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Abstract

We use the 1952 Swedish municipal amalgamation reform to study free-riding and the common pool problem in politics. We expect municipalities that were affected by the reform to increase their debt in anticipation of a merger, and this effect to be larger if they were merged with many other populous municipalities (i.e. facing a large common pool). We use ordinary least squares and matching on the complete cross section of rural municipalities for the period 1947–1951, fixed effects when exploiting the panel features, as well as a geographical instrumental variables strategy. We find an average treatment effect close to the amount that the average merged municipality increased its debt with during this period, which corresponds to 2.8 percent of average income or 63 percent of the average increase in income. However, we do not find larger increases in municipalities that were part of a larger common pool.

Keywords: Common pool, municipal amalgamation, local governments

JEL Classifications: D72, H73, H74, H77, R53

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1. INTRODUCTION

The common pool problem arises in situations where the costs of an activity which benefits a small group are shared among a wider group of people. An everyday example would be a dinner where the participants have decided to split the bill. Fishing and oil drilling provide larger-scale examples. As suggested by Tullock (1959) and Buchanan and Tullock (1962) the problem can also arise in politics. In many cases each politician represents only a group of voters, but has access to a common pool in the form of the total tax base of all voters. Situations of this kind could explain phenomena like logrolling and pork barrel politics.

Weingast et al. (1981) formalize this line of reasoning. In their setting, each district or political unit determines the size of a project. This is done exclusively on the basis of benefits and costs that are associated with the own district. A crucial assumption is what they label *universalism*, which means that all projects are accepted by the central decisive instance (conceivably in one omnibus budget bill). The costs of all projects are financed through taxes levied on people in all districts in a common pool area. The simplest case is one with identical districts and with a proportional income tax. In that case, each district internalizes all marginal benefits from its own projects, but faces only 1/n-th of the marginal cost, inducing larger projects than what is efficient. This principle is usually referred to as the "law of 1/n".

Several articles have examined the common pool problem empirically and most of them assume that each legislator represents one district. The number of legislators determines the size of the common pool and expenditures are expected to grow with its size. Most studies, e.g. Baqir (2002) and Gilligan and Matsusaka (1995), support the law of 1/n, but recently Petterson-Lidbom (2006) reports serious empirical doubts.

As pointed out by Primo and Snyder (2005), there are also a few theoretical caveats to the intuitive common pool story. When the number of citizens in a common pool area is fixed while the number of districts and district size vary, which is the case in most empirical studies, the law of 1/n only holds for publicly provided *private* goods. For local district-specific public goods, spending per capita is independent of the number of districts.[1] Primo and Snyder also show that crowding enhances the law of 1/n, while deadweight cost and partial cost-sharing diminishes it. A "reverse law of 1/n" may even apply for goods that are

[1] Weingast et al's (1981) original model takes district population as fixed when increasing the number of districts. However, since the number of citizens in the common pool area M is fixed, district population m depends on the number of districts n (i.e. $M = n*m$ applies). With M fixed, more districts means smaller districts, each of them with fewer persons who can take advantage of the local public goods. This reduces the marginal benefit of larger projects and cancels out the common pool effect exactly.

sufficiently public in nature and have considerable deadweight costs in taxation or partial cost-sharing.[2]

Another line of criticism takes a step back by asking why a political assembly would adopt decision making rules, or adhere to norms, that allow for taxation which is independent of individual districts' project size. Finally, the degree to which a politician can be said to represent a district varies between electoral systems and political assemblies; it may for instance be greater at the national than at the local level.

In a situation with theoretical objections and mixed empirical evidence there is need for additional studies. We hope to contribute by investigating a case where the conditions are very favorable for the appearance of a common pool effect. We test the common pool hypothesis by studying the 1952 Swedish municipal amalgamation reform. According to theory we expect the municipalities (our units) that knew that they would soon be amalgamated with others to increase their debt before the amalgamation was carried through; especially units that made up a small part of the forthcoming amalgam. Prior to amalgamation, expenditures almost exclusively benefit the own unit, while the financing of debt is shared with the other units in the amalgam. The amalgam is thus a unit's common pool area and changes in debt measure the financial exploitation of the common pool. In our case district size is fixed. It is also clear that the politicians in each unit represent the constituents in their unit in relation to other units in the common pool. Thus, our approach avoids all mentioned theoretical objections.

Our study combines several ways of estimating the common pool effect. First, we analyze the cross section for the whole period 1947−1951. We start with ordinary least squares (OLS). Then, we use geographical instrumental variables (IV) to correct for unit specific and idiosyncratic unobservables as well as measurement errors. Further, we use matching to check the robustness against non-linearities. Second, we exploit the panel feature, which allow us to examine the dynamics by introducing year specific treatment effects. We can also control for macro shocks by using year dummies. We use a within identification strategy with fixed effects (FE) to correct for unit specific unobservables. Also in the panel analyses, we use a geographical IV strategy to adjust for remaining idiosyncratic unobservables and measurement errors. Finally, we divide the sample into three groups based

[2] Another theoretical issue which Crain (1999) addresses it that of district heterogeneity. When districts are similar there are fewer reasons to seek district-specific projects, since all districts favor the same global public goods. For intra-district heterogeneity the reverse is true. Consider a case with two groups of constituents with opposing demands in a district. This district will probably not invest in programs that favor any of those groups, leading to fewer district-specific projects. Inter-district homogeneity and intra-district heterogeneity reduces the common pool effect (but cannot reverse it). Crain finds some empirical support for these two hypotheses.

on population, and perform both cross section and panel analysis allowing for group specific treatment effects.

We find an average treatment effect (ATE) of a merger of 28.7 SEK per capita (in 1951 prices) in the OLS cross section analysis, and 28.6 SEK per capita in the FE panel analysis, both for the period 1947−1951. This explains the whole increase of 27.7 SEK per capita for the treated units during this period, which corresponds to 2.8 percent of average income or 63 percent of the average increase in income. The direction of correction when using matching and IV is ambiguous, but ATE always stays positive and economically sizeable. The positive ATE supports the presence of a common pool effect. However, the treatment effect we find is independent of common pool size with OLS, and varies even negatively with common pool size with FE, contrary to what the law of 1/n predicts. When combined with FE, the use of IV strengthens the negative effect. Allowing for group specific treatment effects, we find that the negative effect only prevails for units with less than 500 inhabitants, and there is some support for a positive effect for units with more than 1,000 inhabitants. These effects are however small compared to the constant treatment effect. The general picture is one of free-riding but without clear support for the law of 1/n.

2. RELATED EMPIRICAL LITERATURE

To highlight the differences between our strategy and previous approaches, we briefly review the empirical literature. Baqir (2002) uses cross sectional data from U.S. cities in the 1990s to examine the common pool problem due to districting. He finds a positive relationship between council size − a proxy for the number of districts − and government spending. This effect is strengthened when using council size in 1960 as an instrument for council size in 1990 in order to remedy possible reverse causation. The main problem with this strategy is that there might be omitted city specific unobserved variables that correlate with both spending and council size that are persistent over time.

Unit specific unobservables can be taken care of with a fixed effects strategy. Gilligan and Matsusaka (1995) use a panel of U.S. states between 1960 and 1990 and find that the number of seats in the upper house is positively associated with both state and local per capita spending.[3] Although the fixed effects approach nicely handles some of the econometric difficulties, several problems remain unsolved. First, we need significant variation over time in council sizes since the time-invariant variation can not be used when employing fixed effects. Second, council size and spending might be determined simultaneously. Third, there might be idiosyncratic omitted variables, such as changing voter preferences. Finally, the use of within variation in the number of districts (seats) means that the district size is varying since the common pool size is fixed. When this is the case, theory does not predict a common pool effect if the projects provide district-specific public goods, as mentioned in the introduction.

