

Embedded Fiscal Centralization: How Economic Openness Increased Fiscal Capacity and Redistribution in Contemporary China

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Abstract

When can trade openness strengthen rather than erode fiscal capacity? I argue that openness builds central fiscal capacity when export opportunities create new firms under the jurisdiction of a centrally controlled tax bureaucracy, limiting local influence over enforcement. China's World Trade Organization (WTO) accession provides a most-likely test: the 2002 enterprise income tax (EIT) reform placed new firms under central collection and gave the center a statutory revenue share. Using a city-level Bartik design that instruments export exposure with tariff-predicted export growth, I find that exposed cities registered more private firms, had more frontline tax bureaucrats in the central tax bureaucracy, and collected more centrally shared EIT. I also show that fiscal centralization enables selective compensation downstream: national revenue pooling leads to greater grants and fiscal dependence in cities where employment in state-owned enterprises (SOEs) declined more. These findings suggest that the fiscal effects of openness depend on how governments organize collection and pool revenue across places.

Keywords: trade liberalization, fiscal capacity, redistribution, taxation, China

1 Introduction

When does trade openness build, rather than erode, fiscal capacity? Existing scholarship generally predicts economic openness leads to fiscal erosion. Tax bureaucrats collect border taxes at visible entry points without detailed information about firms, profits, wages, or households. Trade liberalization can therefore remove dependable and easy revenue before governments replace it with more difficult value-added or income taxes (Baunsgaard & Keen, 2010; Cagé & Gadenne, 2018; Keen & Ligthart, 2002, 2005; Khattry & Rao, 2002). Yet openness also brings opportunities and changes the domestic economy on which taxation rests. Export opportunities can induce new firms to enter, hire workers, and earn profits. Such activity creates an observable domestic tax base (Bachas et al., 2022). Trade liberalization may therefore destroy revenue at the border while creating tax potential behind it. Whether that potential strengthens the fiscal center depends on who controls the new tax source and how the resulting receipts are distributed.

Existing research offers important insights into this process but pays less attention to the political and administrative conditions that govern it. Work on trade and public finance explains tariff losses and border-revenue tax replacement, but generally treats the collection of new tax as a technical process, overlooking central–local conflict and control over tax bureaucracy (Avi-Yonah, 2000; Bastiaens & Rudra, 2018; Boadway & Sato, 2009; Devereux et al., 2002; Gnangnon & Brun, 2019; Haque & Mukherjee, 2005; Mujumdar, 2004; Naito, 2006; Seelkopf et al., 2016; Slemrod, 2004; Sørensen, 2007). Research on fiscal capacity explains how information, enforcement, bureaucratic organization, technology, and political authority turn tax claims into revenue (Besley & Persson, 2009; Bräutigam et al., 2008; Levi, 1988; Moore, 2018; Okunogbe & Tourek, 2024; Pomeranz, 2015; Slemrod, 2019; Tilly, 1992). Yet neither literature explains how these mechanisms operate when fiscal authority is divided across levels of government. In multilevel states, the fiscal returns from newly registered firms are contested. Firms can bargain for favorable tax treatment and use political connections for tax favors. Local governments, seeking investment, employment, and locally retained revenue, may shield favored taxpayers or influence enforcement (Holland, 2016; Rodden, 2002). The central question is therefore not simply whether openness

expands taxable activity, but whether the center can limit local tax bargaining, direct reliable tax bureaucrats to where the caseload grows, and channel the resulting revenue into the central budget.

I argue that openness strengthens central fiscal capacity when export opportunities generate new registered firms with taxable activity that falls under central control. New firm registration increases the workload of a centrally controlled tax bureaucracy, while taxable activity creates potential revenue. The result is not automatic. Newly registered firms can bargain over tax treatment, reporting, and enforcement, while local governments can use their influence over implementation to protect local firms. A centrally controlled tax bureaucracy limits this bargaining by placing personnel decisions, taxpayer information, reporting requirements, and audit priorities under tax bureaucrats accountable to the center. Therefore, the combination of a larger caseload and expected revenue raises the return to deploying more central personnel at the frontline. Building on Ruggie (1982), I call the combination of registered-firm entry, frontline deployment within a central tax hierarchy, and central revenue capture in response to a trade-induced export boom *embedded fiscal centralization*.

The argument therefore predicts three observable responses in export-exposed cities: more private-firm registrations; more Central Tax Administration (CTA) hiring in county-level branches, which collect taxes, rather than prefectural headquarters; and faster enterprise income tax (EIT) growth relative to value-added tax (VAT) and direct central remittances, whose collection and sharing rules did not change. I use these three comparisons to test embedded fiscal centralization.

Fiscal centralization also has a downstream political implication: by bringing revenue under central control, it creates a national pool that can address the spatial mismatch between places that generate new revenue and places that bear concentrated adjustment costs. Embedded liberalism places general social protection within the political foundations of an open international economy (Adserà & Boix, 2002; D. R. Cameron, 1978; Nooruddin & Rudra, 2014; Rodrik, 1998; Ruggie, 1982). Related work explains why governments support groups exposed to market risk but often takes the requisite fiscal resources and administrative channels as given (Hiscox, 2001; Rodrik, 2000). In contrast, I show that, in authoritarian systems, once fiscal revenue has been

centralized, support commonly flows toward organized, visible, or state-linked claimants rather than through broad insurance alone (Albertus, 2024; Chen et al., 2016; R. M. Desai et al., 2009; X. Huang, 2020; Pan, 2020; H. Zhu, 2025). Fiscal spending is one instrument in that contested bargain, and recent work on backlash, trade conflict, and gated globalization underscores the political stakes surrounding openness (Broz et al., 2021; Farrell & Newman, 2023; Goldberg, 2023; L. Jiang et al., 2023; Zeng & Kim, 2025).

China after World Trade Organization (WTO) accession provides a most-likely test of the theory because the mechanism's scope conditions are especially favorable. Strong party-state authority over local regions, an existing CTA network, low dependence on tariff revenue, rapid growth in registered private firms, and intergovernmental channels capable of moving funds across space were jointly present (Choi, 2016; Y. Huang, 1996; Lou, 2008; Yang, 2004).¹ The 2002 EIT reform gave the center a statutory share of income-tax receipts and placed most newly established firms under CTA jurisdiction just as accession expanded opportunities for private firms and new market entrants (Brandt et al., 2017; Fan, Li, & Yeaple, 2015; Gallagher, 2005; Heise et al., 2024; State Administration of Taxation, 2002; State Council, 2001). In a word, China entered this period with substantial fiscal-administrative capacity. Given this institutional foundation, I test whether greater trade exposure activated existing capacity. The mechanism predicts a sequence of registered-firm entry, targeted expansion of frontline CTA branches, and a reform-aligned EIT composition within central taxes.

Empirically, I follow Chinese prefecture-level cities from 2000 to 2007 and use a city-level Bartik design to isolate differences in post-WTO export exposure. The exposure measure combines each city's pre-WTO industrial composition with national industry export growth over the period. Because realized national export growth may also reflect domestic supply conditions, I instrument this measure with tariff-predicted industry export growth constructed from product-level tariff reductions faced by Chinese exporters. The instrument maps these external market-access shocks to cities using the same pre-WTO industry shares, so the identifying variation comes from inherited differences in industrial composition interacted with common foreign tariff changes (Bartik,

1991; Bombardini & Li, 2020; Goldsmith-Pinkham et al., 2020; B. Li, 2018). The tariff-predicted measure strongly predicts realized exposure. China's state-owned enterprise (SOE) restructuring, by contrast, began before WTO accession and had domestic causes (Hurst, 2009; Naughton, 2018), so I treat export expansion and SOE decline as distinct processes. To test the argument's downstream implication—that national fiscal pooling can support places bearing concentrated adjustment costs—I use a separate two-period city panel.

I find that export exposure increased private-firm registrations and expanded frontline tax administration. A one-standard-deviation increase in exposure implies roughly 460 additional private firms and 15 additional foreign-invested firms between 2000 and 2007, while SOE entry did not change systematically. Moving from the 25th to the 75th percentile of exposure implies about six additional advertised CTA positions in 2007, relative to a city mean of eight; the response is concentrated in county-level branches, while the headquarters point estimate is essentially zero. Across the same exposure range, imputed central-share EIT is 48.4% higher, EIT's share of EIT plus VAT is 4.64 percentage points higher, and imputed central-share EIT relative to direct remittances is 24.8% higher. Imputed central-share VAT, whose underlying tax category was already under CTA administration before WTO entry, rises by 15.9%, a smaller and statistically imprecise estimate. The branch recruitment and reform-targeted fiscal-book EIT response align with the collection-jurisdiction and revenue-sharing rules introduced by the 2002 EIT reform. In addition, I show that a ten-percentage-point decline in the SOE employment share predicts 2.9% more in grants from central government, along with greater reliance on grant finance and more recorded retirees. Utility spending, other general spending, and public employment did not change comparably. Moreover, the cities in the top quartile of export exposure overlap only partially with those in the top quartile of SOE employment decline. This geographic separation is consistent with national fiscal pooling across cities.

The article makes three contributions. First, I bring central–local conflict and bureaucratic control into the study of trade and taxation. Research on liberalization explains tariff losses, domestic tax substitution, and tax competition (Bastiaens & Rudra, 2018; Baunsgaard & Keen, 2010;

Cagé & Gadenne, 2018; Keen & Ligthart, 2005; Seelkopf et al., 2016), but national revenue aggregates obscure which level of government captures the fiscal gains from openness and how bargaining over implementation shapes that allocation. I argue that tax laws do not collect revenue on their own: firms seek favorable treatment, local governments may protect investment and locally retained revenue, and locally situated collectors remain exposed to those pressures. Building on research that shows how fiscal rules and bureaucratic control divide authority across levels of government (Beramendi & Rogers, 2020; Oates, 2005; Taliercio, 2004), I show that trade-induced firm entry strengthens central fiscal capacity when the center controls collection. Entry expands both the administrative caseload and the potential tax base, raising the return to deploying front-line CTA personnel. I therefore identify bureaucratic deployment as the missing link between market-created tax potential and central fiscal capacity.

Second, I show how common national institutions generate uneven subnational state capacity. Comparative research emphasizes the territorial reach of bureaucratic rule (Garfias & Sellars, 2026; Levi, 1988; Mazzuca, 2021; O'Donnell, 1993; Véjares, 2025). China's CTA and fiscal rules predated the trade shock; openness did not create that structure. Instead, I show that trade-induced growth in firm entry affected where national capacity was invested. Frontline CTA branches added personnel where the potential tax base expanded, and these personnel assignments are likely to remain for the long term. I therefore treat state capacity not simply as a national stock, but as a territorially uneven phenomenon that responds to trade-induced economic change.

Third, I connect the politics of compensation under openness to the politics of revenue formation. Accounts of embedded liberalism explain why governments respond to trade-related risks (Adserà & Boix, 2002; Rodrik, 1998; Ruggie, 1982), but often treat the fiscal capacity and resources needed for redistribution as given. I argue that compensation depends on institutions that first capture and pool revenue. Centralized revenue can support cross-regional transfers, and those transfers can deepen recipient localities' dependence on the center (Rodden, 2002). In authoritarian systems, I show that fiscal pooling also enables rulers to direct support toward spatially concentrated, administratively visible, or state-linked claimants (Albertus, 2024; R. M. Desai et al.,

2009; X. Huang, 2020; Nooruddin & Rudra, 2014; Pan, 2020).

Section 2 develops the argument and its scope conditions. Section 3 maps the argument onto post-WTO China. Section 4 derives five testable predictions. Section 5 presents the empirical strategy. Sections 6 and 7 report the revenue and fiscal-support evidence. Section 8 concludes.

2 Theory

2.1 Trade Openness and the Taxable Domestic Base

I distinguish two channels through which trade liberalization can affect public revenue. Reductions in a country's own tariffs may erode border receipts, a visible and easily administered source of revenue that lower-capacity tax systems often struggle to replace (Baunsgaard & Keen, 2010; Cagé & Gadenne, 2018; Keen & Ligthart, 2002, 2005; Khattry & Rao, 2002). By contrast, improved access to foreign markets can create export opportunities that expand domestic production, investment, employment, and profits (Bachas et al., 2022). Because the policy change that reduces tariff revenue need not be the one that increases foreign demand, the two channels should be analyzed separately. The net fiscal effect of openness depends on the balance between lost border revenue and gains from domestic taxation. I focus on the latter channel: whether improved foreign market access generates domestic activity that governments can identify and tax.

I distinguish two linked margins through which trade-led growth can affect domestic taxation. At the intensive margin, firms already in the tax system expand output, payrolls, profits, transactions, and tax liabilities. At the extensive margin, new registered-firm entry adds new identifiable taxpayers. This extensive-margin expansion creates extra demand for tax capacity: a larger taxpayer population increases the bureaucracy's caseload and its need for personnel, information processing, and enforcement. New registrations give a firm a legal identity and recorded location, while its accounts, invoices, payrolls, and reporting obligations make its activity more visible to revenue authorities (Bachas et al., 2022). Registered-firm entry therefore expands the identifiable taxpayer population and the administrative caseload. Each entrant creates new classification, mon-

itoring, assessment, and collection tasks. This larger caseload calls for additional investment in bureaucratic resources.

Firms can also enter informally, although such entry is rare in countries with higher fiscal capacity. Such entry can generate production, employment, and income outside taxpayer registries and routine reporting systems, making the activity harder to identify and assess under ordinary tax rules (Abramovsky et al., 2014; Ulyssea, 2018). Registered firms, by comparison, enter the tax administration's reporting, monitoring, and assessment systems. Registration thus makes new activity more legible while requiring more resources to administer a larger population of firms.

Registered-firm entry expands the effective tax base by bringing new legal entities and their taxable income and transactions within the reach of tax administration. Translating that expansion into receipts requires tax rules, administrative capacity, and enforcement (Okunogbe & Tourek, 2024). The empirical analysis therefore examines registration together with CTA staffing and collected EIT.

I argue that, under specific conditions, trade exposure is more likely to induce registered-firm entry, thereby expanding the potential tax base at the extensive margin and increasing demand for tax capacity. Improved market access must induce domestic production and, for the extensive-margin mechanism, registered rather than informal entry. The resulting activity must be subject to ordinary tax rules; informality and broad exemptions can leave new income outside the effective tax base (Abramovsky et al., 2014; Ulyssea, 2018). Tax bureaucrats must also possess baseline systems of registration, accounting, and reporting that connect firms to identifiable taxpayers (Kleven et al., 2016; Pomeranz, 2015). These preexisting systems make registered firms administratively legible, while growth in their number raises the marginal demand for personnel, information processing, monitoring, and enforcement. Even when these conditions hold, border-revenue losses must remain manageable for domestic gains to improve the overall fiscal position.

Extending embedded liberalism (Adserà & Boix, 2002; D. R. Cameron, 1978; Nooruddin & Rudra, 2014; Rodrik, 1998; Ruggie, 1982) to revenue formation, I treat tax-base transformation as the first stage of *embedded fiscal centralization*. Export opportunities should increase registered-

firm entry and, with it, demand for tax-administration resources. Whether the resulting revenue strengthens the fiscal center depends on which level controls collection. The next subsection explains how central–local conflict, bargaining between firms and tax bureaucrats, and bureaucratic control determine how the center captures this expanding tax base.

2.2 Central–Local Conflict over Tax Collection

I argue that an increase in registered firms can expand the revenue available to government, but it also intensifies conflict over who controls collection and receives the proceeds. The central government seeks stable revenue, consistent enforcement, and less dependence on subnational remittance, whereas subnational governments value locally retained revenue, investment, employment, and discretion over firms. Local officials can support registration and information sharing, but their proximity to taxpayers also allows them to protect favored firms, tolerate underreporting, or influence tax treatment. National tax rules may therefore be implemented selectively even when the center has a formal claim over the resulting revenue (Garfias & Sellars, 2022, 2026; Holland, 2016; Mazzuca, 2021; Véjares, 2025).

Firms have incentives to seek favorable tax treatment and use political connections (H. Cai & Liu, 2009; H. Li et al., 2008). When subnational governments influence tax bureaucrats, governments and firms may align: local officials can use tax administration to attract or protect firms, which may provide investment or employment in return. Local officials may instead use discretion to extract resources from firms (Liu, 2018). In either case, tax treatment, information, and enforcement become objects of bargaining (Holland, 2016). I therefore argue that central fiscal authority rests not only on a formal claim to tax revenue but also on institutions that constrain bargaining among firms, local officials, and tax administrators. By limiting local discretion over taxpayer information and enforcement, these institutions make centrally established rules effective in practice. They thus allow the center to convert its legal claim to revenue into revenue it can actually collect (Pomeranz, 2015; Slemrod, 2019).

Tax bureaucrats determine how these rules operate in practice. The location of a tax office

does not determine which level of government controls it. A locally situated office remains part of the central administration when the center controls personnel decisions, reporting requirements, taxpayer information, and audit priorities. These arrangements give the center agents within local economies while reducing their dependence on the local coalitions they regulate. They also limit the ability of subnational governments to protect favored firms, withhold information, or weaken enforcement (Levi, 1988; Okunogbe & Tourek, 2024; Pomeranz, 2015; Slemrod, 2019; Taliercio, 2004).

By placing personnel, taxpayer records, reporting systems, and enforcement decisions under a central chain of command, the central government can reduce its reliance on local tax bureaucrats whose careers and resources depend on local governments. Centralization does not completely eliminate evasion, bargaining, or weak enforcement—it only changes who directs the bureaucrats responsible for addressing these problems and makes the implementation of national tax rules less dependent on local political interests (Garfias & Sellars, 2026; Taliercio, 2004).

The argument therefore predicts that localities experiencing export-driven entry of registered firms receive more centrally controlled tax personnel and generate more revenue within a tax base shared with the central budget. Because administrative control is a preexisting institutional feature, investment in central fiscal capacity occurs through the deployment of personnel across localities. The mechanism has two observable signatures: recruitment concentrates in collection-facing branches, and EIT becomes more prominent relative to VAT and direct-remittance categories whose assignment rules the reform left unchanged. The combination of registered-firm entry, branch-level central recruitment, and reform-aligned fiscal-book EIT shows how export growth strengthens central fiscal capacity. Once shared with the center, revenue can be pooled across localities. The next subsection examines how the center allocates these resources.

2.3 National Revenue Pooling and Selective Support

For central rulers, central revenue becomes politically consequential when it can be used beyond the locality where it is generated. Economic openness often produces a territorial mismatch:

growth centers gain new firms, investment, and taxable activity, while declining regions face unemployment, social-protection obligations, and shrinking local revenue bases. These declining places confront a double burden—rising adjustment costs and lower revenue to finance them. National pooling changes this relationship by placing resources under the authority of a government whose fiscal reach spans both revenue-generating and burden-bearing localities. It separates the geography of extraction from the geography of support, creating the capacity to transfer resources across space (Adserà & Boix, 2002; Beramendi & Rogers, 2020; Oates, 2005; O’Brien et al., 2025; Rodden, 2002; Rodrik, 1998). When local revenue is insufficient, support requires a central government to pool revenue across jurisdictions.

Pooling creates allocative resources, but it does not determine which groups or places receive support. Research on globalization and compensation emphasizes demand for support. Concentrated trade losses generate demands for protection, redistribution, or social insurance (D. Autor et al., 2020; D. H. Autor et al., 2013; Broz et al., 2021; Colantone & Stanig, 2018). In democracies, electoral competition can channel them toward broad constituencies through social insurance, subsidies, or legislated transfers (Adserà & Boix, 2002; Hiscox, 2001; Rodrik, 1998), subject to revenue, administrative capacity, and intergovernmental bargaining.

Authoritarian rulers face weaker electoral pressure for broad redistributive programs and greater discretion to target selected groups and localities (X. Huang, 2020; Pan, 2020). For them, political salience arises less from the severity of hardship alone than from an affected constituency’s capacity for collective action (Chen et al., 2016; R. M. Desai et al., 2009). Spatial concentration, workplace and neighborhood networks, and shared organizational ties lower coordination costs, making protest, disruption, or other challenges to public order more credible (H. Zhu, 2025). Prior state commitments can intensify this threat by giving claimants a common basis for holding the regime responsible and framing economic grievances as violations of obligations the state itself created. For such constituencies, repression may compound rather than contain the political cost, making targeted support an attractive means of purchasing quiescence. The visibility of these groups also plays a role: it allows officials to identify the threatening constituency and

deliver support selectively (Albertus, 2024). Authoritarian rulers should therefore direct pooled resources toward constituencies in which collective-action capacity, inherited obligations, and visibility overlap. When these constituencies are geographically concentrated and their social obligations are administered locally, their claims translate into demands for higher-level fiscal support to those jurisdictions. Diffuse or weakly organized groups may suffer equally severe losses but receive less support because they pose a less credible collective threat.

Intergovernmental transfers are one principal channel through which pooled resources reach burdened localities. Transfers enter subnational budgets, where they can finance social insurance, unemployment relief, income support, adjustment programs, or other public obligations (Nooruddin & Rudra, 2014; Wong & Ngok, 2006). Governments may also respond through public employment and other benefits for relatively privileged losers (Nooruddin & Rudra, 2014). They may also replace fiscal revenue lost during economic contraction, allowing subnational governments to maintain existing services. If the central government chooses to sustain politically important commitments as local resources weaken or demand for spending rises, higher-level transfers should rise as well. Fiscal dependence increases only when transfers rise relative to locally controlled revenue or spending.

