The Emperor’s Tael: Government Commitment Failure and Tax Revolts in Qing China, 1644–1912*  

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**Abstract:** Scholars have recognized that strengthening the state’s fiscal capacity is a prerequisite for long-term economic development. However, a nationwide fiscal centralization reform by the Qing government (1723-1735) failed to achieve this goal. Instead, we find that the centralization reform surprisingly led to more tax revolts. More revolts were observed in areas where the central government reneged on its promise to compensate local governments for reform-related losses in recent years. Moreover, we find no increase in the provision of public goods post-reform. We thus highlight that a fiscal centralization reform can only succeed with credible commitments from the central government.

**Keywords:** Fiscal Centralization, Tax Revolt, Credible Commitments, Limited Government  

**JEL:** N15; N45; H77
**Huohao should never be used for purposes other than maintaining local governments.**

*Imperial decree by Emperor Yongzheng, 1729*

**Huohao can be used by the upper governments for various purposes.**

*Imperial decree by Emperor Qianlong (Son and successor of Yongzheng), 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; De Long and Shleifer, 1994; Dincecco and Katz, 2014; Johnson and Koyama, 2017; North and Weingast, 1989). Strong fiscal capacity has typically been 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 supported this view using case studies such as England before the Glorious Revolution (Johnson and Koyama, 2014; O’Brien, 1988), Prussia in the late 18th and early 19th 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-18th century. However, it failed to strengthen the state’s fiscal capacity (Broadberry et al., 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 collected by local governments 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; therefore, local officials tended to over-collect for personal gain, and the size of huohao exceeded 50% of formal taxes (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 subject to stricter supervision to prevent them from extralegal levies.

The key to the reform was a promise made by the emperor, that the 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 overall tax
burden (Chen, 2008), and to restrain the local officials to grab from society (Zelin, 1984).

However, Emperor Qianlong, Yongzheng’s son and successor, reneged on this promise. In 1744, Qianlong approved an appeal to transfer *huohao* between provinces, and the appropriation of *huohao* gradually became common practice. This led to a situation where local governments did not fully receive their transfer payments, which 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 reduced transfer payment forced the local governments to seek alternative ways to finance their daily activities. With limited bargaining power facing the absolutist imperial ruler, local governments had no choice but to raise additional resources from society. The additional burden was thus shifted to the ultimate victims of centralization: the general public. Without institutional limits on their power, local governments extorted from society to make up for their losses. Therefore, we predict a heavier burden and increased social unrest in the reformed areas where local extortion was more severe, and more so in areas where local appropriation cases took place in recent years, thus forming a stronger expectation of future appropriation for local officials.

To empirically test these predictions, we construct a panel dataset covering 226 prefectures and 268 years (1645–1912) to examine the impact of the Qing fiscal centralization. Among the 226 prefectures, 168 were reformed areas when the imperial edict was published in 1723, and 58 were unreformed areas. The reformed and unreformed areas allow us to use a difference-in-differences (DID) strategy in our analysis. First, we use the number of local tax revolts retrieved from the *Qing Shilu* (Veritable Records of the Qing Emperors) as a proxy for tax burden, and show that the frequency of tax revolts increased by 1.1 times over the sample years post-reform. That is, the reform triggered a significant increase in social unrest due to heavier tax burdens. The significant increase is robust to controlling for population density, extreme weather, and agro-ecological conditions. We also employ the generalized spatial two-stage least squares (GS2SLS) method and find that the results are robust to accounting for spatial autocorrelation. Moreover, to account for the fact that the reform areas may not have been selected at random, we follow the BD-DD analysis a la Baskaran (2014) and Lu et al. (2019), and consider only the reformed prefectures whose neighboring prefectures happened to be unreformed. The design thus allows us to largely overcome the possible systematic differences between reformed and unreformed regions. Our baseline results remain robust to the analysis.
In addition, to rule out the alternative explanation that the increase in tax revolts was due to other confounding socioeconomic changes, we collect the information of other non-tax-related revolts – which by definition were not correlated with the fiscal reform, and show that these non-tax revolts did not show a similar increase after the reform. Taken together, the results reveal that the well-intentioned fiscal centralization failed to curb local grasping for extralegal levies.

Next, we examine the reasons for its failure. As noted above, the key to the success of the reform was the imperial promise, repeatedly emphasized by Emperor Yongzheng, to return transfer payments to local governments, but broken by Emperor Qianlong, Yongzheng’s son and successor. We argue that the central government's failure to make a credible commitment not to appropriate transfer payments led to more aggressive local grabbing, which in turn led to higher tax burdens and thus more tax revolts. Specifically, we use whether the central government had appropriated the transfer payment in previous years as a proxy for local governments’ expectations of the central government's ability to keep its promise. We find that local tax revolts intensified in regions where the central government had appropriated transfer payments in the previous two years, confirming that the broken promise contributed to increased social unrest.

