Taxation, fiscal capacity, and credible commitment in eighteenth-century China: the effects of the formalization and centralization of informal surtaxes[†]

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This article explores a tax reform in eighteenth-century China that formalized county-level informal surtaxes and centralized control over them in the hands of provincial governors, in an effort to strengthen provincial fiscal capacity. The findings show that this reform increased the frequency of famine relief in cases of exceptional disaster relative to other weather conditions. The study shows that the effects were driven by the new fiscal revenues—public funds—at the governors' discretion, not by the central government's relief actions, bureaucratic control over lower officials, or other concurrent fiscal reforms. Moreover, the reform facilitated intertemporal smoothing and inter-regional risk sharing. However, the effects declined as soon as the central government broke its promise and began to appropriate provincial fiscal revenues. These findings not only provide evidence that fiscal centralization could enhance the provision of public goods in a premodern context, but also highlight that it was the lack of a credible commitment by the central government to the provincial governments that accounted for the short-lived effects of the reform.

Centralization is crucial to forming state fiscal capacity, which is in turn conducive to long-term economic development.[1] More specifically, fiscal centralization helps pool resources to provide public goods that have large spillovers across space and over time.[2] This article explores the effect of a reform that formalized and centralized part of the fiscal system in the reign of Emperor Yongzheng (1723–35). The reform formalized county-level informal surtaxes (*huohao*) and centralized control over them in the hands of provincial governors. It

strengthened provinces' fiscal capacity and as a consequence increased the level of famine relief they supplied.

'*Huohao*' denotes all the informal surtaxes that were collected by county governments. Because the bulk of formal tax revenue was remitted directly to the central government, provincial and county governments retained only a limited share of formal tax income and had little discretion over their spending.[3] Therefore, they imposed *huohao* or surtaxes to finance their own expenses.[4] However, these informal revenues were unsanctioned and unmonitored by the central government, so most of the money raised slipped into the officials' own pockets.[5] Provinces only allocated a small portion of either their retained share of formal revenues or their informal revenues to irregular expenses such as famine relief.[6] During 1690 to 1722, provincial governments spent 103,000 taels of silver annually, only 1.6 per cent of their retained provincial revenues, on famine relief.

The *huohao* reform was an endeavour to formalize *huohao* in order to achieve two interconnected policy goals: to reduce corruption and enhance provincial fiscal capacity by centralizing control. First, within a province, *huohao* was to be collected at rates designated by the provincial governor and delivered to the provincial treasury. Then, 60 per cent of the remitted funds were allocated to the county magistrates and provincial governors as 'anticorruption salaries' to finance their regular expenses, reducing their incentive to collect informal surtaxes and seize public revenues. More importantly, 40 per cent of the formalized *houhao* was assigned as public funds (*gongfei*), and used to finance irregular expenses at the governors' discretion. The total annual revenue from the formalized *huohao* amounted to 4.5 million taels of silver, of which 1.8 million were reserved as public funds. In sum, by formalizing and centralizing the informal surtaxes, the reform enhanced provincial fiscal capacity by giving provincial governors more resources and a greater incentive to spend them on public goods. After the reform, during 1723–35 and 1736–60, the provincial governments spent 0.35 million and 1.35 million annually, respectively, on famine relief.

In this article, we examine whether the second goal of the reform was achieved. We test whether the *huohao* reform raised the frequency of famine relief in periods of disastrous weather by exploiting the different timing of the reform process across provinces. We restrict our dataset to 1710–60, as during this period the bulk of famine relief was financed by the provinces. Using a prefecture-level panel dataset, we find that in times of extreme drought and floods, the reform raised the frequency of famine relief by 1.05 times per prefecture, which was more than 100 per cent of the standard deviation of the dependent variable under non-disastrous weather. By exploring the dynamics of the reform's impact, we find no pre-trend, which supports the exogeneity of the reform's timing. Our results are robust to controlling for other initiatives by the central government, such as tax exemptions, the allocation of tribute grain and central fiscal revenues, the enforcement of bureaucratic monitoring, and other concurrent fiscal reforms. We also find that famine relief effectively reduced grain prices when disasters occurred, indicating

[3] Wang, *Land taxation*, pp. 17–19.
[4] Ch'ü, *Local government in China*, pp. 22–32.
[5] Ma, 'Rock, scissors, paper'.
[6] Zelin, *Magistrate's tael*, pp. 26–45.

that governors and magistrates did spend public funds on famine relief and that this had a meaningful impact on the people.

In addition, we find that the impact of the reform was unrelated to the size of anticorruption salaries, while the impact was greater when and where all formalized *huohao* had been remitted to the provinces, and when provincial public funds were larger. All these findings suggest that our main results are indeed driven by strengthened provincial capacity. Moreover, we find that the reform's impact was greater when areas faced exceptional flooding than when they experienced exceptional droughts, and the impact was greater in prefectures that had difficulties with collecting taxes, which suggests that the reform facilitated intertemporal and spatial redistribution of financial resources. These findings further enhance our belief that the main results are driven by strengthened provincial capacity.

However, for this tax reform to have a sustained effect on provincial government capacity, the central government would have to resist the urge to expropriate the newly visible revenues for its own uses. After Emperor Qianlong (1736–96) succeeded to the throne, however, the central government began to make regular checks on the expenditure of provincial public funds, to force inter-provincial transfers of the funds, and to spend them on projects and missions that would previously have been financed by central revenues. These appropriations forced provincial governments to reduce their expenditure on famine relief and withhold anticorruption salaries meant to be paid to the county administrators.

This article contributes to the literature in several ways. First, it provides evidence that fiscal formalization and centralization can enhance the provision of public goods in a premodern context.[7] In premodern Europe, famine relief was not adequately provided until the late nineteenth century.[8] In contrast, in late imperial China, preventing the people from starving was an important source of legitimacy for the state and played a key role in reducing internal conflicts.[9] The Qing state operated a network of granaries and developed sophisticated procedures to report famines and supply relief.[10] Scholars have questioned whether this system was sustainable due to the central government's weak ability to monitor its local agents.[11] Our study finds that the within-province fiscal reform gave provincial governors not only more resources but also a greater incentive not to embezzle public funds—at least for a period. By reducing the principal–agent problem, the state could effectively supply famine relief and enhance people's welfare.

Second, this article highlights that it was the lack of credible commitment that accounted for the short-lived success of this fiscal reform. Following North and Weingast, and Hoffman and Norberg, Dincecco provides empirical evidence that limited government, marked by parliamentary checks on the Crown, paved the way for the success of fiscal centralization.[12] In contrast, in premodern China, there

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[7] Dincecco, 'Fiscal centralization'.

[8] Ó Gráda, 'Making famine history'.

[9] Will and Wong, *Nourish the people*; Rosenthal and Wong, *Before and beyond divergence*, pp. 25–6, 41–3. In premodern Europe, it was state competition and warfare that drove state formation; Hoffman, 'What do states do?'; Dincecco and Prado, 'Warfare'.

[10] Will, *Bureaucracy and famine*.

[11] Will and Wong, *Nourish the people*; Hsiao, *Rural China*; Sng, 'Size and dynastic decline'; Shiue, 'Local granaries'; idem, 'Political economy'.

[12] North and Weingast, 'Constitutions and commitment'; Hoffman and Norberg, *Fiscal crises*; Dincecco, 'Fiscal centralization'.

were no institutional constraints to prevent the central government from extracting the local governments' share of formal fiscal revenues.[13] When the central government began to appropriate formalized *huohao*, provincial governments reduced famine relief and withdrew anticorruption salaries. Hence, the effect of this province-level centralization on the supply of public goods was short-lived. In this light, the reform provides a valuable lesson on the role of political institutions in the Great Divergence.

The rest of the article is organized as follows. Section I explains the historical context of the tax reform. Section II discusses the data sources. Section III presents the baseline results and discusses the endogeneity problem. Section IV introduces a set of robustness checks. Section V explores the heterogeneity of the impacts. Section VI discusses why the impacts of the reform did not endure, and section VII concludes.

I. Historical background

In the pre-industrial era, agricultural economies often experienced crop failures caused by natural disasters. For the rulers of late imperial China, preventing its people from starving was the key to maintaining social stability and the source of the legitimacy of their rule.[14] In the Qing dynasty, the state built a nationwide granary system and developed sophisticated procedures for reporting famines and conducting relief using the state's revenues, grain, and money.[15] However, as shown in figure 1, the frequency of famine relief recorded in the *Qingshilu* (*Veritable Records of Qing*) was much lower than the frequency of disasters under the reigns of Shunzhi (1644–61) and Kangxi (1661–1722). Even after 1680, with the civil war ended, and population growth and fiscal revenue starting to recover, we do not see a rise in famine relief.

