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**TITLE      Making Outsiders' Votes Count: Detecting Electoral  
Fraud through a Natural Experiment**

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# **Making Outsiders' Votes Count:**

## **Detecting Electoral Fraud through a Natural Experiment\***

Kentaro Fukumoto\*\* and Yusaku Horiuchi\*\*\*

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## **Abstract**

Weak electoral registration requirements are commonly thought to encourage electoral participation, but may also promote electoral fraud. For one, candidates and their supporters can more easily mobilize voters outside the district to register and vote for the candidates, even though these voters do not reside within the district. We statistically detect this classic type of electoral fraud for the first time, by taking advantage of a natural experimental setting in Japanese municipal elections. We argue that whether or not a municipal election was held in April 2003 can be regarded as an “as-if” randomly assigned treatment. The difference-in-difference analysis of municipality-month panel data shows that an increase in the new population just prior to April 2003 is significantly larger in treatment municipalities (with an election) than in control ones (without an election). The estimated effects are, in some cases, decisive enough to change the electoral results, especially when the election is competitive. We argue that our approach – “election timing as treatment” – can be applied to investigate not only this type of electoral fraud but also other “electoral connection[s]” (Mayhew 1974) in other countries.

## INTRODUCTION

Despite its fundamental relevance to the functioning of democracy, surprisingly little is known about optimizing requirements for voter registration in order to facilitate genuine electoral participation while preventing electoral fraud. On the one hand, stricter rules discourage people from registering to vote (e.g., Campbell et al. 1960, 276-86; Powell 1986; Rosenstone and Hansen 1993, 196-209; Wolfinger and Rosenstone 1980, ch. 4). On the other hand, weak registration requirements promote electoral fraud and allow even non-eligible voters (e.g., non-resident voters) to register to vote illegally. In the first place, voter registration was introduced mainly to address this concern (Campbell 2005; Keyssar 2000). This tradeoff has long been a target of not only academic but also partisan debates in the U.S. (Campbell 2005, 284; Minnite 2010; Piven and Cloward 2000).

It is difficult, however, to “devise registration systems that would control fraud without shrinking the size of the active electorate” (Converse 1972, 298). This is principally because it is hard to estimate, much less observe, fraudulent behavior, even when we have detailed turnout statistics. For obvious reasons, perpetrators of the fraudulent behavior attempt – and often manage – to conceal their cheating. Certainly, the existing literature of electoral fraud makes much use of qualitative sources. They include cases citizens have brought to the judiciary (Minnite 2010), election disputes parties filed in the legislature (in order to nullify electoral results) (Lehoucq and Molina 2002; Oberst and Weilage 1990; Ziblatt 2009), international election observation reports (Birch 2007), mass media reports (Cox and Kousser 1981; Nyblade and Reed 2008), and archival materials (Campbell 2005). These anecdotal sources are often insightful, but not immune to underreporting the scale of electoral fraud on the whole due to the covert nature of fraud, or overreporting due to the partisanship of those who produce the original

materials (Minnite 2010). In short, as Alvarez, Hall and Hyde (2008, 3) lament, to date there has been “little systematic research on how election fraud can be detected and deterred.”

This article addresses this lacuna, by examining fraudulent electoral registration and detecting it statistically using the case of Japan. In this country, citizens who change their residential address must register it at a municipality office within two weeks. Three months later, voting-age adults are *automatically* registered to vote in the municipality without an additional voter registration process. Some voters change their registered addresses before the election *for the purpose of voting*, and we call this behavior “pre-electoral residential registration.” In many cases, candidates and their supporters allegedly ask voters outside the district to change their registered address to the district and to vote for the candidates. Given an automatic electoral registration process and relatively simple requirements for residential registration, which we explain further later, most of these “relocaters” allegedly transfer their registered address *on paper without changing their actual residence*, in which case their behavior is illegal.<sup>1</sup> Since this tactic is inherently covert, it is difficult to know how commonly people are engaged in it and whether it is an important factor shaping election outcomes.

We make statistical inference about this electoral behavior by taking advantage of a natural experimental setting. In a nutshell, we argue, whether or not a municipal election was held in April 2003 can be regarded as an “as-if” randomly assigned treatment for reasons explained shortly. If this assumption holds, we can reasonably attribute a difference between the increase in “relocated” residential registration in treatment municipalities (*with* an election) and the increase in control municipalities (*without* an election) only to the effect of having an election on pre-electoral residential registration. The effects estimated by difference-in-difference models

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<sup>1</sup> Article 236, Sections 1 and 2, Public Officers Election Act (POEA).

are significant and, in some cases, decisive enough to change the electoral results, especially when the election is competitive.

This study intends to make a substantive and methodological contribution to the broader literature. First, we statistically detect pre-electoral residential registration, which is a variant of a classic kind of electoral fraud, “voting from nonresidential addresses” (Minnite 2010, 49, 53, see also Campbell 2005, 6) by “ghost voters” (Campbell 2005, 195; Hyde 2008, 206, 214) or “phantom voters” (Campbell 2005, 124, 195, 283; Sadiq 2009, 142). As we explore in the next section, the practice was and is common across African, American, Asian, and European countries. No one, however, has ever estimated how many people are engaged in it. To our knowledge, this article provides robust inference of such fraudulent behavior for the first time. This is an essential step in an inquiry into the optimal requirements for electoral registration.

Second, we add “electoral timing as treatment” to the toolkit of political science by demonstrating its usefulness. When an election is approaching, parties and candidates are more inclined to do everything they can do, either legally or illegally. One problem with examining such “electoral connection[s]” (Mayhew 1974) is that we cannot manipulate the occurrence of elections. If we find a variation in the election timing across geographical units, however, it is possible to estimate the treatment effects of an election *per se* on a range of outcome variables, by comparing treatment units (with an election) and control units (without an election). In the concluding section, we will present the contexts, other than Japanese municipality elections, in which electoral timing as a treatment variable can be exploited.

The organization of the paper is as follows. The next section summarizes newspaper reports on pre-electoral residential registration in Japan, and argues that the highly competitive nature of Japanese local elections motivates candidates to engage in it. We also explore similar

examples of voting from nonresidential addresses in many other countries. After reviewing statistical methods used in existing studies to detect electoral fraud, the third section elaborates on our natural experimental design and derives two hypotheses. The following section specifies difference-in-difference models, describes data, and clarifies our predictions. After showing and discussing the estimated results in the penultimate section, we conclude by summarizing our findings and discussing alternative setups where “election timing as treatment” might be applied.

## **PRE-ELECTORAL RESIDENTIAL REGISTRATION**

To repeat, we define pre-electoral residential registration as the change of registered address from one municipality to another before the election for the purpose of voting. In this section, we begin by summarizing patterns of this behavior based on reports found in newspapers. Second, we highlight an important institutional factor underlying such behavior; that is, the extremely competitive nature of municipal elections. Finally, we call attention to the fact that pre-electoral residential registration is by no means specific to Japan but commonly found elsewhere.

### **Patterns of Pre-Electoral Residential Registration**

In Japan, pre-electoral residential registration has been periodically mentioned by legislators in the legislature, the Diet,<sup>2</sup> as well as investigated as a form of electoral fraud.<sup>3</sup> Most investigated

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<sup>2</sup> Examples are found in the Research Special Committee on POEA, the House of Representatives, on April 8, 1964; in the Special Committee on POEA, the House of Councilors, on November 14, 1968; and in the Special Committee on Religious Corporation, the House of Councilors, on November 7, 1995. A government council also referred to it (*Dai 4 Ji Senkyo Seido Shingikai, Senkyo no Tetsuzuki ni Kansuru Shôininkai Inchô Chûkan Hôkoku Yôshi* [Chairman’s Interim Report Outline of Subcommittee on Electoral Procedures, Fourth Electoral

cases are concerned with sub-national (in particular, municipality-level) elections of both mayors and assembly members. For example, in 2003, police sent a total of 99 cases (136 people) to prosecutors – 51 cases (66 people) in municipal elections, 28 cases (43 people) in prefectural elections, and 20 cases (27 people) in national elections – on suspicion of illegal residential registration and its related crimes.<sup>4</sup>

Searching one of the major newspapers, *Asahi Shimbun*, between 1985 and 2009,<sup>5</sup> for the two key expressions, “*kakû ten’nyû*” (false moving in) and “*sagi tôroku*” (false registration), we identified 49 reported cases of illegal pre-electoral residential registration.<sup>6</sup> These cases range from Hokkaido Prefecture in the far northeast of Japan to Okinawa Prefecture in the far southwest, from rural villages to urban areas in Tokyo, and from socialists to conservatives. Below, we summarize these reports and explain what is known about illegal pre-electoral residential registration – who, what, where, when, why and how.

Who mobilizes voters in other municipalities and who actually changes their residential registration when asked to do so? For candidates and core supporters, first contacts are, not

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System Council], February 15, 1966).

<sup>3</sup> The often-cited leading case occurred in 1926 (Supreme Court Judgment, July 4, 1928).

<sup>4</sup> Jichishô Senkyo Kyoku, *Chihô Senkyo Kekka Shirabe* [Survey of the Local Election Results], 2003, p. 418, Keisatsuchô, *Hanzai Tôkeisho* [Criminal Statistics], 2003, Table 59. The related crimes are impersonation in voting (Article 236, Section 3, POEA), ballot stuffing, destruction of ballots and fake ballots (Article 237) and misuse of proxy voting (e.g. on behalf of the disabled, Article 237-2).

<sup>5</sup> The full text online search is only available for articles dated after 1985.

<sup>6</sup> Several key cases are individually cited. See Appendix for the list of these reports.

surprisingly, families (in particular, children) and relatives. In many cases, these people used to be actual residents but left the town or village for education and employment. Since they come back from time to time, for example on New Year's Day, they are most prone to be mobilized. Candidates' friends or even mere acquaintances are also likely to be targeted. Furthermore, employees, partners and clients of companies which candidates (or core supporters) control, are vulnerable to candidates' pressure to change their residential registration. In not a few cases, even gangsters are ordered to do so by their gang boss.

To which address do they change their registration? Typically used addresses (only for the purpose of registration) include candidates' (or their core supporters') homes, the electoral campaign headquarters, and offices or housing provided for employees of companies controlled by candidates (or their core supporters). It is common that candidates select a specific address for the purpose of pre-electoral residential registration, with more than a dozen people with different family names *appearing to* have moved from different places to live in the same house. In an extreme case, 202 people registered their residence as a small house of only about 240 square meters.<sup>7</sup>

How is this type of electoral fraud feasible? It is the simplicity of the registration process: Japanese people can easily change their registered address by submitting a simple form without presenting evidence of *actual* relocation (e.g., water and electricity bills). Moreover, anyone can be a proxy for people "registering" before an election, and can do paperwork on their behalf. In one case, an agent changed the addresses of 128 people in a day.<sup>8</sup> It is important to note that candidates can monitor whether those who are asked to engage in pre-electoral residential

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<sup>7</sup> *Asahi Shimbun*, May 9, 1990.

