Mobilization and Participation:

A Natural Experiment*

Kentaro Fukumoto** and Yusaku Horiuchi***

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** Professor, Department of Political Science, Gakushuin University. Mailing Address: 1-5-1 Mejiro, Toshima, Tokyo 171-8588, Japan. Phone: +81 (3) 3986-0221 ext. 4913, Email: Kentaro.Fukumoto(at)gakushuin.ac.jp, URL: http://www-cc.gakushuin.ac.jp/~e982440/index_e.

*** Senior Lecturer, Crawford School of Economics and Government, ANU College of Asia and the Pacific, the Australian National University. Mailing Address: J.G. Crawford Building No 13, The Australian National University, Canberra, ACT 0200, Australia. Phone: +61 (2) 6125-4295, Email: yusaku.horiuchi(at)anu.edu.au, URL: http://www.horiuchi.org.

Abstract

One of the major hypotheses on the determinants of voter turnout is mobilization: Voters go to the polls when they are asked to do so. In this paper, we test it by taking advantage of a natural experimental setting in Japanese local elections. We argue that variations in timings of mayoral elections in Japan – specifically, whether or not they are held two weeks after prefectural assembly elections – can be regarded as a treatment naturally and randomly assigned. After confirming that a range of observable pretreatment variables are balanced between the treatment and control groups, we show that the average treatment effect on turnout is about one to three percentage points, which is statistically significant. This suggests that the mayoral elections, when they are just around the corner, have "coattail effects" of boosting turnout in the prefectural assembly elections.

INTRODUCTION

In this paper, we test one of the major hypotheses on the determinants of voter turnout: Voters go to the polls when they are asked to do so. Although the hypothesis itself sounds intuitive, whether it is indeed true, how large the effect is, and, more importantly, how to test it still remain controversial. Earlier scholars affirmed it using observational data (e.g., Cox, Rosenbluth and Thies 1998; Rosenstone and Hansen 1993; Verba, Schlozman and Brady 1995; Wielhouwer and Lockerbie 1994). More recent studies (e.g., Green and Gerber 2000) criticize these studies in that they do not cope with endogeneity: candidates and activists have a higher incentive to mobilize voters who are likely to turnout and vote for a specific candidate/party. Such variations in strategic weights upon individual voters or geographical units are difficult to measure and thus to be incorporated in regression models. A solution to the problem is allegedly to design and analyze a randomized field experiment (Arceneaux 2005; Green and Gerber 2000; Green et al 2003; Michelson 2003, 2005; Middleton and Green 2008; Nickerson 2005, 2006; Nickerson et al 2006). This line of experimental research is effective in holding other factors constant, and has contributed a great deal to our confidence in the validity of the mobilization hypothesis of voter turnout.

We argue, however, that there are still two problems in these experimental studies. First, they only estimate the impacts of a particular kind of mobilization; namely, direct mobilization by well-organized, non-partisan or partisan, campaign organizations and their volunteers (i.e., canvassers). Although that is the only kind of mobilization researchers can manipulate in field experiments, it is by not mean the only substantial one. Rather, as earlier studies rightly identified, candidates and parties employ various other kinds of mobilization, such as direct

face-to-face contacts by candidates themselves and indirect mobilization through informal – not necessarily organized – social groups. Preferably, we should examine the *overall* effect of all these kinds of mobilization on turnout. In randomized field experiments, however, we can neither manipulate candidates' behavior nor interpret the effects of indirect, auxiliary, and (often) unexpected mobilization through social networks.

Another problem is that almost all these recent studies based on randomized experiments use the U.S. as a case. Surprisingly little is known, however, about whether (and how much) mobilization efforts boost voter turnout under various institutional and cultural settings in other countries.[1] The lack of experimental studies in other countries is in part due to methodological issues. On one hand, since it is difficult to collect official, individual-level voter turnout records in most countries other than the U.S., we have to rely on data aggregated at certain geographical units (e.g., municipalities, electoral districts, etc.). On the other hand, it is equally difficult, if not impossible, to find a situation where political parties, individual candidates, and/or campaign organizations randomly select some geographical units to make harder mobilization efforts than in other units.

Given these limitations in randomized field experiments, we propose another solution, which deals with the problem of endogeneity and allows us to estimate the overall effect of mobilization in non-U.S. settings; that is to find and examine a *natural* experiment with the

[1] An important exception is a recent study by Karp, Banducci and Bowler (2007). They use seven-country data and argue that party canvassing increases voter turnout more effectively in the single-member district system than in the proportional representation system. Their work, however, is not free from the endogeneity problem we mentioned above.

following situation.[2] Every electoral district is divided into two areas. One area ("treated" area) is chosen randomly and given a treatment, i.e., mobilization, while the other ("control" area) does not receive it. Then, given that we can perfectly control any district-level characteristics by looking within-district variations, we can claim that the difference in turnout between these areas is, on the average, attributed to the presence or absence of the treatment. Furthermore, if the treatment corresponds to the presence or absence of a lower-level election within each district, we can interpret the average treatment effect as the overall effect of mobilization by political candidates and their supporters in the lower-level election concurrently held.

