'No county left behind?' The distributional impact of high-speed rail upgrades in China

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Abstract

Infrastructure investment may reshape economic activities. In this article, I examine the distributional impacts of high-speed rail upgrades in China, which have improved passengers' access to high-speed train services in the city nodes but have left the peripheral counties along the upgraded railway lines bypassed by the services. By exploiting the quasi-experimental variation in whether counties were affected by this project, my analysis suggests that the affected counties on the upgraded railway lines experienced reductions in GDP and GDP per capita following the upgrade, which was largely driven by the concurrent drop in fixed asset investments. This article provides the first empirical evidence on how transportation costs of people affect urban peripheral patterns.

1. Introduction

Infrastructure investments are regarded as key instruments to promote overall economic growth. However, such investments are not evenly distributed across different regions of a country, possibly due to differences in expected returns, budget constraints, planning concerns and so on. Therefore, the regions or sectors receiving more investments may benefit more than less-affected regions or sectors. The distributional consequences will be even more pronounced if investments biased toward one sector or region divert economic activities away from the less-affected sectors or regions.

In this article, I explore this possibility by investigating the distributional impacts of one such infrastructure investment: high-speed rail upgrades in China. This is a useful case to study for several reasons. First, investment in high-speed rail is prevalent in both developed and developing countries. Currently, more than 20 countries in the world have high-speed rails in operation or under construction.[1] China is a relevant case to study the impact of high-speed rail, as it has the largest scale of high-speed railways in the world. In addition, like all such investments, high-speed rail upgrades in China are

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[1]   International Union of Railways (UIC), 2014.

known to favor urban areas. To maintain a high speed, bullet trains stop only in populous urban areas where there are higher demands for time savings, in contrast to small cities and rural areas. Thus, counties with upgraded railway lines may find bullet trains bypassing them (Economist, 2011).[2] That is to say, even though high-speed trains help facilitate economic activities across cities due to significantly less travel time, they may actually hurt smaller counties along the accelerated railway lines by depriving them of access.[3] Finally, high-speed rail upgrades only affect the passenger rail service instead of the freight rail service, which allows us to separate the transport cost of people from the transport cost of goods. This is an advantage that is not present in other forms of transportation infrastructure, such as highways.

A nice feature of the high-speed rail upgrade in China is that the non-targeted counties have been affected by the upgrade process in a quasi-random manner to a large extent, which facilitates a credible empirical analysis on the causal impact of high-speed rail upgrades on affected counties. The two rounds of high-speed rail upgrades, which have been a part of China's railway speed acceleration project since 1997, were implemented in 2004 and 2007. There are two reasons why the upgrade has been quasi-random for the affected counties. First, all of the upgrades were implemented on existing railway lines, which mitigates concerns regarding the problem of selection for high-speed rail placement. Second, as the selection for high-speed rail upgrades mainly depends on the large cities to which the existing railways are connected, the counties in between cities affected by the speed acceleration can be regarded as quasi-random because they were not selected on purpose (Chandra and Thompson, 2000; Michaels, 2008; Datta, 2012). This identification strategy is also known as the 'inconsequential place approach' (Redding and Turner, 2014) in the sense that the unobservable attributes in the affected counties do not affect the placement of high-speed rail upgrades. These institutional arrangements allow me to exploit the quasi-experimental variation in whether counties were affected to examine the distributional impact of the upgrade. Specifically, I examine the impact of high-speed rail upgrades by comparing the economic outcomes of the counties located on the affected railway lines with the counties located on non-affected railway lines, before and after, using county-level statistics collected from statistical yearbooks and other published statistical reports. I apply a difference-in-differences setting to compare the high-speed rail affected and non-affected counties, before and after. The common trend assumption required by a difference-in-differences analysis is satisfactory, as suggested from an event study. I also conduct robustness checks to strengthen my estimation.

My analysis conveys several main findings. First, the estimations reveal that being located on the high-speed railway lines decreases a county's total GDP and GDP per capita by 3–5% on average, which is approximately 252–420 million *yuan* (39–65 million US Dollars), given the average county level GDP as 8.39 billion *yuan*

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[2]   Indeed, as suggested by Figure 1, approximately 3000 out of 6100 passenger train stops in China have been abandoned in the past 10 years due to the speed acceleration of passenger train services, especially after 2004, when high-speed rail upgrading began.

[3]   In the urban planning literature, this is known as the 'tunnel effect,' defined as 'an improvement in access to major cities but at the expense of breaking up the space between them. The increase in dynamism in large nodes is compensated by a decrease in the activity of areas between the connection points' (Albalate and Bel, 2012). The latest World Bank report on China's high-speed rail development also documents the fact that some conventional train services were removed after the introduction of high-speed rail (Bullock et al., 2012).

(approximately 1.29 billion US dollars) in 2006 in the affected regions. Second, the reduction of GDP is likely to be investment driven, as evidenced by the 9–11% reduction of fixed asset investment in the affected counties. Intuitively, when high-speed trains more conveniently connected cities, investment was concentrated in cities rather than in counties in pursuit of higher returns due to expected growth. Finally, I discuss the channels that may account for the investment-driven economic slowdown in the affected counties. Specifically, I test two possible channels: (1) that increases in people's commuting costs due to reduced train services in the affected counties may lead to decreases in economic activities; and (2) that reduction of transport costs of people between large cities may divert economic activities from counties to populous urban districts. I find that the second channel plays a more important role in explaining the negative impact of high-speed rail upgrades.

To my knowledge, this is the first article documenting the distributional consequences of high-speed rail projects to the non-targeted rural areas, which complements the rich body of literature examining the causal relationship between access to infrastructure and various aspects of economic development in both developing and developed countries (Baum-Snow, 2007; Duflo and Pande, 2007; Atack et al., 2010; Ahlfeldt, 2011; Banerjee et al., 2012; Baum-Snow et al., 2012; Datta, 2012; Garcia-López, 2012; Donaldson, 2013; Zheng and Kahn, 2013; Faber, 2014; Jedwab and Moradi, 2014; Ghani et al., 2015; Percoco, 2016; Donaldson and Hornbeck, 2016). Specifically, this article contributes to the transportation cost literature by separating the impact of the transportation cost of people from the transportation cost of goods due to the nature of high-speed train services. The evidence in this article suggests that periphery rural areas may experience an investment-driven reduction in GDP when the transportation cost of people decreases in the urban core. This is different than the impact of highway construction, which is more pronounced in the manufacturing sector due to its function of freight transportation (e.g., Faber, 2014). In addition, this article also provides useful insights in understanding the increasing rural–urban disparity in China in the past few decades, where urban-biased infrastructure investment may have played a role (Kanbur and Zhang, 2005; Xu, 2011).

The article is organized as follows: Section 2 describes the policy background of high-speed rail upgrades in China. Section 3 describes the identification strategy and data sources. Section 4 shows the main findings and robustness checks. Section 5 discusses the heterogeneous impacts of the railway upgrades in different sectors, possible channels through which the impacts may work and the magnitude of such impacts. Finally, Section 6 concludes.

2. Background of China's high-speed rail upgrades

2.1. Railway speed acceleration and high-speed rail upgrades

Mainly in response to the profit loss under the competition of road and air transportation, China's Railways Ministry started several rounds of speed acceleration on existing railway lines spanning from 1997 to 2007.[4] The project had two stages. In the first stage, train speed was increased gradually in the first four waves, namely, in

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[4]   Please refer to Appendix A for more background information about the railway network in China.
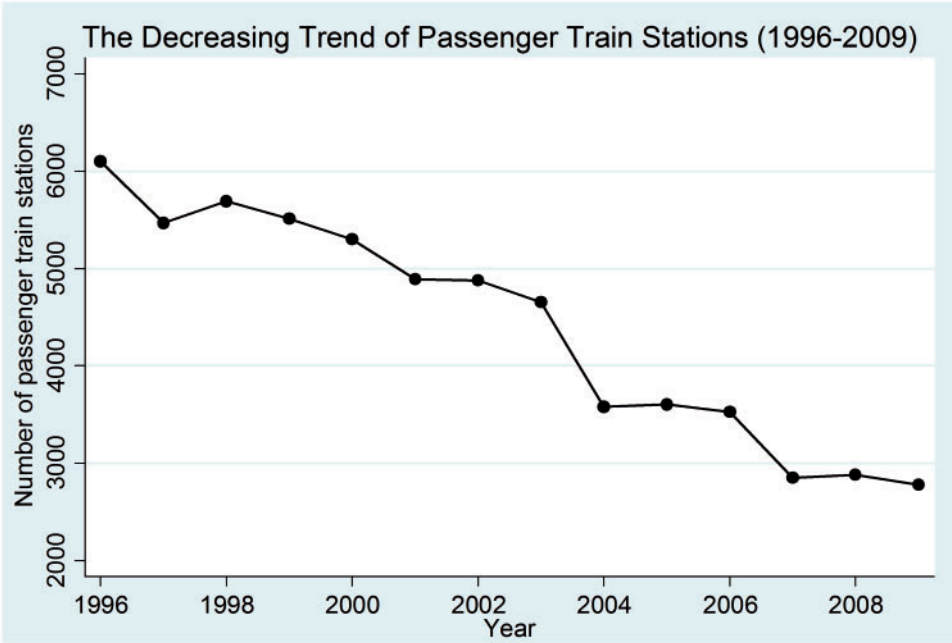
Figure 1. The decreasing trend of passenger train stations (1996–2009).
Source: Author's calculation based on passenger train schedule, 1996–2009.

1997, 1998, 2000 and 2001. In 1997, the first round of speed acceleration was initiated on three main railway lines connecting from Beijing to Shanghai, Guangzhou and Harbin. The average passenger train speed was increased from approximately 48.1 km per hour to 54.9 km per hour. Subsequently, in 1998, 2000 and 2001, another three waves of speed acceleration were implemented on the main railway lines, increasing the average train speed nationwide to 61.6 km per hour by the end of 2001.

