High-Speed Rail Expansion and Inter-City Highway Congestion

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Abstract

This paper studies the impact of high-speed rail (HSR) expansion on highway congestion in China. I exploit the staggered rollout of HSR services across cities between 2013 and 2019. Employing a staggered difference-in-differences strategy, I find that HSR connectivity significantly and persistently reduces highway congestion: congested road length declines by 5.8% and a congestion delay index based on travel speed decreases by 6.7%. Mechanism analysis reveals substantial modal substitution from intercity road passenger transport to rail, while freight traffic in rail remains largely unchanged. The effect is strongest in cities where highways are located farther from population centers and serve primarily long-distance travel. These findings suggest that strategic development of alternative transportation modes can effectively alleviate highway congestion.

Keywords: High-speed rail, Congestion, Transportation infrastructure, China

JEL Codes: R41, R42, L91, O18

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1 Introduction

Traffic congestion is an increasingly severe challenge facing cities across the globe. According to the 2023 INRIX Global Traffic Scorecard, traffic congestion caused economic losses amounting to \$224 billion in the United States, equivalent to 0.8% of GDP, and \$7.8 billion in the United Kingdom, or 0.3% of GDP[1]. In developing countries, the problem is even more acute due to rapid urban population growth, rising private vehicle ownership, and expanding interregional trade (Akbar et al., 2023). The consequences of congestion extend beyond travel time delays; it also increases fuel consumption and contributes substantially to greenhouse gas emissions and local air pollution.

Policymakers have typically pursued two broad strategies to address congestion: increasing the supply of transportation infrastructure (e.g., road expansion or introducing other traffic modes) or managing demand through pricing mechanisms such as congestion tolls and restrictions on automobile ownership or usage (Anderson, 2017). While the supply-side approach is politically more palatable and commonly adopted, its long-run effectiveness remains controversial. A long-standing and influential argument in transportation economics, often referred to as the "fundamental law of highway congestion" or Downs's Law (Downs, 1962, 2000; Duranton and Turner, 2011), concludes that new road capacity induces a proportional increase in vehicle travel over time, leaving congestion levels effectively unchanged. Although this view has shaped infrastructure policy for decades, subsequent empirical studies have produced mixed results.

Much of the variation in these elasticity estimates likely stems from differences in treatment contexts and outcome measures across studies. For instance, some analyses focus on short-term responses(Adler and van Ommeren, 2016; Anderson,

[1] https://static.tti.tamu.edu/tti.tamu.edu/documents/mobility-report-2023.pdf

2014), while others capture long-run general equilibrium adjustments(Beaudoin and Lawell, 2018); some examine expansions of urban roads embedded in dense commuting networks(Chen and Klaiber, 2020), while others consider intercity highways(Garcia-López et al., 2022; Ossokina et al., 2023); and while many studies rely on vehicle kilometers traveled (VKT) as a proxy for congestion(Duranton and Turner, 2011; Chen and Klaiber, 2020; Pang and Shen, 2023), this measure is only indirectly related to actual travel costs.[2] In contrast, more accurate but data-intensive measures, such as traffic speed or travel time(Gu et al., 2021; Kim, 2022), are used less frequently despite being more directly tied to welfare-relevant congestion outcomes. Understanding whether, when, and where transport infrastructure expansion can reduce congestion remains a question of first-order importance for cost–benefit analysis and transportation policy design.

This paper studies the impact of high-speed rail (HSR) openings on highway congestion in China. I exploit the staggered rollout of HSR connections across 259 prefecture-level cities as a natural experiment, combining quarterly, city-aggregate highway congestion data from 2013 to 2019—with speeds and floating-vehicle inputs from over 700 million users—with comprehensive transportation and socio-economic statistics. Using a staggered difference-in-differences approach, I find that opening an HSR station reduces congested highway length by 5.77 percent and the congestion delay index by 6.69 percent. These results are robust to alternative specifications that address recent concerns about biases in traditional two-way fixed-effects estimators under staggered treatment timing; specifically, I apply the bias-corrected estimator of de Chaisemartin and d'Haultfoeuille (2024), and the results remain qualitatively unchanged.

Opening HSR was not random. If more congested cities were prioritized for early

[2](Anas, 2024) discusses that using VKT as the outcome variable may face bias: doubling road length and observing doubled VKT does not imply unchanged congestion. Traffic flow may be distributed more evenly, thus actually reducing congestion.

connection, my DID estimates could be overstated. I therefore split cities at the median opening date into "Early" and "Late" adopters and show—first via balance tests and then via OLS, logit, and probit regressions—that baseline (Q1 2013) congestion does not predict early adoption. Finally, Kaplan–Meier survival curves and log-rank tests reveal no difference in hazard rates by baseline congestion.

Next, I explore mechanisms using city-level panel data on intercity passenger and freight flows. I document substantial mode substitution after HSR entry: intercity road passenger volume falls by 52.8 percent while rail passenger volume rises by 33.6 percent. Road freight volumes increase modestly, likely reflecting freed-up highway capacity; this effect is insufficient to offset the passenger-traffic decline. Railway freight volumes remain unchanged. These patterns suggest that HSR chiefly diverts long-distance passenger flows from highways, thereby easing congestion. Heterogeneity analysis further shows that congestion reduction is greatest in cities whose population centroids lie farther from the highway network, consistent with the fact that China's highways typically skirt urban cores.

This study contributes to the literature on transport supply and road congestion. A long-standing view—known as the Fundamental Law of Road Congestion—argues that added road capacity induces proportional increases in vehicle kilometers traveled (VKT), yielding little net congestion relief (Duranton and Turner, 2011). Empirical support for this view comes from studies in the U.S., Japan, and Europe that use VKT as a proxy for congestion (Hsu and Zhang, 2014; Garcia-López et al., 2022; Gaduh et al., 2022; Pang and Shen, 2023). By contrast, other work finds that travel-time–based measures do fall after road expansions (Ossokina et al., 2023), and theory shows that even with rising VKT, congestion costs can decline when speeds or delays are the outcomes (Anas, 2024). My paper extends this debate by examining a large-scale intermodal expansion—the rollout of China's HSR system—using high-resolution, real-time speed and congestion-distance data. I show that HSR connection delivers a lasting reduction in highway congestion and directly demonstrate passenger-to-rail substitution.

Second, this study relates closely to research on how improvements in public transportation affect road congestion through modal substitution across transport networks. A major strand of this literature exploits short-term disruptions in service—such as transit strikes—as natural experiments. These studies find that temporary reductions in public transit availability sharply increase road congestion, as commuters switch to car travel (Lo and Hall, 2006; Anderson, 2014; Adler and van Ommeren, 2016; Bauernschuster et al., 2017). Such shocks are typically unanticipated and short-lived, capturing only short-run behavioral responses, since commuters have little time to adjust deeper margins like car ownership or residential location. In contrast, the rollout of high-speed rail (HSR) in China represents a permanent, anticipated improvement in intercity transportation infrastructure. This allows for long-run general equilibrium responses, including sustained modal substitution, route re-optimization, and potentially changes in vehicle ownership or long-distance travel frequency. Despite this longer adjustment horizon, I find that HSR openings significantly reduce highway congestion.

My identification strategy resembles that of studies on subway expansions—such as Yang et al. (2018) and Gu et al. (2021) on China's subway growth or Mulalic and Rouwendal (2020)—and of research using other supply shocks, for example taxi drivers' labor supply disruptions (Mosquera, 2024) or ridesharing–technology improvements (Tarduno, 2021; Pang and Shen, 2023). Those papers examine intra-city rail systems and local commuting, whereas I focus on intercity HSR and long-distance highway traffic. My findings align with spatial transportation models (Anas and Kim, 1996; Fajgelbaum and Schaal, 2020; Allen and Arkolakis, 2022; Conwell et al., 2023; Kreindler, 2024; Almagro et al., 2024), which highlight the potential for infrastructure investments in one part of the network to relieve congestion elsewhere through intermodal spillovers and network-wide substitution effects. This study complements those theoretical predictions with empirical evidence that connecting to the HSR network reduces highway congestion in China.

