RAIN, ELECTIONS AND MONEY: THE IMPACT OF VOTER TURNOUT ON DISTRIBUTIVE POLICY OUTCOMES IN JAPAN

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Does voter turnout affect policy outcomes? This long-standing question has been re-visited recently with close empirical scrutiny. These studies, however, commonly suffer from a problem of omitting variables correlated with both causal and outcome variables: specifically, immeasurable political interactions between politicians, organised groups, and individual voters. We address this problem by exploiting an instrument based on new data measuring the amount of rainfall on the voting day. It is a valid instrument not only because it is correlated with voter turnout and uncorrelated with politically relevant omitted variables, but also because it is expected to satisfy the assumption of 'homogeneous partial effects,' which has not been carefully examined in previous studies that took advantage of instrumental variables. Using a large, municipality-level data set from Japan, we show that the turnout effect on the amounts of intergovernmental fiscal transfers is indeed significant, positive, and large.

'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]

Introduction

This paper examines a long-standing but recently re-visited question: Does political participation affect policy outcomes? 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 actively participate (that is, 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 scholars are examining 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). In an effort to obtain unbiased estimates of the effects of voter turnout, these existing studies control relevant covariates using a range of observable variables and fixed effects based on the standard regression framework.

We consider, however, that these studies fail to control for a variety of crucial features of electoral politics, which are often immeasurable. Specifically, voter turnout tends to be high when candidates and their supporters attempt to mobilise votes (for example, Rosenstone and Hansen 1993), when such campaign efforts make certain issues prominent (for example, Hutchings 2003), and when the public becomes more 'attentive' (Martin 2003) to these issues. In turn, such a rise in public attention devoted to locally politicised issues leads policy-makers to allocate a disproportionately larger amount of government resources to municipalities with higher turnouts.

The existing empirical studies provide mixed results, most likely due to this problem of omitting relevant variables: specifically, *political interactions between politicians, organised groups, and individual voters up until the day before the voting day*. For example, in an 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 the five per cent 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 the one per cent level in the 1990 and 1992 cycles. As Martin (2003) correctly admits, 'Voter turnout may be acting as a surrogate for other omitted characteristics' (p. 120).

In this paper, we circumvent this problem by using the voting-day precipitation as an instrumental variable. Rainfall suppresses voter turnout as existing studies suggest (Gomez, Hansford, and Krause 2007; Knack 1994; Tamada 2006), but we can expect that it is uncorrelated with the omitted variables. In addition, as we will discuss the matter carefully later, the rainfall variable is also expected to satisfy the assumption of 'homogeneous partial effects', which has not been carefully examined in previous studies that took advantage of instrumental variables (Dunning 2008).

Specifically, using large municipality-level electoral, budgetary, and meteorological data from Japan, we estimate the effect of voter turnout on the amounts of intergovernmental fiscal transfers of geographically specific projects and programs. We focus on this particular policy variable, 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. The results suggest that the turnout effect is indeed significant, positive, and large.

Why Voter Turnout Affects Delivery of Benefits

Why does voter turnout affect policy outcomes? Why are the existing estimates expected to be biased? In line with our focus on intergovernmental fiscal transfers in Japan, this section gives our answers to these questions. Although we limit our discussion to the primary research topic, we believe that it is generally applicable to studies that analyse the impact of political participation on policy outcomes.

The standard OLS regression models estimating the impact of voter turnout on policy outcomes (with an assumption that one has time-series and cross-section data) can be expressed as follows:

$$Y_{it} = \beta \cdot X_{it} + \delta \cdot W_{it-1} + \alpha_i + \varepsilon_{it} \qquad (1)$$

The outcome variable Y_{it} is the amount of intergovernmental fiscal transfers to municipality i in year t. The causal variable X_{it} is voter turnout in i recorded in the most recent election as of t. The model also includes other observable pre-treatment variables W_{it-1}, a location-specific fixed effect α_i, and an error term ε_{it}.

A standard OLS regression assumes that voter turnout is uncorrelated with the error term after conditioning on W_{it-1} and α_i. We assume, however, that even after controlling for these, voter turnout is a sum of the systematic but unobservable component (ϕ_{it}) and the stochastic component (u_{it}); namely,

$$X_{it}|W_{it-1}, \alpha_i = \phi_j + u_{it}. \qquad (2)$$

If ϕ_{it} is correlated with the outcome variable Y_{it}, OLS regressions suffer from the well-known problem of omitted variable bias. The existing literature on electoral politics suggests that there are ample reasons to believe this. Specifically, we argue that the omitted systematic component constitutes political interactions between political candidates, their supporters, and other individual voters during a campaign period. Such interactions have effects on voter turnout, and in turn on policy outcomes, through the following mechanisms.

