The Productivity of Public Capital: Evidence from Japan's 1994 Electoral Reform[1]

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Abstract

This paper estimates the causal effect of public capital stock on Production, using Japanese prefectural data. We first articulate the difficulty of consistently estimating the regional-level production function with public capital that results from the endogeneity of the public capital stock amount. The public capital amount could be endogenous because of the central government's political decision-making process of public capital allocation or the local government's budgetary constraints.

Japan's electoral reform in 1994 offers an exogenous variation in the public capital investment across regions, and we exploit this event to estimate the causal effect of public capital on production. The reform drastically changed the distribution of political representation in the Lower House across regions, and it accordingly changed the allocation of public capital across regions as well. We cannot reject the null hypothesis that public capital is not productive based on the estimates from this natural experimental identification strategy.

JEL Classification: C25

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1 Introduction

Japan's public investments, such as investments in highways, ports, and water and sewer systems, has drastically decreased in the last decade under strong pressure to cut the public deficit. In the fiscal year of 1994, 13.2 trillion yen had been spent as public project- related expenditures, but the figure declined to 7.8 trillion in 2004 (Cabinet Office (2004)). This extreme budget cut had been based on the presumption that public investment was inefficient and wasting resources. It is often claimed that public capital does not improve a regionfs productivity. However, this presumption is often grounded on anecdotal evidence, widely broadcasted by the media, indicating that specific public infrastructures are not well utilized. For public policy making, a careful statistical examination of the productivity of public capital is important.

The estimation of the marginal productivity of public capital in the US was spurred by Aschauer (1989)'s work, which pointed out that the lower productivity growth in the US during the 1970s is mostly explained by reduced investment in public capital during that period. Munnell (1990) also reported similar findings. Their studies both were based on macro time-series data, and some economists criticized their findings, pointing to the possible endogeneity of public capital because the public capital investment could have been hampered by low tax revenue resulting from the 1970s' stagnated economy.

Based on state-level panel data, Garcia-Mila and McGuire (1992) also found a significant effect of public capital on states' output. However, Holz-Eakin (1994), Evans and Karras (1994) and Garcia-Mila, McGuire, and Porter (1996) cast doubt on the results because such data do not allow for state fixed effects. After allowing for state fixed effects, they did not find any significant effect of states' public capital on states' output. Studies have indicated that allowing for state fixed effects is important because those states with persistently high levels of production tend to hold more public capital. This is a natural consequence of the fact that state-level public capital is likely to be financed through states' tax revenue in the US. Due to this local government budget constraint, the OLS estimates are likely to suffer from an upward bias. After a decade-long dispute, US economists seemed to reach an agreement that state public capital does not positively affect state production.[1]

The estimation of the production function with public capital has attracted economists' interest for a longer time in Japan than in the US. Mera (1973) estimated the regional production function that includes labor, capital, and public capital as inputs. Since his classic work, numerous studies have attempted to estimate the causal effect of public capital stock on production using prefectural data (Asako, Tsuneki, Fukuda, Teruyama, Tsukamoto,

[1]Some studies have paid attention to the fact that public capital could affect different sectors in different ways. See Holtz-Eakin and Lovely (1996) for theory and evidence and Chandra and Thompson (2000) for the interstate highway system's effect on the different sectors of a regional economy.

and Sugiura (1994), Mitsui and Ohta (1995), The Economic Planning Agency (1997), Yamano and Ohkawara (2000) and Miyara and Fukushige (2008)).[2] For example, Yamano and Ohkawara (2000) estimated the production function with prefecture fixed effects without year fixed effects, and the elasticity of public capital on production was estimated to be 0.15, with statistical significance. However, in our calculation with prefecture and year fixed effects based on their data, the significantly positive effect disappears.[3] The regression of public capital on time and prefectural dummies renders $R^2 = 0.994$. This fact by no means implies that Yamano and Ohkawara (2000)'s conclusion was wrong, but it is worth noting that once both year and prefecture fixed effects are taken into account, empirical studies using Japanese data also suffer from multicolinearity between public capital and state and year dummies, as articulated by Ai and Cassou (1997).[4]

The discussion above shows the typical robustness-efficiency trade off that empirical economists face. Researchers can deal with the correlation between

[2]It is worth mentioning that Shioji (2001) incorporated public capital in the neoclassical growth model and estimated the effect of public capital on economic growth using Japanese and US regional data. He emphasized the importance of considering the role of public capital in a dynamic setting because public capital increases the regional output not only through a direct productivity contribution, but also through attracting more private capital to the region. He found that public capital, infrastructure in particular, is growth-enhancing in both Japan and the US. Although we fully appreciate his point, our short-period panel does not allow us to adopt his framework, and thus we stick with the static framework. Technically speaking, we can extend the sample period, but we do not have an exogenous source of variation of public capital except for during the mid-1990s.