Finally, this study contributes to research on the broader economic and environmental impacts of China's HSR system. Previous work has shown that HSR promotes interregional trade and market integration (Zheng and Kahn, 2013), facilitates labor mobility and migration (Dong et al., 2020), enhances firm innovation and entrepreneurship (Gao and Zheng, 2020; Hanley et al., 2022; Tsiachtsiras et al., 2022), encourages urban investment and development (Qin, 2017), and reduces air pollution (Barwick et al., 2022; Yang et al., 2023). This study complements that literature by documenting an additional channel: the effect of HSR expansion on road traffic congestion. By showing that intercity rail can relieve highway pressure through modal substitution, it highlights the importance of accounting for intermodal externalities when evaluating large-scale infrastructure investments. These findings suggest that the welfare benefits of HSR extend beyond travel time savings or productivity gains to include system-wide improvements in traffic conditions and lower congestion-related costs.

The remainder of the paper is organized as follows. Section 2 provides background information and describes the data. Section 3 presents the main empirical results. Section 4 investigates underlying mechanisms. Section 5 discusses the findings in a broader context and concludes.

2 Background and Data

The analysis exploits quarterly data on highway congestion from 2013 Q1 through 2019 Q4. The data come from a leading domestic mapping service provider that integrates floating-vehicle sensors with real-time inputs from over 700 million map users. I obtain city-level aggregates of highway network congestion for each quarter[3].

I focus on two key congestion indicators, both aggregated at the city–quarter level. First, the *Traffic Congestion Delay Index*, defined as the ratio of travel time under congested conditions to travel time under free-flow conditions. It measures the loss in roadway efficiency, with higher values indicating more severe congestion. Second, the *Congested Highway Length*, which captures the total kilometers of highway experiencing congestion within a city, providing a spatial dimension to the severity of delays.

These quarterly indicators allow me to track both the intensity and the geographic extent of highway congestion over time and across cities.

2.1 High-Speed Rail Expansion in China

Over the past two decades, China's high-speed rail (HSR) network has become the world's largest and most heavily utilized, fundamentally reshaping the country's transportation landscape. Initiated in 2003 and accelerated by the 2004 *Medium- and Long-Term Railway Development Plan*, the network grew rapidly. By 2016, annual railway passenger trips reached 2.8 billion—more than double the volume since the inaugural Beijing–Tianjin HSR—and high-speed services accounted for 43.4 percent of total rail traffic. Recent policies, such as the 2016–2030 plan's "Eight Vertical and Eight Horizontal" framework, have further expanded connectivity and economic integration. Figure 1 presents a schematic map of the HSR network as of 2019.

[Figure 1 about here]

[3]Although the dataset extends through 2024, I limit the sample to the pre-COVID period ending in 2019 Q4. Beginning in early 2020, COVID-19 lockdowns in mainland China severely disrupted intercity travel, which would confound estimates of normal traffic patterns.

2.2 Highway Congestion Data

China's highway network has played a crucial role in facilitating regional integration and economic growth. Since rapid expansion began in the late 1990s, highways have become vital conduits for both passenger and freight transport. By the end of 2013, annual highway passenger turnover reached 13.11 trillion person·km, while total highway mileage in operation stood at 104,438 km, accounting for 2.40% of the nation's road network. In the same year, vehicle-kilometers traveled (VKT) on highways totaled 423 billion veh·km, with trucks contributing 147 billion veh·km and buses 276 billion veh·km. Moreover, highway freight accounted for 40.76% of total commercial truck cargo turnover, underscoring the network's pivotal role in long-distance goods transport.

As economic growth, urbanization, and relaxation of household-registration policies have spurred private car ownership and interregional mobility, the highway system has faced growing congestion pressures. Figure 2 illustrates annual growth rates of private car ownership and highway mileage from 2000 to 2020. Although highway mileage has steadily increased, its growth rate has decelerated and consistently lagged behind the surge in private vehicles, intensifying congestion challenges.

This study uses quarterly congestion data (2013–2019) aggregated at the city level. The data come from Amap's *China City Traffic Analysis Report*, published by the Amap Traffic Big Data Intelligence Platform—a leading domestic mapping and navigation service in China. The platform combines floating-vehicle data with real-time traffic information from over 700 million users.[4] Two key indicators are aggregated at the city–quarter level: the *Congestion Delay Index* (the ratio of travel time under congestion to free-flow travel time) and the *Proportion of Congested Mileage*, capturing

[4] An important advantage of the Amap platform is its extensive user base: most in-vehicle navigation systems and ride-hailing apps in China rely on Amap's mapping software. Traffic statistics are based on observations collected between 6 a.m. and 10 p.m. (GMT+8) each day of the quarter.

both efficiency loss and the spatial extent of congestion.[5]

[Figure 2 about here]

2.3 Supplementary data

In addition to the congestion and rail data, I incorporate annual data from the City Statistical Yearbook. This dataset is structured at the city-year level and includes a wide range of social-economic variables. It covers population size, GDP , and detailed data on various types of road construction mileage—including urban roads, rural roads, expressways, and other specialized road networks. Moreover, the dataset provides information about transportation activities, including public transit usage, freight volumes, and other mobility-related statistics, which allow I test Substitution Patterns among traffic modes.[6]

2.4 Descriptive statistics

The data sample is organized at the city–quarter level and covers 2013 Q1 through 2019 Q4 in China. Panel A includes the outcome variables: log congested highway length and the highway traffic delay index of each city, both varying quarterly. treat-post is an indicator equal to 1 once a city has connected to high-speed rail and remains 1 thereafter.

Panel B contains city–traffic variables collected from the China City Statistical Yearbook and the China Regional Economic Statistical Yearbook for 2010–2019 at the

[5]The delay index is the ratio of actual travel time to free-flow travel time for each highway segment. Higher values imply slower speeds and more severe congestion. The city-level index is the average across all segments. A segment is "congested" if its speed falls below 55% of free-flow speed (delay index ¿ 1.8). The total length of such segments is the second outcome variable.

[6]Since the City Statistical Yearbook reports data at an annual frequency, I merge it into the quarterly panel by replicating each city-year observation across four quarters. This approach preserves the quarterly panel structure while allowing consistent inclusion of time-varying controls. These variables change annually rather than quarterly, this interpolation does not affect the identification strategy.

city–year level, including total highway passenger volume (in ten-thousands of persons) and freight volume (in ten-thousands of tonnes), as well as total rail passenger and freight volumes.

Panel C includes socio-economic controls from the China City Statistical Yearbook for 2013–2019 at the city–year level: log population (in ten-thousands of persons), log vehicle ownership, and log GDP (in ten-thousands of yuan).

Panel D contains road-network data for each city and year (from 2013 to 2019) sourced from OpenStreetMap: total road network length and total highway length (both in kilometers). All annual series are merged with the quarterly outcomes to form a unified city–quarter panel for further analysis.

