From Conquest to Rule: Elite Appointments after Rebellion in Imperial China

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Abstract

How do new states rebuild authority after violent conflict? This paper develops a theory of who governs after war. Postwar rulers must consolidate territorial control while co-opting elites. I argue that allocating political office is a form of postwar power sharing. Rulers can govern through *established elites*, who possess recognized authority but uncertain loyalty, or through *military-linked elites*, who rise with the new regime through war and combine military capacity with regime dependence. Rebellion changes this allocation by increasing the need to restore order and revealing information about local political reliability and incumbent performance. In stable areas, they can rely more on *established elites*; after rebellion, rulers have stronger incentives to rely on *military-linked elites*. Using original data from the Qing dynasty (1644–1911), focusing on its early consolidation period (1644–1684), I show that rebellion shifted prefectural office-holding toward *bannermen* and away from examination-linked elites. The paper highlights personnel management as a central tool of postwar state building.

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1 Introduction

How do regimes rebuild the state after violent conflict? Successful foreign conquest, revolution, rebellion, and decolonization leave behind more than battlefield destruction. These episodes of violent rupture transform relations between state and society (Skocpol, 1979; North et al., 2009; Goldstone, 2016), fracture political institutions (Stewart, 2018; Meng and Paine, 2022; Clarke, 2023; Clarke et al., 2026), and unsettle existing social identities and hierarchies (Carter and Gingerich, 2026). Yet military victory does not by itself produce durable rule. New regimes may seize territory by force and still struggle to establish stable authority once the fighting subsides (e.g. Hartzell and Hoddie, 2015). The persistence of weak and failed post-conflict states, from earlier imperial occupations to more recent interventions in Iraq and Afghanistan, underscores the difficulty of converting battlefield success into lasting political control (e.g. Lake, 2016).

Existing research has examined a wide range of ways in which regimes emerging from violence attempt to rebuild authority, ranging from electoral institutions and power-sharing arrangements (Wood, 2000; Hartzell and Hoddie, 2003; Slater, 2010), rule-of-law reforms (Hartzell and Hoddie, 2019), public goods provision (Liu, 2024; Paglayan, 2021), and high-level cabinet appointments (Meng and Paine, 2022), to strategies of coercion and incorporation (Arjona, 2016; Carey and González, 2021). Yet we know much less about a more basic question: who governs after violent conflict? This question matters because no ruler governs alone. Reestablishing order depends not only on institutions and broad strategies of rule, but also on personnel: which elites can help the new state govern, what kinds of skills or ties make them useful, and where different kinds of elites should be deployed.

This question is especially important because postwar state building is defined by a tension between rupture and continuity. War and regime change can create power vacuums: authority fragments, political loyalties become unsettled, and rebellion may emerge or resume (e.g. Qiu, 2022; Staniland, 2015; Bolte et al., 2021). Yet new states rarely govern on a blank slate. Even after regime change, elite networks often survive, however damaged, retaining socioeconomic standing, administrative experience, and informational advantages that are difficult to replace

(e.g. Walder and Hu, 2009; Matsumoto and Okazaki, 2023; Matsuzaki, 2019). In this sense, postwar state building is marked by "dramatic shifts, alongside enduring continuity" (Naseemullah and Staniland, 2016, p. 13).

Building on this tension, I develop a theoretical framework to explain who governs after war. Postwar rulers face two challenges: territorial consolidation and elite co-optation. To consolidate rule, they must decide which elites to bring into office. Political office allocation is an important form of postwar power sharing because it distributes access to state authority across elite groups. I focus on two groups. The first are *established elites*, whose authority rests on recognized channels of elite selection but whose loyalty to the new regime may be uncertain. The second are *military-linked elites*, who rise with the new state through war and bring stronger ties to the ruling coalition as well as military capacity.

Who governs after war should therefore vary across localities and over time. In relatively stable areas, incorporating *established elites* is cost-effective because their recognized authority helps rulers secure compliance without relying primarily on military force. Rebellion changes this calculus through two mechanisms. First, it changes governance needs: recently rebellious localities require post-rebellion consolidation, increasing the value of officials with relevant skills, such as managing logistics and coordinating with military forces. Second, it provides new information about both local political reliability and incumbent performance. At the locality level, rebellion may lead rulers to conclude that local elites or populations are less willing to cooperate with the new regime than previously believed. At the individual level, it may prompt rulers to reassess incumbent performance, particularly when an office-holder failed to detect, report, prevent, or suppress unrest. After rebellion, rulers therefore have stronger incentives to shift office-holding toward *military-linked elites* and away from *established elites*, whose local authority becomes less valuable when their reliability or performance is called into question.

I test this argument during the Qing consolidation of China proper (1644–1684), one of the largest and most consequential episodes of state building in the early modern world. After taking Beijing in 1644, the Manchu conquerors faced the challenge of establishing authority over a vast,

populous, and often hostile realm. That challenge was both territorial and demographic. The territory they sought to govern covered roughly four million square kilometers, comparable in scale to the present-day European Union. Fewer than one million Manchus sought to rule roughly 150 million people (Ho, 1959, p. 264). The new regime also confronted recurrent armed rebellion and widespread resistance to its rule (e.g. Wakeman, 1985). Yet from these fragile beginnings, the Qing consolidated power and ruled China for nearly three centuries (1644–1911). This contrast between initial vulnerability and long-run durability makes the early Qing an especially useful setting for studying how postwar rulers build durable political order.

The case also offers unusual analytic leverage because Qing institutions created clearly differentiated channels of elite recruitment. I classify office-holders as either *established elites* or *military-linked elites*. *Established elites* entered office through the imperial examination system, a long-standing channel of elite selection that had conferred recognized governing authority for nearly a millennium (Stasavage, 2020; Wang, 2022b). Under the law of avoidance, however, officials were generally barred from serving in their home province. Examination-linked elites should therefore be understood as members of a broader established political order rather than as local elites governing their native communities. *Military-linked elites* took the form of *bannermen*, who were incorporated through the military-administrative banner system. The banner system provided military training, tied their political standing and access to office directly to the new dynasty, and mobilized many of them to fight for the regime during its conquest. These characteristics made *bannermen* both useful for governing under insecurity and more dependent on Qing rule (Elliott, 2001; Oxnam, 1975).

I assemble two original datasets through extensive collection and hand coding of historical sources. The first reconstructs prefectural office-holding from Qing gazetteers. Prefectures were roughly comparable in scale to contemporary Chinese cities. Written in Classical Chinese and compiled by local elites and officials, these records provide biographical information on the prefects who governed each jurisdiction. I use this information to identify officials' recruitment backgrounds and classify them as *established elites* or *military-linked elites*. The second dataset

provides a new hand-coded measure of exposure to rebellion drawn from *Warfare in China's Dynasties*. I match historical place names to Qing prefectures and use the resulting data to construct a prefecture-year panel of rebellion incidence.

I use an event-study design to compare changes in prefectural office-holding following rebellion onset with contemporaneous changes in prefectures without a rebellion onset. The results are consistent with the theory. In the years following rebellion, office-holding shifted away from examination-linked elites and toward *bannermen*. At the appointment level, the unconditional probability of a new *bannerman* appointment increased by 10.1 percentage points in the first post-onset year and 6.2 points in the second. Among direct replacements with known recruitment backgrounds, *bannermen* also constituted a larger share of the appointees selected. The mechanism evidence points most strongly to changed governance needs and incumbent-performance updating, with more limited evidence of durable locality-risk updating. Archival records show that recently rebellious localities required officials to manage surrender, coordinate with military forces, organize logistics, restore order, and carry out other tasks of post-rebellion consolidation. They also show that unrest prompted the court to investigate incumbents whose performance may not have met its expectations. Additional tests provide little support for supply-side depletion, a general response to crisis, or ethnic loyalty alone as explanations for these patterns.

This article contributes to three important literatures in comparative politics and international relations. First, it contributes to the bellicist literature on war and state building. Existing work shows that warfare can expand fiscal capacity, centralize authority, and strengthen administrative institutions (Tilly, 1992; Brewer, 2002). These effects vary with the relative concentration of coercion and capital (Tilly, 1992), the form of military mobilization (Scheve and Stasavage, 2010), political geography (Dincecco and Wang, 2018), the method of war finance (Queralt, 2019), the distribution and mobility of wealth and the credibility of rulers and elites (Kenkel and Paine, 2023), and the bargaining power of different social groups (Koehler-Derrick and Lee, 2023). Yet this literature pays less attention to a central way in which war reshapes the state: changes in who holds office. Personnel choices determine which governing capacities rulers deploy across localities. I

address this gap by examining how rebellion changes the composition of subnational political office—who governs conflict-affected localities, which governing capacities rulers prioritize, and how personnel choices change following rebellion.

Second, the article contributes to the literature on postwar political order. Existing work emphasizes external intervention, wartime governance legacies, public goods provision, and elite bargains (Wood, 2000; Slater, 2010; Stewart, 2018; Matsuzaki, 2019; Stewart, 2021; Meng and Paine, 2022; Clarke et al., 2026). Yet it pays less attention to how rulers rebuild authority through the selection and deployment of governing elites after violence. The closest work in this tradition is by Hartzell and Hoddie (2003) and Hartzell and Hoddie (2015), who show that power-sharing arrangements can make postwar peace more durable. This work conceptualizes power sharing primarily as a regime-level bargain over political, military, and economic authority negotiated among elites. I shift the focus from national, one-time settlements to the subnational allocation of political office. In my framework, power sharing is an ongoing process through which rulers repeatedly decide which elites will govern particular localities. This perspective connects the study of postwar order to a broader comparative literature on state agents as intermediaries between central rulers and society (Boone, 2003; Soifer, 2015; Suryanarayan, 2017; Garfias and Sellars, 2021a; Wang, 2022b; Chen et al., 2025; Kuipers, 2025).

Third, the article contributes to the literature on the trade-off between loyalty and competence in personnel selection. This literature often treats rulers as choosing between loyal but less capable agents and competent but less reliable ones. My argument departs from this framework in two ways.

One departure is that competence is multidimensional. Different elites possess different governing strengths. In my setting, *established elites* derive recognized authority from the examination system and are therefore well suited to routine administration. *Military-linked elites*, by contrast, possess skills and experience suited to governing under insecurity, such as coordinating with military forces, managing logistics, and restoring order. Existing studies often emphasize one dimension of competence at a time, including administrative performance (Li and Zhou, 2005;

Zakharov, 2016), repressive capacity in internal security (Hassan, 2017; Blaydes, 2020), and military effectiveness (Talmadge, 2015; Paine, 2022). More broadly, rulers may select and deploy elites based on other dimensions of competence, including fiscal, legal, diplomatic, and mobilizational capacities. This multidimensional view better captures how states allocate personnel across heterogeneous governing tasks.

A second departure is that loyalty and competence do not necessarily pull in opposite directions. *Military-linked elites* may be both politically reliable and well suited to governing after rebellion: reliable because their political standing and careers depend on the new regime, and competent because wartime service gives them military and organizational experience. This contrasts with models in which rulers face a trade-off between military competence and political reliability (Talmadge, 2015; Paine, 2022, e.g.). The loyalty–competence trade-off is therefore not universal. It depends on how elites are recruited and on the specific governance challenges rulers face.

2 A Theory of Who Governs in Postwar States

This section develops the theory in three steps. First, postwar rulers face two linked problems: consolidating territorial control and co-opting elites. Second, local political office turns these problems into a power-sharing choice over which elites govern where. Third, rebellion changes this allocation by altering governance needs and updating rulers' beliefs about locality risk and incumbent performance.

2.1 The Governance Problems of Postwar States

My theory begins from the observation that postwar rulers face two related governance problems. The first is territorial consolidation: restoring control over areas where rebellion has challenged the state's authority. Following Weber (1978), the state claims a monopoly over the legitimate use of force, but military victory does not by itself secure territorial control. A regime may defeat

its main rivals, occupy key cities, and create formal offices while still facing renewed rebellion in some localities. The sources of rebellion vary: former armed allies may demand political influence (Hartzell and Hoddie, 2015; Arjona, 2016), militias may use local power vacuums to build territorial control (Staniland, 2015; Matsuzaki and Schwartz, 2024), and local elites may support renewed resistance if they are not incorporated into the new order (Matsuzaki, 2019). Postwar rule may therefore remain fragile even after battlefield victory. Rulers must still secure places where rebellion threatens territorial control.

The second problem is elite co-optation: bringing existing elites into the new regime. Work on state formation has long shown that managing elites is central to state building because these actors can represent alternative sources of authority (e.g. Tilly, 1978; Bates, 2014; Herbst, 2014). Violence rarely destroys elite structures completely. This persistence is visible even in major upheavals, including transformations as dramatic as the Chinese Revolution and the Meiji Restoration (Walder and Hu, 2009; Matsumoto and Okazaki, 2023). Existing elites often retain authority because they have passed through recognized channels of elite selection. Their status is tied to familiar institutions that have long defined who is entitled to govern. For ordinary subjects, these elites may therefore represent a more recognizable form of authority than a new regime whose rule has not yet become fully institutionalized. If excluded from the new regime, existing elites may resist through rebellion, noncompliance, bureaucratic obstruction, or tax evasion. Their cooperation is therefore valuable, but never guaranteed.

Together, these problems make personnel allocation central to postwar rule. Rulers must decide which elites to incorporate into office and where different types of elites are most useful. Local political office is a key site of this allocation.

2.2 Power Sharing Through Local Political Office

Political office is an important instrument of postwar power sharing. Although power-sharing arrangements often concern national political, military, and economic institutions (Hartzell and Hoddie, 2015, 2019), the exercise of state authority also depends on who governs locally. Local

officials connect the center to society, implement policy, and mediate rule on the ground (Boone, 2003; Naseemullah and Staniland, 2016; Garfias and Sellars, 2021b). Allocating local office therefore allows rulers to determine which elites to incorporate into the state and give them a stake in the new order. Postwar rulers can govern through different kinds of elites, each offering distinct political advantages. *Military-linked elites* offer military capacity and greater dependence on the new regime. *Established elites* offer recognized authority rooted in preexisting institutions of elite selection, but their preexisting standing also makes them less dependent on the new regime.

Consider *military-linked elites* first. Regimes born through violence often inherit a pool of agents whose political careers are tied to the conflict that created the new state. Some of these agents enter civilian office after victory. Their first advantage is military capacity. Because they emerge from conquest, civil war, revolution, or counterinsurgency, they bring experience in what Huntington calls the "management of violence" (Huntington, 1981). This makes them better suited to govern under conditions of insecurity. In contested settings, local populations often align with the actor best able to provide protection and enforce order (Kalyvas, 2006; Arjona, 2016). Once placed in civilian office, *military-linked elites* are therefore better positioned to coordinate armed actors, deter renewed resistance, and serve in dangerous postings where violence remains likely.

Their second advantage is greater regime dependence. *Military-linked elites* entered civilian office through the political transformation that created the new regime rather than through established civilian channels of recruitment. Their office, status, and resources therefore depended more directly on continued access to appointments controlled by the ruler. Unlike *established elites*, whose standing rested on recognized institutions of elite selection that predated the new regime, *military-linked elites* generally lacked a comparable preexisting basis of status within the civilian bureaucracy. O'Donnell's account of bureaucratic authoritarianism in Argentina illustrates this logic: officials with military backgrounds were selected as bureaucrats precisely because they lacked autonomous civilian bases (O'Donnell, 1988). If the regime fell, these elites could lose office, status, and personal wealth (Skocpol, 1979; Clarke, 2023; Clarke et al., 2026).

Shared security threats could further deepen this stake in regime survival, resembling what Slater (2010) calls a *protection pact*.

Turning to *established elites*, their advantage is recognized authority. Their standing rests on recognized institutions of elite selection that predate the new regime. These actors may include tribal leaders and customary chiefs (Bates, 2014), clerical authorities (Grzymala-Busse, 2023), degree holders produced by institutionalized systems of elite recruitment such as the imperial civil service examinations in China (Wang, 2022a; Wen et al., 2024), and hereditary aristocrats or landed elites in Europe and Latin America (Bates, 2016; Mann, 2012; Mazzuca, 2021; Garfias and Sellars, 2022). Incorporating them into office can be a cost-effective strategy of rule because their recognized authority helps rulers secure compliance without relying primarily on coercion. Rulers often govern through inherited structures for this reason: they lower the political cost of rule and connect the state to local society (Matsuzaki, 2019; Boone, 2003; Blaydes, 2020). This logic aligns with what Gerring et al. (2011, 414) describe as the "conservation of pre-existing institutions."

Yet the same source of status that makes established elites valuable also makes them harder to control. Because their standing was produced through institutions that predated the new regime, they did not owe their position solely to the political transformation that brought it to power. They can cooperate with the state, but they can also resist directives, obstruct policy, bargain for concessions, or support renewed opposition.

2.3 How Rebellion Changes Who Governs

In relatively peaceful areas, co-opting *established elites* is often a cost-effective strategy of rule because their recognized authority helps rulers secure compliance. Even when rulers may prefer, in principle, to staff politically sensitive offices with *military-linked elites*, doing so everywhere is rarely feasible or desirable. New states often lack enough trusted personnel to fill all local posts. Heavy reliance on *military-linked elites* can also disrupt routine administration, alienate local society, and make rule appear more militarized. The ruler's problem is therefore not simply

whether to use *military-linked elites*, but where, when, and for how long to use them.

This allocation problem is difficult because rulers rarely observe local risk with precision. They may recognize broad zones of political danger, but before state authority is consolidated, information about latent rebellion risk is often noisy. Over time, rulers may learn more about local society, political loyalties, and administrative performance. In the immediate postwar period, however, broad knowledge of risk is not the same as precise information about which locality will rebel, when rebellion will occur, or whether existing governing arrangements are sufficient to prevent unrest.

Rebellion changes this allocation through two mechanisms: it alters the governance needs of affected localities and provides new information about places and incumbent officials. The first mechanism is changed governance needs. Rebellion increases the need to restore order, deter renewed resistance, monitor local society, coordinate with military authorities, manage surrender, and reestablish central authority. Because *military-linked elites* are expected to be better suited to these tasks, rebellion should shift successor selection toward them and away from *established elites*. These civilian governance tasks often remain important after active fighting has subsided, so the personnel response should emerge with a short administrative lag rather than necessarily during the rebellion itself.

The second mechanism is informational updating, which operates at two levels. At the locality level, rebellion may update rulers' beliefs about locality risk. Local elites may be unwilling or unable to help maintain order, local residents may shelter rebels or withhold information, and existing arrangements of elite co-optation may no longer be sufficient to secure compliance. This localityrisk updating implies a more persistent response than changed governance needs. If rulers come to view a locality as politically unreliable, it should remain more likely to receive *military-linked elites* and less likely to receive *established elites* beyond the immediate aftermath of rebellion.

At the individual level, rebellion may update beliefs about the incumbent official already serving in the locality. If unrest occurs under an incumbent's watch, the center may infer that he failed

to detect emerging threats, prevent escalation, report danger, or adequately support the state's response once violence began. In more serious cases, rulers may suspect negligence, divided loyalty, or complicity. Because this updating concerns the performance of particular officials rather than the suitability of an entire elite category, it can lead either to removal or retention within both groups. Incumbent-performance updating therefore predicts comparable rates of exit and retention among *military-linked* and *established* incumbents, rather than systematically higher exit rates for either group.

3 Building a New State after Dynastic War

I test my argument about the allocation of political office during the Qing consolidation of China proper between 1644 and 1684.[1] The Qing dynasty ruled China from 1644 to 1911 and was the country's last imperial dynasty. I focus on its first four decades, when Manchu conquerors captured Beijing, replaced the Ming dynasty, and extended control across the former Ming territory. Following the historical literature, I treat this period as the dynasty's initial phase of state consolidation, beginning with the capture of Beijing and ending with the defeat of the Zheng regime in Taiwan in 1683 and the island's incorporation into the Qing Empire in 1684 (e.g. Wakeman, 1985; Perdue, 2005; Rowe, 2009).

I select this case for three reasons. First, Qing consolidation was substantively important. It was one of the largest episodes of violent state building in the early modern period. The territory the Qing sought to govern covered roughly four million square kilometers, making it comparable in scale to the present-day European Union and far larger than any single early modern European state. Its population was similarly vast: estimates for China on the eve of the Ming–Qing transition range from roughly 150 million to more than 200 million, compared with approximately 78 million in Europe excluding Russia and the Ottoman Empire around 1600.[2] The transition was

[1] China proper refers to the eighteen provinces historically governed by Han Chinese dynasties.

[2] For the lower estimate of the late Ming population, see Ho (1959); for higher estimates, see Cao et al. (2022). For Europe, de Vries (1984) estimates Europe excluding Russia and the Ottoman Empire at 78.0 million in 1600 and 74.6 million in 1650.

also extraordinarily destructive, with recent demographic estimates suggesting a population decline of roughly 36 million during this period (Cao et al., 2022). Its territorial scale, enormous population, and devastating violence make Qing consolidation an important setting for studying how rulers transform military victory into durable political rule.

Second, the early Qing closely fits the theoretical problem this article examines. Capturing the Ming capital did not give the new dynasty secure control over the former Ming realm. Qing authority remained fragile and uneven for decades. The new regime faced opposition from multiple fronts, including Ming loyalists, defiant generals, bandits, peasant rebels, and gentry-led unrest. Surviving Ming forces and the Zheng regime in Taiwan continued to challenge Qing authority and at times coordinated resistance against the Manchus (Perdue, 2005). For many Han Chinese, Qing rule represented foreign domination, and the court's efforts to impose visible markers of submission deepened resentment. The 1645 hair-shaving order required Han Chinese men to shave the front of the head and braid the remaining hair into the Manchu queue. Refusal could be punished by death, and enforcement of the order generated substantial resistance (Wakeman, 1985). The Revolt of the Three Feudatories posed an even graver threat. Led by powerful Han Chinese military governors who had initially helped the Qing consolidate its rule, the rebellion exposed the continuing fragility of dynastic authority decades after the capture of Beijing. Yet the Qing ultimately rebuilt authority across a vast territory and established one of the most durable states of the early modern world. This contrast between initial fragility and subsequent durability makes the case especially useful for examining how newly established regimes transform conquest into stable rule.

