# Treated Politicians, Treated Voters: A Natural Experiment on Political Budget Cycles*

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**Abstract**

When an election is approaching, incumbent politicians are motivated to manipulate the budget, hoping to increase their chances of re-election. Although this so-called political budget cycle (PBC) has been long debated by economists and political scientists, empirical studies on the PBC have yielded mixed results. This is partially because election timing is not randomly assigned, which makes it difficult to make causal inferences about the impact of an impending election on the budget. There also exist ongoing debates over *how* the budget is manipulated for electoral purposes. We address these issues by exploiting a natural experiment in Japan, where the timing of municipal elections is as good as random. We find that total and capital expenditures follow the PBC, but tax revenue does not. We also find that executive elections are associated with the PBC, but legislative elections are not. These results suggest that the patterns of the PBC are conditional on institutional settings and political contexts, implying a need for theories about electoral influence over the budget to be further refined.

When an election is approaching, incumbent politicians seeking re-election are motivated to manipulate the budget to stimulate the economy and/or reduce the tax burden, hoping to boost their votes. Since Tufte's (1978) seminal work on the topic, economists and political scientists have devoted much scholarly attention to these so-called political budget cycles (PBCs). An observable implication of the PBC theory is that budget expenditures increase and/or revenue decreases in election years as compared to other years. The empirical findings on PBCs, however, are mixed (for reviews, see Aaskoven and Lassen, 2017; de Haan and Klomp, 2013; Franzese, 2002). Existing studies which use data from developing countries or new democracies tend to show patterns consistent with the theory, but findings based on data from developed countries or long-established democracies are inconclusive (Brender and Drazen, 2005; Shi and Svensson, 2006). Beyond the question of *whether* the budget is manipulated for electoral purposes, existing studies are also divided over *how* incumbents maneuver the budget (Vergne, 2009; Wehner, 2013).

We argue that one reason for these mixed empirical findings is methodological—namely, that existing studies on PBCs may not satisfy the assumption of ignorability, which is required to make valid causal inference (Imbens and Rubin, 2015). Specifically, whether or not an election is held within a particular jurisdiction at a particular time—the treatment variable in the study of PBCs—should be independent of the potential outcomes, the total expenditure or revenue and the composition of the budget itself. Previous studies on PBCs typically regress the observed values of these outcomes on a dummy variable indicating whether or not an election was held. Crucially, if the time of the election is not predetermined (e.g., in parliamentary systems), an incumbent government may consider a range of political and economic situations in deciding when to hold the next election so that they can maximize their chances of winning the election and staying in office. In these cases, even after controlling for the observed covariates, the estimated effects of the election indicator could be biased due to any number of unobserved variables that could affect the election timing and the budget, such as the incumbent party leaders' strategic—and inherently unobservable—considerations.

A similar methodological issue would arise even if the timing of an election were fixed. For instance, every four years, a presidential election is held in the United States. This election cycle (which serves as the treatment variable in a regression model) may not be completely independent of macro-economic, non-political, short-term and long-term fluctuations in the U.S. economy (which may be confounders), in addition to changes in the U.S. budget (the outcome variable in the model). To isolate the effect of the election timing from other potentially relevant temporal factors, these confounders should be controlled. But as in studies which focus on elections with flexible timing, it is difficult to confirm that all of the relevant variables have been included in the model.

Acknowledging these methodological challenges, a recent review of the literature on PBCs argues that a "potential fruitful venue for future research could be the use of natural experiments to determine causality...which might improve inference and shed more light on previous findings and theories" (Aaskoven and Lassen, 2017). Our paper answers this call. We leverage a natural experiment in Japan where the timing of municipal elections is as good as random (Fukumoto and Horiuchi, 2011, 2016; Fukumoto and Ueki, 2015). The Japanese case also provides a unique opportunity to examine the effects of both executive and legislative elections on political budget cycles.

We find that, in Japanese municipalities, total and capital expenditures follow the PBC, but tax revenue does not. We also find that executive elections are associated with the PBC, but legislative elections are not. These results suggest that the patterns of the PBC are conditional on institutional settings and political contexts. Our in-depth analysis of the Japanese case, which considers these contexts, implies that a further refinement of theories about electoral influence on the budget is necessary.

# QUESTIONS

We revisit three questions that have been extensively examined in the PBC literature. The first is methodological, and the latter two focus on substantive issues regarding PBCs. In the following, we introduce existing research on each of these questions and then discuss how our analysis of the Japanese case will contribute to the broader literature.

## Identifying Causal Effects

We first consider how to identify the causal effects of election timing. We argue that the mixed findings that have emerged from the extant literature on PBCs arise from the non-ignorability of election timing—namely, that the timing of an election could depend on omitted variables and is therefore endogenous to the potential outcomes of the budget.

Existing studies have most commonly applied one of two approaches to address this problem. One approach is to estimate the effects of an election indicator based on the generalized method of moments with instrumental lags of a dependent variable (e.g., Baskaran et al., 2016; Drazen and Eslava, 2010; Veiga and Veiga, 2007; Vergne, 2009). An underlying assumption is that these lagged dependent variables capture the determinants of the incumbent government's decision to hold the next election. The other approach is to limit observations to cases in which election timing is predetermined (e.g., in presidential systems) so that any possible political considerations regarding the optimal timing of an election are eliminated by design (e.g., Brender and Drazen, 2005; Katsimi and Sarantides, 2012; Khemani, 2004; Shi and Svensson, 2006). Regardless of which approach is taken, however, the ignorability of election timing cannot be guaranteed unless analysts control *all* relevant confounders correlated with both the dependent (budget) and independent (election timing) variables.

In practice, it would be difficult, if not impossible, to define and control all of these potential confounders. For instance, in the U.S., an economy may rise or fall in a non-random manner over the course of four years between presidential elections, regardless of the extent to which politicians intervene in policy processes. To test whether changes in the budget respond to the timing of the election in this context, it would be necessary to isolate the effects of the election timing from other temporal factors that are related to both the budget and the election cycle. Since politicians' strategic considerations and manipulations are inherently unobservable, however, it is difficult to confirm that all of these confounders have been controlled.

Given these concerns, an alternative approach is to exploit variation in the exogenous timing of subnational elections within a country. These types of natural experiments have been used to study PBCs in Germany (Foremny and Riedel, 2014), Indonesia (Sjahrir, Kis-Katos and Schulze, 2013), Italy (Alesina and Paradisi, 2017), and Russia (Akhmedov and Zhuravskaya, 2004). The merit of this approach is that it allows for the estimation of the causal effects of election timing without defining, and controlling for, numerous covariates, because the timing of the election is (allegedly) orthogonal to potential confounders. Unlike multinational comparisons, these within-country comparisons also allow us to hold important country-specific political, economic, and social characteristics constant.

This approach, however, has limits. Since election timing is not completely randomly assigned, one cannot assume that all potential confounders are well balanced across groups. In this case, and in most natural experimental research, researchers should test the balance of covariates between the treated group (where elections are held) and the control group (where elections are not held) (Dunning, 2012, sec. 8.1.1). Nevertheless, few of the natural experiments on PBCs that have been conducted to date actually undertake this test.[1] Moreover, no existing within-country studies on PBCs examine parallel trends, despite the fact that panel data analyses with subnational-unit-specific fixed effects are, in effect, equivalent to a

[1] An exception is Alesina and Paradisi (2017), but they test the balance of just 14 covariates and only briefly mention why the dates of Italian city elections are staggered (see Footnote 1 of their article). Although 11 of their 14 covariates are well balanced (as presented in Table 3 in their article), we are not fully convinced that the ignorability assumption for election timing is satisfied.

difference-in-differences design, for which parallel trends are essential for causal inference.

Our study exploits a natural experiment in Japan (Fukumoto and Horiuchi, 2011, 2016; Fukumoto and Ueki, 2015). As we describe later, several features of Japan's unique history suggest that treatment assignment (election timing) is ignorable. Furthermore, political and fiscal institutions are exactly the same across municipalities within Japan, which makes it a particularly suitable case for within-country comparisons.[2] Finally, unlike previous natural experimental studies on this topic, our analysis involves balance tests for 89 covariates and a careful examination of parallel trends.

## Total Amount or Composition of the Budget

We next consider whether the government manipulates the total amount or composition of the budget. Classic theories, such as that of Tufte (1978), expect that total expenditures increase *and* total revenue decreases just before an election, though both rebound afterward. Empirically, most studies show that governments spend more before an election but do not decrease their tax revenue (for reviews, see Aaskoven and Lassen, 2017; de Haan and Klomp, 2013; Franzese, 2002). This empirical regularity is in part due to the fact that the effect of a tax cut on economic performance is not direct and immediate enough for the government to reap the political benefits (Schuknecht, 2000, 117), and also because expenditures are more decentralized than revenues (Alt and Lassen, 2006, 546). Nevertheless, some research finds contradictory evidence, which suggests that governments will indeed decrease revenue before an election (Alesina and Paradisi, 2017; Foremny and Riedel, 2014; Katsimi and Sarantides, 2012; Persson and Tabellini, 2003).

To add further nuance to the scholarly debate on PBCs, more recent theorists argue that incumbents do not dare to manipulate the *total amount* of the budget because they are concerned about voters' rational expectations about their representatives' behavior—

[2] Relevant laws include the Local Allocation Tax Act, the Local Autonomy Act, the Local Government Finance Act, the Local Tax Act, and the Public Offices Election Act.

namely, that voters will punish rather than reward incumbents for aggravating a fiscal deficit (Brender and Drazen, 2005; Shi and Svensson, 2006). These scholars argue that politicians try to change the *composition* of the budget instead, but the way in which they do so is also a matter of ongoing debate (for a review of this debate, see Vergne, 2009).

On the one hand, Rogoff (1990) argues that the government shifts budgetary resources from capital investment (e.g., the construction of infrastructure, such as roads, schools, and water plants) to current consumption (e.g., social security, subsidies, and wages). This is because current consumption is more "visible" for voters and sends a more effective signal about the incumbent's competence. Akhmedov and Zhuravskaya (2004), Katsimi and Sarantides (2012), and Vergne (2009) provide empirical support for this "visibility" hypothesis.

