Rain, Election, and Money: The Impact of Voter Turnout on Distributive Policy Outcomes*

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Abstract

Does political participation affect policy outcomes? To answer this long-standing but recently re-visited question, we estimate the effect of voter turnout on distributive policy outcomes using a large municipality-level data set from Japan. We argue that existing studies do not sufficiently cope with omitted variable bias – the omission of theoretically relevant variables from analysis; particularly, politics *before and after the voting day*. By exploiting a highly valid instrumental variable based on our new data measuring the amount of rainfall *on the voting day*, we show that the turnout effect is indeed significantly positive and its magnitude is far larger than an estimate based on the ordinary least square regression.

> "*I do not mean to misuse or abuse subsidies from the central government to municipalities. However, municipalities with low turnout rates deserve an appropriate penalty and those with high turnout rates deserve a reward. This is how democracy works.*" (Japanese Prime Minister Yasuhiro Nakasone)[1]

1. Introduction

This paper examines a long-standing but recently re-visited question: Does political participation affect policy outcomes? To answer this question, using a large municipality-level data set from Japan, we estimate the effect of voter turnout on distributive policy outcomes – i.e., the amounts of intergovernmental fiscal transfers of public expenditures for geographically specific projects and programs.

More than half a century ago, Schumpeter (1942) argued that capitalism, especially the proper conduct of entrepreneurship, was "fettered" by the government's pursuit of interests for those who are in power and who actively participate (i.e., intervene) in policy processes. Political scientists, most notably Dahl (1956) and Key (1949), also expressed similar concerns. More recently, in his well known presidential address to the American Political Science Association, Lijphart (1997) argued that low and declining voter turnout is "democracy's unresolved dilemma" because such "unequal participation" would translate into disproportionate influence on policy outcomes.

While Lijphart, as well as earlier scholars, warned of the policy consequences of unequal participation primarily on normative grounds, a growing number of recent

[1] Party President's address at the Liberal Democratic Party's Council of Prefectural Chief Secretaries on May 19, 1983.

scholars examine the effects of political participation (specifically, voter turnout) from positive perspectives (Falaschetti 2003; Fleck 1999; Hill and Leighley 1992; Hill, Leighley, and Hinton-Andersson 1995; Martin 2003; Martinez 1997; Mueller and Stratmann 2003). The rationale underlying their arguments that voter turnout affects policy outcomes is twofold. First, voter turnout is the most useful indicator for policy-makers to estimate how many of the public are "attentive" (Martin 2003) or "responsive" (Franklin and Wlezien 1997) to the policy issues at stake. Second, a rise in public attention or response to locally politicized issues, which is indicated by higher turnout rate, is likely to lead policy-makers to allocate a disproportionately larger amount of government resources. This is because, in cases of intergovernmental fiscal transfers, when pork-barrel projects and programs are politicized, the allocation of public expenditures is expected to solidify ruling parties' stronghold or cultivate new supporters more effectively.[2]

Our study is grounded in this theoretical argument, but we challenge the existing studies from a methodological perspective. While *time order* in independent variables is one of the most important considerations in causal analysis (e.g., King and Zeng 2006), it has not been fully taken into account in the existing empirical studies that investigate the impact of voter turnout on distributive policy outcomes. More concretely, we argue that the existing studies do not sufficiently cope with omitted variable bias – the omission of

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[2] This spending includes not only the implementation of popular projects as initially planned but also implementation (or termination) of unpopular projects with significant compensation to stakeholders. As long as a locality faces a politicized issue, the policy will be implemented at cost or revamped with significant transaction costs.

theoretically relevant variables from analysis; particularly, politics *before and after the voting day*. The vast literatures of electoral, legislative, and distributive politics suggest a number of important pre-election and post-election variables, which are correlated with both voter turnout and distributive policy outcomes. They include electoral mobilization during the campaign period (e.g., Rosenstone and Hansen 1993), day-to-day interactions between politicians and voters in local constituencies (e.g., Fenno 1978), and various vote-buying strategies in legislative processes (e.g., Cox and McCubbins 1986; Dixit and Londregan 1996, 1998; Myerson 1993).

The existing empirical studies indeed provide mixed results perhaps due to misspecification of their statistical models. For example, in a recent empirical study estimating the impacts of voter turnout on the per capita amount of federal expenditures in the United States, Martin (2003) shows that voter turnout has a significantly positive effect (at 5% level) on bi-annual changes in three of six two-year cycles from 1984 to 1994 (House-based analysis, Tables 1a and 1b). The effect is even *negative* and significant at 1% level in the 1990 and 1992 cycle. As Martin (2003) admits, "Voter turnout may be acting as a surrogate for other omitted characteristics" (p. 120).

