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(Citation)
Journal of Development Economics,146:102518

(Issue Date)
2020-09

(Resource Type)
journal article

(Version)
Accepted Manuscript

(URL)
https://hdl.handle.net/20.500.14094/90007863

Time Horizon of Government and Public Goods Investment: Evidence from Japan

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May 21, 2020

Abstract

Whether the longer tenure of political agents leads to better public policies is a central question in political economy. Tenure security extends the time horizons of dictators, which may explain economic growth under extractive institutions. This study estimates the causal impact of longer time horizons of local dictators using sub-national data from 17th-century Japan. Local lords at that time faced the risk of transferring their domains by order of the central government. In 1651, the death of the executive leader of the central government caused a policy reform, and it disproportionally reduced the transfer risk faced by particular local lords (insiders) for plausibly exogenous reasons. By digitizing the historical dataset and using the difference-in-differences method, I find relatively greater agricultural investment in insiders' domains after 1651. Supplemental analyses indicate that this effect is driven by the longer time horizon channel rather than the career concern or local experience channel.

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JEL codes: H41, N45
Keyword: Public Goods Provision, Extractive Institutions, Time Horizon Effect

1 Introduction

Whether longer tenures of political rulers, such as dictators, politicians, or bureaucrats, lead to better public policies is a central question in political economy.[1] Some of the studies addressing this question argue that a dictator who expects to govern his or her domain longer has more incentives to develop this domain through public goods investment or state building because of the higher economic returns and more consumption he or she will derive in the long run (Olson, 1993; McGuire and Olson, 1996; Olson, 2000). This argument potentially explains the economic growth achieved by extractive institutions, under which a very limited subset of society, such as dictators, holds political power (Acemoglu and Robinson, 2012). However, this longer time horizon effect is difficult to empirically test rigorously because dictators' time horizons are endogenous. For example, dictators who provide few public goods may have longer time horizons primarily because of military spending, which might be positively or negatively correlated with economic investment, as is the case for North Korea.

In this study, I examine the longer time horizon effect against the backdrop of the Edo period (1603 to 1868) in Japan. The Edo period provides a suitable historical context for studying this effect because of its several unique features. First, during the Edo period, about 250 local lords (daimyo) ruled their domains (*han*) essentially as dictators. In this study, I refer to a local lord and his vassals collectively as local government. They had economic, administrative, and judicial autonomy and were free from central power, that is, the *Tokugawa shogunate* (the shogunate, hereafter). Under the Tokugawa regime, however, there were two powerful measures that the shogunate used to control local lords: (1) forfeit (*kaieki*), in which the shogunate confiscated the rank and domain of a local lord when a lord violated

[1] See Barro (1973), Besley and Case (1995), Schultz (2008), Padró-i-Miquel (2007), Acemoglu et al. (2011), and Aghion and Jackson (2016), for example.

the code of conduct or died without an heir, and (2) transfer (*tempou*), our focus in this study, in which the shogunate moved a local government from their current domain to a different domain to attain political balance and stability. Upon a transfer, a local government was required to give up his current land and received a similar amount of land in a new location. Because transfers could occur during long-term investment projects and the amount of new land that a government received did not account for the investments they had made in their original land, this transfer risk would have discouraged local governments from investing in their current lands. This unique feature, a mixture of decentralized economies and centralized politics, provides an appropriate context to test for the longer time horizon effect.

Second, the setting provides a plausibly exogenous variation in this transfer risk, allowing for an investigation of the longer time horizon effect. In 1600, *Tokugawa Ieyasu*, a powerful local lord who later became the executive leader of the shogunate (*shogun*), won a war that involved all the local lords in the country, leading to the end of civil conflicts in the middle age. With the emergence of the masterless samurai as a result of the war in 1600, he seized political hegemony after the final battle against his opponent lords in 1615. Under his rule, the local lords faced a forfeit risk. This is partly because they were not allowed to adopt an heir on their deathbed. When the shogunate deprived them of their lands, it usually transferred trustworthy lords who supported the shogunate during the war in 1600 (insiders or *fudai*) to take over these lands (panel A of Figure 1). This policy allowed the shogunate to monitor the opposing lords (outsiders or *tozama*), expand the domain of insiders, and avoid revolt by ordering painful transfer of non-trustworthy and powerful outsider lords. As a result, insiders had a higher transfer risk in the early Edo period, as shown in Figure 2. However, the forfeit and transfer policies generated masses of masterless samurai and increased social instability. Following the death of the shogun from natural cause in 1651 and the inauguration of his ten-year-old son as a new leader, some of these masterless samurai planned uprisings against the shogunate. Facing the risk of an uprising by the masterless samurai, the shogunate deregulated deathbed adoption for inheritance, and the forfeit risk dropped. The policy change meant a decline in the transfer risk, particularly for insiders, who were often transferred when a forfeit occurred. The right part of Figure 2 demonstrates this drop in

the transfer risk. Because both the death of the shogun and the identities of local lords (i.e., whether they supported the shogunate in 1600) are plausibly exogenous to economic conditions, I can conduct a difference-in-differences (DID) analysis to identify the effect of a longer time horizon by exploiting variation in their intensity of treatment.

Third, local lords were secure in their positions in the ruling class; they did not know which other local lords would be transferred to their administrative areas (or constituencies) to replace them. Therefore, it is unlikely that local lords had incentives to change their investment levels to favor particular groups of commoners with investments or obstruct other local lords. Such activities are related to, but different from, the longer time horizon effect and do occur in other contexts, such as electoral competitions (Besley et al., 2012) or ethnic conflicts (Besley and Persson, 2011; Burgess et al., 2015).[2] Although the external validity of an explanation about the effect of stability could generally be open to question, the context provides researchers with a natural setting to focus on the longer time horizon effect.

Tokugawa Japan is also a suitable context for studying economic growth under extractive institutions. Extractive institutions are designed to extract income and wealth from one subset of society to benefit a different subset of society. Acemoglu and Robinson (2012) argues that extractive institutions can achieve economic growth, as with the Soviet Union or modern-day China, although it will not be sustainable growth requiring technological change. Tokugawa Japan is one example of an extractive institution. Local lords—the extractors collecting taxes—were a small subset of society, and their positions were hereditary. Major technological change, such as that during the Industrial Revolution, did not occur during the Edo period, although public investments and the economy both grew in the early Edo period. Moreover, within-country empirical analysis of growth under extractive institutions is scarce, except in the case of modern-day China, because such extractive institutions do not typically produce data. However, in the case of the Edo period, political

[2]Ethnic or religious conflicts among local lords were not a significant issue in Japan during the Edo period, with the exception of one major religious conflict, *Shimabara no Ran*, which was an uprising by the Christians in the Nagasaki area demanding freedom of religion. However, this uprising did not cause conflict among local lords; it ended in the 1620s. Thus, it does not affect the interpretation of the main findings.

and economic records are available owing to the political centralization under the shogunate and the data collection work after the Edo period. Therefore, Tokugawa Japan provides an invaluable setting to examine the economic incentives of political actors under extractive institutions.

I digitized several data sources to conduct the analysis. Information on local governments, such as the identities or dates of transfers, is taken from a Japanese history dictionary, Nagahara, ed (1999), which summarizes the records maintained by the shogunate. The boundaries of domains are identified through an investigation of land ownership conducted by the shogunate in the 1640s. Izumi (2008) compiled scattered documents regarding this investigation and showed land ownership at the village village level. For the outcome variable, I digitized Doboku-Gakkai, ed (1936), which lists agricultural public works, such as irrigation systems, along with the names of the counties and the eras in which the works were initiated[3] based on documents dated 1912. Then, I constructed era- and county-level data for the number of initiated works.

Using data from 1615 to 1715, I find that the number of initiated works financed by local governments significantly increased after 1651 in counties governed by local lords who supported the shogunate. This effect is statistically and economically significant, despite the small number of works during the period. Furthermore, the main result does not change qualitatively when controlled for county-specific trends, local geographies, sizes of local governments, lord characteristics, or fixed effects. Thus, it is robust to using Poisson or negative binomial regressions. In contrast, I find no effect on the number of initiated works not financed by local governments over the same sample period and during periods before 1615. This implies that investment by the public sector does not crowd out investment by the private sector and the main result cannot be explained by local economic fundamentals before 1615.

The change in 1651 might have induced other policy shifts that could affect the interpretation of the reduced-form effect; however, I find no disproportional changes in other policies, such as in-kind transfers between the shogunate and local

[3]Japan adopted its own periodization system, and the era changed quite often for religious and other reasons. See Section 3 for details.

governments. Similarly, the main effect survives control for areas that produced the highest-ranked officers, so the career concern effect is likely not a factor (Li and Zhou, 2005; Iyer and Mani, 2012). Furthermore, military investments by local governments remained unaffected, which is inconsistent with the explanation that peace promotes the reallocation of investment from military to economic infrastructure. Finally, I confirm that the effect is not driven by changes in the characteristics of local lords, such as age or experience as a lord. Overall, these results suggest that the reduced-form effect is explained by the decreased transfer risk. I also address issues related to measurement errors in the number of works and land ownership, finding that the main results are robust against any measurement errors.

Although these results imply a transfer risk effect on local public goods investment, the effect might comprise both a longer time horizon effect and other effects, such as local experience and income shock effects. Local governments that remain in the domain longer are likely to have more information or recovered from temporal income shocks due to previous tranfer. Thus, if these effects explain the main result, then the effect of the policy change should be greater for governments with more experience in the local domain. Furthermore, past transfer experiences would absorb the main effect. However, I did not find any supporting results for these channels.

This study is related to several branches of the literature. First, this analysis contributes to the very limited empirical literature on the longer time horizon effect. Although some studies on the impacts of longer time horizons do exist (Wright, 2008b), to the best of my knowledge, Sanchez de la Sierra (2015) is the only such study that uses plausibly exogenous within-country variation. This analysis considers the behavior of bandits in the modern Eastern Congo and reveals that the threat of military action by the central government against bandits leads to more extraction, such as through pillaging.[4] The present study examines a different context with different treatment and outcome variables to complement the existing literature. By 1615, local governments had already monopolized violence and taxation in their territories; thus, their economic investments serve as a more appropriate outcome variable. Also, military threats have additional effects beyond the longer time

[4] Sanchez de la Sierra (2018) studies the impact of mineral prices on the same bandits.

horizon effect, whereas transfers do not have these effects. For instance, military threats may drive a government to increase military investments to fight against opponents in the case of war and, thus, to decrease public goods provision. In another closely related work, Besley and Reynal-Querol (2017) study hereditary rulers with longer tenure as a dynasty, using cross-country data of the 19th to 21st centuries. They find that economies under hereditary rulers perform better if they are weakly constrained due to the reputational concern channel. On the other hand, this study considers a plausibly exogenous variation in time horizon to govern a particular domain within hereditary rulers in the same country and its impact on long-term investment in a pre-modern society.

Second, more broadly speaking, this work contributes to the empirical literature that analyzes the relationship between political tenure and local public choices or economic performance. Li and Zhou (2005), Dal Bó and Rossi (2011), Smart and Sturm (2013), and Titiunik (2016) examine the tenure effect for bureaucrats in China and politicians in Argentina and the United States. Politicians and bureaucrats with longer tenures have other related channels, such as re-election motives, which differ from the longer time horizon effect of the government.[5][6]

Third, my work enriches the literature on economic growth under extractive institutions or autocracies (Acemoglu and Robinson, 2012). Several studies examine factors leading to economic prosperity (Wright, 2008a,b; Besley and Kudamatsu, 2009; Markevich and Zhuravskaya, 2011; Xu, 2011; Jia et al., 2015) or construct new historical GDP estimates to re-investigate economic growth (Brainerd, 2010; Abad and van Zanden, 2016) under these institutions. However, despite their importance, the political factors leading to growth under these institutions are especially under-investigated, with the exception of studies on modern-day China. I uniquely

[5]There are studies investigating the effect of tenure security in various fields outside the political economy literature. Some studies analyze the effect of longer life expectancy on human capital investment (Jayachandran and Lleras-Muney, 2009) or the impact of strong security for farmers (Besley, 1995; Jacoby et al., 2002; Banerjee and Iyer, 2005; Fetzer and Marden, 2017) and slum-dwellers (Field, 2005; Field and Torero, 2006) on their investments.

[6]As related studies, there are studies investigating the impact of a leader's death. They find that the death of an autocratic leader affects capital markets, economic growth, and changes in political institutions (Fisman, 2001; Jones and Olken, 2005, 2009; Besley et al., 2012). Unlike previous studies, I use both a leader's death and lords' identities to conduct a DID analysis to study the effect of a particular policy that is affected by a leader's death.

exploit a new institutional setting—Tokugawa Japan—by presenting the longer time horizon effect as a suggestive mechanism to explain why such institutions might be able to promote economic growth.

Finally, regarding the literature on Japanese economic history, there is a view that the increased security of local lords' domain was crucial to economic development following the civil conflicts in the *Sengoku* period (1467 to 1590) (Elath, 2005; Hall et al., 2014). Kikuchi (1958) points out that the transfer risk might have affected agricultural investments by local governments. The authors discuss these points without conducting empirical investigations, and the key result of this study supports these viewpoints empirically. Along with other recent studies (Sng and Moriguchi, 2014; Steele et al., 2017), I provide new empirical insight on the political economy of Tokugawa Japan.

The remainder of the paper is organized as follows. Section 2 describes the conceptual framework used to examine the longer time horizon effect and discusses other potential effects associated with transfer risk. Section 3 explains the institutional background, data, and descriptive statistics. Section 4 presents the main result as well as the robustness checks and other potential interpretations of the main result. Section 5 concludes the paper.

2 Conceptual Framework

In this section, I elaborate on the longer time horizon effect. I predict that a higher transfer risk will reduce investments in local public goods, and then explain the interpretation of the effect in the present context. The model is almost the same as that of McGuire and Olson (1996), but I assume that public goods provision takes the form of durable investments in order to highlight the importance of a longer time horizon and explain it in the context of the Edo period.[7]

Consider a two-period model with an agricultural private sector and an autocratic government that earns tax revenue every period from a fixed proportion of agricultural output (tax base, or *kokudaka*) in the private sector in its domain. For

[7] Sng and Moriguchi (2014) also follow Olson's model to compare state capacity between Qing China and Tokugawa Japan.

simplicity, assume that the private sector has an inelastic labor supply and no means of investment or savings. Thus, the private sector consumes output after tax. Output in the private sector is a function of the total amount of agricultural land and its productivity. The government can increase both amounts of land and productivity by reclamation, irrigation systems, or dams. It can also choose its consumption and investment levels for public goods in the initial period, contingent on its revenue. If it invests in the first period, the private sector will have higher output and the government higher tax revenue in the second period. The government consumes all the tax revenue in the second period.

