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Common Pool Problems in Voluntary Municipal Mergers

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Common Pool Problems in Voluntary Municipal Mergers[†]

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Abstract

We analyse free-riding behaviour of Finnish municipalities prior to voluntary municipal mergers. The merger process creates a temporary common pool problem, because of a delay from the initial decision to the actual merger during which municipalities stay autonomous. Using a difference-in-differences strategy, we find that the stronger free-riding incentive a municipality faced the more it increased its debt and spent its cash reserves. These funds were spent mostly on investments and current expenditures. The results can be attributed to the “law of $1/n$ ” rather than to responding to an anticipated loss of political power or voluntary transfers between merging municipalities.

Keywords: Common pool, difference-in-differences, free-riding, law of $1/n$, municipality mergers

JEL Codes: D72, H72, H73, H77

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1 Introduction

The size and number of local governments is a crucial policy decision from the point of view of efficient provision of local public goods and services (e.g. Miceli 1993; Alesina and Spolaore 1997; Ellingsen 1998). Often municipality mergers are seen as an effective way of realizing economies of scale and such reforms are widespread.¹ Major municipal merger reforms have been implemented over time in a number of countries including Canada, Denmark, Germany, Israel, Japan, Sweden and Switzerland (Dafflon 2012; Hansen 2012; Hinnerich 2009; Reingewertz 2012; Weese 2013). However, a possible and somewhat overlooked cost of municipality mergers is that the merger process itself creates a temporary common pool problem among the municipalities who are about to merge. This problem arises because usually there is a delay (in our case at least one calendar year) from the merger decision to the actual merger. Within this window, a single merging municipality can make autonomous decisions and shift some of the costs of additional expenditures or investments to its merger partners by increasing debt or liquidating assets.

These free-riding incentives are directly related to the “law of $1/n$ ” as formalized by Weingast et al. (1981) in the case of multiple identical and geographically distinct jurisdictions.² In their model, the total size of the common pool increases in the number of districts (n) that form the common pool, which is an appropriate description of the municipal merger case.³ Each jurisdiction can propose a project that is always passed (universalism) and funded through generalized taxation on all n jurisdictions. In this model, the

¹ Other potential benefits from larger jurisdictions include internalizing externalities, better state capacity and increased capacity to sustain spending or revenue shocks.

² Baron and Ferejohn (1987 and 1989) extend this line of argumentation into situations where decision-makers need to bargain over which projects are carried out. In this context, they show that the common pool creates incentives not only to increase spending in own jurisdiction, but also to restrain the spending in other jurisdictions. See also Knight (2008) for further results.

³ Primo and Snyder (2008) present a model where the total population size of the common pool is fixed and the size of each district diminishes as n increases, which is not the case in municipal mergers.

share of the tax burden in each jurisdiction is $1/n$. Weingast et al. (1981) show that, in this setting, inefficiency increases with n because a single jurisdiction receives all the benefits from its project while the costs are shared amongst all districts. Municipal mergers are a particularly clean case to test the original law of $1/n$, because the universalism assumption holds as each merging municipality can make autonomous decisions before the merger actually takes place. One distinction to Weingast et al. (1981) is that, in the case of a merger, due to different population sizes of each municipality, the incentives to free-ride (share of costs) are not directly related to the number of municipalities in the upcoming merger, but rather to the size of the participating municipalities relative to the size of the common pool.

The current empirical evidence of common pool problems related to municipality mergers is somewhat mixed and concerns only forced municipal mergers. Hinnerich (2009) and Jordahl and Liang (2010) study Swedish municipality mergers imposed by the central government in the 1950's, the 60's and the 70's. Hinnerich (2009) finds that the smaller a merged municipality was compared to merger partners in terms of population the more the municipality increased its per capita debt prior to merging. Jordahl and Liang (2010), on the other hand, find that a merger as such (or the creation of a common pool) had an effect on debt accumulation, but the relative size of merging municipalities did not seem to matter. The latter evidence is somewhat hard to reconcile with free-riding behaviour as predicted by the law of $1/n$.⁴

⁴ Hansen (2012) analyses the Danish municipal merger reform of 2007. Due to central government imposed fiscal restrictions, Hansen (2012) analyzes only current expenditures and budget overruns and concludes that free-riding took place. However, free-riding takes place only if some of the costs of increased current expenditure can be shifted to the merger partners and Hansen (2012) does not report debt accumulation or changes in asset positions. The common pool problem has been analyzed in a number of other contexts as well and the results from these papers are also somewhat mixed (see e.g. Baqir 2002, MacDonald 2008 and Petterson-Lidbom 2012). However, in these papers, decisions are not based on universalism, but rather involve e.g. bargaining issues, and thus, these papers cannot be seen as clean tests of the Weingast et al. (1981) law of $1/n$.

In this paper, we analyze free-riding behaviour in Finnish municipal mergers. Our institutional setup differs from previous cases in an important way because the Finnish mergers were decided voluntarily at the local level by municipal councils. It is unclear whether we should expect common pool problems to arise in a voluntary setting because one might expect that municipalities can somehow agree or contract not to exploit the common pool or that extensive free-riding would result in a cancellation of the merger.

Somewhat surprisingly though, using difference-in-differences (DID) methods with a continuous treatment we find large free-riding effects also among voluntary mergers. Consistent with the law of $1/n$, the stronger free-riding incentive a municipality faced the more it increased per capita debt and used up its cash reserves. Unlike the previous studies, we can also follow the money to a certain extent. We find that extra funds from the common pool were spent mostly on investment and on current expenditures. Municipalities did not lower their local income tax rate nor did they hire new employees. Overall, due to free-riding, the merged municipalities accumulated about 250 million Euros of debt, corresponding to roughly 20% of their pre-treatment debt stock, and also decreased their cash reserves substantially (140 million Euros).

In addition to shedding light on the benefits and costs of restructuring of local governments, these results should be of wider interest given the common pool concerns related to the European sovereign debt crisis and the involved bail-outs of some European countries (e.g. Lane 2012). Contemporary causal evidence concerning free-riding behaviour of political decision makers is still scant. Moreover, results concerning Finland are particularly interesting, because Finland is perceived to be a country with low corruption and high state capacity and a proponent of austerity measures in the EU debate. Therefore, it seems that politicians are prone to exploiting a common pool whenever given the opportunity to do so.

Besides the law of $1/n$, there are two interesting alternative theoretical explanations for our findings. The first alternative explanation is that pre-merger municipalities with strong free-riding incentives also expect to lose most of their political power in the post-merger council. Such municipalities may want to hastily spend money while they can still make autonomous decisions irrespective of whether the extra spending is funded by the common pool or not. Unlike the previous literature, we test directly between the free-riding and the loss of power hypotheses using variation in the number of merging partners in a given merger and show that municipalities respond only to the free-riding incentives.⁵

The second alternative explanation for free-riding in the context of voluntary mergers is Coasean type transfers between the merging municipalities (Coase 1960). This case arises when a merger increases the welfare of the merger partners as a whole, but decreases the welfare of some of the individual partners. If the benefits are large enough, in principle, the winners can compensate the losers in order buy their approval of the merger. This explanation is, however, unlikely in our case. The municipalities with strong free-riding incentives who exploited the common pool are on average poorer than those with a weaker free-riding incentive, and thus, do not seem to be attractive partners. One would expect that any transfers would go to the other direction. Moreover, Acemoglu (2003) raises some issues on why such Coasean bargaining is unlikely to occur in a political context. We also show that municipalities that were less keen to merge, proxied by the share of councillors who voted in favour of the merger, do not accumulate more debt relative to more keen municipalities, suggesting that we can rule out a major role for Coasean transfers.

