Yardstick Competition and Election Timing*

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Abstract

We study the interplay of election timing and yardstick competition in the context of child healthcare subsidy, where the generosity of policy is measured by the maximum eligibility age. We first document that politicians increase the eligibility age around election timing, similar to political budget cycles. Importantly, such election timing effect is mostly observed when the eligibility age is lower *relative* to those of the neighbors, suggesting that the timing effect is predominately driven by yardstick competition. The increase in eligibility age due to this timing effect driven by neighbors is 5.82 years during the data period, while the timing effect would be only 0.35 years without neighbors' influences. Our paper highlights the importance of neighbors in the election timing effect.

Keywords: yardstick competition, political budget cycles, policy adoption, municipal elections, subsidy for child healthcare
JEL codes: H75, H73, D78, I18

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1. Introduction

Politicians are disciplined by elections in democracies. They consider how voters perceive their competence and, thus, try to signal their competence by adopting policies that are attractive to voters. The literature on pollical budget cycles (PBC, hereafter) documents that such policy change—typically an increase in spending—is concentrated at the timing of the election, which we call the *election timing effect*.[1] Another signal voters can rely on is the politician's performance *relative* to the neighboring governments, as shown in the literature on yardstick competition (e.g., Besley and Case 1995). Given that voters use these two signals together to evaluate politicians, politicians have additional incentives to manipulate the policy around election timing, especially when their policy appears inferior *relative* to their neighbors.

This paper studies whether the election timing effect is driven by yardstick competition and, if so, the degree to which the yardstick competition drives the election timing effect. To explore the question, we use unique monthly data on subsidies for child healthcare in Japan that we newly collected. While the PBC and yardstick literature did not have much interaction, we study their interaction by investigating how the generosity of child healthcare subsidy changes due to the election timing effect, neighbors' generosity, and their interactions.

The key advantage of analyzing Japanese municipalities is that the timing of municipal elections, which occur every four years, is *exogenously different* across municipalities due to idiosyncratic historical reasons such as municipal mergers and deaths of mayors (as discussed later). This unique setting allows us to use the neighbors' election cycle as an instrument for the neighbors' policy level, which is likely to be endogenous. Such endogeneity has been the major empirical challenge in credibly estimating yardstick competition. This setup sharply contrasts with the cases of simultaneous elections (e.g., US state elections) because exclusion restriction of instruments in such cases (e.g., neighbors' characteristics) is unlikely to be satisfied in most cases, as pointed out by Gibbons and Oberman (2012).

We focus on municipal subsidies for child healthcare— where the generosity is measured by the maximum eligibility age for the subsidy—as one key example of the provision of public services at the local level. In the last decade, municipalities in Japan have rapidly expanded

[1] [1] We use the term "election timing effect" in this paper instead of PBC because the policy we consider is about the eligibility age for childe healthcare subsidy, and the eligibility age do not return to the previous level in our data unlike PBC as we explain in more detail in Section 2.2.

subsidies for child healthcare, and there are substantial variations in generosity across municipalities. This specific spending is suitable for studying the interplay of the timing effect and yardstick competitions for the following four reasons.

First, the policies are highly visible to the voters. Municipalities provide full coverage till ages 6, 12, and 15 years old, usually, a multiple of three, which corresponds to the start of primary school, the start of secondary school, and the start of high school in Japan. These discrete numbers are easily comprehensive for voters. These features make child healthcare subsidy one of the populist policies that politicians and voters care about. Note that the subsidy coverage is always a one-way decision: The level of eligibility age does not return to the previous level in our data. Thus, it is conceptually different from PBC, where spending increases before the election and decreases after the election.

Second and relatedly, the policies are easily comparable across municipalities for both politicians and electorates, which makes yardstick competition more likely. Particularly, politicians can easily recognize that their policy appears *inferior* to neighbors' policy level. For example, with discrete numbers, it is evident that the coverage in municipality i with a subsidy up to 6 years old looks *relatively* inferior to that of neighbor municipality j with a subsidy up to 9 years old. Such comparison is also easy for the voters, and politicians can understand that such comparison is easy for voters.

Third, high-frequency data on policy at the *monthly* level—which we manually collected for the first time—is available. Such a high frequency of data on policy turns out to be vital as we also find that politicians increase the eligibility age right *after* the elections, unlike the conventional studies in the PBC literature where spending data is usually observed in annual frequency. This effect is masked by low-frequency (yearly- or quarterly) data used in the literature. To the best of our knowledge, the only paper that uses the monthly data in PBC literature is Akhmedov and Zhuravskaya (2005), which studies a Russian case.[2]

Finally, the welfare calculation is relatively straightforward. Iizuka and Shigeoka (2022a) examine the policy effectiveness of subsidy expansion for child healthcare and suggest that this adoption of populist policy mostly resulted in wasteful healthcare utilization without noticeable

[2] Interestingly, they find very short-lived increase just before election and decrease right after the election, which also highlight the virtue of high-frequency data. They do not consider the neighbors' behavior, though.

health gains. This evidence allows us to quantify the monetary value of additional waste induced by the election and the additional impact of yardstick competition at the election timing.

In sum, we find strong evidence of both the election timing effect and yardstick competitions individually, and finally, evidence of the additional impact of the interaction between the two forces. We have three main findings.

First, we document the presence of the election timing effect in the context of child healthcare subsidies in Japan. Interestingly, we find that incumbents not only expand the eligibility age one year prior to the election—the effect similar to PBC—but also within one year right after the previous election, which is a similar magnitude or even larger than the effect right before the next election. Such a just-after-election pattern disappears for politicians who are elected through uncontested elections, implying that the existence of the elections forces the to-be-elected politicians to promise the subsidy expansion and eventually implement the policy right after the election. This finding also suggests that the politicians think that voters remember the promises made by the incumbents and monitor their actions, at least right after the elections. While this result can possibly be particular to the Japanese setting, we show that low-frequency data used in the other literature are unlikely to detect such political behaviors—even if they exist—because the yearly data cannot distinguish the events that occurred right before and right after the election.

Second, we find strong evidence of yardstick competition. Because the timing of elections differs across the municipalities in Japan, we use the timing of the neighbors' election cycles as instruments for the neighbors' eligibility age to account for the endogeneity of the neighbor's policy choices. Our IV estimates from this novel identification strategy show that the municipality expands the eligibility age when its eligibility age is strictly below that of a neighbor, indicating that politicians in such municipalities try to fill the gap with neighbors and catch up.

Third and most importantly, we find some suggestive evidence that election timing effects are mostly driven by yardstick competition by estimating a model with their interaction effect. We show that politicians care *more* about neighbors' behaviors when their municipalities have inferior coverage, *especially* at the time of their own election. Moreover, the magnitude of the election timing effect becomes small for municipalities that do not have inferior coverage after the inclusion of the interaction effect. This implies that yardstick competition drives the election timing effect for those municipalities that have lower coverage relative to the neighboring municipalities. In particular, this interaction effect is more pronounced for experienced politicians. We find that 2nd+ term politicians indeed change policies around the critical period of their own election cycle in case their coverage falls behind those of their neighbors. On the other hand, the 1st-term (novice) politicians seem to care about the neighbors' actions regardless of the timing of their own election, possibly due to limited experience in learning the "optimal" timing of policy adoption to mimic neighbors.

The magnitude of the yardstick-driven effect on election timing effect is large. We convert the estimates into the increase in eligibility age during the ten years of our sample period. The yardstick-driven effect on election timing effect contributes to the increase in the eligibility age by 5.82 years, while the timing effect without yardstick competition (i.e., without the influence of neighbors) contributes to the increase in the eligibility age by only 0.346 years. These results suggest that the election timing effect is almost entirely driven by the yardstick competition or influence of the neighbors. Since the average yearly outpatient spending at this age range is 731 USD, 5.82 years increase in eligibility age leads to 4,254 USD per person.

Taken together, to the extent that the subsidy-induced utilization of healthcare is wasteful (Iizuka and Shigeoka 2022a), our results question the argument for decentralization (vertical competition) that the local government can deliver more effective public service than the central government. In particular, it is questionable to leave such a populist policy like child healthcare in the hands of the local government, at least in this setting.

This paper contributes to the literature on PBC (Nordhaus 1975; Rogoff and Sibert 1988; Rogoff 1990).[3] Past studies investigate the evidence of PBC in the cross-country setting (e.g., Alt and Lassen 2006; Brender and Drazen 2005; Janků and Libic 2019; Shi and Svensson 2006) and within-county setting (e.g., Akhmedov and Zhuravskaya 2005; Baskaran et al. 2015; Bostashvili and Ujhelyi 2019; Drazen and Eslava 2010; Repetto 2018). The literature, however, focuses on the election cycles of their own jurisdictions and does not consider how the policies of neighboring governments affect them. So far, to the best of our knowledge, the literature on PBC and yardstick competition have evolved independently, and no studies empirically investigate how yardstick competition affects PBC.

[3] See Drazen (2001), Eslava (2011), and de Haan and Klomp (2013) for reviews of literature on the political budget cycle.

This paper is related to the literature on policy adoption and diffusion (Volden et al., 2008). This literature is mostly limited to studying tax competition across the US states (Case et al. 1993; Besley and Case 1995; de Paula et al. 2020). This paper speaks to the underlying mechanism of policy adoption. The literature points out that the adoption of policies reflects either 1) *common preferences or environments*, 2) *learning* across jurisdictions (Volden et al. 2008), or 3) *competition* among jurisdictions (DellaVigna and Kim 2022). We can rule out correlated preferences because we use the neighboring election cycles, which are arguably orthogonal to own election cycles, as the instrument to address the potential endogeneity of simultaneous policy choices. Learning from neighbors through information diffusion is also unlikely as our outcome variable is the change in the eligibility age for an already existing policy (i.e., intensive margin) rather than adopting a new policy (i.e., extensive margin) by learning from the experimentation in other jurisdictions (e.g., Wang and Yang 2021). Taken together, while we cannot completely pin down the underlying mechanisms, yardstick competition is the likely mechanism to explain the policy adoption in our setting; politicians are threatened to look incompetent when neighboring jurisdictions have already provided more generous coverage to older children, which may reduce their reelection probability.

More broadly, among the models of spatial competition, our finding is consistent with yardstick competition (Shleifer 1985; Besley and Case 1993) as we can rule out most of the spatial completion models other than yardstick competition, such as the Tiebout-type model and the benefit spillovers model (Brueckner 2000). Iizuka and Shigeoka (2018, 2022a) show that children (and hence parents) do not move to municipalities with subsidies, suggesting that there is no fiscal externality to other municipalities. In addition, only children who live in the municipality can enjoy the subsidy, and thus there are, by construction, no benefit spillovers to the other municipalities.

