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Expressways, GDP, and the environment: The case of China

Guojun He[a,1,*], Yang Xie[b,1], Bing Zhang[c,d,1]

[a] Division of Social Science, Division of Environment and Sustainability, and Department of Economics, Hong Kong University of Science and Technology, Hong Kong SAR, China
[b] Department of Economics, University of California, Riverside, USA
[c] State Key Laboratory of Pollution Control & Resource Reuse, School of Environment, Nanjing University, China
[d] Population Research Institution, School of Business, Nanjing University, China

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ABSTRACT

In a matched difference-in-differences setting, we show that China's expressway system helps poor rural counties grow faster in GDP while slowing down growth in the rich rural counties, compared with the unconnected rural counties. This heterogeneity cannot be explained by a rich set of county characteristics related to initial market access, factor endowments, and sectoral patterns, but is consistent with the Chinese government's development strategy that more developed regions should prioritize environmental quality over economic growth, while poor regions pursue the opposite. We further investigate the environmental outcomes and find that the expressway connection indeed makes poor counties adopt dirtier technologies, host more polluting firms, and emit more pollution than the unconnected counties do, contrary to what happens to the rich connected counties. These results imply that recognizing the GDP–environment trade-off can help explain the full implications of infrastructure investment and other development initiatives.

1. Introduction

With a vast territory and the world's largest population, China depends heavily on its inter-city expressways (controlled-access highways) to facilitate mass within-country trade. From its inception in the 1980s, China's national expressway network, officially known as the National Trunk Highway System, had expanded to more than 111,000 km by 2015, making it the world's largest expressway system by length.

In this paper, we estimate the impact of a region's connection to this large-scale transport network during its expansion on local economic development, relative to unconnected regions, and we explore the channels through which the relative impact happens. We use comprehensive data of more than 1600 counties in each year during the 13 years from 2000 to 2012, which is, to our knowledge, the largest, most disaggregated, longest, most frequent, and most recent in the literature. To achieve better identification, we leave out the provincial capitals and metropolitan city centers, which the expressways connect by design, and focus on peripheral counties that gained access to expressways because they happened to be located on routes between metropolitan cities. We then compare the economic performance between connected and unconnected counties in a matched difference-in-differences (DiD) setting. We find that, when compared with unconnected counties, expressway connection on average has a statistically insignificant, slightly negative impact on connected counties' real GDP or per capita GDP.

As this estimate is inconsistent with the long-held belief of the Chinese government that providing transport infrastructure can effectively promote economic growth in the peripheral and poor regions (e.g., State Council of China, 2013, p. 3), we explore potential heterogeneity in the relative impact of expressway connection across initial levels of per capita GDP. We find that this slightly negative average relative impact masks significant heterogeneity: expressway connections cause initially poor peripheral counties to grow faster, while causing initially rich peripheral counties to grow more slowly, compared with the unconnected peripheral counties. This heterogeneity is robust to a variety of alternative specifications, such as controlling for different fixed effects and allowing the GDP trends of the counties to vary across different levels of initial income, and it is also robust to using an instrumental variable approach, adopting satellite nightlight density to measure development outcomes, and testing possible spillover effects.

We then search for factors that could help explain this heterogeneity across initial income levels. Guided by theories based on increasing returns to scale (e.g., the home market effect, as in Krugman, 1980, 1991;

* Corresponding author.
E-mail addresses: gjhe@ust.hk (G. He), yang.xie@ucr.edu (Y. Xie), zhangb@nju.edu.cn (B. Zhang).
[1] All the authors have contributed equally to the article.

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Helpman and Krugman, 1985; Faber, 2014) and those based on comparative advantage (e.g., the pollution haven hypothesis, as in Copeland and Taylor, 1994; survey by Copeland and Taylor, 2004), we examine a rich set of county characteristics that should influence the home market effect or indicate a county's comparative advantage, such as the distance from the focal county to its nearest metropolitan city, endowments of land, population, and capital, and the initial sectoral pattern of the local economy. Empirical results show that none of these variables can explain the heterogeneity in the relative impacts of the expressway connection across initial income levels. We thus infer that this heterogeneity can be driven by factors that have been largely overlooked by existing discussions.

To explain our empirical findings, we emphasize the environmental concerns of local governments. Pollution is usually viewed as a necessary input in economic production (e.g., Pethig, 1976), and society needs to balance the environment and economic growth (e.g., Arrow et al., 1995). In our context, the State Council of China (2005) explicitly required the rich regions to prioritize improving environmental quality, while directing the poor areas to promote industrialization and urbanization. It is thus possible that the lower trade cost brought by expressways would help a poor economy enjoy higher GDP at the cost of the environment, while inducing a rich economy to sacrifice more GDP for better environmental quality.

We then examine this possibility by analyzing county-level panel data of local polluting emissions for the same period. We find that the rich peripheral counties indeed become less polluted after the expressway connection, while the poor peripheral counties see greater levels of emissions afterward, compared with the unconnected peripheral counties. Further investigation reveals that expressway connections cause the poor peripheral counties to host more polluting firms, adopt more pollution-intensive technology, and accelerate industrialization, compared to the unconnected peripheral counties, while the opposite happens in the rich peripheral counties. These results are consistent with the hypothesis that poor and rich counties in China make use of expressway connections in different ways, pursuing different development objectives, and that the GDP–environment trade-off is important in understanding the full implications of transport infrastructure improvement.

The rest of the paper unfolds as follows. In Section 2, we review the relevant studies and compare our findings with others. Section 3 describes the empirical setting and discusses our empirical strategy. Section 4 introduces the data and provides descriptive statistics. Section 5 estimates the relative impacts of expressway connection on GDP and per capita GDP. Section 6 explores the nature of the observed heterogeneity. In Section 7, we provide more evidence on the relative impact of expressway connection on local emissions, production cleanliness, the distribution of polluting production, and local sectoral patterns, from which the explanation about the GDP–environment trade-off emerges. Section 8 discusses policy implications and concludes with directions for future research.

2. Link to the literature

The literature on the economic consequences of transport infrastructure improvement has provided important insights for development initiatives (see Redding and Turner, 2015 for a recent review).[2] Narrowing down the focus to China, three papers by Faber (2014), Baum-Snow et al. (2017), and Baum-Snow et al. (2020) have estimated the relative impacts of expressways by comparing jurisdictions that are better connected to the expressway network with those that have less access.[3] These three papers focus on jurisdictional units that are different from each other and our paper. For illustrative purpose, we hypothesize in Fig. 1 a scenario in which Prefecture P_A's central city CC_A enjoys one more access to the expressway network than Prefecture P_B's central city CC_B does, and that this access also brings one peripheral county PC_{A1}, instead of another peripheral county PC_{A2}, into the network. We will refer to these hypothetical jurisdictions when discussing the relationship between our paper and Faber (2014), Baum-Snow et al. (2017), and Baum-Snow et al. (2020).

Both our paper and Faber (2014) compare the peripheral counties that are connected to the expressway system with those that are not, i.e., PC_{A1} versus PC_{A2} in Fig. 1. Faber (2014) instruments a county's expressway connection by a constructed variable that is based on a hypothetical network that would connect all targeted cities at the least cost. He finds that, compared with unconnected peripheral counties, the expressway connection reduced the connected peripheral counties' local GDP growth between 1997 and 2006. He interprets the result as supporting the home market effect: if one takes the peripheral counties as the periphery and the central cities as the core, resources in the connected peripheral counties, such as PC_{A1}, would be attracted to the central cities, such as CC_A, making the growth in the connected peripheral counties slower than in the unconnected peripheral counties, such as PC_{A2}.

Consistent with Faber (2014), we find that the annual relative impact of expressway connection on peripheral counties' GDP growth from 2000 to 2012 is negative on average, and the cumulative average relative impact becomes significantly negative four or five years after the connection. Going a step further, we find that behind this average negative relative impact are a negative relative impact on the rich peripheral counties and a positive relative impact on the poor peripheral counties, and we show that this heterogeneity cannot be explained by certain variables that are supposed to influence the home market effect. We would not have reached these findings if we, as in Faber (2014), had omitted the exploration of the heterogeneity in the relative impact.

Different from Faber (2014) and our paper, Baum-Snow et al. (2017) focus on each prefecture's central city, i.e., they compare CC_A with CC_B in Fig. 1. Instrumenting the stock of radial expressways near a central city in 2010 by the historical road stock in 1962, Baum-Snow et al. (2017) find that the 2010 radial expressways have a negative relative impact on a central city's 1990–2010 population growth. Baum-Snow et al. (2017) interpret the result as the expressways helping relocate the population out of the central cities to their surrounding counties. This interpretation goes against the home market effect if one takes the central cities as the core and the peripheral counties as the periphery in the core-periphery relationship, but it is consistent with our result because both suggest that, below the prefectural level, expressways have a negative relative impact on a more developed jurisdiction and a positive relative impact on a less developed one.

Baum-Snow et al. (2020) shift the focus from jurisdictions below the prefectural level up to the prefectures, i.e., they compare P_A with P_B in Fig. 1. As a result, their findings are not readily comparable to those of Faber (2014) or Baum-Snow et al. (2017), or ours. Using the same instrument as in Baum-Snow et al. (2017), Baum-Snow et al. (2020) find that a greater stock of expressways in 2020 has a positive impact on the

[2] For examples, see Fernald (1999), Chandra and Thompson (2000), Holl (2004), Baum-Snow (2007), Michaels (2008), Datta (2012), Duranton and Turner (2012), Duranton et al. (2014), Baum-Snow (2014), Donaldson and Hornbeck (2016), Frye (2016), Ghani et al. (2016), Jaworski and Kitchens (2016), Alder (2017), Aggarwal (2018), Donaldson (2018), Okoye et al. (2019), Asher and Novosad (2020), Bird et al. (2020), and De Soyres et al. (2020).

