doi:10.1017/S0003055411000268

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

KENTARO FUKUMOTO *Gakushuin University*
YUSAKU HORIUCHI *Australian National University*

Weak electoral registration requirements are commonly thought to encourage electoral participation, but may also promote electoral fraud. As one possibility, candidates and their supporters can more easily mobilize voters who do not reside within the district to register there fraudulently and vote for that district's candidates. 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. A differences-in-differences analysis of municipality–month panel data shows that the 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 decisive enough to change the electoral results 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 electoral connections in other countries.

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 one hand, stricter rules discourage people from registering to vote (e.g., Campbell et al. 1960, 276–86; Rosenstone and Hansen 1993, 196–209; Wolfinger and Rosenstone 1980, chap. 4). On the other hand, weak registration requirements promote electoral fraud and allow even noneligible voters (e.g., nonresident voters) to register to vote illegally. In the first place, voter registration was introduced mainly to address this concern (Campbell 2005). This tradeoff has long been a target of not only academic but also partisan debates in the United States (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, those who engage in fraudulent behavior attempt—and often manage—to conceal their activity. 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 have filed in the legislature (in order to nullify electoral results) (Lehoucq and Molina 2002), 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 overall scale of electoral fraud because of the covert nature of fraud, or overreporting because of 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 addresses must register them at a municipality office within two weeks.[1] Voting-age adults are *automatically* registered to vote in the municipality without an additional voter registration process, as long as their residence remains registered there for at least three consecutive months before the announcement of elections.[2] Some voters change their registered addresses before the election *for the*

Kentaro Fukumoto is Professor, Department of Political Science, Gakushuin University, 1-5-1 Mejiro, Toshima, Tokyo 171-8588, Japan (Kentaro.Fukumoto@gakushuin.ac.jp).

Yusaku Horiuchi is Associate Professor, Crawford School of Economics and Government, College of Asia and the Pacific, Australian National University, J.G. Crawford Building, No. 132, Canberra, ACT 0200, Australia (yusaku.horiuchi@anu.edu.au).

Earlier versions of this article were presented at the 2009 Annual Summer Meeting of the Society of Political Methodology at Yale University and the UCLA Workshop on Japan's Post-bubble Political Economy of 2009. We thank the National Diet Library of Japan, the Japan Research Institute for Local Government, Ryota Natori, Jun Saito, and Jun'ichiro Wada for archival help; Futoshi Ueki for research assistance; and Alison Cumming Thom, Olle Folke, Shigeo Hirano, Karen Long Jusko, Andrew Leigh, Matthew Linley, Sherry Martin, Jacob Michael Montgomery, Costas Panagopoulos, Dan Smith, Rob Weiner, Atsushi Yoshida, other participants in these meetings, and the co-editor and reviewers of this journal for useful comments. Fukumoto also appreciates the financial assistance of the Gakushuin's Abe Yoshishige Memorial Educational Fund, the Gakushuin University's Computer Centre, the Inamori Foundation, the Japan Society for the Promotion of Science [Grant-in-Aid for Scientific Research (B) 20330023], and the Joint Usage/Research Center (2008–2012; Ministry of Education, Culture, Sports, Science and Technology, Japan).

[1] Article 22, Residential Basic Book Act (RBBA).
[2] Article 15, RBBA; Articles 19, 21, and 22, Public Offices Election Act (POEA).

https://doi.org/10.1017/S0003055411000268 Published online by Cambridge University Press

purpose of voting, and we call this behavior "preelectoral residential registration." In many cases, candidates and their supporters reportedly ask voters outside a district to change their registered addresses 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 addresses on paper *without changing their actual residences*, in which case their behavior is illegal.[3] Because this activity 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 inferences 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 preelectoral residential registration. The effects estimated by differences-in-differences models are significant and, when the election is competitive, decisive enough to change the electoral results.

This study seeks to make a substantive and methodological contribution to the broader literature. First, we statistically detect preelectoral 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). As we explore in the next section, the practice was and is common across many countries in Africa, the Americas, the Asia–Pacific region, and Europe. 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 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 article is as follows. The next section summarizes newspaper reports on preelectoral 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 differences-in-differences models, clarifies our predictions, and describes the data. After showing and discussing the estimated results in the subsequent two sections, we conclude by summarizing our findings and discussing alternative setups where "election timing as treatment" might be applied.

PREELECTORAL RESIDENTIAL REGISTRATION

To repeat, we define preelectoral residential registration as a change of registered address from one municipality to another before an 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: the extremely competitive nature of municipal elections. Finally, we call attention to the fact that preelectoral residential registration is by no means specific to Japan but commonly found elsewhere.

Patterns of Preelectoral Residential Registration

In Japan, preelectoral residential registration has been periodically mentioned in the legislature (the Diet),[4] as well as investigated as a form of electoral fraud.[5] Most cases investigated are concerned with subnational (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 related crimes (Sômushô Jichigyôsei Kyoku Senkyo Bu 2003, 418; Keisatsuchô 2003, Table 59).[6]

[3] Article 236, Section 2, POEA.

[4] 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 1966).

[5] The often-cited leading case occurred in 1926 (Supreme Court Judgment, July 4, 1928).

[6] The related crimes are false oath (Article 236, Section 3, POEA); false voting, impersonation in voting, ballot stuffing and ballot stealing (Article 237); and misuse of proxy voting (e.g., on behalf of the disabled, Article 237–2).

https://doi.org/10.1017/S0003055411000268 Published online by Cambridge University Press

Searching one of the major newspapers, *Asahi Shimbun*, between 1985 and 2009,[7] for the two key expressions, *kakû ten'nyu* (false moving in) and *sagi tôroku* (false registration), we identified 48 reported cases of illegal preelectoral residential registration.[8] 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., In what follows, we summarize these reports and explain what is known about illegal preelectoral 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 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. Because 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 bosses.

To which addresses 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 preelectoral 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, at least 202 people registered their residence as a small house of only about 240 square meters.[9]

How is this type of electoral fraud feasible? It is the simplicity of the registration process that makes it so: Japanese people can easily change their registered addresses 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.[10] It is important to note that candidates can monitor whether those who are asked to engage in preelectoral residential registration did, indeed, do so because lists of eligible voters for specific elections are 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.[11] 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.[12]

When does preelectoral residential registration commonly take place? In many cases, "relocaters" change their addresses just before the deadline of eligibility, three months before the election. (We will explain the timing of preelectoral residential registration more in detail in a later section.) Usually, after the election, they move their residential registration back to where they actually live.[13]

Politicians and government officials might argue that these cases are exceptions, but numerous newspaper reports imply that illegal preelectoral 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) 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 newcomers—brought their addresses to this small town. After the case was 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 preelectoral residential registration and not investigating the veracity of residential registration.[14]

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 will engage in electoral fraud. For example, Birch (2007) writes,

[7] The full text online search is only available for articles dated after 1985.

[8] See the Appendix for the list of these reports.

[9] *Asahi Shimbun*, May 9, 1990.

[10] *Asahi Shimbun*, April 20, 1987.

[11] See, for example, *Asahi Shimbun*, December 7, 1991 and April 27, 1998. Impersonation in voting violates Article 237, Section 2, POEA.

[12] See, for example, *Asahi Shimbun*, July 9, 1985, April 30 and May 15, 1987 and July 17, 1994. This ineligible voting violates Article 237, Section 2, POEA.

[13] See, for example, *Asahi Shimbun*, July 17, 1994 and October 14, 1995.