Finally, we examine the long-term effects of the reform on local economic development. The existing literature suggests that fiscal centralization has a positive impact on the provision of public goods in the short run, such as famine relief (Lindert, 2004), and in the long run, such as primary education (Cinnirella and Schueler, 2018). Following the literature, we use disaster relief as a proxy for short-run public goods provision and the number of newly established public academies in the locality as a proxy for long-run public goods provision: we find no increased local public goods provision after the reform, which in turn contributes to the ineffectiveness of the reform. Moreover, the literature suggests that fiscal centralization increases privately provided public goods by limiting local capture and thus creating a more vibrant civil society (Frye and Shleifer, 1997; Olson, 1998; 2000). We therefore use the number of newly established private academies - those built by the local gentry in pre-modern China - as a proxy for public goods provided by civil society. We find no increase in the number of private academies after the reform, implying no improvement in local tax relief. Finally, we use population density as a proxy for economic development and show that the reformed area did not have better local economic growth.
This paper makes three contributions to the literature. First, our empirical findings highlight that the lack of credible commitment was responsible 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 commit 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 17th to the 19th 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. In contrast, pre-modern China had no such institutional constraints on its rulers and government (Ma and Rubin, 2019).1 Thus when the central government reneged on its promise to make transfers to local governments, local leaders did not negotiate with the ruler. Instead, they extracted resources from local residents.2 In this sense, the unlimited government in pre-modern China provides valuable parallel evidence consistent with the European experience.3

Second, although most prior research emphasizes the importance of credible commitments in building state capacity, most empirical studies did not directly measure the degree to which such commitments were fulfilled. They indirectly assessed commitment as an effective institutional constraint that limited the ruler's discretion over public spending.4 However, considering institutional constraints alone did not solve the commitment problem (Stasavage, 2003).5 In the absence of formal political accountability towards the imperial power, informal constraints such as religion (North et al., 2009), Confucian traditions (Kung and Ma, 2014), and cosmological anomalies (Wang, 2006) were only partial substitutes. A direct measure of commitment may better capture agents’ behavior under these institutional constraints. We directly measure “commitment failure” in Qing China as incidences involving the central

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1 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.
2 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.
3 Other studies have used this approach, including Bates and Li (1985); Cox (2016); Dincecco (2011); Dincecco et al. (2011); Hoffman and Rosenthal (1997); Levi (1988) and Timmons (2005).
4 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 (2019) highlights the power of local mine owners who organized corporate enterprises to limit the power of government in colonial Mexico.
5 Stasavage (2003) finds that only when the Whigs controlled the English parliament after 1715 did the creditors believe the loans would be repaid.
government’s failure to make transfer payments to the locals in recent years, which changed locals' expectations of the current year's promise fulfillment. An incident 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 promise. Thus, to the best of our knowledge, our study is the first to examine the impact of "promise failure" after centralization on local government behavior. In the context of the Qing fiscal reform, a paper close to ours is Hao and Liu (2020), which highlights the short-run benefits of the reform. This paper differs from Hao and Liu (2020) in two aspects. First, this paper is the first rigorous empirical investigation that links the failure of fiscal reform to the lack of credible commitment. In addition, this paper further examines and highlights the negative consequences of broken promises, i.e., induced social unrest, thus advancing our understanding of the eventual failure of the reform despite its short-run benefits.

Third, our study makes a new contribution to the literature explaining the great divergence between China and the West by focusing on the design of political institutions regarding the relationship between central and local governments, in addition to existing discussions of social and economic factors such as demography, natural resources, market integration, market access, innovation, technology diffusion, legal system, elite network, and geopolitics. 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.

The remainder of the paper is organized as follows. Section 2 briefly overviews the historical 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 effects of the reform, while Section 7 concludes.

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6 See: 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); Voigtlander and Voth (2006, 2013); and Wang (2022).

7 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).
2. Background

2.1 The Fiscal Centralization Reform in Qing China

From its inception, the Qing's revenues and expenditures were highly decentralized. On the
revenue side, formal taxes submitted to the central government accounted for only 2% of
national income – far less than the formal taxes retained by local governments and the informal
levies they collected (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. Because of their limited bargaining power with the imperial court, they usually
chose not to ask the central government for transfer payments, and instead raised various
informal levies called huohao. This term was originally used to refer to the surcharge levied to
cover the physical wear and tear of silver during the collection of the silver tax, and was usually
less than 10% of the formal tax. It was later used to refer to the 40–50% (or even higher) excess
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 oversight,
local officials tended to over-collect for illicit personal benefits (Brandt et al., 2014).