Finally, this paper is related to the literature on decentralization (e.g., Oates 1972). On the one hand, proponents of decentralization argue that decentralization enables welfare programs to tailor better to local needs. On the other hand, opponents are concerned with negative spatial spillover through yardstick competition. This debate should be dependent on the underlying mechanism of yardstick competition. Our results suggest that an individual municipality may not be able to implement her desirable policy as she is forced to expand subsidies due to unstoppable competition with neighbors. It is even possible that some mayors may recognize the wasteful nature of subsidy expansion, but they still reluctantly implement these policies in order to increase the chance of remaining in office. Therefore, at least in the case of a populist policy like ours, the appropriate level of responsibility may not be as local as a municipality, and regulation by the upper-level government may be necessary.

The rest of the paper is organized as follows. Section 2 describes the institutional background of subsidy and election cycles in Japan and its datasets. Section 3 provides graphical evidence, and Section 4 presents our identification strategy. Section 5 documents the results, and Section 6 discusses the results. Section 7 concludes.

2. Institutional background and data

A database that combines information on Japanese municipal elections in a comprehensive way does not exist. In the same vein, a database that combines municipal subsidy information at the *monthly* level in a systematic way does not exist either. Therefore, we construct such datasets from scratch for both explanatory (election cycles) and outcome (subsidy) variables. We hand-collect both datasets through a variety of sources, including municipality web pages, municipal ordinances, local newspapers, historical archives, and other resources in Japan. As a result, the dataset includes both election and subsidy information for the largest six prefectures (Saitama, Chiba, Tokyo, Kanagawa, Aichi, and Osaka), resulting in covering about 300 municipalities. According to national statistics, these six prefectures cover as much as 44.9% of children ages 0–15 in Japan. We eventually dropped Tokyo (57 municipalities) because special wards in Tokyo did not follow simultaneous elections in 1947, as we describe in the next subsection.[4] Overall, our working sample includes 243 municipalities for ten years between April 2005 and March 2015.[5] The observational unit is each municipality at the monthly level, totaling 120 months. After collecting the data, we directly contacted each municipality and verified the accuracy of our information.

We explain the institutional background related to each dataset and describe the data in detail below.

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[4] Our results are qualitatively similar if we add back municipalities in Tokyo to the sample (results available upon request).

[5] This includes some municipalities that experience mergers. The results are very similar when we limit our sample to the balanced panels as shown later.

2.1. Election cycles

Japanese local governments consist of prefectures and municipalities.[6] The municipality is the lowest level of jurisdiction. The mayor of municipalities is elected through a simple plurality-rule election. There is no explicit nor implicit term limit for mayors. The majority of mayors are nonpartisan and are not subject to the influence of upper jurisdictions (i.e., prefecture). Thus, party affiliation plays little role in this setting, unlike the US, where political alignment plays an important role in election outcomes (DellaVigna and Kim, 2022).

A mayoral election is held every four years except for the cases such as recall and death. On average, each municipality experienced 2.55 elections in our 10-year sample period. Almost all municipalities experienced either 2 (46.6%) or 3 (49.8%) elections.

In our analysis, we exploit the exogenous variation in the election timing to identify the impact of the neighbor's policy.[7] After WWII, all mayoral elections were held on the same day, in April 1947. Given the four-year term of mayors, subsequent elections were scheduled every fourth year (i.e., 1951, 1955, . . . , 2007, 2011) in April if there were no incidents, such as resignation, death, merger, and recall, during the four-year term. By the start of our dataset (i.e., 2005), a large fraction of mayoral elections are not held at the timing of these simultaneous local elections (hereafter "SLE"). Once an election is held off the SLE cycle, the following elections remain off the SLE cycle because the length of the subsequent term is always four years, not the remainder of the previous term. For example, in the case of the 2007 SLEs, among the 247 municipalities that we studied, only 21.4% of mayoral elections were held on April 27, 2007. The majority of municipalities dropped out of the SLE cycles by the 1950s, when the national government encouraged municipal mergers with strong budgetary benefits.[8] Indeed, the municipal merger is the most common reason for municipalities to drop out from the SLE cycle (42.5%), followed by resignation (34.0%), death (18.2%), and others (5.3%), according to Fukumoto and Ueki (2015).

Panel A of Figure 1 shows the timing of mayoral elections during our sample period of 10

[6] There was a total of 47 prefectures (equivalent to the states in the US) and 1,719 municipalities (equivalent to counties in the US) in Japan as of January 2015.

[7] This paragraph heavily relies on Fukumoto and Horiuchi (2012), and Fukumoto and Ueki (2015).

[8] Fukumoto and Horiuchi (2012) displays the cumulative percentages of municipalities, which did not hold an assembly election on April 27, 2003, by years and reasons for deviation from SLEs.

years from April 2005 to March 2015, with a total of 656 elections. Again, while roughly 20% of municipalities follow SLEs, the vast majority of municipalities hold their own elections at different timing. As the figure shows, the timing of elections outside of SLEs spread across the years, supporting the argument that the reasons for deviations from SLEs are very idiosyncratic. Indeed, it is hard to imagine that the factors affecting the deviations from the SLEs, in particular, municipal mergers five decades ago, still have any substantial influence on citizens' and candidates' behavior in the 2000s. To confirm this, we conducted the balance test of municipal characteristics for 2007 and 2011 SLEs held during our sample period. Appendix Table A1 shows that municipal characteristics across two groups (without and with SLEs) are very similar in both 2007 and 2011 SLE, and none of the variables included in our regressions later are statistically different at the conventional levels.[9] In addition, the factors that affected the election timing in the past do not have a geographic correlation, and hence, the timing of two adjacent municipalities is not correlated.

2.2. Subsidy for child healthcare

We briefly provide the background of the Japanese healthcare system related to this study. The Japanese healthcare system is heavily regulated by the government. Under universal coverage, all citizens are obligated to enroll either in an employment-based insurance system or a residential-based insurance system. Regardless of the insurer, people face the same fee schedule and benefits package, both of which are set by the national government.

At the national level, patient cost-sharing—for which the beneficiary is responsible out of pocket—has been set at 30%. Many municipalities provide subsidies for children to cover this remaining cost, which aims to ensure access to essential medical care for children. Children who are eligible for the subsidy receive an additional insurance card, and by simply showing it, they receive a discount at medical institutions. Importantly, *only* residents of the municipality are benefited from the subsidy. In other words, children of residents in municipality *Y* who received treatment in the hospitals in municipality *X* is not benefited from the subsidy in *X*, which is only available for residents in *X*.

[9] Similarly, Fukumoto and Horiuchi (2018) examined the case of SLE 2003 and did the balance test of municipalities characteristics between the municipalities which hold elections in 2003 SLEs, and those which did not hold elections. They find that 14 (7.3%) out of 192 estimates are statistically significant at the conventional five percent levels.

To this end, we develop a novel dataset by hand-collecting data on the timing as well as the contents of subsidy expansion at the exact *month* level for ten years (April 2005–March 2015). This dataset is identical to the one used in Iizuka and Shigeoka (2018, 2022a, 2022b). Panel B of Figure 1 shows the number of municipalities by the exact timing of subsidy expansion during the sample period, with a total of 606 subsidy expansions. While we see more subsidy expansion in some specific year-month, the timing of expansion is widely spread across the sample period.[10] Figure 2 also shows the number of subsidy expansions. This ranges from zero to seven, with an average of 2.45 subsidy expansions. Only two out of 247 municipalities have had no subsidy expansion during our sample period, reassuring that subsidy expansion is a popular policy and is widespread across almost all municipalities.

Importantly, the generosity of the subsidy is largely reflected by the maximum age until which the subsidy is provided (we refer to "eligibility age" hereafter).[11] Figure 3 plots the share of municipalities by eligibility age for outpatient care in our sample period. Note that while the eligibility age is often expressed by school grade (e.g., until the end of junior high school), we loosely use ages throughout this study for convenience, as the school grades are almost completely equivalent to age in Japan owing to the strict enforcement of the school entry rule as well as very rare grade retention and advancement rates (Shigeoka 2015). Ages 6,12,15, and 18 in Figure 3 correspond to the entry into elementary schools, graduation from elementary schools, graduation from junior high schools, and graduation from high school, respectively.

Figure 3 clearly shows that the subsidy expanded rapidly to older ages in the last decade.[12] For example, none of the municipalities provided a subsidy until the age of 15 years in April 2005, the beginning of the sample period. However, this number reaches nearly 80% by the end of our sample period a decade later.

[10] The small jump in April 2008 is explained by the fact that the central government expanded the eligibility age for the national-level subsidy (i.e., 20% coinsurance rate) from 3 to 6 years (the start of primary school). This national-level subsidy expansion eased the budgetary burden on municipalities, as part of the cost to provide free care for below 6 years was covered by the central government, allowing municipalities to expand coverage to older ages.

[11] There are three other dimensions in subsidy (level of copayment/coinsurance, a refund or an in-kind payment, and existence of household income restrictions for subsidy eligibility) but the variations along these dimensions are relatively small (Iizuka and Shigeoka 2012a). Furthermore, politicians exclusively discuss the eligibility age in the official gazette as shown below.

[12] This figure differs that of Iizuka and Shigeoka (2018, 2022a) because we drop Tokyo here, and we do not weight by the number of insuranc claims as in Iizuka and Shigeoka (2018, 2022a).

A few more important features of subsidy data should be noted. First, most of the municipalities stop the expansion at age 15, at least in our sample period, which corresponds to the end of junior high schools. These ceiling effects should be properly controlled for estimation later. Second, there are no single municipalities that lowered the eligibility age in our sample; that is, the policy change is always monotonic.

Again, this specific spending is suitable for studying the election timing effect and yardstick competitions. First, the subsidy for child healthcare is one of the populist policies: while it is highly visible to the electorate, at the same time, it is not so costly. The discrete number (e.g., 6, 12, 15 years old) is highly comprehensive to voters. At the same time, although it may only account for roughly 1–2% of the total annual budget of municipalities, it is one of a few policies that mayors can have the discretion to change policy, unlike policies that target the elderly that are too costly and almost no room change due to budgetary constraints. Second, the comparison with other municipalities is clear with a discrete number, and as a result, it is suitable to study yardstick competitions. For example, it is obvious that the coverage in municipality i, with an eligibility age of 6 years old, falls behind that of neighbor municipality j, with an eligibility age of 9 years old. Finally, high-frequency data at the monthly level is available. To the best of our knowledge, the only paper that uses the monthly data in PBC literature is Akhmedov and Zhuravskaya (2005). Note that we were not able to collect monthly data on other spending categories, limiting us to investigate the possible spending reduction in other categories to offset the increase in spending due to child care subsidy.

2.3. Descriptive statistics

We construct the final dataset by merging the two datasets on election and subsidy information by municipality and year-month. Then for each municipality, we merge information about bordering neighbors, including their subsidy information as well as election cycles (our IVs), where we allow for multiple observations (j) per municipality (i). The summary statistics of the final dataset are described in Table 1. Regarding the election characteristics, 98% of the incumbents are male. On average, the terms are around two, ranging from one to ten, as there is no term limit for mayors in Japan. The fraction of the first term is 39%, and 18% of elections are uncontested. In our data set, 88% of elections follow the scheduled timing (i.e., a four-year schedule without deviation, not following SLE).

