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Local Economic Impacts of Legislative Malapportionment

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Abstract

Malapportionment, or unequal legislative representation, is a highly contested, and yet a common and persistent feature of electoral systems in many countries where more delegates per capita are granted to rural, sparsely populated, and economically struggling regions. Does legislative representation matter to local economies, and if so, how much? In Japan, the Lower House seats were severely malapportioned until an electoral reform substantially equalized the geographical distribution of representation for the 1996 election. We use this episode as a quasi-experimental setting to investigate the causal effect of malapportionment on the relative performance of local economies. There are two main results. First, an additional seat in the Lower House significantly expands local governments' fiscal space. An extra delegate is associated with more fiscal transfers, confirming Horiuchi and Saito (2003), and it also leads to more borrowing and more spending (largely on public capital), suggesting strong flypaper effects. Second and perhaps more interestingly, over-represented communities ultimately do not seem to benefit from this political and fiscal gift. We detect no discernible effects of legislative representation on establishment or employment. Our null results do not seem to be driven by omitted variable bias or general noise in the data. Rather, it is due to crowding-out effects in local labor markets. An additional representation and (the resulting additional transfers) produce more construction and public sector jobs, and yet these positive effects are entirely offset by comparable losses of jobs in other sectors.

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1. Introduction

Malapportionment, or unequal political representation in legislatures, often favors rural and economically struggling regions. It is a highly contested political issue as it violates the "one person, one vote" principle. Rural communities view it as an important political remedy to regional inequalities. Urban communities naturally fear that rural counterparts' delegates will form a strong and stable majority coalition to implement policies that do not necessarily benefit the majority population. Their frequent call for an electoral reform to equalize the number of delegates per capita across districts is met with fierce opposition from rural communities and their representatives. Do over-represented communities indeed benefit from this political gift, and if so, how large is the economic benefit of legislative representation? If malapportionment were eliminated to the detriment of over-represented communities, would they endure severe economic contraction?

Malapportionment is frequently seen in many countries. In the United States (US), the Senate seats are exceedingly malapportioned since every state is given the same number of seats in spite of sizable differences in population.[1] Even though the House seats are allocated more or less in proportion with population size,[2] extreme malapportionment in the Senate leads to uneven distribution of electoral votes per capita in the US Presidential Elections since the number of each state's electoral votes is equal to the total number of legislative seats apportioned to that state.[3]

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[1] For example, California and Wyoming are both represented by two senators even though the population of California, 37,253,956, is 66 times larger than that of Wyoming, 563,626, according the 2010 census.

[2] However, every state is guaranteed one seat in the House, regardless of population size; as a result, the number of seats per capita is still inversely related to state population.

[3] For example, Wyoming is given 3 electoral votes, but California is given only 55 electoral votes, instead of 3x66 = 198 to reflect California's population relative to Wyoming's.

Outside the US, malapportionment is more pervasive in new (or consolidating) democracies (Samuels and Snyder, 2001, Ong, Kasuya, and Mori, 2017) and countries with high income inequality (Ardanaz and Scartascini, 2013). Interestingly, it is a feature of political institutions that is strongly correlated with economic underdevelopment (Bruhn, Gallego, and Onorato, 2009). In many of these countries, malapportionment tends to persist for decades because incumbent politicians, who have benefitted electorally from the existing apportionment scheme, are not motivated to amend it (Bruhn, Gallego, and Onorato, 2009, Boone and Wahman, 2015).

Legislative bargaining models show that malapportionment can lead to favorable budget allocation for over-represented districts via two interactive mechanisms (e.g., Ansolabehere, Snyder, and Ting. 2003, Knight, 2004). First, the vote and bargaining power of each delegate is unrelated to her district population; as a result, if every delegate draws the same amount of central government transfers for her district, those from small districts can draw more per capita. Second, even when the allocation of transfers is institutionally tied to population size to some extent, over-represented districts might be granted more transfers per capita because delegates from districts with fewer voters can be “bribed” more cheaply into supporting the winning coalition’s agenda.

Numerous empirical studies confirm this intuitive prediction for the US (e.g., Atlas, Gilligan, Hendershott, and Zupan, 1995, Ansolabehere, Gerber, and Snyder, 2002, Elis, Malhotra, and Meredith, 2009) and elsewhere (e.g., Bruhn, Gallego, and Onorato, 2009, Horiuchi and Saito, 2003, Pitlik, Schneider, and Strotmann, 2006, Rodden, 2002). Interestingly, some papers find similar bargaining mechanisms on the United Nations Security Council in which member countries trade votes for cash (Kuziemko and Werker, 2006, Dreher, Sturm, and Vreeland, 2009a, and Dreher, Sturm, and Vreeland, 2009b). A question, however, remains as to how much local economies benefit from this political and fiscal gift. This question has yet to be investigated extensively in the literature, which thus far has focused predominantly on the relevance of legislative malapportionment to the geographical allocation of central government transfers.

In principle, fiscal windfall which results from legislative over-representation should, at the minimum, increase over-represented regions' disposable income dollar-for-dollar, but it might even have additional stimulative effects on local economies. Local jurisdictions might reduce distortionary taxes when they receive more transfers, which, in turn, encourages business creation, generates more jobs, and increases output. Spillover effects of transfers will be potentially far-reaching if local governments invest it in public capital with large positive externalities. Moreover, if local economies are characterized by under-employment, credit market friction, and hand-to-mouth consumers, then fiscal windfall will further stimulate spending, which, in turn, will produce more output and income through Keynesian multiplier effects. Of course, the aforementioned conditions might not be present, and over-represented communities might benefit little from such fiscal windfall. Perhaps, distortionary taxations are limited to begin with, or public infrastructure projects with high social return are scarce, or local labor markets are tight. It should not be taken for granted that legislative over-representation leads automatically to the economic revitalization of local communities.

Observationally, over-represented communities often continue to struggle economically, an anecdote that might make casual observers skeptical of the economic significance of legislative malapportionment. The counterfactual scenario, however, is difficult to establish since malapportionment is highly endogenous to regional economic disparities. Population declines in rural areas because of limited economic opportunities. This leads mechanically to increases in the number of delegates per capita, provided that delegate size changes only infrequently and/or trivially. Cross-sectional comparisons of regions with varying levels of legislative representation, thus, does not reveal any causal arrow from representation to local economic performance.

In the present paper, we overcome this empirical challenge by using an electoral reform in Japan which significantly reduced malapportionment for the 1996 the Lower House election as a quasi-experiment. Before the electoral reform, Japan's Lower House seats were severely malapportioned. Table 1 re-produces the Loosemore-Hanby (LH) index of electoral disproportionality of the 20 most malapportioned lower chambers from Samuels and Snyder (2001) and adds the LH index of Japan's Lower House before the reform from Ong, Kasuya, and Mori (2017).[4] Before 1996, Japan indeed had one of the most malapportioned lower house chambers and was ranked similarly to Chile, Argentina, Gambia, Columbia, and Andorra. After the reform, Japan's LH index decreased sharply to .077 (Horiuchi and Saito, 2003), which is approximately the average value in the cross-country data of LH index (Samuels and Snyder, 2001).

[4] LH index is calculated with the following formula:

$$LH = \frac{1}{2}\sum_{i} |s_i - v_i|$$

where s_i and v_i represent the proportion of seats allocated to district *i* and the proportion of population living in district *i*, respectively. LH index measures the proportion of the malapportioned seats (i.e., the proportion of seats that must be reallocated from over-represented districts to under-represented districts in order to entirely eliminate malapportionment). See Loosemore and Hanby (1971) and Samuels and Snyder (2001).

This episode is an appealing setting because, as Bruhn, Gallego, and Onorato (2009) and Boone and Wahman (2015) show, large-scale reapportionment with potential to fundamentally shift the legislative bargaining equilibrium does not present itself to researchers often. Japan's experience provides us with a rare, opportune laboratory that could reveal whether the geographical distribution of delegates affects the geographical distribution of productive activities. Moreover, Horiuchi and Saito (2003) show that the electoral reform indeed led to a dramatic shift of fiscal transfers away from over-represented municipalities. We replicate this important result and build on it in order to trace the economic impact of representation and the resulting fiscal windfall on local communities.

There are two primary findings. First, legislative representation has large and robust effects on local jurisdictions' fiscal policy. Confirming Horiuchi and Saito (2003), we show that the reform led to significant re-distribution of central government transfers away from over-represented municipalities. Additionally, we find that the reform had larger differential effects on spending than transfers as municipalities whose transfers increased (decreased) borrowed more (less). That is, we find strong evidence of flypaper effects (or crowding-in effects).[5] We also find that transfer-induced expenditure is spent mostly on public investment, as opposed to current expenditure.

Second and perhaps surprisingly, the reform ultimately had no significant effect on local economies in terms of establishment and employment in spite of its robust effects on fiscal

[5] These results are consistent with Nagamine (1995), Doi (1996, 2000), and Bessho and Ogawa (2015). One must be cautious when drawing causal interpretation since transfers might be endogenous to local jurisdictions' preference for public goods which is unobservable or difficult to control for (Knight, 2002). Recent papers also suggest that flypaper effects depend on political and institutional context (e.g., Leduc and Wilson, 2017, Lutz, 2010). See Inman (2008) for a recent review of the literature on this "flypaper effects".

transfers. Over-represented municipalities, which received less transfers and spent less on public capital as a result of the electoral reform, do not seem to have undergone any economic retrenchment. In particular, our estimates of local fiscal job multipliers, which we calculate based on the differential effect of the electoral reform on fiscal transfers, are statistically insignificant. Even the most optimistic estimate (i.e., the upper bound of a 95% confidence interval) suggests that the cost per job is 5,000,000 yen (approximately, $50,000), which is approximately 20% more than the median annual income in Japan.

