

# The Emperor's Tael: Fiscal Centralization and Tax Revolts in Qing China, 1644–1912\*

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**Abstract:** Scholars have recognized the formation of state fiscal capacity as a prerequisite for long-term economic development. Yet a nationwide fiscal centralization reform by the Qing government (1723–1735) failed to achieve the same purpose. Instead, we find that the centralization surprisingly led to more tax revolts. More frequent revolts were observed in areas where the central government broke its promise to compensate for local governments' losses in recent years. Furthermore, we find no increase in public good provision post-reform. We thus highlight that a fiscal centralization reform can only succeed with credible commitments from the central government.

**Key Words:** Fiscal Centralization, Credible Commitments, Limited Government

**JEL:** N15; N45; H77

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*Since long before, huohao was collected by the local governments. Hence the governors and magistrates have an incentive to overtax in order to grab personal gains. To put an end to such practices, huohao should be collected by the upper governments and then redistributed to the local governments.*

**Imperial decree by Yongzheng in 1727**

*Huohao should never be used for purposes other than maintaining offices of local governments. In particular, it must not be appropriated by the upper governments for other purposes.*

**Imperial decree by Yongzheng in 1729**

*Huohao can be used by the upper governments for various purposes in case of emergencies, as long as it was reported to the central governments beforehand.*

**Imperial decree by Qianlong (Son and successor of Yongzheng) in 1744**

## **1. Introduction**

Many scholars have recognized the formation of state fiscal capacity as a prerequisite for long-term economic development (e.g., Acemoglu *et al.*, 2001; 2016; Besley and Persson, 2011; DeLong and Shleifer, 1994; Dincecco and Katz, 2014; Johnson and Koyama, 2017; North and Weingast, 1989). Strong fiscal capacity was normally achieved through fiscal centralization, which increased the flow of fiscal revenues to the central government and paved the way for eventual economic growth (Dincecco, 2009; Epstein, 2000). Comparative historical studies have provided support for this view using case studies such as England before the Glorious Revolution (Koyama and Johnson, 2014; O'Brien, 1988), Prussia in the late 18<sup>th</sup> and early 19<sup>th</sup> centuries (Kiser and Schneider, 1994), and Japan during the Meiji Restoration (He, 2013).

The Qing government in China initiated a similar nationwide fiscal centralization reform in the mid-18<sup>th</sup> century. However, it failed to modernize the nation: China stagnated in a Malthusian trap with low per capita income, urbanization rate, and industry growth until

the early 20th century (Broadberry and Guan, 2018; Chen and Kung, 2016). The reform was called the “*huohao* submitted to the public” during Emperor Yongzheng’s reign (1723–1735). *Huohao* refers to all informal levies that local governments collected to maintain their offices and perform their duties (Brandt *et al.*, 2014; Ch’ü, 1962). Prior to the reform, *huohao* was not subject to central government supervision; thus, local officials tended to over-collect for personal gains, and the size of *huohao* exceeded 50% of formal taxes (Ma, 2012; Zelin, 1984). The goal of the centralization reform was to formalize *huohao*: it would be handed to the central government, which would then make transfer payments (*yanglianyin*, or “anti-corruption salaries”) to the local governments, while local governments were under stricter supervision to prevent them from extralegal levies. The key to the reform was a promise made by the emperor, that *huohao* collected by the central government must be redistributed *in full* to the local governments to maintain their offices and provide public services. In this regard, the reform was designed to give the central government a dominant role in distributing fiscal resources without changing the total tax burden (Chen, 2008), and to restrain the local officials to grab from the society (Zelin, 1984). Did the reform restrain the local grabbing hands? Why did the reform fail to foster long-term economic development as in other Eurasian countries? This paper seeks to answer these questions.

In particular, we construct a panel dataset covering 226 prefectures and 268 years (1645–1912) to explore the impact of the Qing fiscal centralization. Among the 226 prefectures, 168 were reformed areas when the imperial order was released in 1723, and 58 were unreformed. The reformed and unreformed areas featured no systematic socio-economic differences, thus allowing us to employ a difference-in-differences (DID) strategy in our analysis. First, we use the number of local tax revolts retrieved from *Qing Shilu* (Veritable Records of the Qing Emperors) as a proxy for tax burden, and show that local tax revolts significantly increased post-reform. The result is robust to including a series of control variables and considering space correlation. We also use a parallel trend test to validate the DID strategy. Moreover, we conduct a robustness check to show that non-tax revolts did not feature a similar surge after the reform. Collectively, the findings exhibit that the well-intended fiscal centralization failed to restrain the local grabbing hands from extralegal levies.

Next, we explore the reasons behind the failure. As mentioned, the key to the reform's success hinged on the imperial promise of returning transfer payments to the local governments, which was stressed repetitively by Emperor Yongzheng, as shown in the epigraph. However, Emperor Qianlong, Yongzheng's son and successor, no longer honored this promise (Zelin, 1984). In the eighth year of his reign (1744), Qianlong approved an appeal to make inter-province transfers of *huohao*, and the appropriation of *huohao* gradually became common practice. We argue that the central government's failure to provide credible commitment not to appropriate the transfer payment led to the more aggressive local grabbing hands. This is because when upper-level governments centralized the funds but failed to commit to the transfer payments, local governments, with limited bargaining power facing the absolutist imperial ruler, had no choice but to procure additional resources from society to fund their functions, which consequently led to more tax revolts. Specifically, we use whether the central government had appropriated the transfer payment in the prior years as a proxy for local governments' expectations of the central government's promise-keeping capacity. We find that, the local tax revolts intensified in regions where the central government was found appropriating the transfer payments in the recent two years.

Lastly, we investigate the long-term impact of the reform on local economic development. Existing literature points to fiscal centralization's positive impact on public goods provision in the short run, such as famine relief (Lindert, 2004), and in the long run, such as primary education (Cinnirella and Schueler, 2018). Echoing the literature, we use disaster relief as a proxy for short-run public goods provision, and use the number of newly established public academies in the locality as a proxy for long-run public goods provision: we find no enhanced local public goods provision post-reform, which, again, add to the inefficacy of the reform. In addition, the literature suggests that fiscal centralization enhances privately-supplied public goods provision by restraining local grabbing hands, thus creating a more vibrant civil society (Frye and Shleifer, 1997; Olson, 1998, 2000). We therefore use the number of newly founded private academies – the ones built by local gentries in pre-modern China – as a proxy for public goods supplied by the civil society. We find no increase in the number of private academies post-reform, which implied no improvement in local tax burden relief. Lastly, we use population density as a proxy of economic development, and show that the reformed area featured

no better local economic growth.