[3]We thank Norihiko Yamano and Toru Ohkawara for providing us with their data.

[4]Ai and Cassou (1997) regressed public capital stock on state and year dummies and obtained $R^2 = 0.996$ using Holz-Eakin (1994)'s data. They found similar results for the data of Evans and Karras (1994).

state (prefecture) unobserved heterogeneity and the stock level of public capital by using a fixed-effects estimation; however, the variation of public capital within a state over time is small. Accordingly, the fixed-effects estimation tends to render imprecise estimates. Thus, to execute a precise estimation, it is necessary to exploit the significant variation of public capital within a region that is caused by an exogenous shock.

The purpose of this study is to identify the causal effect of public capital stock on production, using a recent electoral reform in Japan as a natural experiment that created an exogenous variation of public capital across regions. Japan's electoral reform in 1994 dramatically changed the regional allocation of both political influence and public capital. We exploit this exogenous change in the public capital allocation to estimate the productivity of public capital. The estimation results indicate that we cannot reject the null hypothesis that public capital is not productive and suggest that the OLS estimates suffer from an upward bias.

The rest of this paper is organized as follows. Section 2 reviews the existing literature and introduces the Japanese political economy of public capital allocation and the 1994 electoral reform, and its effect on public capital allocation. Section 3 discusses the empirical methodology, and section 4 describes the data. Section 5 reports the estimation results, and section 6 discusses the results' robustness. The last section concludes.

2 The Political Economy of Public Capital Allocation in Japan and the 1994 Electoral Reform

Japan's electoral reform in 1994 was motivated mainly by an intent to alleviate the inequalities in the legislative representation caused by the rapid urbanization that had been occurring since the 1950s. Consequently, the reform was aimed at equalizing the seats per population across electoral districts. This electoral reform offers an ideal ground to obtain within-prefecture exogenous variations of public capital. The electoral reform caused a drastic change in the distribution of each prefecture's political representation in the House of Representatives (Lower House), and, accordingly, it changed the allocation of public capital investment across prefectures because of pork-barrel politics, as already pointed out by Horiuchi and Saito (2003), in the form of the allocation of subsidies from the central government to local governments. Because subsidies often are tied to public capital investments, we can expect that the electoral reform created an exogenous variation of public capital amounts within prefectures. We use this exogenous variation as an instrumental variable to identify the causal effect of public capital on production across prefectures.

In Japan, the central government provides a large proportion of the funds for public capital and allocates them across regions. Although government bureaucrats draft the fiscal budget plan, politics are involved in the public

capital allocation decision-making process.[5] Okuno (1988), Okuno, Yakita, and Yagi (1994) and Yoshino and Yoshida (1988) have shown that the central government mostly allocated the public capital to rural, less-developed areas to attain the goal of "balanced growth" across regions after the mid 1960s. One major reason why rural areas have attracted more public capital investment per capita than urban areas is the malapportionment of electoral districts in Japan. Under the electoral system that was used before the 1994 reform, the number of seats in the House of Representatives based on the population had been higher in rural areas because the seat allocation had been relatively fixed while Japan experienced urbanization after World War II.[6] Yoshino and Yoshida (1988) showed that the number of seats per capita positively affected the amount of public capital intended for industrial purposes that each region received. Meyer and Naka (1999) showed that the per capita representation of each prefecture determines the amount of subsidy transfer from the central government to the local government. Horiuchi and Saito (2003) used the more detailed municipal data to show the positive and significant relation between the seats per capita and the amount of subsidy transfer.

We used Japan's 1994 electoral reform that included drastic reapportionment as a natural experiment.[7] The reform applied to the election system

[5]For a description of the process by which the Japanese government drafts the fiscal budget plan, see Ishi (1996). For evidence of political intervention in the budget's allocation, see Meyer and Naka (1998).

[6]See Horiuchi and Saito (2003) for evidence.

[7]For details of the electoral reform, see Christensen (1994), Christensen (1996), Chris-

of the House of Representatives (Lower House), while the electoral system for the House of Councilors (Upper House) stayed the same. Before the reform, all 500 members of the House of Representatives were elected by a single, non-transferable vote system with multi-member districts. After the reform, 300 members were elected by the single-member district (SMD) plurality rule, and another 200 members were elected by the proportional representation (PR) system. For the SMD part, a single seat is allocated to each of the 47 prefectures and the rest are allocated according to the size of the prefecture population. For the PR part, seats are allocated to 11 blocs proportional to the population of each bloc. The first Lower House election after the reapportionment occurred in October 1996.