[3] Examples that focus on China, besides the ones that concern expressways and are discussed below, include Banerjee et al. (2020), Zheng and Kahn (2013), Qin (2017), and Alder and Kondo (2018).

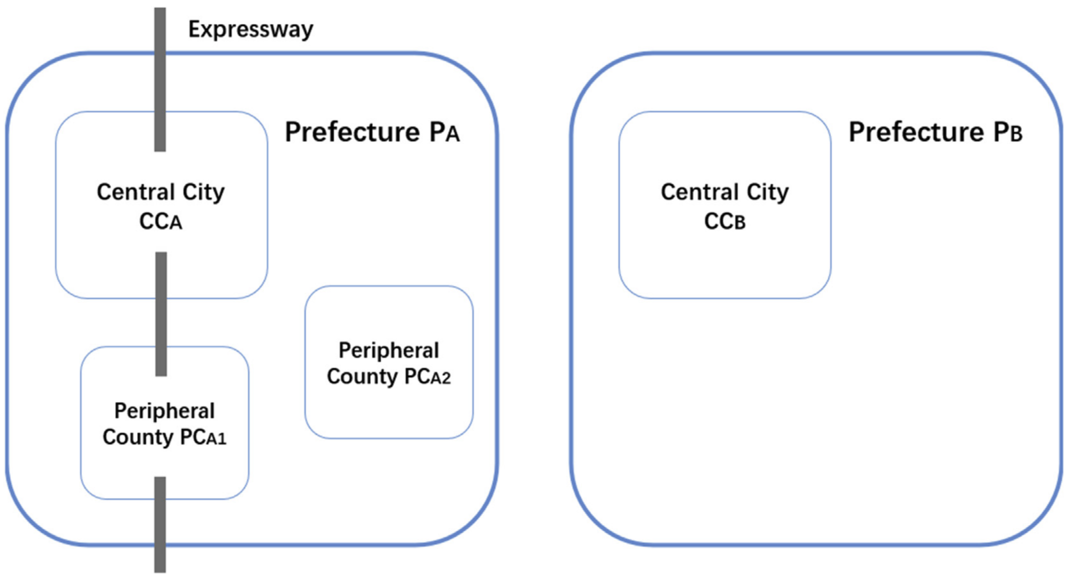

Fig. 1. Different Comparisons in the Literature and the Current Paper.
Notes: Our paper and Faber (2014) compare PC_{A1} with PC_{A2}; Baum-Snow et al. (2017) compare CC_A with CC_B; Baum-Snow et al. (2020) compare P_A with P_B.

prefectures' 2010 GDP, population, and 1990–2010 population growth if the prefectures are regional population centers, while the relative impact becomes negative if they are not.[4]

Combining the results in Baum-Snow et al. (2017), Baum-Snow et al. (2020), and our paper, we can obtain a more comprehensive understanding of the relative impacts of expressways in China. First, when comparing jurisdictions below the prefectural level, expressways in China alleviate *within-prefectural* disparity, as suggested by both Baum-Snow et al. (2017) and us. Second, when comparing different prefectures, expressways reinforce *cross-prefectural* disparity, as suggested by Baum-Snow et al. (2020). Both of these observations are consistent with the objectives laid out in the plan of the expressway network (State Council of China, 2004): to promote "coordinated development of the regional economy," i.e., to reduce economic disparity within a local region, and at the same time "facilitate economies of scale among highly developed prefectural cities and provincial capitals."

This paper also contributes to the literature on the impacts of transport infrastructure improvement on environmental outcomes (e.g., Chomitz and Gray, 1996; Nelson and Hellerstein, 1997; Pfaff, 1999; Cropper et al., 2001; Deng et al., 2011; Chakravorty et al., 2018). Our finding of the heterogeneity in the relative impact on local GDP and our exploration of this heterogeneity motivate us to consider the trade-off between development and the environment. Environment-related outcomes, such as emissions, cleanliness of firms and technology, and industrial sectoral patterns, help explain our results about development outcomes. This integrated way of analyzing both the development and environmental outcomes is rare in the literature, where the development and environmental implications of transport infrastructure improvement are often analyzed separately.

Finally, our paper adds to the literature on how political incentives affect environmental policies and outcomes (e.g., List and Sturm, 2006; Burgess et al., 2012). In the Chinese context, where economic development is often achieved at a high environmental cost, Kahn et al. (2015) and Chen et al. (2018) provide evidence that career concerns push local officials to make greater efforts to reduce local pollution; Jia (2017) documents the positive correlation between local pollution and local officials' political connections, consistent with the hypothesis that political connections increase the political return of a marginal increase in pollution; and He et al. (2018) show that politically-motivated officials are more likely to enforce tighter regulations after environmental quality becomes a criterion for political evaluation. Adding to the literature, our paper suggests that recognizing the political incentives of local governments in China can help us understand the otherwise puzzling heterogeneity in both the economic and environmental performance of local economies after the significant improvement in transport infrastructure.

3. Empirical setting

3.1. Expansion of China's expressway network

The expansion of China's national expressway network took place in several stages. The first expressway in China, constructed in 1984, connected two northern Chinese cities, Shenyang and Dalian. In 1992, the State Council of China approved the "5–7" expressway construction plan, which included five north–south and seven east–west expressways with a total length of over 35,000 km. The objective of the "5–7" network was to connect all provincial capitals and cities with an urban population of over 500,000 by 2020, and the network was completed in 2007, 13 years ahead of schedule.

In 2004, the State Council approved the construction of a larger expressway network known as the "7–9–18" network, which comprises seven radial expressways connecting Beijing with other major cities, nine north–south expressways, and 18 east–west expressways. The "7–9–18" expressway network links all cities with an urban population of more than 200,000, major tourist cities, port cities, and expressway and railway hubs. The new target was achieved in 2011, nine years ahead of schedule.

Many peripheral counties lying between major cities were also connected during this expansion. Our empirical strategy exploits this feature and compares the economic outcomes between connected and unconnected counties before and after expressway construction. More specifically, the treatment group consists of counties that were not targeted by the State Council of China (2004)'s *National Expressway Network Plan* but were connected between 2000 and 2012 simply because they were

[4] This result is consistent with simulations of two new economic geography models in Roberts et al. (2012) and Bosker et al. (2018), where China's expressway system is found to sustain the income gap between richer and poorer prefectures when compared with a counterfactual without the system.

located on expressway routes between metropolitan cities.[5] Unconnected counties serve as the control group. All the urban districts in the targeted cities are excluded from subsequent analysis because their expressway connections are endogenous.[6] As a result, all our analyses are restricted to studying non-targeted peripheral and mostly rural regions. In Fig. 2, we present two maps of China, for 2000 and 2010, where the targeted cities (all urban districts in a prefecture), connected counties, and unconnected counties are denoted by different colors.

3.2. Econometric model

We estimate the effect of the expressway connection on connected counties relative to unconnected counties using a generalized DiD approach:

$$y_{i,t} = \alpha + \beta^* Connect_{i,t} + \rho_t + \mu_i + \varepsilon_{i,t}, \tag{1}$$

where $y_{i,t}$ is real GDP or per capita GDP for county i in year t; $Connect_{i,t}$ is a dummy indicator that equals 1 if county i is connected in year t, and 0 if otherwise; ρ_t is a time effect common to all counties in period t; μ_i is a time-invariant effect unique to county i; and $\varepsilon_{i,t}$ is an error term independent of μ_i and ρ_t. We take the logarithms of the dependent variables so that the estimated coefficient represents the percentage change. The coefficient of interest is β.

To estimate the heterogeneous relative impacts of expressway connection, we introduce the interaction between the treatment dummy and the initial income in the regression and estimate the following equation:

$$y_{i,t} = \alpha + \beta * Connect_{i,t} + \gamma * (X_{i,2000} * Connect_{i,t}) + \rho_t + \mu_i + \varepsilon_{i,t}, \tag{2}$$

where $X_{i,2000}$ is the logarithm of real per capita GDP of county i in the year 2000, and γ is the coefficient of the interaction. The coefficient of interest is γ.

3.3. Identifying assumptions

The routing of expressways is determined by the central and provincial governments. Although counties between major cities were not explicitly targeted by the *National Expressway Network Plan* (State Council of China, 2004), we cannot assume that routings were created randomly. Because the decision-making process is not entirely transparent, a reasonable concern is that the routing choices may not be orthogonal to unobservable factors that may affect the outcomes.

There are two hypotheses regarding the central government's routing decisions. The first is that the central government connects counties based on time-invariant characteristics such as the geographic features of a region, the cost of building expressways, and the regional economic and political importance of a county.[7] However, this type of endogeneity does not threaten our identification. In the DiD setting, the county fixed effects control for all time-invariant factors that may affect the likelihood of a county being connected; the year fixed effects further control for common shocks that affect all counties (such as national policies) in each year. Thus β can still be identified as long as the treatment group and the control group follow parallel pre-treatment trends.