We delve deeply into the nature of heterogeneities across municipalities to address a concern that unobservable shocks in the post-reform period are somewhat linked to the pre-reform level of representation; e.g., over-represented municipalities might have drawn favorable economic shocks which ended up offsetting the negative effect of the reform. We first search for evidence of pre-trend that varies systematically with the pre-reform level of representation. We detect no such pre-trend. Over-represented municipalities were not doing any better (or worse), relative to under-represented municipalities, before the reform.

We then calculate two observable measures of political and economic shocks: (1) the share of votes for the ruling party to capture patronage channels and (2) Bartik industry shift-share to capture cross-municipality heterogeneities in overall economic conditions. We inspect whether changes in these two variables from the pre-reform to the post-reform period are systematically correlated with the pre-reform level of representation. We find no evidence that they are. Furthermore, these control variables turn out to be strong predictors of local economic performance as they boost the goodness of fit in our regression models for changes in economic outcomes, and yet the inclusion of these variables does little to alter our main findings. These patterns suggest that omitted variable bias might be inconsequential.[6]

In terms of mechanisms, we find evidence of crowding-out in local labor markets. After the reform, construction and public sector jobs increased more in under-represented municipalities, suggesting that the electoral reform directly affected employment in sectors that rely heavily on fiscal transfers. However, at the same time, non-construction and non-public sector jobs declined more in under-represented municipalities. In short, Japan's reapportionment experience suggests that, while legislative representation indeed influences the geographical allocation of fiscal resources, its ultimate economic impacts on local communities can be small, if any, if crowding-out effects dominate.

Although this paper primarily contributes to the literature on legislative representation, it is also related to the new but rapidly expanding literature on local fiscal multiplier.[7] A large majority of papers in this literature traces the economic impact of military spending or federal transfers, using the post WWII subnational data from the US. Estimates of local fiscal multipliers in these papers are large, in general, and well above one in some cases. However, there are several notable exceptions.[8] Cohen, Coval, and Malloy (2011) is perhaps the most relevant to

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[6] See Altonji, Elder, and Taber (2005) and, more recently, Oster (2017) for formal analyses of how one can make an inference about the size of omitted variable bias, based on sensitivity analyses.

[7] Chodorow-Reich (2019) synthesizes this literature. Most papers in this literature uses rich and plausibly exogenous variation in government spending in order to estimate the causal impact of fiscal policy on local economies. For example, see Chodorow-Reich, Feiveson, Liscow, and Woolston (2012), Conley and Dupor (2013), Dube, Kaplan, and Zipperer (2014), Dupor and McCrory (2018), Dupor and Mekhari (2016), Feyrer and Sacerdote (2012), Wilson (2012), Nakamura and Steinsson (2014), and Dupor and Guerrero (2017), Shoag (2016), Suárez, Serrato, and Wingender (2016), and Adelino, Cunha, and Ferreira (2017).

[8] In contrast to many of the studies which use fiscal windfalls as an exogenous source of variations, Clemens and Miran (2012) exploits differential sensitivities in state government spending to local economic cycles to identify fiscal spending shocks. Their estimates of local fiscal multiplier are much smaller, suggesting that the stimulative effect of government spending is attenuated by the expectation of future tax increase. Brunet (2018) examines the economic effect of military spending using state-level data from the WWII era and find that local fiscal multipliers

our paper as they find evidence of strong crowding out effects. Brückner and Tuladhar (2014) is also directly relevant to our paper since they estimate local fiscal multipliers using disaggregated prefecture-level data from Japan. Interestingly, they show that central government-financed investment spending has small multipliers ($\approx .4$).[9] In a complementary study, Miyazaki (2018) shows large crowding-out effects of public spending on private investment in Japan, a result analogous to Cohen, Coval, and Malloy (2011).

The rest of this paper is structured as follows. Section 2 describes Japan's electoral reform which sharply reduced differences in delegate size per capita across districts for the 41th Lower House election in 1996. Section 3 sets up the econometric model, clarifies identification strategy, and describes the data. Section 4 shows and interprets the results. Section 6 concludes with discussion.

2. The 1996 Electoral Reform

The 40th Lower House election in 1993 was the last election held under the old multimember single non-transferable vote system. The old system set up 129 multimember districts, in which more than one seat was assigned to each district. The total number of delegates was 511 with the median delegate size being 4. This peculiar electoral system posed a

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then were also small. She attributes it to the conversion of civilian manufacturing as well as to concurrent increase in saving and income tax. Feiveson (2015) finds that the bargaining strength of public sector unions affects the allocation of federal transfers toward increasing wages for public sector workers rather than expanding public sector services. She presents suggestive evidence that local fiscal multiplier varies with the bargaining power of public workers union.

[9] Estimates of local fiscal multipliers outside the US are still limited. See Corbi, Papaioannou, and Surico (2014), Brückner and Tuladhar (2014), Acconcia, Corsetti, and Simonelli (2014), Bucheim and Watzinger (2017), Guo, Liu, and Ma (2016). Estimates of local fiscal multiplier from different countries are wide-ranging, unsurprisingly. Acconcia, Corsetti, and Simonelli (2014) estimate that local fiscal multiplier in Italy is well above unity and close to 2. Guo, Liu, and Ma (2016) find that local fiscal multipliers in China is well below one.

unique coordination problem to both large political parties and their core supporters. For example, in a district with three seats, a large party would strategically nominate 2 candidates in order to either maintain or take back the majority in the Lower House. The problem is that, even when a large party garners more than two-thirds of votes in aggregate, it might win only one seat if too many votes are cast for its stronger candidate, since those votes could not be transferred to the other, weaker candidate. The party must be attentive in order to avoid excessive competition between its own candidates. It is collectively inefficient if two nominees of the same party fight over the party's core voters. The Liberal Democratic Party (LDP) of Japan held the majority in the Japanese Diet in most of the post-WWII periods under this system.[10] It goes without saying that the LDP and its members had to navigate through it adeptly and they did so with success.[11]

However, the LDP leaders were keenly aware that a large party (like the LDP) benefits from a "winner-takes-all" majoritarian system. The party leaders made numerous, repeated attempts to reform the electoral system since the 1950s (Reed and Thies, 2001, McElwain, 2008). Minority parties resisted it fiercely, but more importantly (and perhaps surprisingly), many of the LDP members were consistently opposed to the party leaders' reform proposals. Even though they recognized that the party would collectively strengthen its electoral

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[10] See, for example, Ramseyer and Rosenbluth (1994) for political implications of this electoral system.

[11] For instance, the second district of Yamagata prefecture was given 3 delegates in the Lower House. The largest party, the Liberal Democratic Party, would always nominate two candidates, Koichi Kato and Riichiro Chikaoka. Together, they would garner around 60% of votes. The problem for the LDP (and Chikaoka) was that Kato was overwhelmingly stronger as he would typically win by more than 10 percentage point. Nevertheless, their campaigns were well-coordinated not to steal votes from each other's core supporters. Chikaoka managed to win every time albeit with much smaller margin, consistently giving the LDP two seats from this district. The third seat was occupied by a Socialist, Shogo Abe, who was credited for working closely with the LDP leadership to build the airport in Yamagata. Chikaoka retired early and became a royal political supporter of Kato.

advantage from a majoritarian system, they feared that the reform would risk their own individual electoral fortune.

However, the turning point arrived in the late 1980s and early 1990s when a series of political scandals involving the LDP leadership eroded the public's confidence in the ruling party. Some of the party leaders departed the LDP, and a coalition of smaller parties seized the majority in 1993. The public opinion also turned against the old electoral system which was then perceived to have led to excessive patronage between the LDP politicians and their local supporters. As soon as the LDP formed a coalition government with the Japan Socialist Party and the New Party Sakigake in 1994, the party leaders took this opportunity to decisively reform the electoral system.

The final reform replaced the cumbersome multimember single non-transferable vote system with the mixed-member majoritarian system which combines 300 single-member districts with 200 proportional representation from 11 large regional blocs (Hokkaido, Tohoku, Kita Kanto, Minami Kanto, Tokyo, Hokuriku Shinetsu, Tokai, Kinki, Chugoku, Shikoku, and Kyushu).[12] It was a product of political compromise. On the one hand, the "winner-takes-all" majoritarian single member district system would give electoral advantage to the LDP. On the other hand, small opposition parties were content with the reform as they expected to benefit from proportional representation. The reform was to be implemented in the 41th Lower House election, which was eventually held on October 22, 1996. The LDP won 239 seats (169 from

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[12] The new system is much simpler for voters. A voter has two ballots. On one ballot, she picks her most preferred candidate in her district. The candidate with the most votes is elected. On the second ballot, she picks her preferred party. The seats for proportional representation are allocated to each party in proportion to the share of votes it receives in each bloc.

single member districts plus 70 from proportional representation). Even though it fell short of winning the majority of the Lower House seats, the LDP maintained its coalition government with the Japan Socialist Party and the New Party Sakigake.