Both central and provincial governments were responsible for this dearth of relief. Eighty per cent of formal revenue was remitted to the central government (75 per cent of which was from land taxes), but most was then spent on military missions, central officials' salaries, and major waterworks. In the rare cases when the central government stepped in to offer famine relief, it allocated tribute grain or central silver reserves to the affected regions. However, providing relief in these ways was only feasible in regions near the capital and provinces along the tribute grain routes. High agency costs, due to the geographic size of the empire, prevented the central government from acquiring information and providing famine relief in more remote areas.[16] As shown in table 1, in 1690–1722 the central government conducted just 0.63 relief operations per year on average. It spent 166,000 taels annually (including the values of tribute grain reallocated to the regions experiencing disasters), which equates to only 0.5 per cent of central revenue.

Provincial and county governments retained less than 20 per cent of formal revenue in their treasuries and spent it on three broad categories: (local) military supply, the imperial post, and officials' formal salaries. There was only a very

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[13] Ma and Rubin, 'Paradox of power'.
[14] Rosenthal and Wong, *Before and beyond divergence*, pp. 25–6, pp. 41–3.
[15] Will and Wong, *Nourish the people*; Will, *Bureaucracy and famine*.
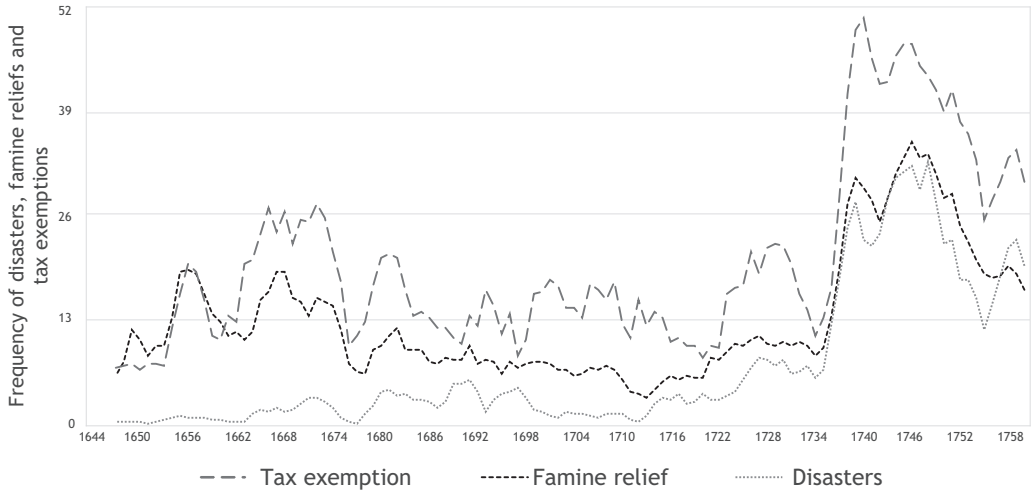[16] Sng, 'Size and dynastic decline'.

Figure 1. *Province-level frequency of disasters, famine relief, and tax exemptions, 1645–1722 (four-year moving average)*

Note: In our empirical studies, we code the frequency of famine relief at the prefecture level as our dependent variable.
Sources: Disasters are from the historical weather maps by the State Meteorological Administration, *Zhongguo*. Famine relief and tax exemption are drawn from *Qingshilu*. We code each incident as one piece of imperial decree (*shangyu*), in which multiple prefectures or provinces were mentioned.

Table 1. *Frequency and expenditure on famine relief (in thousands of taels of silver)*

	Central annual famine relief [a]	Central annual expenditure on relief [a]	Provincial annual famine relief [a]	Provincial annual expenditure on relief [a]
1690–1722	0.63	166.4	1.53	103.2
1723–35	1.31	344.6	5.50	346.5
1736–60	3.92	1,033.2	19.52	1,352.7

	Remitted formal land tax to the centre (including tribute grain) [b]	Retained formal land tax in the provinces (including formalized huohao) [b]
1690–1722	30,120	6,280
1723–35	34,910	10,910
1736–60	34,910	10,910

Sources: a Qingshilu. See details in online app. S1.
b Qinding Hubu Zeli.

limited balance left at the governors' discretion after these spending requirements were fulfilled.[17] Furthermore, governors had to report to the central government every year on how the money was spent. Failure to gain approval could result in the governors' dismissal or demotion.[18] As a result, provincial governors lacked both the (formal) financial resources and the incentives to use them to buy grain for storage or to provide famine relief. As shown in table 1, during 1690–1722

[17] Zelin, *Magistrate's tael*, p. 29.
[18] Ibid., pp. 16–19.

provincial governments conducted only 1.53 relief operations and spent 103,000 taels annually, only 1.6 per cent of provincial retained revenue.[19]

On the other hand, the central government did use tax exemptions to relieve people affected by famine or drought from their tax burdens. However, this practice could undermine both provincial fiscal capacity and the governors' incentive to buy grain storage and conduct famine relief. First, tax exemptions also affected the formal revenues retained by provinces, making it more difficult for them to perform their duties. Second, this practice induced moral hazard in the provinces: as the central government was likely to issue a tax exemption, provincial governors had even less of an incentive to allocate funds to grain storage and famine relief.[20] Both might explain why there was a negative correlation between tax exemptions and famine relief in 1690–1720 (figure 1).

In sharp contrast, in the reign of Yongzheng (1723–35) and in the early years of Qianlong (1736–60), famine relief became much more responsive to disasters, and tax exemptions did not decrease famine relief (figure 1). During 1723–35 there was a shrinking gap between disasters and famine relief. As shown in table 1, the annual frequency of famine relief operations by the central government rose to 1.31, and 344,600 taels-worth of silver and grain were spent per year. During 1736–60 these figures increased to 3.92 operations per year and 1.03 million taels, accounting for 3 per cent of central revenue. An even greater enhancement was seen in the provinces: during 1723–35 the frequency of famine relief by the provincial governments rose to 5.5 operations per year, and 346,500 taels were spent annually. During 1736–60 these figures increased again, to 19.5 operations per year and 1.35 million taels, absorbing 12 per cent of provincial revenue. Altogether, during 1736–60 the Qing government spent 2.38 million taels, or 5 per cent of fiscal revenues, on famine relief per year. That equals 2.1 million *shi* of grain, or 0.3 *shi* for each person who received relief: roughly one-twelfth of their annual consumption of grain.[21]

Why did the Qing state, and especially its provincial governments, take on the responsibility for 'nourishing the people' only after 1723? To explain this, we must take a closer look at the institutional change that reshaped fiscal relations between the centre, the provinces, and the counties.

In Kangxi's reign (1662–1722), both provincial and county governments received only limited portions of formal tax revenues and had to rely on *huohao*, informal levies imposed on top of formal taxes, for their own support. The counties were responsible for collecting and remitting formal taxes, while they could not retain sufficient funds to finance their regular expenses. A typical county retained only 1,000 taels per year for each county (5 per cent of formal tax revenues which amounted to 20,000 for each county). The magistrate thus received 50 taels as his formal salary, but he would need to spend 2,000–3,000 taels maintaining his office:

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[19] In addition, a large amount of famine relief was organized at the local level (within counties). Without funds designated for such purposes, county magistrates 'persuaded' local elites to donate grain or money in exchange for exam titles; Will, *Bureaucracy and famine*, p. 178; Zelin, *Magistrate's tael*, p. 50. Since no formal taxes were allocated, such practices were not recorded in *Qingshilu*. Unfortunately, local gazetteers did not record them in a systematic way.

[20] Shiue, 'Local granaries'; idem, 'Political economy'.

