

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

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## Abstract

Public organizations are often characterized by rigid rules and procedures. Can discretion in personnel decisions improve governance performance? We study how discretion in internal appointments affects the functioning of public organizations, exploiting an organizational reform in China's imperial bureaucracy that modified the appointments of certain governorships from a rule-based process to a more discretionary method. We find that discretionary appointments improved public goods provision and led to greater state responsiveness. A better selection of governors plays an important mechanism: (1) discretion increased observable officer quality measured by experiences and civil exam qualifications; (2) exploiting the quasi-random rotations of governors to prefectures, we show that selected governors performed better than those unselected. We also find evidence consistent with the incentive effect. Finally, we provide evidence suggesting that the benefit of discretion depends on the incentive alignment of decision-makers with the organization.

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# 1 Introduction

Bureaucrats, who perform government functions, are a key element of state capacity (Besley et al., 2021). Public sector organizations, however, face many constraints in utilizing incentive devices due to the multi-tasking problem. This renders the allocation of talent more critical for public organizations. Broadly speaking, there are two approaches to allocating bureaucrats, by rule or by discretion. 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 practice, many public organizations adopt fixed rule-based methods, such as seniority-based promotion and random assignment (Bertrand et al., 2020; Vannutelli, 2021).<sup>1</sup> However, rigid rules can be costly, for it precludes the appointer’s use of (soft) private information in appointment decision, and pays no regard to the considerable heterogeneity in positions, resulting in talent misallocation and organizational inefficiency.

The effect of discretion in appointments *compared* to rule is theoretically ambiguous. While discretion can open the door to favoritism and corruption (Prendergast and Topel, 1996), it also enables the use of private and public information in appointment, especially when the appointer’s incentive is aligned with the organization (Aghion and Tirole, 1997; Alonso and Matouschek, 2008). How does discretion in appointments affect the functioning of the organization? Yet, despite the long-standing Weberian view in favor of limiting discretion, direct causal evidence remains rare. Studying the rule versus discretion trade-off empirically is challenging due to 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 rule).

This paper studies how discretion in appointments affects governance performance in the context of the imperial bureaucracy in 18th century China. Imperial China in the Qing dynasty provides an attractive setting to study these questions. With its competitive recruitment of bureaucrats based on the world’s earliest civil service exam (Elman, 2013; Kung, 2021), China’s imperial bureaucracy is seen as the first prototype of modern bureaucratic organizations (Finer, 1997a; Fukuyama, 2011). We focus on a group of middle-ranking officers—prefecture governors—the decisive authority of local governments. Moreover, the Qing government implemented a rule-based appointment process for the majority of middle and junior level positions, against which it provided a clean

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<sup>1</sup> There are many applications of rules with randomness in the practices of public sector organizations, for example, government procurement auctions (Ferraz, Finan and Szerman, 2015), the assignment of municipal auditors (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; Stasavage, 2020).

benchmark for evaluating the effect of discretion in appointment decisions.

The key advantage of our setting is that we leverage a natural experiment in the appointment method to overcome the empirical challenge. Our analysis centers on an organizational reform in the 1730s aiming to improve the personnel management of prefecture governors. Prior to the reform, governors were centrally appointed to prefectures through an elaborate rule-based process.<sup>2</sup> Following 1736, the appointment of certain governorships was modified to a more discretionary method, in which the Ministry of Personnel (MOP) and the emperor jointly selected governors from the eligible candidate pool. Our identification strategy exploits the fact that the discretionary appointment only applied to prefectures with a high regional importance rating ( $\geq 3$ ), 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 outcomes of prefectures that changed the appointment method with those that remained under the rule-based appointment, before and after the reform.

We construct a unique personnel dataset covering the near universe of prefecture governors, which contains over 10,000 individual records across 250 prefectures for the period 1644–1820. The data provide detailed information on the appointment, rotation, and characteristics of governors (e.g., civil exam qualification, experience, ethnicity). To measure governance performance, we use comprehensive records of government disaster relief, which was an important task of local governments and a crucial indicator of government responsiveness (Besley and Burgess, 2002). The agricultural economy was vulnerable to frequent natural disasters, threatening people’s livelihood and social stability (Jia, 2014). Hence disaster relief was a crucial public good and the Qing government played a very active role in relief activities.<sup>3</sup>

We begin by studying the effects of discretion in appointments on disaster relief and state responsiveness. We show that, before the reform, there were no discernible differences in disaster relief between the treated and control prefectures. The adoption of discretionary appointment significantly increased disaster relief provisions. Following the reform, treated prefectures increased the probability of relief delivery by 6.5 percentage points, compared with other prefectures that retained the rule-based appointment system. Moreover, we find the increase in relief is particularly more pronounced when the

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<sup>2</sup> Officers qualified for prefecture-level positions will be queued in specific sequences to wait for new vacant positions based on seniority. When it was their turn to fill new vacancies, a random lottery matching officers and vacancies would determine the specific position to which they would be appointed. We provide a detailed description of the personnel regulation in China’s imperial bureaucracy in Section 2.1.

<sup>3</sup> In the middle-to-late 18th century, every year there were on average 12.3% prefectures receiving disaster relief.

disaster occurred, consistent with greater state responsiveness.<sup>4</sup> To improve 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.<sup>5</sup> We then use this matched sample to evaluate the effect of discretion among comparable prefectures. Our results hold using the matched DID strategy. In addition, we look at social unrest as another governance outcome. We find a decrease in urban riots after the reform, and The effect is particularly pronounced for riots against governments.

After documenting the effect of discretionary appointment on governance performance, we explore underlying mechanisms in the second part of this paper. We first evaluate whether the improvement in disaster relief is driven by the selection of governors. Under the new system, the Ministry of Personnel and the emperor could use their discretionary judgment to pick a suitable officer for each vacancy from the candidate pool. We provide two pieces of evidence in support of this mechanism. First, using the individual information from the personnel record data, we find that governors selected for treated prefectures through the discretionary process were 14 percentage points more likely to have previous governor experience (a 47% increase relative to the mean). We also find that they were more likely to have merit civil service exam qualifications. Results of these two observable quality measures suggest that the discretionary appointments selected officers with better experience and competence.

Second, the selection effect could also work along unobservable dimensions. To shed light on that, we exploit the exogenous rotations of governors to prefectures within the control group for identification. After the reform, control prefectures still followed the rule-based appointment process, which resulted in a quasi-random allocation of officers across prefectures, including transferring governors to new prefectures. We compare *control prefectures* that, by chance, were assigned a governor who had previously been *selected* to those that were assigned an *unselected* one. This allows us to capture unobservable individual traits behind the selection and insulate our analysis from confounding differences between treated and control prefectures. After validating the balance of prefectures run by selected and unselected governors on a range of prefecture characteristics and disaster occurrence, we find that selected governors did perform better in disaster relief than others serving in control prefectures. These results imply that the selection of higher-quality

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<sup>4</sup> Our findings are not driven by greater demand for relief. We find no differential changes in the incidence of natural disasters before and after the reform.

<sup>5</sup> We rely on as many as six measures in determining the regional importance rating: pre-reform conflict 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.

governors plays an important mechanism driving the improvement of governance performance.

The change in appointment method could also affect governance performance by altering incentives. Treated prefectures might have higher promotion prospects after the reform, incentivizing governors in treated positions to perform well. We test this hypothesis by examining whether the *same* governor performed differently when he switched between treated and control prefectures, in the post-reform period. Holding the selection margin fixed by individual fixed effects, we find that governors increased relief probability when serving in treated positions and were also more likely to be promoted, suggesting that incentives are likely to be another driver of our results.

We also discuss other potential alternative explanations. First, we find no evidence that the increase in disaster relief was driven by the top-down preferential policy in favor of high-rating prefectures. (1) Using the text data from reports between provincial leaders and the emperor, we find that treated prefectures did not receive more attention from the upper governments after the reform. (2) If the upper governments used the importance rating to allocate relief resources accordingly, we should expect high-rating prefectures to have more relief despite appointment methods. We show that it was not the case. (3) We find no heterogeneous effect with respect to upper governments' fiscal resources for redistribution. Second, governors in the treated prefectures might have more connections with senior officials in the central government and thus obtain more resources. We find that governors in the treated prefectures show no difference in hometown connection with top central officials. In addition, they also performed better in timely surveying and reporting disasters, which is unlikely due to connection-induced resource distribution.

In the final section, we study 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 heterogeneity by appointers with varying degrees of alignment with the organization. Of the treated prefectures switching appointment method, the majority were under the discretionary process involving both the Ministry of Personnel (MOP) and the emperor, but a relatively small number of posts were delegated to the provincial leaders.<sup>6</sup> Compared with provincial leaders, the emperor and the MOP had a greater stake in the overall performance of the bureaucracy. We find that the MOP-emperor discretionary appointments had a stronger effect on relief delivery, while dis-

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<sup>6</sup> These prefecture-level jurisdictions were independent department (*zhilizhou*) and prefectures clustering ethnic minorities.

cretionary appointments by provincial leaders had a smaller impact. Moreover, we find suggestive evidence that the provincial leaders were more likely to use discretion to engage in favoritism.<sup>7</sup> These results suggest that the net effects of discretion are contingent on the incentive alignment of the decision-makers.

This paper makes three contributions. First, we contribute to the expanding literature on the personnel economics of the state (reviewed by [Finan, Olken and Pande \(2017\)](#) and [Besley et al. \(2021\)](#)). A large body of work documents how incentives and monitoring work (or fail) in public sectors.<sup>8</sup> Another strand of work examines the selection of bureaucrats and its impacts on performance, focusing on the trade-off between financial vs. prosocial motivation ([Dal Bó, Finan and Rossi, 2013](#); [Deserranno, 2019](#); [Ashraf et al., 2020](#)) and competitive admission exams ([Ornaghi, 2019](#); [Estrada, 2019](#); [Dahis, Schiavon and Scot, 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 allocate higher-level positions to those *already screened* into public organizations.<sup>9</sup> We instead focus on the internal selection of middle-ranking officers — the prefecture governor — in China’s imperial bureaucracy. By showing the effects of discretion in bureaucratic appointments, we provide new evidence on how appointment methods affect organizational performance.

Second, we contribute to recent work on the value of discretion and autonomy in the public sector. Existing work has documented that discretion can give birth to favoritism and biased appointments favoring those with personal or party connections, resulting in poor performance ([Xu, 2018](#); [Barbosa and Ferreira, 2019](#); [Brassiolo, Estrada and Fajardo, 2020](#); [Colonnelli, Prem and Teso, 2020](#); [Akhtari, Moreira and Trucco, 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 ([Duflo 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, Rogger and Williams, 2021](#)). We contribute to this literature by illustrating that discretion in the internal appointment can improve the selection and performance of governors 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 discre-

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<sup>7</sup> 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.

<sup>8</sup> Prior work has examined the role of financial incentives ([Khan, Khwaja and Olken, 2016](#); [Leaver et al., 2021](#)), career incentives ([Bertrand et al., 2020](#)), mission and non-monetary incentive ([Ashraf, Bandiera and Jack, 2014](#); [Khan, 2020](#)), and monitoring ([Olken, 2007](#); [Muralidharan et al., 2021](#); [Vannutelli, 2021](#))

<sup>9</sup> 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, Bertrand and Burgess \(2021\)](#).

tion 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.<sup>10</sup> Compared to previous work, 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. Furthermore, our results suggest that the benefits and costs of discretion depend on the incentive alignment between decision-makers and the organization.

Third, this paper also speaks to the literature on the selection and incentives of local leaders in China's party state. A large body of work provides evidence on the role of economic performance and competence in political selection and promotion (Li and Zhou, 2005; Xu, 2011; Yao and Zhang, 2015), and on the role of connection and loyalty-competence tradeoff (Landry, Lü and Duan, 2018; Jia, Kudamatsu and Seim, 2015; Chen et al., 2023). Another body of work shows that the incentives of local officials shape various social and economic outcomes, such as urban spatial expansion (Wang, Zhang and Zhou, 2020), environmental regulation (He, Wang and Zhang, 2020), population control (Serrato, Wang and Zhang, 2019), and policy experimentation (Wang and Yang, 2021). We add to this literature by probing into China's bureaucratic institution in the imperial period, from which China's contemporary institution largely inherited, and showing how appointment methods affect the functioning of the centralized organization.

