

Discretion, Talent Allocation, and Governance Performance: Evidence from China's Imperial Bureaucracy*

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Abstract

A long intellectual tradition starting with Max Weber views an ideal bureaucracy as a completely rule-based system. However, the effect of discretionary appointment versus a rule-based approach is theoretically ambiguous and direct evidence is scant. We study this question in China's imperial bureaucracy and exploit a unique organizational change that modified the appointments of certain governorships from a rule-based process to a more discretionary method. We show that discretionary appointments improved the quality of governors and led to better governance performance. The positive effects were more pronounced when the incentives of appointers were better aligned with the organization.

JEL codes: D73, H11, H4, M51, N25; O17

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1 Introduction

Bureaucrats, who perform state functions, are a key element of state capacity (Besley et al., 2021). Bureaucracy, however, faces many constraints in utilizing incentive devices due to the multi-tasking problem. This renders the allocation of talent more critical for public organizations. Conventional wisdom holds that the ideal bureaucracy should be a “dehumanized” and completely rule-based system, leaving little room for individual discretion (Weber, 1922). In line with this intellectual tradition, many bureaucracies adopt fixed rule-based appointment methods, such as random draws and seniority-based assignments. However, these rigid rules can be costly, for it precludes the appointer’s use of (soft) private information in allocating talent, and pays no regards to the considerable heterogeneity in positions. Thus, an ideal Weberian bureaucracy may also result in talent misallocation and organizational inefficiency (Garicano and Rayo, 2016).

The effect of discretion in appointments *compared* to rule is theoretically ambiguous. While discretion can open the door to favoritism and results in the selection of less qualified individuals (Prendergast and Topel, 1996; Xu, 2018; Colonnelli et al., 2020), it can also enable the appointer to take advantage of both private and public information in selecting competent bureaucrats, especially when the appointer has incentive alignment with the organization (Hoffman et al., 2018; Voth and Xu, 2020). How does discretion in appointments affect the quality and performance of bureaucrats? Despite the long-standing Weberian view in favor of limiting discretion, direct causal evidence remains rare.

This paper studies how discretion in appointments affects governor quality and its consequences for governance performance in the context of the imperial bureaucracy of Qing China (1644–1911). With its competitively meritocratic recruitment of bureaucrats based on the world’s oldest civil service exam system (Miyazaki, 1981; Elman, 2013; Kung, 2021), China’s imperial bureaucracy shared many prominent characteristics with modern bureaucracies (Fukuyama, 2011).¹ Importantly, the Qing imperial bureaucracy implemented an entirely rule-based appointment process for the majority of middle and junior level positions, against which it provided a clean benchmark for evaluating the effect of discretion in appointment decisions.

The main challenge for the study of discretion is the lack of variations in appointment methods *within* an organization, which makes it hard to compare the counterfactual (i.e., those appointed to the same position but under the default appointment rule). To over-

¹ The ancient Chinese state is deemed the earliest inventor of the modern-style bureaucracy (Finer, 1997a). The Qing imperial bureaucracy fulfilled many other Weberian criteria for a modern bureaucracy: clear hierarchy in the organization, merit-based selection into office, written rules and regulations, a clear separation of offices and officeholders, and salaried offices treated as careers (Metzger, 1973; Fukuyama, 2011).

come this problem, we leverage a natural experiment in the appointment method. Our analysis centers on an organizational reform in the 1730s aiming to improve personnel management. During the early Qing dynasty, prefecture governors were appointed on the basis of a seniority-based random allocation rule.² Following 1736, the appointment of certain governorships was modified to a more discretionary method, in which the emperor would interview and select governors from the eligible candidate list. Our identification strategy exploits the fact that the discretionary appointment only applied to prefectures assigned with a high regional importance rating (≥ 3), which was based on an assessment of governance features with a range of 0–4. Meanwhile, the appointment rule for prefectures with a lower rating (< 3) remained unchanged. We use this unique feature in a difference-in-differences (DID) design to compare the quality and performance of governors in the reformed prefectures vis-à-vis those in non-reformed prefectures, before and after the reform.

This study is based on a large-scale data digitization effort from over 300 volumes of historical gazetteers. We construct a unique personnel dataset covering the near universe of prefecture governors, which contains over 10,000 official appointment records across 250 prefectures for the period 1644-1820. The detailed information on the characteristics of governors (e.g., education, experience, ethnicity) allows us to examine how discretionary appointment affects governor quality. We then use a variety of other data to examine the effect of the reform on governance outcomes such as natural disasters, public goods, and social unrest.

We begin by exploring how discretion in appointments affected the quality of prefecture governors. We find that, before the reform, governors of reformed and non-reformed prefectures showed no difference in personal characteristics. However, after 1736, governors appointed by discretion were 14 percentage points more likely to have served as a governor previously. Relative to the average probability of 0.29, this implies a 47% increase. Event study results show the absence of differential pre-trends between treated and control prefectures. We also find that governors appointed by discretion had a higher degree in civil service exams. To increase the comparability between treated and control prefectures, we employ a strategy combining DID with propensity score matching to identify for each treated prefecture a control prefecture with similar characteristics, based on criteria that determined the adoption of discretionary appointment.³ We then use this matched sample to evaluate

² Qualified bureaucrats were added to the candidate list and had to wait for new appointments based on seniority. Once they were at the top of the candidate list, they would be assigned to prefectures by a random lottery. The lottery in the final allocation is an extreme case of a rule-based system, which was designed to eliminate favoritism and corruption in appointments. There were similar practices in the medieval European city-states where executives and council members were selected by lottery (Finer, 1997b; Stasavage, 2020). We provide more description of the personnel regulation in China’s imperial bureaucracy in Section 2.1.

³ We rely on as many as six measures in determining the regional importance rating: pre-reform conflict

the effect of discretion among comparable prefectures. Our results hold using the matched DID strategy.

Five years after the reform, cumulatively over 80% of governors in the treated prefectures had been newly appointed by the discretionary process. We then study the effects of discretionary appointments on governance performance. We link the governor personnel dataset to a prefecture-year panel to examine whether this personnel reform increased state responsiveness and provisions of disaster relief.⁴ In historical China, preventing people from starvation was vital to maintaining social stability and hence disaster relief was a crucial public good (Jia, 2014; Tanimoto and Wong, 2019). We find that the treated prefectures experienced more relief programs after the reform. More importantly, we find a significantly tighter connection between disaster occurrences and relief provision, indicating greater state responsiveness.⁵ The results are robust to controlling for various confounding factors and using the matched sample of comparable prefectures.

We also examine various other plausible mechanisms besides improvements in governor quality. First, it may be the case that the positive effect on disaster relief provisions could result from the upper governments' preferential policy in resource distribution in favor of high-rating prefectures. We provide several results inconsistent with this mechanism. (1) Using the text data from reports between provincial leaders and the emperor, we find that reformed prefectures did not receive more attention from the upper level of government after the reform. (2) If the upper level of government used the importance rating to allocate relief resources accordingly, we should expect high-rating prefectures also had more relief within the control (treatment) group. We compare prefectures rated "3" with those rated "4" in the treated group with respect to disaster relief but find no significant difference between the two. Similarly, we find no difference between those rated "2" and those rated lower. (3) If the preferential transfer is at play, we should find a more severe biased distribution in regions endowed with fiscal resources. However, we interact the DID term with provincial fiscal surpluses but find no heterogeneous effect. Together, the evidence coalesced in suggesting that preferential treatment in resource distribution was unlikely a major channel.

Second, governors in the treatment group may be able to obtain additional resources because they had better political connections with senior officials in the central government. By examining the hometown ties between the governors and senior officials, we find that governors in the treated prefectures were no more likely than those in the control group

frequency and incidence of weather shock, land tax, population density, distance to the national road, and terrain ruggedness. Notably, the matched sample is also well balanced on a large set of other observables.

⁴ Notably, to the best of our knowledge, the role of importance rating was restricted in the personnel management and did not directly affect other administrative or fiscal arrangements.

⁵ We find no differential changes in the incidence of natural disasters before and after the reform, suggesting that our findings are unlikely to be the results of greater demand for relief.

to have this connection with top officials in the central government. In addition, we show that governors in treated prefectures also performed better in timely surveying and reporting disasters, which is unlikely due to connection-induced resources.

Taken together, these tests above suggest that preferential resource distribution and governor’s connection are unlikely to be major drivers of our results. Moreover, we complement the results on disaster relief with evidence of social unrest as another governance outcome. DID estimates show a decrease in urban riots after the reform.⁶

Finally, we explore the conditions under which discretion is beneficial in appointment decisions. Organizational theory predicts that the net effect of discretion depends on the extent to which an appointer’s interest is aligned with the organization (Holmstrom, 1984; Aghion and Tirole, 1997; Alonso and Matouschek, 2008). We shed light on this by examining the heterogeneous effects of discretion between appointers with varying degrees of alignment with the organization. Of the governorships switched to the discretionary appointment method, the majority were under the discretion of the emperor, with a relatively small number of posts whose appointments were delegated to the provincial leaders.⁷ We thus decompose the treated prefectures into two groups and examine the costs and benefits of discretionary appointment made by the two different groups accordingly. It is obvious that the emperor had a greater stake in the overall performance of the bureaucracy than the provincial leaders, hence his interest was more closely aligned with the organization. In line with this conjecture, we find that the positive effects of discretionary appointment on governor quality and governance performance come mainly from prefectures whose governors were appointed by the emperor; in prefectures whose leaders were chosen by the provincial leaders there is little significant difference in governance performance from that of the control group. Moreover, we find evidence that the provincial leaders were abusing their power of discretion by engaging in favoritism.⁸ Taken together, the results indicate that the net effects of discretion are contingent on the interest alignment of the decision-makers.

This paper makes two main contributions. First, we contribute to the expanding literature on the personnel economics of the state (reviewed by Finan et al. (2017) and Besley et al. (2021)). A large body of work documents how incentives and monitoring work (or fail) in public sectors.⁹ Another strand of work examines the selection of bureaucrats and its impacts

⁶ The effect is particularly pronounced for riots against governments, but we find no impact on conflicts between social groups.

⁷ These prefectures were mainly smaller, less populated jurisdictions or those where there were mounting tensions with various non-Han ethnic groups.

⁸ Governors appointed by provincial leaders were more likely to have an ethnic connection with them and were under much lax monitoring. The same does not hold true for governors appointed by the emperor.

⁹ Prior work has examined the role of financial incentives (Khan et al., 2016; Leaver et al., 2021), career incentives (Bertrand et al., 2020), mission and non-monetary incentive (Ashraf et al., 2014; Khan, 2020), and monitoring (Olken, 2007; Muralidharan et al., 2021; Vannutelli, 2021)

on performance, focusing on the trade-off between financial vs. prosocial motivation (Dal Bó et al., 2013; Deserranno, 2019; Ashraf et al., 2020) and competitive admission exams (Ornaghi, 2019; Estrada, 2019; Dahis et al., 2020; Moreira and Pérez, 2021). While much of this literature centers on the entry-level selection and recruitment of lower-tier civil servants, less well known is how to effectively appoint higher-level bureaucrats from those *already screened* into bureaucracy.¹⁰ We instead focus on the internal selection of middle-tier officers—the prefecture governor—*within* China’s imperial bureaucracy. By showing the effects of discretion in bureaucratic appointments, we provide new evidence on how different appointment methods affect the quality of governors and their impacts on governance performance.¹¹

Second, we contribute to recent work on the value of discretion and autonomy in the public sector. The rule vs. discretion trade-off lies in the balance between the value of information and favoritism. Existing work has documented that discretion can give birth to favoritism and biased appointments favoring those with personal or party connections and consequently worsen performance (Xu, 2018; Barbosa and Ferreira, 2019; Brassiolo et al., 2020; Colonnelli et al., 2020; Akhtari et al., 2022). However, there is also growing evidence on the benefits of discretion and autonomy in different contexts, including public project construction (Rasul and Rogger, 2018), environment regulation (Duffo et al., 2018), officer promotion (Voth and Xu, 2020), public procurement (Decarolis et al., 2020; Bandiera et al., 2021; Bosio et al., 2022), and a wide range of bureaucratic tasks (Rasul et al., 2021).¹² We add to this literature by illustrating that discretion in the internal appointment can improve the quality and performance of selected appointees in the context of a quasi-Weberian bureaucracy. In this regard, our results are in line with the findings of Voth and Xu (2020) that the Admiralty used their discretion to promote better naval officers in the British Royal Navy. On the role of discretion in bureaucrats selection, recent work studies selection effects within a discretionary system of appointment and thus cannot evaluate the net effect of discretionary appointment against rule-based counterpart as the comparing benchmark.¹³ In contrast, our setting allows us to explicitly compare the discretionary appointment *against* its rule-based counterpart *within* the organization and identify the causal effect of discretion.

¹⁰ Existing literature mainly centers on front-line public service providers such as health care workers, teachers, and tax collectors. Important exceptions are Voth and Xu (2020), Xu et al. (2018), and Aman-Rana (2020).

¹¹ Another strand of literature is focused on comparing election and appointment as two general selection methods for officials and judges. See, e.g., Lim (2013), Whalley et al. (2013), and Hessami (2018).

¹² There is a large literature in public procurement that examines the trade-off between discretion and rule and shows mixed evidence on the role of discretion. See, e.g., Palguta and Pertold (2017), Coviello et al. (2018), Szucs (2020), Decarolis et al. (2020), Baltrunaite et al. (2021), and Carril (2021).

¹³ Several studies document the presence of patronage in discretionary appointment settings. For instance, Xu (2018) shows that British colonial governors connected to the Secretary of State were more likely to be allocated to higher salaried colonies. Colonnelli et al. (2020) find that supporters of the winning party in the Brazilian mayor elections are more likely to be employed into the public sector.

Furthermore, our results suggest that the benefits and costs of discretion depend on the incentive alignment between decision-makers and the organization.¹⁴

2 Historical Background

2.1 Bureaucratic Organization and Political Selection in Early Qing China

China has the longest history of bureaucratic governance in the world. Dating back to the birth of the Qin Empire in 221 B.C., the ancient Chinese state was the first to develop a hierarchically organized, professional, trained, and paid bureaucracy. This occurred about two millennia before the first such systems emerged in European states (Finer, 1997a; Kiser and Cai, 2003; Fukuyama, 2011). The imperial bureaucratic system reached its peak in terms of institutionalization in the Qing dynasty (1644–1912). Relying on this sophisticated state machinery, the Qing Empire, founded by the Manchu ethnic group, was the last but arguably the most successful dynasty in Chinese history in terms of territory and population (Rowe, 2009).¹⁵

The territorial administration of the enormous “Inner China” had three layers: eighteen provinces, approximately 250 prefectures, and over 1,700 counties. As the middle level of the local government, the prefecture played a crucial role in governing dozens of counties on the one hand, and implementing various policies handed over from the central and provincial government on the other. The prefecture governor was the decisive authority in matters of administration and judicature in his jurisdiction.¹⁶ Therefore, this paper focuses on the selection of prefecture governors and its effects on local governance.

The recruitment of bureaucrats was based on the civil service examination (also known as the *Keju*), which was swiftly resumed in the inception of the Qing dynasty.¹⁷ Regarded

¹⁴ This also helps to rationalize why we document a positive effect of discretion that is in contrast to existing evidence. In our setting, the emperors’ incentives were closely aligned with the organization since the survival of the regime and their lives were tied with the overall performance of bureaucracy. Thus, they were likely to make better use of discretion.

¹⁵ Originating in the northeastern region of China, Manchu Qing defeated the Ming dynasty (1368–1644) by taking advantage of the massive peasant rebellions and clique machinations of the late Ming dynasty.

¹⁶ The Shunzhi Emperor, the first emperor of the Qing dynasty, even argued that “prefecture governor is the vital element of governability, and how come we fail to achieve good governance if we select right people” (Zhao, 1993).

¹⁷ China’s civil service exam first originated during the short-lived Sui dynasty (581–618). Through the influence of the aristocracy, the civil exam in the subsequent Tang dynasty (618–907) played a limited role in promoting meritocracy, as aristocrats enjoyed disproportionate advantages during the recruitment process. The rulers of the Song dynasty (960–1276) reintroduced and thoroughly implemented this far-reaching recruitment method. Thereafter, the exam degree holders increasingly became the primary source of bureaucrats. After interruptions during the Mongol Yuan dynasty (1276–1368), the civil exam system became fully consolidated in the Ming (1368–1643) and Qing (1644–1912) dynasties (Chaffee, 1985; Yan, 1991). Miyazaki (1981) and Elman (2013) provide two comprehensive studies on the Chinese civil service

as a symbol of meritocracy, the highly competitive and open exam tested writing skills, proficiency in Confucian classics, and practical administrative knowledge (Miyazaki, 1981; Elman, 2013). All male adults, irrespective of their socio-economic background, could take the civil service exam and have a chance to obtain the candidature of officials after achieving a certain level of exam degree. Soon after the civil service examination was reinstated, officials in early Qing China were increasingly recruited via this method, despite the existence of alternative channels (e.g., office purchase, succession, and military achievement). On obtaining a certain level of degree in the civil exam (*Gongsheng* or higher degrees), candidates were qualified for imperial bureaucrats and assigned to positions depending on their civil exam rank. Bureaucrats usually started their careers from positions on the seventh rank, such as a county magistrate or a junior assistant secretary in the Six Ministries, then progressed to a vice prefect before moving to prefecture governor.