This paper makes three contributions to the literature. First, our empirical findings highlight that the lack of credible commitment accounted for the failure of centralization reforms in pre-modern China. As pointed out by North and Weingast (1989), institutional changes during the Glorious Revolution of 1688 enabled the English Crown to make a credible commitment to responsible fiscal policies, which was the key to building state capacity and promoting economic growth. Dincecco (2009) and Dincecco and Katz (2014) provide empirical evidence from Europe from the 17<sup>th</sup> to the 19<sup>th</sup> century that endeavors to build limited governments, marked by parliamentary checks on the ruler's discretion, provided an institutional foundation for the success of fiscal centralization. By contrast, pre-modern China had no such institutional constraints on its rulers and government (Ma and Rubin, 2017).<sup>1</sup> Thus when the central government broke its promise to make transfers to local governments, local leaders did not negotiate with the ruler. Instead, they extracted resources from local residents.<sup>2</sup> In this sense, the unlimited government in pre-modern China provides valuable parallel evidence consistent with the European experience.<sup>3</sup>

Second, although most prior research highlights the importance of making credible commitments in the formation of state capacity, most empirical studies did not directly measure the degree to which such commitments were fulfilled. They indirectly assessed commitment as the presence of effective institutional constraints that limited the ruler's discretion over public expenditures.<sup>4</sup> Yet considering institutional constraints alone did

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<sup>1</sup> In this paper, the meaning of "lack of constraints on government power" is twofold: first, from the perspective of local governments, there were no constraints on the central government and the ruler; second, from the perspective of society, there were no constraints on any level of government.

<sup>2</sup> Unlike North and Weingast (1989), who view the state as a whole, we examine the state and discuss how a change in the central-local relationship affected the state-society relationship.

<sup>3</sup> Other studies have used this approach, including Bates and Lien (1985); Cox (2016); Dincecco (2011); Dincecco et al. (2011); Hoffman and Rosenthal (1997); Levi (1988) and Timmons (2005).

<sup>4</sup> For example, Dincecco (2009) codes a state as being under a "limited government" if (1) parliament has the authority to veto the budget and (2) parliament's power of the purse had to hold for at least two consecutive decades from that year. In a recent study, Garfias (2018) highlights the power of local mine owners who organized corporate enterprises to limit the power of government in colonial Mexico.

not solve the commitment problem (Stasavage, 2003).<sup>5</sup> A direct measure of commitment may better capture agents' behavior under these institutional constraints. We directly measure "commitment failure" in Qing China as incidents involving the central government's failure to make transfer payments to the locals in recent years, which changed the locals' expectations of the current year's promise-keeping. An incidence in which the central government failed to make such a transfer should be interpreted as a lack of institutional constraint on the central government to keep its commitment. Thus, to the best of our knowledge, our study is the first to explore the impacts of "commitment failure" following centralization on local government behavior.

Third, to explain the great divergence between China and the West, many scholars focus on social and economic factors such as demography, natural resources, market integration, market access, innovation, diffusion of technology, law system, and geopolitics (Broadberry and Gupta 2006; Clark, 2007; Goldstone, 2004; Jones 1981; Landes 2006; Lucas 2002; Ma, 2011a; Mokyr, 2007; North *et al.*, 2009; Pomeranz 2000; Pritchett 1997; Rosenthal and Wong 2011; Shiue and Kellor, 2007; Sng and Moriguchi, 2014; Voigtländer and Voth 2006, 2013). Our work provides a fresh new contribution to the literature by focusing on the design of political institutions concerning the relationship between central and local governments. Although many scholars argue that pre-modern China developed a system of meritocratic recruitment and competitive promotions to enhance bureaucratic performance (Chen and Kung, 2018; Huang, 2002; Liu, 2005; Toynbee and Somervell, 1987; Weber, 1915), our findings highlight that in the absence of institutions that limit executive discretion, high-quality and career motivated political elites do not necessarily guarantee an efficient government and economic growth.<sup>6</sup>

The rest of the paper is organized as follows. Section 2 briefly overviews the historical

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<sup>5</sup> Stasavage (2003) finds that only when the Whigs controlled the English parliament after 1715 did the creditors believe the loans would be repaid.

<sup>6</sup> As pointed out in The Federalist Papers, "The aim of every political Constitution, is or ought to be, first to obtain for rulers men who possess most wisdom to discern, and most virtue to pursue, the common good of society; and in the next place, to take the most effectual precautions for keeping them virtuous whilst they continue to hold their public trust" (Madison, 1788 in *Federalist Papers* #57).

background. Section 3 describes the data sources. Section 4 introduces the empirical strategy and reports the results. Section 5 discusses the mechanism of the results. Section 6 investigates the long-term impact of the reform, while Section 7 concludes.

## 2. Background

### 2.1 The Fiscal Centralization Reform in Qing China

Since its inception, the Qing's revenues and expenditures were highly decentralized. On the revenue side, the formal taxes submitted to the central government only accounted for 2% of the national income – far less than the formal taxes retained by the local governments and the informal levies collected by them (Brandt *et al.* 2014). On the expenditure side, local governments were responsible for maintaining their offices and providing public services in their jurisdictions. In practice, the formal taxes retained by the local governments were far from sufficient to fulfill their duties. They normally did not bargain with the center to ask for transfer payments; instead, they raised various informal levies, called *huohao*. This term was originally used to denote the surtax introduced to cover the physical wear of silver during the collection of silver taxes, and normally constituted less than 10% of the formal tax. It was later used to refer to the 40–50% (or even higher) surplus of taxes collected by local governments on top of the formal taxes (Wang, 1973; Zelin, 1984). Most of this excess became part of their informal revenues; only a small portion of *huohao* was used to provide public goods. Since *huohao* was not subject to central government supervision, local officials had an incentive to over-collect to obtain illicit personal gain (Brandt *et al.*, 2014).

The third Qing emperor, Yongzheng, introduced efforts to centralize (1723–1735). His nationwide reform attempted to grant the central government the dominant role in distributing fiscal resources with no change in the total tax burden. This goal was achieved by submitting *huohao* to the central government in most provinces, and making transfer payments (*yanglianyin*) to the local governments to compensate for their losses. The key promise made by the emperor was that *huohao* collected from a particular province must be redistributed *in full* to the local governments within that province to maintain their offices and provide public services. Local officials were under stricter

supervision to prevent them from collecting extralegal levies.