Petterson-Lidbom (2006) finds an exogenous source of common pool size in statutory determinants of council size in Finnish and Swedish local governments and implements a regression discontinuity design. He uses discontinuities which are imposed by statutory law to instrument for council size, which in turn proxies for common pool size.[4] Studying both a panel of Swedish and a panel of Finnish municipalities, he finds the opposite of what the common pool theory predicts. However, also in his set-up we expect a common pool effect only for public goods that are district-specific.

In a paper closely related to ours, Tyrefors (2006) studies the later Swedish amalgamation reform of 1969–1973. The empirical strategy relies on controlling for observable characteristics to account for the principles which were laid out by a governmental committee in 1961: population since the new municipalities were required to have at least 8,000 inhabitants, and a set of mostly economic variables that should capture what is called “scientific principles of functionality”. Tyrefors finds a sizable common pool effect; municipalities making up a relatively small part of an amalgam increased their per capita debt more before the amalgamation. However, selection only on observables is often too bold an assumption when it comes to political processes. As an example, studies by Alesina and Spolaore (1997), Bolton and Roland (1997), and Persson and Tabellini (2000) point out that economic factors and underlying voter preferences influence the unification and break-up of political units. Since it is unlikely that all of these factors can be observed, we have to consider selection on unobservables in our empirical strategy. By using matching estimators, we also allow for non-linear effects. Moreover, there are several reasons –

[3] Other papers using the same strategy are Gilligan and Matsusaka (2001), Bradbury and Crain (2001) and Bradbury and Stephenson (2003). The first analyze an earlier period, the second uses cross country data, and the latter employs local county data in Georgia. They all reach the same conclusion in favor of the law of 1/n.

[4] Note, however, the downside of using this convincing instrument: only a small fraction of the whole sample can be used.

explained in the next section – for believing that the earlier reform of 1952 is more suitable than the later one of 1969–1973 when it comes to estimating a causal and precise common pool effect.[5]

3. SWEDISH MUNICIPAL REFORMS

Through the municipal reform of 1862 Sweden received a uniform administrative system with approximately 2,500 municipalities in 24 counties (SOU, 1978). Formally, there were also 267 districts,[6] which was a level between municipalities and counties, but this historically important level lost its administrative importance in the reform. Each municipality was also classified as rural municipality, borough or city[7]. The municipal districting was based on the old parish borders and less due to functional and economical considerations. Soon the flaws became obvious as the rural municipalities could not provide for increasing welfare demands of the citizens. Emigration from rural areas to the cities worsened the problem.

In 1939 the legislature recognized the problem and in 1943 a commission was appointed to investigate possible remedies (Sandalow, 1971). In 1945 the commission proposed large scale amalgamations of municipalities aiming at more than 2,000 citizens in the new units. It also provided a detailed recommendation on the new districting. The idea was to merge small municipalities without splitting them. The functionality of the new units received little attention. In 1946 a unanimous parliament decided and publicly announced a revision of local government boundaries on the detailed recommendation proposed by the commission (Strömberg and Westerståhl, 1984). After four years of preparatory work, the government decided in 1950 that the new apportionment be executed in 1952, and this was also accomplished. Figure 1 shows a timeline on how the reform process progressed. General

[5] Hanes (2003) is a somewhat related study since he investigates the same municipal amalgamation reform as we do. The important difference is that he studies the period after the reform. He finds economies of scale for small municipalities, but reduced effects for larger ones. His study also addresses the problem of unit specific unobservables, such as the natural affinity between amalgamated municipalities. He uses a Probit model in a first stage to predict amalgamation probabilities, and uses these predicted probabilities in a second stage regression with expenditure as dependent variable. He finds no amalgamation effect in the second stage with this strategy.

[6] The Swedish term is "härad" which often is translated as "hundred". We use the term "district", which is used by some authors. This should not be confused with the modern Swedish districts which are subunits of the municipalities.

[7] The Swedish terms are "landskommun", "köping" and "stad". The first is sometimes translated as "rural commune". We use "rural municipality" due to its lack of normative flavor.

elections were held in 1940, 1944, 1948 and 1952. Municipal elections were held in 1942, 1946 and 1950.

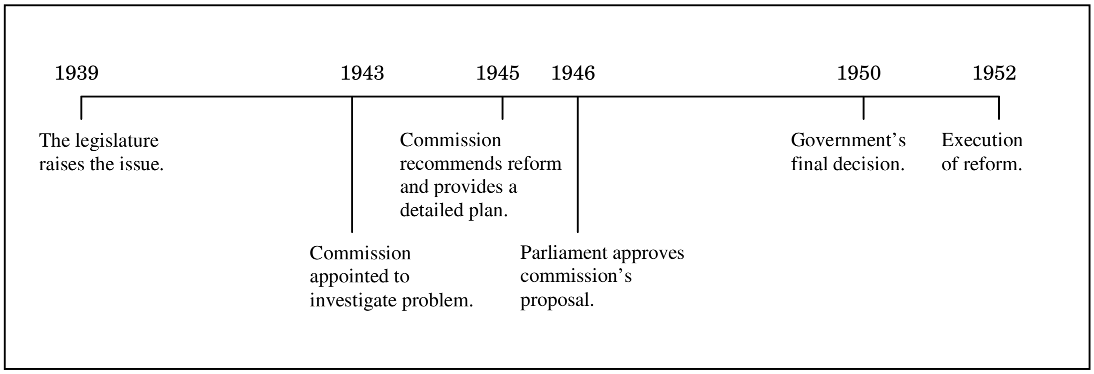

Figure 1. *Timeline on the progress of the reform*

The reform reduced the number of rural municipalities from 2,284 to 823, while all 81 boroughs and 133 cities remained intact and most of them without any changes in borders. Thus, the total number of municipalities declined from 2,498 in 1951 to 1,037 in 1952. Most merging was between rural municipalities in the same district. 304 rural municipalities were unaffected by the reform. The average population increased from about 1,500 to 4,000 due to the reform. The average tax base per municipality more than doubled.

The 1952 reform turned out to insufficient to achieve the intended objectives (Gustafsson, 1978). The growing industrialization and urban development following the Second World War further worsened the situation by making small municipalities even smaller. In 1959 a new commission was appointed and in 1961 a new report suggested further amalgamations targeting 7,000 citizens as the minimum municipality size. This time economic considerations were given much higher priority (SOU, 1978). The main guideline was to form units that should be able to efficiently provide social services. The social structure and geographic unity of the new units were of primary concern. In 1962 the new reform started, and by 1963 the new borders were determined. 1,006 municipalities temporarily formed 282 new *blocks*, but the final amalgamation was voluntary. 37 municipalities were unaffected by the reform. By 1969, only 38 new municipalities had been formed. The government found the proceedings too slow and decided that the reform should be fully implemented by 1974, and this was also achieved.

We choose to work with the 1952 amalgamation reform because it seems to be more exogenous, with less severe selection problems. First, all amalgamations were compulsory in the 1952 reform but not in the last reform, and voluntariness worsens selection problems.

Second, the last reform was more carefully planned with specific economic guidelines, while the 1952 reform was more random in an economic sense. Thus, efficiency arguments permeated the 1952 reform much less than the last reform, which is why the 1952 reform soon was considered inadequate. This means that unobservables are less likely to influence an econometric investigation of the 1952 reform. Third, the different dates of amalgamations in the last reform make the panel unbalanced. Fourth, more rural municipalities were unaffected in the first reform, which provides a better control group to the majority of treated rural municipalities. Fifth, there were more units in the 1952 reform.

The Municipal Administration Act of 1862, which is part of the Swedish Constitution, gives the Swedish municipalities the right to run their own affairs (Sandalow, 1971). However the meaning of this right is not further specified, other than that they have to obey special legislations on some issues. Although this feature often gives the central government the upper hand in conflicts, the Swedish municipalities have much freedom in running their own projects. Generally, they do not have to consult the central government when deciding about municipal specific issues (Gustafsson, 1978). Although there are restrictions on long term debt, there are none on short term debt, and there is no balanced budget rule. According to the Constitution, the municipalities are also entitled to levy local income tax, impose certain charges, and demand remuneration for services and benefits. The municipalities also have a planning monopoly, which means that they can decide how land should be used, even though this formally has to be approved by a central authority. In consequence, Swedish municipalities are major economic actors, even if the large expansion of the public sector and the welfare state is a more recent development after the Second World War.