We provide a solution to this methodological problem by running two-stage least square (2SLS) regressions using a highly valid instrumental variable based on our new data measuring the amount of rainfall *on the voting day*. The rainfall is expected to suppress voter turnout, but it is uncorrelated with any aspects of politics *before and after the voting day*. Therefore, election day precipitation provides ideal inferential leverage as an instrumental variable with which to estimate the impacts of voter turnout on policy outcomes.

In what follows, we first discuss why a variety of pre-election and post-election politics should not be disregarded from analysis. In the third section, we introduce our statistical models and variables. The results are shown in the fourth section – the turnout effect is indeed significantly positive and its magnitude is far larger than the conventional estimate based on the ordinary least square (OLS) regression. The final section concludes by discussing normative implications.

2. Politics Before and After the Voting Day

Although we argue that a higher turnout rate will boost the level of government spending, it is not the only factor that affects the allocation of pork. Given the complexity of electoral, legislative and budgetary processes, our hypothesis testing can be confounded if we ignore important features of pre-electoral and post-electoral politics.

2.1 Pre-election Politics and Government Spending

Conceptually, pre-electoral politics involves features that directly affect voter turnout. While a portion of voter turnout on the day of election can be swayed by a short-run exogenous stimulus (e.g., weather of the polling day), the observed turnout rate also reflects months and years of electioneering activities. Political parties and individual candidates expend significant efforts to mobilize voters by means of direct contact (e.g., candidates shaking hands with their constituents, party leaders' visits to specific districts during the campaign period) and more indirect campaign tactics (e.g. soliciting "block votes" through various social networks, canvassing efforts by local activists). The empirical literature of voting behavior suggests that these mobilization efforts are indeed important determinants of voter turnout (e.g., Cox, Rosenbluth, and Thies 1998;

Rosenstone and Hansen 1993).

It is important to note that these pre-election political activities may also affect policy outcomes, because the ground battle of electioneering activities during the campaign period and the day-to-day interactions with voters are important for political candidates in learning citizens' policy needs. For example, canvassing and small group meetings provide politicians with opportunities to learn about their constituents. When politicians knock on doors, they not only greet and explain their policies to the voters but also listen to the voters' complaints and demands. By doing so at as many households as possible within the district, candidates are able to learn about the constituents they represent, ranging from demographic and employment structures to repair needs of the municipal road network (Curtis 1971, Fenno 1978).

In short, political interactions between candidates and voters before the voting day are important determinants of both voter turnout (our key independent variable) and policy outcomes (our dependent variable). Therefore, we need to control their effects in order to estimate the marginal effect of voter turnout. The problem is that we cannot fully measure such political interactions using aggregate data. Surveys would be an alternative approach to measuring voters' attitudes and behaviors in various political processes, but they are not effective in measuring how much policy benefit voters actually receive.[3] As we will introduce in Section 3, we partially control them by adding two indirect measures

[3] Surveys can measure the perceived level of policy benefits. This variable suffers from not only serious measurement error (as voters do not necessarily know about the amount of benefits they receive) but also simultaneity – Voters who perceive benefits are more likely to say that they actively participate in political processes, including voting in elections.

for the intensity of mobilization activities, which are by no means sufficient in controlling a variety of pre-voting relevant political variables.

2.2 Post-election Politics and Government Spending

Once the voting results are publicly announced, the structure of political bargaining enters a new phase. In parliamentary democracies, political parties and/or intra-party factions form a winning coalition and choose the prime minister. The cabinet is formed and incumbents are assigned to various government and party posts. Then, leaders of the ruling coalition and individual elected incumbents begin coordinating pork-barrel projects and programs so that government resources are allocated in accordance with their reelection strategy.

This geographic allocation of public expenditures is affected by how the ruling parties and/or incumbent politicians configure their optimal resource allocation. Some scholars hypothesize that they have an incentive to direct disproportionately larger amounts of fiscal resources to districts/groups with "swing voters" (e.g., Bickers and Stein 1996; Dahlberg and Johansson 2002; Dixit and Londregan 1996, 1998; Lindbeck and Weibull 1987; and Stein and Bickers 1994). On the contrary, others present and test an alternative "core partisan" hypothesis – larger amounts directed to districts/groups with loyal supporters (e.g., Cox and McCubbins 1986; Levitt and Snyder 1995).