On the other hand, Drazen and Eslava (2010) claim that the government manipulates the composition of the budget in the opposite direction—that is, from current consumption to capital investment (see also Gonzalez 2002 and Schuknecht 2000). The rationale here is that capital investment is more "targetable" to special interest groups or specific geographic constituencies and, thus, more effective for incumbents to win votes. Evidence for this targetability hypothesis comes from Drazen and Eslava (2010), Khemani (2004), and Schuknecht (2000).

We contribute to this ongoing debate with rich public finance data from Japan. While several of the aforementioned studies have examined the impact of election timing on either the total amount or the composition of the budget, we use detailed municipality-level data to test the effects of election timing on both. This allows us to present a more comprehensive picture of how the PBC operates.

## Executive or Legislative Elections

Finally, we consider whether changes to the budget occur in the context of executive or legislative elections. As in debates over the total amount or composition of the budget, existing studies tend to focus on either executive or legislative elections in their analyses

(Vergne 2009, Wehner 2013, 547). In analyses of parliamentary systems, the primary focus is legislative elections because no executive elections are held. In presidential systems, past research tends to examine executive elections because legislative elections are often held at the same time as executive ones (Foremny and Riedel, 2014).[3]

Yet, as Wehner (2013) points out, "the budget-formulation process in any democratic country involves two distinct processes: executive formulation of a budget proposal and legislative review and approval...both of these could be important for explaining electoral budget cycles" (547). But how could one identify changes to the budget that are associated with legislative elections and modifications associated with executive elections, separately? This is usually difficult because the timing of these two types of elections is highly correlated (Foremny and Riedel, 2014, 60). Japan, however, provides a unique opportunity to address this identification challenge because, for several idiosyncratic historical reasons detailed below, Japanese mayoral and assembly members' elections are often held at different times.

# METHODS

Our analysis employs data from municipal elections in Japan to examine these three questions. In this section, we first elaborate on why our research design constitutes a natural experiment.[4] We then specify how we identify the effects of electoral timing on the Japanese budget.

[3]A few studies analyze PBCs in connection with midterm legislative elections (e.g., Katsimi and Sarantides 2012, 355, Persson and Tabellini 2003, 254–255).

[4]For more details, see Fukumoto and Horiuchi (2011) and Fukumoto and Ueki (2015).

## Context and Research Design

Japanese municipal governments operate under a presidential system, in which executive chiefs (i.e., mayors) and members of the legislative branch (i.e., unicameral assembly members) are directly elected. In April of 1947, just before the current constitution came into force, all of the Japanese municipalities held their first executive and legislative elections. Since terms for Japanese mayors and municipal assembly members are four years, subsequent elections were scheduled, in principle, for every fourth year in April. As time advanced, however, a substantial proportion of municipalities dropped out of this four-year cycle of simultaneous local elections (hereafter, SLEs). Once an election was held off the SLE cycle, subsequent elections usually remained off the cycle because the length of the subsequent term is always four years, not the remainder of the previous term.

Our data come from SLEs held on April 27, 2003 (Fukumoto and Ueki, 2015).[5] On this day, 21.3% and 52.2% of 3,010 municipalities held executive and legislative SLEs, respectively.[6] Figure 1 illustrates when municipalities' executive (left panel) and legislative (right

[5]The data are available to download from `http://www-cc.gakushuin.ac.jp/~e982440/research/Fukumoto_Ueki_replication.zip` (last accessed on March 15, 2018). We focus on this year because Japanese municipalities experienced a wave of drastic municipal mergers from 2004 to 2006. At that time, almost half of the municipalities merged and many of them held their first post-merger elections off the SLE cycle. Fiscal policy after the mergers (at least for certain years) could therefore be influenced by not only the election timing itself, but also by municipalities' strategic considerations about mergers (Horiuchi, Saito and Yamada, 2015). In other words, the timing of any post-2004 elections could be non-ignorable. For this reason, we analyze the latest SLEs prior to 2004.

[6]Although we had 3,210 municipalities as of April 27, 2003, following Fukumoto and Horiuchi (2011), we exclude 105 municipalities whose election timing is not determined in the standard way. The excluded municipalities include the 13 largest cities in Japan, which were given special status by an ordinance from the Japanese government, as well as the 23 special wards in the Tokyo Metropolitan Area, whose fiscal institutions and socio-economic situation are very different from the other municipalities (i.e., more urban and wealthier). We also exclude 95 municipalities that merged during our study period (from April 1, 2000 to March 31, 2004) because their treatment status and potential outcomes cannot be well defined. For mergers,

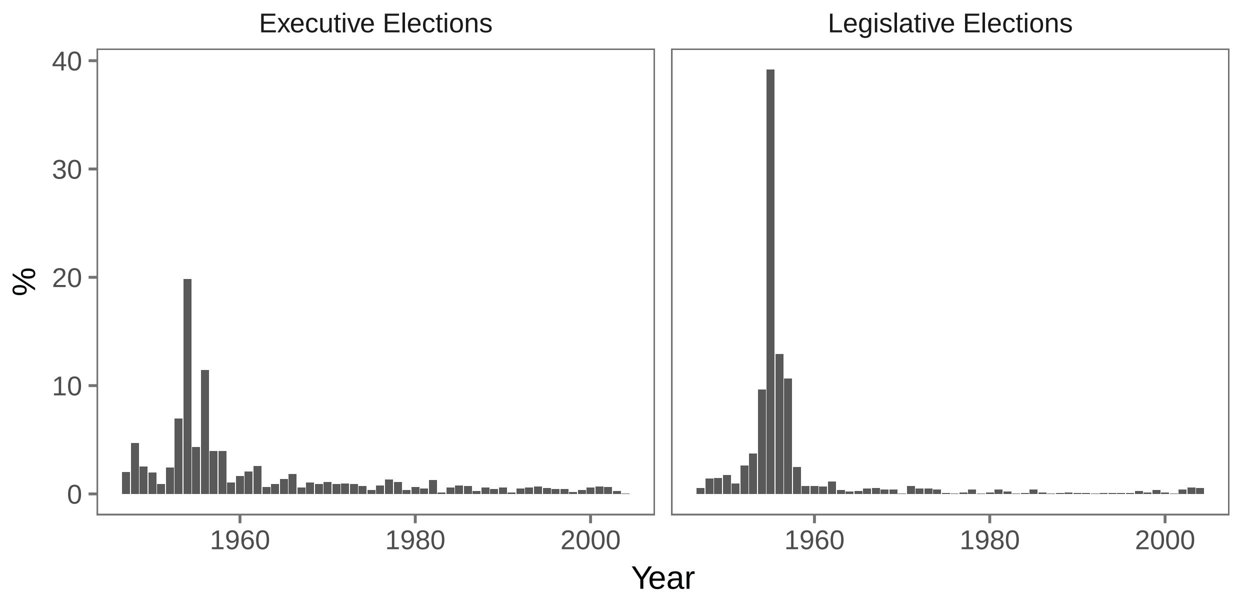


Figure 1: When Municipalities Deviated from the SLE Cycle

panel) elections deviated from the SLE cycle. The horizontal axis shows the calendar year, and the vertical axis represents the percentage of municipalities that dropped off from the SLE cycle in each year (among municipalities which did not hold SLEs on April 27, 2003). Most of them departed from the SLE cycle as early as the 1950s. Figure 2 summarizes *why* these municipalities deviated from the SLE cycle. The most common reason is a municipal merger. Other reasons include resignation and death (executive elections, left panel), and dissolution and general resignation (legislative elections, right panel).

It is important to emphasize that these historical departures from the SLC cycle, or the events preceding them, most likely have not influenced the budgetary and political processes of the early 2000s. Put another way, any pre-treatment variables (i.e., reasons for deviating from the SLE cycle decades ago) that could affect the treatment variable (i.e., whether or not an election was held on April 27, 2003) are unlikely to affect the outcome variables (i.e., the total amount and composition of the budget in the 2000s). We therefore proceed with

we refer to `http://www.soumu.go.jp/gapei/gapei.html` (last accessed on March 15, 2018).

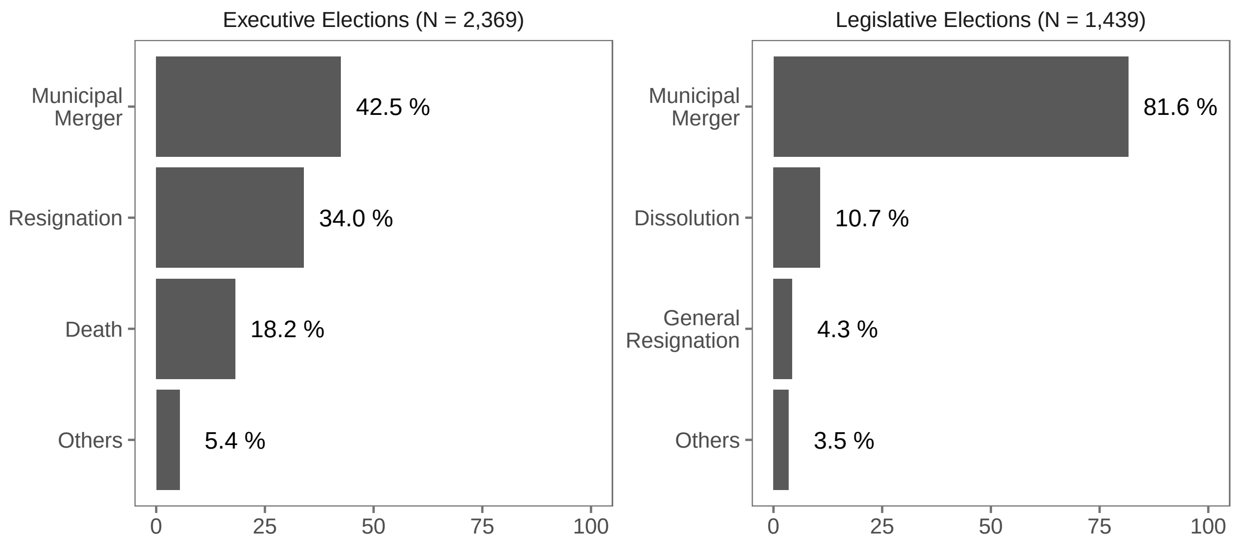


Figure 2: Why Municipalities Deviated from the SLE Cycle

the assumption that the timing of Japanese municipal elections is ignorable.