The Municipal Apportionment Act of 1919 governs the procedure when municipal boundaries are changed. Relevant for us is the Universal Succession law in Paragraph 4, which states that a newly amalgamated municipality inherits the old municipalities' assets and debts. Altogether this means that a municipality can be considered as an economically independent unit with the capacity and possibility to run an economic policy of its own and that a forthcoming amalgamation area is a common pool area.

4. DATA AND DESCRIPTIVE STATISTICS

We use panel data for the period 1946−1951 for all rural municipalities. During this period, there are only a few minor changes in borders. The largely unaffected boroughs and cities are excluded to improve the control group's characteristics. We obtain data from statistical yearbooks published by Statistics Sweden (SCB).[8] We have data for all rural municipalities during this period. Five of them contain missing values in some variables, and are left out. Since we need some first differenced variables we lose one year and are left with five years. The variables we construct and use are presented in Table 1 which describes and shows the means of the untreated control group and the treated (merged in 1952). The sample means are close to the means for the treated since these constitute 86.7 percent of the sample. For more descriptive statistics, see Table A1 in Appendix 1.

Table 1. *Description of variables and means*

Variable	Description	Untreated	Treated
Treat	Dummy: 0 if not amalgamated, 1 if amalgamated	0	1
Ratio	Amalgam's $Pop_{\text{start of 51}}$/Old unit's $Pop_{\text{start of 51}} - 1$	0.000	6.600
NewPop	Amalgam's $Pop_{\text{start of 51}}$ – Old unit's $Pop_{\text{start of 51}}$ *	0.000	3.357
$Debt_i$	Per capita $debt_{\text{end of 46}}$	68.778	42.058
$\Delta Debt_i$	Per capita $debt_{\text{end of 51}}$ – Per capita $debt_{\text{end of 46}}$	37.709	27.749
Inc_i	Sum of per capita income 1947 to 1951	1362.450	1004.837
ΔInc_i	Per capita $income_{51}$ – Per capita $income_{46}$	72.230	43.976
Pop_i	$Population_{\text{start of 47}}$ *	4.258	1.073
ΔPop_i	$Population_{\text{start of 52}}$/$Population_{\text{start of 47}} - 1$	0.016	-0.025
$Dens_i$	$Population_{\text{start of 47}}$/Land Area	0.276	0.251
$Debt_{i,t}$	Per capita $debt_{\text{end of last year}}$	79.939	49.790
$\Delta Debt_{i,t}$	$Debt_{i,t+1} - Debt_{i,t}$	7.542	5.550
$Inc_{i,t}$	Per capita income	272.490	200.968
$\Delta Inc_{i,t}$	$Inc_{i,t} - Inc_{i,t-1}$	14.446	8.795
$Pop_{i,t}$	$Population_{\text{start of year}}$ *	4.308	1.066
$\Delta Pop_{i,t}$	$Pop_{i,t+1}/Pop_{i,t} - 1$	0.001	-0.001
$Dens_{i,t}$	$Population_{\text{start of year}}$/Land Area	0.285	0.250
Area	Land area* (km^2)	78.893	7.617
AreaCoun	Land area/County land area	0.025	0.008
PopDist	District $Population_{\text{start of 51}}$/Unit $Population_{\text{start of 51}} - 1$	6.933	26.241
Units	Number of units: Totally 2,280 units	300	1980

Notes: *i* indexes units and *t* year. Variables with index *t* are the panel version of the cross-section variables. Currency is SEK in 1951 year's prices. The means are displayed. * Values are in thousands.

The first group is our main variables of interest. *Treat* is the treatment dummy which takes the value zero for the untreated and one if a unit is merged in the beginning of 1952. It is a

[8] We gather population and other geographical data from the publication *Årsbok för Sveriges kommuner* and data on financial variables from *Kommunernas finanser*.

common pool dummy. *Ratio* is the ratio between the amalgam's and the old unit's population minus one, which is the proportional increase in population that, due to the amalgamation, has to share the old unit's debt. It measures the size of the common pool. *Ratio* is zero for the untreated and positive for the treated. On average, a treated unit is merged with 6.6 times its own population. *NewPop* is the amalgam's population minus the old unit's population, which is the increase in population due to the amalgamation. *NewPop* equals $Ratio*Pop_{51}$ and is thus the interaction term between *Ratio* and *Pop*.

We use the second group of variables in the cross section analysis for the whole period. $Debt_i$ is the debt in the start of the period. $\Delta Debt_i$ is the change in per capita debt during the period and is the dependent variable. The rest are covariates. The treated has lower means on all covariates in this group. During these 5 years, the average of $Debt_i$ increases 54 percent from 69 SEK to 106 SEK for the untreated, and 67 percent from 69 SEK to 106 SEK for the treated. Whereas the percentage per capita increase is higher for the treated, the per capita increase is actually higher for the untreated. This may be unexpected from theory, even though we are still at the descriptive stage. Average Inc_i increases about 5 percent for both untreated and treated. If we compare $\Delta Debt_i$ with Inc_i and ΔInc_i we see that the average increase in debt during this period corresponds to 2.8 percent of the average income or 63 percent of the average increase in income. The third group of variables is the panel equivalent to the second group. The means show the same pattern as for the cross section equivalents. Average $Inc_{i,t}$ is about 4 times average $Debt_{i,t}$.[9]

The last group of variables is our instrumental variables. These are land area, *Area*, share of county's land area, *AreaCoun*, and district's population relative to own population minus 1, *PopDist*. The treated have on average higher values for all instruments.

Figure 2 and 3 plots the evolution of the average $Debt_{i,t}$ and $\Delta Debt_{i,t}$ during 1947–1952. We use dashed lines for the untreated and solid lines for the treated. The figures show that the average debt increases substantially and in all years during this period. The increase in per capita debt is lower for the treated in 1947, 1950 and 1951 but higher in 1948 and 1949. The figures do not show a clear pattern on how the anticipation of the reform affects the change in debt. However, they suggest that the difference between the treated and the untreated is radically different in 1948 and 1949 compared to 1947, 1950 and 1951, indicating a common pool effect in 1948 and 1949, just before the municipal election in 1950. This timing coincides with predictions from models of electoral budget cycles.

[9] About 50 percent of the municipalities' incomes are taxes and about 15 percent government grants. About 25 percent of the expenditures are used for education and 25 percent for health care.