[21] In this period, the state conducted famine relief in 98 counties per year. Assuming half the population in these counties was affected, roughly seven million people received relief. It is estimated that annual food consumption per person was 3.6–4.5 *shi* in the middle of the eighteenth century.
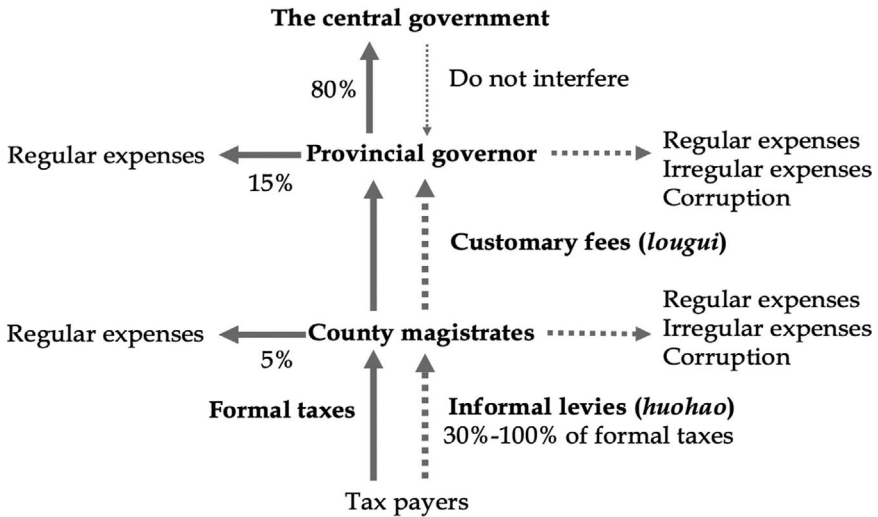
Figure 2. *Formal taxation and informal funding before the reform*

Notes: Regular expenses financed by retained formal taxes included (local) military supply, imperial post, and officials' formal salaries. Regular expenses financed by informal surtaxes included fees necessary to maintain the office, such as the wages paid to clerks, runners, and consultants; maintenance fees for government halls; and so on. Irregular expenses included famine relief, waterworks, and other 'public-interest malfeasance'. The size of *lougui* could be as high as 30,000 taels per year, 100 times the formal salaries of governors.

Sources: Authors' calculations based on numbers from Zelin, *Magistrate's tael*, and Xue, 'Qingdai yanglianyin'.

hiring consultants, clerks, and runners, not to mention other public expenses.[22] In addition, county magistrates had very limited flexibility and discretion in how they used these funds, let alone the portions belonging to the provincial and central governments. This gap between income and expenditure necessitated the collection of a *huohao* or surtax. Similarly, provincial governments also only retained a small share of formal taxes (15 per cent), which was not sufficient for their regular expenses. Hence, it was common for county governments to be forced to pay customary fees (*lougui*) to the provincial government to meet the financial needs of their superiors.[23] These fees came from *huohao* collected by local officials and were delivered to the governors as tributes and gifts. The flow of money in this informal system of public finance is illustrated in figure 2.

Informal surtaxes were collected in a less open manner than formal tax. In many cases, tax agents secretly altered tax rolls, falsified tax receipts, and manipulated weights and measures to extract payments in excess of the formal tax obligation.[24] Surtaxes were not subject to any supervision and approval process, and it was impossible to separate what was needed to provide public goods and maintain operations from the officials' personal gains.[25] Even worse, because provincial governors relied on customary fees provided by their subordinates, there was no incentive for them to monitor lower-level officials and punish their corrupt

[22] Ch'ü, *Local government in China*, pp. 22–32.
[23] Zelin, *Magistrate's tael*, pp. 54–8.
[24] Ibid., pp. 47–53.
[25] Brandt, Ma, and Rawski, 'From divergence to convergence'.

behaviour.[26] As a result, the size of *huohao* amounted to between 30 and 100 per cent of formal taxes (6,000–20,000 taels for each county).[27]

At the start of section I, we argued that provincial governors lacked the financial resources and incentives to buy grain storage and conduct famine relief using their retained share of formal revenues. This was also true for the informal customary fees, but for different reasons. First, given that informal levies were decentralized within each province, county magistrates tended to retain enough funds to satisfy their own needs before they transferred customary fees to the governors. As a result, the customary fees received by a governor were sufficient to cover their regular expenses but not large enough to undertake major irregular expenditures. Second, without proper budgetary management, even if their customary fees were abundant enough to fund relief, the governors had a greater incentive to exaggerate their own regular expenses and to pocket the fees themselves, rather than to save them to provide public goods. In addition, they requested tax exemptions from the centre when emergencies occurred.[28]

In summary, Emperor Kangxi may have believed that the state should be benevolent to its subjects and take care of their well-being. However, the very fiscal institution that the dynasty established made it financially unviable and undermined state agents' incentives to take responsibility for commoners' well-being. Emperor Yongzheng shared the same target as his father, but he was determined to make an institutional change that made this target achievable.

Shortly after Yongzheng succeeded to the throne, he launched a nationwide fiscal reform that formalized *huohao* in an attempt to achieve two interconnected policy goals: reducing corruption and enhancing provincial fiscal capacity. Because corruption is extremely difficult to explore empirically, we focus mainly on whether the second goal was achieved. The timing of the initiation and completion of the reform differed between provinces, but the reasons behind these variations in timing were largely exogenous to provincial characteristics, which means we are able to use them to test the effects of the reform. The design of the reform is illustrated in figure 3.

Under Yongzheng's reformed system, *huohao* within a province was to be collected at rates designated by the provincial governor and delivered to the provincial treasury. On average, the rate was 12 per cent of the formal tax. Then, 60 per cent of the remitted *huohao* was reallocated to the county magistrates and provincial governors for anticorruption salaries that were meant to finance their regular expenses, reducing their incentive to collect informal surtaxes and seize public revenues. A provincial governor would be paid around 10,000–20,000 taels of silver, and a county magistrate would receive 400–2,200 taels of silver per year. More importantly, 40 per cent of formalized *houhao*, called public funds, were kept to finance irregular expenses at the governors' discretion. The total revenues from

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[26] Zelin, *Magistrate's tael*, pp. 108–10. The informal network of funding was also harmful for the collection of formal revenue. As informal levies increased, the tax burden on commoners became unbearable and many counties could not fulfil their formal tax targets. They reported to the governor that it was because of the difficulty caused by famines in the past few years. As a result, the central government waived their formal taxes, so that arrears and deficits were common. See ibid., pp. 74–8.
[27] Ma, 'Rock, scissors, paper'. Commoners without privileges bore more of the burden of the informal surtax, and the rate could be as high as 100% or more of the formal tax.
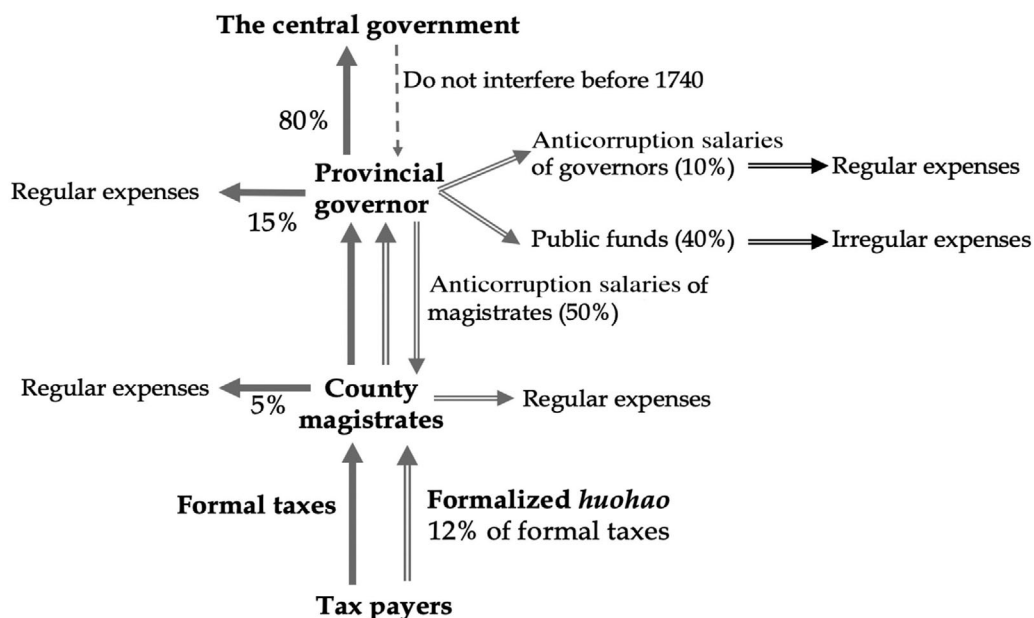[28] Shiue, 'Local granaries'.