The theory yields one distributive prediction and one spatial prediction. Localities where adjustment burdens fall on constituencies capable of collective action—especially those with visible claims rooted in prior state commitments—should receive greater fiscal support from higher levels of government. When this support grows relative to locally controlled revenue or spending, recipient governments should also become more fiscally dependent. Revenue-generating and support-receiving localities, however, need not coincide. Central revenue rises in growth centers, while fiscal support rises in localities bearing politically salient adjustment burdens. Institutions that pool resources across jurisdictions connect these two outcomes.

2.4 Scope Conditions and Comparative Implications

The argument has two sets of scope conditions: those governing central revenue mobilization and those governing selective compensation. Openness strengthens central revenue when four conditions hold. First, it must generate registered firms and domestically reachable taxable activity rather than gains concentrated in informal trade or offshore capital (Okunogbe & Tourek, 2024; Ulyssea, 2018). Second, the center must possess the legal authority and political power to claim the expanding tax base against local governments and incumbent taxpayers. Third, a centrally accountable bureaucracy must be able to identify new taxpayers, enforce the center's claim within local economies, and redeploy personnel as administrative caseloads shift (Taliercio, 2004). Fourth, additional domestic revenue must be large enough to offset any loss of trade-tax revenue (Baungsgaard & Keen, 2010; Cagé & Gadenne, 2018). The revenue mechanism weakens when firms remain informal or mobile, political connections shield them from taxation, local governments control collection, or fragmented authority prevents the center from enforcing its claim (Blanchard & Shleifer, 2001; Careaga & Weingast, 2003; Falleti, 2010; Mazzuca, 2021; O'Donnell, 1993).

Selective compensation requires a different set of conditions. Adjustment burdens must fall on constituencies capable of collective action. Spatially concentrated trade and restructuring shocks can create this potential (Campello & Urdinez, 2021; Dix-Carneiro & Kovak, 2015; Kovak, 2013), but concentration alone does not guarantee a response unless it facilitates organization and makes inaction politically costly (Chen et al., 2016). Officials must also be able to identify the affected constituency and the prior state obligations on which its claims rest. Finally, fiscal institutions must allow centrally pooled revenue to move across jurisdictions, while local institutions must be able to convert that support into pensions, unemployment programs, social assistance, or other benefits. The compensation mechanism is therefore strongest where collective-action capacity, administrative visibility, prior state commitments, and intergovernmental transfer channels coincide.

Political centralization supports both stages but guarantees neither. It helps the center assert tax claims, limit subnational capture, and direct pooled resources to selected constituencies. Yet

without a reachable tax base, a centrally accountable bureaucracy, politically consequential claims, and effective delivery channels, centralized authority cannot produce this sequence.

These conditions may also operate outside China, but the contrast between Vietnam and India is important. Vietnam followed a pattern similar to China's: its WTO-era reforms combined trade integration with modernization of the national tax administration and a shift from border revenue toward domestic taxation of a growing private sector (Shukla et al., 2011). India produced a different institutional outcome: its 2017 goods and services tax (GST) integrated national taxation through the GST Council, center–state revenue sharing, and transitional compensation. This case illustrates how governments may bargain over a more legible tax base when political authority is less centralized (Joseph & Kumary, 2023). The empirical analysis focuses on post-WTO China, where these conditions make it possible to examine the full sequence from registered-firm entry to central revenue mobilization and selective fiscal support.

3 Post-WTO China as a Most-Likely Case

3.1 Central Tax Infrastructure and the 2002 EIT Reform

By the time China joined the WTO, its tax bureaucracy was deeply embedded in local economies but divided between central and local chains of command. The 1994 tax-sharing reform recentralized major revenue streams after the fiscal decentralization of the 1980s and early 1990s, reversing the erosion of central fiscal authority and strengthening the central government's capacity to collect and monitor revenue (Choi, 2016). The reform split tax administration between two parallel bureaucracies: the CTA, controlled by the central government, and the Local Tax Administration (LTA), controlled by local governments (Y. Huang, 1996; Lou, 2008; Yang, 2004). This arrangement gave the central government a network of tax offices embedded in local economies, but not a monopoly over tax administration: local governments retained their own bureaucratic apparatus and substantial fiscal authority. Figure 1 summarizes this dual administrative structure.

Before 2002, local governments controlled EIT collection from private and locally affiliated

firms, while the CTA focused on central and shared taxes, including VAT. This division gave local officials both the incentive and discretion to protect favored firms and relax enforcement to sustain investment. The CTA's local presence alone could not convert private-sector growth into central revenue; doing so also required EIT jurisdiction over new firms and control over the collectors.

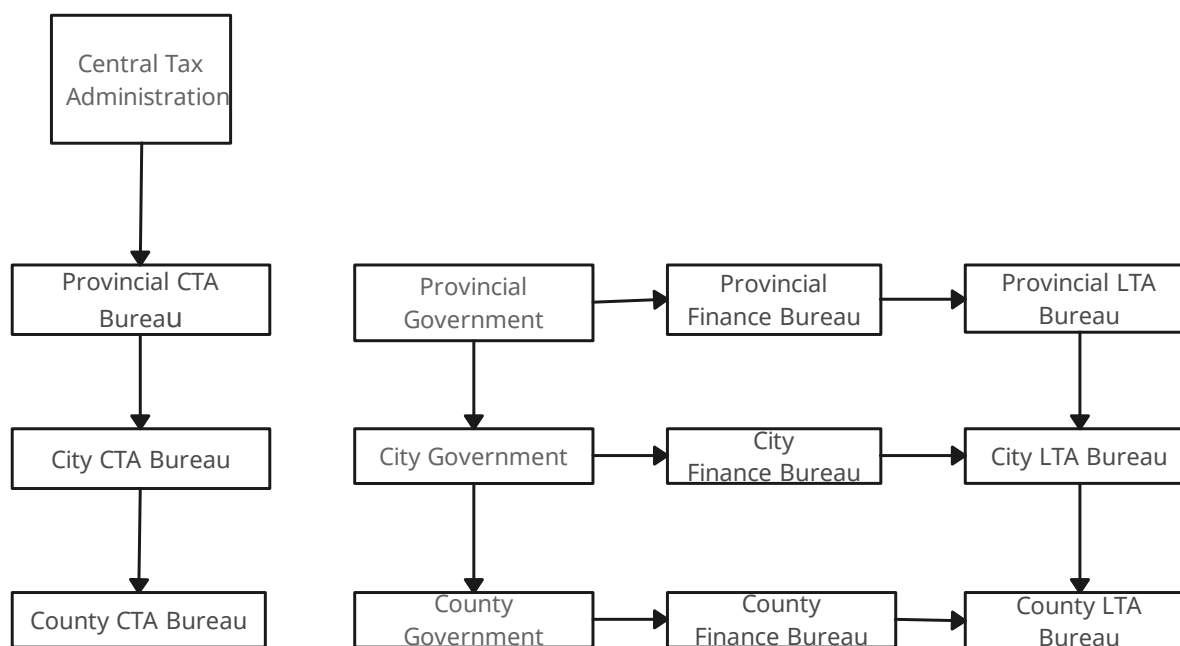


Figure 1. China's tax administration, 1994–2017

Note. Arrows denote formal authority over revenue targets, hiring, promotion, other personnel decisions, and funding. The figure ends in 2017 because the 2018 reform merged the CTA and LTA systems at and below the provincial level, ending the dual structure shown here.

The 2002 EIT reform initially split income-tax receipts equally between the central and local governments, then raised the center's share to 60% from 2003 onward. Implementing rules assigned most newly established firms to the CTA while leaving many pre-2002 firms with their previous collecting agency (State Administration of Taxation, 2002; State Council, 2001). The reform also protected each locality's 2001 income-tax baseline and directed additional central revenue to general transfers, mainly for central and western regions (State Council, 2001). These safeguards limited local losses, while centralized control over CTA personnel made collection from new firms less dependent on local governments. The reform therefore preserved much local

authority over incumbent taxpayers while assigning most new firms to the CTA and most shared income-tax growth to the central budget.²

Because the reform applied prospectively, it changed incentives at the margin. Grandfathering and baseline protection preserved local governments' established revenues and incentives to attract investment and cultivate exporters. Each eligible entrant, however, expanded the CTA-administered tax base. The center's 60% revenue share increased its return to registration, staffing, monitoring, and enforcement. With jurisdiction and personnel rules uniform nationwide, cross-city variation in export opportunities reveals where bureaucratic investment matters most.

3.2 WTO Accession and Private-Firm Entry

WTO accession opened new export opportunities for private firms operating under this fiscal structure. Before WTO accession, direct international trade was largely confined to large SOEs and foreign-invested enterprises (FIEs), leaving most private firms dependent on state-owned or foreign intermediaries. During the three-year phase-in of WTO accession, China dismantled this trading-rights permit barrier; by 2004, private firms could reach foreign markets directly (Brandt et al., 2017; Gallagher, 2005; Naughton, 2018). WTO accession also reduced trade barriers and uncertainty about foreign market access (Fan, Li, & Yeaple, 2015; Heise et al., 2024). These changes were especially consequential for private enterprises (PEs), which had faced the greatest restrictions under the earlier trading-rights regime.

Post-WTO growth extended beyond incumbent exporters. PEs entered export production, supplier networks, logistics, processing, and related services, while FIEs continued to expand. Ownership mattered: SOEs and many FIEs already had established relationships with the state and export channels, whereas private firms benefited disproportionately from expanded trading rights and more certain market access. Their weaker administrative ties to the central government also made them more likely to evade taxes. New PEs were thus both market entrants and legal entities that could be registered, assigned to a tax jurisdiction, and incorporated into routine administration.

Appendix Figures A1 and A2 summarize exports, new registrations, and EIT receipts across

ownership types. The private-sector expansion created a large new population of legal entities with tax identities, accounts, invoices, employment records, and physical establishments. As shown later, the CTA hierarchy, together with the collection and revenue-sharing rules of the 2002 EIT reform, converted this market-created legibility into a centrally administered tax base carrying a statutory central revenue share (Boone, 1994; Okunogbe & Tourek, 2024; Pomeranz, 2015).

3.3 SOE Restructuring, Local Obligations, and Fiscal Support

The fiscal burdens studied here originated in a separate, domestic restructuring of SOEs that began before WTO accession. The government closed, merged, and privatized inefficient firms, hardened budget constraints, and routed tens of millions of workers through the *xiagang* system and related layoff programs from the late 1990s into the mid-2000s (Hurst, 2009; Naughton, 2018). WTO-era competition overlapped with but did not initiate this process. The theoretical link is concurrence plus fiscal linkage, not common causation: private tax-base expansion and concentrated SOE obligations arose in the same reform era, often in different cities.

Laid-off SOE workers formed a politically salient, institutionally connected constituency. State jobs had bundled wages with housing, pensions, medical care, and status, so displacement implicated prior public commitments (H. Y. Lee, 2000; Luo et al., 2023). Losses clustered in older industrial cities, where factory neighborhoods and work-unit networks facilitated petitions and protests (Y. Cai, 2002; Frazier, 2004; Friedman, 2014; Hurst & O'Brien, 2002; C. K. Lee, 2007). Trade integration and market reform also burdened import-competing, migrant, informal, rural, and small-producer groups. Farmers and migrant workers lacked comparable state status, however, and officials tended to ignore their demands. The analysis focuses on SOE workers because their losses were concentrated, their claims invoked public obligations, and the associated employment decline and fiscal burdens are measurable at the city level.

Appendix Figure A4 provides evidence on their demands. In the 2003 Chinese General Social Survey (CGSS), conducted by Renmin University, respondents laid off from public-sector jobs expressed stronger redistributive preferences, placed greater priority on reducing unemploy-

ment, and more often reported declining status and living conditions. Figure 2 documents the sharp decline in SOE manufacturing employment and the provincial association between rising unemployment and subsequent net transfers.

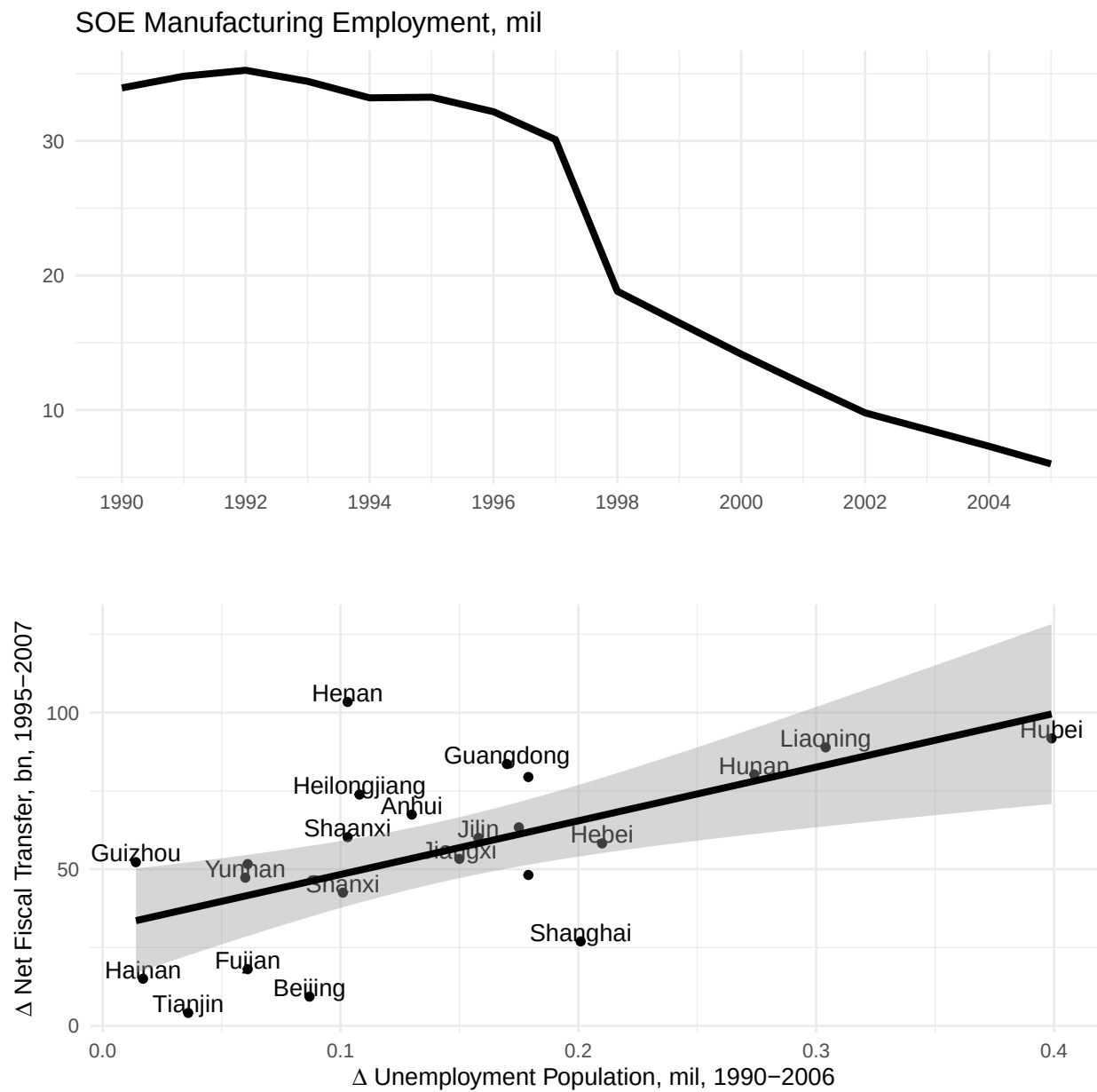


Figure 2. SOE downsizing, unemployment, and fiscal transfers, 1990–2007

Note. The upper panel reports SOE manufacturing employment during 1990–2005. The lower panel plots the 1990–2006 change in provincial unemployment against the 1995–2007 change in net fiscal transfers. Data source: *China Statistical Yearbook*, various years.

China's intergovernmental system linked national revenue pooling to these local obligations. After the 1994 reform, local governments retained major spending and order-maintenance responsibilities while the center rebuilt revenue authority through the CTA (Y. Huang, 1996; Lou, 2008; Tsui, 2005; Yang, 2004). Restructuring widened this vertical mismatch: older industrial localities faced pension arrears, unemployment relief, minimum-livelihood guarantees, reemployment assistance, enterprise closures, and retiree obligations as contraction eroded their own revenue base (Hurst, 2009; Naughton, 2018; Solinger, 2002; State Council, 2000). Higher-level transfers financed these obligations and offset lost revenue.

The 2002 EIT reform directed additional central income-tax revenue to general transfers, mainly for central and western regions (State Council, 2001). It thus connected central revenue gains to a national transfer pool. Empirically, general transfers and fiscal dependence measure resources flowing into local budgets; unemployment, pension, minimum-livelihood, reemployment, and retiree programs trace the obligations those resources supported (Y. Cai, 2002; Hurst & O'Brien, 2002; J. Jiang & Zhang, 2020; H. Y. Lee, 2000; P. Li & Zhang, 2004; Lou & Wang, 2008; Wan et al., 2015).

4 Predictions

I derive five testable predictions by mapping the theory onto post-WTO China's institutional setting.

H1: Registered private-firm entry. Localities more exposed to trade expansion should record more new private-firm registrations. The response should be stronger for private firms than for other ownership categories.

H2: Bureaucratic expansion. More-exposed localities should receive more centrally accountable frontline tax bureaucrats. The response should be stronger in branch offices responsible for tax collection than in city headquarters where administrative positions are located.

H3: Reform-aligned EIT composition. More-exposed localities should have a larger fiscal-book EIT base subject to statutory central sharing. Because the 2002 EIT reform changed EIT collection and sharing but left VAT and direct-remittance rules unchanged, more-exposed localities should also have a more EIT-intensive tax mix.

H4: Burden-responsive fiscal support. Localities experiencing severe SOE restructuring should receive more intergovernmental grants, become more dependent on transfer finance, and exhibit greater worker-linked spending obligations.

H5: Spatial pooling. Revenue-generating and support-receiving localities need not coincide. When export-driven central revenue growth and burden-responsive transfers occur in different places, the national fiscal pool moves resources across localities.

5 Data and Empirical Strategy

The design follows prefecture-level cities from a pre-WTO baseline in 2000 to outcomes in 2007. The baseline predates WTO accession, the export boom, and the 2002 EIT reform; the endpoint allows firm entry, tax recruitment, and revenue effects to appear but precedes the Global Financial Crisis and 2008 stimulus (Bombardini & Li, 2020; B. Tian, 2014).

Prefecture-level cities are the lowest administrative level at which the key economic, employment, and transfer outcomes are consistently available (Bombardini & Li, 2020). They sit between provinces and counties, typically administering an urban core and surrounding counties, coordinating local development and budgetary obligations, and interacting with central and local tax offices. Each prefecture-level jurisdiction encompasses the industrial districts in which export opportunities expanded, the registration authorities that recorded new firms, the CTA branches that administered taxpayers, and the local governments responsible for pensions, unemployment relief, and public services. Aggregating the urban core and surrounding counties therefore captures the fiscal territory over which export exposure, firm entry, tax recruitment, fiscal revenue, grant

receipts, spending, and employment can be linked.

5.1 Export Shock

I measure each city's post-WTO export exposure by combining its pre-accession industrial composition with national industry export growth from 2000 to 2007. This shift-share design builds on Bartik and related approaches used to estimate the local effects of China's WTO accession and trade shocks (D. H. Autor et al., 2016; Baccini & Weymouth, 2021; Bartik, 1991; Bombardini & Li, 2020; Campello & Urdinez, 2021; B. Li, 2018; Retzl, 2025; Scheve & Serlin, 2023). Figure 3 shows the industry-level export expansion that motivates the exposure design.

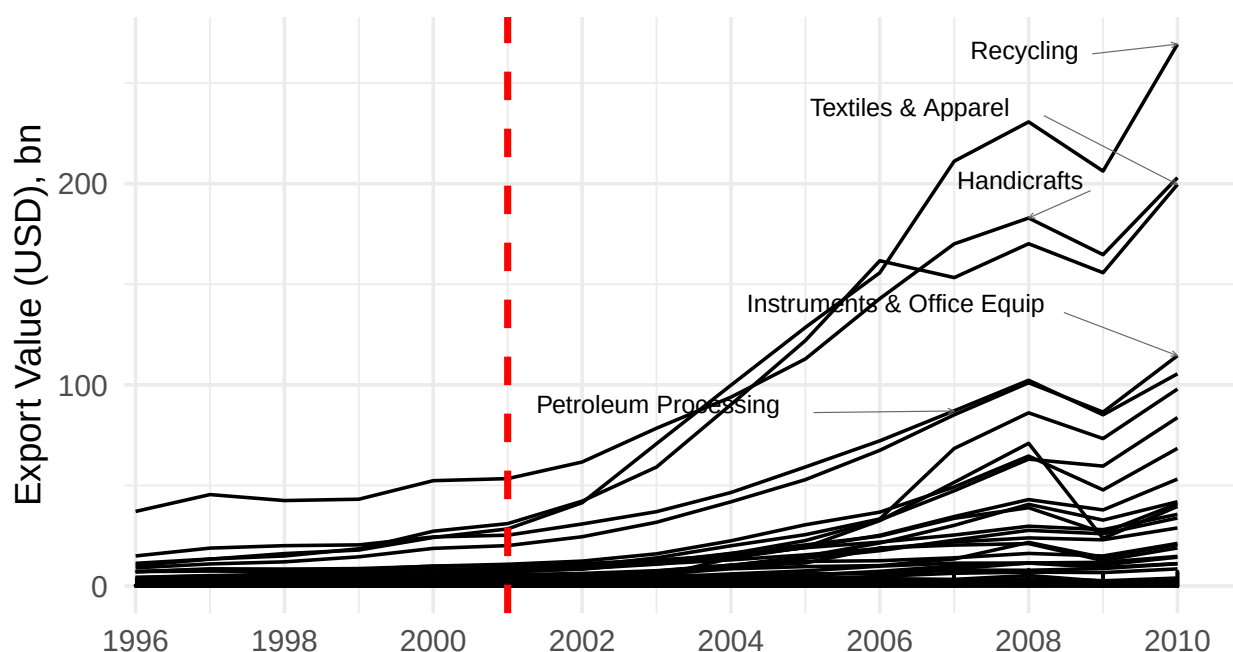


Figure 3. Industry-level export values, 1996–2010

Note. Annual export values are shown for all two-digit industry groups. The dashed line marks China's WTO accession in December 2001. Leader lines connect the five labeled industries to their respective series.