Table 1 about here

3 Empirical Methods and Results

3.1 Identification Strategies

I identify the causal effect of high-speed rail (HSR) connectivity on highway congestion using a staggered difference-in-differences (DID) design. Our baseline specification is:

$$\text{Congestion}_{c,t} \;=\; \alpha\,(\text{HSR}_c \times \text{Post}_t) \;+\; X'_{c,t}\beta \;+\; \pi_c \;+\; \lambda_{y(t)} \;+\; \theta_{q(t)} \;+\; \varepsilon_{c,t}\,, \tag{1}$$

In equation 1, $\text{Congestion}_{c,t}$ measures either the congestion delay index or the share of congested highway mileage for city c in quarter t. The key regressor $\text{HSR}_{c,t}$ is a dummy that switches on in the first quarter a city gains high-speed rail access and remains unity thereafter. The vector $X_{c,t}$ contains controls variable in city level such as log GDP per capita, log population, and log vehicle ownership—to account for evolving city-level economic and demographic conditions. City fixed effects π_c absorb all time-invariant heterogeneity across cities, while year fixed effects $\lambda_{y(t)}$ capture common shocks in calendar year $y(t)$. Seasonal patterns are addressed by quarter fixed effects $\theta_{q(t)}$, with $q(t) \in \{1, 2, 3, 4\}$ indicating the quarter of the year. The disturbance term $\varepsilon_{c,t}$ is clustered at the city level.

To assess the credibility of my identification strategy, I examine pre-treatment trends in congestion outcomes to ensure the parallel-trends assumption holds.

Standard errors are clustered at the city level to address potential correlations across cities due to the highway network. This empirical approach exploits the staggered rollout of HSR connectivity as an exogenous source of variation to isolate the effect of improved transportation infrastructure on congestion.

In a staggered-treatment environment, two-way fixed-effects (TWFE) estimators can be biased. This bias arises because early-treated units later serve as controls for cities treated subsequently, and because treatment effects may be heterogeneous or evolve over time (Baker et al., 2022; de Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021). To address these concerns, I follow de Chaisemartin and d'Haultfoeuille (2024) by excluding always-treated cities—specifically, those connected to HSR before 2013. This exclusion ensures that, in each period, the control group consists only of not-yet-treated cities, thereby mitigating bias from dynamic treatment effects.

3.2 TWFE Baseline

In this section, I present baseline estimates using a staggered difference-in-differences framework to assess the impact of HSR connectivity on highway congestion. I focus on two key measures: the traffic congestion delay index and the logarithm of congested highway length, both at the city–quarter level.

I first estimate the average treatment effect on treated cities using the specification in equation 1. The results appear in Table 2. Panel A reports estimates for the

Traffic Congestion Delay Index (percent change in travel speed), and Panel B reports estimates for log congested highway length (in kilometers).

Table 2 about here

In Column (1), I include only city and year fixed effects. The coefficient on the HSR indicator is negative and significant at the 1 percent level in both panels, implying that HSR connectivity reduces the delay index by 8.9 percent and congested highway length by 3 percent. Column (2) adds quarter fixed effects to account for seasonality, such as the Spring Festival travel surge in Q1 (Xu et al., 2017); the estimates remain virtually unchanged. Column (3) further controls for traffic-related covariates, including log vehicle ownership, total road length, and highway length. Column (4) adds city-level economic controls—log population and log GDP—and again finds significant negative treatment effects. Column (5) includes the full set of traffic and economic covariates, and Column (6) additionally includes quarter fixed effects. In this most saturated specification, HSR connectivity lowers the congestion delay index by 8.4 percent and congested highway length by 4.5 percent. This comprehensive model is my preferred baseline for subsequent analysis.

To capture the dynamic effects of HSR connectivity, I estimate an event-study specification of the form

$$Y_{ct} = \sum_{L=-8}^{8} \alpha_L \, \mathbf{1}\{\text{Period} = L\} + \lambda_{c,\text{year}(t)} + \eta_{q(t)} + \varepsilon_{ct}, \tag{2}$$

where Y_{ct} denotes the outcome of interest (either the traffic congestion delay index or the log of congested highway length) for city c in quarter t. The indicator $\mathbf{1}\{\text{Period} = L\}$ equals one if the observation is L quarters relative to the first quarter of HSR connectivity (with $L = 0$ indicating treatment onset). City-by-year fixed effects, $\lambda_{c,\text{year}(t)}$, control for time-varying city-level factors, while quarter fixed effects, $\eta_{q(t)}$, capture seasonal trends. Standard errors are clustered at the city level.

Panel A of Figure 3 presents the event-study estimates for the congestion delay index, and Panel B shows the corresponding estimates for log congested highway length. As illustrated, the pre-treatment coefficients are statistically indistinguishable from zero, which provides suggestive evidence for the parallel-trends assumption; post-treatment coefficients are significantly negative, indicating that following HSR connectivity, cities experience a marked reduction in highway congestion.

[Figure 3 about here]

3.3 de Chaisemartin and d'Haultfoeuille (2024) DID results

As discussed before, conventional TWFE estimators can be biased in staggered designs due to the use of already-treated cities as controls and the presence of heterogeneous, dynamic treatment effects. To address these issues, I implement the DID approach proposed by de Chaisemartin and d'Haultfoeuille (2024). The core idea of this method is as follows. For each city g, I first determine the quarter when high-speed rail (HSR) is first opened, denoted as F_g. Then, for each lead or lag ℓ, constructing a city-level difference-in-differences (DID) estimator by comparing the change in the congestion delay index from the quarter immediately preceding HSR opening to the ℓth quarter after opening, and subtracting from it the average change over the same period for those cities that remained untreated up to that quarter (and thus share the same baseline pre-treatment status).

Table 3 reports the aggregated event study estimates obtained by the group-by-group DID method for two outcome variables: Panel A shows results for the Traffic Congestion Delay Index, and Panel B for the Log Congested Highway Length. In Column (1), which includes only city and period fixed effects, the estimated coefficients are -0.074 for the delay index and -0.051 for the log length, indicating that HSR opening reduces the congestion delay index by 7.4% and congested highway length by 5.13%, both significant at the 1% level. Columns (2) and (3) add, respectively, traffic-level controls (as defined in Table 2) and macroeconomic controls (city GDP, population, etc.). The point estimates remain negative and of similar magnitude. Finally, Column (4) includes both sets of controls; here, HSR opening is associated with a 6.69% reduction in the congestion delay index and a 5.77% reduction in congested highway length, confirming that the decongestion effect is robust to rich sets of covariates.

Table 3 about here

Figure 4 displays these event-study results. Panel A indicates that prior to HSR connectivity, treated and control cities exhibit similar trends in the traffic congestion delay index, whereas post-treatment, the coefficients become significantly negative. Panel B shows analogous dynamics for the log congested highway length, represent a significant decline after HSR connectivity.

Specifically, I estimate group-time average treatment effects for each cohort of cities that first receive HSR connectivity. For each treated group, I track the dynamic response in highway congestion by estimating event study effects on two key outcomes: (i) the traffic congestion delay index (Panel A) and (ii) the logarithm of congested highway length (Panel B). All regressions control for quarter fixed effects and city-year fixed effects, and standard errors are clustered at the city level.

[Figure 4 about here]

Compared with the TWFE estimates above, these results remain robust: after HSR opening, cities experience substantial improvements in highway congestion, evidenced by faster travel speeds (a reduction in the congestion delay index) and shorter total congested highway segments.

Moreover, this beneficial impact appears long-run, remaining statistically significant for more than two years after HSR connectivity. This contrasts with findings from the United States and Japan, where studies report traffic-flow elasticities near or above one (Duranton and Turner, 2011; Hsu and Zhang, 2014), implying high responsiveness to added capacity. It also differs from Kim (2022), who find that roadwork in California yields only a transient congestion reduction lasting under one year. By contrast, my results align with recent evidence from China and the Netherlands documenting sustained congestion reductions following subway and highway improvements (Gu et al., 2021; Ossokina et al., 2023).