The key factor of this framework is that the government faces a transfer risk in the second period. Without loss of generality, assume that the government obtains zero utility if a transfer occurs in the second period. Then, a higher transfer risk implies a higher discount rate, which reduces investment. This is the longer time horizon effect.

Some details about this framework are noteworthy. First, the shogunate in this period used "the annual inspection system" (*kemi-sei*) for taxes and incentives, wherein the amount of tax was a fixed share of the actual harvest. For higher tax revenue under this system, therefore, it was required to increase the size or productivity of agricultural land. Second, the longer time horizon effect is similar to the effect of property rights on investment (Besley, 1995; Besley and Ghatak, 2010). The exploitation risk for individuals and the transfer risk for the government function similarly. Third, this framework assumes that the utility in the event of a transfer is invariant to public goods investment. This assumption is suitable in the present context, as I will explain in the next section.

The outlined framework predicts that a lower transfer risk will increase investment in local public goods. The increased investment, which is interpreted as the longer time horizon effect, could also be related to another channel. To illustrate the point, I abstract from potential conflicts with other governments, which may affect investment.[8] If the government knew that its land would be taken over by an enemy in the event of a transfer, then the enemy would benefit from the investments in the local domain. This channel would not exist because local lords would

[8] Besley and Persson (2011) and Burgess et al. (2015) are related studies.

not know who would take over at transfers. In addition, I abstract from the effects of experience and income shocks. For example, over time, the government might incorporate information to improve yields on investments.[9] Furthermore, immediately after a transfer, the government may be credit-constrained because of the cost of moving. I test these channels by conducting a heterogeneous impact analysis later in the study.

3 Background and Data

In this section, I describe the political and financial structures of the local governments during the Edo period, the nature of transfer policy, and the cross-sectional variation I exploit for the identification, as well as its change after 1651. Then, I describe agricultural investment in this period and the reasons that transfers discourage this investment. The data are described in the following subsections.

3.1 Background: Political Identity, Government Structure, and Transfer Policy

3.1.1 The political structure of the Edo system and the nature of transfer

We consider Tokugawa Japan to be a version of feudalism, with the coexistence of decentralized and centralized political power as a feature.[10] Local lords could autonomously control economic policy. They had their own courts to tackle disputes in their domains and the right to issue paper money. As I introduced in the introduction, I treat the local lords and their samurai vassals as governors and officers of local governments and refer to such organizations as "local governments." The shogunate possessed its own samurai vassals and domains (*tenryo*) and collected land taxes from these domains using their samurai vassals. In both governments,

[9] The political economy literature suggests that a longer tenure may result in increased corruption through collusive relationships with local agents (Campante et al., 2009; Coviello and Gagliarducci, 2017). However, I do not consider local public investment corruption in this study, because it does not benefit any particular type of agent in the private sector.

[10] See Ikegami (1998), Iwahashi (2004), or Steele et al. (2017) for the difference between Tokugawa Japan and the European feudalism.

samurai vassals were responsible for these kinds of administrative works such as collecting tax, managing their court, communicating with other governments, and discussing policies; being warriors, they also prepared for wars. There were no inter-governmental monetary transfers, although there were in-kind transfers for military construction work, which will be discussed later. Thus, the shogunate was not as powerful as modern central governments or the Meiji central government, which followed the end of the Edo period in 1868.

However, the shogunate has noteworthy similarities to present-day central governments—it had the political authority to affect the tenures of local governments. The policy focus of this study is the shogunate's ability to relocate local governments, which is symbolic of its hegemony. With this policy, local governments were at risk of being moved to new locations to swap their land with the local government in the new domain. This may have affected investments in local public goods, given that the local governments would have to give up their land. Entire governments, including local lords' vassals and their families, were transferred.[11] Historical documents indicate that, as a result, local lords born in the Edo period perceived their domains as land that the shogunate asked them to keep rather than as their own land. A local lord born in 1609, *Ikeda Mitsumasa*, wrote in 1651 to his officers that he was taking care of the land of the shogunate. Another local lord born in 1556, *Todo Takatora*, used similar expressions in his will written in 1630 (Ozawa, 2003).

It is important to contrast the politically centralized system in the Edo period with the regionally decentralized authoritarian regime in modern-day China, both of which can be categorized as politically centralized but economically decentralized systems (Xu, 2011). First, as in the Tokugawa system, the central government in Beijing can reallocate the leaders of the subnational governments, but these transfers only involve the leaders and do not entail the moving of the whole subnational government. Second, the Tokugawa system was more economically decentralized. For example, the shogunate collected tax revenues from its territories in Edo (present-day Tokyo) and other parts of Japan, whereas the central government of China collects tax from the whole country. Thus, the shogunate functioned as a local government economically, unlike the Beijing government. Compared with

[11] See Figure A.1 for its illustration.

modern-day local governments, such decentralized local governments in Tokugawa Japan exerted an amplified time horizon effect on investment.

Transfers typically fit into one of two categories: First, when a local lord forfeited his land because he had no son to inherit it, as a result of a post-war process, or because he violated the law for local lords, the shogunate typically transferred an insider lord, who supported the shogunate in the war of 1600, to the forfeited domain (see panel A in Figure 1). This type of transfer required the appointment of a new insider lord (lord 4 in the picture, typically chosen from among the brothers of an existing insider lord or lower ranked officers) or occurred when the shogunate seized a domain (domain C in the picture), which an insider lord (lord 3 in the picture) previously governed. The shogunate tried to decrease the risk of uprising by expanding their supporters' domain and monitoring non-insiders' domains.[12][13] Second, even if land was not forfeited, the shogunate sometimes swapped insiders' land to reduce the risk of revolt (see panel B in Figure 1).[14] In both types of transfer, the shogunate seemed to account for a local lord's talent and age; for example, if a militarily important domain had a new, young local lord following the death of the previous lord, the shogunate would transfer senior insiders to that domain.

This is the key cross-sectional variation in transfer risk we can exploit: In both types of transfers, the shogunate did not transfer outsiders as often as insiders because outsiders were less loyal to the shogunate and more economically and militarily powerful. By transferring outsiders to militarily important areas, or ordering the transfer itself, which is a very harsh policy for local lords, the risk of revolt would have increased. Also, it would have been a good strategy to expand insider's do-

[12] Another category of lords was *Shimpan*, who were close relatives of the Tokugawa clan. Because, like outsiders, *Shimpan* were not affected by a policy change, I regard insiders as a treated group and outsiders and those close relatives together as a control group. I call the latter non-insiders.

[13] The transfers in 1647 are typical examples for panel A in Figure 1: First, *Terasawa Katataka* forfeited his domain in *Karatsu* because he was held responsible for the *Shimabara* conflict in 1638. In a series of transfers, *Okubo Tadamoto* was moved from *Akashi* to *Karatsu*, *Matsudaira Tadakuni* from *Tamba Sasayama* to *Akashi*, *Matsudaira Yasunobu* from *Takatsuki* to *Tamba Sasayama*, and *Nagai Naokiyo* from *Yamashiro-Nagaoka* to *Takatsuki*. Furthermore, the shogunate seized *Yamashiro-Nagaoka*. All transferred lords were insiders.

[14] In 1622, *Sanada Nobuyuki*, a local lord, on his transfer to Matsushiro, wrote to his vassal, *Ideura Tshushima-no-Kami*: "The Shogun transfers me to Matsushiro because it is a military strategic point in the northern area." See the original letter in the Ueda city museum website: https://museum.umic.jp/sanada/siryo/sandai/130001.html

main and allocate insiders to monitor outsiders efficiently (Fujino, 1975). The data show that about two-thirds of the transfers were among insiders, even though only about 40% of lords were insiders. Of the 151 instances of transfers between 1615 and 1651, 94 were transfers of one insider to another insider's domain, 23 were transfers of one outsider to another outsider's domain, and the rest were transfers between an insider's domain and an outsider's domain (Nagahara, ed, 1999).

Whether local lords belonged to pro-Tokugawa or anti-Tokugawa in 1600 is plausibly exogenous to the (growth of) agriculture investment by them after 1615, especially conditional on observable controls. First, at the war in 1600, it was not sure whether Tokugawa would unify the country because outsiders' were also powerful. Therefore, it is unlikely that insiders were skillful in general, expecting Tokugawa's hegemony and increasing agricultural investment, for example. Second, potential confounders will be their geographical distribution or the size of local government after 1651. The majority of insiders are from the Eastern part of Japan and have a smaller government, which might affect agricultural investment through geographic characteristics or their state capacity. However, I am not aware of other potential confounders that affect both political identity and the (growth of) agricultural investment.

3.1.2 The policy change in 1651

Although the transfer and forfeit policies might have helped decrease the risk of revolt by outsiders, the forfeits and transfers to smaller domains[15] often resulted in the emergence of a masterless (unemployed) samurai class (*ronin*).[16] The shogunate restricted the re-employment of these samurai by the new lords because the masterless samurai joined the opponent group of *Toyotomi Hideyori* during the 1615 siege,[17] the final battle for Tokugawa, to complete their hegemony. The bafuku wanted to avoid the potential risk of another rebellion (Kurita, 1928) by mobilizing

[15] Some unfair transfers occurred in which local lords with different productivities were transferred to each other's domains. Furthermore, the productivity accounted for in the case of a transfer may not be the real productivity. See subsection 3.2 for details.

[16] During the period 1615 to 1715, 80.3% of transfers between domains involved a change in domain size.

[17] Fujii (2011) notes that about 55,000 samurai joined *Hideyori*'s group.

those masterless samurai. As a result, these policies generated a large number of masterless samurai, estimated at 400,000 by 1651 (Kurita, 1940), unintentionally leading to potential social instability.

In 1651, the forfeit policy changed. The third shogun, *Tokugawa Iemitsu*, the grandson of the first shogun, died from disease at the age of 48. His death was officially announced to local lords the following day. *Iemitsu*'s son, *Tokugawa Ietsuna*, who at the time was only 11 years old, was designated to take his place. Although the young leader was supported by his cabinet members, his political power had not consolidated. This was a good opportunity for the masterless samurai to put political pressure on the shogunate and protest its severe local government policies. Immediately after the death of *Iemitsu*, several samurai who were masterless owing to the forfeit and transfer policies, along with their supporters, attempted the *Keian* Uprising, which failed. The uprising, together with another failed uprising by other masterless samurai in 1652, the *Jouou* Uprising, led cabinet members to feel threatened. They revised their forfeit policies for local governments to decrease the number of masterless samurai and the risk of a revolt. More specifically, the shogunate allowed local lords aged 17-50 to adopt an heir on their deathbed to avoid a forfeit.[18]

The reduction in forfeit risk in 1651 meant that the risk of forfeit-related transfers automatically decreased. At the same time, although the shogunate did not officially announce such a change, local lords would have expected a similar decrease in the risk of swapping transfers because transfers were also a source of masterless samurai.[19] Because the transfer risk was higher for insiders, the drop in the transfer risk in 1651 was also greater for insiders.

Some anecdotes show that local lords perceived low transfer risk after 1651. The House of *Naito*, a family of insider lords, experienced two transfers in 1622 and 1747. At the 1747 transfer, a letter was sent from *Naito*'s office in Tokyo to

[18]Though child adoption is relatively uncommon in Japan, adult adoption as a succession policy is a traditional culture (Moriguchi, 2010). It is a common practice for elites, such as politicians and family business owners, even today. See Mehrotra et al. (2013) for its economic impacts on family businesses.

[19]The transfer policy was different from other policies in the sense that they were at the shogunate's discretion. Therefore, there was no clear rule to specify in which case lords will be transferred.

their local domain, stating that a transfer had not occurred for more than 100 years, which surprised their office in Tokyo (Hibi, 2011). A diary also documented the process of the unsuccessful coup in 1651 (Kanzaki, 1988). A Confucian scholar, *Ogyu Sorai*, published his work, *Seidan*, in 1726–1727. He commented that "[...] around the time of *Shimabara* conflict in 1638, transfer risk was higher for insiders and it was unfair. [...] Now, there are not so many differences between insiders and non-insiders." Moreover, *Uesugi Harunori* was born in 1751 and wrote in his will that he received his land from his ancestors. Ozawa (2003) compares this will to other wills written by local lords in the early Edo period, noting that the policy change in 1651 affected local lords' philosophies as rulers.

I exploit the policy change in 1651 to identify the effects of a longer time horizon as a time series variation for the DID analysis. To interpret the DID result by using the policy change as a tenure risk effect, I (i) assume a parallel trend and (ii) conclude that the death of *Iemitsu* did not affect other policies, which influenced the local lords variously depending on their identity. To address the parallel trend assumption, I examine whether the result is robust to controls, such as a unit-level linear trend. For the other policy channels, I explicitly look for evidence of the policy change in 1651.

3.1.3 Other relevant events after 1651

Although the change in 1651 was the most drastic policy shift affecting transfer risk during the Edo period, two other events influenced perceived transfer risk after 1651. First, *Ietsuna* died in 1680, and *Tsunayoshi* was chosen as the fifth shogun. He was chosen as a result of a political race among the cabinet members because *Ietsuna* did not have a child. Immediately after his inauguration, he and his cabinet conducted transfers to call his supporter lords to areas surrounding Tokyo. *Tsunayoshi* often punished local lords, particularly insiders, by transfer and forfeit (Fukai, 2012). Therefore, the forfeit and transfer risk temporally increased during his rule (1680–1708).

Second, revisions of the deathbed adoption policy would have also affected perceived transfer risk. As noted above, after 1651, local lords over 15 years but below 50 years were eligible to adopt sons. Furthermore, lords over 50 years became

eligible in 1664, and all lords became eligible in 1683. Therefore, these policy relaxations also reduced the overall risk of forfeit and subsequent transfers.