## 2 Historical Background

### 2.1 Bureaucratic Organization and Political Selection in Early Qing China

China has the longest history of bureaucratic rule in the world. The ancient Chinese state is deemed the earliest inventor of the modern-style bureaucracy (Finer, 1997a). The imperial bureaucracy of the Qing dynasty 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). The imperial bureaucratic system reached its peak in terms of institutionalization in the Qing dynasty (1644–1912). Relying on this bureaucratic machinery, the Qing Empire was the last but arguably the most

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<sup>10</sup> 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, Prem and Teso (2020) find that supporters of the winning party in the Brazilian mayor elections are more likely to be employed into the public sector.

successful dynasty in Chinese imperial history in terms of territory and longevity (Rowe, 2009).<sup>11</sup>

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 governments on the other. The prefecture governor was the decisive authority in matters of administration and judicature in his jurisdiction.<sup>12</sup>

The recruitment of bureaucrats was based on open and competitive civil service examination (also known as the *Keju*).<sup>13</sup> Soon after the civil service examination was reinstated in the early Qing dynasty, officials were increasingly recruited via this channel, despite the existence of alternative channels (e.g., office purchase, succession, and recommendation). Bureaucrats usually started their careers from 7th-rank positions, such as county magistrate or junior assistant secretary in the Six Ministries, then progressed to vice prefect before moving to prefecture governor.

The bureaucracy of the Qing Empire developed highly institutionalized personnel regulations (Guy, 2010). All bureaucrats were subject to regular evaluation and debriefings on a three-year basis. Each position had specific qualification requirements and a corresponding career trajectory according to the stipulated career tracks. All the movements of bureaucrats between position types strictly followed a “map” of position-to-position career tracks (*Pinjikao*). While High-ranking positions above the deputy-provincial level were appointed by the emperor directly on the advice of his inner circle, the appointment of middle and low-ranking positions (including prefecture governorships) was governed by specific rules and procedures. Officers in certain 5th-rank positions could attain promotion qualification for prefecture-level positions (4th-rank) by the outstanding record in performance evaluation or recommendation from their superior officers.<sup>14</sup>

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<sup>11</sup> 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.

<sup>12</sup> 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).

<sup>13</sup> China’s civil service exam first originated during the short-lived Sui dynasty (581–618). The rulers of the Song dynasty (960–1276) reintroduced and thoroughly implemented this far-reaching recruitment method. After interruptions during the Mongolian rule, the civil exam system became fully consolidated in the Ming (1368–1643) and Qing (1644–1912) dynasties (Miyazaki, 1981; Chaffee, 1985; Elman, 2013). Chen and Kung (2021) show the impact of the commercial revolution on the origin of meritocratic bureaucracy in Song China.

<sup>14</sup> For instance, the promotion to prefecture governor was confined to several certain positions, namely the vice governor (*Tong Zhi*), the county magistrate (*Zhi Zhou*), the censor of the Censorate (*Jiancha Yushi*), the vice director in central ministries (*Lang Zhong* and *Yuanwai Lang*), and the assistant salt controller (*Yanyunsi*



**The appointment rule.** The Ministry of Personnel (MOP) used an elaborate rule-based process to allocate qualified officers to vacancies. The appointment rule was composed of two parts (Zhang, 2010).

The first was *seniority-based queuing*. Officers qualified for the prefecture-level positions would be grouped according to their qualification types (e.g., last position, transfer), then lined up for new vacant positions based on seniority. When it was their turn to fill new vacancies, the assignment followed a certain sequence according to qualification types.

The second was *random allocation to vacancies*. The appointment process took place monthly. Every month, retirement, rotation, promotion, and demotion created new vacancies. The MOP designated the “allocation cohort” from queuing officers corresponding to the number of available vacancies. Finally, officers of this allocation cohort were assigned to prefectures by randomly drawing lots.

The rationale for allocating vacancies at random was to ensure fairness and eliminate cronyism in the appointment process. The lottery allocation was well executed in Qing China. Manipulation in drawing lots was very rare (Will, 2002).<sup>15</sup> In Table 1 and Appendix Figure C1, we provide evidence that in the pre-reform period, governors assigned to treated and control prefectures were similar along a range of individual characteristics, consistent with the random nature in the final allocation. Appendix C provides further discussion and evidence on the appointment rule.

## 2.2 Reform of Appointment Methods for Prefecture Governors in 1736

While the rule-based appointment system effectively limited corruption and favoritism in personnel management, it was likely to result in talent mismatch as it ignored the considerable heterogeneity of officer characteristics and local governance conditions. The disadvantages of such a rigid rule-based procedure became increasingly pronounced after the rulers of the Qing empire 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).

Inspired by a proposal by Jin Hong, the provincial administrative commissioner of Guangxi province, the Qing government implemented a structural reform of the appointment system in the 1730s (Zhang, 2011). The main idea was to introduce more flexibility

*Yuntong*).

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<sup>15</sup> The random allocation of vacancies stemmed from the late Ming dynasty. Sun Piyang, the minister of the MOP during the Wanli Emperor’s reign, introduced the method of drawing lots in 1594 (Will, 2002).

in the appointment decision for a part of prefectures. To identify governance challenges confronting 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 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 was mainly associated with personnel management. 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. After that, prefecture governorships with importance ratings equal to or higher than three switched to a more discretionary method, in which the MOP and the emperor were both involved in selecting governors from the eligible candidate pool. The appointment of other governorships continued to follow the status quo rule.

While the new appointment methods did not change the potential candidate pool, there were more careful considerations during the process of discretion-based selection. Once prefecture governorships with high-importance ratings were vacant, first, 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. When preparing the shortlist, the MOP would use their “discretionary judgment” to select suitable candidates, despite their seniority in the candidate queue or qualification type. Typically, officers’ serving experiences, civil exam backgrounds, award and disciplinary records, and recommendations from provincial leaders or ministers were all taken into account. Then, the emperor would discuss the shortlist with the MOP, select an appropriate candidate for appointment to each post, and interview the chosen candidate face to face before the formal appointment took effect (Zhang, 2010).<sup>16</sup> Discretion could speed up the assignment of prefecture governors and pick candidates who would have been assigned to other equivalent-rank positions if they lined up for vacancies in the rule-based process. Of note, among the treated governorships modified to the discretionary appointment method, most positions

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<sup>16</sup> The appointment could be revoked if the emperor thought the chosen candidate was inappropriate after the interview, and he might sometimes select someone outside the shortlist (Zhang, 2010).

went under the MOP-emperor discretion. A relatively small number of posts were delegated to provincial leaders. These prefecture-level jurisdictions were independent departments (*Zhilizhou*) and prefectures where ethnic minorities congregated.<sup>17</sup>

### 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 exam, ethnicity, and hometown.<sup>18</sup>

**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 higher-ranking officials from official documents, biographies, and other historical archives. The coverage is more systematic for officials higher than the 4th rank. We use this source to construct ethnic and hometown connections between prefecture governors and top officials in central and provincial governments.

**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 more detailed discussion on sources and construction of our personnel data in Appendix B.

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<sup>17</sup> These regions were classified as *Miaojiang* (*Frontier with Miao*), which were deemed in complicated and unstable environments and required sufficient flexibility to deal with unexpected situations. Provincial leaders had potential advantages in local information but might also abuse their discretionary power for corruption.

<sup>18</sup> See Appendix Figure B1 for a sample of an appointment record in a prefecture gazetteer.

### 3.2 Governance Outcome Data

**Disaster relief and tax exemption.** To measure governance performance, we focus on disaster relief, an important public good delivered by local governments. Our data on disaster relief come from [Chen, Xiao and Xiong \(2012\)](#), who collect comprehensive records of government-conducted disaster relief programs from the *Veritable Records of the Qing*. We use the number of relief programs and a dummy variable for relief provision 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. We also collect data on land tax exemption, another common measure 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).

### 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 provides a comprehensive record of various types of natural disasters in historical China, including drought, floods, plagues, locust infestations, hurricanes, earthquakes, snowstorms, 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 B 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 individual 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 merit qualifications in the civil service examination (*Jinshi* or *Juren*), of which 17% had 1st-tier qualification (*Jinshi*). Less than 20% of governors had previous governorship experience. In terms of ethnic origin, most of the governors were Han Chinese and the remaining were from Manchu and Mongolian ethnic groups.<sup>19</sup> Column 3 reports the mean difference in governor characteristics between treated and control prefectures. Before the personnel reform, governors in treated prefectures showed no difference from those assigned to control prefectures.

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<sup>19</sup> The Qing bureaucracy consisted of officials with different ethnic origins including Manchu, Mongolian, and Han people. As the ruling elites, Manchu, Mongolian, and a small fraction of Han people were organized into the Eight Bannerman system that was separated from the remaining Han Chinese.

Bureaucrats with varying civil exam qualifications, experiences, and ethnicity were on average evenly appointed to prefectures, consistent with the appointment rule in the status quo where qualified candidates queued for vacancies based on seniority and were randomly assigned to prefectures. In Appendix Figure C1, we show that the two groups exhibit almost identical distribution patterns on exam qualification rank and years of experience.<sup>20</sup>

## 4 Discretion in Appointment and Governance Performance

### 4.1 Empirical Strategy

In our main analysis, we estimate a difference-in-differences specification comparing the outcomes of treated and control prefectures, before and after 1736, in a prefecture-year panel dataset:

$$Y_{it} = \beta \times Discretion_i \times Post_t + \theta_t + \lambda_i + \epsilon_{it} \quad (1)$$

where  $Y_{it}$  is the outcome if prefecture  $i$  in year  $t$ .  $Discretion_i$  is an indicator for the treatment prefectures that switched to the discretionary appointment, and  $Post_t$  is a dummy for years after 1736.  $\theta_t$  denotes year fixed effects and  $\lambda_i$  denotes prefecture fixed effects. The coefficient of interest  $\beta$  estimates the effect of discretionary appointment on governance performance. Finally, the standard errors  $\epsilon_{it}$  are clustered at the prefecture-level.

A key identification assumption for causal identification is that in the absence of the reform, outcomes in treated prefectures would have evolved similarly to those in untreated prefectures. If this assumption holds, we should observe no systematic difference in trends of disaster relief between the treatment and control groups before the reform. To assess the common trends assumption, we also estimate an event-study specification where the treatment effect is allowed to vary in each period:

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

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

<sup>20</sup> See Appendix C for more discussion on the random allocation in the appointment process.

**Conditional Balance.** As noted in the Historical Background Section, prefectures were not chosen randomly for the 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 determined the treatment adoption and then flexibly control the interactions of these determinants with the  $Post_t$  dummy in regression.<sup>21</sup> Table 2 reports the difference between treated and control prefectures. Unsurprisingly, the 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. Column 5 of Table 2 shows that treated and untreated prefectures are now very comparable in transportation conditions, population density, taxation, and conflict frequency.<sup>22</sup> 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, grain taxation, educational infrastructure, and the strength of clan organizations.

## 4.2 Main Results on Disaster Relief

To investigate the impact of discretion in appointment on governance performance, we focus on disaster relief as our key outcome variable. A sufficient and timely response to natural disasters is an important measure of government responsiveness (Besley and Burgess, 2002). Late imperial China was largely an agricultural economy vulnerable to frequent natural disasters. Crop failures induced by natural shocks threatened people's

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<sup>21</sup> 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 *taxation*; and the frequency of previous major conflicts for *violence*.

<sup>22</sup> Appendix Figure A1 visualized the bias between treated and control prefectures before and after matching. Appendix Figure A2 shows the spatial distributions of the matched sample.

lives and catalyzed conflicts. Therefore, preventing people from starving was essential to maintain social stability (Jia, 2014; Rosenthal and Wong, 2011). Hence disaster relief was the most important public good provided by local governments, and state involvement remained strong throughout the 18th century. In the middle-to-late 18th century, there were on average 12.3% prefectures receiving disaster relief every year. The government operated a nationwide granary system for food distribution and smoothing out grain prices from year to year. Local governments ran “ever-normal granaries” in every county and were supposed to monitor charity granaries in major towns and rural community granaries. It 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).

