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Hirota, Haruaki and Yunoue, Hideo

Musashi University, Nagoya City University

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Haruaki Hirota † Hideo Yunoue ‡

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Abstract

The purpose of this paper is to reveal which kinds of public investment cause municipal mergers to create a fiscal common pool problem in Japan. In particular, we focus on whether municipal mergers increase road and public park construction just before the mergers or not because previous papers reveal only that municipal mergers increase local bonds. The empirical results reveal that the subordinate merger partners rapidly increase their road and public park expenses just before mergers.

JEL Classification: H72, H73, H74, H77

Keywords: fiscal common pool problem, municipal mergers, public investment, subordinate merger partner

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†Haruaki Hirota, Professor, Faculty of Economics, Musashi University ; 1-26-1, Toyotama-kami, Nerima-ku, Tokyo, 176-8534, Japan. E-mail: hirota@cc.musashi.ac.jp

‡Hideo Yunoue, Associate Professor, Faculty of Economics, Nagoya City University; Yamanobata, Mizuhocho, Mizuho-ku, Nagoya, Aichi, 467-0802 Japan. E-mail: yunoue@econ.nagoya-cu.ac.jp

1 Introduction

Since the pioneering study of Hinnerich (2009) was published, which reveals a fiscal common pool problem through municipal mergers in the literature on the law of 1/n, some papers have attempted to estimate the relationship between local bonds or accumulated debts and municipal mergers[1]. In the literature, the size of the fiscal common pool increases in municipal mergers, which explains why some partners of merged municipalities, which increase public projects, are able to receive benefits from their projects even though the costs are shared among their merger partners. Several papers revealed that municipalities increase their local bonds through municipal mergers, which creates the fiscal common pool problem (Hinnerich (2009), Jordahl and Liang (2010), Hansen (2014), Saarimaa and Tukiainen (2015), Hirota and Yunoue (2017))[2] According to Hirota and Yunoue (2017), subordinate merger partners in Japan, which suffer from adverse fiscal conditions and depopulation, create the fiscal common pool problem just before mergers.

The purpose of this paper is to reveal which kinds of public investments municipal mergers engage in to create a fiscal common pool problem in Japan. In particular, we focus on public investment expenses for both road and public park construction just before the mergers. To our knowledge, no empirical study has yet revealed which public investments are increased through the fiscal common pool problem created by municipal mergers. Previous papers revealed only whether the merged municipalities increased their total expenditures, total investment expenses or local bonds just before the mergers. Consequently, in this paper, we identify the causal effects of the municipal mergers and their road and public park construction just before the mergers.

2 Empirical framework

In this study, we reveal which kinds of public investments municipal mergers use to create a fiscal common pool problem in Japan. In particular, we focus on the expenditures for both road and public park construction for both the merged and nonmerged municipalities by using Japan's municipal data just before the mergers. Previous studies did not study which public investments increased through the fiscal common pool problem for municipal mergers, which explains why they reveal only whether the merged municipalities in-

[1] The literature on "the law of $1/n$" was formalized by Weingast et al. (1981)

[2] Bless and Baskaran (2016) did not observe any fiscal common pool problem for voluntary municipal mergers in Germany. Nakazawa (2016) show that merged municipalities have fewer incentives to engage in free-riding. However, Hirota and Yunoue (2017) revealed that the subordinate merger partners positively increase the local bonds just before mergers, addressing sample selection bias by using propensity score matching with the difference in differences method.

crease their total expenditures and local bonds just before the municipal mergers or not.

We use the empirical framework established by Hirota and Yunoue (2017), who applied propensity score matching with the difference in differences method (PSM-DID) to address sample selection bias and time-consistent unobserved effects. Because Japan's central government encouraged municipalities to merge through the Special Municipal Mergers Law (called the carrot and stick policy), between FY2004 and FY2005, municipal mergers in Japan were not random and voluntary. The special law was enforced from FY1999 to FY2005. As a result, the number of municipalities rapidly decreased from 3232 to 1820 during the period, especially in FY2004 and FY2005. For example, the central government, through the special law, stated that the unspecified grants received by municipalities would decrease, if the municipalities did not choose to merge during the enforcement period of the special law. On the other hand, the municipalities would be allowed to receive the same amount in unspecific grants for 10 years as well as before mergers if they chose to merge. Almost all the municipalities that chose to merge were dependent on unspecific grants and faced adverse fiscal conditions. In particular, the central government required the municipalities to identify their potential merger partners at least one or two years ahead if the municipalities wanted to receive special support such as special grants under the special law. Therefore, we focus on the municipalities that mergerd in FY2004 and their behavior in FY2002 and FY2003 as a treatment period. In other words, we consider the period from FY1998 to FY2001 as the pretreatment period and the period from FY2002 to FY2003 as the treatment period. Thus, to address the sample selection problem, we use data from the same periods and use the same propensity scores calculated by Hirota and Yunoue (2017).

