The Unintended Consequences of Removing Trade Distortions: Social Conflict in Qing China

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Abstract. Policy reforms can redistribute economic opportunities, undermining livelihoods in some places while creating them in others. I ask whether such losses and gains affect rebellion. For nearly a century before 1842, Qing China confined Western trade to Canton, so export tea and silk traveled to that port along inland routes that employed many porters and boatmen. The 1842 Treaty of Nanjing opened new northern ports, and the traffic moved to shorter routes. Using a newly constructed county-year panel of rebellion events from Qing imperial archives, covering 427 counties between 1820 and 1860, I find that the annual probability of rebellion increased by 9.6 percentage points in counties along the old routes relative to other counties within the same provinces, equivalent to approximately 68 percent of the sample mean. In contrast, counties along the newly favored routes experienced a relative decline of 5.4 percentage points. Counties on the old routes were also more vulnerable to the Taiping Rebellion (1851 to 1864), one of the deadliest civil wars in history. Reforms that improve the allocation of economic activity can thus have unequal consequences for local political stability.

1 Introduction

Government policies can create enduring economic advantages for particular places, sectors, or occupations. They often do so by confining trade to designated ports, protecting favored industries, or directing subsidies to selected regions (Kline and Moretti, 2014a; Juhász, Lane, and Rodrik, 2024). Over time, such policies generate rents for some groups and sustain workers, traders, and local businesses whose income exists only because of the resulting allocation of activity. When these policies are removed, economic activity shifts

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toward lower-cost alternatives and aggregate welfare may rise, but the consequences are unevenly distributed. Communities whose livelihoods depended on the old allocation become poorer, often suddenly, while others gain new opportunities from the same reform. Such reforms therefore raise a fundamental political economy question: does the disappearance of policy-created economic advantages increase social conflict, and do communities that gain from the same reform experience the opposite response?

The answer is theoretically ambiguous. A loss of employment or income lowers the opportunity cost of participating in violence by reducing the returns to peaceful activity (Becker, 1968; Chassang and Padró i Miquel, 2009). Abrupt reversals in economic prospects may also generate grievances, as emphasized by the literature on relative deprivation and revolution. Davies (1962) argues that revolutions are most likely when a long period of improvement is followed by a sharp reversal, and Gurr (1970) makes perceived deprivation the central cause of collective violence. Both views predict that conflict rises where livelihoods are lost and falls where new opportunities appear. The rapacity view predicts the opposite. If conflict is fought over valuable and contestable resources, the disappearance of rents lowers the returns to appropriation, while the creation of new rents raises them (Grossman, 1991). Neither the loss of existing advantages nor the creation of new ones therefore has an unambiguous implication for social stability, and which force dominates is an empirical question. Observing both sides of the same policy-induced reallocation is especially informative, because the competing views predict opposite responses on each side.

Establishing a causal relationship is difficult. Conflict can itself destroy employment and disrupt commerce, and economic decline often coincides with deteriorating institutions or other shocks that independently produce unrest. Policies that create or remove economic advantages are also rarely random, since they are often introduced or repealed in response to local economic and political conditions. In addition, most existing evidence comes from transitory price or weather shocks rather than from the permanent removal of an economic advantage. I address these problems by studying the end of the Canton System in nineteenth-century China, a policy change imposed from outside through military defeat rather than chosen by the Qing state in response to local conditions (Jia, 2014). Because the change moved trade from one set of routes to another rather than eliminating it, it created both losers and winners, which allows me to study the consequences of losing and gaining economic advantages under the same reform.

Between 1757 and 1842, under what became known as the Canton System, the Qing court confined all Western maritime trade to a single port, Guangzhou, known to Western merchants as Canton. China's main exports, tea from Anhui and Fujian and raw silk from the Jiangnan delta, were produced far to the north, so they had to be carried south to

Canton along a network of rivers and mountain passes. For Anhui tea, the journey took about two months (Fortune, 1999). This traffic supported porters, boatmen, warehouses, inns, and other businesses along the way, and historians estimate that at least one million porters and boatmen made their living from it (Wu, 2018). The restriction thus created a geographically concentrated demand for transport services that existed only because of the policy.

The Treaty of Nanjing, signed in August 1842 at the end of the First Opium War, opened four additional ports (Xiamen, Fuzhou, Ningbo, and Shanghai) and abolished the merchant monopoly at Canton. Shanghai lay much closer to the producing regions, and Anhui tea could now reach a port in about twenty days. Canton's share of China's tea and silk exports, close to one before 1842, collapsed within a decade, and Shanghai became the dominant export port by the early 1850s. The old routes were not physically damaged. Their economic value fell because the policy that had directed commerce through Canton no longer applied, and the income of the people who had carried that commerce fell with it. Counties on the routes serving Shanghai, by contrast, stood to gain.

The two-sided nature of this reallocation is central to my empirical design. I distinguish *negatively exposed* counties, which lay on the routes to Canton before 1842, from *positively exposed* counties, which lay on the routes from the tea districts to Shanghai after 1842. I do not compare these two groups with each other. Instead, I compare each with counties in the same provinces that lay off the corresponding routes. This allows me to study communities that lost an established advantage and communities that gained a new one under the same reform, and to distinguish a policy that moved economic activity across places from a shock that reduced it everywhere.

I construct a new county-year panel of rebellion for 427 counties in six provinces from 1820 to 1860. I extract rebellion events from two Qing imperial archives, the *Qing Shilu* (Veritable Records of the Qing Emperors) and the *Zhupi Zouzhe* (palace memorials with vermilion rescripts), by keyword search followed by a manual reading of each entry in its context. I classify each event by its target, distinguishing violence against local communities, merchants, or property from violence against the Qing state. Because the policy change was itself the outcome of a war, I exclude rebellion events that the archives link to the First Opium War (1839 to 1842), so that the outcome measures domestic unrest rather than wartime hostilities. Unrelated rebellions in the same counties and years are retained. I measure exposure by digitizing the historical transport routes and identifying the counties they passed through. In the four provinces crossed by the old routes (Anhui, Jiangxi, Fujian, and Guangdong), 60 of 277 counties are negatively exposed. In the two provinces of the Shanghai hinterland (Jiangsu and Zhejiang), 33 of 150 counties are positively exposed. For

each sample, I estimate a difference-in-differences model with province-by-year fixed effects, so that exposed counties are compared only with unexposed counties in the same province and year. This absorbs any shock common to a province, including its exposure to the war itself. Identification rests on the assumption that, absent the reform, exposed and unexposed counties in the same provinces would have followed parallel trends in rebellion.

The two sides of the reallocation moved in opposite directions. In my preferred specification, the annual probability of rebellion in negatively exposed counties rose by 9.6 percentage points after 1842 relative to unexposed counties in the same provinces, about 68 percent of the sample mean of 0.140. In positively exposed counties, it fell by 5.4 percentage points, about 43 percent of the sample mean of 0.127. Because each estimate is measured against its own comparison counties, the contrast is not a direct estimate of the difference between the two exposed groups. It shows instead that losses and gains from the same reform moved conflict in opposite directions. Event-study estimates show no differential trend in rebellion between negatively exposed counties and their comparison counties over the two decades before 1842, and the increase builds gradually after the treaty, in line with the gradual shift of exports from Canton to Shanghai. The increase includes both violence against local communities, such as robbery and attacks on merchants, and violence against the state, such as attacks on government offices and officials.

The consequences extended into the largest conflict of the period. The Taiping Rebellion began in Guangxi in 1851, moved north through the interior provinces, and established a rival state at Nanjing that the Qing took more than a decade to defeat. It is among the deadliest civil wars in history. I record the first year in which the archives report Taiping activity in each county and estimate a discrete-time hazard model. Negatively exposed counties faced a hazard of first Taiping activity about 40 percent higher than other counties in the same province, conditional on distance to the Taiping army's advance route, unrest before 1842, climate shocks, and the spread of the rebellion from neighboring counties. These estimates concern how the rebellion spread across counties rather than why it broke out, but they show that the places hurt by the end of the Canton System were also more exposed to the political crisis that followed.

A central concern is that the Treaty of Nanjing followed a war and may have changed local political and military conditions through channels unrelated to the reallocation of trade. The exclusion of war-related events and the province-by-year fixed effects address the most direct of these channels. Beyond them, counties on the imperial courier road network, which mattered to the state but not to the export trade and overlapped heavily with the trade routes, show no change in rebellion after 1842. The results are not explained by competition from imported textiles, which reached the interior mainly after 1860 and is unrelated to route

exposure. They also hold when rebellion is measured relative to county population, when the sample ends in 1850 before the Taiping Rebellion, under propensity-score matching, changes-in-changes, and synthetic difference-in-differences estimators, and with standard errors that allow for spatial correlation. Finally, a uniform political or military response to the treaty would not raise conflict on one side of the reallocation and lower it on the other.

The most plausible explanation is the loss of livelihoods. The opposite signs in the two samples are what the opportunity cost and relative deprivation views predict, and the reverse of what the rapacity view predicts. Within the old routes, the increase is larger in transit counties, where porters and boatmen were concentrated, than in counties that grew tea or silk, although the difference is only marginally significant. Rebellion also rose in nearby counties off the routes, most strongly within 25 kilometers, and the effect fades beyond 100 kilometers, consistent with displaced workers moving locally. After 1842, negatively exposed counties also recorded about 2.4 times as many new branches of the Heaven and Earth Society (*Tiandihui*), the secret society that historians describe as recruiting among unemployed transport workers (Wakeman, 1966; Qin and Meng, 2008). The archives do not provide county-level measures of transport employment or wages, however, so I interpret this evidence on labor displacement as suggestive rather than as a directly estimated first stage.

This paper contributes to three literatures. The first studies whether and why economic losses lead to conflict. Economists have examined how income, commodity prices, and weather shocks affect the incentives for violence, with findings that depend on whether a shock changes the returns to labor or the value of resources that can be appropriated (Miguel, Satyanath, and Sergenti, 2004; Dube and Vargas, 2013; Bazzi and Blattman, 2014). More recent work shows that job losses caused by trade shocks raise crime and violence (Dix-Carneiro, Soares, and Ulyssea, 2018; Dell, Feigenberg, and Teshima, 2019). I add to this literature in two ways. First, most of the shocks it studies are transitory, whereas the end of the Canton System permanently removed a source of income. Second, because the same reform created gainers as well as losers, I can test the rapacity view directly: conflict fell where new opportunities appeared, the opposite of what rapacity predicts. The setting also speaks to the political science literature on relative deprivation (Davies, 1962; Gurr, 1970), which was influential for decades but has been contested, notably by evidence that hardship did not predict collective violence in France (Snyder and Tilly, 1972). The end of the Canton System resembles the reversal that Davies (1962) describes, since the Canton trade had grown for decades before a single treaty moved it elsewhere. The historical records do not measure expectations or grievances, so I cannot separate relative deprivation from opportunity cost, but the results are consistent with both and inconsistent with rapacity.

The second literature studies who bears the costs when policy-created advantages are

removed. Work on trade liberalization and import competition shows that these costs fall on specific regions (Topalova, 2010; Autor, Dorn, and Hanson, 2013), can keep growing for two decades after a reform (Dix-Carneiro and Kovak, 2017), and can reshape political behavior (Autor et al., 2020). Work on industrial and place-based policy raises the same question from the other side: what happens to favored places once support is withdrawn. Agricultural gains from the Tennessee Valley Authority were reversed when its subsidies ended, while its manufacturing gains persisted (Kline and Moretti, 2014b), and regions temporarily protected from British trade during the Napoleonic Wars kept their lead in mechanized cotton spinning (Juhász, 2018). This literature mostly measures the economic costs of adjustment. I show that the costs can also take the form of political violence: removing a long-standing privilege raised organized violence against the state in the places that lost it and fed into civil war. The local political consequences of a reform therefore cannot be inferred from its aggregate economic rationale alone.

Two papers are closest to mine. Cao and Chen (2022) show that the abandonment of the Grand Canal, beginning in 1826, raised rebellion in the counties along it. Like them, I use variation in exposure to an established Chinese transport network and archival records of conflict, but my setting differs in three ways. First, the Grand Canal lost traffic as the state stopped using and maintaining it and the waterway lost navigability, whereas the Canton routes remained intact and lost traffic only because a policy privilege ended. Second, the end of the Canton System was a sharp change concentrated in a single year rather than a decline spread over several decades. Third, because the reform moved traffic rather than destroying it, I observe counties that gained as well as counties that lost, and the two moved in opposite directions. Yan and Zhu (2024) study the same rerouting of the tea trade in Fujian, Jiangxi, and Guangdong and find that transit counties experienced more ordinary social conflict, such as theft and robbery, after 1842. Their analysis deliberately excludes rebellions and other threats to the state. I instead study rebellion, including violence directed against the state, add the counties that gained from the reallocation, and trace the consequences into the Taiping Rebellion.

Finally, this paper contributes to the history of the Opium War and the origins of the Taiping Rebellion. Historians have long suspected a link between the two. Wakeman (1966) describes how porters and boatmen thrown out of work by the decline of the Canton trade swelled the secret societies of Guangdong after the war. Yet historians have also emphasized other sources of the mid-century crisis, including growing population pressure on land (Ho, 1959), the social disorder that followed the outflow of silver in the early nineteenth century (Lin, 2006), and the religious vision of the movement's founder, Hong Xiuquan (Spence, 1996). Evidence on the link between the postwar reallocation of trade and the Taiping Re-

bellion, by contrast, has been largely qualitative. I provide systematic quantitative evidence on this link. The unrest that followed the end of the Canton System was an unintended consequence of the settlement that ended the First Opium War: it raised rebellion along the old routes and made these counties more vulnerable to the Taiping Rebellion as it spread north. My results do not explain why the rebellion broke out in Guangxi, but they help explain where it found support. More broadly, they complement work on climate shocks, cultural norms, and political institutions as sources of unrest in late imperial China (Bai and Kung, 2011; Kung and Ma, 2014; Bai and Jia, 2016), and suggest that China's nineteenth-century crisis was shaped in part by the way the country was opened to the outside world.

The remainder of the paper proceeds as follows. Section 2 describes the Canton System and the reallocation of commerce after 1842. Section 3 describes the data and the construction of the exposure measures. Section 4 presents the empirical strategy and main results, including the estimates for both samples and the analysis of Taiping involvement. Section 5 reports robustness checks. Section 6 examines mechanisms, and Section 7 concludes.

2 Historical Background

2.1 The Canton System before 1842

From 1757 until the signing of the Treaty of Nanjing in 1842, the Qing Court confined all Western maritime trade to a single port: Guangzhou, known to Western merchants as Canton. Under the Canton System, foreign merchants could live and trade only in a designated area outside Guangzhou, and all trade had to go through a licensed group of Chinese merchant houses known as the Thirteen Hongs (Jia, 2014). This institutional arrangement concentrated the entire flow of China's overseas trade, dominated by tea and silk, through a single geographic node for nearly a century.

Because Guangzhou lies at the southern periphery of the country, far from the major centers of commodity production, export goods had to travel considerable distances before reaching the coast. Anhui province was the primary source of green tea, the Wuyi Mountains of Fujian supplied the black teas most prized by European buyers, and the Jiangnan delta, spanning parts of Jiangsu and Zhejiang, specialized in raw silk (Zhang, 2001; Yao, 1962). These commodities were transported southward through a network of mountain paths and navigable waterways that linked the interior provinces to the Pearl River delta. The operation of these routes depended almost entirely on manual labor. Porters carried goods over mountain passes where terrain precluded river transport, while boatmen navigated the river systems connecting the interior to Guangzhou. Historians estimate that at least one million

people earned their livelihoods from trade-related transport employment along these routes (Wu, 2018). To capture exposure to this system, I digitize the pre-1842 inland routes to Canton from historical sources (Zhang, 2001; Yao, 1962; Fortune, 1999) and use them to identify counties whose local economies were tied to the Canton trade.

Figure 1 maps the pre-1842 inland trade routes reconstructed from historical sources. These routes form the geographic basis for the treatment classification used in the empirical analysis below.

