Identifying the effect of mobilization on voter turnout through a natural experiment

Kentaro Fukumoto, Yusaku Horiuchi

A R T I C L E   I N F O

Article history:
Received 1 March 2016
Received in revised form 6 May 2016
Accepted 2 August 2016
Available online 6 August 2016

Keywords:
Turnout
Mobilization
Natural experiment
Bundling treatments
Japan

A B S T R A C T

Although numerous get-out-the-vote field experiments have identified the effects of particular mobilization tactics (e.g., canvassing, phone calls, direct mails) on voter turnout, we do not yet have a full understanding of the causal effect of overall mobilization. We study this by leveraging a natural experiment in Japan, in which the timing of a municipal election is as-if randomly assigned. The results show that almost concurrently held municipal elections boost these municipalities’ voter turnout in prefectoral elections by one to two percentage points. We argue that some unique settings in Japan allow us not only to mitigate omitted variable bias but also to attribute the estimated effect only to mobilization, rather than the effects of cost sharing and psychological stimulus.

© 2016 Elsevier Ltd. All rights reserved.

1. Introduction

Are citizens more likely to go to the polls when they are contacted and asked to vote? To what extent do such mobilization efforts matter in boosting voter turnout? Political scientists have long debated these questions about electoral mobilization (e.g., Verba et al., 1995; Wollinger and Rosenstone, 1980). Early studies suggest that mobilization increases voter turnout by analyzing individual level surveys (e.g., Caldeira et al., 1985; Huckfeldt and Sprague, 1992; Rosenstone and Hansen, 1993) or aggregated election results (e.g., Cox and Munger, 1989; Patterson and Caldeira, 1983). These observational studies, however, are likely to suffer from a problem of endogeneity bias because campaigners strategically target those who would not go to the polls otherwise (Cox, 2015). To address this problem, more recent works employ field experiments (e.g., Gerber and Green, 2008). Since the publication of an influential article by Gerber and Green (2000), numerous studies have randomized a range of get-out-the-vote tactics, such as canvassing, phone calls, leaflets, direct mails, and emails, to estimate the effects of these tactics on voter turnout.

Nonetheless, it is challenging—or even impossible—for experimenters to randomize all theoretically relevant campaign

http://dx.doi.org/10.1016/j.electstud.2016.08.003 0261-3794/© 2016 Elsevier Ltd. All rights reserved.
tactics. Most importantly, it is difficult for researchers to randomize “a campaign that evokes enormous efforts by the party organizations to get out the vote” (Key, 1964, p. 584). Furthermore, although “party politics and partisan efforts at electoral mobilization include a heavy dose of social influence” (Huckfeldt and Sprague, 1992, p. 70), these researcher-driven field experiments are not well suited to measure the effects of non-manipulable and indirect mobilization efforts through social networks.\footnote{An important exception is an experiment by Alvarez et al. (2010), in which a real-world campaign organization of the Democratic Party randomly delivered partisan get-out-the-vote messages prior to the election. Although they claim that they estimate “the effect of an entire campaign” instead of “particular mobilization tactics” (p. 31, emphasis added), their research does not capture the effects of the other partisan (Republican) campaigns, nonpartisan campaigns, or indirect mobilization efforts through social networks.}

Accordingly, despite the voluminous studies on mobilization, we do not yet have a full understanding of the causal effect of overall mobilization on turnout. However, this should be the main quantity of interest in the long-debated theory of mobilization. In the first place, when scholars argue that citizens vote not only because of their intrinsic motivation but also extrinsic mobilization by others, an overarching question is the degree to which voters go to the polls due to mobilization, which includes all direct and indirect, partisan and nonpartisan, and observable and unobservable efforts – not just particular tactics. Understanding how much the level of turnout could be raised through mobilization as a whole is also necessary for policy debates on whether to regulate or liberalize institutional arrangements (e.g., the length of campaign period) that could potentially affect many types of mobilization activities.

To achieve this purpose, we reconsider the usefulness of nonexperimental, observational studies. We focus on a phenomenon that many researchers have noted: voter turnout for a given election tends to be higher when it is held concurrently with another election (Anzia, 2011; Berry and Gersen, 2011; Boyd, 1986, 1989; Caldeira et al., 1985; Dettrey and Schwindt-Bayer, 2009; Engstrom, 2012; Fauvelle-Aymar and François, 2015; Fornos et al., 2004). Usually, scholars regress voter turnout on an indicator variable for the presence of a concurrent election and a number of control variables. However, it is not straightforward to identify the causal effect of overall mobilization on voter turnout by the coefficient for the concurrent election dummy because of two identification problems. The first problem is the omission of some relevant variables. As is often the case in observational studies, it is almost impossible to control all relevant variables, which are correlated with both voter turnout (an outcome variable) and the presence of a concurrent election (a treatment variable). The second difficulty is the “problem of bundling treatments” (Dunning, 2012, pp. 300–302). When running regression with the above-mentioned outcome and treatment variables, the estimated effect may bundle not only the effect of mobilization but also the effects of cost sharing and psychological stimulus, which we will elaborate shortly.