First, during the campaign period, political candidates contact individual voters by door-to-door canvassing, organising small-group meetings, making phone calls, sending direct mailouts, and engaging in other face-to-face activities, and asking them to vote in the upcoming election. These so-called 'get-out-to-vote' (GOTV) campaigns are found to increase voter turnout (for example, Gerber and Green 2000). Candidates also mobilise votes indirectly through their core supporters, activists, and other organised

interest groups and social networks (for example, Rosenstone and Hansen 1993). In the Japanese case, municipal politicians (that is, mayors and assembly members) have always been important hubs of these activists, and they have served as the faucets at the end of 'pipeline of pork' (Scheiner 2005).

Second, when candidates contact the electorate and interact with them, the former not only ask the latter to vote for them but also try to convince what sorts of policy benefits they can deliver to the localities. For example, they may promise to bring fiscal resources for specific public projects and programs (for example, municipal roads, irrigation projects, and educational facilities). As a result of such campaigns, certain issues become more salient, and the electorate, who would otherwise be only weakly motivated to acquire information about issues, candidates, and parties (for example, Benett 1995; Zaller 1992), become more 'attentive' (Martin 2003) to the nature of electoral competition and more informed about candidates' policy positions (for example, Brians and Wattenberg 1995; Goldstein and Freedman 2002). In the words of Hutchings (2003), 'sleeping giants' are awakened by political campaigns that emphasise their interests. The rise in public attention on specific pork-barrel projects further motivates municipal politicians to increase mobilisation efforts. Voters are also more strongly motivated to organise votes — asking neighbors, friends and colleagues to vote for specific candidates — with the hope that their voices will be better heard by politicians, their preferred projects will be implemented, new jobs will be created, and/or politically-general incomes will rise.

Importantly, the higher level of 'issue politicisation' through active campaigns and political interactions between candidates and voters not only increases voter turnout, but also gives candidates, once elected, stronger incentives to direct larger public expenditures to their constituents. An underlying logic is straightforward, although it is not clearly stated in existing studies on the impacts of voter turnout: without making efforts to deliver policy benefits, particularly when 'attentive' publics care much about them, incumbents will be seen as shirking constituency services, not caring about their electorate, or even breaking campaign pledges. This damages their reelection prospects.

In sum, even after controlling for every possible observable variable and adding municipality-specific fixed-effects, it is unreasonable to assume that voter turnout is exogenous. Rather, voter turnout has impacts on policy outcomes, precisely because it is a function of unobservable electoral processes taking place until voters decide whether to go to the polling stations. In other words, paradoxically, voter turnout affects the delivery of benefits because of the presence of omitted variables.

Why Rainfall Is an Effective Instrument

We attempt to cope with this problem of omitted variables using a standard econometric tool, which is to exploit an instrumental variable and run a two-stage least square (2SLS) regression. Our instrument Z_{it} is measured in terms of hourly rainfall statistics for each municipality on each voting day. We assume two standard assumptions for the validity of instruments. First, it has a sufficiently strong correlation with voter turnout (X_{it}). A standard rule of thumb is that an F-test statistic is greater than 10, which is clearly the case in our study. Second, it should be uncorrelated with the error term (ε_{it}), which includes the unobservable systematic component (ϕ_{it}) discussed in the previous section. The second assumption is justifiable, because weather conditions on a specific day in a specific municipality are unrelated to political interactions *before the voting day*. It is also unreasonable to assume that the government will study an election-day weather map while compiling a budget.

To obtain a consistent estimate by running a two-stage least square (2SLS) regression, Dunning (2008) argues that we need to impose a third assumption, which he calls the assumption of 'homogeneous partial effects'.[2] This assumption has not been carefully tested in existing empirical studies using 2SLS regressions. In the following, we carefully discuss the validity of this assumption in our study, by following the notation of Dunning (2008).