The second hypothesis is that the central government connects counties in response to local economic or political shocks. For example, would the government intentionally reroute an expressway to connect a county because it experienced a negative income shock in the previous year? If so, this would threaten our identifying assumption and make pre-treatment trends not parallel, but we believe that this scenario is highly unlikely to be true, because the National Expressway Network was planned years before any county was connected. Moreover, as the central government did not change the routings before construction, there is no evidence that counties could manipulate expressway connections in their favor to cope with temporary economic shocks. Finally, both the "5–7" network and the "7–9–18" network were completed years ahead of schedule. A reasonable assumption would be that a peripheral county did not have *ex-ante* information on the exact time when it would be connected. Appendix Table S2 also shows that the relative impact of expressway connection was mostly negligible in the year of connection, which suggests little evidence that the connected counties gamed around the timing of their connections. Allowing for all these considerations, the expressway connection to a specific county in a specific year is likely to be exogenous, conditional on the county and year fixed effects.

The endogeneity concern can be further alleviated by combining the DiD estimator with matching: for each eventually-connected county in our data, we match it with an eventually-unconnected county that is in the same province and has the most similar level of real per capita GDP in 2000; then we apply the DiD estimators to the matched sample. While our main results are similar using both matched and unmatched samples, conducting the matching before applying the DiD estimators has two merits. First, the test results for the parallel trends assumption, which we will introduce below, are improved using the matched sample, since the standard errors are reduced. Second, it is more proper for us to interpret the income heterogeneity, because connected and unconnected counties in the matched sample are more comparable, sharing a common support in terms of initial income. Therefore, in subsequent analysis, we focus on the results when we use this matched sample, reserving for appendices the results when we use the unmatched sample. That said, the unmatched-sample and matched-sample yield similar results.

More formally, we can test the parallel-trend assumption using an event study approach, following Jacobson et al. (1993): we generate a set of lead- and lag-year indicators of the actual expressway access as independent variables in the regression and test whether the coefficients of the leads are statistically significantly different from zero.[8] Details of the tests are discussed in Appendix I. As will be discussed in Section V, we fail to reject the hypothesis that the connected and unconnected counties follow similar trends before the connected counties become connected.

The final threat to the identification of the DiD approach is selection based on expectation. For example, although the connected and unconnected counties looked similar before expressways were constructed, it is not implausible to speculate that future growth potential or other considerations might determine expressway routing and thus selection into the treatment and control groups. Were this type of selection significant,

[5] Our county-level panel data starts from 2000, and about 15% of the counties were connected before 2000. These counties are not included in our empirical analysis for two reasons. First, they provide no variation in treatment status, so they do not help us identify the treatment effects. Second, since we do not know exactly when they were connected before 2000, we would not be able to properly include the lead and lag indicators of their connection in the parallel-trend tests, which we will discuss below.

[6] The targeted cities include cities with a population of over 200,000, tourist cities, port cities, and expressway and railway hubs. The *National Expressway Network Plan* (2004) referred to targeted cities as the "main controlling nodes." The list of targeted cities is reported in Appendix Table S1. Appendix Fig. 1 shows the targeted cities on the map and draws the expansion of China's national expressways from 1992 to 2010. A (prefectural) city typically includes a few urban districts and a dozen rural counties. If a prefectural city is targeted by the plan, we treat all its urban districts as being targeted and exclude them from subsequent analysis.

[7] In our unmatched sample, before the connected counties were connected, they were in general richer than unconnected counties (see Table 1). This pattern is also documented by Faber (2014), who investigates the early stages of China's expressway construction.

[8] Our yearly panel data allow us to test the parallel-trend assumption more systematically than Faber (2014) does. Limited by data availability, Faber (2014) only shows that expressway connection does not affect the growth in local government revenue between the two years 1990 and 1997, which is weak suggestive evidence for the parallel trends assumption.

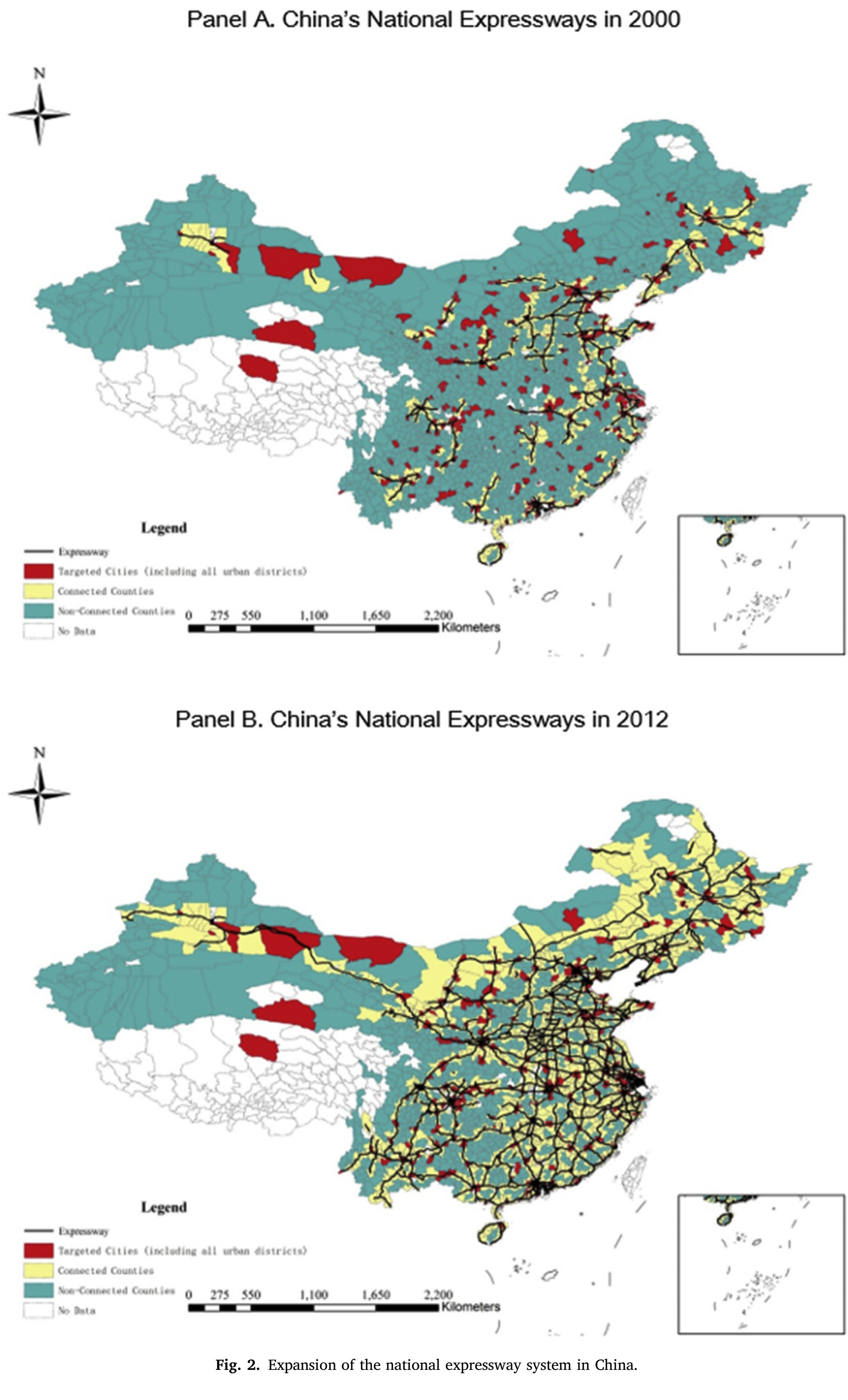

Fig. 2. Expansion of the national expressway system in China.

the parallel-trend test would help little in justifying our identification strategy. To address this type of potential selection, in one robustness check, we estimate the relative impacts of expressway connection on GDP using an instrumental variable approach in the same spirit as Banerjee et al. (2020) and Faber (2014). We first construct straight lines that connect each pair of the targeted cities, and then construct a variable for each county as follows: if the county is connected by one of the hypothetical straight lines, the variable is equal to 1; if otherwise, it is equal to 0. We then use this variable as the instrumental variable of the actual expressway connection. The outcome variable in this instrumental variable regression is the change in real GDP or per capita GDP between 2000 and 2012.

4. Data and summary statistics

4.1. GDP and socioeconomic data

We collect county-level GDP and other socioeconomic data of 2000–2012 from the CEIC database and various statistical yearbooks in China, including provincial yearbooks, *China City Statistical Yearbooks,* and *China County Statistical Yearbooks.*

Our outcome variables are real GDP or GDP per capita at the county-year level. We use these real measures since they are more consistent than the nominal measures with the model that we will propose in Appendix III, where GDP is interpreted as a composite of goods. That said, all the findings still hold when nominal GDP measures are used. To get data for real GDP, we deflate the nominal GDP by province-year CPI from the National Bureau of Statistics of China, taking Beijing-2000 as the base province-year. We also use satellite nightlight densities to measure the economic development of different counties in later robustness checks, as nightlight density is not confounded by the price levels.[9] We collect raw satellite data from the Defense Meteorological Satellite Program, and calculate the nightlight density for all the counties by aggregating satellite images from the daily grid level to the yearly county level.

4.2. Expressway expansion data

Historical geographic information systems (GIS) data on China's National Expressway Network were collected from the PR China Administrative Spatio-Temporal Expressway Database (STED) from the ACASIAN Data Center, Griffith University. The database contains data on China's expressway routes for 1992, 1993, 1998, 2000, 2002, 2003, 2005, 2007, and 2010. By combining the STED database with county-level GIS data, we can identify which counties were connected in which year.[10]

4.3. Pollution data

To understand the channels of the GDP results, we also collect county-level emissions data from China's Environmental Survey and Reporting (ESR) database. The ESR database is maintained by the Ministry of Environmental Protection of China. It is used to monitor the polluting activities of all important polluting sources, including heavily polluting industrial firms, hospitals, residential pollutant discharging units, hazardous waste treatment plants, and urban sewage treatment plants.