In what follows, we refer to whether a municipal mayoral election was held on April 27, 2003 as the *executive treatment variable*, and whether a municipal assembly election was held as the *legislative treatment variable*. Those municipalities that remained in the 2003 SLEs are the (executive or legislative) *treated* group of municipalities, and those that did not are the *control* group of municipalities.

## Identification Strategy

We estimate the effects of the treatment variables based on a difference-in-differences design. The outcome variable is the *change*—from the previous fiscal year to the current fiscal year—in the natural log of the amount for a specific budget category (e.g., total revenue, capital expenditure) in each municipality. We focus on changes from 2001 to 2002 and from 2002 to 2003. This is because it was theoretically during the fiscal year 2002 (i.e., from April 1, 2002 to March 31, 2003) that incumbent politicians of the treated group would have intervened in budgetary processes in order to influence the results of the April 2003 elections.

Although this difference-in-differences approach can effectively control all municipality-specific and time-*invariant* variables, it cannot control short-term changes in demographic, economic, and social characteristics at the municipality level during the early 2000s. It is unlikely, however, that such short-term fluctuations are substantively associated with our treatment variables. This is because, as we noted above, whether a municipality dropped off from the four-year election cycle was, in most cases, an event in the distant past. For example, it would be difficult to imagine that demographic and political considerations for a municipal merger in the 1950s would systematically affect *small, year-to-year changes* in the demographic composition of that municipality in the early 2000s. This ignorability assumption is the crux of our natural experiment; crucially, our difference-in-differences design enables us to estimate the causal effects of the treatment variables without bias.[7]

We use two approaches to check validity of this ignorability assumption. First, we test for balance across dozens of covariates. In order for the ignorability assumption to hold, short-term changes in a range of variables measured at the municipality level should be balanced between the treated group (i.e., municipalities that participated in the 2003 SLE) and the control group (i.e., municipalities that did not). To conduct this test, we substitute a dependent variable with each of the covariates in our difference-in-differences analysis.

Second, we check for parallel trends. Specifically, we measure the change in each of our outcome variables from 2000 to 2001 and examine whether the trends are the same between the treated and control groups. We focus on the change from 2000 to 2001 because these two years precede the fiscal year 2002, the year in which any political manipulations of the budget would occur in the treated group.

[7]See the Online Appendix for the formal presentation of our statistical model and the key assumption necessary for causal identification.

# HYPOTHESES

We now specify our hypotheses. We first introduce hypotheses regarding the effects on expenditure or revenue. We then discuss our predictions regarding budget manipulations in the context of executive or legislative elections.

## Expenditure

In Japan, a fiscal year starts in April and ends in March. As long as incumbents in the (executive or legislative) treated group want to bring about tangible benefits to their voters as close as possible to the SLEs on April 27, 2003, we would expect them to increase total budget expenditures in the fiscal year 2002.[8] We therefore hypothesize that municipalities in the treated group (who held the 2003 SLE), as compared to those in the control group (who did not hold the 2003 SLE), would show the following PBC:

**Hypothesis 1 (Total Expenditures)** *In the treated group, as compared to the control group, total expenditures* increase *in 2002 (as compared to 2001), and* decrease *in 2003 (as compared to 2002).*

Some rational expectation theorists posit that in developed countries and well-established democracies, total expenditures do not follow the PBC because voters would punish incumbents who fail to balance the budget (Brender and Drazen, 2005; Shi and Svensson, 2006). We doubt that this argument holds in Japan for two reasons. First, Japanese municipalities depend on transfers from the central government for almost one-third of their revenue

[8]Some mayors and assembly members in the treated group may plan to retire and thus have no incentive to manipulate the budget for re-election purposes. In addition, some politicians in the control group may face re-election in 2003 but not on the SLE date, and thus have as great an incentive to manipulate the budget as those in the treated group. Similarly, in other fiscal years, some (but not all) incumbents in the control group might want to increase their government's budget expenditures if they are up for re-election in the next year. Our estimates are therefore conservative.

(Sōmushō, 2003, 13), which means that they lack an incentive to balance the budget.[9] In other words, their budget constraints are soft. Second, Japan is notorious for its lack of fiscal transparency (Alt and Lassen, 2006, esp. 534). When politicians are not afraid of scrutiny, they may be more motivated to manipulate the budget for political gain—regardless of whether such maneuvers could be wasteful from other perspectives. Japanese municipal politicians might therefore spend unrestrictedly if they think that doing so will win them votes in an upcoming election.

We previously introduced two key concepts to summarize the debate on political manipulation of capital vs. current expenditure: "targetability" vs. "visibility" (Vergne, 2009). In the case of Japanese municipal elections, the former seems to be more relevant. Many Japanese municipalities cover a relatively small geographical area. Within these compact jurisdictions, politicians can organize interest groups more effectively and work with them more closely; in other words, "public investment projects are easier to *target* to critical constituencies" (Khemani, 2004, 151, emphasis added).[10] It is also easier to fine-tune the timing of capital expenditure than that of current expenditure (Schuknecht, 2000, 118). Finally, since municipalities are relatively small, the "visibility" of public projects is not necessarily low (Katsimi and Sarantides, 2012, 330). We thus expect the following pattern:

**Hypothesis 2 (Capital and Current Expenditures)** *In the treated group, as compared to the control group, capital expenditures* increase *in 2002 (as compared to 2001) and* decrease *in 2003 (as compared to 2002), but current expenditures do not change between these years.*

[9] Existing studies suggest that local governments are more likely to lose fiscal discipline when their own revenue is small (Baskaran et al., 2016) or when the central government can bail them out (Rodden, 2006).

[10] Some studies of Japanese political economy indeed suggest that politicians allocate disproportionately large sums of the budget to targeted constituencies (Hirano, 2006).

## Revenue and Deficit

Politicians may also be motivated to reduce voters' tax burden in the fiscal year leading up to the election (Nelson, 2000; Persson and Tabellini, 2003; Veiga and Veiga, 2007). Reversing the underlying logic of Hypothesis 1, therefore, one might expect that total tax revenue would *decrease* in 2002 (as compared to 2001), and *increase* in 2003 (as compared to 2002). We do not necessarily think, however, that this expectation would hold in the Japanese context because in Japan, the Local Tax Act sets the *standard* (or *de facto* minimum) and maximum tax rates for many taxable items and regulates what items municipalities can tax in the first place.

If municipal governments in the treated group do not raise taxes in the fiscal year 2002, they must finance expenditure surges in the same year in alternative ways.[11] One option is to increase non-tax revenue, the majority of which consists of intergovernmental transfers. Existing studies of Japanese political economy examine how politicians manipulate intergovernmental transfers for electoral purposes (Hirano, 2006; Horiuchi and Saito, 2003; Kohno and Nishizawa, 1990).[12] The other option is to issue bonds (which increases the deficit). In the PBC literature, these tools are considered to be politically feasible because they do not hurt voters (neither directly nor instantly) and are less visible (Persson and Tabellini, 2003; Veiga and Veiga, 2007).

The ways in which Japanese municipal politicians can manipulate non-tax revenues and the process of issuing bonds, however, is not straightforward. On the one hand, local governments have little discretion over intergovernmental transfers. Indeed, existing studies suggest

[11] Total expenditure is financed by local taxes (34.3%), transfers from the national and prefectural governments (38.0%), bonds (or deficits, 10.1%), and other revenues (17.7%; charges, transfers from public business accounts and funds, income from loan principal and interest, money brought forward from the previous year, share payments, burden payments, property income, and contributions) (Sōmushō, 2003, 13 and 56).

[12] Veiga and Pinho (2007) and Veiga (2012) show that intergovernmental transfers (from the EU and the national government) follow the PBC in Portugal.

that politicians in the national legislature, not local politicians, manipulate intergovernmental transfers for their own political gain (e.g., Horiuchi and Saito, 2003). When it comes to bonds, municipalities were not able to issue bonds without their governor's permission until 2005 (Local Autonomy Act, Article 250). Before 2005, municipal politicians needed to develop pipelines with national and prefectural politicians in order to influence the central and prefectural governments' decisions (Horiuchi, Saito and Yamada, 2015; Scheiner, 2005). It is unclear, however, whether municipal politicians could successfully convince national and prefectural leaders to manipulate these fiscal measures for their own benefit. Considering these constraints and uncertainties, we do not have a specific hypothesis regarding the impact of election timing on non-tax revenues and deficits. In addition, we are agnostic about the effects of election timing on *total* revenue, which is a weighted average of the effects of the treatment on tax revenues and non-tax revenues. We do not offer a prediction about which component's effect is larger.

## Executive vs. Legislative Elections

Our expectation regarding whether the PBC is associated with executive elections, legislative elections, or both, is consistent with a more general prediction that "in the presidential system, the chief executive is better able to target narrow constituencies" (de Haan and Klomp, 2013, 394). Furthermore, in the context of Japan, mayors have much more flexibility to determine the budget than do assembly members. Specifically, the Local Autonomy Act imposes identical fiscal institutions on all municipalities. Under this law, a mayor can submit a budget to the assembly, but assembly members cannot (Articles 112, 149, and 211). An assembly may approve, amend, or reject the budget, but an assembly cannot increase the total amount of the budget to the degree that the mayor's power to submit the budget is nullified (Articles 96 and 97). When an assembly amends or rejects the budget, a mayor may resubmit the original budget for reconsideration (Article 176).[13] Given the executive's greater flexibility in crafting the budget, our hypothesis is the following:

**Hypothesis 3 (Branch)** *In the* executive *treated group, as compared to the* executive *control group, expenditures (particularly, capital expenditures) increase in 2002 (as compared to 2001) and decrease in 2003 (as compared to 2002).* Legislative *elections, however, do not generate a similar cycle.*

# ANALYSIS

In this section, we begin by explaining the data we use in our analysis. Next, we check validity of our identification assumption using two different approaches. Finally, we present our results.

## Data

Based on official financial statistics published by Japanese local governments,[14] we use three total amount categories: total expenditure, total revenue, and deficit.[15] We then divide total expenditure into two components: capital expenditure and current expenditure.[16] We also

[13]If the assembly amends or rejects the budget again, the assembly's decision is final, except in some legally mandatory cases (Articles 176 and 177).

[14]Our data come from the *Chihō Zaisei Jōkyō Chōsa* [Survey on Local Government Finance], available at `https://www.e-stat.go.jp/` (last accessed on August 17, 2017). The total amounts for expenditure, revenue, and deficit (in nominal thousand yen) are based on ordinary accounts (consolidated accounts, excluding public business accounts) at the time of settlement for each fiscal year.