The third Qing emperor, Yongzheng (1723–1735), introduced efforts at fiscal centralization.
His nationwide reform attempted to grant the central government the dominant role in
distributing fiscal resources without changing the overall tax burden. This goal was achieved
by submitting huohao to the central government in most provinces, and making transfers
(yanglianyin) to the local governments to compensate for their losses. The key to the reform
was a promise made by the emperor that the 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. Meanwhile, local officials were placed under stricter supervision
to prevent them from collecting extralegal levies.

The reform represented an attempt at fiscal centralization in two ways. First, the reform aimed
solely to re-structure central-local fiscal distributions without other state-building initiatives,
such as recruiting better talent or training incumbent bureaucrats. Second, before the reform,
the local governments financed their own expenditures through informal levies that they collected; after the reform, the central government redistributed funds to the local governments to finance their expenditures. Meanwhile, after the reform, the central government – not the local governments – set the rate of informal levies (Chen, 2008). The reform began in 1723 when these unreformed regions continued to use *huohao* to finance local government expenditures. Figure 1 illustrates the implementation of the reform, where the darker grey regions were the reformed areas, and the light grey regions were unreformed. As shown in the figure, some economically prosperous coastal provinces have been selected for the reform while the more inland, poorer provinces were not. The distribution triggers concerns that the reformed areas were probably not a random choice, thus the people in the two areas might consequently behave differently. To address the issue, we conduct a specific test in the empirical analysis that compares only the reformed prefectures whose neighboring prefectures happened to be unreformed to overcome the possible systematic differences between reformed and unreformed regions.

**Figure 1. Implementation of Huohao Reform**

![Map showing implementation of Huohao Reform](image)

**Notes:** The figure depicts the implementation of *huohao* reform. The reformed prefectures are in dark grey, and the unreformed are in light grey.  
**Sources:** Xue (1984) and Zelin (1984).
2.2 The Broken Promises

After the reform, local governments were less able to collect illegal taxes because they lost their residual claims on *huohao*. According to a report submitted to the emperor in 1735, three high-ranking central officials observed that local officials stopped collecting informal levies after the reform (Zelin, 1984). In the meantime, by design, the transfer payments from the central government enhanced local governments’ ability to provide public goods, which led to an increase in public goods provision in the following decades (Hao and Liu, 2020). The key to the success of centralization in the short run seems to be that the central government kept its promise to provide transfer payments.

However, Emperor Qianlong, Yongzheng’s son and successor, reneged on this promise (Zelin, 1984). At the beginning of Emperor Qianlong's succession, he made great efforts to follow his grandfather, Emperor Kangxi, in order to gain a reputation for leniency (Zelin, 1984). In addition to reducing land taxes (*qianliang*), Qianlong also significantly cut down *huohao*. In the fourth year of his reign (1739), Qianlong issued an exemption of 900,000 taels of land taxes (*qianliang*) in Zhili, 1,000,000 taels in Jiangsu and 600,000 taels in Anhui, and abolished all *huohao* added to major taxes (*zhengfu*). Consequently, these three regions were in serious financial crises. Later in 1739, Qianlong set a precedent by using Henan's surplus from *huohao* to subsidize the financial difficulties of Zhili, Jiangsu, and Anhui. In the ninth year of his reign (1744), Qianlong approved an appeal to transfer *huohao* between provinces, and the appropriation of *huohao* gradually became common practice. In many cases, *huohao* was used to finance projects that should have been funded by the central government, 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 reduced transfer payment urged the local governments to explore alternative ways to finance their daily activities. With limited bargaining power facing the absolutist

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8 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.
imperial ruler, local governments had no choice but to raise additional resources from society. The additional burden was thus transferred to the ultimate victims of centralization: the general public. Without institutional limits on their power, local governments extracted from society to make up for their losses.⁹

The extraction from society was reflected in the increased tax burden. Detailed information on local tax burdens was difficult to obtain. Therefore, we use the occurrence of tax revolts as a proxy. Tax collection was closely linked to social stability, and heavy tax burdens were often the source of tax revolts. In imperial China, one of the most notable tax revolts, Zhiyong Zhibian, was initiated by textile workers in Suzhou in 1601, facing surged tax burden and brutal tax collectors. The association was widespread throughout the world. The literature has pointed to heavy tax burdens as the root cause of peasant revolts in England in 1381 (Ormrod, 1990) and the tax revolts in the United States in the late 1970s (Lowery and Sigelman, 1981). Consequently, using the occurrence of tax revolts as a proxy for tax burden, we have the first prediction for empirical investigation: the tax revolts by local residents became more frequent after the fiscal centralization reform.

