

Gains from trade boom, public goods provision and regional inequality *

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March 27, 2026

Abstract

Are geographically asymmetric gains from trade liberalization reflected in localized public goods provision or are they effectively redistributed across regions to mitigate spatial inequality? Leveraging spatial variation in exposure to China's expanded access to U.S. market, we find robust evidence that Chinese prefectures with greater exposure to the policy experienced significantly faster growth in their tax bases and revenues. Importantly, these same high-exposure prefectures also exhibited accelerated expansion in public goods provision relative to less-exposed counterparts, implying insufficient inter-regional redistribution to mitigate trade-induced inequality. Consequently, inter-prefecture disparities in government revenues, expenditures, and public goods widened markedly between 2000 and 2013. These results highlight how spatially uneven trade gains can amplify fiscal and developmental divides in a fiscally decentralized yet politically centralized system, underscoring the urgent need for targeted regional redistribution mechanisms to counteract trade-induced inequality and promote balanced regional development.

Keywords: Regional inequality, public goods provision, trade liberalization

JEL Codes: F14, F16, H41, H51, H52, R12

*We thank participants at Midwest International Trade Conference, Oct 18-20 2024 University of Rochester and Midwest Econometric Group conference Nov 1-2, 2024 University of Kentucky for their helpful comments and suggestions.

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1 Introduction

Empirical evidence robustly demonstrates that the gains from trade liberalization are usually asymmetrically distributed across regions, generating stark winners and losers within the same country.¹ This has important implications for the fiscal capacities of regions. As booming regions experience expansion in their tax bases and revenues, lagging ones struggle to generate sufficient revenues for essential public goods and services, thereby perpetuating and intensifying cycles of inequality. Yet, despite these profound implications, the pivotal nexus between trade exposure, local public finance, and public goods provision has received scant attention in the literature.²

In an economy with effective regional distribution, asymmetric gains from trade liberalization across regions are not reflected in localized public goods provisions, as the gains are redistributed to lagging regions and equalize outcomes nationwide. In the absence sufficient regional redistribution, uneven gains from trade boom cause uneven regional distribution of public goods. In this paper we test this hypothesis by examining the effects of trade liberalization on public goods provision in China. From 2000 to 2020, China’s global export share skyrocketed from 4% to 15%.³ However, these gains have been profoundly uneven, with coastal provinces specializing in tradable sectors reaping substantial economic windfalls, while interior regions reliant on non-tradable industries have been left behind. Consequently, local governments (LGs) in export-oriented hubs have experienced dramatic expansions in their tax bases and revenues. How has this spatially uneven trade boom reshaped the regional allocation of public goods, and to what extent has it amplified regional inequality?

China presents a compelling case for this inquiry, as it embodies a paradoxical fiscal structure: while politically one of the most centralized nations globally, it is among the most fiscally decentralized in terms of expenditure, with subnational governments responsible for approximately 85% of total public spending (Wingender, 2018; Shen et al., 2012). LGs also collect roughly 60% of government revenues. Nonetheless, as in many unitary states, the central government retains exclusive authority over tax rates and bases, leaving LGs unable to adjust these parameters independently. As elaborated below, LG revenue growth primarily stems from tax base expansions fueled by trade-induced local economic growth.

We leverage China’s fiscal decentralization and the substantial spatial heterogene-

¹See, for instance, Autor et al. (2013, 2019); Coşar and Fajgelbaum (2016); Dix-Carneiro and Kovak (2017); Pavcnik (2017).

²A notable exception is Feler and Senses (2017), which reveals that U.S. regions more exposed to Chinese import competition suffered declines in public goods quality due to eroding local government revenues, driven by economic downturns including reduced employment, sluggish wage growth, and population outflows (Che et al., 2018; Autor et al., 2019; Adda and Fawaz, 2020; Greenland et al., 2019).

³See <https://unctad.org/news/china-rise-trade-titan>.

ity in gains from trade liberalization to examine whether these gains enhance local public goods provision. We combine prefecture-level panel data on local government (LG) revenues and expenditures across various public goods categories with detailed information on the industrial composition of Chinese prefectures. Our identification strategy exploits the U.S. granting of Permanent Normal Trade Relations (PNTR) status to China in 2001, following China’s WTO accession, as an exogenous shock to trade opportunities and LG tax bases. Prior to 2001, the U.S. Congress annually renewed Normal Trade Relations (NTR) status for China, ensuring Chinese exporters faced most-favored-nation (MFN) tariff rates applicable to WTO members. Failure to renew would have subjected Chinese goods to substantially higher Smoot-Hawley tariff rates (Pierce and Schott, 2016). The conferral of PNTR eliminated this recurrent tariff uncertainty, significantly boosting Chinese exports to the U.S. market after 2001. Because Smoot-Hawley tariff rates varied markedly across industries, pre-2001 tariff uncertainty differed substantially by sector—and thus across prefectures, given their differing industrial concentrations. We exploit this plausibly exogenous variation to identify the causal impact of expanded LG tax bases on public goods provision.

Our identification approach closely resembles the shift-share research design (Borusyak et al., 2021; Goldsmith-Pinkham et al., 2020). Here, the “shifts” consist of industry-level reductions in tariff uncertainty, while the “shares” are prefecture-level employment shares in those industries. As demonstrated by Borusyak et al. (2021), identification in shift-share frameworks holds when the shifts are as-good-as-random and numerous (and small) independent shifts exist, irrespective of share exogeneity. These conditions are plausibly met in our context: (i) the magnitude of shifts (tariff uncertainty reductions) stems from the Smoot-Hawley Tariff Act of 1930, which is exogenous to contemporary U.S. and Chinese economic conditions; and (ii) we draw on variation across 487 industries, each comprising only a small fraction of manufacturing employment in any given prefecture. We subject our strategy and inferences to a comprehensive set of robustness checks and diagnostic tests recommended in the shift-share literature.

Our empirical analysis reveals that trade liberalization generated substantial increases in LG revenues in prefectures more exposed to the shock relative to less exposed ones. Crucially, LG expenditures rose in broadly similar proportion in the more exposed prefectures. To illustrate the magnitude of these effects, consider a prefecture at the 75th percentile of the tariff-uncertainty reduction (i.e., more exposed) compared to one at the 25th percentile (less exposed): the former experienced an additional per capita government revenue increase of approximately 2,434 Yuan. A parallel comparison for per capita expenditures reveals an additional 2,034 Yuan in the more exposed prefecture.⁴ These two effects are statistically indistinguishable,

⁴These amounts correspond to approximately \$294 and \$246 at the 2002 exchange rate of 8.28

which underscores the absence of effective inter-regional redistribution mechanisms capable of mitigating trade-induced spatial inequality.

These uneven patterns in revenue and expenditure growth translated directly into differential improvements in key public goods and services. Relative to prefectures at the 25th percentile of exposure, those at the 75th percentile experienced significantly larger increases in educational infrastructure and inputs. Specifically, the number of primary, secondary, and tertiary schools per 1,000 people rose by approximately 8, 0.92, and 0.066, respectively. Per capita government spending on education and science increased by 794 Yuan and 352 Yuan, respectively. Health infrastructure expanded markedly as well, with additional increases of 0.68 hospital beds and 0.88 doctors per 1,000 people—representing roughly 45% of the 2002 baseline levels in these categories. Infrastructure development followed a similar pattern, as evidenced by substantially greater growth in paved road and highway areas in more exposed prefectures.

LG revenue growth primarily stems from tax base expansions fueled by trade-induced local economic growth. The bases for the major tax categories such as VAT, corporate income tax, personal income tax and social security contribution significantly expanded in prefectures more exposed to trade liberalization compared to less exposed prefectures. Moreover, investment in fixed assets such as residential real estate soared in prefectures more exposed to trade liberalization, thus expanding the tax bases for property related taxes which are entirely received by LGs.

Taken together, these findings demonstrate that trade liberalization, by producing spatially asymmetric economic booms, markedly exacerbates fiscal and developmental disparities across China's prefectures. Between-prefecture inequality in government revenues, expenditures, and public goods provision rose sharply from 2000 to 2013. This divergence is evident in both tail-end measures (e.g., the 90th-to-10th percentile ratio) and central dispersion (e.g., the interquartile range), both increasing significantly relative to the baseline. Such patterns highlight the lack of robust inter-regional redistribution from winning to losing regions. In the presence of effective redistribution, the localized benefits of trade exposure on public goods provision would have been attenuated, as gains in more exposed areas would have been shared more equitably nationwide rather than reinforcing spatially concentrated fiscal advantages.

These results carry important policy implications. While governments in advanced economies often deploy targeted programs—such as the U.S. Trade Adjustment Assistance (TAA) or the EU's European Globalisation Adjustment Fund—to support individuals displaced by trade,⁵ analogous mechanisms for redistributing trade gains

Yuan/USD.

⁵TAA offers temporary income support, unemployment benefits, retraining subsidies, or relocation

across regions remain largely absent. Recent evidence links trade-induced regional inequality to heightened political polarization and the rise of right-wing populism in the U.S. and Europe (Autor et al., 2020; Che et al., 2022; Backes and Mueller, 2024). Policies that channel gains from trade toward lagging regions are therefore well justified. The same logic applies to China: regions benefiting more directly from trade (via profits, wages, and employment) also enjoy amplified gains through enhanced public goods, further disadvantaging other areas. Effective regional redistribution is thus essential to counteract trade-driven inequality.

This paper contributes to an emerging literature on trade liberalization’s effects on LG fiscal capacity and public goods provision. For instance, Feler and Senses (2017) document that U.S. locales more exposed to Chinese import competition experienced declines in local public goods quality and amenities, driven by eroded revenue bases from falling housing prices and business activity. Similarly, Dix-Carneiro et al. (2018) find that Brazilian regions facing greater import competition saw rising crime, mediated by adverse labor market outcomes, reduced public goods, and increased inequality—particularly lower per capita revenues and expenditures, including on public safety. In contrast, we show that Chinese prefectures benefiting from reduced U.S. tariff uncertainty experienced tax base and revenue growth, leading to expanded public goods. Collectively, evidence from the U.S., Brazil, and China suggests that trade policies often lack accompanying effective regional redistribution, thereby amplifying inter-regional inequality.

Trade liberalization is also associated with broader socio-economic challenges tied to inequality and constrained LG fiscal responses. Studies show, for example, that U.S. counties more exposed to Chinese imports experienced elevated crime rates (especially property crimes), though mitigated somewhat by higher government transfers (Che et al., 2018). Other work links import competition to declining male earnings, reduced marriage and fertility, higher child poverty (Autor et al., 2019), slower population growth (especially among younger and less-educated groups) (Greenland et al., 2019), and worse mental/physical health and mortality outcomes (Pierce and Schott, 2020; Adda and Fawaz, 2020). While much prior research emphasizes within-region inequality, our analysis highlights how asymmetric trade gains can exacerbate between-region disparities in public goods provision.

The remainder of the paper is organized as follows. Section 2 provides background on fiscal decentralization in China. Section 3 describes the datasets and sources. Section 4 outlines the empirical methodology and identification strategy. Section 5 presents the main results, followed by robustness checks in Section 6 and mechanism analysis in Section 7. Section 8 explores implications for regional inequality in public goods, and Section 9 concludes.

aid to trade-affected workers.