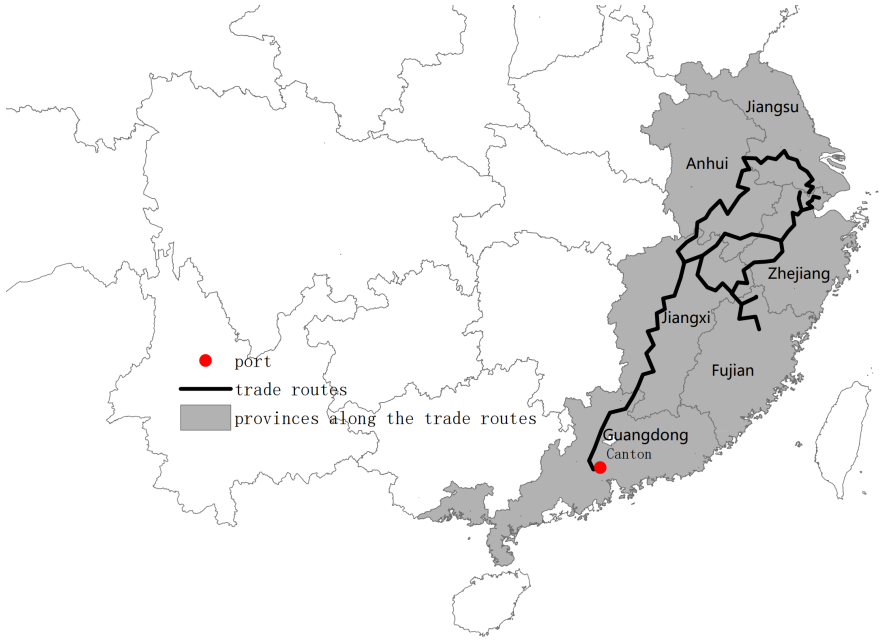


Figure 1: Pre-1842 Inland Trade Routes to Canton and Treaty Ports

Notes: The map shows the historical inland trade routes serving the Canton System across the six provinces in the sample (Jiangsu, Zhejiang, Anhui, Jiangxi, Fujian, and Guangdong), traced as thick black polylines. Red markers indicate the only port Guangzhou (Canton). Routes are digitized from Zhang (2001), Yao (1962), and Fortune (1999).

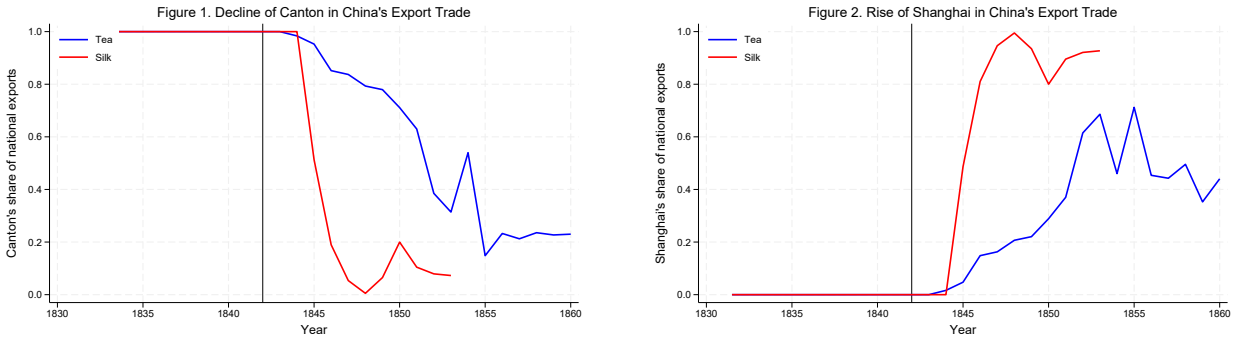
2.2 Opening of the Treaty Ports in 1842 and Trade Reallocation

China's defeat in the First Opium War (1839–1842) brought the Canton System to an abrupt end. The Treaty of Nanjing, signed in August 1842, compelled the Qing court to open four additional coastal cities to foreign trade: Xiamen, Fuzhou, Ningbo, and Shanghai. The monopoly of the Thirteen Hongs was simultaneously abolished, granting foreign merchants the right to trade directly at any of the five treaty ports. These institutional changes eliminated the geographic privilege that had sustained Canton's century-long position as China's exclusive gateway to Western commerce.

Trade reallocation away from Canton and toward the new treaty ports was rapid, driven

primarily by transport costs. Prior to 1842, shipping Anhui green tea to foreign buyers required a journey of approximately two months via the inland routes to Canton; after the opening of Shanghai, the same goods could reach the coast in just over twenty days (Fortune, 1999). Given that transport costs had accounted for as much as one-third of total costs for tea, the cost advantage of the northern coastal ports was decisive.

As Figure 2 illustrates, the consequence was a swift and sustained collapse of Canton's share of China's total export trade.[1] Canton's share, which had stood near unity before 1842, fell sharply in the years immediately following the treaty, while Shanghai emerged within a decade as the dominant center of China's overseas commerce, handling the majority of both tea and silk exports by the early 1850s.



(a) Canton's share of national exports

(b) Shanghai's share of national exports

Figure 2: Decline of Canton and Rise of Shanghai in China's Export Trade, 1830–1860

Notes: Each panel plots the share of China's national exports handled by the indicated port for tea (red) and silk (blue), the two principal commodities of the Canton trade. The vertical dashed line marks 1842, the year of the Treaty of Nanjing. Panel (a) shows the rapid decline in Canton's export share following the treaty. Panel (b) shows the corresponding rise of Shanghai as the dominant center of China's overseas trade by the early 1850s. Source: Yao (1962).

For the workers whose livelihoods depended on the old inland routes, this trade reallocation represented a sudden and severe contraction in demand for their services. As commerce shifted toward coastal shipping, the need for overland and riverine transport labor collapsed. Wakeman (1966) observes that the resulting pool of displaced porters and boatmen became ready recruits for secret societies and armed resistance. The remainder of this paper asks whether this disruption translated into a measurable increase in local social conflict, using county-level variation in exposure to the collapsed Canton trade network as the basis for identification.

[1]The analysis focuses on tea and silk, as these two commodities accounted for the bulk of China's total export value during the mid-nineteenth century.

3 Data

3.1 Rebellions

The main outcome is a binary indicator equal to one if at least one rebellion event occurred in county c in year t. I construct a novel county-year panel covering the period 1820 to 1860 by extracting rebellion events from two official Qing imperial sources: the *Qing Shilu* (Veritable Records of the Qing Emperors) and the *Zhupi Zouzhe* (palace memorials with vermilion rescripts) from the First Historical Archives of China. For the years 1820 to 1829, rebellion records are drawn exclusively from the *Qing Shilu*; from 1830 onward, the panel combines the *Qing Shilu* with the *Zhupi Zouzhe*. These records constitute the official archive of imperial edicts and memorials reporting events of national significance, and are digitized on the Chinese Text Project and Shidian Guji platforms. Chinese historians regard the Qing Shilu as a uniquely authoritative source that offers the most reliable and comprehensive record of social unrest during the Qing Dynasty Yang (1975). To identify rebellion-related entries, I perform a keyword search using a set of terms common in Qing administrative language for civil unrest: *fei* (bandits), with frequent compounds including *tu fei* (local brigands), *jiao fei* (bandits linked to religious organizations), *yue fei* (Guangdong bandits), and *hui fei* (secret-society bandits); *zi shi* (to cause disturbances); *ju zhong* (to gather a mob); and *ni* (insurgent). All candidate entries identified by the keyword search are then manually reviewed in their original textual context to verify relevance.

I further classify each rebellion event by its primary target, following the distinction between anti-social and anti-state violence. *Anti-social violence* refers to collective violence directed primarily against local communities, merchants, property, or commercial resources rather than state authority. Typical cases include robbery and looting targeting merchants or local households, clan feuds involving the destruction of property, and armed clashes over local economic resources. *Anti-state violence* refers to collective violence directed primarily against the apparatus of the Qing state. Typical cases include targeted attacks on *yamen* (government offices), government officials, or military outposts; jail-breaking and armed resistance to arrest; and sectarian or secret-society uprisings explicitly aimed at challenging state authority. This distinction allows me to examine whether the trade shock altered not only the frequency of conflict but also its political character over time.

3.2 Treatment

Binary treatment. The baseline treatment variable Treated_c is a binary indicator equal to one if county c lies on any of the pre-1842 inland trade routes to Canton. I identify

these routes by digitizing three historical sources: Zhang (2001), Yao (1962), and Fortune (1999). A county is classified as treated if its administrative boundary intersects one of these digitized route segments. Three classification rules are applied uniformly to both the negative and positive samples: a county is treated if it is the seat-sharing (*fuguo*) county of a treated administrative city; where the route runs along a boundary river, the counties on both banks are treated; and where the route reaches an offshore anchorage, the county containing the anchorage is treated. Under these rules the negative sample adds three counties (Hezhou, Nanling, and Qingyang, each on a boundary-river segment of the Yangzi bracketed by route counties: Hezhou between Dangtu and Fanchang, Nanling between Wuhu and Tongling, and Qingyang between Tongling and Guichi), and the positive sample adds four (Changzhou, Xinyang, Jiading, and Baoshan), raising the positively exposed treated count from 29 to 33. Treated assignments are validated against county-centroid distance to the digitized routes. In the positive sample, 28 of the 33 treated counties lie within 3.4 km of a digitized route; the remaining five (Yuanhe, Changzhou, Xinyang, Jiading, and Baoshan) are classified under the three rules above rather than by distance. In the negative sample, treated counties lie on average 7 km from a route, and Hezhou, Nanling, and Qingyang are classified under the boundary-river rule. Of the 277 baseline counties in the four provinces (Anhui, Jiangxi, Fujian, Guangdong) covered by the sample, 60 are classified as treated.

Transport and production exposure. The binary treatment pools two economically distinct forms of exposure to the Canton trade network. *Transport-exposed* counties are those whose primary role in the network was to facilitate the movement of goods, serving as transit hubs for porters, boatmen, and commercial intermediaries. *Production-exposed* counties are those whose economies centered on the cultivation or processing of the principal export commodities, chiefly tea and raw silk. Because the trade shock disrupted both the demand for transport labor and the market access of commodity producers, these two groups may have faced different magnitudes of economic disruption. I construct separate indicators for transport and production exposure to test for heterogeneity in the treatment effect.

Continuous exposure. The binary indicator records only whether a county lay on the Canton routes, not how severely trade through those routes contracted after 1842. To capture the magnitude of the disruption, I construct a continuous exposure measure, $\text{TransitLoss}_{c,t} = \text{Treated}_c \times (1 - s_{\text{Canton},t})$, where $s_{\text{Canton},t}$ is Canton's share of China's overseas exports in year t, taken from Yao (1962).[2] The measure is zero for all control counties and for treated

[2]Tea and silk were the two principal commodities of China's overseas trade in this period; $s_{\text{Canton},t}$ is the volume-weighted combination of Canton's tea and silk export shares.

counties before 1842, and rises continuously as Canton's export share is reallocated toward the new treaty ports. To distinguish between tea and silk, which followed different export routes and experienced declines at different times, I construct commodity specific flow loss measures, $\text{FLR}^k_{c,t} = \mathbf{1}\{k \text{ passes through the corridor containing county } c\} \times (1 - s^k_{\text{Canton},t})$ for $k \in \{\text{tea}, \text{silk}\}$. Each treated county is assigned to the commodity or commodities historically transported along its segment of the route network.

Economic intensity. To capture variation in the depth of market integration within the treated group, I construct two continuous intensity measures following Cao and Chen (2022): the pre-1842 density of market towns,

$$\text{TownDensity}_{c,1820} = \frac{\text{MarketTowns}_{c,1820}}{\text{Area}_c} \times 10{,}000,$$

measured in towns per 10,000 km^2, and the share of a county's 1820 market towns located within ten kilometers of the Canton routes. Both measures equal zero for counties off the corridor, and the intensity variable enters the analysis interacted with treatment, $\text{Intensity}_c = \mathbf{1}\{\text{Treated}_c = 1\} \times \text{TownDensity}_{c,1820}$. These measures distinguish counties more or less deeply embedded in the Canton trade network, allowing a test of whether the treatment effect scales with the depth of commercial integration or is instead determined by corridor location alone.

3.3 Contorl Variables

The analysis controls for a rich set of pre-determined county characteristics that may independently predict both route placement and conflict propensity. *Geography.* I include each county's land area (in km^2) and a terrain ruggedness index (Nunn and Puga, 2012), both constructed from county-level shapefiles provided by the Harvard Yenching Institute and the Fudan Center for Historical Geography (Harvard Yenching Institute and Fudan Center, 2007). Rugged terrain raises the cost of state suppression and may independently foster conflict; it may also have shaped the placement of trade routes.

Demography. I account for historical population using county level estimates for 1820 from Zheng and Wu (2026). This source provides direct population estimates for counties in historical China and validates them against prefecture level figures reported in *The Population History of China* (Cao, 2001) and county household records compiled by Liang (1980).

Agriculture. I include indices of suitability for wetland rice and wheat cultivation from IIASA and FAO (2012) as proxies for each county's agricultural productivity and degree

of commercial orientation. I also control for crop failure using a county year indicator constructed from the *Qing Shilu* through the same keyword search and manual verification procedure described above.

Climate. To capture annual fluctuations in climatic conditions, I include a county level temperature anomaly based on the historical reconstructions of Mann et al. (2009a;b). I also control for yearly flood and drought events using indicators derived from the gridded hydroclimate dataset of Hao et al. (2023).

Table 1 presents the means and standard deviations for treated and control counties, together with the difference in means and its standard error. The two groups are broadly similar in their geographic characteristics, particularly in terms of land area and terrain ruggedness, and they exhibit almost identical average temperature anomalies. Treated counties nevertheless experience floods, droughts, and crop failures somewhat more frequently. This pattern may reflect the location of the Canton trade routes along river valleys that were vulnerable to seasonal flooding. Treated counties also tend to have larger populations and greater commercial activity, as indicated by their higher density of market towns. These differences observed before treatment motivate the use of province by year fixed effects in the preferred specifications to account for time varying shocks at the provincial level, together with the full set of time varying controls described above.

3.4 Suggestive Evidence

Before turning to the formal empirical strategy, Figure 3 offers a descriptive overview of the joint geography of the trade shock and the conflict outcome. The map overlays the pre-1842 inland routes to Canton with the spatial distribution of changes in the annual rebellion rate across counties. For each county, this change is defined as the average number of rebellions per year in the post-treaty period (1843 to 1860) minus the average number per year in the pre-treaty period (1830 to 1841), with 1842 excluded as the year of the shock. Expressing the change as a per-year rate makes the two periods comparable despite their different lengths. The counties with the largest increases in the annual rebellion rate, shown in darker shades of gray, are disproportionately concentrated along or near the historical Canton routes rather than distributed uniformly across the four provinces of Anhui, Jiangxi, Fujian, and Guangdong. These patterns motivate the difference-in-differences design, which exploits cross-county variation in pre-1842 route exposure to identify the causal effect of trade-access loss on local unrest.

Table 1: Comparisons between treated and control counties

	Treated (1)	Control (2)	Difference (1)−(2)
County area (km^2)	2117.87	2243.29	−125.42
	(1036.96)	(1061.75)	[154.10]
Ruggedness index	141.691	168.741	−27.050
	(93.279)	(95.692)	[13.883]
Climate anomaly	0.383	0.359	0.024
	(0.049)	(0.052)	[0.008]
Flood	0.463	0.458	0.005
	(0.047)	(0.082)	[0.011]
Drought	0.193	0.225	−0.032
	(0.059)	(0.081)	[0.011]
Population in 1820	489,423	296,348	193,076
	(497,819)	(241,041)	[46,022]
Suitability for wheat	2364.378	2431.706	−67.327
	(884.216)	(1005.531)	[143.054]
Suitability for wetland rice	3098.984	2634.766	464.218
	(1109.992)	(1139.353)	[165.276]
Crop failure	0.169	0.079	0.089
	(0.181)	(0.141)	[0.022]
Market towns	3.983	3.982	0.002
	(2.460)	(2.848)	[0.404]
Counties	60	217	

Notes: The sample consists of the 277 counties in Anhui, Jiangxi, Fujian, and Guangdong, of which 60 lie on the pre-1842 inland routes to Canton. Columns (1) and (2) report means with standard deviations in parentheses. Time-varying variables (flood, drought, climate anomaly, crop failure) are county means over the sample period; climate anomaly and crop failure are coded from 1830 onward. Population, suitability indices, and market towns are measured in 1820. Column (3) reports the treated−control difference with its standard error in brackets.