Although we agree that the political actors' strategic calculations are relevant in shaping distributive policy outcomes, we should treat these variables carefully in our analysis. This is because these existing models of distributive politics typically assume that either voter turnout is fixed or no abstention is observed (e.g., Dixit and Londregan 1996, 1998; Cox and McCubbins 1986). In other words, "swing voters" or "core supporters" can be defined *given voter turnout*. If the level of voter turnout varies across regions/groups, the cost of mobilization may change, thereby changing political actors' optimal allocation strategies. This means that post-election political incentives and strategies are also correlated with both policy outcomes and voter turnout.

As is the case with pre-election political variables, these post-election variables are either unobservable or difficult to measure. In addition, even when some observable indicators are added as control variables in aggregate-data analysis, the relevance of variables, which can only be defined given voter turnout, introduces another complication – "post-treatment bias." We will discuss this problem in detail in the next section.

3. Methods

To estimate distributive consequences of voter turnout, we use a quite large sample of panel observations from Japan. We first discuss potential sources of bias due to the exclusion and inclusion of relevant variables. To cope with these problems, we use an instrumental variable and run two-stage least square (2SLS) regressions. After introducing variables for regressions, we note several issues of model specification.

3.1 Potential Sources of Bias

In Section 2, we argued that there are politically-relevant variables which are correlated with both voter turnout (our key independent variable) and distributive policy outcomes (our dependent variable) – politics before and after the voting day. One methodological problem is that these variables are difficult to measure with aggregate data. If we simply drop these variables from our regression equations, our estimates suffer from the common problem of omitted-variable bias.

There is, however, another problem that has been disregarded in the existing literature; namely, that the inclusion of variables which are, at least in part, consequences of the key causal variable (i.e., voter turnout) will lead to "post-treatment bias" (Rosenbaum 1984). For instance, suppose that our dependent variable is the amount of intergovernmental fiscal transfers based on settled account data, while using the amount of intergovernmental fiscal transfers based on budget data as a control variable. Since each fiscal year's budget is obviously and strongly correlated with that year's closing account, including it as a control variable washes away the effects of many political, economic, demographic and cultural variables. It is a difficult problem to cope with, because "even if dropping out these variables alleviates post-treatment bias on one hand, it will likely induce omitted variable bias on the other" (King and Zeng 2006, 147). In short, neither adding post-treatment variables nor dropping them produces valid causal estimates.

This problem has been largely ignored in the empirical literature of distributive politics, or more generally in the political science literature. It is, however, a serious issue for political scientists, particularly for those who use cross-national and/or time-series data, because a change in a subject of investigation (i.e., a new event, a policy change, or a collective action taken) can cause more than one change in political actors' attitudes, opinions, and behaviors, as well as various other political and policy conditions. Perhaps, we can even say confidently that it is almost impossible to assume, in real life, that when the value of a key independent variable changes, the values of all causally posterior variables – other than a particular dependent variable – remain constant. In political and historical analysis, there should always be some post-treatment variables, in sequential order, between the occurrence of a researcher's key independent variable and the occurrence of his/her dependent variable. For this reason, King and Zeng (2006) see this as "the fundamental problem with much research in comparative politics and international relations" (p. 147).

King and Zeng (2006) propose two ways to avoid this problem. The first is to define *multiple-variable causal effects*; namely, to add both treatment (i.e., key independent) and post-treatment variables in analysis and conduct counter-factual analysis by moving both variables simultaneously while holding other variables constant. The second approach is to measure the two variables using a single dimension. We do not necessarily think that these are widely applicable approaches. Although conducting the suggested method of counter-factual analysis is technically uncomplicated, there is no guarantee that this procedure is based on valid parameter estimates, particularly when the treatment and post-treatment variables are correlated. The second approach is inapplicable, when we are interested in a particular observable variable, such as voter turnout in our study.

The recent statistical literature suggests a more advanced approach of principal stratification (Frangakis and Rubin 2002, Zhang and Rubin 2003). This approach may be valid when we can specify and measure post-treatment variables. For example, in the case of a randomized medical trial, researchers observe who died during the trial – an important post-treatment variable. In observational studies, such as ours, it is difficult to operationalize all relevant post-treatment variables. Acknowledging various limitations in these existing approaches, we decided to take another method, which is to exploit an instrumental variable – a simple, widely used and valid method under certain conditions. As long as we can find a valid instrument, it should help us cope with the bias due to the exclusion of relevant variables, as well as the inclusion of post-treatment variables.[4]

3.2 Endogenous and Included Exogenous Variables

Before introducing our instrumental variable, let us introduce all the other variables in our analysis. They include two endogenous variables – our dependent variable (per capita total transfers) and key independent variable (voter turnout) – and included exogenous variables (i.e., control variables).