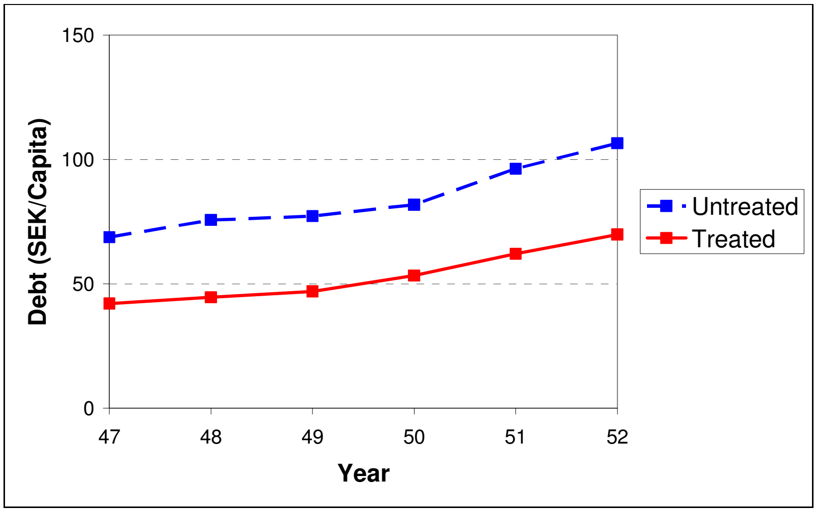

Figure 2. *Descriptive statistics – Evolution of average* $Debt_{i,t}$

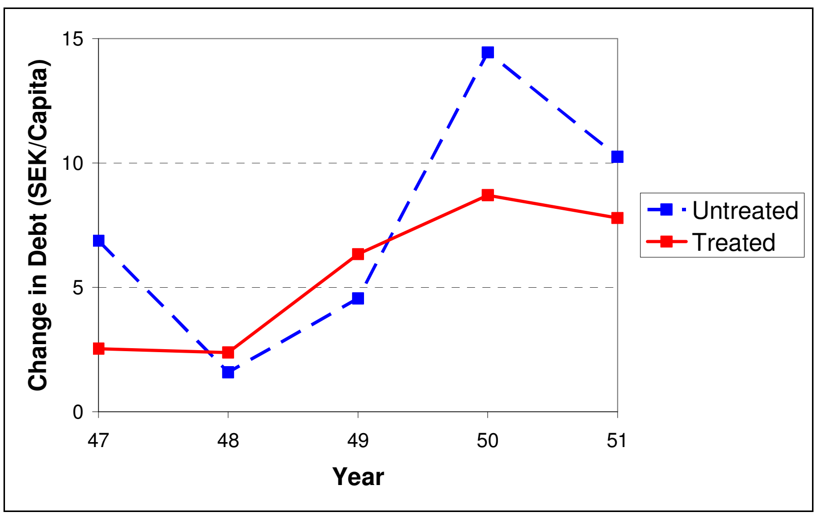

Figure 3. *Descriptive statistics – Evolution of average* $\Delta Debt_{i,t}$

In the group analysis, we divide the sample into three roughly equal sized groups: A, B and C, with populations in the intervals 0-500, 501-1,000 and over 1,000 inhabitants. Table 2 shows the number of units in each group as well as the mean of *Ratio* and $\Delta Debt_i$ (for the whole period) for the untreated and the treated in each group. The mean of *Ratio* is much higher in units with lower population, since these are generally merged with more units which are populous relative to the own unit. We also see that the average increase in debt is higher for the treated in the two groups with fewer than 1,000 inhabitants (as expected from theory), but that the opposite is true for the group with more than 1,000 inhabitants. The result for the whole sample obtained earlier, and shown in Figures 2 and 3, is therefore heavily influenced by the most populous units.

Table 2. *Division of subgroups based on population*

Interval on *Pop*	Units	Mean Ratio	Mean $\Delta Debt_{i,Untreated}$	Mean $\Delta Debt_{i,Treated}$
A: 0 – 0.5	559	14.792	11.986	37.309
B: >0.5 – 1	632	5.268	2.690	40.613
C: >1	789	1.862	69.190	46.581

Notes: Populations are in thousands and the average debts in SEK per capita.

5. EMPIRICAL STRATEGIES

To start with, the choice of change in per capita debt rather than expenditures as our dependent variable avoids some of the possible bias due to unobservables. Compared with studies of government size, factors such as economic growth, scale effects and monopoly rents are not expected to cause us any problems. Omitted variables may still concern us, since there may be selection to treatment (amalgamation) on unobservables, be it economic variables, voter preferences, or natural affinity with neighbor municipalities. We use fixed effects and instrumental variables to mitigate this problem.

5.1 Ordinary Least Squares

Here, we study the aggregate change during the whole period 1947−1951, with the almost complete cross-section of 2,280 municipalities. We assume throughout this paper that the treatment effects are the same for the treated and the untreated. When this is the case, the average treatment effect (ATE) coincides with the average treatment effect for the treated (ATT). To control for selection on observables, we use the control function approach, which is the usual OLS with control variables. To find ATE, we estimate:

$$\Delta Debt_i = \beta_{Treat} Treat_i + \boldsymbol{\beta}_{\mathbf{X}}'\mathbf{X}_i + \boldsymbol{\beta}_{\mathbf{D}}'\mathbf{D}_i + \varepsilon_i, \qquad (1)$$

where i indexes rural municipality units. $\Delta Debt_i$ is the dependent variable, $Treat_i$ is the treatment variable, $\boldsymbol{X}_i = (Inc_i, \Delta Inc_i, Pop_i, \Delta Pop_i, Den_i, 1)'$ is a vector of control variables and $\boldsymbol{D}_i$ is a vector of district dummies, dropping one district to avoid multicollinearity. ε_i is the unit specific error term. From common pool theory, we expect ATE = β_{Treat} to be positive.

We use the between unit variation to identify ATE here. The controls correct for observed differences between units and the district dummies correct for some of the possible selection on unobservables.

We are also interested in the variation of the common pool effect to test the finer details of the law of 1/n. To analyze this, we estimate the following for the treated:

$$\Delta Debt_i = \beta_{Ratio} Ratio_i + \boldsymbol{\beta}_{\mathbf{X}}'\mathbf{X}_i + \boldsymbol{\beta}_{\mathbf{D}}'\mathbf{D}_i + \varepsilon_i, \tag{2}$$

where $Ratio_i$ is the size of the common pool. We call the variation of the treatment effect among the treated the "ratio effect". The law of 1/n predicts the ratio effect (β_{Ratio}) to be positive. We confine ourselves to the treated, since there might be other differences between the treated and the untreated interacting with the ratio effect. However, the results turn out to be independent of this restriction.

We also estimate the following for the whole sample:

$$\Delta Debt_i = \beta_{Treat} Treat_i + \beta_{Ratio} Ratio_i + \boldsymbol{\beta}_{\mathbf{X}}'\mathbf{X}_i + \boldsymbol{\beta}_{\mathbf{D}}'\mathbf{D}_i + \varepsilon_i. \tag{3}$$

Here, β_{Treat} and β_{Ratio} are the constant treatment and ratio effects, net the interaction between them. We thus allow an intercept and a linear treatment effect. In this setting β_{Treat} is no longer ATE, which has to be recovered by adding β_{Treat} to the product of β_{Ratio} and the mean of *Ratio*.

5.2 Fixed Effects

The cross section strategy do not allow us to examine the dynamics, control for time effects or use within-variation. To mitigate this problem, we exploit the panel dimension in our data, which allows us to include year specific treatment effects and year dummies. Because we only have five years, there is still a risk that all years were exceptional. If there are no unit specific unobservables, pooled OLS is efficient. Otherwise the random effects (RE) estimator is more efficient. However, if there are unit specific unobservables which are correlated with the regressors, both pooled OLS and RE are inconsistent. This is the case if we have selection on unit specific unobservables. If the treatment effects are different in different years, we can exploit a within strategy to solve this. When applying an LM-test we reject the null of equal treatment effects in different years, justifying a within approach. The cost is that we cannot identify the levels, but only differences in effects between the years.

We use the FE estimator in the unit dummy equation:

$$\Delta Debt_{i,t} = \boldsymbol{\beta}'_{Treat}\, Treat_i \boldsymbol{Y}_t + \boldsymbol{\beta}_X{}' \boldsymbol{X}_{i,t} + \boldsymbol{\beta}_Y{}' \boldsymbol{Y}_t + \boldsymbol{\beta}_U{}' \boldsymbol{U}_i + e_{i,t}\,, \tag{4}$$

where i indexes units and t years. $\Delta Debt_{i,t}$ is our dependent variable. $Treat_i$ and $Ratio_i$ are multiplied with a vector of year dummies $\boldsymbol{Y_t} = (Y48_t, Y49_t, Y50_t, Y51_t)'$, to allow different effects in different years. We have to leave out one year, 1947 here, and the estimated effects are differences relative the effect in 1947. $\boldsymbol{X}_{i,t} = (Inc_{i.t}, \Delta Inc_{i.t}, Pop_{i,t}, \Delta Pop_{i,t}, Den_{i,t}, 1)'$ is a vector of control variables, and $\boldsymbol{U}_i$ is a vector of unit dummies, dropping one unit. $e_{i,t}$ is the observation specific idiosyncratic error. We drop district dummies since we include unit fixed effects. We also estimate equation (4) with $\boldsymbol{\beta}'_{Treat} Treat_i \boldsymbol{Y}_t$ replaced by $\boldsymbol{\beta}'_{Ratio} Ratio_i \boldsymbol{Y}_t$ for the treated sample to analyze the ratio effect for the treated, as well as the FE equivalent to equation (3) to co-estimate the intercept and ratio effects.