In addition, to identify the fiscal common pool problem for municipal mergers, we use the population as a treatment group, which lead to the decision to be subordinate merger partners. We confirm that the subordinate merger partners remarkably increase their local bonds just before mergers. Therefore, we use the treatment group and consider two patterns exhibited by the subordinate merger partners, which are the same as the previous study. The basic model determines whether the population size of the merger partner is smaller than half of the population size of the new municipality; in this case, the treatment group equals 1 and zero otherwise[3]. Moreover, we use the subordinate merger partner model because the basic model includes some bias involving the dominant merger partners. The dominant merger partners do not like to issue additional bonds before mergers because they

[3]This model is very similar to Hinnerich's (2009) free-rider model. According to Hinnerich (2009), merger partners have an incentive to be a free rider depending on their population size divided by that of the new municipality.

must repay such bonds by themselves after the mergers. Therefore, we use the following subordinate merger partner model if the population size of the subordinate merger partner is smaller than half of the population size of the dominant merger partner; in this case, the treatment group equals 1 and zero otherwise.

To identify which kinds of public investment the merged municipalities increase just before the mergers, we use the same data and empirical framework as the previous study, except for the data on road and public park expenses. Consequently, we estimate a consistent estimator of the average treatment effect on the treated (ATT) of the PSM-DID method[4]. The following equation describes the ATT, which can eliminate sample selection bias by matching the merged municipalities with very similar nonmerged municipalities, which have almost the same propensity score.

$$
\begin{aligned}
A\hat{T}T = & \frac{1}{N_{1t}} \sum_{i=1}^{N_{1t}} [Y_{1ti} - \sum_{j=1}^{N_{0t}} W(i,j) Y_{0tj}] \\
& - \frac{1}{N_{1s}} \sum_{i=1}^{N_{1s}} [Y_{1si} - \sum_{j=1}^{N_{0s}} W(i,j) Y_{0sj}]
\end{aligned}
$$

Y_i is per capita road expenses, and per capita public park construction expenses[5] Y_1 denotes the merged municipality, and Y_0 denotes the nonmerged municipality. N_1 indicates the size of the merged municipalities sample, and N_0 indicates the size of the nonmerged municipalities sample. $W(i, j)$ is a weight assigned to a nonmerged municipality based on the propensity score, and $\sum_j W(i, j) = 1$. In addition, t and s index the pretreatment period and the end of the treatment period. N_{1t} and N_{1s} indicate the number of municipalities at each point in time.

[4]We actually succeed in estimating the probit model for calculating as the same propensity scores that are used in the previous study. To estimate the propensity score using the probit model, we use the following covariates: population, area, share of population over 65, share of population under 15, share of industry, share of grants, and share of debt stock. In addition, we completely satisfy the balancing property of the covariates between the treatment and control groups after propensity score matching. We apply three different matching methods; nearest neighbor, radius, and kernel matching under the common support assumption. The detailed results for the matching methods are available upon request. In addition, see Hirota and Yunoue (2017).

[5]The amount of the road and public park expenses are given in units of one thousand Japanese yen. One thousand Japanese yen is approximately 10 dollars at an exchange rate of 100 Japanese yen to one U.S. dollar. The amount of the public investment expenses for both road and public park construction are items of *Tandoku Jigyo Hi - Doboku Hi from Chihou Zaisei Jyokyo Chosa Hyo (in Japanese).*

3 Empirical results

Figure 1 indicates that there is a substantial difference between the treatment and control group. In the basic model, on average, the per capita road expenses of the treatment group are between 35 and 40, while those of the control group are between 20 and 30. In addition, the per capita road expenses of the treatment group start increasing in FY2000. Nevertheless, the per capita road expenses of the control group exhibit a decreasing trend. The per capita public park expenses of the treatment group repeatedly move up and down during the period, while those of the control group exhibit a decreasing trend. In particular, the per capita public park expenses of the treatment group remarkably increase from FY2002 to FY2003. Likewise, in the subordinate merger partner model, we can confirm a similar movement of public investment with the basic model. We are able to observe a clearly different trend between both groups in the pretreatment period. Therefore, we should suspect that the parallel trend assumption of the DID estimation breaks down due to the special law, which explicitly targets certain types of municipalities such as those suffering from adverse fiscal conditions. That is, we are concerned that the municipal mergers in Japan were not random and voluntary. Thus, we attempt to estimate the PSM-DID method to address sample selection bias.