To address these identification problems, we leverage a natural experimental setup in Japanese local elections, and estimate the effects of (almost) concurrently held municipal assembly and/or mayoral elections (hereafter, municipal elections) on these municipalities’ voter turnout in prefectural assembly elections (hereafter, prefectural elections). Our research design provides us with three identification strategies. The first two strategies that follow a golden rule in experimental design – “block what you can and randomize what you cannot” (Box et al., 1978, p. 103) – mitigate the first identification problem of omitted variables. The third strategy, which focuses on unique institutional settings in Japan, corresponds to the second identification problem of bundling treatments.

First, many electoral districts for the prefectural elections (hereafter, prefectural districts) include multiple municipalities. Thus, we use district fixed effects in regression analysis and leverage an intra-district variation in treatment status. This is an effective empirical strategy for controlling all district-level omitted variables that are constant within each prefectural district.

Second, for historical reasons, the timing of a municipal election is as-if randomly assigned (Fukumoto and Horiuchi, 2011). Therefore, theoretically, even municipality-level omitted variables that are not constant within each prefectural district should be balanced between municipalities with and without their own elections and, therefore, should not produce biased estimates. We empirically confirm the balance of dozens of municipality-level variables.

Finally, Japan holds what we call “proximately concurrent” (but not “exactly concurrent”) local elections. In our case, most of 47 prefectural elections were held on April 13, 2003, while a portion of a few thousand municipal elections were held on April 27, two weeks after the prefectoral elections.\footnote{Fukumoto and Horiuchi (2011), who focus on these elections to detect electoral fraud, explain historical reasons for using data from April 2003 rather than other years (fn. 25).} Given this setting, we estimate the effect of having a municipal election in two weeks on voter turnout in a prefectural election. As we will discuss in detail, this design enables us to attribute the estimated treatment effect only to mobilization, rather than cost sharing or psychological stimulus.

The organization of the paper is as follows. The next two sections detail the two identification problems, and then explain our identification strategies. After testing the balance of covariates, the fourth section shows that voter turnout in a prefectural election is raised by one to two percentage points when a municipal election is scheduled in two weeks. The final section discusses our contribution to the literature and avenues for future research.

2. Identification problems

This section elaborates on the two identification problems – omitted variable bias and bundling treatments.

2.1. Omitted variables

To estimate the effects of concurrent elections, almost all existing studies regress voter turnout in one election on an indicator of whether or not another election is held concurrently.\footnote{For a review of empirical studies taking this approach, see Geys (2006).} The problem with this approach is that it is difficult to distinguish the effect of the concurrence of elections on voter turnout from effects of other contextual variables specific to electoral districts and/or election years. For example, districts with concurrent elections can be socio-demographically different from districts without them; years in which elections are held concurrently can be macro-economically different from other years. As long as such variables are correlated with both outcome and treatment variables and not included in a regression model, the OLS estimator of the coefficient is biased.

The existence of omitted variables is plausible, particularly when election timing is endogenous and manipulable. For example, the timing of some American municipal elections has been manipulated by political parties hoping for better electoral prospects (Anzia, 2012). The election timing of U.S. school board districts might also be strategically decided for partisan reasons.
In Mexico, the administration’s proposal for a single concurrent election date “may well have been more partisan” (Magar, 2012, p. 395). In all these cases, it is difficult to identify the causal effect of election timing on voter turnout without bias, because a range of political considerations and manipulations, such as a governing party’s electoral prospects, may produce a spurious correlation between election concurrence and voter turnout. One way to address the problem is to include many control variables, such as socio-demographic or macroeconomic variables, in a regression model. However, it is impossible to do so perfectly, because we never know the complete set of relevant contextual variables.

2.2. Bundling treatments

The second identification problem is concerned with bundling treatments. In our analysis, we exploit the presence of (almost) concurrent elections as the treatment of mobilization. When multiple elections are held concurrently, citizens encounter mobilization efforts more frequently than otherwise, because a larger number of candidates, activists, and volunteers are engaged in get-out-the-vote campaigns. For instance, Cox and Munger (1989) demonstrate that mobilization activities for gubernatorial and senatorial offices boost voter turnout in concurrently held U.S. House elections. Boyd (1986) argues that “gubernatorial and senatorial races are now routinely multimillion-dollar contests, with media campaigns designed to stimulate interest in candidacies and organizational efforts aimed at registering and mobilizing voters” (p. 92, emphasis added) when they are held concurrently with presidential elections.

However, the presence of concurrent elections may represent other two-alternative treatments as well. The first is the cost sharing between the two elections. This is based on a rational choice model of voter turnout. Wolfinger (1994) claims, “Because the costs of voting do not increase in proportion to the number of choices, while the probabilities both of rewards and of casting a decisive vote are proportionate to the number of choices, rational choice theory must predict that longer ballots produce higher turnout” (pp. 77–78; see also Aldrich, 1993). Their argument can be formalized as follows: a citizen decides to go to polls if the expected benefit of voting exceeds its cost; that is, if the following payoff is positive:

\[
\left( \sum_{i=1}^{n} P_i B_i - C_i \right) - C_D > 0,
\]

where \( i \) is the indicator for each election and \( n \) is the number of elections held on a specific date. For each election \( i \), \( P_i \) is the probability that a citizen casts a decisive vote, \( B_i \) is the difference in benefits between the preferred candidate winning \( \text{vis-à-vis} \) another candidate winning, and \( C_i \) is the cost of voting specific to election \( i \) (e.g., gathering information about candidates). The last term, \( C_D \), is the fixed cost of voting unrelated to the number of concurrent elections, such as “voter registration, locating the polling place, and taking the time to go there” (Wolfinger, 1994, p. 77). When rational-choice theorists claim that voter turnout is boosted by concurrent elections, they assume, often implicitly, that the net payoff specific to each election \( i \) is positive \( (P_i B_i - C_i > 0) \) for a majority of voters. Once \( P_i, B_i, C_i, \) and \( C_D \) are exogenously determined, the total payoff should increase as the number of concurrent elections \( n \) increases, because \( C_D \) is constant. Consequently, citizens are more likely to vote in concurrent elections.

