

Too Much But Never Enough: Government Overburden and Backlashes to State-building in Medieval Japan *

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Abstract

Could state-building undermine the state? We argue that rapid expansion of state reach can generate governance demands that overwhelm the government, thereby fueling grievances and unrest. We test this argument using data from Japan under the Kamakura Shogunate (1185–1333), where the Mongol invasions prompted the state to strengthen its control over regions deemed important for national defense. Difference-in-differences analyses show that this expansion triggered rebellions. It also caused long-run state decay, evidenced by the proliferation of private castles after the Shogunate’s collapse. Further analyses indicate that state-building sharply increased disputes brought to the state for arbitration and heightened frustration over unresolved disputes, which in turn fueled rebellion. Our findings highlight the interplay between state reach and administrative competence as different dimensions of state capacity. When the expansion of state reach outpaces the government’s ability to manage the rising governance demands, such over-extension paradoxically erodes the long-term viability of state institutions.

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1 Introduction

A key dimension of state-building concerns efforts to enhance *de facto* control over territories under *de jure* authority (e.g., [Migdal, 1988](#); [Berman, 2010](#); [Garfias, 2018](#)).¹ This paper argues that such efforts could be politically costly due to the accumulation of governance tasks that accompany deeper state presence. As states extend *de facto* control, demands for dispute adjudication and public goods provision often rise sharply. When these demands grow faster than the government can process them, state expansion risks triggering unrest. Because war can exogenously drive state expansion into previously autonomous areas deemed important for national defense, our argument enriches bellicist theories of state-building ([Tilly, 1992](#); [Centeno, 1997](#); [Dincecco and Wang, 2018](#)). It shows that war can both make and unmake the state by inducing premature state-building, as states take on expanding governance tasks before administrative competence ([Weber, 1978](#); [Evans and Rauch, 1999](#); [Skocpol, 2014](#)) is in place, ultimately raising the risk of unrest and state collapse.

Empirically, we examine the consequences of an ambitious state-building program launched by the Kamakura Shogunate in Japan (1185 – 1333 CE). This reform, triggered by the exogenous shock of Mongol invasions in the late 13th century, rapidly deepened the Shogunate’s control over regions critical to national defense that had previously remained under indirect rule. We geocode information from a diverse array of historical and archaeological sources. Our dataset includes 445 rebellions, 7,138 castles, 4,922 manors, and the family backgrounds of 325 governors and 3,255 local lords.

Using a difference-in-differences (DD) strategy, our analysis shows that provinces targeted by the Shogunate’s state-building program, implemented in response to the Mongol invasions,

¹Throughout this paper, we use phrases such as “state/territorial reach” and “state/territorial penetration” interchangeably, referring to the extension, expansion, or deepening of state control that characterizes the empirical case examined here.

experienced a pronounced, sustained surge in rebellions against the Shogunate relative to untargeted regions. This finding is robust to an extensive battery of checks and tests.

We then empirically document that this initial state expansion led to long-term state *decay*. We use the presence of privately owned castles as an indicator of state absence. These castles enabled local elites to resist state intervention and assume security functions that would otherwise be provided by the state. Our DD analysis reveals a marked rise in newly constructed castles in the century following the collapse of the Kamakura Shogunate in provinces that had initially undergone state-building in response to the Mongol invasions.

We investigate government overburden as a key mechanism behind the political backlash against the ruler triggered by state-building efforts. First, our DD estimates show that the Shogunate's state-building program significantly increased the governance burden in treated provinces, as reflected in a sharp rise in local disputes reported to the Shogunate for arbitration. These disputes, in turn, contributed to a higher incidence of rebellion.

Second, as further evidence of government overburden, we examine politically charged legal labeling. Our findings indicate that the frequency of *Akuto* cases, in which litigants strategically accused their opponents of anti-Shogunate activities to compel state intervention, increased following the Shogunate's state-building program. Further analysis confirms the strategic nature of *Akuto* labeling: the appointment of provincial governors from the Shogunate's core ruling coalition mediated the increase in such cases. These patterns suggest that disputants, frustrated by the state's inability to effectively resolve conflicts, increasingly appealed to these governors who were perceived as direct agents of the Shogunate, by framing their opponents as anti-Shogunate actors, prompting more forceful state intervention. These results thus reflect increasing frustration over the lack of effective governance from an overburdened government.

Third, we conduct heterogeneity analyses to further explore the role of government overburden. We find that the positive effect of state-building on *Akuto* cases was more pro-

nounced in provinces that already faced greater governance burdens. In these settings, additional governance demands further strained an already overextended state apparatus, leading to more acute dissatisfaction. Furthermore, we find that the positive effect of state-building on rebellions was mitigated when the Shogunate addressed government overburden through investments in administrative capacity.

Finally, we complement our results with detailed qualitative cases that illustrate how the deepening of state penetration heightened governance demands, how the Shogunate’s minimal bureaucracy remained inadequate to handle the growing burden of dispute resolution, and how the failure to govern effectively in light of overburden ultimately led to unrest.

We also adjudicate among several other explanations for our main results. We show that the findings are unlikely to be driven by the burdens of war preparation or the Shogunate’s extraction *per se*. We also show that rebellions induced by state-building were led primarily by commoners rather than warriors. These analyses challenge the traditional—and now obsolete—narrative among historians that attributes the collapse of the Kamakura Shogunate to insufficient compensation for warriors after the Mongol wars, which allegedly fueled their discontent (e.g., [Nam, 1990](#)). However, we do find evidence that state-building generated more rebellions in localities with lower initial levels of state penetration, consistent with findings in [Albertus and Gay \(Forthcoming\)](#) on eighteenth-century France.

Theoretical Contributions

These findings advance our understanding of state-building in several ways. The Japanese experience in the shadow of Mongol invasions sheds new light on the relationship between war and state development ([Tilly, 1992](#); [Centeno, 1997](#); [Dincecco and Wang, 2018](#)). Its trajectory—from war-driven expansion to heightened unrest and eventual regime collapse—demonstrates how war can both “make” and “unmake” the state within the same polity over time. We

interpret this case as part of a broader pattern of premature state-building, a framework that also encompasses [Queralt \(2022\)](#). Whereas [Queralt \(2022\)](#) shows how war-induced reliance on external finance at an nascent stage of development undermines state capacity, we foreground a parallel mechanism: rapid territorial penetration driven by the demands of war preparation can leave the state overburdened with new governance responsibilities.

We also contribute to the budding literature on backlashes to state-building ([Garfias and Sellars, 2022b](#); [Albertus and Gay, Forthcoming](#)). Prior research focuses on the potential of state-building to provoke local elites ([Chiovelli et al., 2024](#)), reduce elites’ willingness to suppress dissent ([Garfias and Sellars, 2022b](#)), and erode the cohesion of local security forces ([Chen and Wang, 2024](#)). We uncover a complementary pathway: the state’s inability to meet the governance demands generated by the deepening of its own territorial reach fuels rebellion. This insight aligns with [Finkel and Gehlbach \(2020\)](#)’s argument that reforms under weak states breed grievances through unfulfilled expectations. Whereas their work studies backlash arising from reforms delegated to the local elites for implementation, we show that even efforts implemented directly by central agents can provoke rebellion when the associated increase in governance demands overburdens the state and frustrates its subjects.

Our conceptual contribution is highlighting the intertwined nature of different dimensions of state-building. Following [Mazzuca \(2021\)](#), we distinguish between how much territory the state effectively controls, or state reach (e.g., [Garfias and Sellars, 2021b](#); [Garfias and Sellars, 2022a](#); [Chen and Wang, 2024](#)), and how well it governs that territory, or administrative competence (see [Kohli, 1994](#); [Evans and Rauch, 1999](#); [Wang and Xu, 2018](#); [Vogler, 2019](#); [Gjerløw et al., 2022](#)).² We examine how weaknesses in one dimension condition the consequences of expansion along the other. Our findings show that territorial penetration without commensurate administrative capacity can be destabilizing, trapping weak states in an equi-

²[Mazzuca \(2021\)](#) refers to territorial control as state “formation” and administrative capacity as state “building.”

librium of both beleaguered presence in the localities and administrative paucity at the center across historical and contemporary contexts ([Herbst, 2000](#); [Centeno, 2002](#); [Lieberman, 2003](#); [Acemoglu et al., 2016](#); [Berman, 2010](#); [Guardado, 2018](#); [Finkel and Gehlbach, 2020](#)).

2 State-building, Government Overburden, and Unrest

This section develops an argument about how the rapid deepening of state reach into territories previously outside the state’s *de facto* control can generate grievances and unrest due to government overburden. Throughout the paper, we use “state-building”—alongside terms such as the “extension of state control,” “state expansion or penetration,” and the “deepening of state reach”—interchangeably to refer to the expansion of a state’s *de facto* control over territory. This usage is consistent with recent literature (e.g., [Garfias and Sellars, 2021b](#); [Garfias and Sellars, 2022b](#); [Chen, Wang and Zhang, 2025](#)). Underlying these formulations is a shared conception of territorial reach as the state’s “infrastructural power,” its ability to penetrate territory and project authority over distance (e.g., [Mann, 1993](#); [Herbst, 2000](#)).³

When territorial reach deepens rapidly, the state risks becoming overextended, as governance demands outpace its capacity to respond. Such overextension often manifests as bureaucratic “overburdening” and governance failure, patterns documented in both advanced democracies and developing contexts ([Pressman and Wildavsky, 1973](#); [Fernández-i Marín et al., 2024](#); [Dasgupta and Kapur, 2020](#)). In principle, these pressures can be alleviated through commensurate improvements in administrative capacity, understood as the organizational ability of the state to provide public goods and resolve disputes in territories under

³For recent empirical work linking state penetration to enhanced fiscal capacity and the imposition of direct rule, see [Chen, Wang and Zhang \(2025\)](#) and [Chiovelli et al. \(2024\)](#).

its *de facto* control.⁴ In practice, however, such improvements tend to be slow (Besley et al., 2022) and contingent on fortuitous political circumstances (Geddes, 1994; Grzymala-Busse, 2007). Among the governance tasks intensified by the expansion of territorial reach, dispute resolution is especially demanding and politically consequential.

Dispute Resolution as a Core State Function

Dispute resolution is a particularly daunting governance task because it places sustained organizational demands on the state to deliver predictable and legitimate outcomes (Besley and Persson, 2009). Historically, state authority has been closely tied to the promise of adjudicating local conflicts through fair and uniform procedures (Moore, 1978; Boucoyannis, 2015).⁵ This logic also persists across modern contexts: armed groups treat justice provision as central to political legitimacy (Arjona, 2016; Grasse, Sexton and Wright, 2024; De Bruin et al., 2025), while governments in developing countries institutionalize dispute resolution mechanisms to strengthen authority (Onoma, 2009). That “control of justice means control of people” (Boucoyannis, 2015, p. 4) remains an enduring political principle.

Robust administrative and personnel infrastructures are essential for effective adjudication (Yang, 2004; Wang, 2013; Loyle, 2021). As Justesen and Bjørnskov (2014) argues, weak

⁴Our usage of administrative capacity is deliberately narrower than a general catalogue of state investments: it excludes instruments of socialization such as mass primary education (which may promote order through preference formation, see Paglayan, 2022, 2024) and a state’s territorial *reach* (which we treat as a distinct dimension and the focus of the state-building case in this paper). Instead, it focuses on competent and disciplined personnel and routinized procedures that limit arbitrariness and corruption, enabling authority to translate into reliable governance outputs (Weber, 1978; Evans and Rauch, 1999; Yang, 2004). This understanding also distinguishes our notion of administrative capacity from fiscal capacity, which is typically accompanied by an expansion of central authority and thus also pertains to state reach (Ko and Sng, 2013). Similarly, fiscal legibility and mobilization capacity, both of which reflect the ability of central authorities to monitor, control, and extract resources from local elites, also relate more directly to state territorial reach (Garfias and Sellars, 2021a; Yin, 2020). The manifestation of administrative capacity in our medieval Japan case aligns closely with the notion of legal capacity, which encompasses building arbitration systems, training judges, and registering property, advanced by Besley and Persson (2009), who also treats legal and fiscal capacities as analytically distinct dimensions of state capacities.

⁵Both European (Boucoyannis, 2015; Simpson, 2021) and Chinese (Hui, 2005) monarchs understood judicial quality as a cornerstone of political legitimacy.

states that extend legal authority into peripheral areas are often constrained by corruption, shortages of competent personnel, and entrenched local dispute-settlement norms. Such failures are politically consequential: unmet expectations of justice and fairness frequently spill over into dissatisfaction with broader state institutions (e.g., [Revkin, Alrababah and Myrick, 2023](#); [Feddersen et al., 2024](#)).

Unmet Demands and Increased Unrest

As the state increases *de facto* control over territories, new expectations and responsibilities emerge. When these expectations—whether in dispute resolution or broader governance—go unmet due to government overburden, grievances may intensify and spark rebellion. This connection aligns with theories of relative deprivation and insurgency (e.g., [Gurr, 1970](#); [Wood, 2003](#)). Expanded state presence generates socially negotiated expectations, a dynamic supported by microfoundational insights from behavioral research (see [Finkel and Gehlbach, 2020](#); [Finkel, Gehlbach and Olsen, 2015](#)). Empirically, retrospective blame toward rulers rises where the state’s role expands ([Alkon and Wang, 2018](#)), and insufficient responsiveness is associated with higher dissatisfaction ([Nie and Wang, 2023](#)). In short, when state penetration outpaces administrative capacity, unmet governance demands can translate into unrest.

External War and Why Weak States Expand Nonetheless

The foregoing dynamics underscore the close relationship between state penetration and government overburden in state-building processes. For this reason, states generally *do not* seek to extend control beyond their core territories, leaving peripheral regions under indirect rule when insufficient administrative capacity would make additional governance demands politically risky ([Bruce and Knox, 2009](#)). Rulers instead favor indirect rule or minimal interference, recognizing that full absorption would require bureaucratic investments beyond

their means.⁶ Some strands of development policy have likewise advocated a minimalist, “mediated state” model in settings where administrative capacity remains weak (Menkhaus, 2007). Historically, weak states have typically exhibited deficiencies in both territorial reach and bureaucratization (e.g. Lieberman, 2003; Harl, 2023), making it unsurprising that bureaucratic development often preceded successful centralization (e.g., Duncan, 2000; Heather, 2018; Chen, Wang and Zhang, 2025; Wang and Yang, 2025).

However, this equilibrium can shift when an exogenous increase in the *strategic imperative* of certain regions prompts rulers to assert and deepen control despite the risks. Security threats or broader geopolitical transitions can elevate the importance of particular regions and compel rulers to prioritize expansion, even when administrative foundations remain fragile (Paik and Vechbanyongratana, 2019). As war pressures intensify, regions critical for national defense may be brought under direct control to mobilize manpower and resources. In such cases, states may deepen territorial reach despite limited administrative competence, a choice that can trigger unrest and rebellion, undermining political order. Our framework thus illustrates one pathway in which war can both make and *unmake* the state.

State decay We use state decay to describe a process in which state presence recedes in localities while non-state forms of governance proliferate. Although our argument does not imply that premature state expansion necessarily leads to state decay, decay can occur if heightened rebellion as a result of that expansion contributes to regime collapse without the emergence of a replacement central authority. In such cases, local state agents, already strained by earlier unrest, face growing difficulties maintaining order in the absence of external support. Over time, this erosion of effective authority can displace local state institutions and give rise to the privatization of security by local actors, reflecting a contraction of statehood.

⁶A well-known feature of nomadic empires, where a centralized bureaucracy was minimalist at best, was the practice of indirect rule in conquered territories (Harl, 2023).

Connections with Existing Accounts of Rebellions Induced by State-building

Our framework is intended to be complementary rather than exclusive. It connects with existing accounts that link state-building to rebellion through elite dissatisfaction or peasant discontent, often driven by disruptive fiscal extraction (e.g., [Garfias and Sellars, 2022b](#); [Chiovelli et al., 2024](#); [Albertus and Gay, Forthcoming](#)). While our mechanism does not posit rebellion as a direct response to extraction *per se*, it is possible that multiple mechanisms operate simultaneously in empirical settings.⁷ Furthermore, extraction in our framework may exacerbate elite–peasant tensions by tightening local resource constraints. Such tensions can then surface as an increased volume of disputes brought before the state, thereby raising governance demands and contributing to administrative overburdening.

3 State-building in Kamakura Japan

Japan under the Kamakura Shogunate (1185 - 1333 CE) is an ideal setting to test our argument, as the Mongol invasions provided the exogenous shock compelling the center to expand control over localities. The Shogunate was composed of a samurai warrior class, with its nominal chief referred to as the “Shogun.” The capital of the Shogunate was Kamakura, eastern Japan, near modern-day Tokyo. From 1221 onward, the Hojo clan took over the Shogunate, with its leaders serving as regents for the Shoguns. Meanwhile, the “Emperor” and the imperial court resided in Kyoto and wielded influence over the governance of territories in western Japan (that is, to the west of Kyoto), though the Shogunate acted as the sovereign authority for the whole country.⁸ The Hojos also played a pivotal role in the succession of emperors. Throughout this paper, we use “central government,” “Hojo

⁷Section 6.2 empirically evaluate the contributions of additional mechanisms.

⁸When the Mongols threatened Japan in the late 13th century, it was the Shogunate, not the emperor, that took charge of handling diplomatic communications, directing geostrategic maneuvering, and conducting military preparations, affirming its role as the *de facto* sovereign (e.g. [Ishii, 1990](#); [Choi and Yi, 2021](#)).

clan,” and “Shogunate” interchangeably, as the Hojo was the *de facto* ruler of Japan and the Kamakura Shogunate the *de facto* central government.

