

Do Politicians Free-Ride? An Empirical Test of a Common Pool Problem*

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Abstract

In the twentieth century, there was a significant trend towards larger local governments through boundary reforms in the western world. Boundary reforms provide potential economic benefits but might also give rise to costs driven by opportunistic political behavior. This study uses a compulsory reform in Sweden to test for such behavior. The reform gives a local government incentive to accumulate debt before the merger takes place, since the cost will be shared by all tax payers in the new locality. The strength of this incentive to free ride will be determined by the locality's population size, relative to the future size of the new locality. I find an economically large and statistically significant free-riding effect.

Keywords: amalgamations; annexations; boundary reforms; common pool, difference-in-difference; fiscal policy; free riding; local governments.

JEL codes: D72, E62, H70

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1. Introduction

In the twentieth century, many countries have created larger local governments through boundary reforms or annexations.¹ In Sweden, local governments have been subject to two major boundary reforms in the last sixty years, which have decreased the number of local governments from around 2500 to less than 300. Large scale reforms have been carried out in many other countries such as Canada, Great Britain, Germany, Norway, Australia, New Zealand and recently in Denmark. In the U.S., annexations have frequently been implemented in single-function units of governments. For example, the number of independent school district governments declined from 67,355 in 1952 to 14,721 in 1987,² and Trueblood and Honadle (1994) report over 75 000 annexations of municipalities with a population size over 2500 inhabitants between the years 1980-1989. Moreover, redistricting is a recurrent feature in U.S. politics. Mergers of local governments are still regarded as attractive, where the new proposal of creating even larger regional units in Sweden serves as an example.³ Thus, boundary reforms are a common phenomenon.

The arguments advanced by the proponents of larger local governments are centered around economics of scale in service production and the administration of government, economics of scope and other more subtle "bigger is better"-arguments.⁴ Yet, previous studies provide little empirical evidence in favor of increased efficiency.⁵

While there are numerous of studies of evaluating the effects *after* a boundary reform, there are to my knowledge no studies investigating the effects of a boundary reform

¹ See Sancton (2000) for a comprehensive study on the subject.

² This was primarily the result of a state-imposed consolidation of rural districts. See Nelson (1990) for a discussion.

³ See the Swedish government official reports, SOU (2007:10).

⁴ See Dollery et al. (2006) for a discussion.

⁵ The evidence is clearly mixed and many studies point out that mergers of local governments in fact seem to increase the costs. See Bish (2001) for a comprehensive survey on this matter regarding USA and Canada. See Dollery and Crase (2004) and Byrnes and Dollery (2002) for results and references regarding mergers in Australia. See Groes (2005) for studies in Denmark. For mixed results with regard to Sweden, see Nelson (1992) and Hanes (2003).

before it is executed.⁶ Mergers of local governments may also have potential drawbacks before the merger occurs, as a forward-looking politician may then borrow and overspend since the pool of future taxes can be viewed as a common pool. In addition, the incentive to free ride on the common pool of future taxes is stronger, the smaller a locality's population size is relative to the size of the new locality. Hence, the increase in per capita debt due to mergers should be larger for relatively small than for relatively large localities.

The contribution of this paper is to empirically quantify the size of the free-riding effect caused by boundary reforms. To this end, I use a very large boundary reform in Sweden which reduced the number of local governments from 848 to 278 over the period 1969-1974 and where 94 % of all local governments were subject to the reform. There are a number of features, other than the very large number of mergers, which make this reform attractive from an econometric evaluation point of view. First, the localities were forced to merge by national law and the law imposed variations in the incentives to free ride.⁷ Therefore, it is possible to implement a difference-in-difference approach when evaluating the impact of the reform on the behavior of local governments. Second, some local governments merged before others, which can be used to provide additional evidence on the free-riding behavior, since those that merged later, should have more time to exploit the common pool. Third, the reform was largely based on measurable characteristics, which makes it possible to test for whether the reform was "as-good-as" randomly assigned. In other words, controlling for these characteristics should have little effect on the estimated free-riding effect, if our empirical strategy is reasonable. Fourth, Swedish local governments have a large degree of fiscal freedom.

I find strong support for boundary reforms giving rise to a common pool problem. The estimated free-riding effect is both statistically and economically significant. A local government with a very strong incentive to free ride increases its per capita level of debt by approximately 26 % more than a local government with no incentives to free ride, everything else equal. On average, the effect is about 17 %. Moreover, local governments

⁶ Independently of this paper, Jordahl and Liang (2006) have recently studied a previous boundary reform in Sweden. However, the implementations and the nature of the reforms are very different. See Wångmar (2003) for a discussion of the details of Swedish boundary reforms.

⁷ Specifically, the national government imposed by law which local governments that were subject to merger. Moreover, it decided how the local governments were to be matched.

with more time to free ride increase their debt more. The results are robust to a number of specification checks.

This paper is related to, but distinct from, several other literatures that analyze various types of "common pool" problems, that is, the tendency to over-exploitation when multiple users make independent decisions on how much to exploit a common resource such as current and future tax revenues. The idea of an increasing common pool problem as the number of decision makers increases permeates many questions in economics and political science. There is one literature that deals with the impact of legislature size on total spending.⁸ Another literature deals with the issue why multiparty governments might be expected to spend more than single-party governments.⁹ A third literature analyzes why proportional representation electoral systems might lead to more spending than majoritarian systems.¹⁰ Finally, the literature on weak budget constraints and budgetary rules provides similar situations.¹¹

The outline of this paper is as follows. The next section develops a basic common pool model. The third section provides information and a background to the boundary reform. In the fourth section, data and the empirical framework are discussed. The main results are presented in section five. Section six extends the empirical analysis and robustness checks are carried out. Section seven concludes the paper.

2. A Simple Model of the Common Pool Problem in Boundary Reforms

This section develops a stylized common pool model in line with Persson and Tabellini (1994). The purpose of the model is to motivate the empirical analysis, that is, derive a relationship between the level of debt in a local government and the incentive to free ride due

⁸ The idea of common pool problems in politics goes back to Tullock (1959) and Buchanan and Tullock (1962) and is formalized in Weingast et al. (1981). For more recent studies see, for example, Gilligan and Matsusaka (1995, 2001), Baqir (2002), Bradbury and Crain (2001), Perotti and Kontopoulos (2002), DelRossi and Inman (1999) and Pettersson-Lidbom (2007).