Panel A of Table 3 presents the main results. Column 1 reports the baseline DID results with prefecture and year fixed effects. Prefectures switching to discretionary appointments had 0.4 more disaster relief frequency after the reform compared with other prefectures that retained the rule-based appointment system. This increase is substantial relative to the mean number of programs (0.287). To control for the differential impacts of the initial difference between treatment and control groups, we add the treatment determinant variables interacted with  $Post_t$  dummy in column 2. Our results are robust. Finally, to further increase the comparability between treated and control prefectures, we restrict our analysis to the matched sample with comparable characteristics where each treated prefecture is matched with a control prefecture. We continue to find positive and significant effects on disaster relief. We also use a dummy variable for relief provision as a dependent variable in columns 4–6. We find that treated prefectures increased the likelihood of relief by 6.5 percentage points.

Panel A of Figure 2 illustrates the event study results, in which we estimate the effect on the probability of relief (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. Our results are not driven by changes in demand for disaster relief. Figure 3 shows that treated prefectures were not associated with any noticeable changes in natural disaster frequency and rainfall shock. In Appendix Table A2 and Table A3, we further show that treated prefectures did not experience any change in rainfall levels and the likelihood of various types of disasters.



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

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

where the coefficient of triple differences  $\alpha$  estimates the effect on the *link* between disaster occurrences and relief provision, which captures the effects on the state responsiveness. Panel B of Table 3 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 regional differences. We find consistent results using the dummy variable for the provision of relief (columns 4–6).

In panel B of Figure 2, 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. 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 point estimates (Table 3, Panel B). In Appendix Figure A3, we show that the event study results are also robust to controlling for treatment determinant variables and using the matched sample with comparable characteristics.<sup>23</sup> Taken together, the results are consistent with the finding that appointment reform increased state responsiveness.

**Discussion and Robustness.** We conduct a host of robustness checks. First, 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) (Appendix Table A4).<sup>24</sup> Second, our results are robust to controlling for province-year fixed effects to capture province-specific time-varying shocks, such as the idiosyncratic impacts of provincial leaders (Appendix Table A5). Third, our results hold when excluding the province capitals (Appendix Table A6).<sup>25</sup>

<sup>23</sup> We find a consistent pattern using the number of reliefs (Appendix Figure A4).

<sup>24</sup> Recall that the adoption of the discretionary appointment was determined by the importance rating of each prefecture, with 3 being the cutoff.

<sup>25</sup> As a political and economic hub of a province, provincial capitals might not strictly follow the appointment rule in the pre-reform period.

or using the frequency of natural disaster (Appendix Table A7).

### 4.3 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 DID 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. The results are presented in Appendix Table A8. We find that the appointment reform decreases the probability of urban riots by 0.75 percentage points (columns 1). This effect is significant and large compared with the mean probability of riots (0.84%). The result holds when we 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.

## 5 Mechanisms

In this section, we investigate the mechanism through which the appointment reform affected governance performance. First, discretion in appointment could improve the selection of governors, which is also the initial goal of the reform. Second, the appointment reform might also affect the incentives of governors. Finally, we discuss other potential alternative explanations: preferential policy by upper governments and governors' political connections.

### 5.1 Selection

We first investigate the mechanism that discretionary appointment could improve performance in disaster relief through a better selection of governors. Under the status quo ap-

pointment rule, a high-quality officer who best matched the task of governorship would need to wait for some time for new vacancies subject to his seniority and qualification type. Since new vacancies of different position types occurred in idiosyncratic order, he might be assigned to other equivalent-rank positions (e.g., ministry directors in central government) instead of prefecture governor if those positions went vacant first. Finally, if his allocation cohort turned out to be governorship, he would randomly be assigned to a vacant prefecture regardless of the local condition and his talent. In the new appointment system, discretion enabled the Ministry of Personnel and the emperor to use private and public information in screening and selecting officers from the qualified candidate pool, case by case for each post, despite their seniority in the candidate queue or qualification type.

**Change in observable governor quality.** To investigate the selection mechanism, we first examine whether the governor characteristics in treated prefectures changed after the appointment reform. Using the individual information from the personnel record data, we can look at two observable quality measures, the governor's serving record and civil service exam qualification, as proxies for experience and competence. We replace the outcome variable in equation (1) with the governor quality measures. The individual level results are shown in Table 4. We find substantial impacts of discretionary appointment on both the experience and competence margins. Governors appointed by the discretionary process 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 (0.29), this implies a striking 51% increase (column 1). The effect on competence is also sizeable as shown in column 4 of Table 4: the likelihood of having merit civil exam qualifications increases by 6 percentage points (a 16% increase relative to the mean). In columns 2 and 5, we add interactions between the  $Post_t$  dummy and six treatment determinant variables. Columns 3 and 6 report reports the result using the matched sample with balanced characteristics. We continue to find strong effects on governors' experience and civil exam qualification with litter change in magnitude.

We corroborate our results with 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 two years), and provincial capitals (Appendix Table A9). Second, we find similar results when restricting the sample to relatively homogeneous prefectures rated 3 and 2 (columns 1 and 3 of Appendix Table A10) or controlling for province-year fixed effects (columns 2 and 4 of Appendix Table A10). Third, the results are robust to using the continuous measurement for experiences by years of services (Appendix Table A11).

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 officers were selected for the treated prefectures, the average quality of remaining candidates who were the potential governors for control prefectures might automatically decrease. We argue that this is unlikely to threaten our estimate for two reasons. First, treatment unnecessarily affect the potential outcome in control prefectures under the rule-based appointment process. In many cases, discretion worked by picking officers who could be assigned to other equivalent-rank positions rather than governorship. Second, the number of treatment units is very small relative to the large size of the potential candidate pool qualified for prefecture governorships.<sup>26</sup> We provide further discussion on the spillover effect in Appendix D.

**Comparing selected and unselected governors.** The selection could also work along unobservable dimensions. To further examine the selection effect, we exploit the exogenous rotation of governors to control prefectures in the post-reform period as another empirical design. As described in section 2.1, the rule-based appointment process resulted in a quasi-random allocation of officers across prefectures, including transferring governors to new prefectures. Some governors who had previously been *selected* through the discretionary system were rotated to control prefectures later. These *selected governors* accounted for roughly 10% of governors of control prefectures in the post-reform period (Figure 4). This means that, within the control group prefectures, there would be as good as random variation in governors' selection status. We exploit this variation to compare the outcomes of *control prefectures* that, by chance, were assigned a *selected* governor to those that were assigned an *unselected* one. This allows us to directly test the selection effect on relief performance and capture unobservable individual traits behind the selection. Moreover, this design had the advantage of ruling out confounding differences between treated and control prefectures that might threaten our DID results. We test the selection effect by estimating the following regression, restricting the sample to control prefectures in the post-reform period:

$$Y_{it} = \beta \times Selected_{it} + \theta_t + \lambda_i + \epsilon_{it} \quad (4)$$

where  $Selected_i$  is an indicator equal to one if the serving governor had previously been *selected* through the discretionary system.

We validate the balance of allocation patterns of selected governors across control prefectures. In Appendix Table A12, we show that prefectures' important rating did not

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<sup>26</sup> By looking at the distribution of civil exam rank for the candidate pool in 1774, we estimate that the potential negative spillover effects would only lead to a decrease by 0.99%-2.10% (see Appendix D).

predict the allocation of selected governors. Being assigned a selected governor has little correlation with a wide range of prefecture characteristics, including taxation, population density, geographical features, agricultural suitability, and school, with and without the inclusion of year and prefecture fixed effects. Moreover, we estimate equation (4) using disaster occurrence as outcomes and show that prefectures run by selected governors were also similar in the likelihood of disaster and rainfall shock (Appendix Table A13).

Table 5 reports the results testing whether selected governors performed better than unselected ones. We find significant effects of selection. Officers who had previously been selected for governors by discretion increased the probability of disaster relief by 2.8 percentage points, about a 40% increase relative to the mean. The results remain robust after controlling for the occurrence of natural disasters and the governor's tenure fixed effects and ethnicity fixed effects. The results provide suggestive evidence that discretionary appointments indeed selected higher-performing officers. Taken together, our results on observable governor quality and overall selection effects imply that the selection of governors plays an important mechanism behind the effects on governance performance.

## 5.2 Incentives

We next investigate whether the appointment reform might affect performance by changing the incentive of governors. While the reform did not change the formal regulations for evaluation or promotion, it is possible that treated prefectures were implicitly given higher promotion prospects, inducing governors serving there to exert greater effort.

Explicitly testing this hypothesis is challenging because we do not have direct measures of governors' efforts. Nevertheless, we can exploit the within-officer variation in prefectures' treatment status to examine whether the *same* governor's performance changed when he served in treated and control prefectures. If treated prefectures provided stronger promotion incentives, we should expect better performance when they served in treated prefectures.

To test the role of incentives, we compare the performance of governors serving in treated and control prefectures, in the post-reform period, controlling for disaster occurrence, year fixed effects, and a set of treatment determinant variables. Table 6 reports the results. In column 1, we conduct a cross-sectional comparison without individual fixed effects. Not surprisingly, treated prefectures were more likely to receive relief, consistent with our previous DID results. The estimated difference (0.42 percentage points) could be the combination of the selection effect and incentive effect. We then hold the selection margin constant by adding individual fixed effects in column 2. The marked decrease in coefficient (0.26) is consistent with the important role of the selection effect in driving the

increase of relief provision. Nevertheless, the coefficient remains positive and significant, suggesting that the *same* governors increased relief probability when serving in treated prefectures.

To ensure that we are not capturing differences between single-term selected and unelected governors, we restrict the sample to governors who ever switched between treated and control prefectures in column 3. In column 4, we further control for tenure fixed effects. We find that the relief probability increased by 0.29 percentage points when governors switched from control to treated prefectures. Moreover, we also find positive effects on promotion. While the average likelihood for promotion to higher-ranking positions was as low as 1.2%, officers had a higher promotion likelihood when holding governorships in treated prefectures (columns 5 and 6), consistent with stronger career incentives for treated governorships. In sum, these results suggest that incentives are likely to be another driver of the overall effects.<sup>27</sup>

### 5.3 Preferential policy by upper governments

An alternative explanation is that the positive effect on disaster relief provisions could be the result of upper governments' preferential policy towards the treated prefectures. Prefectures that adopted discretionary appointments might become more salient across jurisdictions as they had high importance ratings, thus both provincial and central governments might place more emphasis on treated prefectures and prioritize them in terms of resource distribution. There was no formal policy change in disaster relief that explicitly favored high-rating prefectures. Nonetheless, we conduct several tests to assess whether this was the case in practice.

First, we investigate whether treated 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. These reports (the palace memorials, *zouzhe*) 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 of a prefecture in reports, therefore, reflect greater attention paid by senior officials and the central government. Following this logic, we construct an attention index

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<sup>27</sup> One may also be concerned that the reform might change the turnover rate of governors. Governors appointed to the treated prefectures might have longer duration of tenure and a longer time horizon could lead to better performance. However, we do not find governors in the treated prefectures having a tenure that was either longer or shorter than those in the control prefectures (Appendix Table A14).

based on the relative frequency of being mentioned in reports.<sup>28</sup> The results are presented in Table 7. In column 1, we find no effect of the reform on the attention index, suggesting that high-ranking local officials did not pay more attention to treated prefectures.

Second, we check whether the 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 ratings also had more relief *within* the control (treatment) group, holding the appointment method unchanged. In column 3 of Table 7, we restrict the sample to treated prefectures and compare rating-4 prefectures with rating-3 prefectures. The result shows no significant difference between 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). We find no evidence of 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.<sup>29</sup> Inconsistent with this prediction, Appendix Table A15 shows no effect of provincial fiscal surplus on disaster relief programs.