The other alternative treatment is psychological stimulus. It is a theoretical foundation of the well-known “surge and decline” phenomenon in U.S. federal elections: voter turnout is often higher in presidential-election years than in midterm years (Burns, 1999; Mondak and McCurley, 1994). Campbell (1960) argues that there are two types of voters — “peripheral voters,” who go to the polls in “high-stimulus” elections but abstain in “low-stimulus” elections, and “core voters,” who go to the polls in both kinds of elections. High-stimulus presidential elections should motivate peripheral voters to cast ballots even in concurrently held, low-stimulus Congressional elections. By contrast, in midterm Congressional elections, while core voters are brought to the polls, peripheral voters abstain from voting due to the lack of sufficient stimulus. An observable implication is the so-called midterm slump in voter turnout.

To sum up, the presence of concurrent elections may measure the effects of not only mobilization but also cost sharing and psychological stimulus. This is a typical case of the problem of “bundling treatments,” which does not allow researchers to “discern the singular effect of any of the intervention’s distinct components” (Dunning, 2012, p. 302). Formally, a regression model considering all the three bundling treatments can be written as follows:

\[
Y = \alpha + \beta_M X + \beta_C X + \beta_P X + \sum_j \gamma_j Z_j + \epsilon
\]

where the outcome variable \( Y \) is voter turnout in one election (e.g., a presidential election), the treatment variable \( X \) is a dummy variable indicating the presence of a concurrent election (e.g., a gubernatorial election), and \( Z_j \) are covariates. \( \alpha \) is the constant term, while \( \beta_M, \beta_C, \) and \( \beta_P \) are the effects of mobilization, cost sharing, and psychological stimulus, respectively; \( \gamma_j \)'s are the coefficients of \( Z_j \)'s; and \( \epsilon \) is an error term. Most importantly, the coefficient \( \beta = \beta_M + \beta_C + \beta_P \) is the effect of the three bundling treatments.

Previous studies (e.g., Boyd, 1986, 1989; Caldeira et al., 1985; Detrey and Schwindt-Bayer, 2009), which regress \( Y \) on \( X \) and \( Z_j \), can identify only the bundling treatments effect, \( \beta \). This simple regression model does not allow these scholars to identify our quantity of interest, \( \beta_M, \beta_C, \) and \( \beta_P \) separately.

3. Identification strategies

A natural experimental setup in Japan can solve the two identification problems discussed in the previous section. The first of the following subsections introduces Japan’s institutional settings and specifies our testable hypothesis. The next two subsections correspond to the identification problems. The first challenge — omitted variable bias — is mitigated by district fixed effects and the exogenous assignment of election timing (the second subsection). As for the second problem, bundling treatments, the design enables us to identify the effect of overall mobilization by its features of “proximately concurrent” elections and “low-to-high” treatment effects (as explained in the third subsection).

3.1. Design

In Japan, the first-order local governments are 47 prefectures, while those of the second order are 3187 municipalities (cities, towns, and villages) as of April 27, 2003. This study is limited to

7 This number does not include 23 special wards in Tokyo Metropolitan Area. The data source is “Shichōson Sū no Sui-i Hyō, Shōsai Ban” [Table of the History of the Number of Municipalities, Detailed Version], available at http://www.soumu.go.jp/main_content/000283129.pdf (accessed November 27, 2015).
By law, executive chiefs and assemblies of Japanese local governments should have had “simultaneous local elections” on April 13 (prefectures) or April 27 (municipalities), 2003, in principle, if their term expired between March 1 and May 31, 2003. Specifically, as many as 93.6% (44) of 47 prefectures had their assembly elections on April 13, 2003. The outcome variable in our analysis is voter turnout in these prefectural assembly elections. By contrast, for reasons given at the end of this section, only 52.2% (1,557) and 21.5% (641) of the 2,984 municipalities held their municipal assembly and mayoral elections, respectively, on April 27, 2003. We call them (legislative and executive) “treated municipalities” (respectively). The remaining municipalities are, accordingly, (legislative and executive) “control municipalities” (respectively).

Given these outcome and treatment variables, we expect the following.

**Hypothesis.** Voter turnout in prefectural elections held on April 13, 2003, (Y in Equation (2)) is higher, on average, in treated municipalities where their own elections are scheduled in two weeks (X = 1) than in control municipalities (X = 0). In other words, we expect β > 0 in Equation (2).

We argue that this is because municipal politicians and their supporters mobilize voters in prefectural elections. To explain how this hypothesis is derived, we must first explain institutional arrangements in greater detail.

According to the *Public Offices Election Act*, campaigns in local elections (e.g., attending rallies, distributing leaflets, canvassing, calling voters, etc.) are allowed for only nine days (prefecture assemblies), seven days (cities), or five days (towns and villages) prior to the election. 