In the second stage, speed acceleration was targeted toward upgrading the existing railway into high-speed rail, with sustained speeds greater than 200 km per hour or higher. In 2004, approximately 1960 km of railroad had been upgraded to high-speed rail, with 19 pairs of city-to-city nonstop passenger trains operating on it. In 2007, the upgraded high-speed rail was expanded to approximately 6000 km with 257 pairs of China Railway High-speed (CRH) trains operating on a daily basis, which significantly shortened the commuting time between large cities. For example, travel time from Beijing to Fuzhou, the provincial capital of Fujian in the south of China, was reduced from approximately 33 h to 19.5 h with the introduction of CRH trains in 2007. Travel time by train was reduced by more than half from Shanghai to Nanchang and Changsha, which are the two provincial capitals in southeast China. According to the Vice Minister of the Chinese Railways Ministry, travel time between cities by CRH trains was reduced by an average of 20–30%.[5]

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5   See http://news.sina.com.cn/c/2007-04-12/151112762996.shtml for more information.
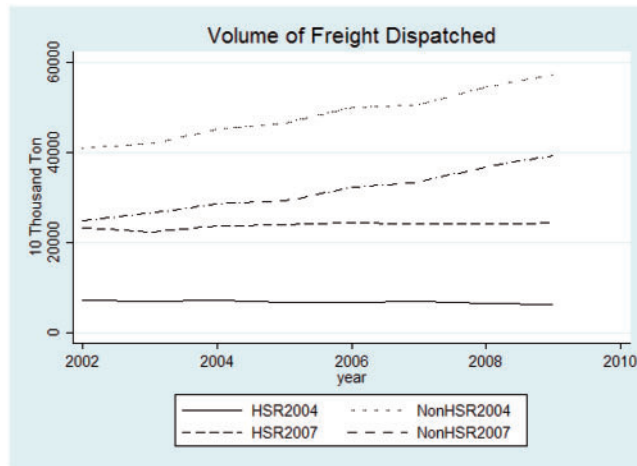
Figure 2. Volume of freight dispatched by upgrade status.
Source: China railroad yearbooks, 2003–2010.

Despite the fact that both passenger and freight services share the same railway lines, railway upgrades do not force out the freight trains along the affected railroads. As shown in Figure 2, the dispatched freight volume remains almost unchanged for upgraded railway lines in 2004 and 2007, while freight volume increases at a relatively constant growth fate for unaffected railway lines. There is a difference in terms of the freight volume growth rate between upgraded and non-affected railway lines that is possibly due to the fact that those non-affected railway lines specialize more in freight services compared with upgraded lines. However, the railway upgrade is unlikely to affect the freight service patterns, as there is no disruption in trends over the years during which the upgrades occurred.

The dramatic expansion of high-speed rail in the year 2007 reflects the "Great Leap Forward" strategy proposed by the ex-Minister of the Chinese Railways Ministry, Zhijun Liu, who was removed from office due to corruption allegations in early 2011. During Liu's tenure, China invested a large amount of money into railway expansion, upgrades and the construction of high-speed railway lines. As most of the high-speed railway lines were updated from existing railways, some slow train services on the upgraded lines were canceled to accommodate CRH trains. As a consequence, the number of operating slow trains significantly decreased with the increase of high-speed rail mileage. For example, in 2002—before the high-speed rail upgrades—352 pairs of daily slow passenger trains operated nationwide. The number dropped to 224 in 2007.[6]

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[6] There is no significant difference in terms of capacity between high-speed rail passenger trains and normal passenger trains. A typical passenger train contains 16–20 coaches, with a capacity of 110 passengers in each coach.

2.2. Program placement

In this article, I focus on the high-speed rail upgrade in 2004 and 2007.[7] As upgrading existing railway lines for speed acceleration is costly, not all of the railway lines were selected for upgrade. In 2004, the three main railway lines connecting Beijing to Harbin, Shanghai and Guangzhou were partially upgraded to high-speed rail, with approximately 20 pairs of nonstop bullet trains operating on them (Figure 3). Later in 2007, upgrades were completed on the abovementioned three railway lines and on two additional main lines (Lianyungang to Urumqi and Beijing to Hong Kong), as well as four other regional lines (Hangzhou to Zhuzhou, Guangzhou to Shenzhen, Wuhan to Jiujiang and Qingdao to Jinan, Figure 4).

3. Data and identification

3.1. Identification strategy

The goal of this article is to study the distributional impact of high-speed rail upgrades in China. Specifically, urban-biased high-speed rail upgrades may hurt the economic growth of non-targeted counties/regions when the connection is consequently +improved between urban areas. To test the above hypothesis, the difference-in-differences strategy is applied to compare the counties located on the affected railway lines to the counties located on other railway lines, before and after each round of high-speed rail upgrades.

The definition of treated and control units is explained in Figure 5. Suppose that there are two types of railway lines: railway lines upgraded into high-speed rail either in 2004 or 2007 and railway lines unaffected by the upgrades. We define counties located on the upgraded lines (i.e., with at least one train station on the upgraded lines) as treated units and counties located *only* on unaffected railway lines (i.e., with at least one train station on the unaffected railway lines and no train station on the upgraded lines) as the control group. It is notable that all of the urban districts in prefecture-level cities have been excluded from the sample because they are likely to be selected on purpose in the high-speed rail upgrade projects.

A problem posed by difference-in-differences analysis is the non-random placement of the treatment group. That is, in our context, the placement of high-speed rail upgrades is not randomly selected. However, the quasi-experimental nature of high-speed rail upgrades at the county level renders the non-random placement problem much less of a concern for two reasons. First, all of the upgrades were implemented on existing railway lines, which mitigates some of the concerns in the problem of selection for high-speed rail placement.[8] Second, as the selection of affected railway lines mainly

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[7] As mentioned in Section 2.1, there were four rounds of speed acceleration in 1997–2001 before the high-speed upgrade. However, we will not focus on that because the scale of the project is small compared with the 2004 and 2007 high-speed upgrades. None of the railway lines in China had been upgraded to high-speed rail before 2004. However, to ensure a cleaner identification, I exclude the observations from 1997 to 2001 in the control group when estimating the impact of high-speed rail upgrades in 2004 and 2007.

[8] In addition to high-speed rail upgrades, China also constructed new high-speed rails, such as those from Beijing to Shanghai and Wuhan to Guangzhou. In observance of the fact that the earliest new high-speed rails started to operate in December 2009, I exclude the county statistics after 2009 in the estimation to avoid the possible intertwined impact of new high-speed rails and high-speed rail upgrades due to network effect. In addition, the counties being affected by newly constructed high-speed rails are also excluded from the estimation.
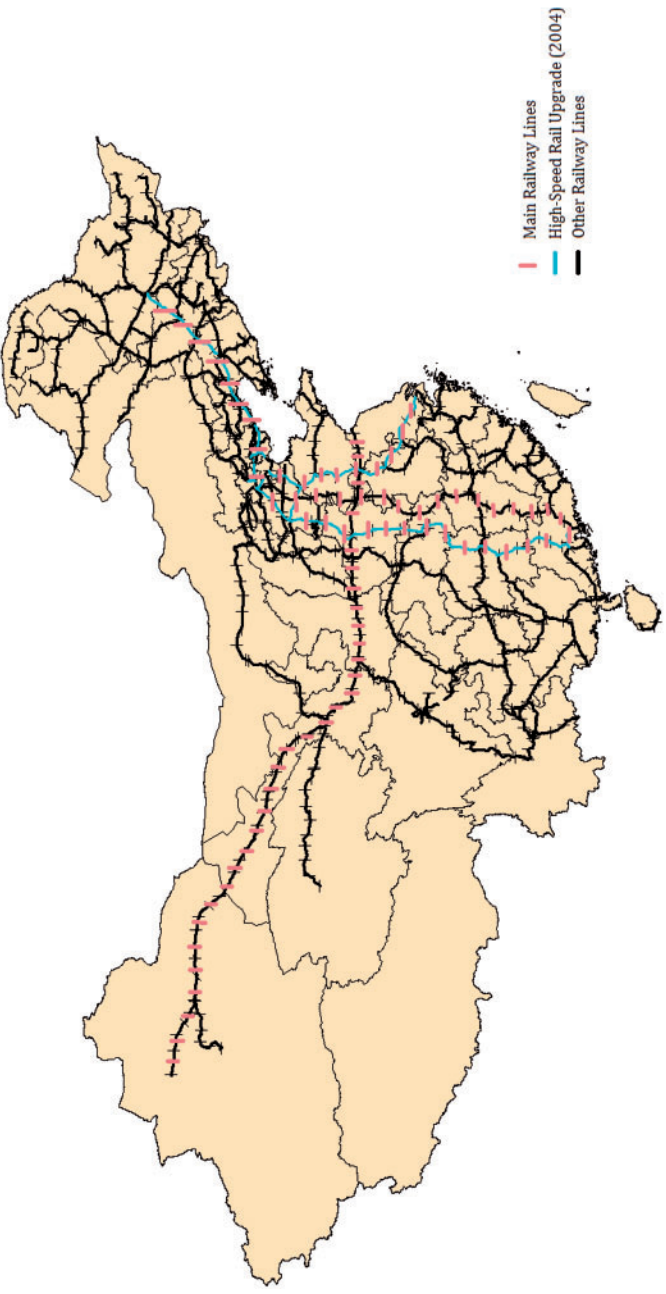
Figure 3. High-speed rail upgrade in 2004.
GIS source: China data center (University of Michigan).

Figure 4. High-speed rail upgrade in 2007.
GIS source: China data center (University of Michigan).

depends on the cities it connects rather than the counties it bypasses, it can be treated as a quasi-natural experiment for the counties located on railway lines. This argument is similar to that of Chandra and Thompson (2000), Michaels (2008) and Datta (2012), all of whom argue that if a highway is built to connect two cities, it must pass through areas that lie between the two, which affects the outcomes in such areas as a quasi-random shock.