Since the amalgamation decision is based on unit specific characteristics, FE efficiently solves the selection issue. However, to recover the levels, we need to assume the level for one year. If we use RE instead, we can include year specific treatment effects for all years and get level estimates. To control for some unit specific effects, we can include district dummies. However, a Hausman test (when $Treat_i$ and $Ratio_i$ are co-estimated) gives $\chi^2 = 81.5$, and rejects the null of consistency of RE, which is why we resort to FE. The FE strategy produces no estimates in levels and to recover the levels we make the cautious assumption that there are no treatment effects in 1947. We report the RE estimates in 1947, which are close to zero, as (weak) support for this.[10] With this assumption, the FE estimates can be interpreted as levels, and mark a lower bound since the level is $\beta_{47}+\beta_Y$ in year $Y = 48$, 49, 50 and 51, which is higher than β_Y if the effects are positive already in 1947.

5.3 Instrumental Variables

FE does not adjust for selection on idiosyncratic unobservables. However, since the details of the amalgamation reform were settled in 1946, which is before our period of study, it seems far-fetched to believe that idiosyncratic unobserved factors during 1947–1951 determined the selection into treatment. For this to be the case, the treatment decision had to be based on variables before 1947 that correlate with unobservables some *particular* (but not all) years between 1947 and 1951, or on what the decision makers before 1947 expected about the

[10] The RE estimates mostly do not differ much from the FE estimates by visual inspection and we do not find the RE estimates totally useless despite the rejection of RE in the Hausman test.

outcome of the unobservables in some particular years between 1947 and 1951. Reverse causation is implausible for the same kind of reason. But, treatment is a political decision, and the actors could have called off the reform, or changed the details of the 1946 decision between 1947 and 1951. The fact that they did not do any of this can be seen as a determinant of treatment and might depend on idiosyncratic unobservables, in which case FE is not enough. We use an IV strategy to address this issue. IV also corrects for possible simultaneity and measurement errors. Further, IV can be applied on the cross section of municipalities to provide level estimates, and at the same time correct for more endogeneity than FE does – given that the instruments can be trusted.

We apply IV on both the cross section OLS and the panel FE set-ups. In the OLS set-up, there are three sources of omitted variables – time effects, unit specific unobservables and idiosyncratic unobservables. IV corrects for all three sources of endogeneity, but in the FE set-up, we only have to deal with the last factor. We use an IV strategy for $Treat_i$ and $Ratio_i$ one at a time as well as on both at once.

We use land area, $Area_i$, and share of land area in the county, $AreaCoun_i$, as instruments for $Treat_i$[11] and estimate the following first-stage equation in the OLS set-up:

$$Treat_i = \alpha_{Area} Area_i + \alpha_{AreaCoun} AreaCoun_i + \boldsymbol{\alpha}'_{\mathbf{X}} \mathbf{X}_i + \boldsymbol{\alpha}'_{\mathbf{D}} \mathbf{D}_i + u_i . \qquad (5)$$

Table 1 shows a negative correlation between our instruments and $Treat_i$. The ceteris paribus sign is however a priori unclear, once we include controls. The estimates of (5) are in column [5] in Table A2 in Appendix 2 and show positive estimate on $Area_i$ and negative on $AreaCoun_i$.

The intuition behind our instruments is that geographically large units have more neighbors which they may be merged with and that the amalgams are of roughly the same size within each county. The reason for including $AreaCoun_i$ is that $Area_i$ alone turns out to be a weak instrument. With both instruments, partial $F(2, 2006) = 32.3$ and partial $R^2 = 0.031$ indicating no problems with weak instruments. Land area measures are geographically given and not politically determined, and therefore arguably exogenous. It is not a socio-economic variable, and is not expected to influence $\Delta Debt_i$, other than through $Treat_i$, when population and density has been controlled for. The two instruments for $Treat_i$, $Area_i$ and $AreaCoun_i$ pass the overidentification test with Sargan's statistic = 0.75 and $p(\chi^2) = 0.38$. We extend

[11] Wu-Hausman's test of endogeneity fails to reject the null of exogeneity of $Treat_i$ with $F(1, 2006) = 0.57$ and $p(F) = 0.45$. To the extent that one is willing to trust this test, OLS is enough. For reasons of precaution we use IV anyway.

equation (5) by instrumenting $Treat_i\boldsymbol{Y}_t$ with $Area_i\boldsymbol{Y}_t$ and $AreaCoun_i\boldsymbol{Y}_t$ in the FE and RE set-ups.

We use district's population/own population minus one, $PopDist_i$, as instrument for the common pool size $Ratio_i$[12] and estimate the following first-stage equation in the OLS set-up:

$$Ratio_i = \alpha_{PopHun} PopDist_i + \alpha'_{\mathrm{X}} \mathrm{X}_i + \alpha'_{\mathrm{D}} \mathrm{D}_i + u_i . \qquad (6)$$

Since most mergers are between units in the same district, we have high positive correlation between $Ratio_i$ and $PopDist_i$ as seen in Table 1. The estimates of (6) in column [6] in Table A2 in Appendix 2 also show positive estimate on $PopDist_i$. We get partial F(1, 1737) = 189.3 and partial $R^2 = 0.098$ for (6), indicating that $Ratio_i$ is a sufficiently strong instrument. The districts' borders are geographical properties, with only historical interest, and are clearly exogenous. Nor do we expect $Popdist_i$ to affect $\Delta Debt_i$ except through $Ratio_i$. We extend equation (6) by instrumenting $Ratio_i\boldsymbol{Y}_t$ with $PopDist_i\boldsymbol{Y}_t$ in the FE and RE set-ups.

When including both $Treat_i$ and $Ratio_i$ we use $Area_i$, $AreaCoun_i$ and $PopDist_i$ as instruments. For FE and RE, we analogously instrument $Treat_i\boldsymbol{Y}_t$ and $Ratio_i\boldsymbol{Y}_t$ with $Area_i\boldsymbol{Y}_t$, $AreaCoun_i\boldsymbol{Y}_t$ and $PopDist_i\boldsymbol{Y}_t$.

5.4 Matching

In the regression approaches, we assume that the nature of the selection on observables is such that the observables affect the dependent variable linearly (in parameter), which might not be a good approximation. A strategy that does not rely on this functional form is matching. In matching we match treated and untreated units according to similarities in observed variables, and we do not have to specify how the variables affect the dependent variable. Thus, selection bias caused by misspecification can be avoided. The identification uses local differences between observations with similar characteristics with respect to observables. This strategy can only be used to evaluate the ATE (which equals the ATT when we assume the absence of group specific treatment effects) and not the ratio effect.

Exact matching is not possible since the variables can take a continuum of values. We use a matching strategy based on propensity scores, which is the estimated propensity for treatment. In the first stage, we estimate the propensity scores. In this we are interested in obtaining as good propensity scores as possible, not in getting the parameter estimates right.

[12] Wu-Hausman's test of endogeneity fails to reject the null of exogeneity also for $Ratio_i$ with F(1, 1736) = 0.030 and p(F) = 0.86. We use IV anyway here as well.