⁹ See, for example, Persson et al. (2007).

¹⁰ See, for example, Persson and Tabellini (1999) and Lizzeri and Persico (2001).

¹¹ Kornai (1979) provides a theory for soft budget constraints. For a survey on the literature of budgetary institutions, see Persson and Tabellini (2002).

to the boundary reform. Debt is introduced to fund spending in the first period in order to generate a relation between debt and the strength of the incentive to free ride.¹²

Consider a setting consisting of local governments $i = 1, 2, \dots, Q$ that will merge in the future. The population size of a locality is N^i and $\sum_{i=1}^Q N^i = N$. In the first period, the local governments are independent. In period two, the local governments are forced to merge, which is publicly known in period one. Individuals have quasi-linear preferences over private utility according to

$$u^i = c_1^i + c_2^i + V(x_2^i), \quad (1)$$

where c_t^i is private consumption in period $t = 1, 2$ and x_2^i is leisure in period two for an agent i and $V(\cdot)$ is well behaved.¹³ Individuals are identical, except for the geographical parameter i . For simplicity, we assume that individuals' only way of saving is through lending to the local government in the first period, where the debt is honored in the second period. Let τ be the tax rate in period two. R is the gross interest rate, b^i is the per capita holding of debt and e^i is initial endowment. l^i is labor $l^i + x^i = 1$. In period one, the consumer chooses the level of saving and in the second period, the consumer chooses leisure and consumption. In the absence of discounting and given the linearity of the utility function, we must have $R = 1$ for an interior solution $b^i \in [0, e^i]$. Hence, the intertemporal budget is:

$$c_1^i + c_2^i = e^i + (1 - \tau)l^i. \quad (2)$$

¹² I will assume universalism. The structure of the reform, where all local governments do have the full power of the spending decisions, gives at hand that this is a reasonable assumption. In fact, the strong underlying assumptions of the decision procedure in the standard common pool model resemble the institutional setting in this case.

¹³ I follow Persson and Tabellini (2002) by first solving for the private equilibrium and then introducing the political dimension. We could introduce a leisure choice also in the first period, with qualitatively the same results.

Maximizing (1) w.r.t. x_2^i s.t. (2) yields the necessary conditions for a private equilibrium and the optimal labor supply function:

$$l^{i*} = 1 - x_2^{i*} = 1 - V_{x_2^i}^{-1}(1 - \tau) \equiv L^i(\tau). \quad (3)$$

Now, consider the public spending dimension of the problem. In the first period, local governments act independently. In the second period they merge, so the debt of the old independent unit is inherited by the new. The local government i can only borrow to provide public goods, g^i , in the first period and thus, the budget constraint in period one is simply $N^i g^{i'} = N^i b^i \Leftrightarrow g^{i'} = b^i$. For period two, the budget constraint, imposing $R=1$, $l^{i*} = L^i(\tau)$ and assuming that public good provision is zero in the second period, is

$$\sum_{i=1}^Q N^i b^i = N \tau l^{i*} = N \tau L^i(\tau) \Leftrightarrow \sum_{i=1}^Q \frac{N^i}{N} b^i = \tau L(\tau). \quad (4)$$

Since the tax rate determines the private equilibrium and by (4), we can describe the private equilibrium in the policy variables $\sum_{i=1}^Q \frac{N^i}{N} b^i \equiv B$, and then the private indirect utility function can be written

$$u^* = \max [c_1^i + c_2^i + V(x_2^i)] = e + (1 - \tau)L^i(\tau) + V(1 - L^i(\tau)) \equiv W(B). \quad (5)$$

Preferences for public spending are assumed to be

$$w^i = W(B) + H(g^i) = W(B) + H(b^i), \quad (6)$$

¹⁴ $L_\tau^i(\tau) = \frac{1}{V_{x_2 x_2^i}(x_2^i)} < 0$. Thus, higher tax rate distorts the labor-leisure choice.

¹⁵ Note that $u_\tau^* = -L(\tau) + L_\tau(\tau)(1 - \tau) + V_L L_\tau(\tau) < 0$ and we must have $W_B(B) \leq W_{b^i}(B) < 0$, and $W_{b^i}(B) = W_B(B) \frac{N^i}{N} < W_{b^j}(B) = W_B(B) \frac{N^j}{N}$ if $N^i > N^j$ for $i \neq j$.

where $H(\cdot)$ is concave and well behaved. Solving backwards, imposing repayment of the debt in period two, we have the optimal condition as $B = \tau L(\tau)$ in period two. In period one, incumbent i maximizes (6) w.r.t. g^i . Hence, the necessary conditions are

$$H_g(b^i) = -\frac{N^i}{N}W_B(B) < -W_B(B), \quad (7)$$

where $H_g(b^i)$ is the marginal utility of the good provided and $-\frac{N^i}{N}W_B(B)$ is the local marginal cost of borrowing. Note that this is smaller than the marginal cost of borrowing for the single locality, $-W_B(B)$.

Thus, spending on public goods in the first period and thus borrowing are too high compared to the social optimal level of borrowing, which is the classical common pool result and the smaller the relative size, the larger are the incentives to borrow, everything else constant. To summarize, the model implies that borrowing should be negatively related to $\frac{N^i}{N}$ where N^i is the current population size of locality i and N is the population size of the future unit.

3. The Swedish Boundary Reform

This section discusses the background to the Swedish boundary reform that reduced the number of local governments from 848 to 278 during the period 1969 to 1974. Moreover, some important features of Swedish local governments are presented.

In 1959, a committee of experts was appointed by the Swedish parliament to analyze the effectiveness of local governments.¹⁶ The committee concluded that most local governments were too small to provide efficient services and suggested a boundary reform as a solution. Moreover, the reform should be guided by the principle that the new localities were to be constructed around economic/geographical centers, i.e., places with a dense population. In addition, they should also be expected to have at least 8000 inhabitants in the long run and they should also be more dispersed with respect to occupational character. The

¹⁶ See the Swedish government official reports, SOU (1961) and the committee on the constitution reports, KU (1962).

logic was that the agricultural sector's part of the economy was decreasing in favor of the manufacturing sector and, in particular, the services sector. The committee considered both the tax base and the tax rate and argued that a harmonization between local governments was desirable. However, initial geographic factors could not be stretched too far. A locality from the southern part of Sweden could not, in practice, merge with a counterpart from the northern part, i.e., a locality should not be geographically divided. This meant that a large locality that was situated in between small localities might be subject to the boundary reform because of its placement; consequently, approximately 96 % of the localities were subject to the reform.