3.2 Background: Agricultural Investment

The decentralized economic structure may have encouraged local governments to invest in the agricultural sector, the primary source of tax revenue at the time. There are many records of agricultural works financed by local governments, such as developing new agricultural lands, digging irrigation canals and dams, and reclaiming land through drainage. For example, an outsider government in Okayama cultivated new fields in its domain and developed about 300 ha in the 17th century. This government's main domain, Bizen province, increased its real tax base by about 30% from the 1590s to 1728 (Doboku-Gakkai, ed, 1936). The local governments often bore the cost of equipment and forced or hired farmers to work, although the work varied in practice across local governments. They sold the land, after completion of the work, to farmers with several years of tax exemption (*Kuwashita Nenki*) (Kikuchi, 1958). *Oki Shinden*, the largest work in Okayama's domain, cost about 20,000 *koku*, which was around 6% of total output in that domain.[20] The boom in investment was not limited to Okayama's domain; from 1598 to 1873, the cultivated land nationwide increased from 2 million ha to 3.3 million ha.

Transfers would have discouraged such agricultural investments by local governments. Upon a transfer, a government was likely to receive almost the same productivity-adjusted amount of land, but local governments had to give up their returns on investment from their original local domains; if a transfer occurred during work on an investment, this work would not be accounted for. Even if the work was finished at the time of the transfer, productivity gains were not considered because land productivity was calculated based on the 1590 land survey. A collection of local laws and records regarding administration during the Edo period (*Jikata Ochibo Shu*) notes that "newly cultivated land will not be accounted as *de jure* tax base at transfer, so it is not good for local lords." Accordingly, Kikuchi (1958)

[20]The *de jure* size of the domain was 315,000 *koku*. One *koku* was worth about 180.39 liters of rice, which is about the amount of rice consumed per capita in a year.

takes the view that transfer risk must have hindered investments by local lords, even though the issue has not been empirically investigated so far.

3.3 Data Sources

I digitize three data sets to obtain information on transfers, identities, land ownership, and the number of public works for the main analysis. I choose the years 1615–1715 as the sample period for my analysis because in the Kyoho era (1716–1736) the shogunate switched to the ''fixed payment system" (*jomen-sei*), wherein the amount of tax is calculated based on the previous periods (ten years, for example). Under this system, local governments would have less incentive to invest in land productivity.[21]

Before describing each data source, I explain the geographical unit of observation using a conceptual picture in Figure 3. The region, province, and county are traditional geographical units from top to the bottom. The public works data are measured at the county (*gun*) level, shown as 16 small rectangles in Figure 3. On the other hand, transfer and identity data are measured at the domain level, marking out the boundary of a lord's feudal power, shown as a gray-shaded area. Domain borders are sometimes consistent with county borders, as in province 1, but do not necessarily overlap as in province 2. A domain border sometimes goes beyond province borders, as in provinces 3 and 4, though it is placed mostly within the same region.[22] To connect the domain-level data to the county-level data, I use land ownership data to identify the lords of villages and calculate the share of lords for each county.

3.3.1 Iwanami Nihonshi Jinten (Iwanami Dictionary of Japanese History)

Nagahara, ed (1999) is a dictionary of Japanese history and a summarized version of diaries and records maintained by the shogunate, such as *Ryueihinamiki* in Fig-

[21] Although we are not aware of records documenting this change in the tax system for local governments, I use this change in the shogunate as a benchmark.

[22] This discrepancy arises because domain borders were set as a result of civil wars in the 15th and 16th centuries, whereas county borders were set as administrative borders in 701 and had no political meaning during the Edo period.

ure A.2. The dictionary is composed of a list of transfers along with the details of local lords (e.g., names and identities) at the domain level and the years in which the transfers occurred. Using this list, I construct domain-year panel data for transfers and identities. In addition, it records local government sizes in terms of the tax base (*kokudaka*).

3.3.2 Kinsei Zenki Gosondaka to Ryoshu no Kiso Kenkyu (Research on Lords and Their Domains in Early Pre-Modern Japan)

This book (Izumi, 2008) is a collection of the records of land surveys (*Kenchi*) conducted by the shogunate in the 1640s.[23] The shogunate conducted several national land surveys to compile data on the ownership and productivity of land during the Edo period. Thus, this book provides a snapshot of land ownership in the 1640s. Using the names of local lords for each village, I calculate the time-invariant share of domains at the county level to connect domain-level transfer data with county-level outcome data.[24]

3.3.3 Meiji Izen Nihon Dobokushi (History of Civil Engineering Before the Meiji Era)

Doboku-Gakkai, ed (1936) is a collection of recorded public works prior to 1868, edited by the Japanese Academy of Civil Engineering.[25] Chapter 2 of the collection covers agricultural infrastructure, such as dams and irrigation systems, along with their names, locations and counties, the bearers of costs (local government or not), and the eras in which the public works were initiated. Japanese historians conventionally use the era to record time during the Edo period. The era changed 14 times from 1615 to 1715 for religious or other reasons. For example, the name of era was changed if the year was ominous in terms of the Chinese sexagenary cycle, if

[23] To compensate for missing data in the investigations by the shogunate, Izumi (2008) uses data for land surveys by the local governments.

[24] In certain instances, more than one name is listed for a village; thus, I reference the first instance to determine the main owner.

[25] This book is commonly used to track the economic development in the literature of Japanese economic history. See Kikuchi (1958), Miyamoto (2004), Takashima (2017), and Kanzaka (2019) for example.

a new emperor (*ten-nou*) rose to power, or if a natural disaster occurred.[26] I construct panel data on the number of new works at the county and era level based on the financial supporter.[27] Unfortunately, the sizes and costs of works and the name of bearers were missing for many records. To the best of my knowledge, no other existing county-level data, such as production or population, could be used as an outcome. This collection also lists the investment on marine and land transportation infrastructure. Though the number of agricultural investment was much bigger and agricultural production was a more important source of tax revenue,[28] I discuss the effect on these investments as an additional analysis.

Using these three data sets, I construct domain-year data for transfers and political identities and county-era panel data for public works for the main analysis. Because one county had several villages and there could have been more than one domain per county, transfer events and political identities are measured as averages based on the share of owners. Because the main domain was spread over multiple counties but within one province in most cases,[29] I use provinces, not counties, as the unit of clustering to be conservative. However, I make a slight modification when a local government governs more than one province within its main domain.[30] After unifying those provinces, I have 55 provinces as the unit for clustering.

[26] See Table A.1 for the full list of eras and the reasons for the changes.

[27] The book describes implicit groups with different styles of explanations: "normal" and "supplemental" works. Typically, more information is provided for normal works, including financial sources. Location and timing information are presented for supplemental works, and they are listed at the end of each section (sections are allocated for each prefecture). In this study, I use all the works.

[28] For example, rice accounts for 96% of the tax revenue in Kumamoto domain in the mid-18th century, although the commerce sector should have developed since the 17th century, our main sample period (Tachiki, 1995).

[29] A local government's villages spread over 3.9 counties on average. If we ignore villages in detached areas, which I define later in the main text, 83% of local governments' domains are located within one province.

[30] Local governments with an entire province or more than one province as their main domains were called *kunimochi*, and I modify the province borders in line with the list of such governments in Kato (1969).

3.3.4 Other sources to capture inter-governmental transfers and military investments

I use two sources to check whether in-kind transfers from the shogunate to local governments systematically changed after 1651, which might explain the main result. Yoshizumi (1968) shows the list of orders from the shogunate to the local governments with the year and name of the domain, and Kato (1969) similarly lists the support from the shogunate to the local governments.

Also, Kato (1969) lists military investment (castle construction or repair works) by local lords themselves with the names of domains and starting years. I use these data to analyze whether the expected risk of revolt decreased after 1651 and whether insiders reallocated their resources from military investments to economic investments.

3.4 Descriptive Statistics

In this subsection, I present descriptive statistics and conduct a pre-analysis. First, I discuss the frequency of transfers and forfeits and the share of political identity at the domain-year level, which is shown in Panel A of Table 1.

On average, from 1615 to 1715, there were about 230 domains and local government were subject to a 1.87% risk of transfer per year, which is considerably higher than that of a forfeit (0.45%).[31][32] Over a 30-year period, this value translates to a 43% transfer risk. Fewer than half of the domains (45%) belonged to insiders.

The data show a clear connection between transfers and forfeits, particularly for insiders. In column (1) of Panel A of Table 2, I show the results of regressing a transfer dummy variable on the total number of forfeits and its interaction with an insider dummy variable. The results show that if there are more forfeits in a year, the local government is more likely to be transferred, particularly for insiders. This

[31] Forfeit risk owing to the lack of a successor was 0.17%.

[32] Past transfer experience did not decrease transfer risk, and local lords could experience more than one transfer. When I regress the transfer dummy variable on the insider dummy variable and a dummy variable indicating whether the lord or his house started to govern the domain by transfer using data from 1615 to 1651, the latter dummy variable had no statistically or economically significant effect (Table A.2.)

finding is consistent with the reasons for transfers discussed above; the shogunate tried to re-allocate local lords, particularly insiders, when the lords of some domains forfeited their land. The results in column (2) indicate whether the actual forfeit risk dropped after 1651 owing to the policy change after the leader's death. The results confirm a significant drop in the forfeit risk from 0.65 % to 0.35%.

To observe the disproportional effect of a death in 1651 on transfer risk, I present a graph of transfers at the year and domain level. Figure 2 depicts the average frequency of transfers per year and the political identities obtained using raw data and non-parametric estimation. The figure shows a higher risk of transfer for insiders before 1651 and a declining trend in 1651, with a greater drop for insiders. This pattern is confirmed by the regression analysis described in Panel B of Table 2. In column (1), I provide the results of a simple ordinary least squares regression with transfer risk on the left-hand side and an insider dummy and a post-1651 dummy, as well as their interaction terms, on the right-hand side. It shows that insiders (others) had 5.26% (1.79%) of transfer risk before 1651 and 2.00% (0.45%) after 1651. I add year and domain fixed effects in columns (2) and (3), and all the results show that the insider domain was subject to a higher transfer risk before 1651 but that this risk dropped in 1651, particularly for insiders.

I present a similar graph and regression analysis for forfeits in Figure A.3 and Table A.3. I find no clear disproportional effect for insiders, and the results are not robust in the regression analysis. The interaction terms are significantly negative in the regression with year and domain fixed effects. This result is difficult to explain because the forfeit policy was not particularly severe for insiders, and the policy change allowing deathbed adoption was uniformly applied. However, even if the death reduced the forfeit risk for insiders, as column (3) suggests, the main interpretation of the reduced-form analysis remains robust because a forfeit is analogous to a transfer. Changes in both the transfer and the forfeit risk would exert effects in

the same direction.[33]

Next, I present the descriptive statistics for public works, which is shown in Panel B of Table 1. I find that the number of new works is low. In 1615–1715, there were 801 works in total. At the county-era level, only 0.018 works were initiated per year. Roughly 17% of the works were recorded as financed by local governments. Fourteen percent of the works were recorded as financed by other bearers. The bearers of these works were typically wealthy community leaders or exemplary farmers, indicating the substantial power of local communities. The other 69% do not have their bearer information. However, the concern of measurement error on the main analysis will be minimal for the following reasons. There might be some works financed by local governments but not recorded as such; this would only attenuate the effect if its recoding error is exogenous. Also, if the project is conducted by local governments, it will be more likely to be officially recorded. On the other hand, I analyze the effect on works financed by agents other than non-local government. I employ two definitions for the outcome variable in the analysis: In one definition, I only count works recorded as financed by those outside local governments; in another definition, I also include works without bearer information.

I use the land survey to compute the average share of land held by insiders for each county. The measure of insiders' land is given by

$$Insider_i = \frac{\sum_{v \in V(i)} Insider_v}{\sum_{v \in V(i)} 1}, \tag{1}$$

where i is the county, $V(i)$ is the set of villages in county i, and $insider_v$ is the average of the dummy variable that takes a value of one if village v is owned by an insider's government during 1615 to 1651. I take the average over this time because, for example, certain domains had lords with different identities in the case of a new local lord following a forfeit.

[33]The forfeit policy in the main text refers to the full forfeit policy, under which an entire domain was taken by the shogunate. The shogunate could also take only part of a local lord's land (*genpou*). From 1615 to 1651, the risk of a partial forfeit was 0.98%, and the risk was higher for insiders. I abstract this distinction from the main analysis because the partial forfeit risk did not drop disproportionally, as the transfer risk did; because the partial forfeit risk is about one-third of the transfer risk; and because the mean change in the domain size at these partial forfeits was only 0.23%.

Figure A.4 depicts the distribution of $Insider_i$, and approximately 60% of counties have no insiders. The average of $Insider_i$ is 0.19, which is lower than the insider share in the number of domains. We obtain this finding because insiders' domains are smaller and there are non-government owners. Non-government owners (neither insiders nor non-insiders) comprise the shogunate and religious groups owning temples and shrines. These owners were not affected by the change in the transfer policy and are considered as non-insiders when calculating the insiders' share.

To demonstrate the geographical pattern, I construct the province-level average insider's share, as shown in Figure A.5. Insiders are mainly in the eastern and northern areas, as expected, because they were supporters of Tokugawa, who was based in Tokyo. However, insiders were spread across the country. Because different regions may have varying economic time-variant conditions, I control for region-level fixed effects and their interactions with the post-1651 dummy to examine whether this spatial imbalance affects my main result.

Note that the data do not cover the entire nation, as shown in Figure A.5. First, I focus on the three main islands of Japan by excluding the territories of (*Hokkaido* and *Okinawa*), which were incorporated into the national territory of Japan only after the 19th century, and isolated islands. In addition, nine provinces were excluded because of missing data or the inability to establish a time-consistent unit of observation owing to splits and mergers. The data set was composed of 62 provinces. The full list of provinces is presented in Table A.4 in the appendix.

4 Results

4.1 Main Results

First, I estimate the reduced-form effect of the policy change on the number of newly initiated works. Accordingly, I estimate the following equation:

$$\text{Outcome}_{it} = \alpha \text{Insider}_i * \textit{After 1651}_t + \eta_t + \mu_i + \epsilon_{it}, \tag{2}$$

where i is a county, t is an era, and η_t and μ_i are era and county fixed effects, respectively.[34] Because this is a simple DID estimate, I need to assume parallel trends to interpret the relationship as causal. However, other factors may possibly correlate with political identity and have time-variant effects on the outcome. I conduct robustness checks by controlling for a county-wise linear trend and the interaction terms of the post-1651 dummy with region-level fixed effects. I allow for the correlation of ϵ_{it} within the same provinces, as noted above.

Panel A of Table 3 presents the results. Column (1) shows that the number of new works financed by local governments was affected by the policy change. The coefficient is 0.0041, indicating that when a county fully governed by insiders is compared with one fully governed by non-insiders, the number of works increases by 0.0041 per year. This effect is economically significant because, on average, there are only 0.0031 works per era by local governments during the sample period. The point estimate does not change when including a county-level linear trend and region-level fixed effects that interact with the post-1651 dummy variable, though the significance decreases, as shown in columns (2) and (3).