According to Yongzheng's design, *huohao* could *not* be used to satisfy the financial needs of the central government and other provincial governments (Imperial decree by Yongzheng in 1729, as shown in the epigraph). The reform represented an attempt toward fiscal centralization in two respects. First, the reform aimed solely to re-structure central-local fiscal distributions without other state-building initiatives such as recruiting better talents or training incumbent bureaucrats. Second, before the reform, the local governments financed their own expenses using informal levies that they collected; after the reform, the central government redistributed funds to the local governments to finance their expenses. Meanwhile, the central government – not the local governments – set the rate of informal levies after the reform (Chen, 2008). The reform started in 1723 when these unreformed regions continued using *huohao* to finance local governments' expenses. Figure 1 illustrates the implementation of the reform, where the darker grey regions were the reformed areas, and the light grey regions were unreformed.

**Figure 1: Implementation of *Huohao* Reform**



**Source:** Xue (1984) and Zelin (1984)

## 2.2 The Broken Promises

After the reform, local governments were less able to extract illegal taxes because they lost their residual claims over *huohao*. According to a report submitted to the emperor in 1735, three high-ranking central officials observed that after the reform, local officials stopped collecting informal levies (Zelin, 1984). In the meantime, by design, the transfer payments from the central government enhanced local governments' ability to provide public goods, which generated an increase in public goods provision in subsequent decades (Hao and Liu, 2019). The key to the success of centralization in the short run appears to be the fact that the central government kept its promise to make transfer payments.

However, Emperor Qianlong, Yongzheng's son and successor, no longer honored this promise (Zelin, 1984). In the eighth year of his reign (1744), Qianlong approved an appeal to make inter-province transfers of *huohao*,<sup>7</sup> and the appropriation of *huohao* gradually became common practice. Many cases involved using *huohao* to finance projects that the central government should have funded, such as military missions (Zelin, 1984). This led to a situation where local governments did not fully receive their transfer payments. After the 1760s, transfer payments were appropriated much more frequently. The appropriation completely changed the dynamics of the central-local interactions. On the one hand, local governments had to continue submitting *huohao* to the central government. On the other hand, the shorted transfer payment urged the local governments to explore alternative ways to finance their daily activities. With limited bargaining power facing the absolutist imperial ruler, local governments had no choice but to procure additional resources from society. The extra burden was thus transmitted to the final victims of centralization: the general public. Without institutional limits on local government's power, they extorted from society to remedy their losses.<sup>8</sup>

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<sup>7</sup> An Imperial decree of 1744 dictated that upper-level governments could use *huohao* for various purposes in emergencies, as long as they reported it to the central government (as shown in the epigraph).

<sup>8</sup> At the peak of post-reform appropriation (1760s), more extralegal levies appeared, with substantial numbers in openly selling vacant public offices. For instance, Zheng Yuanshu, the commissioner of Hunan province under Emperor Qianlong, was notoriously known for selling public offices, averaging for 10,000 taels of gold per position.

Consequently, we have the first prediction for empirical investigation: *the tax burden on local residents became heavier after the fiscal centralization reform.*

As shown, Emperor Qianlong's reneging on his father's promise was the key mechanism to understand the failure of the well-intended fiscal centralization reform. Furthermore, the broken promise by the imperial power was not surprising in itself. Many scholars have argued that limited governments, marked by parliamentary checks on the ruler's discretion, provided an institutional foundation for the success of fiscal centralization (Dincecco, 2009; Epstein, 2000; Hoffman and Norberg, 1994; Stasavage, 2007). By contrast, pre-modern China's central government had no institutional constraints on its power over local governments: a successor could easily break the promises made by his predecessor. Local governments could likewise easily extract from society when they observed the lack of credible commitment in the central government. This leads us to the second prediction for the empirical investigation that *the increased tax burden of local residents was heavier in regions where local governments had higher expectations of the central government's promise-breaking.*

Lastly, in terms of policy impact, the Qing fiscal reform failed to substantially raise central fiscal revenue as a share of GDP in the long run (Ma, 2011b; Zelin, 1984). Local governments were still locked in a decentralized system in which they financed their own expenses by collecting informal levies. Consequently, and echoing the evidence in the literature,<sup>9</sup> we thus predict that *local public goods provision in the reformed area would not improve in the long run.*

In the following sections, we empirically test these predictions.

### 3. Data

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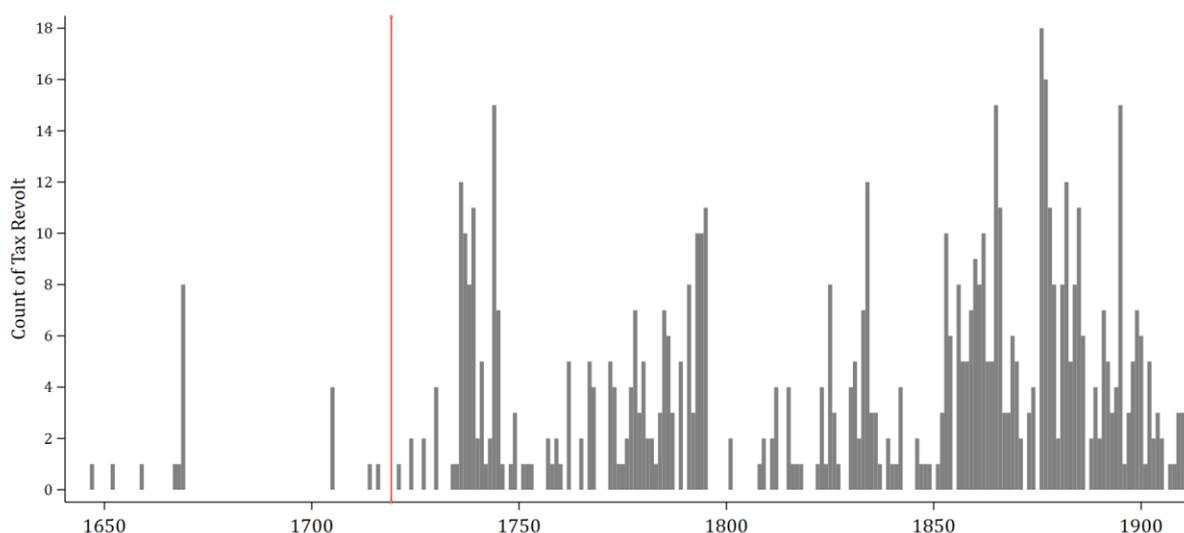
<sup>9</sup> According to a series of wage data for hired unskilled labor, which comes from 178 cases of debt disputes during 1735–1842 (Chen, 2011), the real yearly income would be able to buy around 200 kilos of rice, just enough to support a rural household for a year. Scholars also use urbanization to measure economic prosperity in the premodern world (Acemoglu *et al.*, 2002, 2005; Bairoch, 1988; De Vries, 1976; Nunn and Qian, 2011). In the case of China, Cao (2001) estimated that the urbanization rate dropped slightly from 7.4% in 1776 to 7.1% in 1893. This figure further declined to 4.3% in the 1920s, according to a survey by Stauffer (1922).