3.4 Robustness Tests

It remains possible that planners prioritized highly congested cities for early HSR rollout, which could bias my DID estimates. For example, if early-treated and late-treated cities followed different post-treatment congestion trends—perhaps because early-treated cities also received other transport or economic upgrades—then the estimated negative effect could be overstated. Conversely, if highly congested cities respond more weakly to interventions, I would understate the true effect.

I rule out this concern in three steps. First, I take Q1 2013—the earliest quarter of complete data—as the baseline, and split cities at the median HSR opening date into "Early" and "Late" adopters (never-treated cities are classified as Late)[7]. Table 4 reports balance tests on pre-treatment covariates. Although HSR opening was non-random (cities with higher GDP or greater rail demand tended to connect sooner), Early and Late groups show no significant differences in baseline highway congestion or traffic volumes.

Table 4 about here

[7] Including never-treated cities in the Late group ensures the Late sample spans the full range of post-2013 outcomes.

Second, I formally test whether baseline congestion predicts early adoption by regressing Q1 2013 congestion measures on an Early indicator. Columns (1)–(2) of Table 5 report OLS, and Columns (3)–(4) and (5)–(6) repeat the exercise with logit and probit models using robust errors. In every specification, the congestion coefficients are small and far from significance, implying that pre-treatment congestion does not predict early HSR connection. These balance and regression results show that selection on baseline congestion cannot explain the observed congestion reduction.

Table 5 about here

Third, I use a nonparametric survival analysis to visualize whether baseline congestion predicts HSR timing (Shem-Tov et al., 2024). Figure 5 plots Kaplan–Meier survival curves for two groups—below-median ("Low Congestion," blue) and above-median ("High Congestion," red). The horizontal axis, "Period," measures quarters from 2013 Q1 (period 0) to 2019 Q4; the vertical axis shows the survival probability $S(t)$, i.e. the fraction of cities that have not yet opened HSR by t. Curves start at $S(0) = 1$ and step down whenever a city connects.

If high- and low-congestion cities truly followed parallel pre-treatment trends, their curves should coincide until treatment—and indeed they overlap closely before period 0. After treatment, any policy effect would create a gap; instead the curves track each other throughout. Log-rank tests fail to reject equality of the failure functions ($\chi^2 = 0.66$, $p = 0.418$ for the delay index; $\chi^2 = 0.23$, $p = 0.631$ for log congested length), providing no evidence that baseline congestion predicts early HSR adoption.

[Figure 5 about here]

Besides selection concerns, to ensure that estimated effects are not driven by confounding time trends rather than true HSR impacts, I conduct a placebo analysis.

Specifically, I construct pseudo-treatments by shifting the treatment timing—both forward and backward—by up to eight quarters relative to the actual HSR opening. I then re-estimate Equation (1) for each pseudo-treatment and obtain the point estimates and 95% confidence intervals for the treatment coefficient.

Figure 6 displays these placebo estimates for the two congestion measures. Panel A reports estimates for the traffic congestion delay index, and Panel B reports estimates for the log congested highway length. The horizontal axis indicates the placebo quarter of "opening" relative to the actual opening quarter.

If the effect is truly driven by HSR connectivity, the only statistically significant impact should appear at the actual treatment quarter, while pseudo-treatment estimates in pre- and distant post-treatment quarters should be indistinguishable from zero. Consistent with this expectation, the placebo estimates are generally insignificant. I also perform Wald tests of joint significance for all pseudo-treatment coefficients (see Appendix for details).

[Figure 6 about here]

4 Mechanism and Heterogeneity Analysis

4.1 Substitution among Modes of Transportation

One fundamental insight from spatial economic models of transportation is that different modes are interconnected through network externalities: an improvement in one mode can ripple across the entire system (Fajgelbaum and Schaal, 2020; Allen and Arkolakis, 2022; Brancaccio et al., 2020). In the analysis above, I showed that introducing high-speed rail (HSR) connectivity not only alleviates highway congestion—by speeding travel (a decline in the congestion delay index) and shortening congested segments—but also triggers significant shifts in demand across modes.

I examine these substitution effects using annual city-level data from the City Statistical Yearbook. I estimate Equation 1 for four logged outcomes: *Highway Passenger* (log highway passenger volume, ten-thousands of persons), *Highway Freight* (log highway freight volume, ten-thousands of tonnes), *Rail Passenger* (log railway passenger volume, ten-thousands of persons), and *Rail Freight* (log railway freight volume, ten-thousands of tonnes).

Table 6 about here

Table 6 reports these regressions. In column (1), highway passenger volumes fall by about 52.8% following HSR entry, indicating a large shift from road to rail passenger travel. Highway freight volumes rise by 24.5%—a modest response that does not offset the passenger decline, consistent with reduced expressway congestion. Rail passenger volume increases by 33.6%, while rail freight is essentially unchanged, reflecting HSR's focus on passenger transport.[8] I also test heterogeneity by splitting cities on whether their population centroid is above ("High") or below ("Low") the median distance to the highway network (see Appendix Tables A1–A2).

These results align with spatial-network theory: improving one mode (HSR) reallocates demand across channels, generating system-wide efficiency gains and congestion relief (Allen and Arkolakis, 2022). They also mirror findings for subways, where rail expansions induce road-to-rail mode shifts and reduce urban congestion (Gu et al., 2021).

In sum, the reallocation of travel demand—from a sharp drop in highway passenger traffic to a surge in rail passenger flows—helps explain the observed congestion relief on highways.

[8] This analysis uses City Statistical and Transport Statistical Yearbook data from 2010–2019.

4.2 Heterogeneity Analysis

Although HSR connectivity reduces highway congestion on average, the effect may vary across cities. I examine four potential sources of heterogeneity:

Distance to Highway Network. I compute each city's population centroid (2015 Census) and measure its average distance to all highway segments. A shorter distance implies highways serve local travel; a longer distance suggests reliance on HSR for shorter trips.[9]

Highway Share of Primary Roads. I measure the ratio of highway mileage to total primary road mileage. A higher share indicates a network geared to long-haul travel, potentially amplifying substitution when HSR arrives.

Early vs. Late Adoption. I split cities at the median HSR connection quarter. Early adopters—if selected for their severe pre-treatment congestion—might experience larger congestion reductions. Conversely, late adopters could see stronger shifts if they join an already mature network.

Pre-treatment Congestion Level. I classify cities by their baseline (Q1 2013) congestion delay index. High-baseline cities may react differently than low-baseline ones.

Table 7 about here

[Figure 7 about here]

The regression results, detailed in Table 7 and visually summarized in Figure 7, reveal several patterns.

First, the congestion-reducing impact of HSR is almost entirely concentrated in cities where the population centroid lies relatively far from the highway network. In those "far" cities, the estimated coefficient on HSR is large and highly significant:

[9] The population-weighted centroid uses spatial population data; I then average the distances from this point to each highway segment.

congested highway length falls by 5.3 percent, and the delay index declines by 14.5 percent. In "near" cities, the effect is indistinguishable from zero. This finding helps explain why U.S. studies report traffic-flow elasticities at or above one: in the U.S., highways often penetrate the urban core and serve both intra-city and adjacent-city trips, and suburbanization has reinforced large commuter flows on urban freeways (Duranton and Turner, 2011; Kim, 2022). By contrast, Chinese expressways typically skirt the urban periphery and cater primarily to longer-distance travel. The results align with the theoretical prediction that—when two modes are close substitutes (HSR and highway)—shifting volume to the faster mode can lower overall congestion (Anas, 2024).[10]

Second, the congestion relief from HSR is stronger in cities where highways account for a larger share of primary road mileage. In high–highway-share cities, congested highway length drops by 4.3 percent and the delay index by 7.9 percent after HSR opening. In low–highway-share cities, both point estimates are smaller and statistically insignificant. Intuitively, when ex ante travel demand relies heavily on expressways and local roads are under-developed, the availability of HSR diverts a larger share of long-distance trips off highways, yielding a greater net congestion reduction.