I also present the graphical DID analysis conducted using the era-wise coefficient of Insider_{ij} in Figure 4.[35] The pattern indicates no clear systematic differences before 1651. After 1651, the coefficients fluctuate though they are imprecisely estimated. Still, relevant policy changes are largely consistent with the fluctuation of the coefficients. For example, during the era of *Tsunayoshi*, from 1680 to 1709, the point estimate sometimes shows negative signs. The most negative effect is in the era from 1681, a few years after his inauguration. This is potentially explained by his increased forfeit and transfer to insiders. As for the era from 1684, showing a large positive effect, the age restriction on deathbed adoption was removed in 1683, which temporally reduced the perceived transfer risk for insiders and magnified the effect. I also interpret the large point estimate in the era starting in 1655 and 1658 as

[34] County fixed effects are almost collinear with the boundaries of insiders' domains because these boundaries did not change much over time even though local governments changed within insiders' domains. Therefore, we do not need to add local government fixed effects to remove endogeneity to estimate α.

[35] I also show the level of each era fixed effect, the sum of era-level fixed effects, and the era-wise coefficients of Insider_i in Figure A.6.

a consolidation effect; as *Ietsuna* became older and his political power became consolidated, local governments expected no further policy changes regarding forfeits and transfers, and their perceived transfer risk declined.[36]

The results in columns (4) quantify the effect of a transfer risk using $\text{Insider}_i * \textit{After 1651}_t$ as an instrumental variable. I conduct a two-stage least squares estimate using the county-level average transfer probability for each era as the treatment variable. I find that an additional one percentage point (pp) of transfer risk reduces public works per year by 0.0018 on average though its significance drops.[37]

Political identity is not randomly assigned and may affect the main result. To assess this possibility, I add three key controls to the main specification: flat terrain, the rice suitability index, and local government size. Because of the development of paddy fields in flat areas due to technological progress in civil engineering in the 17th century(Kikuchi, 1958), I employ flat terrain, which may affect the outcome. I also control for the rice suitability index, which would more directly affect local investment opportunities. However, I have no prior belief regarding the relationship between political identity and geographic variables. I add local government size because insiders were originally Tokugawa's vassals and tended to have lower tax revenues than outsiders did, although it remains unclear why the impact of local government size on public goods investment varied by period.

Given the above concerns, I rerun the regression using the three controls described above: $\textit{FlatLandShare}_{ij} * \textit{After1651}_t$, $\textit{RiceSuitability}_{ij} * \textit{After1651}_t$, and $\textit{logGovernmentSize}_{ij} * \textit{After1651}_t$. $\textit{FlatLandShare}_{ij}$ is the county-level proportion of flat land, which is defined as land at less than a two-degree gradient. The measure is constructed using 1 km $\times$ 1 km mesh-level slope information, which is available from the National Land Numerical Information Download Service. I choose 2.86

[36] Figure 4 also shows a drop during the era starting in 1648, which may overestimate the effect owing to mean reversion (Ashenfelter's dip). However, the main effect survives even with the inclusion of control variables for a lag of the outcome variable or an *Insider * 1648* dummy in the main specification, which I add in Table A 5.

[37] That is, with a 2-pp increase in transfer risk from the mean-level outcome variable, no investment would start. This will be a plausible number. If the lord has a 2-pp transfer risk, he is likely to stay in the same domain for 20 years at 33%. This will be a serious risk for long-term investments: Doboku-Gakkai, ed (1936) recorded some of the projects' ending years; the average (median) project completion period was 22 (5) years. They will need even more years to yield positive returns.

degrees as the threshold for determining whether an area is flat or steep.[38] Figure A.7 shows the area with a slope of less than 2.86 degrees; roughly speaking, this area corresponds to plains and basins. $RiceSuitability_{ij}$ is the average rice suitability using gravity irrigation and intermediate inputs provided by the FAO-GAEZ database. To calculate $logGovernmentSize_{ij}$, I use the average size (*kokudaka*) of local governments in 1651, recorded in Nagahara, ed (1999), and construct a county-wise variable using the government's share in the county.

In Panel B, Table 3, I show the correlations between these variables and the share of insiders at the cross-sectional level. The results in columns (1) to (4) indicate that the government size and the flat land share are negatively and positively correlated with the share of insiders, respectively, but the rice suitability index does not exhibit any significant correlation. These differences in flat land share and government size might affect the main result. Therefore, I must control for this variable in the robustness checks. I also control for rice productivity to reduce standard errors.

The results are presented in column (5)–(7), Panel A of Table 3. I control for the above-mentioned variables and find that the relevant coefficient increases. The log of government size has a significantly positive coefficient; because the local government of insiders was smaller than that of outsiders, the coefficient *increases* when I add this control. Overall, non-random assignment does not bias the main result upward. Thus, the positive reduced-form effect will not stem from this bias.[39][40][41][42]

[38] The Ministry of Agriculture, Forestry and Fisheries sets 5/100 (approximately 2.86 degrees) as a criteria to categorize paddy fields as rice terraces (paddy fields in steep areas), which was a common farming practice during the Edo period.

[39] I obtain similar point estimates when controlling for the interaction of geographical and political control variables with the full set of era fixed effects. I omit the result for the sake of brevity.

[40] I also check the robustness to the number of works conducted before t in Table A.6. This value may decrease the incentive to start a new work owing to the lower marginal return, but the main result is robust even after including this control.

[41] Furthermore, I also check robustness to excluding mixed-rule counties, whose insider measures are neither 0 nor 1. The implicit assumption of my main analysis is that villages are equally weighted and the potential return to agricultural investment is heterogeneous across villages, but this assumption may not hold. However, even if we exclude the mixed counties from the DID analysis, the point estimates do not change substantially, although the standard errors increase. See Table A.7.

[42] Because initiating public works is a rare event, I also conduct Poisson and negative binomial regressions as robustness checks in Tables A.8 and A.9 and confirm the main result.

As an additional outcome, I use the number of public works initiated by those outside the local government, such as rich farmers. I use two definitions for the outcome variable: In panel A of Table 4, I count public works recorded as financed by agents other than local governments and having no record about their bearers. In panel B of Table 4, I only count public works recorded as financed by agents other than local governments. Columns (1)–(4) in both panels of Table 4 show the result with the same specification as in Table 3. I observe no significant effects or patterns. Therefore, it seems unlikely that the main result can be attributed to a strong economic boom in insiders' areas after 1651, where both public and private investment increases. This result does not support the view that investment by the public sector crowds out investment by the private sector as well.

Similarly, I compare the balance and trends before 1615 in terms of these works and provide the results in columns (5)–(7). The political borders were not stable before 1615. Therefore, I must interpret these regressions as the effects of geographical rather than political differences between insiders' regions and other regions before 1615. These comparisons imply that insiders' areas after 1615 were exogenous to economic advantages before 1615.

I also analyze the effect on transportation infrastructure as a major non-agricultural investment. Because the land transportation infrastructure did not advance during the Edo period (Doboku-Gakkai, ed, 1936), I count the number of new projects of marine transportation financed by local governments using Doboku-Gakkai, ed (1936). I did not find a robust effect on this outcome.[43] This is because the tax revenue from the non-agricultural sector amounted to a small fraction and the incentive to invest was much higher for the agricultural sector.

4.2 Robustness Checks: Other Changes Before and After the Policy Change

Although the main result is robust to additional controls, I address other potential channels that could explain the reasons underpinning the impact of the policy change.

[43] See Table A.10 for the result.

Thus far, I have observed the effect of a policy change on the number of initiated works and have interpreted it as the effect of tenure risk. However, to attribute identity effects to tenure risk, one needs an exclusion restriction, the assumption that the death of a shogun does not cause another policy change, particularly one that disproportionally affects different identities.

The other four policies besides the transfer policy that the shogunate could impose to intervene in local governments do not explain the reduced-form effect. First, in 1615, the shogunate set up a general law (*Buke Shohatto*) for local lords, specifying ritual and military rules that prohibited, for example, the construction of new castles. This law was uniformly applied to insiders and non-insiders; relevant to the period after 1651, it was revised in 1663 and 1683. The 1683 revision removed the age restriction regarding sons-in-law, as explained above. However, the 1663 revisions involved changes regarding, for example, Christianity and did not affect the incentive to invest in public goods. These revisions were uniformly applied regardless of political identity. Thus, they cannot explain the main result.

Second, the alternate attendance system (*sankin kotai*) does not explain the reduced-form effect. In this system, local lords had to stay in Edo for several months and leave their wives and children behind as hostages upon return to their local regions. This policy was customary since the beginning of the Edo period and was formalized in 1635 when the general law was revised. It may have discouraged investment because of the transportation cost, but there was no policy change to this effect in 1651, and the 1635 formalization was particularly severe for insiders.[44] Thus, the main result was not driven by the policy.

Third, the shogunate could order a local government to incur the cost of public works, but the number of orders did not change before and after 1651. Such public works were executed as in-kind transfers whereby local governments would provide labor to construct or repair castles or temples. In a limited number of cases, the shogunate financially supported the local governments in building castles. There was no explicit written rule for these in-kind transfers. Thus, I check the actual data regarding these in-kind transfers to identify the related policy change in 1651.

[44]Prior to the formalization, there was no such expectation of insiders; however, after 1635, this practice became mandatory.

Yoshizumi (1968) listed orders from the shogunate to the local governments along with information such as the start year and the name of the lord for each work. These orders were not financially supported by the shogunate. Using this list, I constructed a year- and domain-level panel data set composed of the number of new orders for 1615 to 1715. Figure A.8 depicts the identity-wise average number of new orders in each year. No systematic change seems to occur in 1651, and I confirm this conclusion using a simple DID specification and a specification with a domain-level linear trend (Table A.11). Therefore, this channel is unlikely to explain the reduced-form effect.

Fourth, the shogunate supported local governments in building or repairing military infrastructure, but the data suggest that this support does not affect the main result. Kato (1969) listed the castles that were built or re-built by local governments with support from the shogunate. According to this list, from 1615 to 1715, there were only seven cases of assistance from the shogunate. Prior to the policy change, there were four cases, three of which were for insiders. Following the policy change, there were three cases of assistance, all for an insider. The policy change does not coincide with a change in the allocation of support across identities, and the amount of assistance was small. Thus, this channel should not explain the main result.

The death of the shogun may have affected two other political situations for local lords, but the data do not support either of them. First, because only insiders could obtain positions in the shogunate, such as cabinet positions,[45] the death of the shogun may have caused a political race among insiders.[46] A political race could have a positive or negative effect on the level of public goods investment. If public goods investment signified talent, then local lords would likely have invested greater efforts to attain higher positions (career concern effect), which is our main concern. However, a focus on the political race rather than on local governance may

[45]There were several positions for insiders other than cabinet positions (*Roju*): *Kyoto Shoshidai*, *Jisha Bugyo*, *Wakadoshiyori*, *Sojaban*, *Osaka Jodai*, and *Osaka Joban*. Note that they were cross-appointments: even if an insider was appointed to one of these positions, the insider continued to serve as a local lord. *Roju* was the highest position.

[46]Because appointments to these positions did not entail any payments, the positive motivation for receiving these appointments included indirect revenue through strong or intrinsic political power.

have decreased local lords' chances of receiving higher positions.

I can check this possibility by directly controlling for the interaction between "after 1651" and the number of cabinet members (*Roju*) produced by each county from 1615 to 1715. If a career concern effect is present, counties producing cabinet members should have larger numbers of works. Because transfers occurred at the same time as or before appointments to the cabinet, calling insiders to the area surrounding Tokyo, I identify the domain that each lord owned before the transfer as the one that produced cabinet members. I add this control to the regression presented in Table 3, and I show the results in column (1) in Table 5. The cabinet member effect shows negligible and insignificant point estimates. I find similar results when breaking down this variable into the number of cabinet members during 1615–1651 and during 1651–1715 in columns (2)–(4). The only significant coefficient appears in column (3), but this coefficient is negative, which is the opposite of our concern. The result does not change if counties with only non-insiders are excluded in column (5). Therefore, the results do not support the view that the main result is explained by the political race for promotion.[47]

Second, our main result may be driven by a change in local governments' attitudes toward military expenditure after 1651. For example, prior to 1651, military expenditures to prepare for a possible war were greater for insiders than for outsiders. Military tensions may have eased due to the policy change, and insiders might have started investing their revenue in public goods instead. This concern should be taken seriously given that, prior to 1600, certain insiders were militarist groups under Toyotomi's hegemony. I examine this possibility using a list of castle construction and repair works, as summarized in Kato (1969). Castles were major military investments at the time, although such investments were prohibited by the general rule *de jour*. Thus, I can analyze how military investments differed across political identities and periods. There were a total of 18 castle construction or repair

[47] Another test would be to regress the promotion indicator in year t on the number of public works that local lords implemented by year t to see whether investments help with promotion. However, because our public work data cannot identify the starting year of a work or the lords who implemented the work when transfers occur during the era, we cannot perform this regression.

works between 1615 and 1715.[48]

I conduct a regression analysis using the following specification:

$$Castle_{it} = \alpha_0 Insider_{it} + \alpha_1 Insider_{it} * After\ 1651_t + \eta_t + \epsilon_{it}, \tag{3}$$

where i is the domain, t is the year, and $Castle_{it}$ is the number of construction or repair works initiated in domain i at time t. I add domain-level fixed effects and insider-specific trends as robustness checks. Table A.12 shows the results and indicates no systematic pattern between identities and periods. The point estimate of the interaction term is positive, implying that, over a longer time horizon, other types of local public investments, such as castles, also increase. In addition, α_0 is insignificantly negative, which is inconsistent with the above discussion that certain insiders were originally militarist groups and, thus, prioritized military expenditure, particularly prior to the policy change. This channel cannot explain the reduced-form effect.

Third, the change in the forfeit policy in 1651 might have generated senior or experienced insider lords because an insider lord was typically transferred to take over the forfeited domain, and his previously governed domain was then taken from his younger brother, who would eventually become a new local lord. I analyze the effect on the (average of) lord's age and his years of experience as a lord, including experiences in any other domains, in column (1) and (2) in Table A.13.[49] We do not find a significant effect on these, and point estimates show the opposite direction from our concern. The main results are robust to including these controls in column (3) and (4). When we control for lord fixed effects (weighted by the share of villages governed by each lord) in column (5), the effect becomes insignificant. However, this is due to heterogeneous impacts rather than bias: *Experience as a Lord* is short (14.3 years on average) and the specification with lord fixed effects discards the effect for many local lords who only govern in the pre-1651 period or post-1651 period. Change in individual expectations may be greater between different lords governing before and after 1651 than for the same lords governing across the two

[48] I excluded a few works because their first year of construction or repairs predated the establishment of the corresponding domain.