We estimate the following Probit model for the cross section on the whole period 1947–1951:

$$P(Treat_i = 1) = \Phi(\boldsymbol{\varphi}_S' \boldsymbol{S}_i), \tag{7}$$

where *P(*)* is the probability of treatment and *Φ(*)* is the cumulative standard normal distribution. $\boldsymbol{S}_i = (Inc_i, Inc_i^2, \Delta Inc, Pop_i, Pop_i^2, Area_i, AreaCoun_i, 1)'$.

We impose common support, which means that for all sets of regressors, there should be a positive probability of nonparticipation. This is to ensure that we have untreated matches for all treated observations. Roughly, the treated and the untreated should be comparable with respect to the observables. Most districts contain only untreated or treated units, and including district dummies in (7) leads to perfect prediction for most units. Perfectly predicted observations violate the common support assumption and cannot be used. Including district dummies would therefore lead to a massive loss of observations.

A basic matching assumption is that the sample is balanced, which means that the treated and the untreated units are similar with respect to each observable. We check that the means of each variable are the same for the two groups. This is seldom the case for the whole sample, since the treated differ from the untreated on average. But by dividing the sample into propensity score intervals, balancing can be achieved in each interval. If not we have to try another specification. Equation (7) is chosen such that the balancing property is satisfied.

The final matching estimation can be carried out with different algorithms. We use nearest neighbor, kernel, stratification and radius matching. In nearest neighbor matching, each treated is matched with its nearest untreated neighbor. In kernel matching, several neighbors are used with weights given according to a kernel function. In stratification matching, a treated unit is compared with the untreated units within an interval. We use propensity score intervals that fulfill the balancing property. In radius matching, each treated unit is compared with all untreated units with a propensity score in a predefined neighborhood of the treated unit.

6. RESULTS

6.1 Cross Section Results

The OLS results based on equations (1) – (3) are in column [1] – [3] and the IV result based on (5) – (6) are in column [4] – [6] in Table 3 and apply to the cross section for the period

1947–1951. When estimating the ratio effect, we confine us to the treated. White's test indicates heteroskedasticity and we report White's robust standard errors throughout this paper. Additional results can be found in Table A2 in Appendix 2.[13]

Table 3. *Cross section OLS and IV regression estimates*

Dep. Var: $\Delta Debt_i$	[1] OLS:Treat	[2] OLS:Ratio	[3] OLS:Both	[4] IV:Treat	[5] IV:Ratio	[6] IV:Both
$Treat_i$	28.711***		28.745***	60.236		58.667
	(8.383)		(8.374)	(46.121)		(45.300)
$Ratio_i$		-0.077	-0.052		0.013	0.041
		(0.146)	(0.152)		(0.426)	(0.448)
Inc_i	0.049***	0.056***	0.049***	0.055***	0.056***	0.055***
	(0.016)	(0.019)	(0.016)	(0.018)	(0.019)	(0.019)
ΔInc_i	0.626***	0.621***	0.625***	0.630***	0.623***	0.631***
	(0.068)	(0.073)	(0.069)	(0.069)	(0.075)	(0.071)
Pop_i	3.665	1.660	3.602	5.349	1.958	5.316
	(2.602)	(2.165)	(2.658)	(4.215)	(2.410)	(4.138)
ΔPop_i	54.191	82.695**	53.684	60.030	83.201**	60.147
	(44.027)	(41.513)	(43.981)	(43.293)	(41.819)	(43.413)
Den_i	15.600**	17.361**	15.587**	16.506**	17.367**	16.472**
	(6.881)	(7.010)	(6.884)	(6.657)	(7.005)	(6.653)
Districts	Yes	Yes	Yes	Yes	Yes	Yes
Units	2,280	1,980	2,280	2,280	1,980	2,280
R^2	0.332	0.319	0.332	-	-	-

Notes: Robust standard errors in parentheses.* significant at 10%; ** significant at 5%; *** significant at 1%.

We get an ATE of 28.7 SEK per capita with statistical significance at 1 percent in column [1]. The ratio effect is -0.077 SEK per capita (for each 100 percent increase in population due to amalgamation) in column [2]. This estimate is statistically insignficiant and yields a tiny -0.51 SEK per capita for the average treated unit. The co-estimated effects in column [3] are close to the separately estimated effects. However, the estimates are not directly comparable since the interaction between $Treat_i$ and $Ratio_i$ is corrected for in column [3]. β_{Treat} is ATE in column [1] and an intercept effect in column [3], and the ratio effect in column [2] excludes the untreated but not the ratio effect in column [3]. ATE can be recovered from column [3] as $\beta_{Treat}+\beta_{Ratio}*Ratio_{Mean}$ = 28.4 SEK per capita which is close to the separately estimated effect. IV about doubles ATE but makes it statistically insignificant in column [4], and turns the

[13] In Table A2, column [1] – [2] show OLS with a subgroup of controls and column [3] is equivalent to the ratio estimates in column [2] in Table 3 but includes the untreated. Column [4] and [5] in Table A2 show the first step estimates to the IV results in column [4] and [5] in Table 3.

ratio effect for the treated positive in column [5] but the effect is tiny and statistically insignificant. The co-estimated result in column [6] is on par with the separately estimated results.

The positive ATE supports a common pool effect, while the low and statistically insignificant ratio effect is unfavorable of the finer details of the law of 1/n which predicts a positive and sizeable ratio effect. The size of ATE is economically significant since the mean increase in debt for the treated during the period is only 27.7 SEK per capita which corresponds to 2.8 percent of the average income or 63 percent of the average increase in income. Thus, the whole increase can be attributed to the common pool effect.

We get positive estimates for all controls and very small differences between OLS and IV. Higher Inc_i and ΔInc_i plausibly makes the municipalities afford a higher increase in debt. Higher $Dens_i$ and Pop_i also increase $\Delta Debt_i$, as do ΔPop_i which is perhaps counterintuitive. But once we recall that we base identification on the cross section variation here, this last fact might not be that surprising. Inc_i and ΔInc_i are always statistically significant at 1 percent, while Pop_i and ΔPop_i are mostly insignificant.

When employing matching, we first estimate the propensity score equation (7). The estimates as well as the percentage correctly predicted are reported in equation (A1) and Table A3 in Appendix 2. The distribution of propensity scores across 8 strata fitted from the estimates is in Table 5. The common support condition leaves 2,268 usable observations out of 2,280 (12 untreated observations are dropped). The division of strata guarantees that the balancing property is fulfilled. Some strata contain a small number of treated or untreated which might reduce efficiency.

Table 4. *Distribution of propensity scores and division of strata*

Minimum Pscore	Untreated	Treated	Total
0.0003125	75	13	88
0.20	35	18	53
0.40	43	45	88
0.60	67	90	157
0.80	45	135	180
0.90	16	244	260
0.95	4	360	364
0.975	3	1,075	1,078
Total	288	1,980	2,268

Note: There are thus 45 treated with a propensity score in the interval 0.40–0.60.

In Table 5 we present matching estimates based on the common support region outlined in Table 4. In the stratification procedure, we use the division of strata according to Table 4, which ensures balancing within each stratum. With the stratification, the kernel and the radius (r = 0.1) procedures, we use the full set of treated and controls. We also use the full set of treated in all except the radius (r = 0.01) procedure. The ATE estimates are between 13.3 and 35.6 SEK per capita and statistically significant at 10 percent when using kernel and stratification matching. Compared to OLS, the direction of adjustment is ambiguous while the standard errors increase. But the most important result is that ATE stays positive and sizeable.

Table 5. *Matching estimates*

Matching Procedure	Untreated	Treated	ATE	St. Err.
Nearest neighbor	148	1,980	33.968	22.152[b]
Kernel	288	1,980	16.133*	9.622[b]
Stratification	288	1,980	23.792*	13.943[a]
Radius, r = 0.1	288	1,980	13.326	13.794[a]
Radius, r = 0.01	267	1,980	19.541	23.072[a]
Radius, r = 0.001	128	583	35.553	28.367[a]

Notes: [a] Analytical standard errors. [b] Bootstrapped standard errors with 200 replications.
* significant at 10%; ** significant at 5%; *** significant at 1%.