Figure 3. *Formalized* huohao *after the reform*

formalized *huohao* was 4.5 million taels of silver annually, of which 1.8 million was reserved as public funds.[29]

Public funds could be disbursed in emergencies or stored under normal conditions. If no emergency occurred, the governor could retain these funds in the provincial treasury or transfer them to counties where the magistrates bought grain storage. Zelin documents cases where provinces used public funds to buy storage in granaries (1726 in Guangdong, 1730 in Shandong, 1733 in Jiangsu, 1726 in Shaanxi, and so on).[30] The governors could also finance large-scale famine relief and public projects that could not be financed by a single county.[31] For example, the governor of Henan disbursed 2,000 taels to a county that was badly hit by a flood.[32] Other examples include upgrading the provincial exam hall in Henan (in 1729) and enlarging the city wall of the provincial capital in Shaanxi (in 1726).[33] In section III, we test whether the reform raised the frequency of famine relief in periods of disastrous weather. In section V, we explore whether the reform's impact was indeed driven by enhanced provincial fiscal capacity and whether the impact differed across weather types and regions.

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[29] A portion of these funds was earmarked for specific items that were not financed by formal taxes, such as the transportation fee for the silver taxes remitted to the central government.
[30] Zelin, *Magistrate's tael*, pp. 180–3.
[31] Will, *Bureaucracy and famine*, p. 157.
[32] Zelin, *Magistrate's tael*, p. 170.
[33] Ibid., p. 131.

One might ask whether provincial governors (and county magistrates) could have simply pocketed the money earmarked for grain storage and famine relief and falsely reported that the grain was stored and relief delivered. First, as mentioned above, with the anticorruption salaries to maintain their offices, the governors and magistrates had less of an incentive to seize public funds. Second, with customary fees banned, provincial governors had more of an incentive to monitor their subordinates and report their misbehaviour, since their own income no longer relied on county magistrates' informal impositions on tax-payers.[34] As illegal appropriation was more likely to be subject to accusation and punishment, we would expect famine relief to be conducted step by step, according to the procedures established by the central government. In section III, we test whether famine relief in this period did actually reduce grain prices when disasters occurred. This indicates whether governors and magistrates spent the public funds on famine relief effectively.

In summary, the *huohao* reform produced within-province fiscal formalization and centralization that gave provincial governors more formal fiscal revenues and greater incentives to provide public goods, by granting them the flexibility to use public funds. To ensure the governors' fiscal autonomy, Yongzheng insisted that public funds should not be transferred between provinces or be spent on projects and missions that should be financed from the central revenues. In addition, the central government Ministry of Finance was not to interfere with the provincial expenditure of *huohao*. Yongzheng stated that without discretion over their use, provincial governors would again demand customary fees from below, and county magistrates would again collect informal surtaxes.[35] Unfortunately, Yongzheng did not establish any institutional arrangements to prevent these rules from being broken—a typical commitment problem between the central and local government under an authoritarian regime. When central government appropriation of provincial public funds later became common practice, the provinces' capability to provide famine relief was compromised. The aftermath will be discussed in detail in section VI.

II. Data

We use the records of disaster events from the historical weather maps of the State Meteorological Administration.[36] This source classifies weather in 120 'observation stations', each covering one to three prefectures. Each year is assigned to one of five categories (exceptional flood, limited flood, normal weather, limited drought, and exceptional drought) on the basis of rainfall level from May to September (the harvest season) as reported in original government records and local gazetteers. This dataset is widely used by economic historians.[37] In our empirical tests, we define the dummy variable *exceptional disaster* as equalling one if the weather ranking

[34] Ibid., p. 109.
[35] Ibid., p. 269.
[36] State Meteorological Administration, *Zhongguo*.
[37] Examples of works using these data include Shiue and Keller, 'Markets'; Jia, 'Weather shocks'.

is an exceptional flood or exceptional drought. After removing missing weather records, our prefecture-level panel dataset has 9,025 observations.[38]

We use the prefecture-level frequency of famine relief as the key dependent variable. Chen et al. collect records of county-level famine relief from the *Qingshilu* (the day-by-day official records of events during the Qing Dynasty).[39] Ninety per cent of the records of relief do not specify which level of government financed the relief effort or how much grain or silver was distributed. However, according to Will, the central government did not finance famine relief efforts that cost less than 200,000 taels before 1760.[40] Therefore, for the fewer than 100 provincial-level records in the *Qingshilu* (1745–60) that specifically mention a number, we are able to distinguish between central and provincial relief, and calculate the average monetary expenditure. By combining this with our data on relief frequency, we constructed the estimate of the amount spent on famine relief by the central and provincial governments for the three subperiods of 1644–1760 reported in table 1 (the details are given in online appendix S1).

Unfortunately, most of the records of famine relief efforts reported in the *Qingshilu*, which numbered 800 in total from 1745 to 1760, only mention the prefectures involved, and not the sums spent. Therefore, we cannot establish the scale of spending on famine relief in different regions. For this reason, we use the annual frequency of famine relief at the prefecture level as the dependent variable. In addition, we restrict our dataset to 1710–60, as the bulk of relief was financed by the provinces in this period; hence it is a good measure for the effects of the *huohao* reform on provincial fiscal capacity.

To construct a measure of prefecture-level famine relief, we digitized these records and aggregated them at the prefecture level. Each time a county received famine relief, we count this as one famine relief event within the prefecture to which it belonged. In our sample, there were an average of 0.533 famine relief efforts per prefecture per year. Figure S1 in the online appendix shows the total number of prefectures that received famine relief each year.

The timing of the reform across provinces is shown in table 2. The data are taken from Zelin, who records the years when formalized *huohao* was first partially and then fully submitted to the provincial capitals, except for two provinces (Guangxi and Gansu).[41] We refer to Xue to supplement the records for these two provinces.[42] In our baseline setting, we define reform as occurring in the year when the reform was implemented, whether partially or fully. At the start of section V, we explore further whether the effects of the reform differed over these stages. More specifically, *full reform* is set equal to 1 if the counties had fully submitted *huohao* to the provincial capitals. *Partial reform* is equal to 1 if the counties had only partially submitted *huohao*.

We collect several kinds of data for robustness checks. First, an important omitted variable that might threaten our empirical results is the central government's relief efforts. We collect data on three kinds of central government actions for coping

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[38] We drop Sichuan and Yunnan provinces from our sample due to missing weather data in the reform period. In section IV, we check whether the missing data problem affects our results
[39] Chen, Xiao, and Xiong, *Qing shilu jingjishi ziliao*.
[40] Will, *Bureaucracy and famine*, p. 288.
[41] Zelin, *Magistrate's tael*, p. 127.
[42] Xue, 'Qingdai yanglianyin'.

Table 2. *Summary of the fiscal reform*

Province	Huohao *revenue* (taels of silver)	Huohao/*land tax*	Huohao *balance/revenue*	*Partial reform*	*Full reform*
Anhui	218,273	0.157	0.359		1729
Fujian	240,608	0.205	0.158	1730	Never
Gansu	152,830	0.778	−0.081		1725
Guangdong	232,763	0.269	0.028	1724	Never
Guangxi	43,107	0.140	0.080	1728	1728
Guizhou	162,861	2.818	0.382		1725
Henan	421,117	0.143	0.225		1724
Hubei	285,908	0.289	0.278	1723	1728
Hunan	158,946	0.145	−0.001	1723	1728
Jiangsu	439,400	0.118	0.378		1728
Jiangxi	233,032	0.198	0.103	1728	1729
Shandong	473,134	0.157	0.318	1724	1730
Shaanxi	237,178	0.175	0.172	1723	1725
Shanxi	369,254	0.162	0.232		1723
Sichuan	292,900	1.299	0.256		1727
Yunnan	181,124	1.984	0.064		1725
Zhejiang	162,362	0.060	−0.048	1724	1727
Zhili	302,262	0.145	0.114	1723	1729

Sources: Huohao revenue and surplus are from the *Qingding Hubu Zeli*; land tax from Liang, *Zhongguo*, p. 293; and reform timing from Zelin, *Magistrate's tael*, p. 127.

with famine: the yearly amount of tribute grain (in millions of *shi*) allocated to a province, the yearly amount of formal tax revenues (in thousands of silver taels) allocated to a province for famine relief, and the yearly frequency of tax exemptions granted to a prefecture. All these actions were recorded in the *Qingshilu*. Second, we include hundreds of provincial governor fixed effects to account for the possibility that capable and trusted governors were more likely to push through the reform and enforce famine relief. The governor data are drawn from the *Qingdai Zhiguan Nianbiao* (*Chronology of Qing dynasty officials*).[43] Third, we include the yearly number of officials who were accused of misdoings at the provincial level (obtained from the *Qingshilu*) to rule out the possibility that the effects of the reform come entirely from the stricter supervision of officials. Furthermore, we include the adoption of head tax reform in the concurrent period from Liu's paper as a control variable.[44]

To test the effectiveness of famine relief, we draw grain price data from a database constructed by Wang.[45] The data provide detailed monthly grain prices for each prefecture from 1736. We use the average, minimum, and maximum price for each year. In the analysis of heterogeneous effects, we collect data on the total spent on the anticorruption salaries of county magistrates at prefecture level from the *Da-Qing Huidian Shili* (*Authorized Handbook on Administration and Institution of the Grand Qing*).[46] We also obtain the size of public funds and *huohao* revenues of each

[43] Qian, *Qingdai zhiguan nianbiao*.
[44] Liu, 'Tax reform'.
[45] Wang, 'Qingdai liangjia ziliao ku'.
[46] *Da-Qing Huidian Shili*. This handbook was compiled by the Qing government and describes the structure of each administrative institution of the central and local governments, and provides rules for the administration of each kind of issue regulated by the government.