We constructed a panel dataset from a number of historical sources spanning 1644–1912 and covering 226 prefectures, which allows us to empirically test the effect of the reform on Tax burden and whether such effect could be traced back to the lack of credible commitment. The information on the reformed and unreformed areas is retrieved from Xue (1984) and Zelin (1984). Furthermore, we intend to investigate the long-term impact of the reform on economic development and public goods provision. Below we introduce the related data and variables.

### 3.1 Data Descriptions and Sources

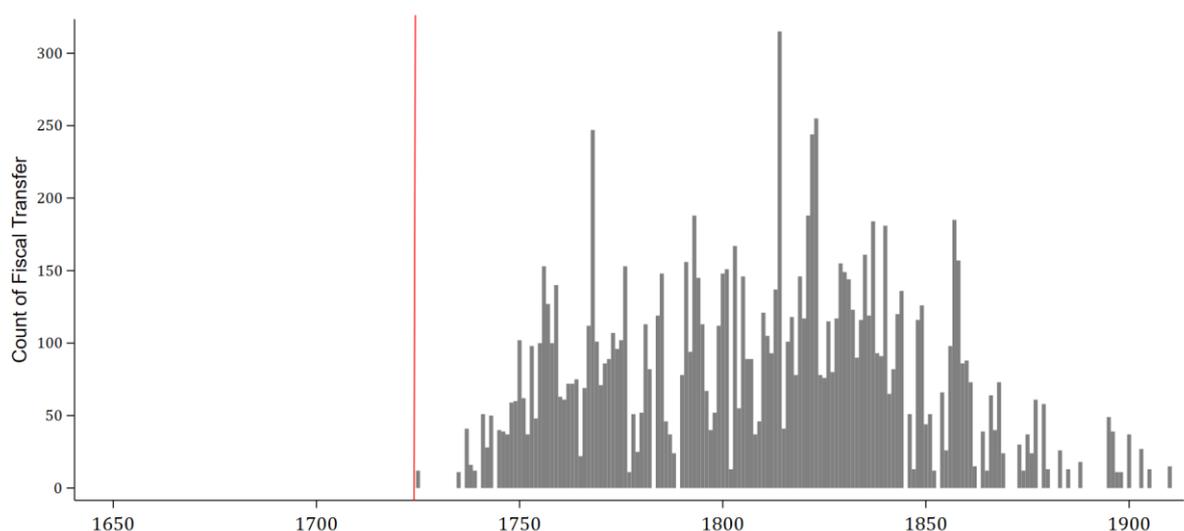
**Tax burden.** We use the number of tax revolts as a proxy for the tax burden, which is not directly available. A tax revolt is the natural consequence of tax over-burden inflicted by local governments on ordinary residents whose burden exceeded the maximum level they can endure. We collect this series of data from *Qing Shilu* (Veritable Records of the Qing Emperors), the official record of imperial edicts, and official memorials about events of national significance. According to Chinese historians, *Qing Shilu* is the most complete and systematic source of original information on social unrest during the Qing dynasty (Yang, 1975). The Qing meticulously compiled detailed records on the place and time of tax revolts. Figure 3 displays the time trend of tax revolts: on average, the frequency of revolts in all prefectures increased from 0.35 per year in the two decades before the reform (1703–1722) to 2.95 per year in the two decades after the reform (1723–1742).

**Figure 3: Time Trends of Tax Revolts**



**Central Government’s Commitment.** We use whether the central government had appropriated the transfer payments (*yanglianyin*) to the local governments in the prior two years as a proxy for the central government’s commitment. A case of recent appropriation signals a commitment failure, thus changing the local officials’ expectations on whether the commitment would be honored this year. The cases of appropriation of *yanglianyin* were collected by searching for relevant keywords in the titles of governors’ reports to the Ministry of Finance. These documents were accessed from the website of the *1<sup>st</sup> Chinese Historical Archive*. Figure 4 shows the time trend of appropriation in all provinces, which displays an inverse U-shape: it increased over time, peaked in the last decade of the 18<sup>th</sup> century, and declined thereafter.

**Figure 4: Time Trend of Appropriations of Transfer Payment**



**Public Goods Provisions and Economic Development.** We intend to evaluate the long-term impacts of the reform through local public goods provisions and economic development. First, we use three metrics to evaluate public goods provisions: disaster relief, the provision of education, and charity. Disaster relief was the most important public service provided by local governments in pre-modern China. The data also comes from *Qing Shilu*, which provides detailed records on the local governments’ places and times of disaster relief. Education provision is measured by the number of newly founded public academies and private academies established by local gentries. The data comes from *the Glossary of private academies in historical China* (Ji, 1996). Lastly, we measure local economic development by population density, a widely accepted measure for pre-

modern economies in the literature (Li, 1997; Acemoglu et al., 2002; Clark, 2007; Campbell and Lee, 2008; Chen et al., 2010; Jia, 2014a; Chen, 2015). The population density data is retrieved from Cao (2020). Table 1 reports the summary statistics of all these variables.

**Table 1: Statistical Summary**

Variables	Mean	Standard deviation	Number of observations	Data sources
Tax revolts	0.011	0.122	60,342	A
Appropriation of transfer payments	0.202	0.624	60,342	B
Famine relief	0.329	1.670	60,342	A
Public academies newly founded	0.256	0.658	59,808	C
Private academies newly founded	0.151	0.579	59,808	C
Population density (ln)	4.216	1.116	60,342	D

**Data sources:** A. *Qing Shilu* (Veritable Records of the Qing Emperors); B. reports of governors to the Ministry of Finance accessed from 1<sup>st</sup> Chinese Historical Archive; C: Ji (1996); D: Cao (2020).

### 3.2 Descriptive Evidence

Table 2 illustrates the difference in the number of tax revolts and other socio-economic measures, such as the numbers of newly founded public and private academies, population density, and extreme weather, between reformed and unreformed areas before 1723. Columns 1-2 show the means of the two areas, respectively, and Columns 3-4 show the difference and the *p*-value. As exhibited in the table, the two areas feature no systematic differences in all the measures, and thus are generally comparable.

