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Public investment and the fiscal common pool problem on municipal mergers in Japan*

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Abstract

The purpose of this paper is to reveal the kinds of public investment that create a fiscal common pool problem through municipal mergers in Japan. In particular, we focus on whether municipal mergers increase road and public park construction just before the mergers because previous papers reveal only that municipal mergers increase local bonds. The empirical results show that 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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1 Introduction

Since the pioneering study of Hinnerich (2009), which reveals a fiscal common pool problem through municipal mergers in the literature on the law of 1/n, scholars have attempted to estimate the relationship between local bonds or accumulated debt and municipal mergers[1]. The literature shows that 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 the benefits from their projects even though the costs are shared among their merger partners.

Several papers have shown 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)). Bless and Baskaran (2016) did not observe any fiscal common pool problem for municipal mergers in Germany. However, Hirota and Yunoue (2017) revealed that subordinate merger partners positively increase local bonds just before mergers in Japan, addressing sample selection bias by using propensity score matching with the difference-in-differences method. According to Hirota and Yunoue (2017), subordinate merger partners that suffer from adverse fiscal conditions and depopulation create the fiscal common pool problem just before mergers by increasing local bonds.

Our main contribution in this paper is to reveal the kinds of public investments that 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 mergers. The reason for this focus is that, in Japan, municipalities are allowed to issue local bonds for public investments while being prohibited from issuing them to make up for fiscal deficits. In addition, it is relatively easier for municipalities to increase investment expenses for both roads and public parks than for other public works, such as river developments, harbor improvements, airport construction, residential construction, and sewage facilities. Municipalities play a role in not only constructing but also repairing roads and public parks. However, under the usual style of fiscal management, many municipalities increase road construction or repair projects at the end of each fiscal year. Since they adopt a single annual budget principle for fiscal management, they hope to use up their annual budgets within each year, and it is difficult to rapidly increase other types of public investment for a short period. In other words, in municipal merger cases, we assume that the merged municipalities rapidly increase their road and public park expenses just before mergers because it is easier to increase those forms of public investment, and they will be able to share the repayment costs among merger partners

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

after 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 municipal mergers on road and public park construction just before the mergers.

The remainder of the paper is organized as follows. Section 2 describes the settings and empirical framework. Section 3 presents the estimation results. Section 4 concludes the paper.

2 Empirical framework

2.1 Settings

In this study, we reveal the kinds of public investments used by municipal mergers to create a fiscal common pool problem in Japan. In particular, we focus on expenditures for both road and public park construction for the merged and nonmerged municipalities by using Japan's municipal data from 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 increase their total expenditures and local bonds just before a municipal merger.

We apply the extended 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 or voluntary. Thus, to address the sample selection problem, we use the same propensity scores calculated by Hirota and Yunoue (2017).

Figure 1 shows the timeline for municipal mergers in Japan. The Special Municipal Mergers 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. Thus, we focus on the consultation periods for at least one or two years just before mergers. For example, the central government, through a 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, municipalities would be allowed to receive the same amount in unspecified 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 unspecified grants and faced adverse fiscal conditions. In particular, the central government required the municipalities to identify their potential merger partners and to legally consult with them at least one or two years in advance if the municipalities wanted to receive support such as special grants under the special law. That is, the central government set a requirement for the merged municipalities that they establish a municipal mergers' committee if they hoped to receive special support by the special law. As a result, all the merged municipalities during this period implemented municipal mergers one or two years after the consultation was completed.

We consider two municipal merger cases, FY2004 and FY2005, in this paper. First, we focus on the municipalities that merged in FY2004 and their behavior in FY2002 and FY2003 as the 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. Second, we also focus on "the rush merged municipalities" that merged in FY2005 and their behavior in FY2003 and FY2004 as the treatment period. Those municipalities' behavior is likely to be different from the previous case because most municipalities that merged in FY2005, according to previous papers, decided to merge just one year before the deadline of the special law. Therefore, we refer to them as "rush merged municipalities". In this case, we consider the period from FY1998 to FY2002 as the pretreatment period and the period from FY2003 to FY2004 as the treatment period.

2.2 Subordinate merger partner as the treatment group

To identify the fiscal common pool problem for municipal mergers, we use the population size as the treatment group, as it leads to the decision to be the subordinate merger partner. We confirm that 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 those found in 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. 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. In addition, Hinnerich (2009) adopts a range of free-rider behavior from weak to strong incentives by the population size divided by that of the new municipalities. Thus, we define the basic model in a similar manner to Hinnerich (2009).