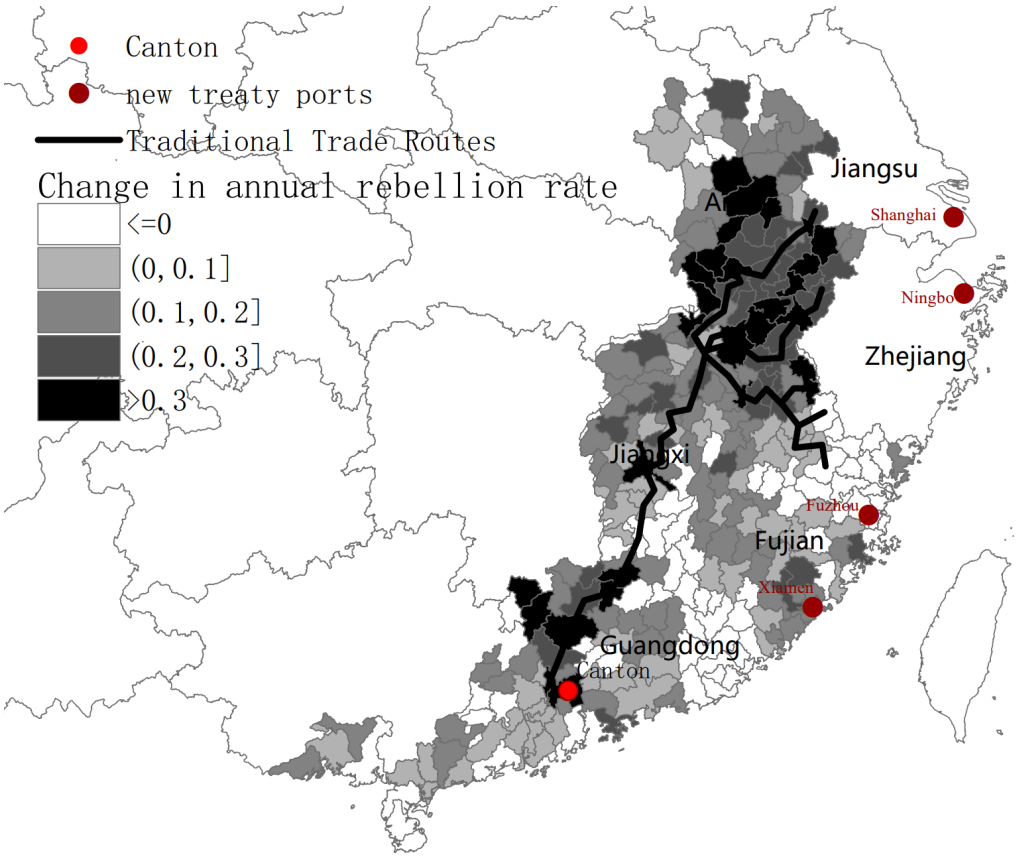


Figure 3: Spatial Distribution of Changes in the Annual Rebellion Rate around 1842

Notes: The map overlays the pre-1842 inland trade routes to Canton (black lines) and the treaty ports opened after the Treaty of Nanjing (red points), with Canton itself marked separately as the sole port open to foreign trade before 1842. Shaded counties show the change in the annual rebellion rate, defined as average rebellions per year in the post-treaty period (1843 to 1860) minus average rebellions per year in the pre-treaty period (1830 to 1841), with 1842 excluded. The change is measured in incidents per year and binned into five categories. Darker shades indicate larger increases in the annual rebellion rate. Counties with no increase or a decrease are shown in white.

4 Empirical strategy and Results

4.1 Empirical Strategy

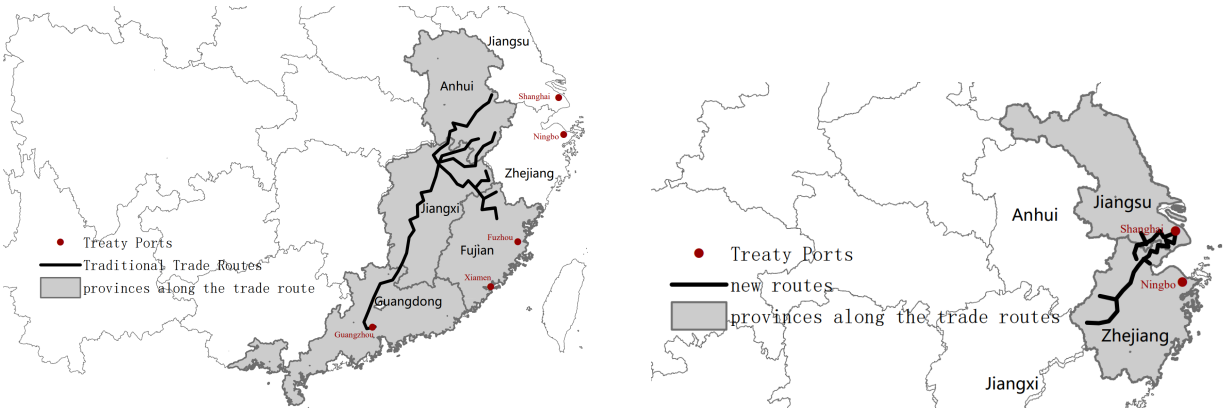
To identify the causal effect of trade-access loss on local social conflict, I exploit county-level variation in exposure to the pre-1842 inland Canton trade routes and estimate a two-way fixed effects difference-in-differences specification:

$$Y_{ct} = \beta\big(\text{Treated}_c \times \text{Post}_t\big) + \lambda_c + \delta_t + X_{ct}\Gamma + \varepsilon_{ct}, \tag{1}$$

where Y_{ct} is an indicator equal to one if at least one rebellion occurred in county c in year t; Treated_c equals one if county c lies on the pre-1842 inland routes to Canton; Post_t equals one for years 1842 and after; λ_c denotes county fixed effects; and δ_t denotes year fixed effects. The county fixed effects absorb all time-invariant differences across counties, including baseline conflict propensity and geography. The year fixed effects absorb common shocks to all counties in a given year. The vector X_{ct} includes the time-varying controls described in Section 3: crop failure, temperature anomaly, drought, and flood. Standard errors are clustered at the county level throughout.

The coefficient of interest, β, identifies the average change in rebellion incidence in treated counties relative to control counties following the 1842 trade shock, under the assumption that, absent the shock, treated and control counties would have followed parallel trends in conflict. The preferred specifications replace year fixed effects with province-by-year fixed effects ($\varphi_{p(c),t}$), which absorb any province-level time-varying confounders, such as differential responses to the First Opium War or province-specific administrative shocks, and thereby sharpen identification to within-province comparisons over time.

Figure 4 illustrates the two sides of the 1842 trade reallocation. The left panel maps the counties that were negatively exposed to the shock, namely those whose pre-1842 role in the Canton trade network was disrupted by the opening of the treaty ports. The right panel maps the counties that were positively exposed, namely those in the coastal hinterlands of the new treaty ports that stood to gain from the reallocation of overseas trade. Comparing conflict dynamics across these two groups provides a test for whether the estimated effects reflect trade disruption specifically rather than a more general response to the 1842 institutional shock.



(a) Negatively exposed counties (b) Positively exposed counties

Figure 4: Negatively and Positively Exposed Counties

Notes: Panel (a) shows counties classified as negatively exposed to the 1842 trade shock, those lying on pre-1842 inland trade routes to Canton (shaded gray), alongside the traditional routes (black lines) and new treaty ports (red points). Panel (b) shows counties classified as positively exposed, those lying in the hinterlands of the new treaty ports opened after 1842.

To assess the validity of the parallel trends assumption and to characterize the dynamics of the treatment effect, I estimate the following event-study specification:

$$Y_{ct} = \sum_{k \neq 0} \beta_k \Big(\text{Treated}_c \times \mathbf{1}\{t \in \mathcal{B}_k\} \Big) + \lambda_c + \varphi_{p(c),t} + X_{ct}\Gamma + \varepsilon_{ct}, \tag{2}$$

where $\mathcal{B}_k$ denotes two-year time bins centered on the reference period 1841–1842, and the omitted category is the bin immediately preceding the treaty (1841–1842). The coefficients β_k for $k < 0$ test for pre-existing differential trends, while coefficients for $k > 0$ trace the post-shock trajectory of the treatment effect.

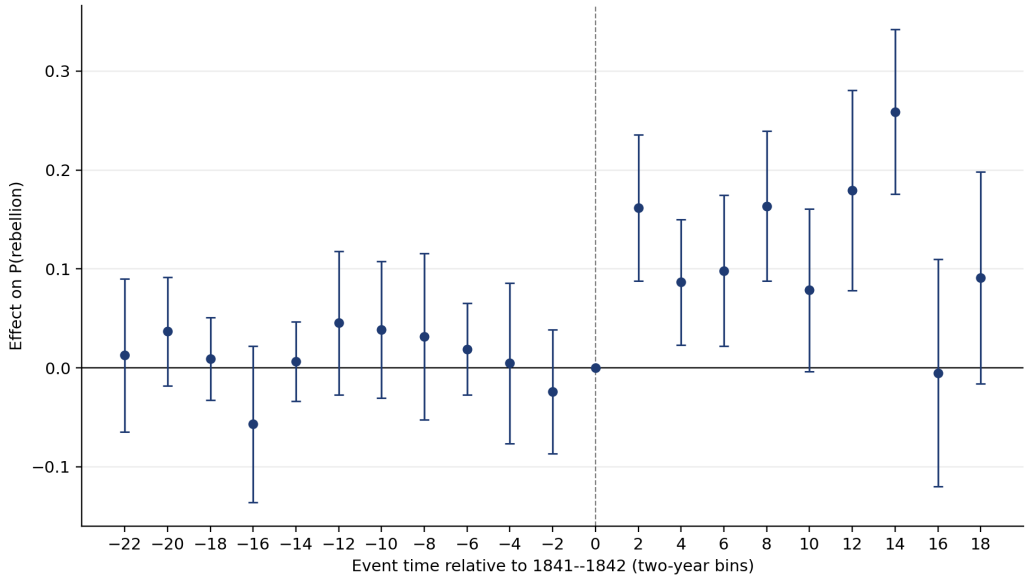


Figure 5: Event-Study Estimates: Trade Shock and Rebellions

Notes: The figure plots event-study coefficients $\hat{\beta}_k$ from equation (2), with 95 percent confidence intervals. The sample covers 277 counties in four provinces from 1820 to 1860. The omitted reference period is 1841 to 1842. Time bins are two years wide. Specifications include county fixed effects, province-by-year fixed effects, and the full set of time-varying controls. Standard errors are clustered at the county level.

The pre-treatment coefficients are small and statistically indistinguishable from zero, and an F-test cannot reject the null that they are jointly equal to zero, supporting the parallel trends assumption. After the treaty, the coefficients rise progressively, indicating that the effect of the trade shock emerged gradually rather than all at once, a pattern consistent with the slow adjustment of trade flows documented in Figure 2.

4.2 Baseline Estimates

Negative trade exposure. Table 2 reports estimates of equation (1) for the full sample of 277 counties between 1820 and 1860. Across all six specifications, the coefficient on *Treated* × *Post-1842* is positive, precisely estimated, and stable.

Column (1) includes county and year fixed effects without additional controls, yielding an estimate of 0.1280. Columns (2) through (6) replace year fixed effects with province-by-year fixed effects and sequentially add time-varying controls for crop failure, climate anomaly, drought, and flood. The estimate remains stable across these specifications. In the preferred specification (Column 6), which includes province-by-year fixed effects and the full set of controls, exposure to the Canton trade shock raised the annual probability of rebellion

by approximately 9.6 percentage points. Relative to the full-sample mean rebellion rate of 0.140, this represents an increase of about 68 percent. The stability of the coefficient across specifications suggests that the estimated effect is not driven by observable time-varying confounders.

Table 2: Trade Shock and Rebellions: Baseline Estimates

	Rebellion occurrence (0/1)					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post-1842	0.1280***	0.1033***	0.1034***	0.1040***	0.0943***	0.0956***
	(0.0178)	(0.0185)	(0.0185)	(0.0184)	(0.0184)	(0.0183)
	[0.0242]	[0.0218]	[0.0219]	[0.0219]	[0.0212]	[0.0215]
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	No	No	No	No	No
Prov.×Year FE	No	Yes	Yes	Yes	Yes	Yes
Climate controls	No	No	Yes	No	No	Yes
Geography controls	No	No	No	Yes	No	Yes
March-route control	No	No	No	No	Yes	Yes
Mean of dependent variable	0.1404	0.1404	0.1404	0.1404	0.1404	0.1404
Number of counties	277	277	277	277	277	277
Observations	11,357	11,357	11,357	11,357	11,357	11,357
Adjusted R^2	0.198	0.324	0.325	0.325	0.326	0.326

Notes: The dependent variable is an indicator equal to one if at least one rebellion occurred in county c in year t. Treated × Post-1842 equals one for counties on the pre-1842 inland routes to Canton in years ≥ 1842. All columns include county fixed effects. Climate controls comprise drought and flood indicators. The geography control is terrain ruggedness × Post-1842. The march-route control is the distance from each county centroid to the Taiping 1851–1853 march route, interacted with Post-1851. Standard errors clustered at the county level are in parentheses; Conley spatial-HAC standard errors with a 500 km cutoff and 10-year serial correlation are in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Positive trade exposure. I next estimate equation (1) for the counties positively exposed to the new treaty ports, those whose geographic position gave them potential access to the reallocated overseas trade. The results, reported in Table 3, show that the coefficient on Treated × Post-1842 is negative and statistically significant across specifications, ranging from about -0.049 to -0.055 and significant at the 5 percent level with county-clustered standard errors, against a pre-1842 mean of 0.098. Counties that gained access to the new treaty-port trade therefore experienced a relative decline in rebellion after 1842, the mirror image of the increase among counties that lost access to the Canton network. This asymmetry is inconsistent with a rapacity mechanism. Under rapacity, the rents created by trade access would raise conflict in the counties that gained access to the new treaty-port trade, yet these counties show a decrease, while the counties that lost access show a large increase. It is

instead consistent with an opportunity-cost interpretation, in which the relevant channel is the value of trade-dependent livelihoods: where access was gained conflict fell, and where access was lost conflict rose.

Table 3: Trade Shock and Rebellions: Positively Exposed Counties

	Rebellion occurrence (0/1)					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post-1842	-0.0536**	-0.0492**	-0.0494**	-0.0538**	-0.0500**	-0.0545**
	(0.0243)	(0.0240)	(0.0240)	(0.0267)	(0.0241)	(0.0268)
	[0.0329]	[0.0317]	[0.0320]	[0.0301]	[0.0309]	[0.0297]
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	No	No	No	No	No
Prov.×Year FE	No	Yes	Yes	Yes	Yes	Yes
Climate controls	No	No	Yes	No	No	Yes
Geography controls	No	No	No	Yes	No	Yes
March-route control	No	No	No	No	Yes	Yes
Mean of dependent variable	0.1268	0.1268	0.1268	0.1268	0.1268	0.1268
Number of counties	150	150	150	150	150	150
Observations	6,150	6,150	6,150	6,150	6,150	6,150

Notes: The sample consists of 150 counties in two provinces (Jiangsu and Zhejiang) from 1820 to 1860, of which 33 are treated. The dependent variable is an indicator equal to one if at least one rebellion occurred in county c in year t; the outcome is set to zero in Opium-War years (1841–1842), as in the baseline. Treated equals one for counties positively exposed through proximity to the post-1842 treaty ports; Post-1842 equals one in years 1842 and after. All columns include county fixed effects. Column (1) adds year fixed effects; Columns (2)–(6) replace year fixed effects with province × year fixed effects. Climate controls comprise drought and flood indicators. The geography control is terrain ruggedness interacted with Post-1842. The march-route control is the geodesic distance from each county centroid to the Taiping 1851–1853 advance route, interacted with Post-1851. Standard errors clustered by county in parentheses; Conley spatial-HAC standard errors (500 km, 10-year, Bartlett) in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Conflict by motive: anti-social and anti-state violence. To examine whether the trade shock raised conflict of a particular character or across the board, I re-estimate equation (1) separately for anti-social and anti-state violence, using as the outcome an indicator for each type of event.