Therefore, the estimation equation of a standard difference-in-differences can be expressed as:

$$\text{Outcome}_{i,t} = \beta_0 + \beta_1 HSR_i * \text{After}_t + \alpha \text{Covariates}_{i,yr2000} + \gamma \text{Year}_t * \text{Province}_i$$
$$+ \delta \text{County}_i + \epsilon_{i,t}, \tag{1}$$

where $Outcome_{i,t}$ is the economic outcome of county i in time t. In this article, I am most interested in two categories of outcome variables: (a) the yearly county-level GDP and GDP per capita, which represents the overall performance of a county and (b) the yearly county-level investment measure, that is, fixed asset investment, which is important because investment is a driving force of GDP growth in China (Yu, 1998; Qin et al., 2006).[9] $HSR_i * After_t$ is the difference-in-differences term, where the dummy variable HSR_i denotes whether county i was affected by high-speed rail upgrades (in 2004 and 2007) or not, and $After_t$ denotes whether it is before or after high-speed rail

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[9] Fixed asset investment includes investment in capital construction, renovation and renewals of existing facilities, real-estate development, other fixed assets by state-owned units and other fixed assets by collective-owned units, as well as private investment in housing construction as defined by the National Bureau of Statistics of China.
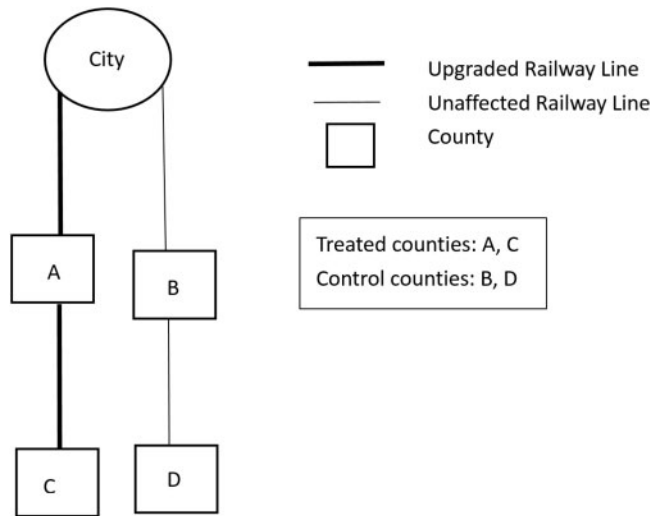
Figure 5. Definition of treated units and control units.

upgrades for each time t.[10] In some specifications, I control for several covariates in county i in 2000, including the share of GDP in the service sector, the total population (in log form), the share of the educated (above compulsory) population and GDP per capita and include interactions of these variables with linear and quadratic time trends.[11] $Year_t * Province_i$ controls for year with province time trends.[12] $County_i$ controls for the county fixed effect. $\epsilon_{i,t}$ is the error term. The robust standard errors are clustered at the county level.

The key assumption in difference-in-differences analysis is a common trend. In this case, the assumption would be violated if counties in the control and treatment groups had different growth patterns prior to the high-speed rail upgrades. To test the common trend assumption, I use an event study analysis to show that the control and treatment groups have similar pre-trends in terms of GDP, per capita GDP and fixed asset investment before the upgrade process. More details about the event study are discussed in Section 4.3.

In addition to the difference-in-differences strategy, several robustness checks have been implemented to make sure that our estimated impact is solely driven by the railway upgrade instead of other unobservables or correlated factors. First, there may be concern regarding the role of regional favoritism in selecting the upgraded railway lines. As Hodler and Raschky (2014) observe from 126 countries, subnational regions have

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[10]   The regression analysis treats 2004 and 2007 as the years in which the two waves of high-speed rail upgrades were installed. The construction of the railway upgrades occurred progressively over the years. However, the operation of the high-speed trains began exactly in 2004 (first round) and 2007 (second round) without any overflow into previous periods. Therefore, we do not have to worry about potential treatments prior to 2004 or 2007.

[11]   The total population and the share of the educated (above compulsory) population were collected from the 2000 Population Census.

[12]   I can also use the year-fixed effect instead of the year by province fixed effect here if the assumption is relaxed so that there is no heterogeneity in terms of growth trends across different provinces. The main findings do not change if the year-fixed effect is used.

more intense night-time light when they are the birth region of the current political leader, indicating the existence of regional favoritism. Burgess et al. (2013) find strong evidence of ethnic favoritism in road construction: districts in Kenya that share the ethnicity of the president have expenditures twice as high for roads and four times the length of paved roads than those districts with a different ethnicity. Therefore, if regional favoritism affects the selection of upgraded counties, it may also affect the same group of counties by other means, such as other infrastructure investments, foreign direct investments and so on. Therefore, the estimated impact of high-speed rail upgrades is likely to be driven by regional favoritism instead of the upgrade itself. However, regional favoritism is likely to have a very limited impact in the program selection of high-speed rail upgrades due there being only one existing railway line connecting large cities in China in most of the cases.[13] Therefore, the government had very limited power to exert regional favoritism in the railway upgrade. We will formally provide more statistics about this issue in Section 4.4.

Second, we estimate Equation (1) using two restricted samples. First, because the positive economic impact of a relatively new railroad may contaminate our estimation of high-speed rail upgrades on existing railway lines, I exclude from the sample the counties with no railroad access until 1996. Another concern related to the research design is that counties too close to high-speed rail cities may also be affected by the railway upgrades even though they are off the treated railway lines, that is, in the control group. To ensure that the control group is not contaminated by the railway upgrades, I drop all counties that are close to the newly established high-speed rail stations as a robustness check.

Third, as mentioned in Bertrand et al. (2004), the standard error of the 'treatment variable' in difference-in-differences analysis may be underestimated due to the serial correlation among the observations of the same object over years. Bertrand et al. (2004) suggest aggregating the data into 'pre' and 'post' periods to minimize the number of periods for each object, which helps to mitigate the serial correlation problem in difference-in-differences analysis. Following this method, I aggregate the data from 2005 to 2009 into 'pre' period (by taking the average of year 2005 and 2006) and 'post' period (by taking the average of year 2007, 2008 and 2009). Similarly, I aggregate the data from 2002 to 2009 into three periods: 'pre' period I (by taking the average of year 2002 and 2003), 'pre' period II (by taking the average of year 2004, 2005 and 2006) and 'post' period (by taking the average of year 2007, 2008 and 2009).

It is notable that our sample is restricted to counties with a railroad at the beginning of our sampling period (2002). As county train stations also vary by size, we further exclude 98 out of 957 counties that own 'large train stations' due to their historical importance in the railway system, as they might have also been considered important connections in the high-speed rail route.[14] However, the estimation results change little even if we include the counties with 'large train stations.'[15]

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[13]   For example, *jinghu xian*, *jingha xian* and *jingguang xian* were the only existing routes directly connecting Beijing to Shanghai, Beijing to Harbin and Beijing to Guangzhou during the period of the upgrade.

[14]   Passenger train stations have been categorized into six levels according to their size and capacity, namely VIP stations and level one to level five stations. We denote 'large train stations' as train stations above level 3 according to the standard in the 1990s.

[15]   I have also dropped two counties (Suizhong and Quanjiao) from the sample because these two counties have had high-speed train stations since 2007. I thank one anonymous referee for this note.

3.2. Railroad data

To estimate the impact of high-speed rail upgrades in 2004 and 2007 on the affected counties, I compare the economic performance of counties located on the upgraded railway lines to the counties located on conventional railway lines before and after the high-speed rail upgrades from 2002 to 2009. Therefore, the railway status information of all of the counties in China as of 2008 is collected from the *People's Republic of China Railroad Atlas* published in 2008. A dataset including the list of counties with access to railroads in 2008 is constructed based on the above information, along with the name of the railway line(s) on which each county is located. In addition, I identify the railroads constructed during each year from 1996 to 2007 along with their bypass counties from the annually published China Railroad Yearbook.

In addition to county railroad status, the frequency of daily passenger train services in each county from 1996 to 2009 is also collected for descriptive purposes. The information is manually compiled from the published passenger train schedules for each year. Each train stop is matched to its county using the *China Train Station Encyclopedia* published in 2003. The train stops that are unlisted in the book are matched using online resources. It turns out that only a very small proportion of train stops (approximately 100 out of 6000 stops) cannot be matched to counties because these stops are very small in size in most cases. Because those small stations are generally serviced by very few trains, this fact affects my descriptive statistics only slightly.

Figure 1 shows the number of operating passenger railway stations from 1996 to 2009. Approximately 3000 passenger train stations were closed during the 10 years of speed acceleration, especially during the high-speed rail upgrades (beginning in 2004). More surprisingly, the number of counties with functioning passenger train stops also decreased during this time period, even with the expansion of new railway lines, as suggested in Figure 6. Hence, the number of counties that have lost train service recently has exceeded the number of counties with new access to railroads. In contrast, the accessibility of railroads in cities has slightly increased.

Figure 7 shows the average daily train stops in each city and county from 1996 to 2009. It is clear that train service is much more frequent in cities than in counties. The average number of daily train stops is approximately 70–90 for cities from 1996 to 2009, compared with merely 20–30 stops for counties. Furthermore, after 2004, the average number of daily train stops indicates a decreasing trend for counties but an increasing trend for cities, in accordance with the fact that the train stations in small counties were skipped after the introduction of high-speed rail.