#### 5.4 Connections and distributive politics

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.<sup>30</sup> Another alternative explanation could be that our results are driven by connection-induced resource allocation. 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

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<sup>28</sup> Specifically, the attention index is calculated as the number of times that a prefecture was mentioned in the reports, divided by the number of times that all prefectures are mentioned in a given year (rescaled by  $\times 100$ ).

<sup>29</sup> 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.

<sup>30</sup> See, for example, Brollo and Nannicini (2012), Burgess et al. (2015), Curto-Grau and Zudenkova (2018), and Jiang and Zhang (2020).

have more connections with the central government. Using the background information from the personnel record 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 column 1 of Table 8. Governors appointed to the treated prefectures did not have more connections with the central government after the reform. Similarly, we do not find they were more connected with top officials in the MOP (column 2), who were directly involved in their appointment decision, suggesting that better connections are unlikely to play a major role.

Second, we investigate the effect of appointment reform on land tax exemptions, another regular measure for coping with disaster shocks that did not entail resource reallocation. In the Qing dynasty, the central government frequently granted land tax exemptions to reduce the tax burden on farmers during disasters (Will, 1990). As the introduction of land tax exemptions was a routine procedure 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 treated prefectures do not perform better in surveying and reporting disasters. The results in columns 2-3 of Table 8 show that this is not the case. Treated prefectures were also more likely to receive tax exemptions after the reform. These patterns are unlikely attributable to governors' connections.

## 6 When is Discretion in Appointment Beneficial?

These overall positive effects of discretion on governance performance may seem to be somewhat counter-intuitive given the negative effects of discretion in public appointments documented by previous works.<sup>31</sup> This contrast lies in the double-edged sword feature of discretion, which could be beneficial because of a better use of information and harmful due to favoritism. In this section, we explore the conditions under which its benefits outweigh its costs.

Different appointers at the top of the organizational hierarchy differ in how they use discretionary power. The overall effect of discretion depends on the extent to which an appointer's interest is aligned with the organization (Holmstrom, 1984; Aghion and Ti-

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<sup>31</sup> See, for example, Xu (2018) on incentives, Colonnelli, Prem and Teso (2020) on patronage and public hiring, and Akhtari, Moreira and Trucco (2022) on education service delivery.



role, 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. Of the treated prefectures switching appointment method, the majority were under the discretionary process involving both the MOP and the emperor, but a relatively small number of posts were delegated to the provincial leaders after the reform.<sup>32</sup> The 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-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). Provincial leaders, however, 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 discretion.<sup>33</sup>

To test these predictions, we break down treated prefectures into two groups, where the governor appointments were subject to MOP-emperor's discretion and provincial leaders, and examine the heterogeneous effects on governance performance. Table 9 reports the results. We find the MOP-emperor discretionary appointments had a strong effect on disaster relief provision (column 1). The increase in relief probability is greater when a disaster occurred (column 2). In contrast, treated prefectures under the provincial leaders generated smaller and insignificant effects (columns 3-4). Figure 5 presents the event-study results broken down by the two treatment groups, confirming the greater effects by the MOP-emperor's discretion.<sup>34</sup>

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 Appendix Table A17 presents the results on ethnic ties. Compared to governors appointed by the status quo rule, governors appointed by provincial leaders are more likely to have an ethnic tie to their provincial leaders. The effects are stronger for Han provincial leaders than for their Bannerman counterparts, who

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<sup>32</sup> These prefecture-level jurisdictions were independent departments (*Zhilizhou*) and prefectures where ethnic minorities congregated. These regions were classified as *Miaojiang/Yanzhang* posts.

<sup>33</sup> 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).

<sup>34</sup> We also find consistent heterogeneous effects in the reduction of social unrest (Appendix Table A16).

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).<sup>35</sup>

We test the effect of discretion on the probability of sanction in Appendix Table A17, 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,<sup>36</sup> this may reflect the slack monitoring on the connected appointees. 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 the functioning of public organizations remain an open question. Using the unique setting of China's imperial bureaucracy, which experienced an organizational reform that some governorships switched from a rule-based appointment system to a more discretionary method, systematic account for this question. Relying on a comprehensive dataset of governor appointment records for the 1644–1820 period, linked with data on governance performance, we find that discretionary appointment improved disaster relief delivery and state responsiveness. We provide evidence that a better selection of governors and change in incentives are likely important mechanisms underlying the positive effects on performance. Moreover, we find that these positive effects are driven by discretionary appointments jointly made by the MOP and the emperor, while

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<sup>35</sup> 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.

<sup>36</sup> Investigation and the final decision of sanction were made by the central government, based on the reports from provincial-level officials.

those by provincial leaders show smaller effects, 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. We show that in an institutional environment where appointment follows a rigid rule-based process, 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. Discretion 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 in the specific context. This implication advances our understanding of the rule versus discretion debate.

We last note that extrapolations to other contexts might be done with caution. In our case, the status quo rule refers to an elaborate rule-based system combining seniority-based features and random allocation to vacancies. Rules with randomness may seem special, but they are frequently utilized in the practices of many public organizations. For instance, it is used in government procurement auctions ([Ferraz, Finan and Szerman, 2015](#)), the assignment of municipal auditors ([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). This also leaves for future work to dig into the rule-discretion trade-off in other contexts besides bureaucratic appointments.

## **Declaration of Competing Interest**

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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# Figures and tables

Figure 1. The distribution of regional importance ratings across prefectures

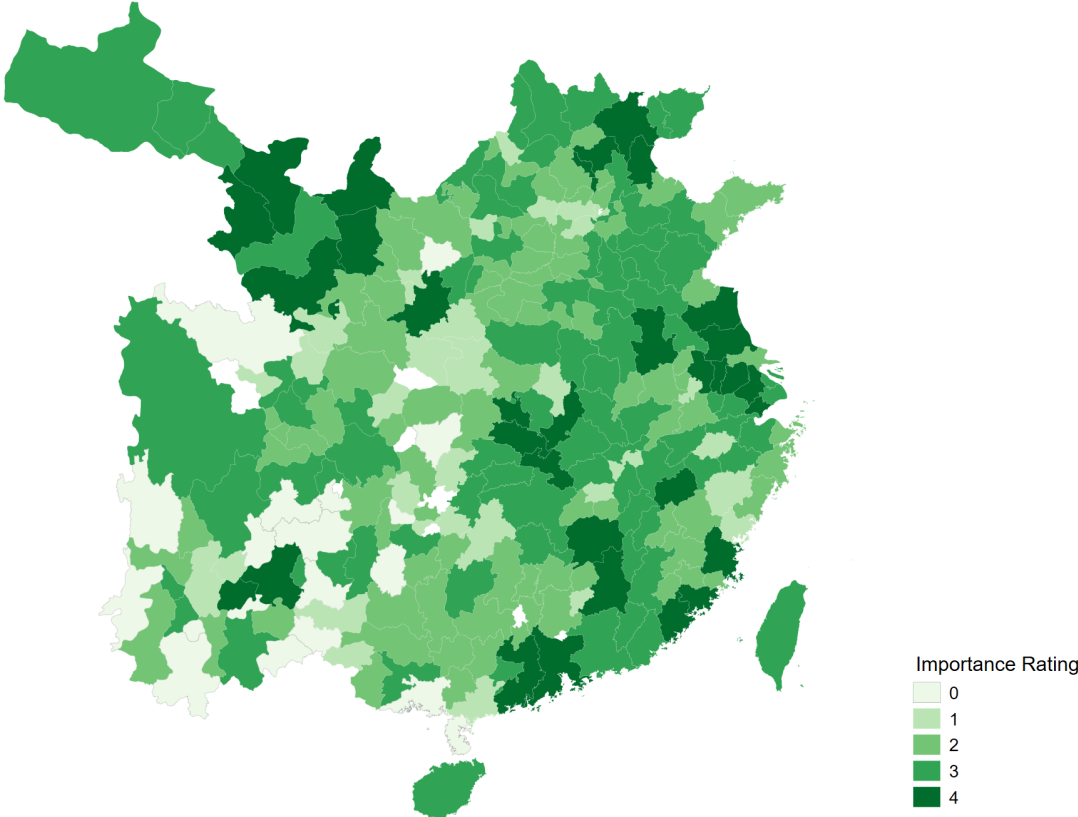
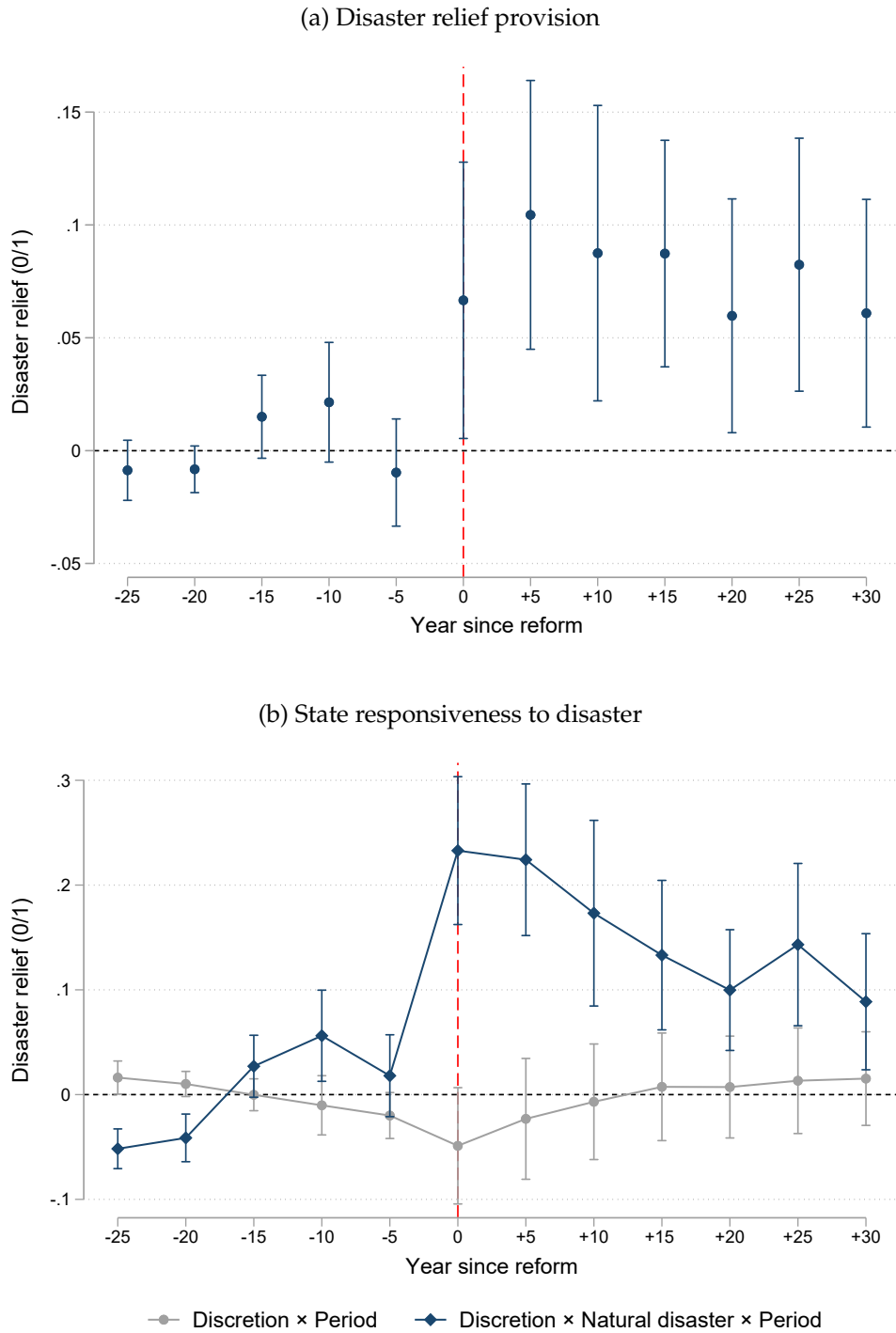
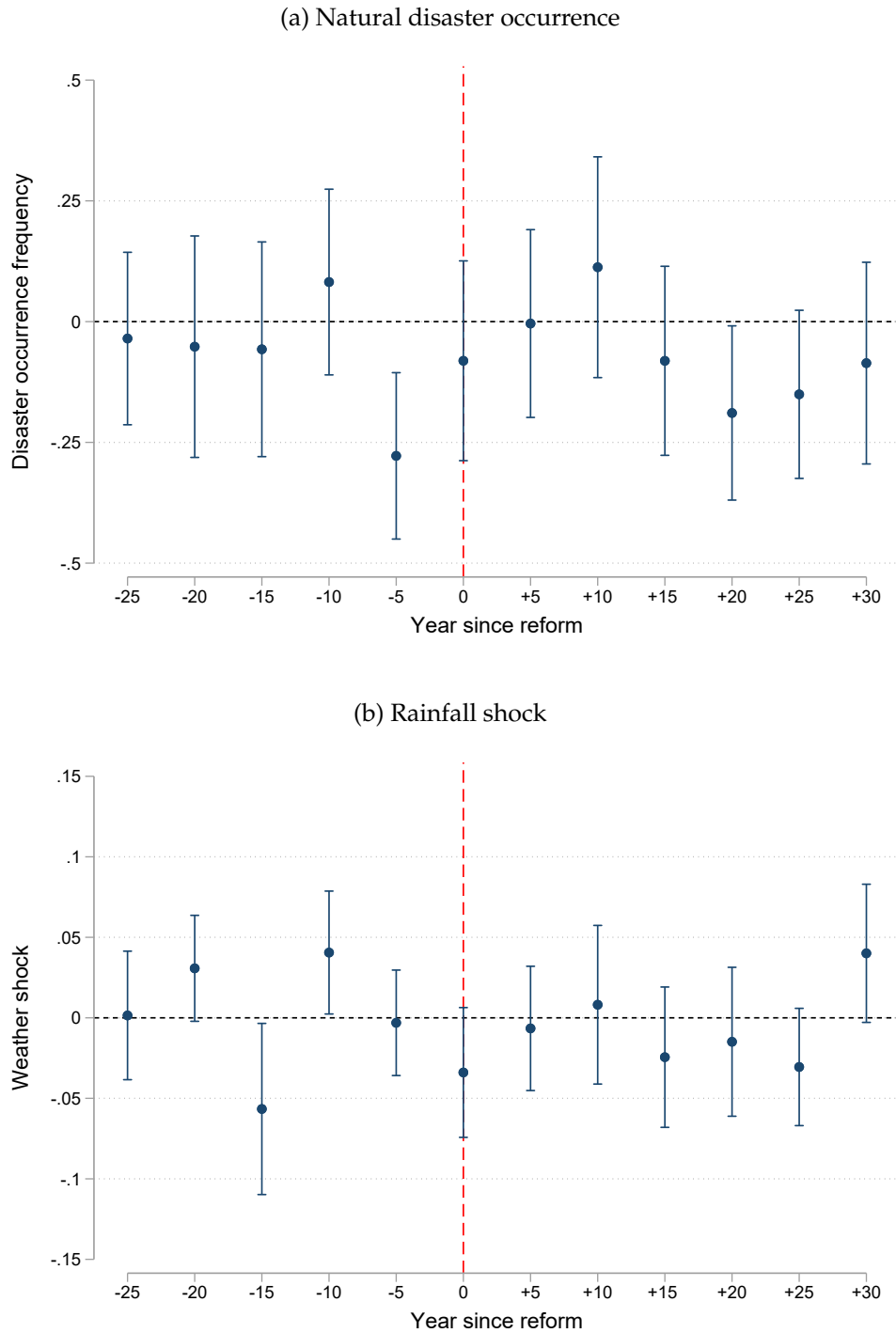