⁵ As an example, think of a small municipality merging with two other municipalities of equal size versus merging with one municipality twice its size. The free-riding incentives are the same in these two cases (municipality's relative size to the common pool), but the distribution of post-merger political power is very different.

Why do we observe free-riding behaviour in voluntary mergers? First, a close examination of the merger agreements reveals that municipalities did foresee a possible common pool problem, at least to a certain extent. Most merger agreements included phrases, such as, “that municipalities should behave responsibly in their economic decision-making prior to merging” or “major investment decisions should be made jointly”. However, the agreements do not include any contingency plans over possible breaches or what exactly would constitute a breach.⁶

Second, it might be difficult for merger partners to observe the exact behaviour of their future partners because of delays in accounting and official statistics production. The financial statements from the final pre-merger year are available only after the merger has already taken place.⁷

Finally, municipal mergers in general differ from the Weingast et al. (1981) model, because political decision makers may face electoral punishment. However, the local politicians from municipalities with strong free-riding incentives (typically small municipalities) face relatively low re-election prospects in the post-merger elections and according to Hyytinen, Saarimaa and Tukiainen (2013) they seem to be aware of this fact in Finland. Furthermore, Saarimaa and Tukiainen (2013) show that after a merger, councillors gain votes mostly from their old pre-merger constituencies. This means that some councillors are lame ducks effectively facing a term limit (e.g. Besley and Case 1995; Ferraz and Finan 2011), while others need to please (mostly) their old voters to assure re-election. As suggested by Aidt and Shevts (2012), the common pool problems may be exacerbated because of re-election concerns if voters reward the politicians who are able to bring home most pork.⁸

⁶ We are unaware of any cases where a merger was cancelled after it was formally accepted by the municipal councils.

⁷ The final financial statements of municipalities are handled by municipal councils in June of the following year.

⁸ Aidt and Shevts (2012) present theoretical and empirical results in which re-election concerns exacerbate the common pool problem. This link arises in their theoretical model

Naturally, the fact that the Finnish mergers were decided voluntarily raises issues of non-random selection that may bias our results.⁹ This selection may relate both to the decision to merge and to the extent of free-riding incentives among the mergers. Reassuringly, we are able to show long common pre-treatment trends for the control and the merger group (to address selection into merging) and for various treatment groups based on the level of free-riding incentives (to address selection based on free-riding incentives). We also show that potential changes in the financial situation coinciding with increases in per capital debt (taxable income, corporate tax revenue and central government grants) cannot explain our findings. The results are also robust to using alternative control groups, placebo tests and adding control variables. Moreover, a generous merger subsidy scheme promoted by the central government may potentially confound our findings. However, we show that explicitly controlling for the amount of subsidy does not affect our results. Furthermore, our estimates suggest that the increases in debt and decreases in cash reserves were in total much larger than the overall amount of central government subsidies granted to the mergers (217 million Euros).

The rest of the paper is organized as follows. In Section 2, we present a short overview of the institutional setting of Finland and especially the merger process. In Section 3, we describe the empirical approach. We describe our data and present the econometric results in Section 4. Section 5 concludes.

because politicians differ in their ability to bring home the pork and elections are an *ex post* selection device that voters use to oust politicians who are unable to deliver the pork.

⁹ Also the forced mergers may be subject to selection due to the behaviour of central government politicians for example.

2 The Finnish mergers

Finland has a two-tier system of government consisting of the central government and municipalities as the local level. In international comparison, Finnish municipalities have extensive tasks. In addition to the usual local public goods and services, municipalities are responsible for providing most of social and health care services and primary and secondary education. This makes municipalities of considerable importance to the whole economy. The GDP share of municipality spending is roughly 18 percent and they employ around 20 percent of the total workforce.

In addition to extensive tasks, Finnish municipalities have extensive fiscal autonomy.¹⁰ Most importantly for our purposes, there are no restrictions on municipal use of debt. Moreover, interest rates do not depend on individual municipality conditions due to a joint liability scheme.¹¹ These features may increase the free-riding incentives. Furthermore, the central government does not enforce additional restrictions on the merging municipalities concerning how they handle their finances before the mergers realize. Municipalities fund their expenses mostly using own revenue sources. The most important sources are local taxes and operating revenues, such as fees. The most important tax instrument is the local income tax. The tax rate is flat and the municipalities can set the level freely. The property tax is of much less importance and municipalities can set the property tax rates only within limits set by the central government. The corporate income tax is a state level tax, but municipalities receive a share of this tax revenue based on profits and employment of firms within their borders. In 2012, the average share of the income tax of total revenue was 46 percent, while the shares of the property and corporate taxes were only 3 percent, respectively.

¹⁰ Finnish municipalities are self-governing entities by constitution, which means that the central government cannot assign new tasks to municipalities without passing legislation.

¹¹ This is organized through Municipality Finance Plc, a credit institution owned by the local government sector.

There are clear regional tax base and cost disparities, which are offset by a central government grant system. The system is based on estimates of average costs and tax bases so municipalities have very limited possibilities to influence the amount of grants that they receive. The grant system covers about 20 percent of total municipal revenues, but this share varies a great deal. The system covers more than 50 percent of all revenues for every fourth municipality.

Due to aging related expected increase in municipal spending and disparities in revenue bases, the central government initiated a plan in 2005 that aimed at reforming municipal revenue structure and more importantly making the production of statutory municipal services more efficient. The main tool for strengthening the operating environment of municipalities in the government plan was municipality mergers. A provisional law enacted in 2007 clearly states that municipalities should have strong enough revenue and labour force bases to cope with the production of statutory municipal services. The municipalities were allowed to decide voluntarily whether and with whom to merge. Following the law, there was 1 merger in 2008 and 32 in 2009, 4 in 2010 and 6 in 2011. The number of municipalities involved in a given merger ranged from 2 up to 10 municipalities.

The central government encouraged mergers using three important policies. First, central government grants were guaranteed not to decrease for the first five years after merging. Second, municipalities were not allowed to lay off their employees during the first five post-merger years. This policy is likely to make it difficult to achieve any substantial efficiency gains from merging in the short term. Third, the central government granted generous merger subsidies to merged municipalities. The subsidy amount depends on the populations of the pre-merger municipalities, the population of the resulting new municipality, the number of participating municipalities in the merger and timing of the merger. The mergers from 2008 and 2009 received

more subsidies than the subsequent ones, possibly explaining why most mergers took place in 2009.¹²

In order to have a clean DID setup with respect to treatment timing, we focus on the 99 municipalities that merged at the start of 2009.¹³ The merger process is as follows. The process usually starts with unofficial discussions which may lead to an initial feasibility study that is conducted by an external consultant. Based on the consultant's report, municipal boards make a proposal of the merger to the municipal councils.¹⁴ This proposal is voted on by the councils. If the proposed merger gains a majority in all the participating councils, the merger goes through. If not, it is cancelled and all the municipalities continue as they were. The mergers we analyze were decided mostly in 2006 and some in 2007. This means that merging municipalities had up to 2 years (for all at least 2008, and for most also 2007) to exploit the common pool. These two years are the treatment period in our DID analysis. Since some of the mergers were decided on 2006, some of the free-riding could have taken place already during that control period year. We return to this issue in our robustness analysis.