[49] I use Ueda et al., eds (2001) to collect their birth year information.

periods. In fact, in column (6), I use a simple specification by excluding counties governed by those lords and find similar point estimates. Columns (7)–(8) show the results for a project not supported by local governments with lord fixed effects, echoing the main results.

Taken together, these results suggest that the reduced-form effect of the policy change (Table 3) is not driven by intra-governmental transfers, the local government's attitude toward the military, military expenditure, and changes in lord-level observed and unobserved characteristics, for example.[50]

4.3 Robustness Checks: Data Quality

Although I interpreted the reduced-form effect as the effect of transfer risk, certain complications arise because the public works data were compiled after 1868 from documents of pre-1868 works. First, small-scale works are likely to be unaccounted for and, thus, the result should be interpreted as the effect of numbers of works that are large enough to be recorded. Second, it is possible that transfers affected the probability of public works being recorded because local government documents may have been lost during transfers. If so, the number of works is not subject to the reduced-form effect but to the probability of works being recorded, which is problematic. To see whether this channel explains the main result, I add the future number of transfers. If the coefficient of the interaction term does not decrease when these variables are added, the expectation channel is operative because the interaction term controlled for future transfer variables does not directly affect the documentation probability. I confirm, by including the future transfer variables, that the effect of the interaction term does not change, nor does this concern drive

[50] As for our concern, fewer transfers might increase the size of government and facilitate investments because transfers often entailed a decrease in government size. This is unlikely to be the channel, because, in 1615–1651, insiders relatively increased the kokudaka in 1615–1651, a good proxy of the number of employed samurai. Furthermore, it was stable in 1652–1716. This is probably because (1) insiders could increase the number of their officers by hiring local elites when they were transferred to a bigger domain. Thus, a higher transfer risk for insider does not necessarily mean smaller number of employed samurai on average. (2) Well-known cases of transfers to a smaller domain in 1615-1651 are cases of outsiders, such as the case of *Fukushima Masanori* (498k *koku* to 45k *koku*) in 1619.

the main result.[51]

In addition, I cannot track the share of insiders in each county over the years, because the land survey data are limited to a single time point. In general, the domain boundaries are stable, and detached areas are an exception. Detached areas were commonly used as adjustments when transfers occurred among local lords, and they had varying productivity levels. Thus, the owners of counties with detached areas were more likely to vary over time. Because transfers generally occurred within domains with the same identity, the county's identity was unlikely to change. Therefore, I do not believe that time-variant owners affect the main result. Furthermore, the measurement error has an attenuation bias if it is classical, and thus the estimated positive coefficient is lower bound. Nevertheless, to be rigorous, I control for the share of non-detached areas, defining the variable as $Mainland_v$, which takes a value of one if the village was owned by a local government whose main castle was in the same or adjacent province and zero if not. It also takes a value of one if the owner differs from the local government as, for example, in the case of ownership by the shogunate or temples. Similarly, I define the county-level counterpart $Mainland\ Share_i = \frac{\sum_{v \in V(i)} Main\ Area_v}{\sum_{v \in V(i)} 1}$, and then control for its interaction term and $After\ 1651_t$. The point estimates do not change substantially, implying that the issue of time-variant owners does not affect the main result.[52]

Finally, I account for the missing information of outcome variable. Doboku-Gakkai, ed (1936) uses a similar but earlier publication Norinsho, ed (1927), but some projects from Norinsho, ed (1927) are dropped. I also add projects supported by the local government only listed in Norinsho, ed (1927) to the main outcome variable, but the result does not change substantially.[53]

[51] Table A.14 presents the results. In column (1), I add the number of transfers during eras $t+1$, $t+2$, ..., $t+5$ to the main specification. However, the coefficient is similar to the main result. In columns (3) and (4), I extend the future transfer variables to $t+10$ and $t+20$, but the result does not change.

[52] The results are presented in Table A.15. Columns (1) and (2) correspond to columns (1) and (2) in Table 3. Here, I still find that the reduced-form and estimated effects are greater in Table 3. Note that $Mainland\ share_i * After\ 1651_t$ has negative signs in columns (1) and (2), suggesting that a greater number of insiders own detached areas, which is natural, because detached areas were used for transfers. In columns (3) and (4), I omit counties with any non-mainland villages.

[53] See Table A.16.

4.4 Time Horizon and Other Explanations

This study reveals the effect of transfer risk on the number of works supported by local governments. However, certain concerns remain with respect to the interpretation. In other words, there may be other explanations for this result. For example, a shorter tenure might hinder local governments from obtaining information about the suitability of works in local domains, which, in turn, could decrease the number of works. Thus, I add a variable that captures tenure length up to 1651; for example, if a local government entered the domain in 1640, this variable takes the value of 11, whereas if the government stayed the same since 1615, this variable takes the value of 36. Using this information, I construct the weighted average of experience, $Experience_i$, using the share of local government. Assuming local knowledge is important, domains with greater experience should be able to further increase the number of works after attaining greater stability from the policy shock. In the econometric model, I add $Insider_i * After\ 1651_t * Experience_i$, which should have a positive coefficient to be consistent with the channel. I demean $Experience_i$ to make the results comparable to the other results. Table 6 shows the results for similar specifications to those in Table 3. In certain specifications, I include $After\ 1651_t * Experience_i$ as a robustness check. The results do not show a significant effect for the interaction terms. Therefore, this evidence is inconsistent with the local experience channel.

In columns (5) and (6), I perform an additional test to separate the transfer risk impact from the real transfer effect. In these specifications, I add the number of transfers in era $t-1$ in column (5) and further add this number in t in column (6).[54] If the reduced-form effect is fully explained by the experience channel, these transfer variables should absorb the reduced-form effect. However, the results still show significant effects of *Insider * After 1651*, suggesting that the main effect is not explained by the direct impact of past transfers, which reflects experience rather than expectation.

This result also rules out the possibility that the cost of transfers has a temporal negative income shock that could be the main channel for the decrease in public

[54]The preferred specification is (5) because we cannot tell whether a transfer happens before an investment starts within era t.

goods investment: if cost plays a key role, counties with shorter experiences would face budget problems and would not increase their public goods investment following the policy change, as the experience channel suggests. Overall, the other alternative channels do not explain the main result. Therefore, the main mechanism of the transfer effect will be the reduced probability of relocating, not the loss of experience or the cost of transfers.

5 Conclusion

The impacts of longer tenures of political actors on public choice and its mechanisms are widely studied in the political economy literature. The longer time horizon effect is one such theory (Olson, 1993; McGuire and Olson, 1996; Olson, 2000), but obtaining an exogenous variation of the time horizon and appropriate data to test this hypothesis is difficult. In this study, I use a plausibly exogenous change in the time horizon and a newly digitized data set for the Edo period to examine the longer time horizon effect. The shogunate could transfer local governments (i.e., local lords and their vassals) to other domains, which would discourage them from investing in their local domains. The transfer risk was higher for insiders who supported the shogunate in the war of 1600 than it was for other lords; the transfer risk dropped more for insiders after 1651 owing to the death of the third shogun and the subsequent policy change to decrease the number of masterless officers generated by such transfers.

The DID estimates reveal that counties owned by insiders saw a greater increase in the number of public works financed by local governments, such as irrigation, after 1651. The point estimate of the DID analysis is 0.0041 projects per year, which is more than the mean of the outcome, a substantial effect. The main result is robust even after control for county-level trends and time-variant geographical and political effects. I do not find an effect on works financed by rich farmers and merchants rather than by local governments both when using data from the sample period and when using data from before 1615. In addition, I conduct robustness checks to confirm the reduced-form effect by investigating other channels that may coincide with the policy change. However, I do not find evidence to support a role for ca-

reer concerns over becoming a cabinet member, nor do I find that the policy change coincides with a change in in-kind transfers from the shogunate or a lower risk of civil war, which would cause local lords to reallocate their resources from military to economic investments. Similarly, I consider the change in lord's characteristics, but the main results do not change qualitatively against controlling for their age or experiences as a lord. I also address measurement issues regarding data set quality by using additional data sources and controlling for counties' proneness to measurement error, and I investigate the relationship between outcomes and transfers subsequently. I also investigate whether the reduced-form effect could have another interpretation besides a longer time horizon effect, such as an experience effect. However, I find no evidence that more experienced insiders have more local knowledge and increase investments more after 1651 or that past transfers decrease the number of projects. All results support the view that the reduced-form effect emerges owing to a longer time horizon effect. The result also suggests that the political stability of local lords under Tokugawa's hegemony played an essential role in economic development during the early Edo period.

More broadly speaking, this study contributes to the literature on economic growth under extractive institutions. Although sustained growth accompanied by technological progress will require inclusive institutions, we observe some extractive institutions that achieve economic growth, such as Soviet Russia, modern-day China, and early Tokugawa Japan. The political mechanism under such economic growth is under-investigated, but the results of this study suggest that longer time horizons of rulers may play a role in economic growth under such institutions.

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Table 1: Descriptive Statistics

Panel A: Domain-level Data	mean	sd
Transfer	0.0187	0.135
Forfeit	0.00454	0.0672
Insider	0.457	0.498
Observations (Domain * Year)	23798	
Panel B: County-level Data		
Public Works Financed by Local Governments	0.00314	0.0289
Other Works	0.0151	0.112
— No Information of Bearers	0.0126	0.105
Observations (County * Era)	5838	

The data period in Panel A is from 1615 to 1715. In panel B, the start years for the eras are 1615, 1624, 1645, 1648, 1652, 1655, 1658, 1661, 1673, 1681, 1684, 1688, 1704, and 1711.
Panel A provides domain- and year-level data, sourced from Nagahara, ed (1999). *Transfer* (*Forfeit*) is a dummy variable that takes the value of one when a transfer (forfeit) occurs. Panel B provides county- and era-level data, sourced from Doboku-Gakkai, ed (1936) and Izumi (2008). *Public Works Financed by Local Governments* is the number of public works per year initiated in the era and recorded as financed by local governments. *Other Works* is the number of public works per year initiated in the era and not recorded as financed by local governments. This includes works with no information of their bearers, shown as — *No Information of Bearers*. In this paper, I consider *Other Works* as *Public Works Not by Local Governments* and conduct a robustness check to include works with no information about their bearers (— *No Information of Bearers*) from *Public Works Not by Local Governments*.

Table 2: Regression Analysis of Forfeits and Transfers

Panel A: Forfeits Drives Transfers and Forfeits Decreased After 1651		
	Transfer	Forfeit
	(1)	(2)
Total Forfeit	0.00420***	
	(0.00123)	
Total Forfeit * Insider	0.00970**	
	(0.00307)	
Insider	0.0167***	
	(0.00243)	
After 1651		-0.00304**
		(0.00101)
Constant	0.00681***	0.00654***
	(0.00100)	(0.000905)
Observations (Domain * Year)	23798	23798

Panel B: Decline in Transfers			
	Transfer		
	(1)	(2)	(3)
Insider	0.0347***	0.0367***	
	(0.00509)	(0.00488)	
After 1651	-0.0134***		
	(0.00204)		
Insider * After 1651	-0.0193***	-0.0214***	-0.0165***
	(0.00499)	(0.00483)	(0.00437)
Constant	0.0179***		
	(0.00209)		
Year FE	No	Yes	Yes
Domain FE	No	No	Yes
Observations (Domain * Year)	23798	23798	23798

Standard errors in parentheses are domain-level cluster robust. $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Data are sourced from Nagahara, ed (1999). The data period is from 1615 to 1715. The table presents domain- and year-level data. *Transfer* (*Forfeit*) is a dummy variable that takes the value of one when a transfer (forfeit) occurs. *Total Forfeit* is the total number of forfeits in each year.

Table 3: Main Result: DID and DID-IV Analysis on Public Works Supported by Local Governments

	Public Works by Local Governments						
	DID			DID-IV	DID		
Panel A: Main Results	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Insider * After 1651	0.00405^{+}	0.00532	0.00546		0.00426^{+}	0.00484^{+}	0.00536^{+}
	(0.00230)	(0.00451)	(0.00489)		(0.00226)	(0.00281)	(0.00279)
Transfer Probability				-0.199			
				(0.142)			
Flat Land Share * After 1651					-0.00189		-0.00252
					(0.00310)		(0.00307)
Rice Suitability * After 1651					0.000658		-0.00172
					(0.0144)		(0.0141)
Log Government Size * After 1651						0.000546	0.000702
						(0.000572)	(0.000571)
Era FE and County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Trend	No	Yes	Yes	No	No	No	No
Province FE * After 1651	No	No	Yes	No	No	No	No
Observations (County * Era)	5838	5838	5838	5838	5824	5824	5810
First KP-stat				4.756			

	Insiders			
	OLS			
Panel B: Correlation Between Insider and Control Variables	(1)	(2)	(3)	(4)
Flat Land Share	0.149			0.242*
	(0.0971)			(0.0995)
Rice Suitability		0.424		-0.0514
		(0.407)		(0.385)
Log Government Size			-0.0698***	-0.0789***
			(0.0155)	(0.0154)
Constant	Yes	Yes	Yes	Yes
Observations (County)	416	416	417	416

Standard errors in parentheses are province-level cluster robust. $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
I use a fixed-effects estimator. The data are presented at the county and era level. The start years for the eras are 1615, 1624, 1645, 1648, 1652, 1655, 1658, 1661, 1673, 1681, 1684, 1688, 1704, and 1711.
Flat Land Share is defined as the (demeaned) value of *Area with less than 2.86 degrees/Area*, as per the 1 km × 1 km mesh-level data from the National Land Numerical Information Download Service (http://nlftp.mlit.go.jp/ksj-e/index.html). Because data are missing for one county (an isolated island), the number of observations is smaller.
Rice Suitability is the average rice suitability index provided by FAO-GAEZ version 3.0. I choose the following parameters: Crop: Wetland rice, Water Supply: Gravity irrigation, Input Level: Intermediate, and Time Period: 1961-1990.
Log Government Size is defined by the log of the weighted average of log Government Size in 1651 (in ten thousands of *kokudaka*, which is a proxy for total land productivity), as adopted from Nagahara, ed (1999). Because the size of the shogunate is not listed, I assign the popular estimate of 8 million *koku* for villages under the shogunate. I also assign a value of zero for villages under religious groups.