[15]The Japanese government calls the sum of the total revenue and the deficit, which we use for our analysis, the "total revenue" (*Sainyū Sōgaku*).

[16]The Japanese government classifies ordinary construction, disaster relief, and unemployment measures as "investment expenditure," which we consider capital expenditure. We consider all the other expenditure items as current expenditure.

divide total revenue into two components: tax revenue and non-tax revenue.[17] We take the natural log of each of these statistics and use them as our outcome variables.

The executive treatment variable is equal to one if a municipality held its mayoral election on April 27, 2003, and zero otherwise. The legislative treatment variable is defined similarly for municipal assembly elections.[18]

With regard to covariates, we use data from Sōmushō Tōkeikyoku (2001–2004). This official database is published annually by the Statistics Bureau of the Ministry of Internal Affairs and Communications and contains 103 variables relating to demographic, social, and economic characteristics for every municipality in Japan. After deleting 14 variables which were not appropriate for our analysis, we were left with 89 covariates.[19] Given that these variables tend to be skewed, we take the natural log of each variable.[20] Descriptive statistics on the variables we use in our analysis are presented in the Online Appendix.

## Validation of Assumption

We check validity of the ignorability assumption required to make our causal interpretation of the treatment effects in two different ways: testing for covariate balance and testing for parallel trends.

To check the balance of municipality-specific and time-variant variables between the

[17] We consider the local tax (*chihō zei*) as tax revenue and classify all the other revenue items as non-tax revenue. For more information on these items, see Footnote 11.

[18] Footnote 5 includes the source of these data.

[19] We dropped six fiscal variables which are parts or derivatives of our outcome variables. We also dropped two variables which are not available before our study period (the year 2000). Finally, we dropped six variables with missing values for more than 5% of municipalities. More details on both the included and excluded covariates are included in the Online Appendix.

[20] We made very minor adjustments for observations with zero or missing values. The complete set of original data and the R scripts used to merge, clean, and analyze the data will be published on the Dataverse once this paper is accepted for publication.

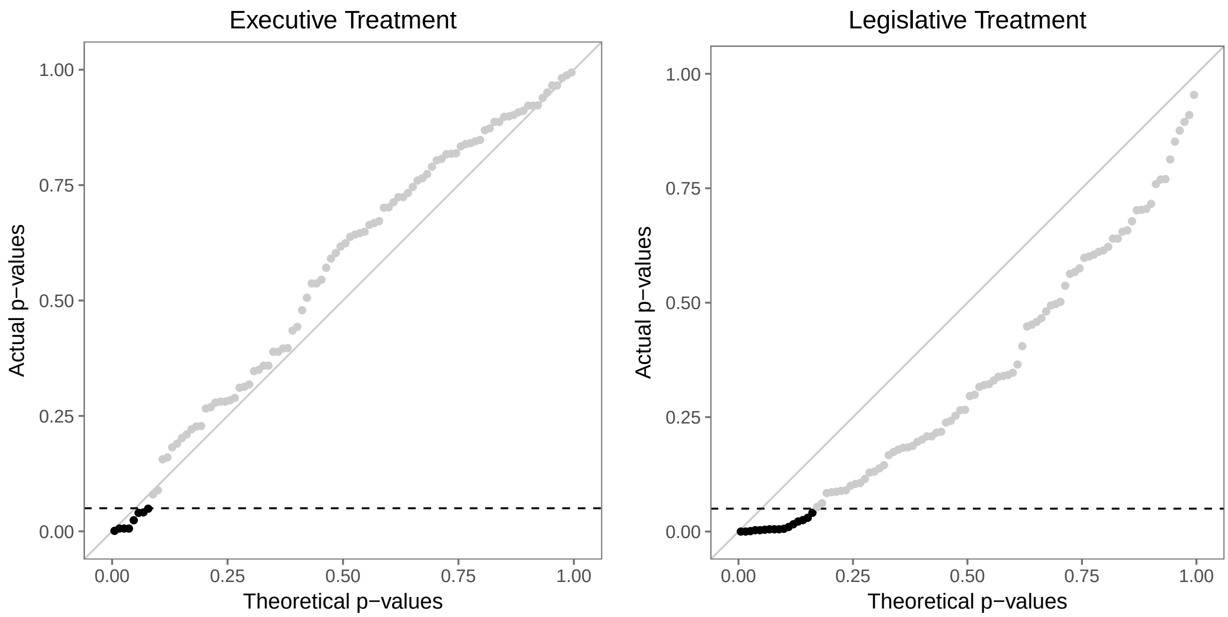


Figure 3: QQ Plots of Covariate Balance

treated and control groups, we estimate the effect of either the executive treatment variable or the legislative treatment variable on each of the 89 covariates based on our difference-in-differences design. If the $p$-value of the coefficient is smaller than 0.05, the particular covariate may not be balanced across groups. Given that we have as many as 89 regression coefficients, however, some of them could be significant at the 0.05 level by pure chance. To account for this possibility, we create a quantile-quantile (QQ) plot using the 89 $p$-values for each treatment variable. If the $p$-values are uniformly distributed between 0 and 1 (and thus are completely at random), the dots should be on the 45-degree line.

Figure 3 displays the QQ plots for the executive treatment (left panel) and for the legislative treatment (right panel). The dotted horizontal lines represent $p = 0.05$, and the $p$-values that are lower than 0.05 are highlighted in black. For the executive treatment (left panel), only six (7%) of the 89 $p$-values fall below 0.05, which suggests that the pattern of significant results could be due to chance. More importantly, the dots are fairly close to the 45-degree line. When we conduct the univariate Kolmogorov-Smirnov test (KS test)

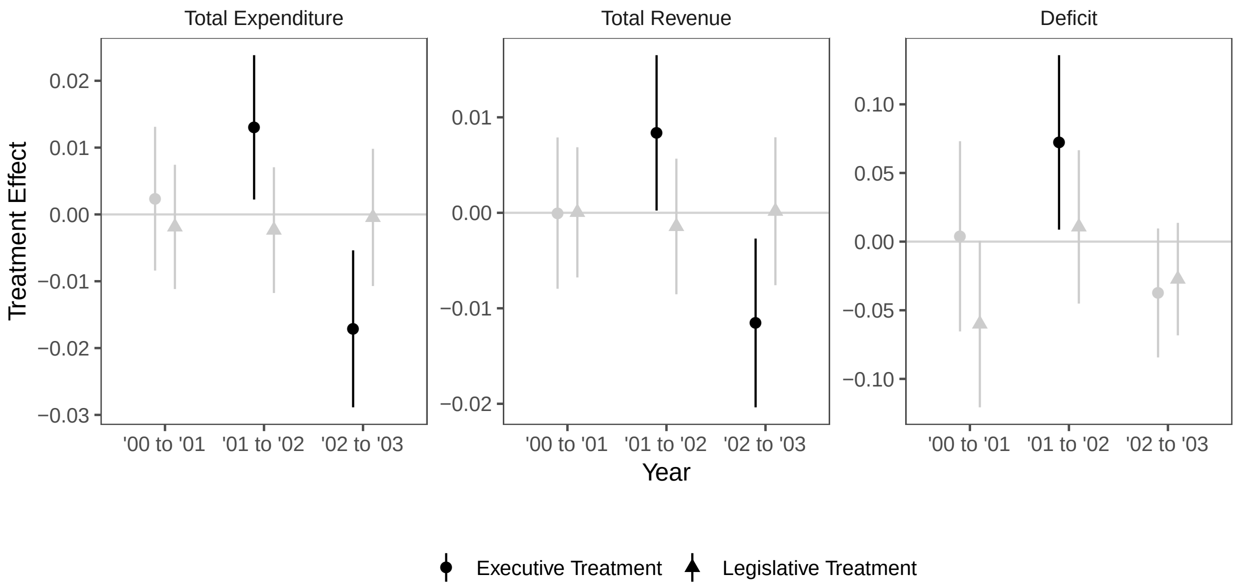


Figure 4: Treatment Effects on the Total Budget

between the actual and theoretical $p$-values, the bootstrapped $p$-value of the KS test is equal to 0.529.[21] For the legislative treatment (right panel), 14 (or 16%) of the 89 $p$-values are smaller than 0.05 and the dots are slightly away from the 45-degree line. In this case, the bootstrapped $p$-value of the KS test is 0.004. To probe these results further, we examine all of the variables that are not balanced between the treated and control groups (listed in the Online Appendix), but we do not find any systematic patterns.

Overall, the results of our balance tests are mixed. While we believe that the assignment of the treatment variables in this analysis is random, we control for the unbalanced variables anyway. This is a standard and recommended practice in observational studies (e.g. Angrist and Pischke, 2009, 175).[22] The results without adding these covariates, which are substantively similar to the results with them, are reported in the Online Appendix.

[21]We use the `ks.boot()` function in the `Matching` library (Version 4.9-2).

[22]Due to some missing values in our data, the number of municipalities used in the regression analyses is $N = 2,964$ (98% of $3,010$) for the executive treatment or $N = 2,855$ (95% of $3,010$) for the legislative treatment.