Table 3. *Summary statistics*

Variable	Definition	Obs.	Mean	Std. dev.
Dependent variables				
Relief	Incidents of famine relief	9,025	0.533	2.348
Average grain price	Mean of grain price in a year	3,672	137.380	39.850
Weather and reform conditions				
Reform (yes = 1)	Whether the province has adopted the reform[a]	9,025	0.713	0.452
Full reform (yes = 1)	Counties of the province have fully submitted *huohao*	9,025	0.293	0.455
Partial reform (yes = 1)	Counties of the province have partially submitted *huohao*[b]	9,025	0.293	0.455
Exceptional disaster (yes = 1)	Whether ranking is exceptional flood or exceptional drought	9,025	0.152	0.359
Exceptional flood	Dummy for exceptional flood	9,025	0.088	0.283
Limited flood	Dummy for limited flood	9,025	0.238	0.426
Limited drought	Dummy for limited drought	9,025	0.183	0.387
Exceptional drought	Dummy for exceptional drought	9,025	0.064	0.245
Control variables				
Punishment	No. of officials being accused and discharged	9,025	0.740	1.463
Tribute grain allocation	Amount of tribute grain (in millions of *shi*) allocated to a province	9,025	0.027	0.120
Central government relief funding	Amount of formal tax revenues (in thousands of silver taels) allocated to a province	9,025	9.231	84.990
Tax exemption	Frequency of tax exemptions for a prefecture	9,025	1.033	3.336
Time-invariant variables				
Difficulty of taxation	Whether collecting tax is difficult	207	0.303	0.461
Anticorruption salaries	Average salary (in silver taels)	207	907.220	224.560

Sources: Authors' calculations based on data described in section II.

province from the *Qinding Hubu Zeli* (*Authorized Rule and Reference of the Ministry of Finance*). Finally, to assess the heterogeneity of the impacts of the reform, we collected data on four regional administrative characteristics that were assessed by the state. For each of the four characteristics, we accordingly construct a dummy variable.[47]

To summarize, our data are composed of a balanced panel with 207 prefectures from the years 1710 to 1760. Table 3 reports the summary statistics.

III. Main results

In section II, we documented that 40 per cent of formalized *huohao* was remitted to provincial governors as provincial public funds to cover irregular expenses and that they had full discretion to use it as they thought fit. Hence, we hypothesize that after the reform, provincial governments were more likely to conduct famine

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[47] The labels used were as follows: *Chong*: having an important location in the imperial post road network; *Fan*: handling onerous administrative affairs due to numerous lawsuits; *Pi*: having difficulties with meeting the land tax quota because the region had low land productivity and a greater propensity to protest against the tax burden; *Nan*: a region where it was difficult to maintain social stability and where there were numerous cases of theft, robbery, and murder. These designations are coded based on Liu, 'Chong, Fan, Pi, and Nan'.

Table 4. *Baseline results: reform and famine relief*

Dependent variable	Relief (1)	Relief (2)	Relief (3)	Relief (4)	Relief (5)	Relief (6)
Exceptional disaster	0.808***	−0.066	−0.031	0.991***	−0.067	−0.003
	(0.101)	(0.045)	(0.037)	(0.100)	(0.055)	(0.044)
Limited disaster				0.370***	−0.025	0.037
				(0.046)	(0.046)	(0.044)
Reform		−0.305***	−0.342***		−0.491***	−0.488***
		(0.113)	(0.081)		(0.105)	(0.085)
Exceptional disaster × reform		1.060***	1.045***		1.270***	1.219***
		(0.164)	(0.156)		(0.171)	(0.160)
Limited disaster × reform					0.393***	0.314***
					(0.076)	(0.075)
Year fixed effects		Y	Y		Y	Y
Prefecture fixed effects		Y	Y		Y	Y
Province-specific time trends			Y			Y
Observations	9,025	9,025	9,025	9,025	9,025	9,025
R^2	0.015	0.096	0.113	0.021	0.100	0.116

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

relief in cases of exceptional harvest failures *relative to* normal situations where no intermediate interventions were needed. To test this hypothesis, we adopt a difference-in-difference specification, including an interaction between the reform dummy and the dummy for an exceptional disaster as follows:

$$\text{Relief}_{it} = \alpha + \beta_1 \text{Reform}_{it} + \beta_2 \text{Exceptional Disaster}_{it} \times \text{Reform}_{it}$$
$$+ \beta_3 W_{it} + \mu_i + \sigma_t + \lambda_{prov} \times t + \varepsilon_{it} \tag{1}$$

where Relief_{it} corresponds to the frequency of famine relief in prefecture i in year t, β_1 measures the effect of the reform on the famine relief conditional on it being normal weather or a limited disaster, and $\beta_1 + \beta_2$ captures the effect of the reform conditional on it being an exceptional disaster. Therefore, β_2 captures the effects of the reform on the responsiveness of provincial governments in the case of exceptional disasters, *relative to* other cases. We will further differentiate among all five types of weather in section V. We also control for exceptional disasters (W_{it}) which are time-variant, prefecture fixed effects (μ_i), and year fixed effects (σ_t). In addition, to allow for heterogeneity in the trends of relief efforts across provinces, we include a set of province-specific linear time trends ($\lambda_{prov} \times t$).[48]

The results of estimating equation 1 are reported in table 4. Column 1 shows that experiencing an exceptional disaster is correlated with a higher frequency of famine relief. Column 2 includes the reform dummy and an interaction between the reform dummy and the dummy for exceptional disasters. Column 3 (our baseline results) further controls for province-specific time trends. The results show that in exceptionally disastrous weather, regions had a higher frequency of famine relief (1.05 relief efforts per prefecture) after the reform relative to relief during periods

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[48] In the baseline regressions, we cluster the standard errors at prefecture level. In col. 3 of online app. tab. S1, we also check that our results are robust to clustering standard errors at province level.

with other weather conditions, and the coefficient is significant at the 1 per cent level. Compared to the mean of the dependent variable (0.53 efforts per prefecture per year), the impact of the *huohao* reform is economically large.

In columns 4 to 6, we use dummy variables both for limited disasters and for exceptional disasters. Columns 5 and 6 interact these two weather dummy variables with the reform dummy variables. Column 6 shows that the effects of the reform are positive and significant for both limited and exceptional disasters but the magnitude is almost four times as large for exceptional disasters (1.22) than for limited disasters (0.31). These results indicate that after the reform provincial governments were more responsive to harvest failures relative to normal harvests, and that they were even more responsive in the case of exceptional disasters.

In columns 5 and 6, we find that regions had a *lower* frequency of famine relief after the reform during *normal* weather (−0.49). Here, we briefly propose two potential explanations without further tests. First, after the reform, the local governments had less incentive to collect informal levies. As a result, farmers' tax burdens might have decreased, leaving them with greater capacity to resist market risks. Therefore, demand for disaster relief under normal weather conditions may have decreased. Second, after the reform, local officials received transfer payments from their provincial governments and thus had less of an incentive to over-report disasters in order to skim income from relief spending. Unfortunately, without more detailed data, we are not able to test these hypotheses formally.

Although we have controlled for prefecture fixed effects and year fixed effects, we are still concerned about reverse causality. That is, those provinces that were constantly stricken by disasters and hence enacted famine relief prior to the reform might have had more of an incentive to introduce the reform and build up their fiscal capacity. This would violate the standard parallel trends assumption in difference-in-difference identification. To test whether this is the case, we use the following event-study specification to explore whether there were pre-trends in famine relief before the reform and how the effects of the reform evolved over time:

$$Relief_{it} = \alpha + \sum_n \beta_{ap}^* Period_{it} + \sum_n \beta_{bp}^* Disaster_{it} + \gamma W_{it} + \mu_i$$
$$+ \sigma_t + \lambda_{prov} \times t + \varepsilon_{it} \tag{2}$$

where *period*$_{it}$ denotes a group of five-year bin dummies that reflect how many years have passed since the adoption of the reform (and how many years before). The estimation results are illustrated in figure 4 with period −5–0 as the omitted reference period.