[14] 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).

https://doi.org/10.1017/S0003055411000268 Published online by Cambridge University Press

"[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" (1538–39).[15] Because 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 intraparty competition also affects the likelihood of candidates engaging in fraudulent activities. Where candidates from the same party compete with one another, they seek to acquire "personal votes" distinguishing them from others running for office with the same party label and, therefore, have incentives to engage in electoral fraud (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 or 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).[16] Under this single nontransferable vote system, the vote margins between candidates tend to be extraordinarily small and dozens of candidates run from the same party or camp.[17]

Figure 1 illustrates an example of the distribution of votes in an assembly election in a municipality with the median population size (about ten thousand), the town of Taneichi in Iwate prefecture. Twenty-two candidates competed for 18 seats in the election on April 27, 2003. The last winner (the 18th candidate) obtained 351 votes, whereas 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 to 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 10, including zero, in about 20% of them (Horiuchi 2005, Table 5.4, 86).[18] 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; see also 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.

Voting from Nonresidential Addresses

Before shifting from qualitative to quantitative analysis to obtain systematic evidence of preelectoral residential registration, we want to emphasize that preelectoral 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 United States. 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). 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" (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 (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 (174).

There are many other stories outside the United States. In Britain, the residency requirement for electors was not enforced from the 15th to the 18th century even before it was formally repealed in 1773 (Hutcheson 1997). In the first half of the twentieth century,

[15] Nyblade and Reed (2008, 928) and Lehoucq and Molina (2002) also present similar views.

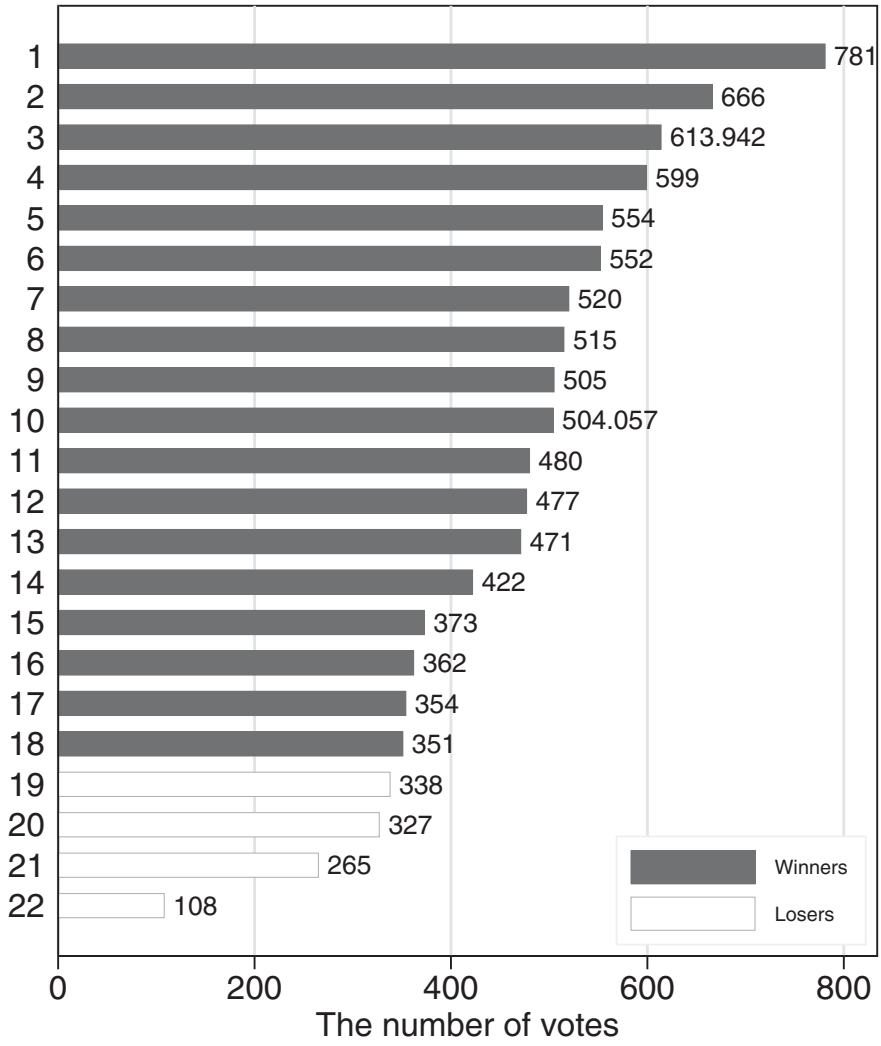
[16] The mayoral election system is essentially the same, except that the number of seats per district is one.

[17] 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.

[18] If, in the m member district, the m-th and $(m+1)$-th candidates receive the exactly same number of votes, the winner is decided by a random draw. Horiuchi (2005, 84) found three cases in his sample of 442 districts.

https://doi.org/10.1017/S0003055411000268 Published online by Cambridge University Press

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%.

Costa Rican parties allegedly "packed the electoral registry with dead or nonexistent individuals" or those who "lacked a permanent residence" (Lehoucq and Molina 2002, 40, 99). They also transported railroad employees to districts where 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 document fraud, so that they could vote for UMNO (Sadiq 2005). Similarly, some illegal immigrants from Afghanistan were registered to vote in Pakistan, and similar movements occurred from Bangladesh to India, from Nigeria to Cameroon, from Togo to Ghana, and from Burkina Faso to the Ivory Coast (Sadiq 2005, 103). Preelectoral residential registration is reportedly found in Korea as well.[19] In the Solomon Islands, many residents were allegedly imported from island to island to be registered and vote.[20] In Australia, the Electoral Commission compiled a list of all suspected cases of electoral fraud for the decade 1990–2000 and found that one of the major types of fraud was false enrollment transferring the principal place of residence, aimed at affecting election outcomes.[21]

NATURAL EXPERIMENTAL DESIGN AND HYPOTHESES

The previous section suggests that preelectoral residential registration or voting from nonresidential addresses may be rampant, whereas we suspect that the crime statistics underrepresent reality. Then how do

[19] *Asahi Shimbun*, April 9, 1988.

[20] *Solomon Star*, August 9, 2010.

[21] Joint Standing Committee on Electoral Matters, the Parliament of Australia (2001).

https://doi.org/10.1017/S0003055411000268 Published online by Cambridge University Press

we know how many people actually are engaged in this behavior? To answer this, we need to cross an important hurdle—the difficulty of identifying whether changing one's address (regardless of actual residence) is motivated by electoral fraud or not. Thanks to a natural experimental setup in Japan, we can cope with this problem and make valid causal inferences.

In this section, we first review some existing statistical studies to detect electoral fraud and argue the advantages 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 timing of preelectoral residential registration. Finally, we propose another hypothesis about how competitiveness affects its magnitude.

Existing Approaches to Detecting Electoral Fraud

In the literature on 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 the 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 that require further investigation. Once readers consider, however, the existence of relevant omitted variables,[22] the misspecification of functional 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).

[22] 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 nonwhite 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 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.

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.[23] For example, Hyde (2007) focuses on the 2003 Armenian presidential election and compares the incumbent's vote shares at polling stations that international election observers visited (the treatment group) with those at the other polling stations they did not visit (the control group). If we assume, as Hyde does, that the observers were 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 be 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 used the notorious butterfly ballot) with absentee voting (which did not) as a natural experiment, though this treatment variable is "not random assignment," as the authors themselves admit.[24] 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 were 3,210 municipalities (i.e., cities, special wards, towns, and villages) in 47 prefectures and each municipality votes for 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. Because their terms were 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.