The exposure measure is:

$$EXPShock_i^{07} = \sum_k \frac{Y_{ik}^{00}}{Y_k^{00}} \cdot \frac{\Delta EXP_k^{00-07}}{Y_i^{00}}, \quad (1)$$

where Y_{ik}^{00} is city i 's industrial output in industry k in 2000, Y_k^{00} is national output in that industry, Y_i^{00} is city output, and ΔEXP_k^{00-07} is national export growth in industry k . I use industrial output shares because the mechanism operates through changes in the taxable production base rather than labor-market exposure. As in other shift-share designs, the identifying assumption is that, absent post-WTO industry export shifts, cities with different baseline industry shares would not have experienced differential trends in firm formation, tax staffing, or fiscal outcomes (Borusyak et al., 2022; Goldsmith-Pinkham et al., 2020).

The intuition is that cities specialized before WTO accession in industries that later experienced large national export growth should have been more exposed to the post-WTO export boom. A city with a large pre-accession presence in fast-growing export industries receives a larger shock than a city concentrated in industries whose exports changed little. Because the weights are measured before the shock, the design uses inherited industrial structure rather than contemporaneous local performance to assign exposure. This is especially useful for the paper's mechanism because the relevant shock is not simply a labor-market displacement shock; it is a shock to the local production base from which new firms could emerge.

The pre-WTO weights generate the cross-city comparison at the core of the Bartik design: cities with different inherited industry portfolios receive different exposure to national industry export growth. This gives the design a difference-in-differences-like structure, but it does not make realized national export growth exogenous. National export growth can reflect Chinese supply conditions, industry policy, or other industry-level changes that also affect firm formation, tax administration, and fiscal outcomes (Borusyak et al., 2022; Goldsmith-Pinkham et al., 2020). To preserve the Bartik comparison while replacing those realized shocks with external market-access variation, I instrument realized Bartik exposure with a tariff-predicted export shock, following

Bombardini and Li (2020) and B. Li (2018). The instrument uses product-level tariff reductions faced by Chinese exporters to predict industry export growth and then maps the predicted component to cities using the same pre-WTO industrial shares:

$$\widetilde{EXPShock}_i^{07} = \sum_k \frac{Y_{ik}^{00}}{Y_k^{00}} \cdot \frac{\widetilde{\Delta EXP}_k^{00-07}}{Y_i^{00}}. \quad (2)$$

where $\widetilde{\Delta EXP}_k^{00-07}$ is tariff-predicted national export growth in industry k . Additional implementation details are provided in Appendix F. Foreign tariff cuts shifted market access for Chinese industries but were not chosen by Chinese cities. Interacting those tariff changes with pre-WTO industrial structure is intended to isolate export exposure driven by external demand conditions rather than by contemporaneous local growth, fiscal policy, or administrative capacity (Kovak, 2013; Y. Tian, 2024). Appendix H.2 tests the design with Rotemberg weights, cumulative industry exclusions, alternative exposure measures, geographic restrictions, and province fixed effects. Appendix H.5 reports pre-treatment tests.

5.2 Data and Outcomes

The outcome data trace the sequence from private-firm entry and CTA recruitment to fiscal revenue and intergovernmental support. Firm formation and CTA staffing are mechanism outcomes: they show whether openness expanded taxable legal entities and whether the state deployed administrative resources to make them enforceably taxable. Firm formation is measured from State Administration for Industry and Commerce registration records, aggregated by city and ownership type between 2000 and 2007. CTA investment is measured with an original dataset of position openings for CTA offices in each prefecture-level city, advertised through the national civil-service examination. This recruitment measure captures new administrative capacity at the extensive margin and distinguishes frontline branch offices from city headquarters. Appendix Table A24 shows that preexisting provincial age composition does not consistently predict subsequent hiring, reducing concern that the measure simply reflects replacement of retiring staff.

The recruitment data provide a consistent city-level measure of new central staffing. Position-opening records identify the location, hiring office, job description, required qualifications, and number of openings for central civil-service positions. Entry into the CTA occurs through the national civil-service examination, making new recruits a selective margin of bureaucratic investment rather than a simple count of existing employees. The distinction between branch offices and headquarters is substantively important: branch offices handle day-to-day taxpayer registration, collection, monitoring, and enforcement, while headquarters positions capture general administration. Stronger branch recruitment therefore identifies fiscal-capacity building at the collection-facing margin.

Export and output shares are constructed from United Nations (UN) Comtrade product-level export data concorded to Chinese industrial classifications and from the *Annual Survey of Manufacturing Firms* (ASMF).³ Tariff data are from UN Comtrade and Guimbard et al. (2012). Fiscal outcomes come from the *National Prefecture and County Finance Statistics Compendium*; SOE employment decline comes from the ASMF. Baseline controls come from the 2000 census and city yearbooks. Appendix Table A5 summarizes the main variables, and Appendix H.4 reports transformed and per-capita specifications.

Pre-WTO manufacturing output determines each city's inherited exposure weights, while foreign tariff reductions generate the external market-access component. Registration records identify the new firms created during the response period, civil-service openings identify the central administrative deployment, and fiscal books record the resulting tax and grant outcomes. These distinct measures test the institutional sequence: the same export shock should first appear in firms, then in the offices responsible for administering them, and finally in shared-tax categories carrying a statutory central revenue share.

The fiscal accounts report EIT, VAT, consumption tax, business tax, and construction tax. I mainly present four city-level outcomes: imputed central-share EIT and VAT, EIT's share of EIT plus VAT, and the log ratio of imputed central-share EIT to direct central remittances. In 2007, the center's statutory share of income-tax receipts was 60%, and most newly established firms fell

under CTA jurisdiction. Because the fiscal books record the local 40% share, I impute central-share EIT as 1.5 times fiscal-book EIT. The CTA already collected VAT and consumption tax before the 2002 reform. VAT was shared 75:25, so I impute the central share as three times the fiscal-book amount. Direct central remittances equal the reported 75% VAT remittance plus consumption tax. Because these rules did not change in 2002, the EIT-to-remittance ratio compares EIT governed by the new rules with tax categories governed by unchanged rules. All outcomes exclude tax rebates and other intergovernmental receipts. Appendix H.3 reports construction, composition, and outlier checks.

Fiscal support is measured as reported intergovernmental grant receipts—tax rebates, general and special transfers, and other subsidies from upper governments—harmonized across the 2000 and 2007 fiscal schedules. Fiscal dependence is the log ratio of grants to spending. Social-protection spending and recorded retiree counts capture obligations associated with state-sector restructuring. Appendix H.6 documents the grant measure and reports no-control and other-fiscal-outcome checks.

These outcomes capture distinct dimensions of support: total grants measure higher-level resources entering a locality, per-capita grants measure their intensity relative to population, and the grant-to-spending ratio measures local budget dependence on intergovernmental finance. Social-protection spending and retiree counts connect these resources to obligations inherited from state-sector restructuring. Together, the measures test whether higher-level finance and restructuring-related obligations rose in contracting SOE localities.

5.3 Empirical Specifications

The export-shock analysis estimates the following cross-sectional two-stage least-squares (2SLS) equation:

$$Y_i = \beta_1 EXPShock_i^{2007} + \beta_2 X_i + \theta_r + \varepsilon_i, \quad (3)$$

where Y_i is a firm, staffing, or fiscal outcome; $EXPShock_i^{2007}$ is instrumented with the tariff-predicted shock; X_i contains controls measured in 2000; and θ_r denotes regional fixed effects. The preferred controls are the baseline manufacturing-employment share, unemployment share, share ages 20–34, male share, and the first three principal components of baseline industrial structure. Post-treatment private-sector growth is not controlled for because it is the first mechanism outcome through which openness creates a taxable base. Downstream GDP, population, or firm growth likewise does not enter the preferred models.

The tariff-predicted exposure strongly predicts realized exposure in the preferred specifications. The full-control, outcome-matched first stages use standard errors clustered by province; their effective F -statistics equal 163.6 for private-firm entry and range from 133.8 to 167.6 across the main firm, staffing, and fiscal outcomes. Tables 1, 2, and 3 report the exact column-specific values. Each model is just identified.

The estimand is the effect of tariff-induced export exposure on private-firm registration, frontline CTA recruitment, and central EIT revenue. The mechanism tests trace the response through private firms, CTA branches, imputed central-share EIT, and EIT relative to VAT. The instrumental-variable (IV) design identifies exposure effects, while the institutional interpretation derives from their alignment across national collection-jurisdiction and revenue-sharing rules, firm entry, CTA recruitment, and fiscal revenue.

The analysis uses three comparisons: private firms versus ownership groups with established state ties, CTA branches versus headquarters, and EIT versus taxes whose rules did not change in 2002. Private-firm entry captures the ownership group that gained disproportionately from WTO-era market access and whose new firms fell under CTA jurisdiction after 2002. Branch recruitment measures frontline deployment within local economies, while headquarters recruitment measures general administration. The 2002 reform changed EIT collection and sharing but left VAT and direct-remittance rules unchanged. EIT's share of EIT plus VAT tests whether the tax mix shifted toward EIT, while the EIT-to-remittance ratio compares EIT with a broader set of taxes outside the reform. Because VAT was already centrally administered, it provides a direct

benchmark for the EIT response. The predicted pattern of centralization is therefore a joint rise in private-firm entry, frontline recruitment, and fiscal-book EIT.

For the fiscal-support results, I estimate a two-period city panel from 2000 to 2007:

$$Y_{it} = \beta_1 SOE_SHR_{it} + \beta_2 X_{it} + \theta_i + \gamma_t + \varepsilon_{it}, \quad (4)$$

where Y_{it} is a reported-grant, spending, retiree, or fiscal-dependence outcome; SOE_SHR_{it} is SOE employment share (remaining); X_{it} contains time-varying controls; and θ_i and γ_t denote city and year fixed effects. Because the regressor is a remaining share, negative coefficients indicate that larger SOE employment-share declines are associated with greater fiscal support. Total and per-capita grants capture the scale and intensity of support, fiscal dependence measures reliance on higher-level finance, and social-protection spending and recorded retirees trace restructuring-related obligations.

Inference follows the structure of each design. The export-shock analysis is cross-sectional, so each city appears once; clustering at the city level is therefore equivalent to heteroskedasticity-robust inference. The main tables cluster standard errors by province to allow correlated shocks across cities within common provincial policy and economic environments (A. C. Cameron & Miller, 2015). Appendix H.1 reports singleton city-cluster and provincial wild-cluster bootstrap checks (A. C. Cameron et al., 2008; MacKinnon et al., 2023). Appendix H also reports outcome transformations, per-capita scaling, joint ownership-sector models, and timing and placebo tests. The two-period SOE models cluster standard errors by city.

The analysis proceeds in two steps. The revenue-side IV tests private-firm entry, front-line CTA recruitment, and tax-specific fiscal outcomes. The SOE panel then tests whether larger restructuring burdens predict reported grant receipts and restructuring-related obligations within cities.

6 Trade Exposure and Central Tax Capacity

6.1 Private-Firm Entry

Table 1 reports the estimated effect of export exposure on firm registration. The effect should be strongest among PEs, which had been more constrained under the pre-WTO trading-rights regime, while FIEs and SOEs were already more integrated into global trade networks.

Model 1 reports the first stage. Models 2–4 estimate effects on new private-firm registrations while progressively adding baseline controls constructed using principal component analysis (PCA) and census covariates. Models 5 and 6 report inverse-hyperbolic-sine (IHS) and per-capita specifications for private-firm registrations, respectively. The remaining columns report private-sector employment and level changes in FIE and SOE registrations.⁴

The results support the first step of the mechanism. Export exposure predicts large and robust increases in domestic private-firm entry across count, IHS, and per-capita specifications, alongside higher private-sector employment. In the full-control count specification, a one-standard-deviation increase in export exposure, 55.6 percentage points in that sample (Appendix Table A5), implies roughly 460 additional private firms between 2000 and 2007. On the same count-change scale, the corresponding increase is roughly 15 FIEs, much smaller than the private-firm response, while SOE entry does not respond systematically. Figure 4 presents the corresponding residualized descriptive correlation.

The ownership contrast separates the mechanism from general city growth: registrations rise most among domestic private firms, which faced the greatest pre-WTO trading constraints and were directly covered by the 2002 EIT reform’s assignment of most new firms to the CTA. Combined with the employment response, roughly 460 additional firms per standard-deviation exposure increase indicates extensive-margin growth rather than expansion among incumbents. Each registration creates recurring identification, filing, invoice, and audit demands, generating both fiscal opportunity and administrative workload.

Table 1. Export Exposure and Firm Formation by Ownership Type

	First Stage		Δ Private Firms			Δ PE Emp		Δ FIE		Δ SOE	
	(1)	(2)	(3)	(4)	IHS	Per Cap.	Emp.	Count	Count	Count	Count
Export Shock		10.407*** (2.812)	11.701*** (2.954)	8.247*** (2.331)	0.007*** (0.002)	0.008*** (0.002)	1.144** (0.420)	0.278** (0.129)		0.038 (0.104)	
Export Shock	1.679*** (0.105)										
Region FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
PCA Ind		N	Y	Y	Y	Y	Y	Y	Y	Y	Y
Controls		N	N	Y	Y	Y	Y	Y	Y	Y	Y
First-stage F	255.0	239.2	205.9	163.6	163.6	163.6	133.8	163.6	163.6	163.6	163.6
Num. obs.	312	310	310	310	310	310	251	310	310	310	310

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Units: export shocks are percentage-point exposure measures; firm outcomes are counts, except private-sector employment is measured in thousands and the per-capita column is per 10,000 baseline residents. Transformations: the column labeled IHS uses inverse hyperbolic sine; the private-sector employment column divides by 1,000; the per-capita column divides private-firm changes by baseline population and multiplies by 10,000; FIE and SOE columns report level count changes, matching the private-firm count outcomes. Controls: baseline 2000 census and industry controls; unemployment share, manufacturing employment share, share ages 20–34, male share, and the first three principal components of baseline industrial structure; columns marked PCA include only the principal components. FE: regional fixed effects in every column. SE: standard errors clustered by province. Sample: included city-level cross-section with one observation per city. Years: firm changes are measured from 2000 to 2007; controls are measured in 2000. Outcome scale: columns 2–4, 8, and 9 report level changes in firm counts; column 5 is IHS-transformed; column 6 is per-capita; column 7 is employment in thousands. Because these city-level models are cross-sectional with one observation per city, city-level clustering is not separately informative; province-level clustering allows arbitrary correlation in unobserved shocks across cities within the same province. The first-stage F row reports the province-clustered excluded-instrument F statistic for each column's exact sample and controls.

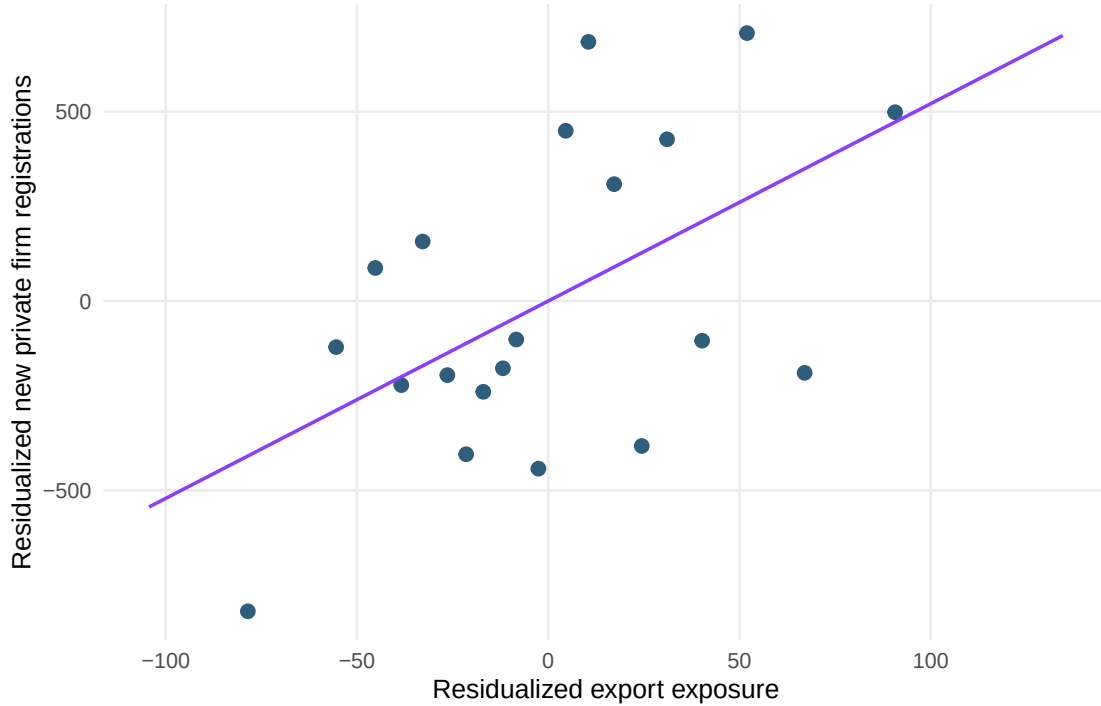


Figure 4. Export exposure and new private firm formation

Note. Binned scatter of residualized export exposure and residualized private-firm formation after controlling for regional fixed effects, baseline industrial principal components, and baseline census controls.

6.2 Frontline CTA Recruitment

Export exposure also shifted CTA recruitment toward frontline branch offices rather than city headquarters. Table 2 reports 2SLS estimates for new CTA recruitment. Except for the smoothed measure in Model 4, the outcomes are measured in 2007 levels. They capture recruitment flows, not staffing stocks or first differences.

Models 1–3 progressively add controls. In Model 3, moving from the 25th to the 75th percentile of export exposure in the included sample, from 74.4 to 151.9 percentage points (Appendix Table A5), is associated with roughly 5.9 additional advertised CTA positions in 2007, a large effect relative to a city mean of about eight positions. Model 4 averages recruitment over 2006–2008 to smooth year-to-year hiring volatility and yields a smaller but still statistically significant effect. Models 5 and 6 show that this response is concentrated in county-level branch offices, which handle day-to-day tax collection, rather than city headquarters. The same interquartile shift

Table 2. Export Exposure and Central Tax Administrative Capacity

	DV: 2007 All			06–08 Mean	Branch	Headquarters
	(1)	(2)	(3)	(4)	(5)	(6)
Export Shock	0.107*** (0.027)	0.099*** (0.031)	0.075*** (0.027)	0.037** (0.015)	0.077*** (0.027)	−0.001 (0.002)
Region FE	Y	Y	Y	Y	Y	Y
PCA Ind	N	Y	Y	Y	Y	Y
Controls	N	N	Y	Y	Y	Y
First-stage F	255.0	215.4	167.6	167.6	167.6	167.6
Num. obs.	312	312	312	312	312	312

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Units: tax-administration outcomes are CTA recruit counts; column 4 is the 2006–2008 mean count; columns 5–6 split branch and headquarters counts. Transformations: outcomes are unlogged levels; export shock is instrumented with tariff-predicted export exposure in 2SLS columns. Controls: baseline 2000 census and industry controls: unemployment share, manufacturing employment share, share ages 20–34, male share, and the first three principal components of baseline industrial structure; columns marked PCA include only the principal components. FE: regional fixed effects in every column. SE: standard errors clustered by province. Sample: included city-level cross-section with one observation per city. Years: main staffing outcomes are measured in 2007, except the mean column uses 2006–2008; controls are measured in 2000. Outcome scale: all outcomes are levels; no outcome is IHS-transformed or per-capita. Because these city-level models are cross-sectional with one observation per city, city-level clustering is not separately informative; province-level clustering allows arbitrary correlation in unobserved shocks across cities within the same province. The first-stage F row reports the province-clustered excluded-instrument F statistic for each column's exact sample and controls.

in exposure implies roughly 6.0 additional branch-office positions, while the headquarters point estimate is essentially zero.