The proposal prescribed voluntary mergers, since it was assumed that the local governments would realize the gains of becoming larger. The national parliament accepted the committee report in general, but added that splitting old localities should be avoided. In the following years, the Swedish government constructed the new units based on the above mentioned principles, the so-called municipality blocks. However, the blocks had no legal meaning. They were theoretical constructs but with their creation, the national government assumed that local governments would merge into these blocks on a voluntary basis. In 1965, Statistics Sweden began to publish statistics for these theoretically constructed units (282 altogether).¹⁷ However, the vast majority of the local governments were reluctant to merge.¹⁸ The Swedish government, led by the Social Democratic Party, was not satisfied with the response of the local governments and decided to make the mergers mandatory but without changing the key elements of the previous proposal.¹⁹

¹⁷ The construction was finished in 1964. 38 old local governments were defined as not being subject to mergers.

¹⁸ Only 35 of the new units had been completed in 1969. Nevertheless, more than 35 mergers took place in the period; 290 localities that existed in 1962 disappeared within the voluntary period and 119 new localities were created, but out of these 119 only 35 reached the complete status.

¹⁹ See the Swedish government proposal, Prop (1969). However, the number of municipality blocks dropped by 7 to 275 and some minor changes were made. Protests from local governments were common but in general, they did not have any success when objecting to the law. See Nielsen (2003) for a summary or, specifically, Wallin (1973). Yet, a few local governments were successful in their protests and did avoid merger. In comparison with the publications of 1965 Dals-Ed, Svedala, Staffanstorp, Bara, Burlöv, Mullsjö, Orsa,

The law of 1969 forced the local governments to merge in line with the constructed municipality blocks before the beginning of 1974. However, it was up to the local governments in the specific block to decide whether to merge at the end of 1970 or at the end of 1973, henceforth denoted 1970/71 and 1973/74. In 1969, Sweden consisted of 848 local governments. In 1970/71, 525 local governments merged into 141 new units. So at the beginning of 1971 there were 464 local governments. At the end of 1973, another 268 merged into 82 new units. At the beginning of 1974, Sweden consisted of 278 local governments.

In addition to the extensive boundary reform, Swedish local governments have attractive features for studying the common pool problem. First, Swedish local governments have a constitutional right of self governance, within a given set of rules and national laws, i.e. they are general-purpose governments.²⁰ They can decide on income taxation, planning of local land, impose some local fees and freely take up short-term debt and the local governmental economy is a substantial part of the Swedish economy. In 1970, the share of GDP, measured as consumption and investments, was approximately 17 %, a share that has remained rather stable ever since.²¹ Second, and perhaps most important, all local governments are governed by the same laws and institutions, which makes them favorable as units of analysis. Thus, the national government sets up the rules of the game and, as in the case of the boundary reform, it did not care about the wishes of the local governments, even if the protests were both strong and numerous.

Turning to the outcome of interest, there were some restrictions on the use of long-term debt until 1979, however. A local government could not take up long-term debt for consumption and the loan should be aimed at accumulating long-term assets. Moreover, the loan was to be used to acquire resources regarded to be in the local political sphere, i.e. building schools or sports stadiums but not military investments, for example. In general, the loan had to be approved by the central government but as long as the principles mentioned

Sundbyberg, Solna, and Håbo avoided merger. Moreover, 6 localities that were regarded as sufficiently large in 1965 actually merged. Thus, 42 units are stable over the whole period.

²⁰ This is true also in practice, which has been shown in several studies. See, for example, Pettersson (2001, 2007).

²¹ This share also includes county council ("landsting") activities.

were fulfilled, the central government could not disapprove and there is no evidence of it ever denying an application.²²

To sum up, the boundary reform has its origins already in 1962 but was voluntary until 1969. Approximately, only 4 % of all local governments in 1964 were regarded as not having to merge but few chose to merge. In 1969, the Swedish government decided on mandatory merger. The decision was taken with a slim majority in the Swedish Parliament. A majority of the local governments also protested but very small changes were made to the theoretically constructed municipality blocks of 1964. After the boundary reform had been completed, Sweden had 278 local governments in 1974.

4. Empirical Framework and Data

In this section, I first describe the empirical framework and motivate why a difference-in-difference approach is appropriate. Then, I describe the data used in the subsequent analysis.

The basic common pool model in section 2 suggests that the level of per capita debt of locality i should be regressed on its population size before the merger, relative to the population size of the new unit, $\frac{N^i}{N}$, since it determines the marginal cost of borrowing. For

expositional purposes, let us define the incentive to free ride, $Freeride = 1 - \frac{N^i}{N} \in [0, 1]$.²³

We could then write

$$Debt_i = \alpha + \beta Freeride_i + u_i, \quad (8)$$

where u_i measures all other determinants (observed or unobserved) of debt. The parameter of interest is β - the free-riding effect due to the common pool problem. The Swedish boundary reform provides us with the required variation in *Freeride*. However, there are likely to exist

²² The logic was that the utility of an investment would also be enjoyed by future tax payers. Some exceptions to the general rule of state approval existed, for example for investments in water and sanitary projects. See Gustafsson (1984) and Wallin et al. (1973).

²³ It is convenient to formulate the relationship as positive, since we can then refer to an increase in the coefficient instead of an increase in the magnitude or an increase in absolute value.

unobserved differences across the localities before the reform, which may be correlated with both *Debt* and *Freeride*, and which would bias the estimated effect in equation (8). Thanks to the way in which the boundary reform was implemented, a difference-in-difference (DID) approach can be used. Since merger was not mandatory before 1969, this suggests that the difference between the year before the merger came into effect (i.e., either in 1970 or 1973) can be taken as the final year and the year before the law was imposed (i.e. 1968) as the pre-treatment year would capture the free-riding effect. However, since our model gives at hand that *Freeride* is continuous, equation (8) becomes

$$\Delta Debt_{it} = \theta + \beta \Delta Freeride_{it} + v_{it}, \quad (9)$$

where Δ is the difference operator; either the difference between 1970 and 1968, or between 1973 and 1968 depending on whether a locality merged in 1970/71 or 1973/74. Since the reform only affected the localities after 1969 but not before 1969, $\Delta Freeride = Freeride$. Thus, (9) can be estimated as

$$\Delta Debt_{it} = \theta + \beta Freeride_{it} + v_{it}. \quad (10)$$

We can also include pre-reform characteristics as control variables in (10). These characteristics can be used for testing whether the variable of interest, i.e., *Freeride*, is "as-good-as" randomly assigned. Thus, these controls should not affect the estimated free-riding effect unless the *Freeride* is correlated with the error term. Adding controls is also motivated from an efficiency point of view. An implicit assumption of the difference-in-difference approach is that the localities that were affected by the reform should have had similar trends in the level of debt before 1968 as those localities that were not affected. As a matter of fact, below I present evidence suggesting that they do have similar trends in the level of debt before 1968.