Table 4 reports the results for anti-social violence. The coefficient on Treated × Post-1842 is approximately 0.037 and significant across specifications, against a sample mean of 0.047. Anti-social violence comprises economically motivated conflict, including robbery, looting, and local armed clashes, the forms of disorder most directly associated with displaced transport workers facing acute income loss.

Table 4: Trade Shock and Rebellions: Anti-Social Violence

	Economic-motive rebellions					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post-1842	0.0392***	0.0497***	0.0494***	0.0500***	0.0471***	0.0468***
	(0.0114)	(0.0112)	(0.0112)	(0.0113)	(0.0112)	(0.0112)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	No	No	No	No	No
Province × Year FE	No	Yes	Yes	Yes	Yes	Yes
Climate controls	No	No	Yes	No	No	Yes
Geography controls	No	No	No	Yes	No	Yes
March-route control	No	No	No	No	Yes	Yes
Mean of dependent variable	0.053	0.053	0.053	0.053	0.053	0.053
Number of counties	277	277	277	277	277	277
Observations	11357	11357	11357	11357	11357	11357
Adjusted R^2	0.075	0.135	0.135	0.135	0.135	0.136

Notes: The dependent variable is an indicator equal to one if at least one anti-social rebellion occurred in county c in year t. Anti-social violence is defined as collective violence directed primarily against local communities, merchants, or commercial property. All columns include county fixed effects. Column (1) includes year fixed effects; Columns (2)–(5) include province × year fixed effects. Standard errors, clustered at the county level, in parentheses.

Table 5 reports the corresponding results for anti-state violence, defined as collective violence directed against state authority. The coefficient on Treated × Post-1842 is approximately 0.041 and significant across specifications, against a sample mean of 0.112. That the effect appears for both anti-social and anti-state violence indicates that the trade shock raised conflict broadly rather than along a single dimension. The anti-state response is of particular interest because organized resistance against the state is the form of conflict most closely connected to the large-scale insurrection of the 1850s, a connection I examine directly in Section 4.4.

Table 5: Trade Shock and Rebellions: Anti-State Violence

	Political-motive rebellions					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post-1842	0.0884***	0.0555***	0.0557***	0.0557***	0.0506***	0.0520***
	(0.0134)	(0.0140)	(0.0139)	(0.0140)	(0.0139)	(0.0139)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	No	No	No	No	No
Province × Year FE	No	Yes	Yes	Yes	Yes	Yes
Climate controls	No	No	Yes	No	No	Yes
Geography controls	No	No	No	Yes	No	Yes
March-route control	No	No	No	No	Yes	Yes
Mean of dependent variable	0.092	0.092	0.092	0.092	0.092	0.092
Number of counties	277	277	277	277	277	277
Observations	11357	11357	11357	11357	11357	11357
Adjusted R^2	0.265	0.418	0.419	0.418	0.418	0.419

Notes: The dependent variable is an indicator equal to one if at least one anti-state rebellion occurred in county c in year t. Anti-state violence is defined as collective violence directed primarily against state authority, including attacks on government offices or officials, jail-breaking, and secret-society uprisings. All columns include county fixed effects. Column (1) includes year fixed effects; Columns (2)–(5) include province × year fixed effects. Standard errors, clustered at the county level, in parentheses.

4.3 Treatment Intensity

Transport versus production exposure. The binary treatment indicator conflates two economically distinct channels through which the Canton trade network shaped local economies: transport-dependent counties whose primary role was to facilitate the movement of goods along the routes, and production-dependent counties whose economies centered on the cultivation or processing of export commodities. To assess whether the treatment effect differs across these two groups, I estimate:

$$Y_{ct} = \beta_1\big(\text{TransportTreated}_c \times \text{Post}_t\big) + \beta_2\big(\text{ProductionTreated}_c \times \text{Post}_t\big) + \lambda_c + \varphi_{p(c),t} + X_{ct}\Gamma + \varepsilon_{ct}, \tag{3}$$

where $\text{TransportTreated}_c$ and $\text{ProductionTreated}_c$ are mutually exclusive indicators partitioning the treated group.

Table 6 reports the results. The coefficient on TransportTreated × Post-1842 is approximately 0.11 to 0.14 and significant at the 1 percent level across all specifications. The

coefficient on ProductionTreated × Post-1842 is smaller and loses significance once the geography control is added, falling to about 0.025 and statistically insignificant in the fully controlled specification. The equality of the two coefficients is rejected in the preferred column ($p = 0.002$). Two further patterns reinforce the transport interpretation. First, the production group fails the parallel-trends test, with a joint pre-1842 F of 4.23 ($p < 0.001$), and its post-treaty rise is concentrated after 1853, during the Taiping period, rather than in the immediate post-1842 years. Second, restricting the sample to the pre-Taiping window of 1820 to 1850 leaves only the transport effect significant (transport 0.114, $p < 0.001$; production 0.026, insignificant; equality $p = 0.016$). The results are best read as evidence that the post-1842 rise in rebellion loaded on transport-exposed counties.

Economic intensity. The binary treatment indicator masks variation in the depth of market integration within the set of treated counties. Following Cao and Chen (2022), I use two continuous measures of a county's integration into the Canton trade network: the pre-1842 density of market towns (towns per 10,000 km^2) and the share of a county's 1820 market towns located within ten kilometers of the Canton trade routes. Both measures equal zero for counties off the corridor. Adapting equations (4) and (5) of Cao and Chen (2022), I estimate a linear intensity specification,

$$Y_{ct} = \beta\big(\text{Intensity}_c \times \text{Post}_t\big) + \lambda_c + \varphi_{p(c),t} + X_{ct}\Gamma + \varepsilon_{ct}, \tag{4}$$

and a more flexible group specification that splits treated counties into intensity terciles,

$$Y_{ct} = \sum_{k=1}^{3} \beta_k\big(\mathbf{1}\{\text{IntensityGroup}_c = k\} \times \text{Post}_t\big) + \lambda_c + \varphi_{p(c),t} + X_{ct}\Gamma + \varepsilon_{ct}, \tag{5}$$

where terciles are defined among the 60 treated counties and control counties form the omitted group. If the trade shock operates with a dose response, the tercile coefficients should rise monotonically in k; if exposure is instead determined by corridor location itself, the three coefficients should be uniform.

Table 7 reports the estimates. The linear specifications in columns (1) and (2) yield positive and significant coefficients for both intensity measures, and the route share result holds under alternative bandwidths of 5 and 20 kilometers. The tercile specifications in columns (3) and (4) reveal that these linear estimates do not reflect a gradient within treated counties. All six tercile coefficients are positive, significant, and tightly clustered, with no monotone pattern: the lowest and highest density terciles yield nearly identical estimates, and equality of the first and third terciles cannot be rejected for either measure. The significant linear

Table 6: Trade Shock and Rebellions: Transport-Exposed versus Production-Exposed Counties

	Rebellion occurrence (0/1)					
	(1)	(2)	(3)	(4)	(5)	(6)
Transport × Post-1842	0.1398***	0.1153***	0.1154***	0.1227***	0.1066***	0.1142***
	(0.0196)	(0.0216)	(0.0216)	(0.0217)	(0.0214)	(0.0215)
	[0.0258]	[0.0229]	[0.0229]	[0.0236]	[0.0224]	[0.0233]
Production × Post-1842	0.0753**	0.0572***	0.0573***	0.0332	0.0462**	0.0252
	(0.0302)	(0.0192)	(0.0194)	(0.0202)	(0.0194)	(0.0199)
	[0.0461]	[0.0456]	[0.0455]	[0.0437]	[0.0448]	[0.0427]
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	No	No	No	No	No
Prov.×Year FE	No	Yes	Yes	Yes	Yes	Yes
Climate controls	No	No	Yes	No	No	Yes
Geography controls	No	No	No	Yes	No	Yes
March-route control	No	No	No	No	Yes	Yes
p-value: Transport = Production	0.061	0.032	0.032	0.003	0.026	0.002
Mean of dependent variable	0.1404	0.1404	0.1404	0.1404	0.1404	0.1404
Number of counties	277	277	277	277	277	277
Observations	11,357	11,357	11,357	11,357	11,357	11,357
Adjusted R^2	0.199	0.325	0.325	0.325	0.326	0.326

Notes: The sample consists of 277 counties in four provinces (Anhui, Jiangxi, Fujian, and Guangdong) from 1820 to 1860 (49 Transport-exposed and 11 Production-exposed). The dependent variable is an indicator equal to one if at least one rebellion occurred in county c in year t. Transport-exposed equals one for treated counties whose primary function in the Canton trade network was to facilitate the movement of goods (transport hubs along the inland trade routes). Production-exposed equals one for treated counties whose economies were primarily oriented toward export commodity production (tea, silk). Post-1842 equals one in years 1842 and after; Post-1851 equals one in years 1851 and after. All columns include county fixed effects. Column (1) adds year fixed effects; Columns (2)–(6) replace year fixed effects with province × year fixed effects. Climate controls comprise indicators for drought and flood (time-varying). The geography control is terrain ruggedness interacted with Post-1842. The march-route control is the geodesic distance from each county centroid to the Taiping 1851–1853 military advance route, interacted with Post-1851. The row p-value: Transport = Production reports the p-value from an F-test of the equality of the two interaction coefficients. Standard errors clustered at the county level are in parentheses; Conley spatial-HAC standard errors with a 500 km spatial cutoff and 10-year serial correlation are in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

coefficients therefore capture the contrast between treated and control counties rather than scaling within the treated group.

The flat tercile profile is informative about the mechanism. Counties on the corridor experience a comparable rise in rebellion regardless of their commercial density or the proximity of their market towns to the route, so exposure is well described by corridor location itself. This pattern accords with the transit labor channel, in which the displaced livelihoods of porters, boatmen, and innkeepers were tied to the through traffic of the corridor rather than to the breadth of local commerce. A mechanism operating through general commercial contraction would instead predict larger effects in more commercialized counties, which the tercile estimates do not show.

Continuous exposure and commodity composition. The transport-production and market-town-density results above characterize *which* treated counties responded most strongly. A complementary question is whether the effect scales with the *magnitude* of the trade disruption itself, rather than with route membership alone. The binary treatment indicator records only whether a county lay on the pre-1842 Canton routes; it does not register how severely trade through those routes actually contracted. To capture this, I construct a continuous exposure measure that interacts route membership with the year-by-year collapse of Canton's export share:

$$\text{TransitLoss}_{c,t} = \text{Treated}_c \times (1 - s_{\text{Canton},t}), \tag{6}$$

where $s_{\text{Canton},t}$ is Canton's combined (tea and silk, volume-weighted) share of China's overseas exports in year t, constructed from the export series in Yao (1962). By construction TransitLoss is zero for all control counties and for treated counties before 1842, and rises continuously thereafter as Canton's share is reallocated toward the new treaty ports. It thus converts the static treatment dummy into a dose that tracks the pace of trade reallocation documented in Figure 2.

I estimate

$$y_{c,t} = \beta \cdot \text{TransitLoss}_{c,t} + X'_{c,t}\gamma + \alpha_c + \delta_{p(c),t} + \varepsilon_{c,t}, \tag{7}$$

where $y_{c,t}$ is rebellion occurrence, α_c are county fixed effects, $\delta_{p(c),t}$ are province-by-year fixed effects, $X_{c,t}$ collects the climate, geography, and march-route controls, and standard errors are clustered at the county level. Table A14 reports estimates across the same sequence of specifications used in the baseline. Column (1) includes only county and year fixed effects; columns (2) through (6) add province-by-year fixed effects and the climate, geography, and march-route controls in turn. The coefficient is positive and highly significant throughout,

Table 7: Treatment Intensity and Rebellion, 1820–1860

	(1)	(2)	(3) TownDensity	(4) RouteTownShare
Town density × Post	0.0049***			
	(0.0010)			
Route town share (10km) × Post		0.1368***		
		(0.0317)		
Intensity tercile 1 × Post			0.0811**	
			(0.0327)	
Intensity tercile 2 × Post			0.1118***	
			(0.0316)	
Intensity tercile 3 × Post			0.1105***	
			(0.0296)	
Route share tercile 1 × Post				0.0863***
				(0.0331)
Route share tercile 2 × Post				0.1041***
				(0.0335)
Route share tercile 3 × Post				0.1140***
				(0.0275)
Observations	11357	11357	11357	11357
Within R^2	0.0089	0.0078	0.0092	0.0092
p-value: tercile 1 = tercile 3			0.450	0.462

Notes: Sample 1820–1860, 60 treated counties. Dependent variable is an indicator for any rebellion in the county-year. Columns (1)–(2) interact Town density (1820 market towns per 10,000 km^2) and Route town share (share of a county's 1820 market towns within 10 km of the Canton routes) with Post-1842; both equal zero for control counties. Columns (3)–(4) split each measure into terciles among the 60 treated counties (20/20/20), with control counties as the omitted group. All columns include county fixed effects, province × year fixed effects, and controls for drought, flood, and ruggedness/wheat/rice-suitability/prefecture-capital interacted with Post-1842. Crop-failure and climate-anomaly controls are omitted because they are uncoded before 1830; including them would restrict the sample to 1830–1860. Standard errors clustered by county in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

declining modestly as province-by-year fixed effects and the full control set are introduced and then stabilizing. The fully controlled estimate implies that a complete loss of Canton's export share, with TransitLoss equal to one, raises the annual probability of rebellion by 0.115. Evaluated at the average post-1842 Canton share loss of about 0.58, the implied effect is roughly 0.067, of the same order as the binary difference-in-differences estimate of Table 2. The two differ in part because the binary estimate also loads the years 1842 to 1844, when Canton's share had not yet fallen and continuous exposure is therefore zero. The stability of the coefficient across specifications indicates that the effect is driven by the collapse of Canton-bound trade rather than by an arbitrary pre-post break, since counties did not respond uniformly at the moment the treaty was signed, but increasingly as the trade on which they depended drained away.

The combined share in equation (6) pools two commodities whose export geographies differed. Tea moved south to Canton along the Anhui and Fujian feeder routes and the Gan-river trunk, whereas Jiangnan silk entered the same trunk from the lower Yangzi. Because the two commodities lost their Canton access on different timetables, decomposing the exposure measure by commodity provides a sharper test of whether the rebellion response tracks the disruption of the transit economy specifically. For each commodity $k \in \{\text{tea}, \text{silk}\}$ I define a corridor-specific flow-loss measure

$$\text{FLR}^{k}_{c,t} = \mathbf{1}\{k \text{ transits corridor of } c\} \times \left(1 - s^{k}_{\text{Canton},t}\right), \tag{8}$$

where the indicator assigns each treated county to the commodities historically carried on its segment of the route network, following Yao (1962), and $s^{k}_{\text{Canton},t}$ is Canton's share of national exports of commodity k. Tea corridors comprise the Gan-river trunk together with the Wannan and Wuyi feeder routes; silk corridors comprise the trunk and the lower-Yangzi approach. I estimate equation (7) with FLR^{tea} and FLR^{silk} entered jointly, restricting the sample to 1830 through 1850, a window in which the movement in Canton's commodity shares reflects treaty-port competition rather than the wartime disruption of trade after 1851.

Table A12 in Appendix reports the results across the same six specifications. Exposure on the tea corridors carries a large and highly significant coefficient throughout, while exposure on the silk corridors is small and statistically indistinguishable from zero in every column, and an F-test rejects equality of the two coefficients. The rebellion response to the Canton trade collapse therefore loads on the tea corridors, where the displaced labor force of porters and boatmen was concentrated. Extending the window to 1860 attenuates the tea coefficient, which remains significant without controls but weakens once the march-route

control absorbs the geography of Taiping advance, consistent with the post-1851 port shares reflecting the wartime disruption of trade; these full-window results are reported in Appendix Table A13. I return to the muted silk-corridor response, and to its bearing on the distinction between the trade-access and labor-displacement interpretations, in Section 6.