In the case of the simultaneous local elections in April 2003, the *de jure* campaign period for prefectural assembly elections held on April 13 was April 4 to April 12, while that for municipal elections held on April 27 was April 20 (cities) or April 22 (towns and villages) to April 26 (see Fig. 1). 

Candidates in the municipal elections and their supporters should join campaigns of prefectural assembly candidates — well before the *de jure* campaign period for their own municipal elections begins — if they hope to make their names known and strengthen ties with voters. 

The *de facto* campaign period for prefectural assembly elections, therefore, becomes the *de facto* campaign period for municipal elections. This mixed motive would make it difficult for the authority to pin down illegal activities, thereby encouraging municipal politicians to deploy their campaigns openly. By using an individual-level survey, Imai (2009) showed that voters in municipalities where their own simultaneous local elections were held in April 2007 indeed experienced more mobilization efforts than did voters in the other municipalities.

Moreover, given the clientelistic relationship between municipal and prefectural politicians (Curtis 1971, ch. 2, Inoue, 1992, Tanaka, 2007), municipal assembly members and mayors also act as a “machine” for prefectural assembly candidates; namely, municipal politicians ask their supporters to vote for their “boss” (oyabun) prefectural assembly candidates. In return, prefectural assembly members reward their “henchmen” (kobun, i.e., municipal politicians) with votes and campaign funds in the next municipal elections. They also provide constituency services, which prefectural governments can provide, to municipal politicians’ supporters, such as responding to their requests for developing/upgrading prefectural roads, agricultural facilities, schools, etc. According to some surveys (Moriwaki 1984, pp. 86, 90, Shinada et al., 2013, pp. 70–72, Tani, 1987, p. 433), two thirds of municipal assembly members are active campaigners during prefectural elections; they engage in prefectural get-out-the-vote efforts through active calls and mails, asking their own supporters to go to prefectural candidates’ rallies, where they make endorsement speeches. In return, half of prefectural assembly members do the same things for municipal assembly candidates.

### 3.2. Omitted variables

The omitted variable problem has impeded previous investigations of the effects of concurrent elections. A solution, as noted by Meredith (2013, p. 759), is a natural experiment: “for estimating spillover effects in observational data … the best case scenario … is that it uncovers something that approximates experimental variation in the outcome.” We argue that we have discovered just such an ideal scenario in Japan: the problem is addressed by employing district fixed effects and utilizing an as-if random assignment of municipal elections.

#### 3.2.1. District-level variables

District fixed effects are powerful enough to control all district-level omitted variables that are constant within each prefectural district. According to the *Public Offices Election Act*, a prefectural district should be, in principle, composed of one city or one county that includes multiple towns and villages. In order to ameliorate the malapportionment of seats, however, a few cities and counties may (in some cases, must) be combined to form a prefectural district. In our sample, the average number of municipalities in a prefectural district is about two. Among 1,022 prefectural districts in our data, as many as 35.4% (28.7%) contain municipalities with at least one legislative (executive) treated municipality and one control municipality. For example, Fig. 2 shows 14 prefectural districts in Tokushima Prefecture.

---

8 We excluded 203 municipalities from the analysis. First, we discarded all 174 municipalities in three prefectures of Ibaraki, Tokyo (strictly speaking, not Prefecture, but Metropolitan Area), and Okinawa, because these prefectures did not hold their assembly elections on April 13, 2003; therefore the outcome variable was missing. Second, we excluded 15 municipalities in Kagoshima Prefecture, because they did not hold the first simultaneous local elections in 1947 under U.S. occupation. Third, we omitted the village of Oga in Akita Prefecture, which was established by reclamation in 1964. The second and third groups were off the simultaneous local election cycle from the beginning and, thus, should follow different data generation processes of the treatment variables from other municipalities. The final set of municipalities excluded from analysis is the 13 largest cities, because their simultaneous local elections (if any) were scheduled on the same day as prefectural elections.

9 Since only 11 prefectures held gubernatorial elections on April 13, 2003, and one of them was uncontested, we will not use voter turnout in gubernatorial elections as an alternative outcome variable (which is almost the same as voter turnout in prefectural assembly elections held on the same day in any case). Accordingly, unless otherwise noted, the term “prefectural elections” in this paper refers to prefecture assembly elections.

10 The *de jure* campaign period for the gubernatorial elections was March 27 to April 12 (17 days). See also fn. 9.

11 Niven (2002) shows that the more recent mobilization activities are, the higher the voter turnout. It is also important to mention the electoral system used in municipal assembly elections: the single non-transferable vote system with an at-large district. As this system tends to produce very small vote margins between candidates, particularly in small towns and villages, candidates are strongly motivated to mobilize voters (Horiuchi, 2005), even taking illegal measures (Fukumoto and Horiuchi, 2011).
differentiated by colors. White borders indicate legislative (upper panel) or executive (lower panel) treated municipalities. Fig. 3 shows an electoral district (the 13th District, highlighted areas) that has both treated (thick black borders) and control (thin black borders) municipalities.