¹² The subsidies were paid to the merged municipalities in annual instalments over a three year period after the mergers had taken place. Nevertheless, municipalities could spend the subsidy before the merger by accumulating debt. Thus, these subsidies potentially confound our results. We address this concern carefully later.

¹³ Saarimaa and Tukiainen (2014) study the determinants of the 2007–2009 Finnish mergers at an aggregate merger level and find evidence that voter preferences for location of services, local politics, previous cooperation and fiscal distress of one of the potential merger partner are associated with merging. Their results suggest that well off municipalities are willing to merge with less well off partners only if they maintain political power in the post merger council. Hyytinen, Saarimaa and Tukiainen (2013) study individual councillors' voting behaviour concerning these mergers and find that politicians' desire to avoid electoral competition is reflected in their decision-making.

¹⁴ Municipal councils are the main seat of power in Finnish municipal decision making. Finland has a proportional open-list election system. Currently, there are eight parties that dominate municipal politics, but some local lists are prevalent as well. Municipal elections are held every four years on simultaneous election dates. The councils that voted for the mergers under scrutiny were elected in October 2004. The elections in October 2008 already used the new post-merger municipal division, although the merger came to effect at the start of 2009.

3 Econometric framework

Our identification strategy is based on the difference-in-differences (DID) method, where a control group of municipalities is compared to treated municipalities before and after a treatment has taken place. According to the law of $1/n$, the free-riding incentive for a municipality should depend on its relative size with respect to the size of the total common pool. In the context of municipality mergers, municipality i with a population of pop_i in merger j with a total population of pop_j internalizes only pop_i/pop_j of the total marginal costs of funds (Hinnerich 2009).¹⁵

For municipality i in merger j we define the free-ride treatment variable as

$$freeride_{ij} = 1 - \frac{pop_i}{pop_j}. \quad (1)$$

Population levels are measured in 2007. The treatment intensity is high when a municipality is small compared to the merger as a whole. This happens when a municipality is part of a merger involving many municipalities and/or merges with much larger partner(s). The treatment is equal to zero for municipalities that did not merge.

We also estimate models where we include a simple dummy indicating whether a municipality decided to merge. The intuition is that simply the creation of a common pool, i.e. the merger decision, leads to free-riding regardless of relative size. Jordahl and Liang (2010) argue that this may be the case if municipalities have limited understanding of all the incentives at work

¹⁵ From the perspective of the Weingast et al. (1981) model, we would need a measure of relative tax bases. Due to many different types of municipal taxes, it is not clear whether e.g. income shares are more appropriate than population shares. Since the correlation between income and population is 0.993 in our data, it makes no practical difference. To facilitate comparison to previous studies, we follow Hinnerich (2009) and Jordahl and Liang (2010) in using a population based measure.

or limited opportunity to exploit the common pool, for example if they can only launch a limited number of investment projects each year. In our case, directly controlling for the merger dummy also alleviates problems related to the merger subsidies.

Since we have municipal level panel data from multiple years we estimate the following type of models:

$$\begin{aligned}
y_{it} = & \mathbf{x}'_{it}\boldsymbol{\beta} + \mu_i + \tau_t \\
& + \delta_1 \cdot \text{merger}_i \cdot d2007_t + \delta_2 \cdot \text{merger}_i \cdot d2008_t \\
& + \alpha_1 \cdot \text{freeride}_i \cdot d2007_t + \alpha_2 \cdot \text{freeride}_i \cdot d2008_t + u_{it},
\end{aligned} \tag{2}$$

where y is one of our outcomes of interest. The vector \mathbf{x} includes time-varying control variables, μ is a municipality fixed effect and τ a year fixed effect. The key explanatory variables in this setup are the interaction terms that include the merger-dummy and the *freeride* measure. The dummy variables $d2007$ and $d2008$ indicate that the observation is from 2007 and 2008, respectively. We allow the treatment effect to vary between the two treatment years for three reasons: First, 2007 may be contaminated in a sense that some of the analyzed mergers were decided only late during that year and may not have had time to respond during 2007. Second, exploiting the common pool by investing may require some preparation time and may be effective only in 2008. Third, it may be optimal to liquidate assets as late as possible because a certain level of cash reserves is needed to run the day to day operations of the municipality.

Alternatively, we can use only the continuous treatment variable. In this case, the model can be written as

$$\begin{aligned}
y_{it} = & \mathbf{x}'_{it}\boldsymbol{\beta} + \mu_i + \tau_t \\
& + \gamma_1 \cdot \text{freeride}_i \cdot d2007_t + \gamma_2 \cdot \text{freeride}_i \cdot d2008_t + u_{it}.
\end{aligned} \tag{3}$$

Since the mergers in our data were decided voluntarily by the municipalities the treatments are not randomly assigned. Main concerns are that the unobservables or the outcomes have different trends. To alleviate these concerns we subject our results to a number of validity and robustness tests.

4 Empirical results

4.1 Data

In presenting our results, we will heavily rely on graphical evidence, but will also provide regression results to confirm that our findings are statistically significant. In the graphical analysis, we divide the municipalities into three groups. The first group consists of the municipalities that did not merge. In addition, we divide the merged municipalities into two equal sized groups based on the *freeride* measure. We label the group of municipalities with an above median value of *freeride* as the “strong incentive” group and the “weak incentive” group consists of the municipalities with a below median value of *freeride*. The municipalities in the strong incentive group are typically small and/or are involved in mergers with more than two municipalities. Of course, this division is somewhat arbitrary and we will fully exploit the continuity of the treatment variable using regression analysis.

We use three sets of variables in our empirical analysis. First, we test the free-riding hypothesis using municipality’s per capita debt stock and cash reserves. Increasing debt or decreasing liquid funds are the most obvious ways a municipality can shift the costs of current expenditures or investments to future merger partners. Second, we use control variables to capture any changes in a municipality’s fiscal situation that may coincide with the merger process. Third, we analyze how municipalities spend the possible extra funds.

Table 1 presents descriptive statistics for our key variables for the groups described above. The numbers in Table 1 are from 2005, i.e. just before any of these municipalities had decided to merge. The municipalities in the strong incentives group are on average smaller and were involved in larger mergers both in terms of merger population and number of municipalities in the merger compared to the weak incentives group. In other respects, the groups are quite similar. The municipalities in the strong incentives group are slightly poorer than the weak incentive group in terms of income tax base (taxable income) and corporate tax revenue, but they receive slightly larger grants.

We use operating margin as our measure of municipal expenditures (apart from investments). Operating margin is an accounting concept which measures annual expenditures net of operating revenue, such as fees. This means that the operating margin equals the revenue deficit that municipalities need to fill using own tax revenue and central government grants.¹⁶ Once you add these revenue sources, the resulting amount is available for investment and depreciation or write-offs.

¹⁶ Operating margin is the correct measure also because some municipalities produce or sell services to other municipalities. These services show up on the municipality's expenditure side, but they also receive operating revenue from the sale of these services that we need to net-out.

Table 1. Descriptive statistics for municipalities prior to merger decisions (in 2005).

