports and the other does not; and type C, where neither exports. As long as $\rho < 1$, we know that wages, which are proportional to output per capita, will be lower in the more distant location, and therefore, if we are in case B, the distant location will not export. It follows that as long as $\rho < 1$, the effect of trade opening will either be the same in both areas (types A and C), or the more connected area will grow faster.

On the other hand, when $\rho > 1$, it is not clear which of the two locations will have lower wages, and the gap between wages is likely to be small. Therefore we are more likely to be in either type A or C equilibria. Given the high average growth rate of approximately 8% in both close and far regions (see Table 2 column 3), scenario A seems more likely at least for the present. This is consistent with the fact that China now has excellent infrastructure and both near and far places are relatively easily accessed.[48] In any case, in both type A and type C equilibria, the effect of trade opening on growth rates is the same both in close and distant places, unless the effect of the trade shock is just big enough to move one area from not exporting to exporting but not the other.

Result 3: In the model with "agriculture", trade opening is likely to benefit the closer area more in terms of growth rates as long as capital is more mobile than goods. But in the reverse case, growth rates in both close and distant areas should react relatively similarly to trade opening. Moreover, as long as China is in a type A equilibrium, Result 1 should continue to hold in this case.

To summarize, a pattern where inequality is higher in more connected areas, but output level differences are small and growth rate differences are absent, is consistent with a setting where capital is less mobile than goods. The lack of a differential growth effect in this scenario is consistent *with an overall beneficial effect of transportation infrastructure*, which is what allows both close and more distant areas to be exporting.

B.3.1. Summary. The point of the simple model is to underscore the fact that infrastructure, in theory, can lead to more or less divergence between close and far areas, and that this depends on the relative mobility of goods and factors. There are, of course, other reasons that affect the extent of divergence. On the one side, for example, there is a natural force of convergence: the government might reasonably plan to construct infrastructure where it was previously absent, so that the connectivity in less-connected regions may improve faster than in the better-connected areas, reducing the difference in trade costs. This is also why we do not focus on the change in infrastructure as an intervening variable, since it is potentially correlated with the level of infrastructure. On the other side, there may be agglomeration effects that lead the places that were initially somewhat well connected to get even better connected. For example, that may be where a new university, export processing zone or airport gets built.

C. Pre-1840 Data

1850 population data were originally printed in county- and prefecture-level Gazetteers of the Qing Dynasty, and later aggregated and translated to modern Chinese by Ge (2000). We manually coded, digitized and geo-referenced the data from Ge (2000), which include 630 counties. Due to changes in historical county boundaries, we ultimately geo-referenced 588 of these counties.[49]

Chong, Pi, Fan, Nan indicators are provided in a modern Chinese translation of the *Provisional History of the Qing Dynasty* by Zhao (1976). For this revision, we read through the text discussion of each county, extracted and manually coded the measures, digitized and geo-coded them at the county level. We have this measure for over 1199 counties. Because of changes in county boundaries over time, the sample for analysis is 1117 counties.[50]

[48] In the model, it is possible for even a very poorly connected area to export because we place no lower bound on the interest rate. But if transportation is really expensive, the interest rate will have to be very negative in the distant areas to permit exporting. It seems likely that capital owners will then prefer to hold cash or gold and therefore, there will not be any exports.

[49] They are mapped in Appendix Fig. A2.

[50] They are mapped in Appendix Figs. A3d - Fig. A.3a.

Buddhist temple locations in 1820 are digitized and geo-coded by Harvard CHGIS.[51]

Table A.1
The Effect of Distance to the Line on Production – Dynamic Panel Estimation

	Dependent Variables Growth											
	All Sectors			Primary			Secondary			Tertiary		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	1 Lag	2 Lag	3 Lag	1 Lag	2 Lag	3 Lag	1 Lag	2 Lag	3.Lag	1.Lag	2.Lag	3.Lag
A. Per Capital GDP Growth												
Ln Distance	0.00386	0.00394		−0.00073	−0.00066	−0.00065	0.00571	0.00582	0.00584	0.00267	0.00266	0.00267
to Hist Line:	(0.00331)	(0.00334)		(0.00397)	(0.00401)	(0.00401)	(0.00507)	(0.00513)	(0.00514)	(0.00333)	(0.00334)	(0.00334)
Observations	1373	1373		1281	1281	1281	1281	1281	1281	1175	1175	1175
Number of Counties	258	258		258	258	258	258	258	258	245	245	245

	B. Rural Household Income Growth								
	10th			50th			90th		
Ln Distance	0.00169	0.00173	0.00173	0.00016	0.00017	0.00017	0.00285	0.00281	0.00281
to Hist Lines	(0.00328)	(0.00333)	(0.00333)	(0.00181)	(0.00183)	(0.00183)	(0.00288)	(0.00294)	(0.00294)
Observations	1655	1655	1655	1655	1655	1655	1655	1655	1655
Number of Counties	122	122	122	122	122	122	122	122	122

Note: All regressions control for the full set of baseline controls in Table 5 column (6). In panel A, the lagged dependent variable is instrumented using 1, 2 or 3 lags of the per capita GDP level for the relevant sector. In panel B, the lagged dependent variable is instrumented with 1, 2, or 3 lags of the relevant income level. The standard errors are clustered at the county level. The sample is an unbalanced panel of counties. The data in panel A are from Provincial Statistical Yearbooks.The data in panel B are from the National Fixed Point Survey. All geographic data are calculated by the authors. Note that Panel A, Column (3) could not be computed due to a highly singular covariance matrix.

[51] They are mapped in Appendix Fig. A4.