Figure 2. Discretionary appointment and governance performance: event study



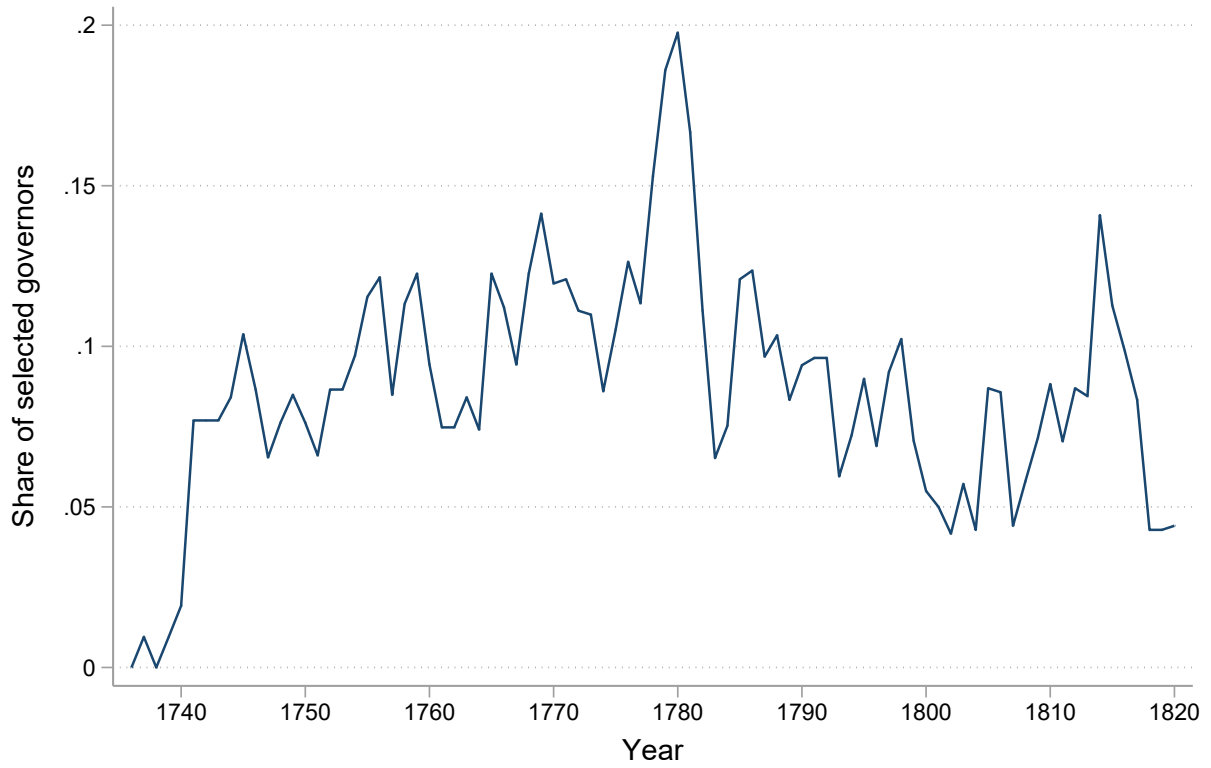
Notes: This figure shows the dynamic effects of discretionary appointment on the performance of disaster relief in 5 years bin. The dependent variable is a dummy variable for relief provision. The vertical solid lines with caps plot the 95% confidence intervals. Standard errors clustered at the prefecture level. The period before 1710 is omitted as the reference group.

Figure 3. Disaster occurrence: event study



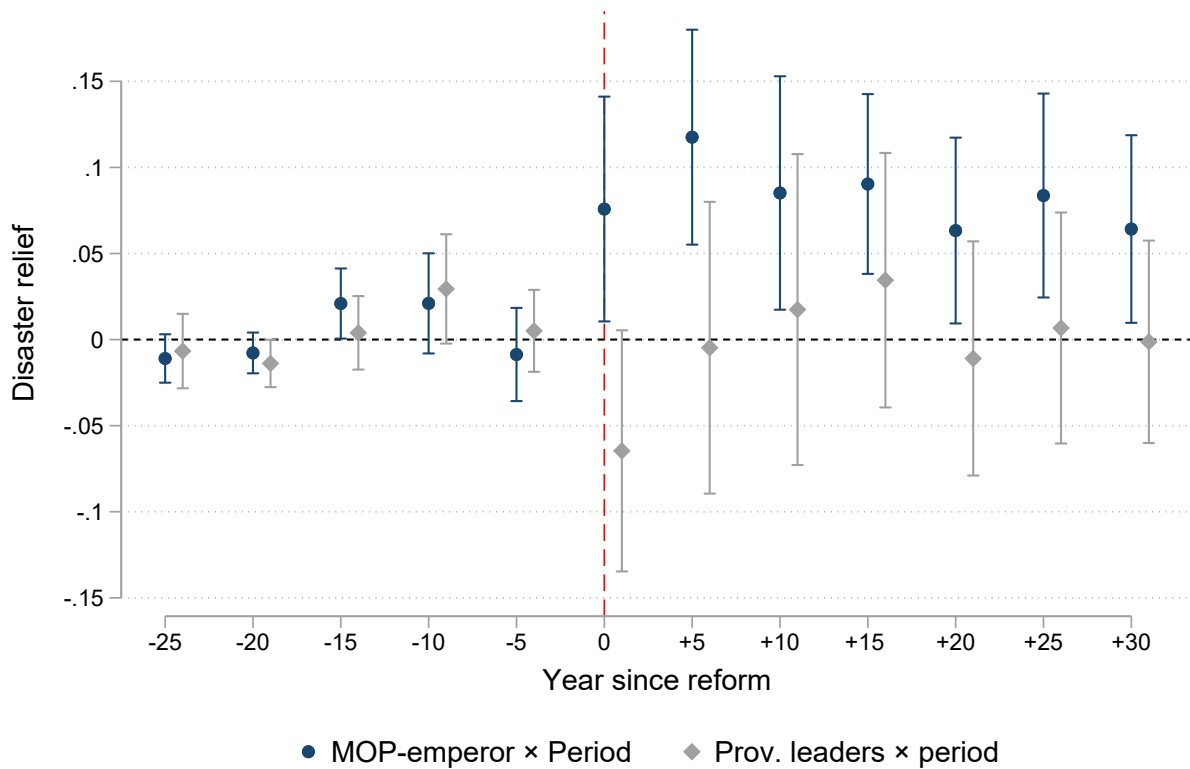
Notes: This figure shows the dynamic effects of discretionary appointment on the performance of disaster relief in 5 years bin. The dependent variable in panel A is the frequency of natural disasters. The dependent variable in panel A is a dummy variable for rainfall shock. The vertical solid lines with caps plot the 95% confidence intervals. Standard errors clustered at the prefecture level. The period before 1710 is omitted as the reference group.

Figure 4. The share of selected governors in control prefectures



Notes: This figure shows the share of selected governors in control prefectures who had previously been selected through the discretionary system, in the post-reform period.

Figure 5. Discretionary appointment and governance performance: event-study results by appointees



Notes: This figure presents the dynamic effects of discretionary appointment on the length of tenure in 5 years bin. The dependent variable is a dummy variable for relief provision. The vertical solid lines with caps plot the 95% confidence intervals. Standard errors clustered at the prefecture level. The period before 1710 is omitted as the reference group.

Table 1. Governors characteristics in pre-reform period: treated vs control prefectures

	(1) Control prefectures	(2) Treated prefectures	(3) Difference
Merit civil exam qualification	0.285	0.293	0.009 (0.015)
Merit civil exam qualification (1st-tier)	0.171	0.177	0.006 (0.012)
Merit civil exam qualification (2nd-tier)	0.114	0.117	0.003 (0.010)
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 the raw differences in means between the control group and treatment group, as well as the corresponding standard errors (in parentheses). \*\*\*, \*\*, \* denote significance at 1%, 5%, 10% levels.



Table 2. 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 <sup>2</sup> )	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)
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 B.4 for the data source of other characteristics in Panel B.