Moreover, we define 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 must repay such bonds themselves after the merger. 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.

2.3 PSM-DID method

To identify the kinds of public investment that the merged municipalities increase just before the mergers, we use the same municipal data between FY1998 and FY2005 and extended empirical framework as in the previous study, except for the data on road and public park expenses. Consequently, we calculate a consistent estimator of the average treatment effect on the treated (ATT) of the PSM-DID method. 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.

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

Y_i is per capita road expenses and per capita public park construction expenses. The amount of road and public park expenses are given in units of one thousand Japanese yen, which is approximately 10 dollars at an exchange rate of 100 Japanese yen to one U.S. dollar. The public investment expenses for both road and public park construction are items of construction expenses without specific grants from upper governments [*Tandoku Jigyo Hi - Doboku Hi from Chihou Zaisei Jyokyo Chosa Hyo* (in Japanese)]. Y_1 denotes the merged municipality, and Y_0 denotes the nonmerged municipality. N_1 indicates the size of the merged municipality's sample, and N_0 indicates the size of the nonmerged municipality's 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.

Recent studies in the applied econometrics field adopted various matching methods: nearest neighbor matching, radius matching, and kernel matching.

Nearest neighbor matching is described by equation (2).

$$min_j ||P_i - P_j|| \tag{2}$$

For each treated observation i, select a control observation j that has the closest X. We select merged municipalities and find the nonmerged municipalities with the closest propensity score.

Radius matching is described by equation (3).

$$(P_j| \; ||P_i - P_j|| < r) \tag{3}$$

Each treated observation i is matched with a control observation j that falls within r.

Kernel matching is described by equation (4). In kernel matching, each treated observation i is matched with several control observations, with weights being inversely proportional to the distance between the treatment and control observations. The weights are defined as follows:

$$W(i,j) = \frac{K(\frac{P_j - P_i}{h})}{\sum_{j=1}^{N_0} K(\frac{P_j - Pi}{h})} \tag{4}$$

h is the bandwidth parameter. In this paper, we use the Epanechnikov kernel function.

3 Empirical results

3.1 Graphical evidence

Figure 2 shows the trends for the treatment of merged years in FY2004 and for the control groups. Figure 2 indicates that there is a substantial difference between the treatment and control groups. 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 FY200, while the per capita road expenses of the control group exhibit a decreasing trend. The per capita public park expenses of the treatment group repeatedly fluctuate 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.

Figure 3 shows the trends for the treatment of merged years in FY2005 and the control groups. Figure 3 indicates that there are similar trends to those in Figure 2. In the basic model, the per capita road expenses of the treatment group are between 25 and 35 in the pretreatment period, while those of the control group are between 25 and 30. However, the per capita road expenses of the control group rapidly decreased in the treatment period, especially in FY2004. The per capita public park expenses of the treatment group repeatedly increased and decreased in the pretreatment period, as shown in Figure 2. Likewise, in the subordinate merger partner model, we confirm a similar movement of public investment as seen in the basic model.

The key to the identification of the standard DID estimation is a common trend assumption. However, we suspect that the common 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. The merged municipalities may have "propensities" that depend on certain conditions, and hence, municipalities suffering from fiscal difficulties might choose to merge. 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.

3.2 Estimation results of the PSM-DID

First, we succeeded in estimating the probit model to calculate the same propensity scores that were used in the previous study. In the probit model for calculating the propensity scores, based on Hirota and Yunoue (2017), we employ Japan's municipal data beginning in FY1998. This means that the Special Municipal Mergers Law had been in force for 7 years, from FY1999 to FY2005. The number of treatment observations for the basic model in FY2004 is 674 municipalities. Additionally, it is 756 municipalities for the basic model in FY2005. The number of treatment observations for the subordinate merger partner model in FY2004 and FY2005 are 497 and 546 municipalities, respectively. 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 to total revenue, and per capita debt stock. In addition, we completely satisfied 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[2].

Table 1 reports the results for the merged year in FY2004 using the PSM-DID method. Of course, we estimate many public investment expenses as outcome variables: river developments, harbor improvements, sewage facilities, residential constructions, airport constructions, etc. However, we cannot confirm a significant difference between the treatment and control

[2]The detailed results for the propensity score matching methods are available upon request. In addition, see Hirota and Yunoue (2017).

groups. Thus, we report the results for both road and public park expenses in this paper[3].