<sup>8</sup> *Asahi Shimbun*, April 20, 1987.

registration did, indeed, do so because a list of eligible voters for a specific election is publicly available.

How do those “relocaters” then vote? Do they actually cast their own ballots? Based on the residence registry, the electoral commission office of a corresponding municipality mails the correct number of voting tickets (i.e., tickets to the polling stations) for all voters at a particular address. On behalf of allegedly “new” voters, actual residents may pass the tickets to other supporters in the municipality who may illegally cast more than one ballot by impersonating.<sup>9</sup> Alternatively, those who are mobilized but still live in other municipalities may travel to the district, receive tickets from someone actually residing in the “moved-in” address, and utilize early voting or vote on Election Day.<sup>10</sup>

When does pre-electoral residential registration commonly take place? In many cases, “relocaters” change their address just before the deadline of eligibility, three months before the election. (We will explain the timing of pre-electoral residential registration more in detail in a later section.) Usually, after the election, they move their residential registration back to where they actually live.<sup>11</sup>

Politicians and government officials might argue that these cases are exceptions, but numerous newspaper reports imply that illegal pre-electoral residential registration is common in some communities and, therefore, an open secret: residents have no sense of guilt in the first place and public officials, who process paperwork and send multiple (even numerous) voting tickets to a single address, pretend not to know about (or, rather, are sometimes complicit in)

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<sup>9</sup> For example, *Asahi Shimbun*, December 7, 1991 and April 27, 1998.

<sup>10</sup> For example, *Asahi Shimbun*, July 9, 1985, April 30 and May 15, 1987 and July 17, 1994.

<sup>11</sup> For example, *Asahi Shimbun*, July 17, 1994 and October 14, 1995.

illegal residential registration. An intriguing and extreme example is a case in the town of Torahime, Shiga Prefecture. In August 1981, four months before the assembly election, as many as 412 people – 10% of its population or 20 times as many as the average number of monthly new comers – brought their addresses to this small town. After being remanded by the Supreme Court, the Osaka High Court declared this election void in April 1985 and criticized the election committee of the town for not suspecting illegal pre-electoral residential registration and not investigating the veracity of residential registration.<sup>12</sup>

### **Close Competition in Municipal Elections**

There remains an important question: why do candidates, particularly those in municipal elections, promote this type of electoral fraud? The literature points out that the higher the level of political competitiveness, the more likely it is that politicians commit acts of electoral fraud. For example, Birch (2007) writes, “[c]andidates in marginal positions are most likely to benefit from electoral malpractice, for their expected gain is highest in relation to their expected loss in terms of potential damage to reputation” (pp. 1538-39).<sup>13</sup> Since a small number of votes may change the outcome of an election, local elections, like close national elections, are an all too common locus of electoral fraud (Alvarez, Hall and Hyde 2008, 12). Some studies suggest that the degree of intra-party competition also affects the likelihood of candidates engaging in fraudulent activities. Where candidates from the same party compete with one another, they seek

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<sup>12</sup> Supreme Court Judgment, January 22, 1985, Osaka High Court Judgment, April 19, 1985. Other extreme cases reported in newspapers are the village of Ieshima, Hyogo Prefecture, in 1994 (where around 600 citizens claimed to move) and the village of Kuriyama, Tochigi Prefecture, in 1995 (*Asahi Shimbun*, July 13 and 17, 1994; September 27 to 29, 1995).

<sup>13</sup> Nyblade and Reed (2008, 928) and Lehoucq and Molina (2002) also present similar views.

to acquire “personal votes” distinguishing them from others running for office with the same party label and, therefore, have incentives to be engaged in electoral fraud (Carey and Shugart 1995; Chang and Golden 2006). “Intraparty competition is less likely to involve competition over issues and government responsibility and more likely to involve pork-barrel politics and expensive personalistic appeals, increasing demand for illegal campaign activities” (Nyblade and Reed 2008, 929).

Japanese municipality elections satisfy both of these common conditions for electoral fraud; they are highly competitive and electoral competition is not only between but also within parties. This is a product of the Japanese electoral system; namely, in assembly elections, each municipality in principle constitutes an at-large electoral district (i.e., one district for one municipality) and the number of seats ranges from 12 to 96, depending on the population size. Every voter has a single vote; that is, he/she votes for only one candidate. Winners are decided simply based on the total number of votes each candidate garners, and redundant or futile votes are not transferred to other fellow candidates (e.g., candidates from the same party).<sup>14</sup> Under this single non-transferable vote system, the vote margins between candidates tend to be extraordinarily small and dozens of candidates run from the same party or camp.<sup>15</sup>

Figure 1 illustrates an example of the distribution of votes in an assembly election in a

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<sup>14</sup> The mayoral election system is essentially the same, except that the number of seats per district is one.

<sup>15</sup> In Japanese municipality elections, many candidates run as independents, because party labels do not necessarily help them to differentiate themselves from other competitors. In reality, however, they are often from the same camp (e.g. conservatives) and battle to win support within the same constituency.

municipality with the median population size (about ten thousand), the city of Taneichi in Iwate Prefecture.

[Figure 1 around here]

Twenty-two candidates competed for 18 seats in the election on April 27, 2003. The last winner (the 18th candidate) obtained 351 votes, while the runner up (the 19th candidate) got 338 votes. The vote margin is only 13, or 0.01% of the total number of eligible voters. Even the fourth last winner (the 15th candidate) maintained as small a margin as 35 votes compared to the first runner up.

According to a more extensive study on municipal assembly elections held between 1987 and 1998 in two prefectures (Saga and Fukuoka), the vote margin between the last winner and the runner up is less than 50 votes in two-thirds of these municipalities and less than ten, including zero, in about 20 percent of them (Horiuchi 2005, Table 5.4, 86).<sup>16</sup> Political scientists and economists have long assumed that “[t]he probability of being decisive... in an election with more than a handful of voters is always small, usually very small, and sometimes infinitesimally small” (Fisher 1999, 267; also see Riker and Ordeshook 1968). The literature of electoral fraud also has not previously found any evidence that fraud changes electoral results (Lehoucq 2003), although competitiveness is the main driver for it. In Japanese municipal elections, however, even a small increase in the number of votes can affect who gets elected. Not surprisingly, such extraordinarily small vote margins give a strong incentive for candidates and their core supporters to mobilize voters, even from outside the district.

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<sup>16</sup> If the last winner and the runner up receive the exactly same number of votes, a winner is decided by a random draw. Horiuchi (2005, 84) found three cases in his sample of 442 districts.

## **Voting from Non-Residential Addresses**

Before shifting from qualitative to quantitative analysis to obtain systematic evidence of pre-electoral residential registration, we want to emphasize that pre-electoral residential registration is a variant of a classic electoral fraud – voting from nonresidential addresses, which has been, and still is, found in many countries. Campbell (2005) reports abundant examples in the U.S. In antebellum America, electoral machines notoriously and illegally imported voters, such as “floaters’ [who] voted several times, usually going from one precinct to another” (p. 19) and “colonizers’... residing in another city or state” (p. 19).<sup>17</sup> After the introduction of a voter registration system, “hundreds’ of names could not be found residing at the listed addresses, which were often abandoned buildings and vacant tenements” (pp. 171-72). “Ward and precinct heelers collect[ed] persons from rooming houses and boarding houses” and gave them (fictitious) names, (fictitious) addresses and a few dollars (pp. 173-74). The following example in 1936 is particularly similar to our story:

[A] resourceful ward heeler would have his relatives residing in neighboring counties or states come to St. Louis on registration day, swear that they were residents of the city, and register to vote, providing the address of the ward heeler as their place of residence.... On Election Day, then, those who had registered under the process could go to the polls, or repeaters hired for this purpose could then vote as the individual registered illegally (p. 174).

There are many other stories outside the U.S. In Britain, the residency requirement for electors was not enforced from the 15th to 18th centuries even before it was formally repealed in 1773 (Hutcheson 1997). In the first half of the twentieth century, Costa Rican parties allegedly “packed the electoral registry with dead or nonexistent individuals” or those who “lacked a

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<sup>17</sup> Keyssar (2000, 159) also describes floaters.

permanent residence” (Lehoucq and Molina 2002, 40, 99). They also transported railroad employees to the districts the employees were not registered and forced them to vote (Lehoucq and Molina 2002, 185-88). In postwar Malaysia, the governing party, UMNO, legalized illegal immigrants from the Philippines and Indonesia by way of a document fraud, so that they could vote for UMNO (Sadiq 2005, 2009). Similarly, some illegal immigrants from Afghanistan were registered to vote in Pakistan, with similar movements from Bangladesh to India, from Nigeria to Cameroon, from Togo to Ghana and from Burkina Faso to the Ivory Coast (Sadiq 2005, 2009). Pre-electoral residential registration is reportedly found in Korea as well.<sup>18</sup> In Solomon Islands, many residents were allegedly imported from island to island to be registered and vote.<sup>19</sup>

## **NATURAL EXPERIMENTAL DESIGN AND HYPOTHESES**

The previous section suggest that pre-electoral residential registration or voting from nonresidential addresses may be rampant, while we suspect that the crime statistics under-represent reality. Then, how do we know how many people actually are engaged in it? To answer this, we need to cross an important hurdle – the difficulty of identifying whether or not changing one’s address (regardless of actual residence) is motivated by electoral fraud or not. Thanks to a natural experimental setup in Japan, however, we can cope with this problem and make valid causal inference.

In this section, we first review some existing statistical studies to detect electoral fraud and argue the advantage of natural experimental design. We then explain why we assume that our treatment variable is an as-if random assignment. The third part derives a hypothesis about the

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<sup>18</sup> *Asahi Shimbun*, April 9, 1988.

<sup>19</sup> *Solomon Star*, August 9, 2010.

timing of pre-electoral residential registration. Finally, we propose another hypothesis about how competitiveness affects its magnitude.

### **Existing Approaches to Detect Electoral Fraud**

In the literature detecting electoral fraud, scholars have traditionally relied on qualitative sources, as we mentioned in the introduction. These provide useful indications, but are presumably the tip of the iceberg and are insufficient for us to draw a comprehensive picture.