Furthermore, Emperor Qianlong’s reneging on his father’s promise was the key mechanism to understand the failure of the well-intentioned fiscal centralization reform. The broken promise of imperial power was not in itself surprising. 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 (Hoffman and Norberg, 1994; Epstein, 2000; Stasavage, 2007; Dincecco, 2009). 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. Similarly, local governments could easily withdraw from society if they perceived a lack of credible commitment from the central government. This leads us to the second prediction for the empirical investigation: the increased tax revolts by local residents were more severe in regions where local governments had higher expectations that the central government would break its promises.

Finally, in terms of policy impact, the Qing fiscal reform failed to significantly increase central

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⁹ 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.
fiscal revenue as a share of GDP in the long run (Ma, 2011b; Zelin, 1984). Local governments were still locked into a decentralized system in which they financed their own expenditures through the collection of informal levies. Consequently, and in line with the evidence in the literature,\textsuperscript{10} we thus predict: \textit{local public goods provision in the reformed area would not improve in the long run.}

In the following sections, we empirically test these predictions.

\section*{3. Data}

We construct a panel dataset from several historical sources covering the period 1644-1912 and 226 prefectures, which allows us to test the reform's effect on tax burden and whether it is due 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. In the following, we present the relevant data and variables.

\subsection*{3.1 Data Descriptions and Sources}

\textbf{Tax Burden.} We use the number of tax revolts as a proxy for tax burden. A tax revolt is a natural consequence of excessive tax burdens imposed by local governments on ordinary residents whose burdens exceed the maximum level they can bear.\textsuperscript{11} We collect this series of data from \textit{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, \textit{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 2 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

\textsuperscript{10} 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 pre-modern world (Acemoglu \textit{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).

\textsuperscript{11} There were, of course, alternate triggers of tax revolts given unchanged tax burden, such as more violent tax collections. However, during our sample period, the tax collection techniques in the Qing dynasty did not witness systematic changes, thus making tax revolts a valid proxy for tax burden.
decades before the reform (1703–1722) to 2.95 per year in the two decades after the reform (1723–1742).

**Figure 2. Time Trends of Tax Revolts**

![Graph showing the yearly number of tax revolts from 1650 to 1900.]

**Notes:** The histogram in the figure depicts the yearly number of tax revolts during 644-1912. The vertical line represents the year 1723 when the huohao reform started.

**Central Government’s Commitment.** We use whether the central government had appropriated the transfer payments (*yanglianyin*) to the local governments in the previous 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 about 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.12 These documents were retrieved from the website of the 1st Chinese Historical Archive. Figure 3 shows the time trend of appropriation in all provinces, which displays an inverted U-shape: it increased over time, peaked in the last decade of the 18th century, and declined thereafter. We focus on local appropriation cases – rather than a cutoff of the first nationwide appropriation in 1744 – for two reasons. First, local governments formed their expectations of the credibility of imperial commitments based on their local experiences. Second, information transmission in pre-modern societies was often stagnant, so the diffusion of appropriation cases could be slow. Taken together, local appropriation cases in the previous years serve as a better proxy for measuring the central government’s commitment.

12 Relevant keywords are *yanglianyin*, appropriation of funds (*zijin nuoyong*), salary (*fenglu*), silver (*yingliang*), high salary against corruption (*gaoxin yanglian*), and fiscal transfer (*caizheng zhuanyi*).
Figure 3. Time Trend of Appropriations of Transfer Payment

Notes: The histogram in the figure depicts the yearly number of fiscal transfers during 1644-1912. The vertical line represents the year 1723 when the huohao reform started.

Public Goods Provision. We intend to evaluate the long-term effects of the reform through local public goods provision and economic development. First, we use three metrics to evaluate public goods provision: disaster relief, public provision of education, and private provision of education. Disaster relief was the most important public service provided by local governments in pre-modern China. The data also come from Qing Shilu, which provides detailed records of where and when local governments provided disaster relief. Public education provision is measured by the number of newly established public academies, while private education provision is measured by the number of private academies established by local gentries. Both data come from the Glossary of private academies in historical China (Ji, 1996). Specifically, when local officials fail to finance their own pocket due to post-reform appropriations, it not only reduces the funds available for public education provision, it also crowds out private education provision because of local officials’ extraction from the private sector.