The imperial bureaucracy of Qing China developed highly institutionalized personnel regulations (Guy, 2010). Each position had specific qualification requirements and a corresponding career trajectory according to the stipulated career tracks.¹⁸ Every bureaucrat was subject to regular work assessments and debriefings on a three-year basis. Crucially, the appointment of each position was associated with specific rules and procedures. High-ranking positions above the deputy-provincial level were appointed by the emperor directly on the advice of his inner circle. Apart from these high-ranking positions, the majority of middle and junior positions followed a seniority-based random allocation rule, administered by the Ministry of Personnel (hereafter, MOP). Bureaucrats who stayed in a position for over 3 years without making any mistake would be automatically added to the candidate list and queue up for new appointments. Once they were at the top of the candidate list according to their seniority (i.e., waiting time on the list), they would be assigned to new positions by a random lottery.

The lottery allocation can be briefly described as three steps (Zhang, 2010). First, every month, retirement, rotation, promotion, and demotion created new vacancies. Second, vacant positions and the corresponding “allocation cohort” from the candidate list were determined. Finally, candidates joined a lottery conducted by the MOP in Beijing, and appointments were made based on the result of drawing lots.¹⁹ As a result, all governors were evenly

exam system. Chen and Kung (2021) show the impact of commercial revolution on the rise of meritocratic bureaucracy in Song China.

¹⁸ In Qing’s bureaucracy, all the movements of bureaucrats between positions strictly followed a “map” of position-to-position career tracks (*Pinjikao*). For instance, the official who was qualified to be a prefecture governor was confined to several positions, namely the county magistrate (*Zhixian*), the censor of the Censorate (*Jiancha Yushi*), the director, and the vice director of six ministers (*Lang Zhong* and (*Yuanwai Lang*), the vice prefect (*Fu Tongpan* and (*Zhizhong*), the associate administrator (*Tong Zhi*), and the assistant salt controller (*Yanyunshi Yuntong*).

¹⁹ The random allocation rule stemmed from the late Ming dynasty. Sun Piyang, the minister of the MOP

allocated across 250 prefectures despite their varying quality. Historians argue that the lottery allocation was largely well executed in Qing China. Corruption and manipulation in drawing lots was very rare (Will, 2002).

2.2 Reform of Appointment Methods for Prefecture Governors in 1736

While the seniority-based random appointment effectively limited corruption and favoritism in personnel management, disadvantages of such a rigid rule-based procedure became increasingly pronounced after the rulers of the Qing dynasty consolidated their power bases and sought to administer the entire country efficiently. Local officials during the reign of the Kangxi Emperor (the second emperor of the Qing dynasty) complained that, even if drawing lots ensured fairness and transparency, it failed to “select” the right man for the right post according to talent and merit” (Wang, 2007, p. 145).

Recognizing that a capable governor was a crucial determinant of local governance, the Yongzheng Emperor (the son of the Kangxi Emperor) launched a program of bureaucratic reforms. Upon ascending the throne in 1722, the emperor enacted an edict asking high-ranking officials to recommend talented people to serve the government. Shortly after that, he stopped asking for recommendations when he realized that ad hoc remedies could not resolve structural obstacles to political selection. Even if talented individuals were recruited through either the civil service exam or other channels, those candidates would not necessarily be assigned to positions requiring strong administrative skills due to the rigid lottery appointment rule. Mismatches in talent remained. Most importantly, while the emperor and his advisors agreed that a paramount goal of public personnel management was to appoint a right man to a right post, the concept of “right” was ambiguous and unable to operate in practice. In other words, what Yongzheng needed was a systematic plan.

Inspired by a proposal by Jin Hong, the provincial administrative commissioner of Guangxi province, the Yongzheng Emperor implemented a structural reform of the appointment system in the 1730s (Zhang, 2011). To identify governance challenges confronting with local governments, the Qing government assessed the governance features of each local jurisdiction using four elements: *Chong*, *Fan*, *Pi*, and *Nan*. *Chong* referred to places on busy highways and was designed to capture the characteristics of traffic hub or military significance. *Fan* stood for places with a great deal of onerous administrative burden. *Pi* represented an area with difficulty in collecting taxes. *Nan* referred to places with a high prevalence of crime and violence. Each prefecture was labeled a regional importance rating according to how many of these four elements were present. For example, prefectures containing all four elements

during the Wanli Emperor’s reign, invented the method of drawing lots to ensure fairness during the process of appointments when the Ming court was mired in faction politics (Will, 2002).

received an importance rating of 4 (Liu, 1993; Hu, 2019). Figure 1 shows the map of prefectures by importance ratings. These ratings largely remained unchanged for quite a long time except for a few adjustments. It is worthy of note as well that the importance rating system was mainly associated with personnel management and, to our knowledge, there was no concurrent policy change in other administrative or fiscal arrangements related to the importance rating.

The appointment reform for prefecture governors was enacted in 1736. Before the reform, prefecture governors were appointed by the lottery rule, with only a few informal exceptions. After that, prefecture governorships with importance ratings equal to or higher than three would be appointed at the discretion of the emperor. Nonetheless, his discretion was confined to the candidate list determined by the eligibility rule. The appointment of other governorships continued to follow the status quo rule of seniority-based random allocation. Figure 2 illustrates the appointment methods after the reform.

More specifically, once prefecture governorships with high importance ratings were vacant, the MOP would provide the emperor with a shortlist of qualified candidates, along with their resumes and serving records, for his screening on a monthly basis. The emperor selected an appropriate candidate for appointment to each post, then interviewed the chosen candidate face to face before the formal appointment took effect (Zhang, 2010). The appointment could be revoked if the emperor thought that the chosen candidate was inappropriate (Wang, 2016). Of note, among the treated governorships modified to the discretionary appointment method, most positions went under the discretion of the emperor, but a relatively small number of posts were delegated to provincial leaders (46 out of 250 prefectures). These prefectures were mainly small jurisdictions with less population or had tension with local ethnic groups.²⁰

3 Data

3.1 Personnel Data

Appointment records. We manually construct a unique dataset of prefecture governors from 1644 to 1820, covering the “High Qing” era (Rowe, 2009) under five emperors’ reigns. We undertake large-scale digitization of over 300 volumes of historical gazetteers compiled in the 18th to 19th centuries. Each gazetteer contains a chapter recording the entire history of official appointments in a region and includes rich data on bureaucrats’ personal backgrounds. For each governor, it includes information on the governor’s name, degree in the civil service

²⁰ These regions were classified as *Miaojiang*, which were deemed in complicated and unstable environments and required sufficient discretion and flexibility to deal with unexpected situations. Provincial leaders had potential advantages in local information but might also abuse their discretionary power for corruption.

exam, ethnicity, and hometown.²¹

Biographical data on senior officials. We supplement our core governor data with a biographical dataset for senior officials, constructed from the Authoritative Biographical Database compiled by the Institute of History and Philology, Academia Sinica. This database provides detailed personal backgrounds and career tracks of officials (especially senior officials) from official documents, biographies, and other historical archives. We use this source to gather information on the ethnic connection between prefecture governors and their corresponding provincial leaders.

Sanction. We complement our appointment records data with the sanction records documented in the *Veritable Records of the Qing (Qingshilu)*, which is a chronological historical book compiled by the Qing government. The *Veritable Records of the Qing* provides official records of imperial edicts and memorials about all important political activities, institutional changes, and personnel adjustments. We manually collect the sanction record of each prefecture governor from the *Veritable Records of the Qing*. We provide a detailed discussion on sources and construction of our personnel data in Appendix A.

3.2 Governance Outcome Data

Disaster relief and tax exemption. Our data on disaster relief come from [Chen et al. \(2012\)](#), who collect comprehensive records of government-conducted disaster relief programs from the *Veritable Records of the Qing*. We use the number of relief programs in a given year and prefecture. Unfortunately, our data do not have systematic information on the amount of grain or money used in each relief action. The average frequency of relief programs per prefecture per year is 0.287. We also collect data on land tax exemption, another common strategy for coping with disasters, from the same data source.

Social unrest. Maintaining social order was a paramount duty of local officials at all levels. We thus use social unrest as an alternative proxy for local governance. The data on social unrest are obtained from [Wu \(2011\)](#). Based on a large number of historical materials (e.g., local gazetteers, *Veritable Records of the Qing*), these data document various types of social unrest that erupted in urban areas in the early–middle Qing dynasty. These data detail the timing, location, background, and demands of protesters for each case of social turmoil. According to demands made during social unrest, we are able to differentiate riots against the government (e.g., due to unsatisfactory policy or corrupt officials) from conflicts between social groups (e.g., an armed confrontation between clans).

²¹ See Appendix Figure A1 for a sample of an appointment record in a prefecture gazetteer.

3.3 Other Data and Prefecture Characteristics

Natural disaster. We construct prefecture-year level data on natural disasters from the *Comprehensive Compilation of Weather Records for the Last Three Millennia of China* (Zhang, 2004), which provide a comprehensive record of various types of natural disasters in historical China, including drought, flood, plague, locust infestations, hurricane, earthquake, snowstorm, etc. In empirical tests, we use both the presence and frequency of disaster.

In addition, we complement the disaster data with rainfall shock data from *State Meteorological Society* (1981) as an alternative measure. The data report yearly discrete rainfall levels throughout China for each prefecture. The rainfall level is classified into five categories: exceptional flood, limited flood, normal, limited drought, and exceptional drought. The level is assigned according to the rainfall level from May to September, the usual harvest season of each year, relative to the normal rainfall level for that region. In our empirical tests, we measure weather shock with an indicator for whether the rainfall level is exceptional flood or exceptional drought.

Palace memorials. To capture the potential effect of preferential policy by the upper level of governments, we construct an attention index to measure the relative degree of attention that each prefecture received from higher governments. More specifically, we use the number of times a prefecture was mentioned in the government reports in a given year as a proxy for attention distribution. These reports, containing all kinds of local affairs, were sent by senior officials (e.g., provincial and sub-provincial leaders) to the emperor directly through the secret reporting system which was the most important communication channel between the emperor and his local agents. Presumably, the more times a prefecture was mentioned in government reports, the more attention and priority was assigned to the prefecture. The data on these reports along with the emperor’s reply, comment, and mandate, known as the palace memorials (*zouzhe*), are derived from the historical archives preserved in the First Historical Archives of China and National Palace Museum (see Appendix A for more details).

Prefecture characteristics. We collect data on the regional importance rating from the *Official Register* (*Jinshenlu*). In total, 114 out of 250 prefectures were treated prefectures with importance ratings higher than 2 (see Appendix Table A1). We collect rich data on prefecture characteristics regarding the treatment criterion variables. Data on major conflicts are from *China’s Military History Editorial Committee* (2003). Data on land tax are from Liang (1980). Data on population density are from Cao (2001). Data on the national road (the courier routes) are from *China Historical GIS* (2016). We calculate the terrain ruggedness by the amount of elevation difference between adjacent cells of a digital elevation grid using data provided by *United States Geographic Services* (USGS).

3.4 Descriptive Statistics

Our personnel dataset covers a total of 10,099 prefecture governor appointments. Table 1 summarizes the personal characteristics of appointed governors in the pre-reform period, by treated and control prefectures. Around 29% of governors had either a Juren or Jinshi degree, the two highest ranks in the civil service examination. Less than 20% of governors had previous governorship experience. In terms of ethnic origin, most of the governors were Han Chinese and remainings were from Manchu and Mongolian ethnic groups.²² Column 3 reports the mean difference of governor characteristics between treated and control prefectures. Before the personnel reform, governors in treated prefectures showed no difference to those assigned to control prefecture. Bureaucrats with varying civil exam ranks, experiences, and ethnicity were on average evenly appointed to prefectures, consistent with the appointment rule in the status quo where candidates waited for appointments based on seniority and were randomly assigned to prefectures.

4 Discretion in Appointment and Talent Allocation

4.1 Empirical Strategy

Our analysis begins by estimating the effects of discretion in the appointment on characteristics of prefecture governors by a difference-in-differences specification. For appointment i for prefecture governorship p made at time t , we estimate the following specification:

$$y_{ipt} = \beta \times Discretion_p \times Post_t + \theta_t + \lambda_p + \epsilon_{ipt} \quad (1)$$

where $Discretion_p$ is the treatment indicator that equals 1 if the regional importance rating of prefecture p is higher than 2, and 0 otherwise. $Post_t$ denotes a dummy set to 1 for years after 1736. θ_t denotes year fixed effects and λ_p denotes prefecture fixed effects. The dependent variable y_{ipt} measures the quality of appointed governors. We use a dummy variable that whether a governor has previously served as governor to proxy for experience. To proxy competence, we use an indicator set to one if a governor had the civil service exam rank equal to or higher than *Juren*. The coefficient of interest β estimates the effect of discretionary appointment on the quality of governors. Finally, the standard errors ϵ_{ipt} are clustered at the prefecture-level.

A key identification assumption for causal identification is that in absence of the reform,

²² The Qing bureaucracy consisted of officials with different ethnic origins including Manchu, Mongolian, and ethnic Han people. As the ruling elites, Manchu, Mongolian, and a small fraction of Han people were organized into an eight Banner system that were separated from the remaining Han Chinese.

the governor quality in reformed prefectures would have evolved similarly to the quality of non-reformed prefectures. If this assumption holds, we should observe no systematic difference in trends of the governors’ quality between the treatment and control groups before the reform. To assess the common trends assumption, we also estimate a flexible DID specification where the treatment effect is allowed to vary in each period:

$$y_{ipt} = \sum_{d=-25}^{+30} \beta_d \times Discretion_p \times Period_d + \theta_t + \lambda_p + \epsilon_{ipt} \quad (2)$$

In Equation (2), $Period_d$ is an indicator for each 5 years bin within the 30-year window around the reform. Period before 1710 is omitted as reference group. β_d captures the difference in governor quality between the treatment and control groups in period d . If the common trend assumption holds, we expect β_d to be insignificant before the appointment reform.

In our setting, all prefectures followed a unified seniority-based random allocation rule prior to the reform, implying that the common trend assumption should hold. As shown in Table 1, the randomness in the status quo appointment method implies that governors assigned to treated and control prefectures were balanced on personal characteristics. Here we provide more evidence on the random nature of the status quo appointment rule. First, Figure 3 shows the distribution of governor quality measures for treated and control prefectures in the pre-reform sample. Panel A reports the years of previous governor experiences, and Panel B reports the civil exam rank.²³ As the figures show, the two groups exhibit almost identical distribution patterns, consistent with the random allocation rule. Second, we conduct a randomness test by regressing governor characteristics on the prefecture’s importance rating separately for different periods (see Appendix Table B1). In each twenty-year window before the reform, we find no marked differences in the governor’s experience, competence, and ethnicity, among prefectures with different importance ratings. Although these exercises might not be sufficient to prove the randomness of the rule, they do suggest that the random allocation rule does not suffer from systematic manipulation.

Conditional Balance. As noted in Historical Background Section, prefectures were not randomly selected into treatment and control groups. Factors determining the treatment adoption might affect outcomes and play a greater role after the reform. We identify key determinants of the importance rating that hence determined the treatment adoption and then flexibly control the interactions of these determinants with the $Post_t$ dummy in regression.²⁴

²³ The highest degree in the civil exam, *Jinshi*, is coded as rank 8. Rank 7 and 6 correspond to *Juren* and *Gongsheng*, respectively. Appendix Table C1 provides the details of the exam rank coding.

²⁴ Given that four elements of governance feature determined the importance ratings, we use a set of variables as treatment determinants: terrain ruggedness and distance to the national road for *transportation*; population density and incidence of rainfall shock for *administrative burden*; the amount of land tax for

Table 2 reports the difference between treated and control prefectures. Unsurprisingly, two groups of prefectures differed significantly in these treatment criteria (column 3). Treated prefectures also had higher rice suitability, collected more grain tax, and had more academies. However, once conditional on the treatment criteria, two groups of prefectures show no significant differences in a large set of observables (column 4). The conditional balance test between treated and control prefectures suggests that the treatment adoption is likely to be independent of other unobservables conditional on the treatment determinants.

Matched Difference-in-Differences. To further increase the comparability between treated and control prefectures, we combine the DID strategy with matching to construct a more suitable control group: we use propensity score matching (PSM) to identify for each treated prefecture a control prefecture with similar characteristics, based on the six treatment criterion variables; then we estimate the DID model in the matched sample. We are able to obtain 89 pairs of treated and untreated prefectures after matching. Table 2, column 5 shows that treated and untreated prefecture are now very comparable in transportation conditions, population density, taxation, and conflict frequency.²⁵ More importantly, within the matched sample, treated and control prefectures are also well balanced on a set of other observables, including agricultural suitability, geographic conditions, informal taxation, educational infrastructure, and the strength of clan organizations.