2 Fiscal decentralization in China

Though China is formally constituted as a unitary state, its fiscal system exhibits a high degree of de facto decentralization, with the central government delegating substantial responsibilities for both revenue collection and public expenditures to sub-national (local) governments (LGs). Local governments collect approximately 50% of total government revenues while accounting for over 85% of total government expenditures (Wingender, 2018). This pronounced expenditure decentralization—among the highest worldwide—underscores the critical role of LGs in fiscal policy implementation and public service delivery.

2.1 Roles of Local Governments in Revenue Collection

In China’s multi-tiered fiscal structure, different levels of government are assigned responsibility for collecting specific tax categories, with revenues allocated to the central government, LGs, or shared between them according to formally established rules. The central government primarily collects consumption taxes, international trade-related taxes (such as tariffs), vehicle purchase taxes, and similar levies, which together comprise roughly 10% of total tax revenues and accrue exclusively to the center. During much of the sample period under study, revenues from major taxes were shared: domestic value-added tax (VAT) at a 75-25 ratio (central-LG), corporate income tax at 60-40, personal income tax at 60-40, and stamp taxes on securities at 97-3. These shared taxes collectively account for more than 55% of total tax revenue. The remaining taxes—most notably social security contributions and property- and land-related taxes—are largely collected and retained by LGs, representing over one-third of total tax revenue (see detailed discussions in Wingender (2018), Shen et al. (2012), and Qiao et al. (2022)).

Consistent with the unitary nature of the Chinese state, LGs receive nearly half of total tax revenues but lack autonomous taxing authority: the central government exclusively determines tax rates and bases (Qiao et al., 2022). This institutional feature strengthens the exogeneity of revenue fluctuations at the local level, as changes in LG revenues and expenditures predominantly stem from exogenous expansions in the tax base driven by economic activity rather than discretionary policy choices by LGs. The present analysis exploits precisely such an exogenous shock to the tax base—an export-oriented trade boom triggered by the U.S. granting of Permanent Normal Trade Relations (PNTR) to China. This policy shift was plausibly exogenous to the pre-existing economic, institutional, or geographic characteristics of Chinese prefectures, thereby providing a robust source of identification for causal effects of interest.

2.2 Roles of Local Governments in Public Goods Provision

China’s expenditure assignment is among the most decentralized globally, with LGs responsible for approximately 85% of total government spending as of 2019 (Qiao et al., 2022). LGs bear the overwhelming share—over 90%—of expenditures in key social sectors, including health, education, environmental protection, general public services, and social security and employment. The Chinese fiscal system operates under a hierarchical budget management framework, in which each tier’s budget encompasses its own outlays as well as those of subordinate tiers. While the central government exerts influence through intergovernmental transfers and overarching policies, provincial governments directly oversee fiscal management at lower levels. Counties shoulder the largest fiscal burden, accounting for roughly 12% of GDP in expenditures and revenues, followed by prefectures (approximately 7% of GDP) and provinces (around 4% of GDP). On the revenue side, prefectural governments collect about 5% of GDP, counties 4%, and provinces 2% (Lam et al., 2016).

County and prefectural governments are primarily responsible for delivering core public goods and services, such as education, health care, sanitation, and local infrastructure. In education, counties finance compulsory education (primary and junior secondary), while prefectures oversee senior secondary (high school) education; tertiary education remains under central government purview through the Ministry of Education (Yuan and Zhang, 2015). The 1995 Education Law further mandates LG commitment to education funding via the so-called “two-growth” rule: (i) LG education spending must grow faster than LG revenues overall, and (ii) per-student education expenditures must increase annually. Given that most LG education outlays are financed from own-source revenues, jurisdictions with larger tax bases enjoy greater fiscal capacity for education investment (Yuan and Zhang, 2015). Nonetheless, lower-tier governments receive substantial supplementary funding from central and provincial sources for teacher salaries, school infrastructure, and related purposes (Hu, 2022).

In the health sector, county and prefectural governments assume primary responsibility for public healthcare delivery (Tan and Wong, 2022). County-level hospitals anchor a three-tier primary healthcare system, with township hospitals serving as intermediaries and village clinics as the foundational layer (Zhu et al., 2021). County hospitals occupy the middle tier in the broader hospital hierarchy, bridging primary care facilities with higher-level (tier-three) institutions offering specialized services (Shi et al., 2021). Prefectural and county governments finance investments in personnel, equipment, and facilities, while central and provincial authorities provide policy guidance and fiscal transfers.

Finally, road infrastructure follows a similar pattern: the Highway Law assigns

construction and maintenance responsibilities to LGs at the appropriate jurisdictional level. Central, provincial, and prefectural governments handle financing and oversight, while county governments execute implementation (Wood and Maurice, 2007; Fan, 2011). This assignment reinforces the pivotal role of subnational governments in translating fiscal resources into tangible public goods provision.

In an online appendix, we theoretically explore the link between local tax base expansion and public good provision in a developing country context where elite capture is a common feature of LGs. We study a situation where the statutory tax rate is determined by the central government and LGs choose optimal enforcement of the statutory taxes as they weigh the demand for public goods by their residents against the elites' (capitalists who pay most of the taxes) pressure for tax relief. The effective tax rate is likely to be much lower than the statutory tax rate in LGs that are strongly influenced by elites. We assume that LGs share a fraction θ of the revenue they collect to the central government, which may either use the revenue for its own purpose or redistributes back to the LGs in inverse tax base-weighting formula ω .

In such environment, the extent to which trade-induced expansion in tax base translates into increased LG revenue depends on the weight the LGs attach to provision of public goods vs. the weight they attach to the elites' demand for tax relief. Furthermore, the link between local tax base expansion and local public goods provision is attenuated by the revenue sharing rule (θ) and intergovernmental redistribution (ω), which reduces the trade induced regional inequality. Below we empirically examine the effect of asymmetric gains from trade on local public goods provision and inequality.

3 Data

Our main dataset is Prefectural-level annual panel data since 1996 on government revenues and expenditures and various measures of public goods provision in health, education, transport, etc. This data originates from the official government statistics annually published on Provincial Yearbook (PYBs). We obtain the digitized version of the data from China Data Center.⁶ We combine this data with a measure of exposure to trade liberalization (China's WTO accession) at Prefectural city level. Each prefecture's exposure to trade liberalization is defined by the prefecture's industrial composition of employment and variation across industries in the extent of reduction in trade policy uncertainty due to China's accession to WTO. Details of the trade exposure measure is discussed below.

⁶It can be accessed here: <https://chinadatacenter.net/>.

4 Empirical methodology

We use prefecture cities as our unit of analysis for three reasons. First, manufacturing employment statistics can be constructed at a detailed industrial classification (489) at prefecture level. We use the Chinese industrial survey data which includes information about prefecture location of the firms. Such detailed industrial variation is crucial for our analysis because most of the variation in our analysis comes from cross-sectional variation across industries as well as across prefectures. Second, provision of public goods and services such as healthcare, education and roads is delegated to prefecture and county governments. However, consistent panel data on public goods and services provision is better available at prefecture level. Third, the central and provincial governments significantly influence the revenues and expenditures of LGs under their jurisdiction via budget transfers. Prefectures serve as an intermediate level of government between the counties and provinces, which not only allows us to capture the redistributive transfers made at higher level governments to some extent but also give us sufficient variation in the data. Table 1 presents summary statistics of our main outcome measures across prefectures for our sample period.

4.1 Measuring local exposure to trade liberalization

Our empirical exercises leverage geographic variation in exposure to trade liberalization, in particular access to U.S market, across Prefectural cities following China's accession to WTO in 2001. More specifically, prior to 2001, the U.S congress votes annually to renew China's Normal Trade Relation (NTR) status in the U.S market. If China's NTR status is approved in congress, Chinese goods will be subject to low NTR tariffs (MFN tariff rates reserved for WTO members). If congress votes against China's NTR status, however, Chinese exports to the U.S. market would be subjected to very high non-NTR tariff rates (also known as column 2 tariff rates) usually imposed on nonmarket economies. The non-NTR tariff rates were determined under Smoot-Hawley Tariff Act of 1930 (Pierce and Schott, 2016). In October 2000, the U.S. congress voted to grant Permanent Normal Trade Relation (PNTR) status to China which would become effective upon China's accession to WTO in 2001.

Though the U.S congress's annual vote on the bill to revoke China's NTR status were all concluded with failure, prior to 2001 Chinese firms faced significant uncertainty in their access to the U.S market because firms were not certain whether their products would be subjected to the low NTR tariff rate or the very high non-NTR tariff rate. Moreover, because the NTRgap, difference between the non-NTR and NTR tariff rates, varied massively across industries, firms (products) in different industries faced significantly different level of uncertainty in their access to the U.S market. We exploit this significant heterogeneity in the level of tariff uncertainty

across industries together with geographic variation in the concentration of industries to construct prefecture level exposure to trade liberalization. [Erten and Leight \(2021\)](#) use similar methodology to study the effect of trade liberalization on structural transformation in China while [Facchini et al. \(2019\)](#) use it to study migration.

Our prefecture-level exposure to China’s accession to WTO (granting of PNTR) is given as:

$$\text{NTRgap}_c = \sum_j \alpha_{jc} \text{NTRgap}_j \quad (1)$$

where NTRgap_j the difference between the non-NTR and NTR tariff rates for industry j ; α_{jc} is industry j ’s employment share in prefecture c in 1998. Our industry classification includes 489 four-digit industries in Chinese Industrial Classification for 2002 (CIC 2002).

Figure 1 presents the distribution of NTRgap across prefectures. In an average prefecture, tariff uncertainty on Chinese exports to the U.S decreased by 25 percentage points. However, notice that there is significant heterogeneity across prefectures ranging from 10 percentage points in the 1st percentile prefecture to 40 percentage points in the 99th percentile prefecture.

4.2 Baseline regression and hypothesis

To estimate the causal effect of trade liberalization on local public finances, we exploit prefecture-level variation in exposure to China’s Permanent Normal Trade Relations (PNTR) status with the United States, which was granted in 2000 and implemented in 2001. Our baseline econometric specification is:

$$Y_{ct} = \beta_0 + \beta_1(\text{PostPNTR}_t \times \text{NTRgap}_c) + \text{PostPNTR}_t \times \mathbf{X}_c' \delta + \gamma_c + \gamma_t + \varepsilon_{ct} \quad (2)$$

where Y_{ct} denotes the per-capita outcome variable in prefecture c and year t . Outcomes include local government revenue and expenditure categories (measured in per-capita terms using end-of-year population to account for migration-driven population changes). The variable PostPNTR_t is an indicator equal to one from 2001 onward and zero otherwise. The vector \mathbf{X}_c contains time-invariant prefecture characteristics measured prior to trade liberalization—most notably the share of manufacturing employment in state-owned enterprises (SOEs) and in foreign-affiliated firms (including wholly foreign-owned enterprises and joint ventures). These pre-reform characteristics are interacted with PostPNTR_t to absorb any differential trends that may arise from time-varying effects of initial industrial structure. Prefecture fixed effects γ_c and year fixed effects γ_t control for time-invariant prefecture heterogeneity and common national shocks, respectively. Standard errors are clustered at the prefecture level.