	No merger		Weak incentives		Strong incentives	
	Mean	SD	Mean	SD	Mean	SD
Number of observations	306		49		50	
Population	12,732	40,029	19,298	23,807	3,455	3,316
Merger population	12,732	40,029	32,434	32,702	44,992	30,111
Merger size (number of municipalities)	1	1	3.06	1.52	4.94	2.74
Merger subsidy (€/per capita)	0	0	381.2	267.0	326.1	180.9
Freeride	0	0	0.43	0.25	0.91	0.05
Debt stock (€/per capita)	1,406	886	1,347	1,102	1,097	894
Cash reserves (€/per capita)	500.7	646.5	496.1	641.8	418.8	586.4
Taxable income (€/per capita)	10,226	2,148	11,368	1,796	10,085	1,443
Corporate income tax (€/per capita)	163.9	96.9	192.7	245.3	138.1	81.8
Central government grants (€/per capita)	1,553	572.0	1,222	566.3	1,404	472.5
Municipal tax rate (%)	18.68	0.64	18.55	0.85	18.69	0.68
Operating margin (€/per capita)	-3,718	427.4	-3,580	417.9	-3,609	404.7
Municipal employees (per capita)	0.060	0.014	0.054	0.013	0.052	0.015
Investment expenses (€/capita)	440.7	363.4	467.2	291.9	487.9	586.7

4.2 DID results

Next we move on to our main results. Figure 1 presents the development of the per capita debt stock and cash reserves from 2000 to 2008, which is the last year the merged municipalities existed as independent entities, and therefore, the last year that municipal level statistics are available for this group. The vertical red line highlights the beginning of the treatment period.

Two observations stand out from the figure. First, both debt stock and cash reserves seem to have common trends in the different groups up to 2005, although the groups differ slightly in terms of cash reserves in 2000 and 2001. The strong incentives group shows a slightly different pre-trend in 2005–2006. This is not a major concern because some municipalities could have been able to exploit the common pool already late 2006. Second, group differences emerge in both outcomes in 2007 and especially 2008. The debt stock increases much faster in the strong incentives group compared to both the

weak incentives and the no-merger group. The weak incentive group also clearly differs from the no-merger group. A similar story is true for cash reserves. Municipalities in both the weak and strong incentives group use up their cash reserves compared to the control group, although the strong incentives group clearly stands out. The evolution of group differences in these outcomes is consistent with free-riding behaviour.

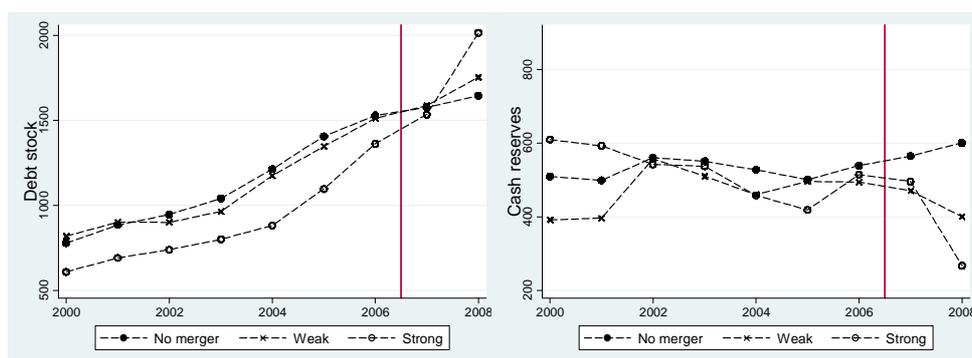


Figure 1. Debt stock and cash reserves in different free-rider groups (€ per capita).

Despite the fact that the groups had similar pre-treatment common trends, a merger decision could coincide with a worsening of municipal finances or population changes. This would mean that increases in per capita debt, for example, are not necessarily driven by free-riding incentives, but instead simply a reaction to simultaneous fiscal distress.

To alleviate these concerns, in Figure 2 we present the development of per capita taxable income, per capita corporate income tax revenue and per capita grants. Together with service fees, these comprise all the relevant municipal income sources.¹⁷ Again, all these variables seem to have clear common pre-treatment trends and more importantly, the trends or differences in trends do not change during the treatment period. This gives us confidence

¹⁷ Reliable data on itemized service fees are not available, partly because many of the service fees are channelled through various municipal cooperation organizations and the related accounting practices vary across municipalities.

that the observed increases in debt and decreases in cash reserves are not driven by group specific changes in fiscal conditions. We also show the development of population levels in different groups due to show that the merger decisions did not cause any sorting responses from citizens.

Figures 1 and 2 alleviate our two potential selection issues: selection into merging and selection based on the extent of free-riding incentives. Figure 1 reports common trends both in mergers compared to not merged and between different free-riding incentive groups. Figure 2 shows that fiscal conditions and population develop identically in different groups. Therefore, either type of potential selection is unlikely to be an issue for our analysis. Moreover, we can see that the treatment period seems to be very typical from financial perspective, e.g. the emerging financial crisis does not seem to be yet influencing the municipalities under scrutiny here.

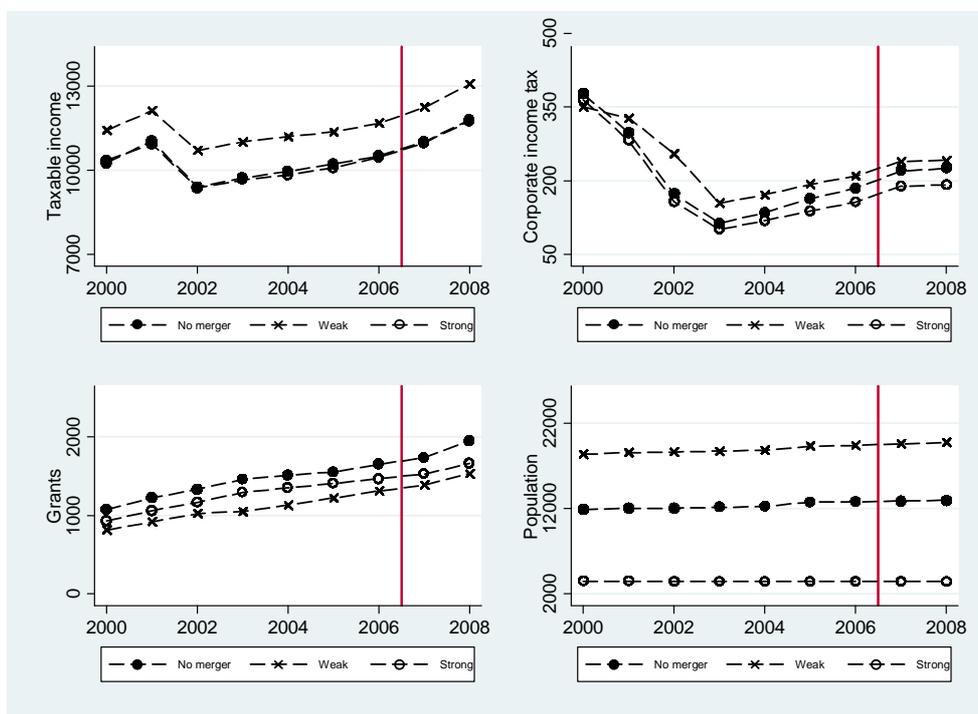


Figure 2. Income tax base, corporate tax revenue, grants and population in different free-rider groups (monetary amounts in per capita terms).