To begin with, we conceptually divide voter turnout into $X_{it} \equiv X_{it}^{(1)} + X_{it}^{(2)}$, where $X_{it}^{(1)}$ is the component of voter turnout not influenced by rainfall and $X_{it}^{(2)}$ is the component of voter turnout influenced by it.[3] In other words, we assume $Cov\left(X_{it}^{(1)}, Z_{it}\right) = 0$ but $Cov\left(X_{it}^{(2)}, Z_{it}\right) \neq 0$. As we discussed in the previous section, theoretically relevant variations in voter turnout are a function of unobservable electoral politics before the voting day. Thus, we are interested in estimating the impact of $X_{it}^{(1)}$ on Y_{it}. Our instrument, however, is correlated with another component of voter turnout, $X_{it}^{(2)}$.

Then, the question is whether a 2SLS regression using Z_{it} as an instrument can produce a consistent estimate of $X_{it}^{(1)}$ on Y_{it}. Dunning (2008) suggests that this is possible, as long as the assumption of 'homogeneous partial effects' is satisfied. It is an assumption that the impact of $X_{it}^{(1)}$ and that of $X_{it}^{(2)}$ are homogenous. Formally, we need to assume that the model (1) is equivalent to the following model.

$$Y_{it} = \left(\beta_1 \cdot X_{it}^{(1)} + \beta_2 \cdot X_{it}^{(2)}\right) + \delta \cdot W_{i-1} + \alpha_i + \varepsilon_i \qquad (2)$$

The condition under which these two models (1) and (2) are equivalent is obviously $\beta_1 = \beta_2 (= \beta)$.

In Dunning's discussion of possible violation of this assumption, he refers to the study of how income affects political attitudes (Doherty, Gerber and Green 2006; here-

inafter referred to as DGG), where lottery winning by survey respondents was utilised as an instrumental variable. Dunning decomposes the overall income (an endogenous variable) into ordinary income ($X_i^{(1)}$) and lottery income ($X_i^{(2)}$), and he points out that their instrument is correlated with $X_i^{(2)}$ but not with $X_i^{(1)}$. He also indicates the possibility that $\beta_1 \neq \beta_2$ because 'lottery winnings may be regarded by subjects as an unusual stream of windfall income and may influence attitudes differently than money earned through work' (p. 291). Under these assumptions, even if $X_i^{(2)}$ is truly exogenous, a 2SLS regression does not produce consistent estimates that DGG intended to produce. Specifically, when $Cov\left(X_i^{(1)}, X_i^{(2)}\right) = 0$, 2SLS asymptotically estimates the impact of lottery income on attitude (i.e., β_2), which is not DGG's interest. When $Cov\left(X_i^{(1)}, X_i^{(2)}\right) \neq 0$, it asymptotically estimates a mixture of β_1 and β_2, which may also not be what DGG intend to study. One critical assumption, which requires DGG's study to produce an estimate of their quantity of interest (β_1), is that the effect of income on attitudes is constant regardless of sources of income.

In our case, we consider that it is reasonable to accept this assumption because the component of turnout influenced by rainfall is *very small* and *unobservable* for policy-makers who influence the amount of intergovernmental fiscal transfers.[4] In DGG's study, on the contrary, the component of income influenced by a lottery is (potentially) *large* and clearly *observable* for respondents who report their political attitudes. Below, we explain why this difference matters.

Suppose that there are two municipalities, which are identical with respect to all relevant (observable and unobservable) characteristics. They even have the same level of voter turnout, say 60 per cent. The only difference, however, is the composition of voter turnout, for instance, in the first municipality $X_{1t}^{(1)} = 59.5\%$ and $X_{1t}^{(2)} = 0.5\%$ but $X_{2t}^{(1)} = 58.5\%$ and $X_{2t}^{(2)} = 1.5\%$ in the other. It is difficult to assume any systematic difference in policy outcomes between these municipalities, because policy makers typically cannot observe or distinguish such a tiny difference between $X_{1t}^{(2)} = 0.5\%$ and $X_{2t}^{(2)} = 1.5\%$, even though X_{it} is clearly observed and published.

Note that if policy makers have sufficient resources and capability to monitor individual-level voting behavior (Nichter 2008, Stokes 2005), they may be able to identify who turned out and for whom they cast their ballot. This does not imply, however, that they can distinguish between $X_{it}^{(1)}$ and $X_{it}^{(2)}$ in each municipality. It is very difficult, if not impossible, for them to verify whether those who voted did so because of the election-day weather or for other reasons. In fact, it is perhaps reasonable to assume that policy makers even do not make efforts to identify a tiny portion of voters who go to the polling stations if and only if weather conditions are good ('weather-induced voters').