Table 3. Discretionary Appointment and governance performance

<i>Panel A: provision of disaster relief</i>						
	Disaster relief programs			Disaster relief (0/1)		
	(1)	(2)	(3)	(4)	(5)	(6)
Discretion × Post	0.431*** (0.117)	0.298*** (0.107)	0.366*** (0.135)	0.066*** (0.018)	0.048*** (0.018)	0.065*** (0.022)
Prefecture FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls × Post		Y			Y	
PSM sample			Y			Y
Observations	30,799	30,580	23,832	30,799	30,580	23,832
<i>Panel B: state responsiveness to natural disaster</i>						
	Disaster relief programs			Disaster relief (0/1)		
	(1)	(2)	(3)	(4)	(5)	(6)
Discretion × Post	0.029 (0.066)	-0.032 (0.070)	0.007 (0.074)	0.009 (0.014)	0.001 (0.014)	0.008 (0.016)
Natural disaster	0.105*** (0.029)	0.139*** (0.028)	0.084** (0.035)	0.030*** (0.005)	0.035*** (0.004)	0.027*** (0.006)
Discretion × Post × Natural disaster	0.782*** (0.124)	0.697*** (0.118)	0.754*** (0.148)	0.112*** (0.016)	0.099*** (0.015)	0.119*** (0.020)
Prefecture FE	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y
Controls × Post		Y			Y	
PSM sample			Y			Y
Observations	30,799	30,580	23,832	30,799	30,580	23,832
Mean of D.V.	0.303	0.303	0.280	0.068	0.068	0.067

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 the prefecture level. \*\*\*, \*\*, \* denote significance at 1%, 5%, 10% levels.

Table 4. Discretionary Appointment Improved Governor Quality

	Experiences			Competence		
	(1)	(2)	(3)	(4)	(5)	(6)
Discretion $\times$ Post	0.151*** (0.024)	0.128*** (0.022)	0.130*** (0.027)	0.060*** (0.022)	0.066*** (0.023)	0.066** (0.026)
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
Mean of D.V.	0.290	0.290	0.276	0.379	0.379	0.373
Observations	9,229	9,149	7,006	9,229	9,149	7,006

Notes: The unit of observation is appointment. Competence is a dummy variable set to 1 if the appointed governor has merit civil exam qualifications (*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 the prefecture level. \*\*\*, \*\*, \* denote significance at 1%, 5%, 10% levels.

Table 5. Selection effect and disaster relief

	Disaster relief (0/1)		
	(1)	(2)	(3)
Selected	0.028** (0.012)	0.028** (0.012)	0.028** (0.012)
Natural disaster		0.087*** (0.011)	0.087*** (0.011)
Prefecture FE	Y	Y	Y
Year FE	Y	Y	Y
Tenure FE			Y
Ethnicity FE			Y
Sample	Control prefectures, post-reform		
Mean of D.V.	0.069	0.069	0.069
Observations	6,542	6,542	6,542

Notes: The unit of observation is prefecture-year. The sample is restricted to control prefectures in the post-reform period. Selected is an indicator equal to one if the serving governor had previously been selected through the discretionary system. Natural disaster is a dummy variable for the occurrence of any natural disasters. Robust standard errors in parentheses are clustered at the prefecture level. \*\*\*, \*\*, \* denote significance at 1%, 5%, 10% levels.

Table 6. Testing the role of incentives

	Disaster relief (0/1)				Promotion	
	(1)	(2)	(3)	(4)	(5)	(6)
Treated prefecture	0.042** (0.018)	0.026* (0.015)	0.031** (0.014)	0.029** (0.014)	0.014*** (0.005)	0.014*** (0.005)
Natural disaster	0.109*** (0.011)	0.088*** (0.009)	0.109*** (0.016)	0.109*** (0.016)		
Year FE	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y
Individual FE		Y	Y	Y	Y	Y
Tenure FE				Y		Y
Sample	Full sample, post reform			Treatment-control switch		
Mean of D.V.	0.121	0.121	0.138	0.138	0.012	0.012
Observations	14,143	13,648	3,915	3,914	3,915	3,914

Notes: The unit of observation is prefecture-year. Columns 1-2 using the full sample in the post-reform period. Columns 3-6 restrict the sample to governors who ever switched between treated and control prefectures. 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. Column Robust standard errors in parentheses are clustered at the prefecture level. \*\*\*, \*\*, \* denote significance at 1%, 5%, 10% levels.

Table 7. Other Mechanisms: Preferential Policy by Upper Governments

	Attention index (1)	Disaster relief (0/1)	
		(2)	(3)
Discretion × Post	0.046 (0.044)		
Importance rating 4 × Post		0.017 (0.038)	
Importance rating 2 × Post			-0.014 (0.013)
Prefecture FE	Y	Y	Y
Year FE	Y	Y	Y
Controls × Post	Y	Y	Y
Sample	All	Treated	Control
Mean of D.V.	0.433	0.097	0.041
Observations	21,157	14,878	15,921

Notes: The unit of observation is prefecture-year. In column 1, the sample is restricted to the period between 1723-1820 due to the availability of report data. 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 × 100). Column 2 restricts the sample to treated prefectures (rating 3 and 4). Column 3 restricts the sample to control prefectures (rating 0, 1, and 2). \*\*\*, \*\*, \* denote significance at 1%, 5%, 10% levels.

Table 8. Other mechanisms: Connection and distributive politics

	Connection with central gov't MOP		Tax exemption (0/1)	
	(1)	(2)	(3)	(3)
Discretion × Post	0.024 (0.027)	0.016 (0.027)	0.043** (0.020)	0.042** (0.020)
Natural disaster				0.072*** (0.007)
Prefecture FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Mean of D.V.	0.352	0.232	0.142	0.142
Observations	30,580	30,580	30,580	30,580

Notes: The unit of observation is prefecture-year. The dependent variable in column 1 is the number of top central officials who shared hometowns with the given prefecture governors in a given year. Connection with MOP is the number of top MOP officials (the Minister and the Vice-minister of Personnel) who shared hometown. In columns 3–4, 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 the prefecture level. \*\*\*, \*\*, \* denote significance at 1%, 5%, 10% levels.

Table 9. Discretionary Appointment and Governance Outcomes by Appointers

	Disaster relief (0/1)			
	(1)	(2)	(3)	(4)
Discretion (MOP-emperor) × Post	0.049** (0.021)	-0.001 (0.017)		
Discretion (MOP-emperor) × Post × Natural disaster		0.094*** (0.015)		
Discretion (Prov. leader) × Post			0.021 (0.027)	0.005 (0.019)
Discretion (Prov. leader) × Post × Natural disaster				0.046 (0.034)
Natural disaster		0.038*** (0.004)		0.041*** (0.005)
Prefecture FE	Y	Y	Y	Y
Year FE	Y	Y	Y	Y
Controls × Post	Y	Y	Y	Y
Sample	MOP-emperor vs. control		Prov. leaders vs. control	
Observations	28,168	28,168	21,161	21,161

Notes: The unit of observation is prefecture-year. In columns 1-2, the sample is restricted to treated prefectures switching to the MOP-emperor's discretion and control prefectures. In columns 3-4, the sample is restricted to treated prefectures switching to 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.

# Appendix

## A Additional Results

Figure A1. Standardized Bias between Treated and Control Prefectures before and after Propensity Score Matching