Spillovers to non-treated counties. The disruption of the Canton trade network may have affected not only counties directly on the routes but also neighboring off-route counties through local market linkages and labor mobility. To test for such spillovers, I restrict the sample to off-route counties and estimate:

$$Y_{ct} = \sum_{k \neq k_0} \beta_k \Big(\mathbf{1}\{\text{DistToRoute}_c \in \mathcal{B}_k\} \times \text{Post}_t \Big) + \lambda_c + \delta_t + X_{ct}\Gamma + \varepsilon_{ct}, \tag{9}$$

where DistToRoute_c is the distance from county c's centroid to the nearest Canton route segment, and $\mathcal{B}_k$ are distance bins (0–25, 25–50, 50–100, 100–200, and 200–400 km). The omitted category is the farthest bin (>400 km), so each β_k measures the post-1842 change in rebellion incidence for off-route counties at distance k relative to the most remote counties.

Figure 6 plots the estimated coefficients. Spillover effects are largest for counties within 25 km of the routes (approximately 0.10–0.12) and decline monotonically with distance, falling to near zero beyond 100 km. The attenuation pattern is consistent with local labor market effects: displaced porters and boatmen who could not find alternative employment near the old routes may have spread into adjacent counties, increasing competitive pressure on existing workers and raising local conflict risk within a restricted geographic radius. The estimated coefficients and their confidence intervals are reported in the Appendix.

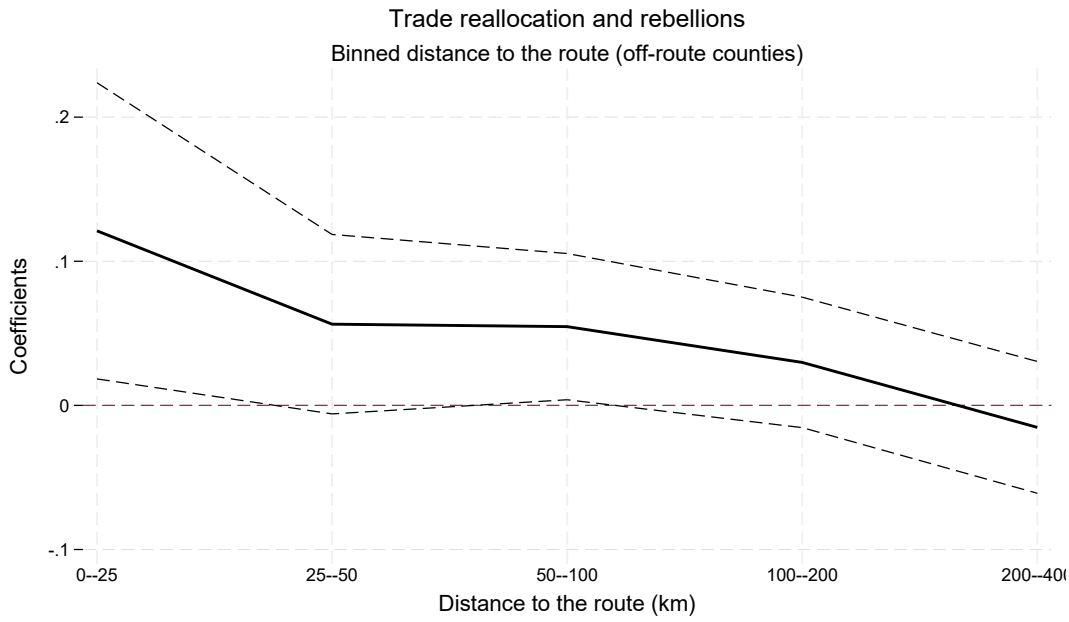


Figure 6: Spillover Effects by Distance to Historical Canton Routes

Notes: The figure plots estimated coefficients $\hat{\beta}_k$ from equation (9), with 95% confidence intervals. The sample is restricted to off-route counties. Distance bins are 0–25, 25–50, 50–100, 100–200, and 200–400 km, with $>$ 400 km as the omitted category. Specifications include county and year fixed effects and controls for crop failure, climate anomaly, drought, and flood. Standard errors clustered at the county level.

4.4 Taiping Rebellion

The trade shock raised anti-state violence in treated counties, the form of conflict most closely tied to organized political resistance. Descriptively, this anti-state activity is concentrated in the 1850s, the decade in which the Taiping Rebellion erupted in Guangxi and expanded through the inland provinces to Nanjing. This coincidence raises the question of whether the 1842 trade shock did not merely elevate background levels of local unrest, but specifically made treated counties more susceptible to absorption into the Taiping movement. Establishing this link would connect the disruption of the Canton system to one of the deadliest civil conflicts in recorded history, contributing to the literature on the origins of 19th-century Chinese social unrest (Kung and Ma, 2014; Bai and Jia, 2016).

I pursue this question by employing a discrete-time hazard model to examine whether trade-shock counties experienced *earlier* entry of Taiping forces, rather than merely higher average incidence. For each county, I define the event as the first year in which $\text{TaipingRebellion}_{ct} = 1$, and model the conditional probability of first entry in year t given that no entry has oc-

curred through year $t-1$:

$$\Pr\big(\text{Event}_{ct}=1 \mid \text{Event}_{c,t-1}=0\big) = 1-\exp\big[-\exp\big(\alpha_t+\mu_{p(c)}+\beta\,\text{Treated}_c+X_c'\Phi+Z_{ct}'\Gamma\big)\big], \quad (10)$$

where the link function is the complementary log-log, which serves as the discrete-time analog of the continuous-time Cox proportional hazards model. The risk set opens in 1851, when the rebellion broke out. Counties exit the risk set after their first Taiping-related entry, and counties that are never affected through 1860 are right-censored. The term α_t captures the year-specific baseline hazard common to all counties, reflecting the overall intensity of Taiping spread in year t, while $\mu_{p(c)}$ are province fixed effects absorbing province-level baseline differences in susceptibility to rebellion. The vector X_c collects time-invariant geography and pre-treatment controls. These include the distance from each county centroid to Jintian Village in Guangxi (the rebellion's point of origin), terrain ruggedness, population, agricultural suitability for wheat and rice, and an indicator for any recorded conflict during the pre-shock period from 1830 to 1841. The vector Z_{ct} includes time-varying climate shocks such as flood and drought indicators with their lagged disaster counterparts, together with a spatial-diffusion control measuring the number of neighboring counties already affected by Taiping in year $t-1$. The coefficient β measures whether trade-shock counties faced a systematically higher conditional hazard of first Taiping entry, conditional on geography, pre-existing instability, climate shocks, and the contagion of the rebellion from already-affected neighbors.

Table 8 reports the estimates across seven specifications that progressively add layers of controls. Column (1) includes only year fixed effects, providing the baseline cross-county comparison conditional on national-level hazard trends. Column (2) replaces these with the more demanding province-by-year fixed effects, which absorb any province-specific trajectory in the spread of the rebellion. Columns (3) through (7) build incrementally on the year-plus-province specification by adding geography in Column (3), pre-1842 conflict history in Column (4), climate shocks in Column (5), and two alternative spatial-diffusion measures based on queen contiguity in Column (6) and a within-50km radius in Column (7). Across all seven columns, the coefficient on Treated remains positive and significant across all seven specifications, ranging from about 0.29 to 0.70. The fact that β barely moves as these controls are layered in constitutes the central finding of this section. The elevated hazard faced by trade-shock counties cannot be explained away by geographic proximity to the Taiping origin in Guangxi, by pre-existing local instability before the 1842 shock, by contemporaneous agrarian or climatic stress, or by the spatial contagion of Taiping forces from already-affected neighbors. Among the controls, drought enters with a large, positive, and highly significant

coefficient across all columns in which it appears, consistent with the well-established link between climatic stress and rural unrest in late imperial China (Kung and Ma, 2014; Bai and Jia, 2016). The spatial-diffusion variable is also strongly significant, confirming the contagious dynamics of the rebellion as it propagated across adjacent counties.

Table 8: Trade Shock and First Taiping-Related Activity: Discrete-Time Hazard Model

	First Taiping Attack						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treated	0.7043***	0.4273***	0.2928**	0.3162**	0.3772**	0.3524**	0.3493**
	(0.1665)	(0.1500)	(0.1486)	(0.1484)	(0.1496)	(0.1556)	(0.1536)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Geography controls	No	No	Yes	Yes	Yes	Yes	Yes
Pre-1842 conflict control	No	No	No	Yes	Yes	Yes	Yes
Climate controls	No	No	No	No	Yes	Yes	Yes
Spatial diffusion (Queen)	No	No	No	No	No	Yes	No
Spatial diffusion (50 km radius)	No	No	No	No	No	No	Yes
Mean of dependent variable	0.1284	0.1284	0.1284	0.1284	0.1284	0.1284	0.1284
Number of counties	277	277	277	277	277	277	277
Observations	1,589	1,589	1,589	1,589	1,589	1,589	1,589
Log-likelihood	-554.52	-486.34	-479.18	-477.86	-466.39	-461.97	-465.37

Notes: The sample consists of 277 counties in four provinces (Anhui, Jiangxi, Fujian, and Guangdong) at risk of first Taiping-related activity from 1851 onward, of which 204 experience the event and 73 are right-censored at the panel's end in 1860. The outcome variable is an indicator equal to one in the first year a county exhibits any Taiping-related activity in the panel (TaipingRebellion$_{ct} = 1$), and zero for all prior years in the risk set; counties exit the risk set after their first event. Estimates are complementary log-log (cloglog) coefficients, interpretable as log hazard ratios; $\exp(\beta)$ gives the proportional hazard ratio. *Treated* equals one for counties on the pre-1842 inland trade routes to Canton. Column (1) includes year fixed effects; Columns (2)–(7) add province fixed effects to the year fixed effects. The geography control set comprises terrain ruggedness and the geodesic distance from each county centroid to the Taiping 1851–1853 military advance route. The pre-1842 conflict control is an indicator for any recorded conflict in 1830–1841. Climate controls comprise current-year drought and flood indicators plus lagged disaster indicators at $t-1$ and $t-2$. Spatial diffusion controls are the $t-1$ lagged count of queen-contiguity neighbors (Column 6) or within-50-km-radius counties (Column 7) already exhibiting Taiping-related activity. Standard errors clustered at the county level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Together, the TWFE and hazard results establish that the 1842 trade shock did more than raise the general level of local unrest. It specifically increased counties' vulnerability to absorption into the Taiping Rebellion. Trade-shock counties were more than twice as likely to experience Taiping-related activity after 1851, and they faced a substantially higher conditional hazard of first Taiping entry. This finding links the disruption of the Canton trade network to the origins of one of the nineteenth century's most consequential political upheavals. It also suggests that the displacement of transport labor in the 1840s created conditions, namely unemployed men organized into existing social networks of secret societies and local militias, that the Taiping movement exploited as it spread northward through the

interior provinces.

5 Robustness Check

5.1 Alternative Estimation Techniques

The baseline specification uses a binary indicator for rebellion as the dependent variable, which treats all county-year events identically regardless of county population size. A larger county recording one rebellion incident represents a lower incidence rate than a smaller county recording the same event. To address this concern, I construct a population-normalized measure of rebellion intensity and re-estimate the baseline specification.

The construction proceeds in two steps. First, I compute a county-year incident rate by dividing the raw rebellion count by the county-level population estimate of Zheng and Wu (2026) for 1820:

$$r_{ct} = \frac{\text{Incidents}_{ct}}{\text{Population}_{c,1820}}. \tag{11}$$

Second, because many county-years record zero rebellions, I apply an inverse hyperbolic sine (IHS) transformation to accommodate the spike of mass at zero while approximating a logarithmic scaling for positive values:

$$Y_{ct} = \text{asinh}(\alpha \cdot r_{ct}), \tag{12}$$

where $\alpha > 0$ is a scaling constant. Because Zheng and Wu (2026) provide population directly at the county level, no imputation from prefecture totals is required; the estimation sample retains 274 of the 277 baseline counties (8,494 county-year observations), the three unmatched counties being excluded.

Table 9 reports the results. Across all six specifications, the coefficient on Treated $\times$ Post-1842 is positive, precisely estimated, and stable. The preferred estimate in Column (2), which includes province-by-year fixed effects, is 0.0444, significant at the 1 percent level. The estimates in Columns (3)–(6), which sequentially add time-varying controls, are approximately 0.037. Two features of the results are noteworthy. First, the coefficient is somewhat smaller in magnitude than the baseline binary estimate of approximately 0.096. This compression is expected: Table 1 shows that treated counties are somewhat more populous than control counties on average, so the per-capita rebellion rate rises by less than the raw incidence probability. Second, the sample restriction to 274 counties does not qualitatively alter the conclusion: the effect of trade-access loss on rebellion intensity is robust to population normalization, confirming that the baseline result is not driven by county size

differences between treated and control groups.

Table 9: Trade Shock and Rebellions: Population-Normalized Dependent Variable

	Rebellions (IHS normalized by 1820 population)					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post-1842	0.0501***	0.0444***	0.0442***	0.0450***	0.0358***	0.0368***
	(0.0116)	(0.0126)	(0.0126)	(0.0125)	(0.0129)	(0.0127)
County FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	No	No	No	No	No
Province × Year FE	No	Yes	Yes	Yes	Yes	Yes
Climate controls	No	No	Yes	No	No	Yes
Geography controls	No	No	No	Yes	No	Yes
March-route control	No	No	No	No	Yes	Yes
Mean of dependent variable	0.063	0.063	0.063	0.063	0.063	0.063
Number of counties	274	274	274	274	274	274
Observations	11234	11234	11234	11234	11234	11234
Adjusted R^2	0.157	0.237	0.238	0.238	0.241	0.242

Notes: The dependent variable is the inverse hyperbolic sine transformation of the rebellion count normalized by the county-level population estimate in 1820 (equation 12). County population in 1820 is taken from the county-level estimates of Zheng and Wu (2026), which build on prefecture totals in Cao (2001) and household shares in Liang (1980). Three counties without a matched population estimate are excluded, yielding 274 counties and 8,494 county-year observations. Treated × Post-1842 equals one for counties lying on the pre-1842 inland routes to Canton in years ≥ 1842. All columns include county fixed effects. Column (1) includes year fixed effects; Columns (2)–(6) include province × year fixed effects and sequentially add controls for crop failure, climate anomaly, drought, and flood. Standard errors, clustered at the county level, in parentheses.

5.2 Ruling Out Import Competition in Handicraft Textiles

One possible alternative explanation for the baseline results is increased competition from imported textile products. Following the opening of the treaty ports, machine spun yarn and machine woven cloth entered China through the new ports, and the resulting pressure on domestic spinners and weavers has long featured prominently in accounts of the late Qing economy (Peng, 1957). If counties located along the Canton routes were also concentrated in regions engaged in handicraft textile production, the estimated treatment effect might capture distress in the textile economy caused by foreign imports rather than the decline of the transit economy. This subsection evaluates that possibility directly.

I measure exposure to textile production using prefecture level indicators for the documented production of textiles and chintz, which I assign to all counties within the corresponding prefecture. This measure captures participation in a regional textile economy rather than production within a particular county, since handicraft textile activity relied on market networks that connected spinning households, market towns, and cloth merchants across prefectural boundaries. Of the 277 counties in the negative exposure sample, 153 belong to prefectures with documented textile or chintz production. Importantly, textile exposure is almost entirely unrelated to Canton route exposure. Approximately 55 percent of both treated and control counties are located in textile producing prefectures. The geographic correlation required for textile competition to confound the baseline estimates is therefore absent.

Table 10 reports four specifications, each of which follows the fully controlled baseline specification in column 6 of Table 2. Column (1) reproduces the baseline estimate for comparison. The estimated treatment effect is 0.104 and is statistically significant at the 1 percent level. Column (2) adds an interaction between the prefecture level textile indicator and Post1842 as a competing source of post treaty distress. The treatment coefficient remains essentially unchanged at 0.104. The textile exposure coefficient is negative at 0.024 in absolute value and is statistically significant at the 10 percent level. The negative sign indicates that textile producing prefectures experienced, if anything, a relative decline rather than an increase in rebellion following the opening of the treaty ports, so this result provides no evidence that competition from imported textiles drove the baseline effect.