The data set consists of all Swedish local governments from 1962-1974 and is constructed from official statistics provided by Statistics Sweden. I have collected additional data based on the report of 1962. The data set is an unbalanced panel where a local

government that merges disappears and merges into the new unit in the next year.²⁴ Furthermore, in the main analysis, the newly created localities of 1971 are dropped, since the behavior after the merger might be affected by previous opportunistic behavior by the merging partners. For a more detailed description of sources and definitions, see the Appendix.

Table 1 shows a summary of important variables for the 848 local governments that existed in 1969. Except for the variable of interest, *Freeride*, and the dependent variable, $\Delta Debt$, I report 1962 year values for the variables mentioned by the committee, i.e. the variables that were decisive for which localities were subject to merger and the content of the matches. The reported variables are potential controls for testing whether *Freeride* is "as-good-as" randomly assigned. Since we know from the proposal of 1962 which variables were important for the boundary reform, we have very good candidates for that test.²⁵ All nominal values are deflated to 1962 SEK using CPI. The theoretical model does not make a distinction between changes in long-term or short-term debt. Long-term debt is likely to capture most of the free-riding behavior, since it can be used to provide local goods such as sports stadiums and alike. These types of investments provide future utility to the old locality. Given the restriction that long-term debt could not be used for consumption, spending could mainly be financed through taxes or short-term debt. If the free-riding motive is at work, a local government should prefer short-term debt to taxes. However, short-term debt must be honored regularly, which should mitigate the free-riding effect. Yet, we can expect a local government to roll over short-term debt in the very last year before the merger but on the other hand, short-term debt could be hard to exploit for a locality if private companies are credit constrained. I will use the sum of short- and long-term debt, but I will also explore this potential difference in the subsequent analysis section.

In Table 1, I have divided the outcome variable $\Delta Debt$ into two sub groups. $\Delta Debt_{70-68}$ is the change of per capita debt for those localities merging 1970/71 and $\Delta Debt_{73-68}$ is the change of per capita debt for those that merge in 1973/74 or are not subject to the reform. We see a clear difference for these variables between the groups. The localities merging already after two years, 1970/71, accumulate 132 SEK on average, while the other groups consisting

²⁴ It sometimes happens that small parts (i.e. some parishes) of a locality merge into a different unit than the major part of the locality. Since I have no data at the parish level, I classify a merging locality according to its major part.

²⁵ For a locality created in the period 1963-1968, I report the initial values.

of those merging in 1973/74 or not merging at all accumulate approximately 250 % more in debt.

Table 1 also shows that there is substantial variation in the incentive to free ride. *Freeride* has an average of around 0.66 but a standard deviation of 0.32. As previously discussed, the proposal of 1962 aimed at creating larger units centered around economic geographic nodes, where the node is understood as a place with a high population density. Thus, a population density measure is needed. Table 1 reports the percentages living in central locations (cities or small towns), denoted Population density. Yet, the most important factor for the committee was population size and the average locality in Sweden had about 9086 inhabitants in 1962. The huge standard deviation indicates that larger cities are substantially larger than a normal sized locality. Furthermore, the committee argued that the new unit should be more dispersed with regard to occupational character. I include two measures for occupational character: share of the working population in the agricultural sector and alike; and share of the working population in the manufacturing sector and alike. Regarding taxes, the tax base, the tax base per capita as well as the tax rate, are presented, where the tax base is measured as taxable income.

[Table 1 about here]

5. Results

This section first presents graphical evidence. Second, regression results based on (10) are provided. The main reason for the graphical analysis, except for investigating a potential effect, is to provide evidence for parallel trends before the reform. With the regression analysis, we make systematic tests, but also check if *Freeride* is "as-good-as" randomly assigned.

5.1. Graphical Evidence

One way of illustrating the potential effect of a reform, when using the difference-in-difference approach, is to plot the level of the variable of interest over time for different treatment groups. Figure 1 shows the level of per capita debt over the years 1962 to 1973 for the localities that never merge and those merging at some time within the period and corresponds to a binary treatment case. The group that never merges is a discussed above small and consists only of 42 local governments. It starts out with a higher per capita debt

than the large group consisting of mergers (997 vs. 609 SEK). Yet, the trends look rather similar until 1969, when the law that forced local governments to merge was passed. The change in the trend in 1969 indicates an average effect of the reform of about 200 SEK. In relation to the average level of debt in 1968, this is approximately 17 %.

[Figure 1 about here]

However, the free riding effect is not precisely measured by this crude division. We know from the model that localities that merge should have different incentives to free ride. More precisely, the model predicts a monotone positive relationship between the incentive to free ride and the change of debt, not just an effect of the reform as such. Thus, the debt policy of a local government with *very* weak incentives to free ride should be more similar to the policy of a local government that is not merging than to the policy of a local government with strong incentives to free ride, everything else equal. Thus, I argue that in order to believe that the common pool problem is the underlying mechanism explaining the increase in debt, we should find a monotone positive relationship between the incentive to free ride and the change of debt. Let us use the structure that our economic model gives at hand and divide the sample into thirds with respect to the incentive to free ride. "Weak" denotes the local governments with weak incentives to free ride by $Freeride \in [0.0, 0.33]$, "Moderate" are those having $Freeride \in (0.33, 0.66]$ and "Strong" those having $Freeride \in (0.66, 1)$.²⁶ Naturally, in the subsequent regression analysis, the full continuum of *Freeride* is imposed in line with the model but for illustrative reasons, the graphical exposition divides the localities into thirds.