Figures 8 and 9 provide the distribution of average daily train stops in counties in 1996 and 2007, respectively. Two stylized facts are revealed in these maps. First, the accessibility to trains is distributed unequally across counties in both years. The county with the least accessibility to railroads had only one daily train service in 1996, while the county with the most accessibility had 345 daily train services. However, in 2007, the county with the most accessibility to the railroad service had 165 train stops, a 50% reduction from 1996. Second, the accessibility to trains decreased during the speed acceleration. The median of daily train services is 18 trains per day in 1996 and 14 trains per day in 2007. The two color-coded maps illustrate the decline in average accessibility to trains that accompanied the speed acceleration occurring between 1996 and 2007.
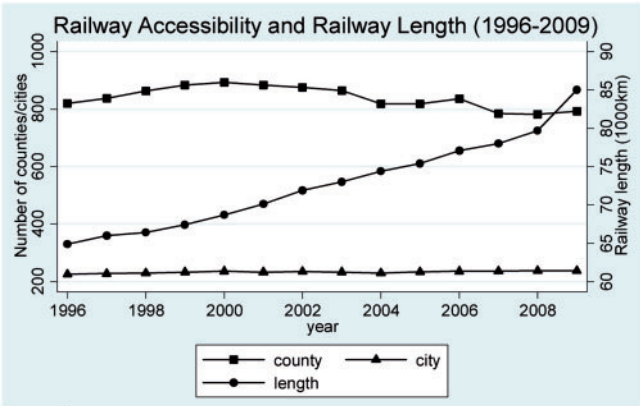
Figure 6. Railway accessibility and railway length (1996–2009).
Sources: 1. Data on railway length is from National Statistical Yearbook; 2. Data on railway accessibility is by author's calculation based on passenger train schedule, 1996–2009.
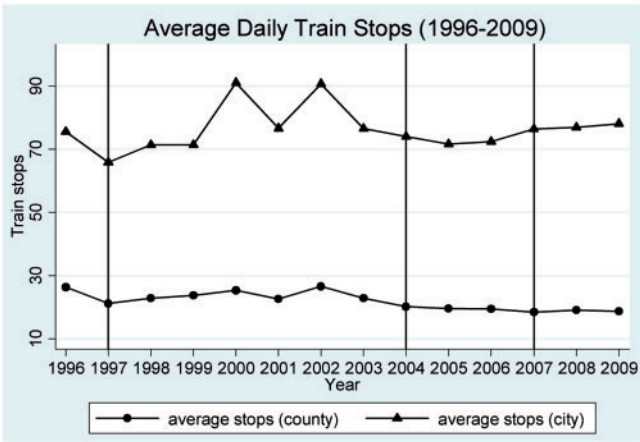

Figure 7. Average daily train stops by city and county (1996–2009).
Source: Author's calculation based on passenger train schedule, 1996–2009.

3.3. County statistics data

The county statistics dataset is collected from the China Economic and Social Development Statistical Database provided by the China National Knowledge Infrastructure, which is compiled from all of the publicly available statistical yearbooks and other published statistical reports.[16] All of the counties and county-level cities in China have been included in the analysis except (1) counties administered by the four municipalities, namely Beijing, Shanghai, Tianjin and Chongqing, as they are directly governed by the municipalities and are too close to the beginning of the main railway lines and (2) counties in Tibet, as none of them had access to railroads until 2007, which

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[16]   The database is available at http://tongji.cnki.net/kns55/Dig/dig.aspx with institutional access.
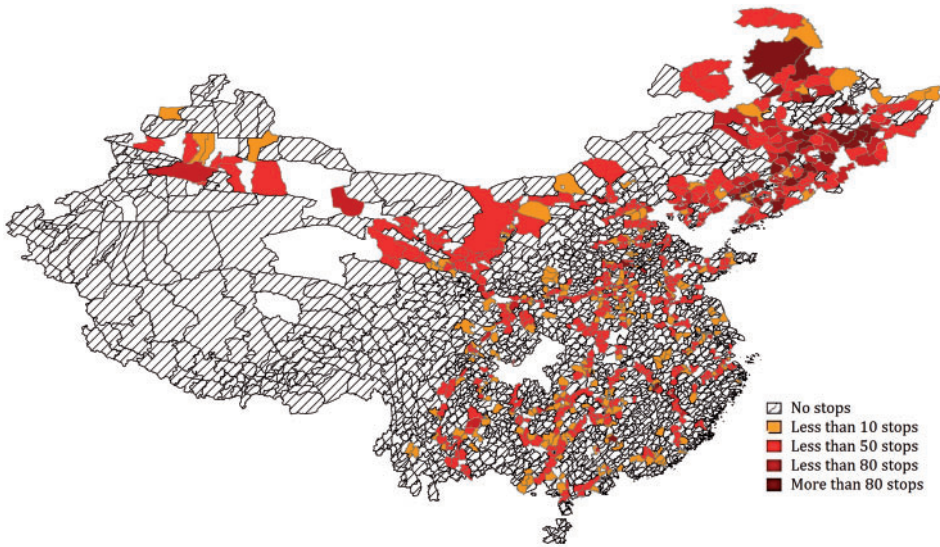
Figure 8. Daily average train stops in the counties, 1996.
GIS source: China data center (University of Michigan). Blank areas are urban districts of prefecture-level cities, which are not included in our analysis.
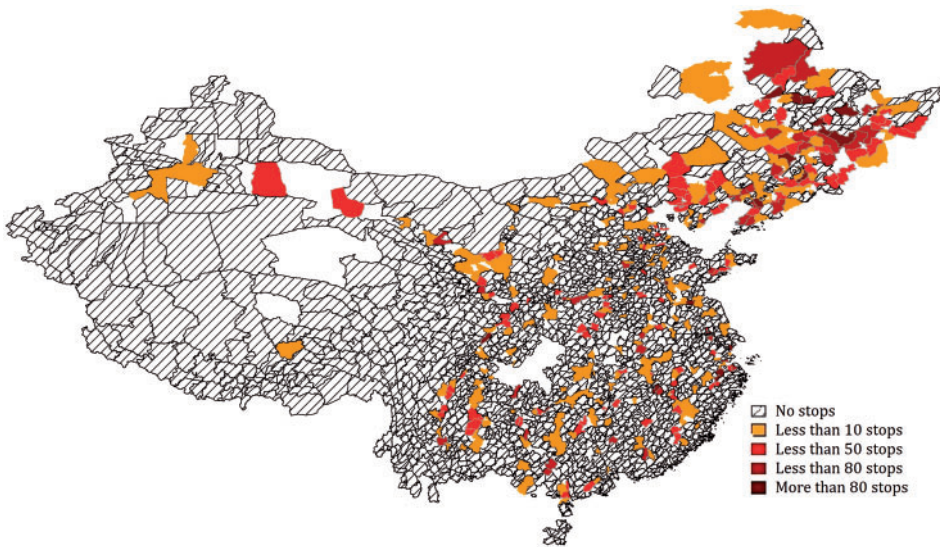

Figure 9. Daily average train stops in the counties, 2007.
GIS source: China data center (University of Michigan). Blank areas are urban districts of prefecture-level cities, which are not included in our analysis.

makes it unnecessary to include them in the sample based on my identification strategy. Therefore, a total of 1878 counties are included in the sample for descriptive purposes, with information on county GDP, GDP per capita and fixed asset investment. As mentioned earlier, only counties with train access before 2002 are included in my

estimation. Thus, there are 957 counties for the purposes of estimation. The time span of the county statistics is from 2002 to 2009.[17]

People may have some concerns regarding the quality of GDP data in China. However, as suggested by Au and Henderson (2006), the GDP and other economic indicators at the local level are indeed of high quality. Because our unit of analysis in this paper is at the county level, there should be little concern that the results are driven by the quality of the data.

4. Findings

4.1. Descriptive statistics

Table 1 shows the descriptive statistics for county level railway status and economic outcome indicators. As mentioned in the previous section, only counties with a railroad before 1996 are included. Thus, a total of 957 counties have been included in the sample, with 171 located on five main railway lines and 786 located on other railway lines. On average, in 2003, approximately 28 trains stopped in counties located on main railway lines on a daily basis, compared with approximately 22 trains stopping in counties located on other railway lines. However, in 2007, both numbers dropped, from 28 to 21 and from 22 to 18, respectively. This is evidence that the reduction in train service accessibility is more severe for counties located on main railway lines than for others. In terms of economic outcomes, counties located on main railway lines on average have a higher GDP, GDP per capita and fixed asset investment. The GDP doubled from 2003 to 2007 for both groups of counties. The fixed asset investment almost tripled for both groups.

4.2. Difference-in-differences estimation

Table 2 shows the difference-in-differences regressions for the impact of high-speed rail upgrades in 2004 and 2007. The results of the estimation are reported for two sub-samples: 2005–2009 (which is tested for the high-speed rail upgrade in 2007) and 2002–2009 (which is tested for the high-speed rail upgrades in both 2004 and 2007). Panel A presents the most parsimonious specification, whereas Panel B shows the results after controlling for pre-existing levels of population, GDP per capita, share of educated (above compulsory) population and share of the service sector in GDP, as well as their interactions with linear and quadratic year trends. The above specification allows for heterogeneous growth trends based on initial levels in 2000.

The results in both panels generally suggest that the high-speed rail upgrades, especially in 2007, hindered economic development in the affected counties. The consistent results in both panels also reassure that the selections based on observables, *and unobservables* have been properly controlled for in the difference-in-differences setting. Columns 1–4 of Table 2 suggest a significant GDP and GDP per capita reduction after the high-speed rail upgrade in 2007 in the counties located on the affected railway lines, which is approximately 3–5% in magnitude. The impact of high-speed rail upgrades on GDP per capita may work through its impact on population

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[17] In some cases, the GDP per capita is not reported in the database, but the GDP and total population data are reported. Thus, I impute the GDP per capita data by the formula GDP per capita $= \frac{\text{GDP}}{\text{population}}$.

Table 1. Descriptive statistics

	2003		2007		Source
	Main RL	Other RL	Main RL	Other RL	
A. Railway status					
Number of counties	171	786	171	786	Peoples Republic of China Railroad Atlas
Average daily train services (with stops)	27.67 (26.22)	21.74 (22.91)	20.79 (20.04)	17.62 (17.87)	China Passenger Train Schedule (annually)
B. Economic outcomes					
GDP (100 million yuan)	47.19 (44.29)	36.72 (37.59)	94.77 (107.65)	73.61 (74.94)	China Economic and Social Development Statistical Database
GDP per capita (1000 yuan)	9.58 (14.87)	7.24 (4.96)	16.06 (15.42)	15.01 (12.19)	China Economic and Social Development Statistical Database
Fixed asset investment (100 million yuan)	15.03 (17.16)	12.56 (15.82)	41.87 (40.59)	36.87 (37.30)	China Economic and Social Development Statistical Database

Notes: 1. Main RL stands for 'main railway lines'; other RL stands for 'other railway lines.' 2. Mean and standard deviation (in parentheses) is reported for each of the variables.