We use the ESR data from 2000 to 2012 in this study. During this period, the monitored polluting sources in total contributed 85% of the total emissions of major pollutants in each county. Monitored polluting sources are required to report their environmental performance to county-level Environmental Protection Bureaus (EPBs) each year. Local EPBs then verify the data and estimate emissions of major pollutants from unmonitored plants based on their total industrial output. The overall emission measures for major pollutants in each county are constructed by summing emission levels reported by monitored plants and estimated emission levels from unmonitored plants. The micro-level emissions data used in this study had been kept confidential for many years before it recently became conditionally open to some researchers.[11]

Emissions degrade environmental quality. Major pollutants in the ESR database include chemical oxygen demand (COD), ammonia nitrogen (NH_3–N), sulfur dioxide (SO_2), and nitrogen oxides (NO_x). In our analysis, we focus on the COD emissions. The COD is a widely-used water quality indicator that assesses the effect of discharged wastewater on the water environment by measuring the amount of oxygen required to oxidize soluble and particulate organic matter in water. Higher COD levels mean a greater amount of oxidizable organic material in the sample, which reduces dissolved oxygen levels. A reduction in dissolved oxygen can lead to anaerobic conditions, which are deleterious to higher aquatic life forms.

One reason for our focus on COD emissions is that COD is the primary measure of water pollution adopted in China.[12] Another reason is that almost all key pollution sources and industries produce and report COD emissions (Lin, 2013; Sinkule and Ortolano, 1995), whereas other pollutants, such as SO_2, tend to be concentrated in a few industries that are tightly controlled by large state-owned enterprises in certain areas of China, rather than by local governments at the county level. As we will introduce below, our explanation focuses on the general environment–GDP trade-off made by the county government, so we opt for the COD measures in empirics.

In practice, we focus on total COD emissions and per capita COD emissions. Total COD emissions are the sum of COD emitted by the key polluting plants and the estimated COD emitted by other polluting plants in each county. Per capita COD emissions are calculated by dividing the total COD emissions by the county's population. We also check the robustness of our results using COD emissions only from key polluting plants and supplement our analysis by further discussing the results of other emissions measures, such as ammonia nitrogen and SO_2.

4.4. Descriptive statistics

We match all the datasets at the county level from 2000 to 2012, during which the national expressway network expanded significantly. By 2012, more than 50% of counties were connected.

In Table 1, we summarize the descriptive statistics of GDP measures in 2000 and 2012 separately for the matched and unmatched samples. From 2000 to 2012, real per capita GDP of our sampled counties increased nearly fivefold. We observe that, in the unmatched sample, the eventually connected counties were generally richer than the unconnected counties in 2000; in the matched sample, the connected and unconnected counties have more similar levels of initial GDP and per capita GDP. Fig. 3 further plots the distribution of per capita GDP in the matched and unmatched samples, respectively. It shows that the connected counties and the matched, unconnected counties share a common support of initial income.

[9] For more discussion on satellite nightlight data, see Chen and Nordhaus (2011), Henderson et al. (2012), and Donaldson and Storeygard (2016).

[10] Because the STED data have gaps over years, we do not know exactly when a county was connected for 12% of the connected counties in the sample. For these counties, we must interpolate the treatment status. Our empirical findings are not sensitive to the way we interpolate. Details on identifying the treatment status of each county-year are given in Appendix II.

[11] More details on the data are given in Lin (2013), Cai et al. (2016), and Wu et al. (2017).

[12] For example, COD abatement is used by the Chinese central government as a key performance indicator for assessing local government efforts at environmental protection. In China's 11th Five-Year Plan (2006–2010), COD was used as a primary criterion (the other being ammonia nitrogen) for setting national abatement targets and performance appraisals.

Table 1
Summary statistics of sampled counties.

Variable	Un-Matched Sample			Matched Sample		
	Overall	Connected	Un-Connected	Overall	Connected	Un-Connected
GDP (million yuan, 2000)	2978	3416	2455	3067	3265	2868
	(6910)	(3241)	(9588)	(4616)	(2942)	(5827)
GDP (million yuan, 2012)	12,925	14,881	10,570	13,249	14,098	12,400
	(27,918)	(15,408)	(37,723)	(19,604)	(14,031)	(23,911)
GDP per capita (yuan, 2000)	5710	6231	5081	6084	6087	6082
	(5114)	(4567)	(5646)	(4287)	(3946)	(4608)
GDP per capita (yuan, 2012)	26,654	28,052	24,961	28,516	27,472	29,559
	(29,377)	(27,713)	(31,210)	(30,823)	(28,432)	(33,051)
# of Counties	1646	897	749	1614	807	807

Notes: County-level nominal GDP and population data are collected from provincial statistical yearbooks, China City Statistical Yearbooks, China County Statistical Yearbooks and China Economic Database from CEIC (www.ceicdata.com). GDP data are deflated over time and across regions using the province-year CPI from the National Bureau of Statistics of China, taking Beijing-2000 as the base province-year. Standard deviations are reported in the parentheses below the means.

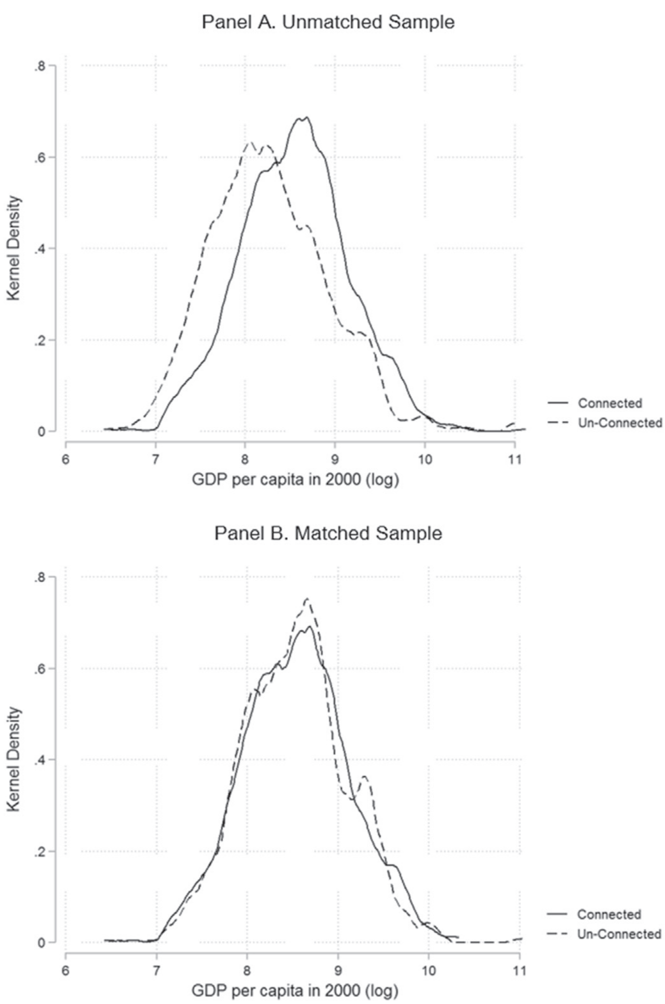

Fig. 3. Distribution of per capita GDP of the connected and unconnected counties in 2000.
Notes: GDP data are deflated, where Beijing-2000 is the base province-year.

5. Relative effects of expressway connection on GDP

5.1. Average relative effect of expressway connection

In Table 2, we report the average effect of the expressway on connected counties' real GDP and per capita GDP relative to the unconnected counties using the matched sample. Our baseline results are presented in Columns 1 and 4, in which only county fixed effects and year fixed effects are included in the regressions. Then we test the robustness of these results by adding different controls. In Columns 2 and 5, we add provincial trends; in Columns 3 and 6, instead of controlling for year fixed effects, we include province-year fixed effects.

Table 2
The impacts of expressways on connected counties' GDP relative to unconnected counties'

	GDP (million yuan, log)			Per capita GDP (yuan, log)		
	(1)	(2)	(3)	(4)	(5)	(6)
Expressway	−0.02	−0.02	−0.02	−0.02	−0.02	−0.02
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
County FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	N	Y	Y	N
Provincial Trends	N	Y	N	N	Y	N
Province-Year FE	N	N	Y	N	N	Y
Obs.	13,440	13,440	13,440	13,347	13,347	13,347
R^2	0.87	0.90	0.91	0.86	0.89	0.90

Notes: This table estimates the relative impacts of expressway connection on GDP measures by comparing the connected and unconnected counties in the matched sample. GDP data are deflated, where Beijing-2000 is the base province-year. We probe the robustness of the estimates' accuracy by clustering the standard errors at three different levels: county level, province level and county and province-year level (multi-way clustering suggested by Cameron et al., 2011). These standard errors are respectively reported in the parentheses below the estimated coefficients. Our preferred specification clusters standard errors at the county level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

We find that the estimated coefficients are negative and stable in all regressions. We further probe the robustness of the estimates' accuracy by clustering the standard errors at three different levels: the county level, the province level, and the county and province-year level (multi-way clustering suggested by Cameron et al., 2011) to deal with different potential correlations in the error term. Clustering the standard errors at the county level controls for arbitrary correlations across different years for the same county; clustering at the province level controls for arbitrary correlations within a province; clustering at both the county and province-year levels accounts for correlations across different years within the same county and correlations across all counties in the same province-year. As shown in Table 2, we find that the significance levels are unaffected by different approaches to clustering standard errors.