4.2 Main Results

Table 3 reports the results of the estimation of Equation (1) separately on experience (columns 1–3) and competence (columns 4–6). The DID estimation shows a large and significant impact of discretionary appointment on both the experience and competence of prefecture governors. We find that governors appointed by discretion are 15 percentage points more likely to have previous governor experience after 1736 compared with governors appointed by the status quo rule. Relative to the mean value of governor experience (i.e., 0.29), this implies a striking 51% increase (column 1). The effect on competence is also sizeable as shown in column 4 of Table 3: the likelihood of having the highest civil service exam degree increases by 6 percentage points (a 16% increase relative to the average proportion). In columns 2 and 5, we add interactions between the $Post_t$ dummy and six treatment determinant variables, including pre-reform conflict frequency and incidence of weather shock, land tax, population density, distance to the national road, and terrain ruggedness. The results remain similar. Finally, to further increase the comparability between treated and control prefectures, we restrict our

taxation; and the frequency of previous major conflicts for *violence*.

²⁵ Appendix Figure B1 visualized the bias between treated and control prefectures before and after matching. Appendix Figure B2 shows the spatial distributions of the matched sample.

analysis to the matched sample with comparable characteristics where each treated prefecture is matched with a control prefecture using the PSM method, based on treatment determinants above mentioned. Columns 3 and 6 report the matched DID results. We continue to find positive and significant effects on both governors' experience and competence with litter change in magnitude.

Next, we estimate the dynamic effect of discretionary appointment in 5-year intervals, focusing on the 30-year window after the reform. Figure 4 shows the results of the estimation of Equation (2) for governor experience (panel A) and competence (panel B). Reassuringly, there are no marked differences before the reform, supporting the common trends assumption. After the reform, the effect on both experience and competence become significant and persistently positive. In Appendix Figure B3, we also find similar results when controlling for interactions between the $Post_t$ dummy and treatment determinants and using the matched sample. Taken together, we document significant positive effects of discretionary appointments on the quality of appointed governors, compared with those under the status quo rule.

Discussion and Robustness. We corroborate our results by a series of additional robustness checks. First, our results are robust to the exclusion of acting governors, short-tenure governors (i.e., term length is less than 2 years), and provincial capitals (Appendix Table B2).²⁶ Second, in columns 1 and 3 of Appendix Table B3, we show that our results hold when restricting the sample to relatively homogeneous prefectures by comparing prefectures with an importance rating of 3 (discretionary appointment) with rated 2 (rule-based appointment).²⁷ Third, our results are also robust to controlling for province-year fixed effects (columns 2 and 4 of Appendix Table B3) and using the continuous measurement for experiences by years of services (Appendix Table B4).

One potential issue in the interpretation of our results is that the discretionary appointment in the treatment group might have a negative spillover effect on the control group. As better bureaucrats were selected for appointments in the treated prefectures, the average quality of remaining candidates who were the potential governors for control prefectures may automatically decrease. This could violate the stable unit treatment values assumption (SUTVA) underlying our causal interpretation of our results (Imbens and Rubin, 2015). However, this is unlikely to affect our estimates because, at a given point in time, the number of treatment units is very small relative to the large size of the candidate pool qualified for prefecture governorships. In our study period, while there were on average 33 new appoint-

²⁶ As a political and economic hub of a province, provincial capitals might not strictly follow the random allocation procedure in the pre-reform period

²⁷ Recall that the adoption of the discretionary appointment was determined by the importance rating of each prefecture, with 3 being the cutoff.

ments in the treatment groups each year, the maximum number of potential candidates who were qualified for the appointment of prefecture governor is 3,329 at any point in time.²⁸ Given the large size of the selection pool, the potential spillover effect is likely very small. In addition, we conduct a rough estimation of such negative spillover effects on governor competence by looking at the distribution of civil exam rank for the candidate pool in 1774. Our estimations suggest that the mean value of competence in the candidate pool would decline by only 0.99%-2.10%. We provide additional discussion on SUTVA in Appendix C.

5 Discretion in Appointment and Governance Performance

As a chief leader who was in charge of a wide range of administrative and judicial affairs in his jurisdiction, a prefecture governor played an important role in local governance. Having established the positive effect of discretionary appointment on governor quality, in this section we examine whether the improvement in governors' quality affects governance performance.

5.1 Effect on Public Goods Provision and State Responsiveness

To investigate the reform's impact on governance performance, we first focus on disaster relief programs as our key outcome variable. In imperial China, disaster relief was the most important public good provided by the local government (Hao and Liu, 2020). Late imperial China was largely a stagnant agricultural economy and trapped within a Malthusian regime (Chen and Kung, 2016; Broadberry et al., 2018). Crop failures induced by weather shocks threatened people's lives and catalyzed conflicts. Therefore, preventing people from starving was essential to maintain social stability (Jia, 2014; Rosenthal and Wong, 2011). The Qing government operated a nationwide granary system and developed a detailed and institutionalized procedure for disaster relief (Will and Wong, 1991; Tanimoto and Wong, 2019). Surveying and reporting natural shocks, assessing economic losses, and implementing relief policies were the declared duties of prefecture governors. Prefecture governors also played a critical role in the cooperation with local elites to mobilize extra resources and human effort in the extreme conditions of sequential bad harvests (Will, 1990). Thus, a sufficient and timely response of governments to natural disasters is an important measure of performance.

To test this, we collect the systematic data of disaster relief and natural disaster records (see Section 3). We estimate the following DID specification in a prefecture-year panel

²⁸ It is noteworthy that candidates should complete a fixed tenure at the previous position before they got promoted, so the real number of candidate pool was smaller than 3,329 and fluctuating all the time.

dataset:

$$Y_{it} = \beta \times Discretion_i \times Post_t + \theta_t + \lambda_i + \epsilon_{ipt} \quad (3)$$

where Y_{it} is the number of relief programs in prefecture i in year t . $Discretion_i$ is an indicator for the treatment prefectures that adopted the appointment reform in 1736, and $Post_t$ is a dummy for years after 1735. $Disaster_{it}$ is a dummy variable that equals 1 if any natural disasters occurred, and 0 otherwise. As before, θ_t denotes year fixed effects and λ_i denotes prefecture fixed effects.

We first estimate coefficient β to examine the overall effect on the provision of relief without the triple differences term. Panel A of Table 4, presents the results. Column 1 reports the baseline DID results with prefecture and year fixed effects. Reformed prefectures with discretionary appointments had 0.4 more disaster relief programs after the reform compared with other prefectures that retained the rule-based appointment system. Relative to the mean number of programs (0.287), this increase is substantial. To control for impacts of initial difference between treatment and control groups, we add the treatment determinant variables (i.e., proxies of importance ratings) interacted with $Post_t$ dummy in column 2.²⁹ If the importance ratings system somehow has a direct impact on local governance besides via the assignment mechanism, its effect should largely be captured by the interactions. Our results are robust to controlling for the interactions. Moreover, column 3 reports the result using the matched sample with balanced characteristics. We continue to find a large increase in relief programs. Beyond the number of relief programs, we also use a dummy variable for the provision of disaster relief as a dependent variable in columns 4–6. We observe a large increase by 6.6 percentage points in the likelihood of relief, relative to the mean of 6.5%.

Figure 5 illustrates the event study results, in which we estimate the effect on the number of relief programs (Panel A) and the presence of relief provision (Panel B) in 5-year bins. We find no discernible differences in relief provision between the treated and control prefectures prior to the reform, supporting the common trend assumption. After the reform, we find a large and significant increase in relief provision, and the effect persists for a long time in our sample period.

Next, to test the effects on the state responsiveness, we estimate the following triple differences specification, adding $Disaster_{it}$ and its interaction with $Discretion_i \times Post_t$:

$$Y_{it} = \beta \times Discretion_i \times Post_t + \gamma Disaster_{it} + \alpha \times Discretion_i \times Post_t \times Disaster_{it} + \theta_t + \lambda_i + \epsilon_{ipt} \quad (4)$$

where the coefficient of triple differences α estimates the effect on the *link* between disaster

²⁹ The proxies of importance ratings include : pre-reform conflict frequency and incidence of weather shock, land tax, population density, distance to the national road, and terrain ruggedness.

occurrences and relief provision, which captures the effects on the state responsiveness. Panel B of Table 4 reports the results. As column 1 shows, we find that the reform led to a stronger link between relief measures and natural disaster occurrences, indicating that governors responded to disasters more actively and timely. Our results remain nearly unchanged when controlling for treatment determinant variables (column 2) and using the matched sample with balanced characteristics (column 3), suggesting that our results are not driven by the regional differences. In addition, we find consistent results using the indicator for the provision of disaster relief (columns 4–6).

In Figure 6, we present the event study results on state responsiveness. We plot the dynamic effects on relief measures (comparing treated and control prefectures) by conditions with and without disaster occurrence. As shown in Panel A, there was no systematic difference between the treated and control prefectures before the reform, either in normal or disaster cases. After the reform, while relief provision remained unchanged for normal cases, we find a sharp and sizeable increase in relief response to natural disasters, consistent with the regression version (Table 4, Panel B). The results in Panel B show a similar pattern using the dummy for relief provision.³⁰ Taken together, the results are consistent with the finding that appointment reform increased state responsiveness.

Discussion and Robustness. One may have a concern that the increase in relief provision may reflect differential disaster incidence between treated and control prefectures. In Appendix Table B5, we find that treated prefectures were not associated with any changes in rainfall level, rainfall shock, or likelihood and frequency of natural disaster. Additionally, we find no effect of the reform on the likelihood of occurrences of disasters regardless of disaster types, (Appendix Table B6), suggesting that our findings are unlikely to be driven by the demand side. Furthermore, we conduct a host of robustness checks. We find similar results when we: (1) restrict the sample to relatively homogeneous prefectures rated 3 and 2 (Appendix Table B7), (2) control for province-year fixed effects to capture province-specific time-varying shocks, such as the idiosyncratic impacts of provincial leaders (Appendix Table B8), or (3) use the frequency of natural disaster (Appendix Table B9).

5.2 Discussion on Mechanisms

How did changes in the method of appointing governors translate into more disaster relief? Our findings on talent allocation (Section 4) indicate that the improvement of governor quality is likely an important mechanism. In this section, we consider two other possible mechanisms through which the appointment reform affected the governance performance:

³⁰ In Appendix Figure B4, we show that the event study results are also robust to controlling for treatment determinant variables and using the matched sample with comparable characteristics.

preferential resource distribution, and governors' connection.

5.2.1 Preferential resource distribution by the upper level of governments

One possible alternative mechanism is that the positive effect on disaster relief provisions could be the result of upper governments' preferential policy towards the reformed prefectures. Specifically, as prefectures that adopted discretionary appointments had high importance ratings and were deemed more challenging to govern, both provincial and central governments may place more emphasis on these prefectures and prioritize them in terms of resource distribution for disaster relief policy. To the best of our knowledge, there is no such policy in disaster relief that favored high rating prefectures. Nevertheless, we conduct several exercises to test whether this is the case in practice.

First, we investigate whether reformed prefectures receive more attention from the upper level of government after the appointment reform. If a preferential policy is at play, a direct prediction of this channel is that senior officials would pay more attention to these regions. To test this prediction, we exploit the government report text data. Those reports were generated from the information transmission system of the Qing government through which the emperor directly communicated with senior local officials and made mandates regarding local governance and policies. A larger number of mentions and discussions of a prefecture in reports, therefore, reflect greater attention paid by senior officials and the central government. Following this logic, we conduct a simple text analysis of these reports to measure the attention received by prefectures. For each prefecture in each year, we construct an attention index based on the relative frequency of being mentioned in reports.³¹

The results are presented in Table 5. In column 1, we find no effect of the reform on the attention index. To examine attention particularly in disaster relief policy, we use disaster-related reports and calculate the attention index using a similar method. Column 2 shows that senior local officials did not pay more attention to reformed prefectures regarding disaster issues.

Second, we check whether relief provision is associated with the importance rating after the reform. If the upper level of government used the importance rating to allocate relief resources accordingly, we would expect that prefectures with higher rating also had more relief *within* the control (treatment) group, holding the appointment method unchanged. In column 3 of Table 5, we restrict the sample to treated prefectures and compare rating-4 prefectures with rating-3 prefectures. The result shows no significant difference between

³¹ Specifically, the attention index is calculated as the number of times that a prefecture was mentioned in the regular reports, divided by the number of times that all prefectures are mentioned in a given year (rescaled by $\times 100$).

high and low rating prefectures within the treatment group. Similarly, within the control group, we find that rating-2 prefectures did not receive more relief after the reform compared to other lower rating prefectures (column 4). These results suggest that there was no any preferential policy in disaster relief directly related to the importance rating.

Third, we examine whether the appointment reform has a larger effect on disaster relief in provinces with a greater fiscal surplus. If upper governments' preferential transfer plays a key role in our results, we would expect the effect to be larger where provincial governments have more fiscal resources for redistributing and financing relief programs. We test this prediction by interacting the DID term with provincial fiscal surpluses, measured by the time-invariant public funds surplus per capita of each province.³² Inconsistent with this prediction, Appendix Table B10 shows no effect of provincial fiscal surplus on disaster relief programs.

5.2.2 Connection-induced resource allocation

The literature on distributive politics shows that partisan alignment and connection with higher-level politicians influence the distribution of public resources in both democracies and autocracies.³³ Another alternative explanation could be that our results are driven by connection-induced resource allocation, rather than the real performance of governors in implementing relief policy. Specifically, governors appointed to the treated prefecture might have better personal connections to senior officials in the central government, enabling them to lobby for resources.

To explore this mechanism, we first test whether governors appointed by discretion have more connections with the central government. Using the background information in the governor appointment data and the biographical dataset for senior officials, we measure a governor's connection with the central government by the number of top central officials who shared hometowns with him. These top central officials included the Grand Secretaries, the Chief Councilor, and the ministers of the Six Ministries. We show the results in columns 1 of Table 6. Governors appointed to the treated prefectures did not have more connections with the central government after the reform, suggesting that better connections are unlikely to play a major role.

Second, we check the effect of appointment reform on land tax exemptions, another regular measure of disaster relief policy that did not entail resource reallocation. In the Qing

³² These data are drawn from the official archives of the Qing's Ministry of Finance (*Qinding Hubu Zeli*, 1781). The provincial public funds surplus is defined by the amount of public funds revenue quota minus regular fixed expenditure.

³³ See, for example, [Brollo and Nannicini \(2012\)](#), [Burgess et al. \(2015\)](#), [Curto-Grau and Zudenkova \(2018\)](#), and [Jiang and Zhang \(2020\)](#).

dynasty, the central government frequently issued land tax exemptions to reduce the tax burden of farmers during disasters (Will, 1990). As the introduction of land tax exemptions was only made by the central government based on the assessment of disasters reported by local officials, it reflects the governor’s performance in surveying, assessing, and reporting disasters. The rationale for this test is that if resource allocation induced by connections is the main driver of the results, we would expect that governors in reformed prefectures do not perform better in surveying and reporting disaster. The results in column 2 of Table 5 show that this is not the case. Treated prefectures under discretionary appointment also experienced a large increase in tax exemptions after the reform. In column 3, we also find a stronger link between tax exemptions and disaster occurrences, consistent with the results of relief provision we documented in Table 4. These patterns are unlikely attributable to governor’s connections.

Overall, while these tests above do not aim to disprove the preferential distribution and connection mechanisms, they suggest that these explanations are unlikely to be major drivers of our results. The empirical patterns imply that the improvement in governors’ quality is the main underlying mechanism. ³⁴

5.3 Additional Results on Social Unrest

We complement the results on public good by examining the effect of appointment reform on social unrest as an additional governance outcome. Maintaining social stability was another crucial declared duty of prefecture governors. Over-taxation, unfair and corrupt sentencing, and the untimely provision of disaster relief could be drivers of social unrest. More importantly, the failure to maintain order was harmful to governors’ career prospects.

To test the effect of the reform on social unrest, we use the same difference-in-differences specification and replace the dependent variable with social unrest. We measure social unrest with a dummy variable set to 1 if an urban riot occurred, and 0 otherwise.³⁵ The key advantage of our data is that the detailed records on riots allow us to distinguish riots against the government (e.g., protests due to unpopular policy or corrupt officials) from conflicts between social groups (e.g., an armed confrontation between clans).

The results are presented in Table 7. We find that the appointment reform decreases the probability of urban riots by 0.63 percentage points (columns 1). This effect is significant and large compared with the mean probability of riots (0.78%). The result holds when we

³⁴ One may also concern that the reform increased the turnover rate of governors. Governors appointed to treated prefectures may have longer tenure, and hence time horizon, which led to better performance. However, we do not find that governors in treated prefectures had longer or shorter length of tenure (Appendix Table B11 and Figure B5)

³⁵ We obtain the data on urban riots from Wu (2011). The data cover the 1681–1796 period.

further control for the treatment determinant variables interacted with $Post_t$ dummy (column 2), and use the matched comparable sample (column 3). In addition, we break down all riot occurrences into riots against the government and conflicts between social groups and separately examine the effect on these two types of unrest in columns 4 and 5. We find that the decrease in social unrest is primarily driven by a reduction in anti-government riots, whereas the effect on conflicts between social groups is small and insignificant. These heterogeneous effects further corroborate our findings regarding governor performance.³⁶

6 When is Discretion in Appointment Beneficial?

Findings we have shown point to a big picture that relative to the rule-based system, discretionary appointments not only improved governor quality but also exerted a positive effect on governance performance. These overall positive effects of discretion may seem to be somewhat counter-intuitive given the negative effects of discretion in public appointments documented by previous works.³⁷ This contrast lies in the double-edged sword feature of discretionary appointment that it could be beneficial because of a better use of information and harmful due to favoritism and patronage. In this section, we investigate the key trade-offs associated with discretion and ask the conditions under which its benefits outweigh its costs.