In Table 2, we report DID results based on Eq. (2) and (3). For each model specification and outcome, we report results both with and without control variables (see Figure 2).¹⁸ We first subject the merger-dummy and *freeride* to a horserace and report results from models where we include both the merger-dummy and the *freeride* variable. It is evident from the results that the merger-dummy is not statistically significant and that free-riding behaviour is tightly connected to free-riding incentives as predicted by the law of $1/n$. In Panel A, the results for cash reserves are not very precise, but this is likely to be due multicollinearity between the merger-dummy and the *freeride* variable. As can be seen from Panel B, once we omit the merger-dummy also the cash reserves results are highly statistically significant. Adding control variables has very little effect on the point estimates, which is, of course, what one would expect from Figure 2.¹⁹

Increasing the *freeride* variable from zero to one increases (decreases) per capita municipal debt stock (cash reserves) on average by 570 (330) Euros by the end of year 2008. These are substantial amounts compared to the starting level of these variables. Overall, due to free-riding, the merged municipalities accumulated about 250 million Euros more debt, corresponding to almost 20% of their 2006 debt stock. The decrease in cash reserves is also substantial, roughly 140 million Euros.²⁰

¹⁸ We also estimated the models using a log linear specification. The results are similar to those in Table 2 (see Table A2 in the Appendix).

¹⁹ All the results are similar and statistically significant also if we do not allow for a different treatment effect for different treatment years, but rather use only one after period (not reported for brevity). In Table A1 in the Appendix, we report a specification with only the discrete treatments. Also these results are consistent with free-riding behaviour and statistically significant.

²⁰ These estimates are based on the point estimates from the models in Table 2 with the control variables, but without the discrete treatment. Calculation accounts for the fact that effects are larger in smaller municipalities and that these outcomes are stock variables.

Table 2. DID results for debt stock and cash reserves.

Panel A: Discrete and continuous treatment				
	Debt stock	Debt stock	Cash reserves	Cash reserves
<i>constant</i>	765.6*** [22.89]	893.6** [380.1]	517.5*** [15.53]	-795.2** [369.09]
<i>merger* 2007</i>	-88.2 [109.1]	-107.9 [113.2]	54.88 [105.0]	15.48 [112.5]
<i>merger* 2008</i>	-183.5 [124.2]	-213.5* [127.3]	33.27 [128.5]	-4.280 [134.1]
<i>freeride* 2007</i>	293.8 [183.4]	303.7 [186.9]	-147.5 [148.8]	-93.63 [155.4]
<i>freeride* 2008</i>	807.4*** [224.2]	821.6*** [225.6]	-395.6** [177.8]	-323.4* [186.8]
<i>controls</i>	no	yes	no	yes
N	3,634	3,634	3,717	3,717
R ²	0.42	0.43	0.02	0.05
Panel B: Continuous treatment				
	Debt stock	Debt stock	Cash reserves	Cash reserves
<i>constant</i>	765.6*** [22.90]	925.4** [375.9]	517.5*** [15.53]	-796.4** [363.8]
<i>freeride* 2007</i>	189.8* [102.7]	176.1* [102.3]	-83.16 [67.86]	-75.46 [66.83]
<i>freeride* 2008</i>	591.4*** [132.2]	569.7*** [131.9]	-356.6*** [80.37]	-328.4*** [83.92]
<i>controls</i>	no	yes	no	yes
N	3,634	3,634	3,717	3,717
R ²	0.42	0.43	0.02	0.05

Notes: All the models include year and municipality fixed effects. The control variables include taxable income, corporate income tax revenue, grants and population. Standard errors are clustered at the municipality level and reported in brackets. ***, ** and * indicate statistical significance at 1, 5 and 10 percent level, respectively.

We are also interested in how municipalities spend the extra money from the common pool. There are a number of ways that municipalities can spend the money that benefits only local taxpayers. First, municipalities can lower either municipal tax rates or service fees and fund the normal level of municipal expenditures using debt and liquid assets. Second, municipalities can increase current spending. Third, municipalities can invest the money into

local projects. An example would be an investment into local schools or other service facilities serving mostly or only local taxpayers also after the merger.

Figure 3 shows trends in municipal income tax rate, operating margin per capita, municipal employees per capita and total investments per capita. There is no change in municipal income tax rate in the treatment period, but the municipalities in the strong incentives group do increase both expenditures (lower operating margin) and investments, although the former is slightly difficult to detect from the figure due to scale differences. Interestingly, the additional expenditure is not directed towards hiring more municipal employees. This result is consistent with free-riding behaviour. Hiring new employees does not guarantee that the benefits remain in the old municipality because employees can be easily shuffled around after a merger. All these variables, except for investments, follow clean pre-treatment common trends.

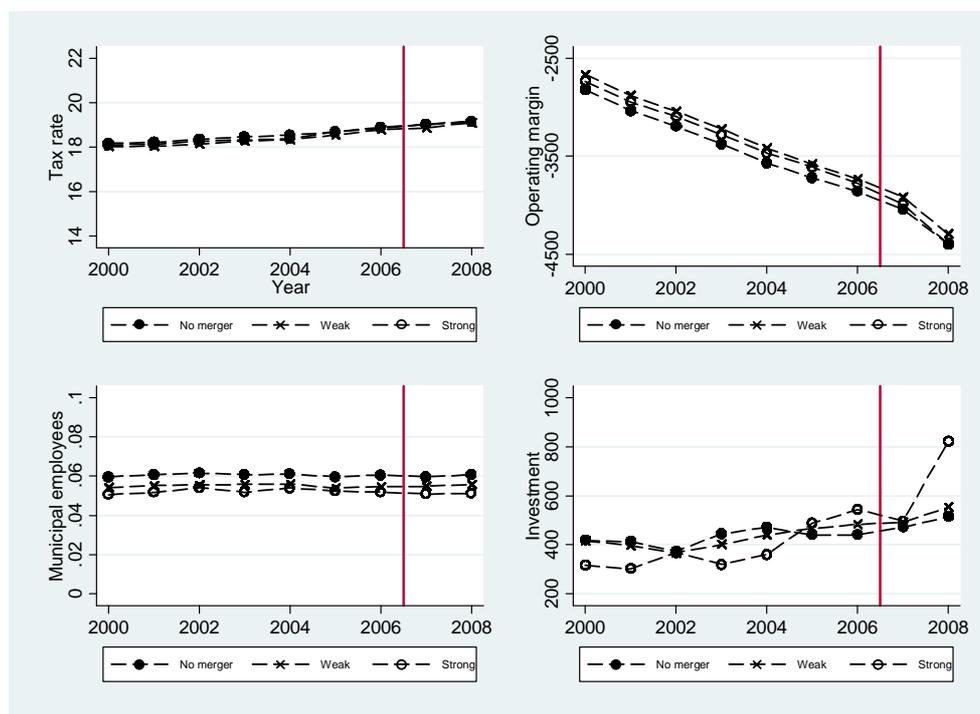


Figure 3. Tax rate, operating margin, municipal employees and investment expenses in different free-rider groups.

Table 3 confirms that the effects in Figure 3 are also statistically significant.²¹ Increasing *freeride* from zero to one increases municipal expenditure flows on average by 70 Euros per capita in 2007 and 160 in 2008. Investments increase by 315 Euros per capita in 2008. Overall, due to free-riding, the merged municipalities' spending increased about 100 million Euros and invested about 140 million Euros more. Therefore, while we can follow some of the money, increases in expenditures and investments only account for about half of what we observe in debt and cash changes.²²

²¹ According to Table 3, also tax rates increase slightly with free-riding incentives. However, the result is only weakly significant and economically very small. Increasing freeriding incentives from zero to one would only lead to about 0.13 percentage point increase in tax rates. The minimum legally possible annual tax increase is 0.25 percentage points, and thus, the effect on tax rates is not substantial.