**Table 2: Tax Revolts in Reformed vs. Unreformed Areas**

	Reformed Areas (1)	Unreformed Areas (2)	Difference (3)	<i>p</i> -value (4)
Tax revolt	0.001 (0.004)	0.001 (0.004)	-0.000 (0.005)	0.954
Extreme weather	-0.002 (0.007)	0.003 (0.008)	-0.005 (0.008)	0.000
Public academies newly founded	0.005 0.009	0.004 0.011	0.001 0.010	0.581
Private academies newly founded	0.001 0.003	0.001 0.004	0.000 0.004	0.808
Population density	0.003 0.002	0.003 0.004	-0.000 0.004	0.832

**Note:** \*, \*\*, and \*\*\* denote significance at the 90, 95, and 99 % levels, respectively

## 4. Empirical Strategies and Results

This section estimates the impact of the reform on a series of outcomes. Our baseline estimation follows the standard DID strategy, where we compare the relative change in the number of tax revolts in the reformed prefectures (treated group) versus the unreformed prefectures (control group). Among the 226 prefectures, 168 were reformed areas when the imperial order was released in 1723, and 58 were unreformed. We estimate the following equation as our baseline specification:

$$Y_{it} = \alpha Treat_i \times Post_t + \beta Disaster_{it} + prefecture_i + p_t + \varepsilon_{it} \quad (1)$$

where  $i$  indexes prefectures and  $t$  indexes years. The outcome of interest, denoted by  $Y_{it}$ , is the number of tax revolts recorded in prefecture  $i$  in year  $t$ . The key explanatory variable of interest is  $Treat_i \times Post_t$ , where  $Treat_i$  is a dummy variable that equals 1 if prefecture  $i$  had reformed, and 0 otherwise.  $Post_t$  is a dummy variable that takes the value of 1 only after 1724, when the reform occurred. The parameter of interest in Eq. (1) is thus  $\alpha$ , which measures the impact of the reform on the number of tax revolts during 1644–1912. The control variable is  $Disaster_{it}$ , a dummy variable that equals 1 if there was a drought or flood in prefecture  $i$  in the year  $t$ , and 0 otherwise. As befits a fixed-effects model,  $prefecture_i$  captures the time-invariant regional characteristics for prefectures  $i$  that may be associated with the reform, whereas  $p_t$  controls for the temporal effects in our estimation.  $\varepsilon_{it}$  is the error term.

The baseline results are presented in Table 3, where “Reform” refers to the key explanatory variable of interest,  $Treat_i \times Post_t$ . Column 1 reports the simple DID estimation, including both year and prefecture fixed effects. The results show that the reform had a positive and significant effect on tax revolts. After the reform, tax revolts increased by a frequency of 1.1 times during the sample years. When extreme weather events were recorded, grain production would decline, and tax revolts could occur even without a rise in the tax burden; Column 2 shows that our results are robust to controlling

for dummies indicating extreme weather.<sup>10</sup> The distribution of tax revolts may be a count variable with a large share of zeros, according to the summary statistics in Table 1. Therefore, in Column 4, we re-estimate the results using a negative binomial model. The results show that the reform is positively and significantly correlated with tax revolts, at 1% significance level. We further calculate the marginal effects to be 0.018, broadly consistent with the estimate in Column 2.

To account for space self-correlation, Columns 1–3 also report the Conley standard errors (in brackets).<sup>11</sup> To further control for potential endogeneity resulting from space self-correlation, in Column 4, we adopt the generalized spatial two-stage least squares (GS2SLS) method and find that the estimated coefficients are close to our baseline, indicating that the effects of the tax reform are robust to considering space self-correlation.<sup>12</sup>

To further allay concerns that our results may be driven by a particular prefecture or time period, we re-estimate the specification in Column 3 by dropping a prefecture each time. We did the same for the time period. The results, reported in Appendix Figures A1 and A2, show that our estimations are robust to this exercise.

Next, we consider a key alternative explanation to our story: the increased tax revolts were due to the confounding socio-economic turbulence other than the fiscal tax reform highlighted in this paper. To rule out the alternative explanation, we hypothesize that the reform should have limited impacts on *other* non-tax revolts. To test the hypothesis, we collect data of non-tax peasant revolts, also from *Qing Shilu*, and present the results in Column 5 of Table 3. As shown, the reform had no significant impact on peasant revolts,

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<sup>10</sup> Ho (1959), Bai and Kung (2011), and Jia (2014) identified extreme weather as an important social destabilizer in premodern China. We divide extreme weather into extreme drought and extreme flood, and find that extreme drought significantly boost the probability of tax revolts, whereas extreme flood had no such impact.

<sup>11</sup> Conley (1999) standard errors adjust for potential spatial interdependence of observations. Typically, spatial independence is assumed to decrease in the distance between two observations. Since prefectures are relatively small spatial units, there is complete independence for prefectures that are two degrees apart. We also tried other cutoff values (1, 3, 4 and 5 degrees), and the results stayed the same.

<sup>12</sup> As shown in Figure 1, the implementation of the reform featured some provincial pattern. Therefore, we re-estimate Column 2 in Table 3 using provincial data, and report the results in Table A1. Our baseline results remain robust.

thus confirming our baseline story.<sup>13</sup>

**Table 3: Centralization Reform and Tax Revolts**

	(1)	(2)	(3)	(4)	(5)
	Tax revolt	Tax revolt	Negative binomial	GS2SLS	Non-tax revolt
Reform	0.012** (0.005)	0.012** (0.005)	1.721*** (0.117)	0.043** (0.020)	0.746 (0.911)
Extreme weathers		-0.001 (0.001)	0.172* (0.093)	-0.003 (0.003)	0.218 (0.154)
Constant	0.005*** (0.002)	0.006* (0.002)	-5.830*** (0.115)	0.006 (0.024)	3.340*** (0.683)
Prefecture fixed effects	YES	YES		YES	YES
Year fixed effects	YES	YES		YES	YES
Number of prefectures	226	226	226	226	226
Number of observations	60,342	60,342	60,342	10,848	8,467
R <sup>2</sup>	0.094	0.094		0.212	0.280

**Notes:**

1. \*, \*\*, and \*\*\* denote significance at the 90, 95, and 99% levels, respectively.
2. Model 5 applies the GS2SLS procedure developed by Kelejian and Prucha (1998; 1999; 2004), which uses exogenous factors and their spatial lags as instruments for endogenous tax reform. We aggregate our data from yearly to five-year periods. Otherwise, the number of observations would be too numerous to estimate.