A handful of recent studies try to detect only partially observable fraudulent activities using various methods of statistical inference. One is a model-based approach, where scholars estimate some statistical (e.g., regression) models and examine irregularities, anomalies or outliers (Myagkov, Ordeshook and Shakin 2009; Wand et al. 2001). For example, Alvarez and Katz (2008) fit a regression model with two independent variables to explain vote share and, then, predict vote share of the next election (where suspicious voting machines were in use) by using the parameter estimates obtained from the analysis of the previous election. If most of the observed vote shares are outside the 95% confidence intervals of the predicted vote shares, one suspects that electoral fraud could have occurred. These model-based studies have succeeded in reasonably calling attention to doubtful cases which require further investigation. Once readers consider, however, the existence of relevant omitted variables,<sup>20</sup> the mis-specified functional

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<sup>20</sup> In the case of Alvarez and Katz (2008), to be more precise, they regress the county-level Democratic vote shares for gubernatorial and senatorial candidates of Georgia in 1998 on the vote share for president in 1996 and the percentage of non-white population. Then, using the coefficient estimates and the same independent variables (but for 2000, rather than 1996), they predict the dependent variable in the 2002 elections. They admit that they “lack many of the sorts of variables that political scientists might use in such a forecasting model” (p. 155). One such

form, or the wrong distributive assumption of an error term, the validity of any model-dependent analysis can be called into question. This approach also “ignores the fraud that can occur *regularly*” (Campbell 2005, 26, emphasis added).

An alternative is a design-based approach, in particular, a natural experiment, which we consider a more convincing method offering valid and robust causal estimates.<sup>21</sup> For example, Hyde (2007) focuses on the 2003 Armenian presidential election and compares the incumbent’s vote shares at polling stations which international election observers visited (the treatment group) vis-à-vis those at the other polling stations they did not visit (the control group). If we assume, as Hyde does, that the observers are randomly assigned to polling stations, there should be no systematic difference between the two groups except for the presence or absence of observers (i.e., treatment). Therefore, a reduction in the average vote share of the treatment group compared with the control group can be safely regarded as the effect of monitoring on voting behavior; namely, that international observers reduce electoral fraud.

It is crucial for a natural experiment, however, that the assignment of a treatment variable is as-if random (Dunning 2008). For instance, in search of a reason for the unnatural surge of votes for Buchanan in Palm Beach County, Florida during the 2000 U.S. Presidential election, Wand et al. (2001, 799) compare election day voting (which uses the notorious butterfly

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variable is an economic variable, e.g., personal income, which is correlated with not only gubernatorial and senatorial but also presidential candidates’ vote shares. Therefore, the omission of personal income might produce biased coefficient estimates, and in turn lead to biased prediction.

<sup>21</sup> Another non-model based method is to apply Benford’s Law (Mebane 2008) or to pay attention to frequency of ultra-close races (Christensen and Colvin 2009).

ballot) with absentee voting (which does not) as a natural experiment, though this treatment variable is “not random assignment,” as the authors themselves admit.<sup>22</sup> In our case, however, treatment is an as-if random assignment, to which we turn next.

### **Treatment: Simultaneous Local Elections**

In Japan, as of April 27, 2003, there are 3,210 municipalities (i.e., cities, special wards, towns, and villages) in 47 prefectures and each municipality elects both a unicameral legislative assembly and a popularly elected mayor. Just before the current Constitution came into force, all municipalities held their elections of assembly members and mayors simultaneously in April 1947. Since their terms are four years, subsequent elections were scheduled, in principle, every fourth year (i.e., 1951, 1955, 1959, ... , 2003, 2007, 2011) on the fourth Sunday in April.

Today, however, a significant proportion of municipalities do not hold their elections during these simultaneous local elections (hereafter SLE). Once an election is held off the SLE cycle for whatever reason, the following elections usually remain off the SLE cycle, because the length of the subsequent term is always four years, not the remainder of the previous term. In our analysis, we focus on timing and reasons for the last deviation from the cycle for each control municipality.

Specifically, in the case of the 2003 SLEs, among 3,105 municipalities we study, 51.6% of municipalities held assembly and 21.4% mayoral elections on April 27, 2003.<sup>23</sup> We call

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<sup>22</sup> Since this key assumption is difficult to satisfy, a natural experiment has been rarely used in the statistical studies of electoral fraud. In a comprehensive review, Lehoucq (2003, 235-36) does not even mention natural experiments as an effective tool to study electoral fraud.

<sup>23</sup> A special law, which is made every four years, stipulates that when mayoral and assembly terms expire between March 1 and May 31, in principle, the elections should be held on the

whether or not each municipal assembly or mayoral election was held on April 27, 2003 the “legislative treatment” or the “executive treatment”, respectively. This cross-municipality variation provides a unique opportunity to study the effects of a municipal election.

Figure 2 displays the cumulative percentages of municipalities in the “legislative control” group, which did not hold an assembly election on April 27, 2003, or the “executive control” group, which did not hold a mayoral election on the same day, by years and reasons for deviation from SLEs.

[Figure 2 around here]

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fourth Sunday of April. While 3,210 municipalities existed as of April 27, 2003, we exclude the following ones from analysis. First, we drop all municipalities in Okinawa Prefecture, 15 municipalities in Kagoshima Prefecture, and the village of Ogasawara in Tokyo Metropolitan Area, because they did not hold the first SLEs in 1947 under the U.S. occupation. Second, we omit the village of Ogata in Akita Prefecture, which was established by reclaim in 1964 and thus was off the normal election cycle from the beginning. The third set of municipalities excluded from analysis are the thirteen large cities designated by ordinance, as their elections were scheduled on the second (not the fourth) Sunday of April every fourth year. Finally, the 23 special wards in Tokyo Metropolitan Area are not used, because their mayors were not elected but appointed between 1952 and 1974. Eventually, there remain 3,105 municipalities to be used for our analysis. We also note that we do not use data for the most recent SLEs held in 2007, because these elections were held after drastic municipal mergers from 2004 to 2006. Analysis of the 2007 data would, thus, introduce a methodological complication; that is, recent municipal mergers and resultant changes in election timings may be politically biased, thereby contaminating the effects of our treatment variables.

The denominator of each bar graph is the total number of control municipalities whose timing of deviation is identified ( $N=1,117$  for municipal assembly elections and 2,059 for mayoral elections).<sup>24</sup> It shows that more than 60% of those municipalities (84.7% for municipal assembly elections and 64.8% for mayoral elections) dropped out of the cycle of SLEs by the 1950s when the national government strongly encouraged municipal mergers by way of special laws and budgetary incentives. Newly established municipalities usually hold their elections before the four years have passed since the previous SLEs.<sup>25</sup> Indeed, municipal merger is the most common reason for those municipalities to drop out of the SLE cycle (80.2% and 43.8% for the legislative and executive control groups, respectively). We cannot imagine, however, that municipal mergers five decades ago, or factors affecting them, still have any substantial influence on citizens' and candidates' behavior in 2003. In particular, it is unreasonable to assume that factors behind municipal mergers in the 1950s affect any short-term *change* during the 2000s, which is our outcome variable. We revisit this important point in the next section.

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<sup>24</sup> We could not obtain information about the timing of deviation for 339 cases of municipal assembly elections and 177 cases of mayoral elections. For 47 cases of municipal assembly elections and 205 cases of mayoral elections, we could identify the timing of deviation but not the reasons for it. We tried to obtain and examine the official Records of Elections issued by the Election Administration Commission in every prefecture for various years or other equivalent materials by way of the Freedom of Information Act. Surprisingly, however, for some prefectures, old Records are not available to the best of our knowledge.

<sup>25</sup> Strictly speaking, after a merger, they must hold their mayoral elections within 50 days and assembly elections within one year (until 1962 for cities and until 1965 for towns and villages) or two years (otherwise).

The other two major reasons for deviation include assembly resolution or mayoral resignation before the expiry of their four-year term (14.2% and 28.3%, respectively) and the death of an incumbent mayor (17.4%).<sup>26</sup> It is extremely unlikely that these long-past events, or the factors which caused them, could affect the *difference* in population movement in a given month compared to the previous year.

In sum, all pretreatment variables (i.e., reasons for deviation) that affect our treatment variable (i.e., election timings) are unlikely to affect our outcome variable (i.e., the short-term *change* in population movement) regardless of whether these pretreatments are observed, unobserved or unobservable. At least, we have no theory to predict the direction of the effects of merger, death, resignation or resolution on short-term political behavior. Thus, we regard whether or not each municipal assembly or mayoral election was held on April 27, 2003, as an as-if randomly assigned treatment.<sup>27</sup>

### **Hypothesis 1: Timing**

We suspect that pre-electoral residential registration occurred in January 2003 for an April election because of the following two institutions. First, Article 9 of the Public Officers Election Act (POEA) stipulates that all eligible voters need to have their address registered in the district for more than three consecutive months before the election. In the case of the 2003 SLE, which was held on April 27, 2003, perpetrators had to change their residential registration *before*

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<sup>26</sup> One of the reasons for the resignation of a mayor is to run for another office, such as a Diet member or a governor. For other miscellaneous reasons, 2.3% (2.2%) of legislative (executive) control municipalities deviated from the SLE cycle.

<sup>27</sup> Note that we do not claim the election timing as *completely* at random (e.g., randomly assigned by the central government).

January 19 (in cities) or 21 (in towns and villages), 2003.<sup>28</sup> Second, residents must pay a residential tax to the municipality with their residential address (at least in the public record) as of January 1 in each year. Any change of their registered address is reported to tax office and their employer who withholds from their salary and pays their tax on their behalf. Since “relocaters” may not like the tax office and their employer to become suspicious of their unnatural moving, they are expected to change their residential registration *after* January 2, 2003 (and move it back to the original address before December 31, 2003).

Given these rules, we hypothesize a noticeable surge in the number of people registering their residential address in municipalities with an election (i.e., in the treatment group) only in January 2003.

**H1:** The treatment effects are positive only in January 2003.

Clearly, we do not argue that all moving to the treatment group in January 2003 is fraudulent. If we observe, however, significant *difference* between the treatment and control groups *only* in January 2003, we are inclined to believe that it is pre-electoral residential registration.

### **Hypothesis 2: Competitiveness**

In the previous section, we argued that electoral competitiveness encourages pre-electoral residential registration. One way to confirm that is to compare the size of treatment effects by the municipality population size, according to which electoral competitiveness varies. Article 91 of the Local Autonomy Law stipulates that the maximum municipal assembly size is a non-linear step function of municipal population size, which can be roughly approximated by a natural log

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<sup>28</sup> The deadlines are not exactly three months before due to procedural issues.

of the population size. For example, the maximum number of seats is 12 for municipalities with less than 2,000 residents, while it is 34 for municipalities with more than or equal to 100,000 but less than 200,000 residents. Thus, the assembly size rises only around threefold, even when the population size centuplicates. The typical number of candidates is slightly larger than the number of seats.

Therefore, as the population size decreases, the vote margin between candidates, which is correlated with the average number of votes each legislator represents, tends to decrease (Horiuchi 2005, Table 5.4, 86).<sup>29</sup> A similar logic applies to mayoral elections; namely, the difference in votes between a winner and a runner up tends to be smaller in smaller municipalities. Thus, we also test the following hypothesis by splitting municipalities into two subsamples: towns and villages, and cities.<sup>30</sup>

**H2:** The treatment effects are larger in towns and villages than in cities.

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<sup>29</sup> Chang and Golden (2006) argue that “the extent of competitiveness tends to increase with the number of candidates, or with what is called district magnitude” (p. 118. See also Persson, Tabellini, and Trebbi 2003). What matters is, however, not the number of candidates or seats but the number of votes necessary to change the electoral outcome, and the latter is in part a function of the number of voters each seat represents.