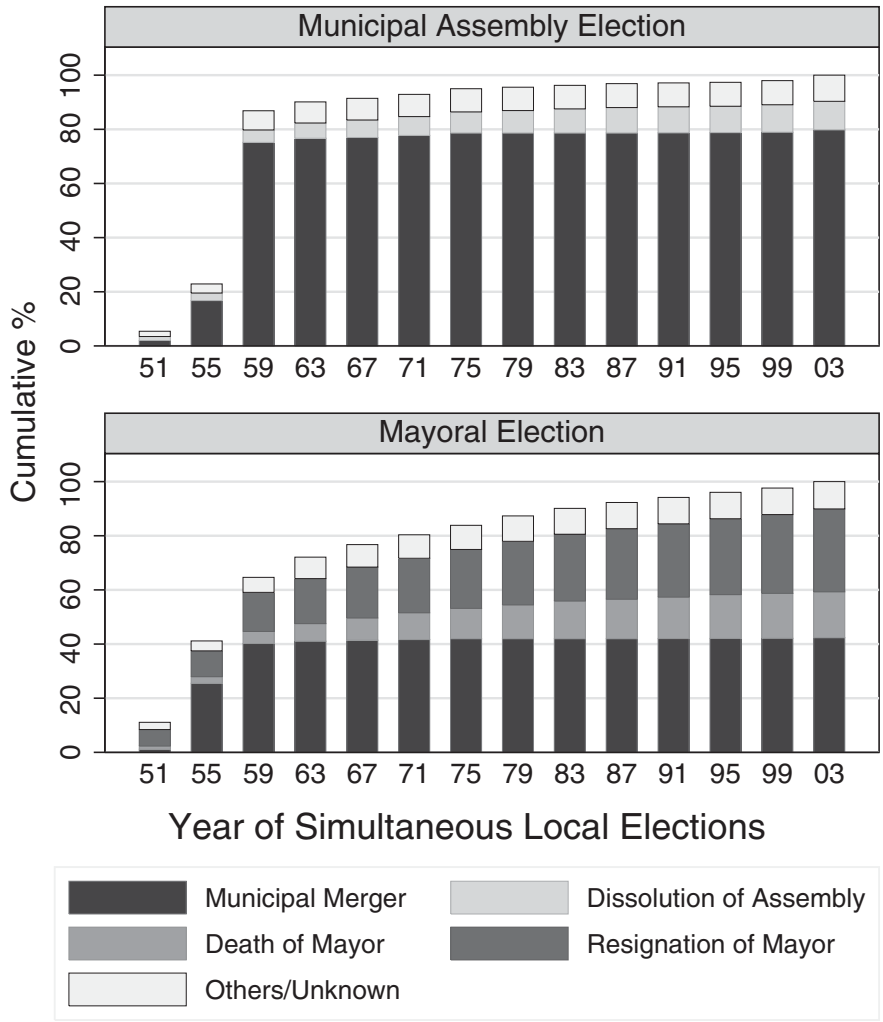
Specifically, in the case of the 2003 SLEs, among 3,105 municipalities that we study, 51.6% of

[23] Another non–model based method is to apply Benford's Law (Mebane 2008) or to pay attention to the frequency of ultraclose races (Christensen and Colvin 2009).

[24] Because this key assumption is difficult to satisfy, a natural experiment has rarely been 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 for studying electoral fraud.

https://doi.org/10.1017/S0003055411000268 Published online by Cambridge University Press

FIGURE 2. When and Why Did Municipalities Drop from the Simultaneous Local Elections?



Note: The figures show the cumulative percentages of "control" observations—municipalities that 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.

municipalities held assembly and 21.4% mayoral elections on April 27, 2003.[25] We call 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. The denominator of each

[25] 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 fourth Sunday of April. Although 3,210 municipalities existed as of April 27, 2003, we exclude the following from analysis. First, we drop all municipalities in Okinawa prefecture, 15 municipalities in Kagoshima prefecture, and the village of Ogasawara in the 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 reclamation in 1964 and thus was off the normal election cycle from the beginning. The third set of municipalities excluded from analysis are the 13 large cities designated by ordinance, as their elections were scheduled on the second (not the fourth) Sunday of April every fourth year and their residential registration system is slightly different from the one commonly used in the other municipalities. Finally, the 23 special wards in Tokyo metropolitan area are not used, because their mayors were not elected but appointed between 1952 and 1974. 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 (Horiuchi 2009). 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.

https://doi.org/10.1017/S0003055411000268 Published online by Cambridge University Press

bar graph is the total number of control municipalities whose timing of deviation is identified ($N = 1{,}438$ for municipal assembly elections and 2,398 for mayoral elections).[26] Figure 2 shows that more than 60% of those municipalities (86.9% for municipal assembly elections and 64.6% 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 four years have passed since the previous SLEs.[27] Indeed, municipal merger is the most common reason for municipalities to drop out of the SLE cycle (79.8% and 42.4% 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.

The other major reasons for deviation are assembly dissolution (10.5%), mayoral resignation (30.6%), and the death of an incumbent mayor (16.9%).[28] It is extremely unlikely that these long-past events, or the factors that 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 that predicts the direction of the effects of merger, death, resignation, or dissolution 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.[29]

Hypothesis 1: Timing

We suspect that preelectoral residential registration occurred in January 2003 for an April election because of the following two institutions. First, all eligible voters need to have their addresses registered in a district for more than three consecutive months before the election. In the case of the 2003 SLE, which was held on April 27, 2003, in order to vote in elections, people had to change their residential registration *before* January 19 (in cities) or 21 (in towns and villages), 2003.[30] Second, residents must pay a residential tax to the municipalities of their residential addresses (at least in the public record) as of January 1 in each year. Any change of their registered addresses is reported to the tax office and their employer, who withholds taxes from their salary and pays them on their behalf. Because "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 addresses 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 movement to the treatment group in January 2003 is fraudulent. If we observe, however, a significant *difference* between the treatment and control groups *only* in January 2003, we are inclined to believe that it is due to preelectoral residential registration.

Hypothesis 2: Competitiveness

In the previous section, we argued that electoral competitiveness encourages preelectoral residential registration. One way to confirm this is to compare the size of treatment effects by municipality population size, according to which electoral competitiveness varies. Article 91 of the Local Autonomy Act stipulates that the maximum municipal assembly size is a nonlinear step function of municipal population size, which can be roughly approximated by a natural log of the population size. For example, the maximum number of seats is 12 for municipalities with less than 2,000 residents, whereas 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

[26] We tried to obtain the official records of elections and/or municipality history books by contacting the national government, all 47 prefectures, more than one thousand municipal election committees, and more than one hundred libraries. We could not, however, obtain information about the timing of deviation for 66 municipal assembly elections and 43 mayoral elections. For 33 municipal assembly elections and 132 mayoral elections, we could identify the timing of deviation but not the reasons for it. For details of the timing and reasons of deviation, see Fukumoto and Ueki (2011).

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

[28] For other miscellaneous reasons, 7.3% (4.6%) of legislative (executive) control municipalities deviated from the SLE cycle.

[29] Note that we do not claim that the election timing is *completely* at random (e.g., randomly assigned by the central government).

[30] Article 9, POEA. The deadlines are not exactly three months before because of procedural issues.

https://doi.org/10.1017/S0003055411000268 Published online by Cambridge University Press

the average number of votes each legislator represents, tends to decrease (Horiuchi 2005, Table 5.4, 86).[31] 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.[32]

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

STATISTICAL METHOD

This section introduces our statistical method for detecting preelectoral residential registration. We first specify a differences-in-differences model and give a rationale for applying this model to our analysis. Second, we clarify the prediction of coefficient estimates. Finally, we create four different data sets to address heterogeneous treatment effects.