Column (3) offers a more direct test by limiting the sample to the 124 counties located in prefectures with no documented production of either textiles or chintz. Import competition in handicraft textiles cannot explain an increase in rebellion in areas without a documented textile economy. The treatment coefficient in this restricted sample is 0.112 and remains statistically significant at the 1 percent level, making it nearly identical to the estimate from the full sample. This restriction is deliberately conservative because it excludes every county located in a textile producing prefecture, including counties for which there is no evidence of local textile activity.

Column (4) introduces an interaction among treatment status, Post1842, and textile exposure. If the baseline relationship were driven by distress in the textile sector, the post treaty treatment effect should be larger in textile producing prefectures. The estimated triple interaction is negative at 0.002 in absolute value and is statistically insignificant, with a (p) value of 0.96. The estimated treatment effect therefore does not differ meaningfully between textile and nontextile prefectures.

The timing of foreign textile penetration provides an additional reason to reject this

interpretation. Historians generally date the substantial displacement of Chinese handicraft cloth by imported textiles to the 1860s and later, after the end of the sample period. Before 1860, imported textiles remained concentrated mainly in areas close to the treaty ports (Peng, 1957). Taken together, the lack of geographic correlation between Canton route exposure and textile production, the stability of the treatment coefficient after controlling for textile exposure, the persistence of the effect in places without documented textile production, the insignificant triple interaction, and the timing of import penetration all indicate that competition from foreign textiles does not account for the baseline findings.

Table 10: Robustness to Import Competition in Handicraft Textiles

	(1) Baseline	(2) Textile control	(3) Non-textile sample	(4) Triple interaction
Treated × Post-1842	0.096***	0.096***	0.105***	0.096***
	(0.018)	(0.018)	(0.034)	(0.029)
Treated × Post-1842 × Textile pref.				−0.001
				(0.039)
Textile pref. × Post-1842		−0.024*		−0.024
		(0.013)		(0.015)
Observations	11,357	11,357	5,084	11,357
Within R^2	0.008	0.008	0.008	0.008

Notes: The specification is identical to Table 2 column (6). Textile pref. indicates that the county lies in a prefecture with documented textile or chintz production. Column (3) restricts the sample to counties in prefectures with no textile or chintz production. Standard errors clustered at the county level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5.3 Placebo tests

I assess the research design with two placebo exercises. First, I assign fake treatment dates within the pre-treaty period, restricting the sample to 1820 to 1841 and setting the post indicator to one at or after 1830, 1832, and 1835 in turn, with the Column (6) specification otherwise unchanged. Table A15 reports the results: the coefficient on Treated × (Fake) Post is small and statistically insignificant at every fake date, in contrast to the large, significant estimate at the true 1842 date shown in the final column. The post-1842 increase is thus specific to the treaty date and does not reflect a pre-existing divergence between treated and control counties.

Second, I conduct a randomization-inference test. In each of 1,000 draws I randomly assign, within each province, the same number of placebo-treated counties as the true treated set, and re-estimate Column (6). Table A16 summarizes the permutation distribution, which

is centered near zero, with a mean of -0.001 and a standard deviation of 0.016; the actual estimate of 0.096 lies beyond every one of the 1,000 placebo estimates ($p < 0.001$). The effect is therefore highly unlikely to arise from the spatial configuration of treated counties alone.

A further check on whether the effect merely reflects major-road status, rather than Canton-trade exposure specifically, is reported in Appendix A.1.

5.4 PSM-DID

Table 1 shows that treated and control counties differ in several characteristics measured before the trade shock. Counties located along the Canton routes had larger populations, experienced floods more frequently, and had somewhat higher densities of market towns. County fixed effects account for all persistent differences between counties. A remaining concern, nonetheless, is that treated counties may differ systematically from control counties in observable characteristics that are associated with subsequent changes in conflict. I address this concern by constructing a matched sample in which treated and control counties are more comparable, and then estimating the baseline specification on this sample.

I estimate the probability of exposure to the Canton routes using a cross sectional logit model with nine pre-treatment covariates: terrain ruggedness, county area, population in 1820, market town density, suitability for rice and wheat cultivation, the frequency of floods and droughts between 1830 and 1841, and the frequency of rebellion over the same period. Each treated county is then matched to the control county with the closest propensity score. I use one to one nearest neighbour matching, impose a caliper of 0.05, and do not allow controls to be reused. Of the 60 treated counties, 54 are matched to 54 distinct control counties, producing a matched sample of 108 counties. The remaining six treated counties lie outside the common support. The matched-sample estimate is 0.070, significant at the 1 percent level.

Appendix Table A7 evaluates covariate balance before and after matching. Prior to matching, several characteristics display substantial standardized differences, particularly population in 1820 and the historical frequencies of floods and droughts, consistent with the imbalances reported in Table 1. Matching substantially improves comparability between the two groups. The mean absolute standardized bias falls by roughly three quarters, and the pseudo R^2 from the treatment model declines sharply. The likelihood ratio test strongly rejects the joint orthogonality of the covariates to treatment before matching, whereas after matching the same hypothesis cannot be rejected. None of the individual covariate differences is statistically significant in the matched sample.

The frequency of rebellion before the trade shock is already similar across treated and control counties before matching and remains balanced afterward, with a small standardized difference in absolute value. This balance is reassuring because it indicates that treated and matched control counties had similar levels of conflict before 1842. Together with the event study evidence of parallel trends, it reduces the concern that the estimated effect reflects pre-existing differences in conflict trajectories.

Appendix Table A8 estimates the baseline difference in differences specification on the matched sample. The coefficient on *Treated* × *Post1842* remains positive and statistically significant at the 1 percent level. The matched-sample coefficient of 0.070 is somewhat smaller than the full-sample estimate of 0.096 but of the same order of magnitude, and the gap is well within the wider confidence interval that the smaller matched sample implies. The matched sample includes both treated and control counties in each of the four provinces, so the province by year fixed effects remain identified. The stability of the coefficient across the full and matched samples indicates that observable differences between counties on and off the Canton routes do not account for the baseline result.

5.5 Changes-in-Changes

The baseline difference in differences estimator imposes a homogeneous treatment effect across all observations. One concern with this assumption is that counties lying on the Canton trade corridor may differ systematically from off corridor counties in ways that interact with the post 1842 environment, so that a common national shock could provoke a differential response unrelated to the corridor mechanism itself. The baseline specification already mitigates this concern within the difference in differences framework, both by absorbing province by year effects and by allowing flexible time effects of pre treatment rebelliousness, and the event study reported above confirms that treated and control counties followed parallel trends before 1842. This section provides further reassurance by employing two alternative procedures that account for systematic differences between the treated and control groups in ways the standard estimator does not.

The first procedure is the changes in changes (CIC) estimator of Athey and Imbens (2006), which generalizes the standard difference in differences model. Rather than comparing mean outcomes across the two groups, the CIC estimator recovers the entire distribution of the counterfactual outcome and estimates quantile treatment effects nonparametrically, comparing this counterfactual distribution to the realized post period distribution for the treated group. The method imposes no functional form on the outcome distribution and permits the treated and control groups to differ in the distribution of unobservable charac-

teristics in arbitrary ways, provided that distribution is time invariant within each group. A positive effect that survives this relaxation is therefore informative regardless of how the two groups differ at baseline.

I follow the two step procedure of Athey and Imbens (2006). The first step uses ordinary least squares to partial out the covariates and the high dimensional fixed effects, namely county fixed effects, year fixed effects, and province by year fixed effects, together with the baseline controls. The second step applies the unconditional CIC estimator to the residuals to recover the quantile treatment effects, with standard errors from 100 bootstrap replications. The estimates are reported in Appendix Table A9.

Two patterns emerge. First, the corridor shock raises rebellion intensity across the interpretable range of the distribution. The overall average treatment effect is positive and significant, and the quantile treatment effects are positive and significant at every quantile from the 25th upward. This confirms the positive effect found in the baseline and shows that it does not depend on comparing group means. I caution that the rebellion intensity outcome has a large mass at zero, since most county years record no incident, so the quantile treatment effects below the 25th percentile fall within this zero mass region, where the estimator carries little information. I report them in Appendix Table A9 for completeness but do not interpret them, and I restrict attention to the 25th percentile and above.

Second, the effect is largest in the upper tail of the distribution, well above the values near the median. The corridor shock therefore did not raise rebellion uniformly. Instead it shifted the upper portion of the intensity distribution, pushing the most affected counties toward substantially higher levels of unrest. This concentration in the upper tail is consistent with the displacement mechanism, since the collapse of corridor traffic released transport labor in concentrated locations along the route, and where local conditions were already combustible this surplus translated into more intense and larger scale uprisings rather than a uniform increase everywhere.

5.6 Synthetic Difference-in-Differences

The second procedure constructs a synthetic counterfactual for the treated group. Whereas the CIC estimator allows the distribution of unobservables to differ across groups in arbitrary ways, the synthetic approach addresses comparability by building a weighted combination of control counties that tracks the evolution of the treated group's outcome before 1842. The post period trajectory of this synthetic control then serves as a projection of what the corridor counties would have experienced absent the trade shock.

The classical synthetic control method of Abadie, Diamond, and Hainmueller (2010)

was designed for settings with a long pre treatment series, which my panel does not provide, since the corridor shock occurs in 1842 and the pre period spans only the years 1820 to 1841. I therefore employ the synthetic difference in differences estimator, which combines unit and time weighting with a difference in differences structure and is more robust to short pre treatment panels than the classical estimator. Treatment is block assigned in 1842 to all corridor counties, and I compute placebo, bootstrap, and jackknife standard errors. The results appear in Appendix Table A11.

The estimated average treatment effect on the treated is positive and significant at the one percent level using all control counties as donors, and it is stable across the three inference procedures. The point estimate and significance match the baseline difference in differences results in both sign and magnitude. To guard against contamination from control counties that lie close to the corridor and may absorb spillovers, I also estimate a specification that drops the control counties within 52 kilometers of the route, a threshold just above the maximum distance among treated counties. The effect is essentially unchanged and remains significant at the one percent level, and the slight increase is consistent with the removal of contaminated comparison units.

Appendix Figure A1 plots the trajectories of the treated and synthetic control groups. The two series diverge after 1842, with corridor counties lying systematically above their synthetic counterfactual throughout the post period. Two features of the figure deserve comment. First, rebellion rises in the synthetic control group as well during the 1850s, reflecting the general political instability of the mid nineteenth century, most notably the Taiping upheaval. Corridor counties, however, begin to separate from their synthetic counterfactual earlier, around 1850 to 1851, and remain above it thereafter. Were the corridor counties simply more sensitive to nationwide turmoil, the two series would have risen in step; the earlier and larger increase among treated counties instead points to a corridor specific shock. Second, I acknowledge a limitation. Because the pre treatment window is short and the outcome is sparse, the synthetic control's pre period fit is imperfect, with a pre 1842 prediction error that is non-negligible relative to the estimated treatment effect. I therefore treat the synthetic difference in differences estimate as corroborating evidence rather than as primary inference, and the two way fixed effects difference in differences specification remains my main result.

Taken together, the two procedures generalize the standard difference in differences methodology by allowing for systematic differences between corridor and off corridor counties, and both confirm that the positive effect of the trade shock on rebellion is not an artifact of comparing dissimilar groups.

6 Discussion of Mechanisms

The results in Section 4 establish that counties exposed to the collapse of the Canton trade network experienced a substantial increase in rebellion probability after 1842. This section discusses the economic channels through which this effect may have operated. Two candidate mechanisms are considered: an opportunity cost channel operating through the displacement of transport labor; and a state capacity channel operating through the erosion of fiscal and administrative resources. I draw where possible on the regression evidence presented in Section 4 to assess the relative plausibility of each channel, and I outline the empirical work required to test them more formally.

6.1 Opportunity Cost Channel

The opportunity cost channel is the most natural explanation for the empirical results. Under this interpretation, the collapse of the Canton trade network destroyed the livelihoods of the large population of transport workers who had sustained themselves through the route economy, including porters who carried goods over mountain passes and boatmen who navigated the river systems linking the interior to Guangzhou. With their primary source of income eliminated and few alternative opportunities in a largely pre-industrial rural economy, these workers faced a sharp decline in the cost of participating in collective violence. As Wakeman (1966) documents, displaced transport workers became a ready pool of recruits for secret societies and armed resistance in the years following the treaty. The prediction of the opportunity cost channel is that the treatment effect should be larger where the trade-dependent labor force was larger, that is, in counties more deeply embedded in the transport economy of the Canton system (Becker, 1968).

I do not have direct evidence on this channel in the form of employment records or wage data for porters and boatmen at the county-year level; such data are not available for mid-nineteenth-century China. The empirical evidence presented in Section 4 is therefore best interpreted as circumstantial but coherent support for the opportunity cost interpretation, rather than a formal test.

Several pieces of evidence point in this direction. First, the negative effect for positively exposed counties argues against an alternative rapacity interpretation, under which greater trade activity generates contestable rents and stimulates conflict among rival claimants (Grossman, 1991). If rent-seeking were the operative mechanism, counties gaining access to the new treaty port trade should also experience increased conflict, a prediction that the data clearly reject. The asymmetry between negative and positive exposure instead points toward a mechanism rooted in income loss rather than rent creation.

Second, the heterogeneity results in Table 6 show that the treatment effect is concentrated among transport-exposed counties, whose primary function in the Canton network was to facilitate the movement of goods, relative to production-exposed counties whose economies centered on commodity cultivation. The difference is marginally significant, with a p-value of 0.10 in the preferred specification, and its direction is consistent with the opportunity cost channel. Transport workers faced a near-complete destruction of demand for their services following the reallocation of trade toward coastal shipping, whereas commodity producers could in principle reallocate land and labor to other markets over time.

Third, the economic intensity results in Table 7 bear on the same logic, though in a way that sharpens rather than simply echoes it. As reported in Section 4.3, the treatment effect does not scale with pre-1842 commercial density: the tercile profile shows no monotone pattern, and equality of the first and third terciles cannot be rejected for either intensity measure, so exposure is well described by corridor location itself rather than by the breadth of local commerce within treated counties. This pattern is consistent with the transit labor channel, in which the displaced livelihoods of porters, boatmen, and innkeepers were tied to the through traffic of the corridor rather than to the density of commercial activity, so that displacement followed corridor placement rather than local market size. It contrasts with Cao and Chen (2022), who find that the rebellion effect of the Grand Canal's closure is significantly stronger in counties with a higher share of market towns within ten kilometers of the canal. The difference is instructive: the Grand Canal carried domestic tribute grain whose handling concentrated in commercial towns, whereas the Canton route economy rested on a transport workforce strung along mountain passes and river segments, for whom proximity to the corridor, not local commercial density, determined exposure to the shock.

Fourth, the geographic attenuation of spillover effects documented in Figure 6, which decays from approximately 0.10 to 0.12 within 25 km of the routes to near zero beyond 100 km, is consistent with local labor market spillovers. Displaced workers who could not find alternative employment near the route corridor may have migrated into adjacent counties, generating downward pressure on local wages and heightened social strain within a restricted geographic radius. The localized nature of the spillovers also argues against explanations based on broader provincial fiscal or political shocks, which would be expected to generate more diffuse geographic patterns.

Fifth, I provide more direct evidence on the link between trade disruption and secret society activity, the recruitment channel emphasized by Wakeman (1966). Qin and Meng (2008) describes this process for Guangdong after the opening of the treaty ports: once Canton lost its monopoly over foreign trade, the small merchants and the laborers who had made their living along the trade routes were thrown out of work in large numbers, and many

of these destitute people turned to the Tiandihui (Heaven and Earth Society) for support, producing a rapid expansion of secret society organization in the province. This account maps directly onto the opportunity cost mechanism: boatmen and porters who lost their livelihoods along the Canton routes became recruits for secret societies. Motivated by this account, I test whether secret society activity after 1842 was systematically more prevalent in the treated corridor counties.