Different appointers at the top of the hierarchy differ in how they use discretionary power. As suggested by scholars, the net effect of discretion depends on the extent to which an appointer's interest is aligned with the organization (Holmstrom, 1984; Aghion and Tirole, 1997; Alonso and Matouschek, 2008). Closely aligned decision-makers have more incentive to use their discretion to select suitable and talented bureaucrats, whereas those with low interest alignment are more likely to use appointment power as a personal tool to engage in favoritism and corruption.

We shed light on this by examining the heterogeneity in who holds the discretionary power over appointments. After the appointment reform, although most prefecture governor posts with high importance ratings were appointed at the discretion of the emperor, the appointment of a small number of posts was delegated to provincial leaders in consideration of the extremely challenging environments of those regions.³⁸ In the Qing dynasty, the

³⁶ Recall that one of the governance features is the *Violence (Nan)* which is referred to places with high levels of crime and violence, we thus expect that the appeasing effect on social unrest should be more pronounced in the prefectures labeled as *Violence*. In line with our prediction, we find the greater effects of discretionary appointment in the prefectures which governance features contain *Violence* (Appendix Table B12).

³⁷ See, for example, Xu (2018) on incentives, Colonnelli et al. (2020) on patronage and public hiring, and Akhtari et al. (2022) on education service delivery.

³⁸ There were 46 prefecture posts out of 250 under the discretion of provincial leaders. These prefectures were mainly smaller jurisdictions with less population or had tension with local ethnic groups.

emperor and provincial leaders had large incentive disparities. The emperor cared about long-term regime survival and had a large stake in the overall performance of the imperial bureaucracy. During the early to the middle period of the Qing dynasty, the emperors were well-educated and diligent rulers deemed to show great statesmanship (Twitchett and Fairbank, 2002; Rowe, 2009). Compared with the emperor, provincial leaders were less closely aligned with organizational performance. Rotated frequently among different provinces, they faced short-term incentives and were more likely to abuse their power for personal gains. Anecdotal evidence documents corruption cases in which provincial leaders took bribes in the appointment of local government posts under his discretionary.³⁹ Therefore, we expect greater *positive* effects of the emperor’s discretion and more *negative* effects of provincial leaders’ discretion.

To test these predictions, we break down treated prefectures into two groups, where the emperor and provincial leaders enjoyed the discretion in appointing corresponding governors, respectively, and then examine the benefits and costs of the discretion of different decision-makers. First, we estimate the heterogeneous effects on the quality of governors. Table 8 shows that the governors appointed at the discretion of the emperor are on average more experienced and competent, whereas governors appointed by provincial leaders show no differences from those appointed under the status quo rule. Second, in Table 9 we find that these heterogeneous pattern in decision-makers also apply to the effects on governance performance. The increase in disaster relief programs, state responsiveness, and reduction in social unrest are mainly driven by prefectures under the emperor’s discretion.

Furthermore, we provide suggestive tests of whether discretionary appointment leads to favoritism. We examine the prefecture governors’ ethnic connection to the appointers and sanctions on governors. Panel A of Table 10 presents the results on ethnic tie. Column 1 shows that, compared to governors appointed by the status quo rule (in control prefectures), governors appointed by provincial leaders are more likely to have an ethnic tie to their provincial leaders. In columns 2 and 3, we examine the favoritism appointments by the ethnicity of provincial leaders. The effects are stronger for Han provincial leaders than for their Bannerman counterparts, who shared the ethnic identity with the emperor and were deemed more loyal to the empire, which is also in line with our incentive alignment hypothesis. In contrast, we do not find similar results for governors appointed by the emperor (column 4).⁴⁰

³⁹ For example, the Shandong province governor Guo Tai received bribes of 1,000 taels of silver for a junior official’s promotion to the magistrate of Pu County, of which appointment was at his discretion (Guy, 2010).

⁴⁰ An alternative interpretation is that connection-based appointments reflect that provincial leaders appointed trusty subordinates to challenging positions to improve governance rather than due to favoritism. However, the results in Table 9 show that provincial leaders’ appointment has no positive effect on governance performance. The empirical patterns present are more likely to be driven by favoritism.

We test the effect of discretion on the probability of sanction in Table 10, Panel B. More interestingly, governors under the discretion of provincial leaders were less likely to be sanctioned (columns 1). Given that sanctions that formally charged local officials with malfeasance were proposed by their provincial superiors,⁴¹ this may reflect the slack monitoring on the connected appointees. Again, we find that the negative effects on the probability of sanction are mainly driven by the Han provincial leaders (column 2), consistent with the differential pattern in favoritism appointments. The same does not hold true for prefecture governors appointed by the emperor (column 4).

Taken together, these results indicate that the benefits and costs of discretionary appointments vary with different decision-makers. Discretion in appointment does not necessarily lead to favoritism and poor performance or to the meritocratic allocation of talent and better performance. Our findings suggest that the net effect of discretion depends on the extent to which the incentives of the decision-makers are aligned with the organization.

7 Conclusions

The allocation of talent in bureaucracies is of great importance, as bureaucrats are fundamental components of state capacity. From ancient empires to modern authoritarian states and even modern democracies, discretion in government appointments has been ubiquitous throughout history. Compared to rule-based appointments, the overall effects of discretionary appointments on bureaucratic performance remain an open question. Using the special setting of the China’s imperial bureaucracy, which experienced an appointment reform that increased the discretion in appointing prefecture governorships, this paper provides a systematic account for this question. Relying on a comprehensive dataset of governor appointment records for the 1644–1820 period, linked with data on public goods and social unrest, we find that discretionary appointment improved the average quality of governors and led to an increase in public goods provision and a reduction in social unrest. Moreover, we find that these positive effects are driven by discretionary appointments made by the emperor rather than provincial leaders, whose incentives are less aligned with organizational performance. Suggestive evidence shows that provincial leaders use their discretionary power to engage in favoritism.

Our findings have broader implications. First, while prominent thoughts in the Weberian tradition emphasize rule-based decision making and the removal of personal judgment as critical for professional bureaucracies, a fixed rule-based appointment is not costless, as it may

⁴¹ Investigation and the final decision of sanction were made by the central government, based on the reports from provincial-level officials.

result in talent misallocation. We show that in an institutional environment where appointment follows a status quo rule of seniority-based random allocation, increasing discretion in appointment can be beneficial. Second, we highlight the role of incentive alignment with the organization. Crucially, our findings do not imply that discretion is always conducive to the performance of an organization. A discretionary appointment is a double-edged sword. Whether discretion is favorable or harmful depends on the extent to which the incentives of the decision-maker are aligned with organizational performance. In other words, it matters *who* uses discretion. This implication advances our understanding of the rule versus discretion debate. In our case, the status quo rule refers to a seniority-based rule combining random allocation, which might be special but not rare in practice. For instance, it is used in the assignment of municipal auditor (Vannutelli, 2021), military conscription (Angrist, 1990; Card and Cardoso, 2012), and the selection of executives and council members in the medieval European city-states (Finer, 1997b, p.964, Stasavage, 2020, p.120). Nevertheless, extrapolations to other contexts should be done with caution. In addition, the discretionary power-holder (i.e., the emperor in our case) is the principal himself and thus directly benefits from the improvement of appointment efficiency and organizational performance. This nonetheless invites further research on different types of power-holders in the discretionary decision-making process.

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Figures and tables

Figure 1. The Distribution of Regional Importance Ratings across Prefectures

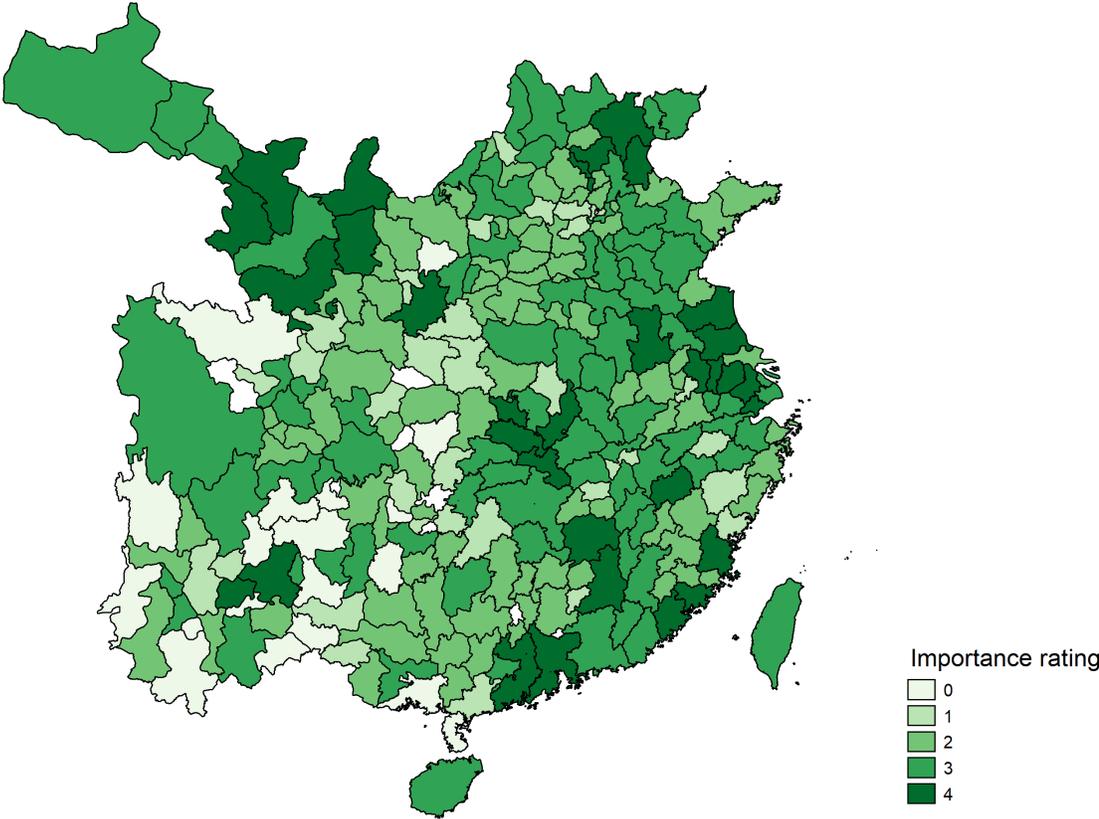


Figure 2. Illustration of Appointment Methods for Prefecture Governors after the Reform

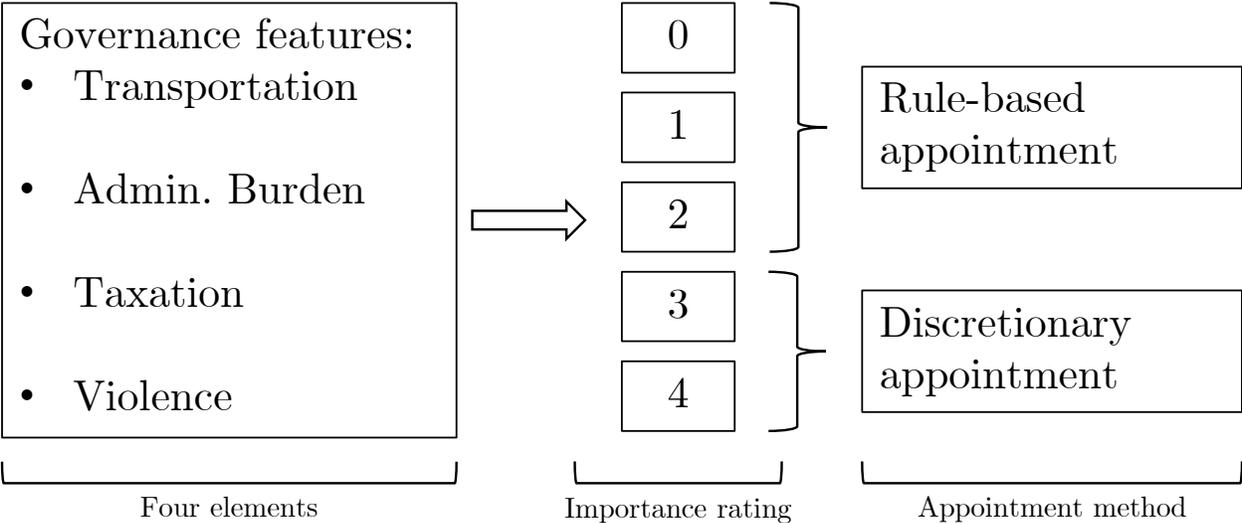
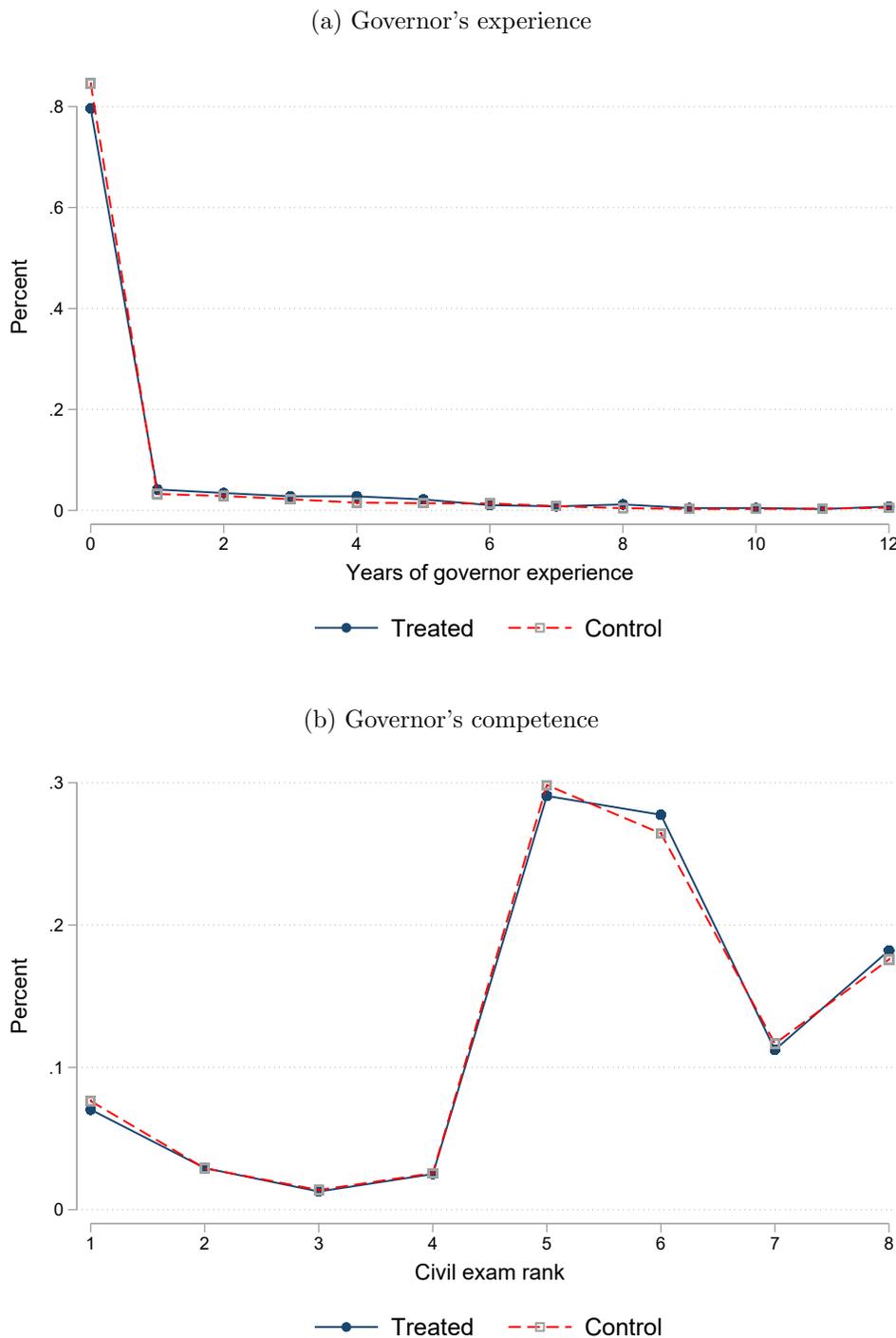
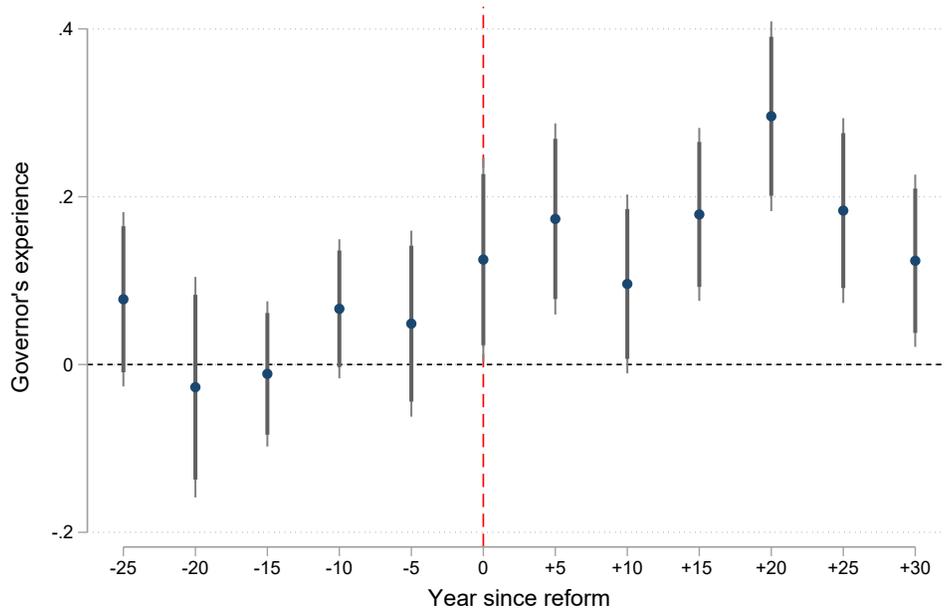