The location of these positions distinguishes targeted investment in tax capacity from a general expansion of public administration. Branch offices handle taxpayer registration, filing, monitoring, and collection, whereas a headquarters-only response would be more consistent with broad administrative reorganization. Instead, export exposure increased recruitment openings in branch offices that directly administer taxes paid by local firms. The concentration of recruitment in branch offices, together with the retirement check in Appendix H.7, points to collection-facing deployment rather than general civil-service growth. Both the quantity and placement of recruitment therefore support H2.

The firm-level analysis in Appendix G, summarized in Appendix Figure A7, provides complementary evidence that profit-reporting alignment increased among firms presumptively assigned to the CTA under the 2002 default rule. From 2002 to 2007, reported profits among these firms moved closer to a constructed profit benchmark.

6.3 EIT and Statutory Central-Share Outcomes

Table 3 shows that export exposure increased CTA-administered EIT recorded in city fiscal books and, after applying the statutory sharing rate, the center's share of that revenue. The first two columns scale fiscal-book EIT and VAT by their statutory central-local shares. The last two columns compare EIT with VAT and consumption-tax remittances. The first-stage F statistic is 139.4 in the first three columns and 139.3 in the fourth. All outcomes exclude tax-rebate transfers returned to local budgets.

The city-level EIT effect is large. Moving from the 25th to the 75th percentile of realized export exposure raises imputed central-share EIT by 0.395 log points, or 48.4% ($p = 0.037$). Under the 2002 rules, the CTA collected EIT from most newly established firms and the center received 60% of receipts. The estimate therefore shows that export exposure expanded centrally administered, centrally shared EIT.

VAT provides a useful comparison. The CTA already administered VAT and remitted the center's 75% share before China's WTO entry, and the 2002 EIT reform did not change its collection or sharing rules. The estimated increase in imputed central-share VAT is smaller—15.9% across the exposure interquartile range—and statistically imprecise ($p = 0.342$).

The composition results in columns 3 and 4 show that exposure tilted the central tax mix toward EIT—whose collection and sharing rules changed in 2002—rather than merely expanding tax revenue overall. A generic export boom could raise both profits and value added, causing EIT and VAT to grow together; if they grew proportionally, the EIT share, $EIT/(EIT + VAT)$, would remain unchanged. Across the exposure interquartile range, however, EIT's share of EIT plus VAT is 4.64 percentage points higher in 2007 ($p = 0.001$); controlling for the 2000 share yields a 4.46-percentage-point difference ($p = 0.002$). The higher share means that EIT became more prominent relative to VAT, whose administration and sharing rules the 2002 reform left unchanged. The log EIT-to-VAT ratio is about 28% higher in the 2007 specification, 22% higher after controlling for its 2000 value, and 21% higher after additionally controlling for its 1997–2000 pre-trend. These estimates are stable under winsorized-share, log-odds, and rank transformations. The

log ratio of imputed central-share EIT to direct central remittances is 0.222 log points, or 24.8%, higher across the exposure interquartile range ($p = 0.008$), extending the same contrast to VAT and consumption-tax remittances whose assignment rules also remained unchanged. The composition results therefore provide a sharper test than tax levels alone: EIT gained relative to tax categories with statutory central shares whose assignment rules did not change.

Together, the city outcomes show that WTO market access created private firms, the CTA added collection-facing staff, and fiscal-book EIT rose both in levels and relative to tax categories whose assignment rules did not change. Applying the statutory 60% share identifies a larger central claim on the EIT generated in more-exposed cities. The national assignment rule placed most new firms under CTA jurisdiction, while branch recruitment expanded central administrative capacity where entry grew. Figure 5 complements the city evidence with the national agency series. Aggregate CTA EIT collection separates sharply from LTA collection after the 2002 reform.

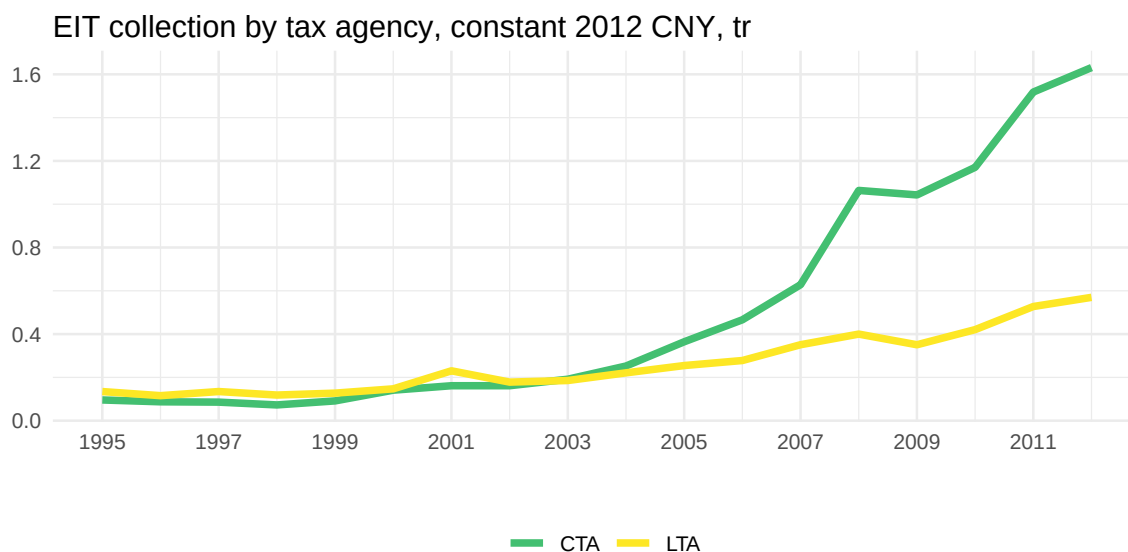


Figure 5. National EIT collection by central and local tax agencies, 1995–2012

Note. The figure reports national EIT collections by the CTA and LTA, measured in trillions of constant 2012 CNY. CTA collection separates sharply from LTA collection after the 2002 EIT reform. Data source: *China Taxation Yearbook*, various years.

Table 3. Export Exposure and Reform-Aligned Tax Outcomes

	Statutory-Share Amounts		Tax Composition	
	Imputed central-share EIT	Imputed central-share VAT	EIT share of EIT + VAT	Log EIT / direct remittances
Export Shock	0.005** (0.002)	0.002 (0.002)	0.059*** (0.017)	0.003*** (0.001)
Effect across exposure IQR	+48.4%	+15.9%	+4.64 pp	+24.8%
Num. obs.	310	310	310	309
First-stage F	139.4	139.4	139.4	139.3
Region FE	Y	Y	Y	Y
PCA industry controls	Y	Y	Y	Y
Baseline census controls	Y	Y	Y	Y

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Each column reports a city-level 2SLS regression of the 2007 outcome on realized export exposure, instrumented by tariff-predicted exposure. All specifications include regional fixed effects, the first three principal components of baseline industrial structure, and baseline census controls; standard errors are clustered by province. Imputed central-share EIT equals 1.5 times fiscal-book EIT under the statutory 60:40 central–local sharing rule; imputed central-share VAT equals three times fiscal-book VAT under the statutory 75:25 sharing rule. These amount outcomes are statutory-share imputations from fiscal-book tax categories, not observed city-level CTA collections. The EIT-mix share is EIT divided by EIT plus VAT. The EIT/direct-remittance outcome is log imputed central-share EIT minus log direct remittances, defined as the reported 75% VAT remittance plus consumption tax. Amount outcomes and the tax ratio are logged; the EIT share is measured in percentage points. The interquartile-range row converts logged effects to percent changes and reports the share effect in percentage points.

7 From Fiscal Capture to Selective Compensation

I now examine whether fiscal centralization is important for directing higher-level resources to localities bearing the burdens of SOE restructuring.

7.1 Policy Evidence

Central leaders treated SOE unemployment as both a fiscal obligation and a threat to social order. In a 1998 speech, Premier Zhu Rongji linked unresolved layoffs to livelihood, reemployment, and social order, warning of “instability in people’s hearts and society” (R. Zhu, 1998). He emphasized that limited state funds required prioritizing laid-off SOE workers and that central special transfers would support central and western regions and old industrial bases. Central leaders therefore made worker support an explicit national fiscal priority.

The central government institutionalized more specific responsibilities through the 2000 urban social-security pilot. The pilot addressed basic pensions, unemployment insurance, minimum-livelihood guarantees, reemployment, and the financing of social-protection obligations during enterprise restructuring (State Council, 2000). Although the center issued these directives, it placed pension, unemployment, minimum-livelihood, and reemployment obligations on local budgets.

I focus on SOE workers because their claims were tied to prior public employment and welfare commitments, their losses were concentrated in identifiable cities, and the fiscal data contain corresponding locality-level outcomes (Y. Cai, 2002; Friedman, 2014; Hurst & O’Brien, 2002; C. K. Lee, 2007; Luo et al., 2023). I treat financing for social-protection programs and replacement of fiscal capacity lost during state-firm contraction as two complementary channels of higher-level support for affected cities.

Figure 6 shows that central revenue and central-to-local transfers both expanded during the 2000s, creating the fiscal scale on which cross-locality support could operate (J. Jiang & Zhang, 2020; Lou & Wang, 2008; Wan et al., 2015). In the next section, the city panel shows how those fiscal resources were distributed across localities carrying different restructuring burdens.

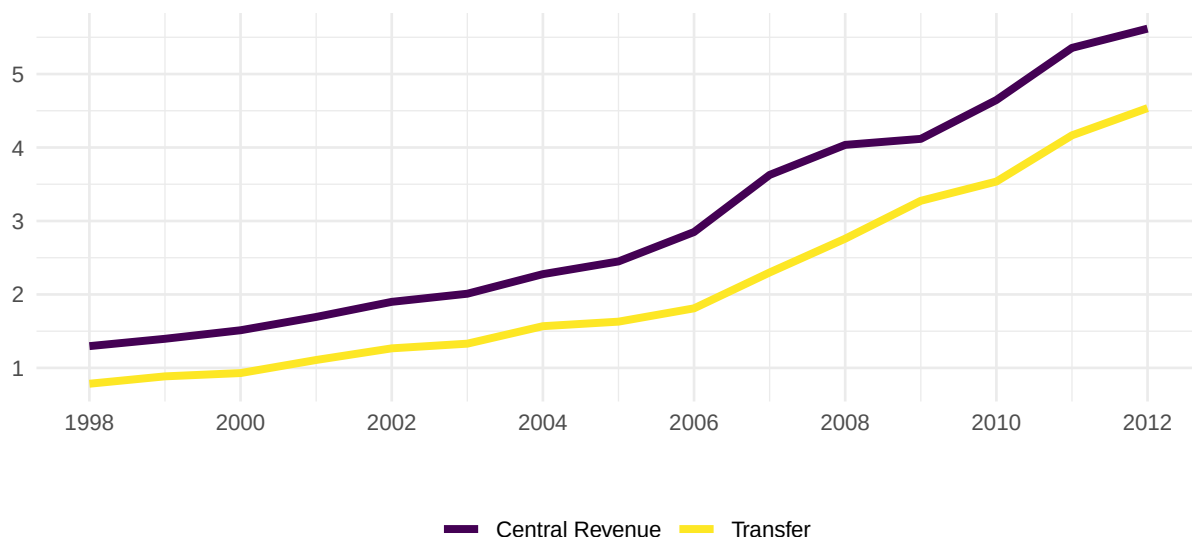


Figure 6. Expansion of central–local public transfers

Note. The figure reports central government revenue and central-to-local fiscal transfers. Data source: *China Finance Yearbook*, various years.

7.2 Fiscal Support for Restructuring Cities

As SOE employment shares contracted between 2000 and 2007, reported grant receipts and recorded retiree counts increased. Table 4 estimates how fiscal outcomes vary with the remaining SOE employment share within cities between 2000 and 2007. The specifications include city and year fixed effects; controlled columns add manufacturing share, ethnic-minority share, GDP per capita, working-age share, and out-of-province migrants, with standard errors clustered by city. Because the regressor is a remaining share, negative coefficients indicate that larger declines are associated with larger outcomes.

The first three columns use reported intergovernmental grant receipts reconstructed from explicitly labeled receipt lines. Cities with larger SOE employment-share declines receive more total grants, and Appendix Table A21 reports corresponding no-control estimates for total, per-capita, and per-SOE measures. A ten-percentage-point lower SOE employment share corresponds to about 4.2% more grants in the no-control model. The grant-to-spending estimate shows that restructuring cities also became more reliant on intergovernmental finance. Across total, per-capita,

Table 4. SOE Employment Burdens, Reported Grant Receipts, and Restructuring-Related Outcomes

	Reported Grant Receipts			Restructuring-Related Outcomes		Fiscal Dependence
	Total	Per Cap.	Per SOE	Soc. Sec.	Recorded Retirees	Grants/Spend
SOE employment share (remaining)	−0.413*** (0.077)	−0.360*** (0.094)	−3.570*** (0.491)	−3.468*** (0.516)	−0.128** (0.052)	−0.117* (0.067)
City FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls	N	Y	Y	Y	Y	Y
Num. obs.	666	664	664	664	664	664

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Units: reported intergovernmental grant receipts and social-security outcomes use source fiscal-statistics currency units; recorded retirees are counts; fiscal dependence is a grants-to-spending log ratio. Transformations: reported grant receipts are $\log(y + 1)$; per-capita outcomes subtract log total population; per-SOE outcomes subtract $\log(\text{SOE employment} + 1)$; social-security and retiree logs are winsorized; fiscal dependence is $\log$ grants minus $\log$ total spending. Controls: columns marked Controls include manufacturing population share, ethnic-minority share, GDP per capita, working-age share, and log out-of-province migrants. FE: city and year fixed effects in every column. SE: standard errors clustered by city. Sample: two-period city panel. Years: panel years are 2000 and 2007. Outcome scale: columns 1, 5, and 6 are logged outcomes; column 2 is a total-population per-capita log; columns 3 and 4 are per-SOE logs; no outcome is IHS-transformed. The grant measure sums fiscal-book receipt lines explicitly labeled tax rebates, general transfers, special transfers, and other subsidies from upper governments. Column 1 omits the added controls; columns 2–6 include them. The regressor is SOE employment share (remaining), so negative coefficients indicate larger outcomes in cities with larger SOE employment-share declines. Appendix H.6 documents schedule harmonization and component checks.

and fiscal-dependence outcomes, higher-level support rose where SOE contraction was greatest.

The fixed-effects design compares each city with itself as restructuring unfolded, absorbing persistent differences in industrial history, administrative status, and baseline fiscal scale. The total-grant result identifies the aggregate resource response; the per-capita result shows that it survives population adjustment; and fiscal dependence captures the changing composition of local finance. Their convergence is consistent with both more grant resources and greater budgetary reliance in cities undergoing deeper state-sector contraction.

The retiree and social-security spending outcomes complement the grant results. Recorded retirees—retirees carried on government accounts whose pensions are budgeted through local-government spending—rise as SOE employment falls. During this period, some layoffs took the form of contract buyouts that effectively moved workers into early retirement. Appendix Table A21 reports corresponding no-control specifications for grants, social-security resources per remaining SOE worker, and recorded retirees. Utility spending, other general spending, and public employment show no consistent response in Appendix Table A22, weighing against a uniform expansion of local budgets. Together, these fiscal responses show higher-level resources meeting the social obligations and revenue pressures created by SOE restructuring.

7.3 The Geography of Revenue and Fiscal Support

Export expansion and SOE contraction were concentrated in different cities, but China's national transfer system allowed fiscal resources to cross that territorial divide. The State Council's plan implementing the 2002 EIT reform explicitly connected additional central income-tax revenue to general transfers, while the city panel shows that grant receipts rose with SOE employment decline.

Two sets of results capture the opposite sides of pooling. Figure 7 assesses whether revenue gains and restructuring-related fiscal support were concentrated in the same cities. EIT growth is higher among cities in the top quartile of export exposure, while grant receipts and recorded retirees rise more in cities in the top quartile of SOE employment-share decline. I find only partial overlap between the two top quartiles.

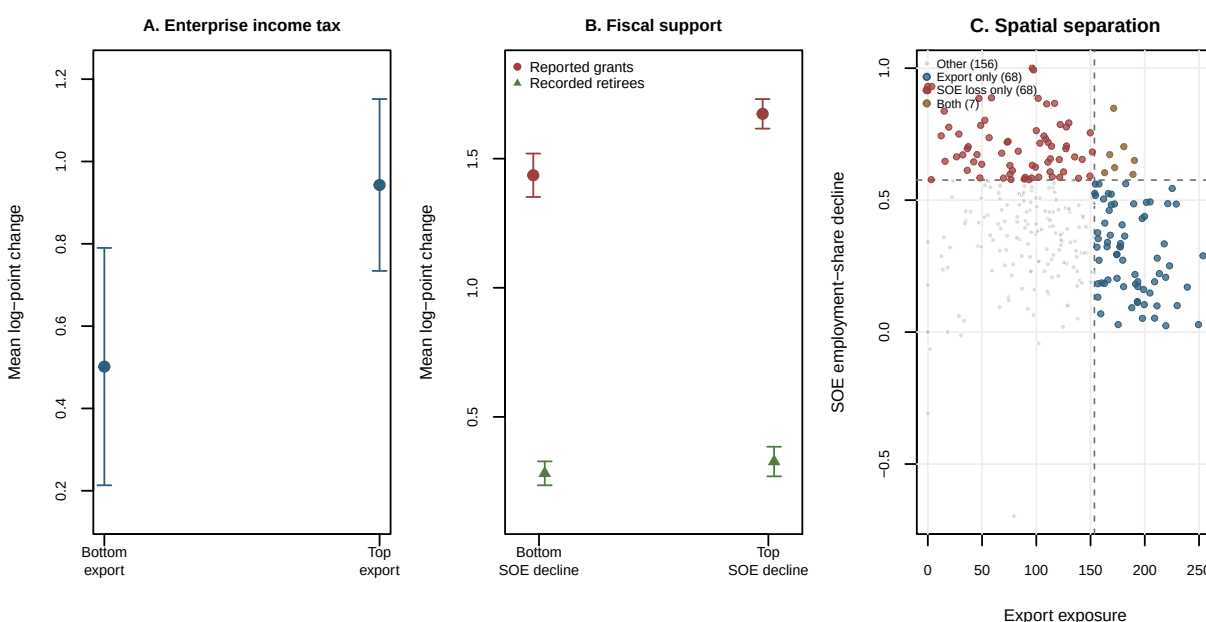


Figure 7. City-level evidence on fiscal pooling

Note. City-level descriptive evidence from the common complete-case sample. Panel A reports the mean 2000–2007 change in log fiscal-book EIT for cities in the bottom and top quartiles of export exposure. Panel B reports mean 2000–2007 changes in log reported intergovernmental grant receipts and winsorized log recorded retirees for cities in the bottom and top quartiles of SOE employment-share decline. In both panels, error bars show 95% confidence intervals around group means. Panel C plots export exposure against the decline in SOE employment share. Dashed lines mark the top-quartile cutoffs. Highlighted points identify cities in only the top export quartile, only the top SOE-decline quartile, and both top quartiles; gray points are all remaining cities.

Pooling changes the territorial logic of adjustment. Under purely local finance, a declining industrial city must meet rising pension and unemployment obligations from a shrinking economic base, while an export center retains the fiscal benefits of expansion. Centralization breaks that correspondence. Central collection jurisdiction over the expanding margin, combined with a statutory central revenue share, creates national capacity, and intergovernmental transfers direct capacity toward places bearing obligations associated with SOE unemployment. The two empirical analyses are consistent with this division of labor across cities: one locates where the national tax base grew; the other locates where grant support rose with restructuring.

Consistent with national pooling, cities with larger SOE employment declines received more intergovernmental grants even though their adjustment burden was geographically distinct from export exposure. This pattern reflects support for politically salient displaced SOE workers and may not extend to groups with weaker political claims. Together, the institutional and budget evidence is consistent with national fiscal support reaching restructuring burdens outside the principal centers of export growth.

8 Conclusion

Post-WTO China shows how openness strengthens central fiscal capacity when new firms enter a centrally controlled tax system. The CTA's hierarchy and the 2002 EIT reform placed most entrants under central administration and gave the center a statutory share of their income-tax revenue. Consistent with this mechanism, export exposure increased private-firm entry, frontline CTA recruitment, and EIT relative to taxes governed by unchanged rules.

The SOE results trace the allocation of pooled resources. Cities with larger declines in state-sector employment received more grants, relied more on grant finance, and recorded more retirees. Grant growth accompanied higher social-security spending per remaining SOE worker, while per-capita and unrelated-spending comparisons indicate a targeted response to restructuring rather than general budget expansion. Together, the export and SOE results show a national fiscal

pool linking capacity building in growth centers to adjustment burdens where affected groups are politically organized.

Embedded fiscal centralization joins the revenue and compensation sides of openness. Registered entry makes firms and taxable activity more legible, but central capacity grows only if collectors answer to the center, frontline personnel reach expanding caseloads, and remittance and revenue-sharing rules direct receipts into a national pool. These institutions reduce dependence on local relationships and allow revenue raised in one locality to finance obligations in another. Openness alone is insufficient.