Table A.2
Robustness to the Exclusion of Border Regions

	Dependent Variables							
	Ln(Per Capita GDP)				Annual Growth in Ln (Per Capita GDP)			
	All	Primary	Secondary	Tertiary	All	Primary	Secondary	Tertiary
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Baseline								
Ln Dist Historical Line	0.0681	−0.0353	−0.0944	−0.0773	−0.00229	−0.00025	−0.00787	−0.00104
	(0.0272)	(0.0216)	(0.0458)	(0.0324)	(0.00339)	(0.00523)	(0.00521)	(0.00357)
Observations R-squared	2744	2266	2266	2199	1110	1018	1018	899
	0.852	0.870	0.722	0.842	0.204	0.225	0.182	0.181
B. Omit Counties in Border Provinces								
Ln Dist Historical Line	−0.0793	−0.0270	−0.1190	−0.0862	−0.00151	0.00322	−0.00570	−0.00205
	(0.0280)	(0.0216)	(0.0471)	(0.0355)	(0.00364)	(0.00520)	(0.00546)	(0.00323)
Observations R-squared	2434	1956	1956	1889	977	885	885	766
	0.852	0.868	0.711	0.830	0.213	0.272	0.223	0.246
	Ln Number of Firms				Average Firm Profits			
	All	Public	Mixed	Individual	All	Public	Mixed	Individual
C. Full Sample								
Ln Distance to Historical Lines	−0.091	−0.062	−0.089	−0.122	−0.105	−0.100	−0.072	−0.028
	(0.022)	(0.025)	(0.033)	(0.032)	(0.040)	(0.048)	(0.067)	(0.043)
Observations R-squared	3321	3321	3321	3321	2763	2416	1642	1503
	0.639	0.680	0.700	0.785	0.449	0.319	0.203	0.342
D. Omit Counties in Border Provinces								
Ln Distance to Historical Lines	−0.086	−0.060	−0.080	−0.088	−0.110	−0.119	−0.078	−0.022
	(0.023)	(0.027)	(0.036)	(0.033)	(0.040)	(0.050)	(0.069)	(0.044)
Observations R-squared	2655	2655	2655	2655	2248	1982	1408	1306
	0.604	0.685	0.710	0.826	0.501	0.359	0.220	0.398
	Ln(HH Income) by Percentile				Annual Growth Rate			
	Gini	10th	50th	90th	Gini	10th	50th	90th
E. Full Sample								
Ln Distance to Historical Lines	−0.0071	−0.0285	−0.0120	−0.0158	−0.00195	−0.00018	−0.00182	−0.00257
	(0.0031)	(0.0255)	(0.0236)	(0.0276)	(0.00058)	(0.00330)	(0.00193)	(0.00270)
Observations R-squared	1897	1897	1897	1897	1533	1533	1533	1533
	0.272	0.454	0.590	0.582	0.036	0.124	0.171	0.095
F. Omit Counties in Border Provinces								
Ln Distance to Historical Lines	−0.0062	−0.0205	0.0001	−0.0031	−0.00214	0.00138	−0.00077	−0.00215
	(0.0031)	(0.0250)	(0.0229)	(0.0268)	(0.00056)	(0.00335)	(0.00198)	(0.00280)
Observations R-squared	1625	1625	1625	1625	1317	1317	1317	1317
	0.268	0.473	0.607	0.599	0.041	0.128	0.180	0.099

Notes: All regressions control for the full set of baseline controls in Table 5 column (6) in the manuscript. All standard errors are clustered at the county level. In Panels A–D, the samples are unbalanced county-year level panel. The data for the dependent variables are from the Provincial Statistical Yearbooks in Panels A and B, and from the Censuses of Manufacturing Firms in Panels C and D. In Panels E and F, the regression in column (5) also controls for the 1 and 2 year lag of the gini, and the regressions in columns (6)–(8) control for the 1 and 2 year lags of the relevant income levels. The sample is an unbalanced panel of villages. Income data are from the National Fixed Point Survey. Panels B, D and F exclude border provinces: Gansu, Yunnan, Heilongjiang, Liaoning, Jilin, Inner Mongolia, Qinghai, Xinjiang, Tibet and Guangxi.