Third, the case provides unusual inferential leverage. Rebellion varied markedly across localities and over time, making it possible to examine whether changes in local violence were followed by changes in political office-holding. The Qing also maintained detailed personnel records and recruited officials through multiple, clearly differentiated channels. These features make it possible to trace changes in who governed affected localities.

4 Governing through Personnel Allocation

Early Qing consolidation was, at its core, a problem of personnel allocation. The new dynasty had to govern a vast and resistant territory with a small conquest elite. Drawing on historical scholarship and Qing official sources, this section shows that Qing rulers viewed examination-linked elites and *bannermen* as distinct personnel reservoirs with different political advantages.

The personnel constraint was severe. Before entering China proper, the Manchu population numbered roughly 700,000 to 800,000,[3] while the Ming realm contained roughly 150 million people (Ho, 1959, p. 264). A conquest elite of this size could not govern such a vast agrarian society on its own. The Qing court had to restore administration, collect revenue, maintain order, and secure cooperation from local society. The regent Dorgon stated the problem directly: "Statecraft has only just begun; governance requires men."[4] Yet many Ming elites hid in the mountains, withdrew from office, remained loyal to the fallen dynasty, or joined anti-Qing resistance (Wakeman, 1985).

The civil service examinations gave the Qing an established institution for drawing educated elites into the new regime. For centuries, imperial dynasties had used the examinations not only to identify administrative talent but also to structure elite competition, distribute office and prestige, and connect local elites to the central state (Chang, 1955; Ho, 1962; Wang, 2022b; Wen et al., 2024). For the Qing, the examination system therefore offered a ready-made mechanism of elite co-optation.

Soon after entering Beijing in 1644, the court reopened the examinations and largely retained Ming precedents in examination content and administrative procedure.[5] Senior Chinese advisers explicitly defended the examinations as a means of securing political cooperation. Fan Wencheng argued that winning the allegiance of scholars would also help win the wider population:

> "The key to governing the empire lies in winning the hearts of the people. Scholars

[3] Guan Donggui, "A Study of Manchu Military Numbers and Population before the Conquest," *Bulletin of the Institute of History and Philology, Academia Sinica*, 41, no. 2 (1969): 179–194.

[4] *Veritable Records of the Shunzhi Emperor*, vol. 5.

[5] See the entries for Shunzhi 2 and Shunzhi 3 in *Draft History of the Qing*, Basic Annals.

> are the most distinguished members of the population; if their allegiance is secured, the allegiance of the people will follow. I therefore request that the provincial and metropolitan examinations be restored, so that pathways into office may be broadened."[6]

The following year, as anti-Qing resistance continued, Zhejiang Governor-General Zhang Cunren made the same connection. Reopening the examinations, he argued, would give literati hope of entering government and redirect their energies away from rebellion.[7] The policy provided an important channel for drawing established elites into the new regime. In the southeast, local elites sometimes transferred administrative records, persuaded residents to surrender, and helped maintain local order.[8] The political value of the examination system thus extended beyond administrative recruitment. By bringing established Han Chinese elites into government, it helped secure cooperation with the new dynasty.

This form of co-optation did not mean allowing elites to govern their own home communities. Qing territorial officials, from county magistrates to prefects and provincial officials, were generally subject to the *law of avoidance* (*huibi*), which restricted service in an official's home province. Examination-linked officials should therefore be understood not as native elites governing their own localities, but as members of a broader established elite stratum whose authority derived from recognized institutions of education, examination, and office-holding.

Examination-based co-optation was indispensable, but it could not by itself secure the new dynasty. The court's second major personnel reservoir was the Eight Banners, the hereditary military organization through which the dynasty mobilized, trained, and governed its Manchu, Mongol, and Han banner populations (Elliott, 2001). The Eight Banners supplied the forces that fought for the rising Qing state and conquered China proper. Some *bannermen* serving during the early consolidation period belonged directly to this conquest generation; others inherited banner status from fathers or ancestors who had served during the dynasty's rise. A later edict

[6] *Draft History of the Qing*, vol. 232, "Biography of Fan Wencheng."
[7] *Collected Materials on the Civil Service Examinations from the Veritable Records of the Qing*, p. 7.
[8] *Jiangnan Gazetteer*, vol. 117.

from the Yongzheng emperor explicitly connected this history of service to civilian appointment: "Our dynasty remembers that your forebears were all old retainers who followed the dynasty in its rise and rendered meritorious service. It therefore extends favor to their descendants and appoints them according to their abilities, hoping to secure their service as trusted instruments of the throne."[9] The edict shows that access to civilian office was one consequence of the political transformation that established Qing rule. Direct or inherited service gave *bannermen* access to appointments outside the established examination system, making their bureaucratic standing more directly dependent on the ruling house. Qing rulers associated banner status with dependability. Although the court acknowledged that *bannermen* were generally less accomplished in literary learning than examination-linked elites, it described them as more honest, upright, and straightforward in temperament.[10]

Banner service also gave Qing rulers reason to expect greater military capacity. The Eight Banners had fought the conquest wars, and members of the conquest generation could bring direct campaign experience into civil office. Younger *bannermen*, even when they had not personally participated in the conquest, were raised within a hereditary military organization that registered them for service and subjected them to military training. Banner affiliation therefore indicated either direct wartime experience or sustained exposure to military discipline and organization (Elliott, 2001; Liu, 2001). Qing rulers themselves described Manchu and Mongol personnel in martial terms, emphasizing physical strength, skill in archery and horsemanship, and familiarity with military techniques.[11] Although the military effectiveness of the Eight Banners declined later in the dynasty (Elliott, 2001), this deterioration should not be projected backward onto the early consolidation period. Between 1644 and 1684, banner affiliation remained a meaningful marker of military experience and training. This background could be valuable for restoring security, coordinating with military authorities, supervising local defense, and rebuilding order after conflict.

[9] *Veritable Records of the Yongzheng Emperor*, vol. 16, Yongzheng 2, second month.
[10] *Imperial Rescripts of the Shizong Emperor to the Eight Banners*, vol. 4.
[11] *Imperial Rescripts of the Shizong Emperor to the Grand Secretariat*, vol. 63.

As the court accumulated information about local conditions, it later developed a more systematic framework for assigning officials according to the expected difficulty and risk of particular localities. The *chong, fan, pi, nan* system, proposed in 1728 and adopted in 1731, classified posts as strategically important (*chong*), administratively burdensome (*fan*), fiscally difficult (*pi*), or especially difficult to govern (*nan*). The later emergence of this system highlights the informational constraints facing new states, which must learn over time which localities are especially difficult to govern and which officials are best suited to them.

The historical evidence therefore presents early Qing governance as a problem of matching personnel to political tasks. Examination-linked officials helped incorporate established Han Chinese elites and secure cooperation with the new regime. *Bannermen* had closer ties to the ruling house and were expected to bring greater military capacity. Because both groups were valuable and banner personnel were scarce, the court had to determine where each type of official was most needed. The remaining question is whether these distinctions shaped actual personnel allocation. Did rebellion systematically shift local office-holding away from examination-linked officials and toward *bannermen*? I now turn to the statistical evidence.

5 Data and Empirical Strategy

I compile two original prefecture-year datasets: one records Qing office-holders, and the other records local exposure to rebellion. I first describe how these datasets are constructed and how officials' recruitment pathways are coded. I then explain the appointment procedure. After introducing the rebellion data and presenting aggregate patterns in rebellion and office-holding, I turn to the event-study design used to estimate how local personnel allocation changed after rebellion onset.

5.1 Data on Qing Prefects

I construct an original dataset of Qing prefects, the civilian officials who represented the imperial state at the prefectural level. Prefects occupied a central position in local governance. Roughly speaking, they were the closest historical equivalents to contemporary municipal leaders in China. They stood below provincial authorities such as governors and governors-general but above county magistrates, and they oversaw the everyday tasks through which imperial rule reached local society: taxation, adjudication, policing, public works, famine relief, education, and coordination with local elites. Because prefects sat at this middle tier, changes in prefectural office-holding provide a useful window into how the court adjusted local governance after rebellion.

The dataset covers the eighteen provinces of China proper between 1644 and 1684, the formative phase of Qing territorial consolidation. The period begins with the Qing seizure of Beijing and ends with the conquest of Taiwan, which historians commonly treat as marking the completion of the dynasty's initial consolidation (Di Cosmo, 1998; Guy, 2017). China proper comprised the core territories governed through the Qing's standardized province–prefecture–county administration (Zhao, 1988; Oxnam, 1975). Frontier regions such as Xinjiang, Tibet, and Mongolia are excluded because they entered Qing rule through different imperial processes and were governed through distinct institutions, including military governments, *banner* and princely arrangements, the Court of Frontier Affairs (*Lifan Yuan*), and local intermediaries (Guy, 2017).

The principal source for identifying prefects is the corpus of local gazetteers, or *difang zhi*. Compiled by local officials and elites, these records contain detailed information on administrative institutions, local society, and official careers. Their administrative and biographical sections are especially valuable for this project because they list officeholders and often record individual officials' places of origin, years of service, and career descriptors.[12]

To build the dataset, I assemble an extensive collection of gazetteers for all eighteen provinces of China proper, relying primarily on provincial compilations and supplementing them with pre-

[12]The Online Appendix contains more background information on the data source.

fectural gazetteers where necessary. I extract each prefect's name, prefecture, birthplace, and years of service and then standardize character forms, prefecture names, and reign-year dates across sources. I convert reign-year dates into calendar years and treat each identifiable period of service as an office tenure. I then expand these tenures into a prefecture-year panel, so that each observation records the prefect serving in a given prefecture in a given year.

Figure A.1 in the Online Appendix reproduces a page from the *Shuntian Prefectural Gazetteer*, illustrating the type of entry used to construct the dataset. In this example, the gazetteer records that the prefect had attained the *jinshi* degree, allowing his recruitment background to be classified as examination-linked.

5.2 Coding Elite Recruitment Pathways

The main measurement task is to identify each prefect's recruitment background. In many settings, elite recruitment pathways are difficult to recover because access to office is informal, politically contested, or strategically obscured. The early Qing offers a more tractable setting because its main routes into office were highly institutionalized and typically left recognizable markers in the historical record. Gazetteers often record officials' examination degrees, student statuses, *banner* labels, hereditary statuses, purchase titles, and other career descriptors. I use these markers to infer each prefect's recruitment background.

I proceed in two steps. First, I code each prefect into one of five specific recruitment pathways: examination, hereditary privilege, purchase, banner affiliation, and ad hoc appointment. Second, I collapse these pathways into broader categories used in the empirical analysis, most importantly the contrast between *exam-linked officials* and *bannermen*. In total, I identify 55 distinct titles, listed in Appendix Table A.1.

The first pathway consists of *examination graduates*, who entered office through the civil service examination system, the central institution through which the imperial state reproduced its governing elite. This system comprised a hierarchical sequence of degrees, each marking a higher level of scholarly achievement and bureaucratic eligibility (Ho, 1962; Elman, 2000; Bai and

Jia, 2016). At the base stood the licentiate (生员), followed by the provincial graduate (举人), and culminating in the presented scholar (进士). The examination channel includes twenty-one distinct titles. Holders of these degrees formed the backbone of the scholar-official elite that staffed the empire's civilian bureaucracy.

The second pathway consists of *hereditary officials*, whose access to office rested on descent from officials or examination elites. Descendants of credentialed families could receive titles that made them eligible for appointment without sitting for the regular examinations. These titles institutionalized the intergenerational reproduction of elite status within official families and tied hereditary officeholding to the broader literati-gentry order. Because this pathway reproduced status within the established literati-gentry order, I group hereditary officials with examination graduates as *exam-linked officials* in the main empirical analysis.

The third pathway consists of *purchased titles*, obtained through financial contributions to the state rather than through examination. Titles such as *jiansheng* (监生), *lijian* (例监), and *juangong* (捐贡) granted nominal eligibility for office in exchange for revenue payments, often through institutions such as the Imperial Academy or provincial treasuries. These titles reflected the state's willingness to commodify access to office as a fiscal expedient (Xu, 1968; Zhang, 2010). Because purchased titles did not indicate regular examination success or banner affiliation, I treat them as a separate residual category.

The fourth pathway consists of *banner*-affiliated officials, whose careers were rooted in the Qing's military-administrative conquest elite rather than in the ordinary civil examination track (Elliott, 2001). This category includes Manchus, Mongols, and Han *bannermen*, that is, ethnic Chinese incorporated into the Eight Banners. Banner affiliation can often be identified directly from officials' names, which differed from standard Han Chinese naming patterns, and in many cases the gazetteers explicitly identify officials as *bannermen*. Some also held examination or student-status markers, but these credentials did not operate in the same way as the ordinary examination for Han Chinese. They often reflected efforts to familiarize *bannermen* with Chinese literary and administrative culture rather than a primary route of elite selection. I therefore

code officials with both a banner marker and an examination or student-status marker as *banner-affiliated*.

The fifth pathway is a residual category of *ad hoc appointees*. These were officials whose biographies do not indicate a standard route through examinations, hereditary privilege, purchase, or banner institutions. Many appear to have entered office through military service or other irregular channels, and some seem to have been elevated from clerical or staff positions into official posts.

The resulting data contain 1,192 unique prefects with identifiable recruitment backgrounds and 6,243 prefecture-year observations that can be matched to an identified recruitment channel. Recruitment background is identified for at least one recorded prefect in 198 of the 199 prefectures in the dataset; only one prefecture, or 0.5 percent, has no identified recruitment channel.

Examination graduates account for 57.6 percent of identifiable prefects, hereditary officials for 5.5 percent, and *bannermen* for 34.4 percent. Purchased-title holders account for 1.8 percent and ad hoc appointees for 0.7 percent. In the empirical analysis, I combine examination graduates and hereditary officials into the category of *exam-linked officials*, which accounts for 63.1 percent of identifiable prefects. Exam-linked officials and *bannermen* together account for 97.5 percent of identifiable prefects, making the allocation between these two groups the central focus of the analysis (Table A.2).

5.3 Appointment Procedure in Qing China

Before introducing the rebellion data and estimation strategy, it is important to clarify how prefectural vacancies arose and how successors were appointed. Prefects were centrally appointed officials and did not serve fixed terms. In my data, the average observed prefectural tenure is about 3.7 years, with a median of 3 years. *Bannermen* have a mean observed tenure of 3.60 years and examination-linked officials a mean of 4.01 years; the median is three years for both groups (Appendix Figure A.3).

Vacancies and personnel changes generally arose through two routes. The first was routine.

A prefectural post could become vacant because of promotion, transfer, death, mourning leave, retirement, illness, or another ordinary personnel event. Information about the vacancy then entered the central personnel system through the administrative hierarchy.

The second route was disciplinary. Reports about an incumbent's conduct could trigger an official review. Accusations of negligence, incapacity, flight, collaboration, or other wrongdoing could originate from provincial superiors, censors, or the Board of War, or could be initiated by direct imperial order. These reports then traveled upward for review. Official transmission could cover roughly 150–300 kilometers per day, depending on urgency (Fairbank and Teng, 1939). Depending on geography and priority, information from localities in China proper could therefore reach the capital within several days or several weeks.

Once the information reached the center, the Board of Personnel registered the vacancy or disciplinary case. In disciplinary cases, the Board assessed the incumbent's conduct and could recommend reassignment, demotion, or dismissal, depending on the severity of the case. It also reviewed potential successors, while final appointment authority rested with the emperor. For strategically important vacancies, the emperor could consult senior officials and make appointment decisions in light of the political and military conditions of the locality.[13]

These institutional details clarify why personnel responses to rebellion should not be expected to appear immediately. Information first had to reach the center, the vacancy or disciplinary case had to be reviewed, a successor had to be selected and approved, and the newly appointed prefect had to travel to his post. They also show that rebellion could change who governed through two distinct margins: whether the serving prefect was replaced and, when turnover occurred, whether the successor was exam-linked or a *bannerman*. The gazetteers do not consistently report why an incumbent left or where he went afterward, so I cannot classify departures as disciplinary or routine. I instead test the closest observable implication of punitive removal: whether a prefect who served both immediately before and during rebellion onset was replaced in the following year.

[13] *Imperially Commissioned Collected Statutes of the Great Qing* (*Qinding Da Qing huidian*), *Siku quanshu* edition, vol. 5, Board of Personnel, Bureau of Civil Appointments.

5.4 Data on Rebellions

To measure rebellion, I draw on *Warfare in China's Dynasties* (*Zhongguo lidai zhanzheng nianbiao*, 中国历代战争年表), a comprehensive reference work that records military confrontations from the pre-imperial period before 221 BCE to the fall of the Qing in 1911. The source is organized around campaigns rather than standardized subnational units, and many campaigns span multiple locations and years. I therefore manually disaggregate each conflict by locality and align the recorded place names with the corresponding Qing prefectures. This procedure yields a prefecture-year panel of rebellion exposure. As with the prefect data, the analysis is restricted to China proper.

I define *rebellion* as armed resistance to Qing rule within prefectures that had entered the Qing administrative system. The conflicts range from large campaigns challenging Qing control to smaller armed peasant uprisings. A prefecture-year is coded as exposed to rebellion when Qing forces engaged rebels within that jurisdiction in that year. The indicator remains equal to one for each year that fighting continued in the prefecture and returns to zero once the source no longer records rebel activity or military engagement there. Multi-year and multi-location campaigns therefore contribute multiple prefecture-year observations rather than being counted only once. I define *rebellion onset* as the first year of a new prefecture-level rebellion episode: a year of recorded rebellion preceded by a year without recorded rebellion in the same prefecture.

The data contain 364 prefecture-year observations of rebellion exposure across China proper between 1644 and 1684, including both geographically limited uprisings and local manifestations of larger campaigns. Rebellion was especially concentrated during this four-decade consolidation period. The exposure rate was 2.45 per 100 observed prefecture-years, compared with 0.15 during the High Qing (1685–1795) and 1.27 during the Late Qing (1796–1911); the corresponding onset rates were 2.17, 0.15, and 1.06 (Appendix Table A.3). Both measures show that rebellion posed a particularly serious challenge during the initial consolidation of Qing rule. Figure 1 further shows that rebellion was geographically dispersed across China proper rather than confined to a small number of localities.

Rebellion episodes within individual prefectures were typically short. Figure 2 shows that 212 of the 238 prefecture-level episodes observed between 1644 and 1684 lasted only one year. The mean episode duration was 1.13 years, and the longest observed episode lasted three years. Rebellion exposure therefore usually captures short-lived local outbreaks rather than prolonged prefecture-level conflict. This pattern also motivates the focus on rebellion onset as the event around which subsequent personnel changes are traced.

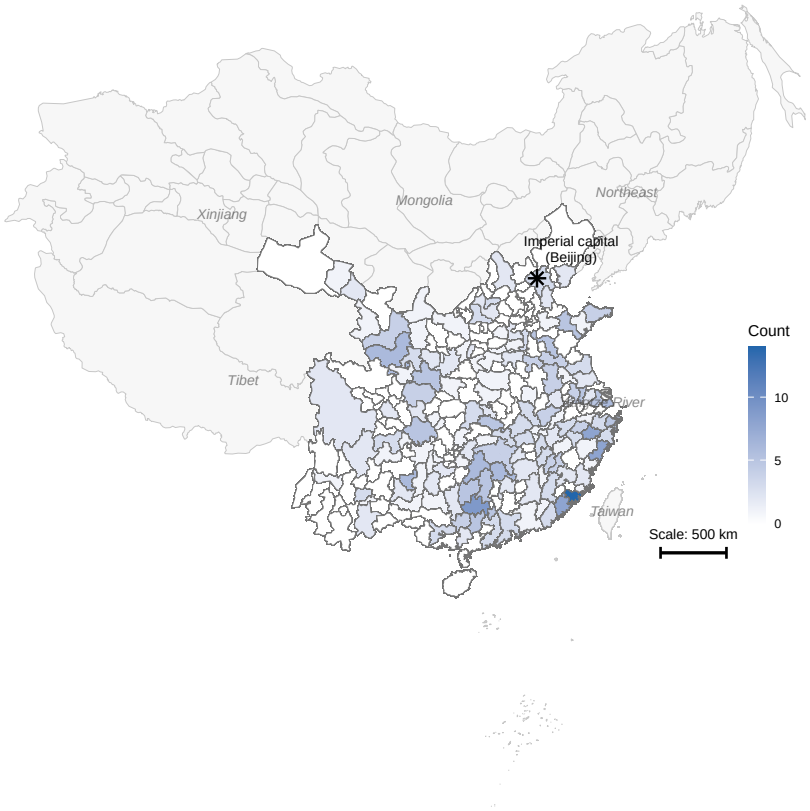
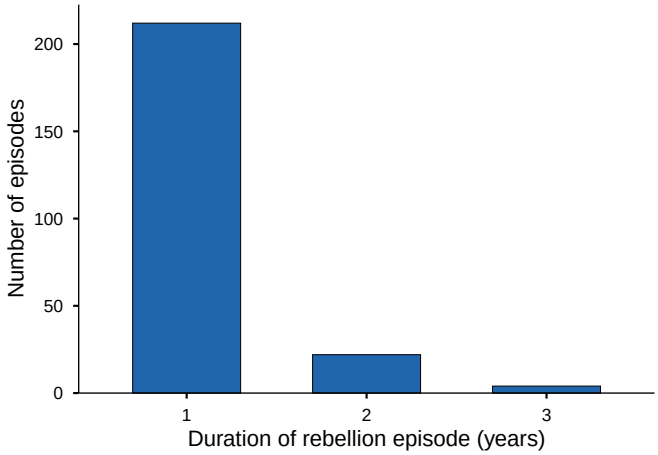


Figure 1: Rebellion Exposure in China Proper within the Qing Empire, 1644–1684

Note: The figure maps recorded rebellion exposure during the Qing consolidation period. The full Qing empire is shown in pale grey for geographic context. Conflict intensity is shaded only for China proper, operationalized as the eighteen Qing provinces. Darker shading indicates prefectures with more years of recorded rebellion exposure between 1644 and 1684. The measure captures local prefecture-year exposure to rebellion.