Third, when splitting the sample by HSR adoption timing, heterogeneity is less clear-cut. Early adopters exhibit a significant drop in the delay index (5.3 percentage points), whereas late adopters show a smaller change (8.7 percent) that does not reach statistical significance. For congested length, both groups have negative coefficients (–4.0 percent for early, –1.4 percent for late), but only the early–adopter estimate is significant. This pattern suggests that early connection accelerates travel-speed improvements, while reductions in total congested lane-kilometers occur more uniformly

[10]While one potential explanation for low congestion elasticity in China is that its highways are tolled, unlike the largely free U.S. expressways, this institutional difference does not fully explain cross-country variation. For example, in Japan, where highways are also tolled, Hsu and Zhang (2014) document elasticities well above one after lane expansions.

once HSR becomes available.

Finally, I examine heterogeneity by baseline congestion levels. Splitting cities at the mean delay index, I find that HSR's congestion-mitigating effects are concentrated among initially high-congestion cities: congested mileage falls by 12.3 percent and the delay index by 4.7 percent, both significant. In low-congestion cities, effects on both metrics are small and indistinguishable from zero.

5 Discussion

As shown above, the expansion of China's high-speed rail (HSR) network is associated with significant reductions in highway congestion at the city level. The results suggest that much of this effect operates through modal substitution: travelers shift from road-based intercity travel to rail. Interestingly, Downs's Law appears not to hold in this context. A key reason lies in the structure of China's urban planning and expressway design. Unlike countries such as the U.S. (Duranton and Turner, 2011), Chinese highways often bypass urban cores and serve primarily long-distance travel. This reduces their exposure to elastic, short-distance commuting demand and makes highway usage less responsive to capacity increases or to changes in local accessibility. The findings underscore how infrastructure configurations and institutional differences shape congestion elasticity and help explain why elasticity estimates vary so widely across countries, timeframes, and roadway types.

Despite these contributions, several limitations remain. First, data constraints force reliance on city-level aggregate measures of congestion, preventing observation of individual travel behavior or distinction between commuting and long-distance trips. This also limits estimation of mode-specific or trip-type-specific elasticities. Second, HSR infrastructure is inherently characterized by network effects: the value of connecting to the HSR system depends not only on local access but also on how many other cities are connected and at what frequency. The highway segments most affected by HSR openings likely differ by corridor alignment and regional connectivity patterns, but the data do not allow disaggregation by road segment or direction.

Future research could address these limitations by using GPS trajectory data or mobile phone location data, e.g. (Chen and Pope, 2020; Abramitzky et al., 2025). Such microdata would allow direct observation of changes in commuting patterns and long-distance travel in response to HSR openings. Combining these data with discrete-choice models, e.g. (Allen and Arkolakis, 2014; Monte et al., 2023), would enable a more precise welfare analysis of modal shifts and congestion relief. In that framework, the congestion-relieving effect of HSR likely represents a positive externality that is not fully internalized in traditional cost–benefit analyses; failing to incorporate these spillovers may underestimate HSR's true social benefits.

Finally, future work could compare supply-side interventions such as HSR construction with demand-side policies like congestion pricing, driving restrictions, or license-plate lotteries (Davis, 2008; Gibson and Carnovale, 2015; Green et al., 2016; Hall, 2018; Gu et al., 2017; Hall, 2021). Each tool generates different aggregate welfare impacts, fiscal costs, and distributional effects. A comprehensive comparison would offer valuable guidance for urban and regional transport planning.

6 Conclusion

This paper investigates whether the expansion of China's high-speed rail (HSR) system reduces highway congestion. Using a staggered difference-in-differences design and user-generated traffic data from 2013 to 2019, I find that opening HSR lines significantly lowers highway congestion in treated cities, both in terms of congested segment length and travel-speed indices. These effects are particularly pronounced in cities where highways are located farther from population centers, suggesting that benefits stem from reduced long-distance, intercity road travel rather than from local commuting. Mechanism analysis shows substantial modal substitution from road-based intercity passenger transport to rail, while freight volumes remain largely unaffected.

These findings contribute to our understanding of the relationship between transportation infrastructure and road congestion. While the "Fundamental Law of Road Congestion" posits that supply-side expansions often induce proportional demand increases, this paper highlights that the relationship is context-dependent. In settings like China—where expressways are designed for long-distance travel and are not deeply embedded in urban commuting networks—infrastructure investments in alternative modes such as HSR can generate substantial congestion relief. The results imply that evaluations of transport projects should account for intermodal spillovers and system-wide interactions. Future research may focus on incorporating higher-resolution data and structural approaches to better assess the welfare consequences of multi-modal infrastructure investments.

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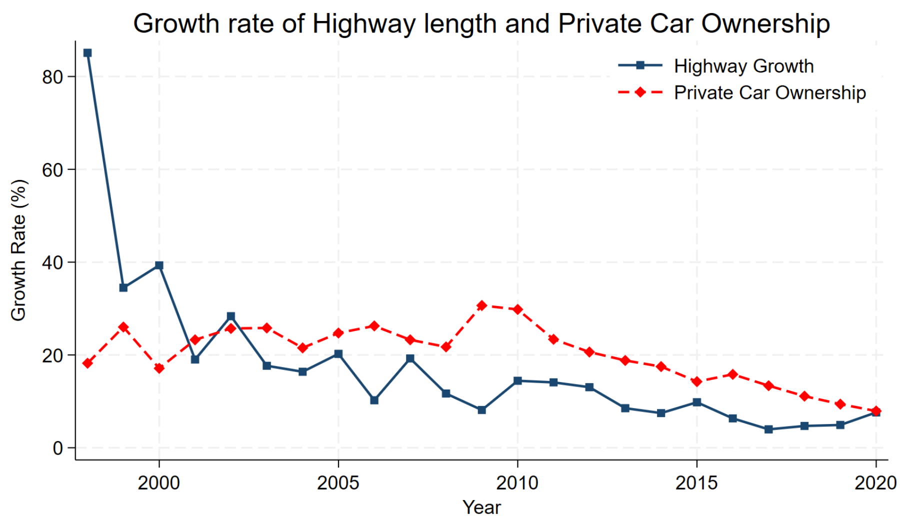
Figure 1. High-Speed Rail Network in China (2000–2023)

Notes: This figure illustrates the spatial expansion of China's HSR network from 2003 to 2023. Black lines indicate routes operational before 2013, while orange lines denote those opened between 2013 and 2020.

Figure 2. Annual Growth Rates of Private Car Ownership and Highway Mileage (1998–2020)

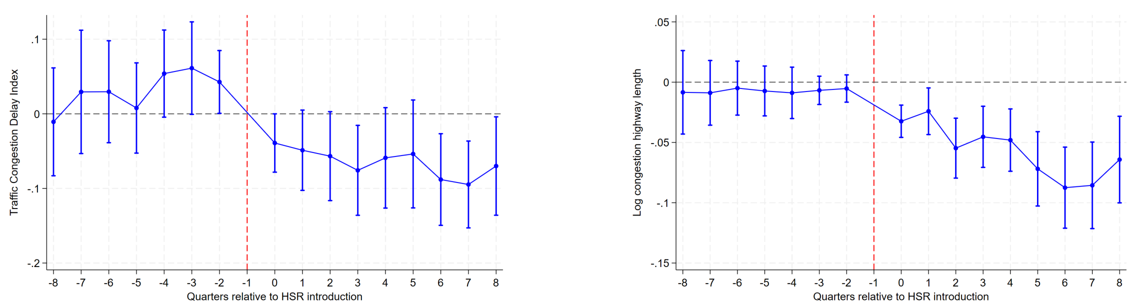


Notes: This figure illustrates the annual growth rates of highway mileage and private car ownership in China over the period from 1998 to 2020. Although highway mileage has consistently increased, its growth rate has gradually slowed down. In contrast, the continuous rise in private car ownership has driven growing demand, contributing to increased congestion pressures. Data are sourced from the China Transportation Yearbook.