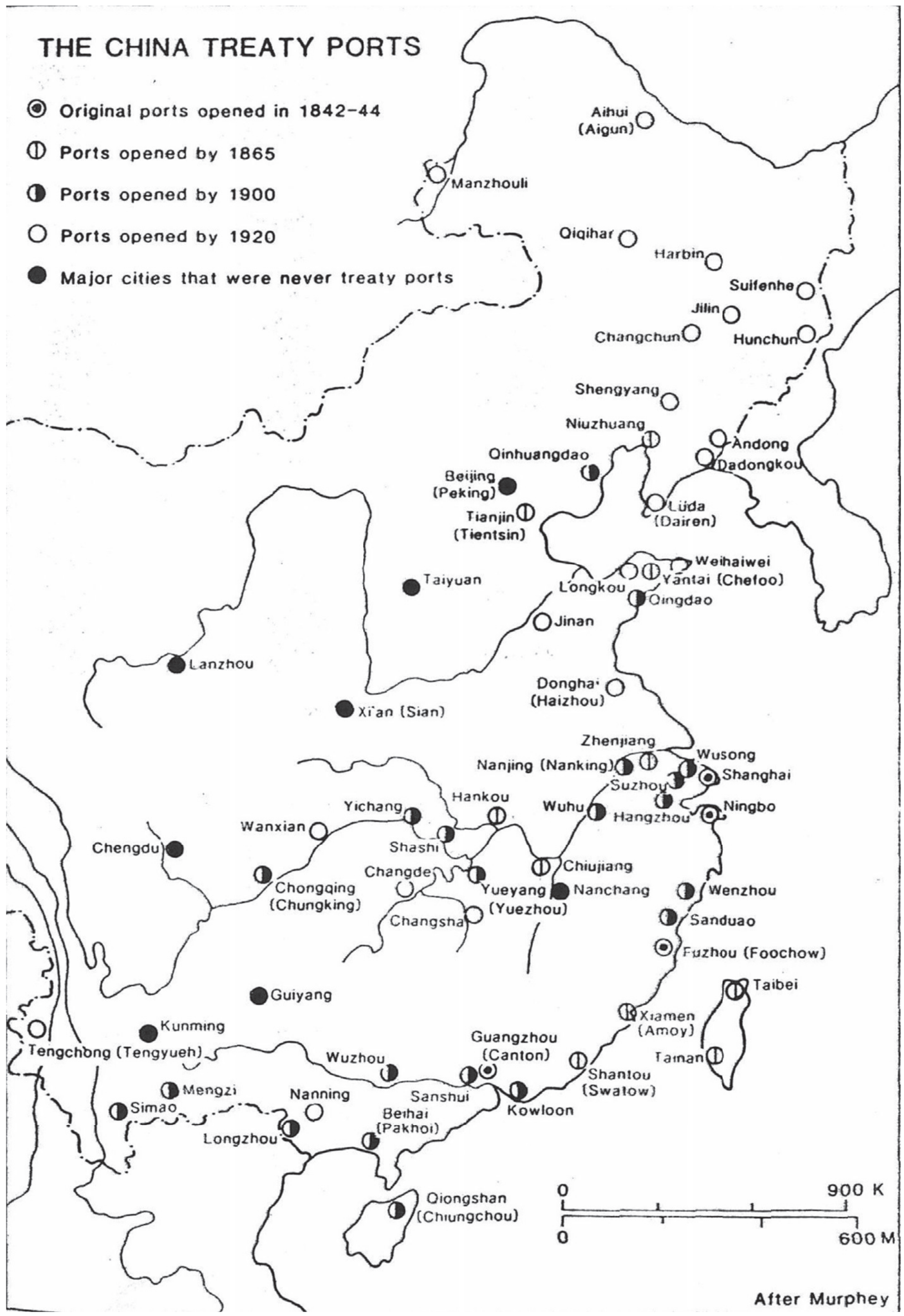


Fig. A.1 Historically Important Cities and Treaty Ports. Source: Murphey (1970) page 35.