An example providing such an ideal setup is from Japan. A considerable proportion of (lower-level) mayoral elections do not participate in "simultaneous local elections" (hereafter, SLEs) held every four years on the fourth Sundays in April due to reasons specific to the early years (e.g., a wave of municipal mergers in the 1950s) and non-political reasons (e.g. the death of a chief executive). On the other hand, most (upper-level) prefectural assembly elections are held every four years on the second Sundays in the month of simultaneous municipal elections, and electoral districts for the prefectural assembly elections typically include multiple municipalities. Therefore, such intra-district variations in election timings – whether mayoral elections are held two weeks after the prefectural assembly elections or held independently at different times – can be regarded as a treatment naturally and randomly assigned.

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[2] We are inspired by a novel study by Ansolabehere, Snyder, and Stewart (2000), which estimates the incumbency advantage in the U.S. by way of a natural experiment. Bowler, Brockington and Donovan (2001) also take advantage of an experimental situation to estimate the effect of cumulative voting (i.e., where voters are allowed to cast multiple votes on a single candidate) on turnout.

Specifically, we expect that voter turnout in the prefectural assembly elections tends to be higher in municipalities in the treated group than those in the control group. It is because, when their own elections are just around the corner, mayoral candidates and their supporters are strongly motivated to join prefecture-level election campaigns and attempt to mobilize votes for their fellow candidates, which will in turn mobilize their own votes for upcoming elections. To put differently, the mayoral elections are expected to have a "coattail effect" of boosting turnout in the prefectural assembly elections.

The organization of the paper is as follows. The next section explains the institutional setting of Japanese local elections and discusses why mayoral candidates are expected to mobilize voters during prefectural assembly elections when their own elections are forthcoming. The third section discusses in full why we consider the election timings as a naturally occurring random assignment of mobilization treatment. Collecting municipality-level data of prefectural assembly elections in 2003, we also confirm that a range of observable pretreatment variables are well balanced between the treated and control groups based on simple t-tests for the mean difference in propensity score. The fourth section shows that the average treatment effect on turnout is about one to three percentage points, which is statistically significant. The final section concludes.

A COATTAIL EFFECT OF MOBILIZATION ON TURNOUT

The Japanese system of local government is composed of 47 prefectures and 3,219 municipalities (i.e., cities, special wards, towns, and villages) as of April 13, 2003.[3] Since the

[3] Source: http://www.e-stat.go.jp (accessed on March 13, 2009). April 13, 2003 is the date when the (simultaneous) prefectural assembly elections were held. This number drastically dropped to

first post-war local elections in 1947, elections of chief executives (governors and mayors) and assembly members have been held, in principle, every four years on the second (prefectural elections) and fourth (municipal elections) Sundays in April.[4] As we noted earlier, however, a significant proportion of municipalities do not hold their elections during the SLEs. Specifically, in the case of 2003 SLEs, as few as 20% of mayoral elections were held in the month of SLEs, while as many as 94% (44 out of 47) of prefectural assembly elections were held in the same month.[5] We take advantage of such variations in election timings.

We hypothesize a variant of "coattail effects" discussed in the literature (e.g., Ames 1994; Ishikawa 1984; and Jones 1997): municipality-level voter turnout rates of the prefectural assembly elections held on April 13, 2003 are, on average, higher if mayoral elections are scheduled in two weeks after the prefectural assembly elections (i.e., April 27, 2003) than

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1,827 by the end of 2006 (Horiuchi and Saito 2009) due to government-initiated municipal mergers. To obtain a sufficiently large number of valid observations, we do not use data from the post-merger period. Analysis of more recent data would introduce another methodological complication; that is, recent municipal mergers and resultant changes in election timings may be politically influenced, thereby contaminating the effects of out treatment variable (election timings in mayoral elections) on turnout in prefectural assembly elections.

[4] Every four year, a provisional law for exception (*rinji tokurei hō*) is enacted and all local government executive chiefs and assemblies expecting the expiry of their terms between March 1 and May 31 should, in principle, have elections on these two specific days in April.

[5] In April 2003, 53% of municipal assembly elections and 23% (11 out of 47) gubernatorial elections were also held during the SLEs. In the third section, we will provide rationales of not focusing on these two other types of local elections.

otherwise. Why? We argue that mayoral candidates and their supporters, who anticipate their elections in two weeks, have an incentive to mobilize voters not only for their own election but also for their fellow prefectural assembly candidates during the campaign period for prefectural assembly elections. According to the Article 33 of the Public Officers Election Act (*Kōshoku Senkyo Hō*), campaigns in local elections are allowed only for nine days (prefecture assembly elections), seven days (mayoral and assembly elections in cities), or five days (mayoral and assembly elections in towns and villages) just prior to the election.[6] More specifically, in the case of 2003 SLEs, the official campaign period for mayoral elections (held on April 27) was from April 20 or April 22 to April 26, while the campaign period for prefectural assembly elections (held on April 13) was from April 4 to April 12. Mayoral candidates and supporters must join prefectural-assembly campaigns (e.g. attending the rallies, distributing leaflets, canvassing, making phone calls to voters, driving a car to the poll for voters, and so on) if they want to make their own names known and strengthen close ties with voters well before the official campaign period for their own elections begins. If asked, they would have claimed that their activities for nine days in the early April were purely for their fellow candidates (from the same party, from the same area, etc.) in order to disguise their grey-zone activities. They are, however, *de facto* electioneering activities for their own.