Figure 3. Event-Study Estimates for Highway Congestion

(a) Panel A: Traffic Congestion Delay Index

(b) Panel B: Log Congested Highway Length

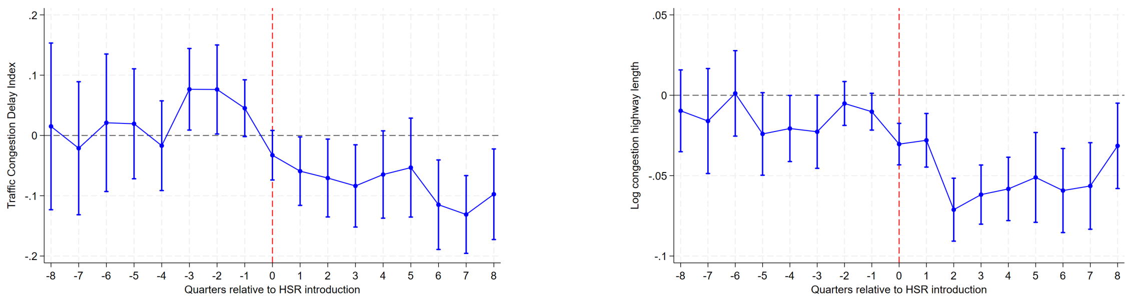


Notes: This figure shows the impact of HSR connectivity on highway congestion at the city level under the TWFE specification. Panel A's dependent variable is the traffic congestion delay index, while Panel B's dependent variable is the log of congested highway length. Data spans from 2013 year to 2019 year. All regressions control for quarter fixed effects and city-year fixed effects, and standard errors are clustered at the city level.

Figure 4. de Chaisemartin and d'Haultfoeuille (2024) Method Estimates for HSR connect and Highway Congestion

(a) Panel A: Traffic Congestion Delay Index

(b) Panel B: Log Congested Highway Length

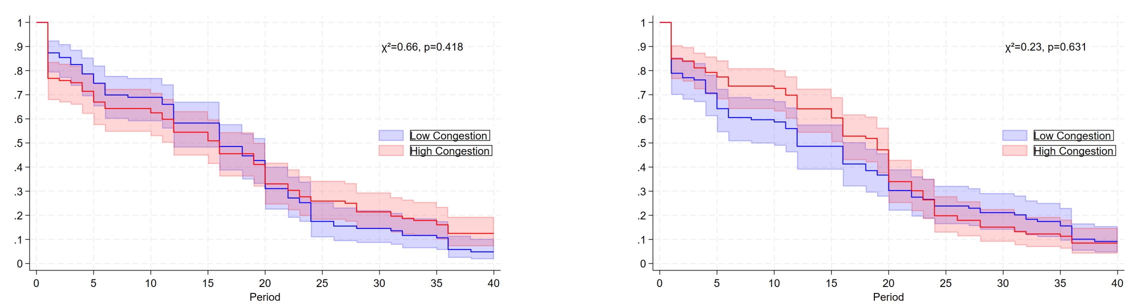


Notes: This figure shows the dynamic effects of HSR connectivity on highway congestion at the city level using the DID approach of de Chaisemartin and d'Haultfoeuille (2024). The dependent variable in Panel A is the traffic congestion delay index, dependent variable in panel B is log of congested highway length. All regressions include quarter fixed effects and city-year fixed effects, and standard errors are clustered at the city level.

Figure 5. HSR Opening and City Pre-treatment Traffic Congestion

(a) Panel A: Traffic Congestion Delay Index

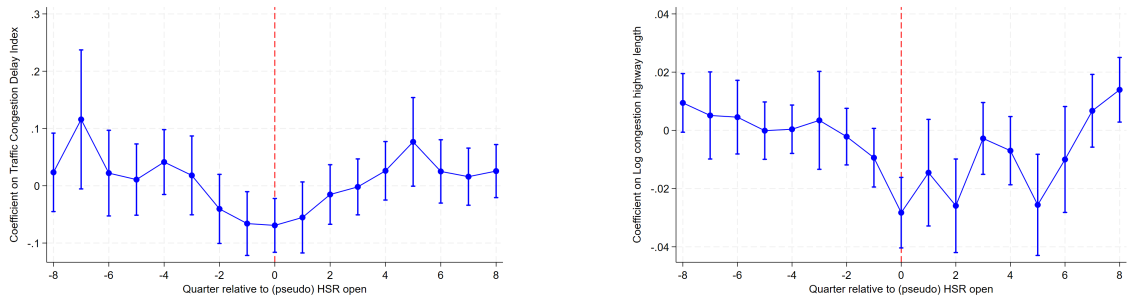
(b) Panel B: Log Congested Highway Length



Notes: This figure plot Kaplan–Meier estimates of the failure function for a city's HSR opening over 40 periods from the baseline (period 14), separately for below-median ("Low Congestion," blue) and above-median ("High Congestion," red) pre-treatment congestion. Shaded bands are 95% confidence intervals. I test equality of the two failure curves using the two-sided log-rank test (Peto–Peto–Prentice), and report the resulting χ^2 and p–values in each panel. There is no evidence that high-congestion and low-congestion cities differ in their hazard of HSR opening.

Figure 6. Placebo Test: Pseudo-treatment Timing Effects on Highway Congestion

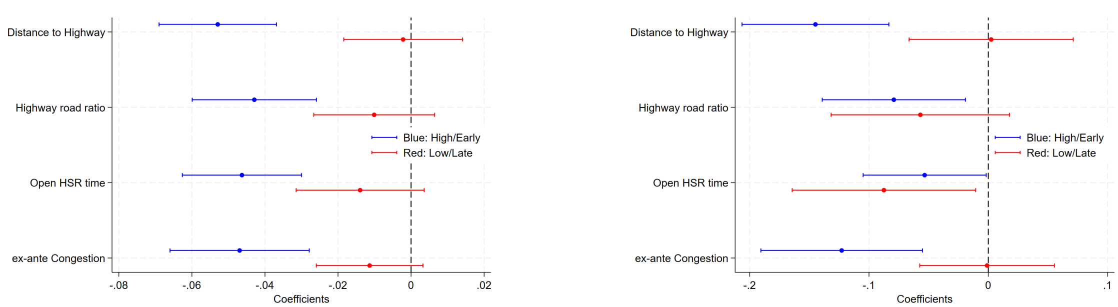
(a) Panel A: Traffic Congestion Delay Index (b) Panel B: Log Congested Highway Length



Notes: The x-axis indicates the placebo quarter of opening relative to the actual quarter of HSR opening. For each pseudo-treatment timing (up to eight periods before and after the actual HSR opening), I re-estimate Equation (1) and plot the point estimates and 95% confidence intervals of the HSR interaction term. In both panels, only the estimate at the actual treatment period is statistically significant, indicating that our results are not driven by confounding time trends. All regression includes quarter fixed effects and city-year fixed effects, Standard errors are clustered at the city level.