China is a most-likely, scope-defining case. It entered the WTO period with an established CTA network, strong central authority over fiscal rules, manageable tariff losses, rapid private-sector growth, and established transfer channels. These inherited capacities enabled the mechanism: central administrative control was established, while frontline deployment could respond to new revenue. Beyond China, the mechanism should operate where openness expands domestically reachable registered activity, central legal claims align with administrative control, tariff losses remain manageable, and transfers can reach concentrated adjustment burdens. Although political centralization may reduce veto points and facilitate targeted support, this article examines only the authoritarian pathway and does not test whether the mechanism differs across regime types.

The mechanism faces less favorable conditions today. China's growth trajectory has slowed since the coronavirus disease 2019 (COVID-19) pandemic, while lower land-lease revenues and rising local debt have tightened subnational fiscal space even as social-spending needs have grown (World Bank, 2025). Trade conflict and tariff uncertainty have also redirected Chinese exports and investment and altered supply-chain relationships (Heise et al., 2024; L. Jiang et al., 2023; Zeng & Kim, 2025). Together, these conditions make it harder for new-firm entry and the resulting tax-base growth to outpace claims on the transfer system. The framework therefore predicts sharper central–local fiscal conflict when pooled resources grow more slowly than local obligations. Comparative research can test whether this mechanism travels by examining how new firms enter the tax system, which level of government collects their taxes, and how revenue is pooled and allocated.

Notes

1. Appendix Figure A3 shows that tariff receipts remained small relative to total tax revenue during the reform period.
2. Appendix Table A1 summarizes the default assignment rule. Appendix Figure A5 reports observed collection jurisdiction by ownership and establishment cohort in the 2007 National Enterprise Tax Survey; Appendix Figure A6 documents the corresponding post-2002 shift toward CTA collection in national EIT series disaggregated by ownership.
3. The ASMF contains balance-sheet information for all SOE manufacturers and non-state manufacturers with annual sales above 5 million Chinese yuan (CNY). Following Fan, Lai, and Li (2015), I conduct standard logic checks and remove observations with missing key identifiers or variables used to construct output, employment, ownership, city, or industry measures; observations with fewer than ten employees; duplicate records; and records with accounting inconsistencies, such as fixed assets exceeding total assets, liquid assets exceeding total assets, current assets exceeding total assets, or paid-in capital not matching the sum of ownership components. I also winsorize firm-level variables before aggregation to reduce the influence of extreme values. Industry-code harmonization, including concordances across GB vintages and mappings to ISIC and HS codes, is implemented using the `gbcrosswalk` R package.
4. The IHS transformation is $\text{IHS}(y) = \sinh^{-1}(y) = \log(y + \sqrt{y^2 + 1})$. It approximates a logarithmic transformation for large positive values while remaining defined at zero and for negative changes (Bellemare & Wichman, 2020; Burbidge et al., 1988).

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Appendix Catalog

This appendix collects institutional details, supplementary descriptive evidence, measurement procedures, and robustness checks that support the main analysis.

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Appendix A	EIT collection jurisdiction under the 2002 EIT reform.
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Appendix G	Firm-level measurement of profit-reporting alignment.
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Appendix A EIT Collection Jurisdiction under the 2002 EIT Reform

Table A1. EIT Collection Jurisdiction under the 2002 Reform

Est. Year	< 2002	$\geq$ 2002
FIE	CTA	CTA
PE	LTA	CTA (default)
SOE	LTA	CTA (default)

This table summarizes the default EIT collection assignment by firm ownership and year of establishment. Implementing exceptions remained, so Appendix Figure A5 reports the observed 2007 distribution across agencies. The nationwide rule nevertheless shifted most newly established PEs and SOEs toward CTA administration.

Appendix B Export Expansion, Firm Registration, and Aggregate EIT Receipts

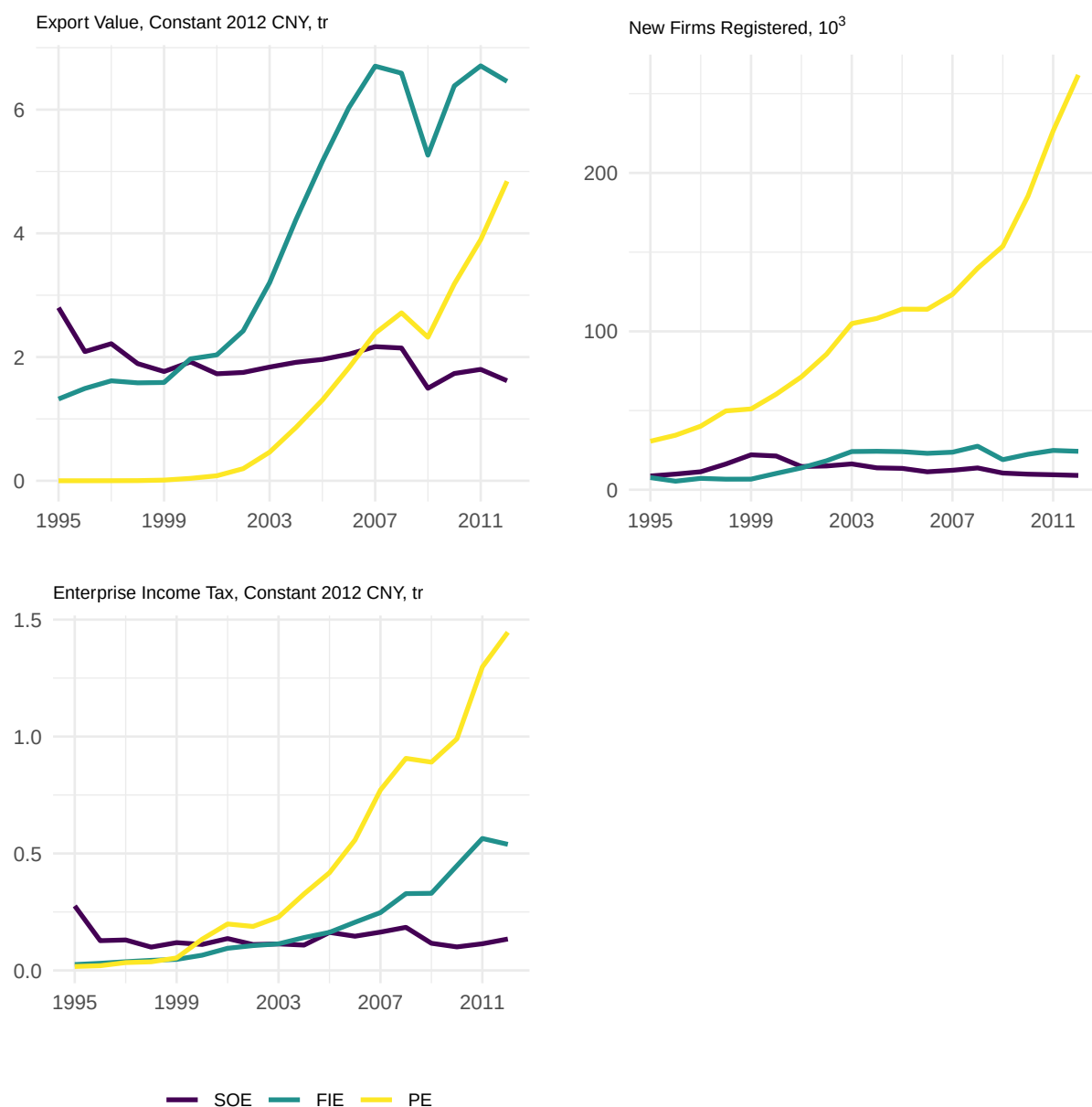


Figure A1. Export expansion, firm registration, and EIT receipts, 1995–2012

Note. The figure reports exports, new registrations, and EIT receipts by ownership type. Data sources: *China Customs Statistics Yearbook*, firm registration records, and *China Taxation Yearbook*.

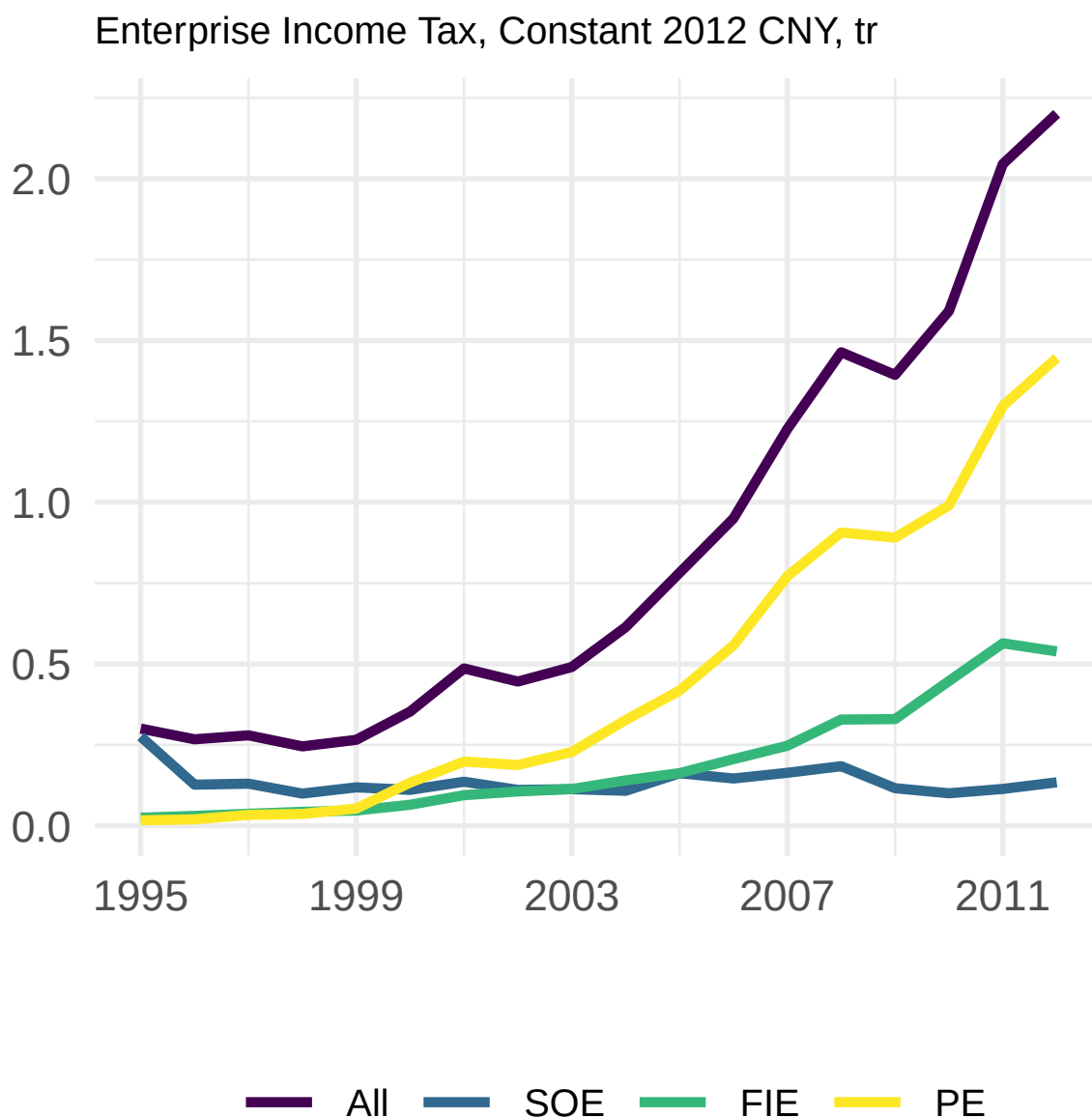


Figure A2. Total and ownership-specific EIT receipts, China, 1995–2012

Note. This figure illustrates total and ownership-specific values of EIT receipts. The difference between total EIT and the sum of SOE, FIE, and PE is due to other ownership categories not plotted. PE includes proprietorships, limited liability companies, and corporations. Data source: *China Taxation Yearbook*, various years.

Appendix C Total Tax Revenue and Tariff Revenue

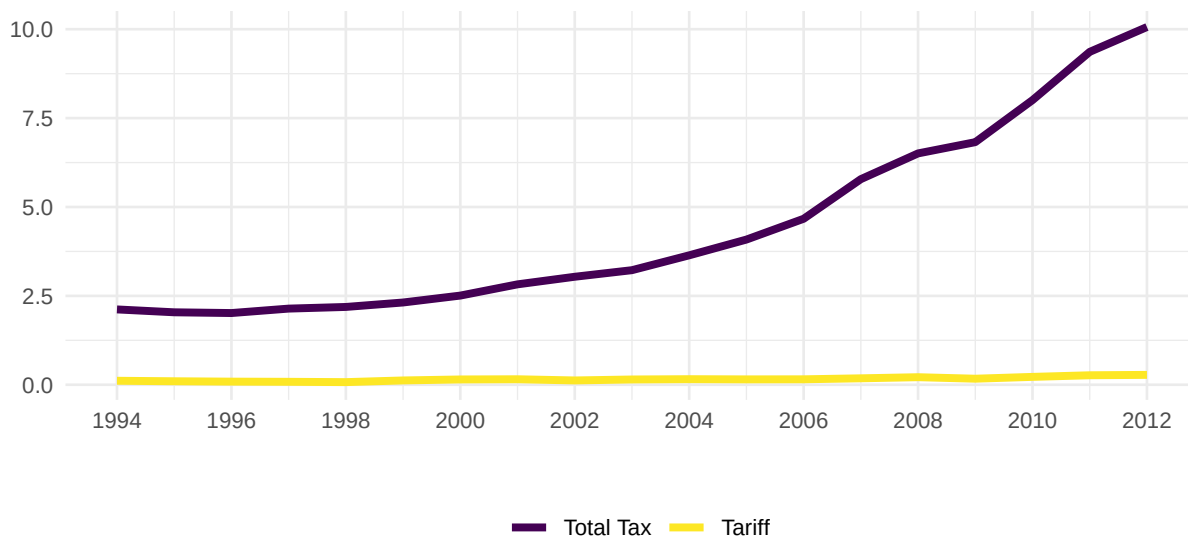


Figure A3. Total tax revenue and tariff revenue, China, 1994–2012

Note. The figure reports total tax revenue and tariff revenue in trillions of constant 2012 CNY. Data source: *China Finance Yearbook*, various years.

Appendix D Worker Attitudes Toward Redistribution

To describe redistributive attitudes among respondents reporting public-sector layoffs, I draw on data from the 2003 CGSS, conducted by Renmin University. The survey includes detailed retrospective and current information on respondents' employment histories, along with a battery of questions on political and economic attitudes. I construct a binary indicator, *Job Loss*, coded as 1 if the respondent reports having been laid off from a public-sector position in an SOE.

Figure A4 shows conditional associations between reported public-sector layoffs and key attitudinal outcomes. Full regression results are reported in Table A2, and Table A3 provides the exact survey wording for each outcome variable in both English and Chinese.

Conditional on the included covariates, respondents reporting an SOE layoff exhibit stronger redistributive preferences and economic grievances. They are more likely to support progressive

taxation (*Tax the Rich*) and to view unemployment reduction as a core governmental responsibility (*Unemployment Priority*). They are also more likely to report a decline in perceived socioeconomic status over the past three years (*SE Status Lower*) and to judge their living conditions worse than those of the average citizen (*Life Worse*).

These respondents are also more likely to agree that workers deserve higher pay (*Workers Deserve High Pay*), and their estimated probability of not being employed full time is about 42 percentage points higher (*Not Employed Full Time*).

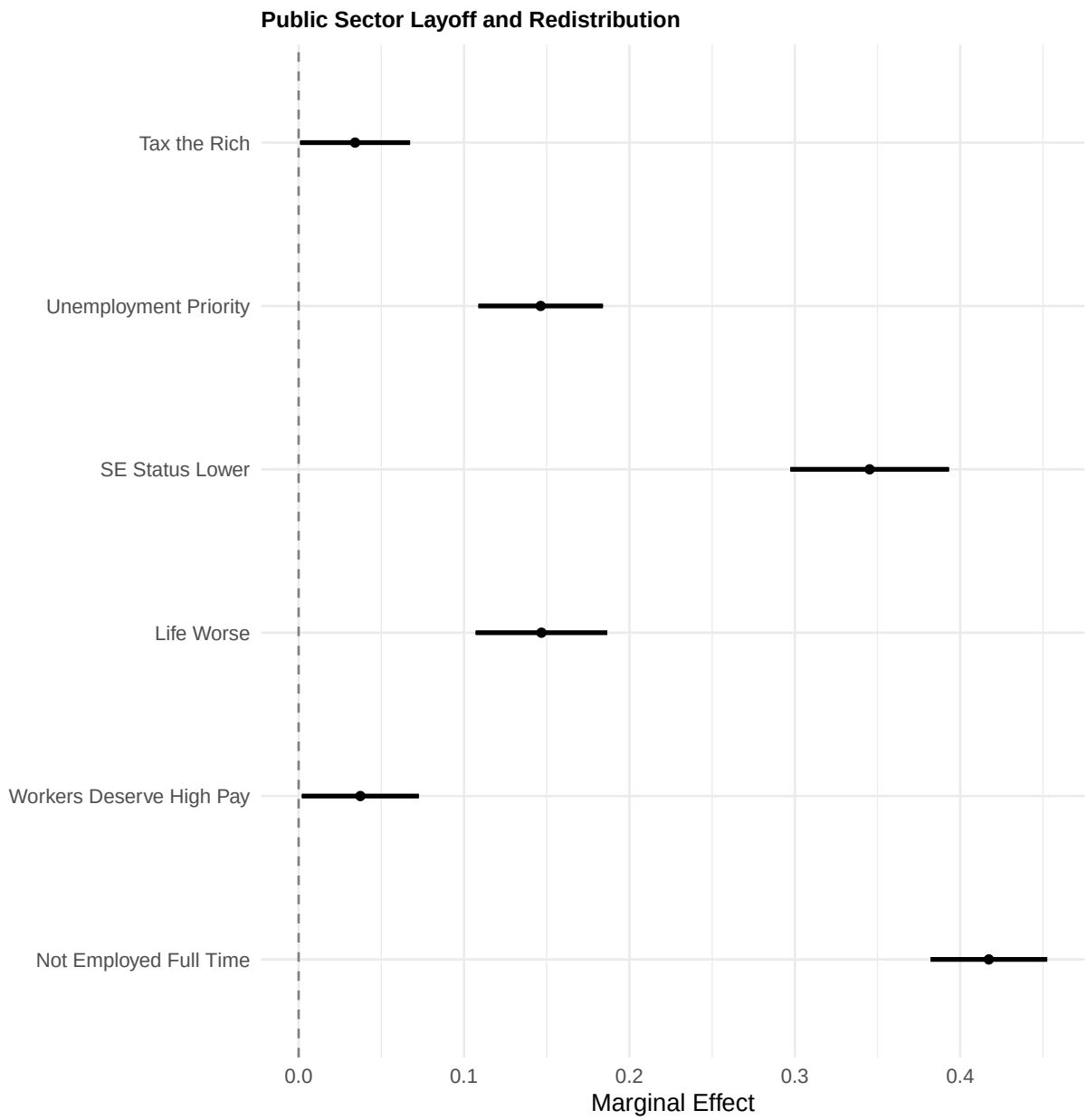


Figure A4. Workers' attitudes toward redistribution, 2003

Note. Standard errors are robust and clustered at the township level. Regression specifications are identical to those presented in Table A2.

Table A2. State Sector Layoff and Redistribution, 2003

	Tax the Rich	Unemployment Priority	SE Status Lower	Life Worse	Workers Deserve High Pay	Not Employed Full Time
Job Loss	0.034** (0.017)	0.146*** (0.019)	0.345*** (0.024)	0.147*** (0.020)	0.037** (0.018)	0.417*** (0.018)
Age	0.004*** (0.001)	-0.001 (0.001)	0.007*** (0.001)	-0.000 (0.001)	0.000 (0.001)	0.012*** (0.001)
Gender	-0.015 (0.015)	-0.051*** (0.015)	0.010 (0.018)	0.028** (0.013)	-0.030*** (0.012)	-0.178*** (0.015)
College	-0.033 (0.028)	-0.091** (0.038)	-0.145*** (0.036)	-0.104*** (0.022)	-0.090*** (0.019)	-0.220*** (0.026)
High School	-0.037** (0.016)	0.014 (0.017)	-0.047** (0.019)	-0.077*** (0.013)	-0.036*** (0.012)	-0.151*** (0.015)
Minority	0.002 (0.029)	0.022 (0.041)	-0.020 (0.046)	-0.016 (0.035)	-0.024 (0.030)	0.003 (0.036)
Prov FE	Y	Y	Y	Y	Y	Y
Occupation FE	Y	Y	Y	Y	Y	Y
Num. obs.	3741	4100	2973	3923	4066	4097

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Standard errors are robust and clustered at the township level. All models include provincial fixed effects.

Table A3. Correspondence Between Variable Key and Survey Questions

Variable Key	Survey Question
<i>Tax the Rich</i>	Do you agree with the following statement? [It should impose more taxes upon the richer to help the poorer]
<i>Unemployment Priority</i>	Do you think [unemployment] should be given priority to be solved first?
<i>SE Status Lower</i>	Compared with three years ago, has your socio-economic status [declined]?
<i>Life Worse</i>	Compared with “the life of ordinary people,” is your life [worse than the life of ordinary people]?
<i>Workers Deserve High Pay</i>	Do you think [workers] should get higher income among the following people?
<i>Not Employed Full Time</i>	Main employment status within the past three months: [not employed full time].