Economic Development and Other Shocks. We collect data on economic development, climate shocks, and technology shocks. First, we measure local economic development by population density, a widely accepted measure for pre-modern economies in the literature (Acemoglu et al., 2002; Clark, 2007; Cao and Chen, 2022). Second, we consider climate shocks as a crucial social destabilizer in pre-modern China (Miguel et al., 2004; Miguel, 2005; Bai and Kung, 2011; Hsiang et al., 2011; Hsiang et al., 2013). Specifically, we use the presence or absence of extreme weather events, which equals 1 if there was a drought or flood in the
prefecture in the year, and 0 otherwise. Last, technological shocks also have long-term effects on regional development (Galor and Weil, 2000; Voigtländer and Voth, 2006; Ashraf and Galor, 2011; Chen and Kung, 2016). During the Qing dynasty, a notable technological shock occurred in agricultural production: the introduction and expansion of three American crops, maize, potato, and sweet potato (Ho, 1959). Hence, we use the maize suitability index, potato suitability index, and sweet potato suitability index (hereafter referred to as the agro-ecological suitability index) to control for technological shocks. The population density data are retrieved from Cao (2020), the extreme weather data are constructed from CAMS (1981), and the agro-ecological suitability index data are from GAEZ (2002). Table 1 reports the summary statistics for all these variables.

<table>
<thead>
<tr>
<th>Variables</th>
<th>Mean</th>
<th>Standard deviation</th>
<th>Number of observations</th>
<th>Data sources</th>
</tr>
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<tbody>
<tr>
<td>Tax revolts</td>
<td>0.011</td>
<td>0.122</td>
<td>60,342</td>
<td>A</td>
</tr>
<tr>
<td>Appropriation of transfer payments</td>
<td>0.202</td>
<td>0.624</td>
<td>60,342</td>
<td>B</td>
</tr>
<tr>
<td>Disaster relief</td>
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<td>1.670</td>
<td>60,342</td>
<td>A</td>
</tr>
<tr>
<td>Public academies newly founded</td>
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<td>0.658</td>
<td>59,808</td>
<td>C</td>
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<tr>
<td>Private academies newly founded</td>
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<td>0.579</td>
<td>59,808</td>
<td>C</td>
</tr>
<tr>
<td>Population density (ln)</td>
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<td>1.116</td>
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<tr>
<td>Extreme weather</td>
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<td>0.500</td>
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<tr>
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<tr>
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<tr>
<td>Sweet potato suitability index</td>
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<td>60,342</td>
<td>F</td>
</tr>
</tbody>
</table>

Sources: A. Qing Shilu (Veritable Records of the Qing Emperors); B. reports of governors to the Ministry of Finance accessed from 1st Chinese Historical Archive; C: Ji (1996); D: Cao (2020); E: CAMS (1981); F: GAEZ (2002).

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). Of the 226 prefectures, 168 were reformed areas when the imperial edict was published in 1723, and 58 were unreformed areas. We estimate the following equation as our baseline specification:
$$Y_{it} = \alpha Treat_i \times Post_t + X_{it} + prefecture_i + p_t + \varepsilon_{it} \quad (1)$$

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 $t$ had reformed, and 0 otherwise. $Post_t$ is a dummy variable that takes the value of 1 only after 1723, 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. $X_{it}$ denotes other time-varying controls, including population density, extreme weather dummies, and the agro-ecological suitability index. As befits a fixed-effects model, $prefecture_i$ captures the time-invariant regional characteristics for prefectures $t$ that may be associated with the reform, whereas $p_t$ controls for the temporal effects in our estimation. $\varepsilon_{it}$ is the error term.

4.1 Baseline Results and Robustness Checks

The baseline results are presented in Table 2, 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, the frequency of tax revolts increased by 1.1 times over the sample years. Column 2 shows that our results are robust to controlling for population density, extreme weather dummies, and the agro-ecological suitability index. The distribution of tax revolts may be a count variable with a large proportion of zeros, according to the summary statistics in Table 1. Therefore, in Column 3, 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, which is broadly consistent with the estimate in Column 2.\textsuperscript{13} To further control for potential endogeneity due to spatial autocorrelation, in Column 4, we employ 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 accounting for spatial autocorrelation.

\textsuperscript{13} To further address 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 estimates are robust to this exercise.
Next, to further interpret our empirical results, we use the following three steps to quantify the reform’s contribution to tax revolts during the sample period (1723–1912). First, we calculate 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 (0.011) by 0.743 to obtain the estimated change in the frequency of tax revolts per year caused by the reform (0.008). Third, we divide this change by the sample average of the annual change in tax revolts (0.048), and obtain the share of the change in tax revolts that can be explained by the change in reform status, 16.7% (0.008/0.048).