Differences-in-differences Model

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

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

The unit of observation is municipality i in month m, year t (January 2001 to December 2004).[33] $Y_{i,t,m}$ is the natural logarithm of the number of relocated residential registrations (W) plus one (i.e., $\log(W+1)$).[34] The dummy variable $D_{t,m}$ indicates whether the observation is for month m in year t, whereas its coefficient $f_{t,m}$ represents that month's fixed effect. These month–year fixed effects control the national average number of relocated residential registrations, 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 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 the 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 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 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.

[31] 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, however, is 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.

[32] Alternatively, we can divide observations by setting some arbitrary cutoff points for the population size. Because 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.

[33] 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 of an assembly member is four years. See also footnote 25.

[34] The data source is Sômushô Tôkei Kyoku (2001–2004). 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 postmerger 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 substantial manner. We do not use moving-out population, simply because similar municipality–month panel data are unavailable.

https://doi.org/10.1017/S0003055411000268 Published online by Cambridge University Press

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

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

By subtracting the second equation from the first, we obtain

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

where Δ 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 differences-in-differences model is that it controls all time-invariant municipality-specific characteristics (c_i) as well as all municipality-specific seasonal effects ($b_{i,m}$).[35] Moreover, it is extremely unlikely that any short-term changes in demographic, economic, and social characteristics during the 2000s that may affect the outcome variable (i.e., $\Delta e_{i,t,m}$) 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 $E(\mathrm{LT}_i \cdot \Delta e_{i,t,m}) = 0$ (where $E(.)$ is the average function), and we obtain unbiased estimates of the causal effects ($\Delta(g_{t,m})$) by using only 36-month dummies ($D_{t,m}$) and their 36 interaction terms with our treatment variable (LT_i) as regressors.[36]

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

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

where the executive treatment variable ET_i is a dummy for 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.

[35] Running a standard OLS regression with all these fixed effects (i.e., the first equation) is possible, but it would become computationally intensive, as our specification has numerous (more than 36,000) dummy variables.

[36] In our preliminary analysis, we also estimated the treatment effects with some pretreatment variables controlled, but the substantive implications do not change.

Prediction

We focus on 36 (= 12 months × 3 years) $\Delta(g_{t,m})$'s and $\Delta(h_{t,m})$'s. Because 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:

$$\Delta(g_{2003,1}) \equiv g_{2003,1} - g_{2002,1} = g_{2003,1} - 0 > 0$$
$$\Delta(h_{2003,1}) \equiv h_{2003,1} - h_{2002,1} = h_{2003,1} - 0 > 0$$
$$\Delta(g_{2004,1}) \equiv g_{2004,1} - g_{2003,1} = 0 - g_{2003,1} < 0$$
$$\Delta(h_{2004,1}) \equiv h_{2004,1} - h_{2003,1} = 0 - h_{2003,1} < 0$$

and all the other $\Delta(g_{t,m})$'s and $\Delta(h_{t,m})$'s are zero.[37]

To test Hypothesis 2, we split the whole sample into a town and village subsample and a city one, and estimate the treatment effects 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 Data Sets

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., $\mathrm{ET}_i = 0$), compared to when there is a mayoral election (i.e., $\mathrm{ET}_i = 1$). 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 data set in two in two ways and estimate average treatment effects for four data sets; (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, $N = 1{,}601$).

More formally, we estimate the following:

$$\Delta(g_{t,m})|_{\mathrm{ET}_i=0} \equiv E(\Delta Y_{i,t,m}|_{\mathrm{LT}_i=1,\ \mathrm{ET}_i=0}) - E(\Delta Y_{i,t,m}|_{\mathrm{LT}_i=0,\mathrm{ET}_i=0}) \quad \textbf{(1)}$$

$$\Delta(g_{t,m})|_{\mathrm{ET}_i=1} \equiv E(\Delta Y_{i,t,m}|_{\mathrm{LT}_i=1,\ \mathrm{ET}_i=1}) - E(\Delta Y_{i,t,m}|_{\mathrm{LT}_i=0,\mathrm{ET}_i=1}) \quad \textbf{(2)}$$

[37] These coefficients work as placebo tests.

https://doi.org/10.1017/S0003055411000268 Published online by Cambridge University Press

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 ($N = 1{,}311$)

$$\Delta(h_{t,m})|_{\mathrm{LT}_i=0} \equiv E\left(\Delta Y_{i,t,m}|_{\mathrm{ET}_i=1,\ \mathrm{LT}_i=0}\right) - E\left(\Delta Y_{i,t,m}|_{\mathrm{ET}_i=0,\ \mathrm{LT}_i=0}\right) \quad \textbf{(3)}$$

$$\Delta(h_{t,m})|_{\mathrm{LT}_i=1} \equiv E\left(\Delta Y_{i,t,m}|_{\mathrm{ET}_i=1,\ \mathrm{LT}_i=1}\right) - E\left(\Delta Y_{i,t,m}|_{\mathrm{ET}_i=0,\ \mathrm{LT}_i=1}\right). \quad \textbf{(4)}$$

RESULTS

Figure 3 shows estimates of the differentiated legislative treatment effects in the executive control group (the first of the four data sets), $\Delta(g_{t,m})|_{\mathrm{ET}_i=0}$, 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), whereas most of the other thirty-four $\Delta(g_{t,m})$'s are not significant.[38] We can thus infer that $g_{2003,1}$ is positive whereas others are zero, which is consistent 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 4 illustrates the corresponding estimates for the executive treatment group (the second data set), $\Delta(g_{t,m})|_{\mathrm{ET}_i=1}$. Compared to Figure 3, Figure 4 shows larger confidence intervals. This is because of 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 a 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 5 indicates the estimates of the differentiated executive treatment effects for the legislative control group (the third data set), $\Delta(h_{t,m})|_{\mathrm{LT}_i=0}$. 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 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 because of the small size ($N = 193$) of the treatment group in this data set, the patterns may suggest some validity of Hypotheses 1 and 2.

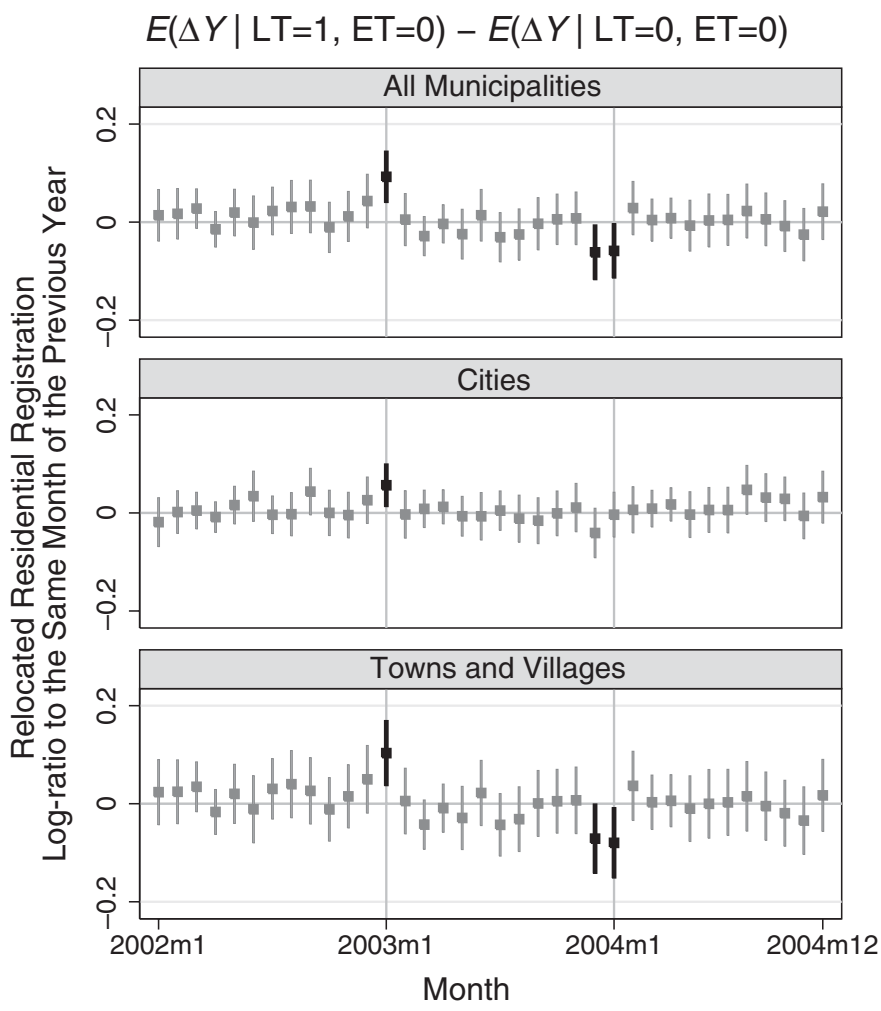
Figure 6 shows the differentiated executive treatment effects of the legislative treatment group (the fourth data set), $\Delta(h_{t,m})|_{\mathrm{LT}_i=1}$. For all municipalities (the top panel), the estimates are positive and significant for January 2003 and negative and significant for January 2004. The coefficients for all other months are insignificant. This is consistent with Hypothesis 1. Whereas 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.