Figure 7. Heterogeneous results of HSR Effects on Congestion Visualized

(a) Panel A: Log of Congested Highway Length (b) Panel B: Congestion Delay Index



Notes: These forest plots display the heterogeneity in the impact of HSR connectivity on highway congestion. The plots split the sample by key dimensions: city distance to highways, highway-to-primary road ratio, timing of HSR opening, and pre-treatment congestion levels. The results illustrate that the congestion-reducing effects of HSR are more pronounced in cities with greater local travel barriers, higher highway capacity ratios, earlier HSR adoption, and higher initial congestion.

Table 1. Summary Statistics

Variable	Obs	Mean	Std. dev.	Min	Max
Panel A. Outcomes and Treatment					
Log congested highway length	6,897	5.77	0.81	0.84	7.69
Congestion delay index	6,989	1.24	0.28	0.89	3.87
HSR connected (treat_post)	7,248	0.47	0.50	0.00	1.00
Panel B. City–Traffic Variables					
Highway passenger volume	10,012	8.37	1.00	3.37	11.96
Highway freight volume	9,988	8.93	0.89	4.96	12.59
Rail passenger volume	4,448	5.78	1.27	0.69	10.25
Rail freight volume	4,452	6.22	1.56	1.10	10.30
Panel C. Socio-Economic Controls					
Log population	7,120	5.82	0.75	0.04	7.31
Log vehicle ownership	6,804	12.88	0.91	9.04	15.48
Log GDP	7,072	16.54	0.86	14.12	19.76
Panel D. Road Network					
Total road length (km)	6,932	13 151.41	6873.37	539.95	40 149.00
Highway length (km)	7,148	423.01	279.39	2.33	2042.48

Notes: Data cover 2013–2019 except for panel B cover from 2010 to 2019. Panel A variables are observed at the quarterly–city level, while Panels B–D derive from annual city statistical yearbooks. Annual and quarterly sources are merged into a unified quarterly–city panel. Panel A reports outcome variables: log congested highway length and congestion delay index, and the treatment indicator (HSR connected). Panel B shows city–traffic variables: highway passenger and freight volumes (logs). Panel C presents socio-economic controls: log population, log vehicle ownership, and log GDP. Panel D contains road network measures: total road length and highway length (in kilometers).

Table 2. TWFE Static Estimation Results

Panel A Dependent variable: Traffic Congestion Delay Index						
	(1)	(2)	(3)	(4)	(5)	(6)
HSR	-0.089***	-0.083***	-0.090***	-0.087***	-0.089***	-0.084***
	(0.022)	(0.022)	(0.023)	(0.022)	(0.023)	(0.023)
lncar			0.006		0.003	0.003
			(0.049)		(0.050)	(0.050)
roadlength			0.000		-0.000	-0.000
			(0.000)		(0.000)	(0.000)
highwaylength			0.000		0.000	0.000
			(0.000)		(0.000)	(0.000)
lnpop				0.164	0.076	0.078
				(0.111)	(0.120)	(0.119)
lnGDP				0.010	0.019	0.019
				(0.064)	(0.068)	(0.069)
Constant	1.286***	1.284***	1.150*	0.156	0.452	0.447
	(0.010)	(0.010)	(0.602)	(1.107)	(1.146)	(1.146)
Observations	6,993	6,993	6,439	6,821	6,423	6,423
Within R-sq.	0.186	0.189	0.189	0.189	0.191	0.195
Fixed effects:						
City FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Quarter FE		✓				✓
Panel B Dependent variable: Log Congested Highway Length						
	(1)	(2)	(3)	(4)	(5)	(6)
HSR	-0.030***	-0.031***	-0.046***	-0.031***	-0.045***	-0.045***
	(0.011)	(0.011)	(0.009)	(0.011)	(0.009)	(0.009)
lncar			0.110***		0.112***	0.112***
			(0.026)		(0.027)	(0.027)
roadlength			-0.000*		-0.000	-0.000
			(0.000)		(0.000)	(0.000)
highwaylength			0.001***		0.001***	0.001***
			(0.000)		(0.000)	(0.000)
lnpop				0.111*	0.065	0.065
				(0.058)	(0.048)	(0.048)
lnGDP				0.036	-0.031	-0.031
				(0.036)	(0.026)	(0.026)
Constant	5.781***	5.781***	4.106***	4.566***	4.210***	4.210***
	(0.005)	(0.005)	(0.337)	(0.626)	(0.549)	(0.550)
Observations	6,897	6,897	6,439	6,807	6,423	6,423
Within R-sq.	0.990	0.990	0.990	0.986	0.989	0.989
Fixed effects:						
City FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Quarter FE		✓				✓

Notes: This table presents two panels of TWFE static regressions. Panel A reports results for opening high-speed railway with the Congestion Delay Index as the dependent variable, and Panel B for opening high-speed railway with the Log of Congested Highway Length as the dependent variable. Columns (1) and (4) include city and year fixed effects only. Columns (2) and (6) further add quarter fixed effects. Column (3) adds traffic-related controls (log of number of cars, highway length in kilometers, and total road length in kilometers). Column (5) adds socio-economic controls (log of city population and log of GDP). Column (6) includes all controls and all fixed effects. 'treat#post' is the variable of interest; stable significant coefficients in both panels indicate that HSR opening reduces highway congestion in connected cities. Robust standard errors clustered at the city level are shown in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3. de Chaisemartin and d'Haultfoeuille (2024) Method Estimation Results

	Panel A: Traffic Congestion Delay Index			
	(1) No control	(2) Traffic controls	(3) Macro controls	(4) Full controls
HSR	-0.0740***	-0.0715***	-0.0738***	-0.0669**
	(0.0247)	(0.0260)	(0.0258)	(0.0284)
Obs	3,244	2,900	3,130	2,891
Fixed effects:				
City FE	✓	✓	✓	✓
Period FE	✓	✓	✓	✓
	Panel B: Log Congested Highway Length			
	(1) No control	(2) Traffic controls	(3) Macro controls	(4) Full controls
HSR	-0.0513***	-0.0576***	-0.0500***	-0.0577***
	(0.00823)	(0.00797)	(0.00819)	(0.00815)
Obs	3,183	2,900	3,120	2,891
Fixed effects:				
City FE	✓	✓	✓	✓
Period FE	✓	✓	✓	✓

Notes: This table reports the estimation results using the de Chaisemartin and d'Haultfoeuille (2024) method. The sample period is 2013Q1-2019Q4. Cities connected before 2013 (i.e. the always-treated group during the sample period) are excluded. The unit of observation is city–quarter. Panel A shows estimation for the outcome variable Traffic Congestion Delay Index, and Panel B for Log Congested Highway Length. Columns (1) to (4) include: no controls; only traffic controls (log of number of cars, highway length in kilometers, and total road length in kilometers); only macro (socio-economic) controls (log of city population and log of GDP); and all controls. All models include city fixed effects and quarter fixed effects. Robust standard errors clustered at the city level are reported in parentheses. ***, **, and * denote statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 4. Characteristics of Early vs. Late HSR Cities at Base period

	Control (Early)	**Treatment** (Late)	**Difference**
Log congested highway length	5.712	5.630	0.082
	(0.628)	(0.566)	(0.279)
Congestion delay index	1.295	1.246	0.049
	(0.298)	(0.300)	(0.190)
Highway passenger volume	8.769	8.857	−0.088
	(0.765)	(1.048)	(0.452)
Highway freight volume	8.934	9.037	−0.103
	(0.804)	(0.915)	(0.346)
Rail passenger volume	5.575	6.091	-0.516^{***}
	(1.170)	(1.368)	(0.003)
Rail freight volume	5.990	6.463	-0.473^{**}
	(1.624)	(1.487)	(0.025)
Log population	5.887	5.762	0.125
	(0.615)	(0.886)	(0.198)
Log GDP	16.242	16.502	-0.260^{**}
	(0.621)	(0.932)	(0.011)
N (cities)	120	131	251

Notes: This table displays differences in baseline characteristics between cities that opened HSR early and those that opened late. Column (1) reports the mean and standard deviation for the early-opening group; Column (2) reports the mean and standard deviation for the late-opening group. Column (3) shows the difference (Early – Late) along with the p–value from a two-sample t-test. Although HSR opening is not randomly assigned,I find no significant pre-treatment differences in baseline congestion levels (both measures) or highway passenger and freight volumes. However, rail passenger and freight volumes do differ significantly, and cities opening HSR early also tend to have higher baseline GDP. Significance: $^{***}p < 0.01$, $^{**}p < 0.05$.