Figure 3. The Distribution of Governor Quality before the Reform: Treated vs. Control Prefectures

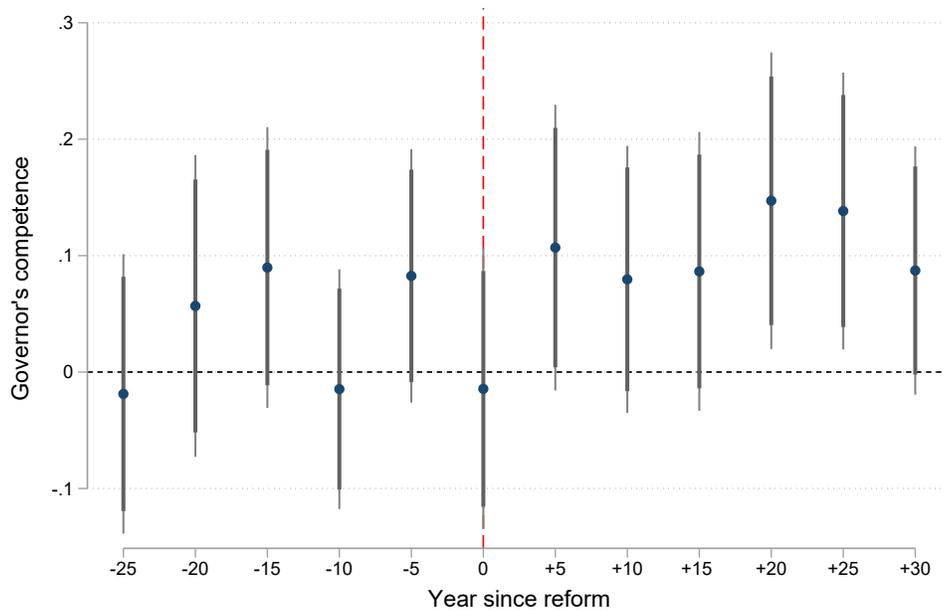


Notes: This figure shows the distribution of governor quality measure for treated and control prefectures in the pre-reform sample. Panel A reports the years of previous governor experiences, and Panel B reports the civil exam rank. See Appendix Table C1 for details of the exam rank coding.

Figure 4. Discretionary Appointment and Governor Quality: Dynamic Effects
 (a) Governor's experience

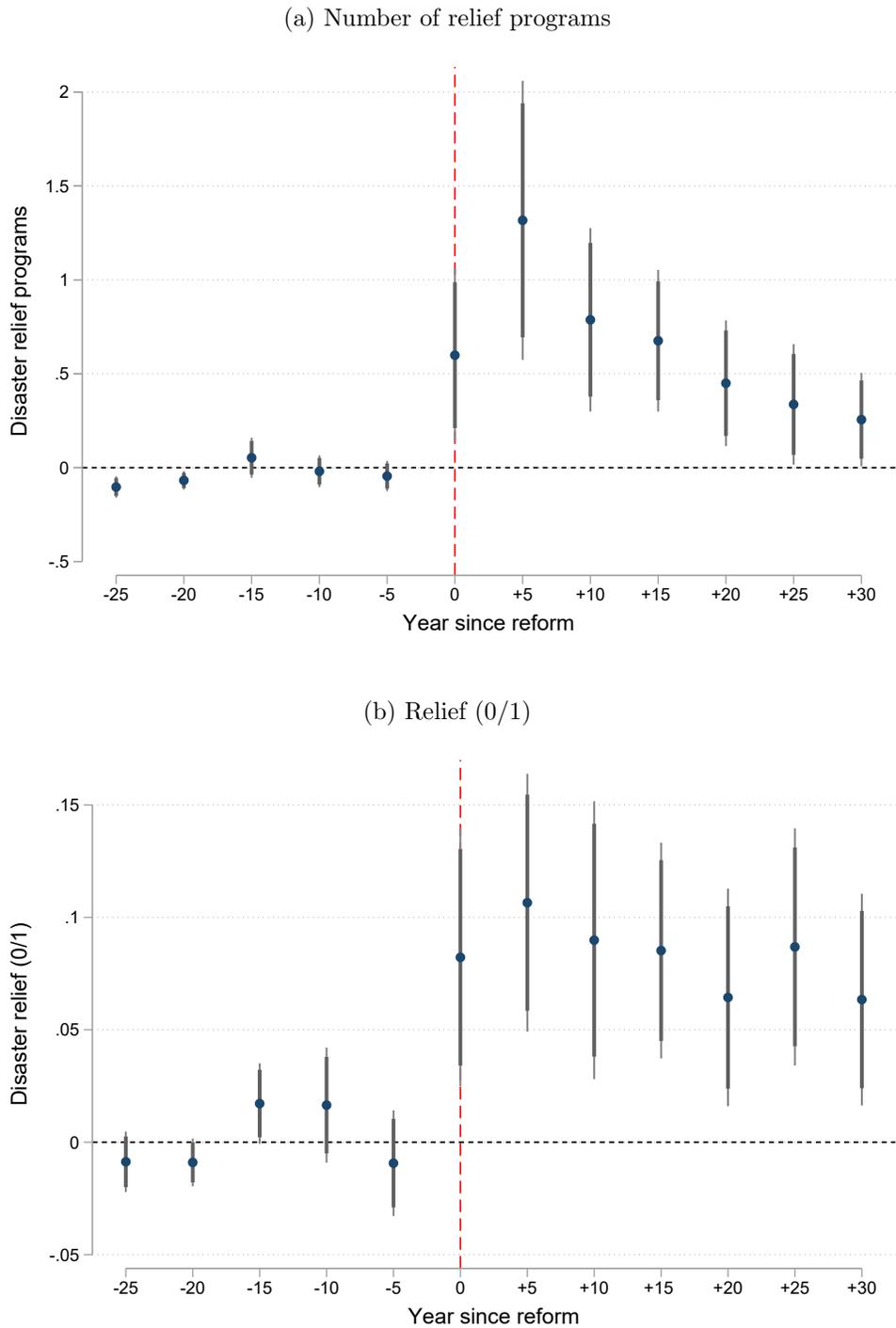


(b) Governor's competence



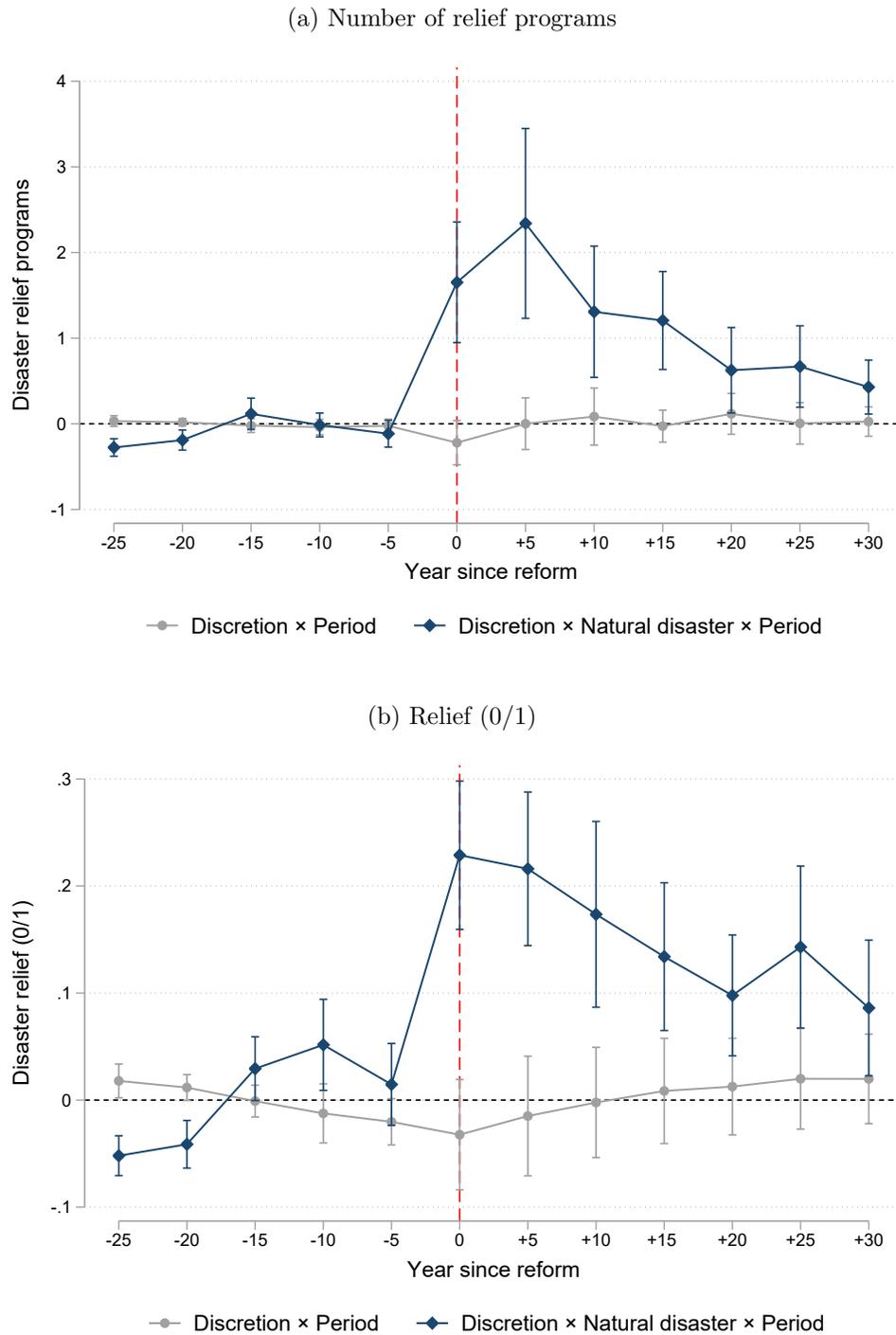
Notes: This figure presents the dynamic effects of discretionary appointment on appointed official's quality in 5 years bin by estimating equation (2). The estimated coefficients for experience and competence are shown in panel A and B, respectively. The thick black lines plot the 90% confidence intervals and the thin grey lines plot the 95% intervals. Standard errors clustered at the prefecture level. Period before 1710 is omitted as reference group.

Figure 5. Discretionary Appointment and Disaster Relief Provision: Dynamic Effects



Notes: This figure shows the dynamic effects of discretionary appointment on the provision of disaster relief in 5 years bin. In panel A, the dependent variable is the number of disaster relief programs. In panel B, the dependent variable is an indicator for the presence of any disaster relief. The thick black lines plot the 90% confidence intervals and the thin grey lines plot the 95% intervals. Standard errors clustered at the prefecture level. Period before 1710 is omitted as reference group.

Figure 6. Discretionary Appointment and State Responsiveness: Dynamic Effects



Notes: This figure shows the dynamic effects of discretionary appointment on the governmental responsiveness to natural disaster in 5 years bin. In panel A, the dependent variable is the number of disaster relief programs. In panel B, the dependent variable is an indicator for the presence of any disaster relief. The vertical solid lines with caps plot the 95% confidence intervals. Standard errors clustered at the prefecture level. Period before 1710 is omitted as reference group.

Table 1. Pre-reform Characteristics of Governors: Treated vs Control Prefectures

	(1) Control prefectures	(2) Treated prefectures	(3) Difference
Civil exam degree: <i>Juren-Jinshi</i>	0.285	0.293	0.009 (0.015)
Civil exam degree: <i>Jinshi</i>	0.171	0.177	0.006 (0.012)
Civil exam degree: <i>Juren</i>	0.114	0.117	0.003 (0.010)
Civil exam degree: <i>Gongsheng</i>	0.265	0.277	0.012 (0.014)
Experienced governor	0.166	0.182	0.016 (0.012)
Years of governor experiences	0.693	0.775	0.082 (0.067)
Ethnicity: Han	0.618	0.634	0.016 (0.016)
Ethnicity: Manchu Bannerman	0.01	0.005	-0.005* (0.003)
Ethnicity: Han Bannerman	0.372	0.36	-0.012 (0.015)
Observations	2,281	1,687	3,968

Notes: The unit of observation is appointment. The sample period is 1644-1735. Column 3 reports raw difference in means between control group and treatment group, as well as the corresponding standard errors (in parentheses). ***, **, * denote significance at 1%, 5%, 10% levels.

Table 2. Comparison of Prefectures Characteristics: Treated vs Control Prefectures

	(1) Control	(2) Treated	(3) Raw difference	(4) Conditional difference	(5) Matched difference
<i>Panel A: Treatment criteria</i>					
Distance to national road (km)	106.485	84.666	-21.819 (14.851)		-3.98 (18.553)
Terrain ruggedness	258.224	184.137	-74.087*** (21.304)		-4.436 (24.641)
Incidence of weather shock	0.119	0.144	0.025** (0.011)		0.007 (0.013)
Population density (Person/km ²)	78.99	146.151	67.160*** (13.787)		12.429 (12.588)
Land tax (1,000 silver teals)	78.848	172.278	93.430*** (16.709)		10.607 (16.327)
Major conflicts	2.485	3.123	0.638 (0.390)		-0.09 (0.473)
<i>Panel B: Other characteristics</i>					
Suitability: wheat	24.061	24.248	0.186 (1.409)	24.700 (11.469)	-2.487 (1.714)
Suitability: rice	9.139	12.904	3.765*** (1.358)	10.836 (10.417)	0.402 (1.566)
Suitability: sweet potato	9.826	7.86	-1.966 (1.223)	8.993 (9.795)	-1.216 (1.470)
Suitability: maize	22.667	22.709	0.042 (1.443)	23.26 (11.914)	-2.811 (1.779)
Latitude	110.896	112.77	1.874** (0.723)	111.888 (5.706)	-0.217 (0.858)
Longitude	30.136	31.377	1.241* (0.632)	30.933 (5.049)	0.664 (0.757)
Coast	0.103	0.167	0.064 (0.043)	0.135 (0.343)	0.045 (0.051)
Distance to Beijing	1,264.18	1,125.87	-138.312* (73.010)	1,180.43 (574.163)	-5.619 (86.314)
<i>Huohao</i> surtax rate	0.134	0.123	-0.011** (0.004)	0.128 (0.031)	-0.002 (0.005)
Grain tax (1,000 shi)	4.751	48.717	43.966*** (12.487)	14.956 (72.872)	16.698 (10.882)
Number of academies	3.287	5.456	2.169*** (0.627)	4.567 (5.154)	1.09 (0.770)
Strength of clan	19.368	81.46	62.093*** (17.924)	43.983 (127.852)	36.730* (19.020)
No. of prefectures	136	114	250	250	178

Notes: Columns 1 and 2 reports the mean for control and treated prefectures, respectively. Column 3 is the raw difference in means between control and treated prefectures. Column 4 reports the difference of these characteristics conditional on the treatment criteria. Column 5 reports the mean difference of the matched sample. The standard errors are reported in parentheses. ***, **, * denote significance at 1%, 5%, 10% levels. See Appendix A.4 for the data source of other characteristics in Panel B.