Thus, we will add district fixed effects to our regression model. The virtue of this strategy is that we can compare the outcome variable between treated and control municipalities within the same prefectural district, while controlling for all district-level—observable and unobservable—variables. The controlled variables include the number of seats, the number and profiles of candidates, the degree of competitiveness, and whether the election is contested or not. They are obviously important variables explaining the level of voter turnout in prefectural elections. As these prefectural districts are nested within each prefecture, we can also completely control all prefecture-level variables, such as whether or not a gubernatorial election was concurrently held with a prefectural assembly election on April 13, 2003. Furthermore, a range of municipality-level variables should be similar among municipalities within such a small geographical area as a prefectural district, partly because an electoral law stipulates that electoral districting should consider geographical features and transportation networks.\(^{14}\) For example, weather on election day (Gomez et al., 2007), degree of urbanization (Richardson, 1973), and density of social networks (Cox et al., 1998) predict voter turnout, but they should not have a large variation within each small prefectural district.

3.2.2. Municipality-level variables

Although adding district fixed effects is a powerful strategy to deal with district-level omitted variables that are constant within each prefectural district, it cannot control municipality-level omitted variables that are not constant within each prefectural district (e.g., features of each municipal election, such as the number of seats, the number and profiles of candidates, the degree of competitiveness, and whether the election is contested or not). If they were correlated with both turnout in a prefectural election (\(Y\) in Equation (2)) and the concurrence of a municipal election (\(X\)), we would not obtain an unbiased estimate of the effect of concurrent elections.

Fortunately, according to Fukumoto and Horiuchi (2011) and Fukumoto and Ueki (2015), the presence or absence of a municipality election on April 27, 2003, is as-if randomly assigned. They present the following rationale: with the establishment of the current constitution, the first elections of assembly members and chief executives in all prefectures and municipalities were called in April 1947. By law, subsequent elections of four-year-term assembly members and chief executives are scheduled to be held simultaneously every four years in April, unless their terms are censored (or postponed) for some reason. Once an election drops off the cycle, new assembly members or chief executives serve not for the remaining (less than four) years but for the full term of four years, and the election typically remains off the cycle thereafter. Note that the reasons for municipalities to drop off the cycle in the past (e.g., municipal mergers in the 1950s, the death of mayors, etc.) are unrelated to incentives and behavior of voters and candidates in elections in 2003.

If the timing of a municipal election is exogenously determined, as Fukumoto and Horiuchi (2011) and Fukumoto and Ueki (2015) claim, then potential municipality-level confounding variables—observable or unobservable—should be balanced; that is, their distributions are expected to be the same between the treated and control municipalities. In this case, the OLS estimator of the effect of election concurrence will be unbiased, \(E(\hat{\beta}) = \beta\).

3.3. Bundling treatments

Why is Japan’s institutional setup valid in addressing the second identification problem, bundling treatments? The answer is that our design does not allow us to interpret the estimated coefficient as the effect of cost sharing or psychological stimulus. Table 1 illustrates four general research designs for effects of (almost) concurrent elections. The two rows represent different timings of concurrent elections. When two elections are held on the same day, we call them “exactly concurrent” (upper row). When they are held close to each other (e.g., a few weeks apart), but not on the same day, we call them “proximately concurrent” (lower row). Among the three bundling treatments considered in the second section, cost sharing can be responsible for the effects of exactly concurrent elections, but not proximately concurrent elections. Hence, in our case, \(\hat{\beta}_c = 0\) in Equation (2). This is because if two elections are held on different days, citizens need to go to the polling stations twice and thus cannot save the shared cost of

---

\(^{14}\) Public Offices Election Act, Article 15, Section 7.
voting ($C_0$ in Equation (1)).

The two columns distinguish which of the two elections is more stimulating. The left column corresponds to the effect of having a high-stimulus election on voter turnout in a low-stimulus election, which we call a “high-to-low” treatment effect. The right column corresponds to the effect of having a low-stimulus election on voter turnout in a high-stimulus election, which we call a “low-to-high” treatment effect. Psychological stimulus can be responsible only for the high-to-low treatment effect, because it is illogical to argue that a low-stimulus election is so exciting that it would draw many citizens to a high-stimulus election. Our case estimates the low-to-high treatment effect. Therefore, $\beta_P = 0$ in Equation (2).

Four general research designs for effects of almost-concurrent elections are constructed based on these two dichotomies. Each cell in Table 1 displays which treatment among the three bundling treatments can be responsible for the effect of concurrent elections. Our case belongs to the bottom right cell (shaded box). Since the prefectural and municipal elections are held on April 13 and April 27, 2003, respectively, this is a case of proximately concurrent elections (lower row). Furthermore, we estimate the low-to-high treatment effect (right column) by regressing turnout of a (high-stimulus) prefectural election on the occurrence of a (low-stimulus) municipal election, not vice versa. Accordingly, our design allows us to attribute the effect of concurrent elections to neither cost sharing ($\beta_C = 0$ in Equation (2)) nor psychological stimulus ($\beta_P = 0$). Therefore, the only possible treatment causing the outcome is mobilization, $\beta \equiv \beta_M + \beta_C + \beta_P = \beta_M$.

4. Results

It is essential for any allegedly natural experiment to be subject to careful scrutiny of assumptions; in particular, the balance of covariates (Dunning, 2012). Thus we begin by checking the balance of municipality-level variables between the treated and control municipalities. Then we estimate the effects of having proximately concurrent and low-stimulus municipal elections on voter turnout.