Thus, we consider that whether or not mayoral and prefectural assembly elections are

[6] In the thirteen cities designated by ordinance (*seirei shitei toshi*), the campaign period for mayoral and assembly elections is 14 days. In our paper, we drop these cities from analysis, because they are not nested in a district for prefectural assembly elections. We will explain the importance of having multiple municipalities – at least one for the treated and another for the control group – nested in a single district shortly.

held back to back is a good measure of our conceptually relevant variable – how hard mayoral candidates and their supporters make a wide rage of mobilization activities – observable or unobservable, direct or indirect – during the prefecture assembly elections. We do not, however, pretend to insist that our measure is an error-free measure to estimate the overall effect of mobilization efforts. There are several potential sources of measurement errors. First, strong incumbent mayors may not work hard during the prefectural-assembly elections, even if their elections are scheduled to be held at the end of April. In fact, it is common that strong mayors do not face any challenger. They even get re-elected without voting (i.e., in an uncontested election). Second, promising challengers may be eager to mobilize votes during the prefectural assembly elections, even if their bid is not a SLE. They are expected to take every possible opportunity to appeal them to vote for them in future mayoral elections.[7] Nevertheless, these measurement errors, if any, would only attenuate our estimated effect of mobilization on voter turnout, rather than biasing it in an unexpected direction. Thus, we have reasonable grounds to believe our estimates as conservative.

The "coattail effects" of electoral behavior at one level of election on electoral outcomes at another level are by no means Japan-specific. For example, the U.S. presidents visit their colleague candidates for the Congress during the midterm election for the purpose of securing a majority in the Congress (Herrnson and Morris 2007, Keele et al. 2004). Gaines (1999), Jones (1997), and Samuels (2000a, 2000b) study how the timings of gubernatorial elections affect federal election outcomes (specifically, the effective number of parties) in Canada, Argentina, and Brazil, respectively. Similarly, Amorim Neto and Cox (1997) study the impact of concurrent

[7] Note that the time lag between the municipal SLEs and other post-SLE municipal elections is at least one month, but usually the lag is from several months to three years. Also see Footnote 4.

presidential elections on the number of parties in a polity. Ames (1994) shows that Brazilian presidential candidates earn more votes in those cities whose mayors belong to the same party. In the literature of Japanese politics, Araki (1990), Asano (1998) and Ishikawa (1984) discuss the "boar's year effect" – a turnout decline in Upper House elections in every twelfth years when they are preceded by SLEs by only a few months. We note, however, that none of these studies using data from federalism and Japan, unlike experimental studies in the U.S., pays careful attention to an endogeneity problem; that is, strategies and behaviors of political actors at one level of elections are a function of the expected outcomes of elections at another levels.

RANDOMNESS OF TREATMENT ASSIGNMENT

It is crucial for a natural experiment that the assignment of a treatment variable is random both in theory and in reality (Dunning 2008). This section establishes that it is indeed the case for our treatment variable, namely, whether or not each municipality is scheduled to have a mayoral election on April 27, 2003.

Randomness in Theory: Timings and Reasons of Deviation from SLEs

Among 3,146 municipalities where the prefectural assembly elections were held on April 13, 2003, 631 municipalities ("treated" municipalities) had mayoral elections (contested or uncontested) on April 27, 2003, while others ("control" municipalities) did not. Most of these control municipalities dropped off more than a few decades ago and/or for reasons unrelated to the current electoral, political, economic and demographic variables. Thus, in theory, all pretreatment variables, which can potentially affect the outcome variable (i.e., voter turnout in the 2003 prefectural assembly elections), are uncorrelated with the treatment variable, regardless

of whether these pretreatments are observed or unobserved.

Figure 1 displays the cumulative percentages of control municipalities – municipalities, which did not have a mayoral election on April 27, 2003 – by years and reasons of deviation from SLEs. The denominator of each bar graph is the total number of those municipalities whose reason for deviation is identified (*N*=1,170).[8] It shows that more than 60% of those municipalities dropped off from SLEs by the 1950s, during which the national government strongly encouraged municipal mergers by way of special laws and budgetary incentives. Newly established municipalities had their municipal elections before the expiry of a four-year term. Indeed, municipal merger is the most common reason to drop off from the SLEs (34.4% among the 1,170 control municipalities whose deviation reason is available). We cannot imagine that municipal mergers five decades ago, or factors affecting them, still have any substantial influence on electoral campaigns and voter turnout in 2003.

Other two major reasons for drop-off include when a mayor quitted before the expiry of his/her four-year term (28.2%) and when an incumbent mayor died (17.2%). The latter include retirement to run for another office such as a Diet member or a governor.[9] It is unthinkable that these past events, or factors which caused them, also affect today's turnout. Mayors never die or resign in order to increase or decrease turnout of the elections of their (not immediate)

[8] For 44.9% (952 out of 2,515) of the control municipalities, departure information is unknown. We tried to obtain and examine the official Records of Elections (*Senkyo no Kiroku*) issued by the Election Administration Commission in every prefecture for various years or other equivalent materials by way of the Freedom of Information Act (FOIA). Surprisingly, however, for some prefectures, old issues are not available to our best knowledge.

[9] For other miscellaneous reasons, 20.1% of municipalities deviated from the SLEs.

successors! At least, we (and probably readers alike) have no theory to predict the direction of the effects of merger, death or resignation on mobilization and turnout.