Limited State Control Before the Mongol Invasions

Before the Mongol invasions, the Shogunate exercised only limited control over Japan. The country was structured around a feudalistic manor system, resembling the decentralized landholding arrangements of medieval Europe (Blaydes and Chaney, 2013). Unlike China, where a sophisticated bureaucracy collected taxes relatively efficiently (Zhao, 2015),⁹ the Kamakura Shogunate lacked a centralized fiscal apparatus.¹⁰

Politically, the Shogunate’s control remained geographically constrained. It exercised direct control over territories east of Kyoto, particularly those near Kamakura. In the west, however, jurisdiction over many administrative matters was shared with the imperial court in Kyoto (Amino, 1997).¹¹ Guided by these principles, disputes over manor ownership or boundaries in western Japan could be lodged to either the imperial court in Kyoto or the Shogunate in Kamakura for adjudication (Mass, 1979). In other words, the Shogunate’s direct rule was mainly confined to the eastern territories, while its rule in the west was largely indirect. However, in matters of national defense, internal security (e.g., suppressing local rebellions), and foreign relations, the Shogunate exercised *de facto* sovereign authority. In these domains, it was the primary governing actor, a role that persisted despite the continued existence of the imperial court (Yamamura, 1991, p. 416).

⁹But see Wang (2022) for variation in bureaucratic capacity *within* Chinese history.

¹⁰Japan was home to approximately 6,000 manors during the Kamakura period. Most of these estates were exempt from taxation, as they were owned by members of the imperial family, aristocrats, or religious institutions. Since manors were the economic foundation of early medieval Japan, disputes over ownership and boundaries were frequent (Ishii, 1990).

¹¹*Goseibai Shikimoku*, the “constitution” of the Kamakura Shogunate promulgated in 1232, began with a series of articles explicitly limiting its power in the west (Ishii, 2004, pp. 435-436).

Weak Administration Throughout the Kamakura Period

The Shogunate governed with a rudimentary administrative apparatus, owing to the persistent absence of a professional bureaucracy.¹² This becomes evident when comparing Kamakura Japan to other historical polities that maintained professional bureaucracies characterized by separate spheres of responsibility, hierarchical organization, and multi-tiered subnational governance. In such systems, lower administrative levels were accountable to higher ones, with subnational governors appointed by the center. These bureaucracies also featured efforts to separate civilian and military functions, along with career mobility for civilian officials through promotions, demotions, and rotations across localities. While this framework was weaker than the Weberian ideals,¹³ scholars have documented its presence in Europe (Heather, 2018), Egypt (Petry, 2014; Stasavage, 2020), the Iranian Plateau (Blaydes, 2017), and Anatolia (Blaydes, 2017; Stasavage, 2020), albeit intermittently. By contrast, premodern Japan lacked comparable systems from the 11th century onward.

The contrast is clearest in comparison with Japan’s East Asian neighbors. Korea and Vietnam, like Japan, borrowed from Chinese administrative models, but unlike Japan, they sustained Chinese-style centralized bureaucracies over long periods, including civil service examination systems that were ostensibly meritocratic and open to most adult males. Japan’s own attempts proved far less durable. By the 10th century, bureaucratic institutions initially modeled on Tang China had largely receded into “virtual irrelevance” (Holcombe, 2017, p.

¹²See Ishii (1990) for a discussion on the primitive nature of the institutions enabling warrior rule in the Kamakura Period, and Sato (1993) for a discussion on the personalistic nature of the Shogunate’s judicial system. There was also *zero* mention of the word “bureaucracy” in the three chapters covering the Kamakura period in *The Cambridge History of Japan* (1990). Note that there is also a line of historical scholarship on the Kamakura period that emphasizes the Shogunate’s efforts to strengthen its administrative capacity (Mass, 1979; Steenstrup, 1996). Our results in Table 5 are consistent with this interpretation. From a *comparative* perspective, however, this section explains why the Shogunate’s administration should still be characterized as minimal using other historical polities as reference points. Section 7 further draws on qualitative evidence to show that the Shogunate’s attempts to enhance administration failed to keep pace with the rapid growth of governance demands.

¹³See the discussion in Vogler (2022) on the “proto-modern” nature of said bureaucracies that fall short of the modern archetype.

120). During the Kamakura Period, the Shogunate governed its territories with hereditary warrior governors (*shugo*) and estate stewards (*jito*), who exercised substantial autonomy without expectations of rotation or demotion (Ishii, 1990). Japan’s enduring lack of a professional bureaucracy—let alone civil–military separation or a hierarchical administrative system—thus followed a trajectory “without parallel in China” (Holcombe, 2017, p. 120).

State Expansion in Response to the Mongols: the *Keigo* Policy

The Mongol invasions marked a decisive shift in the territorial reach of the Kamakura Shogunate. In 1268, a diplomatic envoy sent by Kublai Khan arrived in Japan with a letter demanding the country’s submission. From the moment the document reached Kamakura, it was treated as a declaration of war (*The Cambridge History of Japan*, 1990, p. 416). Rejecting the Mongols’ demand, the Shogunate immediately began war preparations by adopting a series of state-building measures aimed at strengthening its extractive capacity and *de facto* control. Because military planners anticipated that the Mongols would cross the strait from Korea, initial attacks were expected to fall on Japan’s western provinces.

***Keigo* Policy** The prolonged Mongol threat thus prompted a set of initiatives designed to extend the Shogunate’s control over western Japan, most notably the *keigo banyaku* (hereafter the “*Keigo* Policy”). Imposed on 23 western provinces, the policy represented a major transformation in governance (Kuroda, 2004, pp. 87–88). Before its implementation, the Shogunate could mobilize only its own vassals, the *gokenin*, who were scattered across the country, including parts of western Japan (Hosokawa, 2011, p. 22). The *Keigo* Policy extended military and labor obligations to local elites outside the *gokenin* class, including manor managers and non-*gokenin* warriors, who constituted the majority of samurai in the west. It also authorized the direct collection of rice taxes from manors and imposed levies on imperial court subordinates and wealthy landholders who had previously remained beyond

the Shogunate’s reach ([Amino, 2001](#), p. 286).¹⁴ These measures substantially enhanced the Shogunate’s extractive and mobilizational capacity. The 23 provinces covered by the *Keigo* Policy therefore constitute the treated group in our analysis ([Aida, 1958](#), pp. 335–336).¹⁵

The Mongols launched two invasion attempts, in 1274 and 1281. In both cases, the Shogunate mobilized more than 100,000 troops and successfully defeated the Mongols at Hakata Bay, aided by typhoons that devastated the invading fleets. Nevertheless, the threat of a renewed invasion persisted until the 1320s, near the end of the Kamakura Shogunate ([Hioki, 2007b](#), p. 141).

The historical consensus Historians overwhelmingly agree that the *Keigo* Policy fundamentally strengthened the Kamakura Shogunate’s control over the targeted provinces. As Amino Yoshihiko argues, “In order to respond to the pressure [of the Mongols], the Eastern state [the Kamakura Shogunate] had no choice but to expand its dominion...eventually to the entirety of the Western region” ([Amino, 1998](#), pp. 226-227). Hori Kyosuke further emphasizes the long-term consequences of these changes, noting that “in the wake of the invasions, the imperial court had lost ground once again. Rule by warriors [the Shogunate] over the country had taken another step forward” ([Hori, 1988](#), p.198). Similarly, Li Narangoa highlights the centralizing effects of the Mongol threat, stating that “the invasions strengthened the centralized power of the shogunate or military government. In the name of war against foreign invaders, the shogunate was able to issue laws covering areas that were traditionally outside its jurisdiction” ([Narangoa, 2022](#), p.910). These accounts collectively highlight that the Shogunate responded decisively to the Mongol threat by undertaking substantial

¹⁴Tax rates on these manors did not increase despite the expanded tax base, consistent with our finding that the policy did not provoke elite-led rebellion.

¹⁵Although the initial number of targeted provinces was smaller, it soon expanded to 23. We therefore do not treat adoption as staggered in our difference-in-differences design; results are robust to a staggered specification (Appendix Figure [A1](#)).

state-building efforts, which not only addressed the immediate danger but also effectively extended the Shogunate’s control over many provinces in western Japan.

Increased Government Burden

The Kamakura Shogunate governed with a rudimentary administrative apparatus. Its judicial system, for example, lacked centralized record-keeping, enabling litigants to exploit procedural loopholes through “duplicitous suits,” filing the same case multiple times to game the system and drain limited administrative resources (Bovbjerg, 2018, p. 95). The court also faced a stream of speculative litigation, as losing carried few penalties. As a result, even attempts to impose time limits for major cases faltered in the face of a growing backlog (Bovbjerg, 2018, pp. 129-131).

The implementation of the *Keigo* Policy further worsened the scale of governance demands confronting the Shogunate. By expanding *de facto* control into western provinces, the Policy brought a larger set of actors and conflicts within the Shogunate’s purview. One consequence was a dramatic surge in disputes involving contested land rights and resource distribution, directly reflecting intensified governance demands in areas newly subject to Shogunal jurisdiction (Seno, 1958). In our data, the average annual number of disputes lodged to the Shogunate for resolution more than tripled after 1268.¹⁶

In response to these rising demands, the Shogunate established the *Chinzei Tandai*, a new bureau combining administrative, judicial, and military functions based in Kyushu (Narangoa, 2022, p. 910). Led by a core member of the Hojo clan and staffed with local lords serving as judges, the bureau’s central task was to handle disputes from regions recently brought under direct Shogunal control via the *Keigo* Policy.

¹⁶Table 3 of Section 6.1 presents more systematic evidence that *Keigo* state-building substantially increased the number of disputes.

Even with this enlargement, however, the Shogunate’s administration continued to be overwhelmed by the rapid growth in its caseload.¹⁷ The venue remained understaffed, and its arbitrators were not professional officials but warriors who also managed their own domains. Lacking legal expertise, they often relied on formulaic and superficial judgments that prioritized expediency over careful attention to complex landholding realities, generating grievances among those who lost arbitration. Moreover, rulings frequently proved difficult to enforce in the face of local resistance, undermining the credibility of Shogunal authority (Kawazoe, 1963). Finally, because arbitrators were embedded in local power networks, conflicts of interest were pervasive, and proceedings could be manipulated to favor allies or kin, contributing to perceptions of partiality and private gain (Kawazoe, 1973).

***Akuto* cases** The Shogunate’s government overburden in dispute resolution is clearly illustrated by its handling of *Akuto* cases. Literally translated as “evil bands” or “bandits,” the *Akuto* label was frequently applied by litigants in land disputes to describe local adversaries. Plaintiffs used the label as a legal strategy to compel the Shogunate to intervene more forcefully against their rivals (Sherer, 2018). After 1268, the increasing prevalence of *Akuto* designations coincided with a sharp rise in disputes over manor ownership and revenue distribution. As routine adjudication became slower and less effective, litigants increasingly reframed economic conflicts as “evil” or anti-Shogunate activities, anticipating that such categorization would elicit swifter and harsher responses from the Shogunate (Negayama, 2011; Oxenboell, 2018, p. 64). In this sense, the frequency of *Akuto* records serves as an indicator of excessive frustration over the failure of governance under an overburdened state.

¹⁷Columns 3 & 4 of Table 5 in Section 6.1 show that this investment in administrative capacity did help mitigate the effect of the state-building program on rebellion.

4 Data and Identification Strategy

4.1 Outcome Variables

Rebellions The main outcome of interest is rebellion. The accounts of rebellious events are collected from the *New Major Chronological Table of Japanese History* (Hioki, 2007a,b), the most comprehensive chronology available that records economic, political, and cultural events in Japanese history, which has been used by political scientists (Anderson, 2023). We focus on every rebellion in the time span of 1221-1333—the rule of Kamakura Shogunate became stabilized after the Jokyu War in 1221 and was overthrown by rebels in 1333.

Any violent incidents criminalized as challenging the regime are categorized as a rebellion, including elite insurrections, peasant revolts, religious uprisings, and mass protests. In total, there were 445 rebellions across Japan in the period under study.

Disputes and *Akuto* We examine the mechanisms connecting state expansion to backlashes by analyzing the increases in reported disputes and the role of *Akuto*. Our argument expects the volume of disputes arbitrated by the Shogunate’s agencies to rise as a reflection of increased governance burden following the expansion of the state’s territorial reach.

As discussed earlier, *Akuto* served as a legal label strategically employed by litigants to seek attention and demand the Shogunate’s intervention. It is another sign of heightened governance pressures and could potentially reflect initial state failures in dispute resolution, as aggrieved parties now had to weaponize criminalizing rhetoric to compel official action. Similar to the rebellion data, the records of disputes, along with information on *Akuto*, are drawn from the *New Major Chronological Table of Japanese History* (Hioki, 2007a,b).

Castles A long-term implication from our theory is that efforts initially strengthening state presence could eventually cause state decay if the rebellions due to government overburden

led to regime collapse. We measure state decay via privately owned castles. The existence of such castles helps preserve the autonomy of local elites and often presents a credible deterrent against central authority. Thus, areas with greater state penetration tend to have fewer castles, a pattern observed in contexts such as imperial China (e.g., [Zhang, 2021](#); [Chen, Wang and Zhang, 2025](#)) and medieval Europe (e.g., [Dimick, 2015](#)). These castles serve an analogous purpose to defending local autonomy in Japan ([Ferejohn and Rosenbluth, 2010](#)).¹⁸ It is no surprise that, shortly after Tokugawa Ieyasu unified Japan in the early 17th century, he issued the One Castle per Province Command (*ikkoku ichijo rei*) to consolidate the newly established Tokugawa Shogunate, demolishing all but one castle in every domain and reducing the total number of castles from over 10,000 to merely 401.

The information on castles is collected from [Nara National Research Institute for Cultural Properties \(1996\)](#), a database containing over 480,000 archaeological sites of Japan for all periods. We select sites classified as *jokan*, which literally means large buildings combining castles and residency. These facilities were constructed to provide security for local warriors and their subjects, and were thus heavily fortified. The *jokan* category thus excludes the military strongholds built against the Mongols and similar facilities constructed by the Shogunate government. Since the exact construction year was not available for most castles, but only the era, we adopt era as the time period in the panel data. We use the number of *newly* constructed castles in each era as the outcome.¹⁹ To examine the long-run effect of *Keigo* state-building on state decay, we *exclude* the late Kamakura Period (1268-1333) (the post-Mongol era within the Kamakura Period) but include the Nanboku-cho and pre-

¹⁸While some castles were possibly constructed to defend against nearby competitors rather than directly targeting central authorities, these castles still exemplified the “privatization of security” resulting from state decay ([Ferejohn and Rosenbluth, 2010](#)).

¹⁹The eras covered in our sample include (1) Asuka, Hakuho, and Nara Periods (592-794 CE), (2) Heian Period (794-1185 CE), (3) early Kamakura Period (1185-1267 CE), (4) late Kamakura Period (1268-1333 CE), (5) Nanboku-cho Period (1334-1392 CE), and (6) the part of Muromachi Period (1392-1573 CE) before 1467 (as the Sengoku (“Warring States”) Era after 1467 was when central authority completely collapsed). Castles constructed in era (1)-(3) are regarded as pre-treatment outcomes, and those constructed in eras (5) & (6) are regarded as post-treatment outcomes.

Sengoku Muromachi Periods (1334-1467) after the demise of Kamakura Shogunate. There were 1,892 castles built from the 7th century to year 1267 and 5,246 constructed during 1334-1467, the geographical distribution of which are presented in Figure 1.

4.2 Other Data

4.2.1 Controls

We also control for geographic and economic factors. We construct a binary, time-varying measure of *natural disaster* for each province from chronological records in Hioki (2007a,b). We also control for provincial economic productivity by computing the *rice suitability index*, derived from Fischer et al. (2012) (Mitchell and Yin, 2022). We additionally control for the density of archaeological sites with remains of *coins* prior to the Mongol invasion, obtained from Nara National Research Institute for Cultural Properties (1996). This control is important because the influx of coins from China reflects trade acceleration—a process argued to have triggered wealth inequality and grievances among warriors (Yamamura, 1991). Another geographic control is a binary indicator of coastal status, given that coastal provinces faced greater exposure to Mongol threats and China trade.

We use two variables to further ensure that the observed treatment effect reflects the impact of *Keigo* on the targeted provinces *per se*, rather than any generic differences rooted in Japan’s East-West divide—a culturally salient division well-documented in historical scholarship (Amino, 1998). First, we construct *East Japan*, a binary variable coded as 1 for provinces east of Sekigahara (a geographic and cultural boundary demarcating eastern and western Japan). Second, we control for *distance to Yamashiro* (Kyoto), the imperial capital, to account for residual power dynamics between the eastern-based Shogunate and the western-oriented Emperor.

4.2.2 Governors

When further examining the relationship between state-building efforts and the rise of *Akuto* (Section 6.1), we also investigate the role of governors (*shugo*) who belonged to the Hojo ruling coalition. We compile a list of provincial governors for the period 1221–1333 and determine Hojo affiliations based on [Hojo-shi Kenkyukai \(2019\)](#). A governor is classified as a member of the Hojo coalition if he (1) was the *tokuso* himself, the clan leader of the Hojos, (2) was an *ichimon* of the Hojo family,²⁰ or (3) had a *tokuso* as his guardian parent (*eboshi-oya*), receiving a character from the *tokuso* for his name—a practice signifying a strong bond between the governor’s clan and the Hojos. There were 325 provincial governors during 1221–1333. In 1247, only 30% of these governors were from the Hojo coalition. This percentage increased to 62% following the Mongol invasions, and the expansion concentrated in provinces where the *Keigo* Policy was implemented (Appendix Figure A7).