Table 2. The impact of high-speed rail upgrades on county economic outcomes

	Panel A: Without controlling for covariates in year 2000					
	Ln GDP		Ln GDP per capita		Ln Fixed asset investment	
	2005–2009	2002–2009	2005–2009	2002–2009	2005–2009	2002–2009
HSR04*After		−0.04 (0.03)		−0.09 (0.07)		−0.06 (0.05)
HSR07*After	−0.04*** (0.01)	−0.05*** (0.02)	−0.05*** (0.01)	−0.04*** (0.01)	−0.09** (0.04)	−0.10*** (0.04)
County-fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Province*Year-fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.99	0.98	0.92	0.91	0.89	0.90
Observations	4653	7437	4580	6378	3921	6273
	Panel B: Controlling for covariates in year 2000					
HSR04*After		−0.03 (0.03)		−0.08 (0.07)		−0.07 (0.05)
HSR07*After	−0.03** (0.01)	−0.04** (0.02)	−0.05*** (0.01)	−0.04*** (0.01)	−0.09** (0.04)	−0.11*** (0.04)
County-fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Province*Year-fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.99	0.98	0.92	0.91	0.89	0.90
Observations	4643	7421	4570	6363	3911	6258

Notes: 1. Robust standard errors clustered at county level are reported in parentheses. 2. * significant at the 0.1 level; ** significant at the 0.05 level and *** significant at the 0.01 level.

changes. However, as suggested in Table A2, population is basically not affected in the treated counties of the high-speed rail upgrades. There seems to be a 1% increase in overall population after the high-speed rail upgrade in 2007 in one of the two specifications. However, none of the other population measures (rural population, total number of households and total number of rural households) is significantly affected by high-speed rail upgrades.

However, the impact of the earlier upgrade in 2004 is not significant, with a negative magnitude in Columns 2 and 4. The insignificant coefficient can be explained by two facts. First, the mileage of the high-speed rail upgrade in 2004 is 1960 km, which is only one-third of the completed upgrade in 2007 (approximately 6000 km). Second, only 19 pairs of nonstop city transit trains were operating on the upgraded lines in 2004, compared with 257 CRH trains operating on the high-speed rails in 2007. Both facts illustrate that the intensity of the upgrade in 2004 is less than that of 2007. The GDP reduction is likely to be driven by a reduction of investment, as suggested in Columns 5 and 6 of Table 2. The decrease in fixed asset investment in the counties affected by high-speed rail affected in 2007 is approximately 9–11%, which is double compared with the reduction of GDP.

To summarize, the findings in Table 2 suggest that the high-speed rail upgrades negatively impacted the economic growth of the counties located on the affected railway lines. More specifically, the GDP and GDP per capita of such counties decreased by 3–5%, which is approximately 252–420 million *yuan* annually, given the average county level GDP is 8.39 billion *yuan* in 2006 in the affected regions. Furthermore, the reduction of fixed asset investment is doubled compared with GDP reduction, which is approximately 9–11% in terms of magnitude. This can be translated as a reduction of 329–402 million *yuan* annually, given the average county-level fixed asset investment is 3.65 billion *yuan* in 2006 in the affected regions. Therefore, these calculations based on the magnitude of coefficients suggest that the GDP reduction is mainly investment driven and can be explained by the drop in fixed asset investment to a large extent.

4.3. Event study

The OLS estimation suggests that the high-speed rail upgrade in 2007 significantly hurts the economic growth in the affected counties. However, a prerequisite for the validity of difference-in-differences design is that the pre-trend of outcome variables between control and treatment groups should be similar. In this subsection, I present event study graphs that plot the effects of the high-speed rail upgrade in 2007 on the economic performance of the affected counties. These graphs are derived from the following regression model:

$$\text{Outcome}_{i,t} = \beta_0 + \sum_{k=-5}^{2} \alpha_k HSR_i * 1\{\text{Yr}_t = k\}$$
$$+ \alpha \text{Covariates}_{i, yr2000} + \gamma \text{Year}_t * \text{Province}_i + \delta \text{County}_i + \epsilon_{i,t}, \tag{2}$$

where $1\{\text{Yr}_t = k\}$ is an event time indicator equal to 1 for each year before and after the high-speed rail upgrade. Year zero is the year that the high-speed rail upgrade was implemented. For example, in 2007, $\text{Yr}_t = 0$, while in 2006, $\text{Yr}_t = -1$. To compare the effects of the event over years with the year right before the high-speed rail upgrade, 2006 is taken as the baseline year. Therefore, its coefficient ($k = -1$) is not reported in
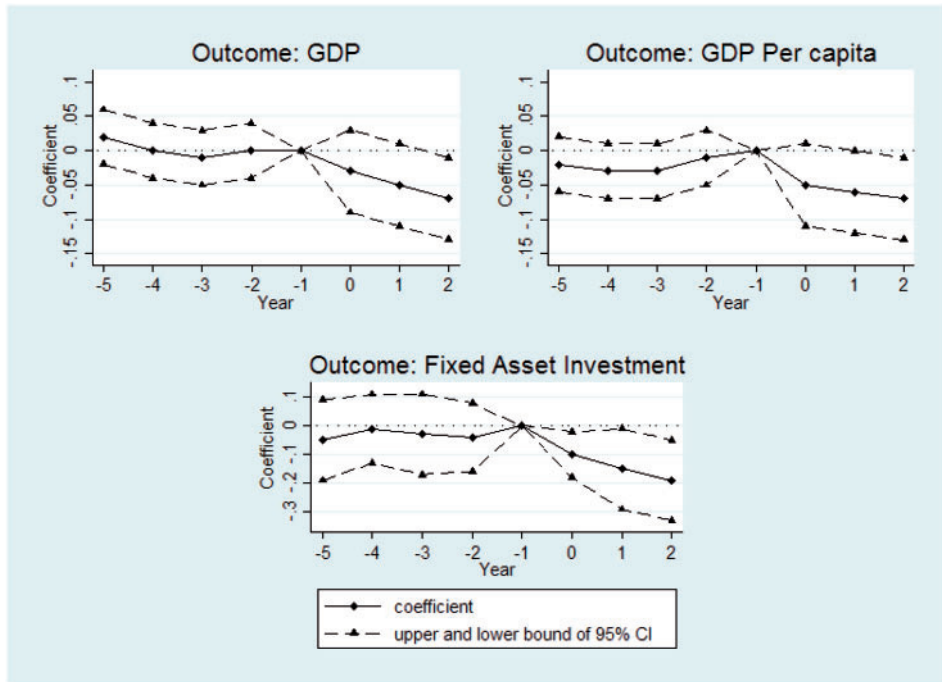
Figure 10. Event study: The impact of high-speed rail upgrade on affected counties.
Notes: 1. Year 0 indicates year 2007, when the second round of high-speed rail upgrade was implemented. Year −1 (year 2006) is the baseline year for comparison. 2. For each coefficient, the 95% confidence interval is reported.

this event study. It is notable that the counties that were affected by the high-speed rail upgrade in 2004 have been excluded from this analysis because the event study focuses on the upgrade in 2007.

Figure 10 plots the event study coefficients, α_k, and 95% confidence intervals within a 7-year event window. The point estimates represent the time path of outcome variables, that is, GDP, GDP per capita and fixed asset investment affected by high-speed rail upgrades relative to non-affected counties conditional on county-level covariates in 2000 and their interactions with linear and quadratic time trends, county and province * year fixed effects. All three graphs support the validity of the design because none of the coefficients are significantly different from zero prior to the high-speed rail upgrade in year zero, which indicates little difference in prior growth trends between the treatment and control groups conditional observable characteristics and fixed effects. The graphs also suggest that there seems to be a drop in GDP, GDP per capita and fixed asset investment right after the high-speed rail upgrade, which is consistent with the previous estimation. Additionally, the negative effect is estimated to grow larger over time.[18]

As a robustness check, I extend the event study to an earlier period by including data from 1997 to 2001. However, I am only able to carry out the analysis for GDP and

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[18] To save space, Figure 10 only presents the results controlling for covariates in 2000. The results remain very similar without controlling for these variables, which are available upon request.

GDP per capita as outcome variables because more than half of the fixed asset investment observations are missing during this period. Figure A2 presents the event study from 1997 to 2009 for GDP and GDP per capita. Again, none of the coefficients are significantly different from zero prior to the high-speed rail upgrade.

4.4. Robustness checks

4.4.1. Regional favoritism

To further verify the irrelevance of regional favoritism, we collect the hometown and working experience information for all of the state-level and vice state-level leaders, as well as the Minister and Vice Minister of Railways during the period from 2002 to 2009. In total, we have 56 counties that are the hometowns of the highest-ranked leaders in the country from 2002 to 2009 and 91 counties that are either the hometowns of such leaders or the counties in which the leaders previously worked. Table 3 provides the statistics to compare the status of those 'politically connected' counties with the 'non-connected' counties in terms of railway upgrades and railway accessibility. Interestingly, we actually find that a slightly lower share of 'politically connected' counties was selected for railway upgrades compared with the share of 'non-connected' counties. Moreover, on average, those 'connected' counties also have less railroad access compared with the rest of the counties. Conditional on having railroad access, the share of counties selected for railway upgrades is approximately the same in both groups.[19] Therefore, it is unlikely that the estimated impact is driven by regional favoritism.

4.1.2. Robustness check dropping counties with new rail access

In this robustness check, I drop the counties that gained railway access from 1997 to 2001 from the sample, including 32 counties along 7 railway lines that opened during this period.[20] All of these counties belong to the control group in our main estimation. However, they might have benefited from the new railway access in the first few years, which led to different growth patterns in our estimation period (2002–2009). Because of this concern, I report the regression results after dropping these counties in Table 4. Fortunately, the results remain very similar to Table 2.

4.1.3. Robustness check dropping counties close to high-speed rail stations

The treatment effect identified in this article is the difference in growth rates between the counties on and off the high-speed rail lines before and after the railway upgrades. One concern related to the research design is that counties too close to the high-speed rail cities may also be affected by the railway upgrade even though they are off the treated railway lines, that is, belonging to the control group. To make sure that the control group is not contaminated by the railway upgrades, I drop all counties within 50 km to the nearest cities with high-speed rail stations established in 2007. Table 5 reports the results using the restricted sample. The results for GDP and GDP per capita

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19   About 18.5% and 18.1% for 'connected' and 'non-connected' counties in Panel A, respectively; 19.4% and 18.1% for 'connected' and 'non-connected' counties in Panel B, respectively.