Randomness in Reality: Balance of Pretreatment Variables

While the previous subsection argued that the assignment of treatment should be independent of pretreatment variables, this subsection confirms that the observed pretreatments are indeed balanced between the treated and control groups. To improve the balance of pretreatment variables further, we employ two kinds of blockings. We also clarify our assumptions.[10]

Blocking A: Prefectural District. The first type of blocking (Blocking A) is to block municipalities by prefectural assembly election districts; namely, we drop all the observations with no within-district variation in the treatment variable. In other words, we only keep causally-relevant observations for prefectural assembly election districts, which contain at least one treated municipality and one control municipality.[11] When estimating the average treatment effects, we add district fixed effects that can perfectly control any observable or unobservable district-specific variable, including the numbers of seats, candidates, and eligible voters in the prefectural assembly elections, the (observable and/or unobservable) nature of electoral competition, such as stakes and closeness in the prefectural assembly elections, the past election

[10] In all (probit and OLS) regression models estimated below, we drop municipalities within uncontested districts for prefectural assembly elections since the outcome variable (i.e., voter turnout) is missing in these municipalities.

[11] It is also important to note that in Japan, electoral districts usually respect municipal boundaries. In other words, each municipality usually belongs to only one prefectural electoral district.

outcomes, and so on.[12]

The district fixed effects can also control prefecture-specific characteristics, because prefectural-assembly districts are obviously subunits within each prefecture. They include issues at stake in each prefectural assembly, whether or not a gubernatorial election was also held on April 13, 2003, the political balance between a governor and prefectural assembly members, and so on.

Blocking B: Municipal Assembly Election. The second type of blocking (Blocking B) is to keep municipalities scheduled to have a municipal assembly election on the same day as a mayoral election (i.e., April 27, 2003).[13] For the same reason as mayors, candidates for a municipal assembly are also expected to mobilize voters if their own election is in two weeks after a prefectural assembly election. Since we will include the population size (log) as a pre-treatment control, which is almost perfectly correlated with the numbers of seats in a municipal assembly (Horiuchi and Saito 2009), Blocking B controls the average level of mobilization efforts by candidates for municipal assemblies and their supporters. Note that the effectiveness of mayoral candidates' mobilization may be in part a function whether legislative candidates also mobilize at the same time. For this possibility of interaction effects, we select observations instead of simply adding a dummy variable for the (upcoming) municipal assembly elections as a control.[14]

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[12] The prefectural assembly elections in Japan use the single non-transferable vote (SNTV) system with the district magnitude (i.e., the number of seats) ranging from one to eighteen.

[13] On this day, 53% of municipalities (1,527 out of 2,881) held their assembly elections.

[14] Strictly speaking, this variable is measured as the same time as our treatment variable, because we know whether mayoral and/or municipal assembly elections are scheduled at the end of March without any time lag. Therefore, it is not appropriate to treat it as a pre-determined

Pretreatment Variables. Since we control district-specific or prefecture-specific pre-treatment variables by Blocking A (and district fixed effects), we only have to check the balance of municipality-specific pre-treatment variables that affect, in theory, our outcome variable – voter turnout in prefectural assembly elections. Conceptually, such variables include the density of social and political networks (e.g., Cox, Rosenbluth and Thies 1998) and the quantity/quality of political benefits incumbents can supply and voters demand; in particular, intergovernmental fiscal transfers (e.g., Scheiner 2005). We expect that the higher the density of networks and/or the higher the perceived benefits conditional on electoral outcomes, the more likely people go to the polls.

Empirically, to measure these two types of relevant variables, we use a range of demographic, economic, and fiscal variables, including the total population (log), the percentage of elderly population aged 65 and over, the percentage of youth population aged under 15, the percentage of women in the population, the percentage of the population in densely inhabited districts (DID) within each municipality, the percentage of population moving into a municipality (since the last Census), the percentage of daytime population, the percentage of nuclear households, the unemployment rate, the percentages of employees in secondary or tertiary industries, the per capita amount of disbursements from the national government to a municipality (*kokko shishutsukin*, log), and the per capita amount disbursements from the prefectural government to a municipality (*todofuken shishutsukin*, log).[15]

Although we expect that these variables are correlated with voter turnout, we do not

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control variable

[15] The data sources are Sōmushō Tōkeikyoku (2002), Kokudo Chiri Kyōkai (2003), and Chihō Zaisei Chōsa Kenkyū Kai (2003).

have any theory that predicts their correlation with our treatment variable (i.e., whether or not a mayoral election was scheduled on April 27, 2003). Thus, we might argue that these pretreatment variables do not confound our estimate of the average treatment effect even if they are not balanced between the treatment and control groups. We just make sure, however, that the assignment of treatment are indeed independent of these pretreatment variables.

Assumptions. We admit that there should be other unobservable or difficult-to-measure variables, which may affect the treatment variable. They include political conditions in the distant past (e.g., factors explaining municipal mergers in the 1950s) and/or non-political conditions (e.g., factors explaining the death of mayors). We assume that these variables are not correlated with the outcome variable, voter turnout in 2003. As we discussed earlier, it is reasonable to assume this. Another assumption is that there is no other municipality-specific variable, which is both correlated with the treatment and outcome variables (given the treatment variable and the blockings). As long as these assumptions are satisfied, we can claim that our estimates of the average treatment effects are unbiased.

Propensity Score. To check the balance between the treated and control groups, we use propensity score, which is now widely used in making causal inference with observational data. Specifically, we will run a probit analysis of the treatment assignment regressed on all the municipality-specific pretreatment variables. Then, using estimated parameters, we calculate the probability of each municipality to be assigned the treatment. This probability is called the propensity score. If the distribution of the propensity score for the treated municipalities and that for the control municipalities are (nearly) matched, we can claim that the pretreatment variables are on average balanced between the treated and control municipalities (Ho et al. 2007).