In the basic model, the per capita road expenses increase between 6.5 and 8.3 in FY2003, which are between approximately 16% and 20% of their average level 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 statistically significant, except for the result of kernel matching for FY2003.

In the subordinate merger partner model, we are able to identify 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 their average level in FY1998. In addition, the per capita public park expenses increased 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; for one reason, the public park expenses of most municipalities were almost zero in FY1998. That is, the merged municipalities rapidly increased their public park expenses just before mergers.

Table 2 reports the results of the merged year FY2005, focusing on "the rush merged municipalities" that merged just before the deadline of the special law. In the basic model, we are able to see the significant difference between the treatment and control groups for road expenses. The per capita road expenses increase between 5.2 and 7.3 in FY2004, which are between approximately 14.9% and 20.8% of their average level in FY1998. However, we cannot observe that the ATTs of the per capita public park expenses are statistically significant in FY2003 and FY2004.

In the subordinate merger partner model of Table 2, we are able to show positive and statistically significant results only for road expenses in FY2004. The per capita road expenses increase between 5.6 and 5.9 in FY2004, which is between approximately 16.1% and 16.9% of the average level of the per capita road expenses in FY1998. On the other hand, we cannot see the substantial difference in the public park expenses in either FY2003 or FY2004 because the merged municipalities in FY2005 choose to merge just before the deadline of the special law. Park construction requires a longer preparatory period than road construction, especially in Japan, which has less land. Thus, "the rush merged municipalities" do not increase their public park expenses in this case.

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

[3]These results for other public investments are available upon request.

of previous papers because they reveal that subordinate merger partners rapidly increase their local bonds just before the mergers. In addition, although most municipalities engaged in little public park construction before the special law was enforced, they clearly increase their construction just before the mergers and rely on the dominant merger partners. 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 subordinate merger partners. In addition, we believe that constructing new public parks is relatively easier for subordinate merger partners than constructing new roads.

3.3 Placebo tests and robustness checks

As mentioned above, the key to the identification of the standard DID estimation is the common trend assumption. For the control group to represent valid counterfactuals for the treatment group, municipalities should follow the same outcome path as those in nonmerged municipalities if they not overly affected. If the common trend assumption was to break down, the significant results obtained in Tables 1 and 2 would represent a pretreatment trend rather than a causal effect. Although we already confirmed the different trends in Figure 2 and 3 and applied propensity score matching to consider sample selection bias, we try to implement placebo tests in the subsection.

Tables 3 and 4 report the results of the placebo treatment periods by using the PSM-DID method. We estimate the same model as that in Table 1 and 2 during the placebo periods in each model. As a result, there are no statistically significant differences in the averages of the treatment and control groups in the pretreatment period. In particular, the estimated impact of the placebo treatment on the merged municipalities is smaller and nonsignificant compared to the main results. Thus, we are able to confirm that the merged municipalities rapidly increase their road or public park expenses just before mergers, expecting to share the costs among merger partners after the merger.

As robustness checks, we additionally estimate the dominant merger partner model in Table 5 by using the PSM-DID method. We use an indicator variable capturing whether the population size of the dominant merger partner is the largest of each merged municipality, in which case the treatment group equals 1 and zero otherwise. In the dominant merger partner model, note that there are almost all nonsignificant results, except for the first column that shows the per capita road expense in FY2003. The dominant merger partners who chose to merge in FY2004 slightly increased their road expenses just before the mergers. However, the impacts are smaller than those of the subordinate merger partners. Therefore, the subordinate merger partners have a stronger incentive to be free-riders, taking advantage of their dominant merger partners. These results demonstrate the existence of the fiscal common pool problem in Japan's municipal mergers, wherein merging municipalities increase road or public park expenses because they are allowed to issue local bonds for public investments.

4 Concluding remarks

The purpose of this paper is to identify the kinds of public investments leading municipal mergers to create a fiscal common pool problem in Japan by extending previous papers. 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 their mergers. These results are 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 partners. The special law is very helpful for subordinate merger partners. In addition, we consider that constructing new public parks is relatively easier than constructing new roads for subordinate merger partners because they hope to complete the merger by the specific deadline of the special law to receive special support from the central government.