The introduction of single member districts necessitated the reapportionment of the Lower House seats. 200 seats in the proportional representation were apportioned simply to the 11 blocs evenly in proportion to the 1990 census population count.[13] The remaining 300 seats for single-member districts were first allocated to 47 prefectures. Every one of 47 prefectures received one seat, regardless of population, and then the remaining 253 seats were distributed to each prefecture in proportion to its population share, based on the 1990 census population count. Panel A of Figure 1 plots the actual delegate size and population (in millions) for each prefecture. As expected, delegate size rises with population.[14] Panel B of Figure 1 plots delegate size per 1 million population and population (in millions). Because every prefecture is given 1 delegate to begin with, delegate size per capita declines with population and it declines more steeply when population is small.[15] That is, some malapportionment remained intact.

After seats were allocated to 47 prefectures, an independent, non-partisan, committee, which excluded any Diet members, was tasked with apportioning seats within each prefecture, based on the following three explicit guidelines (Christensen, 2004):

1. The population of the smallest district must be no smaller than a half of the largest district's population.

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[13] Apportionment for proportional representation is as follows: Hokkaido (9), Tohoku (16), Kita Kanto (21), Minami Kanto (23), Tokyo (19), Hokuriku Shinetsu (13), Tokai (23), Kinki (33), Chugoku (13), Shikoku (7), and Kyushu (23)

[14] As prefecture population increases by 1 million, delegate size rises by 2, approximately, since the national population in the 1990 census was approximately 124 million (i.e., $253/124 \approx 2$).

[15] Note that since delegate size rises only discretely with population, delegate size per capita rises with population in some ranges.

2. Municipalities must not be split
3. Each district must be geographically contiguous and compact.

The committee was not able to adhere to the first two guidelines in some cases. Some municipalities were simply too big and they had to be split up. Others were just too small and not necessarily neighboring one another to produce compact districts.

It is safe to say that the committee used some discretion in drawing new district boundaries. One might be concerned then that the new district map contains information relevant to the future socio-economic prospect of municipalities, thereby posing a threat to our econometric identification. For example, the committee might have been more tolerant of creating small districts if population is projected to increase in those districts. Moreover, the committee might have been more accepting of small districts when a cluster of municipalities are geographically isolated. An econometric concern in both cases is that these municipalities are assigned to small districts non-randomly. Therefore, we chiefly use the variation across original districts and investigate whether the reform had differential effects on municipalities with varying level of representation before the reform. However, it should be noted that the main results still hold under alternative specifications in which we use variations in *changes in representation*, which incorporate the information about the distribution of delegates in the new district map.

Figure 2 contrasts the distribution of delegates per 1 million district population under the old system (the 40th Lower House election in 1993) with that under the new system (the 41th Lower House election in 1996). Recall that the total number of delegates declined from 511 to 500 and that only 300 (or 60%) of them were now elected from single-member districts (the rest are from proportional representation). To reflect this aggregate decline in the number of delegates who represent local districts, the average delegate size per 1 million district population declined from 4.5 to 2.5. More importantly, the new apportionment scheme produced much smaller disparities in the number of delegates per 1 million district population. Figures 3 and 4 display the geographical distribution of delegates per 1 million district population in the 40th and 41st Lower House elections, respectively. The reapportionment visibly reduced geographical differences in representation. Our main question then is whether the reform had any effects on the relative performance of local economies.

3. Data and Empirical Methodology

Our data are at the municipality-level. There were a little over 3200 municipalities at the time of the electoral reform. Municipalities were assigned to 129 multi-member districts before the reform and then re-assigned to 300 newly created single-member districts for the 41st Lower House election. As described in the previous section, the apportionment committee was not able to completely eliminate malapportionment, which might raise a concern that variation in delegate size per capita in the new electoral district map is potentially endogenous. For example, the committee might have been more accepting of creating small districts if those districts' population are forecasted to grow faster than other districts, or if municipalities are so geographically isolated that they could not join a larger, or more appropriately sized, district. Our econometric model, thus, relies on variation in the level of over-representation before the reform to estimate the reform's differential impacts on local economic performance in the post-reform period.

When formulating our econometric models, it is important to keep in mind that Japan's public finance system is highly centralized where the central government collects about 60% of total tax revenue (Doi and Ihori, 2009). The central government provides local governments with a large amount of transfers in order to help them finance the local provision of public services as well as local public infrastructure projects. Transfers to local jurisdictions consists of grants and local allocation taxes.[16] The central government also exerts significant influence over local jurisdictions' fiscal policy. For example, national laws essentially determine local tax rates and sources. When local governments issue bonds, it needs the central government's permission, and a large proportion of local government bonds were purchased by the Trust Fund Bureau of Ministry of Finance during the time period that we study. Thus, informal relationships between local governments and the central government are important. In the regression analysis that follows, we use the sum of grants and local allocation taxes to measure central government transfers, following Horiuchi and Saito (2003).[17] We also use the data on local tax revenue, borrowings, and expenditure in order to trace the impact of transfers on the overall stance of local fiscal policy.

Our basic regression equation is:

$$\Delta Y_{it} = \beta_t + \beta_i + \beta Reform_t \times Delegate_k + \gamma_1 \Delta Population_{it} + \gamma_2 \Delta Elderly_{it} + \varepsilon_{it}$$

[16] The usage of grants is restricted by the central government, while that of local allocation taxes is unrestricted.
[17] In an alternative specification, we add transfers from prefecture government to central government transfers to check robustness. Transfers from prefecture is about less than 5% of a typical municipality government's revenue. The results, which are available upon request, are broadly similar.

The dependent variable, ΔY_{it}, is changes in fiscal and economic outcomes of interest. Subscript *i* and *t* represent municipality and year, respectively. Fiscal variables are: transfers (grants plus local allocation taxes), transfers plus borrowings, local tax revenue, total expenditure, public investment expenditure, and current expenditure (all in 100,000 yen per capita, which is, approximately, 1,000 dollars). Economic variables are: employment and establishment (per capita). β_t captures economy-wide shocks. β_i represents municipality-specific trend in fiscal and economic outcome.

One issue in terms of our data coverage is that the data on employment and establishment are gathered by the Enterprise Survey only every five years (in October and November). We use the data from 1991, 1996, and 2001, which straddle the 41th Lower House election in October 1996, the first election under the new electoral system. The Enterprise Survey also reports employment by industry. We later make use of this multi-level (municipality-industry-year) data to calculate Bartik industry shift-share to inspect whether the pre-reform level of representation is systematically linked to industry shocks after the reform. In sum, our municipality-level data cover one decade from 1991-2001, which is divided into two subperiods, 1991-1996 and 1996-2001. The first period is the pre-reform period (the control period), whereas the second period is the post-reform period (the treatment period).

The data coincide with what is widely known as the "Lost Decade" when the Japanese economy stagnated with the average economic growth rate of less than 1% (Kuttner and Posen, 2001, Hayashi and Prescott, 2002). For the results that we report in this paper, we do not use the pre-1991 period as a part of the control period since economic conditions before 1991 was starkly different (i.e., the average growth rate of the Japanese economy well exceeded 2%

before 1991).[18] Similarly, we do not use the post-2001 period, say from 2001-2006, as a part of the treatment period since the Japanese economy began its steady recovery due to a steady increase in export demand. More importantly, the Japanese government began to overhaul the structure of local public finance in 2003 to shift tax revenue sources to local jurisdictions while reducing central government transfers (Doi and Ihori, 2009). If we are to include the data after 2001, it will be difficult separate out the effects of the electoral reform from the local public finance reform.[19]

The key independent variable is $Reform_t x Delegate_k$. $Reform_t$ is a dummy variable for the 1996 reform (= 1 for 1996-2001 and zero for 1991-1996). $Delegate_k$ represents the number of delegates per 1 million population in multimember district *k* to which municipality *i* was assigned in the 1993 election. Recall that the electoral reform was first implemented for the Lower House election on October 22, 1996. Hence, our assumption is that the reform could not have affected fiscal and economic variables immediately. We verify this assumption in the next section at least with respect to fiscal variables, which are reported on annual basis. β is the key parameter of interest. It captures the effects of the 1996 reform on *relative* fiscal and economic outcomes. For example, if $\beta < 0$ for changes in employment per capita, then it means that local economies of over-represented municipalities (i.e., large $Delegate_k$) created fewer jobs after the reform, relative to under-represented municipalities, *ceteris paribus*.

The crucial identification assumption is

[18] The result of alternative specifications which include the data from 1986-1991 as a part of the control period turn out to be qualitatively similar. These results are available upon request.

[19] An additional consideration is that the post 2000 municipality-level data are not quite comparable as over one-third of municipalities were merged non-randomly as small municipalities with weak fiscal and economic conditions were more motivated to consolidate. Some of these municipalities are documented to have engaged in outsized fiscal spending because of bail-out expectation (e.g., Nakazawa, 2016, Hirota and Yunoue, 2017).

$$E(\varepsilon_{it}|Reform_t \times Delegate_k) = 0$$

It states that idiosyncratic shocks in the post-reform period (conditional of covariates) must be uncorrelated with the degree of over-representation in the 1993 election. In this basic specification, we address two threats to our identification. First, if there is growth convergence (divergence) process, low-income municipalities, which were typically over-represented, tend to grow faster (more sluggishly) than high-income municipalities. Municipality-specific effects are included to account for the possibility that each municipality's fiscal and economic outcomes follow different trend during the 1990s due to either convergence or divergence process.