20   The seven railway lines are as follows: *changjin xian*, *dacheng xian*, *nankun xian*, *xikang xian*, *guangda xian*, *shenyan xian* and *jinwen xian*.

Table 3. Robustness check: Testing of regional favoritism

	Panel A: Hometown connection			
	Connected		Not connected	
	Number	Share	Number	Share
Upgraded	5	8.9%	177	9.7%
Not upgraded	22	39.2%	800	43.9%
No rail	29	51.8%	845	46.4%
Total	56	100%	1822	100%
	Panel B: Hometown and career connection			
Upgraded	7	7.7%	175	9.8%
Not upgraded	29	31.9%	793	44.4%
No rail	55	60.4%	819	45.8%
Total	91	100%	1787	100%

Notes: 1. Panel A only considers hometown connection, while Panel B considers both hometown and career connections. 2. Data are hand collected from online resumes of political leaders by the author.

are very similar to the main findings in terms of significance and magnitudes. However, the coefficients on fixed asset investment are larger (14–15%) compared with the results in Table 2 (9–11%). Such a difference indicates that counties that are further away from the high-speed rail stations experienced more reduction in fixed asset investment compared with counties that are close to the urban core.

4.1.4. Robustness check using aggregated data

The difference-in-differences estimation for the aggregated data following Bertrand et al. (2004) is reported in Table A1. The results are consistent with the estimation using disaggregated data, which further verifies the robustness of the difference-in-differences specification.

5. Discussion

5.1. Heterogeneous impacts in different sectors

The main findings in the previous section suggest that the counties located in the high-speed rail upgrade railway lines have experienced economic slowdown in terms of GDP, GDP per capita and fixed asset investment compared with counties located on the non-affected railway lines. In addition, this negative impact is especially strong for the high-speed rail upgrade in 2007 compared with the earlier round of upgrade in 2004 due to its wider coverage and higher speed.

Because high-speed rail upgrades only affect the passenger rail services while leaving the freight services almost unchanged, a larger negative impact on service industries (which are more sensitive to the transportation cost of people) may be generated rather than on manufacturing industries (which are more sensitive to the transportation cost

Table 4. Robustness check: Dropping counties with new rail access

	Panel A: Without controlling for covariates in year 2000					
	Ln GDP		Ln GDP per capita		Ln Fixed asset investment	
	2005–2009	2002–2009	2005–2009	2002–2009	2005–2009	2002–2009
HSR04*After		−0.05		−0.09		−0.06
		(0.03)		(0.07)		(0.05)
HSR07*After	−0.03***	−0.04**	−0.04***	−0.04***	−0.09**	−0.10***
	(0.01)	(0.02)	(0.01)	(0.01)	(0.04)	(0.04)
County-fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Province*Year-fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.99	0.98	0.92	0.91	0.88	0.90
Observations	4498	7191	4425	6164	3831	6129
	Panel B: Controlling for covariates in year 2000					
HSR04*After		−0.03		−0.08		−0.06
		(0.03)		(0.07)		(0.05)
HSR07*After	−0.03**	−0.03**	−0.04***	−0.04**	−0.09**	−0.11***
	(0.01)	(0.02)	(0.01)	(0.01)	(0.04)	(0.04)
County-fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Province*Year-fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.99	0.98	0.92	0.91	0.88	0.90
Observations	4488	7175	4415	6149	3821	6114

Notes: 1. Robust standard errors clustered at county level are reported in parentheses. 2. * significant at the 0.1 level; ** significant at the 0.05 level and *** significant at the 0.01 level.

of goods). To test this hypothesis, I estimate the impacts of high-speed rail upgrades on industrial and service sector value added in log forms following the same specifications, as shown in Table 2.[21]

Table 6 reports the heterogeneous impacts of the railway upgrades on the industrial and service sectors using the difference-in-differences specification. The results are generally similar in terms of magnitude for the two dependent variables. However, the results for service sector value added are significantly negative in both columns in Panel A and Column 3 in Panel B, while the results for industrial sector value added are not significant in any of the specifications. These estimation results provide some evidence that the upgrades to passenger rail services affect service industries more than manufacturing industries.[22]

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[21] In some cases, the service sector value added is missing in the database, but the agricultural and industrial sectors value added are not missing. Thus, I impute the service sector value added using the formula service sector value added = GDP−agricultural sector value added−industrial sector value added.

[22] It will be interesting to further investigate the impacts of high-speed rail upgrades on different industries within the service sector. However, industry-level GDP is not available in the statistical yearbooks. I then use the total employment in different industries in each county reported in the 2000 and 2010 China Population Census to test the impact of high-speed rail upgrades on employment changes in four industries within the service sector: hotel and restaurant; financial services; real estate and rental services.

Table 5. Robustness check: Dropping counties close to high-speed rail stations (Dist≦50km)

	Panel A: Without controlling for covariates in year 2000					
	Ln GDP		Ln GDP per capita		Ln Fixed asset investment	
	2005–2009	2002–2009	2005–2009	2002–2009	2005–2009	2002–2009
HSR04*After		−0.02		−0.02		−0.05
		(0.04)		(0.09)		(0.08)
HSR07*After	−0.04**	−0.04*	−0.05***	−0.04**	−0.15**	−0.14**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.06)	(0.06)
County-fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Province*Year-fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.99	0.98	0.89	0.91	0.87	0.89
Observations	3460	5514	3397	4822	2804	4506
	Panel B: Controlling for covariates in year 2000					
HSR04*After		0.00		−0.01		−0.06
		(0.04)		(0.08)		(0.05)
HSR07*After	−0.03*	−0.03	−0.05**	−0.03*	−0.14**	−0.15***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.06)	(0.06)
County-fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Province*Year-fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.99	0.98	0.89	0.91	0.87	0.89
Observations	3450	5498	3387	4807	2794	4491

Notes: 1. Robust standard errors clustered at county level are reported in parentheses. 2. * significant at the 0.1 level; ** significant at the 0.05 level and *** significant at the 0.01 level.

5.2. Impacts by distance to the nearest high-speed train station

Counties are not equally distant to high-speed train stations in urban areas. Some are close to urban districts, while others are a few hundred kilometers away. It is thus interesting to examine whether the negative impact of high-speed rail upgrades varies by geographical proximity to high-speed train stations. On the one hand, it is possible that the counties close to the urban core were the most negatively affected because more investment was diverted from such counties to the well-connected urban areas, possibly due to proximity. On the other hand, it is also possible that the counties distant from the urban areas were the most affected because positive agglomeration spillovers from the cities to the nearby counties may offset some of the negative impact in these counties that are close to high-speed train stations.

To test the possible heterogeneous impacts by distance to high-speed train stations, I compute the distance (unit: 100 km) from the centroid of each county to the centroid of its nearest city with high-speed train stations and interact the distance and squared distance with the difference-in-differences coefficient to estimate the possible heterogeneous impacts. Table 7 shows the results. 'HSR07 ∗ After ∗ Distance' and

I do not find significant negative impacts of high-speed rail on employment changes in these four industries from 2000 to 2010.

Table 6. Heterogeneous impacts of high-speed rail upgrades in different sectors

	Panel A: Without controlling for covariates in year 2000			
	Ln (Industrial sector value added)		Ln (Service sector value added)	
	2005–2009	2002–2009	2005–2009	2002–2009
HSR04*After		−0.05		−0.06**
		(0.05)		(0.03)
HSR07*After	−0.02	−0.03	−0.03**	−0.03*
	(0.02)	(0.03)	(0.01)	(0.02)
County-fixed effect	Yes	Yes	Yes	Yes
Province*Year-fixed effect	Yes	Yes	Yes	Yes
R-squared	0.98	0.96	0.98	0.95
Observations	4665	7463	4722	7560
	Panel B: Controlling for covariates in year 2000			
HSR04*After		−0.03		−0.05
		(0.05)		(0.03)
HSR07*After	−0.02	−0.02	−0.02*	−0.03
	(0.02)	(0.03)	(0.01)	(0.02)
County-fixed effect	Yes	Yes	Yes	Yes
Province*Year-fixed effect	Yes	Yes	Yes	Yes
R-squared	0.98	0.96	0.98	0.97
Observations	4655	7449	4712	7544

Notes: 1. Robust standard errors clustered at county level are reported in parentheses. 2. * significant at the 0.1 level; ** significant at the 0.05 level and *** significant at the 0.01 level.

'HSR07 ∗ After ∗ Distance Squard' are the two triple differences terms. In addition to these terms, I also control for each pairwise interaction and the main effects. The results suggest that the impacts of high-speed rail upgrades on GDP and GDP per capita do not vary by the proximity to high-speed train stations in the urban areas. None of the triple difference terms are significant at the 0.1 level. However, the coefficients on 'HSR07 ∗ After ∗ Distance' are significantly negative for fixed asset investment in both panels. In addition, the coefficients on 'HSR07 ∗ After ∗ Distance Squard' are positive and significant in both cases. The coefficients for fixed asset investment in Panel B indicate that the negative impact of high-speed rail on investment increased with distance and reached the maximum at approximately 550 km to the high-speed rail cities. In our sample, the 90th percentile distance of an affected county to the nearest high-speed rail cities is approximately 288 km. Thus, the negative impact of high-speed rail upgrades on fixed asset investment increases with distance for most of the affected counties along the upgraded railway lines. A possible explanation for this result is that the counties relatively close to high-speed rail cities benefited from the cities' positive spillovers compared with counties that were further away. This is also consistent with the findings in Table 5, where counties at least 50 km away from high-speed rail cities experienced a larger reduction in fixed asset investment.