Depending on a combination of the two types of blockings, we analyze the balance of

these municipality-specific variables for the following four types of data: (1) "Data AB" with Blockings A and B, (2) "Data Ab" with Blockings A but not B, (3) "Data aB" with Blockings B but not A , and (4) "Data ab" with neither Blockings A nor B. We use all these four types of data, rather than only using the most restrictive data (Data AB) in order to understand which type of blocking can (or cannot) improve the balance. Note that we do not include district-specific variables when checking the balance using propensity score, because we know *a priori* that all observable and unobservable district-specific characteristics can be perfectly controlled by the fixed-effects, when we later estimate the average treatment effects. In other words, we only need to check the balance with regard to observable municipality-specific variables.

Table 1 presents the results of probit regressions. For the purpose of understanding the balance between the treated and control groups, however, individual coefficient estimates are unimportant. Rather, we need to check the balance by examining the distributions of propensity score, which Figure 2 shows. The distributions for the treated are in solid lines and those for the control are in dashed lines. Except for Model aB, it seems that the two distributions are almost overlapped. The two-sample t-statistics (p-value, two-sided) are −5.201 (0.000), −1.726 (0.085), −6.115 (0.000), and −1.340 (0.181) for Models ab, Ab, aB, and AB, respectively. Therefore, we cannot reject the null hypothesis (i.e., the means of propensity score being the same between the two groups) in the cases of Models Ab and AB. Given the large size of our sample (N=1,112 for Model Ab and 536 for Model AB), these results are striking.[16]

In sum, the observed pretreatment variables are well balanced between the treated and control municipalities as long as we make use of Blocking A. This result suggests the importance

[16] Since the blockings drop municipalities, the number of observations used for analysis shrinks from the number of total observations (i.e., 2,055).

of dropping causally irrelevant observations, which do not vary within each electoral district. They are prefectural-assembly districts composed of only one municipality, those composed of multiple "treated" municipalities only, and those composed of multiple "control" municipalities only. It is also worth noting that as long as Blocking A is applied, whether to drop municipalities scheduled to have a legislative election on the same day with a mayoral election (Blocking B) does not significantly improve the balance of pre-treatments.

ESTIMATION OF TREATMENT EFFECT

Provided that a range of pretreatment variables are balanced between the treated and control groups, the final step of our analysis is to estimate the average treatment effects by running OLS regressions of the outcome variable (voter turnout) on the treatment variable, given blockings and fixed-effects.

Model Specification. Before showing the results of regressions, we need to discuss some issues relevant to model specification. First, to deal with the heterogeneous population size, we weight each municipally by the total number of eligible voters. This also means that we are estimating the average treatment effect among individual voters, rather than municipalities.

Second, when we employ Blocking A, as we noted, we include a set of prefectural-assembly district dummies as fixed effects. Therefore, all district level variables, even if they are unobserved, are completely controlled.[17] In some specifications, we also include all the pretreatment variables, which we used to test the balance previously, as control variables.

[17] To cope with a possible correlation across municipalities within each district, we also use clustered robust standard errors, where clusters are electoral districts for the prefectural assembly elections held on April 13, 2003.

By adding these, the estimates of treatment effects become more efficient and the balance in the treated and control groups are secured.

Finally, we run two sets of regression models – one without Blocking B and another with Blocking B. Since Blocking B was found to be less important than Blocking A, we expect that the estimates in these two sets of regressions are similar. For each set, we run three different models – a model without Blocking A nor the pre-treatment variables; a model with Blocking A but without the pre-treatment variables; and a model with both Blocking A and the pre-treatment variables. The most restrictive model with Blockings A and B are expected to have the highest level of internal validity. Yes, by reducing the number of municipalities through blockings, one may suspect that we are only estimating local average treatment effects, which only hold for a selected set of observations but not for the entire population. To make us interpret whether (and how much) the unbiased local average treatment effect (LATE) estimated from the most restrictive data (with Blocking A and/or B) is different from (possibly biased) average treatment effects estimated from less restrictive samples (without Blocking A and/or B), we report the results of all these six (two by three) model estimations.

Results. Tables 2 (without Blocking B) and 3 (with Blocking B) show the results of OLS regressions. The most important finding is that, irrespective of model specifications, the average treatment effect is positive and significantly different from zero. The point estimates range from 0.938 to 4.000 percentage points. On one hand, the more restrictive the number of observations used for estimation and the more dependent the estimated model, the smaller the magnitude of the effect. On the other hand, as a matter of course, when we include control variables, the standard errors are almost half as large as its counterparts in the case of no control variables. Since Blocking A can bring about the balance of pretreatments (and therefore unbiased

estimates), and since the results using control variables (i.e., Ab' and AB') are dependent on model specification, we conclude that the magnitude of the (local) average treatment effect is about one to three percentage points, considering 95% confidence interval. In other words, on the (weighted) average, individuals in the treated municipalities have about one to three percentage-point higher probability of going to the polling station thanks to mobilization by mayoral campaign than individuals in the control group.

Discussion. The magnitude of our estimate may seem to be small. By comparison, the effect of MoveOn volunteers on turnout in U.S. presidential elections is nine percentage points (Middleton and Green 2008). Niven (2004) shows that face-to-face mobilization efforts increase turnout by five percentage points. The effect of cumulative voting on turnout is also estimated around five percentage points (Bowler, Brockington and Donovan 2001, 910).