In an effort to understand the impacts of voter turnout on policy outcomes, we focus on the geographic allocation of public expenditures. This is because political participation is expected to have the most direct and strongest impacts on distributive policy decisions by legislators, whose space for representation and competition is defined by geographically segmented districts. Specifically, the dependent variable for our analysis is the per capita amount of total transfers for municipality-specific public projects and programs (in log).[5] The total transfers include the following three components: the formulaically allocated portion of grant-in-aid (*chihō kōfuzei futsū kōfukin*), the grant-in-aid that is allocated discretionarily (*chihō kōfuzei tokubetsu kōfukin*), and the national treasury disbursement (*kokko shishutsukin*), which are project-based subsidies.

[4] We are aware of important recent studies on conditions and interpretations of causal effects using instrumental variables (e.g., Angrist, Imbens and Rubin 1996). In this paper, however, we take a traditional approach which relies on the conceptual framework of "structural equation models."

[5] Our data are based on the account settlements (Chihō Zaisei Chōsa Kenkyū Kai, Various years). The municipality population at the end of the fiscal year, which begins on April 1, is from Kokudo Chiri Kyōkai (Various years).

We use total transfers, because it is difficult to estimate the overall political effects including effects of logrolling by using program-specific or type-specific transfers (for discussions about the measurement issue, see Ansolabehere, Gerber and Snyder 2002, 769; Horiuchi and Saito 2003, 674-5).

The key independent variable is voter turnout in the most recent Lower House election.[6] The Lower House elections included in our dataset are the last two elections before the electoral reform of 1994 (i.e., the 1990 and 1993 elections) and the first two elections after the reform (i.e., the 1996 and 2000 elections). The 1990 and 1993 elections were held under the Single Non-Transferable Vote (SNTV) system, while the latter two elections were held under a combination of the single member district (SMD) system and the closed-list proportional representation (PR) system. The changing dynamics of Japanese politics during the 1990s suggest a number of endogenous variables correlated with voter turnout and policy outcomes – different inter-party and intra-party politics, different motivations for individual candidates, different pork allocation strategies, etc. We think that these are all relevant pre-election and post-election variables – though difficult to measure. Thus, their effects should be controlled for by adopting an appropriate statistical method.

The per capita amount of total transfers is determined by various factors other than voter turnout, including the politically relevant variables discussed in Section 2. Furthermore, some municipalities may receive disproportionately larger fiscal transfers than others for non-political reasons, such as economic backwardness and greater

[6] All the electoral variables used in this study are adopted from Mizusaki (1993, 1996, 2000).

functional demand. To alleviate (but not necessarily "solve") this problem, we add the following control variables.

The first two variables are political – the number of seats per capita (in log) and the number of candidates per district magnitude in the most recent Lower House elections. We think that these district-level variables are correlated with the density of mobilization activities during the campaign period. The number of seats divided by the size of constituency (in log) is the main independent variable used in Horiuchi and Saito (2003). They argue that malapportionment will lead to disparity in the allocation of pork barrel projects because the per capita size of available political resources is affected by the number of politicians per voter. Horiuchi and Saito (2003) indeed find a positive and highly significant effect of this variable on the per capita amount of total transfers in Japan. The number of seats per capita is also intended to control mobilization effects. Rosenstone and Hansen (1993) argue that some voters are inclined to go to the polls when they are asked to do so. We expect that the number of candidates (standardized by the number of seats) has a positive effect on voter turnout: the larger the number of candidates engaging in mobilization activities, the higher the probability of voters being contacted directly by candidates or indirectly by party activists and going to the polls.

An important set of variables, which we must include in our analysis using Japanese municipality-level local finance data, is the municipality fiscal strength index (*zaisei-ryoku shisū*) and its squared term (Chihō Zaisei Chōsa Kenkyū Kai, Various years). This index is devised by the Japanese government to appraise formulaic allocation of the grant-in-aid (or general transfers) to each municipality. Therefore, it must be correlated with total transfers per capita. Specifically, the larger the value of this index, the stronger a municipality's ability to raise revenues through local taxes, and thus the lower the amount of intergovernmental transfers received. Since the index reflects a number of demographic and geographical variables, such as the total population, the composition of population by age groups, and each municipality's area size, we do not add these additional demographic and geographical variables in our regression analysis.

An important note is that some municipalities with high values of the municipality fiscal strength index do not receive the formulaic portion of grant-in-aid (i.e., general transfers) from the central government, although these municipalities still receive the national treasury disbursement (i.e., specific transfers) and the non-formulaic portion of grant-in-aid. Therefore, the relationship between the municipality fiscal strength index and the per capita amount of total transfers may be non-linear. To capture these features, we add the squared term of the index. Preliminary analysis based on cross-sectional regressions for each fiscal year suggests that more than 80% of variations in the dependent variable can be explained by this index and its squared term alone.

