



# Identifying the effect of mobilization on voter turnout through a natural experiment<sup>☆</sup>



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## ABSTRACT

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 prefectural 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.

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## 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; Wolfinger and Rosenstone, 1980).<sup>1</sup> Earlier 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).<sup>2</sup> 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.<sup>3</sup>

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

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<sup>1</sup> Throughout this paper, we use the standard definition of mobilization from a classic work by Rosenstone and Hansen, which is “the process by which candidates, parties, activists, and groups induce other people to participate” (Rosenstone and Hansen, 1993, pp. 25–26).

<sup>2</sup> In the studies using the aggregated data, the amount of campaign expenditures is used as a proxy for the level of partisan mobilization.

<sup>3</sup> As of writing this draft (April 27, 2016), Gerber and Green (2000) has been cited 360 times according to the Thomson Reuters' Web of Science. García Bedolla and Michelson (2012) alone undertook 268 get-out-the-vote experiments from 2006 to 2008.

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.<sup>4</sup>

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 prefectural elections.<sup>5</sup> 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.<sup>6</sup> 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 macroeconomically 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

<sup>4</sup> 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.

<sup>5</sup> 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).

<sup>6</sup> For a review of empirical studies taking this approach, see Geys (2006).

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