### Table 2. Centralization Reform and Tax Revolts

<table>
<thead>
<tr>
<th></th>
<th>(1) Tax revolts</th>
<th>(2) Tax revolts</th>
<th>(3) Negative binomial</th>
<th>(4) GS2SLS</th>
<th>(5) BD-DD analysis</th>
<th>(6) Non-tax revolts</th>
</tr>
</thead>
<tbody>
<tr>
<td>Reform</td>
<td>0.011***</td>
<td>0.009***</td>
<td>1.411***</td>
<td>0.013***</td>
<td>0.024*</td>
<td>0.024</td>
</tr>
<tr>
<td>Constant</td>
<td>0.005**</td>
<td>0.042</td>
<td>-10.206***</td>
<td>0.004</td>
<td>-0.007</td>
<td>-2.697</td>
</tr>
<tr>
<td>Controls</td>
<td>NO</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>NO</td>
<td>YES</td>
</tr>
<tr>
<td>Prefecture FE</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>Year FE</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>No. of prefectures</td>
<td>226</td>
<td>226</td>
<td>226</td>
<td>226</td>
<td>59</td>
<td>226</td>
</tr>
<tr>
<td>No. of observations</td>
<td>60,342</td>
<td>60,342</td>
<td>60,342</td>
<td>60,342</td>
<td>15,753</td>
<td>60,342</td>
</tr>
<tr>
<td>R²</td>
<td>0.094</td>
<td>0.095</td>
<td>0.015</td>
<td>0.119</td>
<td>0.330</td>
<td></td>
</tr>
</tbody>
</table>

Notes: ***, **, and *** indicate 10%, 5%, and 1% significance levels, respectively. Standard errors in parentheses are clustered at the prefecture level. Column 4 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, assuming a cutoff window of 500 km and a serial correlation of 50 years.

Meanwhile, a crucial assumption for our DID analysis is that there was no different pre-trend between the reform and non-reform areas. We therefore perform a validity check as follows. We code every ten years as an interval and plot the results in Figure 4, which suggests that there is little systematic difference in tax revolt trends between the reformed and non-reformed areas prior to 1723. This suggests that the reform was exogenous to the social and economic conditions of a region. After the reform, there was an upward trend in the frequency of tax revolts, with a more significant increase over the longer term. We argue that this is because, in the long run, as local governments learned not to trust the central government’s promise of non-appropriation, they began to raise funds more aggressively to finance their expenditures, which eventually led to a heavier tax burden and increased tax revolts. A careful test of this hypothesis
is included in the next section.\textsuperscript{14}

**Figure 4. Number of Tax Revolts before and after Reform**

![Graph showing changes in the number of tax revolts before and after reform](image)

**Notes:** The solid line indicates changes in the number of tax revolts conditional on prefecture fixed effects and period fixed effects. The markers and capped spikes represent the ordinary least squares estimators and 95\% confidence intervals based on standard errors clustered at the prefecture level. The dashed vertical line represents the 1723 treatment date, and the periods are grouped every ten years relative to 1723. The reference groups are the years more than 80 years before 1723.

It is worth noting that, as shown in Figure 4, the number of tax revolts declined and then increased sharply after the 1740s. This seemingly contradictory observation is actually consistent with our story. The reasons are as follows. At the beginning of the reform and after Emperor Yongzheng’s decree banning *huohao* appropriation, local governments tended to act in accordance with the decree. As a result, there was little appropriation and few tax revolts. However, when Emperor Qianlong overturned his father’s decree, broke the promise, and transferred the *huohao* payments in Henan to Zhili, Jiangsu, and Anhui in 1740, local governments soon followed suit, appropriating the transfer payments at the provincial, prefectural, and county levels. The ebb and flow of tax revolts reflect the dynamics of the promise-keeping and promise-breaking periods.

\textsuperscript{14} 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.
4.2 Non-random Selection of Reformed Areas

A key challenge in our analysis regarding regional heterogeneity is that reform areas may not have been selected at random, thus affecting our baseline estimates. As noted in the existing literature, geographic characteristics can affect fiscal capacity and the choice of fiscal reforms, thus affecting potential tax revolts. (Stasavage, 2010). To address this issue, we follow the BD-DD analysis a la Baskaran (2014) and Lu et al. (2019). Specifically, we consider reformed prefecture as the treatment group and the neighboring non-reformed prefecture as the control group. The design thus allows us to largely overcome the possible systematic differences between reformed and unreformed regions, such as geographic attributes, resource endowments, quality of governance, demographics, and culture (Dell, 2010). Figure 5 shows the spatial distribution of these reconstructed treatment and control groups. We report the results in Column 5 of Table 2, which shows that the excess tax revolts remain robust. Thus, the baseline results are less likely to be driven by non-random sample selection.