Table 1 reports the results of the PSM-DID method[6]. In the basic model, per capita road expenses increase between 6.5 and 8.3 in FY2003, which are between approximately 16% and 20% of the average level of the per capita road expenses in FY1998. The figures for FY2002 slightly increase to between approximately 3.2 and 3.7. These results are positive and statistically significant at the 1% or 5% level, except for the result of the most closely matched neighbor in FY2002. However, we cannot observe that the ATTs of the per capita public park expenses are not statistically significant, except for the result of kernel matching for FY2003.

In the subordinate merger partner model, we are able to reveal the positive and statistically significant results for both road and public park expenses in FY2003. Per capita road expenses increase between 4.8 and 6 in FY2003, which is between approximately 10.9% and 13.3% of the average level of the per capita road expenses in FY1998. In addition, per capita public park expenses increase between 1.08 and 1.5 in FY2003, which is between approximately 76% and 107% of the average level of the per capita public park expenses in FY1998.

[6]We estimate many public investment expenses as outcome variables; river development, harbor improvement, sewage facilities, residential construction, airport construction, etc. However, we cannot confirm the statistical differences between the treatment and control groups. In addition, Table A.1 reports the results of the placebo treatment periods. There are no statistically differences in the averages of the treatment and control groups in the pretreatment period.

Notably, relatively small merged municipalities create the fiscal common pool problem by increasing both road and public park construction just before the mergers. These results are obviously consistent with the results of previous papers because they revealed that subordinate merger partners rapidly increase their local bonds just before the mergers. In addition, although most municipalities have engaged in little public park construction before enforcing the special law, they clearly increase their construction just before the mergers and rely on the dominant merger partner. For example, we confirm that the per capita public park expenses of some subordinate merger partners are almost zero in FY1998. The special law is very helpful for the subordinate merger partners. In addition, we believe that constructing new public parks is relatively easier than constructing new roads for the subordinate merger partners.

4 Concluding remarks

The purpose of this paper is to reveal which kinds of public investments cause municipal mergers to create a fiscal common pool problem in Japan. We focus on public investment expenses for both road and public park construction incurred just before mergers by both the treatment and control groups.

The results indicate that relatively small merged municipalities create the fiscal common pool problem by increasing both road and public park construction just before the mergers. These results are obviously consistent with the results of previous papers. The subordinate merger partners clearly increase their construction just before the mergers and rely on the dominant merger partner. The special law is very helpful for the subordinate merger partners. In addition, we consider that constructing new public parks is relatively easier than constructing new roads for the subordinate merger partners because they hope to succeed in merging by the specific deadline of the special law to receive special support from the central government.

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Figure 1: Mean public investment expense between FY1998 and FY2003