4.3 Identification Strategy

We apply a non-staggered difference-in-differences (DD) strategy to assess the impact of state-building on rebellions against the Shogunate, estimating the following model at the province-year level:

$$Y_{pt} = \beta \text{Keigo}_p \times \text{Mongol}_t + \phi_p + \gamma_t + \epsilon_{pt} \quad (4.1)$$

Y_{pt} takes 1 if at least one rebellion occurred in province p and year t and 0 otherwise.²¹

State-building treatment *Keigo* takes 1 for the twenty three provinces subject to the *Keigo* Policy. As discussed in Section 3, the Shogunate greatly enhanced its capacity to extract from and mobilize the local population in preparation for defense against the Mongols

²⁰This category excludes *ichimon* who competed with the *tokuso* for the leadership of Hojo, such as the *nagoe* clan.

²¹Results are consistent using the number of rebellions (Appendix Table A2).

via this state-building policy. *Mongol* takes 1 for year $t \geq 1268$, in which the war preparation began. The interaction between the two variables thus forms the state-building treatment, and the two constituents are also included in the regression separately but their coefficients are absorbed by the province and year fixed effects in ϕ_p and γ_t . We also include the control variables described in Section 4.2.1. If a control is time-invariant, in all specifications we interact it with year dummies to allow its effect to vary over time.

The external threat posed by the Mongols was entirely unforeseen by the Shogunate. The term “Mongol” first appeared in the Japanese records in November 1267. As Kuroda (2004) observes, “from the perspective of the international situation at the time, the fact that the term ‘Mongol’ was mentioned for the first time only at the last moment seems far too late. At that time, Japan appeared to have been extremely ignorant of international affairs” (p.70). This lack of awareness about the latest developments in East Asia can be attributed to Japan’s long-standing cessation of official relations with the continent, which had persisted for centuries (Yamamura, 1991, p.416). Therefore, the Mongol threat represented a truly unexpected shock to the Shogunate, making the state-building treatment exogenous in our analysis. They responded to the external threat with the *Keigo* Policy that remained in effect until the fall of the Kamakura Shogunate, as they had constantly been anticipating another Mongol invasion, making the Policy a long-lasting state-building effort.

5 Main Results

This section reports the main finding in support of the central prediction from our theory: the Shogunate’s premature state-building efforts in the name of national defense led to revolts against it. Figure 2 visualizes the pattern from the raw data. The blue area covers the provinces that would be treated by the *Keigo* Policy from 1268 on, while the red circles denote the number of rebellions. The comparison between the pre-treatment rebellions in the

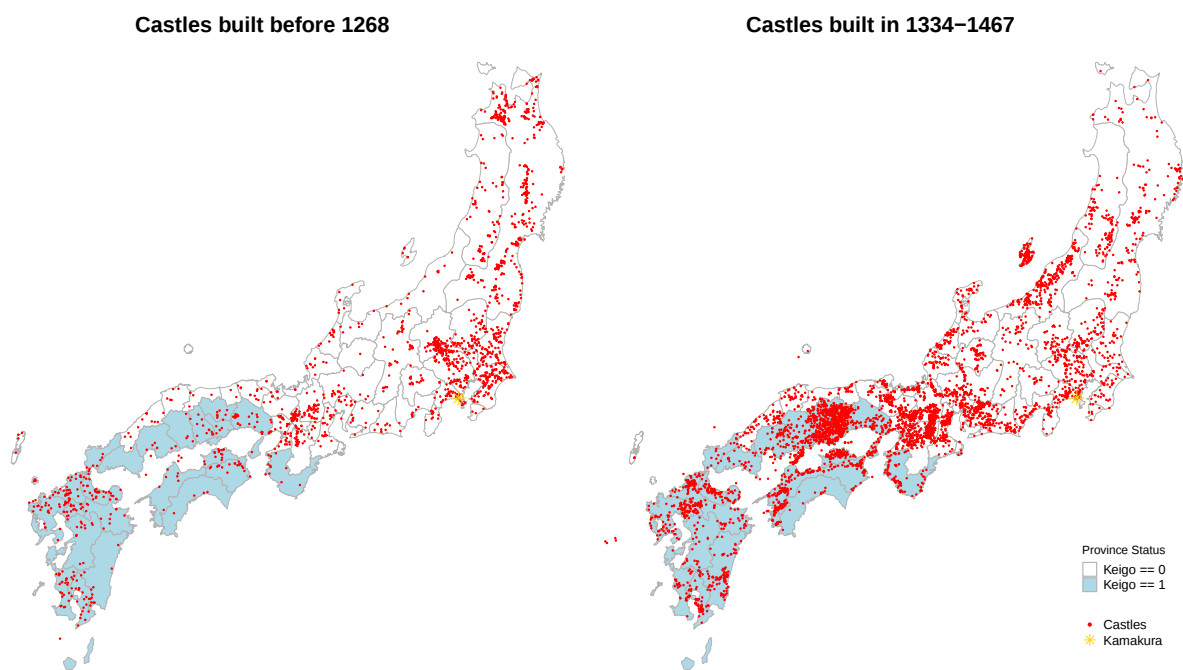


Figure 1: Location of Castles before Mongol Invasion and after the Collapse of the Kamakura Shogunate

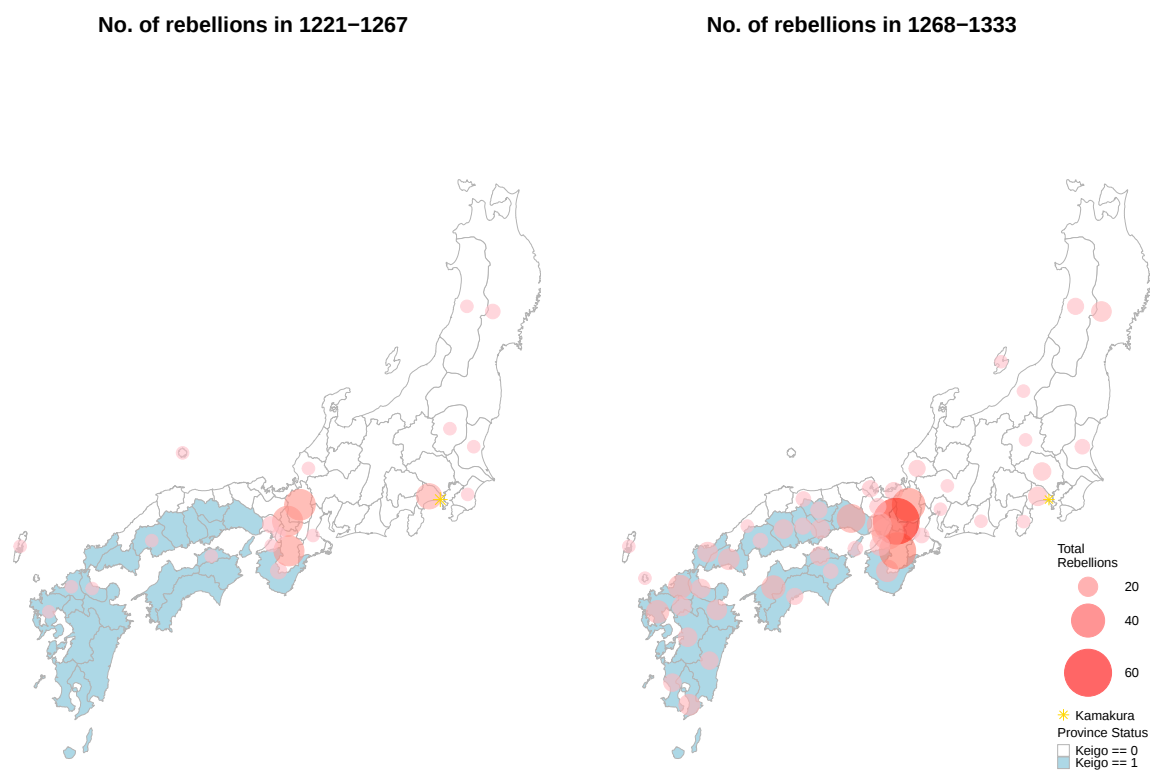


Figure 2: Number of Rebellions before and after Mongol Invasion

left panel and the post-treatment rebellions in the right panel lends support for our argument: the main increase in rebellions after the Mongol invasions occurred in the provinces treated with the *Keigo* Policy of state-building.

5.1 Baseline Findings

Table 1 reports the main finding from the DD specifications in Equation 4.1. Column 1 presents results without controls, Columns 2 to 7 add the following control variables, iteratively: natural disaster indicator, rice suitability index, coinage density, indicator for eastern Japan, distance to Yamashiro, and indicator for coastal provinces. Column 8 includes all control variables.²² Across specifications, the Shogunate’s expansion of control over twenty three provinces in western Japan under *Keigo* increased the probability of rebellion by 4.3 to 6.2 percentage points. Considering that the average probability of rebellion is 4.2%, the effects identified here are sizable.

Table 1: The Effect of State-building on Rebellion

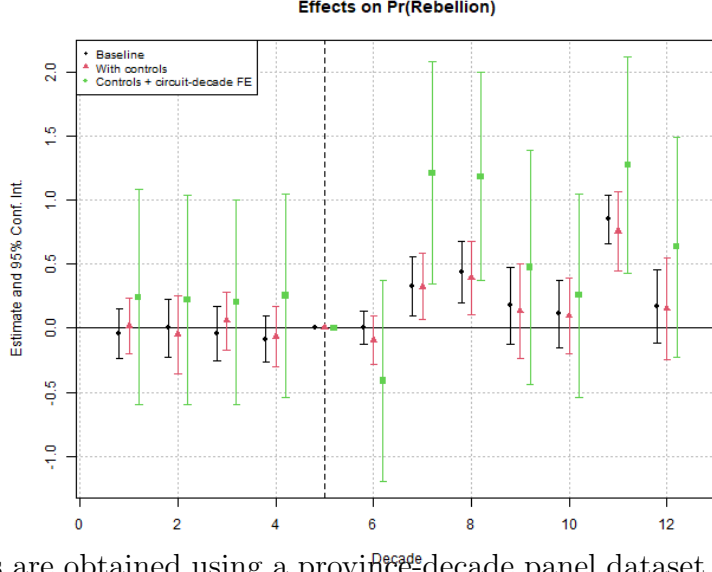
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pr(Rebellion)	Pr(Rebellion)	Pr(Rebellion)	Pr(Rebellion)	Pr(Rebellion)	Pr(Rebellion)	Pr(Rebellion)	Pr(Rebellion)
State-building	0.057**** (0.009)	0.057**** (0.009)	0.056**** (0.009)	0.061**** (0.008)	0.043**** (0.012)	0.062**** (0.009)	0.060**** (0.009)	0.054**** (0.010)
Natural Disaster	No	Yes	No	No	No	No	No	Yes
Rice Suitability	No	No	Yes	No	No	No	No	Yes
Cash Coins	No	No	No	Yes	No	No	No	Yes
East Japan	No	No	No	No	Yes	No	No	Yes
Dist. to Yamashiro	No	No	No	No	No	Yes	No	Yes
Coastal	No	No	No	No	No	No	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean of Outcome	0.042	0.042	0.042	0.042	0.042	0.042	0.042	0.042
Observations	7,684	7,684	7,684	7,684	7,684	7,684	7,684	7,684
R2	0.22	0.22	0.23	0.28	0.26	0.31	0.25	0.39
Within R2	0.01	0.01	0.02	0.08	0.05	0.11	0.04	0.22

Note: This table presents the effect of state-building policy on rebellion. The unit of observation is province-year. Standard errors (clustered at the province level) are in parentheses. Interactions between year dummies and controls are included in estimation but omitted from this table.

* $p < .10$; ** $p < .05$; *** $p < .01$; **** $p < .001$.

²²The time-varying natural disaster indicator is controlled for directly, while others are controlled for by including their interactions with time dummies in the regression because they are time-invariant.

Figure 3: Parallel Pre-trends



Note: The results are obtained using a province-decade panel dataset. The treatment set in at Period 6. Period 5, highlighted with a dashed line, refers to 1258-1267, the $t - 1$ decade. The outcome takes 1 (0) if there was (not) a rebellion in a province-decade.

Parallel Pre-trends. Figure 3 conducts a pre-trend test for our DD strategy. We aggregate the rebellion variable to decade level, and plot the coefficients for each decade for specification without controls, specification with full controls, and an additional specification that further controls for circuit-year fixed effects to account for higher level time-varying unobservables.²³ Estimates for pre-*Keigo* decades are not indicative of statistically significant trends, lending further credence to a causal interpretation of our results. Estimates post-*Keigo* are large and statistically significant for the periods 1278-1287, 1288-1297, and 1318-

²³Circuit (*do*) was an ancient Japanese administrative delineation as well as transportation system established during the 7th century that grouped geographically adjacent provinces (except the capital area) based on common characteristics in economy and society. Provinces within the same circuit were also connected by the same major road (Anderson, 2023). The five provinces near the capital are referred to as “Kinai” and are considered as one circuit-level unit. The Shogunate did not have permanent government offices at this higher administrative level.

1327, indicative of impact of state-building that lasted well beyond the immediate term.²⁴

The full estimation results are reported in Appendix Table A1.

5.2 Robustness Checks

Our findings are robust to a wide range of checks: (i) using a continuous measure of rebellions (Table A2); (ii) accounting for trade exposure and concurrent shocks by conditioning on distance to Ningbo, China and distance to Korea (Table A3); (iii) accounting for baseline state reach and control by conditioning on distance to Kamakura (Table A4); (iv) stringently accounting for location-based time-varying unobservables by conditioning on both longitude and latitude (Table A4); (v) adjusting for terrain and legibility using mean and standard deviation of elevation and river density (Table A5); (vi) controlling for the prevalence of religious manors (Table A5); and (vii) implementing a staggered DD following Callaway and Sant’Anna (2021) (Appendix Figure A1).²⁵ All the time-invariant variables aforementioned are interacted with time dummies. Appendix Table A6 further controls for the interaction between all year dummies and “circuit,” a higher level of administrative label than provinces, across all specifications to flexibly account for more aggregate time-varying unobservables.²⁶ Appendix Section B explains these exercises in detail.

²⁴The effect declines in the final period, 1328–1333, likely because the Shogunate was collapsing and rebellions were erupting nationwide, including in eastern regions near Kamakura.

²⁵In Equation 4.1, the state-building treatment was adopted for the *Keigo* provinces from 1268 onward when the threat of the Mongols first materialized. We prefer this non-staggered introduction of treatment because the temporal variation in the implementation of the *Keigo* policy was minimal. By 1276—merely eight years after the first contact with the Mongols—the *Keigo* policy had been activated across all provinces in the treatment group.

²⁶Note that the *Keigo* policy was implemented all over the entire regions of the Saikaido, Nankaido, and Sanyodo circuits. But our results are robust enough to even survive the collinearity introduced by the stringent inclusion of circuit-year fixed effects.

5.3 Aftermath: State Decay

To examine the impact of state expansion on long-run state decay, we use the number of newly constructed castles in each province as the outcome variable, which serves as a proxy for the strength of local autonomy. The unit of analysis is province-era, and the sample excludes the late Kamakura Period (1268–1333), during which the Mongol invasions occurred. This restricted sample is employed to focus on the consequences of *Keigo* for long-run state development; accordingly, only castles constructed after the collapse of the Kamakura Shogunate are treated as post-treatment outcomes. Table 2 presents the results. The coefficients for the state-building treatment are positive and substantial, though the p -values in Columns 1 and 2 are 0.062 and 0.053. Across all models, the penetration of the state’s reach under *Keigo* paradoxically led to the construction of 20-25 additional private castles per province for the next hundred years, providing strong evidence that premature state-building caused long-term state decay.

Table 2: The Effect of State-building on the Number of New Castles

	(1)	(2)	(3)
	No. Castle	No. Castle	No. Castle
State-building	19.7*	22.5*	24.8***
	(10.4)	(11.4)	(5.25)
Controls	No	Yes	Yes
Province FE	Yes	Yes	Yes
period	Yes	Yes	Yes
Circuit-period	No	No	Yes
Mean of Outcome	20.991	20.991	20.991
Observations	340	340	340
R2	0.48	0.54	0.64
Within R2	0.02	0.13	0.2

Note: This table presents the effect of state-building policy on the number of newly constructed castles. The unit of observation is province-year. Standard errors (clustered at the province level) are in parentheses. We use the same set of control variables as in Table 1, the time-invariant ones of which are interacted with era-dummies.

** $p < .10$; ** $p < .05$; *** $p < .01$; **** $p < .001$.*

6 Mechanisms and Other Explanations

Having established the causal effect of the Shogunate’s state-building on rebellions, we now provide evidence for the mechanism of our theory and evidence adjudicating other explanations.

6.1 Quantitative Evidence For Mechanisms

Disputes

We begin by documenting an increase in governance burden in *Keigo*-affected provinces. Columns 1-2 of Table 3 report results using whether there was a recorded dispute in a province-year as the outcome in the DD specified under Equation 4.1.²⁷ State-building increased the probability of reported dispute by 2.5 to 2.7 percentage points. These effects are larger than the average rate of disputes in the data and suggest a substantial rise in the burden of dispute resolution for the Shogunate as a result of their state-building efforts.

Next, we include the dispute variable on the right hand side in Equation 4.1. Columns 3-4 show that having a reported dispute translates into more than 10 percentage point increase in the probability of rebellion, while the average rate of rebellion is only 4.2 percent. Indeed, government overburden driven by *Keigo* in turn led to backlashes against the Shogunate. The top panel of Appendix Figure A8 reports the complete causal mediation analyses (Imai et al., 2011), with the dispute variable as the mediator, further confirming this channel.²⁸

²⁷We use a binary variable to reduce measurement errors: there are 170 disputes that were directly arbitrated by the Shogunate in our sample from 1221 to 1333 CE. The real volume should have been much larger, as historians repeatedly emphasize dispute resolution as a fundamental aspect of Kamakura governance (e.g. Mass, 1976; Oxenboell, 2018). Only some records of disputes have survived to the 20th century, from which our data sources were produced (Hioki, 2007a,b). We believe that given the relative scarcity of the raw data, a province-year with at least one dispute is qualitatively different from a province-year without a dispute recorded. Our results are consistent using the continuous measure as well (Appendix Table A12).