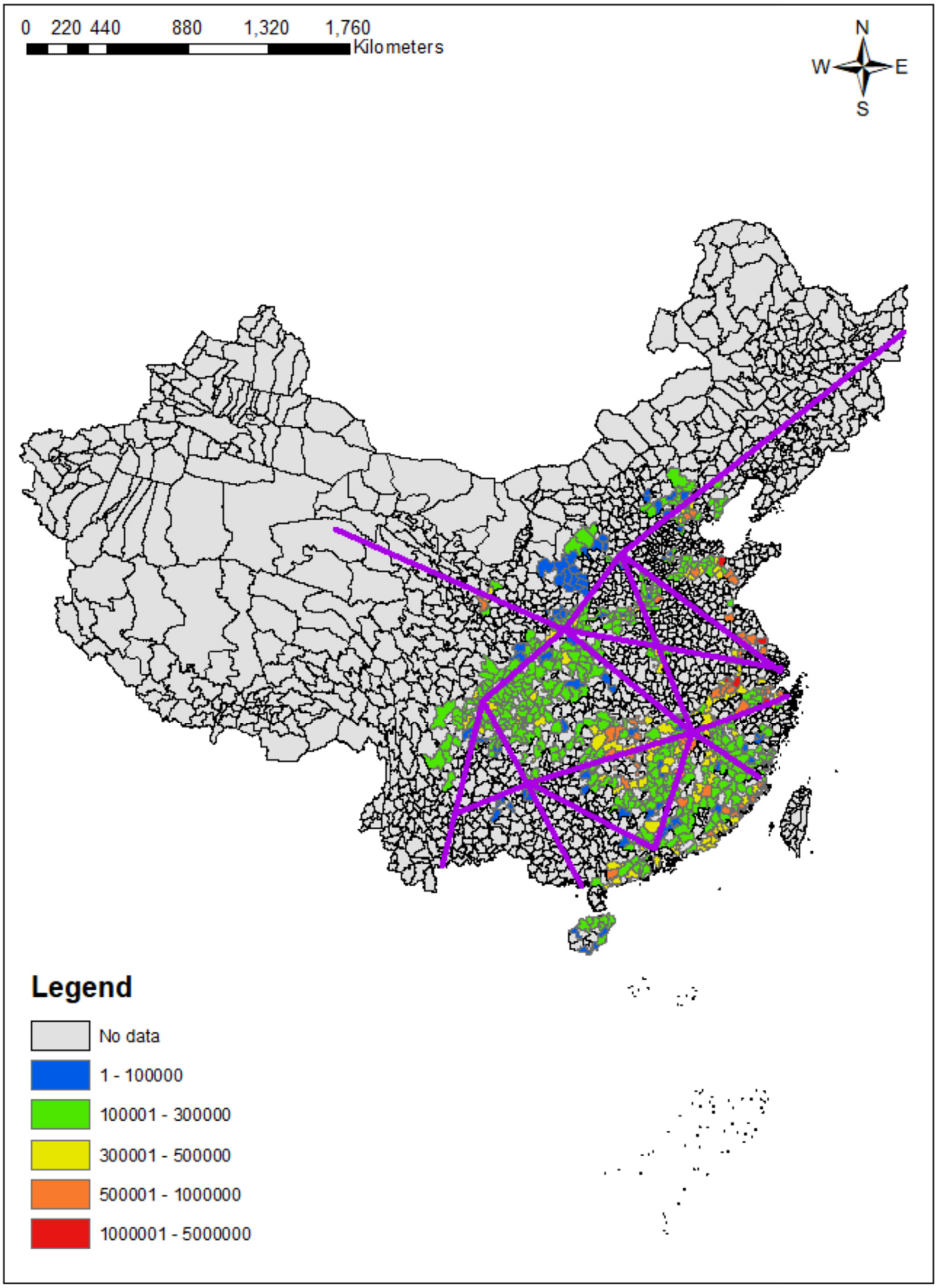
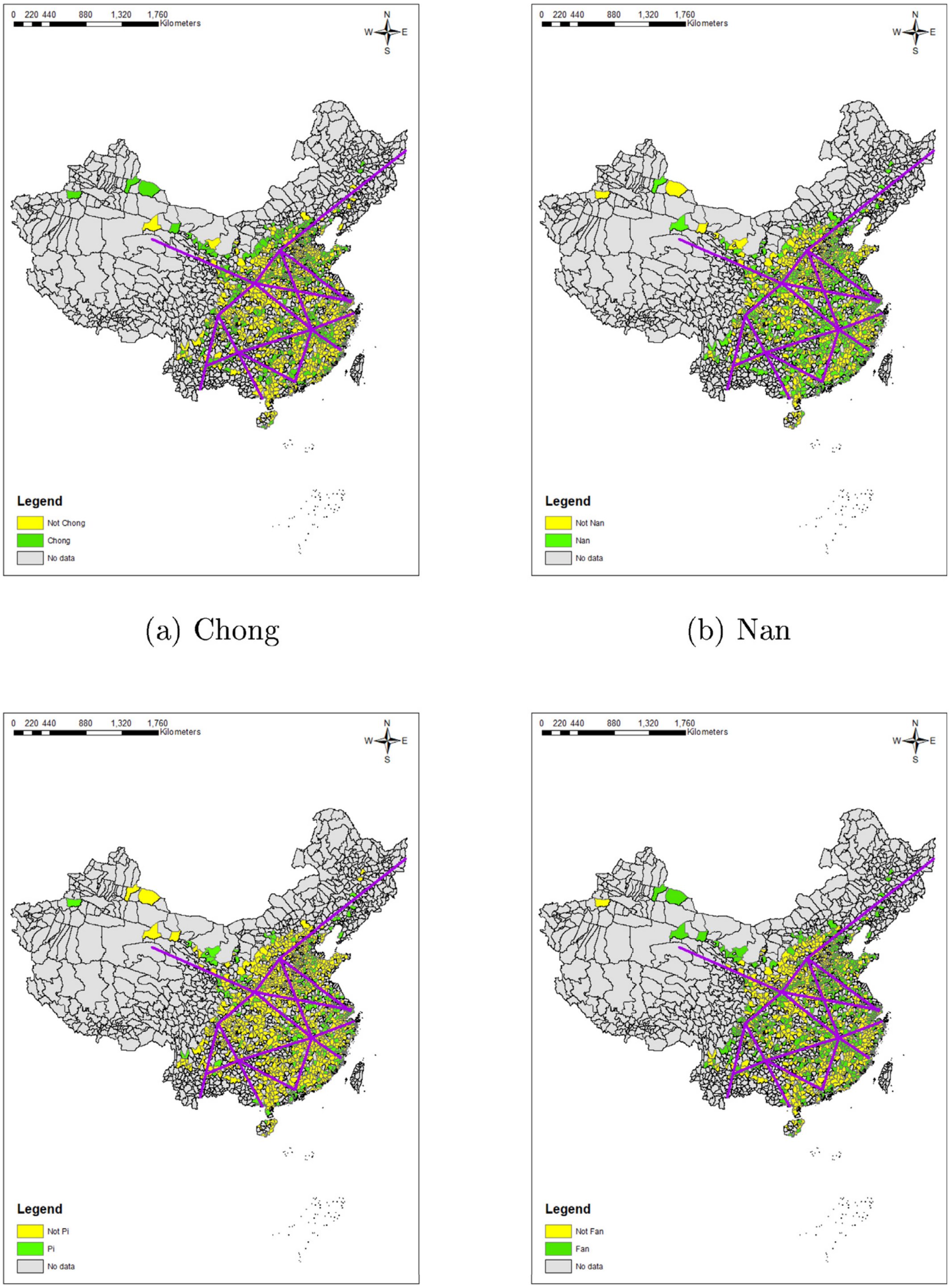


Fig. A.2 1850 Population.