Figure 2: Distribution of Rebellion Episode Durations, 1644–1684



Note: Each bar counts the number of distinct rebellion episodes of a given duration, measured in years, over the 1644–1684 period. An episode is defined as a consecutive run of years in which a prefecture experiences at least one rebellion event. Of 238 total episodes, 212, or 89.1 percent, last a single year. The maximum observed duration is three years.

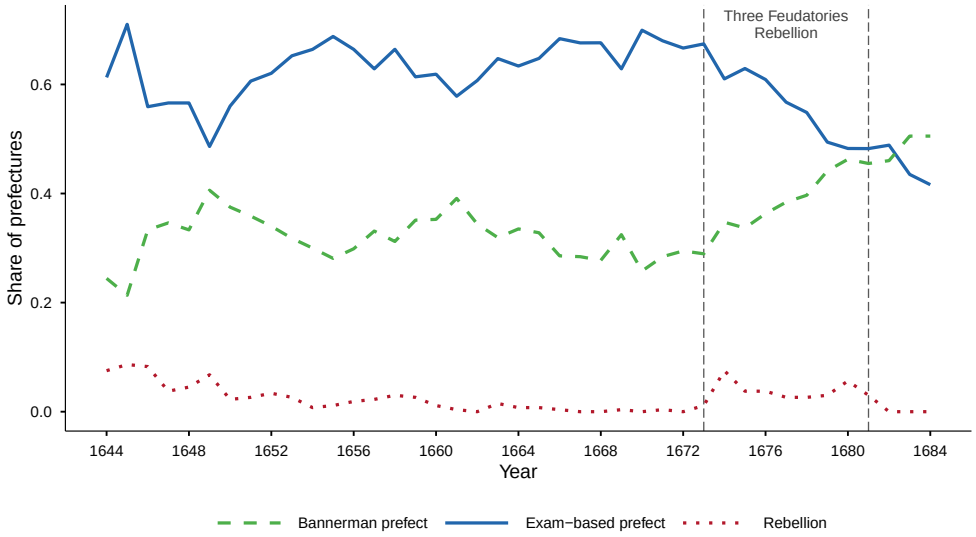
5.5 Descriptive Patterns

Before turning to the event-study analysis, I describe the aggregate relationship between rebellion and office-holding during the Qing consolidation period. Figure 3 plots annual trends in the share of prefectures headed by exam-linked officials, the share headed by *bannermen*, and the share experiencing rebellion between 1644 and 1684. The initial prominence of exam-linked officials likely reflects the Qing's early reliance on examination elites, many of whom had previously served under the Ming (Wakeman, 1985). As rebellion declined through the 1650s and 1660s, the share of prefectures headed by exam-linked officials rose steadily, while *bannerman* office-holding fluctuated. The Three Feudatories Rebellion (1673–1681) disrupted this pattern: rebellion exposure increased, exam-linked office-holding declined, and *bannerman* office-holding rose, exceeding 0.50 by the early 1680s.

These aggregate patterns are consistent with the argument: declining rebellion coincided with rising exam-linked office-holding, while renewed large-scale rebellion coincided with greater reliance on *bannermen*. Empire-wide trends, however, cannot establish whether rebellion changed

who governed the particular prefectures in which it occurred. The event-study analysis therefore exploits prefecture-level variation in the timing of rebellion onset to examine whether office-holding changed within affected prefectures.

Figure 3: Trends in Exam-Linked Prefects, Bannerman Prefects, and Rebellion Exposure, 1644–1684



Note: Each series plots the annual share of prefectures. "Exam-linked prefect" refers to the share of prefectures headed by an official recruited through the "examination or hereditary pathways". "Bannerman prefect" refers to the share headed by a *bannerman*, including Manchu, Mongol, and Han banner officials. "Rebellion" refers to the share of prefectures with recorded rebellion exposure in a given year. The vertical dashed lines mark the Three Feudatories Rebellion, 1673–1681.

5.6 Empirical Strategy

Prefectures that ever experienced recorded rebellion differed from prefectures that never did. They had larger civil-service quotas, were more likely to be located on major rivers, had higher rice suitability, and were more likely to contain provincial capitals (Appendix Table A.4). These differences are unsurprising: rebellion was more likely to occur in populous, politically important, and economically connected places. The empirical design therefore does not rely on a simple cross-sectional comparison between rebellious and non-rebellious prefectures.

I instead use an event-study design that compares changes within prefectures before and after

rebellion onset. By absorbing stable differences across prefectures, the design accounts for time-invariant differences in geography, administrative importance, and underlying rebellion risk. It also shows the timing of personnel change. The resulting estimates capture changes in office-holding and appointment decisions following realized rebellion rather than the court's broader allocation rule over latent rebellion risk.

The treatment event is rebellion onset, defined as a transition from peace to recorded rebellion within a prefecture. Because rebellion could recur within the same prefecture, I treat each new peace-to-rebellion transition as a separate onset. If rebellion recurs, post-rebellion event time is measured from the nearest preceding onset, while pre-rebellion event time is measured from the nearest subsequent onset. A year between two nearby episodes may therefore be coded both as following the earlier onset and as preceding the later one.[14]

Given this, the baseline specification is:

$$Y_{pt} = \alpha_p + \lambda_t + \sum_{\substack{k=-5 \\ k\neq -1}}^{5} \beta_k D_{pt}^k + \varepsilon_{pt}. \tag{1}$$

The unit of analysis is the prefecture-year. The indicator D_{pt}^k equals one when prefecture p in year t is k years from a rebellion onset. The omitted event-time category is $k = -1$, the final full year before rebellion begins. Coefficients for $k < 0$ test whether later-rebellious prefectures were already on different trajectories before onset, while coefficients for $k \geq 0$ trace whether personnel outcomes change after rebellion breaks out.

The comparison category includes never-rebellious prefectures, not-yet-rebellious prefectures, and years outside the ± 5 event window, including post-window years after earlier rebellion episodes. Because some prefectures experience repeated rebellion episodes, the coefficients should be interpreted as average changes around observed rebellion-onset episodes rather than as effects of a single permanent treatment.

The outcome Y_{pt} denotes a personnel outcome for prefecture p in year t. I estimate the spec-

[14] Among prefectures that experience rebellion, 47.4 percent experience more than one episode, and 18.0 percent of event-window observations fall within overlapping onset windows.

ification using two sets of outcomes. The first set consists of office-holding outcomes. These outcomes measure the recruitment background of the prefect serving in a given prefecture-year. In separate models, I examine whether the serving prefect is a *bannerman* or an exam-linked official. These outcomes show whether the aggregate composition of serving prefects changed after rebellion.

The second set consists of appointment outcomes. These provide a more direct test of whether rebellion changed the court's personnel decisions. First, *Turnover* equals one when prefects are recorded in two adjacent years and the identity of the serving prefect changes; it equals zero when the same prefect remains. Years without sufficient incumbent records are excluded. Second, the unconditional *new bannerman* and *new exam-linked* outcomes equal one when turnover brings in a successor from the specified recruitment group and zero in observed adjacent years without such a replacement. Replacement years without a recorded successor background are left missing. Third, the conditional *incoming bannerman* and *incoming exam-linked* outcomes restrict the sample to turnover years with known successor recruitment. The unconditional new-*bannerman* outcome is the primary appointment estimand because it does not condition on turnover. As a stricter coverage check, I also require known recruitment backgrounds in both adjacent years.

Prefecture fixed effects absorb time-invariant differences across places, including geography, long-run economic conditions, administrative importance, and persistent differences in rebellion risk or personnel composition. Year fixed effects absorb shocks common to all prefectures in a given year, including empire-wide court politics, fiscal pressures, military campaigns, and general changes in Qing personnel policy. Standard errors are clustered at the prefecture level.

6 Empirical Results

This section presents the main empirical results in two parts. I first examine office-holding outcomes to determine whether rebellion changed the composition of officials governing affected prefectures. I then turn to appointment outcomes to assess whether this shift emerged through

incumbent turnover, successor selection, or both.

6.1 Office-Holding Results

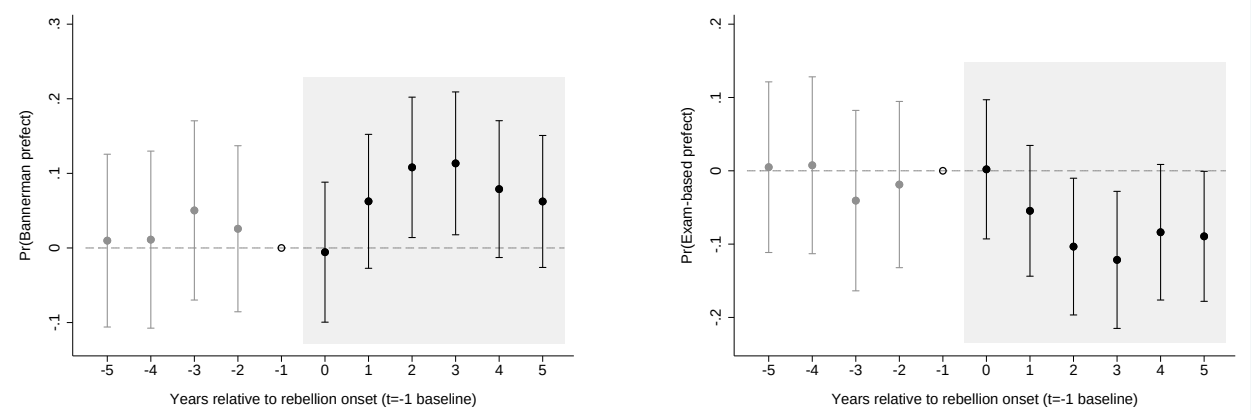
Table A.5 and Figure 4 show a clear shift in who governed prefectures after rebellion onset. Three findings emerge. First, there is little evidence of systematic change before onset: the pre-onset coefficients for both *bannerman* and exam-linked office-holding are small and statistically indistinguishable from zero. Second, the shift does not occur immediately in the onset year. Instead, the main adjustment appears after a lag, consistent with the time required for information to reach the center, personnel decisions to be made, and appointment flows to alter the composition of serving prefects. In the second and third years after rebellion onset, the probability that a prefecture is governed by a *bannerman* rises by 10.8 and 11.3 percentage points, while the probability that it is governed by an exam-linked official falls by 10.3 and 12.2 percentage points.

Third, these changes are substantively large. Relative to the 38 percent share of prefecture-years governed by *bannermen*, the year-two increase represents roughly a 28 percent rise. The nearly parallel increase in *bannerman* office-holding and decline in exam-linked office-holding is consistent with a reallocation between the two main personnel groups. The shift remains evident through the five-year event window, although it attenuates after reaching its peak in years two and three.

These results document a change in the composition of civilian office-holding, not military occupation replacing civil administration. Prefects remained regular civilian officials within the Qing bureaucracy even when they were *bannermen*. The office-holding outcomes, however, do not reveal whether the shift arose through greater incumbent turnover, changes in successor selection, or both. I therefore turn next to appointment-level outcomes.

6.2 Appointment-Level Results

I next examine the personnel flows that produced the shift in office-holding. The office-holding results show who governed each prefecture in each year, but not the appointment flows through



A. Bannerman prefect **B. Exam-linked prefect**

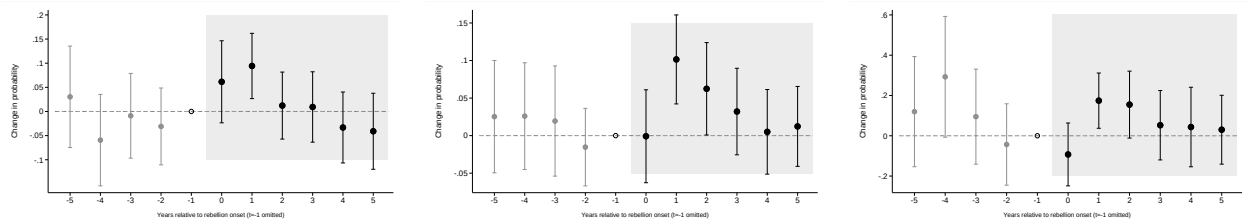
Figure 4: Prefectural Office-Holding after Rebellion Onset

Note: Points report event-study coefficients for years relative to rebellion onset; vertical lines show 95% confidence intervals. Event time (t=-1) is the omitted baseline and is shown at zero. Panel A reports whether the serving prefect was a *bannerman*. Panel B reports whether the serving prefect was exam-linked. All models include prefecture and year fixed effects, with standard errors clustered by prefecture.

which that composition changed. I examine turnover between adjacent recorded incumbents, the unconditional probability of a new *bannerman* appointment on that observable risk set, and the recruitment background of the successor when turnover occurred.

Figure 5 visualizes the results. Panel A shows that turnover increased by 9.4 percentage points in the first year after rebellion onset. Panel B shows that the unconditional probability of a new *bannerman* appointment increased by 10.1 percentage points in the first year and 6.2 points in the second. Panel C restricts the analysis to turnover years and shows that the *bannerman* share increased by 17.4 percentage points in the first year.

Together, these estimates show that rebellion affected both turnover and successor selection. Affected prefectures were more likely to replace a recorded incumbent after onset and more likely to receive a new *bannerman*. Among turnover years, *bannermen* also made up a larger share of selected successors.



A. New prefect appointed B. New *bannerman*, unconditional C. *Bannerman* share, conditional

Figure 5: Rebellion and Appointment-Level Outcomes

Note: Points report event-study coefficients for years relative to rebellion onset; vertical lines show 95% confidence intervals. Event time $t = -1$ is the omitted baseline and is shown at zero. Panel A reports turnover, which equals one when prefect identities differ between adjacent years with recorded incumbents. Panel B reports the unconditional probability of a new *bannerman* appointment on the same adjacent-record risk set; observed years without turnover are coded zero, while replacement years with unknown successor recruitment are missing. Panel C restricts the sample to turnover years and reports whether the incoming prefect is a *bannerman*. All models include prefecture and year fixed effects, with standard errors clustered by prefecture.

6.3 Robustness Checks

The robustness checks examine several concerns: sparse support in the conditional estimates, anticipatory allocation, outcome measurement, event-time and conflict definitions, conflict scale, major campaigns and province-level shocks, territorial incorporation, estimator choice, and record missingness.

I begin with sparse support in the conditional estimates. The conditional coefficient at $t = -4$ is based on a small number of appointments and is similar in magnitude to the post-onset estimates. I identify the appointments that produce the spike, examine whether they come from a single campaign, and remove the complete event windows associated with the contributing rebellion episodes. The spike reflects sparse support across several episodes rather than one major campaign, while the post-onset pattern remains after the implicated windows are removed (Figure A.6). Within-province permutation inference provides stronger support for the unconditional new-*bannerman* outcome than for the conditional share (Table A.9). These checks show that the $t = -4$ spike does not drive the post-onset pattern, while reinforcing the unconditional outcome as the primary result.

A second concern is anticipatory allocation. The court may have assigned *bannermen* to risky prefectures before rebellion occurred, in which case the post-rebellion pattern could partly reflect

earlier personnel decisions rather than adjustment following rebellion. I therefore test whether prefectures that would experience rebellion within the next three or five years were already more likely to receive *bannerman* appointees or less likely to receive exam-linked appointees (Table A.10). The estimates provide little evidence of systematic anticipatory allocation, indicating that the post-onset shift was not a continuation of earlier staffing decisions.

A third concern is outcome measurement. The main appointment analysis focuses on the two elite categories most relevant to the theory: *bannermen* and exam-linked officials. Table A.11 expands the analysis to all recorded recruitment pathways: examination, hereditary privilege, *banner* affiliation, purchase, and other channels. The main appointment pattern remains when all recorded recruitment pathways are included.

Fourth, I examine whether the results depend on the timing convention used to define the event. The baseline specification anchors event time to rebellion onset. As an alternative, I re-anchor event time to the first peaceful year after a rebellion episode ends (Table A.13). The post-rebellion appointment pattern remains under this alternative timing convention.

Fifth, I examine whether the results depend on the definition of rebellion or on an unusually influential prefecture. Because the conflict data do not consistently report casualties, troop numbers, or battle intensity, I use distinctions that can be observed consistently in the source. I first restrict the treatment definition to local unrest, excluding larger military campaigns between the Qing and Ming loyalists (Table A.12). I then exclude the most conflict-exposed prefecture, defined as the prefecture with the largest number of recorded conflict events over the panel (Table A.14). The appointment pattern persists under both restrictions, showing that it is not driven by broad campaign coding or one unusually conflict-exposed prefecture.

Sixth, I examine whether the appointment response varied with observable dimensions of conflict scale. The theory treats rebellion as a threshold signal and does not predict that personnel adjustment should necessarily increase with conflict scale. I nevertheless test whether the response was larger when the first rebellion episode lasted more than one year or extended across multiple prefectures. Table A.15 examines differences by rebellion duration, while Table A.16

examines differences by geographic scope. The results provide little evidence that longer or geographically broader rebellions generated larger shifts in incoming appointment composition.

Seventh, I examine whether the results reflect major campaign dynamics or province-level shocks. The largest rebellion of the period was the Revolt of the Three Feudatories, so I exclude the years 1673–1681 and re-estimate the appointment models (Table A.17). I also allow appointment trajectories to vary across provinces by adding province-specific linear trends (Table A.18) and compare prefectures within the same province and year using province-year fixed effects (Table A.19). The province-year specification absorbs province-wide military mobilization and common personnel shifts. Leave-one-province-out estimates further show that the patterns are not driven by any single province (Table A.20; Figures A.7 and A.8). The appointment pattern persists across these specifications, indicating that it is not attributable to the Three Feudatories campaign, province-wide shocks, or any single province.

Eighth, I consider whether the results reflect initial territorial incorporation rather than personnel adjustment within already administered prefectures. Qing control over China proper remained incomplete in 1644, and rebellion could coincide with the first extension of Qing administration into a locality. The analysis, however, is concentrated in the early-administered core: prefectures with a first recorded Qing prefect by 1649 account for 77 percent of prefecture-year observations and 82 percent of the new appointments used in the appointment-composition analysis (Table A.21). I further add fixed effects for years since the first recorded prefect, distinguishing newly administered, recently administered, and longer-administered prefectures (Table A.22). I estimate the appointment models separately for prefectures incorporated by the end of the 1640s and those incorporated later (Tables A.23 and A.24). The results indicate that initial territorial incorporation does not account for the appointment pattern.

Ninth, I examine estimator choice. The baseline event-study specification uses two-way fixed effects and allows repeated rebellion onsets. To address concerns about staggered timing and heterogeneous treatment effects, I re-estimate the appointment outcomes using the Sun–Abraham interaction-weighted estimator (Sun and Abraham, 2021), defining cohorts by each prefecture's

first rebellion onset and using never-treated prefectures as controls. The same post-onset appointment pattern appears under this estimator, although the estimates are less precise than those from the baseline specification (Table A.25). The results therefore do not depend on the two-way fixed-effects estimator.

Finally, I examine record missingness. Table A.26 reports event studies for whether no prefect is recorded and whether recruitment-channel information is missing. Neither form of missingness increases after rebellion, so rebellion-induced gaps in the records do not drive the appointment results. Moreover, the main appointment outcomes exclude years without recorded incumbents in both adjacent years, so missing prefect records are never coded as no appointment. As a stricter check, I require known recruitment backgrounds for both adjacent incumbents; the appointment pattern persists under this restriction (Table A.27). The results therefore cannot be attributed to changes in record coverage or the coding of missing observations.

6.4 Extension: Persistent Military Risk and Personnel Allocation

The main analysis examines how personnel allocation changed after rebellion. I extend the analysis by asking whether the court also relied more heavily on *bannermen* in strategically exposed areas, even when those areas had not recently experienced rebellion.

I classify a province as a border province if it bordered a frontier region outside the standardized China-proper administrative system during the study period. This definition captures proximity to zones of military exposure and frontier administration, rather than coastlines or borders between provinces within China proper.

Table A.28 shows that border provinces had a higher share of incoming *bannerman* appointments and a lower share of incoming exam-linked appointments. This descriptive pattern extends the main findings beyond responses to realized rebellion: persistent military exposure was also associated with greater reliance on military-linked officials.

7 Exploring Mechanisms

The preceding analyses establish that rebellion changed who governed affected prefectures. The next question is why. I explore two mechanisms drawn from the theory: changed governance needs and informational updating, with the latter operating through place and individual updating. I combine quantitative tests of their observable implications with archival evidence on how Qing officials described disturbed localities, post-rebellion tasks, and the evaluation of incumbents serving in affected prefectures.

7.1 Changed Governance Needs

Rebellion created continuing needs for pacification even after active fighting had ended. A recently rebellious prefecture required not only routine civil administration but also surveillance, military coordination, surrender management, logistical support, and the restoration of political order. Because Qing rulers expected *bannermen* to be better suited to these post-rebellion tasks, rebellion should shift successor selection toward *bannermen* when prefectural posts opened. The appointment-level results are consistent with this mechanism: prefectures became more likely to receive *bannermen* and less likely to receive examination-linked officials.

The archival record clarifies why rebellion could alter local governance needs. Recovering territory militarily did not by itself restore local rule. Even after territory had been retaken, the court still had to manage surrendered populations, restore order, coordinate with military forces, and provision troops. In newly recovered coastal areas, for example, the court reported that popular sentiment remained unsettled and ordered troops to remain stationed in the affected prefectures.[15] This example shows that recovering territory did not eliminate local insecurity: continued military presence and political stabilization remained necessary after formal control had been restored.

Prefects participated directly in meeting these continuing governance needs. The records

[15] *Imperially Commissioned Strategy for the Pacification of the Three Feudatories* (*Pingding Sanni Fanglue*), vol. 52.

describe prefectural officials managing large surrendered populations,[16] assisting efforts to suppress local rebels,[17] helping recover rebel-held territory,[18] carrying imperial instructions intended to induce maritime rebels to surrender,[19] and transporting grain by camel and horse to support military operations.[20] Together, these records show that prefects in unstable localities were expected to do more than administer ordinary civil affairs. They performed the administrative, logistical, and political work through which military recovery was converted into restored rule.