3. Graphical presentation

In this section, before presenting our econometric specification and results in Sections 4 and 5, respectively, we first present the graphical evidence of the election timing effect in Section 3.1 and then the yardstick competition in Section 3.2. We examine their *interaction* in Section 5.

3.1. Election timing effect

The construction of graphical evidence for the election timing effect is straightforward. Combining the timing of elections and subsidy expansion from the two figures (Panels A and B of Figure 1), Figure 4 plots the number of subsidy expansions by the time until the next elections at the monthly level. The vertical line separates a four-year election cycle into each year. The far-left interval corresponds to four years before the election (or just after the previous election), and the far-right interval corresponds to the one year before the next elections, and there are two middle years in between.

The figures have two noticeable patterns. First, there are many subsidy expansions one year before the next election compared to the middle years, consistent with the PBC literature. Second, rather surprisingly, we also see many expansions right after the elections, which are similar in magnitude or even larger than the election timing effect before the election.

We have some anecdotal and supportive evidence for such political behaviors. Some municipalities mandate the candidates create a gazette that summarizes their policies during the municipal elections. Many incumbents often boast of what they have done in the past to signal their competence. The expansion of subsidy for child healthcare is often included as their accomplishment, like “I have expanded subsidy from age 9 to 12 during my term”.

It is noteworthy, however, many candidates also list the policies that they claim they are going to implement once elected (electoral promises). The opponents, by definition, can only make such promises as they are not in the office and thus cannot describe what they have done in the past. However, the incumbent also often posts to-do lists after being elected on the gazette.

Figure 5 is such an example. This is the official gazette for the municipal election at Tsushima city in Aichi prefecture held on April 15, 2018. The sentences in the red box mention the subsidy expansion for child healthcare. The candidate on the right is the incumbent (ひびの一

昭 in Japanese), who promises to raise the eligibility age for free healthcare till the end of junior high school (中学卒業), which is equivalent to age 15. The candidate on the left is the opponent (杉山 良介), who also promises exactly the same level (中 3) of subsidy expansion. The incumbent won this election and implemented the pledged policy one year after, on April 1, 2019.

At a glance, it may look odd as even though politicians promise, there is no reason to follow the pledged promise and actually implement it within a year of the election. This finding suggests that voters indeed monitor their performance at least right after the elections. Interestingly, Panel A of Figure A2 shows that such a pattern right after the previous election disappears for politicians who experience uncontested elections, implying that having the election itself forces the to-be-elected politicians to promise the subsidy expansion and eventually implement the policy right after the election.[13]

While this can be particular to the Japanese setting, we show here that low-frequency data cannot detect such political behaviors because the yearly data cannot distinguish the events that occurred right before and after the election. As we discuss repeatedly, our data advantage is that we have monthly data on the eligibility age. Appendix Figure A1 shows the number of subsidy expansions by year till the next election, assuming that we only have yearly information about when the subsidy expansion is implemented. The figure shows usual PBC patterns only in the election year as we cannot cleanly separate policies implemented in the election years into pre- and post-elections.[14]

3.2. Yardstick competition

Next, we show the graphical evidence of yardstick competition. Figure 6 is the case of Saitama prefecture, just across the north of Tokyo. The figure demonstrates how the subsidy for child healthcare geographically propagates across municipalities. Each graph describes the subsidy level in each April from 2005 to 2014. The darker color indicates that the municipalities

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[13] Panel B of Figure A2 shows that patterns around the elections look similar between the politicians in the 1st term and 2nd-term or above.

[14] Since exact election dates are often available, some studies distinguish the election held first half of the year and second half of the year. If election happens in the first half, the election year is regarded as the pre-election year. On the other hand, when the election happens in the second half, the election year is treated as it is (Brender and Drazen 2005).

have expanded the subsidy to age 15 in the year. The lighter color indicates the municipalities have already expanded the subsidy to age 15 in the past.

The figures show that the subsidy expansion spread through adjacent municipalities, which seems to indicate the presence of yardstick competition. For example, in 2009, all expansions in that year (darker colors) happened next to the municipalities that had already expanded the subsidy in the past (lighter color). In addition, among seven municipalities that expanded that year, two municipalities had an election, which is slightly more than one-fourth. The Year 2010 shows an even stronger pattern of yardstick competition as the subsidy expansion seems to cluster locally. Furthermore, the number of dots is way more than one-fourth. By 2014, the eligibility age in all the municipalities in Saitama prefecture would reach 15.

There are a few other theoretical spatial models that might explain our findings other than yardstick competition (Brueckner 2000, 2003; Revelli 2005). But these models are unlikely to explain our results because our setting has no fiscal externality and little evidence of inter-municipal migration. One such model is the benefit spill-overs model, in which local public spending benefits the citizens of the neighbors (e.g., road construction). This model is completely irrelevant to our case since there is no externality: Only children who live in the municipality can enjoy the subsidy, and hence children who live in neighboring municipalities are not benefited from the subsidy. Another model one can consider a Tiebout-type model in which people move to municipalities with better welfare programs. However, Iizuka and Shigeoka (2022a)—using monthly residence information from insurance claim data—shows that children (and hence parents) do not move to municipalities with subsidy, suggesting that there are many other reasons (such as school quality) that are more likely to affect the migration decisions.

4. Identification strategy

4.1. Empirical challenges

Our main specification follows the standard approach to estimating the PBC and incorporates the influence of neighbors through yardstick competitions. However, there are three issues in incorporating yardstick competitions into the PBC model.

First, for the decision-making of municipality i, when is the policy level of neighbor j most relevant? In other words, how long does it take for the mayor of municipality i to respond to the subsidy expansion by neighboring municipality j if she wants to respond? Is it a three-month or a half-year? In Japan, a municipal assembly is held four times a year, and thus intervals between assemblies are three months on average. Thus, we first start with a lag of three months, assuming that at least three months is necessary to respond to the policy change taken by neighbors. We later experiment with changing this time lag, but the results are robust to the choice of the relevant time period.

Second, which neighbor has the biggest influence on the municipalities among all the neighbors? Based on Besley and Case (1995), voters judge politicians' behaviors relative to those in neighboring municipalities. This would imply that municipalities are most influenced by the behaviors of those municipalities that their voters judge to be the most salient (Baicker 2005).

We assume that all the bordering neighbors can potentially influence, but the weight placed on each neighbor (*neighborliness*) can differ. We examine four metrics: "out-migration," "in-migration," "size of the population, and "per capita income." "Out-migration" and "in-migration" determine the degree of neighborliness by the fraction of those that move into (out-migration) or that move from (in-migration) each neighboring municipality. "Size of the population" and "per capita income" computes weight on the difference in population and per capita income, reflecting that neighbors with similar size of population or per capita income receive more weight. Note that we compute weight so that the weight of bordering neighbors sums up to one.[15] We use the "out-migration" as our baseline. Since an oft-mentioned reason for subsidy expansion for *child* healthcare is to attract younger parents to increase the tax base (even though Iizuka and Shigeoka (2018, 2022a) find little such evidence of subsidy-driven inter-municipality migration), mayors may care more about the voters of the neighboring municipality that attracts citizens from their own jurisdiction.

Third and lastly, the choice of neighbors' policy can be endogenous. For example,

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[15] We construct weight (w_{ij}), where i is its municipality, and j is each adjacent municipality, as follows. For "out-migration" and "in-migration," inter-municipality mobility data come from the 2015 Census. The weight is the fraction of movers from municipality i to j (out-migration) and the fraction of movers from municipality j to i (in-migration). For "Size of the population," the data comes from "Sichoson no Sugata," published by the Statistics Bureau (https://www.e-stat.go.jp/regional-statistics/ssdsview, last accessed on August 1, 2019). Following Case et al. (1993) and Baicker (2005), the weight is based on the difference in population size between i and j, or $w_{ij} = 1/\{\left|\mathrm{Pop}_i - \mathrm{Pop}_j\right| S_i\}$ where $S_i = \sum_j \left|\mathrm{Pop}_i - \mathrm{Pop}_j\right|$. Similarly, for "per capita income," the data come from the same source. The weight is based on the difference in per capita income between i and j or, $w_{ij} = 1/\{\left|\mathrm{Inc}_i - \mathrm{Inc}_j\right| S_i\}$ where $S_i = \sum_j \left|\mathrm{Inc}_i - \mathrm{Inc}_j\right|$.

neighboring municipalities both suffer from a common policy problem, such as low fertility rates, and thus decide to expand the subsidy for child healthcare at a similar timing. Alternatively, the common public or interest groups may simultaneously pressure similar municipalities to implement a similar policy. If we do not account for such common preferences or environments, we are likely to overestimate the influence of neighbors due to a positive correlation in unobserved neighbor characteristics. We exploit the fact that the timing of municipal elections is exogenously different across municipalities due to idiosyncratic historical reasons and hence use the neighbors' election cycle as an instrument for the neighbors' policy level.

4.2. Specifications

For municipality i whose neighboring municipality is j(s), the main specification is written as:

$$A_{it} = \sum_{k\neq 2,3}^{k=1,4} \alpha_{-k} E_{it}^{-k} + \beta 1\left(A_{i\tilde{t}} < A_{j\tilde{t}}\right) + \sum_{k\neq 2,3}^{k=1,4} \rho_{-k}\left\{E_{it}^{-k} \times 1\left(A_{i\tilde{t}} < A_{j\tilde{t}}\right)\right\} + \gamma A_{it-1} + \delta X_{it}' + \theta_i + \mu_t + \varepsilon_{it}, \quad [1]$$

where A_{it} is the maximum eligibility age for the subsidy at time t (in months), and $A_{i\tilde{t}}$ and $A_{j\tilde{t}}$ are analogously defined for i and j at $\tilde{t} = t - 3$ (i.e., three-month lag). E_{it}^{-k} $(k = 1, 4)$ is a dummy that takes one if the year is k year before the next election. The reference year is two middle years.[16] Since election cycles are fixed every four years, we treat them as exogenous.[17] $1\left(A_{i\tilde{t}} < A_{j\tilde{t}}\right)$ is a dummy that takes one if the eligibility age in municipality i is strictly below that of municipality j. The discreetness of the eligibility age allows us to define such a variable cleanly.

We are particularly interested in whether politicians care *more* about neighbors' behaviors just before the next election or just after the previous election. Thus, we also include the interaction terms between election cycle dummies and a dummy that takes one if the eligibility age is below that of the neighbors ($E_{it}^{-k} \times 1\left(A_{i\tilde{t}} < A_{j\tilde{t}}\right)$).

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[16] We also separate middle two yeas into each year but the coefficient of our interests just before and after the elections are quantitively very similar (results available upon request).