(a) Chong (b) Nan

(c) Pi (d) Fan

Fig. A.3 18th Century Qing Dynasty Political Economic Indicators

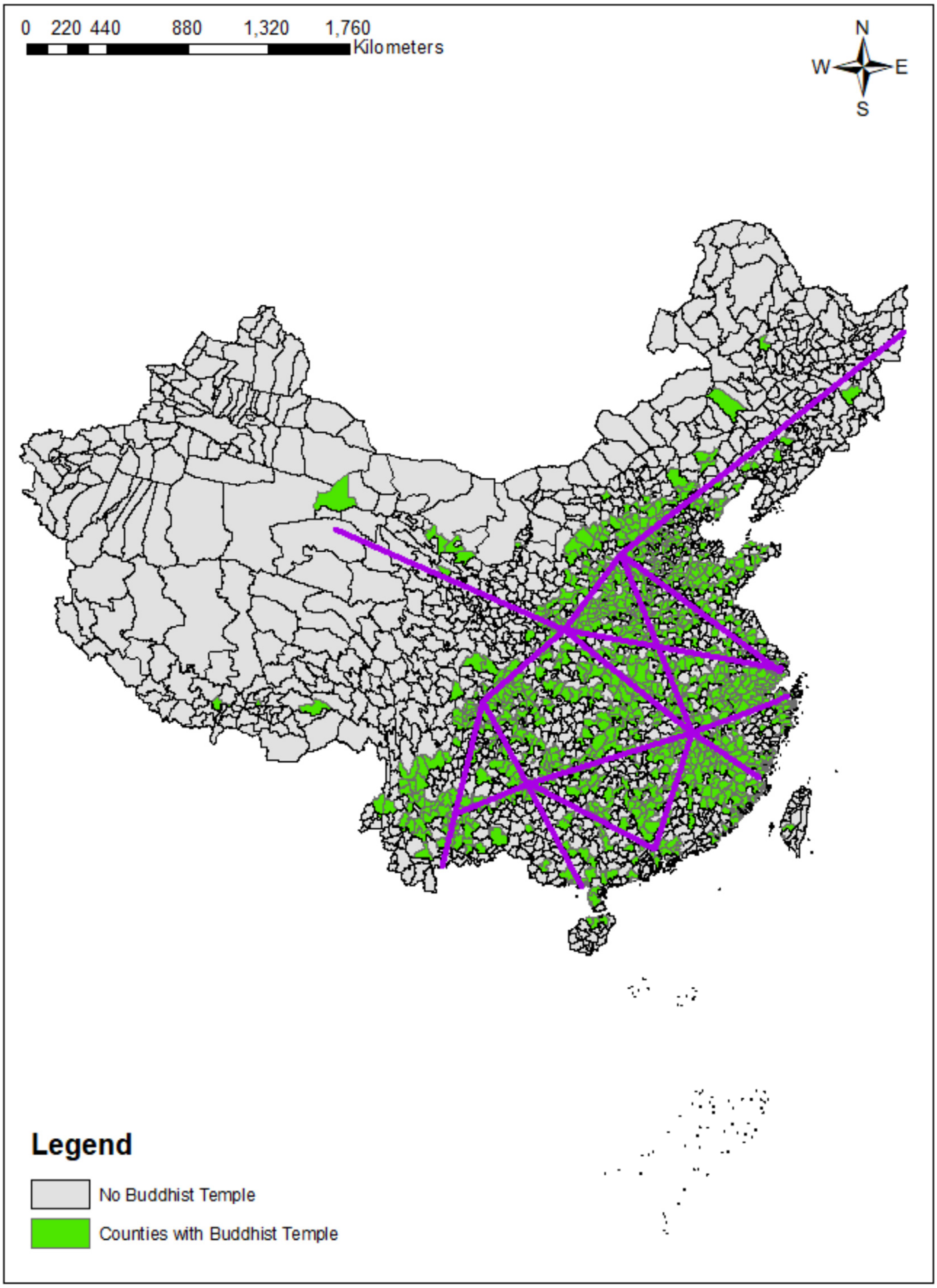