7.2 Informational Updating

7.2.1 Locality-Risk Updating

The first form of informational updating is locality-risk updating: rebellion may alter what the court believes about a locality's political reliability. A rebellious prefecture may signal that local elites are unwilling or unable to help maintain order, that local populations may shelter rebels or withhold information, and that existing arrangements of elite co-optation are no longer sufficient to secure compliance.

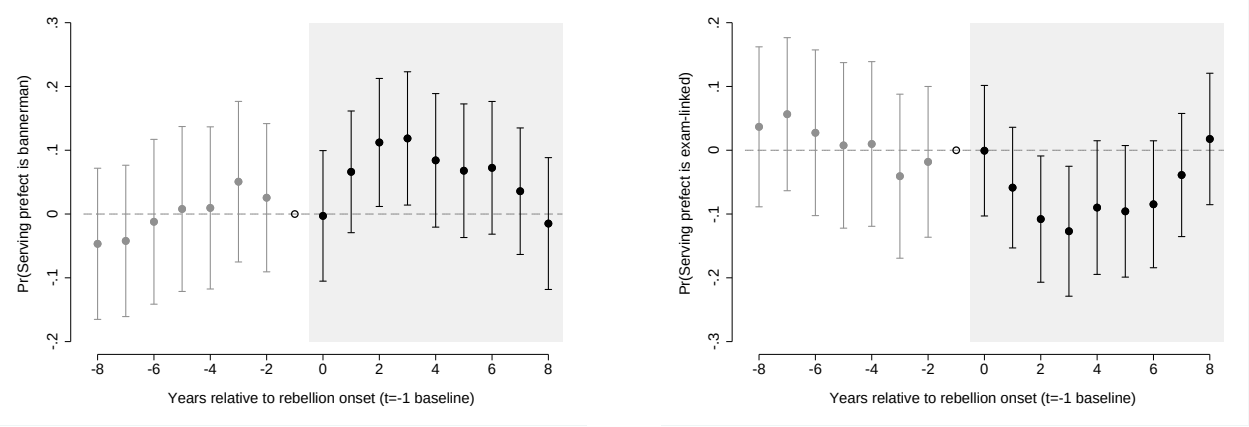
Locality risk updating has a more durable observable implication than changed governance needs. If the appointment shift is driven mainly by immediate post-rebellion consolidation needs, greater reliance on *bannermen* should fade as those needs recede. If rebellion instead leads the court to view the locality as politically unreliable, the personnel response should persist beyond the immediate aftermath of unrest, even if it eventually attenuates. Affected prefectures should therefore remain more likely to be governed by *bannermen* and less likely to be governed by examination-linked officials for some time after rebellion.

I examine this implication using an eight-year post-onset window. This provides a meaningful medium-term horizon: the average prefectural tenure was 3.7 years, so the window spans

[16] *Draft History of the Qing* (*Qingshi Gao*), Basic Annals, vol. 6, "Shengzu I."
[17] *Draft History of the Qing* (*Qingshi Gao*), Basic Annals, vol. 6, "Shengzu I."
[18] *Draft History of the Qing* (*Qingshi Gao*), Basic Annals, vol. 6, "Shengzu I."
[19] *Imperially Commissioned Strategy for the Pacification of the Three Feudatories* (*Pingding Sanni Fanglue*), vol. 35.
[20] *Imperially Commissioned Account of the Kangxi Emperor's Personal Campaign and Pacification of the Northern Desert* (*Shengrenzu Huangdi Qinzheng Shiding Shuomo Fanglue*), vol. 25.

more than two average tenures and allows the personnel response to be observed across multiple appointment cycles.

Figure 6 provides limited support for durable locality-risk updating. The probability that the serving prefect is a *bannerman* rises after rebellion onset, while the probability that he is examination-linked declines. These shifts attenuate in later years, however, and both series move back toward their pre-rebellion levels. The results therefore provide little evidence that rebellion led the court to treat affected prefectures as politically unreliable over a sustained period.



A. Serving prefect is a *bannerman*

B. Serving prefect is examination-linked

Figure 6: Rebellion and Locality-Risk Updating

Note: Points report event-study coefficients for years relative to rebellion onset; vertical lines show 95% confidence intervals. Event time (t=-1) is the omitted baseline and is shown at zero. Panel A reports whether the serving prefect is a *bannerman*. Panel B reports whether the serving prefect is examination-linked. All models include prefecture and year fixed effects, with standard errors clustered by prefecture.

7.2.2 Incumbent Performance Updating

The second form of informational updating is individual performance updating. Unlike localityrisk updating, which concerns the political reliability of the locality, this mechanism concerns the performance of the incumbent official. When rebellion occurred under a prefect's watch, the center could reassess whether he had detected emerging threats, reported danger, prevented escalation, supported the state's response, and remained loyal once violence began. In more serious cases, rulers might suspect negligence, divided loyalty, or complicity. Because this updating concerns the conduct of particular officials rather than the suitability of an entire elite category, it

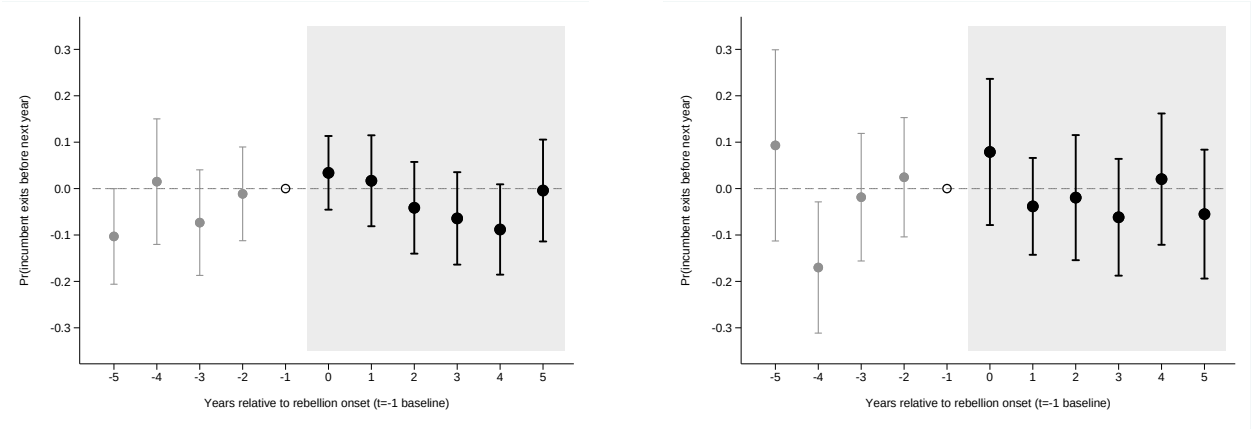
can lead to either removal or retention within both recruitment groups. Incumbent-performance updating therefore predicts broadly comparable post-rebellion changes in incumbent exit among examination-linked officials and *bannermen*, rather than systematically higher removal of one group as a whole.

Figure 7 examines whether the post-rebellion increase in turnover reflected the categorical removal of one recruitment group. Panel A reports exit among examination-linked incumbents, while Panel B reports exit among incumbent *bannermen*. Both groups became more likely to leave office after rebellion, and their six post-onset coefficients do not differ jointly ($p = 0.560$). Although the pre-onset coefficients differ across the two groups ($p = 0.027$), the post-onset evidence does not indicate that the court targeted either group categorically. The pattern is instead more consistent with case-by-case updating and punishment: the court reassessed incumbents individually and removed officials from both recruitment groups whose performance did not meet its expectations (Appendix Table A.7).

The archival record shows what such reassessment involved. During the Revolt of the Three Feudatories, Wu Sangui's forces attacked Changde Prefecture and captured the city.[21] The Board of War subsequently reported that Prefect Weng Yingzhao had closed the city, failed to supply military grain, and joined the rebels. Acting on the Board's recommendation, the emperor approved a further investigation into his conduct.[22] A later Qing case illustrates the same evaluative process. Following the Linqing uprising, prefects and other officials were charged with failing to detect early signs of conspiracy and were dismissed on the ministry's recommendation.[23] Together, the two cases show that rebellion prompted the court to reassess incumbents based on whether they had detected unrest, supported military operations, defended their jurisdictions, and remained loyal to the state.

Taken together, the mechanism evidence provides the strongest support for changed governance needs and incumbent-performance updating. Rebellion created continuing demands for

[21] *Imperially Commissioned Strategy for the Pacification of the Three Feudatories* (*Pingding Sanni Fanglue*), vol. 3.
[22] *Imperial Diaries of Activity and Repose* (*Qijuzhu*), Kangxi 19.
[23] *Imperially Commissioned Account of the Suppression of the Linqing Rebels* (*Qinding Jiaobu Linqing Nifei Jilüe*), vol. 16.



A. Examination-linked incumbent exit

B. *Bannerman* incumbent exit

Figure 7: Rebellion and Individual-Performance Updating

Note: Both panels report the same outcome: whether the incumbent serving in year t exits before year $t+1$. Points report event-study coefficients relative to rebellion onset; vertical lines show 95% confidence intervals. Event time $t=-1$ is omitted and shown at zero. Both models include prefecture and year fixed effects, with standard errors clustered by prefecture, and use the same y-axis scale.

post-rebellion consolidation and prompted the court to reassess the conduct of particular incumbents. The broadly similar post-onset exit responses across recruitment groups are consistent with individual performance updating rather than the categorical removal of one type of official, while the suggestive increase in immediate replacement indicates that rebellion accelerated some personnel reassessment. Evidence for durable localityrisk updating is more limited.

8 Alternative Explanations

The preceding section examined the mechanisms linking rebellion to changes in prefectural officeholding. This section considers three alternative explanations: that the results reflect a general response to crisis rather than rebellion specifically; that conflict depleted the local supply of examination-qualified elites; and that the court favored *bannermen* because of ethnic favoritism rather than characteristics associated with banner status more broadly.

8.1 General Crisis Response

A first alternative explanation is that the post-rebellion pattern reflects a general response to crisis rather than a response to rebellion specifically. Rebellion may weaken administration, create vacancies, or prompt official replacement, but other major shocks could produce similar disruptions.

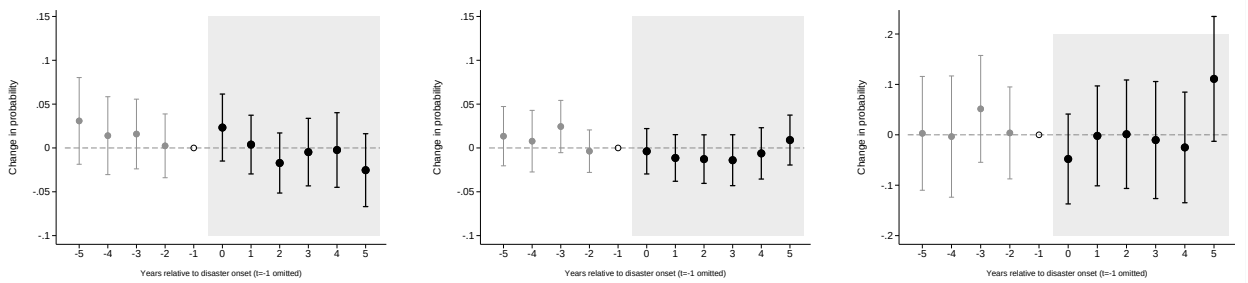
I test this possibility using major natural disasters as placebo events. Disasters damage infrastructure, reduce revenue, require relief, and strain local administration. They may therefore disrupt personnel needs in ways similar to rebellion. Unlike rebellion, however, they do not signal armed resistance to Qing rule or create a direct need to restore political control. If the appointment changes reflect a generic response to administrative disruption, similar changes should follow major disasters.

The disaster measure comes from Hao and Liu (2020). The Hao–Liu exceptional-disaster indicator equals one for an exceptional flood or exceptional drought and zero otherwise. By focusing on the most severe disasters, the test captures shocks likely to produce observable administrative disruption. If the results reflect a generic crisis response, exceptional-disaster onset should similarly change whether prefects are replaced and who succeeds them.

Table A.29 and Figure 8 show no comparable appointment response after disaster onset. Exceptional disasters do not systematically increase turnover, an unconditional new *bannerman* appointment, or the conditional *bannerman* share. This null pattern suggests that the post-rebellion results do not reflect administrative disruption alone.

8.2 Supply-Side Depletion

A second alternative explanation is supply-side depletion. Armed conflict may kill, displace, or otherwise disrupt established elites, reducing the pool of examination-qualified men in affected areas. On this account, the rise in *bannermen* after rebellion is not a deliberate shift in central personnel priorities but a mechanical response to local human-capital loss: as examination-qualified elites become scarcer, the court fills appointments with other types of officials.



A. Turnover B. New *bannerman*, unconditional C. *Bannerman* share, conditional

Figure 8: Appointment Outcomes around Exceptional-Disaster Onsets

Note: Points report event-study coefficients for years relative to onset of an exceptional flood or exceptional drought; vertical lines show 95% confidence intervals. Event time $t = -1$ is omitted and shown at zero. The three outcomes and samples match Figure 5. All models include prefecture and year fixed effects, with standard errors clustered by prefecture.

The Qing appointment system makes this explanation less plausible. Under the *law of avoidance* (*huibi*) (Xu, 1968; Zhao, 1988), prefects were prohibited from serving in their home province. Prefectural offices were therefore not ordinarily staffed by local men. Conflict-induced losses within an affected locality should not directly reduce the pool of officials eligible to govern it because those officials were generally recruited from elsewhere.

I further test this explanation by restricting attention to officials drawn from outside the affected province. I define an outsider as an incoming prefect whose native province differs from the province of appointment and distinguish between *bannerman* and exam-linked outsiders. If the decline in exam-linked appointments were a mechanical consequence of conflict-induced depletion within the affected province, it should disappear among nonlocal appointees, whose availability was not directly reduced by conflict there.

Figure 9 shows that the appointment shift persists within this outsider pool. Among new appointments, rebellion increases the probability of receiving a *bannerman* outsider and reduces the probability of receiving an exam-linked outsider. The post-rebellion shift is therefore difficult to explain as a mechanical consequence of local human-capital loss.

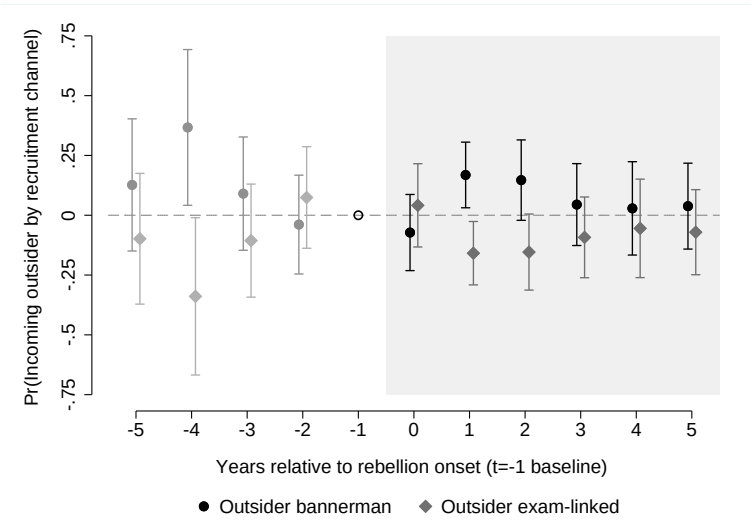


Figure 9: Outsider Appointments after Rebellion Onset

Note: Event-study estimates for incoming outsider appointments by recruitment channel. An outsider is an incoming prefect whose native province differs from the province of appointment. The figure plots coefficients for *bannerman* outsiders and exam-linked outsiders relative to the final full year before rebellion onset. Outcomes are conditional on new appointments with identified recruitment backgrounds and native provinces. Vertical lines show 95% confidence intervals. All models include prefecture and year fixed effects, with standard errors clustered by prefecture.

8.3 Ethnic Favoritism

A third alternative explanation is ethnic favoritism. Because the Qing was a Manchu conquest regime, the post-rebellion shift could reflect preferential appointment of the dynasty's Manchu and Mongol allies rather than reliance on banner personnel more broadly. The Eight Banner system, however, included Manchu, Mongol, and Han Chinese *bannermen*. This internal variation provides a useful test.

The key distinction is between Manchu/Mongol *bannermen* and Han *bannermen*. Manchu and Mongol *bannermen* were closely tied to the ruling coalition: Manchu–Mongol alliances predated the conquest of China proper, and intermarriage between the Manchu ruling house and Mongol elites was common (Elliott, 2001). Han *bannermen*, by contrast, were ethnically Chinese, although their families had been incorporated into the banner system before or during the conquest (Elliott, 2001). This distinction yields a clear implication. If ethnic favoritism alone explains the post-rebellion shift, the increase should be concentrated among Manchu and Mongol *bannermen*. If

the court instead relied on *bannermen* as a broader personnel category, Han *bannermen* should increase as well.

I test this implication by separating Manchu and Mongol *bannermen* from Han *bannermen*. Figure 10 provides little support for an explanation based on ethnic favoritism alone. Han *bannerman* office-holding increases alongside Manchu and Mongol *bannerman* office-holding after rebellion onset. The shift was therefore not confined to the dynasty's Manchu and Mongol allies but extended across the broader banner personnel pool.

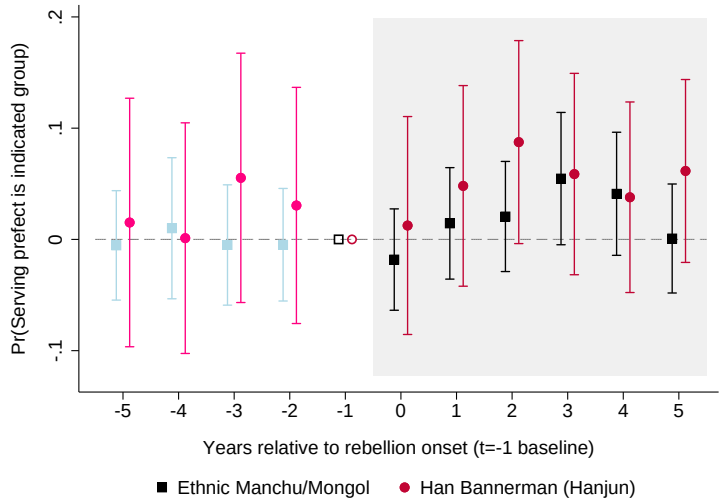


Figure 10: Ethnic Decomposition: Manchu/Mongol vs. Han Bannerman Office-Holding around Rebellion Onset

Note: Event-study estimates decomposing *bannerman* office-holding into Manchu/Mongol and Han *bannerman* categories. Points report coefficients for years relative to rebellion onset; vertical lines show 95% confidence intervals. Event time (t=-1) is the omitted baseline and is shown at zero. All models include prefecture and year fixed effects, with standard errors clustered by prefecture.

9 Scope Conditions and Generalizability

I have argued that postwar rulers use political office to share power with different elite groups. *Established elites* provide recognized authority, while *military-linked elites* are expected to bring military capacity and greater dependence on the new regime. Although the empirical evidence comes from the Qing consolidation of China proper, the argument should generalize under three

conditions.

First, the theory applies when violent rupture disrupts political authority without destroying prior elite recruitment channels altogether. New rulers must inherit at least partially usable elite networks. They then face a problem of reallocation: which existing elites to retain and which new elites to incorporate into rule. The argument is less applicable where rulers seek to destroy prior elite structures or where violence has left few usable elite networks. Khmer Rouge Cambodia illustrates this boundary condition. After 1975, the regime emptied cities, dismantled schools and markets, abolished much of the existing administrative apparatus, and targeted former officials, professionals, intellectuals, and educated urban residents. Governance was organized around revolutionary replacement rather than the co-optation of existing elites.

Second, rulers must have access to differentiated but limited personnel pools with distinct political advantages. The distinction between *established elites* and *military-linked elites* must be substantively meaningful, and rulers must have enough discretion to allocate them across offices, places, and tasks. Scarcity creates the need for selective allocation: if military-linked personnel could be deployed everywhere at little cost, rulers would face less need to decide where they were most valuable.

In the early Qing, this distinction is unusually legible because the examination system and the military-administrative *banner* system marked different routes into office. Elsewhere, *established elites* may be identified through chieftaincy (Baldwin, 2016), aristocratic or landholding status (Bates, 2016), hereditary office, religious authority (Grzymala-Busse, 2023), or other forms of recognized social authority. *Military-linked elites* may instead be identified through service in state armies (Barkey, 2008), participation in rebel organizations (Qiu, 2022), incorporation into security institutions (Hassan, 2017), or membership in military groups (Paine, 2022). This condition also clarifies why the argument applies most directly to early Qing consolidation. As the military effectiveness of the Eight Banners declined later in the dynasty (Elliott, 2001), *bannerman* status became a weaker marker of the military capacity relevant to the argument.

Third, the theory is most applicable when new states continue to face serious security threats.

In the early stages of consolidation, rulers may confront renewed rebellion, uncertain loyalties, and incomplete territorial control. These conditions increase the value of military-linked personnel. As security threats recede, office allocation should increasingly reflect other concerns.

Cases within and beyond China illustrate similar personnel-allocation problems, even when the relevant elite categories differ from those of the Qing. In 1949, as the Chinese Communist Party moved from military victory to urban takeover and administration, even this revolutionary party relied on old-regime personnel in cities. Liu Shaoqi's instructions on urban work emphasized that former personnel should not be removed all at once. He distinguished between former staff in enterprises, who should be kept in their posts, and old government organs, which should be reorganized while retaining personnel with special technical skills.[24] A contemporary American consular report on the Communist takeover of Peiping made a similar observation: "personnel [of the] old regime generally remained at posts," while military committee representatives "took charge and maintained liaison" with the control organ.[25] Inherited personnel maintained urban administration, while revolutionary and military-linked agents supervised the transition.

The Mughal Empire provides another premodern example. In the sixteenth century, Akbar faced the problem of governing a large and socially diverse territory. As Richards (1993) discusses, Akbar drew on Persian-derived administrative traditions and Turko-Mongol traditions of conquest to build a centralized bureaucracy staffed by technically qualified officials and imperial servants, or *mansabdars*. The technically qualified officials helped routinize rule through revenue administration, written orders, records, and standardized procedures. The *mansabdars*, by contrast, were military-linked elites whose rank and access to office were tied to imperial service. They were often posted to strategically important locations, where they helped secure and extend imperial authority.