Hypothesis: We examine two core fiscal outcomes: local government revenue (Y_{ct}^r) and local government expenditure (Y_{ct}^e). Let β_1^r and β_1^e denote the estimated coefficients on the interaction term $\text{PostPNTR}_t \times \text{NTRgap}_c$ in the respective regressions. Trade liberalization is expected to generate heterogeneous local economic gains, with prefectures more exposed to U.S. market access experiencing larger increases in economic activity, employment, and taxable income. This should expand local tax bases and lead to higher own-source revenues. We therefore predict: $\beta_1^r > 0$.

A positive and statistically significant β_1^e on expenditures would indicate that fiscally empowered prefectures translate higher revenues into greater local public spending. Critically, in the absence of substantial inter-regional fiscal redistribution, we expect the expenditure response to closely track the revenue response: $\beta_1^e \approx \beta_1^r > 0$. Such a pattern would constitute evidence that regional fiscal transfers do not meaningfully offset the spatially uneven gains from trade liberalization. Conversely, a small or insignificant β_1^e ($\beta_1^e \approx 0$) despite a robust positive β_1^r would imply the presence of effective compensatory redistribution through higher-level fiscal mechanisms.

A parallel logic applies to specific categories of local public goods provision (education, health, infrastructure, etc.). Positive and significant estimates of β_1 in these regressions—particularly when expenditure responses are commensurate with revenue responses—would further indicate that decentralized fiscal responses dominate, leaving spatial inequality in public service provision largely unmitigated by inter-regional transfers.

4.3 Identification assumption

Identification and causal interpretation of β_1 relies on assumptions about exogeneity of regressor $\text{PostPNTR}_t \times \text{NTRgap}_c$. Notice that this can be alternatively written as $\sum_j \alpha_{jc} \text{NTRgap}_{jt}$ using equation 1, where NTRgap_{jt} becomes zero for all industries for the post PNTR period. This alternative writing of the regressor shows that our identification strategy is similar to the shift-share research design (Borusyak et al., 2021; Goldsmith-Pinkham et al., 2020), where the *shift* is change in tariff uncertainty across industries NTRgap_{jt} and the *share* is the employment share of these industries in the economy of prefectures, α_{jc} .

The literature suggests conditions under which $\sum_j \alpha_{jc} \text{NTRgap}_{jt}$ can be considered as an exogenous shock to prefecture level outcomes Y_{ct} . Borusyak et al. (2021) suggest that exogeneity is achieved as long as the “shifts” are exogenous, regardless of whether or not the “shares” are exogenous. This condition is plausibly satisfied in our setting because across industry variation in the NTRgap_{jt} is driven by Smoot-Hawley Tariff Act of 1930. The only other requirement for identification, other than exogenous shifts, is that shifts-level law of large numbers should hold, i.e., there should be *many*

independent shift-level shocks where each shock contributes small to the average exposure, which is likely guaranteed in our setup given that shift term captures variation across 489 industries and such high industry disaggregation means that each industry’s contribution to the prefecture’s economy is relatively modest. ⁷

5 Results

In this section we present our main estimation results. To facilitate comparison of the estimates across alternative estimation techniques, we standardize NTRgap measure, so that coefficients are interpreted as the effect of one standard deviation higher decrease in tariff uncertainty relative to the average prefecture. Because the distribution of the NTRgap measure can be approximated as normal, we often compare a prefecture at the 25th percentile against a prefecture at 75th percentile (i.e., two standard deviation difference in measured decrease in tariff uncertainty) to benchmark the magnitude of our estimates.

5.1 The Effect on Export and Local Economic Size

We first present evidence demonstrating that China’s granting of Permanent Normal Trade Relations (PNTR) by the United States triggered substantial economic expansion. While prior studies have documented similar effects, we strengthen and corroborate this evidence by incorporating nighttime luminosity data alongside official statistical measures. The use of satellite-based nighttime lights as a proxy for economic activity is a well-established practice, particularly in contexts where official data may be less reliable (Henderson et al., 2012; Martínez, 2022).

Concerns have long been raised regarding the potential manipulation of national income accounts by authoritarian regimes in general—and by local Chinese governments in particular—to overstate jurisdictional economic performance (Martínez, 2022; Fernald et al., 2021; Chen et al., 2019). Aggregate indicators such as GDP are especially vulnerable to such exaggeration, given their prominence as benchmarks of economic success. To address these concerns, we draw on nighttime luminosity data from the National Oceanic and Atmospheric Administration (NOAA). We construct prefecture-level panel measures of nighttime light intensity by computing the average digital number (DN) across all pixels within each prefecture. The DN values range

⁷Goldsmith-Pinkham et al. (2020) suggest an alternative condition under which the exogeneity can be achieved in the shift-share research design. In their approach exogeneity can be achieved as long as the “shares” α_{jc} are exogenous, regardless of whether or not the “shifts” are exogenous. In our setup, this assumption is strong because industrial composition of local economies could be related to the outcome variables via other channels. Hence, our identification strategy relies on the exogeneity of the “shifts”.

from 0 to 63, with higher values indicating greater luminosity and, by extension, more intense economic activity.

Table 2 examines the relationship between our measure of exposure to trade liberalization (the decrease in tariff uncertainty) and key economic outcomes: official GDP per capita growth, average nighttime light intensity growth, and export growth.

Column 1 reveals that a one-standard-deviation larger decrease in tariff uncertainty is associated with approximately 4.9% higher growth in GDP per capita. To illustrate the economic magnitude, comparing prefectures at the 75th percentile of tariff uncertainty reduction to those at the 25th percentile (roughly a 2-standard-deviation difference) implies that the more exposed prefecture experienced about 10% higher GDP per capita growth.

Column 2 shows that a one-standard-deviation larger decrease in tariff uncertainty corresponds to a 6.5% higher increase in nighttime luminosity. This magnitude is strikingly similar to the estimated effect on official GDP per capita growth, providing strong reassurance that the documented GDP effects are not merely artifacts of inflated official statistics.

Finally, Column 3 indicates that exports grew nearly 32% faster (relative to the mean) in prefectures experiencing a one-standard-deviation larger decrease in tariff uncertainty. Equivalently, prefectures at the 75th percentile of exposure saw export growth approximately 64% higher than those at the 25th percentile.⁸

Taken together, the consistent patterns across Table 2 provide robust evidence that the reduction in tariff uncertainty associated with PNTR induced significant real economic expansion in more exposed Chinese prefectures. As demonstrated in subsequent analyses, this expansion meaningfully enlarged the local tax base and boosted prefectural tax revenues.

5.2 The Effect on Government Size

We now turn to the effect of trade boom on LG sizes as measured by per capita revenues and expenditures of prefecture governments. Table 3 presents the effect of decrease in tariff uncertainty on prefecture-level government revenues and expenditures.

The results in Column 1 demonstrate that greater reductions in tariff uncertainty—driven by China’s PNTR accession—lead to substantial increases in LG per capita revenue. Specifically, a one-standard-deviation larger reduction in tariff uncertainty is associated with an additional 1,217 Yuan increase in per capita revenue. This implies economically meaningful differences across prefectures: a prefecture

⁸Note that the sample size in Column 3 is substantially smaller due to missing export data for many prefectures, which likely reflect limited or zero export activity. Imputing zero exports for these observations does not materially alter the point estimate.

at the 75th percentile of exposure (larger reduction in uncertainty) experiences an additional 2,434 Yuan (approximately \$294 at the 2002 average exchange rate of roughly 8.28 CNY/USD) in per capita revenue growth compared to a prefecture at the 25th percentile.

Column 2 reveals a closely parallel pattern for local government per capita expenditure. A one-standard-deviation larger reduction in tariff uncertainty corresponds to an additional 1,019 Yuan increase in per capita expenditure relative to the sample mean. Consequently, the 75th-percentile prefecture sees 2,038 Yuan (approximately \$246 in 2002 USD) more per capita expenditure growth than the 25th-percentile prefecture.

A formal statistical test of coefficient equality— $H_0 : \beta_1^r = \beta_1^e$ (where β_1^r and β_1^e are the estimated effects of trade exposure on revenue and expenditure, respectively)—yields a p-value of 0.758. This high p-value indicates that we fail to reject the null hypothesis of equal coefficients at any conventional significance level. The point estimates themselves are also similar in magnitude (1,217 Yuan vs. 1,019 Yuan per standard deviation), reinforcing that the fiscal response in expenditure closely tracks the revenue response.

The absence of a statistically or economically significant difference between revenue and expenditure effects points to limited effective inter-prefectural fiscal redistribution through higher-level government mechanisms. In other words, China's fiscal system during this period did not substantially counteract the spatially uneven gains from trade liberalization, allowing prosperous prefectures to retain and spend a large share of the additional resources generated by improved export access. This pattern underscores the decentralized nature of fiscal adjustment in the wake of trade shocks and highlights potential implications for regional inequality in public goods provision.

Below we provide further evidence on how the uneven gains from trade liberalization are reflected in specific categories of local public goods such as healthcare, education and roads.

5.3 The Effect on Education

To examine the impact on educational provision, we focus on the number of schools and teachers across primary, secondary, and tertiary levels. Panel A of Table 4 reports the estimated effects of reduced tariff uncertainty on the number of schools. The results indicate that a one-standard-deviation larger decrease in tariff uncertainty is associated with an increase of 4 primary schools per 1,000 people (column 1), 0.46 secondary schools per 1,000 people (column 2), and 0.033 tertiary institutions per 1,000 people (column 3). These point estimates imply economically meaningful

differences: prefectures at the 75th percentile of the decrease in tariff uncertainty exhibit, relative to those at the 25th percentile, approximately 8 more primary schools, 0.92 more secondary schools, and 0.066 more tertiary institutions per 1,000 people.

It should be noted that China experienced a widespread trend of school consolidation during the sample period, which led to a net decline in the total number of primary and secondary schools in many prefectures.⁹ However, this consolidation was markedly stronger in regions experiencing large population outflows due to migration and considerably weaker—or even reversed—in migrant-receiving prefectures. Consequently, our estimates likely reflect a differential pattern: prefectures that benefited more from trade liberalization experienced a substantially smaller decline (or an outright increase) in school numbers compared to less-favored areas.

Panel B of Table 4 presents parallel results using the number of teachers. A one-standard-deviation larger reduction in tariff uncertainty is associated with an increase of 0.547 primary school teachers per 1,000 people (column 1), 0.271 secondary school teachers (column 2), and 0.228 tertiary-level teachers (column 3). Comparing prefectures at the 75th and 25th percentiles of the treatment intensity yields increases of approximately 1.1 primary teachers, 0.54 secondary teachers, and 0.46 tertiary teachers per 1,000 people.

Taken together, these findings provide robust evidence that the fiscal windfall generated by reduced tariff uncertainty was systematically channeled into educational expansion at all levels. Both school and teacher numbers per capita increased across primary, secondary, and tertiary education. Notably, at the primary and secondary levels, the expansion in the number of schools outpaced the growth in teacher numbers, suggesting that new school construction may have partially constrained teacher hiring. At the tertiary level, by contrast, teacher numbers per capita grew more rapidly than the number of institutions, consistent with capacity expansion within existing universities and colleges.