Accordingly, we assume that politicians ignore the small and unobservable portion

of voter turnout and make policy decisions based on the *observed* turnout, which is almost equal to the unobservable portion of politically relevant turnout. In other words, we assume that when voter turnout marginally increases because of good weather, politicians treat it *as if it is due to a change in systematic factors* and, thus, increase the allocation of transfers. This behavioral assumption is conceptually similar to 'Type I error' in statistical inference.[5]

By contrast, in the case of DGG, the subjects are surveyed respondents who can clearly identify whether their income in the past year is mainly from their ordinary income sources or from the lottery. The amount of lottery income is zero for almost all individuals, but this component is fairly large for lottery winners. Furthermore, as Dunning argues, the effect of having a disposable income of US$80,000 per year from ordinary sources and having the same amount of total income with an extraordinary large proportion of lottery income (say, US$20,000 from ordinary sources and US$60,000 from lottery) are expected to be different. This is because individuals know which component of their income can be directly or indirectly influenced by government policy (for example, through taxes, direct payments and deductions, and policies affect employment and business conditions).

In sum, rainfall is an effective instrument to estimate the impact of voter turnout not only because it is correlated with one portion of voter turnout and uncorrelated with other omitted political variables, but also because the portion of turnout influenced by rainfall is small and unidentifiable for policy-makers.

Data and Variables

Now, let us introduce our data from Japan and specific variables for estimation. The outcome variable for our analysis (Y_{it}) is the per capita amount of total transfers for municipality-specific public projects and programs (in log).[6] 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. We use total transfers because it is difficult to estimate the overall political effects 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).

We use the *average* per capita transfers throughout inter-election years when incumbents elected in the previous general election could exert influence on budget-making.[7] 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 causal variable (X_{it}) is voter turnout in the most recent 2000 Lower House election.[8] The Lower House elections included in our data-set are the last two elections before the electoral reform of 1994 (that is, the 1990 and 1993 elections) and the first two elections after the reform (that is, 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. Since the purpose of this paper is not to identify how the impacts of voter turnout on pork-allocation strategies changed before and after the electoral reform, we pool all data for our analysis.[9] Arguably, the changing dynamics of Japanese politics during the 1990s suggest a number of other unobservable variables correlated with voter turnout and policy outcomes — different mechanisms of inter-party and intra-party politics, changes in issues at stakes, heterogeneous motivations for individual candidates, etc. It is important to note, however, 2SLS produces consistent estimates as long as our rainfall instrument is uncorrelated with such changes in Japanese politics. This is undoubtedly true.

The per capita amount of total transfers (Y_{it}) is determined by pre-determined covariates (W_{it-1}) other than voter turnout (X_{it}). 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. Since our panel data set consists of pre- and post-reform periods, it is necessary to include these variables to control for essential aspects of electioneering activities. We think that these district-level variables are correlated with the density of mobilisation activities during the campaign period, and thus with both voter turnout and distributive policy outcomes, as we discussed in Section 2.[10]

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).[11] 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 analysis.

We also add fixed effects (α_i) in our model to control for observable and unobservable factors that are municipality-specific and time-invariant, that is, relatively constant within each municipality for the period of our investigation. 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 can also be controlled for as long as they are time-invariant. Unobservable factors of this type may include intergovernmental legal and administrative relationships, political culture, and historical experience. Furthermore, by running fixed-effect regressions, we can control for any geographical and topographic feature of municipalities, which may be correlated with voter turnout and rainfall, our instrumental variable.

Finally, we add period-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 the overall political climate in each year.

Even with all these control variables and fixed effects, as we discussed earlier, there should be politically relevant but unobservable variables excluded from analysis. To cope with this problem, we exploit election-day precipitation data to serve an important role in our analysis.[12] Through our preliminary analysis, we found that a dummy variable, which is coded '1' if a municipality recorded three millimetres 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.

In Section 3, we already discussed why rainfall is expected to be a valid instrument to estimate the impact of voter turnout on policy outcomes. Due to the following three important institutional reasons, we think that our Japanese data are particularly valid for the purpose of this study. First of all, in Japan, voting typically takes place on Sunday. Therefore, weather conditions affect opportunity cost calculations 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 (for example, *Asahi Shimbun*, 9 November 2003).