We also consider municipality-specific factors and apply the fixed-effect transformation (also known as the within transformation) before data analysis. This transformation allows us to control location-specific factors. For instance, some portion of the subsidy items is allocated to compensate for residents living in proximity to not-in-my-backyard (NIMBY) facilities, such as nuclear and non-nuclear power plants. Other demographic, economic, social, historical or cultural factors, which are not captured by the included exogenous variables, can also be controlled, as long as they are municipality-specific and time-invariant, i.e., relatively constant within each municipality for the period of our investigation. Unobservable factors of this type may include intergovernmental legal and administrative relationships, political culture, and historical experience. Furthermore, by taking the fixed-effect transformation, we can control any geographical and topographic feature of electoral districts, which may be correlated with voter turnout and rainfall, our instrumental variable.

Finally, we add election-specific dummy variables, which are intended to control for inter-temporal nationwide differences in intergovernmental fiscal transfers. For instance, as we noted, our data include the periods before and after the 1994 electoral reform. The overall budget allocation plan may also be different across years depending on macroeconomic conditions and overall political climate in each year.

It is important, however, to note that even with all these control variables and fixed effects, we feel that some politically relevant variables are still excluded from analysis. The two most relevant variables we discussed in Section 2 include day-to-day political interactions between incumbents and voters, mobilization activities during the campaign period, and post-election political calculations of incumbents and/or ruling parties regarding where to target government spending.

3.3 Rainfall as an Instrumental Variable

A standard solution for the omitted variable bias is to find a "good" instrumental variable, which is correlated with voter turnout but not with any causally prior or posterior variable excluded from analysis. It also has to be noted that an instrumental variable in fixed-effect models should retain adequate variation after the fixed-effect transformation. If it does not vary significantly across years *within a municipality*, it tends to show an insignificant effect on voter turnout, thereby introducing serious bias due to the use of "weak instruments" (Bound, Jaeger and Baker 1995; Staiger and Stock 1997).

We exploit election-day precipitation data to serve an important role in our analysis.[7] Through our preliminary analysis, we found that a dummy variable, which is coded "1" if a municipality recorded 3 millimeters or more of rainfall between 6am and 3pm on the day of each Lower House election and "0" otherwise, exhibits a very large effect on voter turnout.[8] Existing studies show that rainfall is indeed correlated with voting behavior (Asano 1998; Gomez, Hansford, and Krause 2007; Knack 1994; Merrifield 1993; Tamada 2006). More importantly, the rainfall on a particular day should be correlated neither with the size of intergovernmental transfers to municipalities during a fiscal year, nor with theoretically relevant but omitted variables – variables relevant to

[7] The rainfall data are retrieved from CD-ROMs published from Kishō Gyōmu Shien Sentā (Various years). The precipitation is measured and collected on the hourly basis over the entire Japanese archipelago, by utilizing both radars and rain gauges. The original data are recorded in a lattice format at approximately 5 km intervals, to each of which latitude and longitude information is attached. The rainfall data are then merged to municipal observations by matching geographic location of city halls and town halls to each of the rainfall lattices (Takeda 2003).

[8] Searching for the best fitted regression model without causal theories (often called "data mining" or "regression fishing") is strongly discouraged in standard regression exercises. But searching for the best specified instrumental variable within available data is regarded as legitimate. It is also important to note that we should not attempt to use as many potential instrumental variables as possible in order to avoid having "weak instruments," which cause serious bias. For a review of the method of instrumental variables and its effectiveness and pitfalls, see Angrist and Krueger (2001).

politics before and after the voting day. Consequently, the rainfall instrument is expected to be highly valid when studying the effects of voter turnout.[9]

Furthermore, due to the following three important institutional reasons, we think that our Japanese data are particularly valid in studying the consequences of voter turnout on policy. First of all, in Japan, voting typically takes place on Sunday from 7am to 8pm.[10] Weather conditions, therefore, affect opportunity cost calculation among citizens – whether or not to go outdoors. Anecdotal evidence suggests that quite a few of the citizens are discouraged from going to the polling stations and decide to stay home when it rains (*Asahi Shimbun*, November 9, 2003).