Fig. A.4 Buddhist Temples in 1820

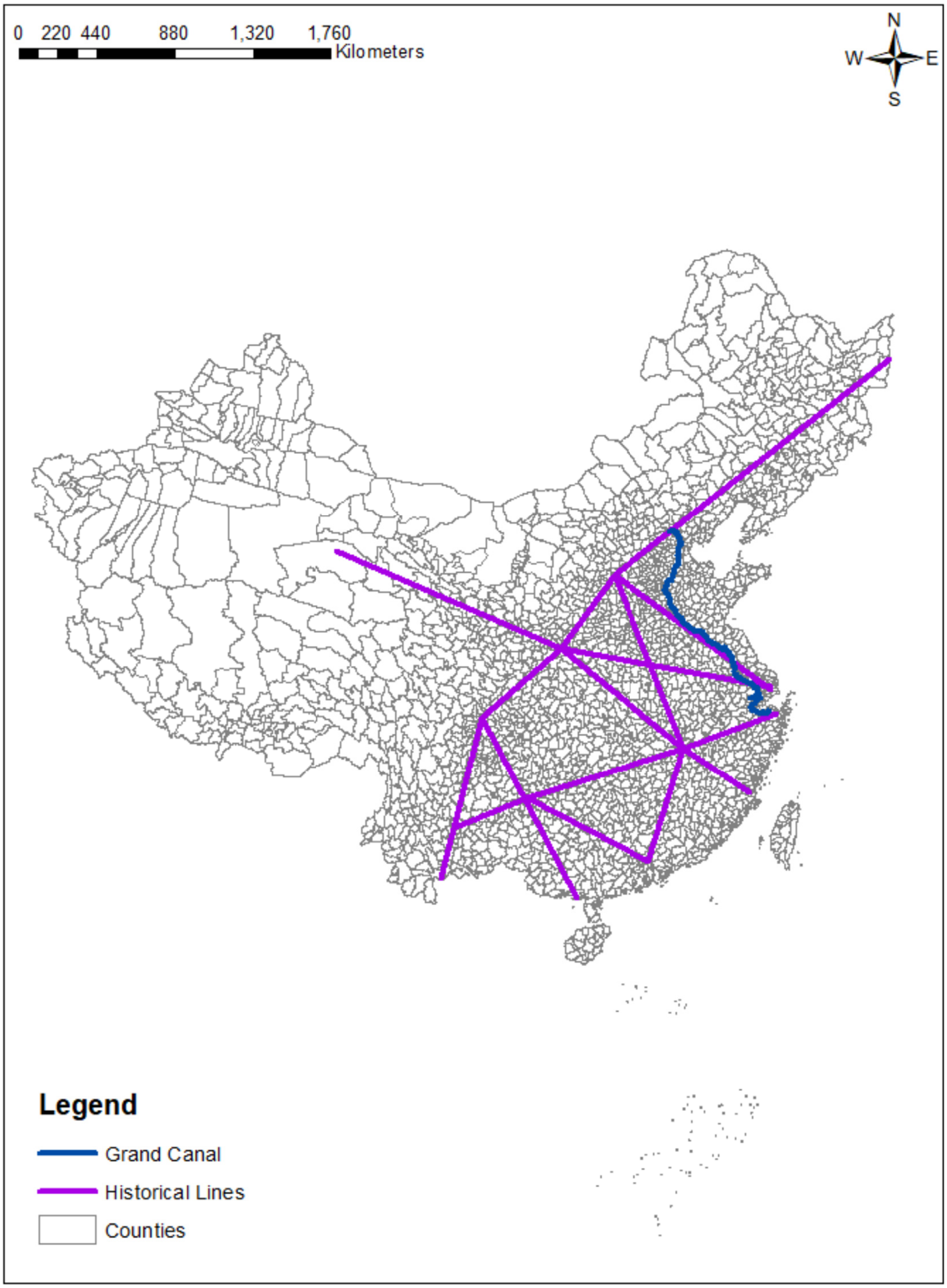
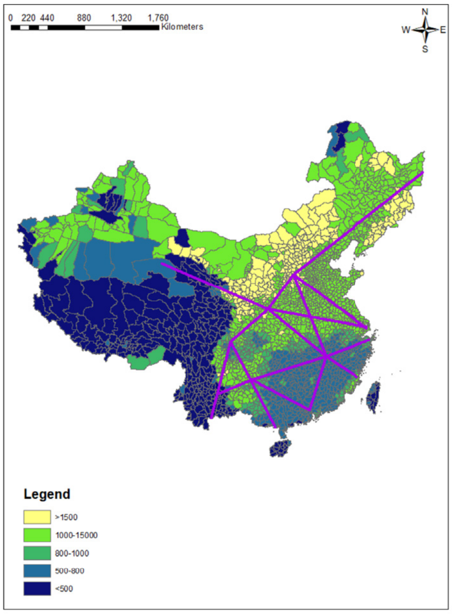
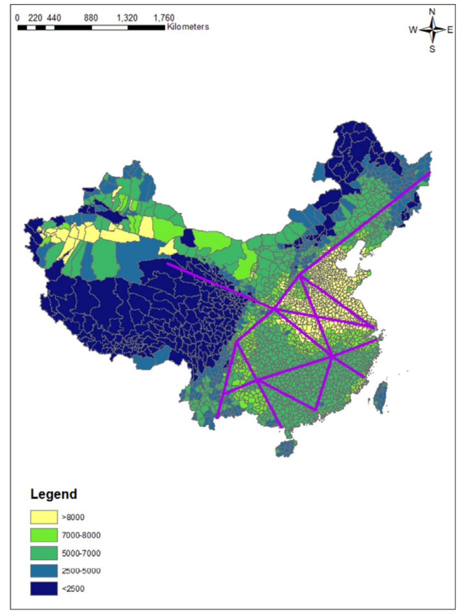