Appendix E EIT Collection Jurisdiction by Ownership and Establishment Cohort

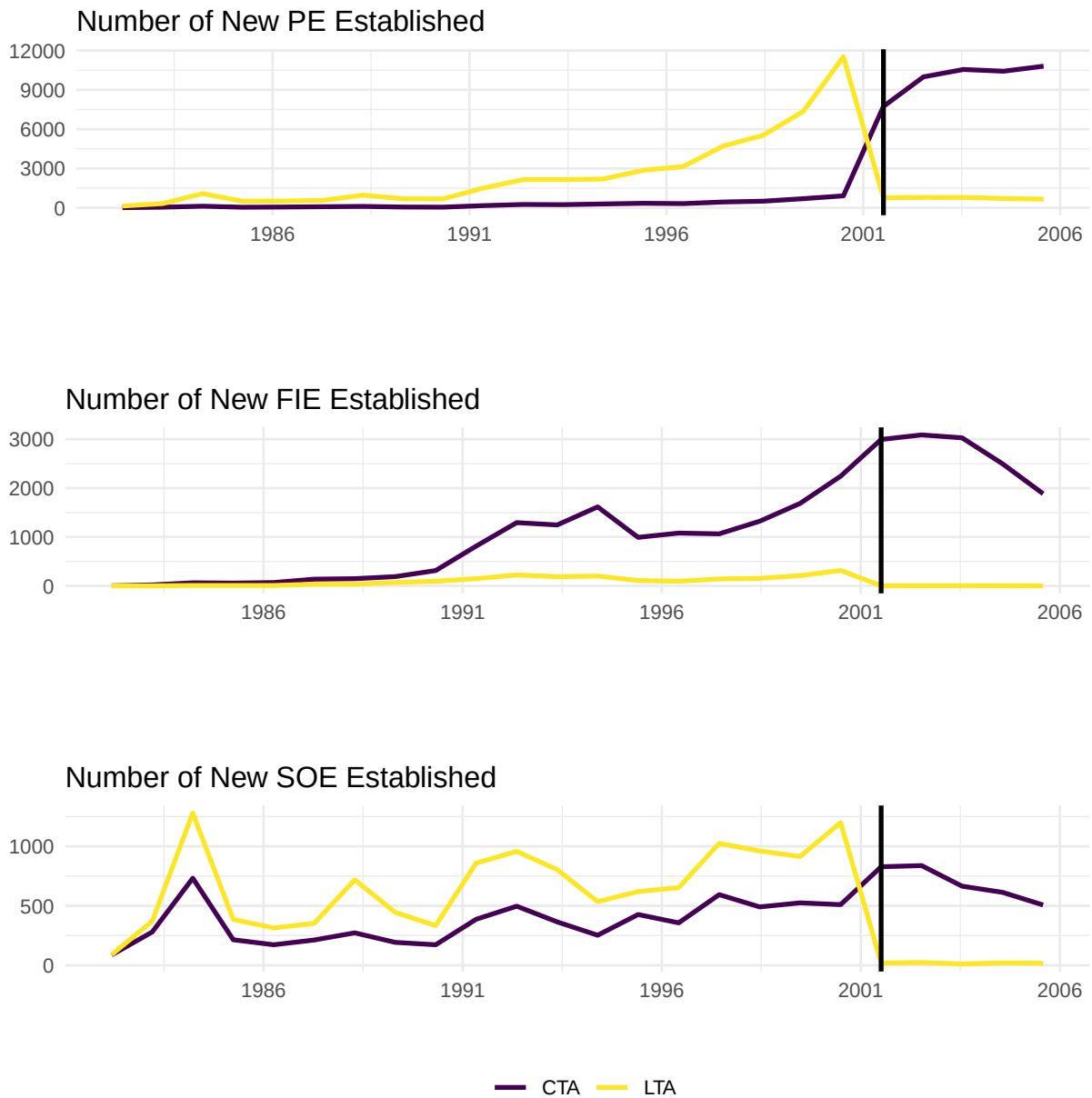


Figure A5. EIT collection jurisdiction by ownership type

Note. This figure uses the 2007 cross-section of the National Enterprise Tax Survey to report the tax agency responsible for EIT collection across ownership types. The y-axis represents the number of firms, and the x-axis groups firms by establishment year, 1986–2006. The vertical black line marks the 2002 EIT reform cutoff. Most new PEs and SOEs established after 2002 were administered by the CTA, while the plotted LTA cases show that the assignment was not categorical; FIEs were already principally administered by the CTA. Data source: National Enterprise Tax Survey, 2007.

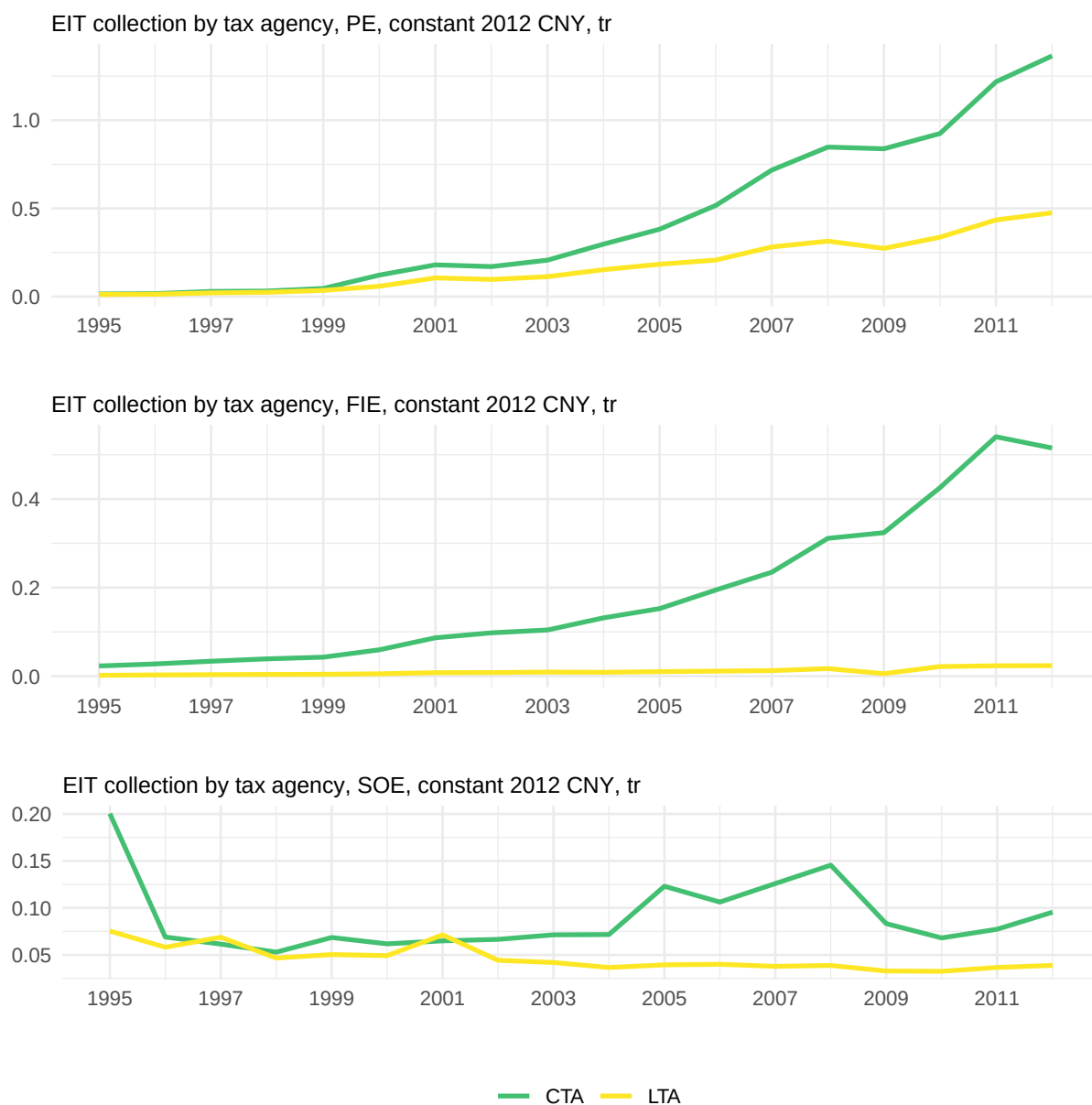


Figure A6. EIT collection by tax agency, 1995–2012

Note. The figure presents the total EIT intakes for the CTA and LTA across ownership types. The CTA began to collect a larger share of EIT from PEs and SOEs after 2002, consistent with the 2002 EIT reform. The CTA collects the EIT from nearly all FIEs. Data source: *China Taxation Yearbook*, various years.

Appendix F Constructing the Tariff-Predicted Export Shock

This appendix constructs the tariff-predicted export shock used as an instrument for realized Bartik export exposure. Following the sequence in Bombardini and Li (2020), I first estimate how the tariffs imposed by destination countries predict Chinese exports, use that relationship to recover fitted export levels, take the change in fitted exports, and then aggregate that change to cities using pre-WTO industrial output weights. The implementation uses product–destination variation rather than first collapsing tariffs to an industry average.

Let p index six-digit HS products, d destination markets, and t the initial or final period. Let X_{pdt} be the FOB value of Chinese exports in product–destination cell pd , and let τ_{pdt} be the applied ad valorem tariff faced by that cell, measured in percentage points. The initial period pairs 2000 export values with the first available tariff observation, from 2001; the final period uses 2007 export values and tariffs. I estimate

$$\ln(X_{pdt} + 1) = \theta \ln(1 + \tau_{pdt}) + \eta_p + \lambda_d + \phi_t + \varepsilon_{pdt}, \quad (5)$$

where η_p , λ_d , and ϕ_t are product, destination, and period fixed effects. Zero export flows are retained through the one-unit offset, and standard errors are clustered by HS product. The regression contains 1,428,604 product–destination–period cells. The estimated tariff coefficient is $\hat{\theta} = -0.0889$ (clustered standard error 0.0084). Because τ_{pdt} is recorded in percentage points, this coefficient is the elasticity of $X_{pdt} + 1$ with respect to $1 + \tau_{pdt}$ in that scale; it is not numerically comparable to a coefficient based on a tariff recorded as a decimal fraction.

Equation (5) makes explicit how tariff changes predict export increases. Holding the fixed effects constant, the tariff-induced fitted change for a product–destination cell is

$$\Delta \widehat{\ln(X + 1)}_{pd,\tau} = \hat{\theta} [\ln(1 + \tau_{pd,2007}) - \ln(1 + \tau_{pd,2001})]. \quad (6)$$

Since $\hat{\theta} < 0$, a tariff reduction makes the bracketed term negative and therefore contributes posi-

tively to predicted export growth. I then recover the fitted export-value level used in the construction:

$$\widehat{X}_{pdt} \equiv \exp \left[\widehat{\theta} \ln(1 + \tau_{pdt}) + \widehat{\eta}_p + \widehat{\lambda}_d + \widehat{\phi}_t \right]. \quad (7)$$

This is the exponentiated fitted value of $\ln(X_{pdt} + 1)$ used in the implementation; the same back-transformation is applied in both periods. As in Bombardini and Li (2020), the fitted level retains the estimated fixed effects. Thus “tariff-predicted” means predicted by the full tariff regression in Equation (5), not that the fitted level is a mechanical function of the tariff term alone.

I concord HS products through ISIC to the two-digit 2002 Chinese industry categories used for the city weights and sum fitted values over products and destinations:

$$\widehat{EXP}_{kt} = \sum_{p \in k} \sum_d \widehat{X}_{pdt}, \quad (8)$$

$$\widehat{\Delta EXP}_k^{00-07} = \widehat{EXP}_{k,2007} - \widehat{EXP}_{k,2000}. \quad (9)$$

Thus $\widehat{\Delta EXP}_k^{00-07}$ is a fitted change in export value. It is neither a tariff change nor an export growth rate, and its construction does not impose a unit elasticity between tariffs and exports.

Finally, I aggregate the fitted industry changes to cities using the same 2000 output weights and normalization as in the realized export shock:

$$\widetilde{EXPSHock}_i^{07} = \sum_k \frac{Y_{ik}^{00}}{Y_k^{00}} \frac{\widehat{\Delta EXP}_k^{00-07}}{Y_i^{00}} \quad (10)$$

$$= \sum_k \frac{Y_{ik}^{00}}{Y_i^{00}} \frac{\widehat{\Delta EXP}_k^{00-07}}{Y_k^{00}}. \quad (11)$$

The second line displays the measure in the shift–share form used by Bombardini and Li (2020): a city’s initial output share in industry k multiplies the national tariff-predicted export change per unit of that industry’s initial output. In the source data, export values are in US dollars and 2000 gross industrial output from the Annual Survey of Manufacturing Firms is in thousands of yuan. The instrument is therefore measured as fitted export dollars per thousand yuan of baseline industrial

output. The realized and predicted measures differ only in whether ΔEXP_k^{00-07} or $\widehat{\Delta EXP_k^{00-07}}$ enters the common aggregation, so their units and normalization coincide.

The exclusion restriction applies to the fitted-export instrument as implemented. Conditional on baseline city characteristics, regional fixed effects, and baseline industrial-structure controls, cross-city differences in fitted export exposure from Equation (11) must affect the outcomes through realized export exposure rather than through an independent city-specific path. Because the fitted export levels retain the estimated fixed effects, this assumption concerns the full fitted-export measure, not the tariff term in isolation. The mechanism tests then ask where that exposure response appears: new private firms, CTA recruitment in local branch offices, and tax-specific fiscal-book revenue. The fiscal contrasts use categories aligned with the statutory collection and sharing rules, and central shares are imputed from those statutory proportions.

Appendix G Measuring Profit-Reporting Alignment

This section provides firm-level evidence on profit reporting among firms presumptively assigned to the CTA under the 2002 default rule. It measures the gap between reported and imputed profits—often described as profit-hiding—and asks how that relationship changes from 2002 to 2007. The exercise complements the city design by documenting whether reporting moved in the direction expected as the presumptive CTA sample became more fiscally legible.

Following H. Cai and Liu (2009), I construct a profit benchmark by subtracting intermediate inputs, financing costs, wages, current depreciation, and VAT from gross output, then compare that benchmark with reported profit. The procedure adapts book–tax-difference approaches used to study corporate reporting and avoidance (M. A. Desai & Dharmapala, 2006; Manzon & Plesko, 2002). Because the benchmark is constructed rather than observed, the resulting gap measures reporting alignment, not tax evasion directly.

Let $PRO_{i,t}$ represent the imputed profit of firm i in year t , $RPRO_{i,t}$ represent the reported profit, and $\pi_{i,t}$ denote the real (unobserved) profit. The imputed profit is calculated as:

$$PRO_{i,t} = Y_{i,t} - MED_{i,t} - FC_{i,t} - WAGE_{i,t} - CURRD_{i,t} - VAT_{i,t}$$

where:

- $Y_{i,t}$ is the firm's gross output;
- $MED_{i,t}$ measures intermediate inputs excluding financial costs;
- $FC_{i,t}$ represents financing costs;
- $WAGE_{i,t}$ is total wages;
- $CURRD_{i,t}$ is current depreciation; and
- $VAT_{i,t}$ is the firm's VAT payment.

The imputed profit $PRO_{i,t}$ is a noisy estimate of real profit $\pi_{i,t}$ and is expected to be positively correlated with it. The relationship is modeled as:

$$\pi_{i,t} = PRO_{i,t} + \eta_{i,t} + \theta_{i,t}$$

where $\eta_{i,t}$ captures the difference in accounting standards across firms¹ and $\theta_{i,t}$ is a random error with mean zero.

The relationship between reported and real profits is then:

$$RPRO_{i,t} = d_{i,t}\pi_{i,t} + e_{i,t} + \xi_{i,t}$$

where $d_{i,t} < 1$ measures the alignment between reported and real profit. A smaller $d_{i,t}$ indicates greater misalignment. $e_{i,t} < 0$ is consistent with under-reporting, and $\xi_{i,t}$ is a random error term with mean zero.

Substituting $\pi_{i,t}$ into the reported profit equation yields:

¹For instance, firms may follow different depreciation rates, leading to discrepancies between reported and real profits.

$$RPRO_{i,t} = d_{i,t}PRO_{i,t} + d_{i,t}\eta_{i,t} + e_{i,t} + \varepsilon_{i,t}$$

where $\varepsilon_{i,t}$ is a random error term. The coefficient $d_{i,t}$ represents reporting alignment: a higher $d_{i,t}$ implies a smaller difference between benchmark and reported profit.

G.1 Data and Econometric Specification

I use an unbalanced panel dataset of major China-based manufacturing firms from 2002 to 2007, drawn from the ASMF administered by China's National Bureau of Statistics. This dataset covers all SOE manufacturers and non-SOE manufacturers with annual sales above CNY 5 million and includes rich financial variables to study firm behavior over time.

I follow *Generally Accepted Accounting Principles* (GAAP) screening procedures to remove erroneous observations.² The ASMF does not report each firm's collecting tax agency, and I do not merge these records to a separate firm-level jurisdiction source. I therefore infer presumptive CTA assignment from establishment cohort: the estimation sample retains firms established on or after January 1, 2002, whose EIT the reform assigned to the CTA by default. This rule-based classification cannot identify the statutory exceptions for mergers, divisions, or ownership conversions that remained with the LTA. The results thus describe reporting alignment among firms presumptively assigned to the CTA, not a sample whose CTA jurisdiction is directly observed.³

G.2 Results

To estimate how $d_{i,t}$ varies over time from 2002 to 2007, I run the following specification in the presumptive CTA sample:

²Specifically, I remove observations with missing key variables, fewer than ten employees, or accounting errors (e.g., fixed asset values exceeding total asset values). I also remove duplicate observations.

³Appendix Table A1 summarizes the default assignment rule. Appendix Figure A5 uses the separate 2007 National Enterprise Tax Survey, in which collecting agency is observed, to show the realized distribution across the CTA and LTA by ownership and establishment cohort.

Table A4. Profit-Reporting Alignment, 2002–2007

	2002	2003	2004	2005	2006	2007
Imputed Pro	0.075*** (0.012)	0.096*** (0.006)	0.099*** (0.006)	0.111*** (0.005)	0.113*** (0.005)	0.120*** (0.005)
City FE	Y	Y	Y	Y	Y	Y
Ind FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Num. obs.	1376	9145	23131	30812	65196	88469

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Standard errors are robust and clustered at the city level. Control variables include firm age and total current assets.

$$RPRO_{i,m,n} = \gamma_1 PRO_{i,m,n} + \gamma_2 X_{i,m,n} + \omega_m + \xi_n + \varepsilon_{i,m,n}$$

where i , m , and n index firm, city, and industry, respectively. $X_{i,m,n}$ includes firm-level covariates such as firm age and size. I include industry and city fixed effects, and $\varepsilon_{i,m,n}$ is the error term.

The key coefficient of interest is γ_1 , which measures $d_{i,t}$. A higher γ_1 implies closer alignment between benchmark and reported profit; a lower value implies a wider reporting gap.

I report the estimates of γ_1 for 2002–2007 in Table A4. The coefficient rises by roughly 60% across the period, indicating that reported profits moved closer to the imputed benchmark in this sample. The pattern is consistent with improved reporting alignment among firms presumptively assigned to the CTA and provides a firm-level counterpart to the city results. As a national repeated-cross-section pattern based on rule-inferred jurisdiction, it corroborates the proposed mechanism without serving as a separate estimate of the effect of local CTA staffing.

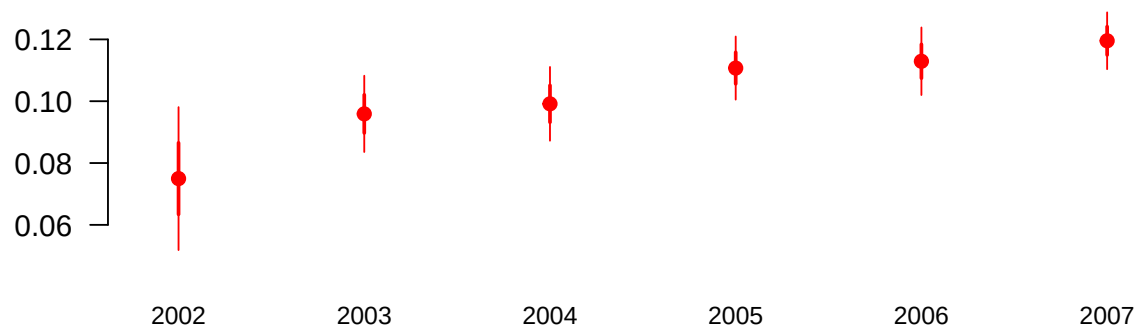


Figure A7. Profit-reporting alignment, CTA firms, 2002–2007

Note. This figure presents coefficient estimates of reported profit on the constructed profit benchmark. A higher positive coefficient indicates closer reporting alignment. Coefficients are estimated using the full specifications.

I interpret the upward trend in Figure A7 as consistent with improving profit reporting among firms under CTA administration. I find that the gap between imputed and reported profits narrowed in the observed sample. Without an untreated comparison group or an exposure interaction, however, I cannot attribute that trend to CTA expansion, professionalization, or WTO accession.