Second, Japan began to age as a nation with elderly dependency ratio increasing rapidly from 12% to 17% during this time period.[20] The aggregate economic effect of rapid increases in so-called "demographic tax" is captured by year effects, β_t, but municipalities did not age homogeneously. Rural municipalities aged faster and their population declined faster than urban municipalities. We control for cross-municipality heterogeneity in the speed of aging and the associated increase in demographic tax by including population growth, $\Delta Population_{it}$, and changes in elderly population relative to total population, $\Delta Elderly_{it}$ (defined as 65-year old or older). Standard errors are clustered at the level of district *k*, which provides us with the primary source of variation to estimate β.

We compile our municipality-level data from two sources. One is the Statistics Bureau of Ministry of Internal Affairs and Communications, from which we draw our fiscal and economic

[20] It has increased to 27% to date.

variables.[21] The data on employment are also disaggregated by industry classifications. We use this data set to calculate Bartik industry shift-share, a key control variable. The population data are available for 1990 and 1995 from the national census. The data on district size are taken from JED-M CD-ROM which is available by the Leviathan Databank. We use this same data source to calculate the share of LDP vote in each municipality to use in robustness check as another control variable for political patronage effects.

We calculate seven outcome variables of interest. We calculate changes in transfers per capita (in 100,000 yen ≈ 1,000 dollar). For 1991-1996 and 1996-2001, we divide changes in transfers by population as of 1990 and 1995, respectively. In similar fashion, we calculate changes in transfers plus borrowings, total expenditure, investment expenditure, and current expenditure, and local tax revenue per capita (in 100,000 yen). We also calculate changes in employment and establishment per capita.

We drop a small number of municipalities from the data.[22] First, we remove municipalities which were affected by the 1995 Great Hanshin Earthquake.[23] Second, there are so-called special wards of Tokyo and ordinance designated cities. These are exceptionally large municipalities and are given more administrative and fiscal autonomy than typical

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[21] The data on income at the municipality-level are also available but it is *taxable income*, which excludes a basic exemption such as exemptions for dependents and various types of deductions, such as deductions for insurance premiums, medical expenses and business expenses of the self-employed. In addition to employment and establishment data, we use the data on taxable income to estimate local fiscal multipliers. Our estimates of local fiscal multiplier turn out to be positive but well below one. Nonetheless, we are concerned that some of the deducted expenditure respond endogenously to both fiscal and income shocks, effectively biasing our estimates and thus refrain from basing our conclusion on these results. The results of local fiscal multiplier are available upon request.

[22] The removal of these municipalities does not alter the central results.

[23] The earthquake scored the magnitude of 6.9 and severely damaged the southern part of Hyogo prefecture, major commercial centers and suburbs. We dropped municipalities that reported earthquake-related death. The results are similar when we drop the entire prefecture of Hyogo.

municipalities. These municipalities are not quite comparable, and thus dropped from the data. Third, as previously discussed, some large municipalities (15 of them) were split into two districts in the new district map. Fourth, there were a small number of municipality mergers and consolidations during our sample period. We remove those merged municipalities. At the end of this process, we dropped fewer than 100 municipalities. Our final data set is a panel data set consisting of 3152 municipalities in 1991, 1996, and 2001. Table 2 summarizes the data.

4. Results

Before we show the basic results, we examine the exact timing of fiscal variables' response to the electoral reform using the annual data around 1996. Recall that the reform which was formulated at the end of 1994 took effects in the 41^{st} Lower House election, which was held on October 22, 1996. Japan's fiscal year starts on April 1^{st} and ends on March 31^{st} (e.g., the 1996 fiscal year starts on April 1^{st}, 1996 and ends on March 31^{st}, 1997). The budget for the 1996 fiscal year was already approved well in advance of April 1^{st}, 1996. Since no significant supplementary budget was approved in 1996 (Posen, 1998), we are likely to spot the differential effect of the reform on fiscal variables not during the 1996 fiscal year, but starting the 1997 fiscal year. To verify this, we estimate the following panel regression using the annual data from 1994-1997:

$$\Delta Y_{it} = \theta_t + \theta_i + \theta_0 (Placebo\ Reform)_t \times Delegate_k + \theta_1 Reform_t \times Delegate_k + \varepsilon_{it}$$

θ_t and θ_i are year-specific effects and municipality-specific effects. As before, the key independent variable is $Reform_t xDelegate_k$. $Reform_t$ is a dummy variable for the 1996 reform (= 1 for 1996-1997 and zero for 1994-1996). $Delegate_k$ represents the number of delegates per 1 million population as before. Additionally, we include ($Placebo\ Reform)_t xDelegate_k$ where ($Placebo\ Reform)_t$ is a dummy variable that equals 1 for 1995-1996 and zero otherwise. We have no compelling reason to expect this interaction term to have a significant coefficient, but it is included to check whether the reform had any effects when it was yet to be implemented. As before, ΔY_{it} is changes in transfers (grants plus local allocation taxes), transfers plus borrowings, local tax revenue, expenditure, public investment, and current expenditure.

The results are reported in Table 3. Note that the coefficient on the interaction of reform dummy with delegate size is negative and statistically significant for transfers (Column 1). In contrast, the coefficient on the interaction term of *placebo reform* with delegate size is not significant. Hence, the reallocation of transfers away from over-represented municipalities to under-represented ones began to occur during the 1997 fiscal year, not the 1996 fiscal year. The results also show similar patterns for transfers plus borrowing (Column 2), total expenditure (Column 4), and investment expenditure (Column 5). Both local tax revenue and current expenditure do not seem to have responded to either placebo reform or actual reform. In sum, the annual data around the implementation of the electoral reform confirms that it affected transfers after the 41st Lower House election, not before, and local jurisdictions responded to this fiscal shock rather quickly by adjusting borrowing and expenditure.

Now, Table 4 displays the results of our basic regression model, using the data from 1991-2001 that include the data on establishment and employment. Column 1 reports a negative coefficient on the interaction term of reform dummy with delegate size per capita for fiscal transfers, which replicates Horiuchi and Saito (2003). The result confirms the central prediction of legislative bargaining model in that the reform led to reallocation of fiscal transfers away from over-represented municipalities to under-represented ones. Though the size of estimated coefficient itself is modest (8,395 yen per capita for an additional delegate per 1 million district population), the overall quantitative implication is non-trivial since delegate size per 1 million district population varied substantially from 2.5 to 7 before the reform. For example, relative to the most over-represented municipalities, the electoral reform gave the most under-represented municipalities fiscal windfall of 37,778 yen (= 8395 x 4.5) per capita, which would translate into 151,110 yen (about 1,500 dollars) for a family of four.

In addition, the electoral reform had similar differential effects on borrowing. After the reform, over-represented municipalities borrowed less than under-represented municipalities, which ultimately shows up as large differential effects of the reform on transfers plus borrowing and total expenditure (Columns 2 and 4). Note that expenditure declined more than yen-for-yen with transfers for over-represented municipalities, relative to under-represented ones, showing strong evidence of flypaper effects (or crowding-in). As for local tax revenue, the estimated coefficient on the interaction of reform dummy with delegate size is positive, yet small and insignificant (Column 3). Hence, the reform did not induce municipalities to undertake significant tax cut, which makes sense in light of the tight control that the central government impose on local jurisdictions' tax policy in Japan. Columns 5 and 6 report that it is mostly public investment spending, not current spending, that shifted with total government spending. Thus, the electoral reform induced significant changes in the geographical distribution of fiscal spending. In contrast, note that the effect of legislative representation on real economic outcomes appears to be muted. The coefficient on the interaction term is not significant for either employment or establishment (Columns 7 and 8). Hence, these results indicate that over-represented communities did not suffer from significant economic contraction which they might have feared as they saw large declines in representation and fiscal transfers.

Our estimates rely crucially on the assumption of parallel trends between over-represented and under-represented municipalities. That is, idiosyncratic economic shocks in the post-reform period, 1996-2001, must be orthogonal to the pre-reform level of representation, conditional on municipality fixed effects and demographic changes which we control for. However, given that the real economic effect of legislative representation is "missing" despite its strong and robust effects on fiscal transfer, a natural concern is that over-represented municipalities might have faced more favorable economic conditions, thereby obscuring the adverse effect of the reform on them. It is difficult to explicitly account for these unobservable shocks, but we assess the seriousness of possible omitted variable bias by delving deeper into the nature of cross-municipality heterogeneities in two ways.

First, we cannot formally test whether the counterfactual trajectory of each municipality's economy in the post-electoral reform period is unrelated to the pre-reform level of legislative representation. However, our identifying assumption will just be less plausible if changes in local fiscal and economic conditions are strongly correlated with the level of representation during the pre-reform period from 1991-1996. The presence of such correlation in the pre-reform period can be detected by chance and does not automatically mean that our identifying assumption is invalid. However, one might be more skeptical of it because economic shocks are typically correlated over time within each locality (e.g., shocks are positively correlated if they tend to persist, whereas negatively correlated if they tend to mean-revert).