Overall, the estimates largely support 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

[38] An exception is December 2003 (negative), which is significant at the 1% level. With the 5% level, the coefficient for December 2002 (positive) also becomes significant. This may suggest that people who are engaged in preelectoral 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 offices and their employers. This pattern is not observed when the other three data sets are used (i.e., Figures 4–6).

https://doi.org/10.1017/S0003055411000268 Published online by Cambridge University Press

FIGURE 3. Legislative Treatment Effects in Executive Control Municipalities



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

data sets) estimates. A combination of these suggests the validity of Hypothesis 1.

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 preelectoral residential registration is more common in smaller municipalities. This supports our Hypothesis 2.

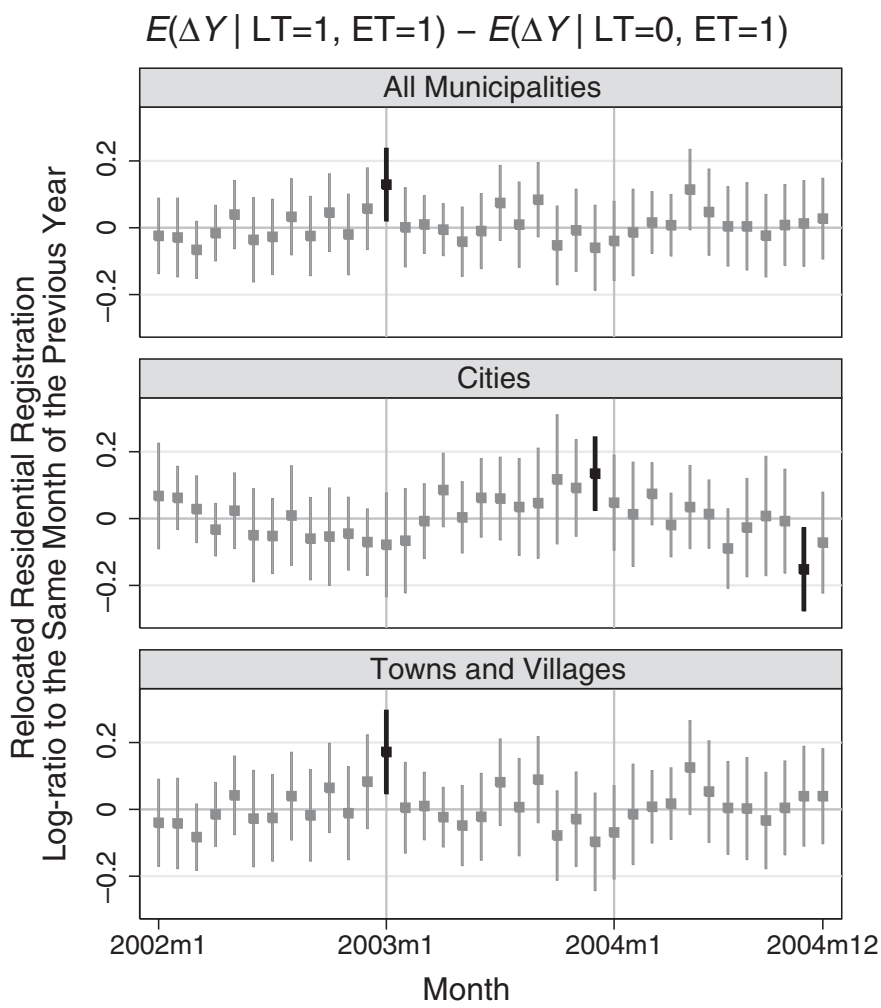
DISCUSSION

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 registrations from January 2001 to December 2004 is 111. The point estimate of the differentiated legislative treatment effect in January 2003 is, for example, about 9.6% ($= 100 \times \exp(0.092) - 100$) if a mayoral election is not held (the top panel in Figure 3). Thus, as many as an extra 11 ($= 111 \times 0.096$) 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 also argue that the estimated sharp increase in the number of residential registrations in January 2003 reflects an increase in the number of "early movers" (who changed their actual residence *before* that month but did not transfer their registered

https://doi.org/10.1017/S0003055411000268 Published online by Cambridge University Press

FIGURE 4. Legislative Treatment Effects in Executive Treatment Municipalities



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

address until an election in April 2003 prompted them to do so) and/or an increase in the number of "real movers" (who changed their actual residence *in* that month),[39] rather than "paper movers" (who did *not* change their actual residence *by* that month). Subsequently, we show some evidence that substantial numbers of paper movers are likely, whereas early and real movers are highly unlikely to occur. We also explain that early and real moving are costly scenarios and early moving is even illegal.

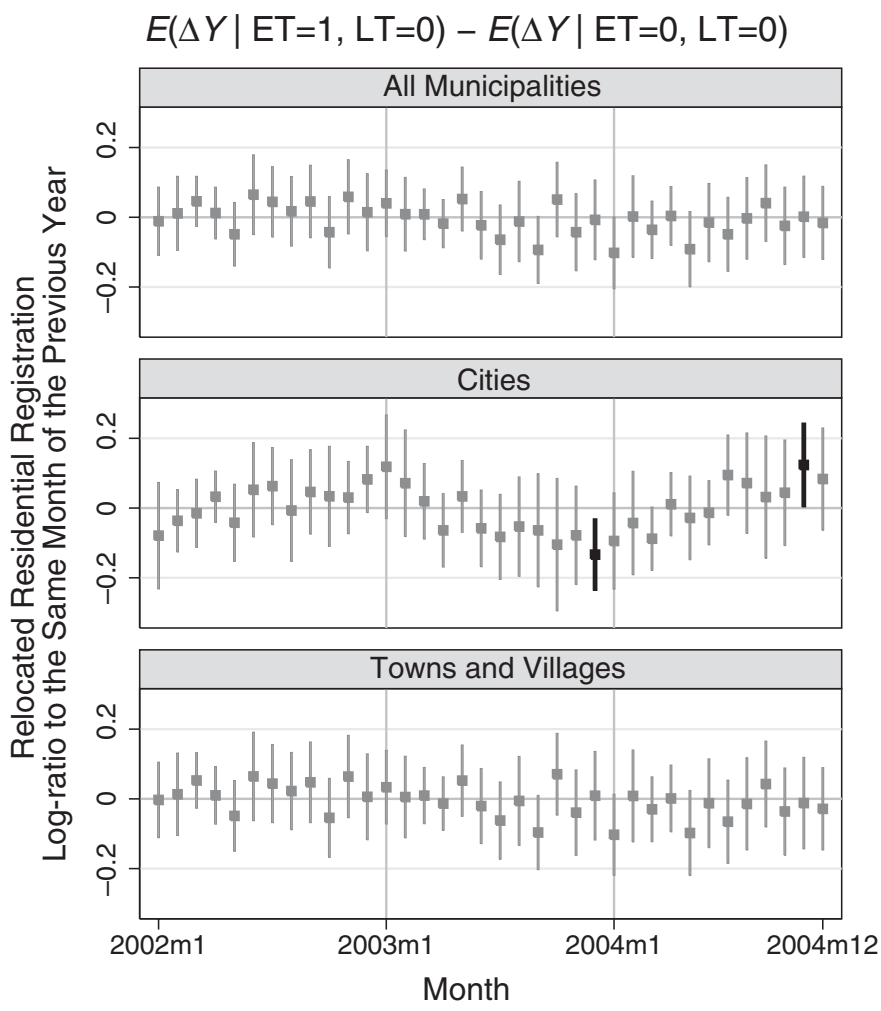
When municipalities are suspicious of an abnormal increase in the number of residential registrations before an election, they sometimes investigate whether (all or suspicious) "new residents" actually reside in their municipalities. We find eight cases where a substantial portion (33% to 84%) of them turned out to be paper movers.[40] By contrast, no news articles listed in the Appendix mention the existence of early