<sup>30</sup> Alternatively, we can divide observations by setting some arbitrary cutoff points for the population size. Since the types of municipalities change (from villages to towns, and then from towns to cities) as the population size increases, both approaches yield substantially the same results.

## STATISTICAL METHOD

This section introduces our statistical method to detect pre-electoral residential registration. We first specify a difference-in-difference model and give a rationale of applying this model for our analysis. Second, we make four different datasets to address heterogeneous treatment effects. Finally, we clarify the prediction of coefficient estimates.

### Difference-in-Difference Model

In estimating the legislative treatment effect, we set the following baseline model:

$$Y_{i,t,m} = \sum_{t,m} (f_{t,m} \cdot D_{t,m} + g_{t,m} \cdot LT_i \cdot D_{t,m}) + c_i + b_{i,m} + e_{i,t,m}.$$

The unit of observation is municipality  $i$  in month  $m$ , year  $t$  (January 2001 to December 2004).<sup>31</sup>

$Y_{i,t,m}$  is the natural logarithm of the number of relocated residential registration ( $W$ ) plus one (i.e.,  $\log(W+1)$ ).<sup>32</sup> The dummy variable  $D_{t,m}$  indicates whether the observation is for month  $m$  in year

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<sup>31</sup> We focus on this period so that our data include two years before and after the key month for our hypothesis (i.e., January 2003). Note that the term of a mayor and an assembly member is four years. See also Footnote 23.

<sup>32</sup> The data source is *Jûmin Kihon Daichô Hôkoku* [Report of Residents' Registration], Annuals, 2001-2004, Table 10, <http://www.e-stat.go.jp/SG1/estat/List.do?lid=000001012090> (access on June 12, 2009). The data for all months before April 2003 were aggregated at the level of municipalities as of April 27, 2003. Municipalities merged into other municipalities after April 27, 2003 are dropped from analysis for the post-merger period, because we cannot define the outcome variable. There is a small fraction (about 1%) of municipalities with zero moving-in population in a given month. To avoid losing these observations, we add one to all observations before the log transformation. Dropping these observations does not change our results in any

$t$ , while its coefficient  $f_{t,m}$  represents that month's fixed effect. These month-year fixed effects control the national average number of relocated residential registration, which varies substantially across months during the period of investigation. For example, the nationwide total is always more than three million in March or April – just before and after the new fiscal/school year begins on April 1. In other months, it is always less than two million. This number is typically the lowest in January (i.e., 1.2-1.3 million). Another observable pattern is that the number of movers in each month gradually declined from 2001 to 2004.

The legislative treatment variable  $LT_i$  is 1 if a municipality assembly election was held on April 27, 2003 and 0 otherwise, and it is interacted with the month-year dummy. Its coefficient  $g_{t,m}$  shows, on average, how many *additional* residential registrations are transferred to treatment municipalities (with a legislative election) in month  $m$  in year  $t$ , as compared to control municipalities (without a legislative election) in the same month in the same year. Note that the control municipalities' average number of relocated residential registrations is equal to  $f_{t,m}$ .

$c_i$  is the municipality-specific time-invariant fixed effect. It includes, for example, which district for prefecture-level and national-level elections each municipality belongs to and geographical attributes of each municipality. More importantly, a range of political and historical factors that caused the treatment assignments (e.g., whether or not a municipality experienced a merger with other municipalities in the 1960s) are obviously constant for each municipality during the period of investigation (i.e., January 2001 to December 2004). Therefore, by adding municipality-specific fixed effects, we can control any observable and unobservable determinants of why some municipalities dropped off from the election cycle and thus raise the

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substantial manner. We do not use moving-out population, simply because similar municipality-month panel data is unavailable.

level of our confidence that the treatment assignment is as-if random.

$b_{i,m}$  is the municipality-specific and month-specific effect. There exist some seasonal effects on moving; for example, as a nationwide pattern, more people move to other municipalities in March and April, as we explained. The number of relocated residential registrations in March/April, however, may be different depending on each municipality's geographical location, population size, etc. Specifically, larger urban municipalities surrounded by smaller rural municipalities are likely to have a larger number of "spring movers" than smaller municipalities.

$e_{i,t,m}$  is the error term, namely, a vector of all the other remaining observable and unobservable variables specific to municipality  $i$  in month  $m$ , year  $t$ . Some time-variant and municipality-specific economic variables, such as monthly inflation and unemployment rates, may explain why people move into a particular municipality at a particular time.

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

$$Y_{i,(t-1),m} = \sum_{t,m} (f_{(t-1),m} \cdot D_{(t-1),m} + g_{(t-1),m} \cdot LTI \cdot D_{(t-1),m}) + c_i + b_{i,m} + e_{i,(t-1),m}$$

By subtracting the second equation from the first, we obtain:

$$\Delta Y_{i,t,m} = \sum_{t,m} (\Delta(f_{t,m}) \cdot D_{t,m} + \Delta(g_{t,m}) \cdot LTI \cdot D_{t,m}) + \Delta e_{i,t,m}$$

where  $\Delta$  is the difference operator,  $\Delta V_{i,t,m} = V_{i,t,m} - V_{i,(t-1),m}$ . We assume that  $\Delta e_{i,t,m}$  independently follows the identical normal distribution. An important merit of this difference-in-difference model is that it controls all time-invariant municipality-specific characteristics ( $c_i$ ) as well as all municipality-specific seasonal effects ( $b_{i,m}$ ).<sup>33</sup> Moreover, when it comes to any short-term

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<sup>33</sup> Running a standard OLS regression with all these fixed effects (i.e., the first equation) is

change in demographic, economic and social characteristics during the 2000s which may affect the outcome variable (i.e.,  $\Delta e_{i,t,m}$ ), it is extremely unlikely that they have any effect on our treatment variables which are assigned *causally prior* to them, as documented in the previous section. This is the crux of our natural experiment. Therefore, we can safely assume that the treatment variable ( $LT_i$ ) is independent of the differentiated error term ( $\Delta e_{i,t,m}$ ); that is, it is as-if randomly assigned. This assumption leads to  $E(LT_i \cdot \Delta e_{i,t,m}) = 0$  (where  $E(\cdot)$  is the average function) and we obtain unbiased estimates of the causal effects ( $\Delta(g_{t,m})$ 's) by using only 36 month dummies ( $D_{t,m}$ ) and their 36 interaction terms with our treatment variable ( $LT_i$ ) as regressors.<sup>34</sup>

In a similar fashion, we derive the model of the executive treatment effect:

$$\Delta Y_{i,t,m} = \sum_{t,m} (\Delta(f_{t,m}) \cdot D_{t,m} + \Delta(h_{t,m}) \cdot ET_i \cdot D_{t,m}) + \Delta e_{i,t,m},$$

where the executive treatment variable  $ET_i$  is a dummy of whether a mayoral election was held on April 27, 2003, and  $h_{t,m}$  is the executive treatment effect in month  $m$  in year  $t$ .

### **Prediction**

We focus on 36 (= 12 months  $\times$  3 years)  $\Delta(g_{t,m})$ 's and  $\Delta(h_{t,m})$ 's. Since the 2003 SLEs were held at  $t = 2003$  and  $m = 4$ , Hypothesis 1 implies that only  $g_{2003,1}$  and  $h_{2003,1}$  (three months before the elections) are positive and all the other  $g_{t,m}$ 's and  $h_{t,m}$ 's are zero. Consequently, we predict the following:

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possible, but it would become computationally intensive as our specification has numerous (more than 36,000) dummy variables.

<sup>34</sup> In our preliminary analysis, we also estimated the treatment effects with some pretreatment variables controlled, but the substantive implications do not change.

$$\Delta(g_{2003,1}) \equiv g_{2003,1} - g_{2002,1} = g_{2003,1} - 0 > 0$$

$$\Delta(h_{2003,1}) \equiv h_{2003,1} - h_{2002,1} = h_{2003,1} - 0 > 0$$

$$\Delta(g_{2004,1}) \equiv g_{2004,1} - g_{2003,1} = 0 - g_{2003,1} < 0$$

$$\Delta(h_{2004,1}) \equiv h_{2004,1} - h_{2003,1} = 0 - h_{2003,1} < 0$$

and all the other  $\Delta(g_{t,m})$ 's and  $\Delta(h_{t,m})$ 's are zero.

In order to test Hypothesis 2, we split the whole sample into a town and village subsample as well as a city one, and estimate the treatment effects by using each subsample. Our anticipation is that the sizes of  $\Delta(g_{2003,1})$ ,  $\Delta(g_{2004,1})$ ,  $\Delta(h_{2003,1})$  and  $\Delta(h_{2004,1})$  are larger in the town and village subsample than in the city one.

#### **Four Datasets**

We assume that each treatment effect is conditional on the value of the other treatment. For example, the legislative treatment effect might be larger when there is no mayoral election (i.e.,  $ET_i = 0$ ), compared to when there is a mayoral election (i.e.,  $ET_i = 1$ ). As core supporters of legislative and mayoral candidates may overlap, legislative candidates can mobilize more voters when a mayoral election is not held than when mayoral candidates are also eager to mobilize the voter pool.

Given this possibility of treatment effect heterogeneity, we divide the dataset into two in two ways and estimate average treatment effects for four datasets; (1) the legislative treatment effect in the executive control group (municipalities II ( $N=1,130$ ) against IV ( $N=1,311$ ) in Table 1, total  $N=2,441$ ), (2) the legislative treatment effect in the executive treatment group (municipalities I ( $N=471$ ) against III ( $N=193$ ), total  $N=664$ ), (3) the executive treatment effect in the legislative control group (municipalities III against IV,  $N=1,504$ ), and (4) the executive treatment effect in the legislative treatment group (municipalities I against II,  $N=1,601$ ).