We suspect our estimate as conservative for the two reasons. One reason is the measurement error of our treatment variable, as we argued in the third section. Conceptually, we want to estimate the impacts of whether or not an incumbent mayor and/or a challenger (or challengers) and their supporters in the next mayoral election in a given municipality mobilize voters during the prefectural assembly elections in April 2003. We believe that whether mayoral elections were scheduled two weeks after the prefectural elections correspond to variations in the levels of overall efforts mayoral campaign made. This correspondence, however, is imperfect and produces attenuation bias in causal estimates. Another reason is the overlap of mobilization efforts. Not only mayoral campaigns but also prefectural campaigns do mobilize voters. More importantly, the latter is active both within the treated and control groups. Thus, our estimate of mobilization effect measures the marginal effect; that is, how many additional voters mayoral campaign can mobilize whom prefectural campaign fails to mobilize. Even with these limitations,

however, it is notable that we still obtained significantly positive effects. As they are likely to be conservative estimates, we believe that the true effect of mayoral mobilization is larger than our estimates.

CONCLUSION

Although the act of voting in elections is the simplest, the most popular, the least expensive, and the most essential mode of political participation in democracy, people do not necessarily exercise this fundamental right. As a result, voter turnout tends to be low and even declining in many industrial democracies (e.g., Franklin 2004; Gray and Caul 2000). To account for this "democracy's unresolved dilemma" (Lijphart 1997), political scientists have extensively studied the determinants of voter turnout over the past decades (Blais 2006 for a review). In this paper, we tested one of the major hypotheses investigated since Rosenstone and Hansen's (1993) seminal work; namely, voters go to the polls when they are asked to do so.

Recent empirical studies of mobilization in the literature of American politics have conducted randomized experiments to test this mobilization hypothesis. We argued, however, that such studies can be done, and have been done, only in the United States where individual records of voter turnout are available. We also argued that these studies may be only estimating the impacts of mobilization by well-organized campaign organizations and canvassers. Considering these limitations in the literature, we attempted to estimate the overall effect of mobilization using a natural experimental situation in Japan.

The approach we took in this paper – finding a natural experimental setting, if conducting a randomized experiment is difficult, if not impossible – can be applied in other democracies. Since we used Japan as a single case, we are not yet ready to make any general

argument about the types and consequences of mobilization in various cultural and institutional setting. We hope that we pave the way for further comparative studies on mobilization, which can improve our understandings of who vote, why they vote, and when they vote in elections.

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Table 1: Probit Estimates for Balance Check

Model	ab	Ab	aB	AB
Blocking A	No	Yes	No	Yes
Blocking B	No	No	Yes	Yes
Population, Total (log)	0.142	−0.002	0.183	−0.347***
	[0.113]	[0.157]	[0.145]	[0.121]
Population, Age≥60 (%)	−0.032	−0.020	0.001	−0.014
	[0.029]	[0.023]	[0.037]	[0.029]
Population, Age<15 (%)	−0.009	−0.024	−0.013	−0.025
	[0.051]	[0.059]	[0.066]	[0.047]
Population, Women (%)	0.123*	−0.013	0.069	−0.040
	[0.074]	[0.065]	[0.087]	[0.065]
Population, DID (%)	−0.002	0.001	−0.002	−0.005
	[0.003]	[0.004]	[0.004]	[0.004]
Population, Moving-into (%)	−0.124*	−0.021	−0.080	−0.065
	[0.066]	[0.069]	[0.080]	[0.071]
Population, Daytime (%)	0.001	−0.007	0.002	0.001
	[0.001]	[0.009]	[0.001]	[0.006]
Nuclear Household (%)	0.025***	0.007	0.046***	0.018
	[0.009]	[0.011]	[0.011]	[0.013]
Unemployment Rate (%)	−0.018	−0.042	0.018	−0.010
	[0.054]	[0.037]	[0.062]	[0.045]
Employed, Secondary Industry (%)	0.006	0.003	0.013	−0.003
	[0.010]	[0.008]	[0.013]	[0.009]
Employed, Tertiary Industry (%)	0.015	0.001	0.020	0.008
	[0.013]	[0.009]	[0.018]	[0.011]
Disbursement, National (log, per capita)	−0.072	0.179*	−0.159	−0.042
	[0.142]	[0.102]	[0.177]	[0.126]
Disbursement, Prefectural (log, per capita)	0.642***	0.203	0.879***	0.079
	[0.188]	[0.141]	[0.230]	[0.142]
Constant	−11.625***	0.071	−12.148***	4.567
	[3.575]	[3.398]	[4.364]	[3.562]
Observations	2,055	1,112	1,168	536
Observations, "treated"	415	336	311	219
Wald Chi-squared	63.99	15.57	75.07	38.72

Note: The treatment variable is 1 ("treated") if a municipality was scheduled to have a mayoral election on April 27, 2003, and 0 ("untreated") otherwise. Clustered robust standard errors are in brackets, where clusters are electoral districts for prefectural assembly elections held on April 13, 2003. All observations are weighted by the number of eligible voters. Blocking A refers to the inclusion of municipalities with at least one "treated" observation and one "untreated" observation within electoral districts for prefectural assembly elections. Blocking B refers to the inclusion of municipalities scheduled to have a municipal assembly election, as well, on April 27, 2003. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2: Estimated Treatment Effects