Figure 2 shows clear evidence in favor of the prediction of the model. Local governments with a strong incentive to free ride in fact acquire on average 200 SEK more debt than those with a weak incentive. Evaluated as the share of the average level of per capita debt for all local governments in 1968, i.e. the year before the law, it is approximately 17 %.

[Figure 2 about here]

²⁶ The newly created local governments in 1971 are dropped. However, the figures are similar when these are included.

It is of further interest to divide the localities on basis of the year of merging. In figure 3, the localities are ordered by merging year, 1970/71 and 1973/74, respectively. The hypothesis is that the longer is the period a local government can free ride, the larger is the effect. Moreover, it is reasonable to think that those merging in 1973/74 are the most reluctant to merge, which might influence the magnitude of the free-riding effect. Finally, since investments take some time to plan and since long-term debt must be approved, we expect a smaller effect of the localities that merge already in 1970/71.

Figure 3 shows the expected pattern for both groups.²⁷ The group merging in 1970/71, and which has strong incentives to free ride, acquires on average approximately 100 SEK or 300 % more in debt than the group with a weak incentive. In relation to the average level of debt in 1968, this is approximately 9%. The local governments with strong incentives to free ride that merge in 1973/74 acquire approximately 350 SEK more per capita debt than those with weak incentives. In relation to the average level of debt in 1968, this is approximately 30%.

The fact that the change in debt is larger for the local governments merging in 1973/74 is reasonable as argued above. A local government with a strong incentive, which merges in 1973/74, acquires approximately 100 % more debt than a local government with a strong incentive which instead merges in 1970/71.²⁸

[Figure 3 about here]

Another question is whether the relation between the incentive to free ride and the debt policy is spurious. One way of investigating this is to calculate the change in debt for some period before the reform. I have done this for various periods, but in general no monotone relationship appears. This gives further graphical support for parallel trends before the reform when dividing the groups based on the incentive to free ride. Moreover, this gives

²⁷ The picture is not driven by outliers. Removing these does not change the relation.

²⁸ Note that the increase in debt for the group merging in 1973/74, with weak incentives, is lower than for the group with weak incentives, merging in 1970/71. However, this is driven by the fact that the group with weak incentives that is merging later consists of all local governments with no incentives to free ride. Removing the local governments with no incentives to free ride, the level is higher for those merging later.

indirect evidence against the driving force being a kind of convergence to some optimal level of debt. Figure 4 shows one example of change in debt between the years 1962 - 1968.

[Figure 4 about here]

The graphical analysis shows there to be a strong positive relationship between the incentive to free ride and change of debt in line with the model. Moreover, it is reassuring that graphically, we find evidence of parallel trends. Finally, the effect is potentially economically large. The next subsection further investigates the effects using regression analysis.

5.2. Regression Results

In this subsection, I will use (10) as the baseline specification, presented in table 2. Furthermore, I investigate the potential different effects depending on merging year, the difference in long- and short-term debt and finally also the argument that debt was used to acquire long-term assets. The extended regressions, tables 3 and 4, also include the pre-reform characteristics potentially related to the reform as previously discussed, county-specific effects and intercept.²⁹ Since our model predicts that there should be no difference in the incentive to free ride between a local government that is merging and is extremely large, relative to the new locality, and a local government that is not subject to reform, we include all 848 observations in our main analysis.

The coefficient of the incentive to free ride without any controls shows the expected sign and is highly significant with the point estimate of 298. Thus, a local government with very strong incentives to free ride acquires 298 SEK more per capita debt than a locality with no incentive to free ride. In relation to the average level of debt in 1968, this is approximately 26 %. In 2006 year values, this is about 2850 SEK or 400 USD. Since the average of Freeride is 0.661, the effect evaluated at the mean is approximately 197 SEK, which is roughly 17 % of the level of debt in 1968.

There are some plausible reasons why the effect of the incentive to free ride might differ between the groups that are merging. First, time in treatment might be important. For example, local governments might have difficulties in borrowing as much as preferred in

²⁹ County effects are introduced since a local government must be a consistent area, which means that most mergers took place within the same county.

one year since they could be credit constrained. Moreover, the law was passed in 1969 and the local governments that merge had already passed their budget for that year. Hence, they had in fact only one year to acquire debt, if they did not foresee the law. Thus, a longer time period in treatment might increase the effect. Specification 2 shows the result from including an interaction of *Freeride* with a dummy, which takes the value of one for those merging in 1970/71 and zero otherwise. If the above arguments are true, we would then expect that allowing the early mergers to have a separate effect should increase the point estimate of *Freeride* and result in a negative estimate for interaction. In fact, the point estimate of *Freeride* increases to 504 SEK and the point estimate of the interaction is negative and of the magnitude -323 SEK. Thus, a local government with a very strong incentive to free ride that is merging in 1973/74 acquires 504 SEK more debt than a local government with no incentives to free ride. For the group merging in 1970, the point estimate is only of the size $504 - 323 = 181$ SEK. Given the set up of the reform, I cannot distinguish between the proposed mechanisms that make the free-riding effect smaller for those merging earlier, but we can conclude even if there is clear evidence of a significantly lower effect, the positive relation remains.

As previously discussed, we could potentially expect a larger effect on long-term debt than on short-term debt. In specifications 3 and 4, the results are shown by using the change in short- and long-term debt as dependent variables.³⁰ The incentive to free ride does not determine the change of short-term debt at any reasonable significance levels. Moreover, the point estimate is small, but with the expected sign. However, using long-term debt as the dependent variable, we get closer to the previous estimates and it is also highly significant. Thus, most of the effect is due to an increase in long-term debt.