[17] Following Khemani (2005) and Cole (2009), we also use as an instrument for E_{it}^{-k} by years until next *expected* election, yielding almost identical results as nearly 90% elections follow scheduled elections (See Table 1).

Our coefficients of interest are α_{-k} $(k = 1, 4)$, β, and ρ_{-k} $(k = 1, 4)$. α_{-k} capture election timing effect relative to the two middle years in the absence of yardstick competition. β captures the effect of yardstick competition without the election timing effect. Finally, ρ_{-k} $(k = 1, 4)$ captures the additional effect of yardstick competition on the election timing effect.

We include lagged eligibility age (A_{it-1}) to capture the monotonicity and ceiling effects of the subsidy expansions as described in Section 2.2.[18] Inclusion of the lagged variable (A_{it-1}) introduces mechanical known endogenous issues. Because our panel is relatively long, we estimate equation [1] using a standard fixed effect estimator. Using Arellano-Bond type GMM estimators yields similar results (results available upon request).

We also include municipality fixed effect (FE), which captures any time-invariant municipality characteristics, such as the preferences for a more generous subsidy policy. We also include year-month FE, which captures any other policies or economic shocks common across all municipalities. The vector X'_{it} include both mayor-level and municipality-level controls. Mayor-level controls are gender and terms of incumbents.[19] Municipality-level controls include faction of the population aged 0-15, 15-64, population density, and log income per capita, while all municipality-level controls are available only at the yearly level. ε_{it} is the error term. To account for serial correlation within the municipalities, standard errors are clustered at the municipality level.

Since $1(A_{i\tilde{t}} < A_{j\tilde{t}})$ is potentially endogenous to the outcome variables of interest, we instrument it by the timing of the neighbor j's and own i's election cycle dummies, $E_{j\tilde{t}}^{-k}$ and $E_{i\tilde{t}}^{-k}$ $(k = 1, 4)$, as well as lagged eligibility ages, $A_{j\tilde{t}-1}$ and $A_{i\tilde{t}-1}$.[20] The exclusion restriction is, in principle, that $E_{j\tilde{t}}^{-k}$ affects A_{it} only through $A_{j\tilde{t}-1}$. Since the timing of the two elections is highly orthogonal to each other, it is likely that the exclusion restriction is satisfied. The relevance is by construction coming from the strength of the political budget cycle of municipality j. Similarly, we instrument interaction terms $(E_{it}^{-k} \times 1(A_{i\tilde{t}} < A_{j\tilde{t}}))$ with the same set of variables interacting

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[18] As most of the municipalities stop expanding subsidy at age 15, the room for expansions are substantially different at age 6 and age 12. A_{it-1} is intended to capture such heterogenous effects.

[19] To construct the gender dummy and terms, we also collect the last election before our sample starts in April 2005.

[20] We obtain qualitatively similar results without own i's election cycle dummies $E_{i\tilde{t}}^{-k}$ and lagged eligibility ages $A_{i\tilde{t}-1}$ as additional instruments (the results available upon request).

with E_{it}^{-k}.

5. Results

5.1. Base results

Table 2 shows the main findings of this paper, where the outcome is the eligibility age. We show the evidence of the election timing effect and yardstick competition step-by-step and finally show the results of the interaction of the election timing effect and yardstick competition by estimating the equation [1].

Column (1) of Table 2 reports the OLS estimates of the election timing effect (α_{-4}, and α_{-1}) *only*—without yardstick competition and its interaction—where the reference year is the middle years. The municipality expands the eligibility age by 0.018 and 0.040 years per month (0.22 and 0.48 years in 12 months) one year *before* the election and four years before the election (or equivalently one year *after* the previous election), confirming the existence of the election timing effect in the context of child healthcare subsidy in Japan as seen in Figure 4.

Columns (2) and (3) of Table 2 report the OLS and IV estimates of yardstick competition (β) *only* (without the election timing effect and its interaction). Here, the neighbor is chosen from the ‘‘neighboring’’ municipalities to which their citizens move most. IV estimate in column (3) is smaller than that of the OLS estimate in column (2), indicating the endogeneity of the policy choices of neighbors due to positively correlated preferences or environments. Column (3) suggests that the municipality expands the eligibility age by 0.068 years per month (0.82 years in 12 months) when its eligibility age is strictly below that of neighbors, confirming the existence of competition at the borders, as seen in Figure 5. Kleibergen-Paap-rk Wald-F-statistic is above 80, suggesting that weak identification is unlikely to be a concern in our setting.[21]

Columns (4) and (5) of Table 2 report the OLS and IV estimates of equation [1] which includes the interaction terms of election timing effect and yardstick competition. Since policy choices are strategic (i.e., endogenous), we focus on column (5) with IV estimates.

The estimates on the interaction term of both just before the next election and after the

[21] To our knowledge, the literature has not yet developed formal methods for detecting weak identification in the presence of multiple endogenous regressors and homoskedasticity. As such, we report the Kleibergen-Paap Wald rk F-statistic that is clustered both at own municipality and neighboring municipality level, along with Cragg-Donald F-statistic, which assumes homoskedastic errors.

previous election (ρ_{-4} and ρ_{-1}) are positive and highly statistically significant at the 1 percent level, suggesting that politicians are more likely to be affected by neighbors' policy choices around one's own election timing. Interestingly, the non-interaction terms of election timing effect (α_{-4} and α_{-1}) are substantially attenuated from the estimates without interaction terms (column (1) of Table 2), suggesting that the election timing effect is mostly, if not entirely, driven by the yardstick competition in this setting.

To our knowledge, the fact that 1) interaction terms are highly relevant and 2) the election timing effect almost disappears once the interaction term is included, combined together is the new finding with significant policy implications. To the extent that the yardstick competition can be suppressed, we can eliminate the major portion of the election timing effect. While it is difficult, if not impossible, to completely remove the influence of elections unless we impose term limits or sort, preventing the effect of yardstick competition can be potentially achievable by government interventions. In this context, it may be advisable not to leave such a populist policy like child healthcare subsidy in the hands of the local government, and it should be centrally determined at higher levels of political institutions.

5.2. Heterogeneity

To shed light on the underlying mechanism of our findings, Table 3 examines heterogeneity by political characteristics. Columns (1) and (2) compare mayors' behavior after being elected through uncontested and contested elections. Mayors, who are elected by contested elections, seem to care more about what other neighbor politicians are doing at the time of elections. In particular, the interaction term of 4 years before the election (i.e., immediately after the election) shows that contested elections are likely to make politicians promise coverage expansion if the municipality's policy is behind, and they hold the promise.

Columns (3) and (4) of Table 3 examine the heterogeneity in terms of mayors (1^{st}-term vs. 2^{nd}+ term politicians.) column (3) demonstrates that for the 1^{st}-term politicians, the interaction terms are neither statistically significant nor economically large, suggesting that 1^{st}-term (novice) politicians do not seem to strategically expand the subsidy to catch up with neighbors, especially around the time of own election. This might reflect the weakness of political foundations to take the initiative in policy making or lack of experience to implement the policy at the "right" timing. In stark contrast, column (4) demonstrates the interaction terms for the 2^{nd}+ term politicians are highly statistically significant, suggesting that our main findings are largely driven by more experienced politicians.

5.3. Welfare loss

How much is the overall cost of raising the eligibility age to cover the healthcare cost of older children? This depends on whether an increase in healthcare spending adds any health benefits to the beneficiaries. Iizuka and Shigeoka (2022a) document that most of the subsidy-induced increase in healthcare utilization reflects low-value care, which does not translate into any short- and medium-term health benefits to the children.

To gauge the size of the "welfare loss," we conduct a back-of-envelope calculation. As discussed earlier, while it is difficult to remove the election timing effect per se as long as elections are held, raising the level of the policy decision to the upper level of government (i.e., prefecture) can eliminate the yardstick competition across municipalities. To the extent that the subsidy-induced healthcare utilization is wasteful, our counterfactual exercise aims at calculating such welfare loss if we could eliminate the portion of the political budget cycles that are *driven* by the yardstick competition (i.e., *interactions*).

Our baseline estimates in column (5) of Table 5 show that interaction terms for one year and four years before the election results in the increase of eligibility age by 0.098 and 0.104, respectively, which is summed to 0.202 years per month. Table 1 shows that $1(A_{i\tilde{t}} < A_{j\tilde{t}})$ takes one with a probability of 0.24, and our sample length is 120 months (10 years). Thus, we can convert the estimates into an increase in eligibility age by 5.82 years (= 0.202×0.24×120) in 10 years. By contrast, the coefficients for one year and four years before the election (without interactions) are only -0.006 (not statistically significant) and 0.018 (p-value<0.05), respectively. This suggests that without yardstick driven effect, the eligibility age increase only by 0.346 years (= 0.202×0.24×120) if yardstick competition does not exist (i.e., no influence of neighbors). These comparisons highlight that the election timing effect is mostly, if not entirely, driven by the yardstick competition, suggesting that the source of the election timing effect is the influence of neighbors (or how much politician thinks that voters care about neighbors' policies), not by the own election timing *per se*, at least in this setting

Then, how much is the additional spending by the total increase in eligibility age by 5.82 years? Based on Iizuka and Shigeoka (2022a), the average yearly outpatient spending in this age range is 731 USD. Thus, to the extent that the subsidy-induced utilization of healthcare is wasteful, 5.82 years increase in eligibility age corresponds to 4,254 USD per person.

5.4. Robustness checks

Which neighbors—. Table 4 reports the estimates from several ways of defining neighborliness: largest migration outflows (baseline) and inflows in columns (1) and (2), similarity in population size, and per capita income in columns (3) and (4). Columns (5) and (6) limit the "neighboring" municipalities to the largest and top 3 to which the citizens move most. It is reassuring that the estimates, in particular on interact terms, are more or less similar across different criteria for defining neighborliness.

Time lags—. So far, we arbitrarily choose three months lags as a reference period. Table 5 shows the estimates from equation [1] where reference periods start from 0 months up to 6 months lag, which is equivalent to the intervals of two municipal assemblies. We are reassured that our results are not particularly sensitive to the choice of the reference period.

Robustness—. We subject the baseline results to a series of other robustness checks where "out-migration" is used to define neighborliness, and three months as the time lag. Table 6 summarizes these results. Column (1) replicates our baseline estimates from column (5) of Table 2 for ease of comparison. Column (2) includes the municipality's linear time trend. It is reassuring that estimates are barely affected. Columns (3) include fixed effects for each of the twelve calendar months in each municipality to account for municipality-specific seasonality. Again, the estimates are similar. Columns (4)–(6) report estimates with different ways of constructing the samples. Column (4) excludes simultaneous election cycles. Column (5) excludes non-scheduled election cycles.[22] Column (6) uses the balanced panel, which includes 221 municipalities. All estimates are quantitatively similar to baseline estimates in column (1).

Appendix Table A3 presents another type of robustness check. We drop each prefecture from the sample to see if the estimates change. We are reassured that none of the particular prefectures drive our results.