---

15 Some readers might suspect that the costs of obtaining information about candidates/issues in one election decreases if another election is forthcoming. We do not consider it a convincing counterargument, for two reasons. First, to the best of our knowledge, no rational choice theorist has argued that information costs would decrease in the case of concurrent elections. Rather, they argue, “This cost of information is an increasing function of the number of elections” (Fauvelle-Aymar and François, 2015, p. 185; see also Wolinger, 1994, p. 77). Second, if information costs decreased in concurrent elections, probable reasons would be media coverage or mobilization. However, in this case, the ultimate cause of higher turnout is not cost sharing but psychological stimulus or mobilization.

16 Note that we cannot regress turnout of a (low-stimulus) municipal election on the occurrence of a (high-stimulus) prefectural election, because prefectural elections occurred in most prefectures.
Fig. 3. Distribution of Treated Municipalities within a Prefecture District.

Table 1
Types of research designs.

<table>
<thead>
<tr>
<th>Treatment election:</th>
<th>High stimulus</th>
<th>Low stimulus</th>
<th>Low stimulus</th>
<th>High stimulus</th>
</tr>
</thead>
<tbody>
<tr>
<td>Outcome election:</td>
<td></td>
<td>Low stimulus</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Two elections are</td>
<td>Mobilization</td>
<td>Cost Sharing</td>
<td>Psychological Stimulus</td>
<td>Mobilization</td>
</tr>
<tr>
<td>exactly concurrent</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Two elections are</td>
<td>Mobilization</td>
<td>Cost Sharing</td>
<td>Psychological Stimulus</td>
<td>Mobilization</td>
</tr>
<tr>
<td>approximately concurrent</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Note: The outcome variable is voter turnout in an “outcome election,” while the treatment variable is whether or not a “treatment election” is held concurrently with the outcome election. Each cell shows possible treatment(s) which can explain the effect of concurrent elections.
in a high-stimulus prefectural election.\textsuperscript{17}

In both balance tests and the estimation of treatment effects, the unit of observations is a municipality that existed as of April 27, 2003.\textsuperscript{18} We establish two treatment variables ($X$): the “legislative treatment” is a dummy variable indicating whether a municipal assembly election was held on April 27, 2003, while the “executive treatment” is a dummy variable indicating whether a mayoral election was held on April 27, 2003.

\subsection*{4.1. Balance of municipality-level variables}

We tested the balance of as many as 96 municipality-level variables (e.g., demographic, economic, and fiscal variables) in a large dataset of municipality-level statistics published annually by the government.\textsuperscript{19} We regress $j$-th municipality-level variables $Z_j$ on either legislative or executive treatment ($X$). We also add 1,022 district dummy variables $D_k$’s, which is equal to one if an observation (municipality) belongs to prefectural district $k$, and zero otherwise.\textsuperscript{20}

$$Z_j = \beta_j X + \sum_{k=1}^{1,022} \delta_{jk} D_k + \epsilon_j$$

If these municipality-level variables are balanced between the two groups, the OLS estimates of their coefficients ($\beta_j$) should not be significantly different from zero.

The results suggest that most municipality-level variables are indeed well balanced. The number of the coefficient estimates is 192, which is equal to the number of municipality-level variables (96) times the number of treatment variables (2). The $p$-values for the estimates are smaller than 0.05 in 14 (7.3%) out of 192 estimates.\textsuperscript{21} Twelve are for the legislative treatment, while two of them are for the executive treatment. No municipality-level variable is significantly correlated with both treatment variables. In sum, these results empirically validate Fukumoto and Horiuchi’s (2011) claim that the timing of a municipal election approximates a municipality voter turnout in a prefectural election (otherwise.\textsuperscript{20})

14 unbalanced municipality-level variables are reported in Online Appendix.

\subsection*{4.2. Effect of overall mobilization}

We estimate the effects of mobilization by two models. In one, municipality voter turnout in a prefectural election ($Y$) is regressed on either legislative or executive treatment ($X$) and district dummy variables $D_k$’s:

$$Y = \beta X + \sum_{k=1}^{1,022} \delta_k D_k + \epsilon.$$ \textsuperscript{(3)}

The other model includes the unbalanced 14 municipality-level variables:

$$Y = \beta X + \sum_{j=1}^{14} \gamma_j Z_j' + \sum_{k=1}^{1,022} \delta_k D_k + \epsilon.$$ \textsuperscript{(4)}

where we renumber $j$ so that $Z_1, \ldots, Z_{14}$ are the 14 unbalanced municipality-level variables.

Some may suspect that the legislative and executive treatments may not be independent of each other. Thus, we divide municipalities into two sub-datasets depending on the status of one treatment variable, and estimate the effects of the other treatment variable. For example, we estimate the effects of the legislative treatment by using all municipalities (the “pooled dataset”), municipalities with a mayoral election (the executive “treated sub-dataset”), and municipalities without it (the executive “control sub-dataset”).\textsuperscript{22}

Given that there are two treatment variables (legislative and executive), two models (Equations (3) and (4)), and three datasets (one pooled and two subsets), the total number of estimates of treatment effects is 12 ($2 \times 2 \times 3$). The results are presented in Fig. 4 and Tables 2 and 3.\textsuperscript{23} In Fig. 4, each horizontal bar shows a 95% confidence interval: if it does not include zero, an estimated effect is statistically significant at the 5% level. Three types of symbols indicate point estimates: black dots ($\bullet$) are based on the pooled dataset; white triangles ($\triangle$) are based on the treated sub-datasets; white boxes ($\Box$) are based on the control sub-datasets.