[24] Liu Shaoqi, "Some Questions Concerning Urban Work," March 12, 1949.

[25] The Consul General at Peiping (Clubb) to the Secretary of State, Peiping, May 11, 1949, in *Foreign Relations of the United States, 1949, The Far East: China*, vol. 8, doc. 359.

10 Conclusion

How rulers rebuild political authority after violent conflict is a central question in the study of state building. I have argued that one important part of the answer is who governs after war. Military victory may determine who holds power, but it does not by itself secure stable rule. Postwar rulers must reestablish territorial control, restore administration, and govern through elites with different sources of authority and political value. The allocation of office therefore becomes an important instrument of postwar reconstruction and power sharing.

Using original data on Qing state consolidation, I show that rebellion changed who governed affected prefectures. After rebellion onset, prefectural office-holding shifted away from exam-linked officials and toward *bannermen*. Appointment-level analyses show how this change occurred: rebellion increased the unconditional probability of a new *bannerman* appointment, and *bannermen* also constituted a larger share of selected successors among direct replacements with known recruitment backgrounds. Alternative explanations based on general administrative crisis, local depletion of examination-qualified elites, or ethnic favoritism alone receive little support.

The broader implication is that postwar state building occurs not only through coercion, institutional design, or national-level elite bargains. It also operates through the continuing allocation of governing personnel across localities. Regimes emerging from conquest, civil war, rebellion, revolution, or decolonization rarely govern from a blank slate. They must decide how to use inherited elites whose authority connects the state to existing society and regime-linked elites whose backgrounds make them more valuable under conditions of insecurity. Political office turns this problem into a practical choice over which elites govern where.

Future research can examine whether similar patterns of personnel allocation appear in other post-conflict settings and whether these early choices shape later state capacity and political stability. Who governs after war is not merely a consequence of state building; it is one of the means through which states are built.

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Online Appendix

From Conquest to Rule: Elite Appointment after Rebellion in Imperial China

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A Data on Prefects in Qing China

A.1 Historical Background about Gazetteers

The prefect dataset is constructed primarily from Qing local gazetteers (*difangzhi*, 地方志). Gazetteers were reference works devoted to a particular administrative jurisdiction, usually a county, prefecture, or province. They assembled information considered important for documenting the history and governance of a locality. A typical gazetteer included sections on geography, administrative boundaries, population, taxation, schools, temples, local customs, degree-holders, notable persons and families, military affairs, public works, and lists or biographies of officials. Because they combine administrative information with locally produced historical and biographical material, gazetteers are an important source for studying local government and society in imperial China (Dennis, 2015).

Gazetteers were generally produced under official sponsorship, but their compilation usually depended on cooperation between serving officials and local elites. Officials authorized, financed, or supervised the project, while local scholars and degree-holders collected earlier texts, family records, biographies, inscriptions, and knowledge of local institutions.

Gazetteers were not issued annually. They were compiled and revised at irregular intervals, often after an earlier edition had become outdated. A new edition could record concrete changes that had accumulated since the previous compilation, such as the creation, abolition, or reassignment of counties; changes in administrative boundaries; the relocation of government offices; revised population or tax figures; and the construction or repair of schools, city walls, granaries, bridges, and water-control works. New editions also updated lists of officeholders, examination graduates, notable families, and important local events. Compilation could therefore serve both practical administrative purposes and the desire of officials and local elites to produce an authoritative account of the locality (Dennis, 2015; Miles, 2017).

Gazetteers circulated through official, educational, and scholarly networks rather than through a general popular readership. Copies were retained in local government offices and schools, consulted by serving and newly appointed officials, and used by local scholars and later gazetteer compilers. Some copies also entered higher-level government offices, academies, private libraries, and collectors' collections beyond the jurisdiction they described (Dennis, 2015). This circulation was especially important in the Qing bureaucracy, where officials were normally appointed away from their native places. A gazetteer provided an incoming official with a written account of the jurisdiction he was expected to govern, including its administrative organization, fiscal conditions, institutions, prominent families, and earlier officeholders.

The structured and geographically specific information contained in gazetteers has also made the broader gazetteer tradition useful for quantitative historical research.

A.2 An Example from a Gazetteer

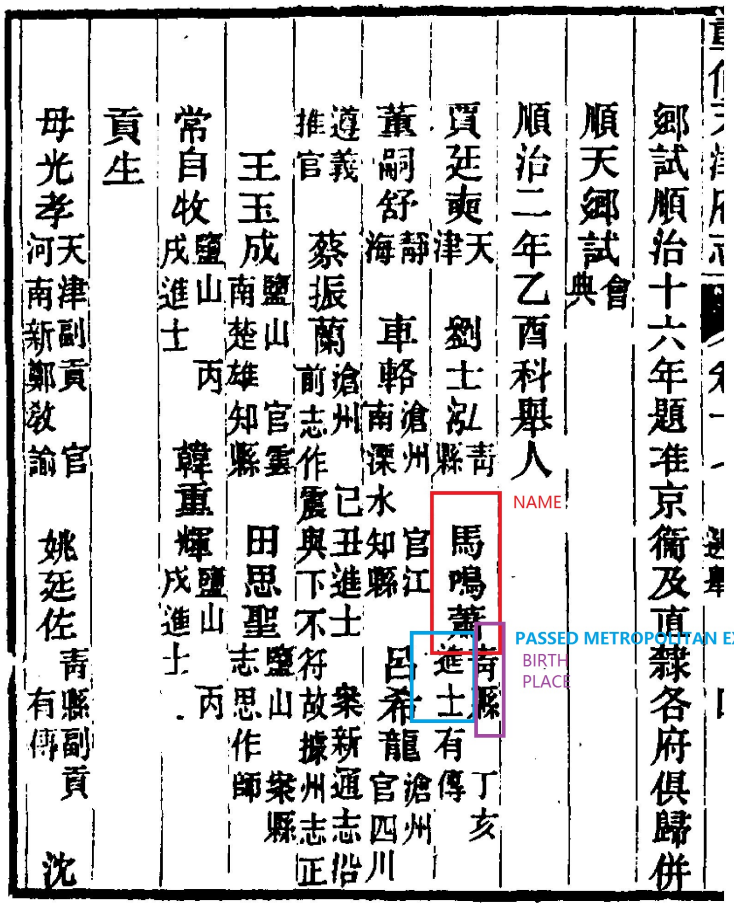


Figure A.1: Example of a Local Gazetteer Entry

Note: Page from the *Shuntian Prefectural Gazetteer* (*Shuntian fu zhi*), revised during the Guangxu Emperor's reign. The entry records the career of Prefect Ma Mingxiao, a native of Qing County and holder of the *jinshi* degree. Local gazetteers such as this provide detailed biographical information on officials and are a key source for the prefect dataset.

A.3 Standardizing Gazetteer Records

Transforming gazetteer entries into a prefecture-year panel requires several steps of standardization. The original sources are written in classical Chinese, use imperial reign-year dates, and often vary in how they refer to officials, places, and recruitment backgrounds. This appendix describes how I standardize these records before using them in the statistical analysis.

First, I standardize place names and link each entry to a Qing prefecture. Gazetteers may refer to localities by historical names, variant spellings, subordinate counties, or administrative units whose boundaries changed over time. I therefore map recorded places to the corresponding Qing prefectural unit used in the panel. This issue is especially frequent for officials' native places.

Second, I convert reign-year dates into calendar years. Gazetteers generally date prefect names by emperor reign period and reign year rather than by the Gregorian calendar. I convert these entries into calendar years using the standard Qing reign chronology. For example, an entry stating that a prefect served in a prefecture in the tenth year of the Kangxi reign is coded as 1671. When an entry provides both start and end years, I code the official as serving in each calendar year covered by the recorded tenure. When only a start year is available, I use other gazetteer entries, prefectural gazetteers, successor information, or official compilations where available to infer the end of the tenure. Cases with insufficient information to identify the serving prefect in a given year are excluded from the known-recruitment prefecture-year sample.

Third, I code each prefect's recruitment channel from the textual record. Gazetteers often identify an official's background through terms such as *jinshi* 進士, *juren* 舉人, *gongsheng* 貢生, *jiansheng* 監生, purchased rank, hereditary status, or banner affiliation. I first record these titles and then use the descriptions to classify each prefect into the recruitment categories used in the analysis.

Finally, I expand the standardized individual-level records into a prefecture-year panel. Each observation identifies the prefect serving in a given prefecture and calendar year, along with the official's standardized name, native place, recruitment channel, and other available background characteristics.

A.4 Recruitment Background Coding

Table A.1: Academic Titles by Coded Recruitment Channel

Recruitment channel	Entries	Titles and career markers
Examination	21	Secretariat drafter (中书舍人), provincial graduate (举人), metropolitan examination alternate (会副), alternate-list degree holder (副榜), alternate tribute student (副贡), merit tribute student (功贡), annual tribute student (岁贡), county school student (庠生), stipend student (廪生), stipend tribute student (廪贡生), grace tribute student (恩贡), specially selected tribute student (拔贡), licentiate (生员), clerk-translator (笔帖式), Hanlin academician (翰林), metropolitan graduate (贡士), tribute student (贡生), tribute student of the Imperial Academy (贡监), tribute student-supervisor (贡监生), presented scholar (进士), provincially selected tribute student (选贡).
Inheritance	5	Official student (官生), official student-supervisor (官监), grace student-supervisor (恩监), hereditary privilege (恩荫), hereditary student (荫生).
Banner	38	Provincial graduate (举人), Guandong affiliation (关东), deputy director in the Inner Court (内院副理), Inner Court consultative appointment (内院咨部), bureau director in the Ministry of Punishments (刑部郎中), alternate-list degree holder (副榜), alternate tribute student (副贡), imperial liaison officer (啓心郎), student of the Imperial Academy (国学士), Imperial Academy student (国学生), student of the Directorate of Education (太学生), Fengtian affiliation (奉天), student (学生), official school student (官学生), official student (官生), annual tribute student (岁贡), stipend student (廪生), grace student (恩生), grace student-supervisor (恩监), hereditary privilege (恩荫), grace tribute student (恩贡), grace-selected student (恩选), specially selected tribute student (拔贡), instructor (教习), *bannerman* (旗人), Han *bannerman* (汉军), licentiate (生员), student-supervisor (监生), clerk-translator (笔帖式), hereditary student (荫生), assistant instructor in a local Confucian school (训导), metropolitan graduate (贡士), tribute student (贡生), tribute student of the Imperial Academy (贡监), tribute student-supervisor (贡监生), rewarded official title (赏官), presented scholar (进士), supplementary tribute student (附贡).
Other	7	Office attendant (供事), military merit (军功), clerk (吏员), appointed official (授官), military provincial graduate (武举), military student (武生), military presented scholar (武进士).
Purchase	3	Purchased student-supervisor (例监), purchased tribute student (捐贡), student-supervisor (监生).

Note: The table reports the academic titles, status labels, and career markers used to code prefects' recruitment channels. The entries column counts marker entries within each coded recruitment channel. Because nineteen markers appear under more than one channel, the seventy-four channel-specific entries correspond to fifty-five unique Chinese titles or career markers. When an official had both a banner marker and an examination or student-status marker, the official was coded as banner-affiliated.

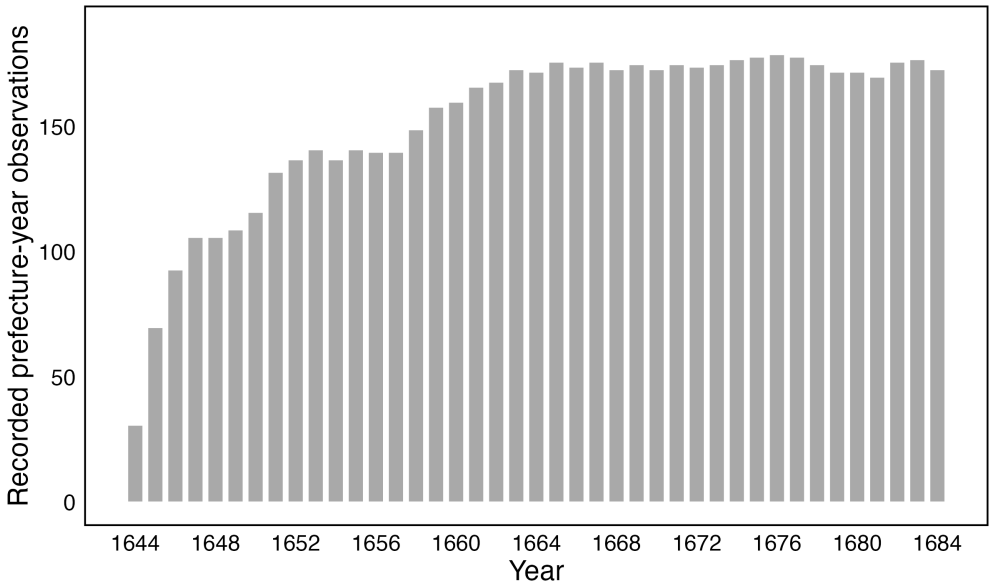
Table A.2: Prefects by Recruitment Channel

	N	%
Examination	687	57.6
Banner	410	34.4
Manchu/Mongol	96	8.1
Han bannerman (Hanjun)	314	26.3
Inheritance	65	5.5
Purchase	22	1.8
Other	8	0.7
Total	1,192	100.0

Note: Unit of observation is the individual prefect. Each prefect is counted once regardless of tenure length. Known-recruitment sample: prefects with an identifiable recruitment channel ($N = 1{,}192$ of 1,355 total). Banner subgroups sum to the Banner row.

A.5 Panel Coverage

Figure A.2: Number of Available Prefecture-Year Observations by Year



Note: The figure reports the number of prefecture-year observations available in the prefectural officeholding panel for each year between 1644 and 1684. Each observation corresponds to one prefecture-year with a recorded prefect.

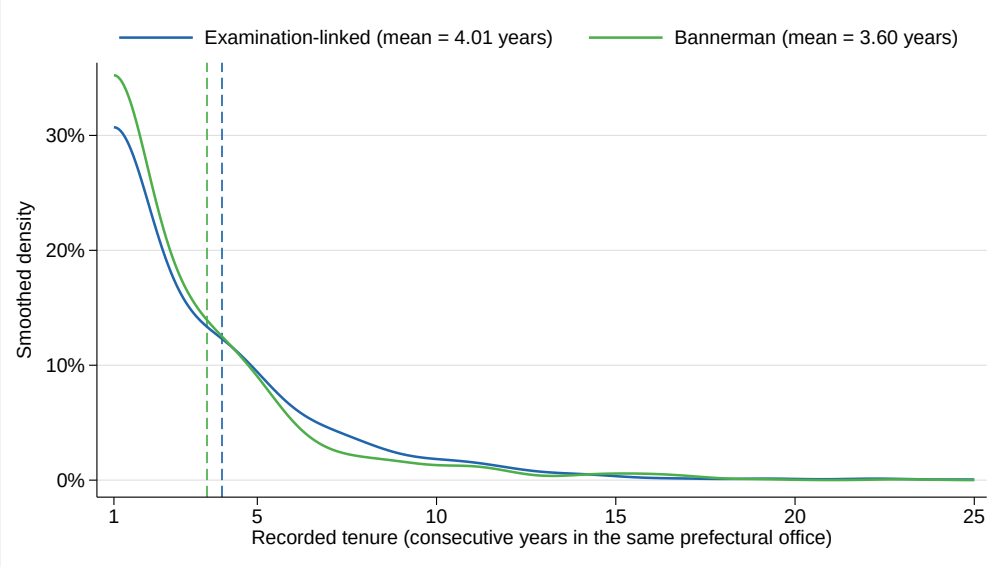


Figure A.3: Tenure by Recruitment Channel

Note: The unit of observation is an observed office tenure: consecutive years in which the same identified official appears in the same prefectural office. The colored curves show smoothed tenure distributions for examination-linked and Bannerman officials. Dashed lines indicate the group means.

B Data on Rebellion

B.1 Conflict Coding

Figure A.4 shows an example of the original text about the Penghu Battle fought in 1683 between the Qing navy and Tungning Kingdom based in Taiwan. The Qing admiral Shi Lang led a fleet to attack the Tungning forces in Penghu. The Tungning admiral Liu Guoxuan was outmaneuvered by Shi Lang. The loss of Penghu resulted in the surrender of Zheng Keshuang, the last king of Tungning, to the Qing Dynasty. The Battle of Penghu also marked the end of significant external enemies of the Manchu-established Qing Empire.

Figure A.4: Example of Coding Conflict

1683 年（清康熙二十二年）

清军攻占澎湖、台湾之战 福建总督姚启圣、内阁学士李光地均奏请清帝玄烨利用郑经死子幼内乱的时机攻取台湾，并推荐施琅任攻台事宜。玄烨授施琅为福建水师提督，加太子少保，令相机进取。施琅认为澎湖为台湾门户，郑军精锐全在澎湖，澎湖不破，台湾断难攻取，如攻占澎湖，台湾不攻自破。琅至军，训练水师，派间谍潜入澎、台联络旧部以为内应。康熙二十二年六月，琅率战舰三百艘，水师二万人，自铜山（东山）出发，连克花屿、猫屿，乘南风进泊八罩。刘国轩以二万人据守澎湖，沿岸建胸墙，筑炮垒，增设火铳火炮，绵亘二十余里；巡弋海面，防守甚严。琅遣游击兰理率七船进攻，冲入郑军船队中，击沉、焚毁郑船数只。时值南潮初涨，清船争先，互相冲撞，不成队形，前锋数船为郑军船队包围，施琅亲督诸船力战得脱。

Note: This paragraph describes one conflict that occurred during the Penghu battle between the Qing navy and the Tungning Kingdom navy in 1683. Shi Lang, Fujian Navy Commander, led 20,000 troops and 300 battle ships sailed from Dongshan County of Fujian Province, attacking some islands near Taiwan.

B.2 Rebellion Exposure across Periods

Table A.3: Rebellion Exposure and Onsets across Qing Periods

Period	Years	Recorded events	Exposure rate	Onset rate
Consolidation period	1644–1684	364	2.45	2.17
High Qing	1685–1795	48	0.15	0.15
Late Qing	1796–1911	566	1.27	1.06

Note: Recorded events sum event counts, while exposure and onset rates use binary prefecture-year indicators per 100 observed prefecture-years. The count and rate columns therefore have different numerators.

C Imperial Civil Service Exams

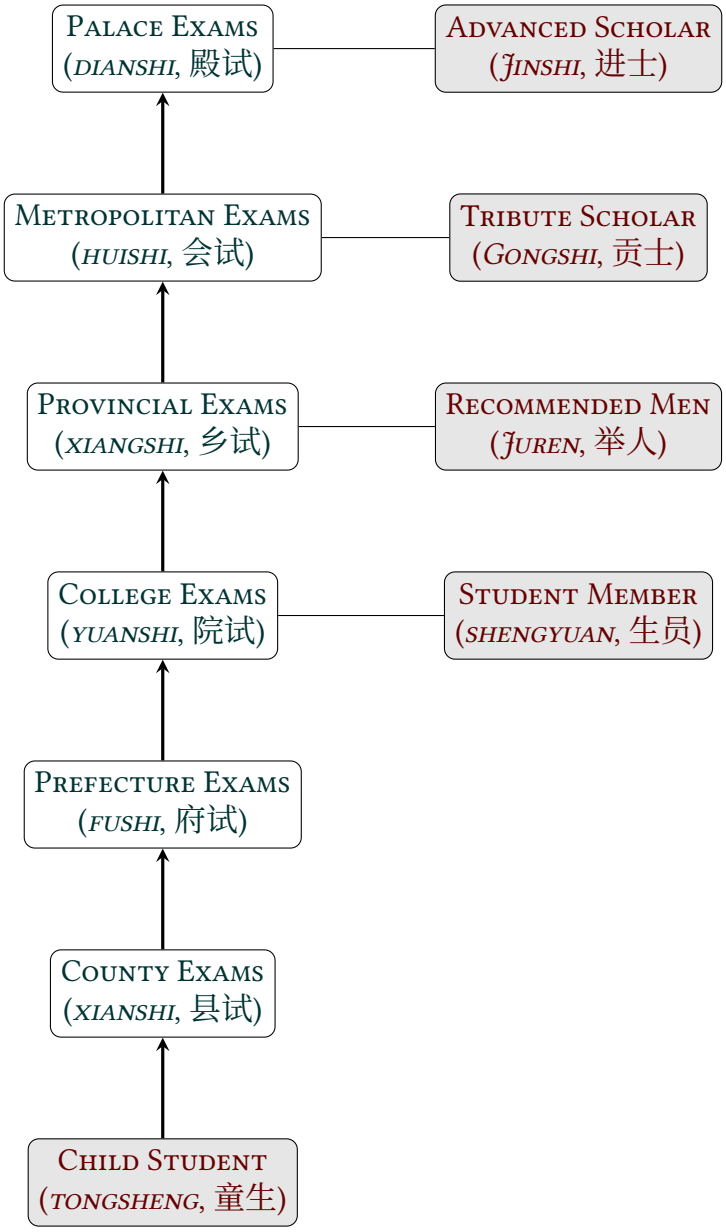
C.1 Exam Content and Format

The Qing imperial court used the Imperial Civil Service Exams to accommodate the Han Chinese elites. The Imperial Civil Service Exams focused on moral indoctrination and had a very rigid format. In terms of exam content, students were required to write two essays: one based on a passage from the Four Books and the other from the Five Classics. Both are books containing the core values and belief systems of Confucianism. The Four Books were selected by a prominent scholar, Zhu Xi, in the Song Dynasty. The Four Books served as a general introduction to Confucian thought. The Five Classics consisted of the Book of Poetry, the Book of Rites, the Book of Documents, the Book of Change, and the Book of Annals. The Book of Poetry is a collection of 305 poems. The Book of Documents is a collection of documents and speeches of rulers and officials. The Book of Rites describes ancient rites and court ceremonies. The Book of Change contains the Chinese divination system of *I-Ching*. Finally, the Book of Annals is a historical record of the state systems in China during 722–481 BC.