[Table 1 around here]

More formally, we estimate the following:

$$(1) \Delta(g_{t,m}) |_{ET_i=0} \equiv E(\Delta Y_{i,t,m} |_{LT_i=1, ET_i=0}) - E(\Delta Y_{i,t,m} |_{LT_i=0, ET_i=0})$$

$$(2) \Delta(g_{t,m}) |_{ET_i=1} \equiv E(\Delta Y_{i,t,m} |_{LT_i=1, ET_i=1}) - E(\Delta Y_{i,t,m} |_{LT_i=0, ET_i=1})$$

$$(3) \Delta(h_{t,m}) |_{LT_i=0} \equiv E(\Delta Y_{i,t,m} |_{ET_i=1, LT_i=0}) - E(\Delta Y_{i,t,m} |_{ET_i=0, LT_i=0})$$

$$(4) \Delta(h_{t,m}) |_{LT_i=1} \equiv E(\Delta Y_{i,t,m} |_{ET_i=1, LT_i=1}) - E(\Delta Y_{i,t,m} |_{ET_i=0, LT_i=1})$$

## RESULTS

### Estimates

Figure 3 shows estimates of the differentiated legislative treatment effects in the executive control group (the first of the four datasets),  $\Delta(g_{t,m}) |_{ET_i=0}$ , for every month from January 2002 to December 2004. Each dot represents our point estimate, whereas each bar shows its 99% confidence interval. Thus, when this bar does not reach the horizontal line at zero, the differentiated legislative treatment effect is statistically significant at the 1% level. These cases are highlighted in black. For all municipalities (the top panel),  $\Delta(g_{2003,1})$  is positive and  $\Delta(g_{2004,1})$  is negative (both are significant), while most of the other thirty-four  $\Delta(g_{t,m})$ 's are not significant.<sup>35</sup> We can thus infer that  $g_{2003,1}$  is positive while others are zero, which is consistent

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<sup>35</sup> An exception is December 2003 (negative), which is significant at the 1% level. With the 95% level, the coefficient for December 2002 (positive) also becomes significant. This may suggest that people who are engaged in pre-electoral residential registration may move not only in January 2003 but also in December 2002. Against our expectation, citizens may not mind that their change of registered addresses is reported to tax office and their employers. This pattern is

with Hypothesis 1. Comparison between the middle and bottom panels demonstrates that the absolute values of  $\Delta(g_{2003,1})$  and  $\Delta(g_{2004,1})$  are larger in towns and villages than in cities. This fits with Hypothesis 2. Moreover,  $\Delta(g_{2003,1})$  and  $\Delta(g_{2004,1})$  are both significant in towns and villages, but only  $\Delta(g_{2003,1})$  is significant in cities.

[Figure 3 around here]

Figure 4 illustrates the corresponding estimates for the executive treatment group (the second dataset),  $\Delta(g_{t,m}) \mid ET_i = 1$ . Compared to Figure 3, Figure 4 shows larger confidence intervals. This is due to the small size of the executive treatment group ( $N=664$  vs.  $N=2,441$ ). For all municipalities (the top panel),  $\Delta(g_{2003,1})$  is positive and significant, though all the other  $\Delta(g_{t,m})$ 's are not significant.  $\Delta(g_{2004,1})$  is negative but insignificant (the top panel). The middle panel displays no systematic pattern in the estimates across months for the city subsample. For the town and village subsample (the bottom panel), however, we obtained the positive, large and highly significant effect for January 2003. The estimate for January 2004 is negative but not significant. These three panels in Figure 4, thus, also suggest the validity of Hypothesis 1 and Hypothesis 2, at least to some degree.

[Figure 4 around here]

Figure 5 indicates the estimates of the differentiated executive treatment effects for the legislative control group (the third dataset),  $\Delta(h_{t,m}) \mid LT_i = 0$ . For the whole group (the top panel),  $\Delta(h_{2003,1})$  is positive and  $\Delta(h_{2004,1})$  is negative, but all months are insignificant at the 1% level. For both the city subsample (the middle panel) and the town and village subsample (the bottom

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not observed when using other three datasets (i.e., Figures 4-6).

panel), most months are insignificant and there is no systematic pattern, although the coefficients for January 2003 are positive and the coefficients for January 2004 are negative (for the town and village subsample, significant at the 5% level and larger than the city subsample). Although these effects are less distinctive than those shown in other figures due to the small size ( $N=193$ ) of the treatment group in this dataset, the patterns may suggest some validity of Hypotheses 1 and 2.

[Figure 5 around here]

Figure 6 shows the differentiated executive treatment effects of the legislative treatment group (the fourth dataset),  $\Delta(h_{t,m}) |_{LT_i=1}$ . For all municipalities (the top panel), the estimates are positive and significant for January 2003, negative and significant for January 2004. The coefficients for all other months are insignificant. This is consistent with Hypothesis 1. While the coefficients are nearly zero in most months for the city subsample (the middle panel), the only significantly positive effect for the town and village subsample is in January 2003. The negative coefficient for January 2004 is significant at the 5% level. These results are consistent with Hypothesis 2.

[Figure 6 around here]

Overall, the estimates largely suggest the validity of our two hypotheses. The differentiated legislative and executive treatment effects tend to be significant in January 2003 and 2004, as summarized in Table 2. An equally important finding shown in Figures 3-6 is that the effects are not significant in most other months. Specifically, for all municipalities (top panels in Figures 3-6), there is only one significant (at the 1% level) coefficient (December 2002) among 136 (i.e., 34 months – excluding January 2003 and January 2004 – times four datasets) estimates. A combination of these suggests the validity of Hypothesis 1.

[Table 2 around here]

Table 2 also shows that the legislative and executive treatment effects tend to be larger in towns and villages than in cities. Most of the coefficients are significant for the town-and-village subsample. The estimated effects for other months are mostly insignificant for both subsamples. These results suggest that pre-electoral residential registration is more common in smaller municipalities. This supports our Hypothesis 2.

### **Discussion**

Some might be afraid that the estimated treatment effects are not large enough to be decisive in an election. As noted earlier, however, in Japanese municipality assembly elections, single-digit votes can be pivotal in determining who gets elected. In our sample, the monthly average number of relocated residential registration from January 2001 to December 2004 is 111. The point estimate of differentiated legislative treatment effect in January 2003 is, for example, about 9.6% ( $=100 * \exp(0.092) - 100$ ) if a mayoral election is not held (the top panel in Figure 3). Thus, as many as an extra eleven ( $=111 * 0.096$ ) people moved into legislative treatment municipalities as compared to legislative control municipalities. This is large enough to switch the last winner and the runner up in nearly a quarter of municipal assembly elections as noted earlier and a small enough number of votes for candidates to mobilize. We emphasize that the effect does exist and we can detect such a small electoral trick using this natural experiment.

Skeptical readers might come up with two alternative interpretations of our results above. First, they may suspect that people physically moved before (rather than in) January 2003 to treatment municipalities (probably not for electoral reasons) but did not transfer their registered address until an election in April 2003 prompted them to do so (i.e., until January 2003). This scenario is implausible. In the first place, if one is found not to have changed residential

registration within two weeks after moving, one needs to pay a fine (up to 50,000 Japanese Yen, equivalent to about 600 U.S. dollars). Moreover, depending on where you are registered to live, rather than where you physically reside, people send their children to public schools and child care centers, receive allowance for dependent children, claim for health insurance if self employed, claim for nursing care insurance, and enjoy public health care, etc. Therefore, it is too costly to take this course.

Second, it is possible and legal that people changed their registered residential address for the purpose of voting in April 2003, actually moved their place of residence in January 2003 in order to do this, and (in most cases) returned to their original residence after the election. Unless they could afford to maintain two residences, however, they would have to physically move their belongings twice, as well as cancelling and setting up all public services and utility contracts. Considering how rarely pre-electoral residential registration is caught, this scenario is highly unlikely, as well.

This linking of government services to residential address is a major reason why those who commit pre-electoral residential registration are sometimes employees (whose health insurance, unlike that of the self-employed, does not depend on where they live), young students (who have no children), and gangsters (who disregard these services). It is true that they will still suffer from lack of some public services, as well as having to undertake paperwork at municipality offices, but they are ready to do so for the purpose of voting and, presumably, for benefits or lack of punishment given by candidates or core supporters.

## **CONCLUSION**

Based on Japan's institutional settings, which produce an as-if random assignment of election timings, we estimated the impacts of having an election on what we call pre-electoral residential

registration – the cross-municipality change of registered address for the purpose of voting. Our difference-in-difference estimates suggest that people do, indeed, move (at least, on paper) before an election and the magnitude of the effect of this is substantial, particularly in small towns and villages where assembly elections tend to be highly competitive.

We suggest that our approach – “electoral timing as treatment” – can be applied to a number of other important contexts. One example is local elections. In G7 countries except France, at least, not all subnational governments of the same level hold elections concurrently. To examine the U.S. in detail, 40 gubernatorial elections have departed from the presidential cycle in various years (Moore, Preimesberger, and Tarr 2001, 1378-79 and ch. 30). Four states (i.e., Louisiana, Mississippi, New Jersey and Virginia) have their state legislative elections in odd-numbered years (Dubin 2007). Around 60% of U.S. cities hold their elections separately from either the presidential or gubernatorial contests (MacManus 1999, 175). Another example is staggered-term chambers where not all districts are up for election at the same time. In the case of the U.S. Senate, only two-thirds states hold elections every two years.<sup>36</sup> Moreover, 26 American states have a staggered term state senate, where, typically, half of the districts are up for grabs every two years.<sup>37</sup> About four-fifths of American cities also have staggered term

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<sup>36</sup> By taking advantage of this setup, Shepsle et al. (2009) show that senators provide more appropriation for states with up-for-reelection senators than states without them.

<sup>37</sup> According to every state homepage (access on January through February 2011) and Dubin (2007), they include Alaska, Arkansas, California, Colorado, Delaware, Florida, Hawaii, Illinois, Indiana, Iowa, Kentucky, Missouri, Montana, Nebraska (unicameral), Nevada, North Dakota, Ohio, Oklahoma, Oregon, Pennsylvania, Tennessee, Texas, Utah, Washington, Wisconsin and Wyoming. Though West Virginia Senate is a staggered term chamber, every two-member district

council and all or some seats of about two-fifths city councils are elected from single-member districts (MacManus 1999, 169-70, 174). In the French Senate, until the 2003 reform acts, districts (departments) were divided into three series, each of which had approximately the same number of seats, and only the districts in one series had indirect elections every three years. In all departments in France, assembly elections are held every three years but only for half of the districts (cantons) alternately. In the case of the Czech Senate, a third of districts return senators every two years.<sup>38</sup>

If, fortunately, we can regard the election timing as an as-if randomly assigned treatment as we do in this paper, the setup becomes a natural experiment. The three classes of U.S. Senators were originally and, then upon admission of new states, assigned by “draw[ing] lots from a box.”<sup>39</sup> In 1959, French Senators’ districts were assigned to one of the three series “based on *the alphabetical and numerical ordering* of departments... [and] the order in which the series would proceed to be elected was decided by *drawing lots*” (Smith 2009, 96-99, emphasis added). We are not sure whether, in the other cases, electoral timing can be considered an as-if randomly assigned treatment. Even if not, however, electoral timing is still worth exploiting as long as we can control relevant pretreatment variables by way of matching or any other proper statistical method. We also emphasize that the outcome variables that can be explained by the timing of

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alternately chooses a four-year term senator every two years. Thus, electoral timing is the same across districts.

<sup>38</sup> <http://www.senat.cz/> (access on February 18, 2011).

<sup>39</sup> U.S. Congress. *Senate Journal*. 1st Cong, 1st sess., May15, 1789, 25, cited in [http://www.senate.gov/artandhistory/history/minute/Senatorial\\_Lottery.htm](http://www.senate.gov/artandhistory/history/minute/Senatorial_Lottery.htm) (access on January 28, 2011).

election are not only (abnormal) population movements but also many other variables that politicians' mobilization efforts might affect, such as pork-barrel expenditures and voter turnout in other levels of elections.