We corroborate these infrastructure results with direct evidence on fiscal allocations in Table 5. A one-standard-deviation larger decrease in tariff uncertainty is associated with an increase in per capita expenditure on education of 393 Yuan (column 1) and on science of 176 Yuan (column 2). The 75th–25th percentile comparison implies additional increases of 794 Yuan in education spending and 352 Yuan in science spending per capita—differences that are both statistically significant and substantively large.

⁹An arguably superior measure of school infrastructure quality would be the number of schools (and teachers) per school-age child. Unfortunately, consistent annual data on school-age population at the prefecture level are unavailable.

5.4 The Effect on Healthcare Provision

We next investigate the impact on healthcare infrastructure, using the number of doctors, hospital beds, and healthcare facilities (hospitals and centers) as key outcomes. The results, reported in Table 6, are striking. A one-standard-deviation reduction in tariff uncertainty is associated with an increase of 0.34 doctors per 1,000 people (column 1) and 0.44 hospital beds per 1,000 people (column 3). These effects translate into substantial differences across the treatment distribution: prefectures at the 75th percentile experienced increases of 0.68 doctors and 0.88 beds per 1,000 people relative to those at the 25th percentile. For context, in 2002 the national averages stood at approximately 1.5 doctors and 1.95 beds per 1,000 people; the estimated effects therefore correspond to roughly a 45% increase relative to this baseline level in an average prefecture.

By contrast, column 2 shows no statistically or economically significant change in the number of hospitals and healthcare centers per 1,000 people. Indeed, this measure remained remarkably stable throughout the sample period. This pattern suggests that local governments responded to increased fiscal resources by intensifying investment within existing facilities—hiring more physicians and adding beds—rather than constructing new hospitals.

5.5 The Effect on Road Infrastructure Expansion

Finally, we assess the impact on transportation infrastructure, focusing on the area of paved roads as well as usage indicators (passenger numbers and freight volume). These measures are selected due to their consistent availability across most prefectures. Results are presented in Tables 7 and 8.

Table 7 shows clear positive effects on road infrastructure. A one-standard-deviation larger decrease in tariff uncertainty is associated with an increase of over 10,850,000 square meters in total paved road area (column 1) and an increase of 3.33 square meters in paved road area per capita (column 2). The 75th–25th percentile comparison implies differences of approximately 20.17 million square meters in total paved area and 6.66 square meters per capita—substantial expansions in road capacity.

Table 8 further reveals significant increases in passenger traffic on both highways and railways in prefectures experiencing larger reductions in tariff uncertainty. By contrast, no statistically significant effect emerges for freight volume, suggesting that the infrastructure improvements primarily enhanced personal mobility rather than goods transport capacity during this period.

6 Robustness of estimation and inferences

Recent literature highlights important limitations in the standard inference procedures commonly applied to shift-share research designs. [Adão et al. \(2019\)](#) demonstrate that regression residuals may exhibit correlation across locations that share similar industry employment compositions. Specifically, regions with comparable industry shares are likely to experience similar unobserved shocks, inducing correlation in the error terms that is driven by economic structure rather than pure geographic proximity. Conventional approaches, such as clustering standard errors by geographic units or higher-level administrative regions, fail to adequately account for this form of dependence, as the relevant correlation structure is tied to industrial similarity rather than spatial contiguity.

[Borusyak et al. \(2021\)](#) propose a transparent and effective solution to this inference challenge, which we adopt in the present analysis.¹⁰ Their method builds on the core identifying assumption of shift-share designs—that the underlying shocks are quasi-randomly assigned—and reformulates the estimation as an equivalent shock-level (industry-level) regression. This transformation is obtained by reweighting the location-level variables using the industry employment shares, thereby shifting the unit of analysis from locations to the shocks themselves. In our application, we implement this transformation by converting the original prefecture-level panel data into an industry-level panel using prefecture-specific industry employment shares. For example, let Y_{ct} denote a prefecture-level outcome variable (such as government revenue) in prefecture c and year t . The corresponding industry-level outcome for industry j in year t is constructed as $Y_{jt} = \sum_c \alpha_{cj} \times Y_{ct}$ where α_{cj} represents the employment share of industry j in prefecture c .¹¹ The same reweighting is applied to the independent variable of interest, the controls, and any other relevant variables. The resulting industry-level regression is econometrically equivalent to the original prefecture-level specification.

Importantly, the transformed industry-level variables do not require direct economic interpretation. Outcomes such as government revenue, public expenditure, the number of primary schools, or hospital beds are inherently defined at the location level, and their aggregation to the industry level carries little substantive meaning. Nevertheless, as [Borusyak et al. \(2021\)](#) emphasize, the transformation remains statistically valid even when the reweighted variables lack clear economic content, and the resulting coefficient estimates retain the same causal interpretation as those obtained

¹⁰[Adão et al. \(2019\)](#) develop a related inference procedure that addresses cross-regional residual correlation in a similar spirit to the approach advanced by [Borusyak et al. \(2021\)](#).

¹¹More precisely, we first residualize the prefecture-level variables by partialling out prefecture and year fixed effects as well as the control variables, following the recommendations in [Borusyak et al. \(2021\)](#), before applying the share-based transformation.

from the location-level regression.

Applying this approach to our baseline specification (Equation 2), we estimate the following industry-level equivalent:

$$Y_{jt} = \beta_0 + \beta_1(\text{PostPNTR}_t \times \text{NTRgap}_j) + \text{PostPNTR}_t \times \mathbf{X}_j' \delta + \gamma_j + \gamma_t + \varepsilon_{jt}, \quad (3)$$

where j indexes industries, \mathbf{X}_j is a vector of industry-level covariates, and γ_j and γ_t denote industry and year fixed effects, respectively. This reparameterization preserves the interpretation of the key coefficient β_1 from the original prefecture-level model.¹²

A central identifying assumption in quasi-experimental shift-share designs is that the underlying shocks are weakly correlated (or uncorrelated) with one another. The validity of this assumption can be tested empirically, and appropriate inference can be conducted by clustering standard errors at a suitable level of industry aggregation. A major advantage of the industry-level regression is that it naturally permits clustering at the industry (shock) level, thereby addressing potential correlation among shocks more directly.

Following [Borusyak et al. \(2021\)](#), we estimate intra-industry correlation coefficients for the shocks using a random-effects model at the two-digit, three-digit, and four-digit industry classifications. The results, reported in Table 9, indicate that the estimated correlations are statistically insignificant at the two- and three-digit levels but become significant at the four-digit level. This pattern suggests that the most conservative and appropriate clustering level is the four-digit industry classification.

We present the industry-level estimation results in Tables A.1 through A.6. All specifications cluster standard errors at the level of 487 four-digit industries and include industry and year fixed effects along with the appropriately transformed control variables from the prefecture-level model. Comparing these results to their prefecture-level counterparts in the main tables yields two key findings.

First, the point estimates from the prefecture-level and industry-level regressions are generally similar in magnitude across specifications.¹³ Second, standard errors in the prefecture-level regressions (clustered geographically) are systematically larger than those in the industry-level regressions (clustered at the four-digit industry level). This pattern is noteworthy because theory predicts that clustering at the shock level should, if anything, produce more conservative inference when residual correlation follows industrial composition patterns. The fact that industry-level clustering yields tighter standard errors implies that, while correlation across locations with similar

¹²Here Y_{jt} can be understood as a weighted average of the outcome (e.g., number of hospital beds) across prefectures, with weights proportional to industry j 's employment share in each prefecture.

¹³Small differences in point estimates between the two specifications may arise from the unbalanced nature of our panel data. [Borusyak et al. \(2021\)](#) further note that such discrepancies can reflect cross-industry or cross-location spillovers as well as heterogeneous treatment effects.

industry structures is a valid theoretical concern, it appears to be of limited practical importance in our setting.¹⁴

Testing the parallel trend assumption

In this section, we evaluate a key identifying assumption underlying the shift-share research design: the conditional orthogonality of the shocks (i.e., that the industry-level shocks are uncorrelated with unobserved determinants of the outcomes conditional on the included controls and fixed effects). Following the practical guidance in [Borusyak et al. \(2021\)](#), we assess the plausibility of this assumption in our context through two complementary diagnostic tests: (i) industry-level covariate balance checks and (ii) pre-trend tests in an event-study framework. Both tests are implemented using the shock-level (industry-level) regression specification, which permits clustering standard errors at the level of the shocks themselves—thereby providing appropriately conservative inference.

We begin by examining pre-trends for a selection of key outcome variables, presented in Figures 3 through 5. These figures display the estimated coefficients on `NTRgap` interacted with year indicators, normalized relative to the coefficient in the baseline year 2001 (omitted category). Across the pre-PNTR period (prior to 2001), the time-varying coefficients are consistently small in magnitude, exhibit inconsistent signs, and are either statistically insignificant or only marginally significant. In contrast, the coefficients become statistically significant and grow in magnitude starting typically from 2003 onward, consistent with the timing of China’s PNTR status and the associated reduction in tariff uncertainty. These patterns provide strong evidence against the presence of differential pre-existing trends that could confound our main estimates. Instead, the results support the interpretation that the documented effects reflect a genuine, period-specific response to the decline in trade policy uncertainty rather than spurious secular trends.

Next, we conduct formal covariate balance tests, reported in Table 10. The table focuses on two predetermined industry-level covariates measured in 1998: the employment share in foreign-owned firms and the employment share in state-owned enterprises (SOEs).¹⁵ To perform the balance test, we first construct shock-level

¹⁴Consistent with this interpretation, the three-digit-industry-clustered standard errors in the replication exercise of [Adão et al. \(2019\)](#) by [Borusyak et al. \(2021\)](#) are very similar in size to the commuting-zone-clustered standard errors originally reported in [Adão et al. \(2019\)](#).

¹⁵We also experimented with several additional control variables commonly employed in the literature on China’s WTO accession and trade liberalization, including measures of contract intensity, production subsidies, export licensing requirements, and MFA quota bindings (see, e.g., [Facchini et al. \(2019\)](#); [Erten and Leight \(2021\)](#)). These variables exhibited negligible explanatory power—both individually and jointly—in our setting, consistent with findings in the referenced studies. Their inclusion or exclusion therefore has no material impact on our main coefficient estimates.

versions of these covariates by applying the industry employment shares to the prefecture-level values, then regress each transformed covariate on NTRgap (along with the full set of fixed effects). The results indicate that the employment share in foreign-owned firms is essentially uncorrelated with NTRgap, supporting the exogeneity of exposure in this dimension. However, the employment share in SOEs shows a statistically significant correlation with NTRgap, raising a potential concern for identification.

To evaluate whether this correlation poses a meaningful threat to causal interpretation, we follow the recommendation in [Borusyak et al. \(2021\)](#) and assess the robustness of our main findings to both including and excluding the SOE employment share (as well as other covariates) in the baseline specification. Across all specifications, our core estimates of the effect of PNTR on the outcomes of interest remain highly stable and statistically significant. This robustness strongly suggests that the observed correlation between NTRgap and SOE employment shares does not drive our results and that the identifying assumption of conditional shock orthogonality holds credibly in our application.

We provide further diagnosis of the distribution of the shocks and the industry shares in the appendix.

7 Mechanisms

In [Section 5](#), we establish that prefectures with greater exposure to trade liberalization experienced a substantial expansion in their tax base, as evidenced by accelerated growth in GDP and exports. This section provides a more granular examination of the mechanisms underlying this tax-base expansion by investigating how trade liberalization affected the revenue bases of the major tax categories that dominate local and central government revenues in China.