The exam was also very rigid in format. Students had to write essays in a form known as the *eight-legged essay*. The essay consisted of eight sections, and each section served a particular goal. There were detailed rules governing different sections of the essay, including restrictions on the number of sentences in total, the number of words in total, the format and structure of the essay, and rhyming techniques (Elman, 2000).

C.2 Exam Hierarchy and Degrees

Figure A.5: The Hierarchy of Imperial Chinese Civil Service Exams and Related Academic Degrees



Note: This figure gives a simplified representation of the hierarchy of the Imperial Civil Service Exams and academic degrees associated with each level of exams. There were also many subcategories associated with each academic degree.

D Main Results

D.1 Balance Table

Table A.4: Treated and Never-Treated Prefecture Balance

	Never treated	Ever treated	Difference	Std. diff.	p-value
Civil service quota	80.346	145.215	64.868	0.949	0.000
Coastal prefecture	0.102	0.163	0.061	0.179	0.148
Main river	0.480	0.748	0.268	0.570	0.000
Rice suitability	1.792	2.179	0.387	0.365	0.003
Foxtail millet suitability	2.991	2.769	-0.221	-0.166	0.181
Sweet potato suitability	2.638	2.607	-0.031	-0.031	0.803
Area / 1,000	13.266	17.474	4.208	0.237	0.056
Provincial capital	0.000	0.133	0.133	0.553	0.000
Prefectures	131	135			

Note: The table compares prefecture-level characteristics across the full 266-prefecture conflict panel for 1644–1684, separating prefectures that ever experience a recorded conflict from those that never do. Each prefecture enters once. Difference is ever treated minus never treated. Standardized differences divide the mean difference by the pooled standard deviation. The p-value is from a two-sided Welch test. These comparisons are descriptive; personnel regression samples are smaller because they require recorded prefects and known recruitment backgrounds and, for turnover, recorded prefects in adjacent years.

D.2 Baseline Results

Table A.5: Office-Holding Outcomes: Baseline Event Study

	(1) Serving *bannerman*	(2) Serving exam-linked official
t=-5	0.010	0.005
	(0.059)	(0.059)
t=-4	0.011	0.008
	(0.061)	(0.062)
t=-3	0.050	-0.041
	(0.061)	(0.063)
t=-2	0.026	-0.019
	(0.057)	(0.058)
t=0	-0.006	0.002
	(0.048)	(0.048)
t=+1	0.063	-0.055
	(0.046)	(0.045)
t=+2	0.108**	-0.103**
	(0.048)	(0.048)
t=+3	0.113**	-0.122**
	(0.049)	(0.048)
t=+4	0.079*	-0.084*
	(0.047)	(0.047)
t=+5	0.062	-0.089**
	(0.045)	(0.045)
Observations	6,241	6,241
Within R^2	0.003	0.004

Note: Outcomes are office-holding indicators for whether the serving prefect in a prefecture-year is a *bannerman* or exam-linked official. The sample is restricted to prefecture-years with known recruitment background. Models include prefecture and year fixed effects. Standard errors clustered by prefecture. Baseline event time is $t = -1$.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.6: Appointment Outcomes: Baseline Event Study

		Unconditional		Conditional on turnover	
	(1)	(2)	(3)	(4)	(5)
	Turnover	Bannerman appointed	Exam-linked appointed	Bannerman prefect	Exam-linked prefect
t=-5	0.030	0.025	0.005	0.120	-0.096
	(0.054)	(0.038)	(0.044)	(0.140)	(0.137)
t=-4	-0.059	0.026	-0.086***	0.293*	-0.266*
	(0.048)	(0.036)	(0.029)	(0.153)	(0.155)
t=-3	-0.009	0.019	-0.032	0.095	-0.106
	(0.045)	(0.037)	(0.039)	(0.120)	(0.120)
t=-2	-0.031	-0.015	-0.013	-0.043	0.069
	(0.041)	(0.026)	(0.035)	(0.103)	(0.106)
t=0	0.061	-0.001	0.065*	-0.093	0.090
	(0.043)	(0.032)	(0.038)	(0.080)	(0.083)
t=+1	0.094***	0.101***	-0.025	0.174**	-0.168**
	(0.034)	(0.030)	(0.028)	(0.070)	(0.067)
t=+2	0.012	0.062**	-0.044*	0.155*	-0.160**
	(0.035)	(0.031)	(0.023)	(0.085)	(0.080)
t=+3	0.009	0.032	-0.031	0.052	-0.098
	(0.037)	(0.029)	(0.028)	(0.088)	(0.087)
t=+4	-0.033	0.005	-0.034	0.043	-0.061
	(0.037)	(0.029)	(0.031)	(0.101)	(0.106)
t=+5	-0.041	0.012	-0.055*	0.030	-0.080
	(0.040)	(0.027)	(0.029)	(0.087)	(0.089)
Observations	6,662	6,503	6,503	1,474	1,474
Within R^2	0.002	0.004	0.003	0.013	0.013
Pre-trend p-value	0.518	0.851	0.064	0.202	0.192

Note: Turnover equals one when the serving-prefect identity changes between adjacent years with recorded incumbents. The unconditional recruitment outcomes use the same adjacent-record risk set and code observed years without turnover as zero. Models include prefecture and year fixed effects. Event time t=-1 is omitted. Columns 1–3 require a recorded prefect in both adjacent years. In columns 2–3, observed years without turnover are coded zero; replacement years with unknown successor recruitment are missing. Columns 4–5 restrict to turnover years with known successor recruitment. Standard errors are clustered by prefecture.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

D.3 Incumbent Exit by Recruitment Group

Table A.7: Incumbent Exit by Recruitment Channel

Event time	Exam-linked	Bannerman	p-value
$t = -5$	-0.103 (0.053)	0.093 (0.105)	0.056
$t = -4$	0.015 (0.069)	-0.170 (0.072)	0.092
$t = -3$	-0.073 (0.058)	-0.019 (0.070)	0.444
$t = -2$	-0.011 (0.052)	0.024 (0.066)	0.293
$t = 0$	0.034 (0.041)	0.079 (0.080)	0.195
$t = +1$	0.017 (0.050)	-0.038 (0.053)	0.959
$t = +2$	-0.041 (0.050)	-0.019 (0.069)	0.473
$t = +3$	-0.064 (0.051)	-0.062 (0.064)	0.962
$t = +4$	-0.088 (0.050)	0.020 (0.072)	0.110
$t = +5$	-0.004 (0.056)	-0.055 (0.071)	0.864
Joint equality test, pre-onset	$p = 0.027$		
Joint equality test, post-onset	$p = 0.560$		

Note: The outcome equals one when the incumbent serving in year t leaves before year $t+1$. Parentheses contain standard errors clustered by prefecture. Both models include prefecture and year fixed effects; $t = -1$ is omitted. The final column reports event-time-specific equality tests from a pooled interaction model.

D.4 Immediate Replacement after Rebellion Onset

Table A.8: Immediate Replacement of the Incumbent Serving at Rebellion Onset

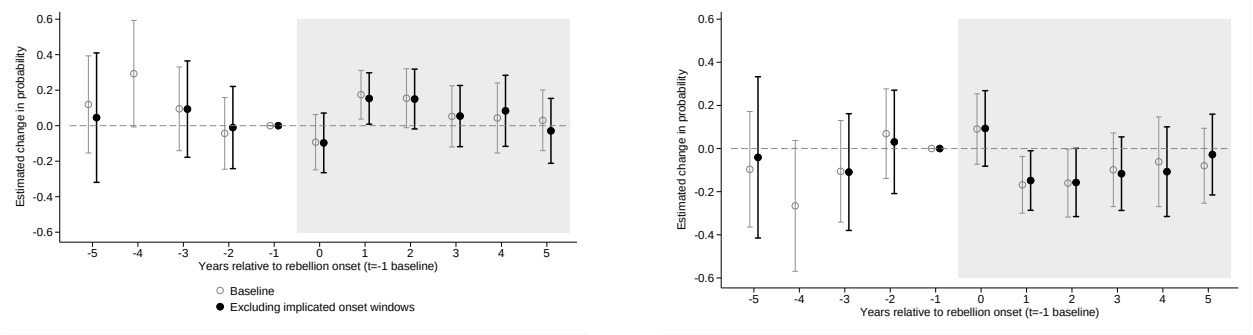
Outcome	Treated	Difference	p-value	N
Incumbent replaced by $t = +1$	0.402	0.094 (0.052)	0.070	4,546

Note: The sample requires the same prefect to be recorded in $t = -1$ and $t = 0$, with a prefect also recorded in $t = +1$. The outcome equals one if the prefect recorded in $t = +1$ differs from the incumbent serving at onset. The adjusted difference compares isolated rebellion onsets with index years having no conflict from $t = -2$ through $t = +1$. The model includes prefecture and year fixed effects and indicators for the incumbent's observed tenure through the index year. Standard errors, shown in parentheses, are clustered by prefecture. A positive estimate is consistent with prompt post-rebellion removal, but the records do not identify whether any departure was disciplinary or routine.

E Robustness Checks

E.1 Sparse Support and Permutation Inference

The conditional appointment coefficient at $t = -4$ is identified by only thirteen appointments with known recruitment backgrounds, seven of which are *bannermen*. Twelve of the thirteen appointments, including all seven *bannermen*, are in the 1640s-core sample. The observations span several rebellion episodes rather than one campaign. Dropping one contributing episode at a time leaves the coefficient between 0.232 and 0.363. After removing the complete ± 5-year window around every contributing onset, the conditional *bannerman* estimates remain 0.153 at $t = +1$ and 0.150 at $t = +2$.



A. *Bannerman* share

B. Exam-linked share

Figure A.6: Appointment Estimates Excluding the Episodes behind the $t = -4$ Cell

Note: The figure compares the baseline conditional appointment estimates with estimates that remove the complete ± 5-year window around every rebellion onset contributing an appointment to the $t = -4$ cell. Event time $t = -1$ is omitted. Vertical lines show 95% confidence intervals.

Table A.9: Appointment Outcomes: Within-Province Permutation Inference

	$t = +1$		$t = +2$		Mean of $t = +1, +2$	
Outcome	Estimate	Perm. p	Estimate	Perm. p	Estimate	Perm. p
Adjacent recorded years						
Turnover	0.094	0.046	0.012	0.802	0.053	0.173
Unconditional						
Bannerman appointed	0.101	0.026	0.062	0.121	0.082	0.016
Exam-linked appointed	-0.025	0.390	-0.044	0.127	-0.034	0.091
Conditional on turnover						
Bannerman prefect	0.174	0.237	0.155	0.169	0.165	0.130
Exam-linked prefect	-0.168	0.243	-0.160	0.124	-0.164	0.101

Note: The table reports baseline event-study estimates and two-sided randomization p-values from 999 permutations. Entire rebellion-onset paths are reassigned across prefectures within the same province, preserving each path's timing and serial structure as well as province-level treatment exposure. The final two columns test the mean of the $t = +1$ and $t = +2$ coefficients. Turnover requires recorded incumbents in both adjacent years and equals one when their identities differ. The two unconditional recruitment outcomes use that same risk set, with observed years without turnover coded zero. The final two outcomes restrict to turnover years with known successor recruitment.

E.2 Anticipatory Allocation

Table A.10: Anticipatory Appointment Tests

		Unconditional		Conditional on turnover	
	(1)	(2)	(3)	(4)	(5)
	Turnover	Bannerman appointed	Exam-linked appointed	Bannerman prefect	Exam-linked prefect
Onset in 1–3 years	0.009	-0.002	0.015	-0.030	0.024
	(0.025)	(0.020)	(0.021)	(0.063)	(0.063)
Onset in 1–5 years	0.002	0.006	-0.002	0.036	-0.033
	(0.023)	(0.018)	(0.018)	(0.062)	(0.062)
Observations	6,483	6,332	6,332	1,418	1,418

Note: The sample excludes active-conflict years. Each row comes from a separate regression and tests whether a future rebellion onset predicts current appointment outcomes. Models include prefecture and year fixed effects. Columns 1–3 require a recorded prefect in both adjacent years. In columns 2–3, observed years without turnover are coded zero; replacement years with unknown successor recruitment are missing. Columns 4–5 condition on turnover with known recruitment background. Standard errors are clustered by prefecture.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

E.3 Outcome Coding

Table A.11: Incoming Appointment Channels After Rebellion

	(1) Exam	(2) Inheritance	(3) Bannerman	(4) Purchase	(5) Other
t=-5	-0.121	0.025	0.120	-0.024	0.001
	(0.143)	(0.072)	(0.140)	(0.019)	(0.004)
t=-4	-0.262*	-0.004	0.293*	-0.014	-0.012
	(0.147)	(0.081)	(0.153)	(0.021)	(0.013)
t=-3	-0.123	0.017	0.095	0.018	-0.007
	(0.104)	(0.051)	(0.120)	(0.040)	(0.006)
t=-2	0.045	0.024	-0.043	-0.022	-0.003
	(0.097)	(0.042)	(0.103)	(0.018)	(0.006)
t=0	0.114	-0.023	-0.093	-0.001	0.003
	(0.088)	(0.033)	(0.080)	(0.023)	(0.004)
t=+1	-0.208***	0.039	0.174**	-0.004	-0.002
	(0.072)	(0.030)	(0.070)	(0.019)	(0.004)
t=+2	-0.154*	-0.007	0.155*	0.006	-0.000
	(0.081)	(0.040)	(0.085)	(0.023)	(0.005)
t=+3	-0.067	-0.031	0.052	0.024	0.022
	(0.090)	(0.021)	(0.088)	(0.026)	(0.028)
t=+4	-0.090	0.029	0.043	-0.019*	0.037
	(0.108)	(0.054)	(0.101)	(0.010)	(0.032)
t=+5	-0.055	-0.025	0.030	0.010	0.040
	(0.090)	(0.017)	(0.087)	(0.038)	(0.035)
Observations	1,474	1,474	1,474	1,474	1,474
Within R2	0.015	0.004	0.013	0.003	0.014
Pre-trend p-value	0.173	0.975	0.202	0.499	0.690

Note: Each column is a separate regression for the incoming prefect's recruitment channel, conditional on a new appointment with known recruitment background. Models include prefecture and year fixed effects. Standard errors clustered by prefecture. Baseline event time is t=-1.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

E.4 Rebellion Definition and Event-Time Definition

Table A.12: Appointment Outcomes Using Local-Unrest Onsets

		Unconditional		Conditional on turnover	
	(1) Turnover	(2) Bannerman appointed	(3) Exam-linked appointed	(4) Bannerman prefect	(5) Exam-linked prefect
t=-5	0.079	0.055	0.024	0.173	-0.140
	(0.064)	(0.048)	(0.052)	(0.156)	(0.154)
t=-4	-0.014	0.045	-0.065*	0.253	-0.230
	(0.054)	(0.039)	(0.038)	(0.178)	(0.184)
t=-3	0.016	0.011	-0.005	0.021	-0.041
	(0.055)	(0.037)	(0.048)	(0.120)	(0.113)
t=-2	0.005	0.014	-0.000	-0.049	0.087
	(0.043)	(0.030)	(0.038)	(0.121)	(0.126)
t=0	0.147***	0.062	0.108**	-0.060	0.076
	(0.053)	(0.041)	(0.047)	(0.087)	(0.087)
t=+1	0.104**	0.131***	-0.022	0.243**	-0.198**
	(0.048)	(0.042)	(0.038)	(0.098)	(0.095)
t=+2	0.052	0.124**	-0.030	0.249**	-0.231**
	(0.050)	(0.050)	(0.029)	(0.108)	(0.108)
t=+3	-0.011	0.024	-0.045	-0.015	-0.107
	(0.050)	(0.038)	(0.036)	(0.137)	(0.131)
t=+4	-0.063	0.001	-0.032	0.038	-0.033
	(0.050)	(0.039)	(0.039)	(0.140)	(0.143)
t=+5	0.002	0.052	-0.049	0.087	-0.155
	(0.061)	(0.047)	(0.038)	(0.125)	(0.116)
Observations	6,662	6,503	6,503	1,474	1,474
Within R^2	0.003	0.005	0.002	0.012	0.010
Pre-trend p-value	0.718	0.723	0.521	0.367	0.437

Note: Turnover equals one when the serving-prefect identity changes between adjacent years with recorded incumbents. The unconditional recruitment outcomes use the same adjacent-record risk set and code observed years without turnover as zero. Local unrest combines the source categories for peasant uprisings and local rebellions. Models include prefecture and year fixed effects. Event time t=-1 is omitted. Columns 1–3 require a recorded prefect in both adjacent years. In columns 2–3, observed years without turnover are coded zero; replacement years with unknown successor recruitment are missing. Columns 4–5 restrict to turnover years with known successor recruitment. Standard errors are clustered by prefecture.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.13: Appointment Outcomes Using Rebellion-Termination Event Time

		Unconditional		Conditional on turnover	
	(1)	(2)	(3)	(4)	(5)
	Turnover	Bannerman appointed	Exam-linked appointed	Bannerman prefect	Exam-linked prefect
t=-5	-0.064	0.031	-0.099***	0.345*	-0.329*
	(0.051)	(0.036)	(0.033)	(0.180)	(0.180)
t=-4	-0.034	-0.016	-0.025	-0.000	-0.016
	(0.041)	(0.029)	(0.035)	(0.121)	(0.118)
t=-3	-0.040	-0.037	0.010	-0.124	0.150
	(0.040)	(0.023)	(0.036)	(0.098)	(0.098)
t=-2	-0.012	-0.020	0.012	-0.029	-0.001
	(0.033)	(0.028)	(0.033)	(0.096)	(0.101)
t=0	0.085**	0.108***	-0.029	0.194***	-0.165**
	(0.036)	(0.031)	(0.028)	(0.067)	(0.068)
t=+1	0.011	0.055*	-0.050**	0.128*	-0.164**
	(0.033)	(0.030)	(0.024)	(0.077)	(0.077)
t=+2	-0.012	0.025	-0.041	0.078	-0.122
	(0.037)	(0.030)	(0.028)	(0.087)	(0.086)
t=+3	0.011	0.028	-0.017	0.040	-0.044
	(0.035)	(0.027)	(0.030)	(0.079)	(0.075)
t=+4	-0.056	0.032	-0.082***	0.157*	-0.171*
	(0.037)	(0.029)	(0.025)	(0.086)	(0.087)
t=+5	-0.029	-0.019	-0.002	-0.010	0.052
	(0.036)	(0.024)	(0.032)	(0.084)	(0.083)
Observations	6,662	6,503	6,503	1,474	1,474
Within R^2	0.002	0.005	0.003	0.014	0.014
Pre-trend p-value	0.629	0.404	0.035	0.148	0.142

Note: Turnover equals one when the serving-prefect identity changes between adjacent years with recorded incumbents. The unconditional recruitment outcomes use the same adjacent-record risk set and code observed years without turnover as zero. Event time is anchored to the first peaceful year after a rebellion spell ends; t=-1 is the omitted last-conflict-year baseline. Models include prefecture and year fixed effects. Columns 1–3 require a recorded prefect in both adjacent years. In columns 2–3, observed years without turnover are coded zero; replacement years with unknown successor recruitment are missing. Columns 4–5 restrict to turnover years with known successor recruitment. Standard errors are clustered by prefecture.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.14: Appointment Outcomes Excluding the Most Conflict-Exposed Prefecture

		Unconditional		Conditional on turnover	
	(1)	(2)	(3)	(4)	(5)
	Turnover	Bannerman appointed	Exam-linked appointed	Bannerman prefect	Exam-linked prefect
t=-5	0.038	0.028	0.010	0.112	-0.088
	(0.054)	(0.039)	(0.045)	(0.139)	(0.136)
t=-4	-0.055	0.027	-0.083***	0.287*	-0.260*
	(0.049)	(0.037)	(0.030)	(0.154)	(0.155)
t=-3	-0.022	-0.001	-0.025	0.021	-0.031
	(0.044)	(0.033)	(0.040)	(0.107)	(0.106)
t=-2	-0.030	-0.022	-0.005	-0.077	0.106
	(0.042)	(0.026)	(0.035)	(0.101)	(0.103)
t=0	0.066	0.002	0.067*	-0.099	0.099
	(0.045)	(0.033)	(0.040)	(0.081)	(0.085)
t=+1	0.086**	0.097***	-0.031	0.175**	-0.169**
	(0.035)	(0.031)	(0.028)	(0.073)	(0.070)
t=+2	0.020	0.067**	-0.039*	0.151*	-0.157*
	(0.036)	(0.032)	(0.023)	(0.085)	(0.080)
t=+3	0.002	0.027	-0.034	0.037	-0.085
	(0.037)	(0.030)	(0.029)	(0.092)	(0.091)
t=+4	-0.038	0.004	-0.038	0.044	-0.065
	(0.038)	(0.029)	(0.031)	(0.104)	(0.110)
t=+5	-0.038	0.012	-0.052*	0.029	-0.079
	(0.041)	(0.027)	(0.029)	(0.086)	(0.087)
Observations	6,624	6,465	6,465	1,465	1,465
Within R^2	0.002	0.004	0.003	0.013	0.013
Pre-trend p-value	0.479	0.774	0.090	0.224	0.224

Note: Turnover equals one when the serving-prefect identity changes between adjacent years with recorded incumbents. The unconditional recruitment outcomes use the same adjacent-record risk set and code observed years without turnover as zero. Prefectures with the maximum total recorded conflict count are excluded. Models include prefecture and year fixed effects. Event time t=-1 is omitted. Columns 1–3 require a recorded prefect in both adjacent years. In columns 2–3, observed years without turnover are coded zero; replacement years with unknown successor recruitment are missing. Columns 4–5 restrict to turnover years with known successor recruitment. Standard errors are clustered by prefecture.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