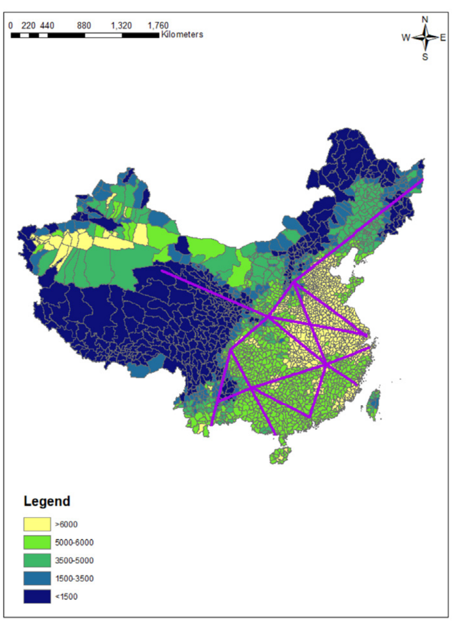
Fig. A.5 The Grand Canal



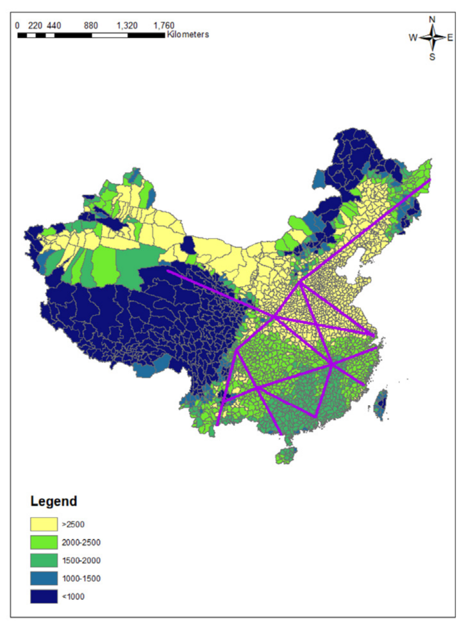
(a) Buckwheat



(b) Maize

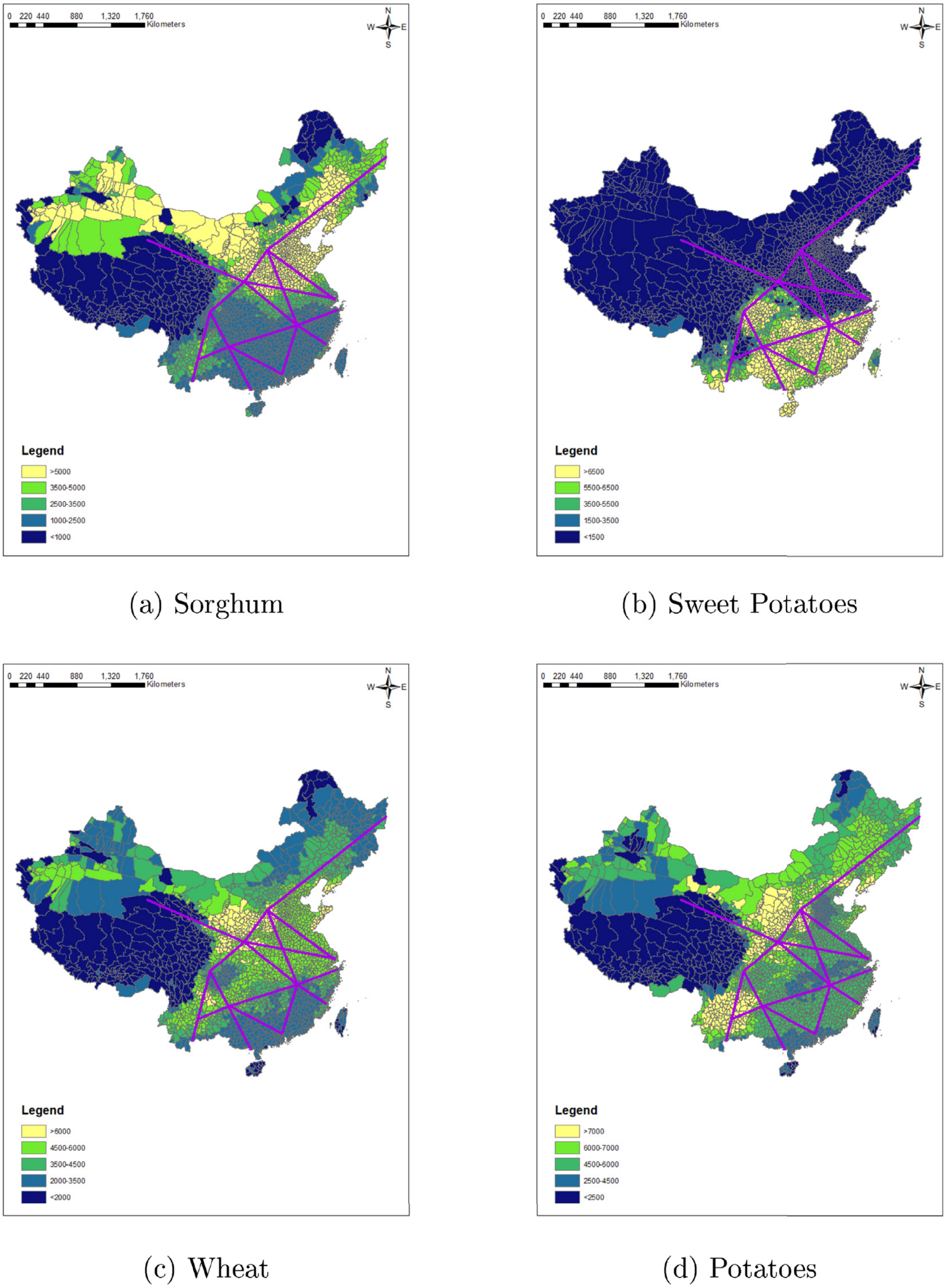


(c) Wet Rice



(d) Soybeans

Fig. A.6 Natural Conditions: Suitability for Cultivating Staple Crops and Weather



(a) Sorghum (b) Sweet Potatoes

(c) Wheat (d) Potatoes

Fig. A.7 Suitability for Cultivating Staple Crops (cont.)