Model	ab	Ab	Ab’
Blocking A	No	Yes	Yes
Blocking B	No	No	No
Upcoming Mayoral Election (Treatment)	4.000***	2.158***	1.412***
	[1.162]	[0.627]	[0.381]
Population, Total (log)			−0.501
			[0.428]
Population, Age≥60 (%)			0.735***
			[0.119]
Population, Age<15 (%)			0.407
			[0.274]
Population, Women (%)			−0.165
			[0.268]
Population, DID (%)			−0.038**
			[0.018]
Population, Moving-into (%)			−1.071***
			[0.262]
Population, Daytime (%)			0.007
			[0.021]
Nuclear Household (%)			−0.080
			[0.056]
Unemployment Rate (%)			−0.622**
			[0.283]
Employed, Secondary Industry (%)			−0.016
			[0.045]
Employed, Tertiary Industry (%)			0.024
			[0.043]
Disbursement, National (log, per capita)			−0.289
			[0.451]
Disbursement, Prefectural (log, per capita)			0.386
			[0.490]
Observations	2,055	1,112	1,112
Observations, “treated”	415	336	336
R-squared (centered for Models Ab and Ab’)	0.017	0.022	0.499
F-statistic	11.85	11.76	36.44
Root MSE	11.34	6.05	4.33

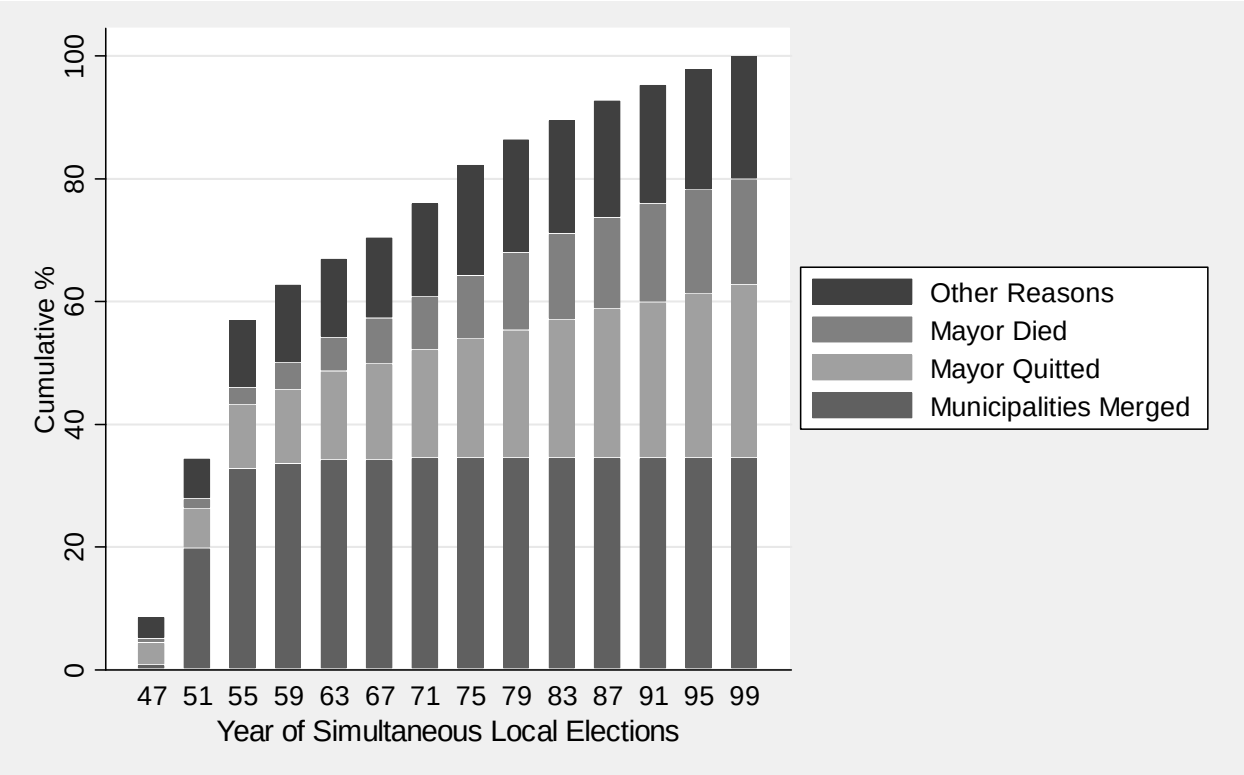
Note: The outcome variable is voter turnout (%) in prefectural assembly elections held on April 13, 2003. Clustered robust standard errors are in brackets, where clusters are electoral districts for the prefectural assembly elections. All observations are weighted by the number of eligible voters. Models Ab and Ab’ include district-specific fixed effects. See Table 1’s Note for the definitions of Blockings A and B. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 3: Estimated Treatment Effects (continued)

Model	aB	AB	AB'
Blocking A	No	Yes	Yes
Blocking B	Yes	Yes	Yes
Upcoming Mayoral Election (Treatment)	3.725***	2.186***	0.938**
	[1.220]	[0.810]	[0.467]
Population, Total (log)			0.091
			[0.557]
Population, Age≥60 (%)			1.020***
			[0.137]
Population, Age<15 (%)			0.995***
			[0.208]
Population, Women (%)			−0.667**
			[0.327]
Population, DID (%)			−0.041**
			[0.016]
Population, Moving-into (%)			−1.097***
			[0.286]
Population, Daytime (%)			0.016
			[0.020]
Nuclear Household (%)			−0.201***
			[0.062]
Unemployment Rate (%)			−0.918***
			[0.302]
Employed, Secondary Industry (%)			0.011
			[0.054]
Employed, Tertiary Industry (%)			0.089*
			[0.046]
Disbursement, National (log, per capita)			−0.896*
			[0.502]
Disbursement, Prefectural (log, per capita)			−0.981
			[0.687]
Observations	1,168	536	536
Observations, "treated"	311	219	219
R-squared (centered for Models AB and AB')	0.021	0.030	0.585
F-statistic	9.33	7.23	40.96
Root MSE	10.28	5.73	3.75