Our model assumes that the increase in debt is used to provide some type of local public good. It seems likely that a local government about to merge would like to bias its use of debt to long-term investments in order to enjoy utility for some future period. Moreover, since we know that long-term debt cannot be used for consumption, it is reasonable that using a measure for long-term investments should yield similar results. Instead of using $\Delta Debt$, a good candidate would be the change in long-term assets as dependent variable. The reason for not using long term-assets as the main dependent variable in the analysis instead of the change in debt is two-fold. First, there are other ways of

³⁰ There is missing data on long- and short-term debt for four local governments. Only the aggregate is reported. These municipalities are excluded.

directing resources to the locality than through acquiring long-term assets. Second, the way long-term assets are reported is more arbitrary since they are not priced at the market and the borderline between short- and long-term assets is sometimes non-trivial.³¹ This means that we can expect measurement error in long-term assets, and OLS would give larger standard errors. However, it is reasonable that the point estimate should be positive. The last specification uses change in long-term assets as the dependent variable where we use the same definition, i.e. the change from the level of 1968 up to the year of merger. Clearly, the point estimate of *Freeride* has the expected sign and is slightly less significant.³²

[Table 2 about here]

Including all variables related to the reform reported in table 3 does not at all change the interpretation qualitatively and very little quantitatively. This suggests that the difference-in-difference approach is sufficient. If we had seen a large discrepancy between specifications, we would doubt that the difference-in-difference estimator was appropriate. Formally, an F-test that all the coefficients are equal to zero cannot be rejected at any reasonable significance level.³³ The effect is of the magnitude of 249 SEK, which is approximately 21 % of the average level of debt in 1968. Evaluated at the mean of the incentive to free ride, the effect is around 164 SEK, which is approximately 14 % of the level of debt in 1968.

[Table 3 about here]

Including the tax rate might seem dubious, even if it the tax rate 7 years before reform, since it is interlinked with debt by the budget constraint and is clearly a choice variable. The reason for including the pre-treatment tax rate as a control is that the committee considered it when setting up the principles for the boundary reform. Thus, it can potentially be an important selection variable. However, if the tax rate depends on past realizations of the tax rate, this could cause endogeneity. It is reassuring that the point estimates, reported in table 4, do not change qualitatively and very little quantitatively when excluding the pre-treatment tax rate.

³¹ A linear regression of Change in debt on Change in long-term assets yields a positive and significant coefficient of 0.26. R^2 is about 0.12.

³² We lose three observations due to missing data.

³³ The F-statistic takes the value of 0.52 with the corresponding p-value of 0.821.

[Table 4 about here]

The results from the regression analysis correspond to the results from graphical analysis. The difference-in-difference approach seems to be reasonable as the estimates are not altered by adding the variables that influenced the reform. Finally, the effects of free riding are found to be large and significant. The next section, however, further deals with the interpretation of the effects and the robustness of the results.

6. Robustness

We have not previously made any distinction among the local governments that were not affected by the law. However, this group consists of two types, namely those originally never chosen to merge and those 35 that voluntarily completed the merger before 1969. Thus, it could be argued that the completed localities have selected themselves into non-treatment and if this group is non-random with respect to debt policy, it could drive the results. Consequently, I will run regressions on a restricted sample, excluding these. Moreover, Table 5 presents regression results when we restrict the sample to the local governments that are merging only, i.e. excluding both "Completed" and "NA". Last, not all mergers that took place in the voluntary period ended in a complete unit, i.e. included some, but not all, merging partners. Thus, we would also like to run a regression with only the local governments that were totally unaffected by voluntary mergers, i.e. those that were geographically intact within the period 1962-1969.

In specification 1, excluding the completed units, we see a drop in the estimate of the coefficient of *Freeride*. However, the effect remains positive, large and significant. Specification 2 further restricts the sample and only looks at the local governments experiencing a change in the incentive to free ride. The estimates of the incentive are of the same magnitude as in the baseline specification and the effect is still highly significant. In specification 3, we restrict the sample to those that were geographically intact within the period 1962-1969. We lose some more observations, but the point estimates are in a similar range as the baseline specification and highly significant. Specification 4 further restricts the sample, and excludes also the group originally never chosen to merge. Thus, we use the units that were geographically intact and did merge in the period where the law applies. The point estimate remains in the same range and is significant at the 5 % level.

Defining 1968 as the pre-treatment year, might lead us to draw the wrong conclusions if the parallel trend assumption is violated. For example, if local governments with a strong incentive to free ride for some reason have a cyclical debt policy, the pre-treatment year might be crucial for our conclusions. The estimated effect is then not a causal effect of the reform. Prolonging the treatment period is thus a robustness test, in the sense that we investigate how sensitive the analysis is to the choice of pre-treatment year. Specification 5 in Table 5 shows the results of using level of ($Debt_{\text{amalgamation year}} - \text{level of } Debt_{62}$) as dependent variable for those merging in the period where the law applies. It is reassuring that the point estimate remains significant at the 5% level and is in a similar range.³⁴ Moreover, the results are also robust when including the number of merging partners.³⁵

[Table 5 about here]

Moreover, the law was passed with a very slight majority, and with about a half of the local governments protesting, but since expectations cannot be observed, we do not know how far back the law was anticipated.³⁶ Evidence on anticipation could be investigated by postulating a model that takes the 1968 level as the final level of debt and see how much of the differential $Debt_{68} - Debt_{62}$ can be explained by *Freeride*. Naturally, we can vary the differential to $Debt_{68} - Debt_{63}$ or $Debt_{68} - Debt_{64}$ etc. I find no significant effect of *Freeride* on these different types of measures when including the controls. Thus, it is reasonable that the law that forced local governments to merge did not affect debt policy before it was passed. Furthermore, it gives evidence of *Freeride* not being spuriously related to our dependent variable.

³⁴ Note that we lose the municipalities that did not exist in 1962. Including the 42 local governments that were not affected by the law decreases the estimate to 213, but it is still significant at the 6 % level.

³⁵ It might be considered that fewer merging partners could influence the results, holding the incentive to free ride constant, since it might be easier to impose social pressure if the localities were few. However, there is no such evidence.

³⁶ See, for example, Gustafsson (1984).

In addition, I have performed numerous robustness checks but, for brevity, I only discuss the most important.³⁷ With regard to different functional forms, the free-riding effect remains significant at the 1 % level, but drops somewhat in magnitude.³⁸ I have considered the annual change of debt and used different panel estimators such as the fixed and random effects with consistent results. Furthermore, I have removed some outliers such as large cities and also those local governments that have combined the decision body of the local government with the county council.³⁹ In general, the point estimate remains large, positive and highly significant. Figures 1 and 4 did not give us any reason to believe that the effect was driven by a convergence to some optimal level of debt. Thus, it is reassuring that adding the debt level of 1962 to the regressions does not affect the estimates.

Finally and maybe most interestingly, are we capturing the specific free riding mechanism as the model prescribes? As with all reduced form estimation, it is hard to prove a specific mechanism. However, making use of the structure of the model, i.e. using the incentive to free ride, instead of a dummy for those merging, gives us evidence that this is not just any reform specific effect. Yet, there are other types of models that might be the correct underlying process generating data.