First, we find that β_{bp} is not significantly different from zero before the reform, thus indicating that there is no reverse causality. The assumption of parallel trends is satisfied. Second, β_{bp} continues to increase over time for 20 years after the reform but shrinks after this, although it remains positive and significant.

In table 5, we formally test whether the timing of the reform was determined by a set of provincial characteristics, such as the average frequency of famine relief and exceptional likelihood of disaster in the years 1710–20, the land tax rate in 1723, the distance to the capital (Beijing) and the number of high-ranking officials in the central government from the province in each year. In columns 1 and 2, we test whether the timing of the reform was driven by these provincial features. In columns
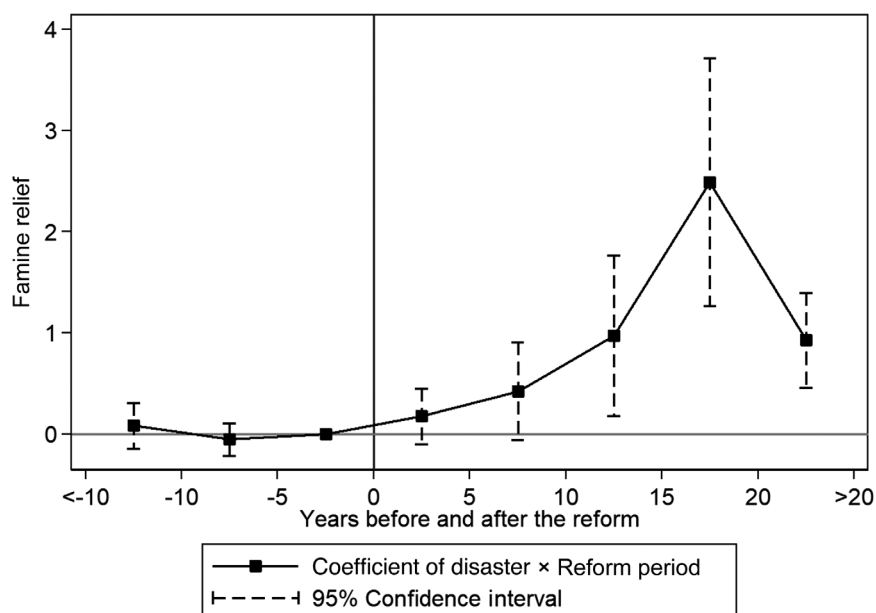
Figure 4. *Dynamic impact on famine relief*
Note: This figure shows the estimation results of β_{bp} in equation 2. Period −5–0 is treated as the reference.
Source: Based on coefficients estimated from equation 2.

3 and 4, we test the order of the implementation of the reforms across provinces. As is shown, the coefficient on the average likelihood of exceptional disaster in 1710–20 is positive and significant, indicating that the provinces with a greater likelihood of disasters preceding the reform introduced the reform relatively later. Other features cannot predict which provinces adopted the reform first. Overall, it was not the case that those provinces that were constantly stricken by disasters and hence enacted famine relief prior to the reform might have had more of an incentive to introduce the reform and build up their fiscal capacity.

Even if the reform did lead to an increase in the frequency of famine relief efforts, that is no guarantee that these interventions were implemented effectively. One possibility is that provincial governors (and county magistrates) simply pocketed the money earmarked for grain storage and famine relief and falsely reported that the grain was stored and that relief was delivered. If this were the case, the reform would have no welfare effects. To rule out this concern empirically, we explore the effectiveness of famine relief by testing whether relief reduced grain prices in years with disastrous weather.

In table 6, we regress the annual grain price on disaster dummies, the frequency of famine relief, and the interactions between them. The sample years start from 1736, the earliest year for which price data are available. As shown by the coefficients of the interaction terms, famine relief was negatively associated with grain price in years with limited disasters and exceptional disasters. The results hold for maximum, minimum, and average yearly prices. These findings confirm that famine relief effectively reduced the grain price when a disaster occurred and

Table 5.   *Reform timing and provincial characteristics*

Dependent variable	Year of reform (OLS)		Order of reform (order logit)	
	(1)	(2)	(3)	(4)
Average relief likelihood, 1710–20	−0.006	−0.006	−0.018	−0.019
	(0.005)	(0.005)	(0.017)	(0.023)
Exceptional disaster likelihood, 1710–20		20.922***		33.733***
		(6.464)		(12.043)
Average land tax rate[a]		−5.644		28.005
		(37.145)		(43.602)
Land tax revenue		−0.000		−0.000
		(0.000)		(0.000)
Political strength, 1710–30[b]		0.007*		0.007*
		(0.003)		(0.004)
Distance to Beijing		−0.005		−0.102
		(0.185)		(0.197)
Observations	18	18	18	18
R^2	0.078	0.621		
Pseudo R^2			0.0557	0.288

Notes: a The average land tax rate is the ratio of the land tax quota to the area of registered farmland.
b Political strength is the total year-number of high-ranking central officials from each province.
Standard errors in parentheses. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

Table 6.   *Effectiveness of famine relief, 1736–60*

Dependent variable	Average grain price (1)	Minimum grain price (2)	Maximum grain price (3)
Relief × exceptional disaster	−1.322**	−0.906**	−1.738**
	(0.528)	(0.411)	(0.672)
Relief × limited disaster	−0.982**	−0.550	−1.414**
	(0.455)	(0.352)	(0.585)
Exceptional disaster	7.259***	4.334***	10.18***
	(1.319)	(1.148)	(1.632)
Limited disaster	2.505**	1.653**	3.356***
	(0.984)	(0.833)	(1.234)
Relief	2.033***	1.425***	2.642***
	(0.482)	(0.360)	(0.626)
Year fixed effects	Y	Y	Y
Prefecture fixed effects	Y	Y	Y
Observations	3,740	3,740	3,740
R^2	0.417	0.369	0.414

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

that the governors and magistrates did spend the public funds on grain storage and famine relief.

IV.   Robustness checks

Although we have ruled out the possibility of reverse causality, we remain concerned about other omitted variables that may correlate with the timing and implementation of the tax reform and the frequency of famine relief. In this section,

Table 7. *Robustness checks*

Dependent variable	Relief (1)	Relief (2)	Relief (3)	Relief (4)	Relief (5)	Relief (6)
Reform	−0.603***	−0.659***	−0.542***	−0.285***	−0.286***	−0.306***
	(0.118)	(0.121)	(0.107)	(0.085)	(0.085)	(0.111)
Exceptional disaster × reform	1.063***	1.101***	1.006***	1.053***	1.054***	0.923***
	(0.163)	(0.163)	(0.170)	(0.156)	(0.157)	(0.230)
Tribute grain allocation[a]	1.911***					
	(0.359)					
Central government relief funding[b]		0.001				
		(0.001)				
Tax exemptions[c]			0.197***			
			(0.026)			
Punishment[d]					−0.003	
					(0.018)	
Head tax reform[e]						0.077
						(0.083)
Exceptional disaster × head tax reform						0.212
						(0.271)
Controls for weather	Y	Y	Y	Y	Y	Y
Prefecture fixed effects	Y	Y	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y	Y	Y
Governor fixed effects				Y		
Observations	9,025	9,025	9,025	9,025	9,025	9,025
R^2	0.108	0.100	0.164	0.155	0.155	0.096

Notes: a Tribute grain allocation denotes the yearly amount of tribute grain (in millions of *shi*) reallocated to a province as food relief (same for all prefectures in a province in a given year).
b Central government relief funding denotes the yearly amount of formal tax revenues (in thousands of silver taels) allocated to a province for famine relief (same for all prefectures in a province in a given year).
c Tax exemptions are the yearly number of tax exemptions for each prefecture.
d Punishment is the yearly number of officials who are accused of misdoing and discharged in a province (same for all prefectures in a province in a given year).
e Head tax reform is an indicator variable that equals one if a province adopted the head tax reform, and zero otherwise.
Robust standard errors in parentheses are clustered at the prefecture level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

we conduct several robustness checks to show that the results are not driven by these confounding factors.