²⁸The identification of average mediation effect (ACME) requires sequential ignorability, a strong assumption especially as it entails no mediator-outcome confounding (Imai et al., 2011). We therefore follow the bias-formula approach in VanderWeele (2010, 2015) and report analytic sensitivity analyses in Figure A9,

Table 3: State-building - Dispute - Rebellion

	(1)	(2)	(3)	(4)
	Pr(Dispute)	Pr(Dispute)	Pr(Rebellion)	Pr(Rebellion)
State-building	0.025*** (0.009)	0.027*** (0.009)	0.054**** (0.009)	0.051**** (0.010)
Dispute			0.106*** (0.033)	0.107*** (0.034)
Controls	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Mean of Outcome	0.021	0.021	0.042	0.042
Observations	7,684	7,684	7,684	7,684
R2	0.07	0.19	0.23	0.4
Within R2	0	0.13	0.01	0.23

Note: The unit of observation is province-year. Standard errors (clustered at the province level) are in parentheses. Interactions between year dummies and controls are included in estimation but omitted from this table.

** $p < .10$; ** $p < .05$; *** $p < .01$; **** $p < .001$.*

Akuto

We further examine governance burden as a pathway linking state-building to rebellion by focusing on *Akuto* cases. As discussed in Section 3, *Akuto* originally denoted “evil bands” or “bandits,” but in practice the label became a strategic device in legal disputes. As the Shogunate’s adjudicatory machinery became increasingly congested, routine channels of dispute resolution grew slower and less effective. In response, litigants invoked the *Akuto* designation to portray their opponents as threats to the Shogunate, thereby prompting state intervention anticipated to be faster and more violent. This strategy transformed what were often local economic or land disputes into politically charged conflicts. In this sense, *Akuto* cases are indicative of government overburden: as ordinary governance became strained,

where the mediated effect is depicted as a fraction of its estimated value under varying degrees of unobserved mediator–outcome confounding. The upper panel in Figure A9 shows that the ACME with dispute as the mediator remains positive unless a confounder shifts disputes by as much as the treatment itself and simultaneously shifts rebellion by an amount comparable to the treatment’s total effect.

frustrated disputants escalated claims through politicized accusations, and disputes entered the category of rebellion because of the violent crackdown.

Columns 1-2 of Table 4 show that *Keigo* state-building indeed led to an increase in the probability of *Akuto* incident.²⁹ The percentage point increase is more than twice the average chance of such cases. These substantial effects suggest that the Shogunate was likely overwhelmed with governance responsibilities after it expanded its control over provinces designated by the *Keigo* Policy, resulting in a further delay of effective dispute resolution that led to growing frustration and grievances.

Note that the Shogunate had long exercised authority over *Akuto* cases across the country, including in western Japan, even before the Mongol invasions (Hioki, 2007a, p.781, p.807).³⁰ This reflected the treatment of *Akuto* primarily as a security concern, for which the Shogunate bore military and policing responsibility nationwide (Otsu et al., 2013, p.99). As a result, the post-1268 increase in *Akuto* cases in *Keigo* provinces cannot be attributed to a mechanical interpretation in which such incidents would appear simply because the Shogunate had only begun exercising authority there; in fact, the authority and duty of the Shogunate to “police” the entire realm of Japan had long existed and been well recognized by the imperial court, both before and after 1268 (Yamamura, 1991, p. 176). The results here instead reflect heightened demands placed on that authority.³¹

²⁹Results are robust to a continuous measure of *Akuto* rebellions (Table A2). Furthermore, one study argues that before the mid-13th century the Shogunate did not suppress *Akuto* in manors without Shogunal estate deputies (i.e., *jito*) (Otsu et al., 2013, p.102). This claim, however, is inconsistent with contemporaneous historical records (Hioki, 2007a) and recent interpretations of the origins of *Akuto* (Sherer, 2018). We control for the cumulative number of *jito* and the results remain unchanged (Appendix Table A18).

³⁰There were 10 *Akuto* incidents in our data between 1221 and 1267, three of which occurred in provinces that would later be covered by *Keigo*.

³¹Furthermore, separate event studies conducted for the effect of state-building on disputes and *Akuto* cases (discussed in Appendix Section C in detail) show that people in *Keigo* provinces initially increased their dispute lodging activities to the state, and frustration and grievances only emerged after experiencing state failures to resolve them due to overburden. This sequence, as further supported by the qualitative evidence in Section 7, suggests that the increase in disputes and rebellions cannot be attributed to the mechanical increase in state presence over previously autonomous territories alone. The mere increase in state presence would not have generated widespread frustration over dispute resolution—frustration as evident through the use of *Akuto* labels appearing only after disputes had emerged—unless state penetration was accompanied by

Table 4: The Effect of State-building on *Akuto* Cases

	(1)	(2)	(3)	(4)	(5)	(6)
	Pr(Akuto)	Pr(Akuto)	Pr(Hojo)	Pr(Hojo)	Pr(Akuto)	Pr(Akuto)
State-building	0.028**** (0.004)	0.023**** (0.006)	0.305*** (0.094)	0.328*** (0.122)	0.024**** (0.004)	0.019**** (0.005)
Hojo Shugo					0.007** (0.003)	0.007** (0.003)
Controls	No	Yes	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean of Outcome	0.01	0.01	0.501	0.501	0.01	0.01
Observations	7,684	7,684	7,307	7,307	7,307	7,307
R2	0.15	0.29	0.7	0.72	0.17	0.3
Within R2	0.01	0.17	0.06	0.12	0.01	0.17

Note: Model 1-2 present the effect of state-building policy on akuto cases and Model 3-6 show the results with Hojo Shugo as the mediator. The unit of observation is province-year. Standard errors (clustered at the province level) are in parentheses. Interactions between year dummies and controls are included in estimation but omitted from this table.

** $p < .10$; ** $p < .05$; *** $p < .01$; **** $p < .001$.*

Our above interpretation of the *Akuto* label as a strategically motivated tool to get the state further involved is in line with the historical narrative (Negayama, 2011; Oxenboell, 2018). We also provide quantitative evidence to further buttress this interpretation with data for governors. If the frustrated dispute contestants indeed used the *Akuto* label to make serious political allegations against the other side, such as accusations of anti-Shogunate activities, one should expect a higher number of *Akuto* cases in provinces governed by members of the Hojo ruling coalition. After all, governors (*Shugo*) from the ruling coalition of the Shogunate were directly viewed as the agents of the Shogunate by the public (Hosokawa, 2011, p. 95).³² We therefore construct a variable taking 1(0) if a province-year was in(out)side the tenure of a governor from the Hojo coalition (Hugo *Shugo*).

the inability to manage these disputes effectively due to overburden. Thus, our findings are not mechanically driven by the mere existence of state institutions.

³²One administrative duty of the governors was to handle local disputes on behalf of the Shogun (Sherer, 2018, p. 408).

Columns 3-4 report the effects of treatment on this mediator, which can be seen as a quantitative validation of historians’ consensus in Section 3 on the expansion of state control as a result of the *Keigo* Policy. Our state-building treatment led to an increase in the probability, by 30.5 to 32.8 percentage points, that the *Keigo*-designated provinces were assigned a Hojo-aligned governor. Because governors in medieval Japanese provinces were hereditary by default, that the *Keigo* Policy could even shift governorship at all is a significant indication of state expansion.

We then add this Hojo *Shugo* variable to Equation 4.1 with *Akuto* as the outcome. Columns 5 & 6 suggest that *Akuto* rebellions indeed increased under the tenure of Hojo coalition governors, confirming our strategic labeling interpretation. The average causal mediation effect (ACME) of the state-building treatment on *Akuto*, with Hojo coalition governors as the mediator, is presented in the bottom panel of Appendix Figure A8.³³

Variation in Pre-invasion Governance Burden

We now take a different approach to highlighting the mechanism, one based on heterogeneity analysis. First, we explore the idea that some provinces could be especially “overwhelmed” by governance burdens. Specifically, the adverse effects of state expansion should be especially pronounced in provinces that already faced greater governance demands. In these localities, the heavy pre-existing burdens meant that further state expansion leading to heightened demand for governance could easily push the administration beyond its capacity, intensifying local frustrations and prompting more frequent strategic usage of the *Akuto* label.

³³The sensitivity analysis following the approach of VanderWeele (2010, 2015) is reported in the bottom panel of Appendix Figure A9. In contrast to the case where disputes serve as the mediator, the mediating role of Hojo coalition governors in the effect of state-building on *Akuto* is substantially more sensitive to unobserved confounding. As shown in the figure, the estimated ACME would be attenuated to zero if an unobserved confounder exerted effects on both *Akuto* and Hojo governors that are on the order of roughly one-quarter of the treatment’s total effect on *Akuto* and one-quarter of its effect on Hojo governors, respectively. We therefore interpret this mediation result with caution.

We add an interaction term between the state-building treatment and the average rate of disputes over the *pre-treatment* period (1221-1267) for each province.³⁴ Columns 1-2 of Table 5 report that the interaction term is positively significant, indicating that state-building indeed triggered more *Akuto* cases in provinces that were already experiencing greater government burdens before the invasion.³⁵

Variation in Administrative Capacity: Distance to Chinzei Tandai

Our second heterogeneity analysis illustrates the importance of government overburden for driving the rebellion result by showing that the effect on rebellion could be mitigated when that overburden is reduced. In 1286, because of the rising governance demands following *Keigo* expansion, the Shogunate built *Chinzei Tandai*, a new government agency in western Japan to handle disputes and related governance issues.³⁶ We construct a variable, called “Active Chinzei,” that takes the *distance* to Chikuzen Province of North Kyushu (where *Chinzei Tandai* was located) for any unit-time observation after 1286 and 0 otherwise. The higher the value of this variable, the less accessible the Shogunate’s enhanced administrative capacity was to local disputants. We thus expect its interaction with the state-building treatment to be positive when examining the impacts on rebellions.

Columns 3-4 of Table 5 report results consistent with our expectation: the positive effect of state-building through *Keigo* expansion on rebellions is more pronounced in areas that were farther away from *Chinzei Tandai* since its establishment.³⁷ The causal effect of pre-

³⁴The interaction terms between this mean dispute variable and year dummies are also controlled for.

³⁵The provincial average of disputes over the pre-treatment period is constructed by using the binary dispute variable. Results are also robust by taking the mean of the continuous measure of disputes across the pre-treatment period for each province (Appendix Table A15).

³⁶Before 1286, disputes from western Japan, including those arising in *Keigo* provinces, could be brought only to provincial governors, the court in Kyoto, or the Shogunate in Kamakura. The establishment of the *Chinzei Tandai* therefore represented an enlargement of the administrative machinery for dispute resolution.

³⁷Because the new variable “Active Chinzei” is an interaction between distance and a post-1286 dummy, we ensure all the “constituents” in the interaction terms are properly included in all specifications. We also include an interaction term between the treatment and the distance to Yamashiro, where another of the Shogunate’s legal agencies, the *Rokuhara Tandai*, was located, in these models.

mature state-building on rebellion could be indeed mitigated when government overburden is mitigated.³⁸

Table 5: Pre-invasion Dispute and Administrative Capacity as Moderator

	(1)	(2)	(3)	(4)
	Pr(Akuto)	Pr(Akuto)	Pr(Rebellion)	Pr(Rebellion)
State-building	0.023**** (0.004)	0.016*** (0.005)	-0.198** (0.083)	-0.267*** (0.088)
State-building * Mean Dispute	0.566** (0.266)	0.642** (0.286)		
State-building * Active Chinzei			0.208**** (0.054)	0.225*** (0.068)
Controls	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Mean of Outcome	0.01	0.01	0.042	0.042
Observations	7,684	7,684	7,684	7,684
R2	0.17	0.31	0.23	0.39
Within R2	0.03	0.2	0.01	0.22

Note: Model 1-2 present the effect of state-building policy on Akuto cases moderated by mean dispute, which represents the mean of dispute variable for each province across 1221-1267. Model 3-4 presents the effect of state-building policy on rebellion moderated by the distance to the Chinzei Tandai, which was activated from 1286. In other words, the Active Chinzei variable takes the value of 0 prior to 1286. The unit of the Active Chinzei variable is 1,000 km. Interactions between year dummies and controls are included in estimation but omitted from this table.

** $p < .10$; ** $p < .05$; *** $p < .01$; **** $p < .001$.*

6.2 Other Explanations

We now adjudicate across other potential explanations for our main finding that *Keigo* state-building caused rebellions.

³⁸The same pattern also holds statistically when we replace rebellion with *Akuto* incident as the outcome. The results are available upon request.

War Burdens

While the Mongols never made meaningful incursions into the *Keigo* provinces, having been repelled at the shores in both invasions, one alternative explanation for our results is that these provinces experienced more rebellions due to the heavy demands of war mobilization imposed on the local population. We address this concern by conducting separate analyses of the effects on rebellions by religious elites, secular elites (warriors), and commoners, respectively. If the results were driven primarily by the burdens of war preparation *per se*, we should not expect any positive effect on rebellions by religious elites, since they bore no mobilization burdens and were instead major beneficiaries of the Shogunate’s defense preparations (Appendix Section D). We would anticipate a sizable increase in warrior rebellions following state-building because they directly bore the war burdens. However, the empirical findings are exactly the opposite: there is no significant effect on warrior rebellions, but a sizable increase in rebellions involving religious elites. Moreover, commoner rebellions appear to have experienced the largest effect, but notably, this effect emerged only *after* the years of actual invasions (1274–1281), during which the burdens of war mobilization should have been greatest.³⁹ This discrepancy further undermines the explanation based on war mobilization. Appendix D discusses these results and their historical context in depth.

Extraction by the State

One may attribute the effect of state-building on rebellion to economic extraction by the Shogunate government, which could create grievances. Our evaluation of this interpretation utilizes two features regarding the management and ownership of estates in medieval Japan.

³⁹Surviving records of manor taxation indicate that the tax rate on commoners did not increase along with the emergence of external threat, even in the *Keigo* provinces. Therefore, there is no evidence to suggest that the elites transferred the burden of war to the peasants. Instead, as our qualitative evidence below suggests, the commoners rebelled due to the administrative inefficiencies of the Shogunate and its agents in settling disputes involving them.

Jito *Jito* were estate stewards appointed by the Shogunate to directly manage medieval estates with power rivaling that of the nominal owner (Seno, 1975; Yasuda, 1985). An increase in Hojo-aligned *jito* following state-building under *Keigo* would indicate an increase in economic extraction of the local population by the central government. However, Table A17 finds no such effect of state-building on *jito* assignment in our DD design.

Hojo ownership of estates Another proxy for the intensity of extraction by the center in each province is *the proportion of manors that were owned by the Hojo clan*.⁴⁰ We collect the ownership transfer records of 4,922 manors from Abe and Sato (1997), which constitute the near universe. We then check whether Hojo ownership affected rebellion in two-period DD models, where the treatment takes 1 for *Keigo* provinces post-1268 and the outcome is the count of rebellions in each period.⁴¹ Appendix Table A19 shows that *Keigo* state expansion indeed brought a larger share of manors under the Hojo ownership. However, this increase in the percentage of manors owned by the center did not in turn trigger more rebellions (Appendix Table A19).

Opposition to Centralized Rule

Rebellions could emerge as local actors were simply opposed to the imposition of centralized rule, regardless of whether it meant the administration is overwhelmed and overburdened. We therefore check whether the treatment effect is moderated by distance to Kamakura, the seat of the Shogunate. Distance to the location of central government is a common measure of state reach and the extent of centralized rule (e.g. Koyama, Moriguchi and Sng, 2018). If this alternative explanation holds, the *Keigo* Policy should cause more rebellions in provinces

⁴⁰This variable also includes manors under the name of the Shogunate, or the so-called *kanto goryo*. Although the yields for the manors are not available, the Hojo rarely transferred ownership of one manor for another within the same province during their entire reign. Consequently, a higher percentage of manors owned by the Hojo corresponds to a greater volume of yield extraction.

⁴¹The information from the manor dataset does not allow us to identify the precise year of ownership transfer.

farther away from Kamakura as local actors in those provinces had enjoyed more autonomy than others but now saw them taken away. However, Column 2 of Appendix Table A4 shows that the treatment effect is not at all moderated by such distance.

Further, we proxy for the propensity of local resistance using a dummy variable taking 1 if a province participated in the anti-Shogunate Rebellion of 1221 (the “Jokyu War”). As shown in Table A13, the effect of state-building remains robust after controlling for the interaction between this indicator and year dummies.

Coordination of Local Elites

Could the expansion of state institutions into newly incorporated territories help local elites overcome coordination problems, making the state itself a focal target of rebellion? If this mechanism were at work, state-building should have stronger effects in provinces with *fewer* clans, where collective action against the Shogunate would be easier. We draw on Morioka (2013), who documents the number of clans in each province of medieval Japan, including aristocrats, samurai, manor managers, and wealthy peasants, totaling 3,255 clans nationwide. We interact the number of clans with the state-building treatment in the rebellion regressions. The interaction term is statistically insignificant (Appendix Table A14).