[22] During our sample period, out of 656 elections, 11.3% (74) had non-scheduled election due to resignation (36), merger (24), death (7), and others (7).

6. Conclusion

This paper studies whether the election timing effect is driven by yardstick competition and, if so, the degree to which the yardstick competition drives the election timing effect. To explore the question, we use unique monthly data on subsidies for child healthcare in Japan that we newly collected. We exploit the institutional setup of the exogenously asynchronous election timing to over the identification issue of such spacial dependence. While the PBC and yardstick literature did not have much interaction, we study their interaction by investigating how the generosity of child healthcare subsidy changes due to the election timing effect, neighbors' generosity, and their interactions.

We find strong evidence of both the election timing effect and yardstick competitions in Japan. We further show the presence of the interaction effect of the election timing effect and yardstick competition and that yardstick competition drives the election timing effect. The yardstick-driven effect on election timing effect contributes to the increase in the eligibility age by 5.82 years, while the timing effect without yardstick competition (i.e., without the influence of neighbors) contributes to the increase in the eligibility age by only 0.346 years. These results suggest that the election timing effect is almost entirely driven by the yardstick competition or the influence of the neighbors.

Since Iizuka and Shigeoka (2022) document that most of the subsidy-induced increase in healthcare utilization seems wasteful, welfare loss from the election cycle is roughly 4,254 USD per person. The findings in this paper question the basic argument in support of decentralization because of effective service delivery and rather suggest that the appropriate level of responsibility for such populist policy may be the upper level of government structure.

Our results even suggest that municipal mayors may lose control of tailoring policy to voter preferences and municipality-specific needs as they are forced to expand subsidies due to unstoppable competition induced by their neighbors. It is even possible that some mayors may recognize the wasteful nature of subsidy expansion, but they just cannot help doing so in order to increase their chance of remaining in office.

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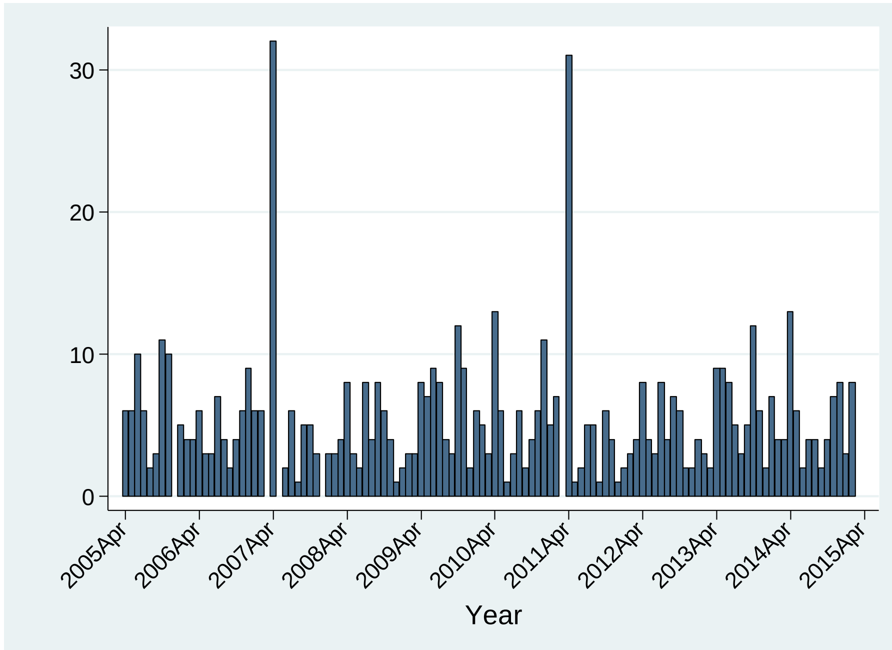
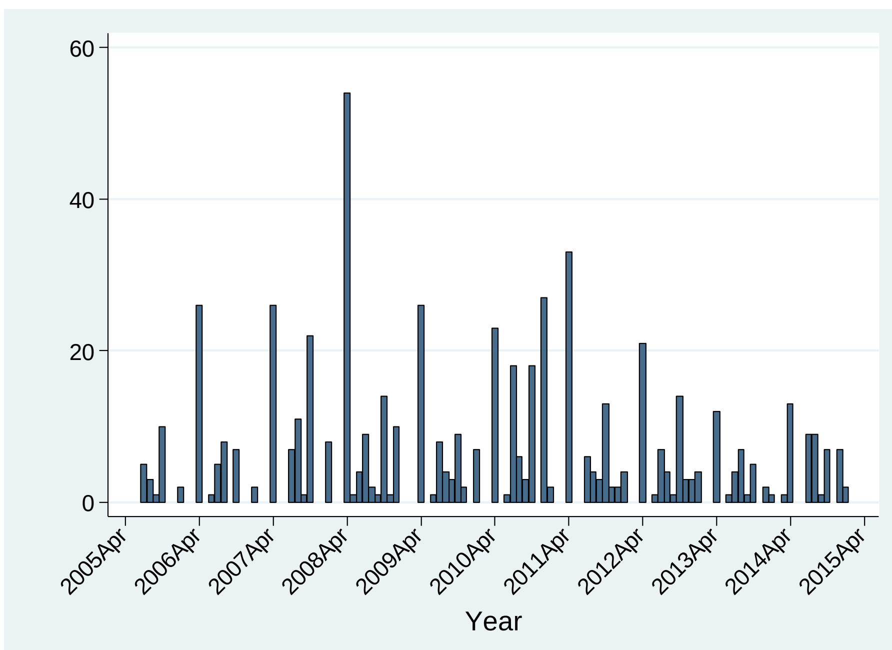
Figure 1: Exact dates of elections and subsidy expansions

Panel A. Municipalities holding elections (monthly)

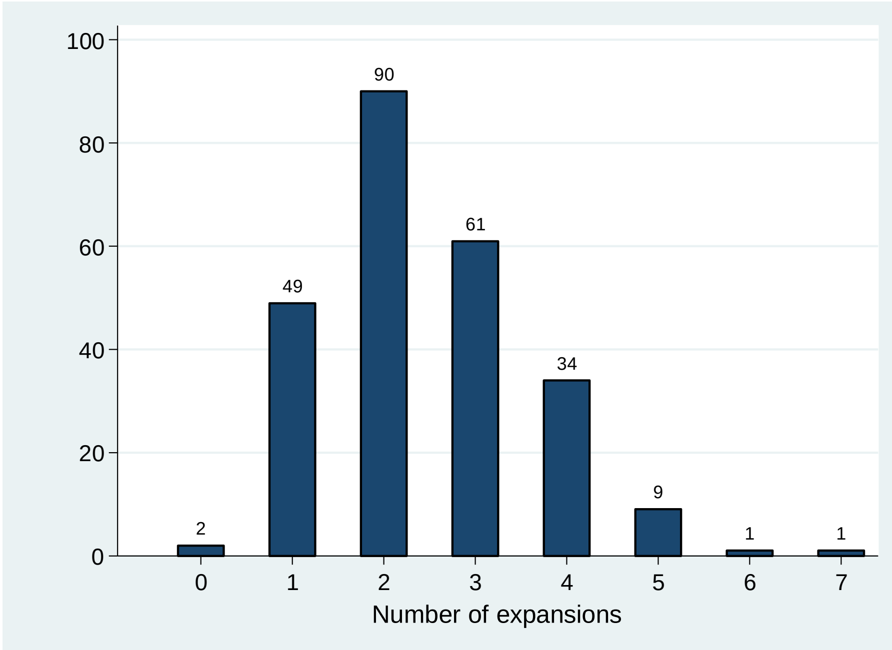


Panel B. Municipalities experiencing subsidy expansions (monthly)



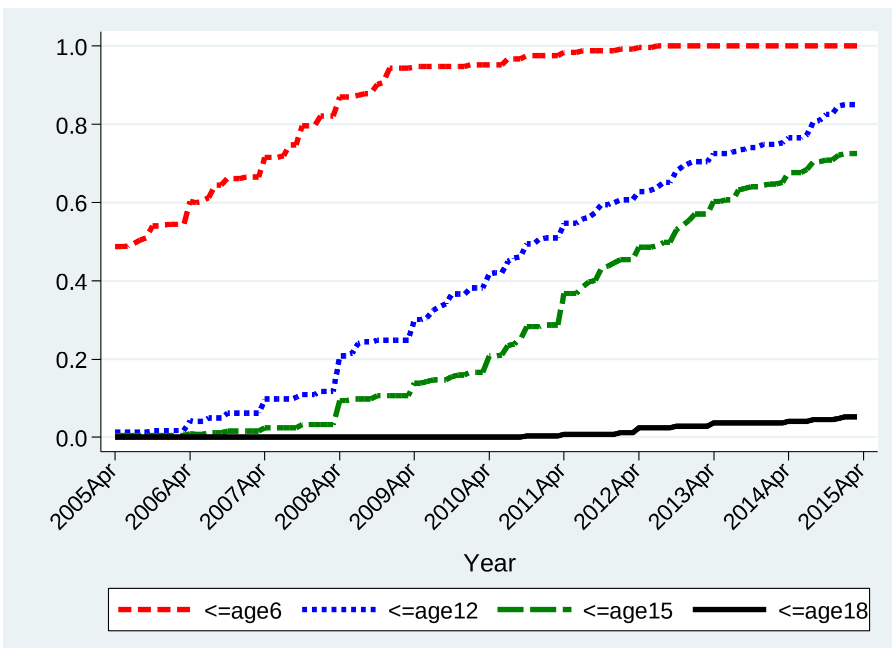
Notes: Panel A plots the number of municipalities holding elections each month from April 2005–March 2015. There is a total of 656 elections. Panel B plots the number of municipalities experiencing subsidy expansions each month during the same time period (see Figure 3 for the precise timing of all policy changes). There is a total of 606 subsidy expansions. The total number of municipalities is 247.