E.5 Heterogeneity by Rebellion Duration and Geographic Scope

Table A.15: Appointment Responses by First-Episode Duration

		Unconditional		Conditional on turnover	
	(1)	(2)	(3)	(4)	(5)
	Turnover	Bannerman appointed	Exam-linked appointed	Bannerman prefect	Exam-linked prefect
t=-5 × multi-year	-0.211	0.024	-0.172**	-0.081	0.214
	(0.156)	(0.128)	(0.070)	(0.192)	(0.182)
t=-4 × multi-year	-0.007	-0.020	0.074	-0.400	0.372
	(0.107)	(0.132)	(0.113)	(0.243)	(0.254)
t=-3 × multi-year	0.245	0.114	0.194	-0.067	0.138
	(0.186)	(0.111)	(0.176)	(0.240)	(0.233)
t=-2 × multi-year	0.004	-0.067	0.062	-0.217	0.330
	(0.176)	(0.095)	(0.127)	(0.226)	(0.251)
t=0 × multi-year	-0.095	-0.104	-0.013	-0.098	0.006
	(0.144)	(0.074)	(0.142)	(0.183)	(0.271)
t=+1 × multi-year	-0.047	0.036	-0.118**	0.013	-0.129
	(0.104)	(0.091)	(0.050)	(0.142)	(0.109)
t=+2 × multi-year	0.087	0.076	0.019	0.009	0.070
	(0.133)	(0.104)	(0.092)	(0.213)	(0.195)
t=+3 × multi-year	-0.073	0.038	-0.052	0.182	-0.083
	(0.110)	(0.089)	(0.083)	(0.206)	(0.210)
t=+4 × multi-year	-0.086	-0.013	-0.103*	0.050	-0.283
	(0.121)	(0.082)	(0.060)	(0.213)	(0.262)
t=+5 × multi-year	-0.024	0.019	-0.015	0.182	-0.101
	(0.113)	(0.077)	(0.093)	(0.218)	(0.223)
Observations	6,662	6,503	6,503	1,474	1,474
Within R2	0.003	0.005	0.004	0.016	0.016
Pre-interaction p-value	0.008	0.378	0.045	0.458	0.382
Post t=0..2 interaction p-value	0.625	0.164	0.056	0.950	0.611
Post t=0..5 interaction p-value	0.844	0.501	0.090	0.924	0.813

Note: Interaction terms compare multi-year with one-year first rebellion episodes. Columns 1–3 require recorded incumbents in both adjacent years; observed years without turnover are coded zero in columns 2–3. Columns 4–5 restrict to turnover years with known successor recruitment. Models include the uninteracted event-time indicators, prefecture and year fixed effects, and prefecture-clustered standard errors. Event time t=-1 is omitted. Among 135 treated prefectures, 15 have multi-year first episodes.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.16: Appointment Responses by First-Episode Geographic Scope

		Unconditional		Conditional on turnover	
	(1) Turnover	(2) Bannerman appointed	(3) Exam-linked appointed	(4) Bannerman prefect	(5) Exam-linked prefect
t=-5 × multi-prefecture	-0.086	0.006	-0.034	0.153	-0.113
	(0.104)	(0.079)	(0.091)	(0.272)	(0.263)
t=-4 × multi-prefecture	0.018	0.144**	-0.068	0.700***	-0.665**
	(0.091)	(0.064)	(0.062)	(0.248)	(0.258)
t=-3 × multi-prefecture	-0.092	0.043	-0.114	0.358	-0.388*
	(0.090)	(0.073)	(0.081)	(0.230)	(0.227)
t=-2 × multi-prefecture	0.084	0.084*	-0.023	0.318	-0.283
	(0.081)	(0.051)	(0.069)	(0.197)	(0.201)
t=0 × multi-prefecture	-0.089	-0.068	-0.032	-0.086	0.052
	(0.093)	(0.070)	(0.077)	(0.155)	(0.163)
t=+1 × multi-prefecture	0.156**	0.100	0.057	-0.077	0.013
	(0.068)	(0.063)	(0.053)	(0.134)	(0.129)
t=+2 × multi-prefecture	0.128*	0.116*	-0.011	0.124	-0.129
	(0.070)	(0.061)	(0.046)	(0.169)	(0.164)
t=+3 × multi-prefecture	0.083	0.084	-0.001	0.066	-0.170
	(0.073)	(0.058)	(0.059)	(0.204)	(0.201)
t=+4 × multi-prefecture	0.042	0.014	0.057	0.094	0.034
	(0.076)	(0.059)	(0.059)	(0.203)	(0.210)
t=+5 × multi-prefecture	0.097	0.072	0.049	0.019	-0.001
	(0.078)	(0.053)	(0.055)	(0.164)	(0.172)
Observations	6,662	6,503	6,503	1,474	1,474
Within R2	0.005	0.007	0.004	0.022	0.022
Pre-interaction p-value	0.547	0.041	0.547	0.044	0.076
Post t=0..2 interaction p-value	0.018	0.043	0.636	0.799	0.866
Post t=0..5 interaction p-value	0.035	0.065	0.777	0.950	0.976

Note: Interaction terms compare multi-prefecture with single-prefecture first rebellion episodes. Columns 1–3 require recorded incumbents in both adjacent years; observed years without turnover are coded zero in columns 2–3. Columns 4–5 restrict to turnover years with known successor recruitment. Models include the uninteracted event-time indicators, prefecture and year fixed effects, and prefecture-clustered standard errors. Event time t=-1 is omitted. Among 135 treated prefectures, 77 have multi-prefecture first episodes.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

E.6 Large Campaigns and Province-Level Shocks

Table A.17: Appointment Outcomes Excluding Three Feudatories Years

		Unconditional		Conditional on turnover	
	(1)	(2)	(3)	(4)	(5)
	Turnover	Bannerman appointed	Exam-linked appointed	Bannerman prefect	Exam-linked prefect
t=-5	0.040	0.048	-0.022	0.170	-0.143
	(0.063)	(0.048)	(0.047)	(0.149)	(0.145)
t=-4	-0.030	0.067	-0.092**	0.266	-0.240
	(0.066)	(0.051)	(0.043)	(0.167)	(0.168)
t=-3	-0.001	0.043	-0.063	0.085	-0.110
	(0.054)	(0.048)	(0.044)	(0.122)	(0.122)
t=-2	0.017	0.007	0.001	-0.004	0.020
	(0.055)	(0.037)	(0.048)	(0.113)	(0.113)
t=0	-0.021	-0.030	0.002	-0.125	0.087
	(0.055)	(0.038)	(0.049)	(0.100)	(0.108)
t=+1	0.055	0.103**	-0.075*	0.192**	-0.198**
	(0.046)	(0.043)	(0.038)	(0.089)	(0.088)
t=+2	-0.047	0.018	-0.062*	0.072	-0.067
	(0.041)	(0.036)	(0.033)	(0.104)	(0.099)
t=+3	0.009	0.045	-0.058*	0.098	-0.160**
	(0.046)	(0.037)	(0.032)	(0.079)	(0.080)
t=+4	-0.053	-0.002	-0.056	0.092	-0.118
	(0.043)	(0.031)	(0.035)	(0.104)	(0.113)
t=+5	-0.077*	-0.001	-0.077**	0.013	-0.075
	(0.042)	(0.028)	(0.032)	(0.090)	(0.096)
Observations	4,960	4,830	4,830	1,170	1,170
Within R^2	0.002	0.004	0.004	0.014	0.014
Pre-trend p-value	0.855	0.654	0.188	0.357	0.356

Note: Turnover equals one when the serving-prefect identity changes between adjacent years with recorded incumbents. The unconditional recruitment outcomes use the same adjacent-record risk set and code observed years without turnover as zero. Calendar years 1673–1681 are excluded. Models include prefecture and year fixed effects. Event time t=-1 is omitted. Columns 1–3 require a recorded prefect in both adjacent years. In columns 2–3, observed years without turnover are coded zero; replacement years with unknown successor recruitment are missing. Columns 4–5 restrict to turnover years with known successor recruitment. Standard errors are clustered by prefecture.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.18: Appointment Outcomes with Province-Specific Linear Trends

		Unconditional		Conditional on turnover	
	(1)	(2)	(3)	(4)	(5)
	Turnover	Bannerman appointed	Exam-linked appointed	Bannerman prefect	Exam-linked prefect
t=-5	0.026	0.021	0.003	0.081	-0.060
	(0.052)	(0.035)	(0.044)	(0.133)	(0.130)
t=-4	-0.064	0.022	-0.089***	0.261*	-0.238
	(0.047)	(0.035)	(0.029)	(0.150)	(0.151)
t=-3	-0.009	0.018	-0.033	0.120	-0.134
	(0.044)	(0.036)	(0.040)	(0.113)	(0.112)
t=-2	-0.029	-0.015	-0.014	-0.013	0.036
	(0.040)	(0.027)	(0.035)	(0.110)	(0.112)
t=0	0.060	-0.006	0.065*	-0.108	0.103
	(0.043)	(0.030)	(0.038)	(0.081)	(0.085)
t=+1	0.093***	0.094***	-0.023	0.144**	-0.144**
	(0.035)	(0.030)	(0.028)	(0.070)	(0.069)
t=+2	0.010	0.056*	-0.043*	0.123	-0.134
	(0.035)	(0.031)	(0.024)	(0.088)	(0.083)
t=+3	0.003	0.022	-0.031	0.037	-0.084
	(0.038)	(0.029)	(0.029)	(0.085)	(0.085)
t=+4	-0.040	-0.006	-0.033	0.014	-0.043
	(0.037)	(0.028)	(0.031)	(0.096)	(0.102)
t=+5	-0.046	0.001	-0.052*	-0.026	-0.024
	(0.040)	(0.026)	(0.029)	(0.084)	(0.085)
Observations	6,662	6,503	6,503	1,474	1,474
Within R^2	0.002	0.003	0.003	0.011	0.011
Pre-trend p-value	0.504	0.885	0.053	0.301	0.266

Note: Turnover equals one when the serving-prefect identity changes between adjacent years with recorded incumbents. The unconditional recruitment outcomes use the same adjacent-record risk set and code observed years without turnover as zero. Models include prefecture and year fixed effects and province-specific linear trends. Event time t=-1 is omitted. Columns 1–3 require a recorded prefect in both adjacent years. In columns 2–3, observed years without turnover are coded zero; replacement years with unknown successor recruitment are missing. Columns 4–5 restrict to turnover years with known successor recruitment. Standard errors are clustered by prefecture.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.19: Appointment Outcomes with Province-Year Fixed Effects

		Unconditional		Conditional on turnover	
	(1)	(2)	(3)	(4)	(5)
	Turnover	Bannerman appointed	Exam-linked appointed	Bannerman prefect	Exam-linked prefect
t=-5	0.051	0.028	0.018	0.182	-0.094
	(0.056)	(0.036)	(0.048)	(0.122)	(0.143)
t=-4	-0.031	0.038	-0.073**	0.354**	-0.380**
	(0.051)	(0.033)	(0.036)	(0.163)	(0.170)
t=-3	0.065	0.056	0.000	0.151	-0.201
	(0.047)	(0.039)	(0.040)	(0.138)	(0.140)
t=-2	-0.010	0.000	-0.020	0.028	-0.020
	(0.043)	(0.028)	(0.037)	(0.150)	(0.145)
t=0	-0.017	-0.063*	0.047	-0.129	0.129
	(0.043)	(0.036)	(0.040)	(0.105)	(0.111)
t=+1	0.061	0.067**	-0.028	0.149*	-0.141*
	(0.037)	(0.031)	(0.031)	(0.078)	(0.079)
t=+2	-0.017	0.042	-0.055**	0.142	-0.155
	(0.039)	(0.034)	(0.025)	(0.111)	(0.102)
t=+3	-0.025	-0.007	-0.030	-0.001	-0.065
	(0.040)	(0.033)	(0.030)	(0.120)	(0.120)
t=+4	-0.054	0.007	-0.053*	0.030	-0.012
	(0.039)	(0.029)	(0.031)	(0.126)	(0.131)
t=+5	-0.069	-0.003	-0.071**	0.090	-0.159
	(0.046)	(0.032)	(0.032)	(0.118)	(0.123)
Observations	6,645	6,486	6,486	1,313	1,313
Within R^2	0.002	0.003	0.003	0.016	0.018
Pre-trend p-value	0.545	0.505	0.368	0.132	0.145

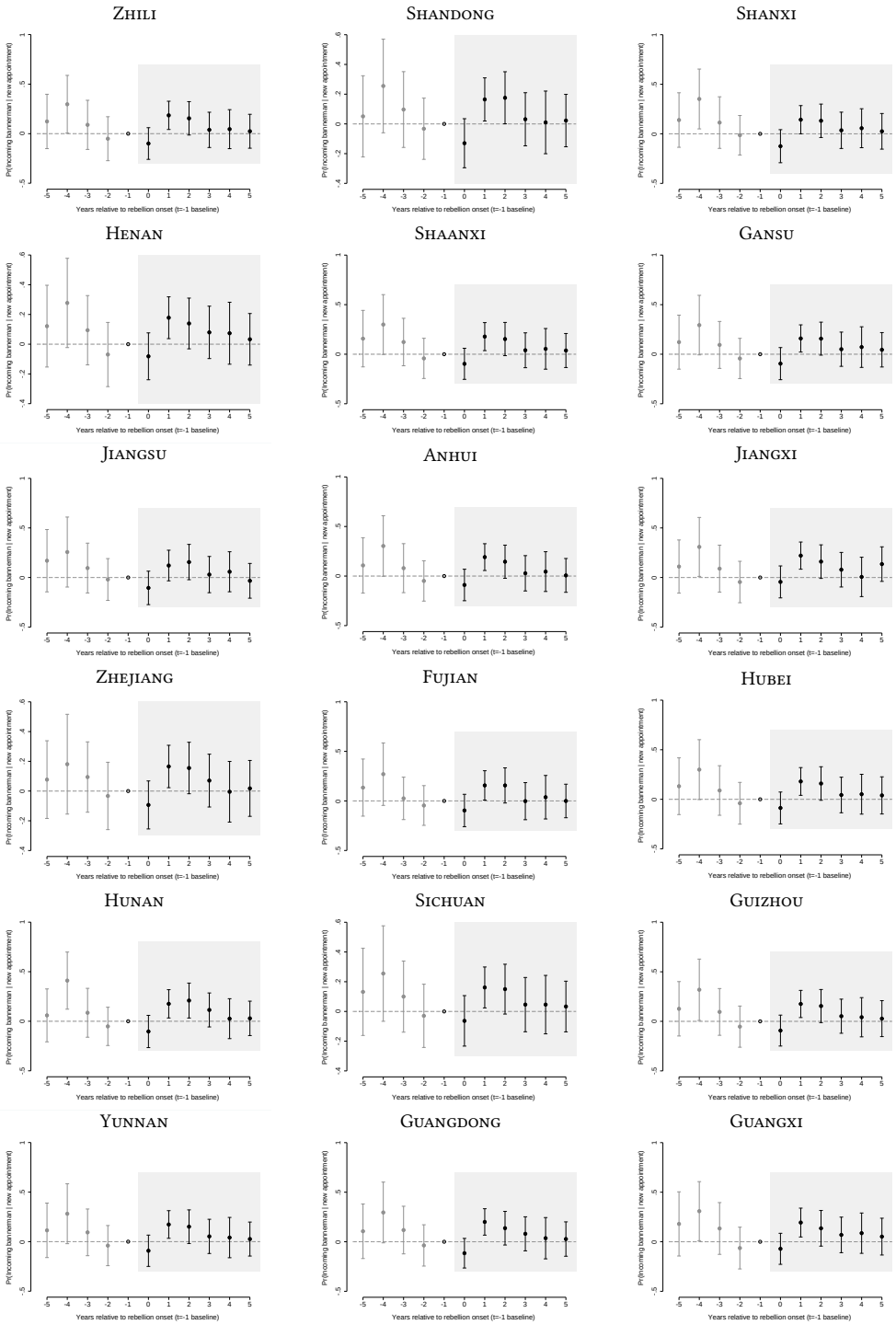
Note: Turnover equals one when the serving-prefect identity changes between adjacent years with recorded incumbents. The unconditional recruitment outcomes use the same adjacent-record risk set and code observed years without turnover as zero. Models include prefecture and province-year fixed effects. Event time t=-1 is omitted. Columns 1–3 require a recorded prefect in both adjacent years. In columns 2–3, observed years without turnover are coded zero; replacement years with unknown successor recruitment are missing. Columns 4–5 restrict to turnover years with known successor recruitment. Standard errors are clustered by prefecture. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.20: Appointment Outcomes: Leave-One-Province-Out Estimates at $t = +1$

		Unconditional		Conditional on turnover	
Excluded province	Any appointment	Bannerman appointed	Exam-linked appointed	Bannerman prefect	Exam-linked prefect
Zhili	0.088**	0.101***	-0.031	0.185**	-0.178**
	(0.035)	(0.031)	(0.027)	(0.073)	(0.069)
Jiangsu	0.089**	0.080***	-0.014	0.121	-0.129*
	(0.036)	(0.030)	(0.029)	(0.079)	(0.076)
Anhui	0.102***	0.111***	-0.028	0.191***	-0.185***
	(0.035)	(0.031)	(0.027)	(0.069)	(0.065)
Zhejiang	0.093**	0.099***	-0.021	0.165**	-0.160**
	(0.036)	(0.032)	(0.030)	(0.073)	(0.070)
Jiangxi	0.078**	0.111***	-0.044	0.221***	-0.213***
	(0.036)	(0.032)	(0.027)	(0.070)	(0.067)
Fujian	0.098**	0.097***	-0.021	0.157**	-0.153**
	(0.038)	(0.033)	(0.031)	(0.076)	(0.073)
Henan	0.096***	0.102***	-0.024	0.178**	-0.169**
	(0.035)	(0.031)	(0.028)	(0.072)	(0.069)
Shandong	0.099***	0.097***	-0.019	0.165**	-0.157**
	(0.035)	(0.030)	(0.028)	(0.074)	(0.071)
Shanxi	0.103***	0.095***	-0.012	0.142*	-0.140**
	(0.035)	(0.029)	(0.029)	(0.073)	(0.071)
Hubei	0.087**	0.105***	-0.025	0.180**	-0.161**
	(0.036)	(0.031)	(0.029)	(0.071)	(0.069)
Hunan	0.097***	0.104***	-0.023	0.176**	-0.170**
	(0.036)	(0.032)	(0.029)	(0.073)	(0.070)
Shaanxi	0.089**	0.100***	-0.024	0.178**	-0.173**
	(0.035)	(0.030)	(0.028)	(0.072)	(0.069)
Gansu	0.087**	0.093***	-0.026	0.159**	-0.153**
	(0.034)	(0.030)	(0.028)	(0.070)	(0.067)
Sichuan	0.095***	0.100***	-0.024	0.161**	-0.157**
	(0.036)	(0.031)	(0.029)	(0.070)	(0.067)
Guangdong	0.099***	0.111***	-0.030	0.200***	-0.193***
	(0.036)	(0.031)	(0.028)	(0.068)	(0.065)
Guangxi	0.105***	0.110***	-0.029	0.194***	-0.183**
	(0.037)	(0.033)	(0.030)	(0.074)	(0.071)
Yunnan	0.093***	0.105***	-0.031	0.173**	-0.173**
	(0.035)	(0.031)	(0.028)	(0.071)	(0.068)
Guizhou	0.096***	0.103***	-0.024	0.175**	-0.168**
	(0.035)	(0.030)	(0.028)	(0.070)	(0.067)

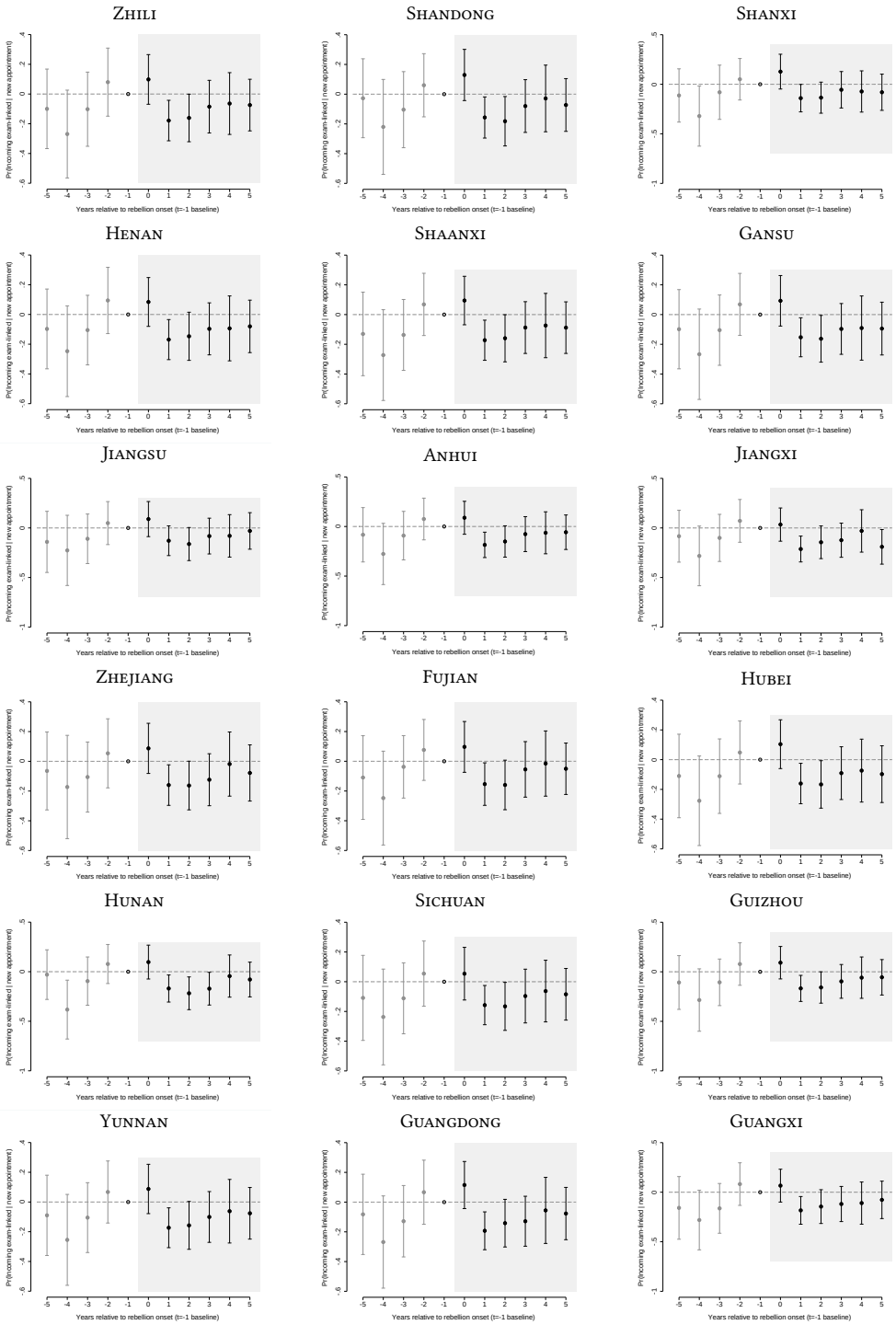
Note: Each pair of rows reports the $t = +1$ coefficient and its standard error after excluding the named province. Each regression includes the complete event-study specification with prefecture and year fixed effects. Columns 1–3 require a recorded prefect in both adjacent years. In columns 2–3, observed years without turnover are coded zero; replacement years with unknown successor recruitment are missing. Columns 4–5 restrict to turnover years with known successor recruitment. Standard errors are clustered by prefecture.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure A.7: Leave-One-Province-Out: Incoming *Bannerman* Appointments



Note: Each panel drops one province and re-estimates the conditional incoming-*bannerman* event study. The shaded area begins at $t = -0.5$. Vertical lines show 95% confidence intervals. All models include prefecture and year fixed effects, with standard errors clustered by prefecture. Event time $t = -1$ is omitted.