First, one might be concerned that our results could be driven by the central government's responses to disasters. Several tools were available to the central government: it could redistribute tribute grain (that should be delivered to the capital, Beijing) to the affected regions; it could allocate its formal revenues (whether remitted to the central silver reserve or retained in provincial treasuries) to these regions; or it could announce tax exemptions to relieve the people's burden. It is likely that the central government's relief decisions might have taken into account whether the *huohao* reform had already been implemented in a province. In addition, the presence of officials originating from each province in the central government may have also played a role in influencing the implementation of the reform and in enforcing famine relief. In either case, our main result could be driven by the relief actions taken by the central government.

In columns 1 to 3 of table 7, we control for the yearly amount of tribute grain (in millions of *shi*) allocated to a province, the yearly amount of formal tax revenues (in thousands of silver taels) allocated to a province for famine relief, and the yearly frequency of tax exemptions for a prefecture. All these actions were properly

recorded in the *Qingshilu*. As shown, our main results are robust to these controls, and the magnitudes of the coefficients remain unchanged.

Another possibility is that the emperor would have appointed his trusted subordinates as provincial governors. In this case, those provinces with a capable and trusted governor might be more likely to implement the reform, and capable governors might also be better able to enforce famine relief. To eliminate such bias, in column 4 of table 7 we include individual governor fixed effects, a set of dummy variables that correspond to each governor.[49] The impact of the reform remains positive and significant (1.05).

In addition, we need to rule out the possibility that the effects of the reform came from stricter supervision and the more severe punishment of officials, which may be correlated with the implementation of the reform. In column 5, we control for the yearly number of officials who were accused of misdoing and discharged at the provincial level. Our results are robust to these controls, and the magnitudes of the estimated coefficients are similar.

Furthermore, in the 1720s, Emperor Yongzheng launched another, concurrent reform (provinces differ in the timing of the reform). This reform abolished the head tax on households and raised the formal land tax on landowners by an amount equivalent to the head tax quota (roughly 11 per cent of the land tax).[50] By reducing tax evasion, the reform enhanced fiscal capacity, which could drive our main results. In column 6, we include a set of post-head tax reform dummies and their interaction with exceptional disasters. We find that the impact of the centralization reform remains positive and significant (0.92).

Finally, we might be concerned that the difference between the reign of Yongzheng and that of Qianlong, who may have had different policy preferences, might be driving the results. Another concern is that after 1741, provincial governors started to lose their discretion over the use of *huohao*. In this case, our results might be driven by some unobservable variables that correlate with the reform status after 1741, rather than the enhanced provincial capacity fuelled by the reform. To address the first concern, in column 1 of online appendix table S1, we restrict our sample years to Yongzheng's reign (1723–36) to see if our main results hold. The results show that the impacts of the reform remain positive and significant, although the coefficient is reduced to 0.48. To address the second concern, in column 2, we allow for the additional years up to 1741 and include interactions between provincial fixed effects and a dummy for Yongzheng's reign. This is another way to control for Yongzheng's province-specific policy preferences. The results are similar to our baseline results in table 4. [51]

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[49] There were 133 governors during our sample years. The average term of duty in the sample was 6.9 years.

[50] Guo, *Qingdai de nongye*. Liu, 'Tax reform', explores the tax incidence of the head tax reform and finds that the head tax reform increased protests by commoners but had no effects on protests by the gentry, suggesting that the *de facto* effects of the reform hurt commoners rather than landowners.

[51] Furthermore, we check whether the problem of missing disaster data affects our results in online appendix S2. First, we find no significant relationship between the data being missing and reform in the full sample of 249 prefectures from 18 provinces (col. 1 of online app. tab. S2). Second, we suppose the weather was normal when the weather variable was missing (because extreme weather was more likely to be recorded). Then we re-estimate the impact of reform with and without a dummy for missing data as a control variable in cols. 2 and 3. The missing dummy shows no significant effect on relief. We find little difference in results with and without a dummy, suggesting that missing data are largely exogenous. The results are similar to our baseline results in both significance and magnitude.

Table 8. *More evidence on provincial fiscal capacity*

Dependent variable	Relief (1)	Relief (2)	Relief (3)	Relief (4)
Reform	−0.301***	−0.237**	−0.525***	−0.525***
	(0.112)	(0.111)	(0.099)	(0.099)
Exceptional disaster × reform	0.837***		0.491**	0.614***
	(0.219)		(0.219)	(0.226)
Exceptional disaster × reform × I (high salary)[a]	0.445			
	(0.316)			
Exceptional disaster × reform partial[b]		0.501**		
		(0.207)		
Exceptional disaster × reform full[b]		1.195***		
		(0.187)		
Exceptional disaster × reform × *huohao* balance ratio[c]			2.964***	3.464***
			(1.113)	(1.198)
Limited disaster × reform				0.150
				(0.112)
Limited disaster × reform × *huohao* balance ratio[c]				1.455*
				(0.811)
Controls for weather	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y
Prefecture fixed effects	Y	Y	Y	Y
Observations	9,025	9,025	9,025	9,025
R²	0.097	0.097	0.099	0.104

Notes: a I (high salary) is a dummy variable for prefectures with above-median anticorruption salary.
b Reform partial (full) is a dummy variable indicating whether a region had adopted the reform with partial (full) *huohao* centralization.
c The *huohao* balance ratio is the *huohao* balance divided by the total formalized *huohao* tax revenue.
Robust standard errors in parentheses are clustered at the prefecture level. ***, **, and * denote significance at the 1%, 5%, and 10% levels, respectively.

V. Heterogeneities

In the previous two sections, we have established that the reform raised the frequency of famine relief in disastrous weather, which effectively reduced the grain price in these cases. In this section, we will explore heterogeneities of the impacts of the reform to show that it was the strengthened provincial fiscal capacity that led to a rise in famine relief.

In section I, we documented that 40 per cent of formalized *huohao* was remitted to provincial governors as *provincial public funds* to cover irregular expenses and that they had full discretion to use them. Meanwhile, 50 per cent of formalized *huohao* was transferred back to the counties, as anticorruption salaries, to pay for their regular expenses. Another 10 per cent was used by the governors to cover their regular expenses. Hence, if our result is indeed driven by provincial fiscal capacity, we should observe three patterns of heterogeneities of the impacts of the reform.

First, the impact should be indifferent to the size of the anticorruption salary. One might be concerned that the subsidized county magistrates who became less corrupt and more responsive to the citizens might explain the rise in famine relief. In column 1 of table 8, we maintain the specifications from column 2 of table 4 except that we include a triple interaction among exceptional disaster, the reform dummy, and a dummy variable for prefectures with above-median salary. As shown, the effects of reform remain unchanged, whereas the coefficient on the triple interaction

is positive but statistically insignificant, indicating that the impact of reform cannot be explained by the better incentives of subsidized county magistrates.

Second, the impact should be greater when and where all formalized *huohao* was remitted to the provinces. This is because in provinces which experienced partial reform, the anticorruption salary of the magistrates was retained by the counties before remaining funds were remitted to the provincial governors. In this case, county magistrates tended to retain their full salary, and they were less likely to remit sufficient public funds to the governors than in the provinces with full reform, where all *huohao* was first remitted to the provincial government. In column 2, we distinguish between cases in which *huohao* was partially transferred to the provincial capitals and those in which it was fully transferred. As shown, for both reforms, the effects of the reform are positive and significant, but the effect was stronger for full reform (1.2) than for partial reform (0.5).

Finally, the impact should be greater in provinces with larger amounts of public funds in the hands of governors. The data were available for each province in 1741, as shown in table 2, and these numbers barely changed through to 1760. In columns 3 and 4, we include triple interactions between exceptional disaster (or limited disaster), the reform dummy, and the *huohao* balance ratio, defined as the amount of public funds in the hands of the governors as a share of formalized *huohao* revenues. As shown in column 4, the coefficient on the triple interactions is positive and significant, and the marginal effect of public funds is stronger for exceptional disaster (3.5) than for limited disaster (1.5). The magnitude of the reform's impact is substantially smaller (0.61), suggesting that the impacts of reform can be partially explained by the size of public funds.

These results provide strong evidence that the impact of the reform was driven by its strengthening of provincial fiscal capacity. This is consistent with our finding in section IV that the impacts were not driven by the actions of the central government.

In section I, we documented that with greater provincial fiscal capacity, the governors disbursed public funds as famine relief in disasters and bought grain storage in normal situations: in short, they undertook intertemporal smoothing. The funds also financed large-scale public goods provision that could not be financed by a single county: inter-regional risk sharing. Hence, we should observe the following patterns of heterogeneities in the reform's impact.