Second, unlike other democracies where candidates and parties keep on mobilizing voters until polling stations close, neither candidates nor political parties in Japan are allowed to deploy any campaign activity on the polling day. The media are also

[9] An important assumption in our estimation (formally called "exclusion restriction") is that the weather conditions affect the outcome variable *only* through voter turnout. One may argue that rainfall may also affect vote shares, which may in turn affect the budget allocation. This additional causal path is plausible, but we cannot simply add additional endogenous variables in a standard regression framework. This is because, as we discussed in 2.2, theoretically-relevant vote shares or their derivations (e.g., vote shares of parties forming a ruling coalition *after a general election*) are, at least partially, post-treatment. Ideally, we should build a statistical model with partially post-treatment endogenous variables and make proper causal interpretations. We leave this for future research.

[10] The polling hour was from 7am to 6pm until the 1996 Lower House election.

expected to be neutral (and quiet) until the polling stations close. Thus, given the absence of political mobilization efforts on the voting day, a short-run exogenous stimulus such as weather conditions is a plausible variable that is in isolation of other variables affecting a turnout rate. In other words, we can safely assume that there is no other systematic political variable on the voting day.

Finally, in our panel data, there is indeed a substantial variation in the amount of rainfall *within each municipality* across elections. Unlike elections in the United States which take place in fixed intervals, the timing of Japan's Lower House elections is endogenous. The prime minister can dissolve the Lower House and call for a general election any time before the four-year term expires. This means that there can be a seasonal variation of elections when panel data that include multiple Lower House elections are used. In our case, the Lower House elections were held in February 1990 (winter), July 1993 (summer), October 1996 (fall) and June 2000 (rainy season before summer), and nearly half of the municipalities have such variations across these elections. This intra-municipality variation is critical when we conduct fixed-effect regression analysis.

3.4 Model Specification

Using the variables introduced above, we run three regression models. The first is an ordinary least square (OLS) regression without employing our instrumental variable. We consider OLS estimates as biased due to the omission of variables concerning politics before and after the voting day. We use it, however, to produce a conventional estimate and to examine how the coefficient of voter turnout will change by using the instrumental variable.

The other two models are two-stage least square (2SLS) regressions with the rainfall instrumental variable. While the second model includes all observations, the third model excludes municipalities where our rainfall dummies are either 0 or 1 for all the voting days in our study. The idea is equivalent to the non-parametric preprocessing of data based on matching (Ho, Imai, King, and Stuart 2007). In our third model using pre-processed data, each municipality has at least one observation with rainfall and another without rainfall. In other words, all municipalities in the third model have intra-municipality variations in the instrumental variable. Balancing our data in this manner (and dropping causally irrelevant municipalities) is more likely to produce consistent estimates of the effects of rainfall on voter turnout.

In order to cope with a possible problem of heteroskedasticity, all the three regressions are weighted by the municipality population size, which exhibits a wide variation ranging from less than 200 to 1.5 millions.[11] These are also more appropriate than un-weighted regressions, because we intend to estimate the effects of voter turnout across individuals rather than municipalities. For this reason, when we evaluate the marginal effect of voter turnout, we should hold other variables constant at their weighted means rather than un-weighted ones.

One remaining specification issue with regard to our statistical models is that in each regression model, we use the *average* per capita transfers during several years, in which legislators elected in the previous election could exert influence on budget-making. We prefer to use this dependent variable, instead of each fiscal year's per capita transfers,

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[11] Note that our dependent variable is denominated by the municipal population and our key independent variable is denominated by the total number of eligible voters, which are equivalent to (automatically) registered voters in the case of Japan.

because taking the average for several years can minimize stochastic (and causally posterior) factors within each municipality. Specifically, our dependent variable covers transfers in the fiscal years (FY) 1991-1993 for the 1990 election, FY 1994-1996 for the 1993 election, FY 1997-2000 for the 1996 election, and FY 2001-2003 for the 2000 election. The total number of observations (for our first and second models without data pre-processing) is 12,620, which is 3,155 municipalities multiplied by four elections.[12]

4. Results

Descriptive statistics for variables used in this study are shown in Table 1, which include those based on all observations (N = 12,620) and on pre-processed observations (N = 5,940). Since all the observations dropped by pre-processing are municipalities without rainfall on all the four voting days, the probability of having rainfall is obviously higher in the pre-processed data (27.6%) than the complete data (13.8%). More importantly, however, the distributions of other variables are quite similar between the two datasets. This is unsurprising because whether municipalities are dropped from analysis is random by nature and thus uncorrelated with any of these variables. This similarity also means that we do not need to be seriously concerned about selection bias in our third model.

Table 2 shows the results of three fixed-effect regressions (second-stage).[13] In the

[12] Due to a small number of municipal amalgamations (50 cases of mergers between the 1990 election and the end date of FY 2003), the number of municipalities is not exactly constant during the period of investigation. We thus use the pre-merger municipal population as a weight and make a balanced panel before data analysis.