Table 4: DID Analysis of Public Works Not Supported by Local Governments

	Public Works Not Supported by Local Governments						
	1615–1715				1573–1614		
Panel A: Including Works Without Bearer Information	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Insider * After 1651	0.00440	0.00176	0.00861	0.0130			
	(0.00855)	(0.0123)	(0.0178)	(0.0127)			
Insider					0.00273	0.000636	
					(0.00217)	(0.00313)	
Insider * Linear Trend						0.000146	0.000146
						(0.000201)	(0.000201)
Panel B: Excluding Works Without Bearer Information							
Insider * After 1651	-0.000969	0.00286	0.00392	-0.00167			
	(0.00209)	(0.00420)	(0.00500)	(0.00250)			
Insider					0.000253	-0.000373	
					(0.000796)	(0.000439)	
Insider * Linear Trend						0.0000437	0.0000437
						(0.0000583)	(0.0000583)
Era FE and County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Trend	No	Yes	Yes	No	No	No	No
Province FE * After 1651	No	No	Yes	No	No	No	No
Geographical and Political Controls * After	No	No	No	Yes	No	No	No
Observations (County * Era)	5838	5838	5838	5810	1251	1251	1251

Standard errors in parentheses are province-level cluster robust. $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
I use a fixed-effects estimator. Data are presented at the county and era levels. The start years for the eras are 1615, 1624, 1645, 1648, 1652, 1655, 1658, 1661, 1673, 1681, 1684, 1688, 1704, and 1711 in columns (1)–(3) and 1573, 1593, and 1596 in columns (4)–(6). *Geographical and Political Controls* comprises *Flat Land Share*, *Rice Suitability* and *Log Government Size*. See Table 3 for details of these control variables.
When counting the number of projects, I include (exclude) projects without information of their bearers in Panel A (B).

Table 5: The Data Do Not Support the Career Concern Interpretation

	Public Works by Local Governments				
	(1)	(2)	(3)	(4)	(5)
Insider * After 1651	0.00474^{+}	0.00385	0.00485^{+}	0.00462^{+}	0.00667^{+}
	(0.00261)	(0.00240)	(0.00258)	(0.00263)	(0.00346)
Cabinet Leader 1615-1715 * After 1651	-0.00121				
	(0.00120)				
Cabinet Leader 1615-1651 * After 1651		0.00181		0.00242	0.00247
		(0.00579)		(0.00582)	(0.00593)
Cabinet Leader 1651-1715 * After 1651			-0.00175	-0.00186	-0.00197
			(0.00127)	(0.00142)	(0.00146)
Era FE and County FE	Yes	Yes	Yes	Yes	Yes
Observations (County * Era)	5838	5838	5838	5838	2282

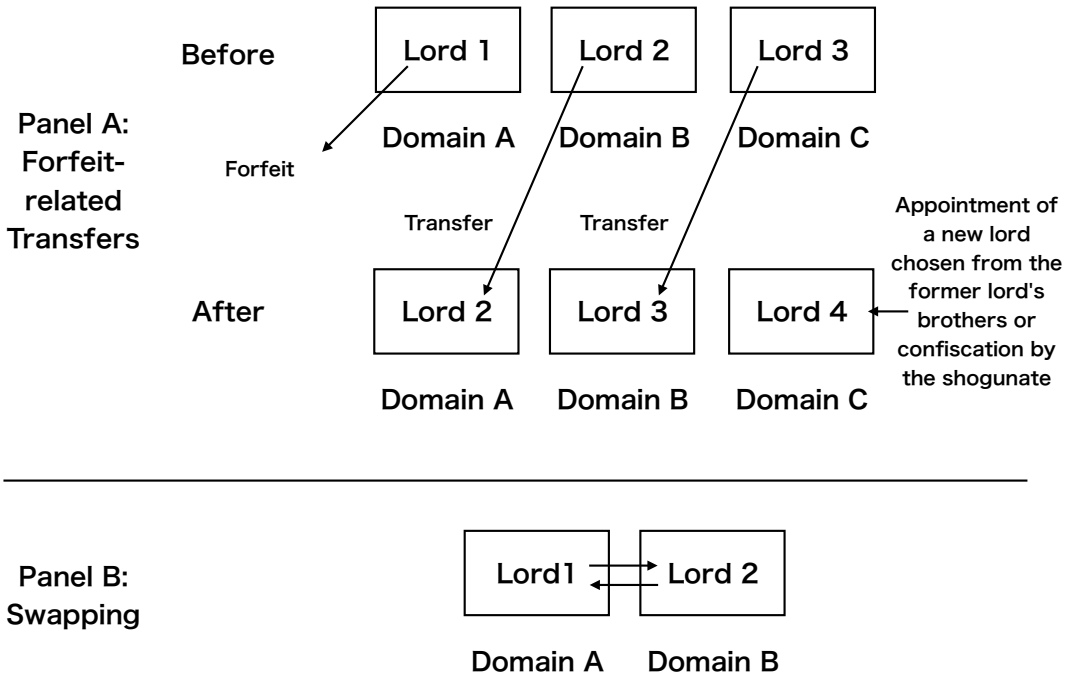
Standard errors in parentheses are province-level cluster robust. $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
I use a fixed-effect estimator. Data are presented at the county and era levels. The start years for the eras are 1615, 1624, 1645, 1648, 1652, 1655, 1658, 1661, 1673, 1681, 1684, 1688, 1704, and 1711. *Cabinet Members 1615-1715* is defined as the number of cabinet members during 1615–1715 produced by each domain, with a weighted average taken at the county level. *Cabinet Members 1651-1715* and *Cabinet Members 1615-1651* are similarly defined.
Column (5) exclude the county which has zero insider share.

Table 6: Other Channels: No Role of Local Experience?

	Public Works by Local Governments					
	(1)	(2)	(3)	(4)	(5)	(6)
Insider * After 1651	0.00421^{+}	0.00321	0.00553	0.00319	0.00487^{+}	0.00469^{+}
	(0.00238)	(0.00287)	(0.00481)	(0.00578)	(0.00254)	(0.00247)
Insider * Last Transfer Before 1651 * After 1651	0.0000632	0.000127	0.0000826	0.000231		
	(0.0000894)	(0.000124)	(0.000188)	(0.000206)		
Last Transfer Before 1651 * After 1651		-0.0000527		-0.000123		
		(0.0000686)		(0.0000968)		
Transfer Prob at Era $t-1$	No	No	No	No	Yes	Yes
Transfer Prob at Era t	No	No	No	No	No	Yes
Era FE and County FE	Yes	Yes	Yes	Yes	Yes	Yes
County-level Trend	No	No	Yes	Yes	No	No
Observations (Country * Era)	5838	5838	5838	5838	5421	5421

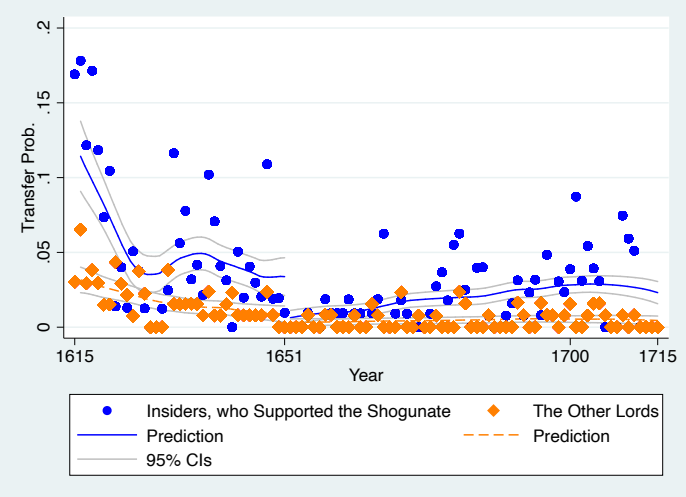
Standard errors in parentheses are province-level cluster robust. $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
I use a fixed-effects estimator. Data are presented at the county and era levels. The start years for the eras are 1615, 1624, 1645, 1648, 1652, 1655, 1658, 1661, 1673, 1681, 1684, 1688, 1704, and 1711.
Last Transfer Before 1651 is defined as the weighted average of the number of years from the last transfer of domains before 1651 at the county level.

Figure 1: Two Types of Transfer



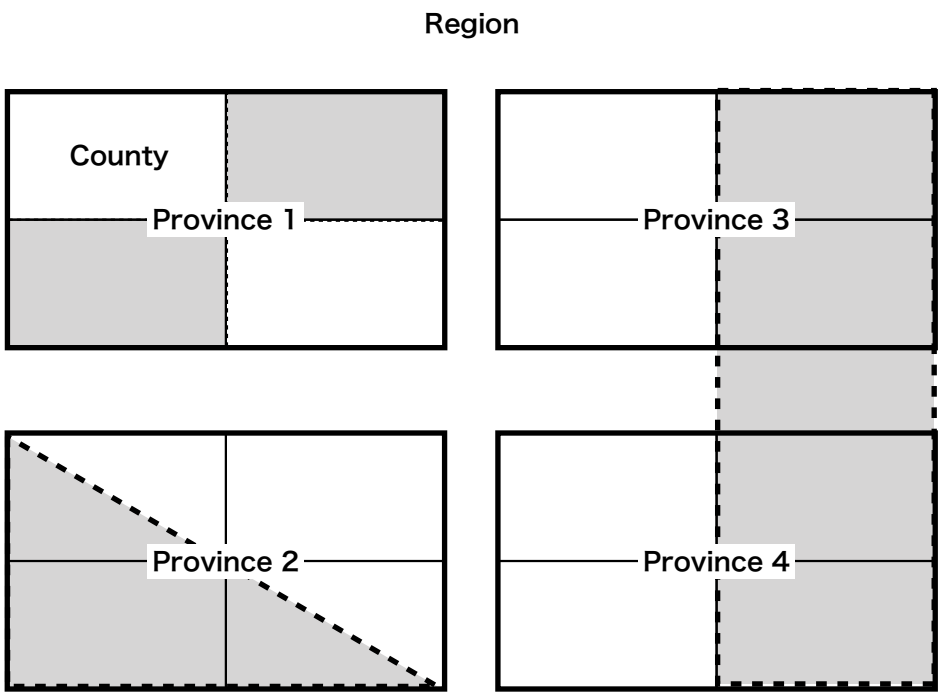
Notes: This figure explains two types of transfers. When lords were transferred, their vassals also were transferred. See the main text for details.

Figure 2: Transfer Risk



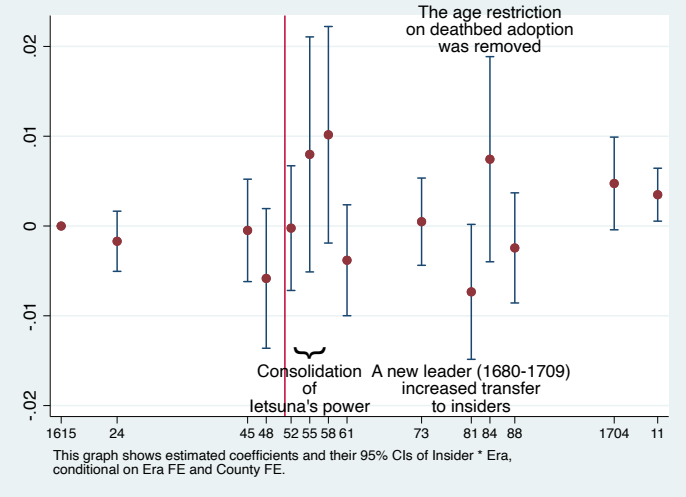
Source: See Section 3. The points indicate the transfer probability for each year and identity. The lines indicate identity-wise nonparametric estimates for periods before and after 1651. The bands indicate the 95% confidence intervals (CI).

Figure 3: County Boundaries and Domain Boundaries



Notes: The boundary of counties (provinces) are indicated by 16 small (4 big) rectangles. Gray-shaded areas indicate the boundary of domains.

Figure 4: Graphical Representation of the Main Results using DID Analysis



A Appendix Not For Publication

Table A.1: List of Eras from 1615 to 1700

Period	Reason of change
1615–1624	Enthronement of a new emperor
1624–1645	Sexagenary cycle
1645–1648	Enthronement of a new emperor
1648–1652	Ominous sound of the previous era name
1652–1655	Inauguration of *Ietsuna*
1655–1658	Enthronement of a new emperor
1658–1661	Fire in Tokyo
1661–1673	Fire in the Emperor's Palace
1673–1681	Fire in Kyoto
1681–1684	Sexagenary cycle
1684–1688	Sexagenary cycle
1688–1704	Enthronement of a new emperor
1704–1711	A Big Earthquake
1711–1716	Enthronement of a new emperor

Source: Yoneda, ed (2003).

Table A.2: Transfer Risk and Past Experience of Transfer

	Transfer	
	(1)	(2)
Starting Governance By Transfer	-0.00184	0.00325
	(0.00950)	(0.00432)
Insider		0.0352***
		(0.00532)
Constant	0.0552***	0.0167***
	(0.00791)	(0.00304)
Observations (Domain * Year)	3113	7900

Standard errors in parentheses are domain-level cluster robust. $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Data are sourced from Nagahara, ed (1999). The data period is from 1615 to 1651. Column (1) uses only insiders, and column (2) uses the full sample. Data are presented at the domain and year levels.

Transfer is a dummy variable that takes the value of one when a transfer occurs. *Starting Governance By Transfer* is an indicator variable that takes a value of one if the local lord or his house started to govern the domain after a transfer.

Table A.3: Regression Analysis: Forfeit

	Forfeit		
	(1)	(2)	(3)
Insider	-0.00214	-0.00201	
	(0.00182)	(0.00183)	
After 1651	-0.00392**		
	(0.00137)		
Insider * After 1651	0.00220	0.00212	-0.00658**
	(0.00198)	(0.00197)	(0.00212)
Year FE	No	Yes	Yes
Observations (Domain * Year)	23798	23798	23798

Standard errors in parentheses are domain-level cluster robust. $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
Data are sourced from Nagahara, ed (1999). The data period is from 1615 to 1715. Data are presented at the domain and year levels.
Forfeit is a dummy variable which takes the value of one when a forfeit occurs.