To conclude, we hope that future scholars in many other electoral contexts will estimate, with the help of appropriate statistical tools, how many people are engaged in electoral fraud and how much (and in what situations) their behavior affects election outcomes. "The internationalization of research on electoral fraud" (Alvarez, Hall and Hyde 2008, 240) provides us with various situations, which enable scholars to examine institutional (and cultural) causes of not only legal participation (voting) but also illegal participation (fraud), and consider an optimal electoral registration system that balances both.

## APPENDIX: Newspaper Reports

Prefecture	Municipality	Type	Election Day	Reported On
Shiga	Torahime	Town L	12/6/1981	4/20/1985
Tokyo	Setagaya	Ward P	7/7/1985	7/9/1985
Hiroshima	Tojo	Town P	4/12/1987	5/15/1987
Tokyo	Meguro	Ward LEG	4/26/1987	4/20/1987
Niigata	Shirone	City L	4/26/1987	4/30/1987
Hyogo	Amagasaki	City L	4/26/1987	5/15/1987
Hiroshima	Tojo	Town E	4/26/1987	5/15/1987
Yamaguchi	Kaminoseki	Town E	4/26/1987	5/15/1987
Ehime	Ikata	Town L	4/26/1987	5/15/1987
Mie	Kisei	Town E	2/4/1990	12/5/1989
Tokyo	Suginami	Ward N	2/18/1990	3/9/1990
Ibaraki	Itako	Town E	2/10/1991	7/26/1991
Yamanashi	Yamanakako	Village L	4/21/1991	4/17/1991
Osaka	Osaka	City P	4/21/1991	4/10/1991
Hyogo	Itami	City L	4/21/1991	4/22/1991
Tottori	Hiezu	Village L	4/21/1991	4/23/1991
Saga	Ogi	Town L	7/7/1991	11/2/1991
Ibaraki	Fujishiro	Town L	8/4/1991	12/18/1991
Chiba	Kimitsu	City L	9/22/1991	4/25/1992
Nara	Kanmaki	Town E	12/22/1991	10/14/1995
Hyogo	Ieshima	Town E	5/15/1994	7/17/1994
Osaka	Osaka	City L	4/9/1995	4/13/1995
Miyazaki	Suki	Village LE	4/23/1995	5/12/1995
Tochigi	Kuriyama	Village LE	8/6/1995	9/27/1995
Chiba	Shirako	Town L	11/26/1995	12/4/1995
Miyazaki	Kushima	City E	11/17/1996	11/22/1996
Osaka	Higashiosaka	City E	12/7/1997	4/27/1998
Gifu	Takatomi	Town P	4/11/1999	11/25/1999
Hokkaido	Todohokke	Village E	4/25/1999	5/12/1999
Aomori	Nishimeya	Village LE	4/25/1999	4/28/1999

*Note:* L = Municipality Assembly, E = Mayor, P = Prefecture Assembly, G = Governor, N = National Diet.

### APPENDIX: Newspaper Reports (continued)

Prefecture	Municipality	Type		Election Day	Reported On
Aomori	Rokkasho	Village	L	4/25/1999	5/20/1999
Osaka	Neyagawa	City	E	4/25/1999	4/27/1999
Okinawa	Yonakuni	Town	E	4/25/1999	2/28/2000
Kagoshima	Kamiyaku	Town	E	4/23/2000	4/18/2000
Saitama	Otaki	Village	E	7/7/2002	8/1/2002
Fukuoka	Maebaru	City	L	11/10/2002	5/20/2003
Hokkaido	Tomakomai	City	P	4/13/2003	4/16/2003
Tochigi	Oyama	City	P	4/13/2003	4/15/2003
Kanagawa	Yokohama	City	L	4/13/2003	4/24/2003
Osaka	Osaka	City	L	4/13/2003	4/15/2003
Osaka	Osaka	City	L	4/13/2003	4/14/2003
Hyogo	Kobe	City	L	4/13/2003	4/15/2003
Fukuoka	Nijo	Town	L	4/27/2003	5/20/2003
Okayama	Asakuchi	City	L	4/23/2006	1/18/2007
Kumamoto	Kosa	Town	L	2/18/2007	3/2/2007
Tokyo	Taito	Ward	L	3/18/2007	3/27/2007
Oita	Bungotakada	City	P	4/8/2007	4/19/2007
Tokyo	Minato	Ward	L	4/22/2007	4/25/2007
Hyogo	Itami	City	L	4/22/2007	4/25/2007

*Note:* L = Municipality Assembly, E = Mayor, P = Prefecture Assembly, G = Governor, N = National Diet

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**Table 1:** Four Types of Municipalities

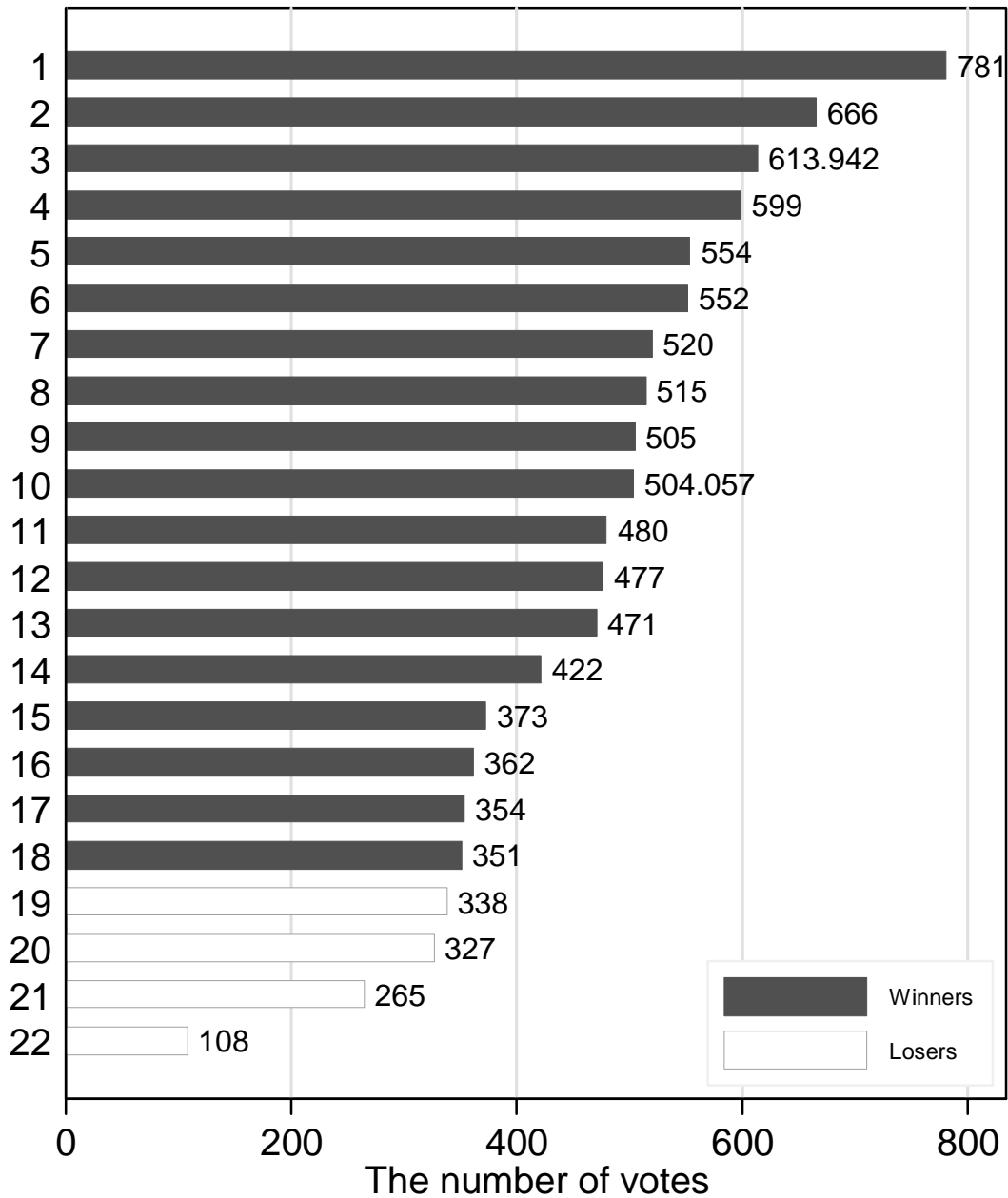
		Was a mayoral election held in April 2003?	
		Yes ( $ET_i=1$ )	No ( $ET_i=0$ )
Was a municipal assembly election held in April 2003?	Yes ( $LT_i=1$ )	I ( $N=471$ )	II ( $N=1,130$ )
	No ( $LT_i=0$ )	III ( $N=193$ )	IV ( $N=1,311$ )

**Table 2:** Summary of Estimated Treatment Effects

year/month	municipalities included	treatment effect	coefficient estimate	standard error	
January 2003 ( $t=2003, m=1$ )	all	$\Delta(g_{t,m}) \mid ET_i = 0$	0.092	0.020	**
		$\Delta(g_{t,m}) \mid ET_i = 1$	0.129	0.042	**
		$\Delta(h_{t,m}) \mid LT_i = 0$	0.040	0.037	
		$\Delta(h_{t,m}) \mid LT_i = 1$	0.076	0.029	**
	cities	$\Delta(g_{t,m}) \mid ET_i = 0$	0.056	0.017	**
		$\Delta(g_{t,m}) \mid ET_i = 1$	-0.079	0.060	
		$\Delta(h_{t,m}) \mid LT_i = 0$	0.118	0.058	*
		$\Delta(h_{t,m}) \mid LT_i = 1$	-0.017	0.023	
	towns and villages	$\Delta(g_{t,m}) \mid ET_i = 0$	0.103	0.026	**
		$\Delta(g_{t,m}) \mid ET_i = 1$	0.172	0.049	**
		$\Delta(h_{t,m}) \mid LT_i = 0$	0.033	0.041	
		$\Delta(h_{t,m}) \mid LT_i = 1$	0.102	0.037	**
January 2004 ( $t=2004, m=1$ )	all	$\Delta(g_{t,m}) \mid ET_i = 0$	-0.059	0.022	**
		$\Delta(g_{t,m}) \mid ET_i = 1$	-0.040	0.046	
		$\Delta(h_{t,m}) \mid LT_i = 0$	-0.102	0.040	*
		$\Delta(h_{t,m}) \mid LT_i = 1$	-0.083	0.031	**
	cities	$\Delta(g_{t,m}) \mid ET_i = 0$	-0.004	0.018	
		$\Delta(g_{t,m}) \mid ET_i = 1$	0.047	0.055	
		$\Delta(h_{t,m}) \mid LT_i = 0$	-0.094	0.054	
		$\Delta(h_{t,m}) \mid LT_i = 1$	-0.044	0.021	*
	towns and villages	$\Delta(g_{t,m}) \mid ET_i = 0$	-0.080	0.028	**
		$\Delta(g_{t,m}) \mid ET_i = 1$	-0.069	0.054	
		$\Delta(h_{t,m}) \mid LT_i = 0$	-0.103	0.045	*
		$\Delta(h_{t,m}) \mid LT_i = 1$	-0.092	0.041	*