Table 5. Regression of Early Treatment on Pre-treatment Variables

	Dependent variable: Early (1 = Early)					
	OLS		Logit		Probit	
Variable						
Congestion length	−0.0575		−0.2323		−0.1449	
	(0.0530)		(0.2163)		(0.1345)	
Delay index		−0.1365		−0.5520		−0.3454
		(0.1033)		(0.4269)		(0.2647)
Constant	0.8481***	0.7059***	1.4052	0.8319	0.8763	0.5209
	(0.3024)	(0.1339)	(1.2365)	(0.5525)	(0.7681)	(0.3436)
Observations	251	259	251	259	251	259
R^2	0.0047	0.0067	0.0034	0.0048	0.0034	0.0048

Notes: This table reports the estimated relationship between base period (2013 Q2) congestion levels and the likelihood of early HSR opening, using OLS, logit, and probit models. First and second columns in each method show the effect of log congested highway length and congestion delay index, respectively, on the binary indicator for early opening. Robust standard errors are in parentheses. None of the congestion coefficients is statistically significant at conventional levels, indicating that pre-treatment congestion does not predict which cities open HSR early. Sample sizes and R^2/Pseudo-R^2 are reported at the bottom. Robust standard errors in parentheses. Significance: $^{***}p < 0.01$.

Table 6. HSR Connectivity and Traffic Mode Substitution

	Highway Passenger (1)	Highway Freight (2)	Rail Passenger (3)	Rail Freight (4)
HSR	-0.528***	0.245***	0.336***	-0.016
	(0.042)	(0.039)	(0.054)	(0.060)
Mean dep. var.	8.366	8.935	5.779	6.225
Observations	10012	9988	4448	4452
City FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
City Characteristics	Yes	Yes	Yes	Yes

Notes: This table reports the estimated effects of HSR connectivity on four logged outcomes: highway and rail passenger/freight volumes. All regressions include city and quarter fixed effects, as well as city-level controls (City Characteristics). Standard errors, clustered at the city level, are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 7. Heterogeneity in HSR Effects on Highway Congestion

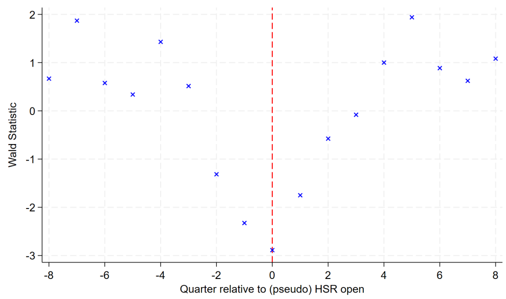
	Congestion length		**Delay index**	
	(1) High	(2) Low	(3) High	(4) Low
Panel A: City Distance				
HSR	-0.053***	-0.002	-0.145***	0.002
	(0.008)	(0.008)	(0.031)	(0.035)
Observations	3,450	3,447	3,510	3,483
Panel B: Highway Ratio				
HSR	-0.043***	-0.010	-0.079**	-0.057
	(0.009)	(0.008)	(0.030)	(0.038)
Observations	3,521	3,376	3,517	3,476
Panel C: HSR Opening Time (Early vs. Late)				
HSR	-0.046***	-0.014	-0.053**	-0.087**
	(0.008)	(0.009)	(0.026)	(0.039)
Observations	3,634	3,263	3,630	3,363
Panel D: Pre-Treatment Congestion				
HSR	-0.047***	-0.011	-0.123***	-0.001
	(0.010)	(0.007)	(0.034)	(0.028)
Observations	3,585	3,312	3,539	3,454
Fixed Effects				
City-year FE:	✓	✓	✓	✓
Quarter FE:	✓	✓	✓	✓

Notes: These forest plots display the heterogeneity in the impact of HSR connectivity on highway congestion. The four panels correspond to different grouping criteria: Panel A: City Distance (High vs. Low); Panel B: Highway Ratio (High vs. Low); Panel C: HSR Opening Time (Early vs. Late); and Panel D: Pre-Treatment Congestion (High vs. Low). All regressions include city-year fixed effect and quarter fixed effect. Standard errors (in parentheses) are clustered at the city level. *** $p < 0.01$, ** $p < 0.05$, and * $p < 0.1$.

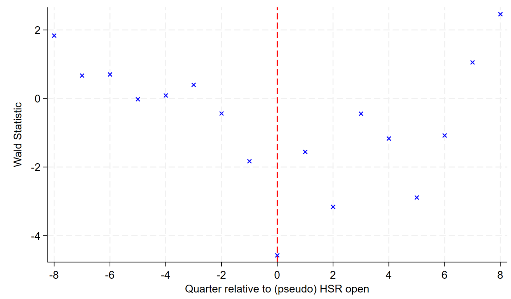
A Appendix Tables and Figures

A.1 Placebo Event-Study Wald Statistics

Panel A: Delay Index



Panel B: Congested Length



Notes: This figure shows the Wald statistics for the placebo event-study coefficients of the Treat×Post interaction from 8 periods before to 8 periods after the actual HSR opening. Panel A shows results for the congestion delay index, and Panel B for the log congested highway length. Largest Wald statistic occurs at the true opening date, providing evidence of the validity of our identification.

Appendix Table A1. Modes Substitution of road by City Distance to Highway

	Passenger		Freight	
	(1) High	(2) Low	(3) High	(4) Low
HSR	-0.402***	-0.659***	0.328***	0.158***
	(0.059)	(0.053)	(0.061)	(0.046)
Constant	8.472***	8.654***	8.774***	8.922***
	(0.020)	(0.021)	(0.021)	(0.018)
Mean of dep. var.	8.34	8.40	8.89	8.98
Observations	5,000	4,984	2,240	2,260
City FE	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓

Notes: This table reports the heterogeneous effects of HSR connectivity on highway road passenger and freight volumes, stratified by whether a city's population centroid is farther than (High) or closer than (Low) the median distance to the highway network. Dependent variables are the log of highway passenger volume (cols. 1–2) and log of highway freight volume (cols. 3–4). All regressions include city and quarter fixed effects; standard errors clustered at the city level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Appendix Table A2. Modal Substitution of railway by City Distance to Highway

	Passenger		Freight	
	(5) High	(6) Low	(7) High	(8) Low
HSR	0.328***	0.343***	0.001	-0.032
	(0.090)	(0.062)	(0.110)	(0.055)
Constant	5.658***	5.781***	6.118***	6.340***
	(0.013)	(0.013)	(0.016)	(0.011)
Mean of dep. var.	5.71	5.85	6.12	6.33
Observations	5,012	5,004	2,208	2,192
City FE	✓	✓	✓	✓
Quarter FE	✓	✓	✓	✓

Notes: This table reports the heterogeneous effects of HSR connectivity on rail passenger and freight volumes, stratified by population-centroid distance to the highway network. Dependent variables are the log of rail passenger volume (cols. 5–6) and log of rail freight volume (cols. 7–8). All regressions include city and quarter fixed effects; standard errors clustered at the city level are in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.