Figure 5. Implementation of Huohao Reform: a BD-DD Analysis

Notes: The figure depicts the implementation of huohao reform. The reformed prefectures are in dark grey, and unreformed prefectures adjacent to them are in light grey, which is the sample region for our study. Sources: Xue (1984) and Zelin (1984).

4.3 Alternative Explanation
Finally, we consider an important alternative explanation to our story: the increased tax revolts were due to the confounding socioeconomic turbulence other than the fiscal tax reform highlighted in this paper. For instance, it is possible that local governance failure, extreme weather, and interethnic conflicts may also contribute to local peasant revolts. To rule out the alternative explanation, we collect data on these non-tax peasant revolts from *Qing Shilu*: if the alternative explanation holds, we expect that the reformed areas had higher frequencies of non-tax peasant revolts. We test the hypothesis and present the results in Column 6 of Table 2. As shown, the reform had no significant impact on peasant revolts, thus ruling out the alternative explanation.\(^{15}\)

So far, we have shown that the fiscal centralization reform led to more social unrest – in the form of tax revolts – in reformed areas. In the next section, we link this increased social unrest to the broken promises of the Qing imperial court.

### 5. Reform without Commitment

This section examines whether the increase in tax revolts after the tax reform was due to broken promises, i.e., appropriated transfer payments. The idea is as follows. The promise to return transfer payments in full helped shape local governments' expectations. When local governments *expected* the central government to break the promise, local officials had to increase extralegal levies to compensate for their losses, leading to an increased tax burden and thus more tax revolts. We use the average annual number of transfer payment appropriations in the last two years for a locality as a proxy for the local government's expectation of central commitment. We focus on the broken promises *in the locality* – rather than a nationwide event – to highlight that if the central government has appropriated *local* funds frequently in recent years, then the local government should have a lower expectation that the central government will keep its promise this year, and thus have higher incentives to increase local extralegal levies, which would then lead to more tax revolts. Our econometric specification is as follows:

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\(^{15}\) 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.
\[ Y_{it} = \gamma_1 Reform_{it} \times Appropriation_{it} + \gamma_2 Appropriation_{it} + X_{it} + prefecture_i + p_i + \epsilon_{it} \] (2)

In the specification, \( Appropriation_{it} \) captures the average annual number of transfer payment appropriations in the last two years for a locality, and \( Reform_{it} \) refers to \( Treat_i \times Post_t \) as in the baseline analysis. We expect areas with more previous appropriation cases to exhibit higher frequencies for tax revolts, due to stronger expectations of future appropriation.

Table 3 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 population density, extreme weather dummies, and the agro-ecological suitability index. The 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>
<thead>
<tr>
<th>Table 3. Centralization Reform and Appropriation of Transfer Payments</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1) Tax revolts</td>
</tr>
<tr>
<td>Reform*Appropriation</td>
</tr>
<tr>
<td>(0.008)</td>
</tr>
<tr>
<td>Appropriation</td>
</tr>
<tr>
<td>(0.004)</td>
</tr>
<tr>
<td>Constant</td>
</tr>
<tr>
<td>(0.002)</td>
</tr>
<tr>
<td>Controls</td>
</tr>
<tr>
<td>Prefecture FE</td>
</tr>
<tr>
<td>Year FE</td>
</tr>
<tr>
<td>No. of prefectures</td>
</tr>
<tr>
<td>No. of observations</td>
</tr>
<tr>
<td>( R^2 )</td>
</tr>
</tbody>
</table>

*Notes: *, **, and *** indicate 10%, 5%, and 1% significance levels, respectively. Standard errors in parentheses are clustered at the prefecture level.*

There are two interesting observations from the results in Table 3 regarding the learning process of local government officials. First, at the beginning of the reform, local officials could learn from various sources, from local anecdotes to imperial decrees, about the implementation of the reform, especially whether the central government kept its promise. They changed their actions accordingly when they observed openly broken promises, such as the contradictory decrees in the epigraph. For all local government officials, recently broken promises were one of the strongest signals on which to base their actions, which is well reflected in the intensification effects of prior appropriation in Table 3.
Moreover, as shown in all three columns, the positive effects of the reform on tax revolts remained significant. This shows that even local officials who did not observe the recent breach of promise continued to believe that the central government could break the promise at any time. Therefore, it is necessary to prepare for it by extracting some assets today. This reasoning reflected the fundamental lack of commitment in the institutional design: if policies could change overnight, if the father’s imperial decree could be arbitrarily replaced by the son’s opposite decree, none would be trusted.

6. The Long-term Impact of the Reform

This section investigates the reform’s long-term impact on public goods provision and economic development. In a comparative context, successful fiscal reforms have often been associated with improved public goods provision 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 improved after the reform. Columns 1-2 in Table 4 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.