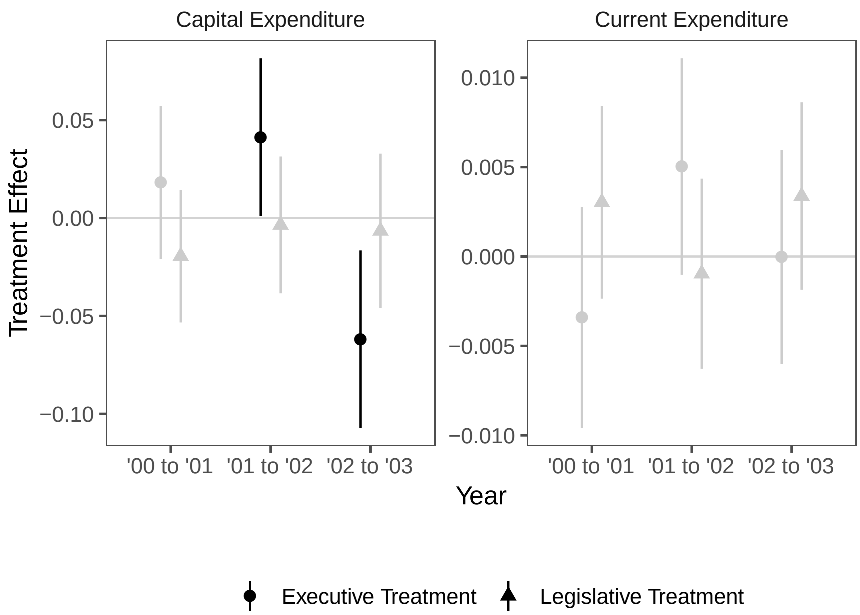


Figure 5: Treatment Effects on Expenditure Composition

Next, we turn to the parallel trends. Figure 4 displays the estimated effects of each treatment variable on the three outcome variables for the total amount of the budget: total expenditure (left panel), total revenue (middle panel), and deficit (right panel). In each panel, the horizontal axis indicates year $t-1$ to year $t$ (where $t \in \{2001, 2002, 2003\}$ and only the last two digits are displayed), and the vertical axis represents the treatment effects on the changes in each outcome variable. Dots correspond to point estimates, and bars illustrate the 95% confidence intervals. If a bar does not cover zero, it is statistically significant at the 0.05 level and is drawn in black; otherwise, the bar is shown in gray. For each year, the left circle and bar show the coefficient for the executive treatment, and the right triangle and bar show the coefficient for the legislative treatment. Similarly, Figure 5 shows the coefficient estimates for the composition of the expenditure: capital expenditure (left panel) and current expenditure (right panel). Finally, in Figure 6, the coefficients for the composition of the revenue are shown: tax revenue (left panel) and non-tax revenue (right panel).

To assess the parallel trends, we focus on the treatment effects on the changes from 2000

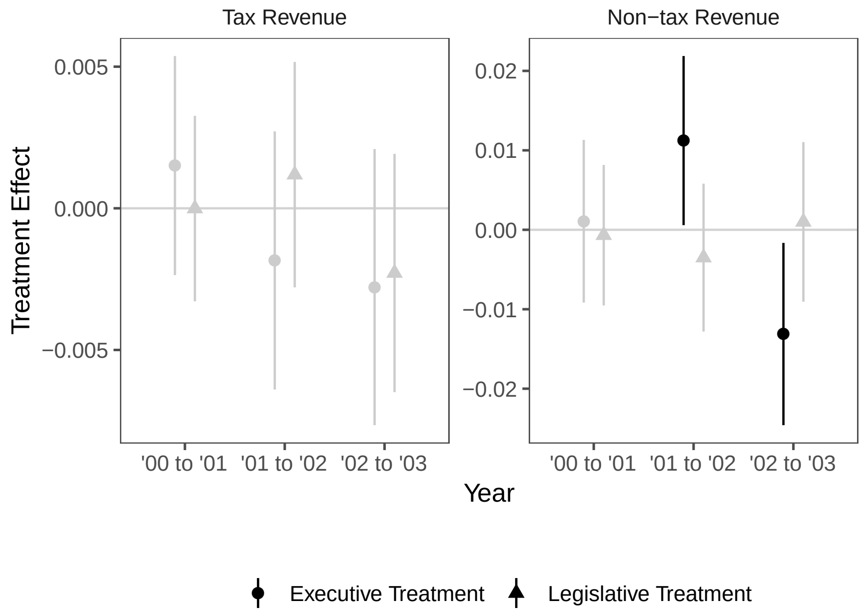


Figure 6: Treatment Effects on Revenue Composition

to 2001 (the leftmost column in each panel). To repeat, because the municipal elections in the treatment group were held in April 2003, the first month of the fiscal year 2003, we expect that political budget manipulations happened *during the fiscal year 2002*, which ends in March 2003. Therefore, the trends prior to the fiscal year 2002 should be free from the influence of the SLE in 2003 and thus similar between the treatment and control groups.

It turns out that all 14 (= 7 outcome variables $\times$ 2 treatment variables) coefficients are not significantly different from zero at the 0.05 level. With these results, we can conclude that the parallel trends hold and that our estimates for the treatment effects for the two subsequent years (from 2001 to 2002, and from 2002 to 2003) are most likely unbiased.

## Treatment Effects

To test our hypotheses, we now focus on the middle and rightmost columns in each panel of Figures 4 through 6. They show the executive or legislative treatment effects on the changes in each outcome variable from 2001 to 2002 (middle column) and from 2002 to 2003 (rightmost column). If political manipulations of the budget took place during the fiscal year 2002, the signs of the coefficients should be opposite between those for the changes from 2001 *to* 2002 and those for the changes *from* 2002 to 2003.

We first look at the executive treatment effects—the left circle and bar in each column—on total expenditure (left panel of Figure 4). We see a clear pattern of the PBC: the treatment effect is positive and statistically significant for the change from 2001 to 2002, and negative and statistically significant for the change from 2002 to 2003. This implies that there was an increase in total expenditure during the fiscal year 2002, one year before the 2003 SLE. The effect sizes are almost the same in the pre-election (0.013) and post-election ($-0.017$) years. After considering the trend in the control group, our estimates suggest that total expenditure in the treated group is 1.3% ($= e^{0.013} \times 100$) or 1.7% ($= e^{0.017} \times 100$) larger in 2002 than in 2001 or 2003, respectively. Therefore, in the context of executive elections, these results support Hypothesis 1.

How do mayors make up for this pre-electoral expenditure surge? The executive treatment effects on total revenue (middle panel of Figure 4) reveal a similar PBC pattern. Regarding the executive treatment effects on the deficit (right panel of Figure 4), the effect on the changes from 2001 to 2002 is positive and statistically significant, but the effect on the changes from 2002 to 2003 is insignificant. In the Online Appendix (Figure 7), we show the alternative estimates obtained from running our model without adding the unbalanced covariates. In this case, the results are in line with the PBC pattern: the deficit increases in 2002 (vs. 2001) and decreases in 2003 (vs. 2002), and the effects are both statistically significant. Here, however, the effect on the change in the total revenue from 2002 to 2003 becomes insignificant. Overall, although municipalities should finance an expenditure surge in the fiscal year 2002 by increasing either the revenue or the deficit in the same year (or both), the evidence provided by our analysis is inconclusive.

We now look at the composition of the expenditure and the composition of the revenue, respectively. Figures 5 and 6 show clear patterns. Specifically, only the executive treatment effects on capital expenditure (left panel of Figure 5) fit the PBC pattern. The effect size is substantial: capital expenditure in the treated group (compared to the control group) is 4.2% ($= e^{0.041} \times 100$) or 6.4% ($= e^{0.062} \times 100$) larger in 2002 than in 2001 or 2003, respectively. The executive treatment effects on current expenditure (right panel of Figure 5) are not significant. These results support Hypothesis 2: the fluctuation in capital expenditure is the main driver of the PBC in total expenditure.

On the revenue side, the executive treatment effects on non-tax revenue (right panel of Figure 6) are in accordance with the PBC pattern; the effect on the changes from 2001 to 2002 is significantly positive, and that from 2002 to 2003 is significantly negative. When we conduct our analysis without controlling for any covariates, however, the effect on the change from 2001 to 2002 becomes insignificant (Online Appendix, Figure 9, right panel). Thus, the PBC pattern of results for non-tax revenue is not robust. By contrast, the executive treatment effects on tax revenue (right panel of Figure 6) are never significantly different from zero in any fiscal year. The lack of a systematic effect of tax revenue is what we would expect given the institutional constraints that Japanese local governments face.

Finally, we examine the legislative treatment effects. As shown by the right triangle and bar in each column, the legislative treatment effects are consistently insignificant on each of our seven outcome variables (see Figures 4 through 6). These results strongly support Hypothesis 3.

# CONCLUSION

In this paper, we tested a series of hypotheses about the PBC using data from Japanese municipalities. Japan provides a particularly suitable case for this inquiry because the treatment variable in empirical analyses of the PBC—the timing of an election—can be assumed to be ignorable. This is a key assumption that many previous empirical studies in other countries do not carefully check or fail to satisfy.

We validated our assumption of ignorability by checking the balance of numerous covariates across the treatment and control groups, and by examining parallel trends for each of our seven outcome variables. We controlled for all of the unbalanced covariates in our regression analyses to further increase confidence in our causal inference. The results of our investigation show that expenditure (in particular, capital expenditure) follows the PBC in association with mayoral elections; total and capital expenditures are 1.2–1.7% and 4.1–7.2% larger in the pre-electoral year than in other years, respectively; the government may finance a pre-electoral (capital) expenditure surge by raising not tax but other forms of revenue or by borrowing more money; and legislative elections are not associated with the PBC. These findings hold even if we do not control any unbalanced covariates (Online Appendix).

Based on these results, we argue that PBCs are conditional on institutional settings and contexts. We call for further comparative institutional analyses, which will deepen our understanding of how institutions produce incentives and how politicians respond to these incentives in order to maximize votes and their chances of staying in power.

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# ONLINE APPENDIX

## Identification Strategy

Formally, to estimate the effects of the executive treatment variable, we use the following model:

$$Y_{it}^{(k)} = \beta_t^{E(k)} X_i^E + \mu_i^{E(k)} + \epsilon_{it}^{E(k)}, \tag{1}$$

where

$$\epsilon_{it}^{E(k)} = \sum_j \gamma^{E(k,j)} Z_{it}^{(j)} + \widetilde{\epsilon^{E(k)}}_{it}. \tag{2}$$

The unit of observation is a municipality $i \in \{1, 2, \ldots, 3010\}$ in the fiscal year $t \in \{2000, 2001, 2002, 2003\}$ for the variables included in the budget category $k$ (e.g., total revenue, capital expenditure). The outcome variable $Y_{it}^{(k)}$ is the natural log of the amount for each variable in the budget category $k$. The executive treatment variable is denoted by a dummy variable $X_i^E$, and its coefficient $\beta_t^{E(k)}$ is the executive treatment effect on each variable in the budget category $k$ in year $t$. The model also includes $\mu_i^{E(k)}$, a municipality-level fixed effect. The error term is denoted by $\epsilon_{it}^{E(k)}$, which contains municipality-specific and time-variant elements, including covariates $Z_{it}^{(j)}$'s multiplied by their coefficients $\gamma^{E(k,j)}$, where $j$ is an index for each covariate.