To address this concern, we use the pre-reform data from 1991-1996 to inspect whether there is any pre-existing trend that varies systematically with the pre-reform level of legislative representation. More formally, we estimate the following cross-sectional regression:

$$\Delta Y_{i91-96} = \eta_{91-96} + \eta Delegate_k + \eta_1 \Delta Population_{i91-96} + \eta_2 \Delta Elderly_{i91-96} + \varepsilon_{i91-96}$$

As in the baseline model, ΔY_{i91-96} is changes in fiscal and economic outcomes of interest, but only from 1991-1996. Subscript *i* represent municipality. The coefficient on $Delegate_k$ captures the presence of cross-municipality correlation between the level of representation and fiscal and economic outcomes before the electoral reform. For instance, suppose $\eta < 0$ for changes in employment per capita. Then, it means that over-represented municipalities' economies performed worse than under-represented ones even before the reform took effects. In this case, if shocks reverted back to the "normal level" in the post-reform period, thereby positively affecting over-represented municipalities, then we might not detect the negative economic effect of the reform on over-represented municipalities, even if there is.

Table 5 displays the results of this pre-trend test. The coefficient on delegate size is statistically insignificant for employment and establishment (Columns 7 and 8), suggesting that economies of over-represented municipalities were not doing any better (or worse), relative to under-represented municipalities before the reform. Interestingly, the coefficient on delegate size is also insignificant for fiscal variables (Columns 1-6). That is, legislative representation is not strong predictor of change in central government transfers or change in spending in the pre-reform period. In sum, changes in fiscal and economic outcomes of municipalities from 1991-1996 are not systematically related to the pre-reform level of legislative representation.

We recognize that the pre-trend test is not a formal test of our identification assumption. If shocks in the pre-reform period are weakly correlated with shocks in the post-reform period, then the absence (or presence) of pre-trend says little about the credibility of our identifying assumption. Hence, a concern still is that entirely different shocks might have developed in the post-reform period. If these new shocks are linked to the pre-reform level of representation through some unmeasured mechanisms, then we have omitted variable bias. We conjecture that there might be two ways in which our identification assumption is violated to produce the results (or null results) that we obtain.

First, there are patronage channels in which votes for the ruling party are exchanged for favorable policies. Patronage effects do not confound our results if they work independently from the effects of legislative representation. However, if the electoral support for the LDP strengthened in over-represented municipalities after the reform (perhaps to compensate for the negative effects of reduced representation), then the economic effect of representation appears to be “missing” in the data, not because representation does not matter, but because patronage effects offset the effects of representation. Second, suppose that under-represented municipalities faced negative economic shocks in the post-reform period and that the central government responded by allocating more transfers to these municipalities. Then, relatively speaking, over-represented municipalities would receive less transfers, and yet it would appear that fiscal retrenchment had little negative economic effects on these municipalities since they faced more favorable economic conditions to begin with, relative to under-represented municipalities.

To probe these two possibilities, we calculate (1) the vote share of the ruling party to capture the effects of patronage and (2) Batrik industry shift-share to proxy for local economic conditions. There are compelling anecdotes as well as statistical evidence that patronage is an important factor in the formulation of the LDP's economic policies in Japan (Ramseyer and Rosenbluth, 1994, Meyer and Naka, 1999, Tamada, 2009, Catalinac, de Mesquita, and Smith, 2018). Similarly, Bartik industry shift-share is widely considered as a strong predictor of local economic performance (e.g., Bartik, 1991, Blanchard and Katz, 1992, Bound and Holzer, 2000, Imai and Takarabe, 2011, Kerwin, Hurst, and Notowidigdo, 2018).[24] We use the data on employment in over 100 sectors to compute the municipality-specific average employment share of each industry in population and multiply it with the national growth rate of employment in the corresponding industry. We examine how changes in these two variables, which proxy for potentially confounding political and economic shocks, are dispersed across municipalities with varying level of representation. In addition, in the spirit of Altonji, Elder, and Taber (2005) and also, more recently, Oster (2017), we informally check the size of potential omitted variable bias by examining how sensitively the coefficient on our key independent

[24] Recent papers question whether Bartik industry shift-share cleanly captures exogenous industry shocks (Jaeger, Ruist, and Stuhler. 2018, Adão, Kolesár, and Morales, 2018, Borusyak, Hull, and Jaravel, 2018, and Goldsmith–Pinkham, Sorkin, and Swift, 2018). Our purpose is not to identify exogenous shocks, but to probe whether our key independent variable is somehow linked with a well-known correlate of local economic performance to assess the significance of omitted variable bias.

variable and R-squares change when we directly control for these observable measures in regression analyses.

Figure 5 shows a scatter plot of changes in the LDP vote share from the 40th Lower House election to the 41th election (vertical axis) against the pre-reform level of representation (horizontal axis). It does not appear to exhibit any strong correlation, suggesting that the electoral support for the LDP did not necessarily increase (or decrease) in over-represented municipalities. We also calculate the correlation coefficient, which turns out to be statistically insignificant. Similarly, Figure 6 displays a scatter plot of changes in Bartik shift-share from the pre-reform period to the post-reform period (vertical axis) against the pre-reform level of representation (horizontal axis). These two variables do not appear to be correlated, either. Again, we find that the correlation between these variables is insignificant. Thus, economic conditions do not appear to have shifted in favor of over-represented municipalities in the post-reform period.

Tables 6-8 display the results of our basic regression model but control for the share of votes for the LDP and Bartik industry shift-share. For transfers and transfers plus borrowings (Columns 1-6, Table 6), even when we include these control variables, the coefficient on the interaction of reform with delegate size per one million district population remain virtually unchanged. Similarly, for local tax revenue (Columns 7-9, Table 6), the coefficient on the interaction term also remained similar to the basic results. The coefficient on Bartik industry shift-share is positive and significant for local tax revenue, suggesting that local tax revenue rises with positive economic shocks. For total expenditure and investment expenditure (Columns 1-6, Table 7), the inclusion of the LDP vote share and Bartik industry shift-share does not materially change the coefficient on the interaction term. For current expenditure (Columns 7-9, Table 7), the coefficient on the interaction term remained statistically insignificant.

Similarly, we do not detect any notable change in the coefficient on the interaction term for employment and establishment (Table 8). Both LDP vote share and Bartik industry shift-share have positive and significant coefficients for employment, indicating that the support for the LDP is associated with more employment and that positive economic shocks generate more jobs. Note that the inclusion of Bartik industry shift-share increases R-squares from 25% to 29%, a non-trivial improvement in goodness of fit, relative to the baseline model, although this is not a surprising result, considering that Bartik industry shift-share is documented to be a strong predictor of local economic performance in a variety of settings. In sum, to the extent that unobservable shocks are correlated with these observable and relevant political economic shocks, these results indicate that omitted variable bias might be fairly small, if any.

Thus far, we show that the geographical distribution of legislative representation matters to the geographical distribution of fiscal transfers and expenditure. Nevertheless, the effect of representation (and associated fiscal transfers and expenditure) on local economies is muted in the data, and it does not seem to be driven by counterbalancing, unobservable political and economic factors. Another possible explanation for the "missing" economic impact of legislative representation is that the data on employment and establishment are noisier than the data on fiscal variables. The data on employment and establishment are collected based on survey that are conducted every five years, whereas the data on fiscal variables are self-reported annually by each local fiscal authority (and the data on intergovernmental transfers are verified by the central government for consistency).

To probe into the severity of measurement error, we estimate local transfer job multipliers and construct a 95% confidence interval around our estimates. This econometric exercise helps us uncover a possible range of estimates. Armed with estimates of local fiscal job multipliers, we examine whether legislative representation possibly has a large positive economic effect and yet we are just unable to pin it down with precision, or alternatively, the effect is indeed small. In addition, we look at construction and public sector employment, separately, from employment in other sectors to explore the underlying mechanism. We expect fiscal spending to have mechanical, direct effects on construction and public sector jobs, to the extent that a portion of fiscal transfer is used to hire more workers to build public infrastructure and enhance public services.

Probing into how much employment changed in these sectors is also informative about measurement error. If we do not find any significant effects of fiscal transfers on employment in these sectors, it will be quite puzzling and might indicate that the general noisiness of the data is an overriding issue. If, however, we find positive local fiscal job multipliers in construction and public sectors and negative multipliers in other sectors, then the missing effect of representation on local economies can be explained, not by measurement error, but by crowding-out effects; i.e., fiscal transfers are used to hire construction and public sector workers who otherwise would have worked in other sectors.

For local fiscal job multipliers, we estimate the instrumental variable regression:

$$\Delta Transfer_{it} = \alpha_t + \alpha_i + \alpha Reform_t \times Delegate_k + \varepsilon_{it}$$

$$\Delta Employment_{it} = \rho_t + \rho_i + \rho \Delta \widehat{Tranfer}_{\iota t} + \upsilon_{it}$$

The dependent variable in the first stage regression is changes in transfers per capita. The instrumental variable is the interaction of reform dummy with the pre-reform delegate size per 1 million district population. The dependent variable in the second stage is changes in employment per capita. The coefficient on "instrumented" changes in transfers, ρ, is local fiscal job multiplier, measuring the number of jobs created by an additional transfer of 100,000 yen (approximately, $1,000). We estimate multipliers separately for employment in construction and public sectors and employment in other sectors. We also include demographic controls as well as the share of votes for the ruling party and Bartik industry-shift share for sensitivity checks.