Consistency with recent historical research Results in Section 6.2 challenge the traditional view in the historical literature that the post-*Keigo* rise in rebellion was driven primarily by warrior discontent in western regions newly brought under state control. Earlier accounts, such as Nam (1990), attribute the collapse of the Kamakura Shogunate to insufficient compensation for warriors after the Mongol wars. Our findings instead align more closely with recent scholarship that has revised this interpretation (Hosokawa, 2011; Goza, 2014).⁴²

⁴²Notably, the Ashikaga family, the clan that ultimately overthrew the Kamakura Shogunate, did not originate from *Keigo*-affected provinces or western Japan in general.

Nevertheless, we do not claim that elite-based explanations played no role. Using the average number of castles for each province over the pre-treatment period to capture variation in prior state exposure, we show that the effect of *Keigo* on rebellion is stronger in provinces with weaker initial state exposure (meaning larger number of private castles constructed before 1268, see Appendix Table A20, Columns 1–2).⁴³ This finding suggests that centralization-driven elite discontent—possibly operating through extraction—may still have been present in the Japanese case (Garfias and Sellars, 2022*b*; Albertus and Gay, Forthcoming). Our framework of premature state-building is not meant to be exclusive but complementary. As discussed in Section 2, extraction may exacerbate elite–peasant tensions by tightening local resource constraints, which can surface as a higher volume of disputes brought before the state, thereby increasing governance demands and contributing to government overburden. This possibility of complementarity is supported in Columns 3–4 of Appendix Table A20, where the same interaction is also significant for *Akuto* cases.

7 Qualitative Evidence

This section presents qualitative evidence on the overwhelming administrative burden caused by state expansion and how it contributed to the accumulation of grievances that ultimately led to rebellions.

Even before the Mongol threat, the Shogunate’s governance relied on a thin legal-administrative apparatus. Its judicial practices operated with limited centralized record-keeping and a largely personalistic judicial process (Bovbjerg, 2018; Sato, 1993). These institutional features made adjudication slow and easy to exploit, including through “du-

⁴³The main effect of the treatment variable in this interaction specification—that is, the effect of treatment when the average number of pre-treatment castles equals zero—remains large and statistically significant. Because the variable for the average number of pre-treatment castles is standardized to have mean zero and standard deviation one, a value of zero corresponds to the average level of prior state exposure. The estimated main treatment effect in this interaction specification therefore represents the effect of state-building when initial state exposure is at its average level.

plicitous suits” and speculative litigation, and they limited the center’s ability to verify claims and close cases efficiently (Bovbjerg, 2018). Because the Shogunate’s territorial reach remained relatively constrained, the scale of governance demands placed on these institutions remained contained.

The *Keigo* reforms, however, dramatically exacerbated the scale of the problem. By extending *de facto* control territorially, the Shogunate drew a broader set of land and revenue conflicts into its adjudicatory orbit, sharply increasing the volume of disputes requiring state attention (Seno, 1958). Facing these rising demands, the Shogunate created the *Chinzei Dangisho* in 1286, later reorganized as the *Chinzei Tandai*, to bring administrative, judicial, and military functions closer to Kyushu and the western provinces (Narangoa, 2022, p. 910). This institutional enlargement added capacity, helping partially mitigate conflict (Table 5).

Nevertheless, even this specialized “booster” remained handicapped by the Shogunate’s deeper structural limitations. It could not compensate for the absence of a professional bureaucratic class capable of processing the state’s newfound responsibilities. The *Tandai*’s deliberation council (*hojoshu*) consisted of only four to five local samurai who were not professional jurists and who continued to govern their own estates and territories (Seno, 1958). A contemporary chronicle captures these strains in compressed form, describing how officials faced a flood of “court affairs, state matters, miscellaneous lawsuits, and criminal cases,” and that proceedings became “as crowded as a marketplace or a forest,” while insiders “secretly communicate through carriages and horses to accept bribes” (Seno, 1958, p. 85).

Under these constraints, dispute resolution often became either swift but superficial or protracted and inconclusive. Influenced by conservative legal principles that defended existing hierarchies, rulings were often in favor of religious institutions regardless of the case specificities (Bovbjerg, 2018). At the other extreme, proceedings could be delayed for as long as *twenty years* when defendants requested postponements (Takehi, 2009, p. 70).

Administrative weakness also surfaced in basic fact-finding: the Shogunate’s own deputies charged with applying the law often lacked elementary knowledge of the laws they were meant to apply. One such case, occurring around year 1280 in Kii Province (under *Keigo*), exposed the Shogunate’s inability to resolve conflicts over revenue distribution between a manor owner (Jakuraku Temple) and a local samurai. The dispute devolved into an extended exchange of legal documents: The samurai cited a specific Shogunate law to support his claim, while the Temple’s representative challenged the law’s very existence. The Shogunate’s own deputies designated for adjudicating such disputes, on the other hand, demonstrated a conspicuous lack of administrative competence as they themselves could not even verify whether the cited law existed. Ultimately, the government failed to mediate or issue a definitive ruling after a long delay ([Kasamatsu, 2022](#)). Similar cases throughout the Kamakura period underscored the Shogunate’s beleaguered bureaucratic structure, as disputing parties generally bore the burden of proving the existence of the laws they invoked. Over time, this inaction led to an increasing number of falsified legal codes, further clouding judicial decisions and eroding what little administrative capacity the Shogunate possessed ([Kasamatsu, 2022](#)).

The combination of delay, uncertainty, and discretionary authority also created opportunities for self-interested intervention. For instance, the Otomo clan, one of the leading families within the *Chinzei Tandai* and long interested in a territory owned by a shrine, leveraged their administrative authority empowered by this institution to successfully acquire a “legal cloak” to legitimately manage the territory. Their position in the state allowed them to intervene in the shrine’s internal affairs—such as adjudicating petitions—thereby transforming a state administrative function into a tool for private gains ([Kawazoe, 1973](#)).

These governance failures were politically consequential because they transformed unresolved disputes into durable grievances and, at times, open challenge. In Iyo Province, Benbo Shoyo, a manor deputy (*daikan*) appointed amid inter-lord conflict in the 1310s, became a focal point of grievance. After repeated peasant complaints, he was dismissed twice but

regained office by paying “reinstatement fees” to officials ([Amino, 2001](#), p. 493). Eventually, frustrated over the government’s prolonged failure to effectively resolve their dispute against this local bully⁴⁴, peasants labeled him a “pirate *Akuto*,” and many fled the manor ([Hioki, 2007b](#), p. 131). Episodes like this illustrate how rapid territorial penetration overwhelmed the state with governance responsibilities faster than the Shogunate’s administrative machinery could keep pace, fostering grievances that could culminate in rebellion.

8 Conclusion

Our empirical analyses show that the Kamakura Shogunate’s state-building measures significantly increased local rebellions against its rule. Further analyses of mechanisms indicate that the expansion of state reach sharply raised societal demands for governance, particularly in dispute resolution. The resulting overburden left the government unable to meet these demands, fueling grievances and unrest.

In the long run, the adverse effects of overextended state control became evident through an apparent rise in the construction of new privately owned castles. This increase reflects chronic decay of the state’s ability to maintain basic order as local populations increasingly turned to self-defense. Altogether, these findings underscore a critical dilemma of state-building: rapid expansion of state reach that generates government overburden can ultimately undermine the long-term survival of state institutions.

Our paper is inspired by prior research that employs the case of historical Japan to advance general theories in political science ([Ferejohn and Rosenbluth, 2010](#); [Mukoyama, 2022](#); [Kubota, Ito and Harada, 2023](#); [Wang, Mitchell and Yin, 2025](#); [Harada and Kubota, 2025](#)). Medieval Japan merits scholarly attention because its political trajectory shares important

⁴⁴Shoyo’s appointment in the 1310s itself reflected the Shogunate’s inability to resolve other disputes. As the Shogunate repeatedly fail to resolve said inter-lord conflict, Shoyo, known for his mobilization power in the region, was invited by one local lord to expel the other, hence the “appointment.”

similarities with that of medieval Europe—fragmented governance, feudal forms of vassalage, and the absence of enduring centralized rule. At the same time, it parallels many contemporary settings characterized by fragile states, civil wars, and domestic violence. Examining state-building and state decay in Japan thus provides valuable leverage for developing generalizable theories about the origins, collapse, and long-term fragility of political order.

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Appendix A Estimation Results for Event Studies

Table A1: Estimation Results for Event Studies Reported in Figure 3

	(1)	(2)	(3)
	Pr(Rebellion)	Pr(Rebellion)	Pr(Rebellion)
Keigo Province x Decade FE 1	-0.042 (0.098)	0.017 (0.106)	0.241 (0.420)
Keigo Province x Decade FE 2	0.0010 (0.113)	-0.049 (0.153)	0.222 (0.408)
Keigo Province x Decade FE 3	-0.042 (0.108)	0.056 (0.112)	0.203 (0.400)
Keigo Province x Decade FE 4	-0.086 (0.090)	-0.066 (0.119)	0.251 (0.396)
Keigo Province x Decade FE 6	-8.98e-15 (0.064)	-0.094 (0.094)	-0.413 (0.391)
Keigo Province x Decade FE 7	0.327*** (0.115)	0.322** (0.129)	1.21*** (0.434)
Keigo Province x Decade FE 8	0.435**** (0.119)	0.391*** (0.145)	1.18*** (0.408)
Keigo Province x Decade FE 9	0.174 (0.150)	0.132 (0.183)	0.475 (0.459)
Keigo Province x Decade FE 10	0.110 (0.131)	0.093 (0.149)	0.254 (0.397)
Keigo Province x Decade FE 11	0.847**** (0.093)	0.754**** (0.156)	1.27*** (0.424)
Keigo Province x Decade FE 12	0.168 (0.144)	0.151 (0.199)	0.634 (0.430)
Controls	No	Yes	Yes
Province FE	Yes	Yes	Yes
Decade FE	Yes	Yes	Yes
Circuit-Decade FE	No	No	Yes
Mean of Outcome	0.217	0.217	0.217
Observations	816	816	816
R2	0.5	0.54	0.66
Within R2	0.15	0.23	0.2

Note: This table presents the estimation results for event studies reported in Figure 3. The unit of observation is province-decade. Standard errors (clustered at the province level) are in parentheses. Interactions between decade dummies and controls are included in estimation but omitted from this table.

** $p < .10$; ** $p < .05$; *** $p < .01$; **** $p < .001$.*

Appendix B Robustness Checks In Detail

We subject our main finding, that the penetration of the central authority’s territorial reach under the *Keigo* Policy increased rebellions against the central government, to a series of robustness checks, explained below.

Alternative measure. Columns 1-2 of Table A2 show that our results are robust to a continuous variable for rebellions.

Distance to China. 12th century saw increasing trade between Japan and China through the Port of Ningbo in the Southern Song Dynasty (1127 - 1279 CE). Distance to Ningbo from each Japanese province during the Kamakura period could function as an additional control for economic development. Furthermore, the Mongol invasions of Japan also took place simultaneously with the Mongol conquest of southern China, potentially leading to disruption in trade.⁴⁵ Columns 1-2 of Table A3 show that the results remain substantively similar and statistically significant after including the interaction between distance to Ningbo and the year dummies, for both binary and continuous measures of rebellion.

Distance to Korea. Since the main Mongol fleets during both the 1274 and 1281 attempts of invasion departed from Korea, distance to Korea should have played a crucial role in determining which province would fall under the *Keigo* Policy of state-building for national defense by the Shogunate. Results in Columns 3-4 of Appendix Table A3 remain substantively similar and statistically significant after controlling for the interaction between distance to Tsushima, the main island in the middle of the strait separating Korea and Japan which the Mongols used as a temporary naval base during the invasions, and the year dummies.

Distance to Kamakura. State capacity decreases in the distance to the capital (Sng and Moriguchi, 2014; Ko, Koyama and Sng, 2018), and the Shogunate’s limited capacity

⁴⁵Moreover, during the second attempt of Mongol landing in Japan in 1281, in addition to the main path through Korea as done in 1274, the Mongols also opened a second route in which their fleets directly departed from Ningbo to attack the southern flank of western Japan.

to suppress rebellions in regions far from Kamakura may have encouraged violent uprisings as a means of expressing grievances. In the baseline results presented in Table 1, we have accounted for this potentially confounding factor by controlling for east-west divide and the distance to Kyoto, where the imperial court was located, both of which capture the power projection of the Kamakura Shogunate. We now further control for the distance to Kamakura (interacted with the year dummies) explicitly in the model as a robustness check. The results with this control variable included remain statistically and substantively significant (see Column 1 in Appendix Table A4).

Longitude and latitude. We further control for the interactions between longitude and latitude of each province and the year dummies, respectively. This stringent test is reported in Columns 3-4 of Appendix Table A4, where our results remain robust both statistically and substantively, for both binary and continuous measures of rebellion.

Mean and standard deviation of elevation. We include the mean and standard deviation of elevation for each province as control variables, both of which are interacted with year dummies. Although challenging terrains may hinder the legibility for the state and simultaneously affect rebellion considerations, incorporating terrain variables in our models does not affect the results (Columns 1-2 in Appendix Table A5).

River density. Another geographical factor that may jointly influence a province's legibility and rebellion risk is river density, defined as the length of rivers per square kilometer. Again, including this variable as a control does not alter our results (Columns 3-4 of Appendix Table A5).

Religious manors. Religious institutions, such as temples and shrines, were often known for their rebellious attitudes toward the state in Medieval Japan (Farris, 2009, p. 98). More importantly, during the Kamakura period, local warriors often had conflicts with monks over resources (Kakehi, 2009, pp. 51-68).

We control for the share of manors owned by temples and shrines, which is calculated from Seno (2003), as a robustness check. This variable is time-invariant, as the number of manors across Japan had stabilized prior to the establishment of the Kamakura Shogunate. Hence, this variable is also interacted with year dummies in our specifications. Columns 5-6 of Appendix Table A5 indicate that the effect of state-building remains statistically significant and substantively consistent when we control for the percentage of religious manors.⁴⁶

Staggered treatment adoption. In the baseline analysis focusing on the estimation of Equation 4.1, the state-building treatment was adopted for the *Keigo* provinces from 1268 onward when the threat of the Mongols first materialized. We prefer this non-staggered introduction of treatment because the temporal variation in the implementation of the *Keigo* policy was minimal. By 1276—merely eight years after the first contact between the Kamakura Shogunate and the Mongols—the *Keigo* policy had been activated across all provinces in the treatment group.

To reflect the nuanced historical reality that the state-building policy was implemented at three different timings—1271 for the 9 provinces in Kyushu,⁴⁷ 1275 for the 4 provinces surrounding Nagato, and 1276 for the remaining 10 provinces (Aida, 1958, pp. 314-315), we employ a DD strategy with staggered treatment adoption, following the procedures in Callaway and Sant’Anna (2021). Specifically, we estimate the effects of the state-building policy on rebellion by comparing provinces treated at different times to those not yet treated in each year. This approach accounts for variations in the timing of treatment adoption and allows for differences in the impact across provinces and over time. The results presented in Appendix Figure A1 confirm that the state-building policy increased the likelihood of rebellion in provinces subject to the *Keigo* policy, irrespective of their treatment cohort

⁴⁶In fact, in the state-building process, religious manors were not the primary target of resource extraction. For further discussion on the role of religious organizations, refer to Section 6.2.

⁴⁷In 1271, the Shogunate issued the first mobilization decree targeting warriors in Kyushu (Aida, 1958, p.149).

(1271, 1275, or 1276). The magnitude of effects, ranging from 5 to 7 percentage points, is consistent with those reported in Table [1](#).

Table A2: The Effect of State-building on the Count of Rebellions and *Akuto* Cases

	(1)	(2)	(3)	(4)
	#Rebellion	#Rebellion	#Akuto	#Akuto
State-building	0.052*** (0.018)	0.062**** (0.014)	0.028**** (0.005)	0.023**** (0.006)
Controls	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Mean of Outcome	0.058	0.058	0.01	0.01
Observations	7,684	7,684	7,684	7,684
R2	0.2	0.68	0.15	0.28
Within R2	0	0.6	0.01	0.16

Note: This table presents the effect of state-building policy on the number of rebellions and akuto cases. The unit of observation is province-year. Standard errors (clustered at the province level) are in parentheses. Interactions between year dummies and controls are included in estimation but omitted from this table.

** $p < .10$; ** $p < .05$; *** $p < .01$; **** $p < .001$.*

Table A3: The Effect of State-building on Rebellions, Controlling for Distance to China and Korea

	(1)	(2)	(3)	(4)
	Pr(Rebellion)	#Rebellion	Pr(Rebellion)	#Rebellion
State-building	0.052**** (0.012)	0.064**** (0.017)	0.054**** (0.011)	0.064**** (0.015)
Controls	Yes	Yes	Yes	Yes
Dist. to Ningbo	Yes	Yes	No	No
Dist. to Korea	No	No	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Mean of Outcome	0.042	0.058	0.042	0.058
Observations	7,684	7,684	7,684	7,684
R2	0.43	0.69	0.41	0.69
Within R2	0.26	0.62	0.25	0.61

Note: This table presents the effect of state-building policy on rebellion. The unit of observation is province-year. Standard errors (clustered at the province level) are in parentheses. Interactions between year dummies and controls are included in estimation but omitted from this table.

** $p < .10$, ** $p < .05$, *** $p < .01$, **** $p < .001$.*

Table A4: The Effect of State-building on Rebellions, Accounting for Distance to Kamakura and Coordinates

	(1)	(2)	(3)	(4)
	Pr(Rebellion)	Pr(Rebellion)	Pr(Rebellion)	#Rebellion
State-building	0.047**** (0.013)	0.041** (0.019)	0.053**** (0.012)	0.067**** (0.018)
State-building * Dist. Kamakura		0.010 (0.016)		
Controls	Yes	Yes	Yes	Yes
Dist. Kamakura	Yes	Yes	No	No
Coordinates	No	No	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Mean of Outcome	0.042	0.042	0.042	0.058
Observations	7,684	7,684	7,684	7,684
R2	0.43	0.43	0.44	0.7
Within R2	0.27	0.27	0.28	0.62

Note: This table presents the effect of state-building policy on rebellion. The unit of observation is province-year. Standard errors (clustered at the province level) are in parentheses. Interactions between year dummies and controls are included in estimation but omitted from this table.