We then test the parallel-trends assumption following Jacobson et al. (1993). The estimated coefficients of the leads and lags of the treatment dummies are plotted in Fig. 4. It shows that, before the connected counties were connected to the expressway system, they and the unconnected counties had similar GDP trends. This suggests that the timing of expressway connection is unlikely to be endogenous to county GDP, as

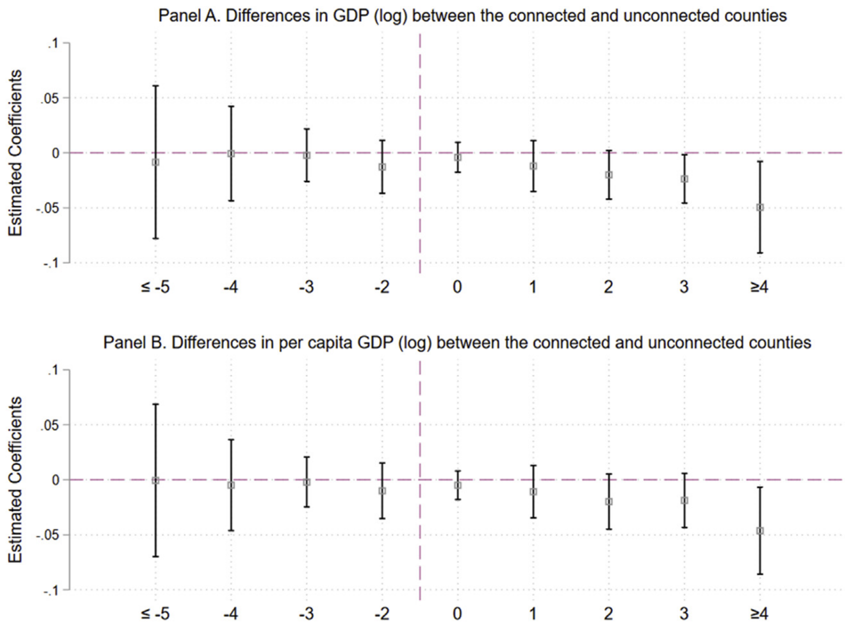

Fig. 4. Tests for Parallel Trends.
Notes: The figure plots the estimates and the 95% confidence intervals of the coefficients in the event study regressions following Jacobson et al. (1993), where the estimation is based on the matched sample. GDP data are deflated, where Beijing-2000 is the base province-year. The horizontal axes denote years before or after the expressway connection, where the year just before the connection year is the benchmark. See Appendix I for more details.

the coefficients of the leads and lags are statistically insignificant in the first couple of years before or after the year of connection.

In the long run, we also see that the lag term becomes negative and statistically significant in the longer run.[13] This implies that the negative relative impact of expressways on GDP takes time to materialize.

5.2. Heterogeneous relative effects of expressway connection

In this section, we explore the heterogeneity in the relative effects of expressway expansion on GDP across different levels of initial income. The baseline results are reported in Columns 1 and 4 of Table 3. The estimated coefficients of the expressway connection dummy are positive and statistically significant, while the coefficients of the interaction between the connection dummy and the initial income are negative and statistically significant at the 1% level. In other words, the relative impact of expressway access on GDP is more negative in initially richer counties than in initially poor counties.

Using information from the distribution of per capita GDP in 2000, we can further predict the relative impacts of expressway connection at different initial income levels. In Fig. 5, we plot the predicted relative impacts with their 95% confidence intervals, based on estimates in Columns 1 and 4 of Table 3. In Fig. 5, we observe that expressway connection positively affected real GDP in the poor peripheral counties (statistically significant for the poorest 25%) and negatively affected real GDP in the rich peripheral counties (statistically significant for the richest 40%), compared with the unconnected peripheral counties.

We check the robustness of the findings in several different ways. First, we control for provincial time trends in the regressions (Columns 2 and 5 of Table 3) and find that the conclusions remain the same. Second, instead of including year fixed effects dummies, we include province-year fixed effects in the regressions in Columns 3 and 6. The province-year fixed effects account for annual shocks that are common to all counties in a province, for example, business cycles and differential trends and policies across provinces. The relative effect is thus identified by comparing the outcomes of two counties in the same province in the same year. We find that, even in this case, the relative effect of expressway connection has a strong heterogeneity. These findings are also robust to different ways of clustering the standard errors.

Second, instead of interacting the expressway dummy with the continuous measure of initial per capita GDP, we construct an income group indicator that is equal to one if a county is in the high-income group in 2000.[14] This allows us to include income group-specific year fixed effects so that poor counties and rich counties can have different dynamics of GDP growth, independent of expressway connection. The regression results are summarized in Table 4. In Columns 1 and 5, we include county fixed effects, year fixed effects, and provincial trends. Columns 2 and 6 control for province-year fixed effects. Columns 3 and 7 allow poor counties and rich counties to grow with different trends; and, finally, in Columns 4 and 8, we include year fixed effects separately for the two income groups.[15] These regressions again confirm that expressway connection has highly heterogeneous relative impacts on the local economy.

Third, as we do not have price indices at the county-year level, there is a concern that our deflation factor, which is based on province-year variations, could be mismeasured. To address this issue, we use the satellite nightlight density as the outcome variable, which measures local economic activities and is independent of the price level. We report the corresponding results in Appendix Table S3. Again, all the results remain similar.

Next, we use an indicator variable about whether the focal county is on one of the straight lines that connect different pairs of targeted cities

[13] In Appendix Table S2 we summarize the regression results. While Faber (2014), Baum-Snow et al. (2017), and Bosker et al. (2018) all examine a long difference of the outcome variable using data from only two years, our yearly panel data allow us to examine the dynamics of the relative effect.

[14] The results from the linear specification suggest that the positive relative effects are statistically significant for the poorest 20% counties, so we divide the counties into two groups by the 20th percentile of their GDP per capita in 2000. Slightly perturbing the cut-off does not affect the conclusion.

[15] In Appendix Table S2, we also test the parallel trends assumption within each income group, and still find that the parallel trends assumption within each group holds.

Table 3
Heterogeneous relative impacts of expressway connection across initial income.

	GDP (million yuan, log)			Per capita GDP (yuan, log)		
	(1)	(2)	(3)	(4)	(5)	(6)
Expressway	0.87***	0.75***	0.80***	1.06***	0.92***	0.98***
	(0.21)	(0.18)	(0.18)	(0.22)	(0.19)	(0.19)
	(0.41)	(0.29)	(0.29)	(0.44)	(0.29)	(0.29)
	(0.27)	(0.22)	(0.24)	(0.29)	(0.23)	(0.24)
Expressway*GDP pc	−0.11***	−0.09***	−0.10***	−0.13***	−0.11***	−0.12***
(yuan, log, Year, 2000)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)
	(0.05)	(0.03)	(0.03)	(0.05)	(0.03)	(0.03)
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
County FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	N	Y	Y	N
Provincial Trends	N	Y	N	N	Y	N
Province-Year FE	N	N	Y	N	N	Y
Obs.	13,440	13,440	13,440	13,347	13,347	13,347
R^2	0.87	0.90	0.91	0.86	0.89	0.90

Notes: This table estimates the heterogeneous relative impacts of expressway connection on GDP measures by comparing the connected and unconnected counties in the matched sample. GDP data are deflated, where Beijing-2000 is the base province-year. We probe the robustness of the estimates' accuracy by clustering the standard errors at three different levels: county level, province level and county and province-year level (multi-way clustering suggested by Cameron et al., 2011). These standard errors are respectively reported in the parentheses below the estimated coefficients. Our preferred specification clusters standard errors at the county level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

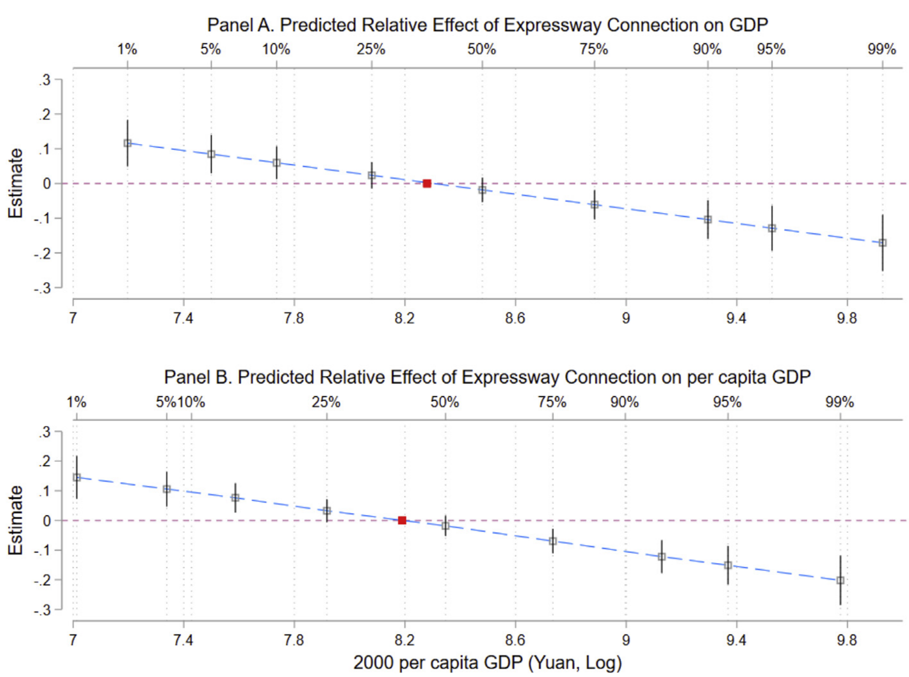

Fig. 5. Predicted Heterogeneous Relative Impacts of Expressway Connection.
Notes: The figure shows the predicted relative effects of expressway connection at different initial income levels, and their 95% confidence intervals, where the relative effects are estimated by comparing the connected and unconnected counties in the matched sample. GDP data are deflated, where Beijing-2000 is the base province-year. The relative impacts are positive for poor regions and negative for rich regions. The prediction is based on Table 3, Columns 1 and 4.

as the instrument for actual expressway connection, and report the results in Appendix Table S4. Again, we find the same pattern of a highly heterogeneous relative impact of expressway connection as our matched DiD result. In Appendix Table S5, we estimate the relative effect of expressway connection using the unmatched sample and find similar results.[16] In Appendix Table S7, we check whether the results are driven by a few counties that have usually high or low initial income. We drop the observations whose initial per capita GDP value is in the top or bottom 1% percentiles of all observations, and re-estimate Equations (1) and (2). This exercise, again, yields the same results.