The change of seat allocations across prefectures is tabulated in Table 1. The first column tabulates the number of seats assigned for each prefecture before the electoral reform, and the second column tabulates the number of seats assigned for the SMDs after the reform. The third column tabulates the estimated number of seats elected by the PR system after the reform. Seats for the PR system are allocated for 11 blocs, so we allocated the seats assigned to each bloc using the weight that is proportional to the number of voters for each prefecture. The fourth column tabulates the total number of seats allocated to each prefecture, which is the sum of the third and fourth columns. A striking finding is that the total number of seats allocated to each prefecture did not change much as a result of the electoral reform, as

tensen (1998) and Horiuchi and Saito (2003)

indicated by the first and fourth columns. However, the number of seats that are directly elected by voters changed drastically, as evidenced by comparing the first and the second columns.

In our analysis, we focus on the change in the number of seats that are directly elected by politicians' names. This is because those politicians who are elected by their names in their respective electoral districts presumably have a stronger incentive to engage in pork-barrel politics than politicians who are elected by their party names. In addition, it is widely believed that those Lower House members elected from the SMD have more political influence than those who are elected by the PR system. Reflecting this widely shared view, those Lower House members who are elected from the SMD are called Gold members, while those who are elected from the PR system are called Silver members (Asahi Shinbun (2000) and Yomiuri Shinbun (2000)).

Considering the difference in incentives for pork-barreling and political authority between SMD-elected and PR-elected politicians, the electoral reform resulted in the reduction of the political power of urban areas, which is an unintended consequence of the electoral reform. The large change in the number of Lower House members who are directly elected by their names by prefectures presumably created a large variation in the public capital allocation.

3 Empirical Model

We assume that the prefecture-level production function is a standard Cobb-Douglas production function that exhibits a constant return to scale with respect to private inputs: labor and private capital.[8] The production function expressed in terms of output per labor hour is:

$$\ln(y_{it}/l_{it}) = \beta_0 + \beta_1 \ln(k_{it-1}/l_{it}) + \beta_2 \ln g_{it-1} + year\beta_3 + c_i + u_{it}, \quad (1)$$

where y_{it} is the gross prefecture domestic product, l_{it} is the total hours worked defined by person-hour, k_{it-1} is the service flow of private capital, g_{it-1} is the service flow from government capital stock, *year* is the set of year dummy variables, i is the subscript for prefecture, and t is the subscript for year. The error term consists of time-invariant prefecture effects c_i and the idiosyncratic shock to production u_{it}. If the prefecture effects are not correlated with the explanatory variables, (i.e. $E(c_i|(k_{-1}/l)_i, g_i, year) = 0$, where $x_i \equiv [x_{i1}, x_{i2}, ..., x_{iT}]$), and the idiosyncratic error term is strictly exogenous (i.e. $E(u_{it}|(k_{-1}/l)_i, g_i, year, c_i) = 0$), then the OLS estimation renders a consistent estimator.

The first assumption, $E(c_i|(k_{-1}/l)_i, g_i, year) = 0$, often is refuted in empirical studies because those regions with high output levels tend to have high levels of public capital stock because of local government budgetary constraints in the US. This assumption is also likely to be violated in Japan

[8]The constant return to scale assumption is tested with current data and the assumption is not rejected.

because the central government tends to allocate public capital to the permanently stagnant regions to attain "balanced growth." Reflecting the difference in the source of the endogeneity of public capital, the OLS estimates are larger than the fixed effects estimates in the US (Holz-Eakin (1994) and Evans and Karras (1994)), but the OLS estimates are smaller than the fixed effects estimates in Japan (Asako, Tsuneki, Fukuda, Teruyama, Tsukamoto, and Sugiura (1994) and Yamano and Ohkawara (2000)).