[39] To be exact, if citizens moved their actual residences between December 15, 2002 and January 19 (in cities) or 21 (in towns and villages), 2003, changed their residential addresses between January 4 and 19 (or 21), and continued to live there substantially until April 27, they are recorded as January movers and can legally vote in their new municipalities on April 27. (Note that municipality offices are closed between December 29 and January 3.)

[40] These are 84% (= 135/161) in Kanmaki town, Nara prefecture in 1991 (*Asahi Shimbun*, March 12, 1992), 70% (= 211/303) in Ikata town, Ehime prefecture in 1987 (*Ehime Shimbun*, February 1 and April 19, 1987), 63% (= 81/128) in Meguro ward, Tokyo metropolis on January 17, 1987 (*Asahi Shimbun*, March 29, 1987), 62% (= 235/380) in Yamanakako village, Yamanashi prefecture in 1991 (*Asahi Shimbun*, March 12, 1991), 43% (= 93/214) in Hayakawa town, Yamanashi prefecture in 1977 (*Asahi Shimbun*, August 27, 1977), 42% (= 83 of about 200) in Akehama town, Ehime prefecture in 1978 (*Ehime Shimbun*, February 15, 1987), 38% (= 40/106) in Kuriyama village, Tochigi prefecture in 1995 (*Asahi Shimbun*, July 31, August 4, September 28 and 29, 1995), and 33% (about 20 of about 60) in Suki village, Miyazaki prefecture in 1995 (*Asahi Shimbun*, May 12, 1995). These ratios should be seen as underestimates of fraudulent registrations, because the numerators only include confirmed fraudulent cases (i.e., excluding cases impossible to determine even after investigation).

https://doi.org/10.1017/S0003055411000268 Published online by Cambridge University Press

FIGURE 5. Executive Treatment Effects in Legislative Control Municipalities



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

or real movers. Some even report that most suspect citizens changed their residential registrations back to the original municipalities they came from, as soon as the municipal authorities started conducting in-depth interviews and sent a warning. These "quick returners" as good as admitted that they were paper movers.

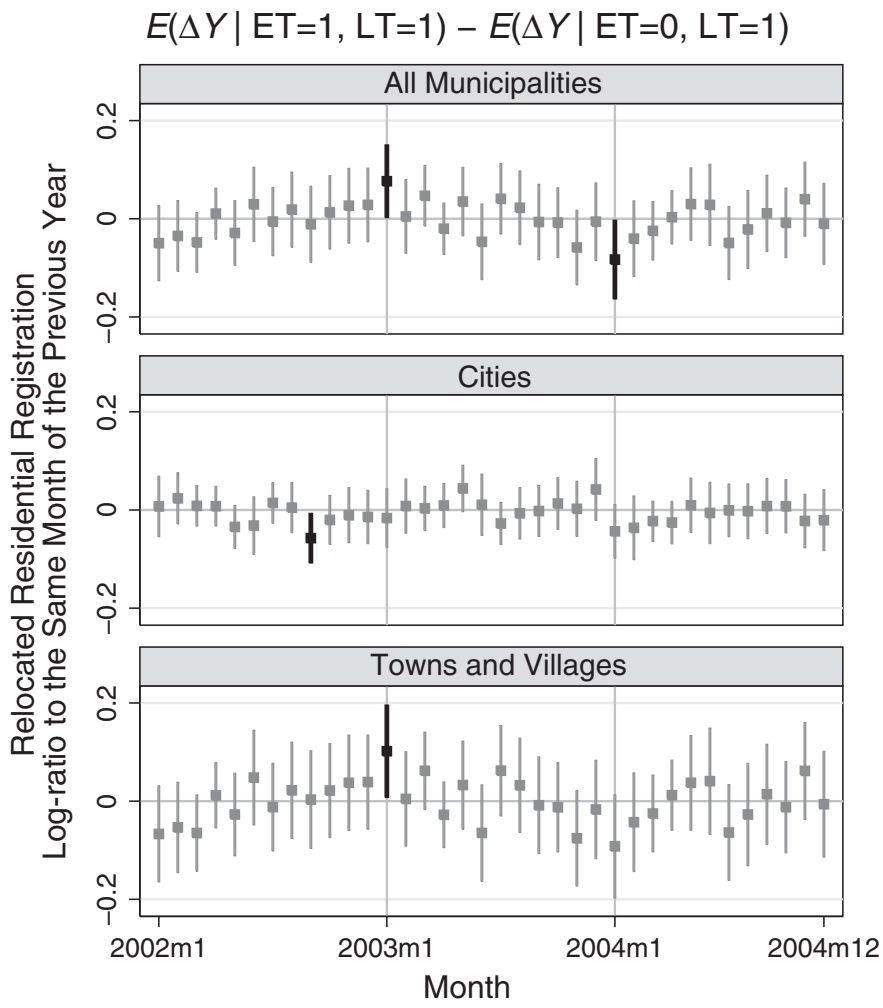
Early and real movers would have to incur many direct and indirect costs. Depending on where people are registered to live, rather than where they physically reside, they send their children to public schools and child care centers, receive allowances for dependent children, have claims for health insurance if self-employed, have claims for nursing care insurance, and access public health care, etc.[41] Therefore, early moving would be enormously inconvenient and expensive. Real movers would have to physically move their belongings, as well as cancelling and setting up all public services and utility contracts during one of the most important holiday seasons for Japanese people (and they would then have to repeat the same actions again if they returned to their previous addresses after the election). They could well have to take a three-month leave from their work/school and/or find a temporary job/school in the relocated municipality as well. Considering how rarely preelectoral residential registration is caught, these scenarios are highly unlikely.

We also call attention to the fact that early moving would be illegal. Because they are late for the deadline of registration, they have violated the registration

[41] This linking of government services to residential addresses is a major reason that those who undertake preelectoral residential registration are sometimes (reported to be) 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). Sometimes only one parent is engaged in preelectoral residential registration. 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 gaining benefits from candidates or core supporters.

https://doi.org/10.1017/S0003055411000268 Published online by Cambridge University Press

FIGURE 6. Executive Treatment Effects in Legislative Treatment Municipalities



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

law.[42] Moreover, we strongly doubt that they honestly report their actual dates of moving and pay the fine (up to 50,000 Japanese yen, equivalent to about 600 U.S. dollars), because the burden of proof rests with the applicants. We thus expect that they lie about the dates, but if they do so, they commit the criminal offence of making false public records as well.[43]

In sum, we argue that paper movers are most likely to exist, whereas early and real movers are possible but highly unlikely. It is important to remember that early moving is in fact illegal. And if real moving were to exist by any chance, it would be indeed very curious and unusual behavior.