Table 3. Discretionary Appointment Improved Governor Quality

Mean of D.V.	Experiences			Competence		
	(1)	(2)	(3)	(4)	(5)	(6)
Discretion \times Post	0.148*** (0.023)	0.127*** (0.022)	0.126*** (0.027)	0.059*** (0.022)	0.066*** (0.023)	0.065** (0.026)
Year FE	Y	Y	Y	Y	Y	Y
Prefecture FE	Y	Y	Y	Y	Y	Y
Controls \times Post		Y			Y	
PSM sample			Y			Y
Observations	9,389	9,309	7,166	9,389	9,309	7,166

Notes: The unit of observation is appointment. Competence is a dummy variable set to 1 if the appointed governor has civil exam degree *Juren* or *Jinshi*. Experiences is a dummy variable set to 1 if the appointed governor has previously served as prefecture governor. Controls include six criterion variables that determined the treated prefectures: pre-reform conflict frequency and incidence of weather shock, land tax, population density, distance to the national road, and terrain ruggedness. In columns (3) and (6), we use the matched sample where treated prefectures are one-to-one matched to control prefectures, using propensity score matching based on criteria that determined the adoption of discretionary appointment. Robust standard errors in parentheses are clustered at prefecture level. ***, **, * denote significance at 1%, 5%, 10% levels.

Table 4. Discretionary Appointment Promoted Disaster Relief

<i>Panel A: provision of disaster relief</i>						
	Disaster relief programs			Disaster relief (0/1)		
	(1)	(2)	(3)	(4)	(5)	(6)
Discretion \times Post	0.398*** (0.109)	0.250** (0.103)	0.328** (0.128)	0.066*** (0.017)	0.044*** (0.016)	0.062*** (0.021)
Prefecture FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls \times Post		Y			Y	
PSM sample			Y			Y
Observations	33,627	33,408	24,185	33,627	33,408	24,185
<i>Panel B: state responsiveness to natural disaster</i>						
	Disaster relief programs			Disaster relief (0/1)		
	(1)	(2)	(3)	(4)	(5)	(6)
Discretion \times Post	0.025 (0.060)	-0.057 (0.069)	-0.005 (0.069)	0.013 (0.012)	0.001 (0.013)	0.009 (0.015)
Natural disaster	0.121*** (0.027)	0.153*** (0.026)	0.101*** (0.034)	0.031*** (0.004)	0.037*** (0.004)	0.029*** (0.005)
Discretion \times Post \times Natural disaster	0.730*** (0.121)	0.648*** (0.115)	0.697*** (0.145)	0.105*** (0.015)	0.091*** (0.014)	0.111*** (0.019)
Prefecture FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls \times Post		Y			Y	
PSM sample			Y			Y
Observations	33,627	33,408	24,185	33,627	33,408	24,185
Mean of D.V.	0.287	0.287	0.287	0.065	0.065	0.065

Notes: The unit of observation is prefecture-year. Natural disaster is a dummy variable for the occurrence of any natural disasters. Controls include six criterion variables that determined the treated prefectures: pre-reform conflict frequency and incidence of weather shock, land tax, population density, distance to the national road, and terrain ruggedness. In columns 3 and 6, we use the matched sample where treated prefectures are one-to-one matched to control prefectures, using propensity score matching based on criteria that determined the adoption of discretionary appointment. Robust standard errors in parentheses are clustered at prefecture level. ***, **, * denote significance at 1%, 5%, 10% levels.

Table 5. Other Mechanisms: Preferential Policy by Upper Government

	Attention index	Attention index on disaster	Disaster relief programs	
Mean of D.V.	0.413	0.423	0.287	0.287
	(1)	(2)	(3)	(4)
Discretion \times Post	0.036 (0.051)	-0.085 (0.128)		
Importance rating 4 \times Post			0.185 (0.242)	
Importance rating 2 \times Post				0.099 (0.077)
Prefecture FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Sample	All	All	Treated	Control
Observations	23,322	22,350	15,231	18,396

Notes: The unit of observation is prefecture-year. In columns 1 – 2, the sample is restricted to the period between 1723-1820 due to the availability of report data. In column 1, the attention index is calculated as the number of times that a prefecture had been mentioned in the regular reports by the senior local officials, relative to the mention times of all prefectures (rescaled by $\times 100$). In column 2, attention index on disaster is calculated in similar method but only based on the reports of which topic is disaster issue. Column 3 restrict the sample to treated prefectures (rating 3 and 4). Column 4 restrict the sample to control prefectures (rating 0, 1, and 2). ***, **, * denote significance at 1%, 5%, 10% levels.

Table 6. Other Mechanisms: Distributive Politics and Connection with Central Government

	Connection with central gov't	Tax exemption (0/1)	Tax exemption (0/1)
Mean of D.V.	0.351	0.132	0.132
	(1)	(2)	(3)
Discretion \times Post	0.019 (0.024)	0.044** (0.019)	0.017 (0.017)
Natural disaster			0.057*** (0.007)
Discretion \times Post \times Natural disaster			0.052*** (0.015)
Prefecture FE	Y	Y	Y
Year FE	Y	Y	Y
Observations	33,627	33,627	33,627

Notes: The unit of observation is prefecture-year. The dependent variable in column 1 is the number of top central officials who shared hometown with the given prefecture governors in a given year. In columns 2-3, the dependent variable is a dummy that takes the value one if a prefecture was granted tax exemption. Natural disaster is a dummy variable for the occurrence of any natural disasters. Robust standard errors in parentheses are clustered at prefecture level. ***, **, * denote significance at 1%, 5%, 10% levels.

Table 7. Discretionary Appointment Reduced Social Unrest

	Urban riots	Urban riots	Urban riots	Riots against Gov't	Riots between social groups
Mean of D.V.	0.0078	0.0078	0.0078	0.0054	0.0027
	(1)	(2)	(3)	(4)	(5)
Discretion \times Post	-0.0063** (0.0026)	-0.0065** (0.0026)	-0.0070*** (0.0026)	-0.0047** (0.0023)	-0.0015 (0.0015)
Prefecture FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Controls \times Post		Y		Y	Y
PSM sample			Y		
Observations	23,826	23,641	17,116	23,641	23,641

Notes: The unit of observation is prefecture-year. All dependent variables are indicators set to 1 if riots occurred, and 0 otherwise. Controls include six criterion variables that determined the treated prefectures: pre-reform conflict frequency and incidence of weather shock, land tax, population density, distance to the national road, and terrain ruggedness. In column 3, we use the matched sample where treated prefectures are one-to-one matched to control prefectures, using propensity score matching based on criteria that determined the adoption of discretionary appointment. Robust standard errors in parentheses are clustered at prefecture level. ***, **, * denote significance at 1%, 5%, 10% levels.

Table 8. Discretionary Appointment and Governor Quality by Appointers

Appointer:	Experiences		Competence	
	Emperor (1)	Prov. leaders (2)	Emperor (3)	Prov. leaders (4)
Discretion (Emperor) \times Post	0.164*** (0.024)		0.065** (0.026)	
Discretion (Prov. leader) \times Post		-0.027 (0.033)		0.055 (0.035)
Year FE	Y	Y	Y	Y
Prefecture FE	Y	Y	Y	Y
Controls \times Post		Y		Y
Observations	8,466	5,943	8,466	5,943

Notes: The unit of observation is appointment. In columns 1 and 3, the sample is restricted to governor appointments made by emperor's discretion and those in control groups. In columns 2 and 4, the sample is restricted to governor appointments made by the provincial leader's discretion and those in control groups. Competence is a dummy variable set to 1 if the appointed governor has civil exam degree *Juren* or *Jinshi*. Experiences is a dummy variable set to 1 if the appointed governor has previously served as prefecture governor. Robust standard errors in parentheses are clustered at prefecture level. ***, **, * denote significance at 1%, 5%, 10% levels.

Table 9. Discretionary Appointment and Governance Outcomes by Appointers

Appointer:	Disaster relief (0/1)			Urban riots		
	Emperor (1)	Prov. leaders (2)	Emperor (3)	Prov. leaders (4)	Emperor (5)	Prov. leaders (6)
Discretion (Emperor) \times Post	0.043** (0.020)		-0.003 (0.017)		-0.006** (0.003)	
Discretion (Prov. leader) \times Post		0.024 (0.025)		0.008 (0.017)		-0.004 (0.003)
Natural disaster			0.040*** (0.004)	0.041*** (0.005)		
Discretion (Emperor) \times Post \times Natural disaster			0.088*** (0.014)			
Discretion (Prov. leader) \times Post \times Natural disaster				0.048 (0.036)		
Prefecture FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls \times Post		Y		Y		Y
Observations	28,168	21,161	28,168	21,161	19,867	15,062

Notes: The unit of observation is prefecture-year. In columns 1, 3 and 5, the sample is restricted to treated prefectures under the discretion of the emperor and control prefectures. In columns 2, 4, and 6, the sample is restricted to treated prefectures under the discretion of provincial leaders and control prefectures. Natural disaster is a dummy variable for the occurrence of any natural disasters. Robust standard errors in parentheses are clustered at prefecture level. ***, **, * denote significance at 1%, 5%, 10% levels.

Table 10. Discretionary Appointment and Favoritism

Panel A: Ethnic tie				
Dependent variables:	Ethnic tie			
Appointer:	Prov. leaders	Prov. leaders (Han)	Prov. leaders (Bannerman)	Emperor
	(1)	(2)	(3)	(4)
Discretion (Prov. leader) \times Post	0.080** (0.038)	0.177** (0.087)	-0.001 (0.032)	
Discretion (Emperor) \times Post				0.022 (0.014)
Prefecture FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Observations	5,803	1,279	4,498	8,546

Panel B: Sanction				
Dependent variables:	Sanction			
Appointer:	Prov. leaders	Prov. leaders (Han)	Prov. leaders (Bannerman)	Emperor
	(1)	(2)	(3)	(4)
Discretion (Prov. leader) \times Post	-0.008* (0.004)	-0.034** (0.017)	-0.002 (0.006)	
Discretion (Emperor) \times Post				0.007 (0.006)
Prefecture FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Observations	5,803	1,279	4,498	8,546

Notes: The unit of observation is appointment. In columns 1-3, the sample is restricted to governor appointments made by the provincial leader's discretion and those in control groups. In column 4, the sample is restricted to governor appointments made by emperor's discretion and those in control groups. Ethnic tie is a dummy variable set to 1 if the appointed governor share ethnicity to his appointer (0 otherwise). Sanction is a dummy variable indicating whether the governor was officially sanctioned during this governorship. Robust standard errors in parentheses are clustered at prefecture level. ***, **, * denote significance at 1%, 5%, 10% levels.

Appendices

A Additional Details on Data Source

A.1 Personnel data

Appointment records. We obtain data on appointment records from Chinese local gazetteers. The local gazetteers of China were an encyclopedia of socio-political-economic events of a region (e.g., the entire nation, province, prefecture, county, town, or village) and have been major primary sources for the study of China’s local history. The publications, updated regularly, were compiled by a host of distinguished scholars and gentry whose reputation was at stake at the time and who were either themselves or able to interview first-hand witnesses to the events. The gazetteers were rather structured and standardized and normally consisted of several chapters recording officials and celebrities, natural disasters and relief activities, local products, temples, and schools, delineating the landscape, administrative zoning, and much more. (more details see [Dennis \(2015\)](#)). Based on this comprehensive and authoritative source, we construct a dataset documenting almost the entire history of appointments of prefecture governors and their background from the 1644 to 1820 period. To ensure data quality, we use both county, prefecture, and province gazetteers and cross-check the appointment information in different sources. [Figure A1](#) presents a sample of an appointment record in a prefecture gazetteer.

Biographical data on senior officials. The main data source of senior officials is the “Authoritative Biography Database” constructed by the Institute of History Philology, Academia Sinica, which is built on a variety of first-hand historical archives and biographic records in the Qing dynasty.⁴² The primary material of the dataset is the biographic packets and drafts from the archives of the Qing historiography institute. China has a long history to compile the official history books by the government, this practice is well retained by the Qing dynasty. The ruler of Qing not only established a professional agency, the Qing historiography institute but also recruited a large number of experts and scholars to collect the materials. Given the biography of prominent officials was the critical component of the history books, experts in the Qing historiography institute had to collect the pertinent background information and career trajectory of these officials. One byproduct of this effort was the biographic packets and drafts mentioned above. This source is rather valuable because it is immune to be falsified, not like the official history books would experience, given it is the raw material.

The second material of the database is the resume of Qing officials. The production of

⁴² <https://newarchive.ihp.sinica.edu.tw/sncaccgi/sncacFtp?@0.7913158946858703>

this material was due to the unique interview institution of the Qing dynasty. According to this institution, officials who were assigned to challenging positions had to visit the emperor handing over their resumes for the emperor’s review before they fulfilled the duty. The emperor then decides whether to override the assignment if he thought the official was not appropriate to the position. Most of the resumes survive so that the compiler can merge them into the main database. By combining and merging these sources into a dataset, the “Authoritative Biography Database” provides the most comprehensive and authoritative records of Qing officials, especially the senior ones.

Sanction. The sanction data are sourced from the *Veritable Records of the Qing* compiled by the Qing government. The publication is the official systematic chronology of imperial edicts and all types of political, social, economic, and military events and developments. Activities involving imperial household, personnel management of bureaucracy, and fiscal issues are the paramount focus of the *Veritable Records of the Qing*. To ensure truthfulness, accuracy, and completeness, the drafter could access a variety of raw materials (e.g., government reports, edicts, and financial accounts) and even be allowed to observe the court discussion and private activities of the emperor on a daily basis. It is therefore an ideal source of searching for the sanction records of an official. We collect all the punishment against a prefecture governor and merge it into our master dataset.

A.2 Governance outcome and natural shocks

We collect the government relief data, our proxy for governance outcome, from [Chen et al. \(2012\)](#), which is the direct excerpts of relief activities of the Qing government from the *Veritable Records of the Qing*. We enumerate the relief programs and tax exemption a prefecture received from the central government. We collect the records of disasters from *Comprehensive Compilation of Weather Records for the Last Three Millennia of China* compiled by [Zhang \(2004\)](#). This publication excerpts relevant information regarding natural shocks from thousands of local gazetteers available to the compiler. Given that gazetteers are compiled by local scholars and gentry who are either themselves or able to interview first-hand witnesses to the events, the records in the gazetteers, therefore, are rather reliable. We obtain the rainfall data from [State Meteorological Society \(1981\)](#) which is extensively used in the study of Chinese history (e.g., [Hao and Liu \(2020\)](#)).

A.3 Palace memorials

The palace memorials were the private reports sent by senior officials in the Qing dynasty, which were designed to facilitate communication between senior officials and the emperor

(Wu, 1967). Through this institution, senior officials reported information regarding local governance to the emperor directly and independently. By doing so, the emperor could make a timely decision and allocate resources accordingly. Thanks to the Yongzheng Emperor who issued an edict in 1729 stipulating that all palace memorials should be copied out and reserved at the imperial palace in case the original one was missing or damaged, nearly all palace memorials, including the emperor’s reply, were well-preserved. These documents now are reserved at two places separately, that is, the First Historical Archives of China ⁴³ and the National Palace Museum ⁴⁴. Both two organizations make a great effort to classify, compile and digitize these important documents. By combining the two sources, we have 281,251 pieces of secret reports during 1644-1820, 10,104 of which contains disaster relief activities but 6,978 of disaster relief reports only involves discussion of general conditions of disaster in a province and are not used in our analysis since we explore the cross-prefecture variation. Thanks to their endeavor, we now have sufficiently detailed information on each piece of the palace memorial including the reporter’s name, official title, accurate reporting date, and content of each palace memorial. We enumerate the times a prefecture was mentioned in the palace memorial in a given year to capture the importance and priority attached by the senior officials and the emperor.

A.4 Other prefecture characteristics

We collect a set of additional prefecture characteristics data to check the balance between the treated and control prefectures. We collect the agriculture suitability for wheat, rice, sweet potato, and maize from FAO (2012). Data on geographical features (latitude, longitude, proximity to coast) are from China Historical GIS (2016). Beyond formal land tax, we extract the data on the grain tax-in-kind and the informal *Huohao* surtax rate from the *Authorized Rule and Reference of the Ministry of Finance (Qinding Hubu Zeli)*. Data on Confucian academies, the key educational infrastructure in pre-modern China, are from Ji (1996). Finally, to proxy for the strength of clans, we use the number of genealogies compiled in a prefecture. The data on genealogy are obtained from Shanghai Library (2009).

⁴³ http://www.lsdag.com/nets/lsdag/page/topic/Topic_1697_1.shtml?hv=

⁴⁴ <http://npmhost.npm.gov.tw/ttscgi/ttswebnpm?@0:0:1:npmmeta::/tts/npmmeta/dblist.htm@@0.22065529270830952>

Figure A1. Example of a Prefecture Governor Appointment Record

直隸南雄州志卷之四	職官志 二 續編	國朝	知府	順治 胡奇	江南當塗人四年隨 終部院入粵委補 題授夙爛將略氣槩英偉見者肅然性 剛直守正不阿當鼎革之際釐剔嚴明商 民安堵因與營弁張友法隙被誣去官郡 人訟之 得直	鄭龍光	陸世楷	字韜生又字兩為號蘧知浙江平 湖人已丑進士七年任 有傳 字孝山 浙江平湖人 丙戌拔貢 十三年任 寬厚有容 剛柔相濟 興學造士 清廉愛民 歷任一十九載 按院趙特疏 題薦云苦 心調劑 衝劇潔已 軫恤瘡痍 以憂去 士民 惜之 有傳
Name of governor								
Hometown								
Degree in civil exam								
Year of appointment								

Table A1. Regional Importance Rating and Designation of Governance Features

Importance rating	Number of governorships	Transportation (<i>Chong</i>)	Burdensome (<i>Fan</i>)	Taxation (<i>Pi</i>)	Violence (<i>Nan</i>)
0	18	0	0	0	0
1	31	0.516	0.226	0.0323	0.226
2	87	0.575	0.851	0.0805	0.494
3	83	0.771	0.988	0.265	0.976
4	31	1	1	1	1
Total	250	0.644	0.776	0.244	0.648

Notes: This table shows the mean of governance feature designations among prefectures with different importance ratings. At the middle of the 18th century, The Qing Empire had 250 prefecture-level jurisdictions in the inner China area (not including the capital, Shuntian fu).