Figure A.8: Leave-One-Province-Out: Incoming Exam-linked Appointments



Note: Each panel drops one province and re-estimates the conditional incoming-exam-linked event study. The shaded area begins at $t = -0.5$. Vertical lines show 95% confidence intervals. All models include prefecture and year fixed effects, with standard errors clustered by prefecture. Event time $t = -1$ is omitted.

E.7 Territorial Incorporation

Table A.21: Sample Composition by Timing of Qing Administration

	1640s core	Post-1640s
Prefecture-year observations	5,136	1,526
Share of prefecture-year observations	77.1%	22.9%
New appointments	1,214	260
Share of new appointments	82.4%	17.6%

Note: The 1640s core includes prefectures with a first recorded Qing prefect by 1649. Post-1640s prefectures first enter the administrative panel in 1650 or later. Prefecture-year observations correspond to the turnover-analysis sample after fixed-effect singleton removal. New appointments have known recruitment background.

Table A.22: Appointment Outcomes with Incorporation-Age Fixed Effects

		Unconditional		Conditional on turnover	
	(1)	(2)	(3)	(4)	(5)
	Turnover	Bannerman appointed	Exam-linked appointed	Bannerman prefect	Exam-linked prefect
t=-5	0.035	0.029	0.006	0.128	-0.105
	(0.054)	(0.038)	(0.044)	(0.141)	(0.138)
t=-4	-0.056	0.029	-0.085***	0.293*	-0.266*
	(0.048)	(0.036)	(0.029)	(0.151)	(0.153)
t=-3	-0.006	0.021	-0.031	0.111	-0.124
	(0.045)	(0.037)	(0.039)	(0.118)	(0.116)
t=-2	-0.025	-0.011	-0.010	-0.037	0.061
	(0.041)	(0.026)	(0.035)	(0.103)	(0.106)
t=0	0.064	0.001	0.067*	-0.090	0.087
	(0.043)	(0.032)	(0.038)	(0.080)	(0.084)
t=+1	0.096***	0.102***	-0.024	0.169**	-0.163**
	(0.035)	(0.030)	(0.028)	(0.070)	(0.067)
t=+2	0.013	0.063**	-0.043*	0.155*	-0.160*
	(0.036)	(0.032)	(0.023)	(0.086)	(0.081)
t=+3	0.005	0.029	-0.032	0.047	-0.093
	(0.037)	(0.029)	(0.029)	(0.088)	(0.087)
t=+4	-0.035	0.004	-0.034	0.036	-0.053
	(0.038)	(0.029)	(0.031)	(0.101)	(0.105)
t=+5	-0.040	0.012	-0.054*	0.028	-0.077
	(0.040)	(0.027)	(0.029)	(0.088)	(0.089)
Observations	6,662	6,503	6,503	1,474	1,474
Within R^2	0.002	0.004	0.003	0.013	0.013
Pre-trend p-value	0.545	0.847	0.072	0.165	0.145

Note: Turnover equals one when the serving-prefect identity changes between adjacent years with recorded incumbents. The unconditional recruitment outcomes use the same adjacent-record risk set and code observed years without turnover as zero. Models add fixed effects for years since the first recorded prefect, grouped as nonpositive, 1–2, 3–5, 6–10, and 11 or more years. Prefecture and year fixed effects are included. Event time t=-1 is omitted. Columns 1–3 require a recorded prefect in both adjacent years. In columns 2–3, observed years without turnover are coded zero; replacement years with unknown successor recruitment are missing. Columns 4–5 restrict to turnover years with known successor recruitment. Standard errors are clustered by prefecture.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.23: Appointment Outcomes in the 1640s-Core Sample

		Unconditional		Conditional on turnover	
	(1)	(2)	(3)	(4)	(5)
	Turnover	Bannerman appointed	Exam-linked appointed	Bannerman prefect	Exam-linked prefect
t=-5	0.018	0.035	-0.010	0.163	-0.147
	(0.065)	(0.052)	(0.051)	(0.168)	(0.164)
t=-4	-0.054	0.045	-0.104***	0.375**	-0.360**
	(0.060)	(0.048)	(0.033)	(0.150)	(0.151)
t=-3	-0.014	0.022	-0.047	0.135	-0.164
	(0.050)	(0.046)	(0.041)	(0.133)	(0.128)
t=-2	-0.028	-0.010	-0.019	-0.008	0.034
	(0.048)	(0.034)	(0.041)	(0.119)	(0.122)
t=0	0.014	-0.025	0.042	-0.122	0.106
	(0.047)	(0.033)	(0.042)	(0.084)	(0.089)
t=+1	0.099**	0.113***	-0.039	0.193***	-0.193***
	(0.039)	(0.036)	(0.031)	(0.073)	(0.070)
t=+2	-0.003	0.049	-0.043	0.124	-0.147
	(0.039)	(0.036)	(0.027)	(0.095)	(0.092)
t=+3	0.007	0.057	-0.051*	0.126	-0.181**
	(0.042)	(0.036)	(0.030)	(0.093)	(0.090)
t=+4	-0.028	0.013	-0.040	0.075	-0.103
	(0.045)	(0.036)	(0.036)	(0.111)	(0.115)
t=+5	-0.040	0.023	-0.049	0.061	-0.087
	(0.047)	(0.032)	(0.035)	(0.093)	(0.095)
Observations	5,136	4,998	4,998	1,214	1,214
Within R^2	0.002	0.005	0.003	0.019	0.020
Pre-trend p-value	0.789	0.873	0.038	0.067	0.045

Note: The sample is restricted to prefectures whose first recorded incumbent appears by 1649. Models include prefecture and year fixed effects. Event time t=-1 is omitted. Columns 1–3 require a recorded prefect in both adjacent years. In columns 2–3, observed years without turnover are coded zero; replacement years with unknown successor recruitment are missing. Columns 4–5 restrict to turnover years with known successor recruitment. Standard errors are clustered by prefecture.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.24: Appointment Outcomes in Later-Incorporated Prefectures

		Unconditional		Conditional on turnover	
	(1)	(2)	(3)	(4)	(5)
	Turnover	Bannerman appointed	Exam-linked appointed	Bannerman prefect	Exam-linked prefect
t=-5	0.126	0.003	0.100	-0.018	0.118
	(0.100)	(0.012)	(0.091)	(0.151)	(0.176)
t=-4	-0.060	-0.010	-0.035	-0.396	0.460*
	(0.059)	(0.013)	(0.060)	(0.238)	(0.252)
t=-3	0.087	0.029	0.078	-0.169	0.415
	(0.108)	(0.046)	(0.101)	(0.316)	(0.367)
t=-2	-0.003	-0.021	0.043	0.019	-0.008
	(0.071)	(0.018)	(0.058)	(0.199)	(0.201)
t=0	0.258**	0.081	0.174**	0.003	0.043
	(0.103)	(0.095)	(0.086)	(0.190)	(0.192)
t=+1	0.043	0.042	0.024	0.079	-0.002
	(0.078)	(0.045)	(0.062)	(0.205)	(0.200)
t=+2	0.039	0.092	-0.045	0.313	-0.208
	(0.084)	(0.069)	(0.047)	(0.221)	(0.180)
t=+3	-0.016	-0.092***	0.045	-0.536***	0.521***
	(0.089)	(0.023)	(0.074)	(0.160)	(0.161)
t=+4	-0.084	-0.039	-0.012	-0.343**	0.329*
	(0.064)	(0.030)	(0.058)	(0.160)	(0.179)
t=+5	-0.054	-0.045	-0.060	-0.320	0.006
	(0.055)	(0.046)	(0.040)	(0.389)	(0.334)
Observations	1,526	1,505	1,505	260	260
Within R^2	0.012	0.009	0.009	0.058	0.046
Pre-trend p-value	0.616	0.314	0.677	0.495	0.277

Note: The sample is restricted to prefectures whose first recorded incumbent appears in 1650 or later. Models include prefecture and year fixed effects. Event time t=-1 is omitted. Columns 1–3 require a recorded prefect in both adjacent years. In columns 2–3, observed years without turnover are coded zero; replacement years with unknown successor recruitment are missing. Columns 4–5 restrict to turnover years with known successor recruitment. Standard errors are clustered by prefecture.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

E.8 Estimator Choice

Table A.25: Appointment Outcomes: Sun–Abraham First-Onset Estimator

		Unconditional		Conditional on turnover	
	(1)	(2)	(3)	(4)	(5)
	Turnover	Bannerman appointed	Exam-linked appointed	Bannerman prefect	Exam-linked prefect
t=-5	0.027	0.022	0.004	0.147	-0.124
	(0.048)	(0.031)	(0.041)	(0.130)	(0.129)
t=-4	-0.050	0.031	-0.082***	0.306**	-0.316**
	(0.041)	(0.032)	(0.026)	(0.153)	(0.151)
t=-3	-0.007	0.014	-0.025	0.042	-0.064
	(0.037)	(0.029)	(0.035)	(0.127)	(0.127)
t=-2	-0.032	-0.012	-0.021	-0.064	0.034
	(0.031)	(0.020)	(0.027)	(0.097)	(0.105)
t=0	0.062	0.003	0.063*	-0.103	0.097
	(0.041)	(0.033)	(0.037)	(0.084)	(0.089)
t=+1	0.085***	0.092***	-0.024	0.158**	-0.156**
	(0.031)	(0.027)	(0.026)	(0.070)	(0.068)
t=+2	0.012	0.059**	-0.041*	0.149*	-0.157*
	(0.032)	(0.029)	(0.022)	(0.086)	(0.081)
t=+3	0.005	0.031	-0.032	0.037	-0.071
	(0.035)	(0.028)	(0.025)	(0.094)	(0.096)
t=+4	-0.037	0.001	-0.033	0.053	-0.065
	(0.036)	(0.029)	(0.030)	(0.099)	(0.102)
t=+5	-0.049	0.008	-0.059**	0.032	-0.099
	(0.038)	(0.025)	(0.028)	(0.089)	(0.087)
Observations	6,662	6,503	6,503	1,474	1,474

Note: Interaction-weighted Sun–Abraham estimates use first rebellion onset as the treatment cohort and never-treated prefectures as controls. Columns 1–3 require a recorded prefect in both adjacent years. In columns 2–3, observed years without turnover are coded zero; replacement years with unknown successor recruitment are missing. Columns 4–5 restrict to turnover years with known successor recruitment. Prefecture and year fixed effects are included; standard errors are clustered by prefecture. Event time $t = -1$ is omitted.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

E.9 Record Missingness

Table A.26: Record Coverage and Recruitment-Channel Missingness around Rebellion Onset

	Gazetteer coverage	Missing recruitment channel	
	(1) No prefect	(2) Recorded prefect	(3) New appointment
t=-5	0.029	0.041	0.014
	(0.028)	(0.043)	(0.083)
t=-4	0.010	0.091**	0.075
	(0.025)	(0.043)	(0.102)
t=-3	0.008	0.053	0.041
	(0.023)	(0.036)	(0.057)
t=-2	0.000	0.041	0.013
	(0.023)	(0.035)	(0.080)
t=0	-0.079***	0.028	-0.002
	(0.021)	(0.034)	(0.049)
t=+1	-0.085***	0.014	0.059
	(0.017)	(0.030)	(0.044)
t=+2	-0.077***	0.002	-0.007
	(0.019)	(0.027)	(0.037)
t=+3	-0.073***	-0.017	0.017
	(0.020)	(0.027)	(0.057)
t=+4	-0.099***	-0.041	-0.033
	(0.016)	(0.028)	(0.055)
t=+5	-0.095***	-0.058***	-0.079
	(0.015)	(0.022)	(0.053)
Observations	10,906	6,869	1,633
Pre-trend p-value	0.856	0.247	0.906
Post-event joint p-value	< 0.001	0.166	0.553

Note: Column 1 equals one when no prefect is recorded in a prefecture-year. Column 2 is restricted to recorded prefects and equals one when recruitment-channel information is missing. Column 3 is restricted to new appointments and equals one when the incoming prefect's recruitment channel is missing. All models include prefecture and year fixed effects, with standard errors clustered by prefecture. Event time $t = -1$ is omitted. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A.27: Appointment Outcomes with Complete Adjacent Recruitment Records

		Unconditional		Conditional on turnover	
	(1)	(2)	(3)	(4)	(5)
	Turnover	Bannerman appointed	Exam-linked appointed	Bannerman prefect	Exam-linked prefect
t=-5	0.029	0.032	0.004	0.143	-0.119
	(0.059)	(0.042)	(0.048)	(0.136)	(0.135)
t=-4	-0.041	0.046	-0.081**	0.294*	-0.260
	(0.054)	(0.041)	(0.036)	(0.165)	(0.169)
t=-3	-0.023	0.019	-0.046	0.086	-0.100
	(0.056)	(0.045)	(0.043)	(0.141)	(0.140)
t=-2	-0.065	-0.044*	-0.015	-0.126	0.155
	(0.043)	(0.026)	(0.038)	(0.113)	(0.115)
t=0	0.030	-0.015	0.044	-0.090	0.076
	(0.048)	(0.036)	(0.039)	(0.092)	(0.096)
t=+1	0.075**	0.086***	-0.012	0.130*	-0.129*
	(0.036)	(0.032)	(0.030)	(0.078)	(0.075)
t=+2	0.032	0.069**	-0.042*	0.164*	-0.168**
	(0.037)	(0.034)	(0.025)	(0.090)	(0.085)
t=+3	-0.010	0.021	-0.042	0.036	-0.097
	(0.039)	(0.031)	(0.031)	(0.099)	(0.098)
t=+4	-0.048	-0.022	-0.032	-0.044	0.016
	(0.040)	(0.028)	(0.033)	(0.111)	(0.123)
t=+5	-0.044	0.010	-0.065**	0.039	-0.100
	(0.039)	(0.028)	(0.029)	(0.092)	(0.095)
Observations	5,949	5,949	5,949	1,337	1,337
Within R^2	0.002	0.004	0.003	0.013	0.012
Pre-trend p-value	0.472	0.267	0.218	0.118	0.108

Note: The sample requires a recorded incumbent with known recruitment background in both year t and year t-1. It is therefore stricter than the baseline adjacent-record sample. Models include prefecture and year fixed effects. Event time t=-1 is omitted. Columns 1–3 require a recorded prefect in both adjacent years. In columns 2–3, observed years without turnover are coded zero; replacement years with unknown successor recruitment are missing. Columns 4–5 restrict to turnover years with known successor recruitment. Standard errors are clustered by prefecture.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

F Extension: Persistent Military Risk and Personnel Allocation

Table A.28: Incoming Prefect Appointments in Border Provinces

	Bannerman		Exam-linked	
	(1)	(2)	(3)	(4)
Border province	0.089***	0.092***	-0.087***	-0.089***
	(0.030)	(0.029)	(0.028)	(0.027)
Observations	1,476	1,476	1,476	1,476
R^2	0.007	0.083	0.006	0.084
Year fixed effects	No	Yes	No	Yes

Note: The sample consists of new prefect appointments with known recruitment channel. The outcome is whether the incoming prefect is a *bannerman* in columns 1–2 and exam-linked in columns 3–4. Border province is a time-invariant province-level indicator; province and province-year fixed effects would absorb this coefficient. Standard errors clustered by prefecture are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

G Alternative Explanations

G.1 Exceptional-Disaster Placebo

Table A.29: Appointment Outcomes Around Disaster Onsets

		Unconditional		Conditional on turnover	
	(1) Turnover	(2) Bannerman appointed	(3) Exam-linked appointed	(4) Bannerman prefect	(5) Exam-linked prefect
t=-5	0.031	0.013	0.021	0.003	0.021
	(0.025)	(0.017)	(0.022)	(0.058)	(0.060)
t=-4	0.014	0.008	0.013	-0.004	0.013
	(0.023)	(0.018)	(0.019)	(0.061)	(0.061)
t=-3	0.016	0.024	-0.005	0.051	-0.045
	(0.020)	(0.015)	(0.016)	(0.054)	(0.055)
t=-2	0.002	-0.004	-0.004	0.004	-0.014
	(0.019)	(0.012)	(0.013)	(0.047)	(0.047)
t=0	0.023	-0.004	0.026*	-0.048	0.068
	(0.019)	(0.013)	(0.015)	(0.045)	(0.045)
t=+1	0.004	-0.011	0.015	-0.002	0.013
	(0.017)	(0.014)	(0.015)	(0.051)	(0.051)
t=+2	-0.017	-0.013	-0.006	0.001	0.007
	(0.017)	(0.014)	(0.015)	(0.055)	(0.055)
t=+3	-0.005	-0.014	0.006	-0.010	0.026
	(0.020)	(0.015)	(0.016)	(0.059)	(0.060)
t=+4	-0.002	-0.006	0.004	-0.025	0.048
	(0.022)	(0.015)	(0.017)	(0.056)	(0.056)
t=+5	-0.025	0.009	-0.031*	0.111*	-0.099
	(0.021)	(0.015)	(0.017)	(0.063)	(0.064)
Observations	6,662	6,503	6,503	1,474	1,474
Within R^2	0.001	0.001	0.002	0.005	0.006
Pre-trend p-value	0.728	0.512	0.745	0.911	0.902

Note: Turnover equals one when the serving-prefect identity changes between adjacent years with recorded incumbents. The unconditional recruitment outcomes use the same adjacent-record risk set and code observed years without turnover as zero. The placebo anchors event time to disaster onsets. Models include prefecture and year fixed effects. Event time t=-1 is omitted. Columns 1–3 require a recorded prefect in both adjacent years. In columns 2–3, observed years without turnover are coded zero; replacement years with unknown successor recruitment are missing. Columns 4–5 restrict to turnover years with known successor recruitment. Standard errors are clustered by prefecture.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

G.2 Supply Depletion of Exam Officials

Table A.30: Appointment Outsider after Rebellion Onset

	(1) Any outsider	(2) Outsider *bannerman*	(3) Outsider exam-linked
$t = -5$	0.002	0.127	-0.098
	(0.006)	(0.141)	(0.139)
$t = -4$	-0.004	0.367**	-0.339**
	(0.003)	(0.166)	(0.168)
$t = -3$	-0.003	0.090	-0.106
	(0.003)	(0.121)	(0.121)
$t = -2$	0.009	-0.039	0.074
	(0.010)	(0.105)	(0.108)
$t = 0$	-0.022	-0.072	0.041
	(0.020)	(0.081)	(0.089)
$t = +1$	0.002	0.168**	-0.159**
	(0.003)	(0.070)	(0.068)
$t = +2$	-0.002	0.147*	-0.154*
	(0.002)	(0.086)	(0.081)
$t = +3$	-0.003	0.045	-0.092
	(0.002)	(0.087)	(0.086)
$t = +4$	-0.003	0.029	-0.055
	(0.002)	(0.100)	(0.105)
$t = +5$	-0.002	0.038	-0.071
	(0.002)	(0.092)	(0.091)
Observations	1,444	1,444	1,444
Within R^2	0.013	0.013	0.012
Pre-trend p-value	0.775	0.129	0.112

Note: Outsider equals one when the incoming prefect's native province differs from the appointment province. Outcomes condition on new appointments with known recruitment background and known native province. All models include prefecture and year fixed effects, with standard errors clustered by prefecture. The omitted baseline period is $t = -1$. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

G.3 Ethnic Favoritism

Table A.31: Manchu/Mongol and Han Bannerman Office-Holding

	(1) Manchu/Mongol	(2) Han *bannerman*
$t = -5$	-0.005	0.015
	(0.025)	(0.057)
$t = -4$	0.010	0.001
	(0.032)	(0.053)
$t = -3$	-0.005	0.055
	(0.028)	(0.057)
$t = -2$	-0.005	0.031
	(0.026)	(0.054)
$t = 0$	-0.018	0.012
	(0.023)	(0.050)
$t = +1$	0.014	0.048
	(0.026)	(0.046)
$t = +2$	0.021	0.087*
	(0.025)	(0.047)
$t = +3$	0.055*	0.059
	(0.030)	(0.046)
$t = +4$	0.041	0.038
	(0.028)	(0.044)
$t = +5$	0.001	0.062
	(0.025)	(0.042)
Observations	6,241	6,241
Within R^2	0.002	0.002

Note: Outcomes are office-holding indicators for whether the serving prefect in a prefecture-year is a Manchu/Mongol *bannerman* or a Han *bannerman*. These are mutually exclusive subgroups of the broader *bannerman* category. The table reports event-study coefficients relative to rebellion onset. The omitted baseline period is $t = -1$, the final full year before rebellion onset. All models include prefecture and year fixed effects. The sample is restricted to prefecture-years with known recruitment background. Standard errors, clustered by prefecture, are reported in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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