Finally, we consider the concerns about two types of spillovers of the treatments that could confound our results. First, one might suspect that the expressway connection of one county could affect the average economic performance of other counties, either connected or unconnected, since all counties are ultimately connected to the national market. We believe that this type of spillover is less of a concern, because each county in our sample is small compared with the national market. Therefore, the impact of one county's expressway connection on all other counties would not be strong on average. That said, one county's expressway connection might still significantly affect their unconnected neighbors. To address this type of potential spillovers, we focus on counties that were never connected in the sample and estimate the relative impacts of

[16] The parallel-trend tests using the unmatched sample are also summarized in Appendix Table S6.

Table 4
Heterogeneous relative impacts of expressway connection across different initial income groups.

	GDP (million yuan, log)				Per capita GDP (yuan, log)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Expressway	0.10***	0.11***	0.06**	0.06**	0.11***	0.12***	0.06**	0.07**
	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)
High Income*Expressway	−0.16***	−0.16***	−0.10***	−0.10***	−0.17***	−0.18***	−0.10***	−0.11***
	(0.02)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.04)	(0.04)
County FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Provincial Trends	Y	N	N	N	Y	N	N	N
Province-Year FE	N	Y	N	N	N	Y	N	N
Income Group Trends	N	N	Y	N	N	N	Y	N
Income Group * Year FE	N	N	N	Y	N	N	N	Y
Obs.	13,440	13,440	13,440	13,440	13,347	13,347	13,347	13,347
R^2	0.90	0.91	0.87	0.87	0.89	0.91	0.87	0.87

Notes: This table estimates the heterogeneous relative impacts of expressway connection on GDP by comparing the connected and unconnected counties in the matched sample. GDP data are deflated, where Beijing-2000 is the base province-year. Standard errors are clustered at the county level and reported in the parentheses below the estimated coefficients. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

having at least one of the neighboring counties connected to the expressway system on the GDP of these never-connected counties. In practice, we apply Equation (2) to the subsample of unconnected counties, substituting $Connect_{i,t}$ with a "neighbor connected" indicator that equals 1 if at least one of county i's neighboring counties is connected at year t, and 0 otherwise. The coefficients of this indicator and its interaction with the initial income reveal the potential spillover effect and its heterogeneity. As reported in Appendix Table S8, the relative effect of "neighboring connection" is positive for low-income unconnected counties, while it is negative for high-income unconnected counties. This finding shows that some spillovers do exist, but their effect works against the heterogeneity pattern in our main result, rather than contributing to it.[17] Were there no such spillover, the heterogeneity we find in our main result would be even stronger.

To summarize, these robustness checks lend additional credibility to our main finding: expressway connections help poor peripheral counties grow faster in GDP while slowing the rich peripheral ones down, compared with the unconnected peripheral counties.

6. Understanding the heterogeneity

We now search for factors that could help explain the heterogeneity in the relative impacts of expressway connection across initial GDP per capita. We use two major groups of theories in the literature as guidance. The first group is based on increasing returns to scale, for example, the theory of the home market effect. The home market effect conjectures that, because of economies of scale, market integration brought by expressway connections can cause mobile productive factors to move from peripheral counties to core metropolitan areas to enjoy a larger home market, reducing the economic output in the peripheral counties (e.g., Krugman, 1991; Faber, 2014). Although this is inconsistent with our positive estimate of the relative impact on the GDP of the poor peripheral counties, it is consistent with our negative estimate of the relative impact on the rich peripheral counties. Therefore, we should consider those factors that are supposed to influence the home market effect.

The second major group of theories is based on comparative advantage. In a comparative advantage framework, a lower trade cost will encourage regions to specialize in the sector in which they have a comparative advantage. Although the realization of a comparative advantage can increase the real wages in a region (e.g., Copeland and Taylor, 1994), the theory does not generate clear-cut predictions about the impact on the local GDP. That said, the low-income and high-income regions may have different comparative advantages; for example, as in the pollution-haven hypothesis, the low-income regions may have a comparative advantage in polluting industries while the high-income ones may not. Therefore, we should consider those factors that can indicate a county's comparative advantages along different dimensions.

We consider the following eight county characteristics in 2000 that could have the potential to explain our heterogeneity result:

1. The initial distance between the focal county and its nearest targeted city. This distance indicates the initial trade cost and access to the nearby market before the expressway connection. In theory, given a lower initial trade cost and easier access to the nearby market, i.e., a closer distance between a peripheral county and its nearest targeted city, the home market effect on this county should be more negative.

2 and 3. The initial land area and population. These two variables are prominent in both the theory of the home market effect and the theories based on comparative advantage: the home market effect should be less negative when the focal area has greater endowments of land and labor or a bigger consumer population; a greater endowment of land or labor also indicates a stronger comparative advantage in the land-intensive or labor-intensive sector.

4. The initial land area per capita. This variable measures the relative abundance of land to labor and indicates whether the focal county initially had a strong comparative advantage along the dimension of the labor/land-intensive sectors.

5 and 6. The initial number of polluting industrial firms and the initial value of the polluting industrial output. The former variable indicates the endowment of polluting capital in the focal county, and a greater endowment would make the home market effect on the county less negative. The latter variable indicates whether the county initially specializes in the polluting sector and has a strong comparative advantage in this sector.

7 and 8. The initial shares of agriculture and manufacturing in GDP. These variables indicate whether the focal county initially had strong comparative advantages along the agriculture/non-agriculture and manufacturing/non-manufacturing dimensions, respectively.

To investigate whether these factors can explain our main result, we first examine whether they are correlated with the initial GDP per capita. Table 5 reports that counties that had less land and population, hosted more polluting firms, produced more polluting industrial output, and had a lower share of agriculture and a higher share of the manufacturing sector in GDP in 2000 tend to be richer in 2000.

We then examine whether the heterogeneity within the relative impact of expressway connection across initial income can be explained

[17] Compared with the results in Table 3, we see that the coefficients of both the treatment indicator and the interaction term are substantially smaller. This is reasonable because the relative effect of having a neighboring county connected should be weaker than that of being directly connected on one's own.

Table 5
Predictors of initial income.

	GDP per capita *(log)*							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Population	−0.06**							
(log)	(0.03)							
Land area		−0.08***						
(log)		(0.03)						
Land per capita			−0.02					
(log)			(0.02)					
# Polluting industrial firms				0.30***				
(log)				(0.02)				
Polluting industrial output value					0.20***			
(log)					(0.01)			
Agriculture in GDP						−0.01***		
(%)						(0.00)		
Manufacturing in GDP							0.01***	
(%)							(0.01)	
Distance to nearest targeted city								0.00
(100 km)								(0.00)
Obs.	1054	1052	1052	1051	1132	1139	1139	1139
R^2	0.005	0.014	0.002	0.259	0.247	0.118	0.033	0.001

Notes: This table shows how the eight county characteristic variables in 2000 that are supposed to influence the home market effect or indicate comparative advantages predict the GDP per capita in 2000. GDP data are deflated, where Beijing-2000 is the base province-year. Heteroscedasticity-robust standard errors are reported in the parentheses below the estimated coefficients. ***p < 0.01, **p < 0.05, *p < 0.1.

Table 6
Explore the heterogeneity patterns.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Outcome Variable: GDP (million yuan, log)									
Expressway	0.87***	1.61***	0.44	0.71***	0.86***	0.89***	0.89***	0.91***	2.00***
	(0.21)	(0.24)	(0.31)	(0.22)	(0.25)	(0.21)	(0.29)	(0.21)	(0.34)
Expressway*GDP pc	−0.11***	−0.11***	−0.10***	−0.10***	−0.10***	−0.12***	−0.11***	−0.11***	−0.23***
(Year, 2000)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)
Expressway*X	0.00	−0.11***	0.05**	0.07***	−0.00	0.01	−0.00	0.00*	/
(Year, 2000)	(0.00)	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)	(0.00)	(0.00)	/
Panel B. Outcome Variable: GDP per capita (yuan, log)									
Expressway	1.06***	1.76***	0.57*	0.84***	1.03***	1.07***	1.08***	1.10***	2.15***
	(0.22)	(0.25)	(0.33)	(0.24)	(0.26)	(0.22)	(0.31)	(0.22)	(0.35)
Expressway*GDP pc	−0.13***	−0.13***	−0.11***	−0.11***	−0.13***	−0.14***	−0.13***	−0.14***	−0.24***
(Year, 2000)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.04)
Expressway*X	−0.00	−0.11***	0.05**	0.07***	0.00	0.01	−0.00	0.00**	/
(Year, 2000)	(0.00)	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)	(0.00)	(0.00)	/
X Indicator	Distance (100 km)	Population (log)	Land area (log)	Land per capita (log)	# Polluting industrial firms (log)	Polluting industrial output value (log)	Agriculture (%)	Manufacturing (%)	All
County FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Obs.	13,347	12,245	12,236	12,236	12,210	13,264	13,347	13,347	12,144
R^2	0.86	0.86	0.86	0.86	0.86	0.86	0.86	0.86	0.87

Notes: This table estimates the heterogeneity in the relative impacts of expressway connection on GDP measures across the initial income and other variables by comparing the connected and unconnected counties in the matched sample. GDP data are deflated, where Beijing-2000 is the base province-year. Standard errors are clustered at the county level and reported in the parentheses below the estimated coefficients. ***p < 0.01, **p < 0.05, *p < 0.1.

by potential heterogeneities across these eight variables. Starting with Equation (2), we introduce additional interaction variables between the expressway connection dummy and these variables. If introducing these additional interaction variables cannot significantly dilute the heterogeneity in impact across initial income, this suggests that the heterogeneity cannot be explained by these eight variables.