The second assumption, $E(u_{it}|(k_{-1}/l)_i, g_i, year, c_i) = 0$, also may be violated, although this rarely has been pointed out in previous studies. To articulate this point, we assume that the public capital of prefecture i in year $t-1$ is determined as:

$$\ln g_{it-1} = \delta_0 + \delta_1 \ln(k_{it-1}/l_{it}) + \delta_2 \ln y_{it} + z_{it-2}\gamma + year\delta_4 + \alpha c_i + v_{it}, \quad (2)$$

where z is the vector of instrumental variables that determine the public capital stock, but does not directly determine the output level after conditioning on c_i (i.e. $E(u_{it}|(k_{-1}/l)_i, z_i, c_i, year) = 0$). If $\alpha \neq 0$, then $E(c_i|(k_{-1}/l)_i, g_i, year) = 0$ is violated because the unobserved heterogeneity that determines prefectures' output also determines the level of public capital. If $\delta_2 \neq 0$, then $E(u_{it}|(k_{-1}/l)_i, g_i, c_i, year) = 0$ is violated because the current shock to production affects the current level of public capital. The coefficient δ_2 is expected to be positive if the public capital investment in region i at year t is limited by the tax revenue in the same region at the same time. However, if the central government heavily invests in the stagnating regions,

δ_2 is expected to be negative. Under the condition, $\delta_2 \neq 0$, the productivity of public capital, which is β_2 in (1), is identified only if the instrumental variables exist for public capital; i.e., $\gamma \neq 0$ in (2). The fixed-effects instrumental variable estimator is consistent if $E(u_{it}|(k_{-1}/l)_i, z_i, c_i, year) = 0$. This implies that we can obtain a consistent estimator even when u_{it} and g_{it} are correlated, as far as u_{it} and z_{it-2} are not correlated conditional on $(k_{-1}/l)_i$ and c_i.

The number of Lower House members who are elected by their names who represent prefecture i in year $t-2$ is used as the instrumental variable for public capital stock in the corresponding prefecture and year. Because we do not have theoretical guidance of functional form to relate the number of directly elected members and the log of public capital, we tried several specifications and decided to include the number of members who are elected by their names as z_{it-2} in (2).[9] The reduction of the total number of directly elected Lower House members in 1996 might have changed the total amount of public capital allocation, but this effect is captured by the year dummies.

A drawback of the fixed effects estimation is that the estimator is susceptible to the measurement error in the independent variable when most of the within-variation is a product of time-varying measurement error (Wooldridge (2001), pp. 311-344). To overcome this limitation, we implement the first-

[9]Note that this functional form issue is not very important because this is an auxiliary regression function to attain an instrumental variable estimation of the equation (1). The functional form only affects the efficiency of the instrumental variable estimator, and not the consistency.

difference estimation applied to the time-series average of the periods before and after the electoral reform. This first-difference estimator applied to the averaged data is less susceptible to the measurement error when the measurement error does not have a heavy serial correlation because the measurement error cancels itself out by taking the time-series average. As a result, the degree of asymptotic bias reduces compared with the straightforward first-difference estimator.

4 Data

We used the aggregate data of 47 prefectures between 1994 and 1999. These years were chosen in order to include data before and after the electoral reform. The first Lower House election after the electoral reform took place in October 1996, and the last election before the reform took place in June 1993. The effect of reapportionment on public capital allocation presumably started appearing in fiscal year 1997. Because our production specification allows a one-year time lag before public capital contributes to the production, the effect of electoral reform on production presumably starts appearing from 1998. Thus, we have four years of observations before the reform (1994-1997) and two years of observations after the reform (1998-1999).

We used gross prefecture product (y) as the measure of prefectural value-added product. These data were taken from Cabinet Office (Each Year). As for person-hour labor input (l), we multiplied the average number of work hours and the number of regular workers taken from Ministry of Labor (Each

Year).[10] The private and public capital stock amounts were taken from Doi (2002). These stock data include the amounts at the end of the fiscal year, and the effects of the privatization of the telephone, tobacco, and railway public companies (*Dendenkosya, Senbaikosya*, and *Nihon Kokuyu Tetsudo*) and the 1995 Hanshin-Awaji earthquake were adjusted. Because the private and public capital stock amount was measured at the end of the fiscal year, we used the stock amount in year $t-1$ to explain the output flow in year t. The number of seats in the Lower House was taken from Ministry of Home Affairs (Each Year), which reported the number of seats for each election year. This is the number of seats elected from multiple-member districts before the reform and from single-member districts after the reform. Because Lower House members who occupied seats in year $t-1$ exercised their political influence on the budget plan for the fiscal year of t, the number of seats in year $t-2$ is used to explain the public capital amount in year $t-1$. As we discuss later, controlling for the capital utilization rate is important, and we used the annual electric power bought by large-scale buyers reported in Federation of Electric Power Companies of Japan (Each Year). Table 2 reports the descriptive statistics of the data.