I assemble county-level records of post-1842 Tiandihui foundings from Qin and Meng (2008) and Qin (2005) and estimate a cross-sectional Poisson regression of the form

$$\log E[\text{Foundings}_c] = \beta \, \text{Treated}_c + X_c'\gamma + \delta_{p(c)}, \tag{13}$$

where Foundings_c is the number of secret society foundings recorded in county c after 1842, Treated_c indicates a county on the pre-1842 Canton routes, X_c controls for log population, market town density, and terrain ruggedness, and $\delta_{p(c)}$ is a province fixed effect. The exponentiated coefficient $\exp(\beta)$ is the incidence-rate ratio, the proportional difference in the expected number of foundings between treated and control counties. Table 11 reports the results. In the preferred specification, a treated county had roughly 2.4 times the expected number of post-1842 secret society foundings relative to a control county in the same province with comparable population, commercial density, and terrain ($p < 0.01$), and the positive association is stable across Poisson and ordinary least squares estimators and across the narrower 1842 to 1860 window. Foundings are most numerous in Guangzhou prefecture, the terminus of the Canton corridor and the jurisdiction most exposed to the post-1842 reallocation of trade, exactly as the mechanism would predict. This is a cross-sectional association rather than a causal estimate, since province fixed effects cannot fully separate the corridor from the broader geography of secret society activity, and I therefore read it as suggestive corroboration rather than a formal test. Taken together with the qualitative account in **?** and the indirect evidence above, it lends further weight to the interpretation that the trade shock raised conflict by displacing trade-dependent labor.

6.2 State Capacity

A further possibility is that the trade shock operated through a weakening of state capacity. The Canton trade supported private livelihoods, but it also supplied the Qing state with considerable fiscal revenue. Customs duties levied on goods moving through Guangzhou and along the inland routes fed into provincial finances and the imperial household treasury. As overseas commerce shifted toward the new treaty ports after 1842, the revenue tied to the Canton trade may have fallen. Diminished fiscal resources could in turn have eroded

Table 11: Trade Shock and Post-1842 Secret Society Founding: County Cross Section

	(1) Poisson	(2) Poisson	(3) OLS	(4) Poisson	(5) Poisson
Treated (Canton route)	0.8829**	1.0365***	0.1448*	0.9078*	0.9522*
	(0.3897)	(0.3851)	(0.0818)	(0.5275)	(0.5631)
Incidence-rate ratio exp(β)	2.418	2.819		2.479	2.591
Province FE	Yes	Yes	Yes	No	Yes
Prefecture FE	No	No	No	Yes	No
Controls	Yes	No	Yes	Yes	Yes
Observations	274	277	274	127	215

Notes: Dependent variable: number of post-1842 Tiandihui (secret society) foundings per county (columns 1–4); column 5 counts foundings during 1842–1860. Sample: 277 baseline counties, 60 Canton-route treated. Columns 1, 2, 4, 5 are HDFE-Poisson (pyfixest `fepois`, HC1-robust SE); column 3 is OLS with robust SE. The incidence-rate ratio is exp(β). Controls: log county population, market-town density, ruggedness. Column 4 absorbs prefecture fixed effects (Poisson drops all-zero prefecture groups, reducing the sample). Cross-sectional associations, not causal.

the government's capacity to maintain order, by constraining the funds available for military garrisons, yamen staff, and the granaries that underpinned famine relief and social insurance (Cao and Chen, 2022).

Two features of the setting make this channel unlikely to drive the results. A large portion of Canton customs revenue flowed to the imperial household rather than staying with local governments, so the decline of the Canton trade did not map directly onto an equivalent loss of county level fiscal resources. In addition, the preferred specifications absorb province by year fixed effects, which net out fiscal and administrative shocks common to a province in a given year, so that identification rests on comparisons among counties within the same province and year. For state capacity to account for the baseline estimates, administrative capacity would have had to weaken more steeply in counties along the former Canton routes than in their within province neighbors, and to do so specifically after 1842. That is a stringent requirement.

I nonetheless test the mechanism directly. If state capacity buffered the trade shock, counties with stronger pre-1842 administration should have been better positioned to suppress emerging unrest, and the treatment effect should be smaller where initial capacity was greater. I capture baseline administrative capacity with three proxies.

The first is the county's regional importance rating under the Qing "chong fan pi nan" classification, which graded jurisdictions from one to four according to their administrative weight and the difficulty of governing them. More highly rated posts were treated as more demanding and tended to receive abler officials and larger administrative resources (Liu and

Zhang, 2025; Zhang, 2011). The ratings were fixed through three empire wide revisions completed between 1733 and 1747 and changed little afterward, so they were set well before the trade shock. I take the rating from personnel records spanning 1760 to 1798 and limit the analysis to the 260 sample counties for which it is available.

The second proxy indicates whether a county served as a prefecture capital. Such counties housed higher tiers of government and typically held denser concentrations of bureaucratic and military resources. The third proxy is the presence of Green Standard troops before 1842, a more direct gauge of the coercive resources at local authorities' disposal. I take battalion troop quotas from Luo (1984) and assign them to counties using the headquarters assignment procedure of Cao and Chen (2022). I build two versions: a headquarters measure that gives the full battalion quota to the county holding its headquarters, and a shared measure that splits the quota across all counties in the battalion's jurisdiction. Counties with no recorded garrison take a value of zero, so the military measures span all 277 counties in the baseline sample.

I interact each proxy with treatment status and the post 1842 indicator. For the regional importance rating, I estimate

$$Y_{ct} = \beta_1 \left(\text{Treated}_c \times \text{Post}_t\right) + \beta_2 \left(\text{Treated}_c \times \text{Post}_t \times \text{Rank}_c\right) + \beta_3 \left(\text{Post}_t \times \text{Rank}_c\right) + \lambda_c + \varphi_{p(c),t} + X_{ct}\Gamma + \varepsilon_{ct}, \tag{14}$$

where Rank_c is the county's regional importance rating, centered on its sample mean. The prefecture capital specification replaces Rank_c with the indicator PrefCap_c. For military capacity, I estimate

$$Y_{ct} = \beta_1 \left(\text{Treated}_c \times \text{Post}_t\right) + \beta_2 \left(\text{Treated}_c \times \text{Post}_t \times \text{Soldiers}_c\right) + \beta_3 \left(\text{Post}_t \times \text{Soldiers}_c\right) + \lambda_c + \varphi_{p(c),t} + X_{ct}\Gamma + \varepsilon_{ct} \tag{15}$$

where $\text{Soldiers}_c = \text{asinh}(\text{troops}_c/100)$ is the inverse hyperbolic sine of the county troop quota, centered on its sample mean. I estimate this specification with both the headquarters and shared measures. The capacity variables and their interactions with Treated_c are time invariant and are absorbed by the county fixed effects. The coefficient of interest is β_2, which the state capacity mechanism predicts should be negative, since stronger initial capacity should dampen the effect of the trade shock on rebellion.

Table 12 presents the results. The main difference in differences coefficient stays positive and highly significant throughout, and for a county at the average value of each capacity measure the estimated effect remains substantial. None of the interaction estimates lends

convincing support to the state capacity mechanism.

For the regional importance rating, the interaction is positive and significant only at the ten percent level. Greater administrative importance is associated with a larger rather than a smaller increase in rebellion, contrary to what the state capacity mechanism predicts. A more plausible reading is that administratively important counties often sat on active commercial corridors and held larger concentrations of workers whose livelihoods depended on trade, so that the positive interaction is consistent with the transport exposure gradient documented in Section 4.3.

The interaction with prefecture capital status is negative in sign, which would be consistent with a cushioning effect, but it is statistically insignificant and therefore offers no reliable evidence that prefecture capitals contained the rise in rebellion more effectively. The Green Standard troop measures are similarly weak: under both the headquarters and the shared assignment, the interaction is statistically indistinguishable from zero, so the size of the local Green Standard establishment does not appear to moderate the effect of the trade shock. Taken together, the positive interaction for the regional importance rating and the null results for prefecture capital status and military presence provide little support for declining state capacity as the principal mechanism.

A more direct test would require county or prefecture level data on fiscal revenue, military strength, or administrative staffing before and after 1842. The Green Standard quotas used here describe the military establishment before the shock but do not vary over time, and richer annual records are only now emerging through the digitization of Qing archival materials. I therefore leave a direct study of changes in local fiscal and administrative capacity to future research. Even so, the evidence presented here, together with the absence of any increase in conflict among positively exposed counties documented in Section 4.2, points away from an erosion of state capacity and toward the displacement of workers whose livelihoods depended on the Canton trade network.

7 Conclusion

This paper has studied the causal effect of trade disruption on local social conflict using the 1842 Treaty of Nanjing as a source of exogenous variation in counties' exposure to the collapse of the Canton trade network. The main finding is that counties on the pre-1842 inland routes to Canton experienced a significant and persistent increase in rebellion probability after 1842, relative to other counties in the same provinces. The effect is robust to alternative specifications and dependent variable constructions. Counties positively exposed to the new treaty ports experienced a relative decline; these patterns are jointly consistent

Table 12: Trade Shock and Rebellion: Heterogeneity by Baseline Administrative Capacity

	(1)	(2)	(3)	(4)
Treated × Post1842	0.0838***	0.1031***	0.0918***	0.0924***
	(0.0190)	(0.0211)	(0.0178)	(0.0181)
Treated × Post × Quefen	0.0184			
	(0.0213)			
Treated × Post × Prefecture capital		-0.0594		
		(0.0427)		
Treated × Post × Soldiers (asinh, HQ)			0.0064	
			(0.0133)	
Treated × Post × Soldiers (asinh, share)				-0.0000
				(0.0131)
County FE	Yes	Yes	Yes	Yes
Province×Year FE	Yes	Yes	Yes	Yes
Controls (Table 2, col. 6)	Yes	Yes	Yes	Yes
Observations	10,660	10,660	11,357	11,357
Counties (clusters)	260	260	277	277
Adjusted R^2	0.326	0.326	0.326	0.326

Dependent variable: `occurrence`, a binary indicator for any rebellion event in the county-year. Columns (1)–(2) restrict the sample to the 260 counties with a non-missing CGED-Q quefen rank (17 counties lacking a quefen record are excluded); columns (3)–(4) use all 277 counties, because the military capacity proxy has no missing values (counties with no Green Standard garrison are true zeros). Frozen baseline panel, 1820–1860, 60 treated counties. Standard errors clustered by county in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Column (1) centers `quefen_y1798_num` at its in-sample mean (1.91), so `Treated` × `Post1842` is the DID at the average county. The military capacity terms in columns (3)–(4), asinh(soldier/100), are likewise centered at their in-sample means (HQ: 0.97; share: 0.89). `soldier_hq` assigns each Green Standard battalion's troop quota to its single garrison (HQ) county; `soldier_share` instead splits each battalion's quota equally across all counties it administers. Troop data are from Luo Ergang, *Lüying bing zhi*, with counties assigned following the rule of Cao & Chen (2022). Controls as in Table 2, column (6).

with an opportunity-cost mechanism driven by labor displacement rather than a rapacity mechanism driven by contestable rents.

The effect holds when conflict is disaggregated by motive, appearing for both anti-social and anti-state violence, which indicates that the shock raised disorder broadly rather than along a single dimension. The anti-state response, descriptively concentrated in the 1850s, coincides with the outbreak of the Taiping Rebellion. Trade-shock counties were substantially more likely to experience Taiping-related conflict and faced a higher conditional hazard of first Taiping entry, establishing a link between the disruption of international trade and the origins of one of the most consequential civil conflicts in recorded history.

More broadly, the results illustrate how international trade reallocation, even when it expands aggregate trade volumes, can have deeply destabilizing consequences for the specific communities and workers embedded in old networks. Understanding these distributional effects of trade integration remains important for contemporary debates about the social costs of de-globalization and the political economy of trade adjustment.

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A Appendix

Table A1: Robustness: Alternative Geographic Controls

	Rebellion occurrence (0/1)			
	(A1)	(A2)	(A3)	(A4)
Treated × Post-1842	0.0956***	0.0950***	0.0936***	0.0919***
	(0.0183)	(0.0190)	(0.0184)	(0.0191)
	[0.0215]	[0.0212]	[0.0214]	[0.0225]
Geographic control	Ruggedness	Wheat	Rice	Log(pop.)
County FE	Yes	Yes	Yes	Yes
Prov.×Year FE	Yes	Yes	Yes	Yes
Climate controls	Yes	Yes	Yes	Yes
March-route control	Yes	Yes	Yes	Yes
Mean of dependent variable	0.1404	0.1404	0.1404	0.1398
Number of counties	277	277	277	274
Observations	11,357	11,357	11,357	11,234
Adjusted R^2	0.326	0.326	0.326	0.325

Notes: The dependent variable is an indicator equal to one if at least one rebellion occurred in county c in year t. Each column re-runs the kitchen-sink specification (Column 6 of Table 2) replacing terrain ruggedness with an alternative geographic control. All four candidates are highly correlated in our four-province sample ($|r| > 0.74$) and convey essentially the same information. Standard errors clustered at the county level are in parentheses; Conley spatial-HAC standard errors with a 500 km cutoff and 10-year serial correlation are in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Restricting the sample to the pre-Taiping window of 1820 to 1850, which removes any mechanical role for the Taiping-era surge, leaves the estimate virtually unchanged: the Column (6) coefficient is 0.096, essentially identical to the full-sample baseline of 0.096. Table A2 reports the full six-column specification.

A.1 Robustness to major-road status

A natural concern is that the effect reflects major-road status in general rather than Canton-trade exposure specifically, because the courier road network (*yilu*) overlaps geographically with the Canton routes. Table A3 probes this. Panel A assigns a placebo

Table A2: Robustness: Excluding the Taiping Period (1820–1850)

	Rebellion occurrence (0/1)					
	(1)	(2)	(3)	(4)	(5)	(6)
Treated × Post-1842	0.0759***	0.0947***	0.0945***	0.0956***	0.0947***	0.0955***
	(0.0220)	(0.0210)	(0.0210)	(0.0207)	(0.0210)	(0.0207)
	[0.0233]	[0.0241]	[0.0241]	[0.0241]	[0.0241]	[0.0240]
Observations	8,587	8,587	8,587	8,587	8,587	8,587
Counties	277	277	277	277	277	277

Notes: Frozen baseline panel restricted to 1820–1850 (excluding the Taiping period), 60 treated counties; the six-column specification is identical to Table 2. Cluster-robust standard errors (county) in parentheses; Conley spatial-HAC standard errors (500 km, 10-year) in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

treatment to counties on the courier network using the full sample; Panel B excludes the Canton-treated counties; Panel C additionally excludes control counties within 100 km of the Canton routes; Panel D is a horse race that enters the Canton treatment, the courier indicator, and a 100 km spillover ring jointly. On the full 1820 to 1860 frozen panel the courier coefficient is positive and significant in Panels A and B, but it falls to 0.028 and becomes insignificant once near-corridor counties are removed in Panel C, and in the horse race the Canton treatment remains large and significant (0.090) while the courier term is an order of magnitude smaller. The courier association therefore reflects the geographic proximity of courier counties to the Canton corridor rather than an independent major-road effect.