6.2 Panel Results

The FE and FE-IV panel results based on equation (4) are reported in column [1] – [3] and [4] – [6] in Table 6. The ratio effect is still estimated only for the treated. Compared to the cross section approach, year specific treatment effect and year dummies are introduced as well as unit fixed effects instead of district dummies. In FE, we need to leave out the treatment effect for one year and we do so for 1947. The aggregate effect can be calculated by summing the effects from 1947 to 1951, i.e. $\beta_{47} + \sum_{Y=48}^{51}(\beta_{47} + \beta_Y) = 5\beta_{47} + \sum_{Y=48}^{51}\beta_Y$. When doing this we assume that there is no effect in 1947. We report the RE estimates[14] for 1947 as a justification for this assumption. The aggregate effect then becomes $\sum_{Y=48}^{51}\beta_Y$. RE equivalents to Table 6 are in Table A4 in Appendix 3 and are qualitatively similar to the FE results.

[14] The RE estimation includes year-specific effect for all years and replaces fixed effects with district dummies.

Table 6. *Panel FE and IV-FE estimates*

Dep. Var: $\Delta Debt_{i,t}$	[1] FE:Treat	[2] FE:Ratio	[3] FE:Both	[4] IV:Treat	[5] IV:Ratio	[6] IV:Both
$\Sigma_Y \beta_{TreatY}$	28.631***		30.563***	14.238		32.027
	(9.782)		(9.837)	(26.118)		(35.504)
$\Sigma_Y \beta_{RatioY}$		-0.288	-0.349		-1.524	-1.229
		(0.271)	(0.271)		(1.030)	(1.205)
$Treat_i Y47_t$	0.244		-0.195	16.263		22.305*
(RE)	(2.220)		(2.219)	(10.380)		(12.442)
$Ratio_i Y47_t$		0.021	0.022		0.138	
(RE)		(0.045)	(0.045)		(0.232)	
$Treat_i Y48_t$	7.042***		7.331***	-1.452		4.570
	(2.677)		(2.699)	(7.907)		(10.985)
$Treat_i Y49_t$	11.451***		11.267***	10.235		10.788
	(3.594)		(3.625)	(8.269)		(11.318)
$Treat_i Y50_t$	3.121		3.643	3.730		9.753
	(3.661)		(3.680)	(8.443)		(11.371)
$Treat_i Y51_t$	7.018**		8.322**	1.726		6.916
	(3.423)		(3.514)	(8.182)		(11.110)
$Ratio_i Y48_t$		-0.042	-0.048		-0.553*	-0.399
		(0.075)	(0.075)		(0.319)	(0.380)
$Ratio_i Y49_t$		0.030	0.016		-0.131	-0.068
		(0.094)	(0.094)		(0.322)	(0.380)
$Ratio_i Y50_t$		-0.077	-0.096		-0.423	-0.422
		(0.067)	(0.067)		(0.326)	(0.379)
$Ratio_i Y51_t$		-0.199	-0.220		-0.416	-0.339
		(0.140)	(0.140)		(0.333)	(0.385)
$Inc_{i,t}$	0.139***	0.164***	0.136***	0.143***	0.161***	0.139***
	(0.035)	(0.041)	(0.035)	(0.021)	(0.022)	(0.021)
$\Delta Inc_{i,t}$	-0.032	-0.048	-0.030	-0.035**	-0.045***	-0.033**
	(0.030)	(0.035)	(0.030)	(0.016)	(0.017)	(0.016)
$Pop_{i,t}$	-6.679	-3.885	-5.936	-6.381	-3.490	-5.038
	(11.073)	(18.776)	(11.083)	(8.384)	(16.662)	(8.746)
$\Delta Pop_{i,t}$	-27.343*	-21.591	-25.431	-24.996*	-23.664	-25.451*
	(15.406)	(17.243)	(15.472)	(13.632)	(15.122)	(13.778)
$Den_{i,t}$	59.277	116.899	56.434	59.640	110.343**	56.150
	(57.711)	(98.373)	(57.751)	(38.904)	(54.791)	(39.010)
Years	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.016	0.018	0.017	-	-	-

Notes: Robust standard errors in parentheses.* significant at 10%; ** significant at 5%; *** significant at 1%.

We get the dynamic pattern of ATE from FE in column [1]. The effects are positive in all years relative to 1947. The effects are statistically significant at 1 percent in 1948 and 1949 and at 5 percent in 1950. The solid line in Figure 4 plots the development. The effect peaks at 11.5 SEK per capita in 1949, 3 years before the reform and 1 year before the municipal election in 1950. The RE estimate for 1947 is tiny 0.24 and statistically insignificant, which supports that the effects in the following years are due to the anticipation of the reform. 1947 is the first year after the parliament's approval of the reform and long enough before the execution of the reform to serve as baseline year of no ATE. Assuming the absence of ATE in 1947, the FE estimates can be interpreted as levels of the effects. The aggregate ATE is then 28.6 SEK per capita and statistically significant at 1 percent and very close to the OLS estimate of 28.7 SEK per capita. Thus, allowing year specific effects, including year dummies, exploiting the within-variation and controlling for unit specific unobservables do not change the overall effect.[15]

With IV-FE in column [4], the effect turns negative in 1948 and is very much reduced in 1951. None of the estimates are statistically significant. But the dashed line in Figure 4 shows that the dynamic pattern stays the same and ATE still peaks in 1949. The IV-RE estimate for 1947 is now positive and large. Thus the aggregate effect assuming no effect in 1947 probably underestimates the real total effect, and is statistically insignificant at 14.2 SEK per capita, which is half of the estimated aggregate effect in the FE case. In sum, controlling for idiosyncratic errors yield a similar dynamic pattern and still renders a sizeable aggregate effect, but the standard errors increase.[16]

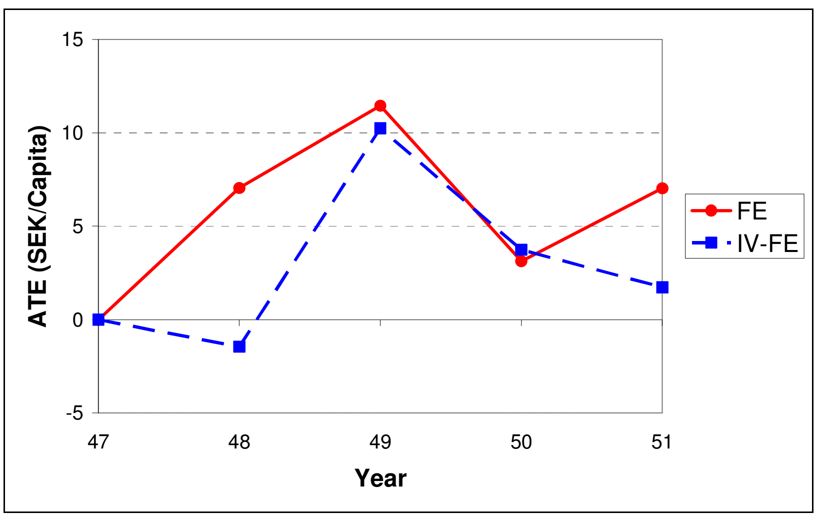

Figure 4. *Dynamic evolution of ATE*

[15] The RE estimates in column [1] in Table A4 in Appendix 3 resemble the FE estimates.
[16] The IV-RE estimates in column [4] in Table A4 in Appendix 3 are higher than the IV-FE estimates, but show similar variation in effects between years. The dynamic pattern is thus robust, but not the aggregate effect estimates.