B Additional Results

Figure B1. Standardized Bias between Treated and Control Prefectures before and after Propensity Score Matching

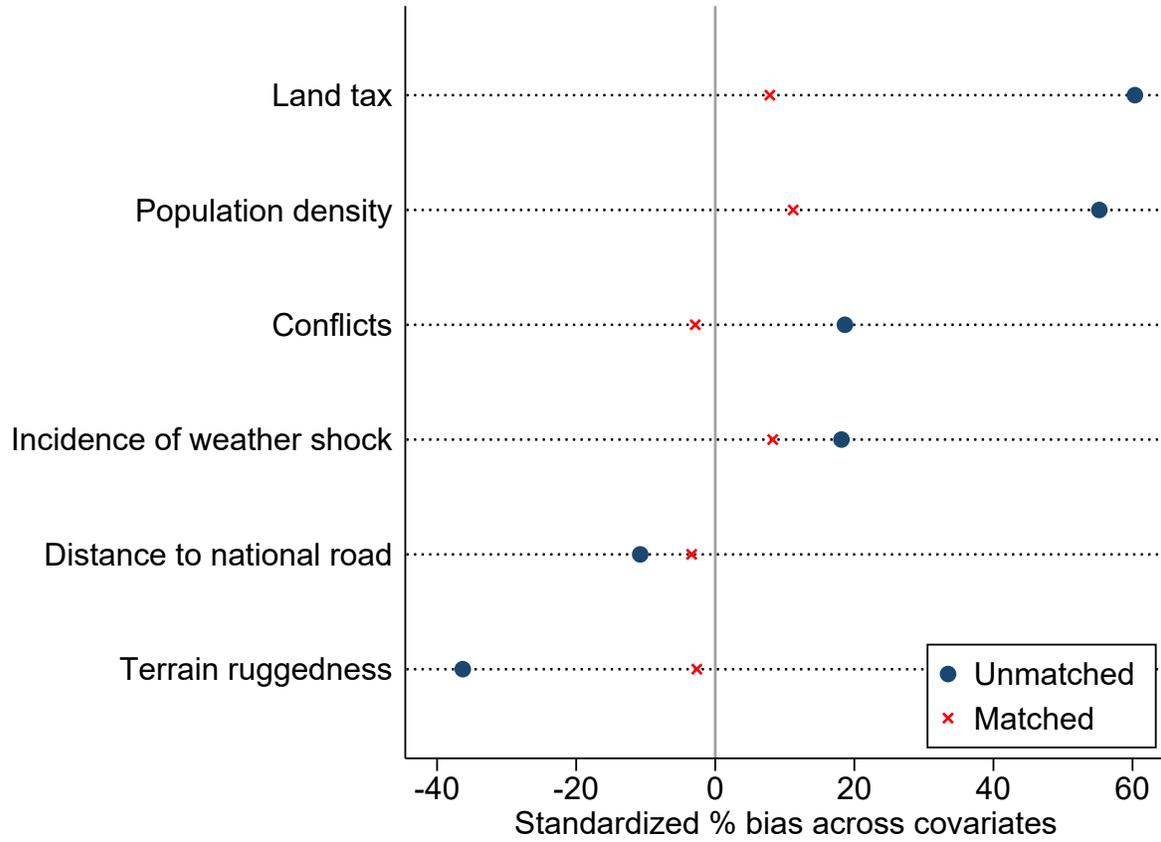


Figure B2. The Distributions of Matched Treated and Untreated Prefectures

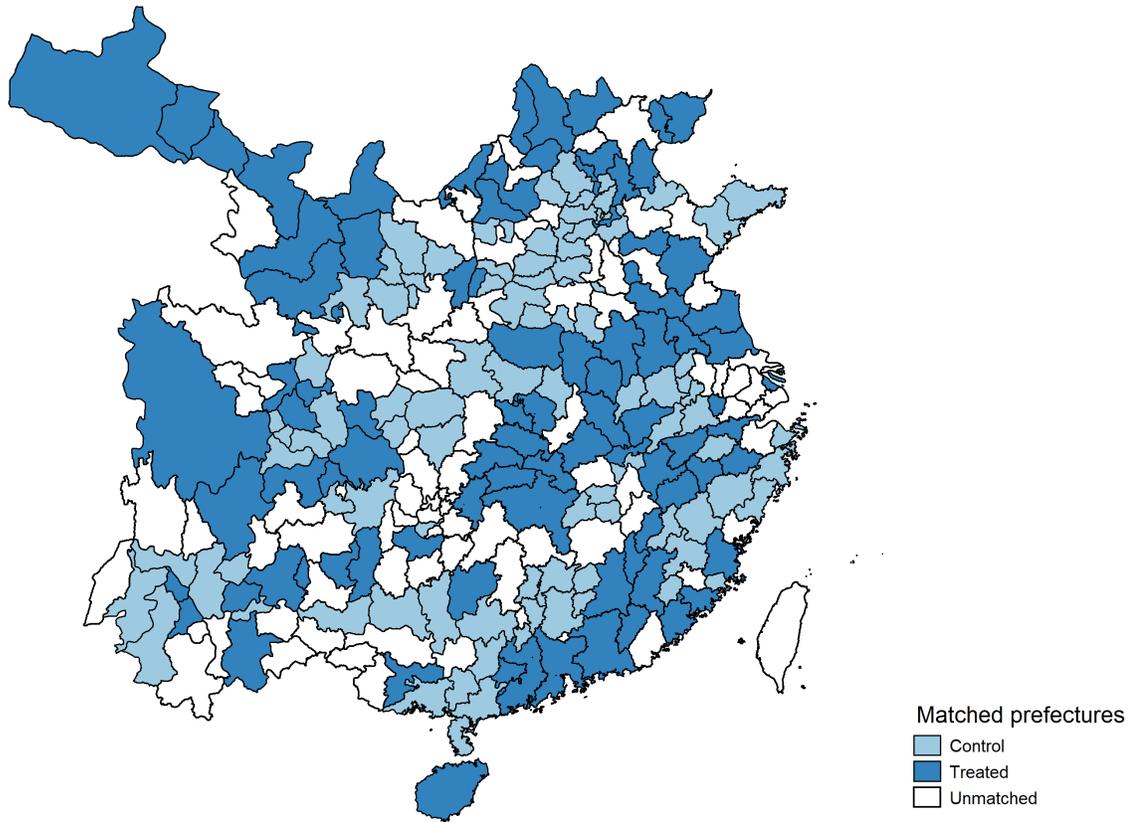
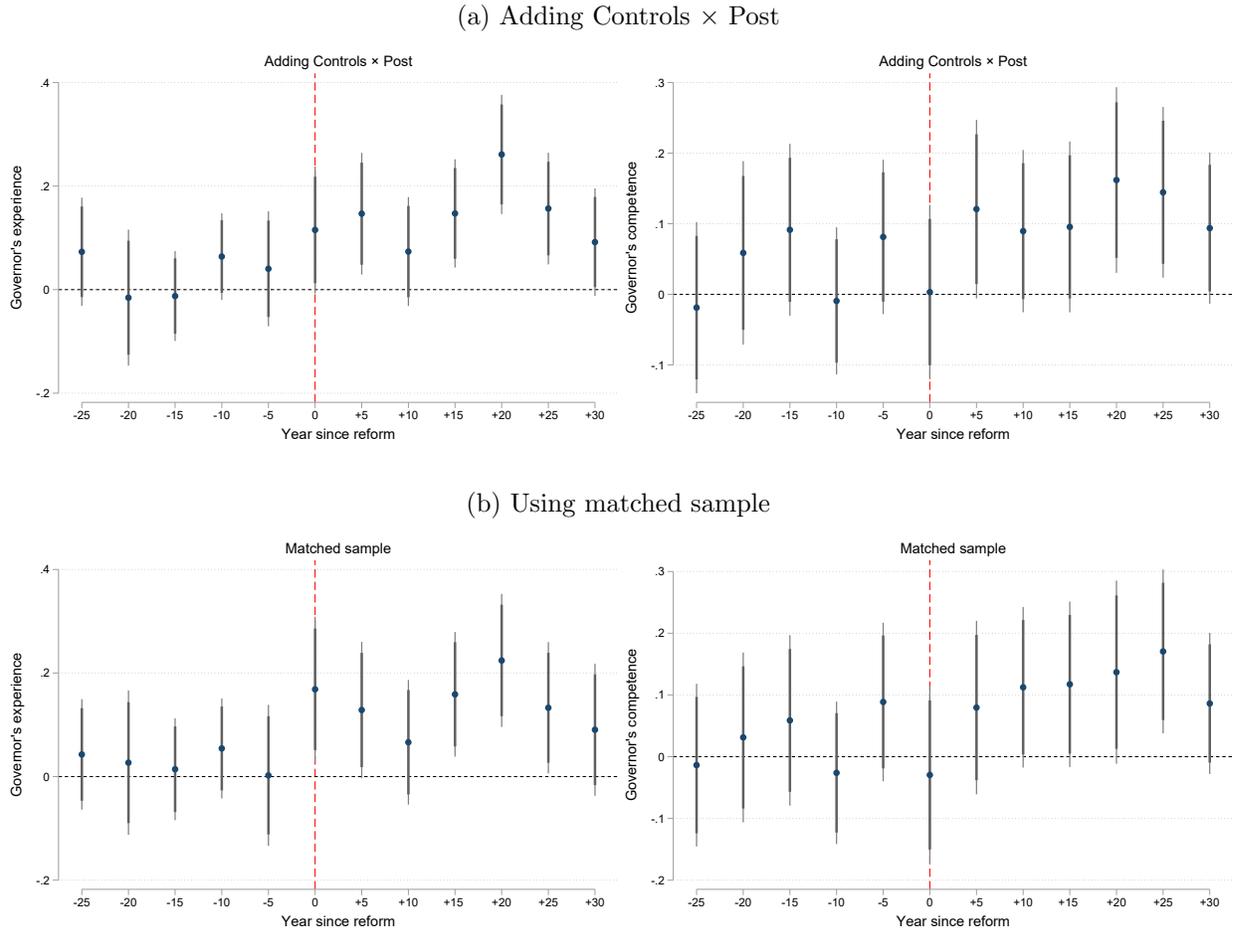


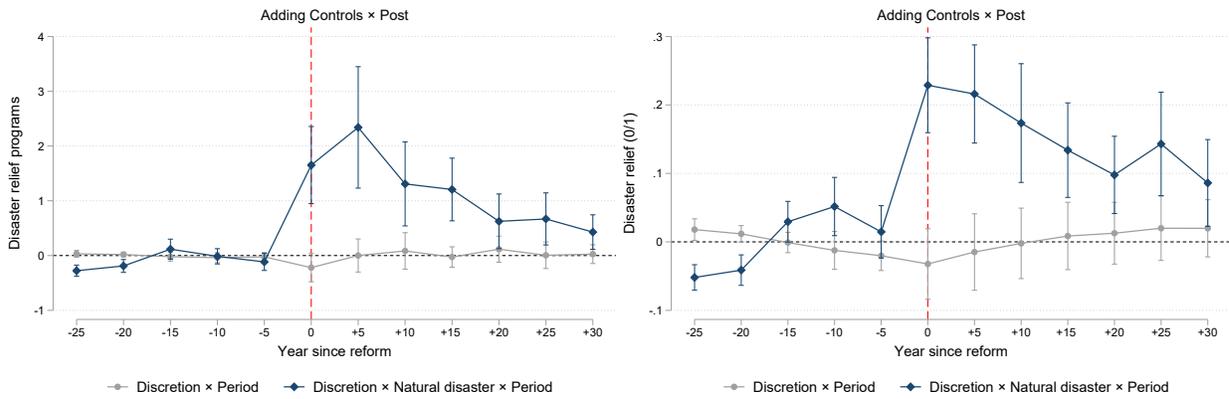
Figure B3. The Dynamic Effects of Discretionary Appointment and Governor Quality: Additional Robustness



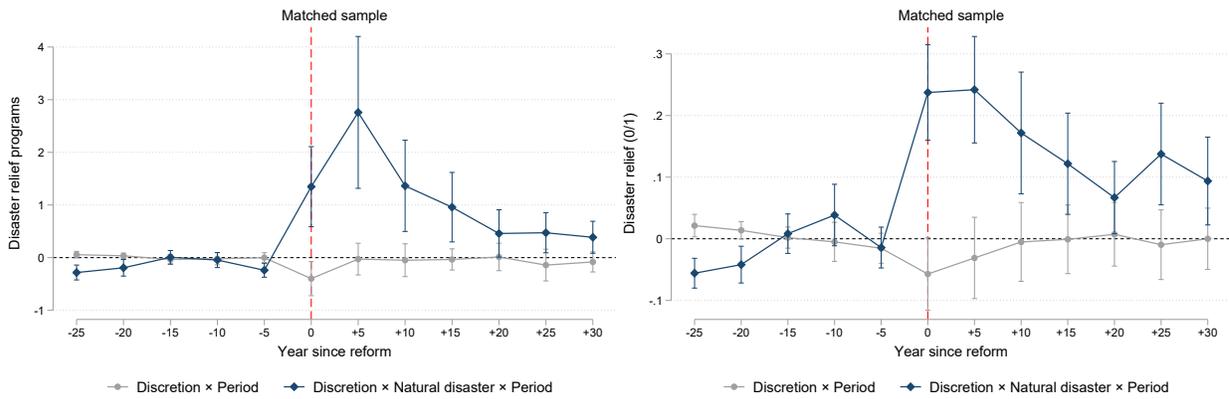
Notes: This figure shows the dynamic effects of discretionary appointment on appointed official's quality in 5 years bin. Panel A include interactions between treatment criterion variables and post dummy: pre-reform conflict frequency and incidence of weather shock, land tax, population density, distance to the national road, and terrain ruggedness. Panel B uses the matched sample where treated prefectures are one-to-one matched to control prefectures, using propensity score matching based on treatment criterion variables. The thick black lines plot the 90% confidence intervals and the thin grey lines plot the 95% intervals. Standard errors clustered at the prefecture level. Period before 1710 is omitted as reference group.

Figure B4. The Dynamic Effect of Discretionary Appointment on State Responsiveness: Additional Robustness

(a) Adding Controls \times Post

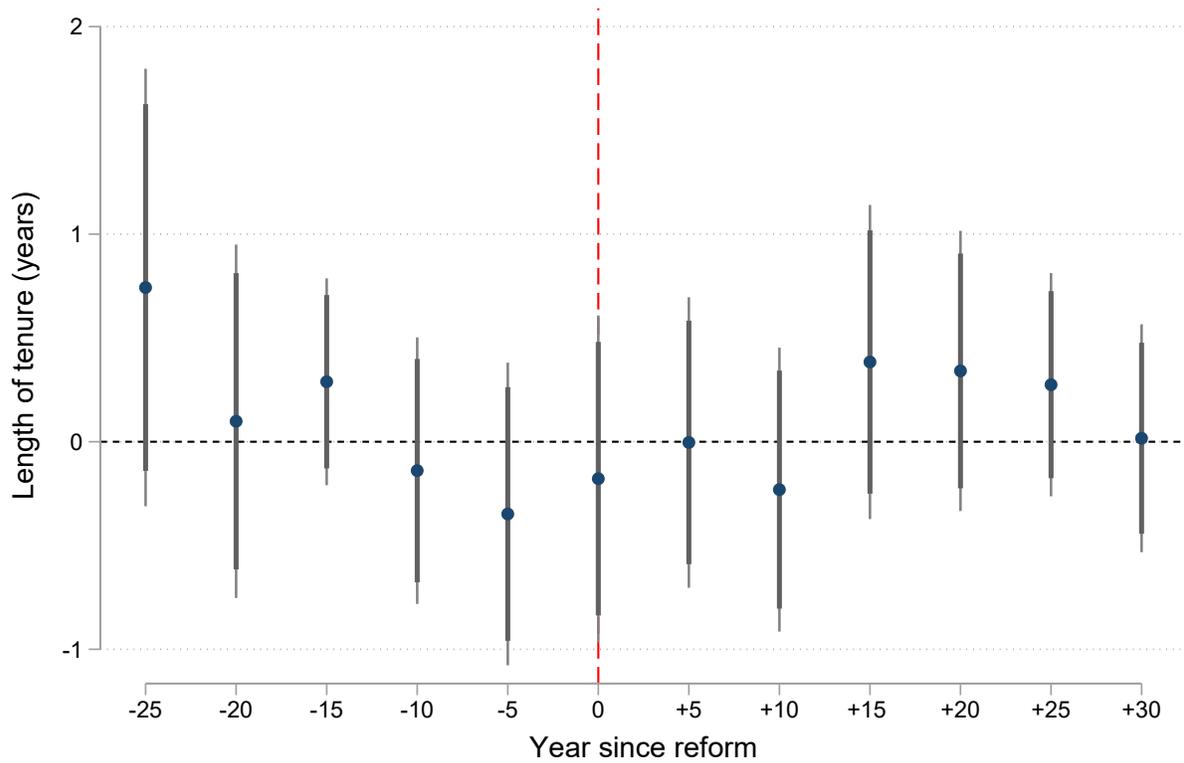


(b) Using matched sample



Notes: This figure shows the dynamic effects of discretionary appointment on state responsiveness in 5 years bin. Panel A include interactions between treatment criterion variables and post dummy: pre-reform conflict frequency and incidence of weather shock, land tax, population density, distance to the national road, and terrain ruggedness. Panel B uses the matched sample where treated prefectures are one-to-one matched to control prefectures, using propensity score matching based on treatment criterion variables. The vertical solid lines with caps plot the 95% confidence intervals. Standard errors clustered at the prefecture level. Period before 1710 is omitted as reference group.