²² Our asset data is not detailed enough to track all the money. In public discussion, there has been speculation that municipalities try to protect their assets e.g. by setting up foundations that can be used to distribute benefits to residents of the old municipality even after a merger. This type of asset conversion cannot be detected from our data.

Table 3. DID results for tax rate, operating margin, municipal employees and investment.

	Tax rate	Tax rate	Operating margin	Operating margin
<i>constant</i>	18.15***	18.70***	-2794***	-2361***
	[0.015]	[0.217]	[6.204]	[112.0]
<i>freeride</i> * 2007	0.072	0.068	-54.62	-67.70**
	[0.053]	[0.053]	[34.55]	[32.33]
<i>freeride</i> * 2008	0.130*	0.128*	-110.4**	-156.4***
	[0.067]	[0.068]	[51.98]	[46.96]
<i>controls</i>	no	yes	no	yes
N	3,727	3,727	3,727	3,727
R ²	0.61	0.61	0.93	0.94

	Municipal employees	Municipal employees	Investment	Investment
<i>constant</i>	0.058***	0.038***	404.1***	588.2***
	[0.000]	[0.006]	[15.43]	[215.7]
<i>freeride</i> * 2007	-0.0004	-0.0009	56.67	53.31
	[0.001]	[0.001]	[70.72]	[71.48]
<i>freeride</i> * 2008	-0.001	-0.001	327.2***	314.77***
	[0.002]	[0.002]	[87.60]	[89.05]
<i>controls</i>	no	yes	no	yes
N	3,727	3,727	3,723	3,723
R ²	0.01	0.03	0.03	0.03

Notes: All the models include year and municipality fixed effects. The control variables include taxable income, corporate income tax revenue, grants and population. Standard errors are clustered at the municipality level and reported in brackets. ***, ** and * indicate statistical significance at 1, 5 and 10 percent level, respectively.

4.3 Testing alternative explanations

Next we turn to alternative explanations. First, there has been some discussion in the literature on whether the *freeride* is the relevant measure when it comes to free-ride incentives in merger situations. For example, Hansen (2012) uses the number of municipalities in a merger as a measure of free-ride incentives. He argues that the relevant measure of free-riding incentives is the number of decision-makers, which in our case is the number of municipalities, not the

relative population sizes of municipalities. This is also what the result by Weingast et al. (1981) is indeed about. However, the law of $1/n$ arises only because the districts in their model are of equal size, and thus, the share of the cost burden for each district follows the law of $1/n$. When districts or in our case municipalities are asymmetric in size within a merger, free-riding incentives, i.e. the share of cost burden, depend on the relative size of municipalities. Nevertheless, we subject these two measures to a horserace in Table 4.²³ Even though the measures are highly correlated, it becomes evident from columns (2), (5), (9) and (12) that the municipalities respond according to their relative size to the common pool rather than according to the number of partners.

Second, municipalities that expect to lose most of their political representation in the post-merger council may want to spend as much as possible when they can still make autonomous spending decisions. This is because in the post-merger council bargaining considerations, in the spirit of Baron and Ferejohn (1987 and 1989), start to play a role. The results in Tables 2 and 3 could be due these considerations, because the free-ride measure is highly correlated with expected loss of power. Fortunately, the horserace between the number of merging partners and the *freeride* provides also a good test between this loss of power hypothesis and the law of $1/n$. Expectations over the relative political power of a pre-merger municipality in the post-merger council are actually increasing in the number of municipalities for a given value of *freeride*.²⁴ Thus, debt accumulation should decrease with the number of municipalities in a merger given *freeride* if concern over loss of power is driving the results. Therefore, the results in Table 4 show that

²³ For comparison, columns (1) and (8) of Table 4 report the benchmark results from Table 2.

²⁴ In proportional elections, from the point of view of a small municipality, a merger with a single much larger partner and a merger with two roughly same sized municipalities can be very similar in terms of population shares or *freeride*. However, these mergers are different when it comes to post-merger political power. Proposal power, coalition formation and bargaining possibilities are very different when councillors from a small pre-merger municipality negotiate with two equal sided municipalities compared to one much larger municipality.

expectations over the distribution of post-merger political power do not seem to be a relevant consideration during the pre-merger phase.

Third, another potential alternative explanation is spending the merger subsidy beforehand. On the one hand, once the merger has been agreed upon the merger subsidy simply increases the size of the common pool. On the other hand, a concern is that municipalities may simply respond to an increase in future income and not to the free-riding incentives. The first potential issue is that all the merged municipalities receive subsidies whereas the other municipalities do not. However, previously we have directly controlled for merging and also relied on within merger differences in free-riding incentives to identify our key results. The second potential issue is that the size of the subsidy to each merger is highly correlated with our free-riding measure, because the subsidy depends on the populations of the pre-merger municipalities, the population of the new municipality and the number of municipalities in the merger. Based on columns (3), (6), (10) and (13) of Table 5, we can overrule this alternative explanation because controlling for the amount of subsidy does not change the point estimates of *freeride*. However, we cannot entirely rule out that the subsidy may play an independent role in decreasing cash reserves. It should also be pointed out that the total amount of subsidies paid to these mergers (217 million Euros) was only about half of the total free-riding we observe.

Fourth, instead of free-riding, our findings could be due to Coasean transfers between the merging municipalities. This case arises when a merger increases the welfare of the merger partners as a whole, but decreases the welfare of some of the individual partners. If the benefits are large enough, the winners may want to buy the approval of the losers and debt may be the mechanism to achieve this credibly. However, this explanation does not seem plausible because the municipalities with strong free-riding incentives are on average poorer than merging municipalities with a weaker free-riding incentive, and thus, do not seem to be attractive partners (see Figure 2). In

addition, we test whether municipalities that were reluctant to merge accumulate more debt. We measured this reluctance using the vote share of councillors in favour of the merger in the municipality council merger votes. The vote share does not have a direct effect on debt or cash (columns (4) and (11)) and controlling for it does not change the results concerning free-riding behaviour (columns (7) and (14)).

Table 4. Tests for alternative explanations

Panel A: Debt stock	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<i>freeride</i> * 2007	176.1*				204.1	46.89	119.9
	[102.3]				[196.5]	[161.2]	[152.1]
<i>freeride</i> * 2008	569.7***				510.6**	434.8**	644.2***
	[131.9]				[254.6]	[193.1]	[201.0]
<i>coalition size</i> * 2007		21.56			-5.312		
		[13.50]			[26.37]		
<i>coalition size</i> * 2008		78.53***			11.12		
		[17.95]			[35.43]		
<i>subsidy</i> * 2007			0.358*			0.300	
			[0.196]			[0.307]	
<i>subsidy</i> * 2008			0.851***			0.312	
			[0.244]			[0.358]	
<i>voteshare</i> * 2007				143.6*			62.27
				[79.89]			[115.3]
<i>voteshare</i> * 2008				355.4***			-82.5
				[103.261]			[145.1]
N	3634	3634	3634	3634	3634	3634	3634
R ²	0.43	0.43	0.43	0.43	0.43	0.44	0.43
Panel B: Cash reserves	(8)	(9)	(10)	(11)	(12)	(13)	(14)
<i>freeride</i> * 2007	-75.46				-10.92	-11.555	-81.54
	[66.83]				[105.3]	[67.65]	[90.92]
<i>freeride</i> * 2008	-328.4***				-268.1	-164.3*	-272.2**
	[83.92]				[184.9]	[87.77]	[119.2]
<i>coalition size</i> * 2007		-13.41			-11.99		
		[8.358]			[11.75]		
<i>coalition size</i> * 2008		-46.21***			-11.17		
		[9.601]			[23.35]		
<i>subsidy</i> * 2007			-0.163			-0.148	
			[0.137]			[0.163]	
<i>subsidy</i> * 2008			-0.591***			-0.383**	
			[0.152]			[0.169]	
<i>voteshare</i> * 2007				-49.711			6.763
				[55.334]			[73.36]
<i>voteshare</i> * 2008				-249.6***			-61.36
				[70.887]			[97.99]
N	3717	3717	3717	3717	3717	3717	3717
R ²	0.05	0.05	0.05	0.05	0.05	0.05	0.05