To further interpret our empirical results, we use the following three steps to quantify the reform's contribution to tax revolts during the sample year (1723–1912). First, we calculated the change in the share of prefectures that had completed the tax reform during the sample years. 168 out of 226 prefectures had completed the reform, representing a change of 74.3%, or 0.743. Second, we multiply our estimated coefficient (0.012) by 0.743, and get the estimated change in the frequency of tax revolts per year led by the reform (0.009). Third, we divided this change by the sample average of the yearly change in tax revolts (0.048), and obtained the share of change in tax revolts that can be explained by the change in reform status, 18.6% (0.009/0.048).

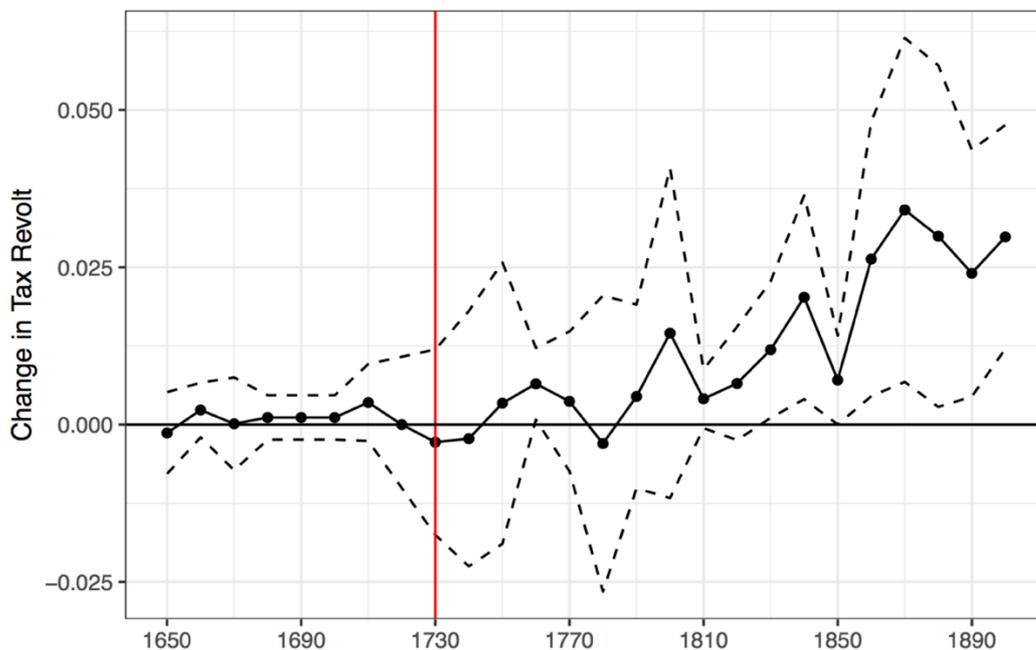
Meanwhile, a crucial assumption for our DID analysis is that there was no different pre-trend between the reform and non-reform areas. We thus perform a validity check as

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<sup>13</sup> In the previous section we mentioned that the emperor enforced stricter supervision and punishment on local officials in the reform regions to prevent them from collecting extralegal levies (Zelin, 1984). We believe that taking this into account would not change our results: stricter supervision (and punishment) would reduce tax revolts, our dependent variable. Since our baseline results report that reform had a positive and significant impact, neglecting this variable would only lead to underestimating the impacts.

follows. We code every ten years as an interval and plot the results in Figure 5, which suggests that there is little systematic difference in the tax revolt trend between the reformed and unreformed areas prior to 1723. This suggests that the reform was exogenous to the social and economic conditions of a region. After the reform, the frequency of tax revolts was upward trending, with a more significant increase in longer terms. We argue that this is because, in the long run, when the local governments learned not to trust the central government’s promise of non-appropriation, they began to procure to finance their expenses more aggressively, which eventually led to a heavier tax burden and increased tax revolts. We test this hypothesis in the next section.<sup>14</sup>

**Figure 5: Number of Tax Revolts before and after Reform**



**Note:** The solid line indicates changes in the number of tax revolts conditional on prefecture fixed effects and period fixed effects. The dotted lines indicate 95% confidence intervals. Standard errors are clustered at the province level.

## 5. Reform without Commitment

This section examines whether the increased tax revolts after the fiscal reform were due

<sup>14</sup> A few scholars find that fiscal reform in the 1850s shifted the major fiscal source from agricultural taxes to commercial taxes (*lijin*) (Luo, 2010; He, 2013). Some may also worry that the Taiping Rebellion around the same time could alter the results. To exclude the potential impacts from this structural break and other confounding factors, we redo our main estimation without the data after 1850 and the results remain robust.

to the broken promises, i.e., the appropriated transfer payments. The idea is as follows. The commitment to fully returning the transfer payments helped form local governments' expectations. If local governments *expected* the central government to break the promise, then the local officials had to increase extralegal levies to compensate for their losses, leading to increased tax burdens, and thus more tax revolts. We use the average annual count of transfer payment appropriation in the recent two years for a locality as the proxy of the local government's expectation of central commitment. If the central government had frequently appropriated the funds in the recent years, then the local government should have a lower expectation for the central government to honor the promise this year, and thus have higher incentives to increase local extralegal levies, which then lead to more tax revolts. Our econometric specification is as follows:

$$Y_{it} = \alpha Treat_i \times Post_t + \gamma_1 Treat_i \times Post_t \times Appropriation_{it} + \gamma_2 \times Appropriation_{it} + \beta Disaster_{it} + prefecture_i + p_t + \varepsilon_{it}$$

Table 4 reports our findings. Column 1 only includes year fixed effects, and Column 2 includes both year and prefecture fixed effects. Column 3 further controls for extreme weather dummies. All estimations are reported with standard errors clustered at the province level using the wild cluster bootstrap method. Results in all columns confirm that more frequent appropriations intensified local tax revolts, thus validating our story that the broken commitment led to more local extralegal levies, and, consequently, more tax revolts.