Moreover, the literature suggests that a second way in which fiscal centralization enhances the provision of public goods is by empowering the central government to enforce a better environment for economic growth, including better protection of property rights, enforcement of contracts, and preservation of markets (Besley and Persson, 2011; Mann, 1986; Dincecco, 2010). Moreover, when the central government gains authority over local public revenues and
redistributes them to local governments to finance their expenditures, local governments lose their residual claims on local taxes and thus their incentive and ability to grab from society (Frye and Shleifer, 1997; Olson, 1998; 2000). In the context of Qing fiscal centralization, if the reform succeeds in restraining local governments’ grabbing hand and thus reduces the local tax burden, we expect to see a revitalized local society and economic development. In pre-modern China, the local gentry were the most active participants in the local community and were often involved in providing public goods with private funds (Xue, 2021). Therefore, we use the number of newly established private academies as a proxy for public goods provided by the local community and present the results in Column 3 of Table 4. We find no increase in the number of private academies after the reform, implying no improvement in local tax relief. Finally, we use population density as a proxy for economic development and show in Column 4 that the reformed area did not have better local economic growth. Overall, we find that neither the government nor the local elite were able to provide more public goods in the long run. This is consistent with previous findings that the impact of the reform on the provision of public goods in both the public and private sectors became insignificant as the central government failed to keep its commitment.  

<table>
<thead>
<tr>
<th>Table 4. Public Goods Provision and Economic Development</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1) Disaster relief</td>
</tr>
<tr>
<td>Reform</td>
</tr>
<tr>
<td>Constant</td>
</tr>
<tr>
<td>Controls</td>
</tr>
<tr>
<td>Prefecture FE</td>
</tr>
<tr>
<td>Year FE</td>
</tr>
<tr>
<td>No. of prefectures</td>
</tr>
<tr>
<td>No. of observations</td>
</tr>
<tr>
<td>R^2</td>
</tr>
</tbody>
</table>

Notes: *, **, and *** indicate 10%, 5%, and 1% significance levels, respectively. Standard errors in parentheses are clustered at the prefecture level.

7. Discussion and Conclusion

Comparative historical studies, primarily based on the European experience in the pre-modern

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16 For economic development, we do not have modern indicators such as GDP per capita to capture the development in the Qing dynasty. Instead, economic history literature has relied on population density and urbanization indicators (e.g., De Vries, 1976; Bairoch, 1988; Acemoglu et al., 2002; 2005; Clark, 2007; Campbell and Lee, 2008; Nunn and Qian, 2011). However, no urbanization data in the early and the mid-Qing dynasty was available. Therefore we focus on population density as our proxy for economic development.

17 The reform did not alter the way of tax collection nor the tax base. The allocation of tax burden was still based on land ownership, hence the reform did not change relative tax burden on elites vis-à-vis non-elites.
period, have shown how fiscal centralization led to a significant increase in central government revenues, which laid the institutional foundation for later economic growth. European countries such as England, France, the Netherlands, and Prussia strengthened the bureaucratic fiscal state beginning in the 17th century. They centralized tax revenues in order to increase the fiscal income of the central government (Johnson and Koyama, 2014). In the 1680s, England replaced cabotage tax farming with direct collections of customs and excise taxes. By the 1750s, the central government had gained control over 90 percent of tax revenues (O’Brien, 1988). As a result, the central government financed most local public goods, such as transportation infrastructure and justice (Brewer, 1989). During the Napoleonic wars, France, the Netherlands, and Prussia introduced tax reforms to end the tax-raising privileges of local elites and churches and instead channel tax revenues to the central government. 18

However, in pre-modern China, government reforms to achieve fiscal centralization did not lead to modernization. We find that the centralization reform in 18th century China failed to reduce the grasping hands of the local government. It also led to more tax revolts. The dramatic differences in the effects of centralization across Eurasia motivated us to examine the differences in the institutional arrangements that supported centralization. We show that the lack of institutional constraints caused the central government to renege on its promise to compensate local governments: money intended for transfer payments was often appropriated by higher-level governments. In response, local governments had to extract additional resources from society. The additional procurement also led to a limited increase in the provision of local public goods, either by local governments or by the gentry, suggesting no positive impact on local development. Thus, our results provide further evidence that limited governments and credible commitments provided an institutional foundation for the success of fiscal centralization, offering a different institutional perspective for understanding the great divergence between China and the West.

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18 Austria-Hungary, France, Spain, and Italy did not achieve fiscal unification in the same period and lagged behind in fiscal capacity and economic growth (Vries, 2015; Dincecco, 2009).
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Appendix

Figure A1: Dropping a Prefecture once a Time

Notes: 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

Notes: 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.