[13] The coefficients of other control (included exogenous) variables also tend to show

first OLS regression, the coefficient of voter turnout is small (0.052) and insignificant at any conventional level. In the other two models using the rainfall instrumental variable, this effect is much larger (1.744 in Model 2 and 2.366 in Model 3) and statistically highly significant. The effect is particularly large after dropping observations which are causally irrelevant in our two-stage least-square regressions. From these results, we conclude that the effect of voter turnout on per capita intergovernmental transfers is positive, large, and highly significant. We will interpret the substantial magnitude of its effect shortly after examining the results of first-stage regressions, which are shown in Table 3.

In the first-stage regressions, we are mainly interested in the effects of our instrumental variable. The rain dummy indeed shows negative and highly significant effects. The econometric literature suggests that the F test statistic of an excluded instrument (or a set of instruments) should reach roughly ten in the context of a single endogenous regressor (Staiger and Stock 1997). Otherwise, the 2SLS estimates suffer from the problem of "weak instruments." In our cases, the test statistics are 95.11 in Model 2 and 118.43 in Model 3. There is no doubt that our rainfall instrument is sufficiently strong. It is also worth noting that the magnitude of the effect of rainfall on voter turnout is surprisingly large. The estimated coefficient in Model 3 (-0.011) implies that if a municipality has a total rainfall of only 3 millimeters for 9 hours (from 6am to 3pm), voter turnout drops by 1.1 percentage points. Since the total number of eligible/registered voters in Japan is about 100 million, our result indicates that if there is a small amount of rainfall during the polling hours throughout the Japanese archipelago, more than a million

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highly significant effects with expected signs. We do not, however, provide interpretation to these coefficients because they are not main quantities of our interests.

Japanese voters will be discouraged from going to the polls.

Finally, let us also evaluate the substantive effects of voter turnout on per capita transfers based on a simple post-estimation analysis. The weighted average of voter turnout during the period of investigation is 64.2% (in all data) and 64.3% (in pre-processed data). The standard deviations are 0.093 and 0.092, respectively. The weighted average per capita total transfers (in log) is 4.401 (in all data; 81.53 thousand Japanese Yen) and 4.333 (in pre-processed data; 76.19 thousand Japanese Yen). Using these figures as benchmarks, we evaluate how much the amount of transfers will increase if voter turnout increases from one standard deviation below the mean to one standard deviation above the mean. The results are shown in Table 4.

The estimated effect based on the OLS regression is almost nil. The per capita transfers only increase by about 800 Japanese Yen, which is less than a 1% increase (81.13 to 81.93 thousands). By contrast, the 2SLS regressions suggest quite large effects of voter turnout on per capita transfers. The total amount of transfers increases by about 30 thousand Japanese Yen (26.68 thousands in all data and 33.51 thousands in pre-processed data). In terms of the percentage increase, it is 38.5% (69.28 to 95.96 thousands) in our complete sample and 54.7% (61.25 to 94.76 thousands) in the pre-processed sample. The substantially large and positive estimates in the 2SLS regressions also imply that, in terms of the distributive benefits voters receive, the difference between municipalities with high voter turnout and those with low voter turnout is large and significant.

5. Conclusion

In this paper, we examined the impact of political participation on policy outcomes by using Japanese municipality-level data of voter turnout and intergovernmental fiscal transfers. After discussing possible confounding factors, we estimated marginal effects of voter turnout by employing municipality-specific fixed effects, various control variables, and most importantly, a highly effective instrumental variable based on rainfall data. The results suggest that the act of voting – the simplest, the most popular, the least expensive, but most essential model of political participation in democracy – does indeed bring about sizable differences in policy outcomes. After controlling for many other factors, municipalities with high voter turnout tend to receive significantly larger benefits than municipalities with low voter turnout.

In democracies, legislators respond to policy needs of voters, but our analysis suggests that their responsiveness is indeed contingent on the level of voter turnout. In the words of Japan's former prime minister (see the preamble of this paper), delivery of policy benefits that are commensurate to the level of participation may be nothing more than an example of how democracy actually works. However, as Lijphart (1997) lamented, unequal benefits based on unequal participation still remains "democracy's unresolved dilemma."