I use the reporting exercise to supply mechanism-consistent context, not fiscal tracing. I show that centrally administered firms became more aligned with a constructed profit benchmark during the same institutional period in which the CTA expanded. I treat the city-level recruitment evidence as the stronger test of exposure-specific administrative deployment; neither analysis identifies the center’s retained share or links a particular tax payment to later fiscal support.

Appendix H Additional Identification and Robustness

Analyses

I group the supplementary analyses by inference, identification and exposure construction, revenue measurement and fiscal composition, transformations and per-capita scaling, placebo and pre-treatment tests, fiscal-support outcomes, and sensitivity diagnostics. I do not treat any single table as a replacement for the article’s sequential evidence. Instead, I use these checks to assess the stability of the institutional sequence across exposure definitions, outcome scales, inference choices, timing comparisons, and influential observations and to sharpen the interpretation of the revenue results. I use HC2 standard errors in Tables A17–A19 because they are one-observation-per-city transformation, sector-composition, and pre-WTO placebo diagnostics, not alternatives to the province-clustered main inference.

H.1 Data Summary and Inference Diagnostics

Table A5 summarizes the raw variables underlying the main empirical tables. Table A6 re-estimates the headline Bartik specifications with city-clustered standard errors. Its EIT column uses the same outcome, controls, and 310-city sample as Main Table 3; only the standard-error calculation changes. Because each city appears once in these cross-sectional specifications, this is a singleton-city robust-inference diagnostic rather than an alternative clustering design. The point estimates are unchanged, while the standard errors do not allow province-level dependence. Table A7 bootstraps the first stage and several OLS associations with realized exposure at the province level. Because realized exposure is endogenous, the outcome rows are neither reduced forms nor wild-bootstrap IV tests. They show directional consistency under a different small-cluster calculation but do not replace the province-clustered 2SLS inference.

Table A5. Summary Statistics for Main Empirical Variables

Variable	N	Missing	Mean	SD	P25	Median	P75	Max
Realized export exposure (firm sample)	310	2	112.423	55.598	74.757	111.557	152.408	253.703
Tariff-predicted exposure (firm sample)	310	2	36.321	26.086	13.460	32.937	52.614	109.136
Realized export exposure (staffing sample)	312	0	112.051	55.783	74.386	111.131	151.938	253.703
Δ Private firms	310	2	995.248	1731.536	237.000	492.000	888.000	16659.000
Δ Private-sector employment	251	61	115.921	219.406	11.300	50.684	154.945	1693.329
Δ FIE firms	310	2	31.284	134.720	0.000	7.500	20.750	2107.000
Δ SOE firms	310	2	-25.165	62.612	-44.000	-19.500	2.000	630.000
All CTA recruits	312	0	7.968	14.236	0.000	3.000	12.000	130.000
Mean CTA recruits, 2006–2008	312	0	5.159	7.470	1.500	3.500	5.750	77.500
Branch CTA recruits	312	0	7.638	13.674	0.000	3.000	11.250	126.000
Headquarters CTA recruits	312	0	0.330	3.904	0.000	0.000	0.000	65.000
Log imputed central-share EIT	310	2	9.833	1.706	8.874	9.809	10.892	14.817
Log imputed central-share VAT	310	2	11.563	1.349	10.865	11.611	12.366	15.260
Log imputed central-share EIT / direct remittances	309	3	-2.099	0.732	-2.575	-2.123	-1.578	-0.091
EIT share of EIT + VAT (pp)	310	2	28.080	11.966	18.957	27.299	36.432	70.911

Summary statistics use the included analysis-ready city sample. Export exposures are in percentage points. The first two rows use the 310-city private-firm sample; the realized-exposure standard deviation in that sample is 55.598, rounded to 55.6 in the private-firm discussion. The third row uses the 312-city staffing sample; its realized-exposure standard deviation is 55.783 and its 74.386–151.938 interquartile range is the 74.4–151.9 spread used to interpret the CTA-recruitment estimates. Firm variables are 2000–2007 changes; CTA outcomes summarize 2007 recruitment, except the stated 2006–2008 mean. Imputed central-share EIT equals 1.5 times fiscal-book EIT, and imputed central-share VAT equals three times fiscal-book VAT. The EIT/direct-remittance ratio subtracts the log of reported 75% VAT remittances plus consumption tax from log imputed central-share EIT. These are fiscal-book and statutory-share measures, not observed agency collections. Fiscal amount outcomes are logged 2007 values; the EIT share is EIT divided by EIT plus VAT in percentage points.

Table A6. City-Clustered Standard Errors

	Firm Growth	Tax Bureaucracy	Tax Revenue
	Δ PE	CTA recruits	EIT
Export Shock	8.247*** (2.585)	0.075*** (0.029)	0.005** (0.002)
Region FE	Y	Y	Y
Controls	Y	Y	Y
Cluster	City	City	City
Num. obs.	310	312	310

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Units: private-firm outcome is a count change; tax bureaucracy is CTA recruit counts; EIT uses source fiscal-statistics currency units before logging. Transformations: EIT is logged; export shock is instrumented with tariff-predicted export exposure; no outcome is converted to per-capita units. Controls: baseline 2000 census and industry controls: unemployment share, manufacturing employment share, share ages 20–34, male share, and the first three principal components of baseline industrial structure. FE: regional fixed effects in every column. SE: standard errors clustered by city as a singleton-city diagnostic. Sample: included city-level cross-section with one observation per city. Years: firm changes are measured from 2000 to 2007; fiscal outcomes are measured in 2007; controls are measured in 2000. Outcome scale: private firms and tax bureaucracy are levels; EIT is a log outcome; no outcome is IHS-transformed or per-capita. Each city appears once, so city-clustered inference is equivalent to robust inference in this cross-sectional setting. The EIT column re-estimates the headline EIT specification on the same 310-city sample; only the standard-error calculation changes.

Table A7. Provincial Wild-Cluster Bootstrap OLS Checks for Bartik Outcomes

Model	Term	Estimate	Wild-bootstrap SE	p-value	N
First stage	exp_shock_pred_p	1.679	0.105	< 0.001	312
Firm formation OLS check	exp_shock_raw_p	5.211	1.530	< 0.001	310
Tax bureaucracy OLS check	exp_shock_raw_p	0.060	0.025	0.015	312
EIT OLS check	exp_shock_raw_p	0.004	0.002	0.029	310

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. The table reports provincial wild-cluster bootstrap checks using 500 bootstrap replications and clusters defined by `prov_cd`. The first row bootstraps the first stage. The outcome rows regress each outcome on realized export exposure with regional fixed effects and baseline controls. Because realized exposure is endogenous, those rows are OLS association checks rather than reduced forms or bootstrapped IV estimates. They are supplemental diagnostics, not replacements for the province-clustered 2SLS estimates.

H.2 Identification and Exposure Checks

Goldsmith-Pinkham et al. (2020) caution that in shift-share designs, the identifying variation may be heavily concentrated in a small subset of industries, especially when numerous initial regional shares are involved. To quantify the influence of each industry's initial share on the overall identifying variation, they propose calculating Rotemberg weights. Higher weights indicate greater influence of a specific industry on the coefficient estimate.

Following their recommendation, I compute Rotemberg weights for Export Shock. Table A8 presents the Rotemberg weights of the top 10 industries for Export Shock. To assess whether the most influential industry shares mechanically drive the results, I reconstruct the export shock and its predicted component after excluding the five industries with the highest Rotemberg weights. Table A9 keeps the original main shock as the comparison column, then rebuilds the robustness shock using baseline industry exposure data and removes the top industries cumulatively. The private-firm and CTA-recruitment estimates remain near 8.7 and 0.078, respectively. Imputed central-share EIT remains positive, with coefficients near 0.003 after the first exclusion, and imputed central-share VAT remains close to zero. The broader EIT-plus-individual-income-tax bundle remains positive and statistically significant at the 10% level after every exclusion, with coefficients near 0.004. Tables A10, A11, and A12 provide additional no-top-Rotemberg results. The stable signs across the sequential exclusions show that no single high-weight industry generates the institutional sequence.

Table A8. Top 10 Industries: Rotemberg Weights

Industry code	Industry	β	α_k
356	Furnace and electric furnace manufacturing	0.2066	0.3583
359	Metal casting and forging	0.0696	0.2328
282	Synthetic fiber manufacturing	0.2425	0.2023
137	Fruit, vegetable and nut processing	0.2693	0.1805
233	Reproduction of recorded media	0.3208	0.1656
376	Aircraft and spacecraft manufacturing	0.0881	0.1630
133	Vegetable oil processing	0.1649	0.1426
266	Special chemical products manufacturing	0.0218	0.1424
171	Cotton and chemical fiber textile processing	0.1002	0.1334
371	Railway transport equipment manufacturing	0.2391	0.1318

Units: β is the industry-specific Rotemberg coefficient and α_k is the Rotemberg weight share. Industry labels are concise English translations of the GB/T 4754–2002 industry names. Transformations: weights are computed from residualized city-level exposure and fiscal-capacity variation using baseline firm-output shares; industry shifts are recovered from the saved main city-level shock so the full reconstructed exposure equals the paper's main export shock; no outcome is logged, IHS-transformed, or converted per capita in this descriptive table. Controls: residualization uses baseline 2000 unemployment share, manufacturing employment share, share ages 20–34, male share, and the first three principal components of baseline industrial structure. FE: regional fixed effects enter the residualization step. SE: no regression standard errors are reported because this is a descriptive weight table. Sample: included city-level cross-section matched to upstream industry firm-output shares. Years: baseline shares and controls are measured in 2000; the saved main export shock measures the paper's 2000–2007 exposure window. Outcome scale: descriptive weights and coefficients are in levels; no table outcome is IHS-transformed or per-capita.

Table A9. Sequential Rotemberg Exclusions: 2SLS Estimates

	Full main shock	Drop top 1 +356 Furnaces	Drop top 1-2 +359 Casting/forging	Drop top 1-3 +282 Synthetic fibers	Drop top 1-4 +137 Fruit/veg/nuts	Drop top 1-5 +233 Recorded media
Δ Private firms	8.247*** (2.331)	8.675*** (2.402)	8.673*** (2.401)	8.668*** (2.401)	8.668*** (2.401)	8.668*** (2.401)
CTA recruits, 2007	0.075*** (0.027)	0.077*** (0.028)	0.077*** (0.028)	0.078*** (0.028)	0.078*** (0.028)	0.078*** (0.028)
Imputed central-share EIT	0.005** (0.002)	0.004 (0.002)	0.004 (0.002)	0.004 (0.002)	0.004 (0.002)	0.004 (0.002)
Imputed central-share VAT	0.002 (0.002)	0.001 (0.002)	0.001 (0.002)	0.001 (0.002)	0.001 (0.002)	0.001 (0.002)
EIT + IIT (appendix)	0.005** (0.002)	0.004** (0.002)	0.004** (0.002)	0.004** (0.002)	0.004** (0.002)	0.004** (0.002)
Region FE	Y	Y	Y	Y	Y	Y
PCA Ind	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Units: export shocks are percentage-point exposure measures; private firms are count changes; CTA recruits in 2007 are counts; imputed central-share EIT equals 1.5 times fiscal-book EIT; imputed central-share VAT equals three times fiscal-book VAT, and the EIT-plus-IIT row is an appendix sensitivity equal to 1.5 times the sum of fiscal-book EIT and individual income tax; these are statutory-share imputations, not observed agency collections. Transformations: the imputed central-share outcomes and the appendix EIT-plus-IIT bundle are logged; the first column uses the original main export shock and instrument, while each exclusion column instruments its reconstructed export shock with the corresponding tariff-predicted shock; top-weighted industries are cumulatively excluded one at a time. Controls: full baseline 2000 census and industry controls in every column. FE: regional fixed effects in every column. SE: standard errors clustered by province. Sample: included city-level cross-section with cumulative top-Rotemberg industry exclusions. Years: firm changes are measured from 2000 to 2007; fiscal outcomes are measured in 2007; controls are measured in 2000. Outcome scale: private firms and CTA recruits are level outcomes; the statutory-share outcomes and the appendix EIT-plus-IIT bundle are log outcomes; no outcome is IHS-transformed or per-capita. Each cell reports a separate full-control 2SLS coefficient for the outcome in that row. The first column uses the original main export shock for comparison; subsequent columns cumulatively exclude the top Rotemberg-weight industries listed in Table A8 from the robustness shock, one additional industry at a time. Sequential exclusion order: top 1 removes 356 (Furnace and electric furnace manufacturing); top 2 adds 359 (Metal casting and forging); top 3 adds 282 (Synthetic fiber manufacturing); top 4 adds 137 (Fruit, vegetable and nut processing); top 5 adds 233 (Reproduction of recorded media).

Table A10. Export Shock and Change in Firm Registration: 2SLS Estimates with Top Five Rotemberg Industries Removed

	First Stage		Δ PE		Δ PE Emp		Δ FIE	Δ SOE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Export Shock		10.400*** (2.812)	11.696*** (2.954)	8.242*** (2.332)	1.149** (0.419)	0.275** (0.130)	0.038 (0.104)	
$\widetilde{EXPShock}_i^{07}$	1.679*** (0.105)							
Region FE	Y	Y	Y	Y	Y	Y	Y	
PCA Ind	N	N	Y	Y	Y	Y	Y	
Controls	N	N	N	Y	Y	Y	Y	
Num. obs.	312	310	310	310	251	310	310	

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Units: export shocks are percentage-point exposure measures; firm outcomes are counts, except private-sector employment is measured in thousands. Transformations: the no-top export shock is instrumented with the no-top tariff-predicted shock; private-sector employment divides by 1,000; no firm outcome is logged in this robustness table. Controls: baseline 2000 census and industry controls: unemployment share, manufacturing employment share, share ages 20–34, male share, and the first three principal components of baseline industrial structure; columns marked PCA include only the principal components. FE: regional fixed effects in every column. SE: standard errors clustered by province. Sample: included city-level cross-section with top Rotemberg industries removed from the shock. Years: firm changes are measured from 2000 to 2007; controls are measured in 2000. Outcome scale: outcomes are level changes; no outcome is IHS-transformed or per-capita. Because these city-level models are cross-sectional with one observation per city, city-level clustering is not separately informative; province-level clustering allows arbitrary correlation in unobserved shocks across cities within the same province. The export shock and instrument exclude the five highest-Rotemberg-weight industries listed in Table A8; the no-top shocks are reconstructed from baseline firm-output shares by subtracting each excluded industry's contribution from the robustness shock.

Table A11. Export Shock and Central Tax Administrative Capacity: 2SLS Estimates with Top Five Rotemberg Industries Removed

	DV: All			06-08 Mean	Branch	HQ
	(1)	(2)	(3)	(4)	(5)	(6)
Export Shock	0.107*** (0.027)	0.099*** (0.031)	0.076*** (0.027)	0.038** (0.015)	0.077*** (0.027)	−0.001 (0.002)
Region FE	Y	Y	Y	Y	Y	Y
PCA Ind	N	Y	Y	Y	Y	Y
Controls	N	N	Y	Y	Y	Y
Num. obs.	312	312	312	312	312	312

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Units: tax-administration outcomes are CTA recruit counts; column 4 is the 2006–2008 mean count; columns 5–6 split branch and headquarters counts. Transformations: outcomes are unlogged levels; the no-top export shock is instrumented with the no-top tariff-predicted shock. Controls: baseline 2000 census and industry controls: unemployment share, manufacturing employment share, share ages 20–34, male share, and the first three principal components of baseline industrial structure; columns marked PCA include only the principal components. FE: regional fixed effects in every column. SE: standard errors clustered by province. Sample: included city-level cross-section with top Rotemberg industries removed from the shock. Years: main staffing outcomes are measured in 2007, except the mean column uses 2006–2008; controls are measured in 2000. Outcome scale: all outcomes are levels; no outcome is IHS-transformed or per-capita. Because these city-level models are cross-sectional with one observation per city, city-level clustering is not separately informative; province-level clustering allows arbitrary correlation in unobserved shocks across cities within the same province. The export shock and instrument exclude the five highest-Rotemberg-weight industries listed in Table A8; the no-top shocks are reconstructed from baseline firm-output shares by subtracting each excluded industry's contribution from the robustness shock.

Table A12. Export Shock, Firm Entry, and Statutory-Share Outcomes with Top Five Rotemberg Industries Removed

	Dependent variable: Log Imputed Central-Share EIT			Dependent variable: Log Imputed Central-Share VAT				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Export Shock	0.005** (0.002)				0.002 (0.002)			
Δ PE		0.319*** (0.079)				0.212*** (0.054)		
Δ FIE			1.373 (0.837)				0.871 (0.564)	
Δ SOE				-1.490 (1.049)				-1.841** (0.780)
Region FE	Y	Y	Y	Y	Y	Y	Y	Y
PCA Ind	Y	Y	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
Num. obs.	310	308	308	308	310	308	308	308

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Units: imputed central-share EIT equals 1.5 times fiscal-book EIT under the 60:40 sharing rule, and imputed central-share VAT equals three times fiscal-book VAT under the 75:25 sharing rule; these are statutory-share imputations, not observed agency collections; firm-sector regressors are firm-count changes in thousands. Transformations: the imputed central-share EIT and VAT outcomes are logged; firm-sector regressors divide firm-count changes by 1,000; columns 1 and 5 instrument the no-top export shock with the no-top tariff-predicted shock, while the remaining columns report controlled OLS associations. Controls: baseline 2000 census and industry controls: unemployment share, manufacturing employment share, share ages 20-34, male share, and the first three principal components of baseline industrial structure; columns marked PCA include only the principal components. FE: regional fixed effects in every column. SE: standard errors clustered by province. Sample: included city-level cross-section with top Rotemberg industries removed from the shock. Years: fiscal outcomes are measured in 2007; firm changes are measured from 2000 to 2007; controls are measured in 2000. Outcome scale: all outcomes are log levels; no outcome is IHS-transformed or per-capita. Because these city-level models are cross-sectional with one observation per city, city-level clustering is not separately informative; province-level clustering allows arbitrary correlation in unobserved shocks across cities within the same province. The export shock and instrument exclude the five highest-Rotemberg-weight industries listed in Table A8; the no-top shocks are reconstructed from baseline firm-output shares by subtracting each excluded industry's contribution from the robustness shock.

Table A13 then varies the exposure measure and estimating sample for private-firm formation, CTA recruitment, and EIT's share of EIT plus VAT. Column 1 reproduces the main 2SLS specification. Column 2 uses the exposure measure through 2010, while column 3 enters tariff-predicted exposure directly as a reduced-form regressor. Column 4 reconstructs both the realized export shock and its tariff-predicted instrument from the underlying industry-level components and cities' 2000 output weights, holding the sample, controls, and regional fixed effects fixed. This reconstruction provides the baseline for column 5, which removes the industry with the highest Rotemberg weight. Columns 6 and 7 exclude coastal province-level units and provincial capitals and municipalities, respectively. Column 8 replaces region fixed effects with province fixed effects. The three outcomes retain the predicted positive sign in every variant. The firm and recruitment estimates are especially stable. The EIT-VAT composition estimate is positive and statistically significant at the 5 percent level in seven specifications and at the 10 percent level when provincial capitals and municipalities are excluded, consistent with a shift toward the tax base directly affected by the 2002 reform.

Table A13. Alternative Exposure Specifications

	Main 2SLS	2010 exposure	Predicted exposure	Industry shock	Drop top ind.	No coastal	No capital/muni.	Province FE
Δ Private firms	8.247*** (2.331)	4.949*** (1.508)	13.395*** (3.669)	8.675*** (2.402)	8.675*** (2.402)	5.339** (2.507)	2.032** (0.894)	9.389*** (2.021)
CTA recruits	0.075*** (0.027)	0.048*** (0.017)	0.123*** (0.044)	0.077*** (0.028)	0.077*** (0.028)	0.068** (0.032)	0.051*** (0.016)	0.034 (0.025)
EIT/(EIT+VAT) share (pp)	0.059*** (0.017)	0.039*** (0.012)	0.099*** (0.029)	0.060*** (0.018)	0.060*** (0.018)	0.055** (0.024)	0.033* (0.018)	0.082*** (0.018)
Estimator	2SLS	2SLS	OLS	2SLS	2SLS	2SLS	2SLS	2SLS
Region FE	Y	Y	Y	Y	Y	Y	Y	N
Province FE	N	N	N	N	N	N	N	Y
Controls	Y	Y	Y	Y	Y	Y	Y	Y
Sample	All	All	All	All	All	No coast	No capital/muni.	All

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Units: export shocks are percentage-point exposure measures; private firms and CTA recruitment are count outcomes; EIT/(EIT+VAT) is measured in percentage points. Transformations: columns 1, 2, 4, 5, 6, 7, and 8 instrument raw exposure with the corresponding tariff-predicted exposure; column 3 enters tariff-predicted exposure directly; column 2 uses the ready-data 2010 exposure variant; columns 4 and 5 use reconstructed industry shocks; column 5 excludes the highest-Rotemberg-weight industry from the shock. Controls: baseline 2000 census and industry controls; unemployment share; manufacturing employment share; share ages 20–34, male share; and the first three principal components of baseline industrial structure. FE: regional fixed effects in columns 1–7 and province fixed effects in column 8. SE: standard errors clustered by province. Sample: included city-level cross-section; column 6 excludes coastal province-level units; column 7 excludes provincial capitals and municipalities. Years: firm changes are measured from 2000 to 2007; fiscal outcomes are measured in 2007; controls are measured in 2000. Outcome scale: private firms and CTA recruitment are level outcomes; EIT/(EIT+VAT) is a percentage-point share outcome; no outcome is IHS-transformed or per-capita. Additional check for alternative exposure and sample specifications. Coastal province-level units are province codes 12, 13, 21, 31, 32, 33, 35, 37, 44, 45, and 46. Provincial capitals are identified by city-code suffix 01; municipalities are province codes 11, 12, 31, and 50.