Table A3: Robustness to Major-Road Status: Courier-Road Placebo and Horse Race

	(1) FE only	(2) Full controls
Panel A: Courier × Post-1842, full sample		
Courier × Post-1842	0.0068	0.0422***
	(0.0162)	(0.0140)
Observations	11357	11357
Panel B: excluding Canton-treated (60) counties		
Courier × Post-1842	0.0046	0.0373**
	(0.0168)	(0.0148)
Observations	8897	8897
Panel C: also excluding counties within 100 km of the Canton route		
Courier × Post-1842	-0.0166	0.0276
	(0.0198)	(0.0205)
Observations	4674	4674
Panel D: horse race (full sample, full controls)		
Treated × Post-1842	0.0897***	0.1118***
	(0.0179)	(0.0207)
Courier × Post-1842	0.0295**	0.0282**
	(0.0137)	(0.0135)
Spillover (≤100 km) × Post-1842		0.0344**
		(0.0154)
Observations	11357	11357

Notes: Frozen baseline panel, 1820–1860. Dependent variable is an indicator for any rebellion in the county-year. The treatment is *Courier road* × Post-1842, for counties on the CHGIS Ming courier-road network. Column (1) includes county and year fixed effects only; Column (2) adds province × year fixed effects and the baseline controls (drought, flood, ruggedness×Post-1842, march-route×Post-1851). Panel A uses all 277 counties; Panel B excludes the 60 Canton-treated counties; Panel C additionally excludes control counties within 100 km of the Canton routes. Panel D is a horse race entering the Canton treatment, the courier indicator, and a ≤100 km spillover ring jointly. Standard errors clustered by county (OID) in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Continuous Timing Decomposition

	(1) 1820–1860
TransitLoss × Early (1842–1850)	0.1694***
	(0.0413)
TransitLoss × Late (1851–1860)	0.1041***
	(0.0281)
p-value: Early = Late	0.143
Observations	11,357

Notes: Frozen baseline panel, 1820–1860, opium-zeroed rebellion indicator, 60 treated counties. $\text{TransitLoss}_{ct} = \text{Treated}_c \times (1 - s_{\text{Canton},t})$ is interacted with an early-period dummy (1842–1850) and a late-period dummy (1851–1860). Controls follow Column (6) of Table 2; county and province × year fixed effects included. Standard errors clustered by county in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Binary Timing Decomposition

	(1) 1820–1860
Treated × Early (1842–1850)	0.0955***
	(0.0208)
Treated × Late (1851–1860)	0.0958***
	(0.0233)
p-value: Early = Late	0.989
Observations	11,357

Notes: Frozen baseline panel, 1820–1860, opium-zeroed rebellion indicator, 60 treated counties. The binary treatment is interacted with an early-period dummy (1842–1850) and a late-period dummy (1851–1860); the two coefficients are statistically indistinguishable, so the effect is not confined to the Taiping years. Controls follow Column (6) of Table 2; county and province × year fixed effects included. Standard errors clustered by county in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Endogeneity Robustness: Standardized Continuous Exposure, Pre-1851 Window

	Rebellion occurrence (0/1)
TransitLoss (standardized)	0.0308***
	(0.0083)
Observations	8,864

Notes: Frozen baseline panel, opium-zeroed rebellion indicator, 60 treated counties, restricted to the pre-1851 window (year ≤ 1851) to isolate the treaty-port transition from the Taiping period. $\text{TransitLoss}_{ct} = \text{Treated}_c \times (1 - s_{\text{Canton},t})$ is standardized to mean zero and unit standard deviation; the coefficient is the effect of a one-standard-deviation increase in continuous exposure. Controls follow Column (6) of Table 2; county and province × year fixed effects included. Standard errors clustered by county in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Covariate Balance: Propensity-Score Matching

Covariate	Standardized % bias (unmatched)
Terrain ruggedness	-28.9
County area	-11.5
Population in 1820	49.4
Market town density	12.5
Rice suitability	40.1
Wheat suitability	-7.7
Flood frequency (1830–41)	88.5
Drought frequency (1830–41)	-51.3
Pre-shock rebellion (1830–41)	1.9
Mean absolute % bias	32.4 $\rightarrow$ 8.4 (matched)

Notes: Frozen baseline panel. Propensity score from a logit of the Canton-route treatment (60 treated) on the nine covariates above. Matching is 1-to-1 nearest-neighbour, caliper 0.05, no replacement; 54 of 60 treated counties are matched to 54 controls. The % bias column reports the standardized treated−control difference before matching; the mean absolute standardized bias across covariates falls from 32.4 to 8.4 after matching.

Table A8: Propensity-Score-Matching Robustness: Baseline DID on the Matched Sample

	(1) Full sample	(2) Matched sample
Treated $\times$ Post-1842	0.0957***	0.0699***
	(0.0183)	(0.0226)
County FE	Yes	Yes
Province $\times$ Year FE	Yes	Yes
Four controls	Yes	Yes
Observations	11,357	4,428
Counties (clusters)	277	108

Notes: Frozen baseline panel, 1820–1860. Column (1) is the full 277-county sample; column (2) restricts to the 108 counties retained by 1-to-1 nearest-neighbour propensity-score matching (caliper 0.05, no replacement): 54 of the 60 treated counties are matched to 54 distinct controls. The six unmatched treated counties lie outside the common support. All columns include county fixed effects, province $\times$ year fixed effects, and the four baseline controls (drought, flood, ruggedness$\times$Post-1842, march-route$\times$Post-1851). Standard errors clustered by county in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Changes-in-Changes: Quantile Treatment Effects (IHS rebellion rate per 1820 population)

Quantile	QTE	Bootstrap SE	95% CI
Overall (ATE)	0.0699***	0.0155	[0.0395, 0.1002]
q5†	0.0271	0.0211	[−0.0142, 0.0684]
q10†	0.0193	0.0145	[−0.0092, 0.0478]
q25	0.0268***	0.0087	[0.0097, 0.0439]
q50	0.0405***	0.0058	[0.0292, 0.0518]
q75	0.0547***	0.0048	[0.0453, 0.0641]
q90	0.2020***	0.0196	[0.1637, 0.2404]
q95	0.2927***	0.0447	[0.2052, 0.3803]

Frozen baseline panel, 1820–1860, 60 treated and 214 control counties (274 counties, 11,234 county-years). Outcome is the inverse-hyperbolic-sine of the rebellion rate per 1820 population, as in Table 9. Changes-in-changes estimates follow Athey and Imbens (2006), implemented via Kranker's `cic` command; the Melly–Blaise variant used by Cao and Chen (2022) was unavailable, though the estimand is identical. County, year, and province-by-year fixed effects together with the Column (6) controls are partialled out in a first step; the changes-in-changes estimator is applied to the residuals. Because the outcome has a large mass at zero (86.0% of county-years record no incident), quantile treatment effects below the 25th percentile rest on sparse information and should be read with caution; estimates at and above the 25th percentile are significant, with the largest effects in the upper tail. Standard errors from 100 bootstrap replications. Stars: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

† Quantile treatment effects at q5 and q10 fall within the zero-mass region of the outcome distribution and are reported for completeness but not interpreted; the identified and interpretable range is q25 and above.

Table A10: Summary of Estimated Effects Across Methods (Frozen Baseline Panel, 60 Treated Counties)

Method	Outcome	Estimate (SE)	N	Counti
Two-way FE DID (Table 2, col. 6)	Occurrence (0/1)	0.0956*** (0.0183)	11,357	277
Two-way FE DID (Table 9, col. 6)	IHS rate	0.0368*** (0.0127)	11,234	274
PSM-DID, matched sample (Table A8)	Occurrence (0/1)	0.0699*** (0.0226)	4,428	108
Changes-in-Changes, overall ATE (Table A9)	IHS rate	0.0699*** (0.0155)	11,234	274
Synthetic DID, ATT, all donors (Table A11)	IHS rate	0.0488*** (0.0098)	11,234	274

All estimates are computed on the frozen baseline panel (1820–1860) with 60 treated counties. The binary two-way FE DID a the PSM-DID estimate the effect on the rebellion occurrence indicator, reported in probability units (percentage-point effect The IHS specifications (normalized two-way FE DID, changes-in-changes, and synthetic DID) use the inverse-hyperbolic-sine the rebellion rate per 1820 population; their coefficients are in IHS units and are therefore not directly comparable in level the binary estimates. Standard errors in parentheses are county-clustered for the DID and PSM-DID rows, from 100 bootstr replications for changes-in-changes, and placebo-based for synthetic DID. Across every method the estimate is positive a significant at the 1 percent level. Stars: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A11: Synthetic Difference-in-Differences: ATT on the IHS Rebellion Rate

	ATT	Placebo SE	Bootstrap SE	Jackknife SE	p	Treated/Con
v1: all donors (214)	0.0488***	0.0098	0.0122	0.0108	<0.001	60/214
v2: buffer 52 km (168)	0.0530***	0.0099	0.0112	0.0112	<0.001	60/168

Frozen baseline panel, 1820–1860, 60 treated counties; outcome is the inverse-hyperbolic-sine of the rebellion rate per 1820
DID follows Arkhangelsky et al. (2021), with treatment block-assigned at 1842. Given the pre-treatment window (1820–1841),
pre-treatment fit is imperfect (pre-period RMSPE ≈ 0.028, about 0.57 of the estimated ATT); I therefore report synthetic
evidence rather than primary inference. Synthetic DID is preferred over the classical synthetic control method because it i
pre-treatment panels.
v1 uses all 214 control counties as donors (N=11,234); v2 drops 46 controls within 52 km of the corridor, leaving 168 donors
errors are placebo, bootstrap, and jackknife, each from 100 replications where applicable. Stars: * $p < 0.10$, ** $p < 0.05$, **

Table A12: Commodity Composition: Tea vs Silk Corridor Exposure (1820–1850)

	(1)	(2)	(3)	(4)	(5)	(6)
FLR tea	0.459***	0.505***	0.504***	0.475***	0.505***	0.474***
	(0.137)	(0.132)	(0.132)	(0.129)	(0.132)	(0.129)
FLR silk	0.045	0.041	0.040	0.054	0.041	0.053
	(0.036)	(0.036)	(0.036)	(0.036)	(0.036)	(0.036)
Province × Year FE	No	Yes	Yes	Yes	Yes	Yes
Climate controls	No	No	Yes	No	No	Yes
Geography control	No	No	No	Yes	No	Yes
March-route control	No	No	No	No	Yes	Yes
p-value: FLR tea = FLR silk	0.004	0.001	0.001	0.003	0.001	0.003
Observations	8,587	8,587	8,587	8,587	8,587	8,587
Adjusted R^2	0.099	0.184	0.184	0.185	0.184	0.185

Notes: Frozen baseline panel, 1820–1850, opium-zeroed rebellion indicator, 60 treated counties. $\text{FLR}^k_{ct} = \mathbf{1}\{\text{commodity } k \text{ transits county } c\text{'s corridor}\} \times (1 - s^k_{\text{Canton},t})$; tea corridors comprise the Gan-river trunk with the Wannan and Wuyi feeders, silk corridors the trunk and lower-Yangzi approach. Hezhou, Nanling, and Qingyang are assigned to the tea corridor. All columns include county fixed effects; columns (2)–(6) add province × year fixed effects and the baseline controls in turn. Standard errors clustered by county in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A13: Commodity Composition, Full Window (1820–1860)

	(1)	(2)	(3)	(4)	(5)	(6)
FLR tea	0.160***	0.090**	0.090**	0.082*	0.077*	0.071*
	(0.050)	(0.042)	(0.042)	(0.043)	(0.041)	(0.042)
FLR silk	0.072*	0.061*	0.061**	0.071**	0.055*	0.064**
	(0.037)	(0.031)	(0.031)	(0.032)	(0.031)	(0.031)
Province × Year FE	No	Yes	Yes	Yes	Yes	Yes
Climate controls	No	No	Yes	No	No	Yes
Geography control	No	No	No	Yes	No	Yes
March-route control	No	No	No	No	Yes	Yes
p-value: FLR tea = FLR silk	0.253	0.641	0.635	0.865	0.718	0.910
Observations	11,357	11,357	11,357	11,357	11,357	11,357
Adjusted R^2	0.197	0.323	0.323	0.323	0.324	0.325

Notes: Frozen baseline panel, 1820–1860, opium-zeroed rebellion indicator, 60 treated counties. $\text{FLR}^k_{ct} = \mathbf{1}\{\text{commodity } k \text{ transits county } c\text{'s corridor}\} \times (1 - s^k_{\text{Canton},t})$; tea corridors comprise the Gan-river trunk with the Wannan and Wuyi feeders, silk corridors the trunk and lower-Yangzi approach. Hezhou, Nanling, and Qingyang are assigned to the tea corridor. All columns include county fixed effects; columns (2)–(6) add province × year fixed effects and the baseline controls in turn. Standard errors clustered by county in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

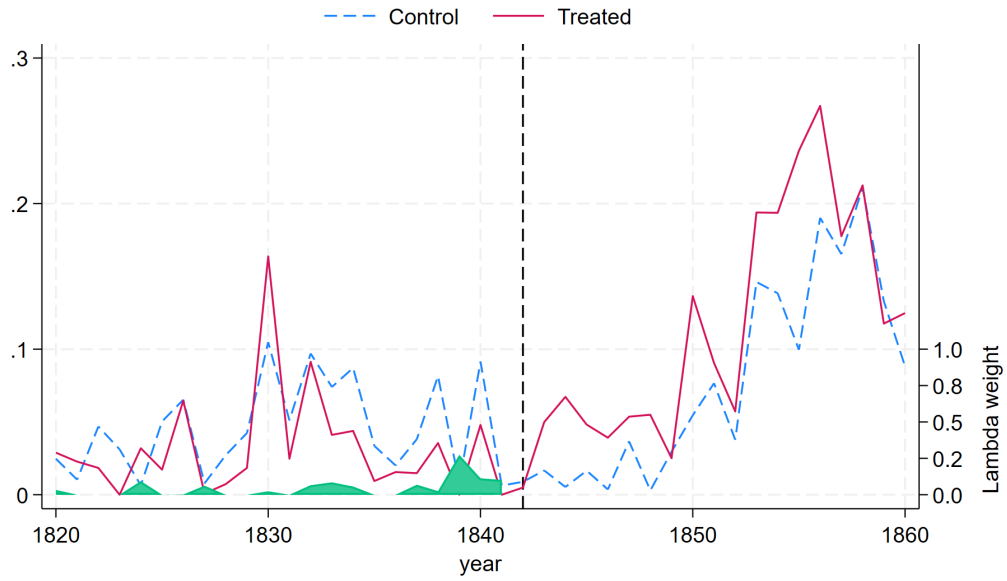


Figure A1: Synthetic Difference-in-Differences: Treated versus Synthetic Control Trajectories. The solid line is the corridor (treated) group and the dashed line is the synthetic control. The vertical dashed line marks the 1842 treatment date. The shaded area in the pre-treatment period and the right axis (*Lambda weight*) report the time weights selected by the synthetic difference-in-differences estimator.

Table A14: Continuous Exposure: Effect Scales with Canton Trade Collapse

	(1) 1820–1860
TransitLoss (continuous)	0.1148***
	(0.0259)
Observations	11,357
R-squared	0.3516

Notes: Frozen baseline panel, 1820–1860, 60 treated counties; the outcome is the opium-zeroed rebellion indicator, as in Table 2. $\text{TransitLoss}_{ct} = \text{Treated}_c \times (1 - s_{\text{Canton},t})$, where $s_{\text{Canton},t}$ is Canton's combined (tea and silk, volume-weighted) share of national exports, held at 1 through 1843 and declining thereafter; the series is set to 1 for 1820–1841 so TransitLoss is zero before the treaty. The specification follows Column (6) of Table 2. Standard errors clustered by county in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A15: Placebo Timing: Fake Treatment Dates on the Pre-Treaty Sample

	Placebo FakePost (1820–1841 sample)			Actual
	FakePost 1830	FakePost 1832	FakePost 1835	Post-1842
Treated × (Fake)Post	0.0209	-0.0034	-0.0158	0.0957***
	(0.0183)	(0.0209)	(0.0159)	(0.0183)
	[0.0260]	[0.0291]	[0.0293]	[0.0215]
p-value	0.255	0.872	0.323	0.000
Observations	6,093	6,093	6,093	11,356
Counties	277	277	277	277

Notes: Columns (1)–(3) restrict the sample to the pre-treaty period 1820–1841 and assign a placebo treatment date (FakePost = 1 in years at or after 1830, 1832, 1835). Each specification mirrors Column (6) of Table 2: county fixed effects, province × year fixed effects, climate controls, and ruggedness interacted with FakePost; the march-route control is omitted because it has no variation before 1851. Column (4) is the actual estimate (Treated × Post-1842) on the full 1820–1860 window. Cluster-robust standard errors (county) in parentheses; Conley spatial-HAC standard errors (500 km, 10-year) in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A16: Randomization Inference: Within-Province Permutation of Treatment

	Treated × Post-1842
Actual estimate	0.0957
Permutation mean	-0.0008
Permutation SD	0.0164
95% permutation interval	[-0.0324, 0.0320]
p (two-sided, $\lvert\beta_{\text{perm}}\rvert \geq$ actual)	0.000
p (one-sided, $\beta_{\text{perm}} \geq$ actual)	0.000

Notes: 1,000 placebo permutations (seed 12345). In each draw, within each province the same number of counties as the true treated set is randomly assigned as placebo-treated, and the Column (6) specification is re-estimated. The actual estimate sits at the 100.0 percentile of the permutation distribution.