Value-added tax (VAT), corporate income tax, and personal income tax collectively account for more than half of total government revenue. These taxes are shared between the central government and local governments (LGs) in the following proportions: VAT (75–25), corporate income tax (60–40), and personal income tax (60–40). In addition, LGs derive a non-negligible portion of their revenues from land- and property-related taxes, including real estate taxes.

[Table 12](#) reports the estimated effects of trade liberalization on key indicators of manufacturing activity and the tax bases for the primary tax categories. Column (1) indicates that gross industrial production (across private, state-owned, and foreign-owned firms) rose by 18 percent in prefectures with one standard deviation (s.d.) higher exposure to trade liberalization. Column (2) shows a corresponding 11 percent increase in total sales revenue. Column (3) reveals that pre-tax profits—the

direct basis for corporate income tax—increased by 13.2 percent. Finally, Column (4) demonstrates that VAT revenue, which constitutes nearly one-third of total government revenue, grew by 6.7 percent in response to a one-s.d. increase in exposure. Taken together, these results indicate a robust expansion in the tax bases for corporate income tax and VAT, which jointly represent nearly half of total revenue accruing to both central and local governments.

Personal income tax contributes over 5 percent of total government revenue. Table 13 examines the implications of trade liberalization for the personal income tax base. Column (1) shows that total employment increased by 15 percent in prefectures with one-s.d. higher exposure. This finding aligns with a growing body of evidence documenting large-scale labor reallocation and worker migration toward regions more exposed to trade liberalization (Facchini et al., 2019; Imbert et al., 2022). Column (2) further reveals an 8.9 percent increase in total wage payments—the foundation of the personal income tax base—following the same exposure shock.

Several major revenue sources are reserved exclusively for LGs, including social security contributions, deed taxes, urban maintenance and development tax, land appreciation tax, urban land use tax, and house property tax. Social security contributions are closely tied to employment and wage levels, both of which rise significantly in more exposed prefectures (as shown in Table 13).

Table 14 evaluates the impact of trade liberalization on fixed-asset investment, which serves as a proxy for the tax base underlying land- and property-related taxes. Column (1) indicates that total fixed assets increased by 14.1 percent in response to a one-s.d. rise in exposure. Column (2) reports an 8.9 percent increase in fixed-asset investment overall, while Column (3) shows that this growth is predominantly driven by a striking 18.9 percent rise in investment in residential real estate. These patterns provide compelling suggestive evidence that the tax bases for property-related levies expanded substantially in prefectures benefiting from trade liberalization.

Finally, land sales represent a critical revenue source for Chinese LGs, with land-sale revenues surging markedly during our sample period. Table A.7 investigates whether this boom bears any systematic relationship to exposure to trade liberalization. The results indicate no significant association: Column (1) finds no meaningful link between the area of land sold and the trade shock; Columns (2) and (3) similarly show insignificant effects on land-sale revenue and profit.

In summary, the evidence presented in Tables 12–14 demonstrates that trade liberalization induced significant and broad-based expansion in the tax bases of major revenue categories. In particular, heightened industrial production translated into higher profits (and thus greater corporate income tax revenue) and increased value added (and thus higher VAT collections). Concurrently, employment growth drove substantial increases in wage bills, bolstering personal income tax and social

security contribution revenues. Collectively, these categories account for more than 80 percent of total tax revenue accruing to LGs. In contrast, revenue from land sales shows no systematic connection to the reduction in trade policy uncertainty.

Feler and Senses (2017) document that, in the United States, exposure to import competition from China eroded LG fiscal capacity by triggering relative declines in housing prices and business activity—the primary bases for local property and sales taxes. Our findings highlight a parallel mechanism operating in the opposite direction: Chinese prefectures more exposed to trade liberalization experienced pronounced growth in industrial production, profits, fixed-asset investment (especially in real estate), employment, and wage incomes. These channels collectively generated a substantial strengthening of LG fiscal capacity, thereby enhancing their ability to finance and deliver improved public goods and services within their jurisdictions.

8 Implications for Regional Inequality

The analysis in the preceding sections demonstrates that trade liberalization contributed to a disproportionate expansion of public spending and public goods provision in prefectures more exposed to trade relative to those less exposed. This section examines the implications of these uneven distributional effects for regional inequality over time. To assess this, we compute two standard measures of between-prefecture inequality in government budgets and public goods provision for each year from 2000 onward.

Table 15 reports these results. Panel A presents the inter-quartile range (IQR) for each measure, normalized relative to the base year 2000 to facilitate interpretation of trends. The findings reveal a marked increase in inequality across most dimensions. The IQR for per capita government expenditure rose by more than sevenfold between 2000 and 2013, while the IQR for per capita government revenue increased fivefold over the same period. Similar upward trends in inequality are observed for key public goods, including the number of doctors, tertiary schools, and teachers in secondary and tertiary education (all expressed per 1,000 people). In contrast, inequality in the number of primary and secondary schools declined over time. This exception is largely attributable to China’s nationwide school consolidation policy, which substantially reduced the total number of such schools. Although the policy was implemented countrywide, its effects were more pronounced in regions experiencing out-migration and population decline, leading to sharper reductions in school numbers in those areas.

Panel B employs an alternative inequality measure—the difference between the 90th and 10th percentiles (90-10 gap)—again normalized to 2000. This metric captures changes in extreme inequality between the top and bottom deciles. The pat-

terns are broadly consistent with those in Panel A, albeit somewhat less pronounced. Overall, these results indicate that both inequality among initially more unequal regions and extreme disparities between the most and least advantaged prefectures widened over the period, with the former showing particularly dramatic growth.

These findings contribute to a broader literature documenting trade-induced inequality. In developed economies, globalization has been linked to rising disparities through mechanisms such as income redistribution from lower- to higher-skilled groups or regions (Autor et al., 2013; Hakobyan and McLaren, 2016; Case and Deaton, 2020; Borusyak and Jaravel, 2024).¹⁶ Parallel evidence exists for developing countries (Pavcnik, 2017; Helpman et al., 2016; Dix-Carneiro et al., 2018; Bombardini and Li, 2020; Li and Coxhead, 2011; Zhu et al., 2024). While much of this work focuses on individual- or sector-level income inequality, fewer studies examine inter-regional dimensions. Our results extend this literature by showing that trade liberalization can also widen regional inequality—specifically through divergent trajectories in local government revenues and public goods provision—even in a setting with substantial inter-governmental fiscal transfers such as China. This persistence of disparities despite redistribution suggests that mitigating trade-induced regional inequality may require more targeted policies aimed at supporting adversely affected regions.

9 Conclusions

This study investigates whether the geographically asymmetric gains from trade liberalization are reflected in localized public goods provision or are effectively redistributed across regions to mitigate spatial inequality. Leveraging the U.S. granting of Permanent Normal Trade Relations (PNTR) status to China as a quasi-experimental shock, we find robust evidence that Chinese prefectures with greater exposure to the policy experienced significantly faster growth in per capita GDP, exports, and local government revenues. Critically, these same high-exposure prefectures also exhibited accelerated expansion in public goods provision relative to less-exposed counterparts.

A key testable implication of effective inter-regional redistribution would be that winning regions experience relative increases in fiscal revenues but not in public goods spending, as surplus revenues are transferred to support lagging regions and equalize outcomes nationwide. However, our results decisively reject this pattern: high-exposure prefectures show substantial and statistically significant relative increases across multiple dimensions of public infrastructure and services. These include (i)

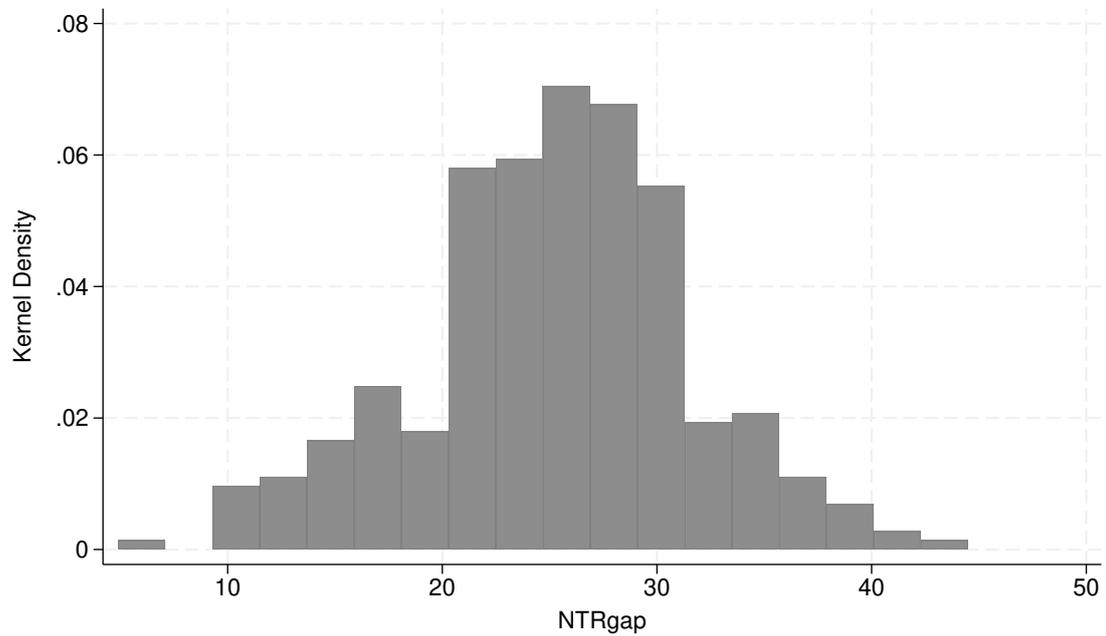
¹⁶Notably, Case and Deaton (2020) highlight how globalization exacerbates income inequality by shifting resources toward the affluent; see also Rodrik’s overview https://drodrik.scholar.harvard.edu/files/dani-rodrik/files/a_primer_on_trade_and_inequality.pdf.

expansions in the number of primary, secondary, and tertiary schools as well as teaching staff; (ii) growth in hospital beds and medical personnel; and (iii) extensions in paved road coverage.

These findings provide compelling evidence that regional fiscal redistribution mechanisms in China have been insufficient to offset the spatially concentrated benefits of trade liberalization. Rather than dampening trade-induced disparities through compensatory transfers, the observed pattern indicates that gains in economic activity and fiscal capacity in export-oriented regions translate directly into enhanced local public goods provision. Consequently, trade-induced regional income inequalities—such as those between more export-integrated coastal or urban areas and less exposed inland or rural regions—are not only sustained but actively reinforced by parallel inequalities in access to education, healthcare, and transportation infrastructure. In the absence of stronger redistributive policies, trade liberalization risks exacerbating spatial divergence in both private incomes and public service quality, with potentially long-lasting implications for social cohesion, human capital formation, and balanced national development. Our results thus highlight the importance of robust inter-governmental transfer systems in ensuring that the aggregate welfare gains from globalization are more equitably shared across space.