Plausible models, that could generate a similar relation of the incentive to free ride and the change in debt are, for example, models related to free riding on the electorate within a locality such as Tabellini and Alesina (1990) and Persson and Svensson (1989) due to changes in the reelection probability.⁴⁰ Being the incumbent in a small local government and having to merge with a large one affects the probability of reelection negatively. In the former model, an incumbent about to loose have incentives to increase spending and debt, if she fears a defeat but party affiliation should not matter. In the latter model, where politicians care about different size of spending, an incumbent will react if she fears being ousted, but the

³⁷ The statistical information can be received from the author upon request.

³⁸ I have tried with both quadratic and cubic forms of all controls.

³⁹ The following municipalities had combined decision bodies: Stockholm, Gothenburg, Malmö, Helsingborg and Gävle in 1962; Stockholm and Gothenburg in 1963-66; Stockholm, Gothenburg and Malmö in 1967-70; Gothenburg, Malmö and Gotland in 1971-74. There are 24 counties in Sweden.

⁴⁰ Pettersson-Lidbom (2001) shows that the model in Tabellini and Alesina (1990) is not consistent with Swedish data on local governments in the period after the reform. However, the model in Persson and Svensson (1989) is consistent with Swedish data on local governments. Moreover, the Swedish elections and party system is described.

sign of the effect will depend on party affiliation. However, different parties will always behave opposite with respect to debt.

To indirectly check whether the mechanism in the model in Persson and Svensson (1989) is captured by *Freeride* we could interact the incentive to free-ride and the party affiliation of the incumbent. I define a dummy to equal 1 if the incumbent is left wing and zero otherwise and both a specific intercept and the interaction is added to the baseline specification. I use the election result from the elections 1966/67. Column 1 in table 6 presents the results. The results show that there is no evidence that parties behave differently and the estimate of *Freeride* is not affected substantially.⁴¹

With respect to the mechanism proposed by Tabellini and Alesina (1990), we need to have a measure on how the reelection probability is altered. One way is to use the election results in the last election and make use of number of votes an incumbent received in relation to the sum of all votes in the match. However, since citizens cast their votes on parties in Sweden, not on individuals, it is reasonable to represent the reelection probability with the sum votes for the incumbent's *party* in the match, divided by number of eligible voters. For example, if the incumbent is a left wing in municipality i then the 1 minus the

probability of reelection is defined as: $1 - P_i = 1 - \frac{\sum_{i=1}^Q \text{Left wing votes}_i}{E}$, where E is the total sum of eligible voters in the match. If politicians sort themselves according to types this seems like a reasonable proxy for the re-elections probability, since it proxies the incumbent party probability of winning. If the mechanism proposed by Tabellini and Alesina (1990) is at work $1 - P_i$ should be positively related to change of debt.

The results are shown in columns 2 in table 6. There is no evidence that debt accumulation is driven by election considerations and estimate of *Freeride* is not affected substantially. This gives indirect evidence that *Freeride* is not capturing effect related to changes in the reelection probability, but as stated previously, we should interpret this finding with caution given the reduced form estimation strategy.

[Table 6 about here]

⁴¹ The number of observations drops somewhat due to unclear majority. An unclear majority is due either to a 50/50 split of the seats or that a non-partisan party holds the balance of power.

7. Conclusion

Boundary reforms of local governments were a frequent phenomenon in the western world in the 19th century, and the trend of creating larger local governments seems to continue with the latest reform in Denmark. Boundary reforms can give rise to costs driven by opportunistic political behavior, however. This paper argues that the extensive reform of 1969, which forced a vast majority of Swedish local governments to merge, gave rise to the same free-riding incentives as in a basic common pool model. I use a difference-in-difference approach to evaluate the effect. Since the reform was based on observable characteristics, we can also check for whether the reform was "as-good-as" randomly assigned. The free-riding effect is significant and large. Evaluated at the mean of the incentive to free ride, the effect is around 197 SEK, which is approximately 17 % of the level of debt in 1968. The local governments that did merge before the deadline ran out also free ride less, which is consistent with the notion of the time in treatment being of importance. In line with the theory, we expect a local government to invest in large projects such as sports stadiums and alike in order to enjoy utility for a long future period, which is confirmed by the study. Moreover, the results are robust and survive numerous specifications and different types of estimators.

Local government representatives thus seem to care about their region and behave opportunistically in the geographical sense, as predicted by theory. This paper shows that when the geographical identity and the common pool of future taxes are clearly defined and when the timing and decision procedure is close to the basic common pool model, politicians behave in line with this. In this sense, the results confirm the fundamental assumed mechanism in the common pool model.

The policy conclusion from this is rather evident and is connected to future research. If larger local governments are more efficient and can enjoy scale economics, the free-riding incentives before a merger can offset some of the potential gains. Having one large stadium in a locality might be efficient if there are scale effects, but not two medium sized ones. Thus, voluntary or more constrained mergers seem to be the most efficient way, given the goal of large efficient local governments. Hence, one of the reasons why previous research shows little evidence of efficiency after mergers might be that the new localities are suffering from sub optimal investments before the reform, which is a natural next step for future research. If larger local governments are not more efficient, the opportunistic behavior before a merger adds an extra cost to the cost-and-benefit analysis of boundary reforms.

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Appendix 1: Statistical Sources and Definition of Variables

The data is collected from the sources: Kommunernas Finanser (The Municipal Finances) (1962-1974) and Årsbok för Sveriges Kommuner (Statistical Yearbook of the Swedish Municipalities), (1962-1974), Statistics Sweden, Stockholm. Information on debt and long-term assets is collected from Kommunernas Finanser 1962-1974. Population, population density, share working in the agricultural and the manufacturing sector and alike, tax rate and tax base are collected from Årsbok för Sveriges Kommuner 1962-1974. Furthermore, Ivarsson (1992) has been useful for determining with what other local government a local government is merging. All nominal terms are deflated by CPI with 1962 as the base year.

Freeride is defined as $1 - \frac{N^i}{N} \in [0, 1)$ where N^i is the population size in local government i and N is the size of the new locality.⁴²

$\Delta Debt_{70-68}$ is the per capita debt level of 1970/71, minus the debt level of 1968.

$\Delta Debt_{73-68}$ is the per capita debt level of 1973/74, minus the debt level of 1968.

$\Delta Debt$ is the per capita debt level in 1973/74 or 1970/71, minus the debt level of 1968.