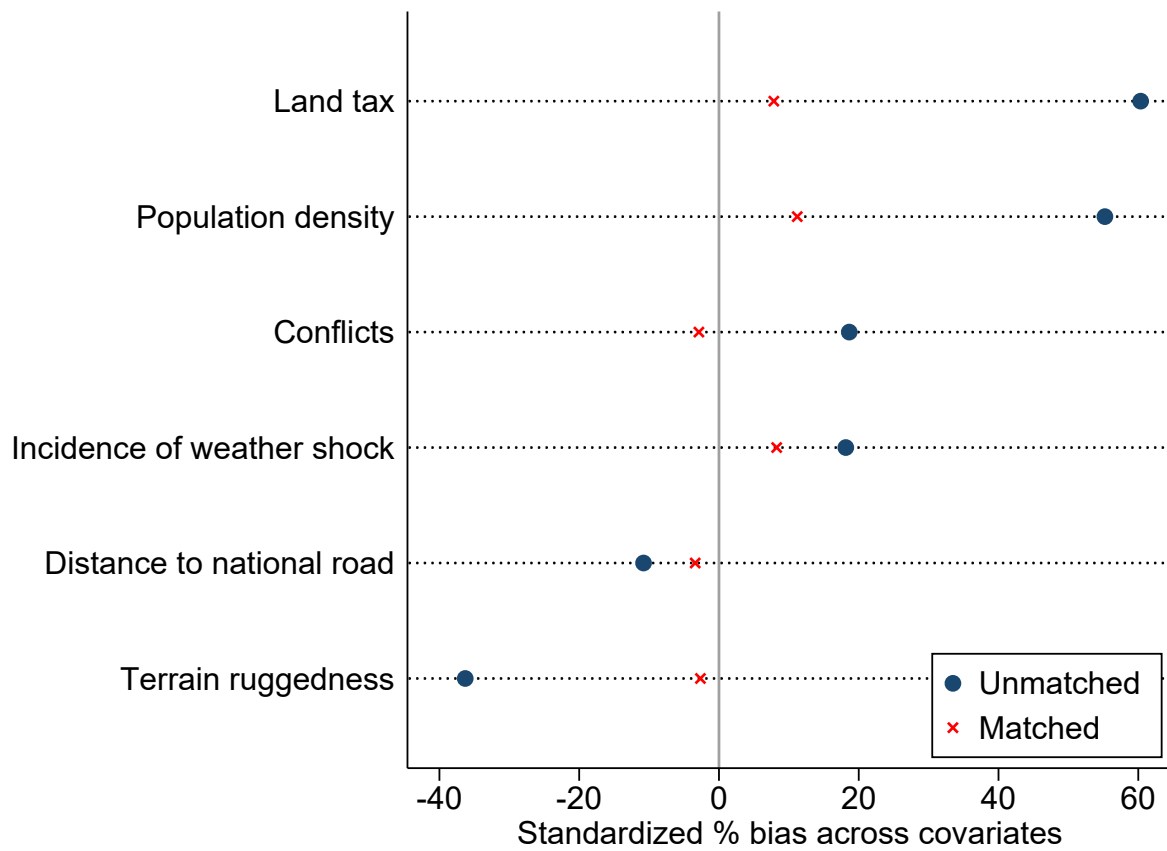




Figure A2. The Distributions of Matched Treated and Untreated Prefectures

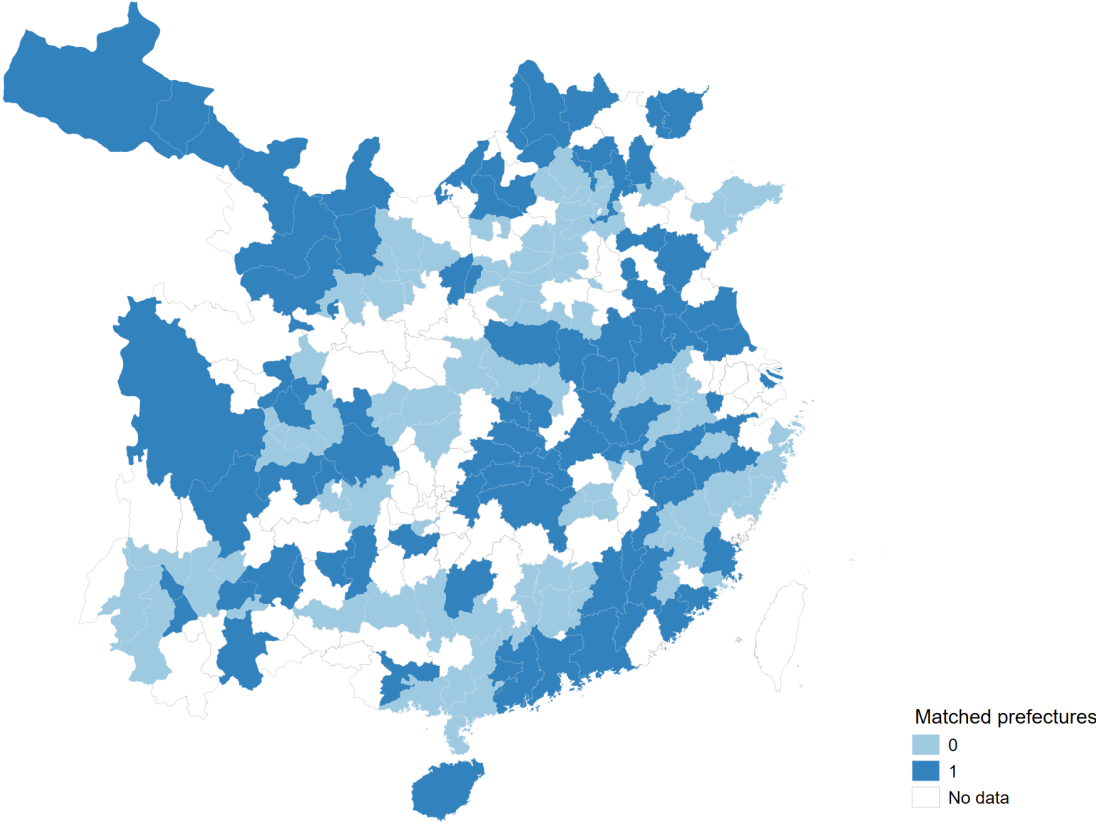
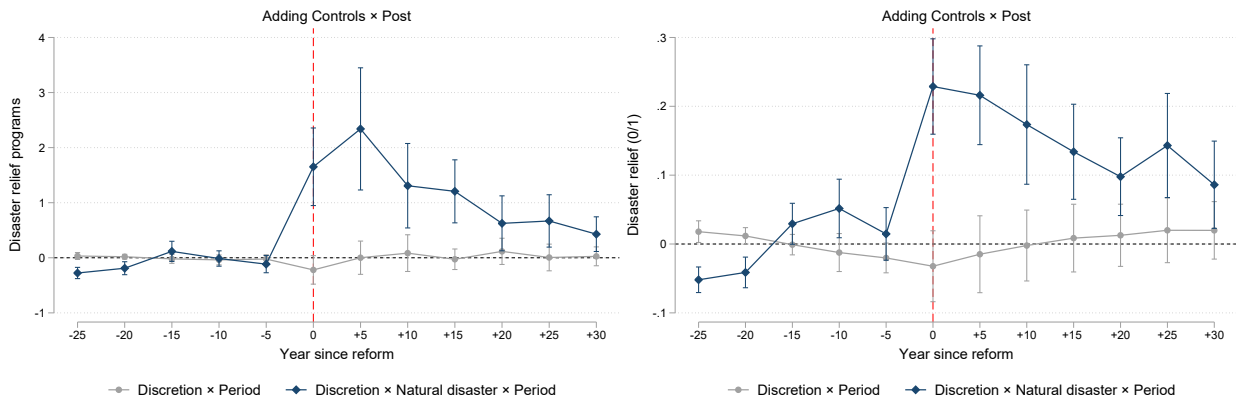
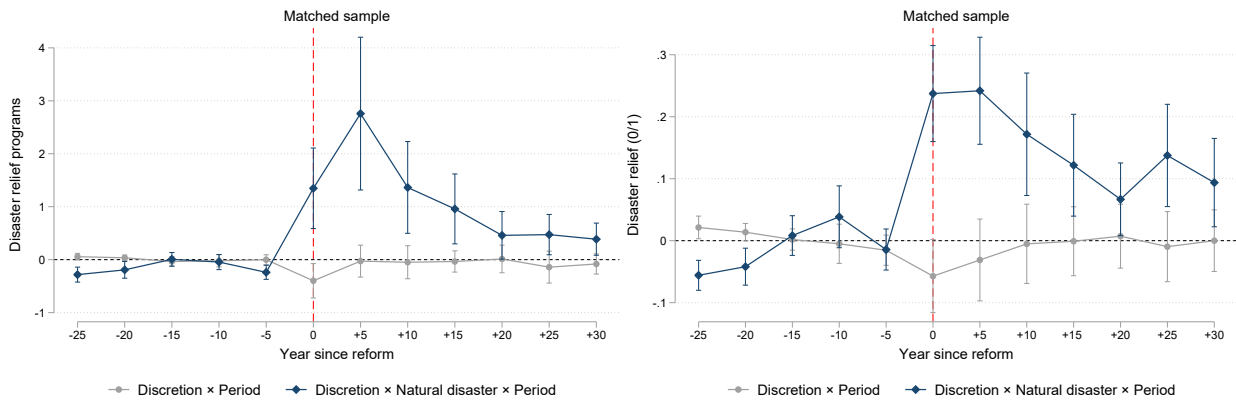


Figure A3. The Dynamic Effect of Discretionary Appointment on State Responsiveness: Additional Robustness

(a) Adding Controls × Post

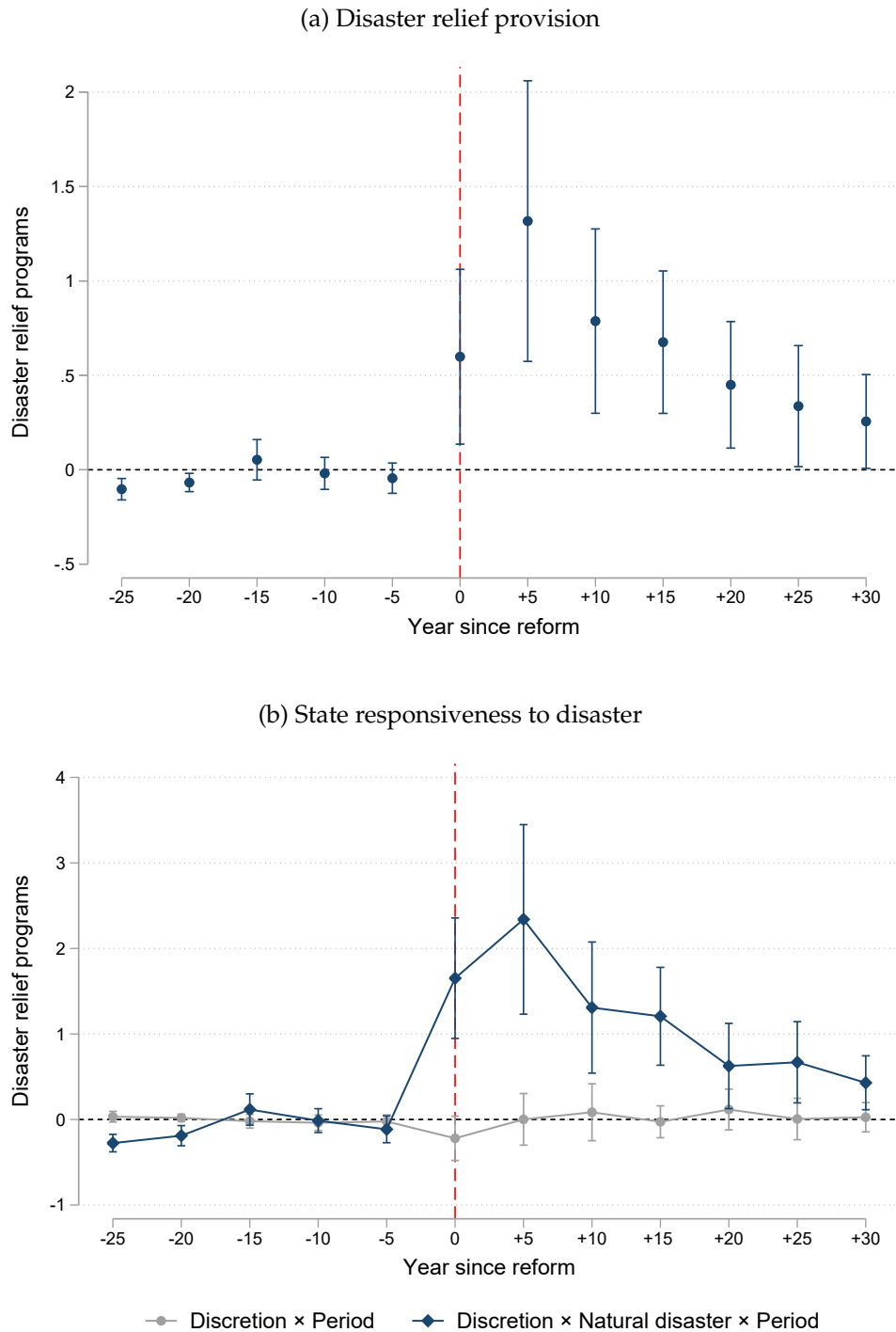


(b) Using matched sample



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 A4. Discretionary Appointment and Governance Performance: Number of relief programs



Notes: This figure shows the dynamic effects of discretionary appointment on the performance of disaster relief in 5 years bin. The dependent variable is the number of disaster relief programs. 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 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).

Table A2. 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 × Post	-0.022 (0.022)	-0.009 (0.006)	-0.009 (0.006)	0.004 (0.018)	-0.061 (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 A3. 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 × Post	-0.01 (0.013)	-0.006 (0.012)	0.007 (0.006)	-0.011 (0.009)	-0.01 (0.006)	-0.003 (0.002)	0.000 (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 A4. Comparing Prefectures with Rating 3 vs. Rating 2

	Relief programs		Relief (0/1)	
	(1)	(2)	(3)	(4)
Discretion × Post	0.366*** (0.134)	0.002 (0.079)	0.057*** (0.021)	0.002 (0.016)
Natural disaster		0.094*** (0.035)		0.028*** (0.005)
Discretion × Post × Natural disaster		0.742*** (0.146)		0.112*** (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 A5. Adding Year  $\times$  Province FE

	Relief programs		Relief (0/1)	
	(1)	(2)	(3)	(4)
Discretion $\times$ Post	0.395*** (0.097)	0.110* (0.064)	0.062*** (0.014)	0.020* (0.011)
Natural disaster		0.047* (0.026)		0.020*** (0.003)
Discretion $\times$ Post $\times$ Natural disaster		0.580*** (0.098)		0.084*** (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 A6. Drop Province Capitals

	Relief programs			Relief (0/1)		
	(1)	(2)	(3)	(4)	(5)	(6)
Discretion $\times$ Post	-0.002 (0.068)	-0.07 (0.070)	0.006 (0.077)	0.005 (0.014)	-0.006 (0.013)	0.0060 (0.016)
Natural disaster	0.108*** (0.030)	0.139*** (0.030)	0.076** (0.036)	0.029*** (0.005)	0.035*** (0.005)	0.025*** (0.006)
Discretion $\times$ Post $\times$ Natural disaster	0.702*** (0.136)	0.619*** (0.129)	0.768*** (0.166)	0.111*** (0.019)	0.097*** (0.017)	0.127*** (0.022)
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
Excluding provincial capital	Y	Y	Y	Y	Y	Y
Observations	28,161	28,065	22,143	28,161	28,065	22,143

Notes: The unit of observation is prefecture-year. This table uses the sample excluding the provincial capitals. Robust standard errors in parentheses are clustered at prefecture level. \*\*\*, \*\*, \* denote significance at 1%, 5%, 10% levels.