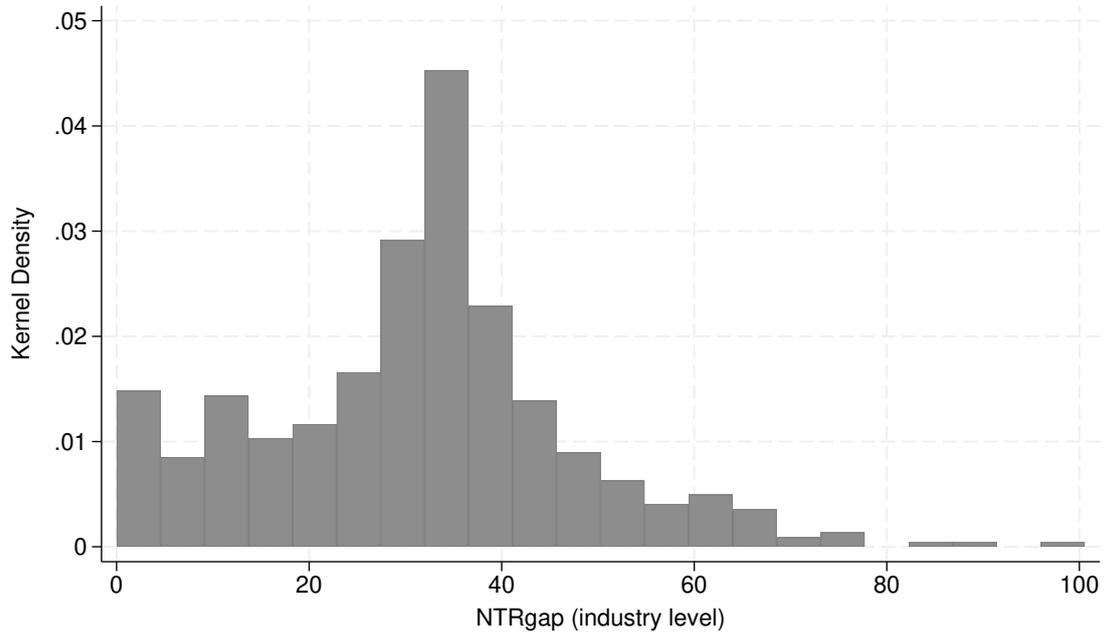
Figures

Figure 1: Variation in the NTRgap across Prefectures



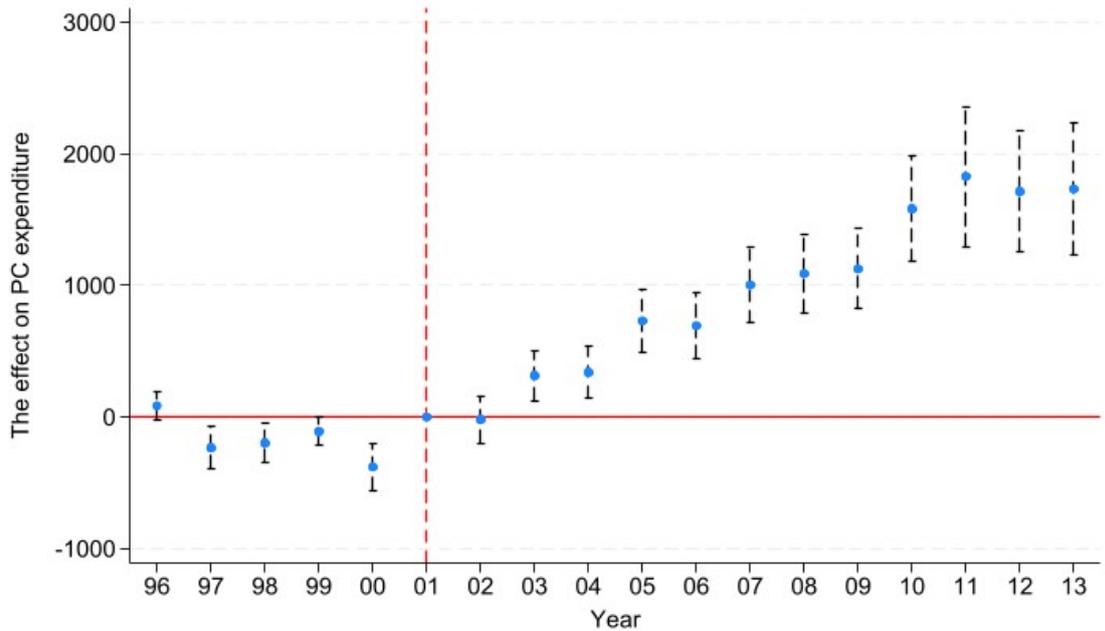
Notes: This figure presents variation in NTRgap calculated as $NTRgap_c = \sum_j \alpha_{jc} NTRgap_j$ across prefecture cities, where where $NTRgap_j$ the difference between the non-NTR and NTR tariff rates for industry j ; α_{jc} is industry j 's employment share in prefecture c in 1998.

Figure 2: Variation in the NTRgap across Industries



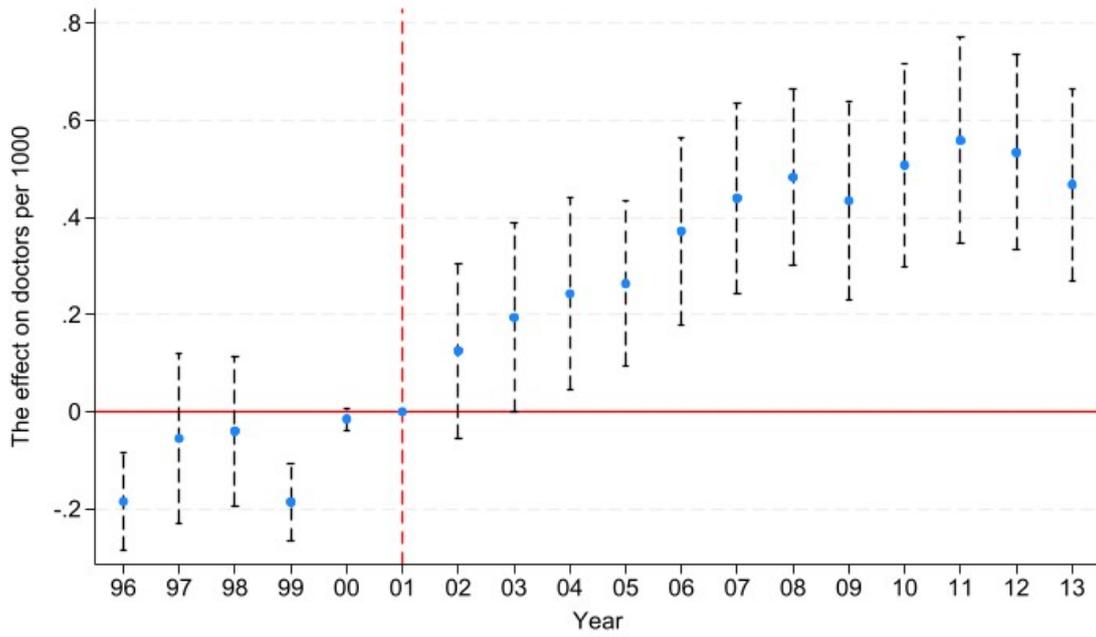
Notes: This figure presents variation in industry level $NTRgap_j$ which is the difference between the non-NTR and NTR tariff rates for industry j .

Figure 3: Dynamic effects: PC government expenditure



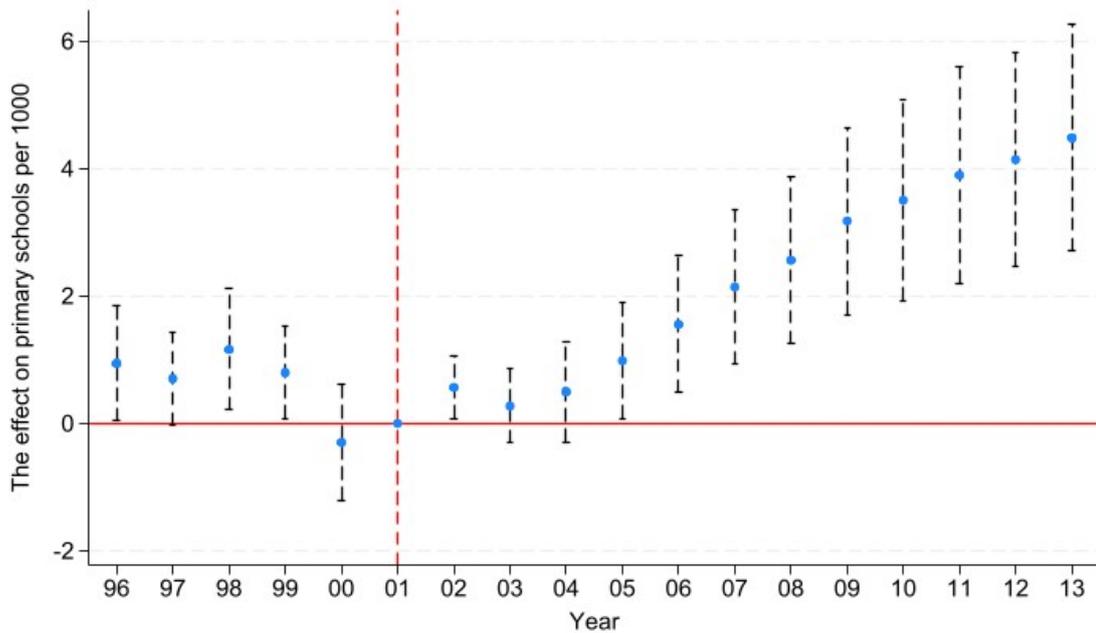
Notes: This figure presents dynamic effects on PC government expenditure of decrease in tariff uncertainty. Results are based on industry-level regressions.

Figure 4: Dynamic effects: number of doctors per 1000 people



Notes: Notes: This figure presents dynamic effects on number of doctors per 1000 people of decrease in tariff uncertainty. Results are based on industry-level regressions.

Figure 5: Dynamic effects: number of primary schools per 1000 people



Notes: Notes: This figure presents dynamic effects on number of primary schools per 1000 people of decrease in tariff uncertainty. Results are based on industry-level regressions.

Tables

Table 1: Summary statistics

	Mean	S.d
Log GDP per capita	9.51	0.94
Log Nightlight	0.87	1.11
Log Exports	9.52	4.00
Per capita budget expenditure	2830.95	4314.29
Per capita budget revenue	1625.95	3059.87
Doctors (per 1000 people)	1.81	1.06
Hospital (per 1000 people)	6.53	9.52
Hospital beds (per 1000 people)	3.20	1.68
Primary schools (per 1000 people)	31.61	22.93
Secondary schools (per 1000 people)	6.08	1.70
Tertiary schools (per 1000 people)	0.15	0.19
Primary school teachers (per 1000 people)	4.55	1.10
Secondary school teachers (per 1000 people)	3.75	0.84
Tertiary school teachers (per 1000 people)	0.70	1.05
Per capita education expenditure	775.25	837.44
Per capita science expenditure	88.24	304.05
Paved road area (1000 square meters)	1301.93	1909.71
Per capita paved road (in meter squares)	9.37	6.24
Highway passengers per capita	19.20	39.12
Railway passenger per capita	1.30	1.90
Highway freight (tones per person)	16.76	49.34
Railway freight (tones per person)	4.22	18.39

Notes: This table presents summary statistics of main outcome variables over the sample period. All monetary values are in constant 2010 Yuan.