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 preelectoral residential registration—the cross-municipality change of registered address *for the purpose of voting*. Our differences-in-differences estimates suggest that people do, indeed, move (most likely, only 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. As argued, this pattern of voting behavior is a variant of a classic type of electoral fraud. The practice is common in many countries, though we are the first to provide robust inference of this fraudulent behavior.

We believe that our study, with a focus on Japan, paves the way for further development of comparative studies on electoral fraud, which we believe is an effective way to advance a longstanding policy debate—how to design an optimal registration system to discourage fraudulent behavior while still encouraging genuine electoral participation. To balance costs (illegal participation) and benefits (legal participation), we need to

[42] See footnote 1.
[43] Article 157, Criminal Act.

https://doi.org/10.1017/S0003055411000268 Published online by Cambridge University Press

TABLE 2. Summary of Estimated Treatment Effects

Year/Month	Municipalities Included	Treatment Effect	Coefficient Estimate	Standard Error
January 2003 ($t = 2003$, $m = 1$)	All	$\Delta(g_{t,m}) \mid_{\mathrm{ET}_i = 0}$	0.092	0.020**
		$\Delta(g_{t,m}) \mid_{\mathrm{ET}_i = 1}$	0.129	0.042**
		$\Delta(h_{t,m}) \mid_{\mathrm{LT}_i = 0}$	0.040	0.037
		$\Delta(h_{t,m}) \mid_{\mathrm{LT}_i = 1}$	0.076	0.029**
	Cities	$\Delta(g_{t,m}) \mid_{\mathrm{ET}_i = 0}$	0.056	0.017**
		$\Delta(g_{t,m}) \mid_{\mathrm{ET}_i = 1}$	−0.079	0.060
		$\Delta(h_{t,m}) \mid_{\mathrm{LT}_i = 0}$	0.118	0.058*
		$\Delta(h_{t,m}) \mid_{\mathrm{LT}_i = 1}$	−0.017	0.023
	Towns and villages	$\Delta(g_{t,m}) \mid_{\mathrm{ET}_i = 0}$	0.103	0.026**
		$\Delta(g_{t,m}) \mid_{\mathrm{ET}_i = 1}$	0.172	0.049**
		$\Delta(h_{t,m}) \mid_{\mathrm{LT}_i = 0}$	0.033	0.041
		$\Delta(h_{t,m}) \mid_{\mathrm{LT}_i = 1}$	0.102	0.037**
January 2004 ($t = 2004$, $m = 1$)	All	$\Delta(g_{t,m}) \mid_{\mathrm{ET}_i = 0}$	−0.059	0.022**
		$\Delta(g_{t,m}) \mid_{\mathrm{ET}_i = 1}$	−0.040	0.046
		$\Delta(h_{t,m}) \mid_{\mathrm{LT}_i = 0}$	−0.102	0.040*
		$\Delta(h_{t,m}) \mid_{\mathrm{LT}_i = 1}$	−0.083	0.031**
	Cities	$\Delta(g_{t,m}) \mid_{\mathrm{ET}_i = 0}$	−0.004	0.018
		$\Delta(g_{t,m}) \mid_{\mathrm{ET}_i = 1}$	0.047	0.055
		$\Delta(h_{t,m}) \mid_{\mathrm{LT}_i = 0}$	−0.094	0.054
		$\Delta(h_{t,m}) \mid_{\mathrm{LT}_i = 1}$	−0.044	0.021*
	Towns and villages	$\Delta(g_{t,m}) \mid_{\mathrm{ET}_i = 0}$	−0.080	0.028**
		$\Delta(g_{t,m}) \mid_{\mathrm{ET}_i = 1}$	−0.069	0.054
		$\Delta(h_{t,m}) \mid_{\mathrm{LT}_i = 0}$	−0.103	0.045*
		$\Delta(h_{t,m}) \mid_{\mathrm{LT}_i = 1}$	−0.092	0.041*

Note: $\Delta(g_{t,m}) \mid_{\mathrm{ET}_i = j}$, a conditional legislative treatment effect. $\Delta(h_{t,m}) \mid_{\mathrm{LT}_i = j}$, 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).

investigate both types of behavior, whereas previous endeavors have focused predominantly on the latter. For systematic and comparative inquiry into fraudulent behavior in elections, we argue that our approach—electoral timing as treatment—may serve as a useful tool that can be applied to a number of other contexts.

One example is local elections. In G7 countries, except France, not all subnational governments of the same level hold elections concurrently. To examine the United States in detail, 40 gubernatorial elections have departed from the presidential cycle in various years (Moore, Preimesberger, and Tarr 2001, 1378–79 and chap. 30). Four states 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.[44] This is the case for the senates of the United States, France, the Czech Republic, and 26 American states, as well as some American city councils and French department assemblies.[45]

If we can regard the election timing as an as-if randomly assigned treatment, as we do in this article, the setup becomes a natural experiment. The senates of the United States and France are promising examples. Even if not, however, electoral timing is still worth exploiting as long as we can control relevant pretreatment variables by matching or any other proper statistical method. We also emphasize that the outcome variables that can be explained by the timing of elections are not only (abnormal) population movements. The timing of elections can explain many other variables that politicians' mobilization efforts might affect. They include pork-barrel expenditures and voter turnout in other levels of elections (Fukumoto and Horiuchi 2009; Fukumoto, Horiuchi, and Tanaka 2011).

To conclude, we hope that future scholars 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 in many other electoral contexts. "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.

[44] By taking advantage of this setup, Shepsle et al. (2009) show that U.S. senators provide more appropriations for states with up-for-reelection senators than states without them.

[45] Based on all state homepages (accessed in January and February 2011) and Senate, Parliament of the Czech Republic (2011). We also refer to Dubin (2007) and MacManus (1999, 169–70, 174).

https://doi.org/10.1017/S0003055411000268 Published online by Cambridge University Press

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
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
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

L = municipality assembly, E = mayor, P = prefecture assembly, G = governor, N = national Diet.

REFERENCES

Alvarez, R. Michael, Thad E. Hall, and Susan D. Hyde. 2008. *Election Fraud: Detecting and Deterring Electoral Manipulation.* Washington, DC: Brookings Institution Press.