Figure 2: Number of subsidy expansions



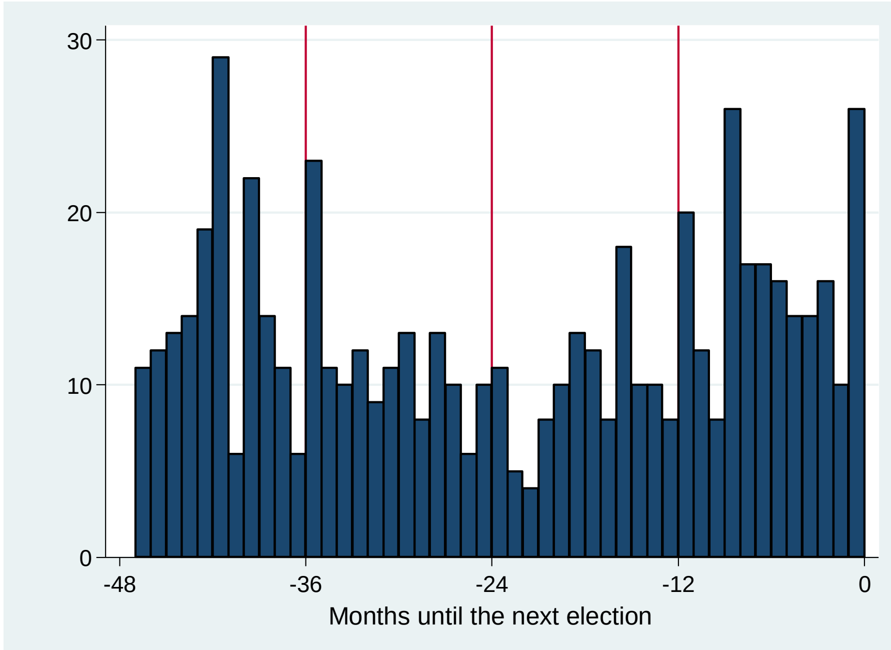
Notes: The figure plots the number of municipalities that experience a particular number of subsidy expansions from April 2005–March 2015 (see Panel B of Figure 1 on the precise timing of all policy changes). Only two municipalities out of 247 municipalities did not experience any subsidy expansions. There is a total of 606 subsidy expansions. The average number of expansions per municipality is 2.45.

Figure 3: Time series of maximum age covered by healthcare subsidy



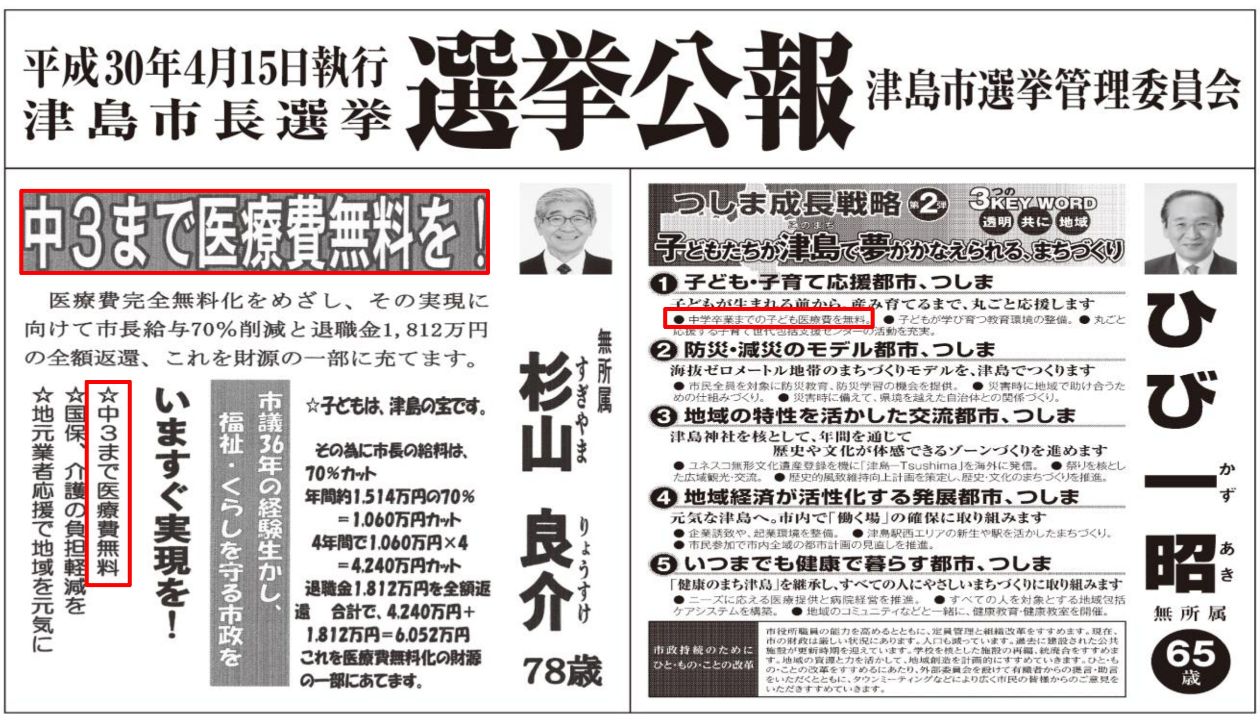
Notes: The figure plots the share of municipalities in our insurance claims data by the maximum age for the subsidy eligibility for outpatient care at the monthly level from April 2005–March 2015 (see Figure 1-B on the precise timing of all policy changes). There is a total of 247 municipalities. Ages 6,12,15, and 18 correspond to the entry into elementary schools, graduation from elementary schools, graduation from junior high schools, and graduation from high school, respectively.

Figure 4: Timing of the subsidy expansions vis-à-vis election



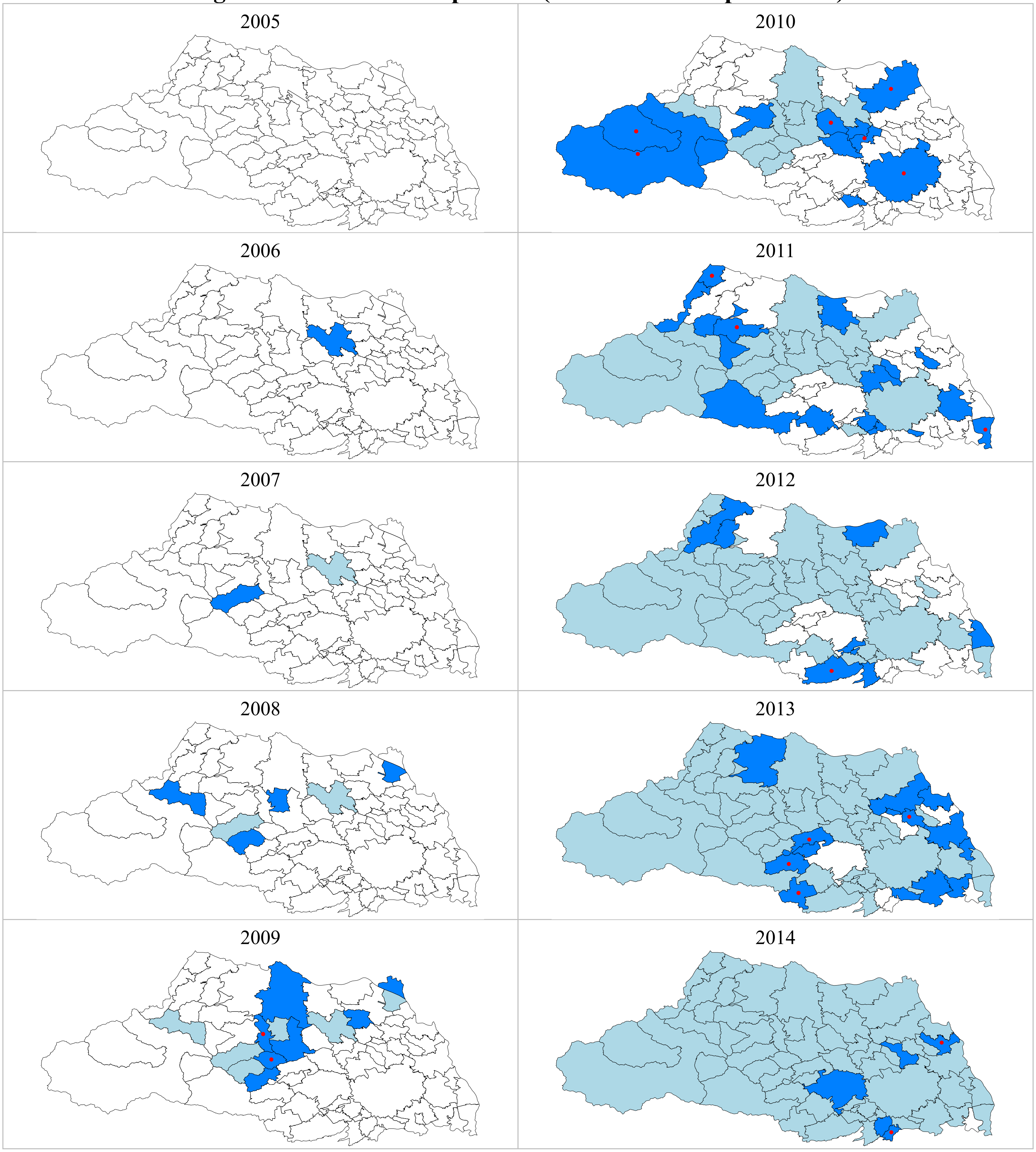
Notes: The figure plots the number of subsidy expansions by the month until the next election. There is a total of 606 subsidy expansions. There is a total of 247 municipalities.

Figure 5: The official gazette for elections



Notes: The figure is the official gazette for the municipal election at Tsushima city in Aichi prefecture held on April 15, 2018. The sentences in the red box mention the subsidy expansion for child healthcare. The candidate on the right is the incumbent (ひび 一昭), who promises to raise the eligibility age for free healthcare till the end of junior high school (中学卒業 on the right or 中 3 on the left in the gazette) which is equivalent to age 15. The candidate on the left is the opponent (杉山 良介), who also promises the same subsidy expansion. The incumbent won this election and implemented the policy one year after, on April 1, 2019.

Figure 6: Yardstick competition (case of Saitama prefecture)



Notes: Each graph describes the subsidy level every April from 2005 to 2014 in Saitama prefecture. The darker color indicates that the municipalities have expanded the subsidy to age 15 (the end of junior high school) in the year. The lighter color indicates that municipalities have expanded the subsidy to age 15 in the past. The red dots indicate that there was an election in the same year as the subsidy expansion.

Table 1: Summary statistics

Variable	N	Mean	SD	Min	Max
A. Subsidy characteristics					
Expansion dummy	29,428	0.02	0.14	0	1
Eligibility age (A_i)	29,428	9.33	4.35	2.5	18
No more than 6 ($A_i \leq 6$)	29,428	0.88	0.33	0	1
No more than 9 ($A_i \leq 9$)	29,428	0.50	0.50	0	1
No more than 12 ($A_i \leq 12$)	29,428	0.40	0.49	0	1
No more than 15 ($A_i \leq 15$)	29,428	0.28	0.45	0	1
No more than 18 ($A_i \leq 18$)	29,428	0.01	0.11	0	1
1 ($A_i < A_j$)	29,428	0.24	0.42	0	1
B. Election characteristics					
Female	29,428	0.02	0.13	0	1
Terms	29,428	2.07	1.19	1	10
1^{st} term	29,428	0.39	0.49	0	1
2^{nd}+ term	29,428	0.61	0.49	0	1
Uncontested election	29,428	0.18	0.39	0	1
Scheduled election	29,428	0.88	0.33	0	1
Simultaneous election	29,428	0.10	0.30	0	1
C. Municipality characteristics					
Population btw 0-14	29,428	0.14	0.02	0.08	0.19
Population btw 15-64	29,428	0.65	0.04	0.44	0.75
Population btw 65+	29,428	0.21	0.05	0.09	0.48
Population density	29,428	2,705	2,690	9	14,020
Per capita income	29,428	3.26	0.40	2.41	4.94

Notes: Subscripts *i* and *j* indicate own and neighboring municipality, respectively. Panels A and B are hand-collected by authors. For Panel C, all variables come from “Sichoson no Sugata,” published by the Statistics Bureau (https://www.e-stat.go.jp/regional-statistics/ssdsview, last accessed on August 1, 2019).