Second, unlike other democracies where candidates and parties keep on mobilising 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 expected to be neutral (and quiet) until the polling stations close. Thus, given the absence of political mobilisation 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 not fixed. The prime minister can dissolve the Lower House and call a general election any time before the four-year term expires. This means that there can be a (potential) 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, as an instrumental variable in fixed-effect regressions 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).

Results

Using the variables introduced in Section 4, we run three regression models.[13] 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 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.

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.[14]

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 per cent) than the complete data (13.8 per cent. More importantly, however, the distributions of other variables are quite similar between the two data-sets. 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).[15] In the 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 2SLS 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. Similar to previous studies, the rain dummy shows small negative effects. Specifically, voter turnout drops by 1.1 percentage points in municipalities that recorded 3 millimetres or more of rain (Model 3). The effects are highly significant. 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.

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 per cent (in all data) and 64.3 per cent (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 one per cent 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 per cent (69.28 to 95.96 thousands) in our complete sample and 54.7 per cemt (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.

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 the necessary methodological issues, we estimated marginal effects of voter turnout by employing a highly effective instrumental variable based on election-day rainfall data. The results suggest that the act of voting — the simplest, the most popular, the least expensive, but the most essential model of political participation in democracy — does indeed bring about sizable differences in policy outcomes. Municipalities with high voter turnout tend to receive significantly larger benefits than municipalities with low voter turnout.

Our analysis suggests that, as far as our Japanese case is concerned, the responsiveness of legislators to policy needs of voters 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 (per cent)	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 (per cent)	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 (per cent)	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
R2	0.719	0.661	0.592
(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
R2	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 (per cent). 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, per cent)	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.

Notes

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

2   Dunning distinguishes his argument from recent discussions in the literature on 'local average treatment effects' or 'complier average causal effects' (for example, Imbens and Angrist 1994), which are heterogeneous effects of a treatment variable *across individuals or units*. Dunning assumes that the coefficients are common to all units, while focusing on causal heterogeneity *across portions of a treatment variable*. In this study, we follow Dunning's assumption. Similar to Dunning, we also present our arguments based on a standard regression framework rather than a potential outcome framework.

3   Depending on theoretical interests, one may further divide $X_t^{(1)}$ into several sub-components, which may or may not be observable. Observable components could include, for instance, the number of votes cast for the ruling coalition and the opposition. Unobservables could include the number of 'core' supporters for both the ruling and the opposition parties. In this study, however, we are interested in the *average* effect of voter turnout (*vis-à-vis* abstention), which can be considered a mixture of heterogeneous effects on various sub-components of $X_t^{(1)}$ on fiscal transfers.

4   The small but significant effects of rainfall on turnout are already reported in the existing studies (Gomez, Hansford, and Krause 2007; Knack 1994; Tamada 2006).

5   We do not assume $\beta_2 = 0$, which is equivalent to assuming that policy makers can detect a very small portion of $X_t^{(2)}$ in X_t and do not reward those 'weather-induced voters'.

6   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 1 April, is from *Kokudo Chiri Kyōkai* (Various years).

7   We prefer to use this dependent variable, instead of each fiscal year's per capita transfers, because taking the average for several years can minimize stochastic factors within each municipality.

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

9   Such a study examining how the electoral reform changed the causal effects of voter turnout on policy outcomes is both theoretically and substantially valuable. Nevertheless, we do not pursue it in this study, because we do not have sufficient variations in key variables within each municipality if we divide our data into pre-reform and post-reform periods. Doing so would result in further methodological complications. Reliable rainfall data sets at the municipal level exist only after 1989. After the 2000 election, there has been a sequence of drastic municipal mergers in Japan, which makes us difficult to compile a valid panel dataset.

10   Horiuchi and Saito (2003) find a positive and highly significant effect of the number of seats divided by the size of constituency on the per capita amount of total transfers in Japan.

11   Since the relationship between the index and the per capita amount of total transfers may be non-linear, we add its squared term. Our preliminary analysis suggests the validity of this non-linearity assumption.

12   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 utilising 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).

13   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. 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.

14   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.

15   The coefficients of other control (included exogenous) variables also tend to show highly significant effects with expected signs. We do not, however, provide interpretation to these coefficients because they are not main quantities of our interests.

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