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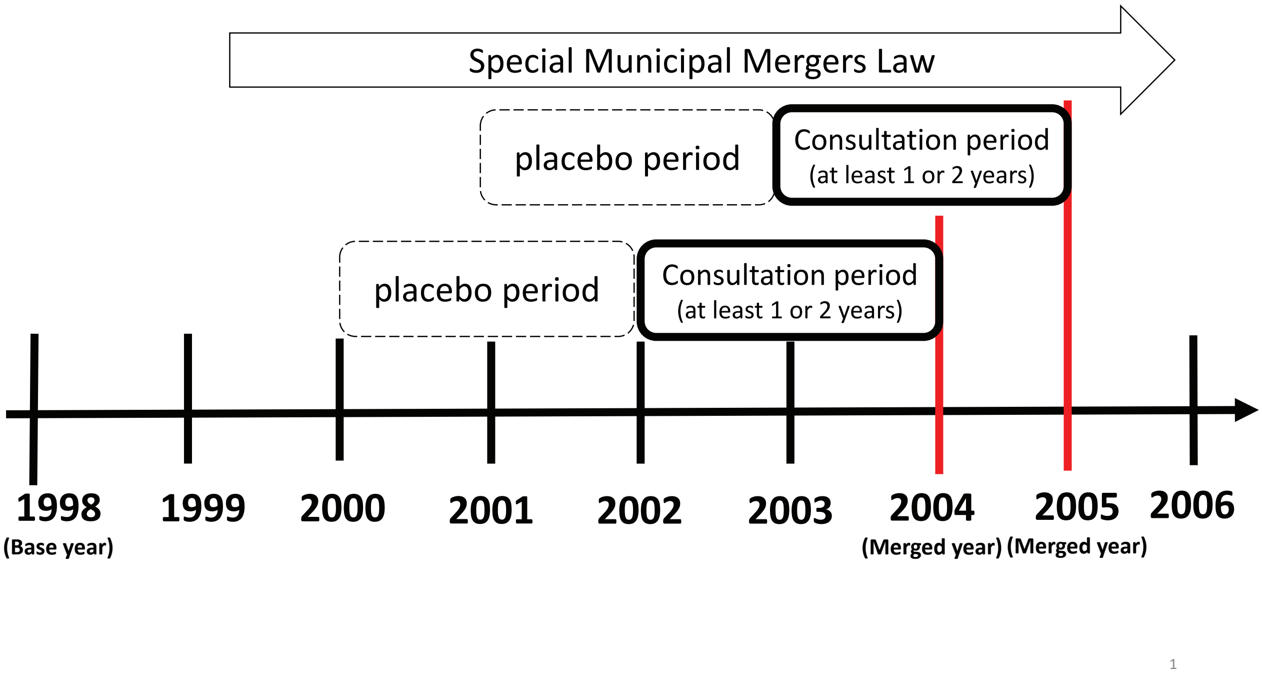
Figure 1: Timeline for municipal mergers in Japan



Notes: The figure shows the timeline of municipal mergers for FY2004. Additionally, we focus on the consultation period between FY2003 and FY2004 as treatment periods for municipalities that chose to merge in FY2005, followed by placebo periods between FY2001 and FY2002.