** $p < .10$; ** $p < .05$; *** $p < .01$; **** $p < .001$.*

Table A5: The Effect of State-building on Rebellions, Controlling for Elevation, River Density, and Religious Manor

	(1)	(2)	(3)	(4)	(5)	(6)
	Pr(Rebellion)	#Rebellion	Pr(Rebellion)	#Rebellion	Pr(Rebellion)	#Rebellion
State-building	0.054**** (0.010)	0.063**** (0.013)	0.054**** (0.010)	0.061**** (0.014)	0.053**** (0.011)	0.064**** (0.014)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Elevation	Yes	Yes	No	No	No	No
River Density	No	No	Yes	Yes	No	No
Religious Manors	No	No	No	No	Yes	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean of Outcome	0.042	0.058	0.042	0.058	0.042	0.059
Observations	7,684	7,684	7,684	7,684	7,571	7,571
R2	0.41	0.69	0.4	0.68	0.41	0.69
Within R2	0.24	0.61	0.23	0.6	0.24	0.61

Note: This table presents the effect of state-building policy on rebellion. The unit of observation is province-year. Standard errors (clustered at the province level) are in parentheses. Interactions between year dummies and controls are included in estimation but omitted from this table.

** $p < .10$; ** $p < .05$; *** $p < .01$; **** $p < .001$.*

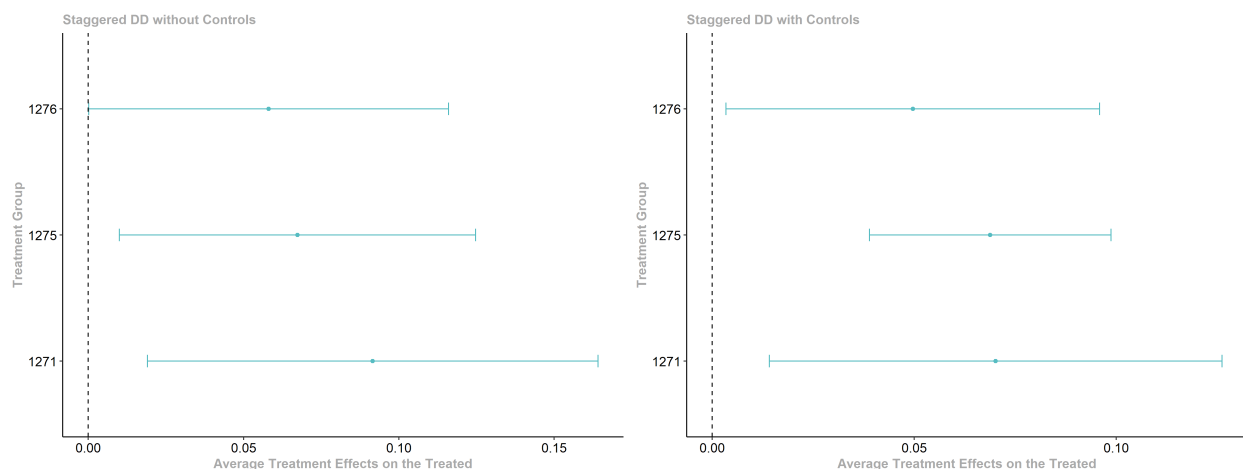


Figure A1: Average Treatment Effects on the Treated Using Staggered DD

Average treatment effects on the treated (ATT) estimated via the doubly robust method of [Callaway and Sant’Anna \(2021\)](#). Point estimates correspond to the overall effect for each group—defined as all provinces that initiate the state-building policy in the same period—while the error bars denote the 95% simultaneous confidence bands derived from bootstrapping. The left panel employs no additional covariates, whereas the right panel includes a baseline set of controls additively: natural disaster occurrence, rice suitability index, coin density, East Japan indicator, coastal indicator, and distance to Yamashiro. In both estimations, the control group consists of provinces that have not yet been treated. The results with circuit-year fixed effects are not estimable because their inclusion absorbs all the variation necessary for this method.

Table A6: The Effect of State-building on Rebellion, with Circuit-Year Fixed Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Pr(Rebellion)	Pr(Rebellion)	Pr(Rebellion)	Pr(Rebellion)	Pr(Rebellion)	Pr(Rebellion)	Pr(Rebellion)	Pr(Rebellion)
State-building	0.076**** (0.011)	0.076**** (0.011)	0.078**** (0.012)	0.073**** (0.011)	0.076**** (0.011)	0.075**** (0.011)	0.076**** (0.011)	0.079**** (0.012)
Natural Disaster	No	Yes	No	No	No	No	No	Yes
Rice Suitability	No	No	Yes	No	No	No	No	Yes
Cash Coins	No	No	No	Yes	No	No	No	Yes
East Japan	No	No	No	No	Yes	No	No	Yes
Dist. to Yamashiro	No	No	No	No	No	Yes	No	Yes
Coastal	No	No	No	No	No	No	Yes	Yes
Mean of Outcome	0.042	0.042	0.042	0.042	0.042	0.042	0.042	0.042
Province FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Circuit-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,684	7,684	7,684	7,684	7,684	7,684	7,684	7,684
R2	0.44	0.44	0.45	0.50	0.47	0.51	0.46	0.59
Within R2	0.00	0.00	0.02	0.12	0.05	0.14	0.03	0.27

Note: This table presents the effect of state-building policy on rebellion. The unit of observation is province-year. Standard errors (clustered at the province level) are in parentheses. All models include Circuit-Year fixed effects. Interactions between year dummies and controls are included in estimation but omitted from this table.

** $p < .10$; ** $p < .05$; *** $p < .01$; **** $p < .001$.*

Appendix C Discussion on the sequence of disputes and *Akuto*

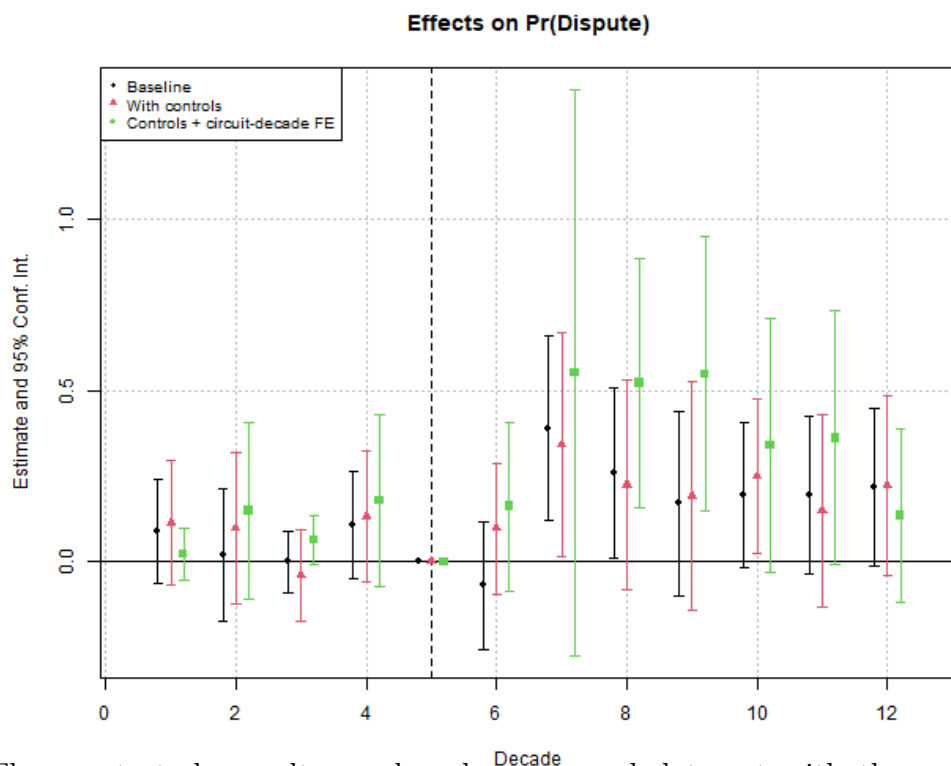
One may wonder: why would disputants choose to file cases with the Shogunate if the Shogunate was already perceived as incapable of providing efficient conflict resolution? We argue that people initially tried resorting to the state after state-building, and frustration and grievances only emerged after experiencing state failures. We conduct separate event studies for the effect of state-building on disputes and *Akuto* cases. Together with the event study for rebellion in Figure 3, they suggest a sequential order of effects consistent with our argument.

Provinces that received the state-building treatment already began to see more disputes handled by the Shogunate in as early as the second decade since Japan's first contact with the Mongols (Appendix Figure A2, plotted based on Table A7). On the other hand, it took three decades until the treatment effect on *Akuto* cases to emerge (Appendix Figure A3, plotted based on Table A8). This contrast shows that people's frustration with the state's ability to resolve disputes came after trying.

Equally importantly, Appendix Figure A2 further indicates that the effect on disputes did not last long - the results from the baseline model and model with controls are no longer consistent from the 1290s on. Appendix Figure A3, on the other hand, shows a longer-lasting effect on *Akuto* with results from all three specifications reaching the highest, statistically significant magnitudes in the early 1300s. This contrast again suggests that people's frustration with the state, with political labeling as the last resort, likely emerged after having tried to rely on the state as arbitrator and losing trust after experiencing failures. Moreover, it is noteworthy that the effect on rebellions in Figure 3 also appears to last longer than the short-term effect on disputes in Appendix Figure A2, again underpinning

the idea that grievances emerged after people tried to rely on the state but experienced disappointment.

Figure A2: Event Studies, Effect of State-building on Disputes



Note: The event study results are based on a panel data set with the unit of analysis being province-decade. Period 5, highlighted with a dashed line, represents 1258-1267. The outcome variable is the probability that a dispute occurs in that decade.

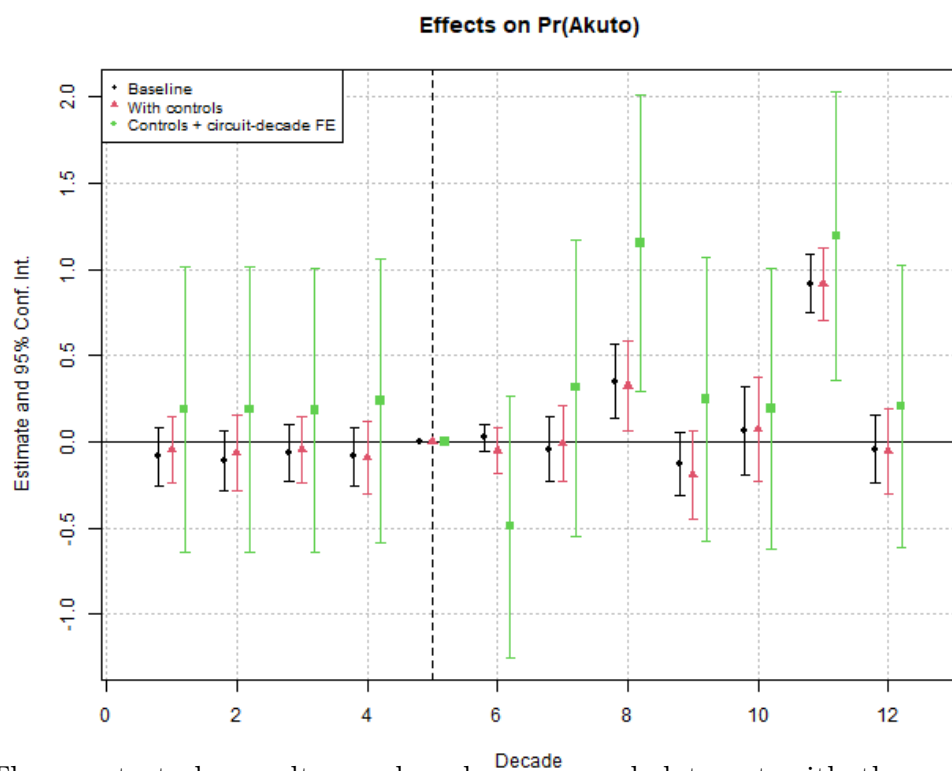
Table A7: Estimation Results for Event Studies Reported in Figure A2

	(1)	(2)	(3)
	Pr(Dispute)	Pr(Dispute)	Pr(Dispute)
Keigo Province x Decade FE 1	0.089 (0.076)	0.114 (0.090)	0.022 (0.039)
Keigo Province x Decade FE 2	0.021 (0.097)	0.099 (0.111)	0.149 (0.130)
Keigo Province x Decade FE 3	-8.62e-15 (0.045)	-0.040 (0.066)	0.064* (0.036)
Keigo Province x Decade FE 4	0.109 (0.078)	0.132 (0.096)	0.179 (0.126)
Keigo Province x Decade FE 6	-0.069 (0.093)	0.096 (0.096)	0.162 (0.123)
Keigo Province x Decade FE 7	0.390*** (0.135)	0.343** (0.163)	0.553 (0.414)
Keigo Province x Decade FE 8	0.258** (0.125)	0.225 (0.153)	0.521*** (0.182)
Keigo Province x Decade FE 9	0.171 (0.135)	0.192 (0.168)	0.549*** (0.201)
Keigo Province x Decade FE 10	0.195* (0.106)	0.250** (0.114)	0.340* (0.186)
Keigo Province x Decade FE 11	0.194* (0.115)	0.148 (0.141)	0.362* (0.186)
Keigo Province x Decade FE 12	0.218* (0.116)	0.223* (0.131)	0.136 (0.127)
Controls	No	Yes	Yes
Province FE	Yes	Yes	Yes
Decade FE	Yes	Yes	Yes
Circuit-Decade FE	No	No	Yes
Mean of Outcome	0.153	0.153	0.153
Observations	816	816	816
R2	0.32	0.39	0.49
Within R2	0.04	0.13	0.13

Note: This table presents the estimation results for event studies reported in Figure A2. The unit of observation is province-decade. Standard errors (clustered at the province level) are in parentheses. Interactions between decade dummies and controls are included in estimation but omitted from this table.

** $p < .10$; ** $p < .05$; *** $p < .01$; **** $p < .001$.*

Figure A3: Event Studies, Effect of State-building on *Akuto* Cases



Note: The event study results are based on a panel data set with the unit of analysis being province-decade. Period 5, highlighted with a dashed line, represents 1258-1267. The outcome variable is the probability that an *Akuto* case occurs in that decade.

Table A8: Estimation Results for Event Studies Reported in Figure A3

	(1)	(2)	(3)
	Pr(Akuto)	Pr(Akuto)	Pr(Akuto)
Keigo Province x Decade FE 1	-0.086 (0.085)	-0.045 (0.096)	0.186 (0.416)
Keigo Province x Decade FE 2	-0.108 (0.088)	-0.066 (0.109)	0.186 (0.416)
Keigo Province x Decade FE 3	-0.064 (0.081)	-0.045 (0.096)	0.181 (0.413)
Keigo Province x Decade FE 4	-0.086 (0.085)	-0.090 (0.106)	0.235 (0.412)
Keigo Province x Decade FE 6	0.022 (0.039)	-0.051 (0.068)	-0.492 (0.380)
Keigo Province x Decade FE 7	-0.043 (0.094)	-0.010 (0.112)	0.314 (0.431)
Keigo Province x Decade FE 8	0.349*** (0.108)	0.323** (0.130)	1.15*** (0.430)
Keigo Province x Decade FE 9	-0.130 (0.091)	-0.193 (0.127)	0.245 (0.413)
Keigo Province x Decade FE 10	0.066 (0.128)	0.071 (0.150)	0.192 (0.407)
Keigo Province x Decade FE 11	0.914**** (0.085)	0.914**** (0.104)	1.19*** (0.419)
Keigo Province x Decade FE 12	-0.042 (0.098)	-0.056 (0.126)	0.205 (0.409)
Controls	No	Yes	Yes
Province FE	Yes	Yes	Yes
Decade FE	Yes	Yes	Yes
Circuit-Decade FE	No	No	Yes
Mean of Outcome	0.081	0.081	0.081
Observations	816	816	816
R2	0.49	0.54	0.72
Within R2	0.32	0.39	0.31

Note: This table presents the estimation results for event studies reported in Figure A2. The unit of observation is province-decade. Standard errors (clustered at the province level) are in parentheses. Interactions between decade dummies and controls are included in estimation but omitted from this table.

** $p < .10$, ** $p < .05$, *** $p < .01$, **** $p < .001$.*

Appendix D Comparing the Effects of State-building on Religious, Elite, and Mass Rebellions

Another potential explanation for our results is that the *Keigo* provinces experienced more rebellions simply because the heavy demands of war mobilization placed on the local population drove them to revolt. However, we argue that war burdens were *not* the main cause of rebellions in provinces where active state-building occurred.⁴⁸ If the burdens of war had been the primary driver of rebellions, the *Keigo* policy would have triggered *more* uprisings by warriors, who bore most of the mobilization and fighting, and *fewer* revolts initiated by temples and shrines. These religious organizations bore little of the war’s burdens—they were neither directly mobilized for battle nor tasked with *Keigo* duties. On the contrary, they were among the beneficiaries of the war: the Shogunate compensated their “contributions” to defeating the Mongols—primarily through conducting prayers to subjugate the foreign invaders (*ikoku kofuku*)—via the *shinryo kogyo* policy. This policy supported temples and shrines in reclaiming land estates encroached upon by warriors (Kaizu, 1994). Following this policy, the Shogunate favored religious organizations in a majority of the land disputes in Kyushu involving them and secular elites (Seno, 1958). Thus, if war burdens were the primary cause of rebellions in *Keigo* provinces, we would expect *no* association between state-building and the likelihood of rebellions initiated by temples and shrines. After all, their burden was minimal compared to that of warriors.