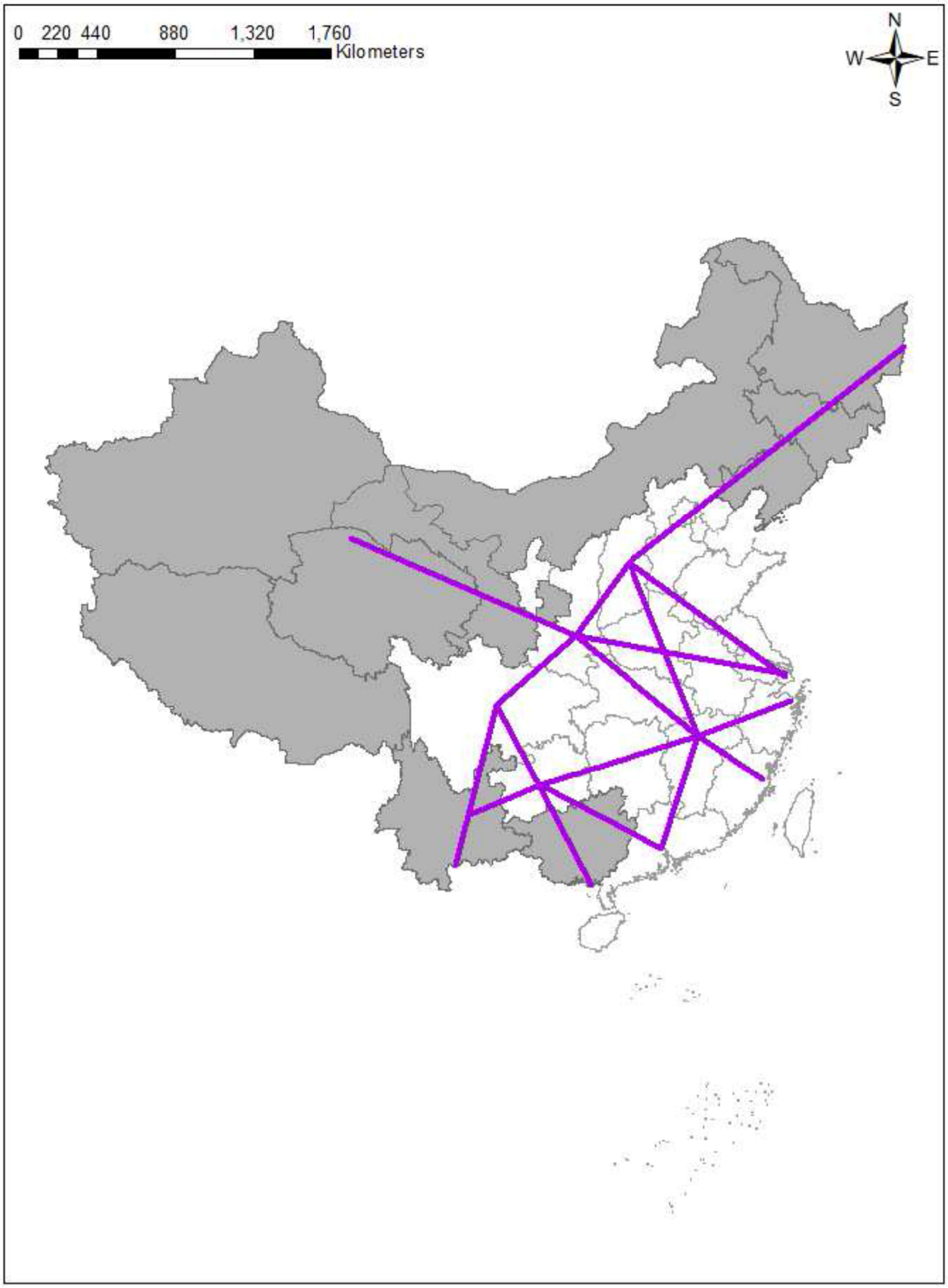


Fig. A.8 Omitted Border Provinces

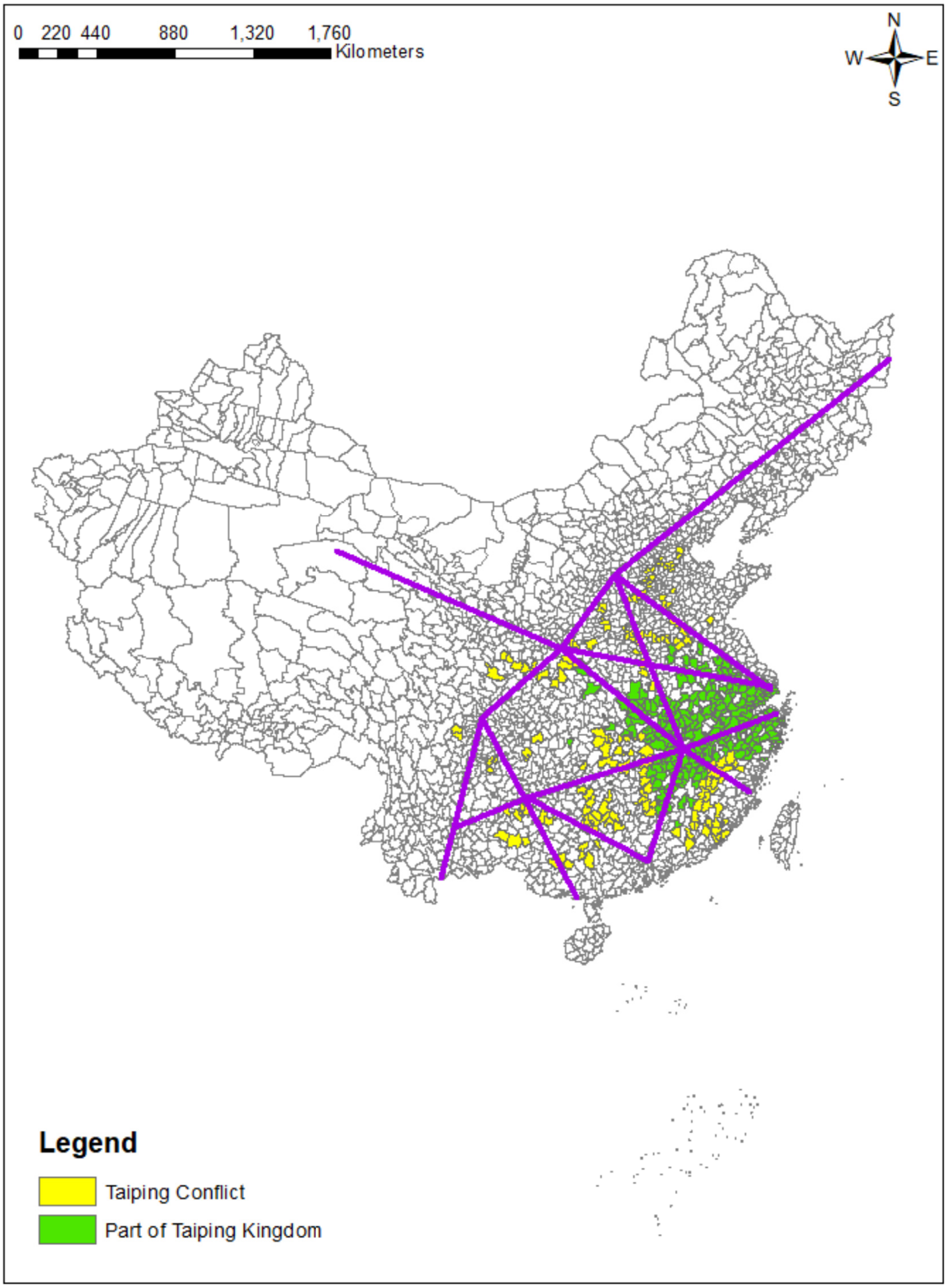


Fig. A.9 Taiping Rebellion

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