Table 2: The effect on real GDPPC and night time light

	(1)	(2)	(3)
	Log Real GDPPC	Log Average Nightlight	Log export
PostNTR \times <i>NTRgap</i>	0.049*** (0.012)	0.065*** (0.013)	0.322* (0.172)
<i>N</i>	3791	3751	2804
<i>R</i> ²	0.967	0.930	0.874

Notes: The estimation is based on Prefecture-level data between the 1997 and 2013. All regressions include Prefecture and year fixed effects. Standard errors are clustered at Prefecture level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: The effect on government per capita expenditure and revenue

	(1)	(2)
	Expenditure	Revenue
PostNTR \times <i>NTRgap</i>	1218.237*** (435.899)	1020.218** (473.013)
<i>N</i>	4261	4262
<i>R</i> ²	0.813	0.734
Mean of d. v	2830.95	4314.29

Notes: The estimation is based on Prefecture-level data between the 1996 and 2013. All regressions include Prefecture and year fixed effects. Standard errors are clustered at Prefecture level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: The effect on education (measured per 1000 people)

	(1)	(2)	(3)
	Primary School	Secondary School	Tertiary School
Panel A: Number of schools			
PostNTR \times <i>NTRgap</i>	4.106* (2.274)	0.460*** (0.170)	0.033** (0.016)
<i>N</i>	4315	4139	4147
<i>R</i> ²	0.776	0.809	0.851
Mean of d. v	31.61	6.08	0.15
Panel B: Number of teachers			
PostNTR \times <i>NTRgap</i>	0.547*** (0.185)	0.271** (0.111)	0.228*** (0.087)
<i>N</i>	4322	4321	4136
<i>R</i> ²	0.794	0.727	0.861
Mean of d. v	4.55	3.75	0.70

Notes: The estimation is based on Prefecture-level data between the 1996 and 2013. All regressions include Prefecture and year fixed effects. Standard errors are clustered at Prefecture level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: The effect on expenditure on education and science (in per capita terms)

	(1)	(2)
	Educational expense	Expenditure on science
PostNTR \times <i>NTRgap</i>	393.116*** (90.278)	176.351*** (59.802)
<i>N</i>	3496	3495
<i>R</i> ²	0.818	0.522
Mean of d. v	775.25	88.24

Notes: The estimation is based on Prefecture-level data between the 1996 and 2013. All regressions include Prefecture and year fixed effects. Standard errors are clustered at Prefecture level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: The effect on health care facilities (measured per 1000 people)

	(1)	(2)	(3)
	Doctors	Hospitals	Hospital beds
PostNTR \times <i>NTRgap</i>	0.339*** (0.110)	-0.935 (0.767)	0.442*** (0.135)
<i>N</i>	4294	4289	4299
<i>R</i> ²	0.735	0.519	0.846
Mean of d. v	1.81	6.53	3.20

Notes: The estimation is based on Prefecture-level data between the 1996 and 2013. All regressions include Prefecture and year fixed effects. Standard errors are clustered at Prefecture level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: The effect on paved roads

	(1)	(2)
	Paved road area (1000 meter squares)	Paved road per capita (meter square per person)
PostNTR \times <i>NTRgap</i>	1085.366*** (181.758)	3.327*** (1.022)
<i>N</i>	3440	3440
<i>R</i> ²	0.868	0.883
Mean of d. v	1301.93	9.37

Notes: The estimation is based on Prefecture-level data between the 1996 and 2013. All regressions include Prefecture and year fixed effects. Standard errors are clustered at Prefecture level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 8: The effect on transport facilities (measured in per capita values)

	(1)	(2)	(3)	(4)
	Highway passengers	Railway passengers	Highway freight	Railway freight
PostNTR \times NTRgap	18.746*** (4.975)	0.259** (0.120)	0.874 (1.146)	-0.974 (0.783)
N	4256	3848	4257	3913
R^2	0.634	0.631	0.371	0.252
Mean of d.v	19.20	1.30	16.76	4.22

Notes: The estimation is based on Prefecture-level data between the 1996 and 2013. All regressions include Prefecture and year fixed effects. Standard errors are clustered at Prefecture level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9: Correlation of shocks within industry classes

Post2002 \times NTRgap	Coefficient	Std. err.
CIC2d	0.3356	0.3470
CIC3d	0.1029	0.1364
CIC4d	0.4812	0.2109

Notes: This table reports within industry-class correlation in NTRgap shock by pooling before and after 2002 periods. The first row shows correlation within 2-digit CIC codes (32), the second row shows correlation within 3-digit CIC codes (168) and the third row shows correlation within 4-digit CIC codes (487).

Table 10: Pre-shock covariate balance

	(1)	(2)
	Share of employment in foreign owned firms	Share of employment in state owned firms
NTRgap	0.118 (0.074)	-0.097*** (0.013)
N	487	487
R^2	0.016	0.095

Notes: This table reports regression of our key control variables (share of employment in foreign owned firms and share of employment in state owned firms in 1998) on NTRgap. The control variables are aggregated across prefectures at industry level using employment shares.

Table 11: Characteristics of normalized shares

	Inverse HHI (effective sample size)	Maximum share (across-industry years)
	1,628	0.0022
Number of Industries	487	
Number of Years	18	

Notes: This table reports inverse HHI of the normalized shares ($1/\sum_{j,t} \alpha_{jt}^2$) and the maximum of the shares across industry-years ($\max_{j,t} \{\alpha_{jt}\}$).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 12: Industrial Production, Sales Revenue, Profit and VAT

	(1)	(2)	(3)	(4)
	Industrial production	Sales Revenue	Profit	VAT
PostNTR \times <i>NTRgap</i>	0.180*** (0.025)	0.110*** (0.022)	0.132*** (0.044)	0.067** (0.028)
<i>N</i>	3254	2733	3866	3937
<i>R</i> ²	0.931	0.932	0.947	0.915

Notes: The estimation is based on Prefecture-level data between the 1997 and 2013. All regressions include Prefecture and year fixed effects. Standard errors are clustered at Prefecture level. Industrial production is gross value of industrial production by firms owned by private, state and foreign entities. Profit is total before tax profit of businesses. Sales revenue is total revenue from sales of products and VAT is value-added tax collected. All dependent variables are converted to real values using CPI and are measured in log units.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 13: Employment and wage growth

	(1)	(2)
	Employment	Total wages
PostNTR \times <i>NTRgap</i>	0.151*** (0.054)	0.089*** (0.018)
<i>N</i>	3944	3937
<i>R</i> ²	0.864	0.930

Notes: The estimation is based on Prefecture-level data between the 1997 and 2015. All regressions include Prefecture and year fixed effects. Standard errors are clustered at Prefecture level. Total wages is converted to real values using CPI. All dependent variables are measured in log units.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 14: Investment in fixed assets

	(1)	(2)	(3)
	Total Assets	Investment in fixed assets	Investment in residential realestate
PostNTR \times <i>NTRgap</i>	0.141*** (0.026)	0.089** (0.036)	0.189*** (0.044)
<i>N</i>	3937	3943	3931
<i>R</i> ²	0.924	0.933	0.856

Notes: The estimation is based on Prefecture-level data between the 1997 and 2015. All regressions include Prefecture and year fixed effects. Standard errors are clustered at Prefecture level. All dependent variables are converted to real values using CPI and are measured in log units.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 15: Trends in regional inequality

Year	Budget		Health		Schools			Teachers		
	Expe.	Rev	Beds	Doctors	Prim	Sec	Ter	Prim	Sec	Ter
Panel A: Inter-quartile ranges										
2000	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
2001	1.30	0.92	0.96	0.92	0.96	1.04	1.04	1.00	1.07	1.02
2002	1.56	0.84	0.94	0.92	0.92	1.07	1.13	1.01	1.17	1.35
2003	1.54	0.99	0.93	0.92	0.90	0.95	1.14	1.04	1.21	1.36
2004	1.88	1.15	0.96	0.90	0.91	1.01	1.14	1.10	1.19	1.64
2005	2.21	1.48	0.93	0.97	0.95	0.99	1.15	1.01	1.16	1.60
2006	2.88	1.90	0.96	1.02	0.91	0.90	1.34	1.07	1.08	1.94
2007	3.21	2.33	0.95	1.01	0.88	0.89	1.32	1.08	1.06	1.95
2008	3.93	2.77	0.94	1.14	0.92	0.83	1.33	1.04	1.08	2.13
2009	4.98	3.05	1.01	1.22	0.89	0.88	1.53	1.02	1.19	2.27
2010	5.80	3.54	1.11	1.19	0.83	0.86	1.42	1.05	1.18	2.34
2011	6.00	4.33	1.06	1.16	0.74	0.81	1.51	0.95	1.20	2.45
2012	6.42	4.88	1.09	1.14	0.74	0.78	1.48	1.01	1.23	2.29
2013	7.25	5.08	1.07	1.22	0.70	0.80	1.42	0.89	1.30	2.13
Panel B: 90th - 10th percentiles										
2000	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
2001	1.20	1.07	1.04	1.04	0.99	1.04	1.08	0.99	0.97	1.12
2002	1.17	1.04	0.95	0.88	0.99	1.06	1.07	1.07	0.99	1.31
2003	1.32	1.07	0.95	0.88	1.06	1.05	1.13	1.10	0.97	1.24
2004	1.45	1.35	0.95	0.85	1.05	1.03	1.12	1.13	1.06	1.61
2005	1.84	1.80	0.92	0.90	0.96	1.03	1.23	1.10	1.03	1.44
2006	2.17	2.16	0.96	0.93	0.90	0.99	1.25	1.16	1.00	1.64
2007	2.64	2.40	1.02	1.02	0.87	0.99	1.18	1.21	1.00	1.44
2008	2.97	2.67	1.04	1.02	0.80	0.97	1.19	1.09	1.00	1.55
2009	3.45	3.15	1.04	0.98	0.76	0.96	1.36	1.08	1.00	1.53
2010	3.76	3.51	1.20	1.03	0.73	0.87	1.36	1.06	0.97	1.44
2011	4.07	3.89	1.19	1.05	0.67	0.85	1.17	0.98	1.08	1.30
2012	4.48	4.27	1.20	1.06	0.72	0.86	1.34	1.03	1.09	1.38
2013	4.87	4.86	1.35	1.19	0.62	0.78	1.31	0.95	1.02	1.33

Notes: This table reports inequality across prefectures of public good measures for each year relative to the values in 2000. Budgets are defined in per capita terms. All other variables are defined as numbers per 1000 people.

Appendix A Diagnosis of distribution of the shocks and industry shares

In this subsection we provide diagnosis of key assumptions in our identification strategy following [Borusyak et al. \(2021\)](#). A key assumption of identification in the shift-share research design where identification comes from the exogeneity of the shifts or quasi-random “shocks” is that the number of shocks should be sufficiently large and should not be highly concentrated. Using our notations, these would mean that in the shocks $\text{NTRgap}_j, j \in \{1, \dots, J\}$: (i) J is sufficiently large, and (ii) there is sufficient variation in NTRgap_j across $j \in \{1, \dots, J\}$. In our setting, $J = 487$ (the number of industries in four-digit industry classification of China in 2002) and the distribution of NTRgap_j across industries is given in figure 2. The standard deviation and inter-quartile ranges of the distribution show that our industry shocks show sufficient variation.