Figures 1 and 2 implement a "visual" IV estimation. Figure 1 plots the relation between the change of the log of the stock amount of public capital and the change in the lagged number of directly elected members in the

[10]Regular workers (*Jōyō Rōdōsha*) include all those who work without fixed-term contracts. This definition includes both part- and full-time workers.

House of Representatives, after adjusting for the log of (private capital / total hours worked) and year effects. The difference is taken for the average of the 1994-1997 period (before the electoral reform) and the average of the 1998-1999 period (after the electoral reform). This figure shows that the prefectures that experienced a relatively large reduction in the number of seats also experienced a relatively large reduction in public capital allocation. In particular, it is notable that urban prefectures such as Tokyo, Osaka, Aichi, and Fukuoka experienced large reductions in seat allocation and, accordingly, large reductions in public capital allocation. Overall, we can roughly see that the number of seats in the House of Representatives affects the stock amount of public capital.

Figure 2 plots the relation between the regression-adjusted change in the log of (output/total hours worked) and the regression-adjusted change in the lagged number of seats. This figure indicates that the change in the number of seats negatively affects the change in prefectural labor productivity. If the number of seats allocated to each prefecture affects production only through public capital provision, then the productivity of public capital is the ratio of the slope coefficient of Figure 2 and the slope coefficient of Figure 1. These two figures imply that public capital does not contribute to production in a causal sense.

5 Results

Table 3 tabulates the estimation results of the prefectural labor productivity function that include public capital and the private capital-labor ratio as explanatory variables. Column (1) reports the OLS regression result. This result indicates that a 10-percent rise in public capital increases output per labor hour by 0.4 percentage points. This coefficient is statistically significant at the 5 percent level.

However, the coefficient for public capital becomes negative after the possible correlation between public capital and prefecture unobserved heterogeneity is taken care of by the first-difference estimation applied to period-averaged data, as reported in Column (2). The coefficient is negative, but we cannot reject the null hypothesis that public capital does not affect output per labor hour because of the large standard error. The fact that the coefficient differs between the OLS and the first difference implies that the unobserved prefecture productivity heterogeneity and the stock amount of public capital are positively correlated. Previous studies have pointed out the negative correlation between public capital and prefecture productivity heterogeneity, but the difference in the sample period may explain the discrepancy. The central government policy may have changed during the period from the targeting toward low productivity areas to the targeting toward high productivity areas. Also, the local government budgetary constraint on public capital investment may have become more binding during

the period.

In addition, we should note that the coefficient for log private capital per capita virtually does not change from the OLS regression. This suggests that the measurement error in capital stock does not spoil our first-difference estimation.

The first-difference estimator discussed above could be biased when the idiosyncratic part of the error term is correlated with public capital. For example, if the government allocates more public capital to a region that temporarily stagnates, then the idiosyncratic part of the error term and public capital are negatively correlated, and the first-difference estimator suffers from downward bias. To avoid this bias, the exogenous variation of the allocation of public capital caused by the electoral reform is exploited. More specifically, the change in the number of seats directly elected across prefectures is used as an instrumental variable for the potentially endogenous variable, which is the change of public capital stock.

Table 4 Column (1) reports the results of the first-stage regression for the first-difference IV estimation, which are the first- difference regression results on the determination of the public capital allocation across prefectures. In addition to the number of directly elected seats in the Lower House, the specification includes the log of the private capital-labor ratio, which is included in the second-stage regression. Table 4 Column (1) reports the result of the OLS regression applied to the first-differenced data, and it indicates that a 10-seat increase in the number of directly elected seats in the Lower House

results in about a 4 percent increase in public capital allocation. This coefficient is statistically significant, and the F-statistic for the excluded variable is above 10. The number of seats explains the allocation of public capital across prefectures, and this evidence is consistent with the previous finding by Horiuchi and Saito (2003). This finding assures that having directly elected seats in the Lower House works as a good instrument for public capital allocation, in that it affects prefectural production only through this allocation.

Given a strong instrument for the change in public capital stock, the second-stage regression is implemented. The result of the first-difference instrumental variable estimation applied to the averaged data is reported in Table 3 Column (3). According to the estimation result, public capital negatively affects labor productivity, but the estimate is very imprecise, and we cannot reject the null hypothesis that public capital does not affect labor productivity. Hausman-Wu statistics suggest that we cannot reject the null hypothesis that the change in public capital allocation is exogenous. Thus, the first-difference estimation applied to the averaged data is the most-preferred estimation method. To summarize the results, we cannot reject the null hypothesis that public capital does not improve labor productivity.