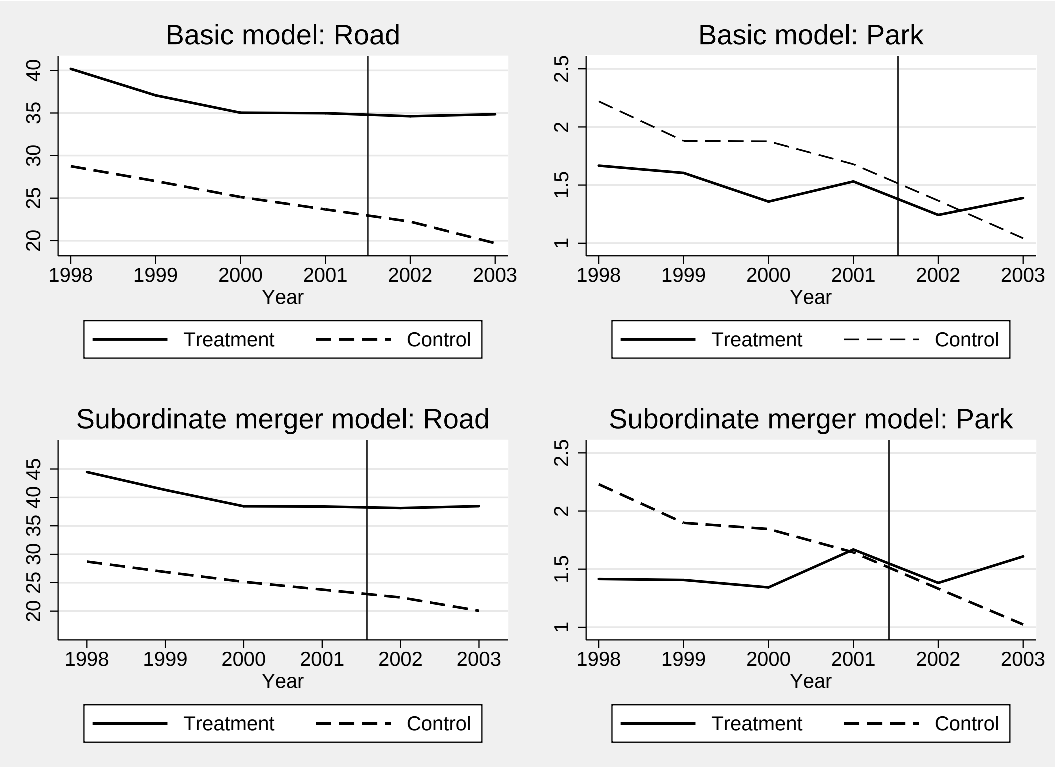

Table 1: Estimation results

	Basic model				Subordinate merger model			
	Road		Park		Road		Park	
	2003	2002	2003	2002	2003	2002	2003	2002
Nearest Neighbor	8.308***	3.212	0.441	-0.408	4.837*	1.212	1.078*	0.137
	(2.033)	(1.996)	(0.563)	(0.687)	(2.772)	(2.674)	(0.586)	(0.807)
On support: Treatment	674	674	674	674	497	497	497	497
On support: Control	2,015	2,118	2,015	2,118	2,190	2,293	2,190	2,293
Radius	7.516***	3.693**	0.815	0.229	5.624***	3.024	1.488***	0.828*
	(1.608)	(1.599)	(0.513)	(0.505)	(1.965)	(1.969)	(0.502)	(.0477)
On support: Treatment	673	673	673	673	493	493	493	493
On support: Control	2,010	2,113	2,010	2,113	2,189	2,291	2,189	2,291
Kernel	6.401***	3.177**	0.859*	0.258	5.929***	3.155	1.521***	0.836*
	(1.590)	(1.584)	(0.509)	(0.500)	(1.946)	(1.950)	(0.494)	(0.467)
On support: Treatment	674	674	674	674	495	496	495	496
On support: Control	2,015	2,118	2,015	2,118	2,190	2,293	2,190	2,293

Notes: ***, **, and * indicate statistical significance at the 1, 5, and 10 percent level, respectively. Standard errors are reported in brackets. Standard errors of the radius and kernel matching are calculated using the bootstrap method. We used 100 bootstrap iterations. The r for radius matching is 0.01, and the bandwidth for kernel matching is 0.06.

A Appendix

Table A.1: Estimation results of the placebo treatment periods

	Basic model				Subordinate merger model			
	Road		Park		Road		Park	
	2001	2000	2001	2000	2001	2000	2001	2000
Nearest Neighbor	0.866	-1.561	-0.347	-0.652	-2.876	-1.603	0.195	0.533
	(1.953)	(1.864)	(0.769)	(0.598)	(2.713)	(2.356)	(0.936)	(0.623)
On support: Treatment	674	674	674	674	497	497	497	497
On support: Control	2,132	2,136	2,132	2,136	2,307	2,311	2,307	2,311
Radius	2.016	0.260	0.381	0.147	1.379	0.083	0.982	0 .666
	(1.514)	(1.397)	(0.631)	(0.532)	(1.853)	(1.708)	(0.704)	(0.537)
On support: Treatment	673	673	673	673	493	493	493	493
On support: Control	2,127	2,131	2,127	2,131	2,305	2,309	2,305	2,309
Kernel	1.480	-0.068	0.375	0.176	0.206	-1.472	1.002	0.681
	(1.494)	(1.378)	(0.627)	(0.528)	(1.834)	(1.690)	(0.695)	(0.529)
On support: Treatment	674	674	674	674	496	496	496	496
On support: Control	2,132	2,136	2,132	2,136	2,307	2,311	2,307	2,311

Notes: ***, **, and * indicate statistical significance at the 1, 5, and 10 percent level, respectively. Standard errors are reported in brackets. Standard errors of the radius and kernel matching calculated using the bootstrap method. We used 100 bootstrap iterations. The r for radius matching is 0.01, and the bandwidth for kernel matching is 0.06.