Figure 2: Public investment expense of merged year in FY2004

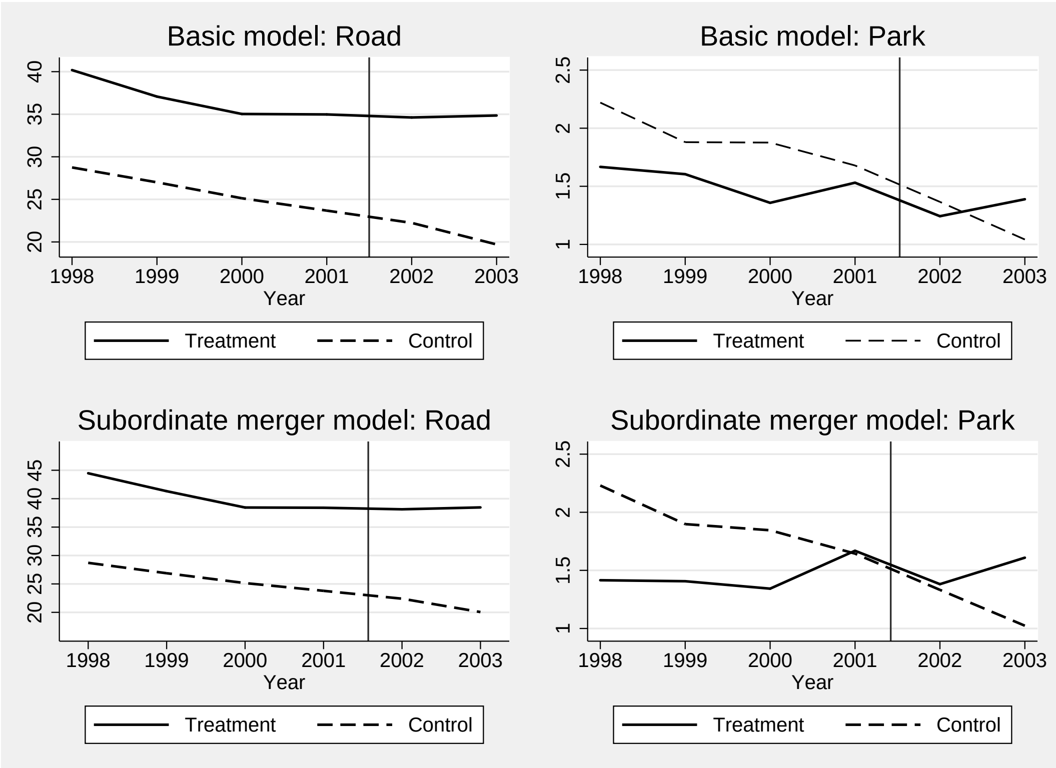


Figure 3: Public investment expense of merged year in FY2005

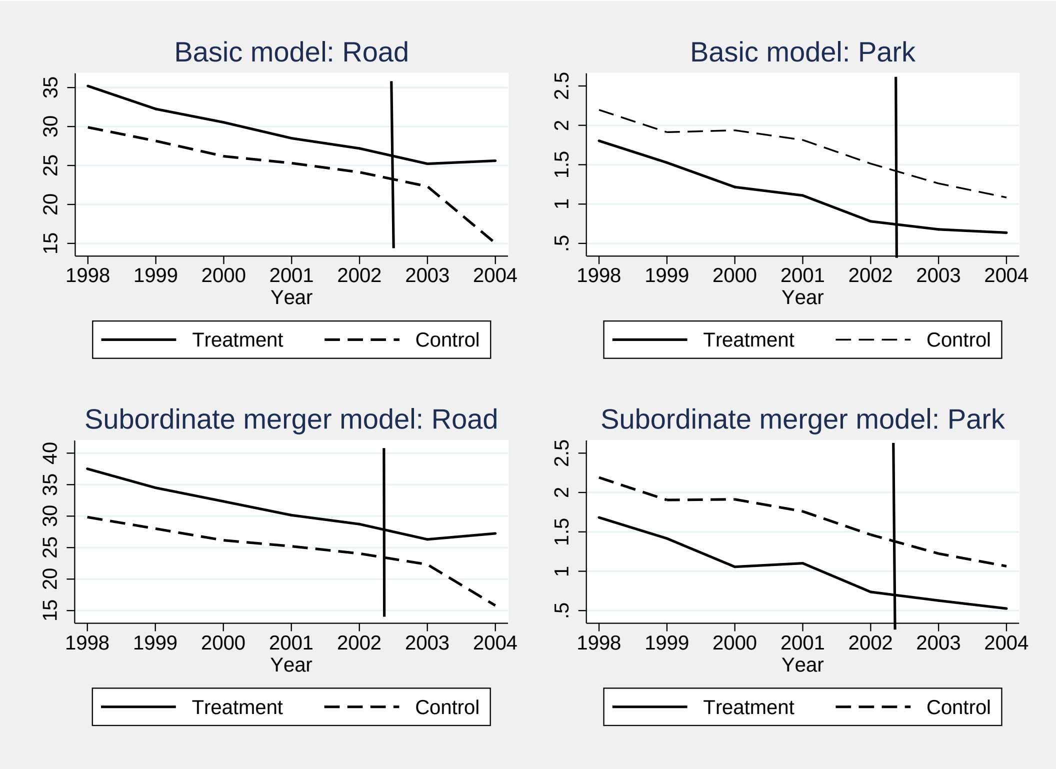


Table 1: Estimation results for merged year in FY2004

	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 levels, 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.