**Table 4: Centralization Reform and Appropriation of Transfer Payments**

	(1) Tax revolt	(2) Tax revolt	(3) Tax revolt
Reform	0.015*** (0.004)	0.009** (0.004)	0.009** (0.004)
Reform*Appropriation	0.005** (0.002)	0.005** (0.002)	0.005** (0.002)
Appropriation	-0.002*** (0.000)	-0.001 (0.001)	-0.001 (0.001)
Extreme weathers			-0.001 (0.001)
Constant	0.002 (0.002)	0.005** (0.002)	0.006** (0.002)
Prefecture fixed effects	NO	YES	YES
Year fixed effects	YES	YES	YES
Number of prefectures	226	226	226
Number of observations	59,890	59,890	59,890
R <sup>2</sup>	0.081	0.094	0.095

**Note:** \*, \*\*, and \*\*\* denote significance at the 90, 95, and 99% levels, respectively

There are two interesting observations from the results in Table 4 concerning the learning process of local government officials. First, upon the inception of the reform, the local officials could learn from various resources, from local anecdotes to imperial decrees, about the execution of the reform, particularly whether the central government honored the commitment. They changed their actions correspondingly when they observed openly broken promises, such as the contradicting decrees in the epigraph. To any local government officials, recent promise-breaking was one of the strongest signals they could base their actions upon, which was well-reflected by the intensification effects of prior appropriation in Table 4.

Furthermore, as shown in all three columns, the positive effects of reform on tax revolts remained significant. This shows that even the local officials who did not observe recent promise-breaking continued to believe that the central government could break the promise on any given day. Thus one must prepare for it by grabbing some fortunes *today*. The rationale echoed perfectly with the fundamental lack of commitment in the institutional design: when policies could change overnight, when the father's imperial decree could be replaced with an opposite one for the son's arbitrarily, none would be trusted.

## **6. The Long-term Impacts of the Reform**

This section investigates the long-term impacts of the reform on both public goods provision and economic development. In a comparative context, successful fiscal reforms were often accompanied by better public goods provisions and economic development in two ways. First, fiscal centralization helps overcome the problems of local governments—which have short-term visions and free ride on others—to pool resources to invest in public goods that have large spillovers over time and space. Examples include national defense (Gennaioli and Voth, 2015; Hoffman, 2007), primary education (Cinnirella and Schueler, 2018), famine relief and poverty relief (Lindert, 2004), and transportation infrastructure (Bogart, 2010; Tang, 2014). Similarly, if the Qing centralization were successfully accompanied by transfer payments to the local government, we expect the local governments to be more capable of providing public goods both in the short and long run. Echoing the literature, we use disaster relief as a

proxy for short-run public goods provision, and use the number of newly established public academies in the locality as a proxy for long-run public goods provision, to see whether local public goods provision was enhanced post-reform. Columns 1-2 in Table 5 present the results after controlling for extreme weather, prefecture, and year fixed effects. Column 1 shows that local government post-reform did not provide better disaster relief. Similarly, the local government did not increase the supply of public academies post-reform, as shown in Column 2.

In addition, the literature suggests that the second way that fiscal centralization enhances public goods provision is through empowering central governments to enforce a better environment for economic growth, such as better protecting property rights, enforcing contracts, and preserving the market (Besley and Persson, 2011; Mann, 1986; Dincecco, 2010). Moreover, when the central government gains authority over local public revenues and redistributes to local governments to finance their expenses, local governments lose their residual claims over local taxation, and thus their incentive and ability to grab from society (Frye and Shleifer, 1997; Olson, 1998, 2000). In the context of the Qing fiscal centralization, if the reform successfully restrained the grabbing hand of local governments, thus reducing the local tax burden, we expect to see a vitalized local society and economic development. In pre-modern China, local gentries were the most active local community participants and were often involved in providing public goods with private funds (Xue, 2021). Therefore, we use the number of newly founded private academies as a proxy for the public goods supplied by the local community, and present the results in Column 3 of Table 5. We find no increase in the numbers of private academies post-reform, which implied no improvement in local tax burden relief. Lastly, we use population density as a proxy of economic development, and show in Column 4 that the reformed area featured no better local economic growth.

Overall, we find that neither the government nor the local elite was capable of providing more public goods in the long run. This is consistent with previous findings that as the central government failed to keep its commitment, the impacts of the reform public good provision became insignificant, both in public and private arenas.<sup>15</sup>

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<sup>15</sup> The reform did not alter the way of tax collection nor the tax base. The allocation of tax burden was still based on

**Table 5: Public Goods Provision and Economic Development**

	(1)	(2)	(3)	(4)
	Disaster relief	Public academy	Private academy	Pop. density
Reform	0.070 (0.085)	0.003 (0.083)	0.0656 (0.0453)	0.0319 (0.0687)
Extreme weathers	0.227*** (0.024)	0.012 (0.009)	0.0276*** (0.0100)	0.0109** (0.00458)
Constant	0.183*** (0.048)	0.248*** (0.042)	0.363*** (0.0225)	4.194*** (0.0355)
Prefecture FEs	60,342	59808	59,808	60,342
R <sup>2</sup>	0.126	0.181	0.274	0.900

**Note:** \*, \*\*, and \*\*\* denote significance at the 90, 95, and 99% levels, respectively

## 7. Discussion and Conclusion

Comparative historical studies, primarily based on European experiences in the pre-modern era, have illustrated how fiscal centralization led to a significant rise in revenues for the central government, which laid the institutional foundation for later economic growth. European countries such as England, France, Holland, and Prussia have experienced a strengthening of the bureaucratic fiscal state since the 17<sup>th</sup> century. They centralized tax revenues in order to increase the central government's fiscal revenues (Johnson and Koyama, 2014). In the 1680s, England replaced cabal tax farming with direct collections of customs and consumption tax. By the 1750s, the central government had gained control over 90% of the tax revenue (O'Brien, 1988). As a result, the central government financed most local public goods, such as transportation infrastructure and judicial affairs (Brewer, 1989). During the Napoleonic wars, France, Holland, and Prussia introduced fiscal reforms to abolish the privilege of tax collection by local elites and churches, and instead channeled fiscal revenue to the central government.<sup>16</sup>

Yet, in pre-modern China, government reforms to achieve fiscal centralization did not result in modernization. We find that the centralization reform in 18<sup>th</sup> century China

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land ownership, hence the reform did not change relative tax burden on elites vis-à-vis non-elites.