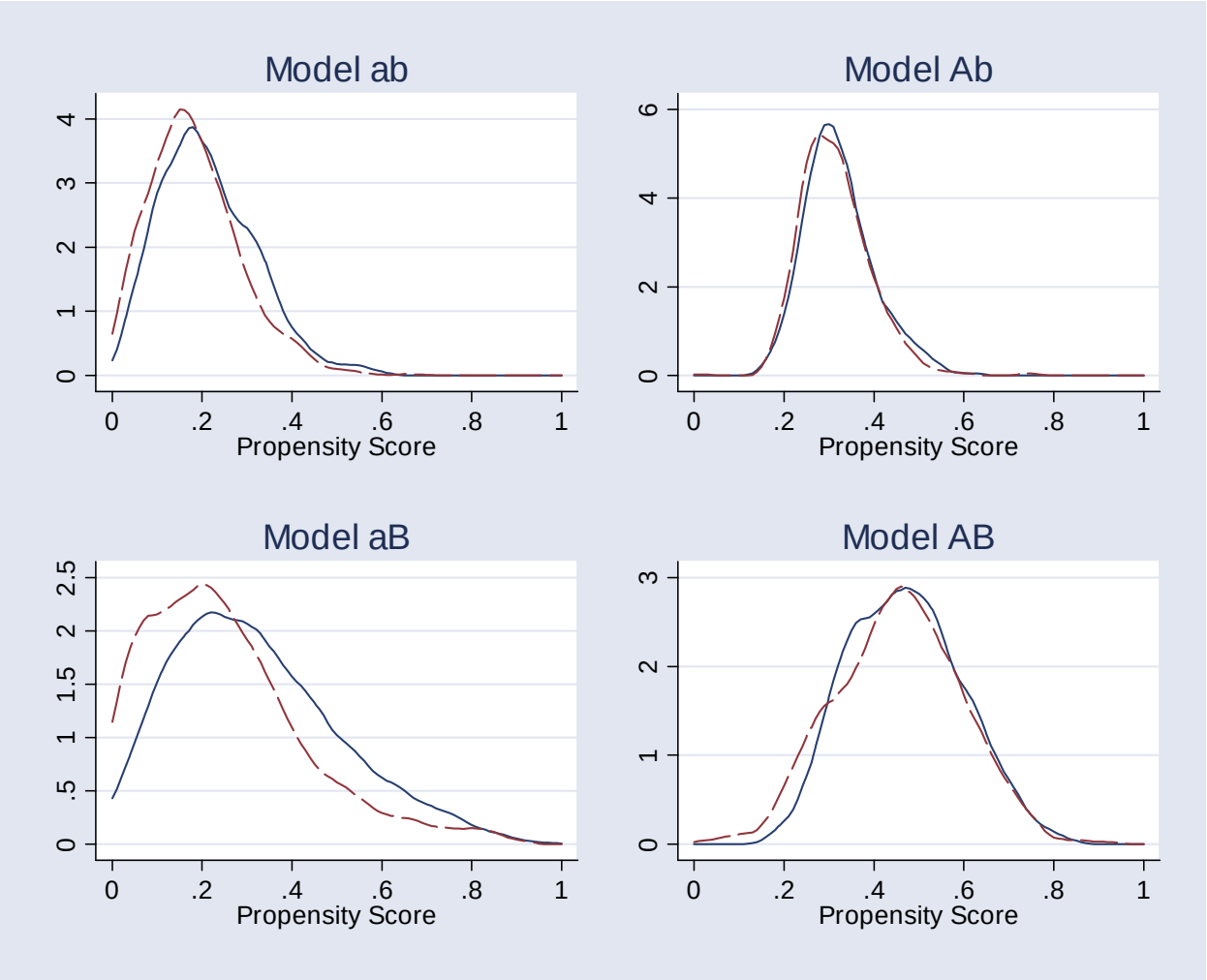
Note: The outcome variable is voter turnout (%) in prefectural assembly elections held on April 13, 2003. Clustered robust standard errors are in brackets, where clusters are electoral districts for the prefectural assembly elections. All observations are weighted by the number of eligible voters. Models AB and AB' include district-specific fixed effects. See Table 1's Note for the definitions of Blockings A and B. * significant at 10%; ** significant at 5%; *** significant at 1%

Figure 1: When and why did muncipalities dropped from SLEs?



Note: The figure shows the cumulative percentages of "control" municipalities – municipalities, which did not have a mayoral election on April 27, 2003 – by years of reasons of deviation from simultaneous local elections (SLEs). The denominator of each bar graph is the total number of those municipalities whose reason for deviation is identified (*N*=1,170).

Figure 2: The Distributions of Propensity Score



Note: The solid lines are the distributions of propensity score (i.e., the predicted probability of being "treated") for "treated" observations, whereas the dashed lines are that for "control" observations. The two-sample t-statistics (p-value, two-sided) are −5.201 (0.000), −1.726 (0.085), −6.115 (0.000), and −1.340 (0.181) for Models ab, Ab, aB, and AB, respectively.