Fig. 4 illustrates that the point estimates of the treatment effects are all positive, as expected, and their magnitudes are about one to two percentage points. These findings do not change substantially regardless of whether or not we add the 14 unbalanced variables, and whether we use a pooled dataset or a sub-dataset. A close look at Fig. 4 may suggest that the legislative treatment effects are slightly larger than the executive treatment effects. Most likely, this is because the number of municipal candidates — active campaigners during prefectural elections — is much larger in assembly elections than in mayoral ones. As a consequence, the effects of mobilization on voter turnout tend to be larger. These nuanced differences are consistent with our theoretical arguments. In short, the results suggest that, in Japan, when a municipal election is scheduled in two weeks, voter turnout in a prefectural election increases.

The magnitude of the estimated effects — one to two percentage points — is not so small, considering effects of concurrent elections found in the previous studies. Voter turnout in the Japanese lower house election is higher by three percentage points when the upper house election is held concurrently (Asano, 1998). In the U.S., when a senatorial election is held concurrently, voter turnout in House contests increases by 1.1 (Cox and Munger, 1989) or six percentage points.

\textsuperscript{22} An alternative approach is to regress on the legislative and executive treatments and their interaction term using the pooled dataset and estimate conditional effects. In Online Appendix, we report the results of the interactive models and discuss why we prefer to report the results based on estimations using sub-datasets as our main results.

\textsuperscript{23} The number of observations is smaller than the number of municipalities mentioned in Section 3.1 (namely, 2,984) because municipalities in uncontested prefectural districts are not included due to missing values. When we include the 14 unbalanced municipality-level variables, the number of observations decreases slightly due to some missing values in these variables. The coefficient estimates of the 14 unbalanced municipality-level variables are reported in Online Appendix.
points (Caldeira et al., 1985). Concurrently held gubernatorial races increase turnout in presidential elections by six to eight percentage points (Boyd, 1986, 1989) and turnout in Congressional elections by 1.4 percentage points (Cox and Munger, 1989). In Latin American countries, voter turnout for legislative elections held concurrently with presidential elections is five percentage points higher than otherwise (Fornos et al., 2004). Worldwide, concurrently held legislative elections raise voter turnout in presidential elections by

![Fig. 4. Estimates of Treatment Effects.](image-url)

**Table 2**

<table>
<thead>
<tr>
<th>Specification</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
</tr>
</thead>
<tbody>
<tr>
<td>Municipality-Level Variables</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
</tr>
<tr>
<td>Executive Treated Sub-dataset</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
</tr>
<tr>
<td>Executive Control Sub-dataset</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
</tr>
</tbody>
</table>

| Point estimate | 2.067 | 2.248 | 1.813 | 2.089 | 1.813 | 2.051 |
| Standard error | 0.379 | 1.461 | 0.442 | 0.331 | 1.161 | 0.389 |
| p-value | 0.000 | 0.125 | 0.000 | 0.000 | 0.190 | 0.000 |

| Num. of prefectural districts | 668 | 248 | 594 | 631 | 357 | 567 |
| Num. of municipalities | 1,922 | 417 | 1,505 | 1,864 | 600 | 1,464 |
| Only prefectural districts with a variation in treatment variable | 217 | 31 | 184 | 216 | 30 | 184 |
| Num. of municipalities | 1,305 | 90 | 946 | 1,293 | 47 | 941 |
| Num. of treated municipalities | 675 | 49 | 444 | 670 | 48 | 441 |
| Num. of control municipalities | 630 | 41 | 502 | 623 | 39 | 500 |

**Note:** The unit of observations is a municipality. The treatment variable (X) is the legislative treatment. The outcome variable (Y) is voter turnout (%) in a prefectural election. Clustered standard errors are reported where clusters are districts. Each specification includes district fixed effects.

**Table 3**

<table>
<thead>
<tr>
<th>Specification</th>
<th>7</th>
<th>8</th>
<th>9</th>
<th>10</th>
<th>11</th>
<th>12</th>
</tr>
</thead>
<tbody>
<tr>
<td>Municipality-Level Variables</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
</tr>
<tr>
<td>Legislative Treated Sub-dataset</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
</tr>
<tr>
<td>Legislative Control Sub-dataset</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
<td>✓✓✓</td>
</tr>
</tbody>
</table>

| Point estimate | 1.597 | 1.842 | 1.492 | 1.010 | 0.923 | 0.513 |
| Standard error | 0.473 | 0.668 | 0.877 | 0.388 | 0.485 | 0.799 |
| p-value | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.058 |

| Num. of prefectural districts | 668 | 487 | 398 | 631 | 465 | 382 |
| Num. of municipalities | 1,922 | 1,033 | 889 | 1,864 | 1,003 | 861 |
| Only prefectural districts with a variation in treatment variable | 174 | 111 | 61 | 171 | 111 | 58 |
| Num. of municipalities | 1,127 | 526 | 264 | 1,109 | 521 | 252 |
| Num. of treated municipalities | 339 | 213 | 85 | 332 | 211 | 81 |
| Num. of control municipalities | 788 | 313 | 177 | 788 | 310 | 171 |

**Note:** The unit of observations is a municipality. The treatment variable (X) is the executive treatment. The outcome variable (Y) is voter turnout (%) in a prefectural election. Clustered standard errors are reported where clusters are districts. Each specification includes district fixed effects.
two percentage points (Dettrey and Schwindt-Bayer, 2009). Note that these estimates are likely to estimate the effects of the bundling treatments, which include not only the effect of mobilization ($\beta_m$ in Equation (2)) but also the effects of cost sharing ($\beta_c$) and psychological stimulus ($\beta_p$). Accordingly, we argue that our estimates, which capture only the effect of mobilization, are fairly substantial.