Population density is measured as the share living in a densely populated area ("tätort"). It is a dichotomous variable divided into eight groups, where the first group A means no population in a densely populated area. B means 0.1- 9.9 % living in a densely populated area and so forth up to the last group H, where all live in a central location. The averages of the intervals of the groups are used. The variable is calculated from census in 1960 and 1965. I use the values from 1960 for those local governments existing in 1962.

Tax rate is the tax rate on income from work that the local government fully decides upon ("borgerlig skatt").

Tax base is the tax base which is the sum of the income that is taxable in the local government.

Tax base per capita is the tax base divided by population.

Population is the population in a local government.

Share in agricultural sector is the share of the working force that worked in the agricultural sector and alike at the time of the census 1960.

Share in manufacturing sector is the share of the working force that worked in the manufacturing sector and alike at the time of the census 1960.

⁴² Sometimes one or some parishes in a municipality do merge into a unit that is different from the unit where the largest part ends up. Since there is limited data at the parish level, I ignore this and classify the merging partners considering the largest parts of the former municipalities.

Change in long-term assets is defined in analog with the change of debt.

Eligible voters are the number of citizens in a local government, eligible to vote.

Left wing incumbency is defined if the two left wing parties, the Social democrats and the Communist party, have more than 50 % of the seats in the local parliament.

Right wing incumbency is defined if the three right wing parties, the Conservatives, the Liberals and the Agrarian party, have more than 50 % of the seats in the local parliament.

Appendix 2. Tables

Table 1
Summary Statistics

Variables	N	Mean	S.D.
$\Delta Debt_{70-68}$	525	132.149	334.697
$\Delta Debt_{73-68}$	323	213.914	941.590
<i>Freeride</i>	848	.661	.321
Population density	848	.467	.299
Population	848	9086	34233
Share in agricultural sector	848	.286	.203
Share in manufacturing sector	848	.327	.198
Tax rate	848	9.890	1.762
Tax base	848	477406	2594048
Tax base per capita	848	37.239	11.963

Table 2
The Effects of the incentive to free ride on the change of debt and change of assets

Variables of interest	Dependent Variables				
	Change in total debt		Change in short term debt	Change in long term debt	Change in long term assets
	(1)	(2)	(3)	(4)	(5)
<i>Freeride</i>	298*** (112)	504*** (186)	124 (106)	175*** (33)	143** (74)
Freeride x Dummy = 1 if merging 1970/71		-323* (194)			
Controls	No	No	No	No	No
N	848	848	844	844	845
R ²	0.022	0.039	0.006	0.023	0.003

Notes: Robust standard errors in parentheses.

*** Significant at the 1% level, ** significant at the at the 5% level, * significant at the at the 5% level at the 10% level

Table 3

The Effects of the incentive to free ride on the change of debt and change of assets. Including controls.

Variables of interest	Dependent Variables				
	Change in total debt		Change in short term debt	Change in long term debt	Change in long term assets
	(1)	(2)	(3)	(4)	(5)
<i>Freeride</i>	249*** (89)	455*** (145)	67 (68)	181*** (56)	166* (98)
<i>Freeride</i> × <i>Dummy</i> = 1 if merging 1970/71		-355** (180)			
Controls	Yes	Yes	Yes	Yes	Yes
N	848	848	844	844	845
R ²	0.098	0.142	0.089	0.115	0.078

Notes: Robust standard errors in parentheses.

*** Significant at the 1% level, ** significant at the at the 5% level, * significant at the at the 5% level at the 10% level

Pre-treatment controls: population density, population, share in agricultural sector, share in manufacturing sector, tax base, tax base per capita, tax rate, county effects , constant .

Table 4

The Effects of the incentive to free ride on the change of debt and change of assets. Including all controls but the tax rate.

Variables of interest	Dependent Variables				
	Change in total debt		Change in short term debt	Change in long term debt	Change in long term assets
	(1)	(2)	(3)	(4)	(5)
<i>Freeride</i>	259*** (94)	478*** (159)	76 (74)	182*** (55)	167* (103)
<i>Freeride*Dummy = 1 if merging 1970/71</i>		-379** (191)			
Controls	All but tax rate	All but tax rate	All but tax rate	All but tax rate	All but tax rate
N	848	848	844	844	845
R ²	0.098	0.142	0.089	0.023	0.077

Notes: Robust standard errors in parentheses.

*** Significant at the 1% level, ** significant at the at the 5% level, * significant at the at the 5% level at the 10% level

Pre-treatment controls: population density, population, share in agricultural sector, share in manufacturing sector, tax base, tax base per capita, county effects , constant .

Table 5

The Effects of the incentive to free ride on the change of debt and change of assets. Robustness.

Variables of interest	Effects for different sub-groups				New dependent variable
	Exclude completed	Exclude completed and those never subject to merge	Using only those that are geographically intact	Using only those that are geographically intact but exclude those never subject to merge	level of Debt _{amalgamation year} - level of Debt ₆₂
	(1)	(2)	(3)	(4)	(5)
<i>Freeride</i>	186*** (70)	235*** (91)	194*** (75)	248** (102)	284** (112)
Controls	Yes	Yes	Yes	Yes	Yes
N	813	771	736	694	694
R ²	0.085	0.095	0.083	0.0890	0.110

Notes: Robust standard errors in parentheses.

*** Significant at the 1% level, ** significant at the at the 5% level, * significant at the at the 5% level at the 10% level

Pre-treatment controls: population density, population, share in agricultural sector, share in manufacturing sector, tax base, tax base per capita, tax rate, county effects, constant .

Table 6
The Effects of the incentive to free ride on the change of debt. Including reelection probability

Variables of interest	Dependent Variables	
	Change in total debt	
	(1)	(2)
<i>Freeride</i>	236*** (80)	275*** (111)
<i>Freeride*Left-wing</i> = 1 incumbent is left-wing	107 (256)	
Proxy for 1 minus probability of reelection		54 (186)
Controls	All	All
N	783	783
R ²	0.098	0.0980

Notes: Robust standard errors in parentheses.

*** Significant at the 1% level, ** significant at the at the 5% level, * significant at the at the 5% level at the 10% level

Pre-treatment controls: population density, population, share in agricultural sector, share in manufacturing sector, tax base, tax base per capita, county effects, constant and a constant for left wing incumbency.