Alvarez, R. Michael, and Jonathan N. Katz. 2008. "The Case of the 2002 General Election." In *Election Fraud: Detecting and Deterring Electoral Manipulation*, eds. R. Michael Alvarez, Thad E. Hall, and Susan D. Hyde. Washington, DC: Brookings Institution Press, 149–61.

https://doi.org/10.1017/S0003055411000268 Published online by Cambridge University Press

Birch, Sarah. 2007. "Electoral Systems and Election Misconduct." *Comparative Political Studies* 40 (12): 1533–56.
Campbell, Angus, Philip E. Converse, Warren E. Miller, and Donald E. Stokes. 1960. *The American Voter.* New York: Wiley.
Campbell, Tracy. 2005. *Deliver the Vote: A History of Election Fraud, an American Political Tradition–1742–2004*. New York: Carroll and Graf.
Chang, Eric, and Miriam Golden. 2006. "Electoral Systems, District Magnitude and Corruption." *British Journal of Political Science* 37 (1): 115–37.
Christensen, Ray, and Kyle Colvin. 2009. "Stealing Elections: A Comparison of Election-night Corruption in Japan, Canada, and the United States." In *Political Change in Japan: Electoral Behavior, Party Realignment, and the Koizumi Reforms*, eds. Steven R. Reed, Kenneth Mori McElwain, and Kay Shimizu. Stanford, CA: Shorenstein Asia–Pacific Research Center, 199–218.
Converse, Philip E. 1972. "Change in American Electorate." In *The Human Meaning of Social Change*, eds. Angus Campbell and Philip E. Converse. New York: Russell Sage Foundation, 263–337.
Cox, Gray W., and J. Morgan Kousser. 1981. "Turnout and Rural Corruption: New York as a Test Case." *American Journal of Political Science* 25 (4): 646–63.
Dai 4 Ji Senkyo Seido Shingikai. 1966. *Senkyo no Tetsuzuki ni Kansuru Shôiinkai Iinchô Chûkan Hôkoku Yôshi* [Chairman's Interim Report Outline of Subcommittee on Electoral Procedures]. February 15.
Dubin, Michael J. 2007. *Party Affiliations in the State Legislatures: A Year by Year Summary, 1976–2006*. Jefferson, NC: McFarland.
Dunning, Thad. 2008. "Improving Causal Inference: Strengths and Limitations of Natural Experiments." *Political Research Quarterly* 61 (2): 282–93.
Fisher, J. A. 1999. "The Probability of Being Decisive." *Public Choice* 101: 267–83.
Fukumoto, Kentaro, and Yusaku Horiuchi. 2009. "Mobilization and Participation: A Natural Experiment." Presented at the Annual Summer Meeting of the Society for Political Methodology, New Haven, CT.
Fukumoto, Kentaro, Yusaku Horiuchi, and Shoichiro Tanaka. 2011. "Treated Politicians, Treated Voters: A Natural Experiment to Estimate Electoral Effects on Fiscal Expenditure." Presented at the Annual Meeting of the Midwest Political Science Association, Chicago.
Fukumoto, Kentaro, and Futoshi Ueki. 2011. "Shichôson Senkyo ga Tôitsu Chihô Senkyo kara Itsudatsu Shita Jiki to Riyû." ["When and Why Did Municipalities Drop from the Simultaneous Local Elections?"] Gakushuin University. Unpublished Manuscript.
Horiuchi, Yusaku. 2005. *Institutions, Incentives and Electoral Participation in Japan: Cross-level and Cross-national Perspectives.* London: Routledge Curzon.
Horiuchi, Yusaku. 2009. "Understanding Japanese Politics from a Local Perspective." *International Political Science Review* 30 (5): 565–73.
Hutcheson, John A., Jr. 1997. "Elections and the Franchise." In *Britain in the Hanoverian Age, 1714–1837: An Encyclopedia*, eds. Gerald Newman and Leslie Ellen Brown. New York: Garland Publishing, 222–24.
Hyde, Susan D. 2007. "The Observer Effect in International Politics: Evidence from a Natural Experiment." *World Politics* 60 (1): 37–63.
Hyde, Susan D. 2008. "How International Election Observers Detect and Deter Fraud." In *Election Fraud: Detective and Deterring Electoral Manipulation*, eds. R. Michael Alvarez, Thad E. Hall, and Susan D. Hyde. Washington, DC: Brookings Institution Press, 201–15.
Joint Standing Committee on Electoral Matters, the Parliament of Australia. 2001. *User Friendly, Not Abuser Friendly: Report of the Inquiry into the Integrity of the Electoral Roll.* http://www.aph.gov.au/house/committee/em/ElecRoll/Report.htm (accessed June 10, 2011).
Keisatsuchô. 2003. *Hanzai Tôkeisho* [Criminal Statistics].
Lehoucq, Fabrice. 2003. "Electoral Fraud: Causes, Types and Consequences." *Annual Review of Political Science* 6: 233–56.
Lehoucq, Fabrice E., and Ivan Molina. 2002. *Stuffing the Ballot Box: Fraud, Electoral Reform, and Democratization in Costa Rica.* Cambridge: Cambridge University Press.
MacManus, Susan. 1999. "The Resurgent City Councils." In *American State and Local Politics: Directions for the 21st Century*, eds. Ronald E. Weber and Paul Brace. New York: Seven Bridges, 166–93.
Mayhew, David R. 1974. *Congress: The Electoral Connection.* New Haven, CT: Yale University Press.
Mebane, Walter R. 2008. "Election Forensics: The Second-digit Benford's Law Test and Recent American Presidential Elections." In *Election Fraud: Detecting and Deterring Electoral Manipulation*, eds. R. Michael Alvarez, Thad E. Hall, and Susan D. Hyde. Washington, DC: Brookings Institution Press, 162–81.
Minnite, Lorraine C. 2010. *The Myth of Voter Fraud*. Ithaca, NY: Cornell University Press.
Moore, John L., Jon P. Preimesberger, and David R. Tarr, eds. 2001. *Congressional Quarterly's Guide to U.S. Elections*. Washington, DC: Congressional Quarterly.
Myagkov, Mikhail, Peter C. Ordeshook, and Dimitri Shakin. 2009. *The Forensics of Election Fraud: Russia and Ukraine.* Cambridge: Cambridge University Press.
Nyblade, Benjamin, and Steven R. Reed. 2008. "Who Cheats? Who Loots? Political Competition and Corruption in Japan, 1947–1993." *American Journal of Political Science* 52 (4): 926–41.
Persson, Torsten, Guido Tabellini, and Francesco Trebbi. 2003. "Electoral Rules and Corruption." *Journal of the European Economic Association* 1 (4): 958–89.
Piven, Frances Fox, and Richard A. Cloward. 2000. *Why Americans Still Don't Vote: And Why Politicians Want It That Way*. Rev. and updated ed. Boston: Beacon.
Riker, William H., and Peter C. Ordeshook. 1968. "A Theory of the Calculus of Voting." *American Political Science Review* 62 (1): 25–42.
Rosenstone, Steven J., and John Mark Hansen. 1993. *Mobilization, Participation, and Democracy in America*. New York: Macmillan.
Sadiq, Kamal. 2005. "When States Prefer Non-citizens over Citizens: Conflict over Illegal Immigration into Malaysia."*International Studies Quarterly* 49 (1): 101–22.
Senate, Parliament of the Czech Republic. 2011. "How Are Senators Elected?" http://www.senat.cz/volby/ (accessed February 18, 2011).
Shepsle, Kenneth A., Robert P. Van Houweling, Samuel J. Abrams, and Peter C. Hanson. 2009. "The Senate Electoral Cycle and Bicameral Appropriations Politics." *American Journal of Political Science* 53 (2): 343–59.
Sômushô Jichigyôsei Kyoku Senkyo Bu. 2003. *Chihô Senkyo Kekka Shirabe* [Survey of the Local Election Results].
Sômushô Tôkei Kyoku. 2001–2004. *Jûmin Kihon Daichô Jinkô Idô Hôkoku* [Report of Population Migration in Residential Basic Book]. http://www.e-stat.go.jp/SG1/estat/List.do?lid=000001012090 (accessed June 12, 2009).
Wand, Jonathan N., Kenneth W. Shotts, Jasjeet S. Sekhon, Walter R. Mebane, Jr., Michel C. Herron, and Henry E. Brady. 2001. "The Butterfly Did It: The Aberrant Vote for Buchanan in Palm Beach County, Florida." *American Political Science Review* 95 (4): 793–810.
Wolfinger, Raymond E., and Steven J. Rosenstone. 1980. *Who Votes*? New Haven, CT: Yale University Press.

https://doi.org/10.1017/S0003055411000268 Published online by Cambridge University Press