H.3 Revenue Measurement and Fiscal-Composition Checks

Main Table 3 reports imputed central-share EIT and VAT and two tax-composition comparisons. Appendix Table A14 shows that business and construction taxes also grew with export exposure, as expected when exporting expands the broader local tax base. The institutional test therefore comes from tax composition rather than from an exclusive increase in one revenue category: EIT rises relative to VAT, whose collection assignment did not change in 2002. Appendix Table A15 reports logged EIT and VAT estimates under alternative outlier treatments. Appendix Table A16 shows that the EIT–VAT composition estimates are robust to baseline and pre-trend controls, winsorization, and log-odds and rank transformations. The statutory rules establish CTA administration and provide the central-share imputations. A broader EIT-plus-individual-income-tax bundle serves as an additional income-tax sensitivity check.

Table A14. Export Exposure and the Local-Tax Benchmark

	Business Tax + Construction Tax
Export Shock	0.005*** (0.002)
Effect across exposure IQR	+52.9%
Num. obs.	310
First-stage F	139.4
Region FE	Y
PCA industry controls	Y
Baseline census controls	Y

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. The table reports a city-level 2SLS regression of the logged 2007 local-tax benchmark on realized export exposure, instrumented by tariff-predicted exposure. The benchmark is the sum of fiscal-book business tax and construction tax, categories principally administered by the LTA during the sample period. The model includes regional fixed effects, the first three principal components of baseline industrial structure, and baseline census controls; standard errors are clustered by province. The interquartile-range row converts the logged effect to a percent change. City fiscal books identify tax categories rather than agency-coded receipts, so this is an assignment-based benchmark, not observed total LTA collections.

Table A15. City-Level Statutory-Share Results and Outlier Diagnostics

	Full log	Log p1/p99	Log p5/p95
Imputed central-share EIT	0.005** (0.002)	0.005** (0.002)	0.004* (0.002)
Imputed central-share VAT	0.002 (0.002)	0.002 (0.002)	0.001 (0.002)
EIT sample size	310	310	310
VAT sample size	310	310	310
Region FE	Y	Y	Y
Province-clustered SE	Y	Y	Y

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Each cell reports a separate city-level 2SLS estimate of realized export exposure, instrumented by tariff-predicted exposure. All models include regional fixed effects and baseline census and industrial controls, with standard errors clustered by province. Imputed central-share EIT equals 1.5 times fiscal-book EIT under the 60:40 sharing rule; imputed central-share VAT equals three times fiscal-book VAT under the 75:25 sharing rule. These are statutory-share imputations, not observed agency collections. The columns use logged amounts. Winsorization is applied once to raw 2007 amounts at the stated cutoffs.

Table A16. Export Exposure and EIT-VAT Tax Composition

	Log central EIT	EIT share of EIT + VAT	+ Baseline share	Winsorized share
<i>Panel A. Amount, share, and baseline specifications</i>				
Export Shock	0.005** (0.002)	0.059*** (0.017)	0.057*** (0.016)	0.060*** (0.017)
Effect across exposure IQR	+48.4%	+4.64 pp	+4.46 pp	+4.67 pp
Region FE	Y	Y	Y	Y
PCA industry controls	Y	Y	Y	Y
Baseline census controls	Y	Y	Y	Y
Num. obs.	310	310	309	310
First-stage <i>F</i>	139.4	139.4	138.9	139.4
<i>Panel B. Ratio and transformation specifications</i>				
Export Shock	0.003*** (0.001)	0.003*** (0.001)	0.002** (0.001)	0.003*** (0.001)
Effect across exposure IQR	+28.1%	+22.3%	+20.8%	+0.25 log-odds +0.39 SD
Region FE	Y	Y	Y	Y
PCA industry controls	Y	Y	Y	Y
Baseline census controls	Y	Y	Y	Y
Num. obs.	310	301	264	310
First-stage <i>F</i>	139.4	140.5	104.2	139.4

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Each cell reports a city-level 2SLS coefficient on realized export exposure, instrumented by tariff-predicted exposure. Standard errors are clustered by province. Baseline census controls are unemployment share, manufacturing employment share, the share ages 20–34, and the male share; industry controls are the first three principal components of baseline industrial structure. Panel A reports log imputed central-share EIT, the 2007 EIT share of EIT plus VAT, the same share controlling for its 2000 value, and the share winsorized at the 1st and 99th percentiles. Panel B reports the log EIT-to-VAT ratio, specifications that add its 2000 value and its 1997–2000 pre-trend, and log-odds and inverse-normal-rank transformations of the EIT share. The IQR row uses each column's estimation-sample exposure range; logged outcomes are converted to percent changes, share outcomes to percentage points, and the transformed-share outcomes to their indicated scales. Fiscal-book categories do not include intergovernmental transfers or identify the collecting agency. The 2002 reform brought EIT liabilities for most newly established firms under CTA administration and central revenue sharing; VAT was already centrally administered, and its assignment did not change in that reform.

H.4 Transformations, Per-Capita Scaling, and Sector Composition

I next examine whether the main results depend on outcome scale, skewed count changes, or a particular ownership-composition interpretation. In Table A17, I report log, IHS, and per-capita variants of the main firm and tax-bureaucracy outcomes. In Table A18, I enter PE, FIE, and SOE firm changes jointly to test whether the EIT association reflects private-firm formation rather than generic firm growth.

Table A17. Outcome Transformations and Per-Capita Scaling

	Firm Registration			Tax Bureaucracy
	Log PE	IHS PE	PE Per Cap.	Log CTA
Export Shock	0.007*** (0.002)	0.007*** (0.002)	0.008*** (0.003)	0.006** (0.002)
Region FE	Y	Y	Y	Y
Controls	Y	Y	Y	Y
SE	HC2	HC2	HC2	HC2
Num. obs.	310	310	310	312

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Units: firm-registration outcomes are count changes; tax-bureaucracy outcome is CTA recruit counts; per-capita private-firm formation is per 10,000 baseline residents. Transformations: log outcomes are $\log(y + 1)$ for nonnegative changes, with negative changes set to missing only for log diagnostics; IHS denotes inverse hyperbolic sine and preserves negative changes; per-capita private-firm formation divides by baseline population and multiplies by 10,000. Controls: baseline 2000 census and industry controls: unemployment share, manufacturing employment share, share ages 20–34, male share, and the first three principal components of baseline industrial structure. FE: regional fixed effects in every column. SE: HC2 heteroskedasticity-robust standard errors. Sample: included city-level cross-section with one observation per city. Years: firm changes are measured from 2000 to 2007; tax-bureaucracy outcome is measured in 2007; controls are measured in 2000. Outcome scale: columns 1 and 4 are log outcomes; column 2 is IHS; column 3 is per-capita. Additional check of outcome transformations and per-capita scaling.

Table A18. Firm-Sector Composition and EIT Revenue

	Log EIT
PE	0.468*** (0.074)
FIE	−2.422*** (0.782)
SOE	−0.263 (1.302)
Region FE	Y
Controls	Y
SE	HC2
Num. obs.	308

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Units: EIT uses source fiscal-statistics currency units before logging; PE, FIE, and SOE regressors are firm-count changes in thousands. Transformations: EIT is logged; PE, FIE, and SOE changes are divided by 1,000 and entered jointly. Controls: baseline 2000 census and industry controls: unemployment share, manufacturing employment share, share ages 20–34, male share, and the first three principal components of baseline industrial structure. FE: regional fixed effects in every column. SE: HC2 heteroskedasticity-robust standard errors. Sample: included city-level cross-section with one observation per city. Years: EIT is measured in 2007; firm changes are measured from 2000 to 2007; controls are measured in 2000. Outcome scale: EIT is a log-level outcome; no outcome is IHS-transformed or per-capita. Additional check of firm-sector composition and EIT revenue.

H.5 Placebo and Pre-Trend Tests

In Table A19, I use changes from 1997 to 2000 rather than baseline levels to test whether future predicted exposure is already associated with pre-treatment fiscal growth. Most placebo estimates are small and statistically insignificant, although the EIT placebo is positive at the 10% level. Table A20 reports the restructuring-side leads. The later SOE decline does not predict earlier grant growth at conventional significance levels ($p = 0.103$), and the social-spending lead is near zero.

Table A19. Pre-WTO Placebo Tests

	Pre-WTO Change, 1997–2000		
	Revenue	EIT	Transfers
Predicted Export Exposure	0.000 (0.001)	0.005* (0.003)	0.001 (0.001)
Region FE	Y	Y	Y
Controls	Y	Y	Y
SE	HC2	HC2	HC2
Num. obs.	305	264	305

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Units: revenue, EIT, and transfer variables use source fiscal-statistics currency units before logging. Transformations: outcomes are pre-WTO changes measured as $\log(y_{2000} + 1)$ minus $\log(y_{1997} + 1)$; the regressor is tariff-predicted export exposure. Controls: baseline 2000 census and industry controls: unemployment share, manufacturing employment share, share ages 20–34, male share, and the first three principal components of baseline industrial structure. FE: regional fixed effects in every column. SE: HC2 heteroskedasticity-robust standard errors. Sample: included city-level cross-section with one observation per city. Years: outcome changes are measured from 1997 to 2000; controls are measured in 2000. Outcome scale: all outcomes are log changes; no outcome is IHS-transformed or per-capita. Placebo check testing whether future predicted exposure is already associated with pre-treatment fiscal growth.

Table A20. Lead Placebo Tests for Compensation Timing

	Lead Placebos	
	Grants 97–00	Social Sec. 98–00
SOE employment-share decline, 2000–2007	0.113 (0.067)	0.012 (0.314)
Region FE	Y	Y
Controls	Y	Y
Cluster	Province	Province
Outcome years	1997–2000	1998–2000
Timing	Lead placebo	Lead placebo
Num. obs.	328	330

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Units: reported intergovernmental grant receipts and social-security outcomes use source fiscal-statistics currency units; SOE employment decline is a share change. Transformations: outcomes are log changes, computed as $\log(y_{end} + 1)$ minus $\log(y_{start} + 1)$; SOE employment-share decline is SOE employment share in 2000 minus SOE employment share in 2007, so positive values indicate larger declines. Controls: baseline 2000 manufacturing population share, ethnic-minority share, GDP per capita, working-age share, and log out-of-province migrants. FE: regional fixed effects in every column. SE: standard errors clustered by province. Sample: city-level cross-section with available SOE employment and fiscal outcomes. Years: SOE employment decline is measured from 2000 to 2007; lead-placebo outcomes are changes from 1997 to 2000 for reported grant receipts and 1998 to 2000 for social-security spending. Outcome scale: all outcomes are log changes; no outcome is IHS-transformed or per-capita. The 1997 fiscal schedule groups grants more coarsely than the 2000 schedule. Because SOE employment is observed consistently only in 2000 and 2007, the timing columns test whether the later employment decline already predicts pre-2000 grant or social-spending growth rather than estimating a full annual event study.

CTA recruitment records are unavailable before 2002. I therefore present descriptive LTA staffing data for the 13 cities of Hubei Province in Figure A8, where pre-treatment city-level records are available. I find no visually common break around 2002, but the series concern a single province and a different tax administration. Because a comparable city-level regression series is unavailable, I use the figure as descriptive context rather than a formal pre-trend test.

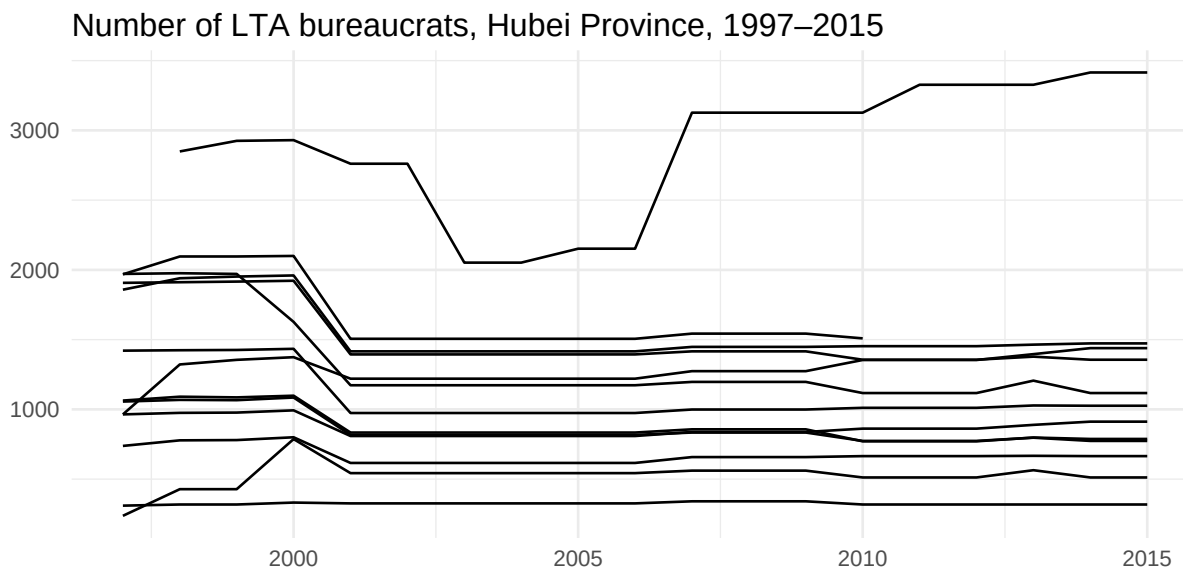


Figure A8. Descriptive LTA staffing series for Hubei cities

Note. This figure shows LTA staffing over time in Hubei Province. Each line represents a city in the province.

H.6 Compensation Spending and Fiscal-Support Checks

For the two-period fiscal-support checks, I cluster standard errors at the city level. The reported-grant measure harmonizes explicit tax-rebate, general-transfer, special-transfer, and other upper-government subsidy lines across the 2000 and 2007 fiscal schedules. It excludes prior-year balances, inter-fund transfers, other balancing receipts, and the 2007 line combining national-debt on-lending with carryover items. The schedules disaggregate grants differently, so I harmonize the explicitly labeled receipt lines and aggregate city-and-county accounts consistently across years.

Table A21. Reported Grant Receipts and Worker-Linked Outcomes without Controls

	Reported Grant Receipts			Worker-Linked Outcomes	
	Total	Per Capita	Per SOE	Soc. Sec.	Recorded Retirees
SOE employment share (remaining)	−0.413*** (0.077)	−0.501*** (0.103)	−3.564*** (0.444)	−3.300*** (0.481)	−0.101* (0.058)
City FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Controls	N	N	N	N	N
Cluster	City	City	City	City	City
Num. obs.	666	664	666	666	666

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Units: reported intergovernmental grant receipts and social-security spending use source fiscal-statistics currency units; recorded retirees are counts. Transformations: reported grant receipts are $\log(y + 1)$; grant per-capita outcomes subtract $\log$ total population; per-SOE outcomes subtract $\log(\text{SOE employment} + 1)$; social-security and retiree logs are winsorized. Controls: none. FE: city and year fixed effects in every column. SE: standard errors clustered by city. Sample: two-period city panel. Years: panel years are 2000 and 2007. Outcome scale: columns 1 and 5 are logged outcomes; column 2 is a total-population per-capita log; columns 3 and 4 are per-SOE logs; no outcome is IHS-transformed. Reported grant receipts sum fiscal-account lines explicitly labeled as tax rebates, subsidies, or transfers and exclude carryovers, inter-fund transfers-in, and residual financing. The schedules change their component disaggregation across years, and city-and-county totals may include within-prefecture transfers. All columns omit time-varying controls.

The resulting outcome captures the intergovernmental resources reported as received by each local government; it does not assign every receipt to a unique upper-government tier after consolidation.

Table A21 reports no-control estimates for total, population-normalized, and SOE-worker-normalized grants alongside social-security spending per remaining SOE worker and recorded retirees. The per-SOE measures scale fiscal resources by the remaining SOE workforce and therefore capture the intensity of resources relative to restructuring burdens. Table A22 shows that utility spending, other general spending, and public employment remain unrelated to SOE decline. Together, the total-grant, fiscal-dependence, per-SOE, and retiree results locate the response in intergovernmental support and worker-linked obligations rather than in general budget expansion.

Table A22. SOE Employment Share and Other Fiscal Outcomes

	Other Fiscal Outcomes		
	Utility	Other Spend	Public Emp.
SOE employment share (remaining)	−0.190 (0.118)	0.144 (0.204)	0.034 (0.034)
City FE	Y	Y	Y
Year FE	Y	Y	Y
Controls	Y	Y	Y
Num. obs.	664	662	664

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Units: spending outcomes use source fiscal-statistics currency units; public employment is a worker count. Transformations: spending and public-employment outcomes are $\log(y + 1)$, with the public-employment log winsorized. Controls: manufacturing population share, ethnic-minority share, GDP per capita, working-age share, and log out-of-province migrants. FE: city and year fixed effects in every column. SE: standard errors clustered by city. Sample: two-period city panel. Years: panel years are 2000 and 2007. Outcome scale: all outcomes are logged levels; no outcome is IHS-transformed or per-capita. The outcomes capture less-direct general public-sector responses. The regressor is SOE employment share (remaining), so negative coefficients indicate larger outcomes in cities with larger SOE employment-share declines.

H.7 Sensitivity, Leverage, and Staffing Composition Checks

In Table A23, I report additional sensitivity checks for private-firm formation and CTA recruitment. I winsorize the dependent variable and the exposure/instrument pair, exclude provincial capitals and municipalities, and drop observations flagged as high leverage in the corresponding covariate-adjusted diagnostic regressions. I use these checks to assess whether a small number of extreme cities or administrative centers drive the estimates.

Table A23. Outlier Sensitivity and Leverage Checks

	Main 2SLS	Winsor Y	Winsor X/Z	Winsor all	No capital/muni.	Drop leverage
Δ Private firms	8.247*** (2.331)	8.129*** (2.221)	8.122*** (2.251)	8.004*** (2.141)	2.032** (0.894)	6.126*** (1.913)
CTA recruits	0.075*** (0.027)	0.056*** (0.018)	0.074*** (0.026)	0.056*** (0.018)	0.051*** (0.016)	0.061** (0.024)
Estimator	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Region FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Winsor Y	N	Y	N	Y	N	N
Winsor X/Z	N	N	Y	Y	N	N
Sample	All	All	All	All	No capital/muni.	No high leverage

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Units: export shocks are percentage-point exposure measures; private firms and CTA recruits are count outcomes. Transformations: all columns estimate the same 2SLS specification as the main headline models; columns 2 and 4 winsorize the dependent variable at the 1st and 99th percentiles; columns 3 and 4 winsorize the raw exposure and tariff-predicted instrument at the 1st and 99th percentiles within the estimation sample. Controls: baseline 2000 census and industry controls: unemployment share, manufacturing employment share, share ages 20-34, male share, and the first three principal components of baseline industrial structure. FE: regional fixed effects in every column. SE: standard errors clustered by province. Sample: included city-level cross-section; column 5 excludes provincial capitals and municipalities; column 6 excludes high-leverage observations. Years: firm changes are measured from 2000 to 2007; CTA recruitment is measured in 2007; controls are measured in 2000. Outcome scale: private firms and CTA recruits are level outcomes; no outcome is logged, IHS-transformed, or per-capita. Additional check for outlier sensitivity. High-leverage observations are identified separately for each outcome using hat values from the corresponding OLS design with export exposure, the full control set, and region indicators; the cutoff is 2p/n. Provincial capitals are identified by city-code suffix 01; municipalities are province codes 11, 12, 31, and 50.

One potential concern is that the observed recruitment patterns may simply reflect replacement hiring driven by retirements. To address this concern, I use provincial-level data on the composition of CTA bureaucrats and examine whether the share of specific age and education groups can predict future recruitment. Table A24 presents the coefficients from regressions that estimate the relationship between future hiring and the share of bureaucrats in various age and education cohorts.

If recruitment primarily replaces retiring tax bureaucrats, I would expect a strong positive correlation between the share of senior-age bureaucrats and subsequent recruitment. Similarly, I would expect a lower share of college-educated staff—more common in older cohorts—to predict higher future hiring if the government seeks to replace retiring tax bureaucrats with younger, more educated recruits. I do not find consistent evidence for this substitution-based explanation, suggesting that the need to replace retiring tax bureaucrats does not solely drive CTA expansion.

Table A24. Bureaucrat Traits in 2000 and Future Recruits

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Below 25 Share	714.858 (1724.461)						
25–35 Share		–181.859 (1421.602)					
35–45 Share			–1934.295* (1118.151)				
45–54 Share				615.084 (1435.340)			
Above 59 Share					2225.701 (2487.137)		
Mean Age						–15.466 (44.731)	
College Share							480.015 (1353.528)
Num. obs.	30	30	30	30	30	30	30

*** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Standard errors are robust.