Figure B5. No Impact on Governor's Tenure in Office – Treatment vs. Control



Notes: This figure presents the dynamic effects of discretionary appointment on the length of tenure in 5 years bin. The thick black lines plot the 90% confidence intervals and the thin grey lines plot the 95% intervals. Standard errors clustered at the prefecture level. Period before 1710 is omitted as reference group.

Table B1. Randomness of Appointment in Pre-reform Period

	(1)	(2)	(3)	(4)	(5)
Sample period	1644-1659	1660-1679	1680-1699	1700-1719	1720-1735
Panel A		Dependent variable: experience			
Importance rating: 1	-0.038 (0.078)	0.035 (0.107)	-0.035 (0.056)	-0.174 (0.110)	-0.072 (0.080)
Importance rating: 2	-0.024 (0.077)	-0.001 (0.101)	0.011 (0.054)	-0.104 (0.109)	-0.052 (0.070)
Importance rating: 3	-0.024 (0.077)	0.019 (0.101)	0.046 (0.056)	-0.082 (0.108)	0.003 (0.070)
Importance rating: 4	0.005 (0.080)	0.015 (0.102)	0.028 (0.062)	-0.021 (0.110)	0.067 (0.073)
Observations	862	767	863	782	1,126
R-squared	0.002	0.001	0.004	0.012	0.011
Panel B		Dependent variable: competence			
Importance rating: 1	-0.103 (0.137)	0.034 (0.101)	0.02 (0.066)	0.004 (0.085)	-0.038 (0.059)
Importance rating: 2	-0.093 (0.132)	-0.046 (0.098)	-0.005 (0.062)	-0.039 (0.070)	-0.009 (0.040)
Importance rating: 3	-0.141 (0.132)	-0.024 (0.099)	-0.022 (0.062)	0.033 (0.072)	0.007 (0.040)
Importance rating: 4	-0.089 (0.136)	-0.058 (0.099)	0.023 (0.070)	0.015 (0.080)	0.024 (0.045)
Observations	862	767	863	782	1,126
R-squared	0.003	0.003	0.002	0.006	0.001
Panel C		Dependent variable: ethnicity (Han Chinese)			
Importance rating: 1	0.084 (0.094)	-0.067 (0.095)	0.014 (0.096)	0.015 (0.102)	0.002 (0.059)
Importance rating: 2	-0.024 (0.090)	-0.019 (0.082)	-0.03 (0.089)	-0.082 (0.089)	-0.041 (0.044)
Importance rating: 3	-0.042 (0.092)	0.014 (0.082)	0.009 (0.089)	0.001 (0.092)	-0.036 (0.046)
Importance rating: 4	-0.024 (0.102)	-0.092 (0.086)	-0.051 (0.094)	0.029 (0.097)	-0.015 (0.050)
Observations	862	767	863	782	1,126
R-squared	0.005	0.006	0.002	0.009	0.001

Notes: The unit of observation is appointment. This table shows the randomness of pre-reform appointment by regressing governor characteristics on the prefecture's importance rating separately for different period (columns 1-5) before the reform. In panel A, the dependent variable is an indicator for previously governor experience. In panel B, the dependent variable is an indicator for *Juren* or *Jinshi* civil exam degree. In panel C. In panel C, the dependent variable is a dummy variable set to 1 if the governor was Han Chinese. Robust standard errors in parentheses are clustered at prefecture level. ***, **, * denote significance at 1%, 5%, 10% levels.

Table B2. Discretionary Appointment and Governor Quality: Sub-samples

	Experiences			Competence		
	(1)	(2)	(3)	(4)	(5)	(6)
Discretion \times Post	0.141*** (0.024)	0.135*** (0.027)	0.114*** (0.025)	0.066*** (0.023)	0.063** (0.026)	0.055** (0.022)
Prefecture FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Excluding acting governors	Y			Y		
Term length >1 years		Y			Y	
Excluding provincial capital			Y			Y
Observations	8,567	6,420	8,408	8,567	6,420	8,408

Notes: The unit of observation is appointment. Competence is a dummy variable set to 1 if the appointed governor has civil exam degree *Juren* or *Jinshi*. Experiences is a dummy variable set to 1 if the appointed governor has previously served as prefecture governor. Robust standard errors in parentheses are clustered at prefecture level. ***, **, * denote significance at 1%, 5%, 10% levels.

Table B3. Additional Checks on the Discretionary Appointment Effect

	Experiences		Competence	
	(1)	(2)	(3)	(4)
Discretion \times Post	0.122*** (0.028)	0.148*** (0.027)	0.060** (0.026)	0.054** (0.025)
Prefecture FE	Y	Y	Y	Y
Year FE	Y		Y	
Rating 2 vs. rating 3 sample	Y		Y	
Year \times Province FE		Y		Y
Observations	8,567	6,420	8,408	8,567

Notes: The unit of observation is appointment. Competence is a dummy variable set to 1 if the appointed governor has civil exam degree *Juren* or *Jinshi*. Experiences is a dummy variable set to 1 if the appointed governor has previously served as prefecture governor. In columns 1 and 3, the sample is restricted to prefectures where the importance rating is 2 or 3. Robust standard errors in parentheses are clustered at prefecture level. ***, **, * denote significance at 1%, 5%, 10% levels.

Table B4. Alternative Measurement of Governors' Experience

Mean of D.V.	Years of governor experiences			
	1.228	1.228	1.228	1.228
	(1)	(2)	(3)	(4)
Discretion \times Post	0.715*** (0.142)	0.635*** (0.134)	0.712*** (0.168)	0.756*** (0.163)
Prefecture FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Controls \times Post		Y		
PSM sample			Y	
Year \times Province FE				Y
Observations	9,389	9,309	7,166	8,862

Notes: The unit of observation is appointment. Experience is measured by the total years of service as a governor before the current appointment. In column 3, we use the matched sample where treated prefectures are one-to-one matched to control prefectures, using propensity score matching based on criteria that determined the adoption of discretionary appointment. Robust standard errors in parentheses are clustered at prefecture level. ***, **, * denote significance at 1%, 5%, 10% levels.

Table B5. Changes in the Incidence of Natural Disasters

	Rainfall level	Drought (rainfall level=5)	Flood (rainfall level=1)	Disaster (0/1)	Disaster (frequency)
Mean of D.V.	2.928	0.057	0.071	0.837	0.34
	(1)	(2)	(3)	(4)	(5)
Discretion \times Post	-0.021 (0.022)	-0.008 (0.006)	-0.009 (0.006)	0.005 (0.018)	-0.058 (0.058)
Prefecture FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Observations	33,627	33,627	33,627	33,627	33,627

Notes: The unit of observation is prefecture-year. Robust standard errors in parentheses are clustered at prefecture level. ***, **, * denote significance at 1%, 5%, 10% levels.

Table B6. Changes in the Incidence of Natural Disasters: by Disaster type

	flood	drought	locust	snowstorm	hurricane	earthquake	plague
Mean of D.V.	0.226	0.13	0.044	0.071	0.05	0.006	0.025
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Discretion \times Post	-0.01 (0.013)	-0.006 (0.011)	0.007 (0.006)	-0.009 (0.009)	-0.009 (0.006)	-0.003 (0.002)	0 (0.004)
Prefecture FE	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y
Observations	33,627	33,627	33,627	33,627	33,627	33,627	33,627

Notes: The unit of observation is prefecture-year. Robust standard errors in parentheses are clustered at prefecture level. ***, **, * denote significance at 1%, 5%, 10% levels.

Table B7. Comparing Prefectures with Rating 3 vs. Rating 2

	Relief programs		Relief (0/1)	
	(1)	(2)	(3)	(4)
Discretion \times Post	0.347** (0.133)	-0.007 (0.078)	0.056*** (0.020)	0.003 (0.015)
Natural disaster		0.103*** (0.034)		0.030*** (0.005)
Discretion \times Post \times Natural disaster		0.721*** (0.148)		0.106*** (0.017)
Prefecture FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Observations	23,171	23,171	23,171	23,171

Notes: The unit of observation is prefecture-year. Natural disaster is a dummy variable for the occurrence of any natural disasters. Robust standard errors in parentheses are clustered at prefecture level. ***, **, * denote significance at 1%, 5%, 10% levels.

Table B8. Adding Year \times Province FE

	Relief programs		Relief (0/1)	
	(1)	(2)	(3)	(4)
Discretion \times Post	0.362*** (0.092)	0.085 (0.059)	0.057*** (0.013)	0.017 (0.010)
Natural disaster		0.055** (0.026)		0.021*** (0.003)
Discretion \times Post \times Natural disaster		0.564*** (0.100)		0.081*** (0.013)
Prefecture FE	Y	Y	Y	Y
Year \times Province FE	Y	Y	Y	Y
Observations	33,595	33,595	33,595	33,595

Notes: The unit of observation is prefecture-year. Natural disaster is a dummy variable for the occurrence of any natural disasters. Robust standard errors in parentheses are clustered at prefecture level. ***, **, * denote significance at 1%, 5%, 10% levels.

Table B9. Using Disaster Frequency as a Robustness Check

	Relief programs			Relief (0/1)		
	(1)	(2)	(3)	(4)	(5)	(6)
Discretion \times Post	0.030 (0.067)	-0.059 (0.076)	-0.020 (0.078)	0.027* (0.014)	0.013 (0.015)	0.0200 (0.017)
Natural disaster	0.097*** (0.017)	0.106*** (0.017)	0.096*** (0.022)	0.015*** (0.001)	0.016*** (0.002)	0.014*** (0.002)
Discretion \times Post \times Natural disaster	0.323*** (0.058)	0.302*** (0.056)	0.331*** (0.067)	0.035*** (0.004)	0.031*** (0.004)	0.040*** (0.006)
Prefecture FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls \times Post		Y			Y	
PSM sample			Y			Y
Observations	33,627	33,408	24,185	33,627	33,408	24,185

Notes: The unit of observation is prefecture-year. Natural disaster is the frequency of natural disaster records. Controls include six criterion variables that determined the treated prefectures: pre-reform conflict frequency and incidence of weather shock, land tax, population density, distance to the national road, and terrain ruggedness. In columns 3 and 6, we use the matched sample where treated prefectures are one-to-one matched to control prefectures, using propensity score matching based on criteria that determined the adoption of discretionary appointment. Robust standard errors in parentheses are clustered at prefecture level. ***, **, * denote significance at 1%, 5%, 10% levels.

Table B10. The Role of Fiscal Capacity

	Disaster relief program (1)
Discretion \times Post	0.307** (0.129)
Discretion \times Post \times Prov. fiscal capacity	0.037 (0.039)
Prov. fiscal capacity \times Post	0.009 (0.010)
Prefecture FE	Y
Year FE	Y
Observations	33,504

Notes: The unit of observation is prefecture-year. Provincial fiscal capacity is measured by the public funds surplus per capita of each province (silver-taels per thousand population). Robust standard errors in parentheses are clustered at prefecture level. ***, **, * denote significance at 1%, 5%, 10% levels.

Table B11. Discretionary Appointment and the Length of Tenure

Mean of D.V.	length of tenure		
	3.433 (1)	3.433 (2)	3.433 (3)
Discretion \times Post	0.100 (0.149)	0.010 (0.148)	0.110 (0.157)
Prefecture FE	Y	Y	Y
Year FE	Y	Y	Y
Excluding acting governors		Y	
Excluding provincial capital			Y
Observations	9,389	8,567	8,408

Notes: The unit of observation is appointment. Robust standard errors in parentheses are clustered at prefecture level. ***, **, * denote significance at 1%, 5%, 10% levels.

Table B12. Comparing Prefectures with High vs. Low Prevalence of Violence

Prevalence of violence	Urban riots		Riots against government		Riots between social groups	
	Low	High	Low	High	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)
Discretion \times Post	-0.0384 (0.0250)	-0.0066** (0.0030)	-0.0405 (0.0250)	-0.0047** (0.0020)	0.0022 (0.0020)	-0.0015 (0.0020)
Prefecture FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Observations	10,208	18,792	10,208	18,792	10,208	18,792

Notes: The unit of observation is prefecture-year. All dependent variables are dummies set to 1 if riots occurred, and 0 otherwise. Robust standard errors in parentheses are clustered at prefecture level. ***, **, * denote significance at 1%, 5%, 10% levels.

C Discussion on SUTVA

In this section, we provide additional discussion on the stable unit treatment values assumption (SUTVA) underlying our causal identification strategy.

The coefficient of β in equation (1) estimates the causal effect of discretion under the SUTVA (Imbens and Rubin, 2015) that the potential outcomes for control group units do not vary with the treatment applied to other units. This might be violated if the governor quality in the control group decreased because the best talents had been picked and appointed to prefectures in the treatment group, thus decreasing the average quality of remaining bureaucrats in the candidate pool who were available for appointments in the control group prefectures.

In practice, this is unlikely to affect our estimates because, at a given point in time, the number of treatment units is very small relative to the large size of the candidate pool qualified for prefecture governorships. According to the personnel regulations of the Qing’s bureaucracy, candidates who qualified to be appointed as prefecture governors were confined to certain types of positions, such as county magistrates or senior staff in central departments. We are thus able to calculate the maximum number of bureaucrats in the candidate pool according the “map” of position-to-position career tracks (*Pinjikao*).⁴⁵ The total number of potential candidates who were qualified for the appointment of prefecture governor is 3,329. In our study period, there were on average 33 new appointments in the treatment groups each year, indicating that the selections for the treated prefectures only involved 1% (33 out of 3,229) of the candidate pool. Given the small number of treatment units, this is unlikely to have substantial effects on the potential outcomes for control groups.

We complement this argument with a rough estimation of such negative spillover effects on governor competence by looking at the distribution of civil exam rank in the candidate pool. To this end, we collect data on the personal backgrounds of bureaucrats who were on the candidate list for prefecture governor. We construct a candidate pool of 2,594 bureaucrats, with some missing, from the *Official Register* in 1774.⁴⁶ We code their civil exam backgrounds into eight ranks (Table C1). Figure C1 presents the distribution of the civil exam rank for the candidate pool. There were 1,262 bureaucrats with civil exam rank equal to or higher than *Juren* (rank ≥ 7). The mean of the competence measure is 0.4865, which can be viewed as the potential outcome for the control groups. Based on the mean of governor competence in the treatment group after the reform (0.4719), the mean value of competence in the candidate pool would decline 0.0048 ($33 \times 0.4719 / 3,229$), translating to mere a 0.99% decrease. In an

⁴⁵ Data source: *Statutes and Precedents of the Qing (Da Qing Huidian Shili)*.

⁴⁶ the *Official Register (Jinshenlu)* was a roster of nearly all regular positions and their holders in Qing’s bureaucracy, produced on a quarterly basis during the middle-to-late Qing period (Chen et al., 2020).

extreme case, if all the 33 newly appointed governors in the treatment group in that year had the top-2 exam ranks, the mean value of competence in the candidate pool would decline 0.0102, translating to only a 2.1% decrease (0.0102/0.4864).

Table C1. Civil Exam Rank Coding and the Distribution in Candidate Pool, 1774

Civil exam rank	(1) Exam background	(2) Frequency	(3) Percent
8	<i>Jinshi</i>	692	26.68
7	<i>Juren</i>	570	21.97
6	<i>Gongsheng</i>	213	8.21
5	<i>Jiansheng</i>	533	20.55
4	Purchased Jiansheng	46	1.77
3	Qualification by other background	80	3.08
2	<i>Shengyuan</i>	40	1.54
1	No degree	420	16.19
Total		2,594	100

Notes: This table reports descriptions of the eight ranks categories and the distribution of civil exam rank among candidates eligible for prefecture governor in 1774. Rank coding is based on [Chang \(1955\)](#).

Figure C1. The Distribution of Civil Exam Rank for Candidate Pool in 1774