Next, the exposure shares $\alpha_{jt} \propto \sum_c \alpha_{jct}$ ¹⁷ should not be highly concentrated. [Borusyak et al. \(2021\)](#) report two important diagnosis statistics to check the concentration of the normalized shares which we follow here. Panel A of Table 11 reports these statistic. The first is the inverse of the Herfindahl-Hirschman Index (HHI) of the shares calculated as $1/\sum_{j,t} \alpha_{jt}^2$. In our setting this statistics is 1,628, which according to standards set by [Borusyak et al. \(2021\)](#) is sufficiently large. This index is a measure of effective sample size. The second statistics we report is the maximum of the normalized share across industry-years, which is 0.0022. Again this shows that the normalized shares are not concentrated because the largest share is still very small (0.22%). Together, these two statistics suggest that the assumptions required for consistency of the estimator $\mathbf{E}[\sum_{j,t} \alpha_{jt}^2] \rightarrow 0$, as $J \times T \rightarrow \infty$ are satisfied in our setting.

Note that while the shares (α_{jc}) are calculated as industry employment relative to *total* employment in a prefecture, we exclude the shares for agriculture and service following [Borusyak et al. \(2021\)](#). This is for two reasons. First, because agriculture and service sectors account for significant share of employment (relative to each of the four-digit industries), including these sectors would lead to concentration of the shares. Second, the trade policy change does not affect services and affects agriculture only marginally since agriculture is not a major export commodity for China. In other words, these two sectors do not have the same treatment probability as the manufacturing industries. Including them in to the identifying variation would lead to serious violation of Assumptions 1 and 2 in [Borusyak et al. \(2021\)](#). Following their suggestion we include manufacturing employment share of prefectures in 1998

¹⁷ α_{jt} is employment shares aggregated across prefectures and normalized to add up to one across industry-years.

interacted with year fixed effects to account for different trajectories of prefectures with different initial manufacturing presence.

Appendix B Shock-level estimation results

Table A.1: The effect on government per capita expenditure and revenue

	(1)	(2)
	Expenditure	Revenue
PostNTR \times <i>NTRgap</i>	1130.875*** (153.127)	1148.762*** (148.132)
<i>N</i>	8765	8765
<i>R</i> ²	0.320	0.375

Notes: This table reports estimation results from (industry) shock-level transformation of our prefecture-level panel data used in the main regression. All regressions include industry and year fixed effects. Robust standard errors clustered at industry-level in parenthesis.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.2: The effect on education (measured per 1000 people)

	(1)	(2)	(3)
	Primary School	Secondary School	Tertiary School
Panel A: Number of schools			
PostNTR \times <i>NTRgap</i>	1.828*** (0.477)	0.469*** (0.043)	0.027*** (0.005)
<i>N</i>	8765	8757	8765
<i>R</i> ²	0.202	0.331	0.233
Panel B: Number of teachers			
PostNTR \times <i>NTRgap</i>	0.514*** (0.043)	0.306*** (0.027)	0.135*** (0.025)
<i>N</i>	8765	8765	8765
<i>R</i> ²	0.460	0.341	0.269

Notes: This table reports estimation results from (industry) shock-level transformation of our prefecture-level panel data used in the main regression. All regressions include industry and year fixed effects. Robust standard errors clustered at industry-level in parenthesis.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.3: The effect on expenditure on education and science (in per capita terms)

	(1)	(2)
	Educational expense	Expenditure on science
PostNTR \times <i>NTRgap</i>	267.131*** (35.320)	90.290*** (15.184)
<i>N</i>	5844	5844
<i>R</i> ²	0.322	0.387

Notes: This table reports estimation results from (industry) shock-level transformation of our prefecture-level panel data used in the main regression. All regressions include industry and year fixed effects. Robust standard errors clustered at industry-level in parenthesis.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.4: The effect on health care facilities (measured per 1000 people)

	(1)	(2)	(3)
	Doctors	Hospitals	Hospital beds
PostNTR \times <i>NTRgap</i>	0.399*** (0.061)	-0.055 (0.331)	0.493*** (0.078)
<i>N</i>	8765	8765	8765
<i>R</i> ²	0.218	0.048	0.234

Notes: This table reports estimation results from (industry) shock-level transformation of our prefecture-level panel data used in the main regression. All regressions include industry and year fixed effects. Robust standard errors clustered at industry-level in parenthesis.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.5: The effect on paved roads

	(1)	(2)
	Paved road area (1000 meter squares)	Paved road per capita (meter square per person)
PostNTR \times <i>NTRgap</i>	683.240*** (99.032)	1.829*** (0.245)
<i>N</i>	6331	6331
<i>R</i> ²	0.330	0.186

Notes: This table reports estimation results from (industry) shock-level transformation of our prefecture-level panel data used in the main regression. All regressions include industry and year fixed effects. Robust standard errors clustered at industry-level in parenthesis.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.6: The effect on transport facilities (measured in per capita values)

	(1)	(2)	(3)	(4)
	Highway passengers	Railway passengers	Highway freight	Railway freight
PostNTR \times <i>NTRgap</i>	17.808*** (1.417)	0.195*** (0.030)	0.450 (0.288)	-0.461*** (0.164)
<i>N</i>	8765	8765	8765	8765
<i>R</i> ²	0.242	0.113	0.025	0.056

Notes: This table reports estimation results from (industry) shock-level transformation of our prefecture-level panel data used in the main regression. All regressions include industry and year fixed effects. Robust standard errors clustered at industry-level in parenthesis.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.7: Trade shock and revenue from land sales

	(1)	(2)	(3)
	Log Area	Log Revenue	Log Profit
PostNTR \times <i>NTRgap</i>	-0.063 (0.090)	-0.121 (0.112)	0.033 (0.118)
<i>N</i>	2660	2553	1716
<i>R</i> ²	0.725	0.850	0.739

Notes: The estimation in columns 1-2 are based on Prefecture-level data between the 1999 and 2013. Column 3 is based on data for 1999-2007. All regressions include Prefecture and year fixed effects. Standard errors are clustered at Prefecture level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix C Theoretical framework

Consider an economy with arbitrary number of local governments (LGs) indexed by $i = 1, \dots, N, N \geq 2$, administered under a single central government. The central government sets statutory tax rates, defines tax bases, and delegates the responsibility to collect tax revenues and provide public goods to the LGs. The central government also makes decisions regarding external policies, such as foreign trade policies. The LGs share a fraction $\theta \in [0, 1)$ of the collected revenue to the central government, which the central government can use for its own purpose or redistributes back to the LGs.

The local economies have capitalists who pay tax on the surplus (call it ‘value-added’) they generate Y_i .¹⁸ Statutory rate of such tax s is determined by the central government. Suppose that capitalists can influence the LGs (elite capture). To appease the capitalists, the LGs could choose to partially enforce the statutory tax code by choosing effective tax rate of $0 \leq t_i < s$.¹⁹ Let $e_i = t_i/s$ is the enforcement effort. We assume that LGs face $C(e_i)$ enforcement costs. These costs include both political costs of upsetting elites and administrative costs. We assume this enforcement cost is linear in effort e_i , and the cost scaled by tax base Y_i is given by $C(e_i) = \gamma e_i Y_i$, where $s > \gamma > 0$ is enforcement cost intensity. After paying the central government’s share and enforcement costs, the LG invests in local public goods G_i . For now, we assume there is no redistribution from the central government. Below we show how redistribution affects the link between local tax base and local public goods.

When making decision about how much public good to provide, the LGs weigh the residents’ demand for public good against the capitalists demand for tax relief (lower effective tax rate). Assuming a simple log linear utility functions, the LG maximizes the following objective function a subject to the above constraint:

$$\max_{t_i \in [0, s], G_i} U_i(t_i, G_i) = \alpha_i [\ln G_i] + (1 - \alpha_i) [\ln(1 - t_i)Y_i] \quad (4)$$

where α_i are the weights attached to utility of the residents and $1 - \alpha_i$ are the weights attached to the capitalists’ demands. In LGs with strong elite capture, α_i is lower. The LG’s budget constraint can be written as

$$G_i \leq (1 - \theta - \frac{\gamma}{s})t_i Y_i \quad (5)$$

Under this simple functional form assumptions, we obtain the following closed

¹⁸Including workers who pay a constant fraction τ of wage income W_i they earn by inelastically supplying a unit of labor to capitalists gives similar conclusion.

¹⁹For instance, [Chen \(2017\)](#) shows that local authorities usually collect effective taxes that are far less than the statutory taxes, unless during periods of budget stress.

form solutions for the optimal effective tax rate and public goods:

$$t_i^* = \alpha_i \quad (6)$$

$$G_i^* = \alpha_i \phi_\theta Y_i \quad (7)$$

where $\phi_\theta = (1 - \theta) - \frac{\gamma}{s}$. Both t_i^* and G_i^* are positive unless $\alpha_i = 0$, i.e., full elite capture or we have a corner solution with enforcement costs larger than net local revenue $s(1 - \theta) \leq \gamma$. Higher elite capture is associated with lower taxation and lower public good provision.

Notice that the optimal effective tax rate is determined independent of the tax base by the exogenous weights attached to utility of residents α_i by the local government. This is a feature of our simplified model. However, the level of local public goods is influenced by expansion in the tax base $dG_i^* = \alpha_i \phi_\theta dY_i > 0$ and the higher θ , the weaker is the link between local tax base expansion and local public goods expenditure.

Implications for regional inequality: To see the implication of export boom for inequality through the lens of our simple model, we rewrite the optimal public goods as $G_i^* = \phi_\theta \alpha_i Y_i$. The variance of the G_i^* across LGs is given by $\text{Var}(G_i^*) = \text{Var}(\alpha_i \phi_\theta Y_i) = (\phi_\theta)^2 [E[Y_i^2] \text{Var}(\alpha_i) + E[\alpha_i^2] \text{Var}(Y_i) + 2E[\alpha_i] E[Y_i] \text{Cov}(\alpha_i, Y_i)]$. This suggests trade-induced increase in regional income inequality (increase in $\text{Var}(Y_i)$) will cause inequality in public goods provision across regions. However, this effect is attenuated by higher revenue sharing parameter θ .

The role of transfers: Consider the case where the central government remits its share of revenue to LGs. Such transfers T_i could help to mitigate inequality if it is designed as pro-poor (pro-low-tax base) regions. To see this, suppose the pro-poor targeting takes the following form $T_i = \omega_i B$ where $\omega_i = \frac{1/Y_i}{\sum_j 1/Y_j}$ (inverse-tax-base weighting). The central government budget constraint is $\sum_{i=1}^N T_i \leq \sum_{i=1}^N \theta t_i Y_i = B$. Under this transfer program, the optimal effective tax and public goods are given by:

$$\tilde{G}_i^* = \alpha_i \tilde{\phi}_i Y_i \quad (8)$$

where $\tilde{\phi}_i = \phi_\theta + \omega_i \theta = (1 - \theta) - \frac{\gamma}{s} + \omega_i \theta$. Here, the link between local tax base expansion and local public good provision is attenuated by inter-regional transfer. As a result, the variance of $\text{Var}(\tilde{G}_i^*) < \text{Var}(G_i^*)$, i.e., the redistribution reduces inequality in public goods.

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