Table 9 displays estimates of local fiscal job multipliers. Given that the first stage F-statistics is barely above 10, we also report Anderson-Rubin weak instrument robust test for the statistical significance of estimated local fiscal job multipliers as well as Anderson-Rubin weak instrument robust confidence set at 95% level. For total employment (Columns 1-2), point estimates of local fiscal job multipliers are all insignificant and range from -0.02 to -0.01 with confidence sets ranging from [-.07, .01] and [-.05, .02], respectively. Hence, in terms of the number of jobs created by an additional transfer of 10,000,000 yen (or 100,000 dollars), our confidence sets are [-7, 1] and [-5, 2]. These are indeed wide confidence sets, compared to those reported in the literature (Chodorow-Reich, 2019). However, the upper bound is 2. That is, even under highly optimistic scenarios, the cost per job is 5,000,000 yen (approximately, $50,000), which is 20% more than the median annual income in Japan.

Local fiscal job multipliers for construction and public sector jobs are positive (Columns 3-4). The reported Anderson-Rubin weak instrument robust confidence sets are also mostly positive, suggesting that fiscal transfers seem to have produce more jobs in these sectors, as expected. For employment in other sectors, coefficients on fiscal transfers are negative (Columns 5-6). These results suggest that, as fiscal transfers declined in over-represented municipalities after the reform, fewer jobs were made available in construction and public sectors, and yet more jobs were created in other sectors. Taken together, the missing effect of representation is driven not necessarily by general measurement error in the survey data, but most likely by crowding-out phenomena in local labor markets.

5. Conclusion

This paper uses a rare electoral reform episode in Japan where malapportionment in the Lower House was largely corrected in a short period of time for the 1996 election in order to better understand the local economic impact of unequal legislative representation. We find that representation has robust effects on local jurisdictions' fiscal space. Over-represented municipalities received less transfers after the reform, relative to under-represented municipalities. Moreover, the former also borrowed less and spent less after the reform, suggesting that the electoral reform shifted the relative stance of local fiscal policies in different municipalities. However, we detect no significant effect of representation on local economies. Over-represented municipalities which effectively endured fiscal retrenchment after the reform, did not experience declines in economic activities, relative to under-represented municipalities. The "missing" real economic effect of representation does not seem to be driven by offsetting shocks or noise in the data. Rather, the results show that over-represented municipalities lost construction and public sector jobs as a result of fiscal retrenchment, but they gained jobs in other sectors.

Based on these results, we conclude that the 1996 electoral reform transformed the geographical distribution of representation and central government transfers, and yet it did not have first-order impacts on the geographical distribution of productive activities. That is, economic activities vary geographically with other overriding economic and location-specific determinants, but those determinants cannot be easily altered by legislative representations. Are these results anomaly and specific to a particular setting in Japan whose economy endured a decade-long stagnation during the period that we study? Undoubtedly, more research on other countries' experiences are needed to better understand how legislative malapportionment affects fiscal and economic outcomes of local communities. A natural research avenue is to study local economic impacts of malapportionment in countries where urban, under-represented, communities struggle to fund poverty-reducing public infrastructure. Large scale reapportionment, like the one implemented in Japan, is more likely to revitalize local economies of those communities.

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Figure 1: District Size and Prefecture Population

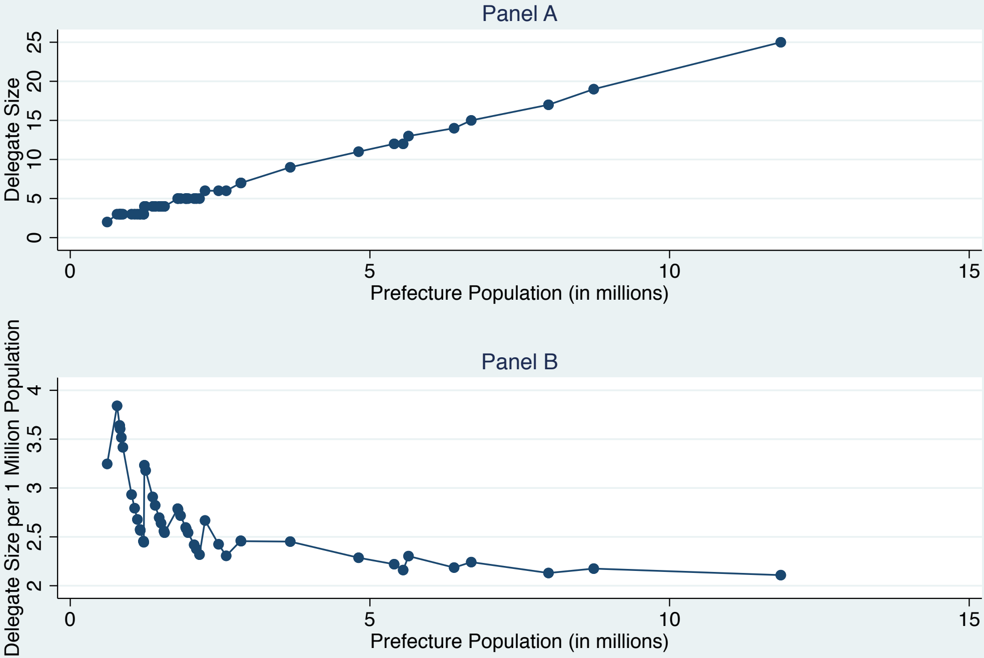

Figure 2: Distribution of Delegate Size per 1 Million District Population

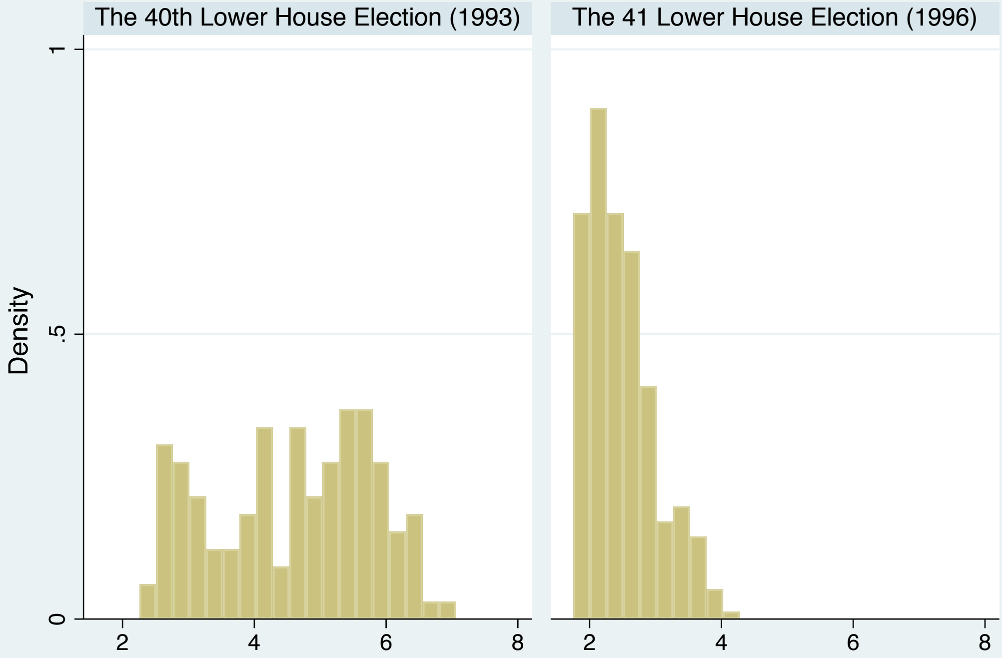

Figure 3: Geographical Distribution of Delegates (40th Election in 1993)

Figure 4: Geographical Distribution of Delegates (41st Election in 1996)