Notes: All the models include year and municipality fixed effects. All the models include the following control variables: taxable income, corporate income tax revenue, grants and population. Standard errors are clustered at the municipality level and reported in brackets. ***, ** and * indicate statistical significance at 1, 5 and 10 percent level, respectively.

4.4 Additional validity checks

The fact that the mergers in our data were not randomized raises the possibility of selection bias. In this section, we report additional validity checks concerning the causal interpretation of the DID results. First, we test formally for the common pre-trends assumption concerning our main outcomes of interest using placebo treatment periods. These results are reported in Table 5. To focus the analysis around the placebo treatments, we use only 2 control and 2 treatment period years. In each row, we report a parameter estimate for the *freeride* variable from a single regression, while varying the treatment period definition across rows. The placebo regressions largely confirm the common pre-trend assumption. This is not surprising given the trends we observe in Figure 1. The only concern is that some effects seem to take place already at year 2006. As mentioned earlier, some mergers were decided already very early on in 2006 and we may be observing real free-riding effects already at the end of 2006.²⁵ In the last rows of both panels, we also report the real treatment period result with this shorter panel to alleviate potential concerns of statistical inference raised by Bertrand et al. (2004).

We report the placebo regressions for the operating margin and investment results in Table A1 in the Appendix. The results for the operating margin verify the common trends assumption. However, the results concerning the investment decisions are more problematic because we observe some statistically significant placebo effects. Thus, the investment results should be addressed with more caution than our main results on debt and cash.

²⁵ One way to deal with the attenuation bias caused by contamination would be to compare the mergers based on the distance in time before and after each decision date instead of across years. However, the statistics are available only on annual basis, and therefore, the current approach is more attractive.

Table 5. Placebo treatments for debt stock and cash reserves.

Analysis period	Treatment	Coeff.	Std. Err.
Panel A: Debt stock			
2000–2003	<i>freeride</i> * 2002	-28.47	45.89
	<i>freeride</i> * 2003	-57.39	64.11
2001–2004	<i>freeride</i> * 2003	-41.94	44.48
	<i>freeride</i> * 2004	-122.6	76.62
2002–2005	<i>freeride</i> * 2004	-97.61*	57.47
	<i>freeride</i> * 2005	-48.40	87.90
2003–2006	<i>freeride</i> * 2005	0.541	62.73
	<i>freeride</i> * 2006	157.0	100.1
2004–2007	<i>freeride</i> * 2006	177.3**	76.03
	<i>freeride</i> * 2007	248.1***	91.64
2005–2008	<i>freeride</i> * 2007	138.4**	61.22
	<i>freeride</i> * 2008	504.3***	108.3
Panel B: Cash reserves			
2000–2003	<i>freeride</i> * 2002	-41.99	76.73
	<i>freeride</i> * 2003	-42.26	74.94
2001–2004	<i>freeride</i> * 2003	-22.47	48.64
	<i>freeride</i> * 2004	-87.52*	48.81
2002–2005	<i>freeride</i> * 2004	-58.39*	34.43
	<i>freeride</i> * 2005	-55.62	39.19
2003–2006	<i>freeride</i> * 2005	-15.04	32.95
	<i>freeride</i> * 2006	10.79	43.75
2004–2007	<i>freeride</i> * 2006	33.97	41.14
	<i>freeride</i> * 2007	-26.64	74.83
2005–2008	<i>freeride</i> * 2007	-50.87	69.88
	<i>freeride</i> * 2008	-286.9***	83.17

Notes: All the models in the table use four years of data with two control period years and two treatment period years. The models include the following control variables: taxable income, corporate income tax revenue, grants and population. Standard errors are clustered at the municipality level. ***, ** and * indicate statistical significance at 1, 5 and 10 percent level, respectively.

Second, we use an alternative control group, which might be more similar to the treatment group (the mergers) in terms of unobservables. In Figure 4, we have divided the non-merged municipalities into those that never considered any particular merger seriously enough to vote on it and to those that actually voted for a merger, but the merger did not subsequently take

place. The pre-treatment trends of per capita cash reserves look similar, although there are differences in levels. The pre-treatment trends of per capita loan stock for the voted-group look more similar to those of the merger group. However, for both outcomes, only the merger group shows any response to the treatment.

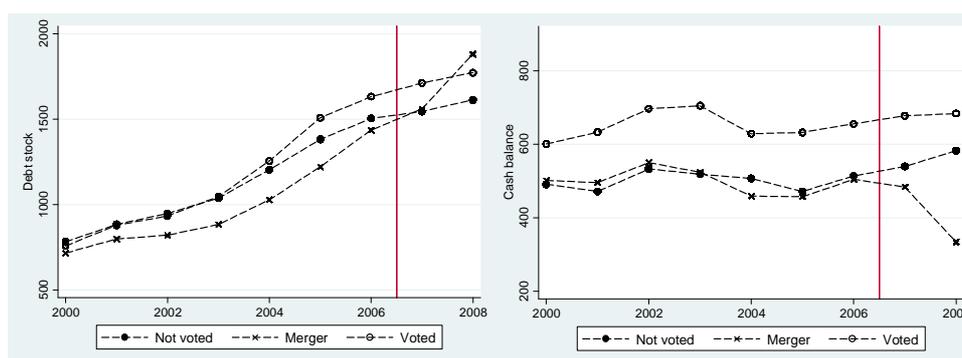


Figure 4. Debt stock and cash reserves in two different non-merger groups compared to all mergers (€per capita).

Third, we repeat our main analysis using only the municipalities that merged. These results are reported in panel B of Table A1 in the Appendix. The results are almost identical to those in panel A of Table 2, as expected. These results are therefore valid within the merger sample and cannot be driven by sample selection issues related to the merger decision. Of course, this does not rule out potential endogeneity issues related to *freeride*. However, the previously reported common trends also with respect to *freeride* are comforting in this respect.

Based on our main results and these additional validity checks, the results seem to be internally valid. However, one could be concerned about generalizability of results that are derived from 99 municipalities in a single year that was an unusual period in the developed world due to the financial crisis. However, the mergers in our data involve about every fourth Finnish municipality and for the Finnish municipalities, year 2008 was business as usual (see Figure 2). Moreover, the mergers were decided already in 2006 and

2007. At the very least, the results are likely to generalize to countries with similar institutional features, since the severity of this problem depends especially on the extent of fiscal autonomy of local governments. Free-riding should also depend on the time lag between the decision and the actual implementation of a merger.