Table 7. Heterogeneous impacts of high-speed rail upgrade by distance to high-speed train station

	Panel A: Without controlling for covariates in year 2000		
	Ln GDP	Ln GDP per capita	Ln Fixed asset investment
HSR07*After*Distance	0.00	0.00	−0.09*
	(0.01)	(0.01)	(0.05)
HSR07*After*Distance squared	0.00	0.00	0.00*
	(0.00)	(0.00)	(0.00)
County-fixed effect	Yes	Yes	Yes
Province*Year-fixed effect	Yes	Yes	Yes
R-squared	0.99	0.91	0.88
Observations	4029	3956	3350
	Panel B: Controlling for covariates in year 2000		
HSR07*After*Distance	0.00	0.00	−0.11**
	(0.01)	(0.01)	(0.05)
HSR07*After*Distance squared	0.00	0.00	0.01**
	(0.00)	(0.00)	(0.00)
County-fixed effect	Yes	Yes	Yes
Province*Year-fixed effect	Yes	Yes	Yes
R-squared	0.99	0.91	0.88
Observations	4019	3946	3340

Notes: 1. Robust standard errors clustered at county level are reported in parentheses. 2. * significant at the 0.1 level; ** significant at the 0.05 level and *** significant at the 0.01 level. 3. The unit of distance is 100 km.

5.3. Channels

There are two possible channels that may lead to economic slowdown in the counties affected by high-speed rail. First, because high-speed trains force out some of the conventional train services in the affected counties, train accessibility decreases in those counties, which implies an increase in transportation costs (of people) in such places. As a consequence, the affected counties will become less integrated, and economic activities will decline in response to the increase in transport costs. Second, high-speed rail upgrades connect large cities more tightly because they reduce commuting time between large cities. According to the core periphery theory, it is likely that more economic activities will divert from small counties to large cities if the transport cost reduction connecting large cities is greater. Two possible explanations will be tested in the rest of the section.

A county that is 'affected' by high-speed rail, that is, located on the high-speed rail upgrade lines, does not necessarily have a train service reduction after the rail upgrade because a county's overall train services are also determined by the services provided by other railway lines that pass through the county. Therefore, to test the validity of the first channel, I further examine the heterogeneous impact of high-speed rail upgrades in the affected counties that experienced train service reduction in 2007 (group A) and in other affected counties that did not experience train service reduction during the same period (group B). If the increased transport cost due to train service reduction is a

channel for reduced economic activities in the affected counties, the negative impact of high-speed rail in group A should be larger than that in group B, as group A experienced a larger increase in transport costs.

Table 8 presents the comparison of high-speed rail's impact between the above-mentioned two groups. The variable 'train service not reduced' equals one if the observations belong to group B and otherwise zero. The interaction term between the double difference coefficient ($HSR * After$) and the transport cost dummy variable (train service not reduced) indicates the difference of high-speed rail's impact between the two groups. All of the pairwise interactions and the main effects have been controlled for in the specification. It is shown that two out of the three interaction terms are positive in both panels, which works in favor of the hypothesis that the negative impact of high-speed rail upgrades on group B is smaller than that on group A. However, the difference in terms of impact is not statistically significant, which provides weak support to the first channel.

To test the second channel, that is, whether higher reduction of transport cost between large cities after the railway upgrades diverted more economic activities from small counties to large cities, I collected the highway status of all counties in the sample before and after the high-speed rail upgrade in 2007. As transport cost connecting cities is lower in counties with highway access, the change of city-to-city transport cost is generally smaller in these counties after the introduction of high-speed rail. Therefore, if the second channel works, the high-speed rail's negative impact on diverting economic activities away should be smaller in these counties compared with its impact in counties that had no highway network prior to the railway upgrade.

Table 9 displays the comparison of high-speed rail's impact between counties with and without highway access prior to the high-speed rail upgrade in 2007. The dummy variable 'connected to highway before 2007' equals one if the county was connected to the highway network before 2007 and otherwise zero. Similar to the test of the first channel, an interaction term between the double difference coefficient and highway status (HSR07 * After * Connected to Highway before 2007) is included in the regression to test the differential impact in counties with different highway access. The pairwise terms and the main effects have been included in the regressions as well. It is shown in Table 9 that all three interaction terms have positive coefficients, indicating that counties with highway access prior to 2007 suffered less from high-speed rail upgrades than counties without highway access. Specifically, the differential impact between the two groups is most significant for GDP in terms of both magnitude and significance. Therefore, the second channel is likely to play a role in explaining counties' reduced economic activities due to high-speed rail upgrades.

5.4. Magnitude of the impact

The main results suggest that high-speed rail upgrades negatively impact the GDP growth rate of the counties located on the affected railway lines by 3–5%. Answers to the following two questions may help us better understand the magnitude of such impacts. How large is the impact compared with the average economic growth rate in those areas? The annual GDP increased from 4.38 billion *yuan* in 2002 to 13.65 billion *yuan* in 2009 in the affected counties, with an overall growth rate of 311.6%, translating into an annual growth rate of 17.6%. This implies that the magnitude of a 3–5% GDP reduction is economically significant for the economic growth of the counties affected

Table 8. Channels: Increased trade cost in affected counties (2005–2009)

	Panel A: Without controlling for covariates in year 2000		
	Ln GDP	Ln GDP per capita	Ln Fixed asset investment
HSR07*After*Train service not reduced	0.02	0.00	0.04
	(0.02)	(0.02)	(0.05)
County-fixed effect	Yes	Yes	Yes
Province*Year-fixed effect	Yes	Yes	Yes
R-squared	0.99	0.92	0.89
Observations	4678	4603	3945
	Panel B: Controlling for covariates in year 2000		
HSR07*After*Train service not reduced	0.02	0.00	0.04
	(0.02)	(0.02)	(0.05)
County-fixed effect	Yes	Yes	Yes
Province*Year-fixed effect	Yes	Yes	Yes
R-squared	0.99	0.92	0.89
Observations	4688	4593	3935

Notes: 1. Robust standard errors clustered at county level are reported in parentheses. 2. * significant at the 0.1 level; ** significant at the 0.05 level and *** significant at the 0.01 level.

Table 9. Channels: Transport cost reduction between large cities (2005–2009)

	Panel A: Without controlling for covariates in year 2000		
	Ln GDP	Ln GDP per capita	Ln Fixed asset investment
HSR07*After*Connected to highway before 2007	0.06**	0.03	0.03
	(0.03)	(0.03)	(0.06)
County-fixed effect	Yes	Yes	Yes
Province*Year-fixed effect	Yes	Yes	Yes
R-squared	0.99	0.92	0.88
Observations	4617	4544	3870
	Panel B: Controlling for covariates in year 2000		
HSR07*After*Connected to highway before 2007	0.07**	0.04	0.05
	(0.03)	(0.03)	(0.06)
County-fixed effect	Yes	Yes	Yes
Province*Year-fixed effect	Yes	Yes	Yes
R-squared	0.99	0.92	0.89
Observations	4612	4539	3865

Notes: 1. Robust standard errors clustered at county level are reported in parentheses. 2. * significant at the 0.1 level; ** significant at the 0.05 level and *** significant at the 0.01 level.

by the upgrade. Given that high-speed rail upgrades lead to a significant economic slowdown in the affected counties, the next question to ask is whether the GDP reduction in counties outweighs the economic gains in cities, which makes high-speed rail upgrades an unattractive investment in terms of its economic returns. Given that the average GDP for the 183 affected counties is 8.39 billion *yuan* in 2006, the total loss of GDP in the 183 counties is 76.77 billion *yuan* in 2007.[23] During our sample period, a total of 80 cities have been connected with high-speed rail in 2007; thus, the net economic return of the investment in its first year would be positive as long as the average economic benefit in cities exceeds 76.77/80 = 0.96 billion *yuan*. In Appendix B, I briefly discuss the issues with estimating high-speed rail's impact on better connection between cities. Because there is no credible estimate on the causal impact of high-speed rail upgrade on GDP growth in cities, I use Panel B of Table A3 as a possible benchmark, where the estimated benefit of the upgrade is 16.76–21.67 billion *yuan* of GDP increase in the affected cities. Based on that, the benefit that high-speed rail upgrades have brought to cities seems to be more than enough to compensate for the losses in counties.

6. Conclusion

This article studies the distributional consequences of high-speed rail upgrades in China, which reduce the transport costs of people between large cities at the cost of bypassing peripheral counties. Applying a difference-in-differences strategy, I draw the following main conclusions. First, comparing GDP and GDP per capita of counties located on the affected railway lines to counties located on other railway lines, evidence suggests that there is a 3–5% significant reduction in GDP and GDP per capita after the 2007 high-speed rail upgrade in the counties located on the affected railway lines. Second, the GDP reduction in the high-speed rail bypassed counties, which is approximately 252–420 million *yuan*, given the average county level GDP is 8.39 billion *yuan* in 2006, can be largely explained by the concurrent drop in fixed asset investment. Finally, the reduced transport cost connecting city nodes is likely to be a channel accounting for the negative impact of high-speed rail. Together, these results imply that the economic activities divert from peripheral counties to the urban core when the transport cost of people connecting between urban areas is reduced.

One caveat of the article is that we ignore the 'general equilibrium' effect of high-speed rail upgrades in the overall transportation network. For example, the counties not selected for high-speed rail upgrades or that have no railway accessibility could still be impacted indirectly by the project because they were connected with the affected counties by railroads, highways, roads and other modes of transportation. Similarly, these counties were also connected with the cities upgraded into high-speed rail in some ways. Therefore, the 'control counties' could actually experience both negative spillovers from the counties left behind and positive spillovers from the cities that are better connected after the railway upgrades. However, in our analysis, we assume that the counties that are not on the upgraded railway lines were not affected by the project, which might not be true. Although we have tried our best to mitigate concerns

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[23] I use 5% here as the maximum negative impact of high-speed rail upgrades.

regarding this issue, for example, by providing the event study, we await future research to understand more about the 'general equilibrium' effect in infrastructure investment.

Acknowledgement

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Appendix

A. A Railway network in China

China is the third Asian country to adopt a railroad system, after Japan and India. The first railroad in China, constructed in 1876 by the British, was a local railway near Shanghai. During the 73 years after the first railroad in China and before the founding of the People's Republic of China, approximately 23,000 km of railroad were constructed in China. However, half of them were destroyed during World War II.