To evaluate war burden as an alternative explanation, we quantitatively compare how state-building influenced rebellions led by religious organizations versus those led by secular actors. Specifically, we estimate the effect of state-building treatment on the probability of rebellions initiated by religious organizations (Columns 1–2 in Appendix Table A16), by

⁴⁸While a warrior who participated in the wars against the Mongols did petition the Shogunate for compensation, he ultimately did not launch a rebellion (Conlan, 2010).

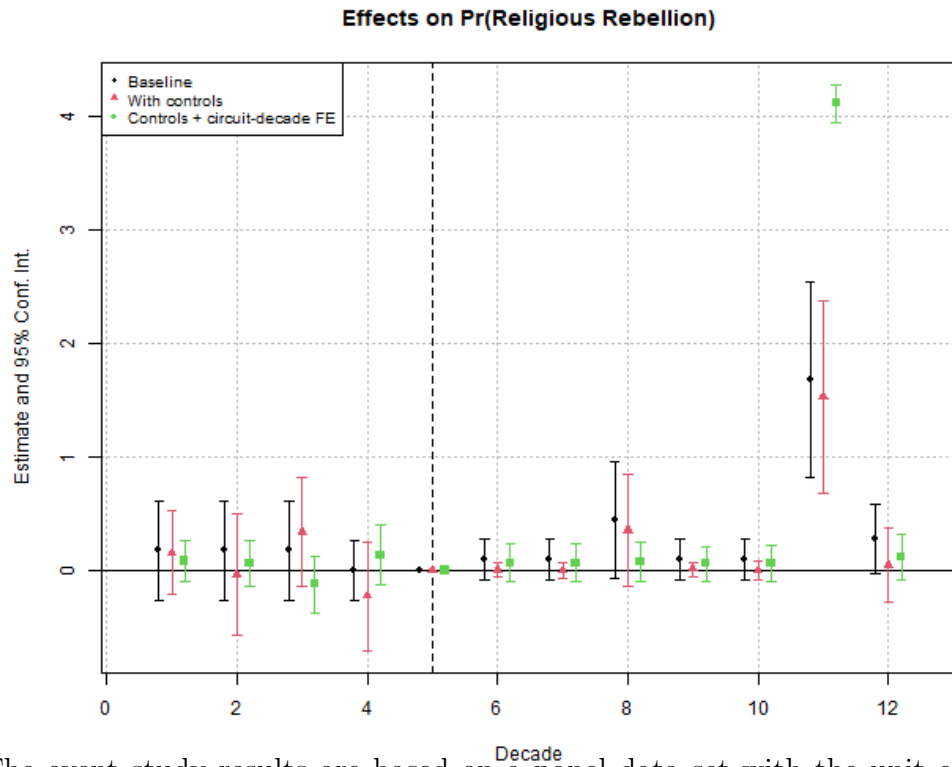
secular elites (Columns 3–4), and by secular masses (Columns 5–6). All outcome variables are standardized to enable a direct comparison of the effect sizes across models.

The results suggest that state-building *increased* the likelihood of religious rebellions—a pattern that cannot be explained by the cost of war. Moreover, state-building had no effect on rebellions by warriors (Columns 3–4), despite their bearing the heaviest war burden. This finding also confirms an argument in recent historical scholarship, which posits that the downfall of the Kamakura Shogunate cannot be attributed to the discontent of local warriors, as they were not heavily involved in the anti-Shogunate movement but instead chose to rush to defend the *Chinzai Tandai*, the Shogunate’s agency in Kyushu (Taka, 2012, pp. 60–61).

While the largest treatment effect appears for rebellions by the secular masses, the event study reveals that *Keigo* provinces did not experience a significant rise in mass rebellions until 1288–1297—after the second and final Mongol invasion ended in 1281—with such rebellions persisting well into the 14th century.⁴⁹ If war burdens were the primary driver of rebellions, we would expect heightened unrest, especially those initiated by secular actors, closer to the periods of active warfare in 1274 and 1281. Instead, our empirical results show the opposite, thus offering no support for the war-burden explanation for our main finding. The grievances of the religious organizations and the masses resulted from the Shogunate’s administrative failure, specifically from its inability to resolve their disputes (see more detailed discussion in section 7).

⁴⁹See Appendix Figures A4, A5, and A6 for the event study results for these three outcome variables. These figures are plotted based on the estimation results in Table A9, A10, and A11, respectively.

Figure A4: Event Studies, Effect of State-building on Religious Rebellions



Note: The event study results are based on a panel data set with the unit of analysis being province-decade. Period 5, highlighted with a dashed line, represents 1258-1267. The outcome variable is the standardized probability of a rebellion instigated by religious organizations occurring within that decade.

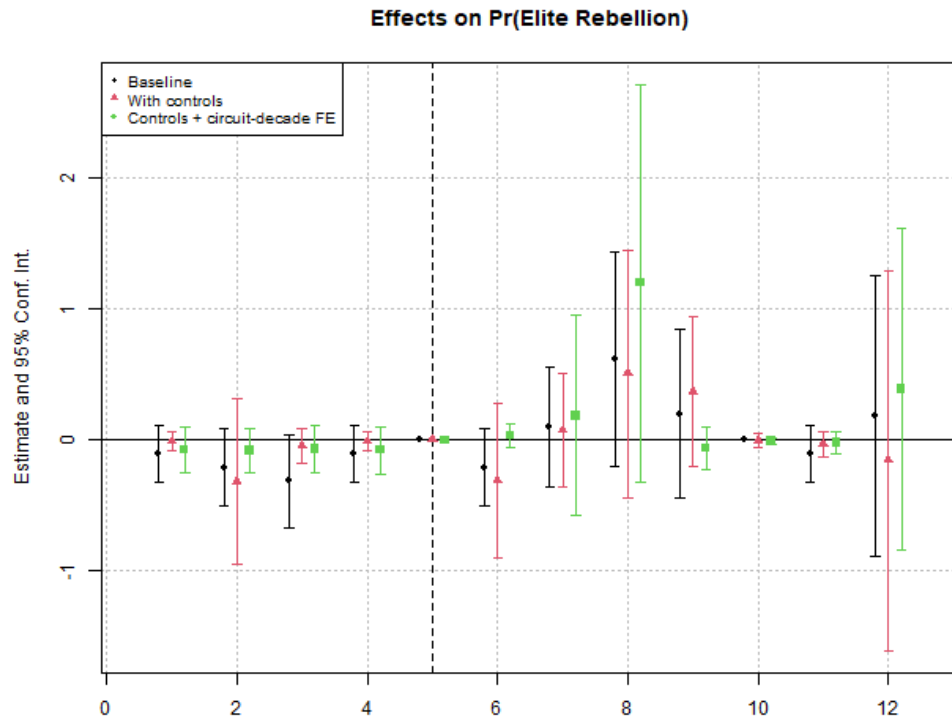
Table A9: Estimation Results for Event Studies Reported in Figure A4

	(1)	(2)	(3)
	Pr(Religious Rebellions)	Pr(Religious Rebellions)	Pr(Religious Rebellions)
Keigo Province x Decade FE 1	0.176 (0.219)	0.151 (0.185)	0.081 (0.093)
Keigo Province x Decade FE 2	0.176 (0.219)	-0.036 (0.270)	0.056 (0.100)
Keigo Province x Decade FE 3	0.176 (0.219)	0.339 (0.243)	-0.121 (0.126)
Keigo Province x Decade FE 4	-2.6e-14 (0.130)	-0.228 (0.240)	0.134 (0.131)
Keigo Province x Decade FE 6	0.090 (0.091)	0.002 (0.031)	0.064 (0.082)
Keigo Province x Decade FE 7	0.090 (0.091)	-0.004 (0.037)	0.064 (0.082)
Keigo Province x Decade FE 8	0.443* (0.260)	0.352 (0.250)	0.074 (0.085)
Keigo Province x Decade FE 9	0.090 (0.091)	0.007 (0.029)	0.059 (0.077)
Keigo Province x Decade FE 10	0.090 (0.091)	-0.007 (0.041)	0.062 (0.082)
Keigo Province x Decade FE 11	1.68**** (0.431)	1.52**** (0.424)	4.12**** (0.082)
Keigo Province x Decade FE 12	0.270* (0.154)	0.046 (0.165)	0.114 (0.101)
Controls	No	Yes	Yes
Province FE	Yes	Yes	Yes
Decade FE	Yes	Yes	Yes
Circuit-Decade FE	No	No	Yes
Mean of Outcome	0.000	0.000	0.000
Observations	816	816	816
R2	0.65	0.69	0.83
Within R2	0.11	0.2	0.28

Note: This table presents the estimation results for event studies reported in Figure A4. The unit of observation is province-decade. Standard errors (clustered at the province level) are in parentheses. Interactions between decade dummies and controls are included in estimation but omitted from this table.

** $p < .10$; ** $p < .05$; *** $p < .01$; **** $p < .001$.*

Figure A5: Event Studies, Effect of State-building on Elite Rebellions



Note: The event study results are based on a panel data set with the unit of analysis being province-decade. Period 5, highlighted with a dashed line, represents 1258-1267. The outcome variable is the standardized probability of a rebellion instigated by secular elites occurring within that decade.

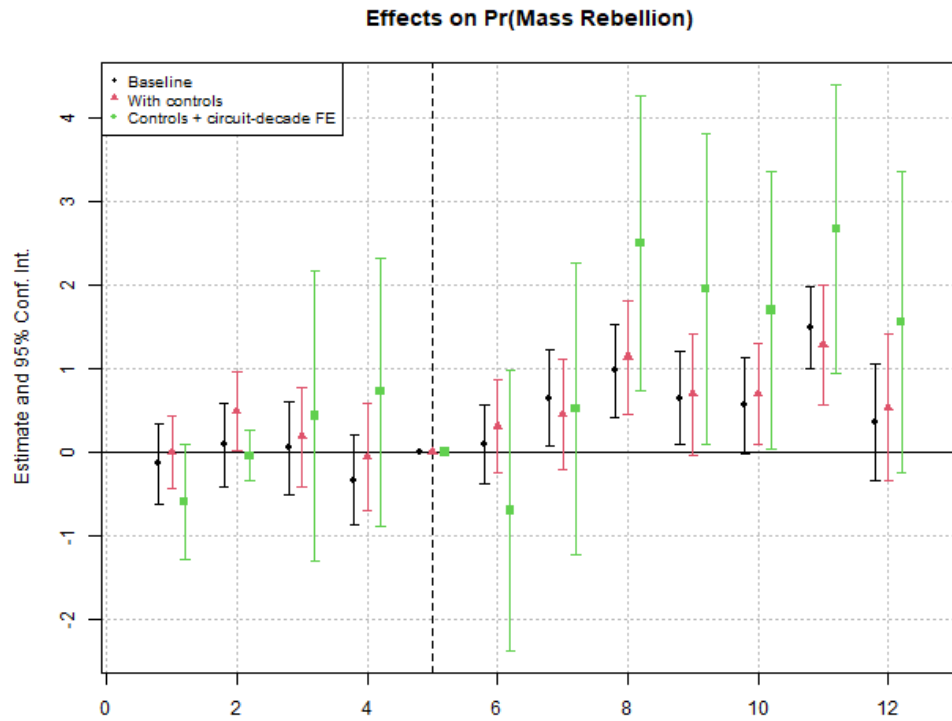
Table A10: Estimation Results for Event Studies Reported in Figure A5

	(1)	(2)	(3)
	Pr(Elite Rebellions)	Pr(Elite Rebellions)	Pr(Elite Rebellions)
Keigo Province x Decade FE 1	-0.105 (0.106)	-0.008 (0.036)	-0.073 (0.089)
Keigo Province x Decade FE 2	-0.211 (0.149)	-0.317 (0.316)	-0.079 (0.085)
Keigo Province x Decade FE 3	-0.316* (0.180)	-0.045 (0.065)	-0.070 (0.089)
Keigo Province x Decade FE 4	-0.105 (0.106)	-0.009 (0.038)	-0.079 (0.090)
Keigo Province x Decade FE 6	-0.211 (0.149)	-0.310 (0.297)	0.033 (0.047)
Keigo Province x Decade FE 7	0.101 (0.232)	0.079 (0.217)	0.186 (0.383)
Keigo Province x Decade FE 8	0.614 (0.411)	0.506 (0.474)	1.20 (0.760)
Keigo Province x Decade FE 9	0.202 (0.321)	0.370 (0.287)	-0.064 (0.084)
Keigo Province x Decade FE 10	-8.33e-16 (5.63e-15)	-0.005 (0.026)	-0.008 (0.014)
Keigo Province x Decade FE 11	-0.105 (0.106)	-0.035 (0.047)	-0.020 (0.043)
Keigo Province x Decade FE 12	0.183 (0.536)	-0.155 (0.728)	0.391 (0.617)
Controls	No	Yes	Yes
Province FE	Yes	Yes	Yes
Decade FE	Yes	Yes	Yes
Circuit-Decade FE	No	No	Yes
Mean of Outcome	0.000	0.000	-0.000
Observations	816	816	816
R2	0.22	0.3	0.43
Within R2	0.02	0.12	0.11

Note: This table presents the estimation results for event studies reported in Figure A5. The unit of observation is province-decade. Standard errors (clustered at the province level) are in parentheses. Interactions between decade dummies and controls are included in estimation but omitted from this table.

** $p < .10$; ** $p < .05$; *** $p < .01$; **** $p < .001$.*

Figure A6: Event Studies, Effect of State-building on Mass Rebellions



Note: The event study results are based on a panel data set with the unit of analysis being province-decade. Period 5, highlighted with a dashed line, represents 1258-1267. The outcome variable is the standardized probability of a rebellion instigated by secular masses occurring within that decade.

Table A11: Estimation Results for Event Studies Reported in Figure A6

	(1)	(2)	(3)
	Pr(Mass Rebellions)	Pr(Mass Rebellions)	Pr(Mass Rebellions)
Keigo Province x Decade FE 1	-0.242 (0.254)	0.032 (0.258)	0.653 (1.16)
Keigo Province x Decade FE 2	-0.120 (0.289)	0.016 (0.352)	0.602 (1.15)
Keigo Province x Decade FE 3	-0.117 (0.236)	-0.058 (0.267)	0.660 (1.15)
Keigo Province x Decade FE 4	-0.242 (0.254)	0.062 (0.262)	0.648 (1.12)
Keigo Province x Decade FE 6	-0.062 (0.169)	-0.172 (0.265)	-1.23 (1.12)
Keigo Province x Decade FE 7	0.918*** (0.323)	0.981*** (0.369)	3.50*** (1.22)
Keigo Province x Decade FE 8	1.04*** (0.337)	1.11*** (0.394)	3.33*** (1.13)
Keigo Province x Decade FE 9	0.120 (0.403)	-0.009 (0.478)	1.41 (1.27)
Keigo Province x Decade FE 10	0.122 (0.376)	0.229 (0.408)	0.636 (1.10)
Keigo Province x Decade FE 11	2.38**** (0.263)	2.14**** (0.432)	3.53**** (1.19)
Keigo Province x Decade FE 12	-0.014 (0.424)	0.163 (0.514)	1.47 (1.21)
Controls	No	Yes	Yes
Province FE	Yes	Yes	Yes
Decade FE	Yes	Yes	Yes
Circuit-Decade FE	No	No	Yes
Mean of Outcome	0.000	0.000	0.000
Observations	816	816	816
R2	0.41	0.51	0.65
Within R2	0.17	0.31	0.27

Note: This table presents the estimation results for event studies reported in Figure A6. The unit of observation is province-decade. Standard errors (clustered at the province level) are in parentheses. Interactions between decade dummies and controls are included in estimation but omitted from this table.

** $p < .10$; ** $p < .05$; *** $p < .01$; **** $p < .001$.*

Appendix E Additional Tables and Figures for Mechanisms and Other Explanations

Table A12: State-building - Number of Disputes - Rebellion

	(1)	(2)	(3)	(4)
	#Dispute	#Dispute	Pr(Rebellion)	Pr(Rebellion)
State-building	0.028*** (0.010)	0.028*** (0.010)	0.054**** (0.009)	0.048**** (0.010)
#Dispute			0.100*** (0.030)	0.094*** (0.030)
Controls	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Mean of Outcome	0.022	0.022	0.042	0.042
Observations	7,684	7,684	7,684	7,684
R2	0.07	0.22	0.23	0.43
Within R2	0	0.16	0.01	0.27

Note: This table shows results with the number of dispute as the mediator. The unit of observation is province-year. Standard errors (clustered at the province level) are in parentheses. Interactions between year dummies and controls are included in estimation but omitted from this table.

** $p < .10$; ** $p < .05$; *** $p < .01$; **** $p < .001$.*

Table A13: The Effect of State-building on Rebellion, Controlling for Jokyu Rebellion

	(1)	(2)
	Pr(Rebellion)	#Rebellion
State-building	0.050**** (0.011)	0.056**** (0.015)
Controls	Yes	Yes
Jokyu Rebellion	Yes	Yes
Province FE	Yes	Yes
Year FE	Yes	Yes
Mean of Outcome	0.042	0.058
Observations	7,684	7,684
R2	0.43	0.7
Within R2	0.27	0.63

Note: This table presents the effect of state-building policy on rebellion. The unit of observation is province-year. Standard errors (clustered at the province level) are in parentheses. Since all Jokyu rebellions occurred within the control group, where Keigo was never implemented, the interaction term between the treatment and the Jokyu rebellion dummy is automatically excluded from the estimation. Interactions between year dummies and controls are included in estimation but omitted from this table.