5 Conclusions

In this paper, we analyse free-riding behaviour of Finnish municipalities during a recent wave of municipal mergers using DID methods. Consistent with the law of $1/n$, the stronger free-riding incentive a municipality faced the more it increased per capita debt and used up its cash reserves prior to merging. We also find that the funds were spent mostly on new investment and on current expenditures. Municipalities did not lower their local income tax rate nor did they hire new employees.

The results are somewhat surprising because these mergers were decided voluntarily at the local level by municipality councils. It would seem plausible that municipalities could pre-empt free-riding on the mergers agreements or that free-riding would lead to a cancellation of the merger. However, this does not seem to be the case. Although the contents of the formal merger agreements suggest that municipalities were anticipating these issues, they seem to have had only limited ability to observe and contract upon free-riding.

These results show that free-riding is a concern not only in forced (see e.g. Hinnerich 2009), but also in voluntary mergers. Therefore, our results should be of wider interest, not only because mergers are used extensively in various countries, but also because common pool problems are present in many other contexts, such as (local) governments bail outs.

Besides providing a test of the law of $1/n$, our results suggest that local jurisdiction mergers are likely to involve a substantial (one-time) cost due to free-riding. This leads to two policy implications. First, during a merger

process, some financial constraints on the local level may be a good idea to mitigate common pool problems. Second, for the merger policy to achieve the goal of decreasing public spending without reducing service quality, the scale economies need to be fairly large. Some recent evaluations suggest quite large decreases in expenditures (see e.g. Reingewertz, 2012 and Blom-Hansen et al. 2014). However, Moisio and Uusitalo (2013) do not find any expenditure effects from the previous Finnish merger wave during the 1970's. Due to the institutional constraints on reducing municipal employment after mergers, the analysis of the effects of the mergers studied here is not yet interesting, and thus, is left for future work.

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Appendix. Additional results

Table A1. Alternative specifications.

Panel A: Discrete treatment				
	Debt stock	Debt stock	Cash reserves	Cash reserves
<i>constant</i>	765.6*** [22.94]	1005*** [372.0]	517.5*** [15.57]	-838.3** [359.3]
<i>merger* 2007</i>	108.26 [67.56]	95.87 [67.58]	-44.92 [48.16]	-48.17 [48.17]
<i>merger* 2008</i>	357.6*** [87.97]	336.9*** [88.04]	-234.0*** [59.23]	-222.7*** [60.92]
<i>controls</i>	no	yes	no	yes
N	3,634	3,634	3,717	3,717
R^2	0.42	0.43	0.02	0.05
Panel B: Continuous treatment within mergers				
	Debt stock	Debt stock	Cash reserves	Cash reserves
<i>constant</i>	724.6*** [55.57]	791.83 [888.6]	502.3*** [34.37]	-49.45 [354.7]
<i>freeride* 2007</i>	292.7 [184.9]	274.8 [187.6]	-147.5 [150.0]	-166.5 [166.0]
<i>freeride* 2008</i>	806.4*** [226.0]	786.2*** [232.3]	-395.6** [179.2]	-409.3** [197.2]
<i>controls</i>	no	yes	no	yes
N	864	864	890	890
R^2	0.46	0.46	0.05	0.08

Notes: All the models include year and municipality fixed effects. The control variables include taxable income, corporate income tax revenue, grants and population. Standard errors are clustered at the municipality level and reported in brackets. ***, ** and * indicate statistical significance at 1, 5 and 10 percent level, respectively.

Table A2. Placebo treatments for operating margin and investment.

Analysis period	Treatment	Coeff.	Std. Err.
Panel A: Operating margin			
2000–2003	<i>freeride</i> * 2002	13.74	15.40
	<i>freeride</i> * 2003	0.829	19.38
2001–2004	<i>freeride</i> * 2003	-6.724	17.41
	<i>freeride</i> * 2004	-2.629	21.75
2002–2005	<i>freeride</i> * 2004	-0.121	17.89
	<i>freeride</i> * 2005	5.998	27.58
2003–2006	<i>freeride</i> * 2005	8.899	23.55
	<i>freeride</i> * 2006	-27.58	27.16
2004–2007	<i>freeride</i> * 2006	-32.98	22.49
	<i>freeride</i> * 2007	-69.59**	34.57
2005–2008	<i>freeride</i> * 2007	-53.28*	31.42
	<i>freeride</i> * 2008	-135.9***	45.95
Panel B: Investment			
2000–2003	<i>freeride</i> * 2002	73.35	74.09
	<i>freeride</i> * 2003	-49.04	48.38
2001–2004	<i>freeride</i> * 2003	-75.17	45.95
	<i>freeride</i> * 2004	-64.31	57.16
2002–2005	<i>freeride</i> * 2004	-41.26	58.32
	<i>freeride</i> * 2005	119.9*	64.60
2003–2006	<i>freeride</i> * 2005	174.8**	69.52
	<i>freeride</i> * 2006	227.5***	78.43
2004–2007	<i>freeride</i> * 2006	128.9*	77.95
	<i>freeride</i> * 2007	25.00	89.16
2005–2008	<i>freeride</i> * 2007	-62.57	78.62
	<i>freeride</i> * 2008	213.1**	97.59

Notes: All the models in the table use four years of data with two control period years and two treatment period years. The models include the following control variables: taxable income, corporate income tax revenue, grants and population. Standard errors are clustered at the municipality level. ***, ** and * indicate statistical significance at 1, 5 and 10 percent level, respectively.

Table A3. Results from log-specifications.

Panel A: Discrete and continuous treatment				
	Debt stock	Debt stock	Cash reserves	Cash reserves
<i>constant</i>	6.279***	6.529***	5.358***	3.237***
	[0.032]	[0.573]	[0.044]	[0.502]
<i>merger* 2007</i>	0.144	0.113	0.125	0.086
	[0.183]	[0.186]	[0.206]	[0.211]
<i>merger* 2008</i>	0.089	0.055	0.105	0.08
	[0.193]	[0.197]	[0.297]	[0.301]
<i>freeride* 2007</i>	0.174	0.204	-0.294	-0.23
	[0.286]	[0.289]	[0.271]	[0.276]
<i>freeride* 2008</i>	0.622**	0.657**	-1.126**	-1.038**
	[0.299]	[0.302]	[0.442]	[0.446]
<i>controls</i>	no	yes	no	yes
N	3,631	3,631	3,702	3,702
R ²	0.28	0.29	0.05	0.06
Panel B: Continuous treatment				
	Debt stock	Debt stock	Cash reserves	Cash reserves
<i>constant</i>	6.279***	6.512***	5.358***	3.220***
	[0.032]	[0.573]	[0.044]	[0.501]
<i>freeride* 2007</i>	0.343**	0.337**	-0.147	-0.129
	[0.143]	[0.143]	[0.129]	[0.128]
<i>freeride* 2008</i>	0.727***	0.722***	-1.003***	-0.944***
	[0.150]	[0.150]	[0.211]	[0.212]
<i>controls</i>	no	yes	no	yes
N	3,631	3,631	3,702	3,702
R ²	0.28	0.29	0.05	0.06

Notes: All the models include year and municipality fixed effects. The control variables include taxable income, corporate income tax revenue, grants and population. Standard errors are clustered at the municipality level and reported in brackets. ***, ** and * indicate statistical significance at 1, 5 and 10 percent level, respectively.