Table 1: Summary Statistics

Variables	Mean	SD	Min	Max
All observations for Models (1) and (2)				
Total Transfers Per Capita (in log)	4.401	0.788	1.985	8.837
Voter Turnout (%)	0.642	0.093	0.399	0.981
Rain Dummy	0.138	0.345	0	1
Seats Per Capita (in log)	1.366	0.384	0.752	2.247
Candidates Per Seat	3.071	1.391	1.2	8
Municipality Fiscal Strength Index	0.727	0.280	0.040	2.273
Municipality Fiscal Strength Index (squared)	0.607	0.423	0.002	5.168
Selected observations for Model (3)				
Total Transfers Per Capita (in log)	4.333	0.769	2.184	8.837
Voter Turnout (%)	0.643	0.092	0.409	0.981
Rain Dummy	0.276	0.447	0	1
Seats Per Capita (in log)	1.357	0.372	0.752	2.247
Candidates Per Seat	3.094	1.422	1.2	8
Municipality Fiscal Strength Index	0.767	0.286	0.040	2.257
Municipality Fiscal Strength Index (squared)	0.669	0.448	0.002	5.093

Note: The number of observations is 12,620 for Models (1) and (2) and 5,940 for Model (3). All observations are weighted by the municipality population.

Table 2: Regression Results

Variables	(1) OLS	(2) 2SLS	(3) 2SLS
Voter Turnout (%)	0.052	1.744	2.366
	(0.038)	(0.421)	(0.446)
Seats Per Capita (in log)	0.083	0.041	0.014
	(0.008)	(0.014)	(0.019)
Candidates Per Seat	0.014	0.007	0.016
	(0.002)	(0.003)	(0.003)
Municipality Fiscal Strength Index	-4.256	-4.021	-3.922
	(0.089)	(0.114)	(0.163)
Municipality Fiscal Strength Index (squared)	1.157	1.128	1.262
	(0.042)	(0.047)	(0.074)
1993 Election Dummy	0.125	0.228	0.290
	(0.004)	(0.026)	(0.027)
1996 Election Dummy	0.287	0.534	0.615
	(0.009)	(0.062)	(0.065)
2000 Election Dummy	0.187	0.388	0.456
	(0.009)	(0.051)	(0.054)
The number of observations	12,620	12,620	5,940
The number of panels (municipalities)	3,155	3,155	1,485
R^2	0.719	0.661	0.592
$(\text{Mean Squared Error})^{0.5}$	0.114	0.125	0.144

Note: The dependent variable is total transfers per capita (in log). Standard errors are in parentheses. All regressions are weighted by the municipality population and include municipality fixed effects. The 1990 Election is a base category for election dummies. Model (3) excludes municipalities, which did not have rainfalls in all the four Lower House elections.

Table 3: First-Stage Regression Results

Variables	(2) 2SLS	(3) 2SLS
Seats Per Capita (in log)	0.024	0.021
	(0.002)	(0.003)
Candidates Per Seat	0.004	0.003
	(0.000)	(0.001)
Municipality Fiscal Strength Index	-0.152	-0.107
	(0.024)	(0.033)
Municipality Fiscal Strength Index (squared)	0.022	-0.006
	(0.011)	(0.015)
1993 Election Dummy	-0.059	-0.054
	(0.001)	(0.001)
1996 Election Dummy	-0.146	-0.143
	(0.002)	(0.003)
2000 Election Dummy	-0.118	-0.114
	(0.002)	(0.003)
Rain Dummy	-0.009	-0.011
	(0.001)	(0.001)
The number of observations	12,620	5,940
The number of panels (municipalities)	3,155	1,485
R^2	0.820	0.821
(Mean Squared Error)$^{0.5}$	0.030	0.030
F test statistic of an excluded instrument	95.11	118.43

Note: The dependent variable is voter turnout (%). Standard errors are in parentheses. All regressions are weighted by the municipality population and include municipality fixed effects. The 1990 Election is a base category for election dummies. Model (3) excludes municipalities, which did not have rainfalls in all the four Lower House elections.

Table 4: Marginal Effects of Voter Turnout

	Models		
	(1)	(2)	(3)
Total Transfers Per Capita (in log, mean)	4.40	4.40	4.33
Estimated Coefficient of Voter Turnout	0.05	1.74	2.37
Voter Turnout (mean, %)	0.64	0.64	0.64
Voter Turnout (standard deviation, SD)	0.09	0.09	0.09
A. Predicted Total Transfers Per Capita (mean - 1 SD)	81.13	69.28	61.25
B. Predicted Total Transfers Per Capita (mean)	81.53	81.53	76.19
C. Predicted Total Transfers Per Capita (mean + 1 SD)	81.93	95.96	94.76
Difference = C − A	0.80	26.68	33.51

Note: The means and standard deviations are weighted by the municipality population. The measurement unit of predicted total transfers per capita is 1,000 Japanese Yen.

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