In the case of the previous year $t-1$, it follows:

$$Y_{i(t-1)}^{(k)} = \beta_{t-1}^{E(k)} X_i^E + \mu_i^{E(k)} + \epsilon_{i(t-1)}^{E(k)}. \tag{3}$$

By subtracting Eq. 3 from Eq. 1, we obtain the following difference-in-differences (hereafter, DID) model:

$$\Delta Y_{it}^{(k)} = \Delta\beta_t^{E(k)} X_i^E + \Delta\epsilon_{it}^{E(k)}, \tag{4}$$

where $\Delta$ is the difference operator, which is defined as $\Delta V_t \equiv V_t - V_{t-1}$ for a generic variable $V_t$, and

$$\Delta\epsilon_{it}^{E(k)} = \sum_j \gamma^{E(k,j)} \Delta Z_{it}^{(j)} + \Delta\widetilde{\epsilon^{E(k)}}_{it}. \tag{5}$$

In the main text, we call $\Delta\beta_t^{E(k)}$ the effect of the executive treatment variable on the changes in an outcome variable. In a similar fashion, we derive the DID model for the legislative treatment variable:

$$\Delta Y_{it}^{(k)} = \Delta\beta_t^{L(k)} X_i^L + \Delta\epsilon_{it}^{L(k)}, \tag{6}$$

where the legislative treatment variable is denoted by $X_i^L$, and $\Delta\beta_t^{L(k)}$ is the effect of the legislative treatment variable on the changes in an outcome variable.

In our specification, municipality fixed effects $\mu_i^{(k)} \in \{\mu_i^{E(k)}, \mu_i^{L(k)}\}$ are cancelled out by design (hereafter, we subsume superscripts $E$ and $L$ unless otherwise noted). As we discuss in the main text, we assume that the determinants of election timing are unlikely to influence municipality-specific and time-variant variables in the early 2000s. Formally, we make the following assumption:

$$\textbf{Assumption 1:} \quad \mathbb{E}(X_i \Delta\epsilon_{it}^{(k)}) = 0,$$

where $\mathbb{E}(\cdot)$ is the expectation operator. This holds when the treatment variable ($X_i$) is independent of the differentiated error term ($\Delta\epsilon_{it}^{(k)}$). Under Assumption 1, we can obtain unbiased estimates of the differentiated treatment coefficient ($\Delta\beta_t^{(k)}$) by applying the ordinary least squares (hereafter, OLS) method to Eqs. 4 or 6. No other variables are necessary as controls.

We check the validity of Assumption 1 empirically in the following two ways. First, if this assumption holds, the differentiated covariates $\Delta Z_{it}^{(j)}$'s should be balanced between the treated group ($X_i = 1$) and the control group ($X_i = 0$). We regress each $\Delta Z_{it}^{(j)}$ on $X_i$ and examine whether its coefficient is zero: namely,

$$\Delta Z_{it}^{(j)} = \zeta_t^{(j)} X_i + \varepsilon_{it}^{(j)}, \tag{7}$$

where $\zeta_t^{(j)} = 0$.

But what if some covariates do not pass this test? If $\zeta_t^{(j)} \neq 0$ for $j = 1, 2, \ldots, J$, running an OLS estimation with Eqs. 4 or 6 would not lead to unbiased estimates. In this case, as a second-best approach, we will substitute Eq. 5 into Eqs. 4 or 6 and regress $\Delta Y_{it}^{(k)}$ on not only $X_i$ but also $J$ unbalanced differentiated covariates $\Delta Z_{it}^{(j)}$'s:

$$\Delta Y_{it}^{(k)} = \Delta\beta_t^{(k)} X_i + \sum_{j=1}^{J} \gamma^{(k,j)} \Delta Z_{it}^{(j)} + \Delta\widetilde{\epsilon^{(k)}}_{it}. \tag{8}$$

With this alternative model, the identification assumption should be also altered. Specifically, we can obtain unbiased estimates of $\Delta\beta_t^{(k)}$ in Eq. 8 by way of OLS as long as the following assumption holds:

$$\textbf{Assumption 1}'\textbf{:} \quad \mathbb{E}(X_i \Delta\widetilde{\epsilon^{(k)}}_{it}) = 0.$$

This is similar to the original Assumption 1, but the differentiated error term is conditional on the covariates included in estimation.

Another approach to check the validity of Assumption 1 is to examine the parallel trends. Let

$$Y_{it}^{(k)}(0) \equiv Y_{it}^{(k)} - \beta_t^{(k)} X_i, \tag{9}$$

which is observed if $X_i = 0$ but is potential (not observed) if $X_i = 1$. Under Assumption 1, we can derive parallel trends of $Y_{it}^{(k)}(0)$ from year $t-1$ to year $t$ between the treated and control groups:

$$\begin{aligned} & \mathbb{E}(\Delta Y_{it}^{(k)}(0)|X_i = 1) - \mathbb{E}(\Delta Y_{it}^{(k)}(0)|X_i = 0) \\ = & \, \mathbb{E}(\Delta Y_{it}^{(k)} - \Delta\beta_t^{(k)} X_i|X_i = 1) - \mathbb{E}(\Delta Y_{it}^{(k)} - \Delta\beta_t^{(k)} X_i|X_i = 0) \quad (\because \text{Eq. } 9) \\ = & \, \mathbb{E}(\Delta\mu_i^{(k)} + \Delta\epsilon_{it}^{(k)}|X_i = 1) - \mathbb{E}(\Delta\mu_i^{(k)} + \Delta\epsilon_{it}^{(k)}|X_i = 0) \quad (\because \text{Eq. } 1) \\ = & \, \mathbb{E}(\Delta\epsilon_{it}^{(k)}|X_i = 1) - \mathbb{E}(\Delta\epsilon_{it}^{(k)}|X_i = 0) \quad (\because \Delta\mu_i^{(k)} = 0) \\ = & \, 0. \quad (\because \text{Assumption 1}) \end{aligned} \tag{10}$$

Since we observe $Y_{it}^{(k)}(0) = Y_{it}^{(k)}$ in the case of $X_i = 0$ but not $Y_{it}^{(k)}(0)$ in the case of $X_i = 1$, as we noted above, we cannot check Eq. 10 empirically. Instead, we compare the trends of

$Y_{it}^{(k)}$ from 2000 to 2001 between the treated and control groups:

$$\begin{aligned}
&\mathbb{E}(\Delta Y_{i,2001}^{(k)}|X_i = 1) - \mathbb{E}(\Delta Y_{i,2001}^{(k)}|X_i = 0) \\
&= \mathbb{E}(\Delta Y_{i,2001}^{(k)}(0) + \Delta\beta_{2001}^{(k)}|X_i = 1) - \mathbb{E}(\Delta Y_{i,2001}^{(k)}(0)|X_i = 0) \quad (\because \text{Eq. } 9) \\
&= \Delta\beta_{2001}^{(k)}. \quad (\because \text{Eq. } 10)
\end{aligned}$$

We focus on the trend from 2000 to 2001 because these two years are well before incumbent politicians would intervene in budgetary processes for the purposes of influencing the results of the SLEs in April 2003. Therefore, we presume:

$$\Delta\beta_{2001}^{(k)} = 0, \tag{11}$$

which means that any change in the amount of expenditure or revenue from 2000 to 2001 is, on average, the same between the treated and control groups. If our OLS estimate of $\Delta\beta_{2001}^{(k)}$ in Eqs. 4 or 6 is not significantly different from zero, we can be more confident that Assumption 1 is satisfied. Similarly, in order to examine validity of Assumption 1′, we only have to test Eq. 11 by using Eq. 8.

## Covariates

The differentiated covariate is defined as follows:

$$\Delta Z_{it}^{(j)} \equiv \Delta Z_i^{(j)} \equiv Z_{ia(j)}^{(j)} - Z_{ib(j)}^{(j)},$$

where $a(j)$ and $b(j)$ are years for which we use the values of covariate $j$. These years differ by covariates ($j$'s) depending on data availability, but we use the same set of years for all outcome years ($t$'s).

If $Z_{i,2000}^{(j)}$ is available in Sōmushō Tōkeikyoku (2001–2004), we take $a(j) = 2000$. In that case, $b(j)$ is the latest available year before 2000. For instance, if covariate $j$ is recorded every year, $b(j) = 1999$. In the case of the census, however, because covariate $j$ is recorded every fifth year, $b(j) = 1995$. In the case of the economic census (*Jigyōsho Kigyō Tōkei Chōsa*), which is conducted one year later than the census, we set $a(j) = 2001, b(j) = 1996$.[23] Otherwise, we take the first and second latest available years before 2000 as $a(j)$ and $b(j)$, respectively.

Although each issue of Sōmushō Tōkeikyoku (2001–2004) reports 100 variables in each year, a few variables come and go over the years. Thus, in sum, four issues of Sōmushō Tōkeikyoku (2001–2004) contain 103 variables. Among them, we do not consider six fiscal variables which are parts or derivatives of the outcome variables (their variable codes in Sōmushō Tōkeikyoku (2001–2004) begin with "D"). There are also two variables which are available only after 2000 and thus $a(j) > b(j) \geq 2000$ (their codes are H5602 and J250502). Table 1 summarizes $a(j)$ and $b(j)$ for the remaining 95 ($= 103 - 6 - 2$) variables. The table also clarifies publication years $p(j)$ and $q(j)$ of the issue of Sōmushō Tōkeikyoku (2001–2004) we refer to for $Z_{ia(j)}^{(j)}$ and $Z_{ib(j)}^{(j)}$, respectively. If more than one issue of Sōmushō Tōkeikyoku (2001–2004) reports $Z_{ia(j)}^{(j)}$ or $Z_{ib(j)}^{(j)}$, we refer to the latest issue.

For covariate A1700 (the number of foreigners) and A1801 (population in Densely Inhabited Districts), because no municipality takes the value of zero, we suspect that zeroes are recorded as missing values and substitute zero with missing values (before calculating the log).[24] We discard six covariates whose values are missing in more than 5% of the 3,010 municipalities (their codes are C5401, C5403, C5405, H6104, K3101, and K4201). For the remaining 89 ($= 95 - 6$) covariates, we add one before we calculate their log as $Z_{it}^{(j)}$.