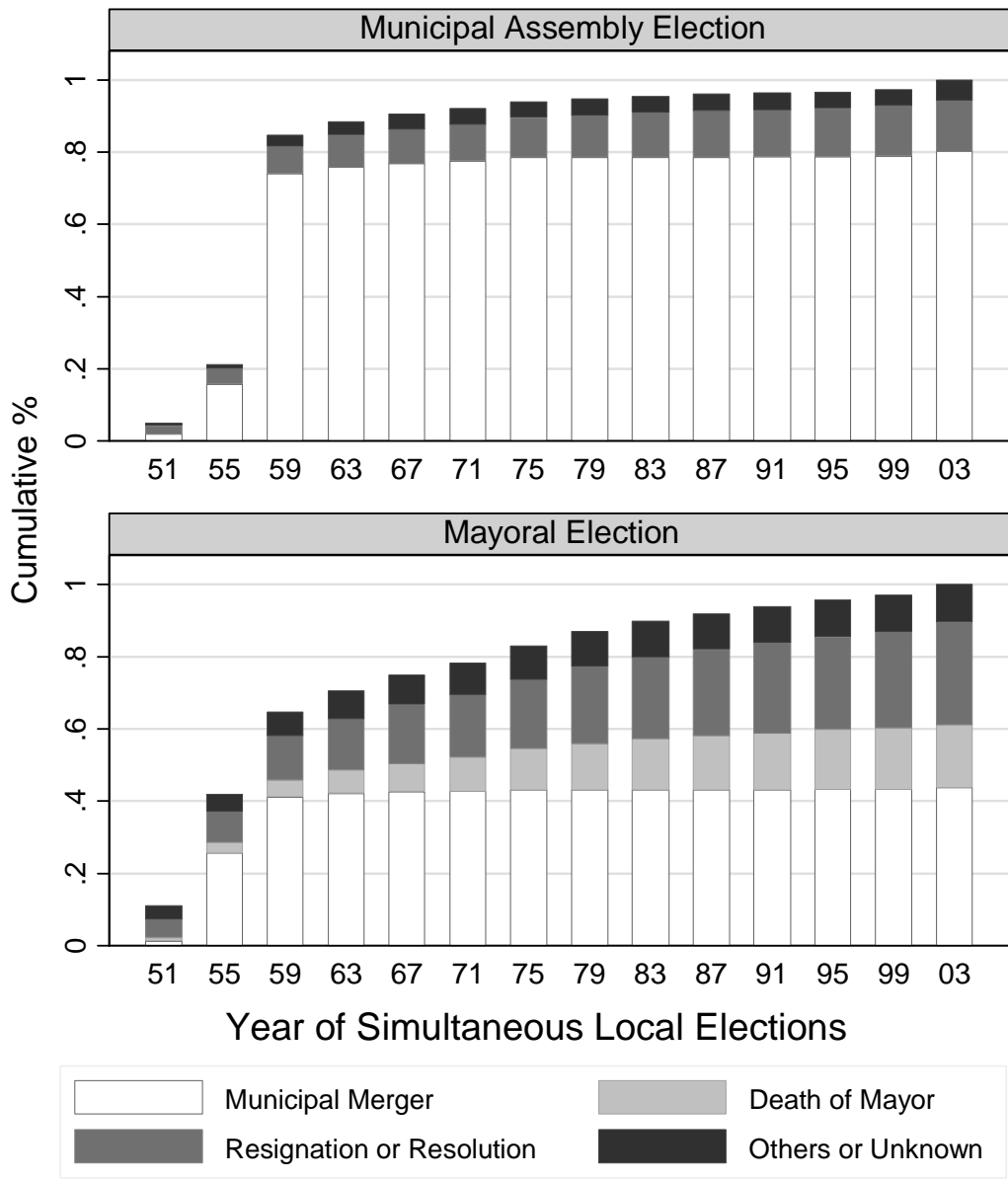
Note:  $\Delta(g_{t,m}) \mid ET_i = j$ , a conditional legislative treatment effect.  $\Delta(h_{t,m}) \mid LT_i = j$ , a conditional executive treatment effect. The outcome variable is the log-ratio of relocated residential registration to the same month of the previous year. \*\* significant at the 1% level, \* significant at the 5% level (two-sided).

**Figure 1:** Sample Distribution of Votes in a Municipal Assembly Election



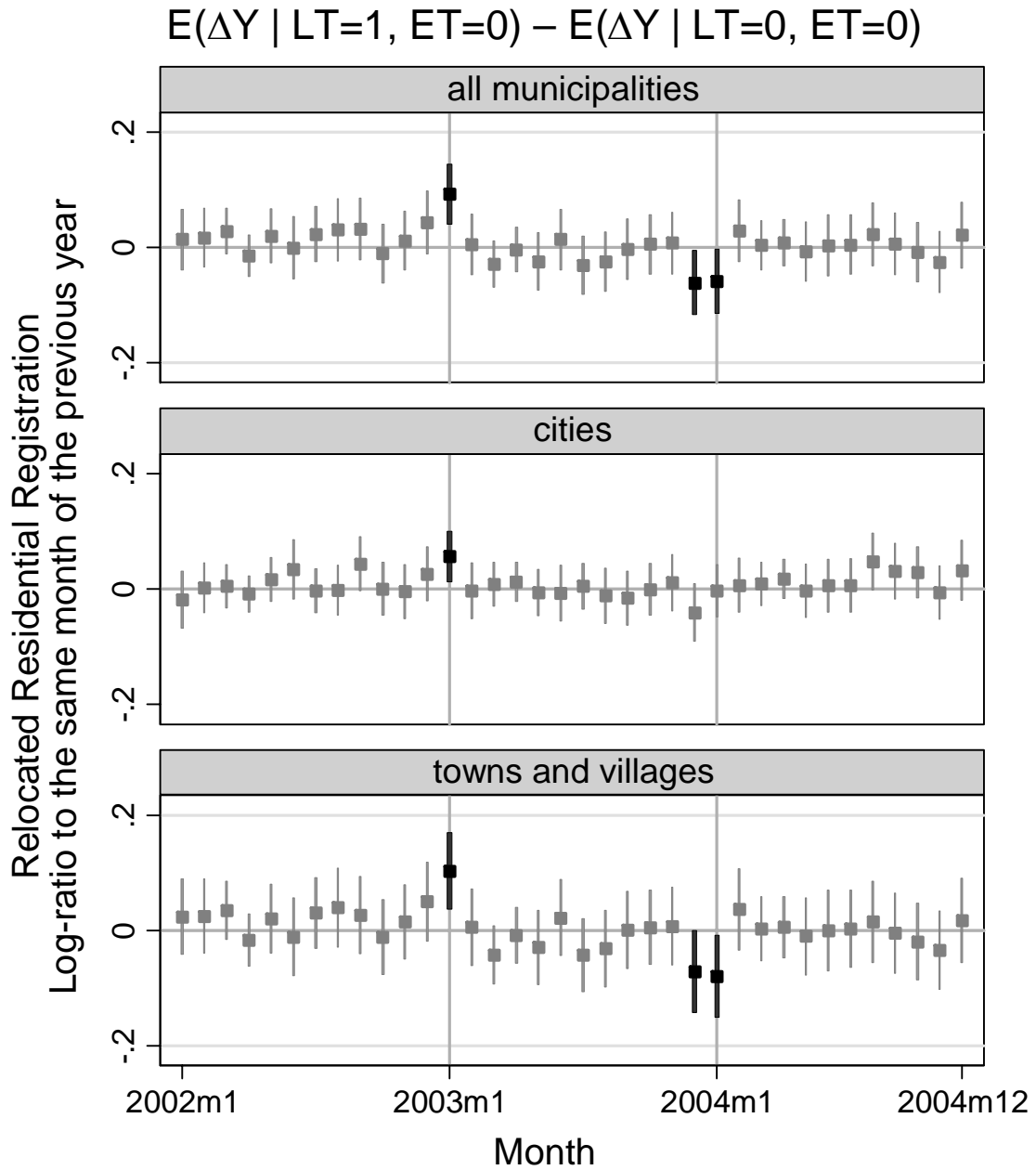
*Note:* The figure shows the distribution of votes in the Taneichi Municipal Assembly Election in Iwate Prefecture, which was held on April 27, 2003. Twenty-two candidates competed for 18 seats. The total number of eligible voters was 11,699. Voter turnout was 87.6%.