First, although the data for actual grain storage are not available for most years and areas, we can test for intertemporal smoothing by investigating the difference in the reform's impact among five weather types. Floods and droughts may have different impacts on harvests. Floods occurred unexpectedly and caused a great loss of lives, property, and infrastructure. Hence, they required immediate intervention.[52] As column 2 of table 9 shows, taking normal weather as the baseline, the effect of the reform was the strongest for exceptional flood conditions (1.43), followed by exceptional droughts (0.80), limited flood conditions (0.42), and limited droughts (0.27). This suggests that the stronger fiscal capacity produced by the reform enabled provinces to carry out famine relief in emergencies such as in an exceptional flood. [53]

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[52] Will, *Bureaucracy and famine*, pp. 17–18.
[53] In contrast, droughts occurred in a cumulative fashion, which allowed the governments to take several months or several years to supply famine relief.

Table 9. *Evidence on intertemporal smoothing and inter-regional risk sharing*

Dependent variable	Relief (1)	Relief (2)	Relief (3)	Relief (4)
Reform	−0.319***	−0.503***	−0.324***	−0.506***
	(0.114)	(0.110)	(0.111)	(0.102)
Exceptional flood × reform	1.237***	1.430***		
	(0.198)	(0.204)		
Exceptional drought × reform	0.605***	0.800***		
	(0.174)	(0.168)		
Limited flood × reform		0.422***		
		(0.095)		
Limited drought × reform		0.273***		
		(0.072)		
Exceptional disaster × reform			0.751***	0.961***
			(0.176)	(0.188)
Exceptional disaster × reform × difficulty of taxation			1.019***	1.008**
			(0.382)	(0.414)
Limited disaster × reform				0.392***
				(0.107)
Limited disaster × reform × difficulty of taxation				−0.012
				(0.180)
Controls for weather	Y	Y	Y	Y
Year fixed effects	Y	Y	Y	Y
Prefecture fixed effects	Y	Y	Y	Y
Observations	9,025	9,025	9,025	9,025
R^2	0.099	0.102	0.100	0.104

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

Second, if there was inter-regional risk sharing using provincial public funds, we should see a greater impact of the reform on prefectures where it was difficult to raise taxes. This is because before the reform, such regions faced tighter budget constraints and were less able to supply famine relief in disastrous weather. The reform achieved within-province centralization, enabling governors to distribute public funds across different regions within their provinces for relief purposes. Hence, the effect of the reform may have been greater in these prefectures relative to others. To explore this, we use one of the designations by the Qing government indicating the characteristics of a prefecture: *Pi* (taxes that were difficult to collect). In columns 3 and 4 of table 9, we add the triple interaction terms for difficulty of taxation, disaster, and reform. Columns 3 and 4 show that the effect of the reform on exceptional disasters was greater in prefectures that had difficulties with collecting taxes (1.008 more times) relative to those prefectures that had no such difficulties. However, there was no such heterogeneity in limited disasters. These findings indicate that there was inter-regional risk sharing (or redistribution) within a province.

Finally, we explore whether the centralization reform led to improvements in grain storage capacity. Since the data for actual grain storage are largely unavailable, we instead use the number of granaries established in each year as a proxy. We are able to do this for a sample covering 1661–1796, including all counties in Hunan Province that completed the reform in 1728. Although there was no difference in the timing of the reform within that province, the reform may have

had heterogeneous effects based on the different county characteristics. As shown in online appendix table S3, the reform led to an increase in new granaries being established annually in counties that were *chong* (a county that was at an important location on the imperial post road network) and *pi* (a county with difficulties in collecting taxes) compared to other counties. This helps explain our findings in tables 4 and 8 that provincial governments were able to extend more famine relief in cases of exceptional harvest failures and that the effect of the reform during exceptional disasters was stronger in areas which faced difficulties in collecting taxes.

In all, the patterns of heterogeneities in the reform's impacts support the argument that it was the reform's strengthening of provincial fiscal capacity that led to a rise in famine relief.

VI.   Were the effects long-lasting?

As shown in figure 4, the reform was effective in enhancing the provincial capability for famine relief in disasters for 20 years after the initiation of the reform (the mean start year is 1726). This effect reached a turning point 20 to 25 years later (1745–9), shortly after Qianlong allowed the central government to appropriate the provincial revenues that came from the legalized surtaxes. In 1741, the fifth year after Qianlong succeeded to the throne, he approved a proposal to place *huohao* under the supervision of the Ministry of Finance.[54] In 1744, Qianlong approved the proposal that Henan Province should transfer some of its retained *huohao* to support Zhili Province during a financial shortage. Before long, the appropriation of *huohao* became common practice. Many cases involved using *huohao* to finance projects that should have been financed by the central government, such as military missions.[55] This led to a situation in which the transfer payments were not fully available to the local governments.

In table 10, using province-level panel data, we test whether the appropriation of *huohao* by the central government reduced the fiscal capacity of provincial governments. The independent variable is the number of reports that mentioned that the retained *huohao* was to be transferred to another province or to the central government according to the Ministry's command.[56] There were more than 100 such incidents in Qianlong's reign (1736–96). Columns 1 and 2 show that each additional incident was associated with 0.78 less famine relief (10 per cent of its mean) in the province affected. In columns 3 and 4, we find that each incident is associated with 0.016 more incidents of withholding transfer payments to the counties in the province. Both results show that the provincial-level fiscal autonomy and capacity that had been established by the reform were undermined by these appropriations by the central government.

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[54] In the same way that the provinces' retained portions of the formal tax were supervised, a provincial government had to create a budget for *huohao* and report to the Ministry of Finance on how much and for what purpose *huohao* was spent. This essentially meant that *huohao* was treated as a formal tax; Zelin, *Magistrate's tael*, pp. 283–6.

[55] Ibid., p. 299.

[56] The cases of appropriation of *huohao* were collected by searching for relevant keywords in the titles of the reports of the governors to the Ministry of Finance (*hubu tiben*). These documents were accessed from the website of the First Historical Archives of China (http://lsdag.com).

Table 10.   *Undermining provincial fiscal autonomy, 1737–96*

Dependent variable:	Relief (mean: 8.35)		Transfer payment to counties withheld (mean: 0.23)	
	(1)	*(2)*	*(3)*	*(4)*
Appropriation of *huohao*	−0.757***	−0.782***	0.016***	0.016***
	(0.276)	(0.274)	(0.006)	(0.006)
Exceptional disaster		5.303***		−0.050**
		(1.219)		(0.025)
Year fixed effects	Y	Y	Y	Y
Province fixed effects	Y	Y	Y	Y
Observations	1,020	1,020	1,020	1,020
R^2	0.106	0.124	0.161	0.164

Notes: Standard errors are in parentheses. ***, **, * indicate significance at the 1%, 5%, 10% levels, respectively. The mean of *huohao* appropriation cases is 1.96.
Sources: The cases of appropriation of *huohao* and transfer payment to counties withheld were collected by searching for relevant keywords in the titles of the reports of the governors to the Ministry of Finance (*hubu tiben*). These documents were accessed from the website of the First Historical Archives of China (http://lsdag.com).

VII.   Conclusion

The *huohao* reform in eighteenth-century China formalized and centralized county-level informal surtaxes in the hands of provincial governments in an effort to strengthen their fiscal capacity. We have shown that this reform increased the frequency of famine relief in provinces that experienced exceptional disasters, and in turn reduced grain prices. These effects were driven by the new fiscal revenues—public funds—that it placed at governors' discretion; they were not driven by the central government's relief actions, other concurrent fiscal reforms, or transfer payments from the provinces to the counties. Moreover, the reform facilitated intertemporal smoothing and inter-regional risk sharing within provinces. The reform is an example of successful fiscal reform aimed at public goods provision in a premodern economy.

However, the positive effect of this reform disappeared as soon as the central government started to appropriate the new source of provincial income. Once the central government failed to keep its promise to leave this revenue for the provincial government, the provincial governments withheld transfer payments to the county governments, and the county magistrates in turn started to collect informal payments from the people again. Without credible commitment, improvements to institutions were transient. The long-term impact of the eventual failure of the reform was negative. Greater tax resistance may even have hindered the state from launching another centralization reform in the later Qing periods.[57] In this sense, unlimited government in premodern China provides a valuable lesson for understanding the role of political institutions in the great divergence.

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[57] Kuhn, *Rebellion and its enemies*.

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Supporting information

Additional supporting information may be found online in the Supporting Information section at the end of the article.

S1. Estimating frequency and expenditure of famine relief by the central and provincial governments
S2. Supplementary figures and tables