## Descriptive Statistics

Table 2 shows the descriptive statistics for the two treatment variables ($X_{it}$), and Table 3 summarizes the descriptive statistics for the seven differentiated outcome variables for three years ($\Delta Y_{it}^{(k)}$). Note that, when the value of the deficit is zero, we substitute one with zero

[23] `http://www.stat.go.jp/data/jigyou/2006/gaiyou.htm` (last accessed on December 20, 2017).

[24] For instance, if a (rural) municipality $i$ lacks a Densely Inhabited District, it should hold that $Z_{it}^{(A1801)} = 0$ by definition.

Table 1: Covariates: Codes, Record Years, and Publication Years

| Code | $p(j)$ | $a(j)$ | $q(j)$ | $b(j)$ |
|---|---|---|---|---|
| A1101 | 2004 | 2000 | 2001 | 1995 |
| A1301 | 2004 | 2000 | 2001 | 1995 |
| A1302 | 2004 | 2000 | 2001 | 1995 |
| A1303 | 2004 | 2000 | 2001 | 1995 |
| A1700 | 2004 | 2000 | 2001 | 1995 |
| A1801 | 2004 | 2000 | 2001 | 1995 |
| A4101 | 2003 | 2000 | 2002 | 1999 |
| A4200 | 2003 | 2000 | 2002 | 1999 |
| A5101 | 2002 | 2000 | 2001 | 1998 |
| A5102 | 2002 | 2000 | 2001 | 1998 |
| A6107 | 2004 | 2000 | 2001 | 1995 |
| A7101 | 2004 | 2000 | 2001 | 1995 |
| A710101 | 2004 | 2000 | 2001 | 1995 |
| A810102 | 2004 | 2000 | 2001 | 1995 |
| A810105 | 2004 | 2000 | 2001 | 1995 |
| A811102 | 2004 | 2000 | 2001 | 1995 |
| A8201 | 2004 | 2000 | 2001 | 1995 |
| A8301 | 2004 | 2000 | 2001 | 1995 |
| A9101 | 2003 | 2000 | 2002 | 1999 |
| A9201 | 2003 | 2000 | 2002 | 1999 |
| B1101 | 2002 | 2000 | 2001 | 1998 |
| B1103 | 2002 | 2000 | 2001 | 1998 |
| C120110 | 2004 | 2000 | 2003 | 1999 |
| C120120 | 2004 | 2000 | 2003 | 1999 |

| Code | $p(j)$ | $a(j)$ | $q(j)$ | $b(j)$ |
|---|---|---|---|---|
| C2101 | 2004 | 2001 | 2002 | 1996 |
| C2104 | 2004 | 2001 | 2002 | 1996 |
| C2105 | 2004 | 2001 | 2002 | 1996 |
| C2201 | 2004 | 2001 | 2002 | 1996 |
| C2204 | 2004 | 2001 | 2002 | 1996 |
| C2205 | 2004 | 2001 | 2002 | 1996 |
| C3101 | 2003 | 2000 | 2002 | 1999 |
| C3401 | 2003 | 2000 | 2002 | 1999 |
| C3404 | 2003 | 2000 | 2002 | 1999 |
| C3501 | 2003 | 1998 | 2001 | 1996 |
| C3502 | 2003 | 1999 | 2001 | 1997 |
| C3503 | 2003 | 1999 | 2001 | 1997 |
| C5401 | 2002 | 2000 | 2001 | 1998 |
| C5403 | 2002 | 2000 | 2001 | 1998 |
| C5405 | 2002 | 2000 | 2001 | 1998 |
| E1101 | 2002 | 2000 | 2001 | 1998 |
| E1501 | 2002 | 2000 | 2001 | 1998 |
| E2101 | 2002 | 2000 | 2001 | 1998 |
| E2401 | 2002 | 2000 | 2001 | 1998 |
| E2501 | 2002 | 2000 | 2001 | 1998 |
| E3101 | 2002 | 2000 | 2001 | 1998 |
| E3401 | 2002 | 2000 | 2001 | 1998 |
| E3501 | 2002 | 2000 | 2001 | 1998 |
| E4101 | 2002 | 2000 | 2001 | 1998 |

| Code | $p(j)$ | $a(j)$ | $q(j)$ | $b(j)$ |
|---|---|---|---|---|
| E4501 | 2002 | 2000 | 2001 | 1998 |
| F1101 | 2004 | 2000 | 2001 | 1995 |
| F1102 | 2004 | 2000 | 2001 | 1995 |
| F1107 | 2004 | 2000 | 2001 | 1995 |
| F1108 | 2002 | 2000 | 2001 | 1995 |
| F2201 | 2004 | 2000 | 2001 | 1995 |
| F2211 | 2004 | 2000 | 2001 | 1995 |
| F2221 | 2004 | 2000 | 2001 | 1995 |
| F2401 | 2004 | 2000 | 2001 | 1995 |
| F2402 | 2004 | 2000 | 2001 | 1995 |
| F2403 | 2004 | 2000 | 2001 | 1995 |
| F2404 | 2004 | 2000 | 2001 | 1995 |
| F2405 | 2004 | 2000 | 2001 | 1995 |
| F2701 | 2004 | 2000 | 2001 | 1995 |
| F2705 | 2004 | 2000 | 2001 | 1995 |
| F2801 | 2004 | 2000 | 2001 | 1995 |
| F2803 | 2004 | 2000 | 2001 | 1995 |
| G1201 | 2003 | 1999 | 2001 | 1996 |
| G1401 | 2003 | 1999 | 2001 | 1996 |
| H5501 | 2003 | 2000 | 2001 | 1998 |
| H5601 | 2003 | 2000 | 2001 | 1998 |
| H5603 | 2002 | 2000 | 2001 | 1998 |
| H5604 | 2002 | 2000 | 2001 | 1998 |
| H5614 | 2004 | 2000 | 2003 | 1999 |

| Code | $p(j)$ | $a(j)$ | $q(j)$ | $b(j)$ |
|---|---|---|---|---|
| H6101 | 2004 | 2001 | 2002 | 1996 |
| H6102 | 2004 | 2001 | 2002 | 1996 |
| H6103 | 2004 | 2001 | 2002 | 1996 |
| H6104 | 2004 | 2001 | 2002 | 1996 |
| H7110 | 2002 | 1999 | 2001 | 1998 |
| H7111 | 2002 | 1999 | 2001 | 1998 |
| H7112 | 2002 | 2000 | 2001 | 1998 |
| H7121 | 2002 | 1999 | 2001 | 1998 |
| H7501 | 2002 | 2000 | 2001 | 1998 |
| H9101 | 2003 | 2000 | 2002 | 1999 |
| I510120 | 2003 | 2000 | 2002 | 1999 |
| I5102 | 2003 | 2000 | 2002 | 1999 |
| I5103 | 2003 | 2000 | 2002 | 1999 |
| I6100 | 2004 | 2000 | 2002 | 1998 |
| I6200 | 2004 | 2000 | 2002 | 1998 |
| I6300 | 2004 | 2000 | 2002 | 1998 |
| J2311 | 2003 | 2000 | 2002 | 1999 |
| J2503 | 2003 | 2000 | 2002 | 1999 |
| J2506 | 2003 | 2000 | 2002 | 1999 |
| J4101 | 2002 | 2000 | 2001 | 1998 |
| K2102 | 2003 | 2000 | 2002 | 1999 |
| K3101 | 2002 | 2000 | 2001 | 1998 |
| K4201 | 2002 | 2000 | 2001 | 1998 |

Table 2: Descriptive Statistics of Treatment Variables ($X_{it}$)

| Name | Mean | S.D. | Min. | Max. | N |
|---|---|---|---|---|---|
| Executive Treatment ($X_{it}^E$) | 0.213 | 0.409 | 0 | 1 | 3,010 |
| Legislative Treatment ($X_{it}^L$) | 0.522 | 0.500 | 0 | 1 | 3,010 |

Table 3: Descriptive Statistics of Outcome Variables ($\Delta Y_{it}^{(k)}$)

| Name ($k$) | Year ($t$) | Mean | S.D. | Min. | Max. | N |
|---|---|---|---|---|---|---|
| Total expenditure | 2001 | −0.007 | 0.113 | −0.605 | 0.561 | 3010 |
| | 2002 | −0.022 | 0.111 | −0.616 | 0.487 | 3010 |
| | 2003 | −0.009 | 0.121 | −0.790 | 0.720 | 3010 |
| Capital expenditure | 2001 | −0.051 | 0.404 | −2.085 | 1.484 | 3010 |
| | 2002 | −0.080 | 0.411 | −2.047 | 2.092 | 3010 |
| | 2003 | −0.111 | 0.465 | −2.226 | 1.663 | 3010 |
| Current expenditure | 2001 | 0.008 | 0.067 | −0.709 | 0.591 | 3010 |
| | 2002 | −0.006 | 0.065 | −0.661 | 0.639 | 3010 |
| | 2003 | 0.012 | 0.065 | −0.604 | 0.479 | 3010 |
| Total revenue | 2001 | −0.022 | 0.084 | −0.633 | 0.516 | 3010 |
| | 2002 | −0.041 | 0.084 | −0.649 | 0.514 | 3010 |
| | 2003 | −0.032 | 0.092 | −0.613 | 0.537 | 3010 |
| Tax revenue | 2001 | 0.000 | 0.048 | −0.447 | 0.870 | 3010 |
| | 2002 | −0.014 | 0.046 | −0.360 | 0.445 | 3010 |
| | 2003 | −0.041 | 0.051 | −0.544 | 0.597 | 3010 |
| Non-tax revenue | 2001 | −0.029 | 0.107 | −0.723 | 0.619 | 3010 |
| | 2002 | −0.049 | 0.109 | −0.929 | 0.595 | 3010 |
| | 2003 | −0.030 | 0.118 | −0.821 | 0.743 | 3010 |
| Deficit | 2001 | 0.181 | 0.789 | −13.228 | 12.892 | 3010 |
| | 2002 | 0.155 | 0.686 | −7.675 | 14.890 | 3010 |
| | 2003 | 0.179 | 0.498 | −9.040 | 2.097 | 3010 |

before calculating the log ($Y_{it}^{(k)} = \log(0+1) = 0$).

There are 18 unbalanced, differentiated covariates which we control in Eq. 8 for either the executive or legislative treatment. Their codes and names are as follows. See Sōmushō Tōkeikyoku (2001–2004) for more details.