Finally, note that four of the five insignificant estimates (Specifications 2, 5, 9, and 12) are for specifications with small numbers of observations used in estimation. By adding district fixed effects, in effect, we only use municipalities in prefectural districts with a variation in a treatment variable. The numbers of these municipalities (i.e., the effective numbers of observations) are shown in the last three rows of Tables 2 and 3. The numbers are more than 1,000 in the case of the pooled dataset, but less than 100 in the case of the executive-treated sub-dataset (Specification 1) and even less than 100 in the case of the executive-treated sub-dataset (Specifications 2, 5, and 12). In all five specifications, including Specification 1, the $p$-values are greater than 0.05 because their standard errors are large — not necessarily because their point estimates are small.²⁴

5. Conclusion

In this paper, we have shown that voter turnout in Japanese prefectural elections is, on average, higher by one to two percentage points when municipalities hold their own elections in two weeks. Even candidates in lowest-level (i.e., municipal) elections have the ability to mobilize a nontrivial number of voters to the polls in middle-level (e.g., prefectoral) elections, as much as highest-level politicians (e.g., national lawmakers) do in other contexts (Asano, 1998; Cox and Munger, 1989; Dettrey and Schwindt-Bayer, 2009). The effects are statistically significant under various specifications.

Importantly, our design not only mitigates the problem of omitted variable bias, but also guarantees that the effects of proximately concurrent elections are attributable only to mobilization — not to cost sharing or psychological stimulus. We claim to have successfully identified the causal effect of overall mobilization on voter turnout. While numerous studies show the effects of particular mobilization tactics based on randomized field experiments, to the best of our knowledge, this is the first empirical study estimating the effect of all sorts of mobilization activities — partisan and nonpartisan, direct and indirect campaign efforts. We hope that our findings shed light on a problem that has not been fully examined in the literature: To what degree do citizens vote not because of their intrinsic motivation but because of extrinsic mobilization?

We would suggest three avenues for future research. The first is the causal effect of overall mobilization in other contexts. While this study shows that the overall mobilization effect in Japanese local elections is one to two percentage points, the magnitude is very likely contextual. If we examined other settings — such as national-level elections or elections in other countries — the sizes of overall mobilization effects could well be different. Research on various setups will suggest the average size of overall mobilization effects and enable us to understand how, and why, overall mobilization effects vary in different settings. This endeavor is also relevant to the debate on institutional prescriptions for “democracy’s unresolved dilemma” (Lijphart, 1997) — low turnout in many democracies. Changes in election rules may encourage or discourage mobilization activities.

The second promising research avenue is to explore election timing as a treatment in natural experiments. Randomized experiments have identified narrowly specified treatment effects. As argued above, however, political scientists are often interested in theoretically broader causes and consequences of elections. Observational studies could leverage as-if random variations in election timing to fill the gap in knowledge about political behavior during the period leading up to the election day, as well as political and policy consequences of elections. Besides our case of Japan, similar settings exist in the U.S. (Anzia, 2011; Berry and Gersen, 2011; Titunik, 2016), France (Fauvelle-Aymar and François, 2015), Germany (Garman, 2016), and Mexico (Magar, 2012; Rosas and Langston, 2011), though we believe there still remain many other countries yet to be investigated. We hope that the present study mobilizes researchers to take advantage of intriguing natural experiments, thereby enriching our understanding of political behavior.

Finally, to understand the relationship between national and local politicians in Japan, it will be useful to revisit the scholarly debate on the so-called “year-of-the-Boar phenomenon” — every twelfth year (year of the Boar), voter turnout in upper house elections (held once every third year in June or July) tends to be low shortly after simultaneous local elections (held once every fourth year in April). Although the pattern is noticeable and widely known, what causes it is still a matter of debate. Asano (1998) and Ishikawa (1984) argue that it is because local politicians have the least incentive to mobilize their supporters for their boss national politicians, while Araki (1990) doubts this explanation. Imai and Kabashima (2008) claim that local politicians are tired of mobilizing (see also Wakayama (2006)), while Mifune (2008) suspects fatigue of voters, rather than politicians. It should be particularly interesting to investigate whether the relationship between national and local politicians has changed after the electoral reform of 1994 or after the change of power from the long-dominant Liberal Democratic Party to the Democratic Party of Japan in 2009. As Horiuchi (2009) argues, it is important to shed more light on the influence of local politics on national-level politics, and Japan would provide an interesting case study for pursuing this research agenda.

Online Appendix

Online appendix of this article can be found at http://dx.doi.org/10.1016/j.electstud.2016.08.003.

References


²⁴ Online Appendix reports the estimation results of the interactive models using the pooled dataset (also see fn. 22). The point estimates of the treatment effects are very similar to those in Fig. 4, which is based on the non-interactive models. The confidence intervals tend to be narrower when using the interactive models because of inappropriately larger (effective) number of observations used in estimation.