Table A.4: Data Coverage

Excluded Isolated Islands	*Oki*, *Sado*, *Iki*, *Tsushima*, and *Ryukyu*
Excluded Provinces for Various Reasons	*Wakasa*, *Yamashiro*, *Satsuma*, *Kamihusa*, *Simohusa*, *Hitachi*, *Shinano*, and *Simotsuke*
Covered Provinces	*Awa*, *Aki*, *Awa*, *Iga*, *Ise*, *Izu*, *Iyo*, *Imba*, *Ugo*, *Uzen*, *Echigo*, *Echizen*, *Echu*, *Totomi*, *Simotsuke*, *Kaga*, *Kawachi*, *Iwami*, *Iwashiro*, *Kii*, *Oumi*, *Kai*, *Mikawa*, *Sanuki*, *Shima*, *Suo*, *Izumo*, *Suruga*, *Kozuke*, *Setsu*, *Sagami*,*Osumi*, *Yamato*, *Tajima*, *Tango*, *Tanba*, *Awaji*, *Tikugo*, *Tikuzen*, *Nagato*, *Tosa*, *Hyuga*, *Noto*, *Harima*, *Hoki*, *Iwaki*, *Higo*, *Hizen*, *Hida*, *Bingo*, *Bizen*, *Bichu*, *Owari*, *Mimasaka*, *Mino*, *Musashi*, *Bungo*, *Buzen*, *Mutsu*, *Rikuzen*, *Rikuchu*, and *Izumi*

Table A.5: Robustness Check: Mean Reversion

	Public Works by Local Governments			
	(1)	(2)	(3)	(4)
Insider * After 1651	0.00405^{+}	0.00415^{+}	0.00278	0.00407
	(0.00230)	(0.00239)	(0.00250)	(0.00300)
Insider * 1648	No	No	Yes	Yes
Public Works by Local Governments at the Era t-1	No	Yes	No	No
Log Government Size * After 1651	No	No	No	Yes
Flat Land Share * After 1651	No	No	No	Yes
Rice Suitability * After 1651	No	No	No	Yes
Era FE and County FE	Yes	Yes	Yes	Yes
Observations (Country * Era)	5838	5838	5838	5810

Standard errors in parentheses are province-level cluster robust. $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

The first column replicates column (1) in Table 3.

I use a fixed-effects estimator. data are presented at the county and era levels. The start years for the eras are 1615, 1624, 1645, 1648, 1652, 1655, 1658, 1661, 1673, 1681, 1684, 1688, 1704, and 1711.

Flat Land Share is defined as the (demeaned) value of *Area with less than 2.86 degrees/Area*, as per the 1 km × 1 km mesh-level data from the National Land Numerical Information Download Service (http://nlftp.mlit.go.jp/ksj-e/index.html). Because data are missing for one county (an isolated island), the number of observations is smaller. *Rice Suitability* is the average rice suitability index provided by FAO-GAEZ version 3.0. I choose the following parameters: Crop: Wetland rice, Water Supply: Gravity irrigation, Input Level: Intermediate, and Time Period: 1961-1990. *Log Government Size* is defined by the log of the weighted average of log Government Size in 1651 (in ten thousands of *kokudaka*, which is a proxy for total land productivity), as adopted from Nagahara, ed (1999). Because the size of the shogunate is not listed, I assign the popular estimate of 8 million *koku* for villages under the shogunate. I also assign a value of zero for villages under religious groups.

Table A.6: Robustness Check: Controlling for the Accumulated Number of Public Works

	Public Works							
	by Local Governments				Not by Local Governments			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Insider * After 1651	0.00354	0.00513	0.00515	0.00470^{+}	-0.00428	0.000618	0.00671	0.00171
	(0.00235)	(0.00430)	(0.00468)	(0.00280)	(0.00723)	(0.00992)	(0.0143)	(0.00996)
Number of works up to $t-1$	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Era FE and County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Trend	No	Yes	Yes	No	No	Yes	Yes	No
Province FE * After 1651	No	No	Yes	No	No	No	Yes	No
Geographical and Political Controls * After	No	No	No	Yes	No	No	No	Yes
Observations	5838	5838	5838	5810	5838	5838	5838	5810

Standard errors in parentheses are province-level cluster robust. $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

I use a fixed-effects estimator. Data are presented at the county and era levels. The start years for the eras are 1615, 1624, 1645, 1648, 1652, 1655, 1658, 1661, 1673, 1681, 1684, 1688, 1704, and 1711.

Number of works up to $t-1$ is the sum of the numbers of both types of works that began before t.

I employ the definition used in Panel A of Table 4 (including unidentified bearer cases) for the number of public works not by local governments.

Table A.7: Robustness Check: Main Result without Mixed-ruled Counties

	Public Works by Local Governments			
	(1)	(2)	(3)	(4)
Insider * After 1651	0.00543	0.0107	0.00926	0.00570
	(0.00446)	(0.00975)	(0.0103)	(0.00487)
Era FE and County FE	Yes	Yes	Yes	Yes
County Trend	No	Yes	Yes	No
Region FE * After 1651	No	No	Yes	No
Geographical and Political Controls * After 1651	No	No	No	Yes
Observation (Country * Era)	3794	3794	3794	3780

Standard errors in parentheses are province-level cluster robust. $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

I conduct a negative binomial regression. The start years for the eras are 1615, 1624, 1645, 1648, 1652, 1655, 1658, 1661, 1673, 1681, 1684, 1688, 1704, and 1711. *Geographical and Political Controls* comprises *Flat Land Share*, *Rice Suitability*, and *Log Government Size*. See Table 3 for the details of these control variables.

This table only uses counties for which $Insider_i$ takes a value of zero or one.

Table A.8: Robustness Check: Main Result with Poisson Regression

	Count of Public Works							
	by Local Governments				Not by Local Governments			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Insider * After 1651	1.911^{+}	2.143	2.611	2.501*	0.213	-0.317	-0.289	0.198
	(1.161)	(1.838)	(2.124)	(1.169)	(0.421)	(0.764)	(0.793)	(0.442)
Era FE and County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Insider Share * Trend	No	Yes	Yes	No	No	Yes	Yes	No
Region-level Trend	No	No	Yes	No	No	No	Yes	No
Region FE * After 1651	No	No	Yes	No	No	No	Yes	No
Geographical and Political Controls * After 1651	No	No	No	Yes	No	No	No	Yes
Observations	1134	1134	1134	1134	2268	2268	2268	2268

Robust standard errors are in parenthesis. $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

I conduct a Poisson regression. The start years for the eras are 1615, 1624, 1645, 1648, 1652, 1655, 1658, 1661, 1673, 1681, 1684, 1688, 1704, and 1711. *Geographical and Political Controls* comprises *Flat Land Share*, *Rice Suitability*, and *Log Government Size*. See Table 3 for the details of these control variables.

I employ the definition used in Panel A of Table 4 (including unidentified bearer cases) for the number of public works not by local governments.

Note that many observations are excluded because of zero outcomes.

There are eight regions (*do*).

Table A.9: Robustness Check: Main Result with Negative Binomial Regression

	Count of Public Works					
	by Local Governments			Not by Local Governments		
	(1)	(2)	(3)	(4)	(5)	(6)
Insider * After 1651	1.889^{+}	1.435	2.494*	-0.0110	-0.0965	-0.182
	(1.092)	(1.405)	(1.146)	(0.340)	(0.363)	(0.360)
Era FE and County FE	Yes	Yes	Yes	Yes	Yes	Yes
Insider Share * Trend	No	Yes	No	No	Yes	No
Geographical and Political Controls * After 1651	No	No	Yes	No	No	Yes
Observations	1134	1134	1134	2268	2268	2268

Standard errors are in parentheses. $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

I conduct a negative binomial regression. The start years for the eras are 1615, 1624, 1645, 1648, 1652, 1655, 1658, 1661, 1673, 1681, 1684, 1688, 1704, and 1711. *Geographical and Political Controls* comprises *Flat Land Share*, *Rice Suitability*, and *Log Government Size*. See Table 3 for the details of these control variables.

I employ the definition used in Panel A of Table 4 (including unidentified bearer cases) for the number of public works not by local governments.

Note that many observations are excluded because of zero outcomes.

There are eight regions (*do*).

Table A.10: No Effect on Marine-Transportation Infrastructure

	Marine Transportation Infrastructure Projects by Local Governments			
	(1)	(2)	(3)	(4)
Insider * After 1651	-0.000271	0.00196	0.00202	-0.000337
	(0.000811)	(0.00126)	(0.00131)	(0.000855)
Era FE and County FE	Yes	Yes	Yes	Yes
County Trend	No	Yes	Yes	No
Region FE * After	No	No	Yes	No
Geographical and Political Controls * After	No	No	No	Yes
Observations	5838	5838	5838	5810

Standard errors in parentheses are province-level cluster robust. $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

I use a fixed-effects estimator. Data are presented at the county and era levels. The start years for the eras are 1615, 1624, 1645, 1648, 1652, 1655, 1658, 1661, 1673, 1681, 1684, 1688, 1704, and 1711.

The outcome variable is the number of new infrastructure project related to marine transportation financed by local governments documented in Doboku-Gakkai, ed (1936).

Flat Land Share is defined as the (demeaned) value of *Area with less than 2.86 degrees*/*Area*, as per the 1 km × 1 km mesh-level data from the National Land Numerical Information Download Service (http://nlftp.mlit.go.jp/ksj-e/index.html). Because data are missing for one county (an isolated island), the number of observations is smaller. *Rice Suitability* is the average rice suitability index provided by FAO-GAEZ version 3.0. I choose the following parameters: Crop: Wetland rice, Water Supply: Gravity irrigation, Input Level: Intermediate, and Time Period: 1961-1990. *Log Government Size* is defined by the log of the weighted average of log Government Size in 1651 (in ten thousands of *kokudaka*, which is a proxy for total land productivity), as adopted from Nagahara, ed (1999). Because the size of the shogunate is not listed, I assign the popular estimate of 8 million *koku* for villages under the shogunate. I also assign a value of zero for villages under religious groups.

Table A.11: Robustness Check: Importance of the Shogunate's Orders

	Orders from the Shogunate		
	(1)	(2)	(3)
Insider	-0.00584		
	(0.00736)		
Insider * After 1651	0.00323	0.00160	0.00442
	(0.00514)	(0.00453)	(0.00610)
After 1651	-0.0108***		
	(0.00296)		
Domain FE and Year FE	No	Yes	Yes
Domain-level Trend	No	No	Yes
Observations (Domain * Year)	19713	19713	19713

Standard errors are domain-level cluster robust. $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

I use a fixed-effects estimator in columns (2)–(3). The data period is from 1615 to 1715. Data are presented at the domain and year levels.

The outcome variable is the number of orders from the shogunate to each local government. Those orders are to mobilize local lords and their vassals for infrastructure projects of the shogunate.

Table A.12: Robustness Check: Changes in Military Incentives

	Castle		
	(1)	(2)	(3)
Insider	-0.00346		
	(0.00214)		
Insider * After 1651	0.00126^{+}	0.000278	0.00127
	(0.000749)	(0.000863)	(0.000936)
Year FE	Yes	Yes	Yes
Domain FE	No	Yes	Yes
Insider * Trend	No	No	Yes
Observations (Domain * Year)	19713	19713	19713

Standard errors in parentheses are domain-level cluster robust. $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
I use a fixed-effects estimator in columns (2)–(3). The data are for the period 1615 to 1715, and are presented at the domain and year levels. The outcome variable is the number of new construction or repair works of local lords' castle in each domain and year.

Table A.13: Robustness Check: Lords' Age and Experience as a Lord

		Experience	Public Works				
	Age	as a Lord	by Local Governments			Not by Local Governments	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Insider * After 1651	1.211	-5.344	0.00414^{+}	0.00150	0.00126	0.00425	-0.00442
	(2.299)	(3.253)	(0.00226)	(0.00284)	(0.00272)	(0.00837)	(0.0147)
Age			0.0000264			0.0000145	
			(0.0000387)			(0.000176)	
Experience as a Lord			0.0000462			0.000122	
			(0.0000916)			(0.000271)	
Era FE and County FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lord FE	No	No	No	Yes	No	No	Yes
Sample	Full	Full	Full	Full	Partial	Full	Full
Observations (County * Era)	5824	5824	5824	5824	2255	5824	5824

Standard errors in parentheses are province-level cluster robust. $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.
I use a fixed-effects estimator. The data are for the period from 1615 to 1715, and are presented at the domain and year levels.
Age is age of lords and *Experience as a Lord* is their current age minus their age when he became a lord for the first time in his life (ex. by succession). *Lord FE* is lord fixed effects weighted by the share of villages they govern.
Column (5) excludes the country governed by lords who only have governed some counties before 1651 or after 1651.

Table A.14: Robustness Check: Effect of Transfers on Recording

	Public Works by Local Governments		
	(1)	(2)	(3)
Insider * After 1651	0.00416^{+}	0.00408	0.00432
	(0.00239)	(0.00248)	(0.00276)
Era and County FE	Yes	Yes	Yes
N of Transfers at Era t' $(t+1 \leq t' \leq t+5)$	Yes	Yes	Yes
N of Transfers at Era t' $(t+6 \leq t' \leq t+10)$	No	Yes	Yes
N of Transfers at Era t' $(t+11 \leq t' \leq t+20)$	No	No	Yes
Observations (Country * Era)	5838	5838	5838

Standard errors in parentheses are province-level cluster robust. $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

I conduct an ordinary least squares analysis. Data are presented at the county and era levels. The start years for the eras are 1615, 1624, 1645, 1648, 1652, 1655, 1658, 1661, 1673, 1681, 1684, 1688, 1704, and 1711.

Column (1) includes the number of transfers during eras $t+1, t+2, \cdots, t+5$ as control variables. Columns (2) and (3) add similar variables by extending the sequence to $t+10$ and $t+20$, respectively.

Table A.15: Robustness Check: Controlling for Detached Areas

	Public Works by Local Governments			
	Full Sample		Only Mainland	
	(1)	(2)	(3)	(4)
Insider * After 1651	0.00406^{+}	0.00532	0.00574	0.0106
	(0.00233)	(0.00449)	(0.00453)	(0.00974)
Mainland Share * After 1651	-0.000400	-0.00175		
	(0.00133)	(0.00180)		
Era FE and County FE	Yes	Yes	Yes	Yes
County-level Trend	No	Yes	No	Yes
Observations (County * Era)	5838	5838	2940	2940

Standard errors in parentheses are province-level cluster robust. $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

I used a fixed-effects estimator. Data are presented at the county and era level. The start years of the eras are 1615, 1624, 1645, 1648, 1652, 1655, 1658, 1661, 1673, 1681, 1684, 1688, 1704, and 1711.