Table A7. Using Disaster Frequency as a Robustness Check

	Relief programs			Relief (0/1)		
	(1)	(2)	(3)	(4)	(5)	(6)
Discretion $\times$ Post	0.061 (0.069)	-0.022 (0.074)	-0.005 (0.079)	0.031** (0.015)	0.016 (0.015)	0.0200 (0.018)
Natural disaster	0.091*** (0.017)	0.099*** (0.017)	0.091*** (0.021)	0.014*** (0.002)	0.015*** (0.002)	0.013*** (0.002)
Discretion $\times$ Post $\times$ Natural disaster	0.335*** (0.058)	0.316*** (0.057)	0.343*** (0.068)	0.036*** (0.004)	0.032*** (0.004)	0.041*** (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 A8. Discretionary Appointment and Social Unrest

	Urban riots	Urban riots	Urban riots	Riots against Gov't	Riots between social groups
Mean of D.V.	0.0084	0.0084	0.0084	0.0059	0.0028
	(1)	(2)	(3)	(4)	(5)
Discretion $\times$ Post	-0.0075** (0.0030)	-0.0068** (0.0027)	-0.0055* (0.0030)	-0.0051** (0.0025)	-0.0016 (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	21,952	21,774	15,736	21,774	21,774

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 A9. 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 A10. Discretionary Appointment and Governor Quality: Additional Robustness

	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 A11. 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 × 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 × Post		Y		
PSM sample			Y	
Year × 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 A12. Balance on prefecture characteristics: selected vs. unselected governors

	Selected governor			
	(1)	(2)	(3)	(4)
Importance rating: 1	0.0118 (0.031)	0.0148 (0.021)		
Importance rating: 2	0.024 (0.029)	-0.0142 (0.021)		
log Land tax			0.0167 (0.012)	0.0093 (0.014)
log Population density			-0.0064 (0.017)	-0.0076 (0.016)
log Distance to national road			-0.0001 (0.005)	-0.0093** (0.004)
log Terrain ruggedness			-0.0195 (0.019)	0.0098 (0.023)
Number of major conflicts			-0.0035 (0.003)	-0.002 (0.003)
Incidence of weather shock			-0.2283 (0.138)	0.0606 (0.163)
Suitability: wheat			-0.002 (0.002)	-0.001 (0.002)
Suitability: rice			0.0014 (0.002)	-0.0008 (0.002)
Suitability: sweet potato			-0.0035** (0.002)	0.002 (0.002)
Suitability: maize			-0.0006 (0.002)	-0.0014 (0.001)
Latitude			0.001 (0.003)	-0.0005 (0.009)
Longitude			-0.0018 (0.006)	0.0039 (0.012)
Coast			0.0866** (0.042)	0.0959* (0.057)
log Distance to Beijing			-0.0373 (0.041)	-0.0897 (0.074)
log Grain tax			0.0002 (0.013)	0.0203 (0.015)
Number of academies			0.0049 (0.004)	0.0044 (0.003)
Strength of clan			-0.0001 (0.000)	0.0000 (0.000)
Province FE		Y		Y
Year FE		Y		Y
Mean of D.V.	0.104	0.104	0.104	0.104
Observations	6,542	6,542	6,542	6,542

Notes: The unit of observation is prefecture-year. The sample is restricted to control prefectures in the post-reform period. Selected is an indicator equal to one if the serving governor had previously been selected through the discretionary system. Robust standard errors in parentheses are clustered at the prefecture level. \*\*\*, \*\*, \* denote significance at 1%, 5%, 10% levels.

Table A13. Balance on disaster occurrence: selected vs. unselected governors

	Rainfall shock (1)	Drought (rainfall level=5) (2)	Flood (rainfall level=1) (3)	Disaster (0/1) (4)	Disaster (frequency) (5)
Selected	0.0128 (0.014)	0.0013 (0.011)	0.0116 (0.010)	0.0056 (0.020)	0.0971 (0.082)
Prefecture FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y
Mean of D.V.	0.1195	0.0498	0.0697	0.2821	0.4734
Observations	6,542	6,542	6,542	6,542	6,542

Notes: The unit of observation is prefecture-year. The sample is restricted to control prefectures in the post-reform period. Selected is an indicator equal to one if the serving governor had previously been selected through the discretionary system. Robust standard errors in parentheses are clustered at the prefecture level. \*\*\*, \*\*, \* denote significance at 1%, 5%, 10% levels.

Table A14. Discretionary Appointment and the Length of Tenure

Mean of D.V.	length of tenure		
	(1)	(2)	(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 A15. The Role of Provincial Fiscal Capacity

	Disaster relief program (1)
Discretion × Post	0.351*** (0.134)
Discretion × Post × Prov. fiscal capacity	0.038 (0.040)
Prov. fiscal capacity × Post	0.009 (0.009)
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 A16. Discretionary Appointment and Social Unrest

Appointer:	Urban riots	
	Emperor (1)	Prov. leaders (2)
Discretion (Emperor) × Post	-0.007** (0.003)	
Discretion (Prov. leader) × Post		-0.003 (0.003)
Prefecture FE	Y	Y
Year FE	Y	Y
Controls × Post		Y
Observations	18,250	13,882

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. Robust standard errors in parentheses are clustered at prefecture level. \*\*\*, \*\*, \* denote significance at 1%, 5%, 10% levels.

Table A17. Discretionary Appointment and Favoritism

<i>Panel A: Ethnic tie</i>				
Dependent variables:	Ethnic tie			
Appointer:	Prov. leaders	Prov. leaders (Han)	Prov. leaders (Bannerman)	Emperor
	(1)	(2)	(3)	(4)
Discretion (Prov. leader) × Post	0.080** (0.038)	0.177** (0.087)	-0.001 (0.032)	
Discretion (Emperor) × 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
<i>Panel B: Sanction</i>				
Dependent variables:	Sanction			
Appointer:	Prov. leaders	Prov. leaders (Han)	Prov. leaders (Bannerman)	Emperor
	(1)	(2)	(3)	(4)
Discretion (Prov. leader) × Post	-0.008* (0.004)	-0.034** (0.017)	-0.002 (0.006)	
Discretion (Emperor) × 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.

## B Additional Details on Data Source

### B.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 [B1](#) 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 and Philology, Academia Sinica, which is built on a variety of first-hand historical archives and biographic records in the Qing dynasty.<sup>37</sup> 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 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 em-

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<sup>37</sup> <https://newarchive.ihp.sinica.edu.tw/sncaccgi/sncacFtp?@@0.7913158946858703>

peror 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.

## B.2 Governance outcome and natural disaster

We collect the government relief data, our proxy for governance outcome, from [Chen, Xiao and Xiong \(2012\)](#), 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 disaster 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 \(2020\)](#)).

## B.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 <sup>38</sup> and the National Palace Museum <sup>39</sup>. 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. 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.

#### B.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 \(2009\)](#).

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<sup>38</sup> [http://www.lsdag.com/nets/lsdag/page/topic/Topic\\_1697\\_1.shtml?hv=](http://www.lsdag.com/nets/lsdag/page/topic/Topic_1697_1.shtml?hv=)

<sup>39</sup> <http://npmhost.npm.gov.tw/ttscgi/ttswebnpm?@0:0:1:npmmeta::/tts/npmmeta/dblist.htm@@0.22065529270830952>



Figure B1. 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								

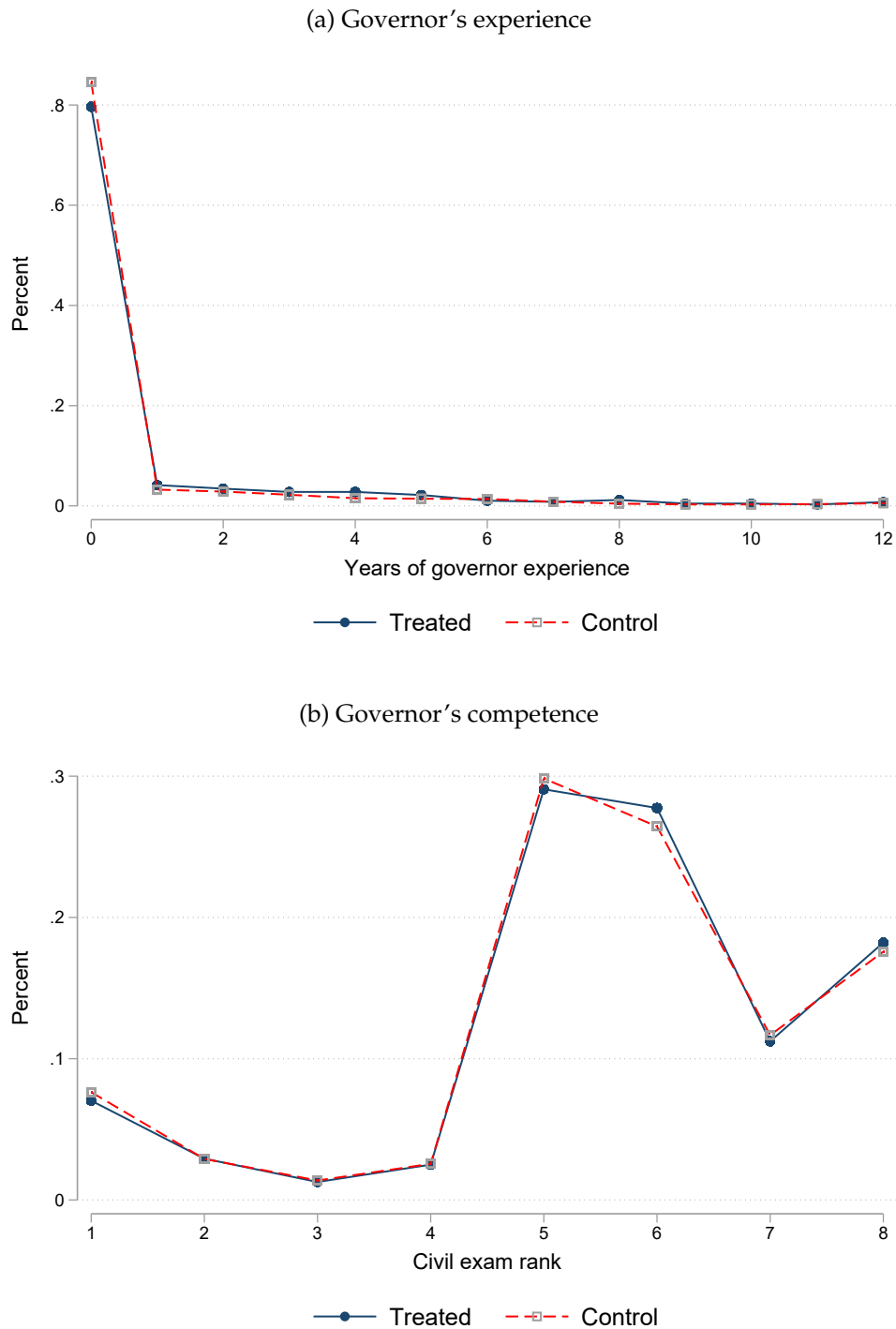
## C Discussion on the random appointment rule

Here we provide more evidence on the random nature of the status quo appointment rule. First, Figure C1 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.<sup>40</sup> 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 C1). 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

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<sup>40</sup> 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 D1 provides the details of the exam rank coding.

Figure C1. The Distribution of Governor Quality before the Reform: Treated vs. Control Prefectures



Notes: This figure 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. See Appendix Table D1 for details of the exam qualification rank coding.

Table C1. Randomness of Appointment in Pre-reform Period

Sample period	(1) 1644-1659	(2) 1660-1679	(3) 1680-1699	(4) 1700-1719	(5) 1720-1735
<b>Panel A</b>					
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
<b>Panel B</b>					
Dependent variable: merit civil exam qualification					
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
<b>Panel C</b>					
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 appointments by regressing governor characteristics on the prefecture's importance rating separately for different periods (columns 1-5) before the reform. In panel A, the dependent variable is an indicator of previous governor experience. In panel B, the dependent variable is an indicator for merit civil exam qualification. 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 the prefecture level. \*\*\*, \*\*, \* denote significance at 1%, 5%, 10% levels.

## D Discussion on the Spillover Effect

In this section, we provide additional discussion on the spillover effect of personnel reform.

The results of Table 4 estimate the causal effect of discretion on governor quality under the stable unit treatment values assumption (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 selection effect on the treated group had a negative spillover effect on the control group, because best talents had been picked and appointed to prefectures in the treatment group, decreasing the average quality of remaining officers in the candidate pool who were available for appointments in the control group prefectures.

In practice, this is unlikely to threaten our estimates for two reasons. First, under the rule-based appointment process, high-quality officers unnecessarily would be assigned to control governorships provided that they had not been selected for treated governorships, due to the idiosyncratic occurrence of new vacancies. In many cases, discretion worked by picking candidates who could be assigned to other equivalent-rank positions rather than governorship, thus not changing the potential outcome in control prefectures.

Second, at a given point in time, the number of treatment units is 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 to the "map" of position-to-position career tracks (*Pinjikaao*).<sup>41</sup> 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.<sup>42</sup> We code their civil

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<sup>41</sup> Data source: *Statutes and Precedents of the Qing (Da Qing Huidian Shili)*.

<sup>42</sup> the *Official Register (Jinshenlu)* was a roster of nearly all regular positions and their holders in Qing's

exam backgrounds into eight ranks (Table D1). Figure D1 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  $\geq 7$ ). 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 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 D1. 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 D1. The Distribution of Civil Exam Rank for Candidate Pool in 1774