Table 2: Baseline results

	Election cycle	Yardstick		With interactions	
	OLS	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)
1 year before election	0.018*			0.007	-0.006
	(0.009)			(0.007)	(0.008)
4 years before election	0.040***			0.033***	0.018**
	(0.009)			(0.007)	(0.007)
$1(A_{i\tilde{t}} < A_{j\tilde{t}})$		0.090***	0.068***	0.069***	0.020
		(0.011)	(0.013)	(0.012)	(0.015)
1 year before election × $1(A_{i\tilde{t}} < A_{j\tilde{t}})$				0.047**	0.098***
				(0.022)	(0.037)
4 years before election × $1(A_{i\tilde{t}} < A_{j\tilde{t}})$				0.042**	0.104***
				(0.021)	(0.033)
R-squared	0.98	0.98	0.91	0.98	0.91
N	126,890	126,890	126,890	126,890	126,890
Cragg-Donald-Wald F-statistic			9004.6		2649.3
Kleibergen-Paap-rk Wald-F-statistic			80.0		29.8

Notes: The outcome is an eligibility age for the subsidy. The estimates α_{-k} $(k = 1, 4)$ and β from estimating equation [1] are reported with standard errors clustered at the municipality level reported in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.10

Table 3: Heterogeneity

	Type of previous election		Terms	
	Uncontested	Contested	1st term	2nd+ term
	(1)	(2)	(3)	(4)
1 year before election	0.010	-0.013	-0.010	-0.013
	(0.027)	(0.009)	(0.016)	(0.011)
4 years before election	0.050**	0.022***	0.034**	0.032***
	(0.020)	(0.008)	(0.015)	(0.010)
$1(A_{i\tilde{t}} < A_{j\tilde{t}})$	0.091***	0.010	0.082***	0.016
	(0.034)	(0.017)	(0.025)	(0.020)
1 year before election × $1(A_{i\tilde{t}} < A_{j\tilde{t}})$	0.134	0.102***	0.025	0.179***
	(0.111)	(0.039)	(0.054)	(0.058)
4 years before election × $1(A_{i\tilde{t}} < A_{j\tilde{t}})$	-0.141**	0.119***	-0.029	0.102**
	(0.069)	(0.036)	(0.047)	(0.048)
R-squared	0.86	0.90	0.87	0.90
N	22,007	104,883	49,636	77,254
Cragg-Donald-Wald F-statistic	500.2	2243.3	1024.3	1723.9
Kleibergen-Paap-rk Wald-F-statistic	13.6	27.2	16.9	28.2

Notes: The outcome is an eligibility age for the subsidy. The estimates α_{-k} $(k = 1, 4)$, β, and their interactions ρ_{-k} $(k = 1, 4)$ from estimating equation [1] are reported with standard errors clustered at the municipality level reported in parentheses. "Out-migration," which determines the degree of neighborliness by the fraction of those that move into each neighboring municipality, is used to construct the neighborliness. Significance levels: *** p<0.01, ** p<0.05, * p<0.10

Table 4: Choice of neighbors

Choice of neighbor	*Out-migration (baseline)*	*In-migration*	*Size of population*	*Per capita income*	*Out-migration (top1)*	*Out-migration (top3)*
	(1)	(2)	(3)	(4)	(5)	(6)
1 year before election	-0.006	-0.007	-0.006	-0.004	-0.004	-0.009
	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)	(0.008)
4 years before election	0.018***	0.017**	0.020***	0.021***	0.021**	0.021***
	(0.007)	(0.007)	(0.007)	(0.007)	(0.009)	(0.007)
$1(A_{i\tilde{t}} < A_{j\tilde{t}})$	0.020	0.018	0.027*	0.029*	0.056*	0.030*
	(0.015)	(0.015)	(0.016)	(0.017)	(0.029)	(0.016)
1 year before election × $1(A_{i\tilde{t}} < A_{j\tilde{t}})$	0.098***	0.105***	0.095***	0.086***	0.086**	0.111***
	(0.037)	(0.037)	(0.034)	(0.033)	(0.034)	(0.039)
4 years before election × $1(A_{i\tilde{t}} < A_{j\tilde{t}})$	0.104***	0.113***	0.096***	0.090***	0.100**	0.094**
	(0.033)	(0.034)	(0.034)	(0.033)	(0.045)	(0.037)
R-squared	0.91	0.91	0.91	0.91	0.91	0.91
N	126,890	126,890	126,660	126,336	28,421	126,890
Cragg-Donald-Wald F-statistic	2649.3	2663.4	2543.2	2599.9	386.6	2647.6
Kleibergen-Paap-rk Wald-F-statistic	29.8	29.9	35.3	36.1	21.3	29.7

Notes: The outcome is an eligibility age for the subsidy. The estimates α_{-k} $(k = 1, 4)$, β, and their interactions ρ_{-k} $(k = 1, 4)$ from estimating equation [1] are reported with standard errors clustered at the municipality level reported in parentheses. Column (1) replicates column (5) of Table 2 for ease of comparison. Significance levels: *** p<0.01, ** p<0.05, * p<0.10

Table 5: Length of lag

t is lagged by x months	*0*	*1*	*2*	***3***	*4*	*5*	*6*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1 year before election	-0.005	-0.005	-0.006	**-0.006**	-0.003	-0.005	-0.002
	(0.008)	(0.008)	(0.008)	**(0.008)**	(0.009)	(0.009)	(0.009)
4 years before election	0.026***	0.025***	0.023***	**0.018*****	0.023***	0.021**	0.023**
	(0.008)	(0.008)	(0.008)	**(0.007)**	(0.008)	(0.009)	(0.009)
$1(A_{i\tilde{t}} < A_{j\tilde{t}})$	0.020	0.021	0.023	**0.020**	0.012	0.015	0.009
	(0.016)	(0.016)	(0.016)	**(0.015)**	(0.016)	(0.016)	(0.016)
1 year before election × $1(A_{i\tilde{t}} < A_{j\tilde{t}})$	0.073**	0.073**	0.074**	**0.098*****	0.065**	0.067**	0.054*
	(0.034)	(0.034)	(0.033)	**(0.037)**	(0.031)	(0.032)	(0.030)
4 years before election × $1(A_{i\tilde{t}} < A_{j\tilde{t}})$	0.092***	0.094***	0.101***	**0.104*****	0.096***	0.101***	0.086***
	(0.035)	(0.035)	(0.035)	**(0.033)**	(0.033)	(0.034)	(0.032)
R-squared	0.91	0.91	0.91	**0.90**	0.90	0.90	0.90
N	130,250	129,130	128,010	**126,890**	125,770	124,650	123,530
Cragg-Donald-Wald F-statistic	3839.5	2879.8	2880.1	**2649.3**	3009.6	3053.6	3076.8
Kleibergen-Paap-rk Wald-F-statistic	35.1	28.1	31.0	**29.8**	30.3	29.8	28.6

Notes: The outcome is an eligibility age for the subsidy. The estimates α_{-k} $(k = 1, 4)$, β, and their interactions ρ_{-k} $(k = 1, 4)$ from estimating equation [1] are reported with standard errors clustered at the municipality level reported in parentheses. "Out-migration," which determine the degree of neighborliness by the fraction of those that move into each neighboring municipality, is used to construct the neighborliness. Column (4) with 3 month-lag is the baseline, which is identical to column (5) of Table 2. Significance levels: *** p<0.01, ** p<0.05, * p<0.10

Table 6: Robustness checks

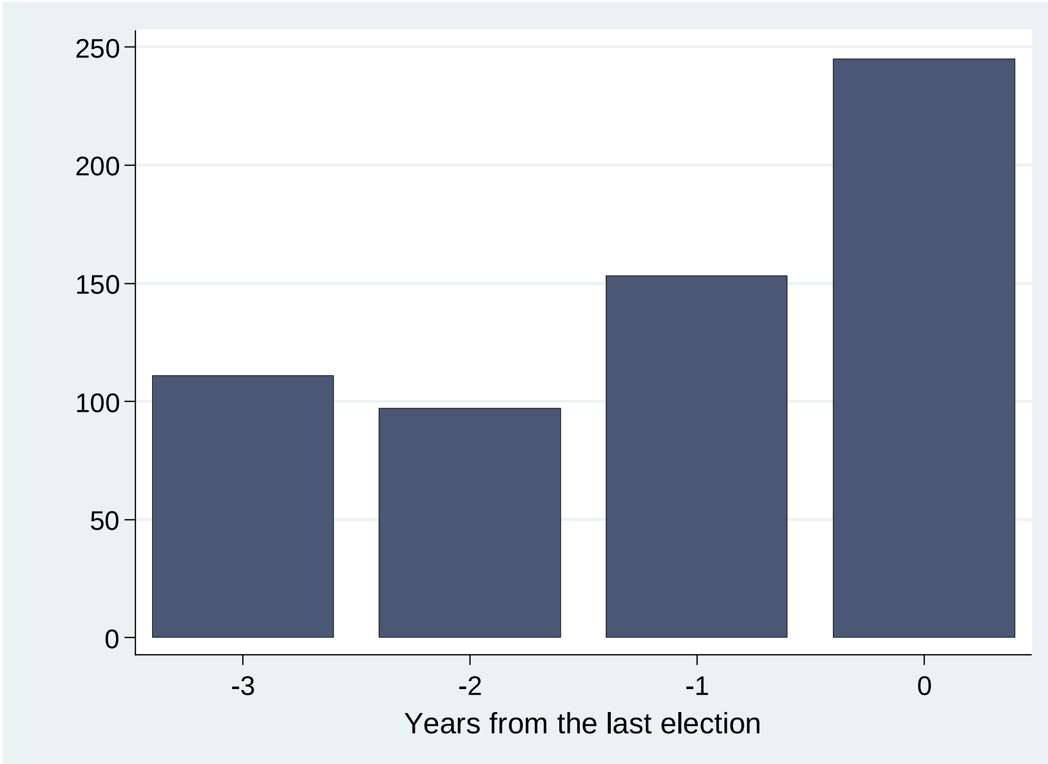
	Baseline	Mun trend	Each calendar month FE. by mun	Drop simultaneous elections	Drop non-scheduled elections	Balanced panel
	(1)	(2)	(3)	(4)	(5)	(6)
1 year before election	-0.006	-0.008	-0.003	-0.008	-0.004	-0.005
	(0.008)	(0.009)	(0.008)	(0.009)	(0.009)	(0.008)
4 years before election	0.018**	0.018**	0.020***	0.019***	0.016**	0.016**
	(0.007)	(0.008)	(0.007)	(0.007)	(0.008)	(0.007)
$1(A_{i\tilde{t}} < A_{j\tilde{t}})$	0.020	0.029*	0.026*	0.021	0.017	0.022
	(0.015)	(0.015)	(0.015)	(0.017)	(0.017)	(0.015)
1 year before election × $1(A_{i\tilde{t}} < A_{j\tilde{t}})$	0.098***	0.093**	0.089**	0.121***	0.108***	0.102***
	(0.037)	(0.039)	(0.036)	(0.045)	(0.041)	(0.038)
4 years before election × $1(A_{i\tilde{t}} < A_{j\tilde{t}})$	0.104***	0.096***	0.099***	0.111***	0.112***	0.094***
	(0.033)	(0.034)	(0.033)	(0.037)	(0.038)	(0.033)
R-squared	0.91	0.86	0.92	0.91	0.91	0.91
N	126,890	126,890	126,890	110,230	111,345	120,008
Cragg-Donald-Wald F-statistic	2649.3	2806.6	2660.5	2258.9	2448.6	2502.1
Kleibergen-Paap-rk Wald-F-statistic	29.8	28.7	29.9	25.3	28.3	28.5
Mun FE, time FE	X	X	X	X	X	X
Other covariates	X	X	X	X	X	X
Mun trend		X				
Calendar month by mun FE			X			