I define a village as a mainland village if the castle of the local government governing the village is in the same or adjacent provinces or if another type (e.g., the shogunate, temples, etc.) of lord owns the village. *Mainland Share* is the share of such mainland villages in a county. In columns (3) and (4), I only use counties for which *Mainland Share* takes a value of one.

Table A.16: Supplementing the Outcome Variable with an Additional Data Source

	Public Works by Local Governments			
	(1)	(2)	(3)	(4)
Insider * After 1651	0.00412^{+}	0.00520	0.00534	0.00544^{+}
	(0.00229)	(0.00452)	(0.00490)	(0.00278)
Log Government Size * After 1651				0.000719
				(0.000579)
Flat Land Share * After 1651				-0.00250
				(0.00307)
Rice Suitability * After 1651				-0.00101
				(0.0142)
Era FE and County FE	Yes	Yes	Yes	Yes
County Trend	No	Yes	Yes	No
State FE * After	No	No	Yes	No
Observations (County * Era)	5838	5838	5838	5810

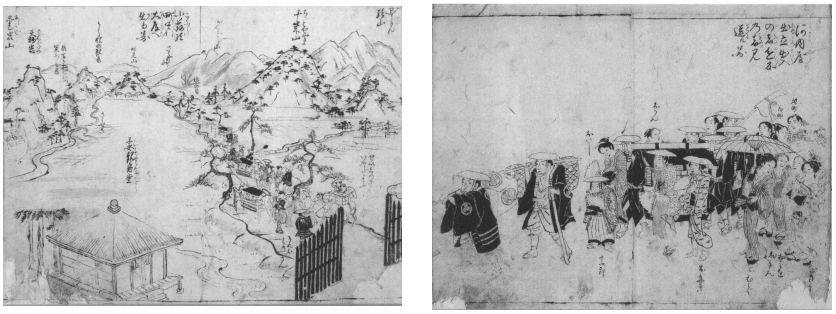
This table shows regression analyses with only projects recorded in Norinsho, ed (1927) added to the main outcome variable and based on the same specifications in columns (1)–(3) and (7) of Table 3.

Standard errors in parentheses are province-level cluster robust. $^{+}$ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

I use a fixed-effects estimator. Data are presented at the county and era levels. The start years for the eras are 1615, 1624, 1645, 1648, 1652, 1655, 1658, 1661, 1673, 1681, 1684, 1688, 1704, and 1711. *Transfer Probability* is the average transfer risk (percent) at the domain and era level.

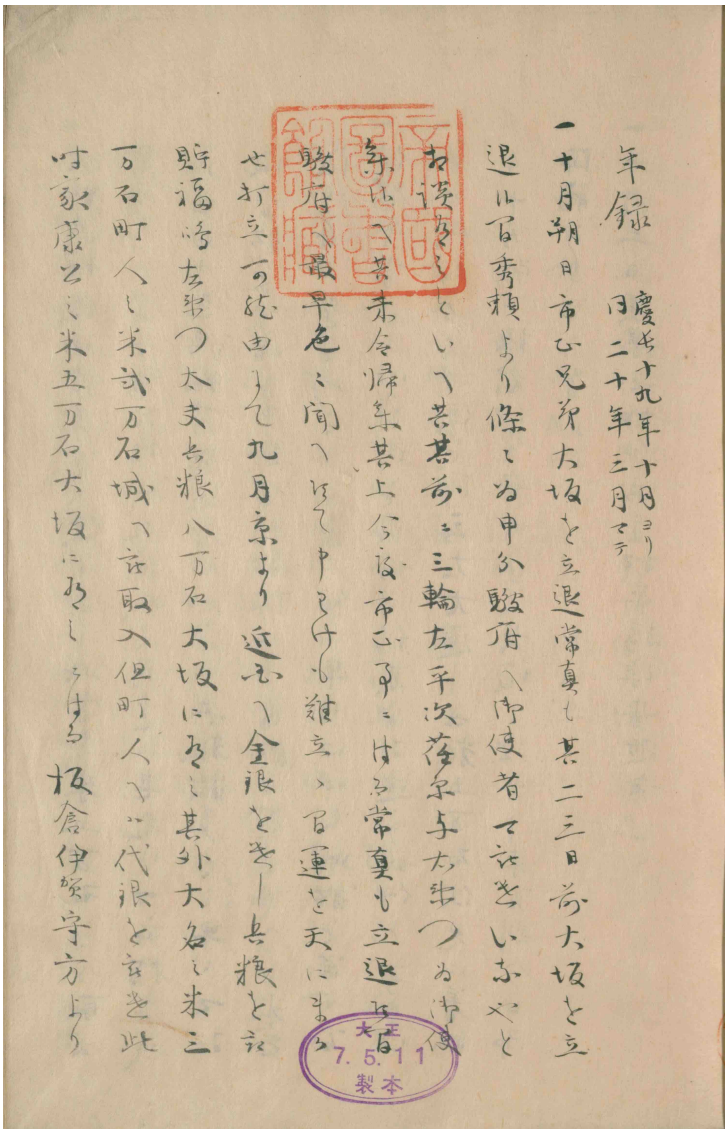
Flat Land Share is defined as the (demeaned) value of *Area with less than 2.86 degrees/Area*, as per the 1 km $\times$ 1 km mesh-level data from the National Land Numerical Information Download Service (http://nlftp.mlit.go.jp/ksj-e/index.html). Because data are missing for one county (an isolated island), the number of observations is smaller. *Rice Suitability* is the average rice suitability index provided by FAO-GAEZ version 3.0. I choose the following parameters: Crop: Wetland rice, Water Supply: Gravity irrigation, Input Level: Intermediate, and Time Period: 1961-1990. *Log Government Size* is defined by the log of the weighted average of log Government Size in 1651 (in ten thousands of *kokudaka*, which is a proxy for total land productivity), as adopted from Nagahara, ed (1999). Because the size of the shogunate is not listed, I assign the popular estimate of 8 million *koku* for villages under the shogunate. I also assign a value of zero for villages under religious groups.

Figure A.1: Okunigae Emaki (Diary of Transfer)



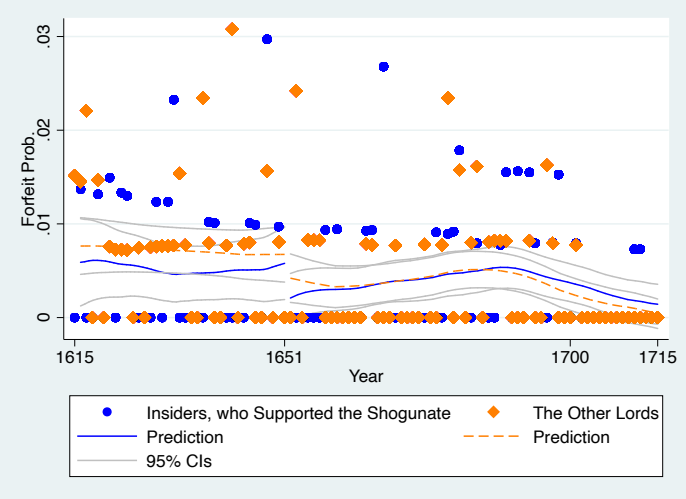
Notes: These illustrations are the work of *Otowako Yamada*, who was a painter and the wife of a samurai officer. She included these illustrations in the diary of her government's transfer from *Yamagata* to *Tatebayashi* in 1846 (*Okunigae Emaki*). These pictures are copied from Tatebayashi City History Compiling Committee (2013).

Figure A.2: Page from *Ryueihinamiki*



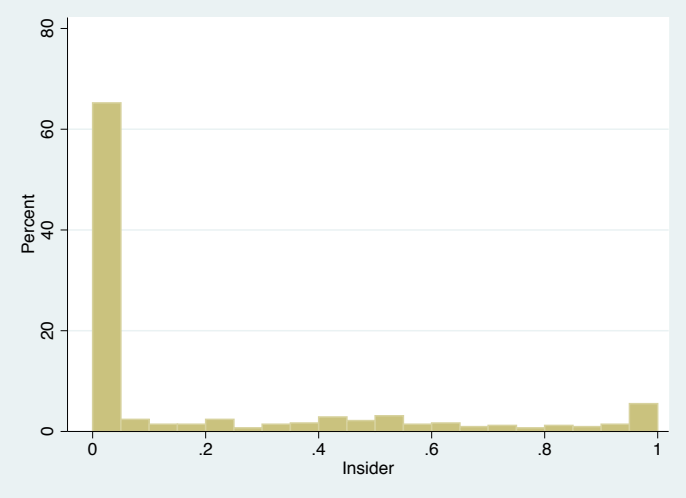
Source: National Archive Digital Collection. URL: http://dl.ndl.go.jp/info:ndljp/pid/2607434. This page records what happened in the shogunate from Oct. 1614 to Mar. 1615.

Figure A.3: Forfeit Risk



Source: See Section 3. The points indicate of the forfeit probability for each year and identity. The lines indicate identity-wise nonparametric estimates for before 1651 and after 1651. The bands indicate the 95% confidence intervals (CI).

Figure A.4: Distribution of Insiders' Shares at the County Level



Notes: This shows the cross-sectional distribution of *Insider* at the county level.

Figure A.5: Province-level Average Insider Share

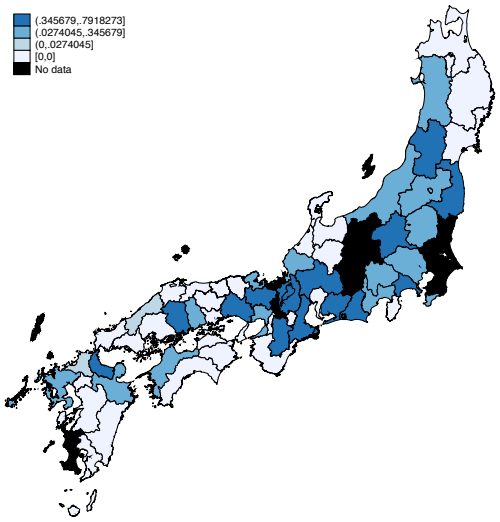


Figure A.6: Graphical Representation of the Main Results using DID Analysis (2)

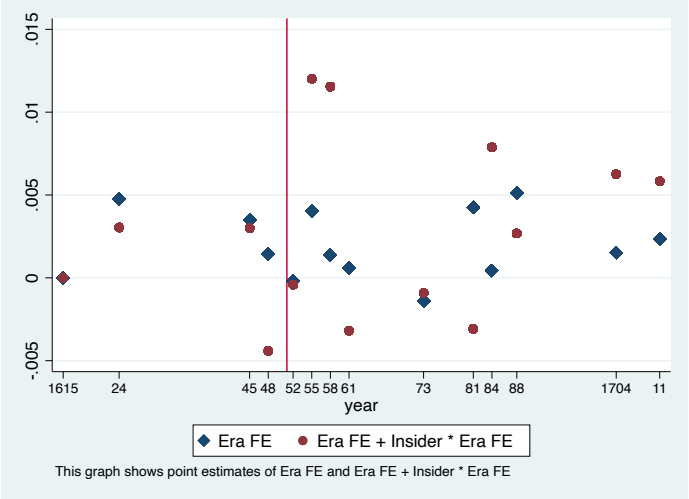
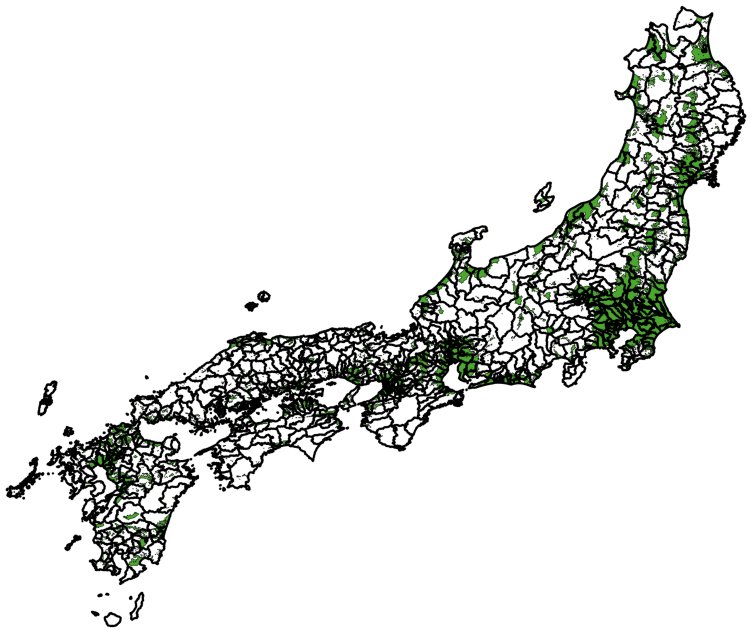
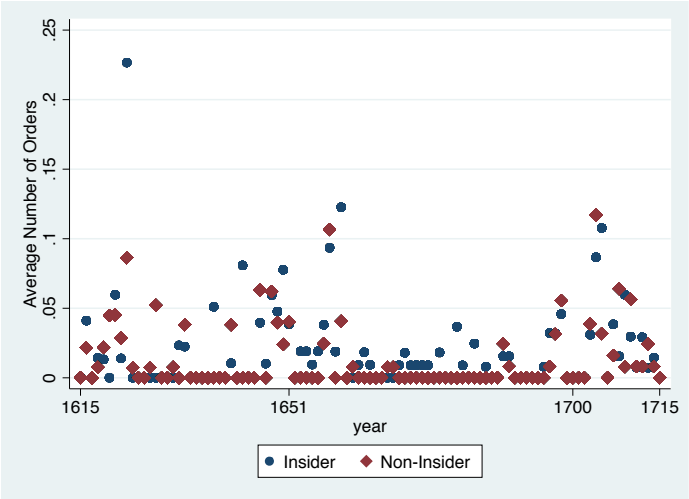


Figure A.7: Distribution of Flat Area



Source: National Land Numerical Information Download Service. The colored area denotes an average slope of less than 2.86 degrees.

Figure A.8: Average Number of Orders from the Shogunate to Local Governments by Identity



This figure shows the average number of orders from the shogunate to the local governments by each identity. The shogunate orders them to provide labor to construct or repair castles or temples.