**A1301:** the population of those 15 years old and younger

**A1303:** the population of those 65 years old and older

**A1700:** the number of foreigners

**A5101:** the number of immigrants

**B1103:** the livable area (km$^2$)

**C2104:** the number of offices in the secondary industry

**C3502:** the number of commercial shops

**C3503:** the number of commercial employees

**E3401:** the number of junior high school teachers

**F1107:** the number of the completely unemployed

**F2201:** the number of workers in the primary industry

**F2403:** the number of entrepreneurs with employees

**F2404:** the number of entrepreneurs without employees

**F2701:** the number of workers who work where they live

**F2803:** the number of commuters from outside municipalities

**H5601:** the population whose waste the municipality disposes of

**H5603:** the amount of garbage collected (tons)

**J4101:** the number of individuals insured with the national health insurance

The executive treatment is unbalanced with regard to six covariates: C3502, C3503, E3401, F1107, F2403, and J4101. The legislative treatment is unbalanced with regard to 14 covariates, which are all of the above except C3502, C3503, E3401, and F2403 ($18 - 4 = 14$). Table 4 displays descriptive statistics of these 18 unbalanced, differentiated covariates ($\Delta Z_i^{(j)}$).

Table 4: Descriptive Statistics of Unbalanced Covariates ($\Delta Z_{it}^{(j)}$)

| Code | Mean | S.D. | Min. | Max. | N |
|---|---|---|---|---|---|
| A1301 | −0.138 | 0.149 | −6.447 | 0.982 | 3010 |
| A1303 | 0.137 | 0.142 | −6.824 | 0.441 | 3010 |
| A1700 | 0.308 | 0.601 | −3.466 | 3.296 | 3010 |
| A5101 | −0.028 | 0.175 | −1.151 | 1.242 | 3010 |
| B1103 | 0.009 | 0.056 | −0.523 | 0.528 | 2991 |
| C2104 | −0.092 | 0.133 | −3.912 | 0.644 | 3009 |
| C3502 | 0.003 | 0.083 | −0.405 | 1.26 | 3010 |
| C3503 | 0.037 | 0.160 | −0.916 | 1.391 | 3010 |
| E3401 | −0.029 | 0.089 | −0.871 | 0.496 | 3010 |
| F1107 | 0.137 | 0.277 | −3.638 | 1.447 | 3008 |
| F2201 | −0.217 | 0.192 | −5.724 | 1.172 | 3009 |
| F2403 | 0.045 | 0.244 | −4.466 | 1.522 | 3009 |
| F2404 | −0.164 | 0.162 | −5.961 | 0.547 | 3010 |
| F2701 | −0.097 | 0.158 | −7.621 | 0.701 | 3010 |
| F2803 | 0.105 | 0.156 | −3.296 | 1.314 | 3010 |
| H5601 | −0.008 | 0.163 | −8.25 | 1.494 | 2966 |
| H5603 | 0.058 | 0.185 | −1.319 | 1.520 | 2880 |
| J4101 | −0.001 | 0.126 | −0.736 | 0.963 | 2967 |

## Alternative Analysis of Treatment Effects

As a robustness check, we report the OLS estimates of $\Delta\beta_t^{(k)}$'s by using Eqs. 4 and 6, where we do not control for any covariates. The estimates of $\Delta\beta_t^{(k)}$'s are unbiased as long as Assumption 1 is satisfied. In this alternative specification, because we have no missing values in $X_{it}$ and $\Delta Y_{it}^{(k)}$, we use all municipalities (i.e., $N = 3,010$). Below, we call attention to the differences from the results using the covariates, which we report in the main text.

Figure 7, compared to Figure 4, shows that, when we do not use any covariates, the estimate of $\Delta\beta_{2002}^{\mathrm{E(rev.total)}}$ ceases to be significant, but the estimate of $\Delta\beta_{2003}^{\mathrm{E(deficit)}}$ becomes significant. Thus, we do not have conclusive evidence on whether total revenue and deficit

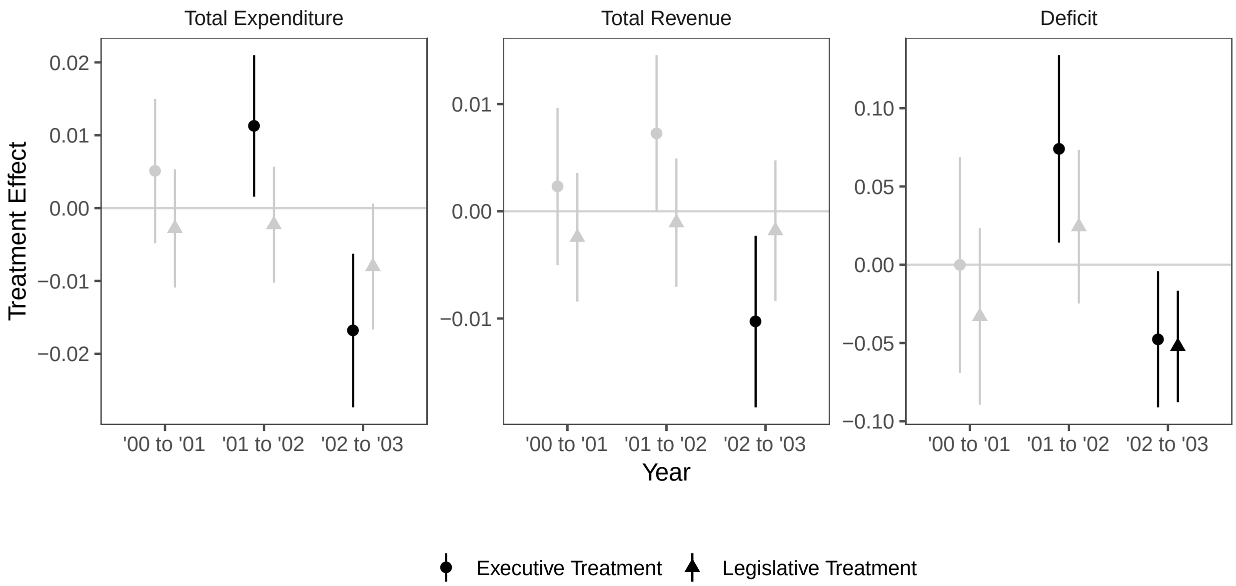


Figure 7: Treatment Effects on the Total Budget

are in line with the PBC pattern and finance the pre-electoral surge observed in 2002. Figure 8, which corresponds to Figure 5, demonstrates that the estimate of $\Delta\beta_{2003}^{\mathrm{L(exp.capital)}}$ becomes significantly negative, but the estimate of $\Delta\beta_{2002}^{\mathrm{L(exp.capital)}}$ is still insignificant and negative. Thus, we are not yet confident in the legislative treatment effects on capital expenditures. Figure 9 delivers essentially the same substantive implications as Figure 6. In sum, our evidence is robust to how we deal with the (unbalanced) covariates.

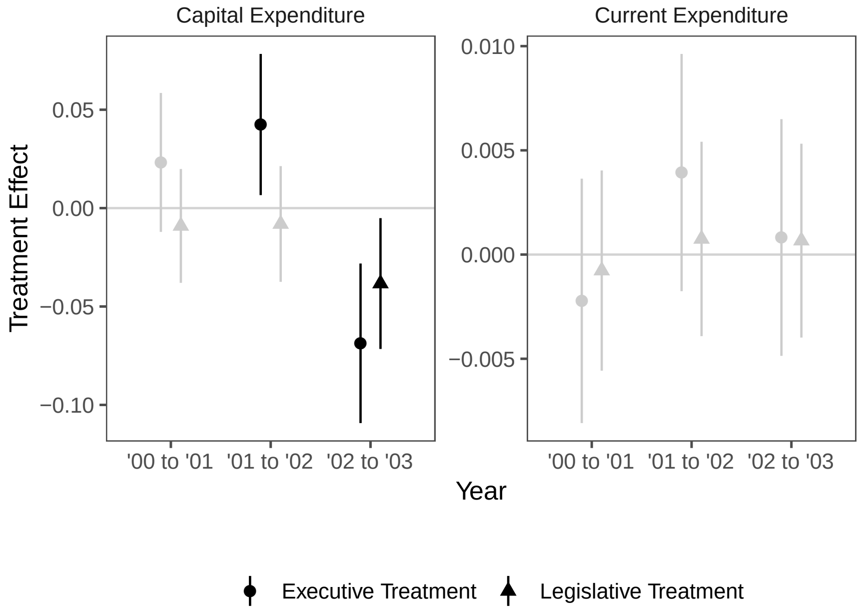


Figure 8: Treatment Effects on the Expenditure Composition

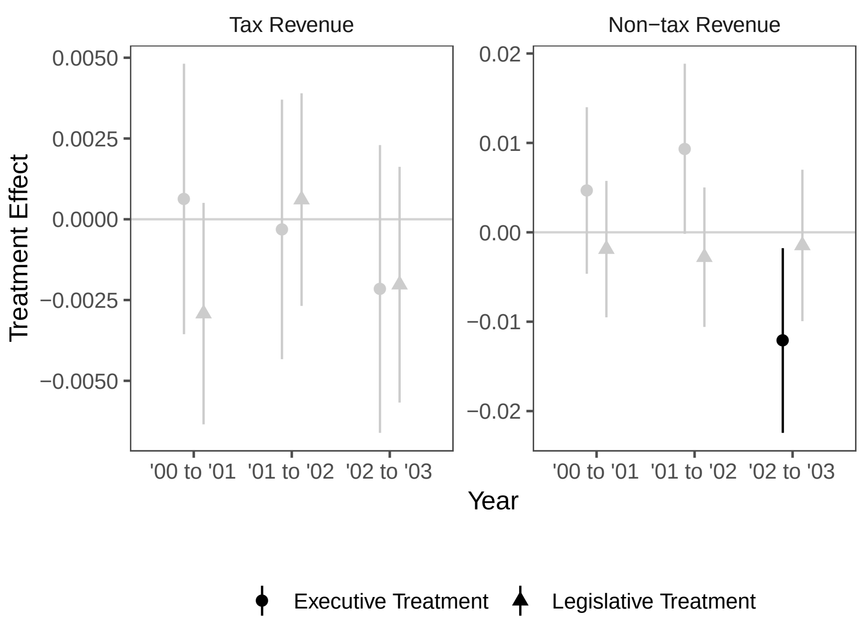


Figure 9: Treatment Effects on the Revenue Composition