Table 6 shows the results. Comparing each column among Columns 1–8 in this table with our baseline estimates in Table 3, we find that the heterogeneity in impact across initial income is always robust. Column 9 in Table 6 further shows that, when we include all these additional interaction variables in one regression, not only does the coefficient of the expressway–GDP per capita interaction remain statistically significantly negative, it becomes greater in magnitude. We conclude that the heterogeneity across initial income cannot be explained by these eight variables.[18]

The factors that we have considered include variables that indicate the initial trade cost, access to nearby markets, endowments of land, labor, and polluting capital, size of the consumer population, and

[18] We are not arguing that the home market effect and comparative advantage are irrelevant in the empirical context. For example, Columns 4 and 8 in Table 6 suggest that the relative impact of expressway connection is more positive or less negative if the focal county initially has stronger comparative advantages in the land-intensive sector and the manufacturing sector, respectively. Nevertheless, these heterogeneities cannot explain the robust heterogeneity across initial income.

comparative advantages along different dimensions. To explain our empirical finding, we now search for other factors.

7. An explanation about the GDP–environment trade-off

7.1. Relevance of environmental concerns

Local governments (or residents) can have non-economic considerations when they respond to their connections to the expressways. In many countries, a major non-economic consideration is the environment.

Environmental concerns are especially salient in the Chinese context during our sample period. Since the market reform in the 1980s, local officials had been evaluated primarily based on local economic performance, often measured by GDP growth (e.g., Li and Zhou, 2005; the survey by Xu, 2011). In the early 2000s, however, the Chinese government started seeking integrated solutions to the intertwining economic, environmental, and social problems it faced, and explicitly put environmental considerations into policy decisions.[19] In particular, the central government directed the poor regions to aim at improving their economic performance, while emphasizing the environment for the rich regions (State Council of China, 2005):

> *Relatively developed regions should … insist on prioritizing the environment, … optimize the industrial structure, … and take the lead in reaching the emission reduction target …. The regions of great development potential should … scientifically and reasonably utilize the carrying capacity of the environment to promote industrialization and urbanization …. Depending on the main ecological function of each region, different regions should formulate development strategies with different characteristics.*

The same directive required local governments to follow this two-stage scheme of development objectives in evaluating officials, a practice later fully institutionalized by the Communist Party of China (Organization Department of the Communist Party of China, 2013).[20]

This two-stage scheme can have a real impact on the connected counties when local governments try to find a balance between GDP growth and environmental quality. This is because the role of government in the local economy is particularly influential in China, as the local government has strong control over natural resources (notably land), capital flows, and even labor in their jurisdiction. In practice, it is well known that local governments in China have been actively choosing investors, industries, and talent to implement their development strategies by offering tax rebates, infrastructure improvements, price discounts for land use, exemptions from regulations, and other generous industrial policies (e.g., Qian and Roland, 1998; Bai et al., 2014). As the cost of attracting/pushing away certain industries becomes lower because of expressway connections, local governments can more easily respond to the central government's two-stage development objectives.

We indeed observe that local governments have exercised such practices. For example, Guangdong province, which has the largest provincial economy in China, explicitly required manufacturing industries in rich regions to relocate to less-developed regions, and its Party Secretary advocated that, without this mandate, the government "would in the long run not be able to create a beautiful living environment for the people" (Southern Daily, 2008). Since the 2000s, governments in the rich regions in China have used higher pollution levies and stricter environmental mandates to push polluting industries away, while governments in the poor regions have welcomed them (e.g., Lin and Sun, 2016; Wu et al., 2017).[21]

Given all these considerations, we now examine whether the observed heterogeneous relative impacts of expressway connection on local GDP are associated with heterogeneous relative impacts on local polluting emissions, and how the heterogeneity is achieved by changes in production technologies, the distribution of polluting production, and local sectoral patterns. This will shed light on whether and how the rich and poor counties were pursuing different development strategies between GDP growth and environmental preservation in response to the expressway connection.

7.2. Relative effects of expressway connection on emissions

In Table 7, we examine the relative impact of the expressway on COD emissions and per capita COD emissions. We find that the expressway on average slightly decreases emissions in the connected counties, compared with the unconnected counties, and this relative effect is statistically insignificant. When we interact the expressway connection dummy with 2000 per capita GDP, a strong heterogeneity emerges: the relative effect of expressway connection becomes more negative in richer counties. This finding is robust to including different controls and using different ways to cluster the standard errors. In Fig. 6, we also predict the heterogeneous relative impacts of expressway connection on COD emissions at different initial income levels, and find that poor regions emit more COD and rich regions emit less COD after expressway connection, compared with the unconnected counties.[22] This heterogeneity suggests that the higher GDP that expressway connection brings to the poor counties compared to the unconnected counties comes with an environmental cost; in the connected rich counties, however, GDP is sacrificed for better environmental performance than in the unconnected counties.

In Table 8, we examine several additional emission measures. In Columns 1 to 4, we use COD emissions from the key polluting plants as the outcome variable and find similar results. In Columns 5–8, we investigate the relative impact on ammonia nitrogen (NH_3–N) emissions.[23] Consistent with the result in Table 7, poor counties emit more ammonia nitrogen and rich counties emit less after expressway connection, compared with the unconnected counties.

In Columns 9–12, we examine the relative impact of expressway connection on sulfur dioxide (SO_2) emissions, and the heterogeneity result disappears. We should, however, not interpret this as evidence against the hypothesis that local governments respond to expressway connection by different development strategies. This is because local county governments in our sample have little power to regulate SO_2 emissions. In China, roughly 70% of SO_2 emissions are produced by the electricity and heating industries (mostly power plants), and the remaining 25–30% are emitted by the mineral products and metal industries. Most plants in these industries belong to large state-owned enterprises and are not controlled by county governments. Therefore, the disappearance of the heterogeneity result for SO_2 emissions is not surprising.

[19] This transition was coined in 2003 as the "Scientific Outlook on Development" under the name of President Hu Jintao.

[20] The practical significance of these political incentives of Chinese local governments has been documented by a series of empirical studies. For example, Sun et al. (2014) and Zheng et al. (2014) provide consistent evidence that, in 2004–2009, better environmental performance contributed to promotion of city mayors in China. Sun et al. (2014) further show that the impact was more prominent for mayors of larger cities. Kahn et al. (2015) document that local officials' efforts in pollution reduction increased after the reform.

[21] Although not directly about local government, Bombardini and Li (2020) provide suggestive evidence that higher income drives demand for a cleaner environment among Chinese prefectures.

[22] In Appendix Table S9, we conduct parallel trends tests and find that pre-connection trends for COD and per capita COD are parallel. In Appendix Fig. 2, we predict the relative impacts at different initial income levels and show that the relative effect is positive in poorer counties and negative in richer counties.

[23] Ammonia nitrogen is also an important measure of water pollution. It serves as a nutrient in water bodies and consumes large amounts of oxygen. As a result, rich ammonia nitrogen is toxic to fish and other aquatic organisms and leads to eutrophication in the water.

Table 7
The impacts of expressways on connected counties' emissions relative to unconnected counties'

	COD Emissions (ton, log)				Per capita COD Emissions (kg, log)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Expressway	−0.09	2.84***	1.89**	2.45***	−0.12	3.21***	2.18**	2.79***
	(0.09)	(0.76)	(0.85)	(0.86)	(0.09)	(0.82)	(0.92)	(0.94)
	(0.05)	(0.81)	(0.75)	(0.67)	(0.06)	(0.88)	(0.86)	(0.78)
	(0.10)	(0.87)	(0.90)	(0.96)	(0.09)	(0.91)	(0.97)	(1.06)
Expressway*GDP pc		−0.34***	−0.23**	−0.30***		−0.39***	−0.27**	−0.34***
(yuan, log, Year, 2000)		(0.09)	(0.10)	(0.10)		(0.10)	(0.11)	(0.11)
		(0.10)	(0.09)	(0.08)		(0.10)	(0.10)	(0.09)
		(0.10)	(0.11)	(0.11)		(0.11)	(0.11)	(0.12)
County FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	N	Y	Y	Y	N
Provincial Trends	N	N	Y	N	N	N	Y	N
Province-Year FE	N	N	N	Y	N	N	N	Y
Obs.	13,338	13,338	13,338	13,338	13,205	13,205	13,205	13,205
R^2	0.69	0.69	0.70	0.72	0.64	0.64	0.66	0.67