Figure 5: Changes in Vote Share for the LDP vs. Delegate Size

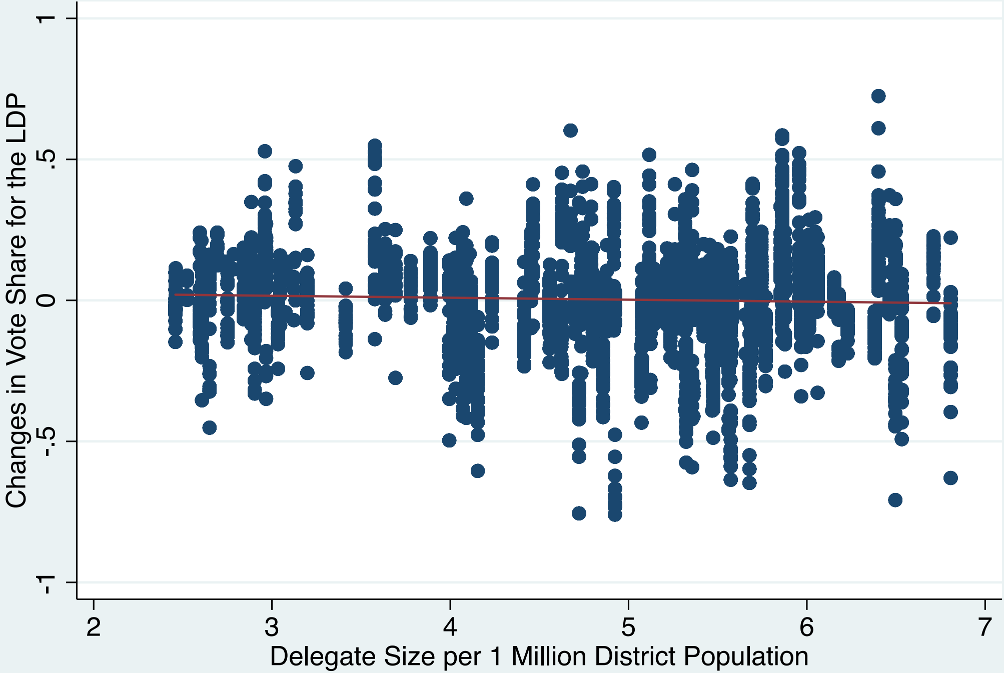

Figure 6: Changes in Bartik Industry Shift-Share vs. Delegate Size

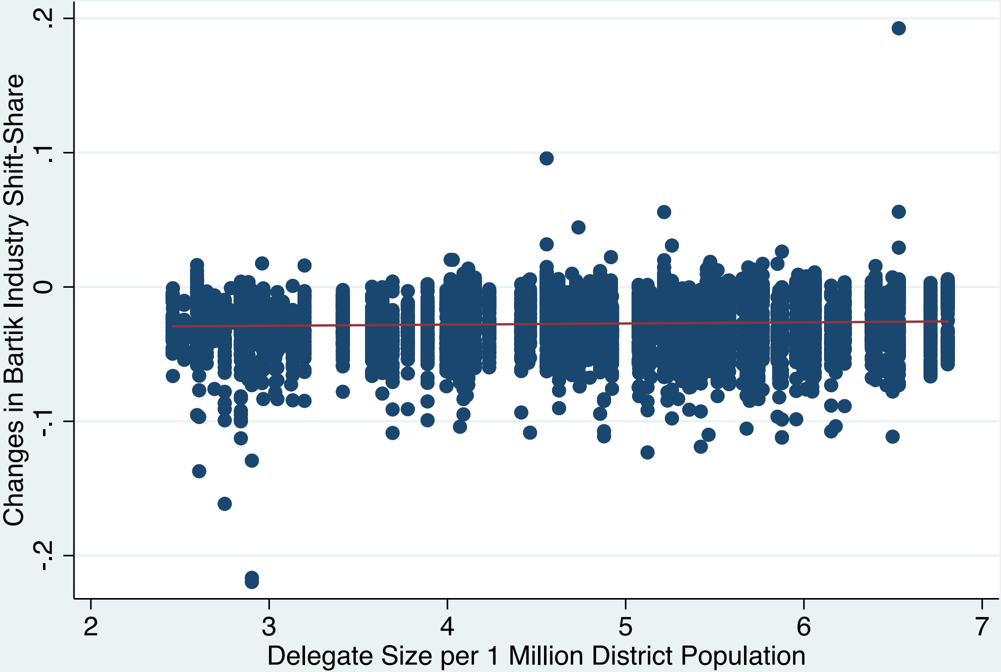

Table 1: Lower Chamber Malapportionment, Twenty Most-Malapportioned Cases (plus Japan Before 1996)

Ranking	Country	LH Index	Ranking	Country	LH Index
1	Tanzania	0.262	12	Argentina	0.141
2	Korea	0.208	13	Gambia	0.140
3	Ecuador	0.204	14	Columbia	0.132
4	Kenya	0.195	**15**	***Japan***	***0.131***
5	Ghana	0.178	16	Andorra	0.131
6	Zambia	0.173	17	Spain	0.096
7	Iceland	0.168	18	Brazil	0.091
8	Bolivia	0.168	19	Georgia	0.090
9	Malawi	0.166	20	Turkey	0.086
10	St Lucia	0.162	21	Seychelles	0.081
11	Chile	0.151			

This table re-produces the Loosemore-Hanby (LH) index of electoral disproportionality of the 20 most malapportioned lower chambers from Samuels and Snyder (2001) and adds the LH index of Japan's Lower House before the reform calculated by Ong, Kasuya, and Mori (2017).

Table 2: Summary Statistics

Variables	mean	sd
Changes in Transfers per Capita	0.183	0.715
Changes in Transfers plus Borrowings per Capita	0.265	1.346
Changes in Local Tax Revenue per Capita	0.0726	0.293
Changes in Total Expenditure per Capita	0.228	1.873
Changes in Investment Expenditure per Capita	-0.159	1.666
Changes in Current Expenditure per Capita	0.387	0.523
Changes in Employment per Capita	0.000678	0.0441
Changes in Establishment per Capita	-0.00166	0.00490
Changes in Employment per Capita (Construction & Public Sector)	0.000473	0.0140
Changes in Employment per Capita (Non-Construction & Non-Public Sector)	0.000205	0.0379
Bartik Industry Shift-Share	-0.00204	0.0250
Vote Share for the Ruling Party (the LDP)	0.492	0.196
Delegate Size per 1 Million District Population	4.905	1.123
Population Growth	-0.0127	0.0586
Changes in Elderly's (65 Years or Older) Share in Population	0.0300	0.00902

The data cover 3152 municipalities for two time periods, 1991-1996 and 1996-2001. All of fiscal variables are measured in 100,000 yen (approximately, 1,000 dollars).

Table 3: Timing of Fiscal Variables' Response to the 1996 Electoral Reform

	(1)	(2)	(3)	(4)	(5)	(6)
	Changes in Transfers per Capita	Changes in Transfers plus Borrowings per Capita	Changes in Local Tax Revenue per Capita	Changes in Total Expenditure per Capita	Changes in Investment Expenditure per Capita	Changes in Current Expenditure per Capita
(Placebo Reform, 1995-1996)x(Delegate Size per 1 Million District Population)	-0.00651	-0.00890	0.00382	0.00232	0.0104	-0.00807
	(0.0104)	(0.0218)	(0.00519)	(0.0298)	(0.0311)	(0.00951)
(Reform, 1996-1997)x(Delegate Size per 1 Million District Population)	-0.0280*	-0.0517**	-0.00139	-0.0619**	-0.0545*	-0.00747
	(0.0150)	(0.0228)	(0.00568)	(0.0292)	(0.0299)	(0.00773)
Observations	9,456	9,456	9,456	9,456	9,456	9,456
R-squared	0.002	0.005	0.010	0.005	0.004	0.006
Number of municipalities	3,152	3,152	3,152	3,152	3,152	3,152

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

The data cover 3152 municipalities for three time periods, 1994-1995, 1995-1996, and 1996-1997. All of fiscal variables are measured in 100,000 yen (approximately, 1,000 dollars). (Placebo Reform, 1995-1996) is a dummy variable that equals 1 for 1995-1996 and zero otherwise. (Reform, 1996-1997) is a dummy variable that equals 1 for 1996-1997 and zero otherwise. Regressions include year fixed effects and municipality fixed effects. Standard errors are clustered by electoral districts.

Table 4: Differential Effects of the 1996 Electoral Reform on Fiscal and Economic Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Changes in Transfers per Capita	Changes in Transfers plus Borrowings per Capita	Changes in Local Tax Revenue per Capita	Changes in Total Expenditure per Capita	Changes in Investment Expenditure per Capita	Changes in Current Expenditure per Capita	Changes in Employment per Capita	Changes in Establishment per Capita
Reformx(Delegate Size per 1 Million District Population)	-0.0839***	-0.154***	0.00916	-0.197**	-0.157**	-0.0405	0.000780	5.15e-05
	(0.0235)	(0.0541)	(0.00925)	(0.0783)	(0.0647)	(0.0258)	(0.00115)	(0.000116)
Population Growth	0.679	-0.816	0.256	-2.891	-1.817	-1.073***	0.125***	0.00941**
	(1.439)	(3.057)	(0.310)	(3.421)	(3.517)	(0.404)	(0.0306)	(0.00408)
Changes in Elderly's (65 Years or Older) Share in Population	11.13***	16.74***	0.125	26.73***	17.70**	9.034***	-0.0219	-0.000678
	(3.156)	(6.009)	(1.008)	(8.423)	(7.961)	(2.410)	(0.114)	(0.0119)
Observations	6,303	6,303	6,303	6,303	6,303	6,303	6,304	6,304
R-squared	0.135	0.197	0.082	0.234	0.110	0.518	0.250	0.101
Number of municipalities	3,152	3,152	3,152	3,152	3,152	3,152	3,152	3,152

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

The data cover 3152 municipalities for two time periods, 1991-1996 and 1996-2001. All of fiscal variables are measured in 100,000 yen (approximately, 1,000 dollars). Reform is a dummy variable that equals 1 for 1996-2001 and zero otherwise. Regressions include year fixed effects and municipality fixed effects. Standard errors are clustered by electoral districts.