Table 2: Estimation results for merged year in FY2005

	Basic model				Subordinate merger model			
	Road		Park		Road		Park	
	2004	2003	2004	2003	2004	2003	2004	2003
Nearest Neighbor	5.236**	-1.704	-0.024	-0.203	5.670**	-2.892	-0.096	-0.677
	(2.182)	(1.731)	(0.557)	(0.456)	(2.391)	(2.050)	(0.549)	(0.509)
On support: Treatment	756	756	756	756	546	546	546	546
On support: Control	1,322	2,110	1,322	2,110	1,540	2,327	1,540	2,327
Radius	7.325***	-0.987	0.175	-0.301	5.844***	-1.956	0.041	-0.249
	(1.603)	(1.331)	(0.359)	(0.326)	(1.710)	(1.510)	(0.357)	(0.325)
On support: Treatment	755	755	755	755	545	546	545	546
On support: Control	1,300	2,087	1,300	2,087	1,528	2,311	1,528	2,311
Kernel	6.937***	-1.028	0.144	-0.359	5.921***	-1.887	0.075	-0.277
	(1.562)	(1.326)	(0.350)	(0.324)	(1.658)	(1.500)	(0.344)	(0.321)
On support: Treatment	756	756	756	756	546	546	546	546
On support: Control	1,322	2,110	1,322	2,110	1,540	2,327	1,540	2,327

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.

Table 3: Placebo tests for merged year in FY2004

	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 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.

Table 4: Placebo tests for merged year in FY2005

	Basic model				Subordinate merger model			
	Road		Park		Road		Park	
	2002	2001	2002	2001	2002	2001	2002	2001
Nearest Neighbor	-1.729	-2.118	-0.644	-0.927	-4.235**	-0.776	-0.553	-0.729
	(1.604)	1.501	(0.494)	(0.679)	(2.044)	(1.915)	(0.450)	(0.785)
On support: Treatment	756	756	756	756	546	546	546	546
On support: Control	2,215	2,230	2,215	2,230	2,423	2,447	2,423	2,447
Radius	-1.023	-1.107	-0.468	-0.329	-1.489	-1.456	0.387	-0.252
	(1.281)	(1.240)	(0.332)	(0.354)	(1.456)	(1.464)	(0.332)	(0.350)
On support: Treatment	755	755	755	755	546	546	546	546
On support: Control	2,192	2,207	2,192	2,207	5,415	2,430	2,415	2,430
Kernel	-1.003	-1.013	-0.513	-0.384	-1.370	-1.253	-0.401	-0.279
	(1.276)	(1.234)	(0.331)	(0.352)	(1.449)	(1.456)	(0.328)	(0.346)
On support: Treatment	756	756	756	756	546	546	546	546
On support: Control	2,215	2,230	2,215	2,230	2,432	2,447	2,432	2,447

Notes: ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, 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.

Table 5: Dominant partner model

	Dominant partner model (Merged year in FY2004)				Dominant partner model (Merged year in FY2005)			
	Road		Park		Road		Park	
	2003	2002	2003	2002	2004	2003	2004	2003
Nearest Neighbor	3.575**	2.578	-0.042	0.231	-1.809	-2.426*	-0.052	-0.256
	(1.509)	(1.582)	(0.612)	(0.710)	(1.306)	(1.370)	(0.699)	(0.671)
On support: Treatment	218	218	218	218	297	297	297	297
On support: Control	2,503	2,601	2,503	2,601	1,805	2,525	1,805	2,525
Radius	1.618*	1.197	0.324	0.505	-0.125	-1.355	-0.593	-0.626
	(0.942)	(0.917)	(0.304)	(0.295)	(0.973)	(1.062)	(0.510)	(0.543)
On support: Treatment	218	218	218	218	297	297	297	297
On support: Control	2,503	2,601	2,503	2,601	1,805	2,525	1,805	2,525
Kernel	1.305	0.906	0.320	0.528	-0.887	-1.945*	-0.576	-0.568
	(0.962)	(0.935)	(0.314)	(0.306)	(1.011)	(1.090)	(0.516)	(0.551)
On support: Treatment	218	218	218	218	297	297	297	297
On support: Control	2503	2601	2503	2601	1805	2525	1805	2525

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.