**Figure 2:** When and Why Did Municipalities Drop from the SLEs?



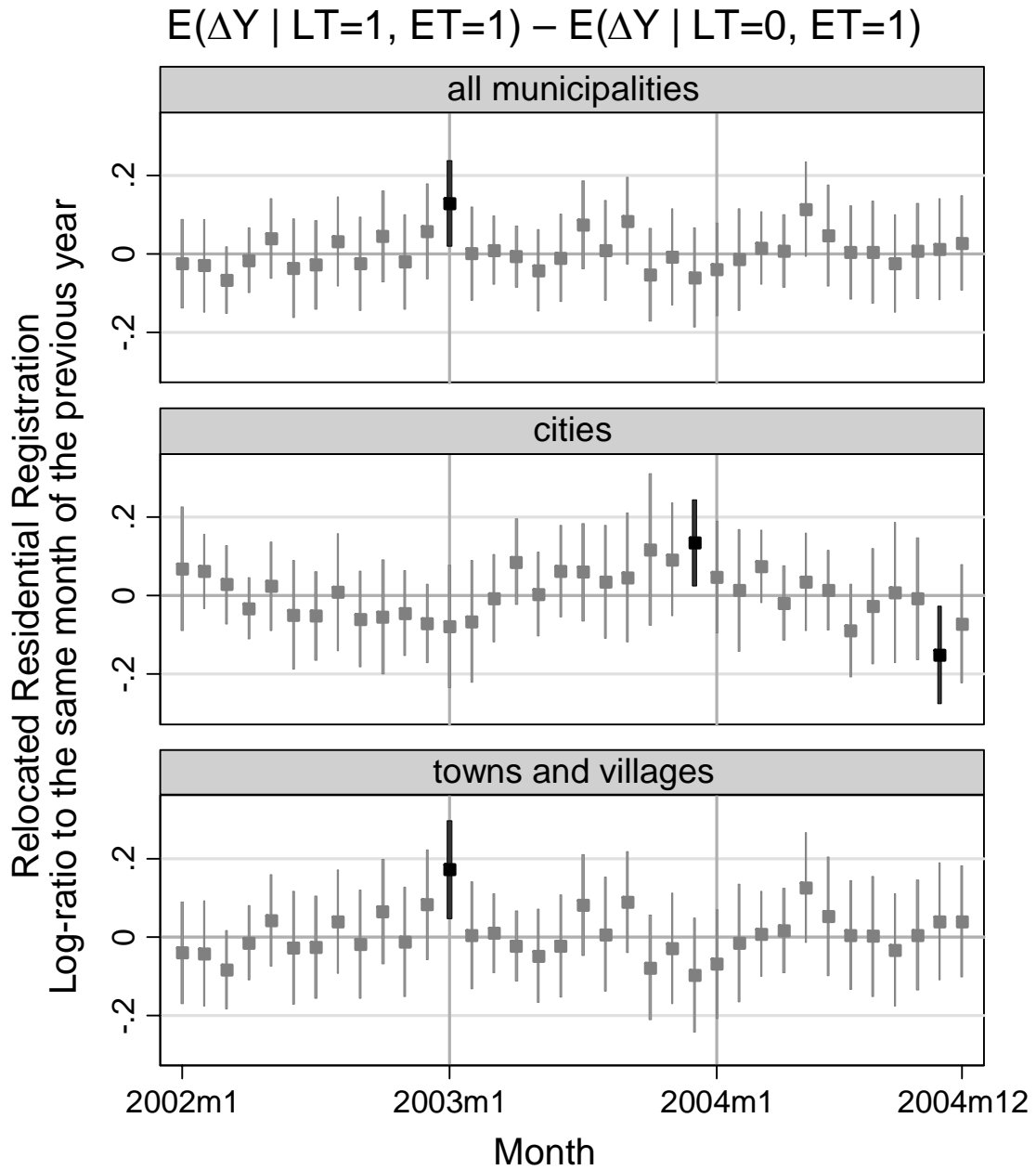
*Note:* The figures show the cumulative percentages of “control” observations – municipalities, which did not have a municipal assembly or mayoral election on April 27, 2003 – by years and by reasons of deviation from simultaneous local elections. For example, the “59” bar represents municipalities whose last deviation from the SLE cycle happened before the 1959 SLE.

**Figure 3:** Legislative Treatment Effects in Executive-Control Municipalities



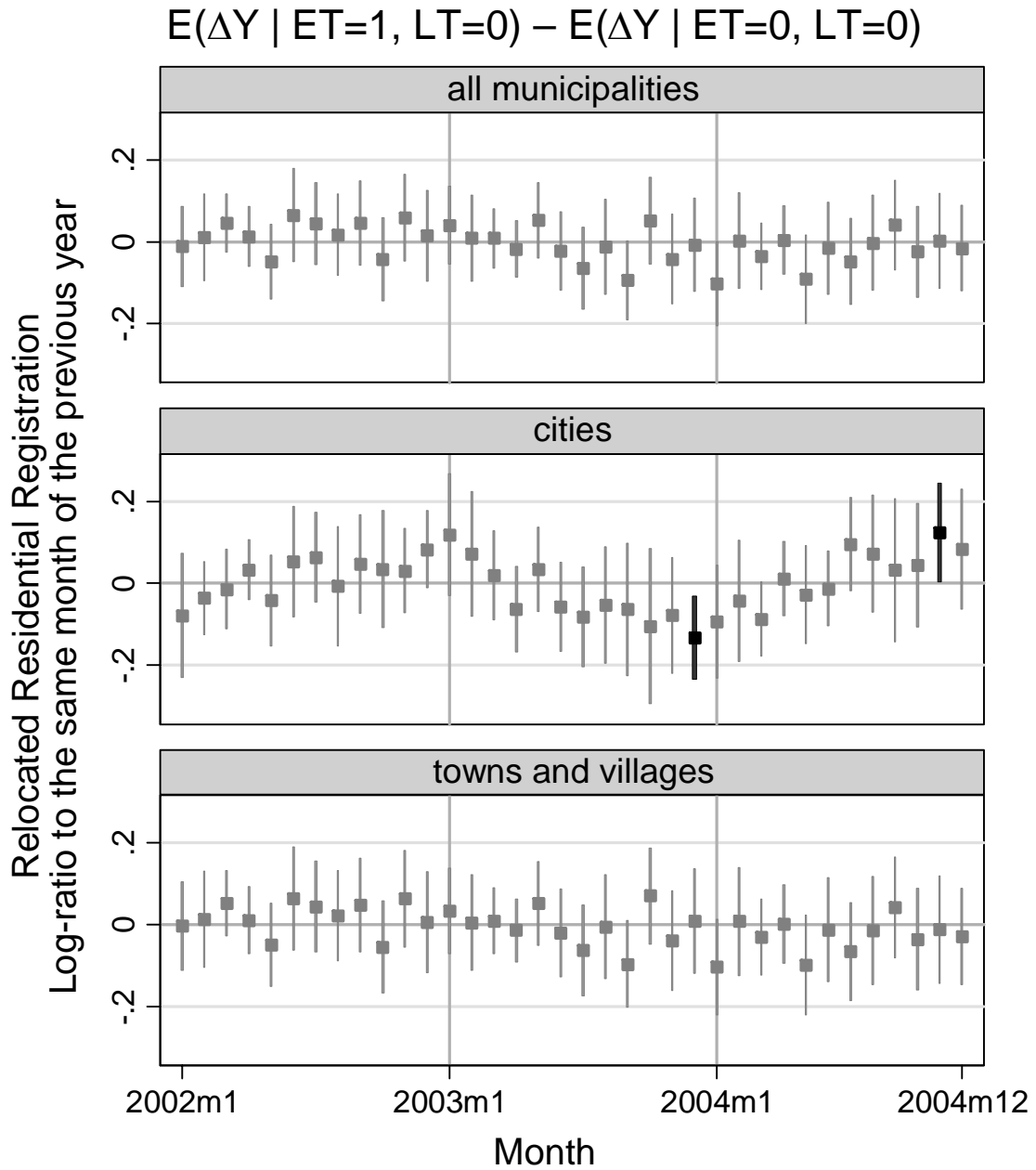
*Note:* The figure shows the OLS regression coefficients and their 99% confidence intervals of each treatment variable multiplied by a month dummy. The outcome variable is the log-ratio of relocated residential registration to the same month of the previous year.

**Figure 4:** Legislative Treatment Effects in Executive-Treatment Municipalities



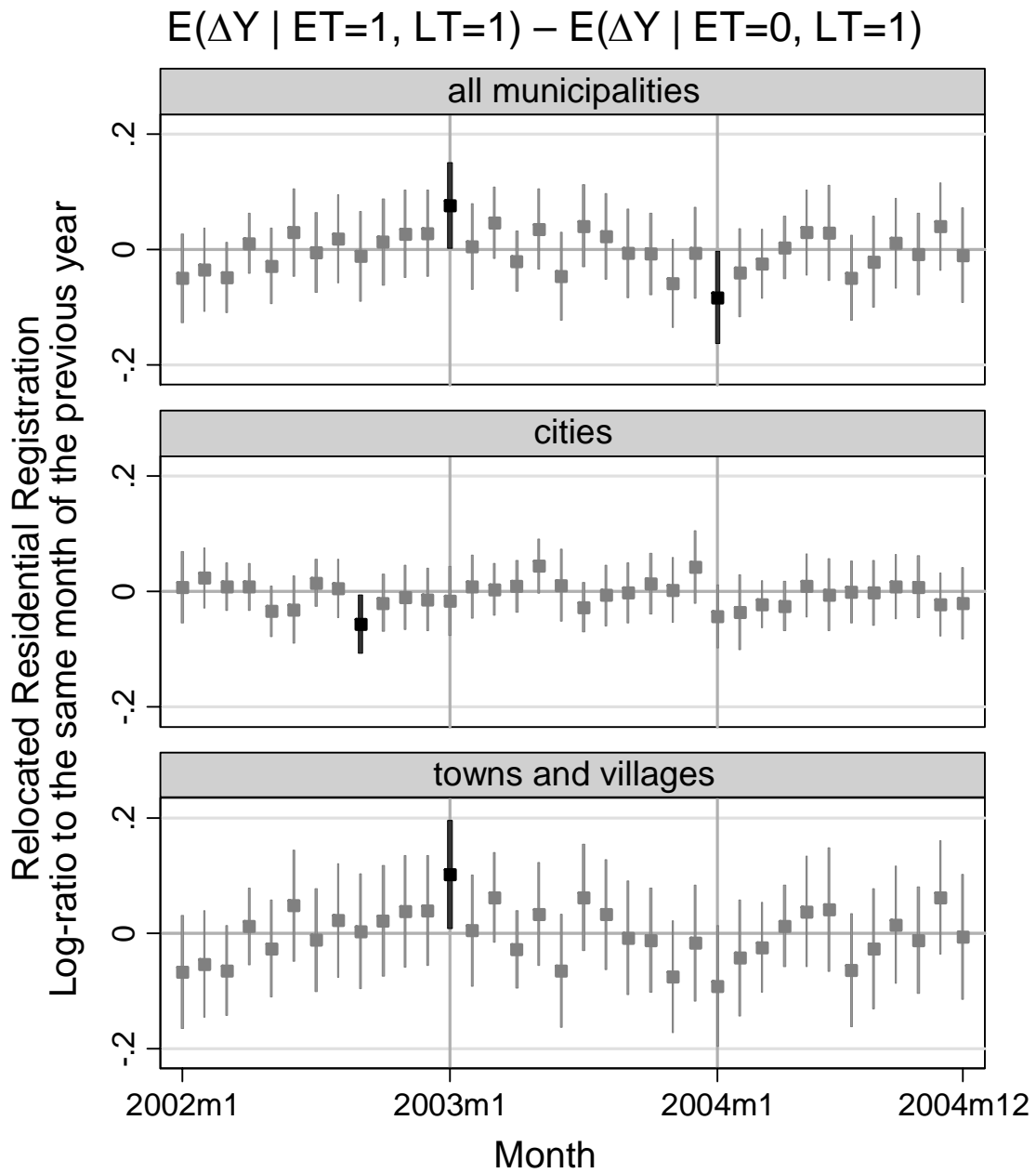
*Note:* The figure shows the OLS regression coefficients and their 99% confidence intervals of each treatment variable multiplied by a month dummy. The outcome variable is the log-ratio of relocated residential registration to the same month of the previous year.

**Figure 5:** Executive Treatment Effects in Legislative-Control Municipalities



*Note:* The figure shows the OLS regression coefficients and their 99% confidence intervals of each treatment variable multiplied by a month dummy. The outcome variable is the log-ratio of relocated residential registration to the same month of the previous year.

**Figure 6:** Executive Treatment Effects in Legislative-Treatment Municipalities



*Note:* The figure shows the OLS regression coefficients and their 99% confidence intervals of each treatment variable multiplied by a month dummy. The outcome variable is the log-ratio of relocated residential registration to the same month of the previous year.