<sup>16</sup> Austria-Hungary, France, Spain, and Italy did not achieve fiscal unification in the same period and lagged behind in fiscal capacity and economic growth (Peer Vries, 2015; Dincecco, 2009).

failed to reduce the grabbing hands of the local government. Furthermore, it led to more tax revolts. The dramatic difference in the effects of centralization across Eurasia motivated us to examine the differences between them in institutional arrangements supporting centralization. We show that the lack of institutional constraints caused the central government to fail to keep its promise to compensate the local government: the money intended for transfer payments was frequently appropriated by the upper-level governments. In response, the local governments needed to procure additional resources from society. The additional procurement further led to the limited increase of local public goods provision, either by the local governments or the gentry, suggesting no positive impacts on local development overall. Our findings thus serve as further evidence that limited governments and credible commitments provided an institutional foundation for the success of fiscal centralization, which offers another institutional perspective to help understand the great divergence.

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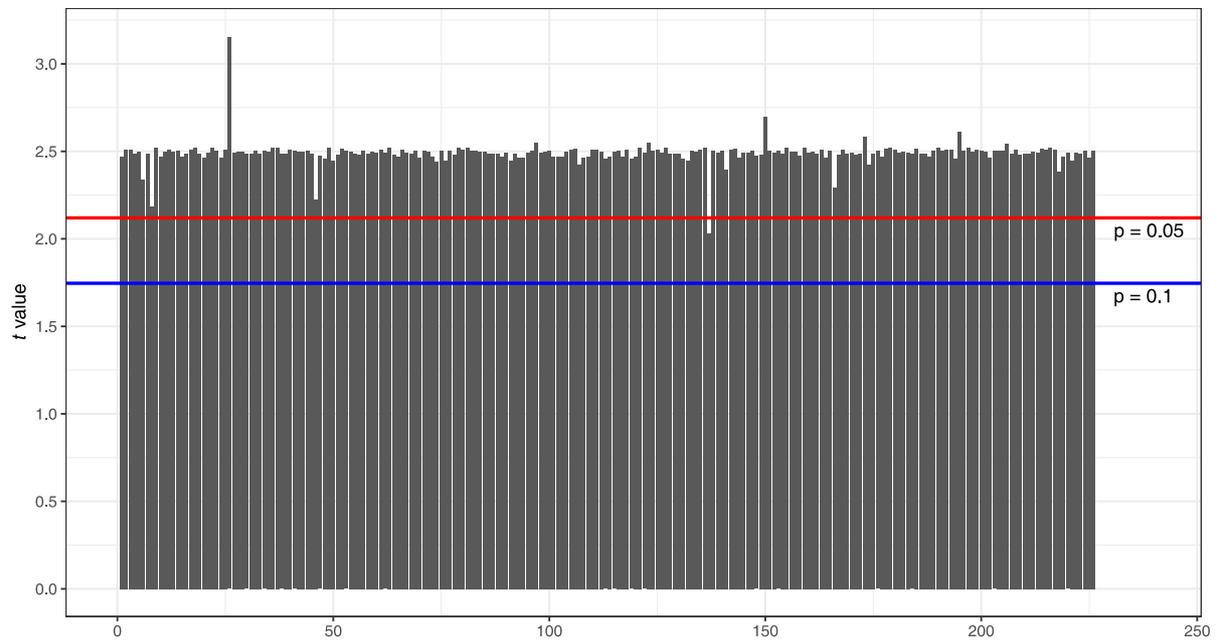
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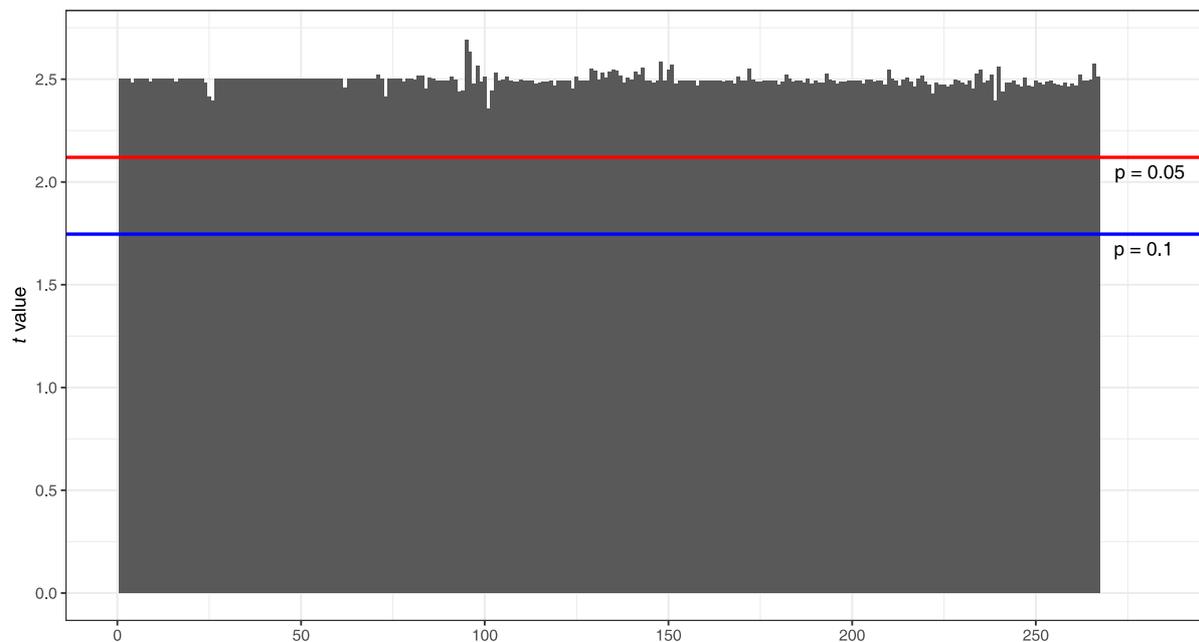
## Appendix

**Figure A1: Dropping a Prefecture once a Time**



**Note:** the horizontal axis represents the serial number of the prefecture that was dropped off; the vertical axis represents the t-value obtained from the regression with this prefecture dropped off.

**Figure A2: Dropping a Year once a Time**



**Note:** the horizontal axis represents the serial number of the year that was dropped off; the vertical axis represents the t-value obtained from the regression with this year dropped off.

**Table A1: Provincial Data**

	(1)	(2)
	Tax revolt	Tax revolt
Reform	0.012*** (0.002)	0.012*** (0.00196)
Extreme weathers		-0.00121 (0.00224)
Constant	0.005*** (0.001)	0.006*** (0.002)
Prefecture fixed effects	YES	YES
Year fixed effects	YES	YES
Number of province	17	17
Observations	4,539	4,539
R-squared	0.196	0.196

**Notes:**

1. \*, \*\*, and \*\*\* denote significance at the 90, 95, and 99% levels, respectively.
2. Robust standard errors are clustered at provincial levels.