In 1949, railroad construction resumed and has been emphasized in almost all of China's 'Five-Year Plans.' By the late 1990s, the operating railroad length had been increased to approximately 66,000 km, with 6 main railway lines connecting several largest cities in different parts of the country: 1) Beijing–Shanghai (*jinghu xian*); 2) Beijing–Harbin (*jingha xian*); 3) Beijing–Guangzhou (*jingguang xian*); 4) Beijing–Hong Kong (*jingjiu xian*); 5) Lianyunguang–Urumqi (*longhai-lanxin xian*) and 6) Beijing–Baotou (*jingbao xian*).

In late 2002, the new Minister of Railways, Zhijun Liu, proposed his 'Great Leap Forward' strategy, which encouraged further expansion of the railroad network and many technology upgrades, including high-speed rail upgrades and construction.[24] The *Mid-long Term Railway Network Plan* enacted by the State Council in 2005 set the goal of expanding railroad length to 100,000 km by the end of 2020, which was further revised to 120,000 km in 2008, with a budget of approximately 4000 billion *yuan* (State Council, 2004, 2008). By the end of 2007, all of the provinces in China had been connected with railroad networks, as suggested in Figure A1. However, it is clear that the railroad coverage in the west, a relatively poor area, is significantly lower than in the east.

B. The impact of high-speed rail placement on cities at the prefecture level

It is shown in the previous section that less connectivity to the outside due to high-speed rail upgrade is detrimental to the small counties located on the affected railway lines. Another relevant question to ask is as follows: have large cities benefited from better connectivity due to high-speed rail placement? It is difficult to identify clearly the impact of high-speed rail on cities because they are connected to the high-speed rail 'on purpose' instead of being 'quasi-randomly' assigned. Therefore, the identification strategy used for counties cannot be applied to the analysis of prefecture-level cities. However, to provide some suggestive evidence, an OLS analysis with exactly the same setting as Equation (1) has been conducted using all of the prefecture-level cities with railroad access no later than 1996.[25]

Table A3 (Panel A and B) shows the 'correlation' of high-speed rail upgrades on prefecture-level cities in terms of both level and growth. Interestingly, high-speed rail placement does not correlate with high economic growth in the affected cities because none of the coefficients on the double difference term are significant, although seven out of nine coefficients have positive signs. However, the level regressions show that GDP and fixed asset investment levels significantly increase in cities with high-speed rail upgrades, while changes in the level of GDP per capita does not seem to correlate with high-speed rail.

In general, the correlation analysis in cities provides some suggestive evidence that high-speed rail upgrades have only a mild impact on economic growth in the prefecture-level cities. The result, though interesting, is not very surprising for two main reasons. First, a city economy has a much larger base than a county economy. Therefore, a positive shock in transportation technology may have only a trivial impact on economic growth rate, though its impact on economic levels may not be trivial. Second, cities generally have multiple, well-developed modes of transportation networks, including not only railroads but also highways, air and, in the coastal areas, water. Thus, the marginal productivity increase from a technological improvement to the railway system may not play an important role. However, the marginal productivity decrease due to lost connectivity to railroad transportation is likely to be more detrimental in counties, as they generally have a less-developed transportation network.

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[24] See http://www.curb.com.cn/pageshow.asp?id_forum=000106 for more information.
[25] All of the GDP, GDP per capita and fixed asset investment measures include only urban areas (districts) affiliated with the prefecture-level city.
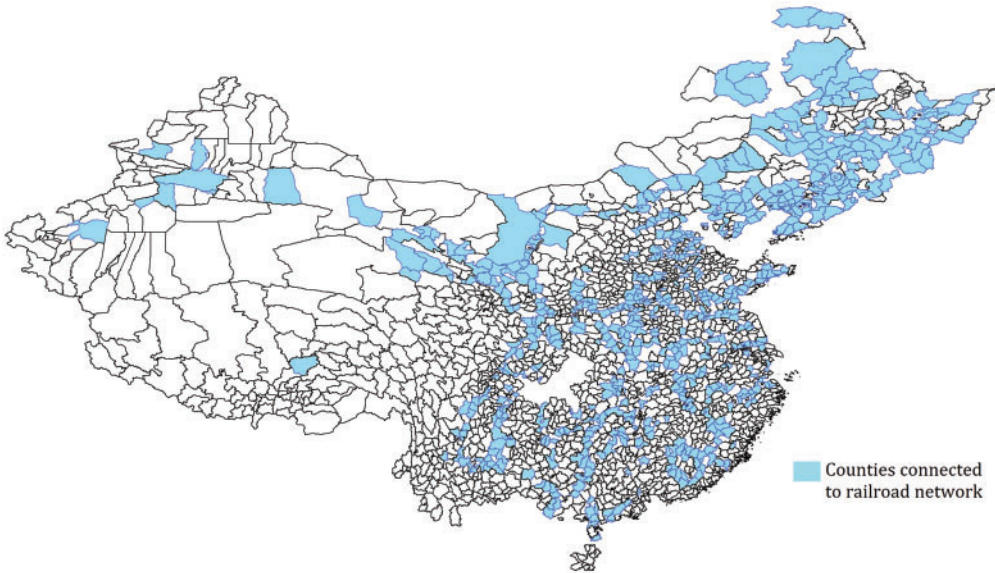
Figure A1. Counties connected to railroad network by year 2007.
GIS source: China data center (University of Michigan) and *People's Republic of China Railroad Atlas*. Blank areas are urban districts of prefecture-level cities, which are not included in our analysis.
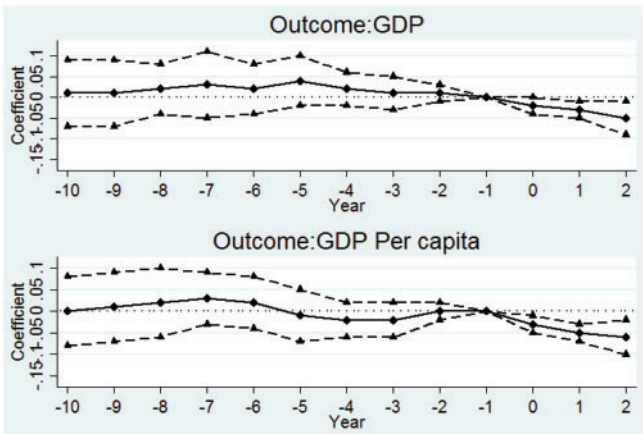

Figure A2. Event study: The impact of high-speed rail upgrade on affected counties (extended period).
Notes: 1. Year 0 indicates year 2007, when the second round of high-speed rail upgrade was implemented. Year −1 (year 2006) is the baseline year for comparison. 2. For each coefficient, the 95% confidence interval is reported.

Table A1. The impact of high-speed rail upgrades on county economic outcomes (aggregated data)

	Dependent variables					
	Ln GDP		Ln GDP per capita		Ln Fixed asset investment	
	2005–2009	2002–2009	2005–2009	2002–2009	2005–2009	2002–2009
HSR04*After		−0.04		−0.10		−0.07
		(0.03)		(0.10)		(0.05)
HSR07*After	−0.04**	−0.04**	−0.05***	−0.05***	−0.10*	−0.11**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.05)	(0.04)
County-fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Year-fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.99	0.98	0.95	0.92	0.89	0.92
Observations	1870	2804	1870	2515	1606	2443

Notes: 1. Robust standard errors clustered at county level are reported in parentheses. 2. Year-fixed effect instead of year*province-fixed effect is used in the regressions since the estimation of year by province trend requires a panel data of more than two periods. 3. * significant at the 0.1 level; ** significant at the 0.05 level and *** significant at the 0.01 level.

Table A2. The impact of high-speed rail upgrades on demographics

	Dependent variables							
	Ln Total population		Ln Rural population		Ln Total households		Ln Rural households	
	2005–2009	2002–2009	2005–2009	2002–2009	2005–2009	2002–2009	2005–2009	2002–2009
HSR04*After		−0.01		−0.01		0.00		−0.02
		(0.00)		(0.03)		(0.01)		(0.03)
HSR07*After	0.00	0.01**	0.02	0.01	0.00	0.00	0.00	0.02
	(0.00)	(0.00)	(0.03)	(0.02)	(0.00)	(0.01)	(0.04)	(0.02)
County-fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province*Year-fixed effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.99	0.99	0.95	0.96	0.99	0.99	0.94	0.96
Observations	4751	7625	4632	7489	4750	7624	4638	7503

Notes: 1. Robust standard errors clustered at county level are reported in parentheses. 2. * significant at the 0.1 level; ** significant at the 0.05 level and *** significant at the 0.01 level.

Table A3. The impact of high-speed rail upgrades on prefecture level city economic outcomes

| | Panel A: Log regressions | | | | | |
| | Ln GDP | | Ln GDP per capita | | Ln Fixed asset investment | |
	2005–2009	2002–2009	2005–2009	2002–2009	2005–2009	2002–2009
HSR04*After		0.01		0.03		0.02
		(0.03)		(0.02)		(0.06)
HSR07*After	−0.01	0.01	−0.01	0.02	0.04	0.01
	(0.02)	(0.03)	(0.02)	(0.02)	(0.04)	(0.05)
City-fixed effect	Yes	Yes	Yes	Yes		Yes
Province*Year-fixed effect	Yes	Yes	Yes	Yes		Yes
R-squared	0.99	0.99	0.98	0.98	0.97	0.96
Observations	1176	1884	1185	1896	1177	1883

| | Panel B: Level regressions | | | | | |
| | GDP (100 million) | | GDP per capita (yuan) | | Fixed asset investment (100 million) | |
	2005–2009	2002–2009	2005–2009	2002–2009	2005–2009	2002–2009
HSR04*After		122.82		1141.43		87.23**
		(100.77)		(1153.80)		(43.24)
HSR07*After	167.61**	216.69**	445.97	461.56	100.83**	109.12**
	(75.81)	(106.42)	(954.34)	(1211.27)	(41.41)	(47.20)
City-fixed effect	Yes	Yes	Yes	Yes		Yes
Province*Year-fixed effect	Yes	Yes	Yes	Yes		Yes
R-squared	0.95	0.99	0.95	0.91	0.90	0.80
Observations	1176	1884	1185	1896	1177	1883

Notes: 1. Robust standard errors clustered at city level are reported in parentheses. 2. * significant at the 0.1 level; ** significant at the 0.05 level and *** significant at the 0.01 level.