6 Discussion

The discussion so far has assumed that public capital contributes to prefectural production as an input; however, the traditional Keynesian argument claims that public capital investment stimulates effective demand and

increases production through mobilizing unemployed resources. The pure productivity effect of public capital on production can be estimated by accurately capturing the level of private inputs because the Keynesian effect operates through a change in the utilization of private inputs. Thus, by controlling for the level of private inputs, we can rule out the Keynesian effect.

However, as is commonly discussed in macroeconomics, measuring the service flow from capital is very difficult. If the utilization of capital is not appropriately measured, a high capital utilization induced by public capital investment results in an upward bias for the coefficient for private capital. This bias could be transmitted to the coefficient for public capital through the correlation between private and public capitals. To reduce this potential bias, we use the variation in electricity usage to capture the varying capital utilization rate, as in Burnside, Eichenbaum, and Rebelo (1996). The regression model that includes electricity usage as an additional explanatory variable is estimated by OLS, first-difference, and first-difference IV, as in the previous section. The results of the first-stage estimation appear in Column (2) in Table 4. The results indicate that the change in the number of seats in the Lower House is a valid instrument, as it enters the regression significantly, even after controlling for electricity usage. The results of the second-stage regression appear in Columns (4) through (6) of Table 3.

The results of the second-stage estimation do not change significantly. The coefficients for private capital decline, and this change implies that the

coefficient for private capital input was upward-biased because of the omission of the capital utilization rate. The coefficients for public capital did not change much, however. Overall, the analysis that attempts to control for the capital utilization rate by using electricity usage confirms the robustness of the previous results that public capital does not improve labor productivity.

7 Conclusion

This paper has estimated the productivity of public capital using Japan's 1994 electoral reform as a source of the exogenous variation of public capital allocation across regions. The estimation of public capital productivity is very difficult because the public capital amount is endogenous.

The 1994 electoral reform drastically increased the political representation of rural areas and increased the relative allocation of public capital to rural areas compared with urban areas because of pork-barrel politics. We exploited this exogenous variation of public capital to estimate its productivity. The OLS estimates that neglect the endogeneity of public capital indicate the positive effect of public capital on labor productivity, but the first-difference estimation results suggest that the OLS estimator is upward-biased. The first-difference IV estimation that exploits the change in seats allocation as IV confirms the robustness of the results for the first-difference estimation. Overall, the estimation results allowing for correlated prefecture heterogeneity with public capital fail to reject the null hypothesis that public capital does not increase labor productivity.

Our estimates statistically confirm a belief widely held among the general public and the media that public capital in Japan is not productive. Our first-difference instrumental variable estimates that exploit the exogenous change of public capital allocation because of electoral reform warrant the validity of the first-difference estimates. Future policy discussions on public capital provisions should continue to be based on estimation results that pay extra attention to the endogeneity of public capital allocation.

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Table 1: Number of Seats in the House of Representative before and after the 1994 Electoral Reform
The first election after the reform took place in October 1996.

Period	Before the Reform 1995	After the Reform 1996		
Electoral system	Single non-transferable vote system with multi-member district. (*Chusenkyo-ku*)	Single-member district (SMD) plurality rule (*Shosenkyo-ku*)	Proportionally representative (PR) system (estimates) (*Hirei Daihyo*)	Total
Prefecture				
Hokkaido	23	13	8	21
Aomori	7	4	2	6
Iwate	7	4	2	6
Miyagi	8	6	3	9
Akita	7	3	2	5
Yamagata	7	4	2	6
Fukushima	12	5	3	8
Ibaragi	12	7	3	10
Tochigi	10	5	6	11
Gunma	10	5	6	11
Saitama	20	14	6	20
Chiba	19	12	5	17
Tokyo	43	25	17	42
Kanagawa	22	17	7	24
Niigata	13	6	5	11
Toyama	6	3	2	5
Ishikawa	5	3	2	5
Fukui	4	3	2	5
Yamanashi	5	3	2	5
Nagano	12	5	6	11
Gifu	9	5	3	8
Shizuoka	14	9	5	14
Aichi	22	15	10	25
Mie	8	5	3	8
Shiga	5	3	2	5
Kyoto	10	6	4	10
Osaka	28	19	13	32
Hyogo	19	12	8	20
Nara	5	4	2	6
Wakayama	5	3	2	5
Tottori	4	2	1	3
Shimane	5	3	1	4
Okayama	10	5	3	8
Hiroshima	13	7	4	11
Yamaguchi	9	4	2	6
Tokushima	5	3	1	4
Kagawa	6	3	2	5
Ehime	9	4	2	6
Kochi	5	3	1	4

Fukuoka	20	11	7	18
Saga	5	3	1	4
Nagasaki	9	4	2	6
Kumamoto	9	5	3	8
Oita	6	4	2	6
Miyazaki	5	3	2	5
Kagoshima	9	5	2	7
Okinawa	5	3	2	5

Note: After the electoral reform, 200 members of the Lower House were elected by the proportionally representative system from 11 blocks. We allocated these seats to prefectures proportionally, to the number of voters in each prefecture that consists of a block. Because of rounding, the total number of PR seats does not add up to 200.