Notes: This table estimates the heterogeneous relative impacts of expressway connection on emission measures by comparing the connected and unconnected counties in the matched sample. GDP data are deflated, where Beijing-2000 is the base province-year. We probe the robustness of the estimates' accuracy by clustering the standard errors at three different levels: county level, province level and county and province-year level (multi-way clustering suggested by Cameron et al., 2011). These standard errors are respectively reported in the parentheses below the estimated coefficients. Our preferred specification clusters standard errors at the county level. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

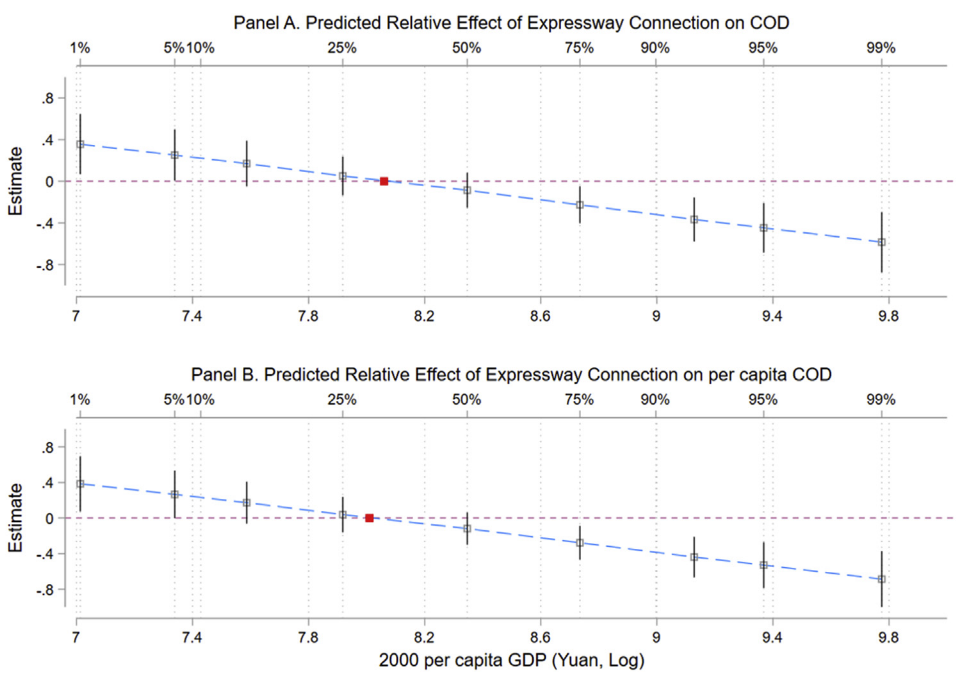

Fig. 6. Predicted Relative Impacts of Expressway Connection on Emissions at Different Income Levels.
Notes: The figure shows the predicted relative effects of expressway connection at different initial income levels, and their 95% confidence intervals, where the relative effects are estimated by comparing the connected and unconnected counties in the matched sample. GDP data are deflated, where Beijing-2000 is the base province-year. The relative impacts are positive for poor regions and negative for rich regions. The prediction is based on Table 7, Columns 2 and 6.

7.3. Relative effects of expressway connection on production cleanness, polluting firms, and local sectoral patterns

To shed light on how the heterogeneous relative effects on both GDP and polluting emissions are achieved, we examine several other outcome variables in Table 9, following the regression specified in Equation (2).

We report the results for COD emission intensity in Columns 1 and 2 in Table 9, which tell whether expressway connection causes the key polluting plants in each county to use cleaner technology to reduce emissions per RMB-value of output, compared to the unconnected counties. The strong heterogeneity suggests that the emission intensity increases in poor connected counties and decreases in rich connected counties, compared with the unconnected counties. This pattern indicates that, after the expressway connection, firms in poor counties adopt more pollution-intensive technology, while firms in rich counties adopt cleaner technology, compared with the unconnected counties.

We then turn to the distribution of polluting production. The regression result in Column 3 in Table 9 shows that connected counties have a similar number of key polluting firms as non-connected counties, on average; however, the strongly heterogeneous results in Column 4 suggest that the number of key polluting firms increases in poor, connected counties but decreases in rich, connected counties, compared with the unconnected counties. Consistent results can also be found in the total industrial output value. In Columns 5 and 6, the value of industrial output from heavily polluting firms increased in poor counties but decreased in rich counties, compared with the unconnected counties.

Table 8
The relative impacts of expressway connection on other emission measures.

	COD Emissions from Key Polluting Sites (ton, log)		Per capita COD Emissions from Key Polluting Sites (kg, log)		NH3–N Emissions (ton, log)		Per capita NH3–N Emissions (kg, log)		SO2 Emissions (ton, log)		Per capita SO2 Emissions (kg, log)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Expressway	−0.05	3.55***	−0.09	4.03***	−0.08	3.64***	−0.13	4.31***	−0.07	−0.35	−0.08	−0.17
	(0.09)	(0.78)	(0.10)	(0.85)	(0.11)	(1.19)	(0.13)	(1.34)	(0.06)	(0.56)	(0.06)	(0.56)
Expressway*GDP pc		−0.43***		−0.49***		−0.44***		−0.52***		0.03		0.01
(yuan, log, the Year, 2000)		(0.09)		(0.10)		(0.14)		(0.16)		(0.07)		(0.07)
County FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Obs.	14,551	14,551	14,408	14,408	11,380	11,380	11,249	11,249	13,548	13,548	13,413	13,413
R^2	0.07	0.08	0.07	0.07	0.14	0.14	0.16	0.16	0.13	0.13	0.12	0.12

Notes: This table estimates the heterogeneous relative impacts of expressway connection on other emission measures by comparing the connected and unconnected counties in the matched sample. GDP data are deflated, where Beijing-2000 is the base province-year. Standard errors are clustered at the county level and reported in the parentheses below the estimated coefficients. ***p < 0.01, **p < 0.05, *p < 0.1.

Table 9
The Channels through which Expressways Affect the Local Economy.

	COD Emission Intensity (ton per RMB-value of output, log)		Number of Key Polluting Firms (log)		Output Value of Key Polluting Firms		Share of the Secondary Industry (%, log)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Expressway	−0.04	2.77***	−0.07	1.06**	0.04	1.17***	−0.00	0.63***
	(0.08)	(0.81)	(0.05)	(0.42)	(0.03)	(0.27)	(0.01)	(0.14)
Expressway*GDP pc		−0.33***		−0.13***		−0.13***		−0.07***
(yuan, log, the Year, 2000)		(0.09)		(0.05)		(0.03)		(0.02)
County FE	Y	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y	Y
Obs.	14,531	14,531	14,711	14,711	14,713	14,713	8051	8051
R^2	0.11	0.11	0.54	0.54	0.39	0.39	0.21	0.21

Notes: This table estimates the heterogeneous relative impacts of expressway connection on environmental and economic outcomes by comparing the connected and unconnected counties in the matched sample. GDP data are deflated, where Beijing-2000 is the base province-year. Standard errors are clustered at the county level and reported in the parentheses below the estimated coefficients. ***p < 0.01, **p < 0.05, *p < 0.1.

Finally, in Columns 7 and 8 in Table 9, we show that the share of manufacturing increased in poor regions but decreased in rich counties, compared with the unconnected counties. These results suggest that access to a larger market can help poor counties industrialize while helping rich counties de-industrialize.

7.4. *Emerging explanation*

The empirical results in this section outline a consistent image reconciling our main result of the heterogeneous relative impacts of expressway connection on local counties' GDP across initial income levels. Expressways bring about an opportunity for local governments in both rich and poor counties to rebalance between GDP growth and environmental concerns. For the poor counties, it is an opportunity to grow their GDP; they host more polluting industries and promote industrialization, at the cost of environmental quality. The rich counties, in comparison, respond by pushing polluting production out, de-industrializing, and reducing emissions, sacrificing some of their GDP. These different responses are consistent with the different development objectives faced by different local governments.

In Appendix III, we illustrate this explanation in a trade model, which spotlights how the local government's preference between GDP and environmental quality can shape the local economy's response to an economic opportunity that reduces trade cost. We show a proposition to justify the possibility of the observed heterogeneous relative impacts of expressway connection on both local GDP and emissions.

8. Concluding remarks

Our findings have several implications. First, we confirm that transport infrastructure is important for early-stage development and can effectively promote the economic growth of poor regions. In our context, China's efforts in improving infrastructure and expanding the expressway network can help explain its success in alleviating poverty in the past thirty years. This finding would be masked behind the slightly negative relative impact of expressway connection on peripheral counties on average if we did not explore the heterogeneity of the relative impact.

Second, while using the Chinese government's two-stage scheme of development objectives to reconcile the empirical findings seems China-specific, the insights behind this argument can be general. At various stages of economic development, the government often prioritizes certain objectives over others, and social preference can play an important role in shaping the economy. Since it is possible for these factors to drive first-order impacts on the economy, environment, and society, they deserve more attention in the literature.

We conclude with some directions for further investigation. First, the mechanism behind the observations in the literature that the expressways simultaneously reinforce cross-prefectural disparity and reduce within-prefectural disparity deserves further research. Second, our reduced-form estimates may fail to detect important general equilibrium effects. For example, the aggregate impact of many counties' connections on the national market could be non-negligible. Finally, it is logically impossible to argue that our proposed explanation is the only explanation for the empirical results. If our explanation is indeed general, however, we would expect other productivity-enhancing shocks, such as China's accession to the WTO, to have similar heterogeneous relative impacts on the local economy and environment. Further analyses on these issues are warranted.

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Appendix A. Supplementary data and other appendices

Supplementary data and other appendices to this article can be found online at https://doi.org/10.1016/j.jdeveco.2020.102485.

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