Table 5: Correlation between Fiscal and Economic Outcomes and Delegate Size per 1 Million District Population Before 1996 (Pre-Tretnd Test)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Changes in Transfers per Capita	Changes in Transfers plus Borrowings per Capita	Changes in Local Tax Revenue per Capita	Changes in Total Expenditure per Capita	Changes in Investment Expenditure per Capita	Changes in Current Expenditure per Capita	Changes in Employment per Capita	Changes in Establishment per Capita
Delegate Size per 1 Million District Population	0.0294	0.0548	-0.00249	0.0492	0.0429	0.00623	-0.000776	0.000131
	(0.0194)	(0.0357)	(0.00767)	(0.0464)	(0.0366)	(0.0176)	(0.000870)	(0.000158)
Population Growth	-1.023**	-2.237***	0.677***	-3.090***	-1.489*	-1.601***	0.301***	0.0312***
	(0.409)	(0.693)	(0.173)	(0.892)	(0.796)	(0.237)	(0.0149)	(0.00218)
Changes in Elderly's (65 Years or Older) Share in Population	8.156**	9.813	0.195	10.86	1.009	9.850***	-0.173*	-0.0121
	(3.558)	(6.222)	(0.533)	(7.951)	(7.174)	(2.353)	(0.0921)	(0.0117)
Observations	3,152	3,152	3,152	3,152	3,152	3,152	3,152	3,152
R-squared	0.029	0.026	0.017	0.022	0.007	0.080	0.196	0.143

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
The data cover 3152 municipalities from 1991-1996. All of fiscal variables are measured in 100,000 yen (approximately, 1,000 dollars). Standard errors are clustered by electoral districts.

Table 6: Sensitivity Checks with Vote Share for the Ruling Party (the LDP) and Bartik Shfit-Share

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Changes in Transfers per Capita			Changes in Transfers plus Borrowings per Capita			Changes in Local Tax Revenue per Capita		
Reformx(Delegate Size per 1 Million	-0.0839***	-0.0836***	-0.0817***	-0.154***	-0.153***	-0.154***	0.00916	0.00929	0.0126
District Population)	(0.0235)	(0.0238)	(0.0237)	(0.0541)	(0.0550)	(0.0547)	(0.00925)	(0.00916)	(0.00928)
Population Growth	0.679	0.687	0.670	-0.816	-0.805	-0.800	0.256	0.260	0.231
	(1.439)	(1.429)	(1.404)	(3.057)	(3.047)	(3.017)	(0.310)	(0.311)	(0.311)
Changes in Elderly's (65 Years or Older)	11.13***	11.14***	11.13***	16.74***	16.76***	16.76***	0.125	0.132	0.116
Share in Population	(3.156)	(3.171)	(3.162)	(6.009)	(6.043)	(6.040)	(1.008)	(1.003)	(1.000)
Bartik Industry Shift-Share			1.687			-0.493			2.834***
			(3.285)			(5.728)			(0.610)
Vote Share for the Ruling Party (the LDP)		0.0455	0.0504		0.0584	0.0570		0.0191	0.0273
		(0.184)	(0.183)		(0.316)	(0.314)		(0.0452)	(0.0458)
Observations	6,303	6,303	6,303	6,303	6,303	6,303	6,303	6,303	6,303
R-squared	0.135	0.135	0.135	0.197	0.197	0.197	0.082	0.082	0.092
Number of municipalities	3,152	3,152	3,152	3,152	3,152	3,152	3,152	3,152	3,152

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The data cover 3152 municipalities for two time periods, 1991-1996 and 1996-2001. All of fiscal variables are measured in 100,000 yen (approximately, 1,000 dollars). Reform is a dummy variable that equals 1 for 1996-2001 and zero otherwise. Regressions include year fixed effects and municipality fixed effects. Standard errors are clustered by electoral districts.

Table 7: Sensitivity Checks with Vote Share for the Ruling Party (the LDP) and Bartik Shfit-Share

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Changes in Total Expenditure per Capita			Changes in Investment Expenditure per Capita			Changes in Current Expenditure per Capita		
Reformx(Delegate Size per 1 Million District Population)	-0.197**	-0.196**	-0.193**	-0.157**	-0.157**	-0.152**	-0.0405	-0.0393	-0.0410
	(0.0783)	(0.0794)	(0.0796)	(0.0647)	(0.0655)	(0.0657)	(0.0258)	(0.0255)	(0.0257)
Population Growth	-2.891	-2.855	-2.882	-1.817	-1.814	-1.856	-1.073***	-1.041**	-1.026**
	(3.421)	(3.411)	(3.375)	(3.517)	(3.506)	(3.476)	(0.404)	(0.406)	(0.406)
Changes in Elderly's (65 Years or Older) Share in Population	26.73***	26.81***	26.80***	17.70**	17.71**	17.68**	9.034***	9.106***	9.115***
	(8.423)	(8.464)	(8.461)	(7.961)	(8.005)	(8.008)	(2.410)	(2.401)	(2.409)
Bartik Industry Shift-Share			2.612			4.064			-1.452
			(7.143)			(6.282)			(1.314)
Vote Share for the Ruling Party (the LDP)		0.196	0.203		0.0175	0.0292		0.178	0.174
		(0.427)	(0.428)		(0.331)	(0.331)		(0.157)	(0.156)
Observations	6,303	6,303	6,303	6,303	6,303	6,303	6,303	6,303	6,303
R-squared	0.234	0.234	0.234	0.110	0.110	0.110	0.518	0.519	0.520
Number of municipalities	3,152	3,152	3,152	3,152	3,152	3,152	3,152	3,152	3,152

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The data cover 3152 municipalities for two time periods, 1991-1996 and 1996-2001. All of fiscal variables are measured in 100,000 yen (approximately, 1,000 dollars). Reform is a dummy variable that equals 1 for 1996-2001 and zero otherwise. Regressions include year fixed effects and municipality fixed effects. Standard errors are clustered by electoral districts.

Table 8: Sensitivity Checks with Vote Share for the Ruling Party (the LDP) and Bartik Shfit-Share

	(1)	(2)	(3)	(4)	(5)	(6)
	Changes in Employment per Capita			Changes in Establishment per Capita		
Reformx(Delegate Size per 1 Million District Population)	0.000780	0.000851	0.00173	5.15e-05	4.49e-05	8.52e-05
	(0.00115)	(0.00115)	(0.00116)	(0.000116)	(0.000115)	(0.000111)
Population Growth	0.125***	0.127***	0.119***	0.00941**	0.00923**	0.00888**
	(0.0306)	(0.0305)	(0.0299)	(0.00408)	(0.00408)	(0.00403)
Changes in Elderly's (65 Years or Older) Share in Population	-0.0219	-0.0176	-0.0221	-0.000678	-0.00107	-0.00128
	(0.114)	(0.114)	(0.111)	(0.0119)	(0.0119)	(0.0117)
Bartik Industry Shift-Share			0.761***			0.0349***
			(0.117)			(0.0102)
Vote Share for the Ruling Party (the LDP)		0.0105*	0.0127**		-0.000976*	-0.000875
		(0.00600)	(0.00638)		(0.000584)	(0.000590)
Observations	6,304	6,304	6,304	6,304	6,304	6,304
R-squared	0.250	0.251	0.294	0.101	0.102	0.111
Number of municipalities	3,152	3,152	3,152	3,152	3,152	3,152

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The data cover 3152 municipalities for two time periods, 1991-1996 and 1996-2001. Reform is a dummy variable that equals 1 for 1996-2001 and zero otherwise. Regressions include year fixed effects and municipality fixed effects. Standard errors are clustered by electoral districts.

Table 9: Local Fiscal Job Multipliers (Instumental Variable Estimation)

	(1)	(2)	(3)	(4)	(5)	(6)
	Changes in Employment per Capita		Changes in Employment per Capita (Construction & Public Sector)		Changes in Employment per Capita (Non-Construction & Non-Public Sector)	
Changes in Transfers per Capita	-0.00924	-0.0211	0.00999*	0.00809	-0.0192	-0.0292**
	(0.0138)	(0.0153)	(0.00564)	(0.00582)	(0.0121)	(0.0142)
Population Growth	0.131***	0.133***	0.00933	0.0106	0.121***	0.122***
	(0.0333)	(0.0432)	(0.00937)	(0.00841)	(0.0341)	(0.0434)
Changes in Elderly's (65 Years or Older) Share in Population	0.0810	0.213	-0.0830	-0.0603	0.164	0.273
	(0.216)	(0.253)	(0.0922)	(0.0938)	(0.199)	(0.240)
Bartik Industry Shift-Share		0.797***		0.105**		0.692***
		(0.141)		(0.0506)		(0.149)
Vote Share for the Ruling Party (the LDP)		0.0137		0.00592***		0.00782
		(0.00846)		(0.00216)		(0.00870)
Observations	6,302	6,302	6,302	6,302	6,302	6,302
R-squared	0.232	0.166	0.014	0.112	-0.011	-0.196
Number of municipalities	3,151	3,151	3,151	3,151	3,151	3,151
First Stage F Statistic	12.77	11.90	12.77	11.90	12.77	11.90
P-Value of Anderson-Rubin Weak IV Robust Test	0.501	0.140	0.0674	0.158	0.0996	0.0172
Anderson-Rubin Weak IV Robust Confidence Set	[-.045748, .020736]	[-.071534, .007431]	[-.000514, .028093]	[-.003203, .026305]	[-.05613, .004258]	[-.081339,-.005108]

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

The data cover 3152 municipalities for two time periods, 1991-1996 and 1996-2001. Changes in transfer capita are measured in 100,000 yen (approximately, 1,000 dollars). The instrumental variable is Reformx(Delegate Size per 1 Million District Population) where Reform is a dummy variable that equals 1 for 1996-2001 and zero otherwise. Regressions include year fixed effects and municipality fixed effects. Standard errors are clustered by electoral districts.