Table 2: Descriptive Statistics
Sample: All 47 prefectures between 1994 and 1999.

	Mean	Standard Deviation	Minimum	Maximum
Output (Billion Yen)	10.66	13.57	2.11	84.62
Employment (Thousands Persons)	828.91	1054.34	172.86	6557.15
Annual Person Hours (Million Hours)	1571.52	1962.32	327.74	12219.9
Private Capital Stock (Billion Yen)	19.47	24.24	3.16	159.20
Public Capital Stock (Billion Yen)	14.35	12.30	4.22	68.1
Electric Power (Million KWh)	5498.95	5287.92	603	25876
Number of Seats in Lower House Directly Elected	8.18	6.50	2	43

Note: N = 235. Output, public capital stock, and private capital stock are denominated in the 1990 price. Employment indicates the number of workers without fixed-term contracts (*Joyo-Rodosha*), including both full- and part-time workers.

Table 3: The Prefecture-level Production Function
Sample: All 47 prefectures between 1994 and 1999
Dependent Variable: log (Output/Labor)

	(1)	(2)	(3)	(4)	(5)	(6)
Model	OLS	First difference applied to averaged data	First difference instrumental variable applied to averaged data	OLS	First difference applied to averaged data	First-difference instrumental variable applied to averaged data
Instrument	-	-	Change in Seat Allocation	-	-	Change in Seat Allocation
Log (Public Capital) t-1	0.04	-0.16	-0.38	0.05	-0.22	-0.40
	(0.02)	(0.27)	(0.55)	(0.02)	(0.24)	(0.50)
Log (Capital / Labor) t-1	0.41	0.39	0.40	0.31	0.34	0.35
	(0.09)	(0.20)	(0.21)	(0.10)	(0.19)	(0.19)
Log (Electric Power / Labor)	-	-	-	0.05	0.13	0.13
				(0.02)	(0.04)	(0.04)
N	235	47	47	235	47	47
R2	0.48	0.08	0.07	0.51	0.25	0.24
Hausman-Wu Test (t-statisitcs)	-	-	0.47	-	-	0.41

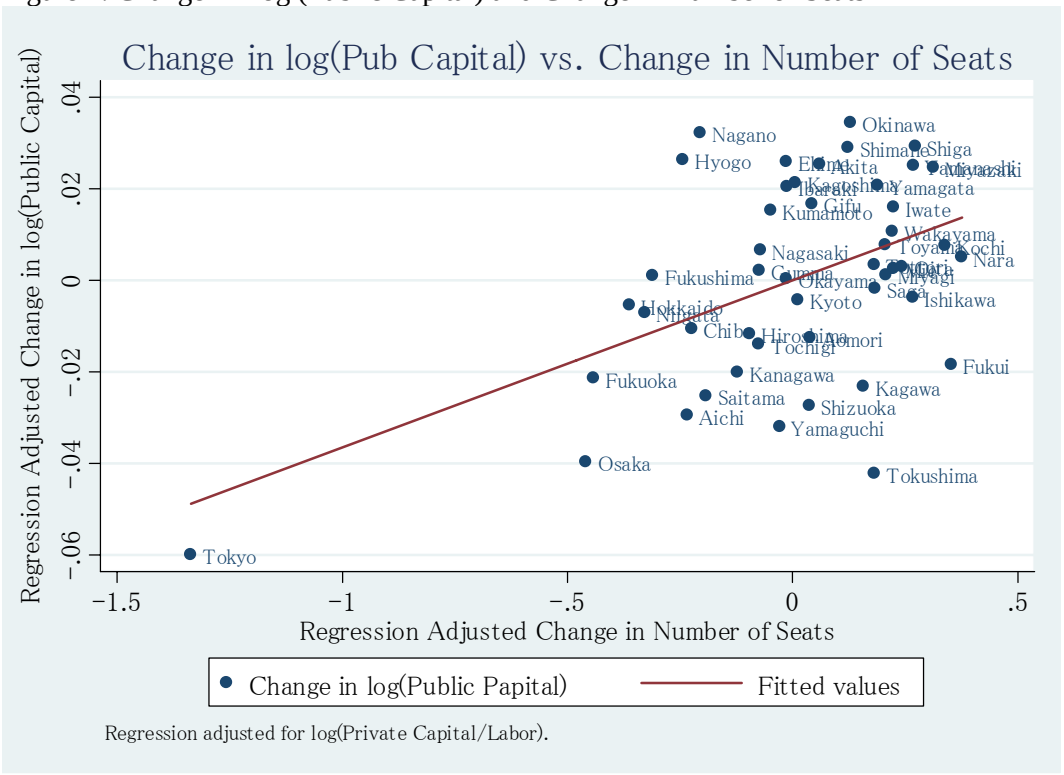
Note: The first-difference estimation is applied to the averaged data. The years between 1994 and 1997 are treated as “before” the electoral reform and the years 1998 and 1999 are treated as “after” the electoral reform. The first difference is obtained by subtracting the average of the “before” period from the average of the “after” period.