Notes: The outcome is an eligibility age for the subsidy. The estimates α_{-k} $(k = 1, 4)$, β, and their interactions ρ_{-k} $(k = 1, 4)$ from estimating equation [1] are reported with standard errors clustered at the municipality level reported in parentheses. "Out-migration," which determines the degree of neighborliness by the fraction of those that move into each neighboring municipality, is used to construct the neighborliness. Column (1) replicates our baseline estimates from column (5) of Table 2. Column (2) includes the municipality-specific linear trend. Column (3) includes fixed effects (FE) for each of the twelve calendar months in each municipality to control for municipality-specific seasonality. Column (4) excludes simultaneous election cycles. Column (5) excludes non-scheduled election cycles. Column (6) uses the balanced panel, which includes 221 municipalities. Significance levels: *** p<0.01, ** p<0.05, * p<0.10

Online Appendix (Not for Publication)

Yardstick Competition at Election Timing
(by Hitoshi Shigeoka and Yasutora Watanabe)

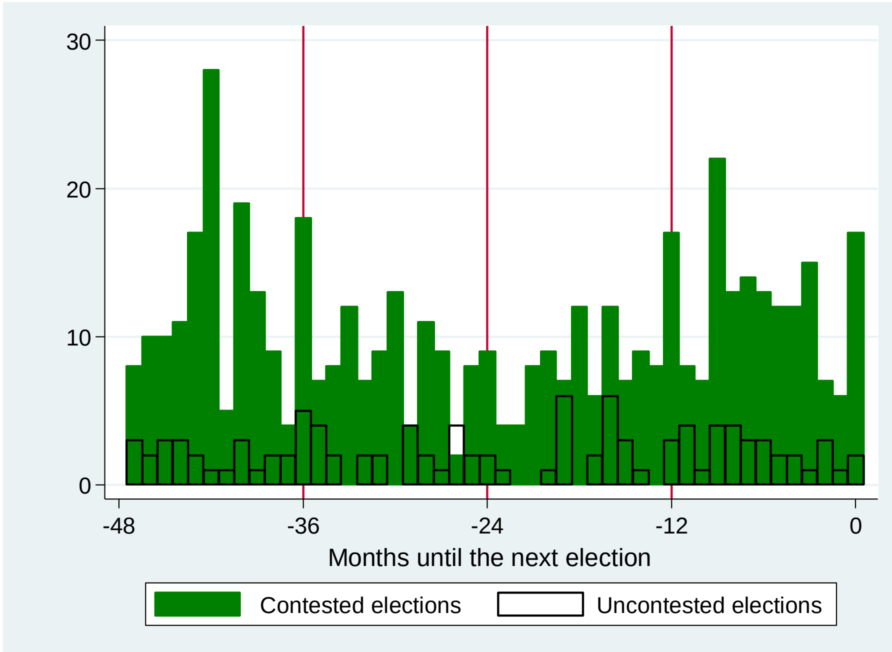
Figure A1: Year-level aggregations



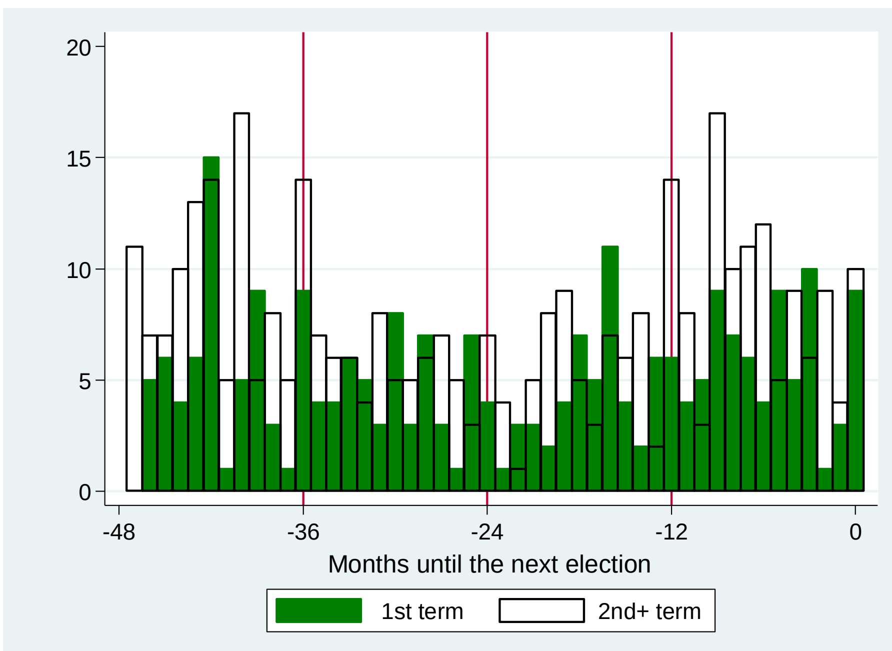
Notes: This figure shows the number of subsidy expansions by year till the next election, assuming that we only have yearly information about when the subsidy expansion is implemented.

Figure A2: Timing of the subsidy expansions (heterogeneity)

Panel A. Contested vs. uncontested elections



Panel B. 1st term vs. 2nd+ term



Notes: Panel A plots the number of subsidy expansions by the month until the next election for two types of elections: contested and uncontested. There is a total of 606 subsidy expansions, of which 497 (72%) are contested and 111 (18%) are uncontested. Panel B plots the same for 1^{st}-term and 2^{nd}+ term. 245 (40.4%) are implemented during the first term, and 361 (59.6%) are implemented during the 2^{nd}+ term.

Table A1: Balanced checks

	Simultaneous elections	Not in simultaneous elections	*Dif* =(1)-(2)
	(1)	(2)	(3)
A. 2007 elections			
Population btw 0–14	0.140	0.140	0.000
	[0.01]	[0.02]	(0.000)
Population btw 15–64	0.670	0.670	0.000
	[0.05]	[0.04]	(0.010)
Population btw 65+	0.190	0.190	0.000
	[0.06]	[0.05]	(0.010)
Population density	3648.600	2535.450	762.220
	[2851.94]	[2614.92]	(483.510)
Per capita income	1.240	1.220	0.010
	[0.10]	[0.12]	(0.020)
Number of municipalities	32	214	
B. 2011 elections			
Population btw 0–14	0.132	0.132	-0.003
	[0.016]	[0.020]	(0.003)
Population btw 15–64	0.629	0.636	-0.004
	[0.045]	[0.035]	(0.007)
Population btw 65+	0.235	0.228	0.008
	[0.057]	[0.050]	(0.010)
Population density	3629.955	2589.725	653.153
	[2870.764]	[2681.578]	(506.495)
Per capita income	1.162	1.140	0.006
	[0.104]	[0.111]	(0.021)
Number of municipalities	31	216	

Notes: The table compares the municipal characteristics across two groups (without and with simultaneous elections) in each 2007 and 2011 elections.

Table A2: Distribution of eligibility age (A_i)

A_i	N	%
2.5	1,283	4.36
3.5	1,308	4.44
4.5	709	2.41
5	12	0.04
5.5	360	1.22
6	10,301	35.00
6.5	291	0.99
7	353	1.20
7.5	24	0.08
8	105	0.36
9	2,527	8.59
9.5	24	0.08
10	180	0.61
11	36	0.12
12	3,548	12.06
15	7,957	27.04
16	32	0.11
17	24	0.08
18	354	1.20
Total	29,428	100

Notes: Ages 6,12,15, and 18 correspond to the entry into elementary schools, graduation from elementary schools, graduation from junior high schools, and graduation from high school, respectively. Age 9 corresponds to the 3rd grade of elementary school. Ages 6, 9, 12, and 15 account for 82.7% of all age distributions. Only 1.39% are above age 15, indicating the ceiling effects.

Table A3: Drop one prefecture at a time

Exclude	*Saitama*	*Chiba*	*Kanagawa*	*Aichi*	*Osaka*
	(1)	(2)	(3)	(4)	(5)
1 year before election	-0.001	-0.009	-0.007	-0.004	-0.006
	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)
4 years before election	0.015*	0.022***	0.018**	0.015**	0.021***
	(0.008)	(0.008)	(0.008)	(0.007)	(0.008)
$1(A_{i\tilde{t}} < A_{j\tilde{t}})$	0.023	0.012	0.015	0.028*	0.018
	(0.016)	(0.019)	(0.016)	(0.016)	(0.017)
1 year before election × $1(A_{i\tilde{t}} < A_{j\tilde{t}})$	0.078**	0.105**	0.111**	0.078*	0.115***
	(0.035)	(0.045)	(0.043)	(0.041)	(0.041)
4 years before election × $1(A_{i\tilde{t}} < A_{j\tilde{t}})$	0.098***	0.092**	0.119***	0.081**	0.133***
	(0.033)	(0.039)	(0.038)	(0.037)	(0.037)
R-squared	0.91	0.91	0.91	0.91	0.90
N	91,193	100,488	110,378	98,563	106,938
Cragg-Donald-Wald F-statistic	2008.26	2051.91	2,338.70	2,075.78	2,135.83
Kleibergen-Paap-rk Wald-F-statistic	29.34	24.79	26.05	23.72	26.10

Notes: The outcome is an eligibility age for the subsidy. The estimates α_{-k} $(k = 1, 4)$, β, and their interactions ρ_{-k} $(k = 1, 4)$ from estimating equation [1] are reported with standard errors clustered at the municipality level reported in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.10