* $p < .10$; ** $p < .05$; *** $p < .01$; **** $p < .001$.

Table A14: The Effect of State-building on Rebellion, Moderated by the Count of Clans

	(1)	(2)
	Pr(Rebellion)	Pr(Rebellion)
State-building	0.056*** (0.017)	0.056*** (0.018)
State-building * Clans	0.015 (0.343)	-0.150 (0.366)
Controls	No	Yes
Province FE	Yes	Yes
Year FE	Yes	Yes
Mean of Outcome	0.042	0.042
Observations	7,684	7,684
R2	0.24	0.45
Within R2	0.03	0.29

Note: This table presents the effect of state-building policy on rebellion. The unit of observation is province-year. Standard errors (clustered at the province level) are in parentheses. Interactions between year dummies and controls are included in estimation but omitted from this table.

* $p < .10$; ** $p < .05$; *** $p < .01$; **** $p < .001$.

Table A15: The Effect of State-building on *Akuto* Cases, Moderated by the Mean of Dispute Counts before Mongol Invasion

	(1)	(2)
	Pr(Akuto)	Pr(Akuto)
State-building	0.023**** (0.004)	0.016*** (0.005)
State-building * Mean Dispute	0.555** (0.265)	0.646** (0.277)
Mean Dispute	Yes	Yes
Controls	No	Yes
Province FE	Yes	Yes
Year FE	Yes	Yes
Mean of Outcome	0.01	0.01
Observations	7,684	7,684
R2	0.17	0.34
Within R2	0.03	0.23

Note: This table presents the effect of state-building policy on akuto cases. Mean dispute represents the mean of the number of disputes for each province across 1221-1267. The unit of observation is province-year. Standard errors (clustered at the province level) are in parentheses. Interactions between year dummies and controls are included in estimation but omitted from this table.

p < .10; ** p < .05; * p < .01; **** p < .001.*

Table A16: The Effect of State-building on Religious and Non-religious Rebellions

	(1)	(2)	(3)	(4)	(5)	(6)
	Pr(Religious)	Pr(Religious)	Pr(Elite)	Pr(Elite)	Pr(Mass)	Pr(Mass)
State-building	0.058 (0.042)	0.121** (0.056)	0.093 (0.061)	0.064 (0.065)	0.303*** (0.036)	0.216*** (0.061)
Controls	No	Yes	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean of Outcome	0	0	0	0	0	0
Observations	7,684	7,684	7,684	7,684	7,684	7,684
R2	0.26	0.58	0.09	0.21	0.13	0.33
Within R2	0	0.43	0	0.14	0.01	0.23

Note: This table presents the effect of state-building policy on the probability of religious and non-religious rebellions. The outcome variables are all standardized. The unit of observation is province-year. Standard errors (clustered at the province level) are in parentheses. Interactions between year dummies and controls are included in estimation but omitted from this table.

*†p < .10; * p < .05; ** p < .01; *** p < .001.*

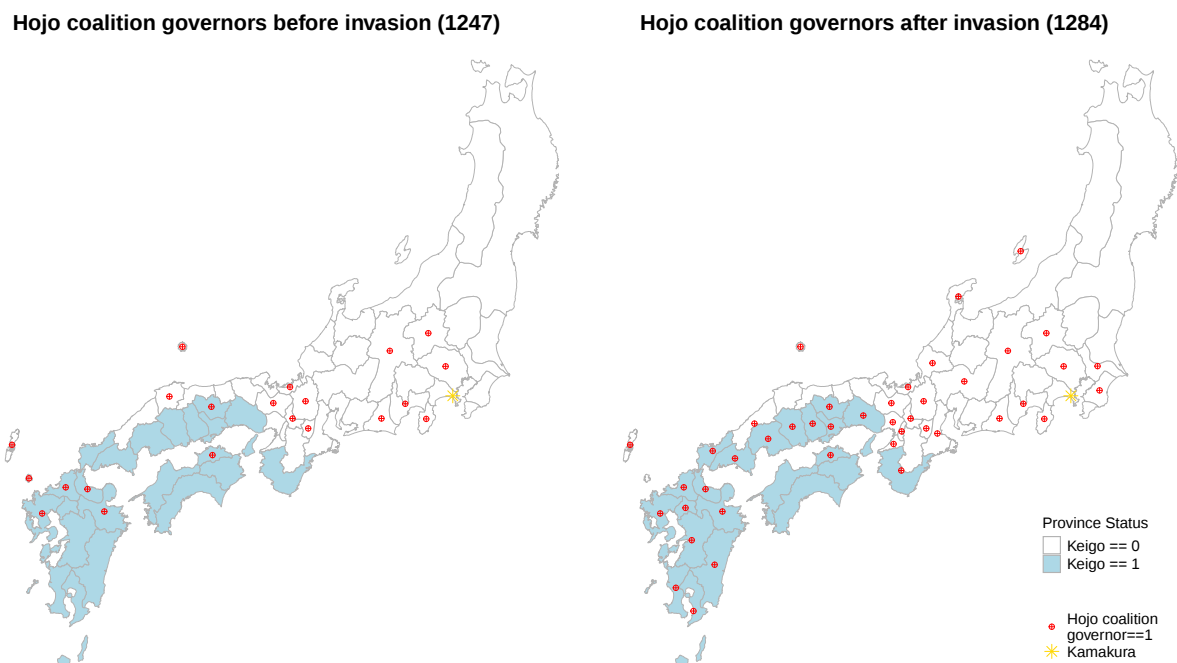


Figure A7: Hojo Coalition Governors before and after Mongol Invasion

Table A17: The Effect of State-building on Jito

	(1)	(2)	(3)	(4)
	Cumulative Jito	Cumulative Jito	New Jito	New Jito
State-building	2.11 (3.48)	-0.555 (5.56)	-0.023 (0.081)	-0.042 (0.101)
Controls	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Mean of Outcome	8.218	8.218	0.083	0.083
Observations	7,684	7,684	7,684	7,684
R2	0.68	0.69	0.02	0.06
Within R2	0	0.04	0	0.03

Note: This table presents the effect of state-building policy on the number of cumulative and new jito. The unit of observation is province-year. Standard errors (clustered at the province level) are in parentheses. Interactions between year dummies and controls are included in estimation but omitted from this table.

* $p < .10$; ** $p < .05$; *** $p < .01$; **** $p < .001$.

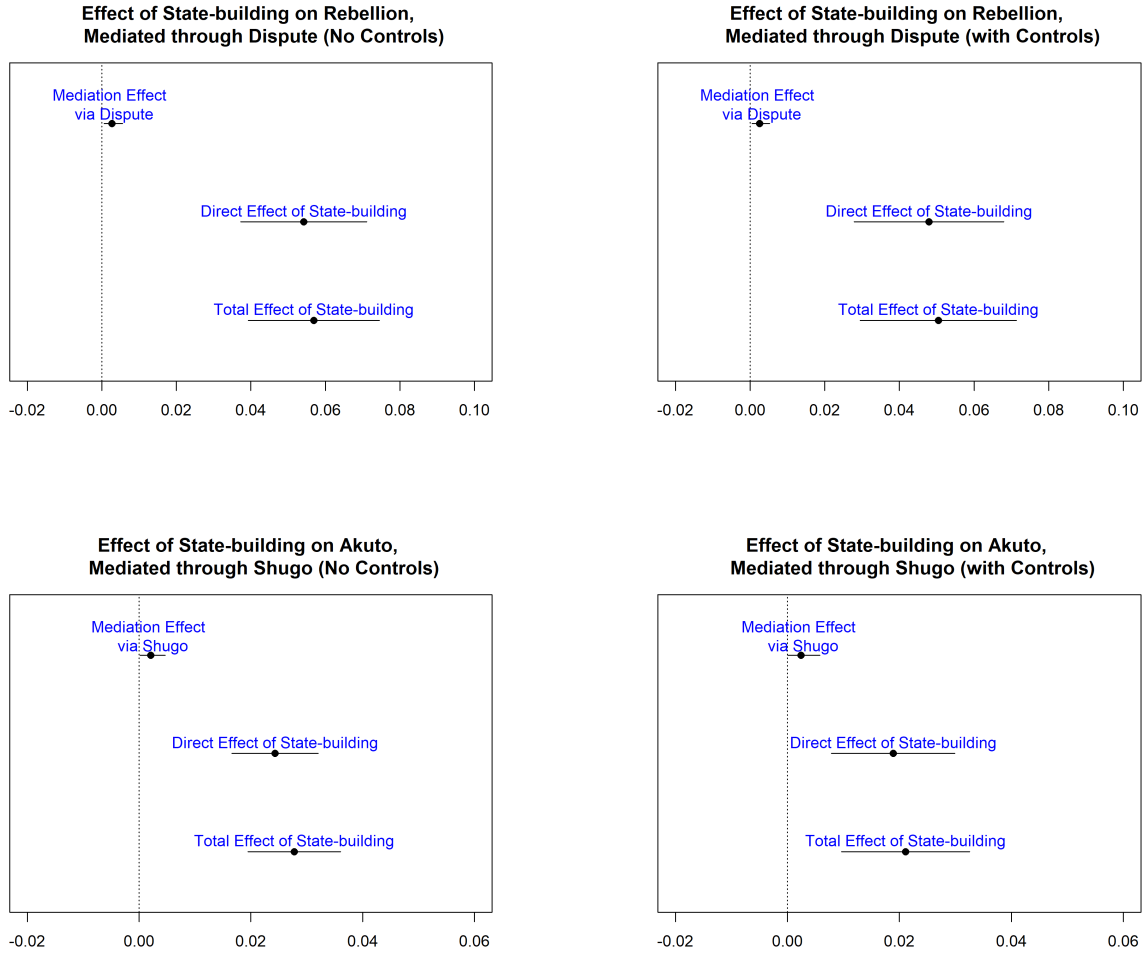


Figure A8: Mediation Analysis Results based on Table 3 and Table 4

Mediation analyses using the approach recommended in Imai et al. (2011), with the average causal mediation effect (ACME) with Dispute (Hojo-aligned Governor, i.e. Hojo “Shugo”) as the mediator for the impact of state-building on Rebellion (Akuto) in the upper (lower) panel. 95 % Confidence Intervals are used.

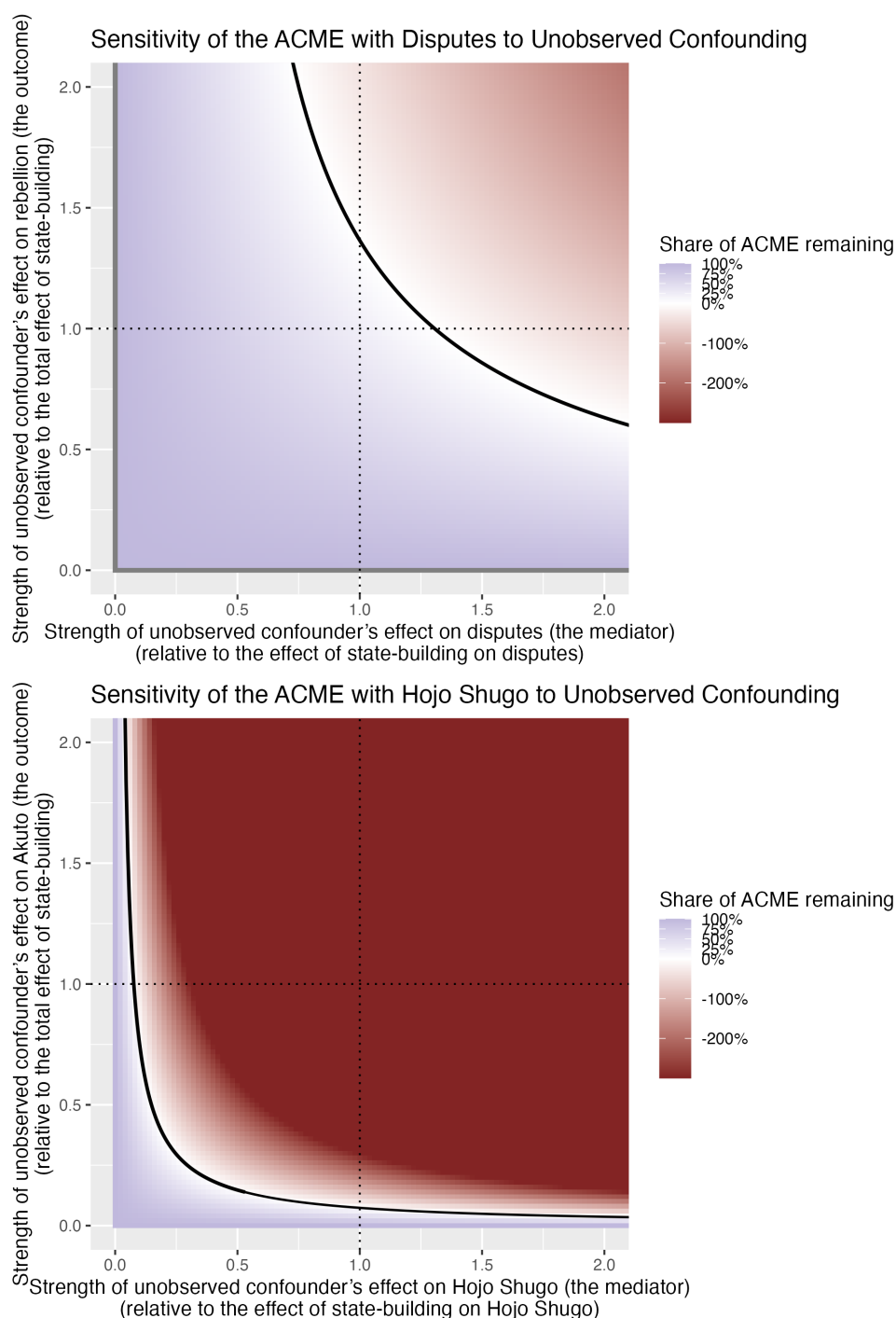


Figure A9: Sensitivity Analyses of ACME for Figure A8

Note: The figure reports analytic sensitivity analyses for the average causal mediation effect (ACME), using the approach recommended in VanderWeele (2010) and VanderWeele (2015). Color indicates the share of the ACME remaining under unobserved mediator–outcome confounding. The horizontal and vertical axes scale the strength of an unobserved confounder relative to the effect of state-building on the mediator and outcome, respectively. The upper (lower) panel reports sensitivity results for the “mediation effect” in upper (lower) panel of Figure A8

Table A18: The Effect of State-building on *Akuto* Cases, Controlling for the Cumulative Number of Jito

	(1)	(2)	(3)	(4)	(5)	(6)
	Pr(Akuto)	Pr(Akuto)	Pr(Hojo)	Pr(Hojo)	Pr(Akuto)	Pr(Akuto)
State-building	0.027**** (0.004)	0.023**** (0.006)	0.308*** (0.094)	0.327*** (0.121)	0.024**** (0.004)	0.019**** (0.005)
Hojo Shugo					0.007** (0.003)	0.008** (0.003)
Controls	No	Yes	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean of Outcome	0.01	0.01	0.501	0.501	0.01	0.01
Observations	7,684	7,684	7,307	7,307	7,307	7,307
R2	0.15	0.29	0.7	0.72	0.17	0.31
Within R2	0.01	0.17	0.07	0.12	0.01	0.17

Note: Model 1-2 present the effect of state-building policy on akuto cases and Model 3-6 show the results with Hojo Shugo as the mediator. The unit of observation is province-year. Standard errors (clustered at the province level) are in parentheses. Interactions between year dummies and controls are included in estimation but omitted from this table.

p < .10; **p < .05; *p < .01; ****p < .001.*

Table A19: State-building - Share of Hojo Manors - Rebellion

	(1)	(2)	(3)	(4)
	Hojo Manor	Hojo Manor	No. Rebellion	No. Rebellion
State-building	0.004 (0.003)	0.004* (0.002)	2.87** (1.22)	3.78**** (0.994)
Hojo Manor Share			-44.4 (46.4)	18.1 (15.5)
Controls	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes
Post FE	Yes	Yes	Yes	Yes
Mean of Outcome	0.01	0.01	3.306	3.306
Observations	134	134	134	134
R2	0.93	0.94	0.84	0.97
Within R2	0.02	0.09	0.05	0.83

Note: This table shows results with the share of Hojo manors as the mediator. 2-period DID. Standard errors (clustered at the province level) are in parentheses. Interactions between year dummies and controls are included in estimation but omitted from this table.

p < .10; **p < .05; *p < .01; ****p < .001.*

Table A20: The Effect of State-building on Rebellion and *Akuto*, Moderated by Pre-1268 Castles

	(1)	(2)	(3)	(4)
	Pr(Rebellion)	Pr(Rebellion)	Pr(Akuto)	Pr(Akuto)
State-building	0.077**** (0.014)	0.071**** (0.016)	0.039**** (0.008)	0.035**** (0.010)
State-building * Pre-1268 Castles	0.084** (0.036)	0.084* (0.042)	0.046** (0.021)	0.053** (0.024)
Controls	No	Yes	No	Yes
Province FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Mean of Outcome	0.042	0.042	0.01	0.01
Observations	7,684	7,684	7,684	7,684
R2	0.22	0.45	0.15	0.33
Within R2	0.01	0.3	0.01	0.22

Note: This table presents the effect of state-building policy on rebellion and akuto cases, moderated by the standardized number of castles built before 1268 (a variable that has mean zero and standard deviation one). The unit of observation is province-year. Standard errors (clustered at the province level) are in parentheses. Interactions between year dummies and controls are included in estimation but omitted from this table.

p < .10; ** p < .05; * p < .01; **** p < .001.*