Table 4: The Determination of Public Capital Assignment across Prefectures (First Stage)
Sample: All 47 prefectures between 1994 and 1998.
Dependent Variable: Change in log (Public Capital)

	(1)	(2)
Change in (Number of Seats/10)	0.04	0.04
	(0.01)	(0.01)
Change in Log (Capital / Labor)	-0.03	-0.03
	(0.10)	(0.10)
Change in Log (Electric Power / Labor)	-	0.01
		(0.02)
F-statistics for instrument	13.97	13.70
R^2	0.24	0.25
N	47	47

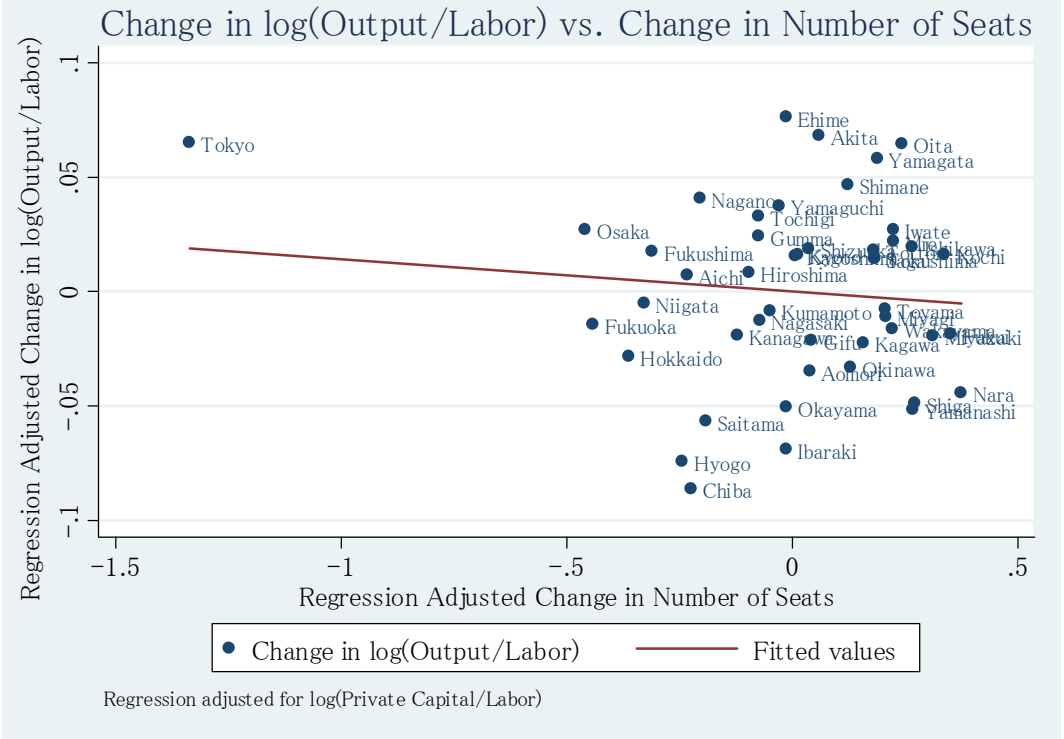
Note: Standard errors are in parentheses. Standard errors for the OLS and IV estimation are robust against panel clustering.

Figure 1: Change in Log (Public Capital) and Change in Number of Seats



Regression adjusted for log(Private Capital/Labor).

Figure 2: Change in Log (Output/Labor) and Change in Number of Seats



Regression adjusted for log(Private Capital/Labor)