We get the dynamic pattern of the ratio effect from FE in column [2]. The effects are small and insignificant relative to 1947. The solid line in Figure 5 plots the development. The largest effect is -0.20 SEK per capita, one year before the reform. The RE estimate for 1947 is very close to zero, 0.021 SEK per capita. The aggregate ratio effect is -0.29 SEK per capita, which is larger in size than the OLS estimate, and gives -1.9 SEK per capita for the average unit. Thus, with the FE correction, we obtain a small unexpected negative ratio effect.[17]

With IV-FE, the effects turn highly negative for all years with statistical significance in 1948. The dashed line in Figure 5 plots the dynamic pattern. The IV-RE estimate for 1947 is close to zero and the IV-FE estimate can be interpreted as levels. The aggregate ratio effect is -1.5 SEK per capita and much larger than the unexpected negative ratio effect found using FE.[18]

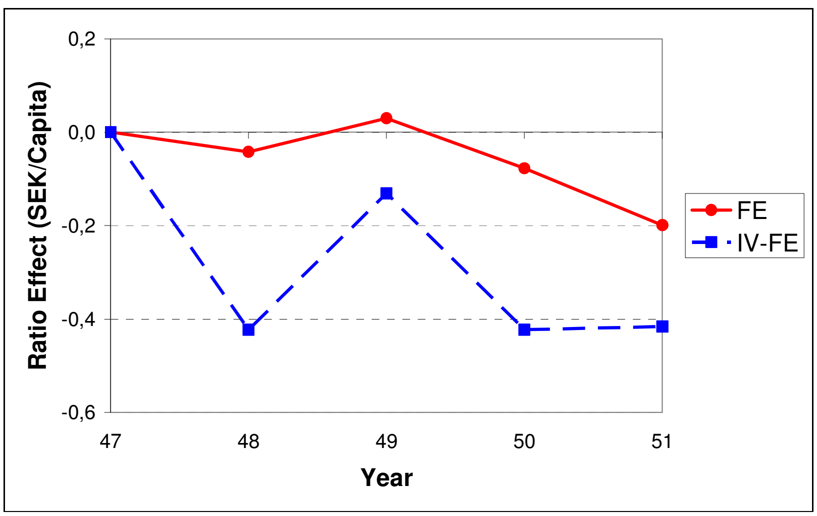

Figure 5. *Dynamic evolution of the ratio effect for the treated*

The co-estimated intercept and ratio effects from FE in column [3] in Table 6 are close to the separately estimated effects in both size and pattern. Aggregate ATE can be recovered as $\sum_{Y=48}^{51} \beta_{Treat} + \sum_{Y=48}^{51} \beta_{Treat} * Ratio_{Mean} = 28.3$ SEK per capita. The co-estimated effects from FE-IV in column [6] give a similar pattern and the same size on the aggregate ratio effect as the separately estimated effects, but an aggregate ATE of 23.9 SEK per capita, which is much higher than the separately estimated effect, but close to the FE result.

[17] The RE estimates in column [2] in Table A4 in Appendix 3 also resemble the FE estimates.
[18] The RE estimates in column [4] in Table A4 in Appendix 3 resemble the FE estimates as well.

The aggregate co-estimated intercept and ratio effects are plotted in Figure 6, where we show the effects when varying *Ratio*. The fat solid line shows the FE result and the fat dashed line the IV-FE result. The co-estimated OLS and IV results are also shown, the former in the thin solid line and the latter in the thin dashed line. While the common pool effect is constant in the cross section analysis, it decreases with the common pool area in the panel analysis. For units with very large common pools the panel result even suggest a negative treatment effect, since the negative ratio effect becomes larger than the positive intercept effect. While the mean *Ratio* for the treated is 6.6, the largest *Ratio* is 218.3, and the panel result certainly predicts an unexpected negative treatment effect for some units (9 units with the FE-estimates).

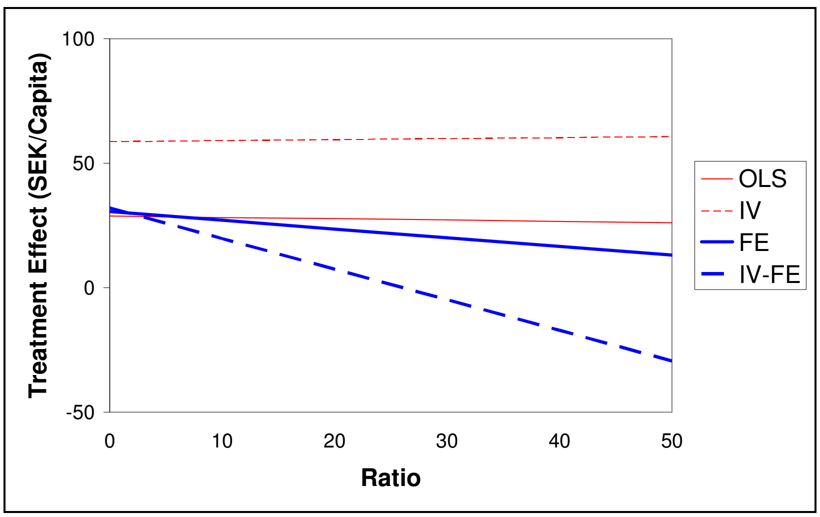

Figure 6. *Intercept and ratio effect for different years and in aggregate*

Looking at the controls we see that while the signs on β_{Inc} and β_{Dens} are positive as in the cross section analysis, $\beta_{\Delta Inc}$, β_{Pop} and $\beta_{\Delta Pop}$ now have negative signs. Since we only use within-variation and no between-variation there is no reason to believe that the signs on the controls should stay the same. Higher $Inc_{i,t}$ and $Dens_{i,t}$ still means a better economy which makes the units afford a higher $\Delta Debt_{i,t}$. Higher $\Delta Inc_{i,t}$ on the other hand causes the units to reduce $\Delta Debt_{i,t}$. For $Pop_{i,t}$ and $\Delta Pop_{i,t}$, an increase in population within a unit means more persons to spread the debts on, which plausibly decreases $\Delta Debt_{i,t}$ since it is measured in per capita terms.

6.3 Ratio Results by Population Groups

To analyze the unexpected ratio effect for the treated, we divide the sample into 3 groups, A, B and C with 0–500, 501–1,000 and >1,000 inhabitants, and estimate the ratio effects with all previous set-ups but allowing for group specific effects. The results are presented in Table 7. In the panel set-ups, we report the usual aggregate effect assuming no effect in 1947. Alternative results, estimating the groups separately, or including an interaction term between *Ratio* and *Pop* (in 1951), *NewPop*, that restricts the ratio effect to depend linearly on population, are in Table A5 and A6 and equations (A2) and (A3) in Appendix 4.

The results for the least populous units in group A are close to the results for the whole sample. The OLS estimate in column [1] is small and negative and the IV estimate in column [2] is small and positive. The FE and FE-IV estimates in column [3] and [4] show the same dynamic pattern as in Figure 5. The aggregate effect is negative for FE and highly negative for FE-IV. The other groups' ratio effects differ very much from the ratio effect for the whole sample which suggests that the ratio effect is different across groups.[19] Thus, the size of the ratio effect for the whole sample is highly influenced by and only representative for the small units. The standard errors are high and none of the group-specific effects are statistically significant.

The ratio effect appears to increase with population as the group-specific estimates are mostly lowest for group A and highest for group C. On the other hand, the FE-IV estimates show a reverse trend. The results also suggest that there is a negative ratio effect for group A, no effect for group B and a positive effect for group C. But the IV estimate in column [2] hints at the absence of a ratio effect for small units rather than a negative effect. In sum, there is no robust evidence for the presence of a ratio effect, neither a clear pattern of how such an effect varies with population.[20]

[19] A similar analysis for ATE, either with $Treat_i$ estimated alone or co-estimated with $Ratio_i$, reveals only small and non-systematic group specific effects, and does not change the overall qualitative pattern obtained without group specific effects.

[20] The group by group estimation results in Table A5 and A6 as well as the interaction term specification results in Appendix 4 give some further support that the ratio effect increases with population with a negative effect for less populous units but a positive effect for more populous ones, and some estimates are sizeable and statistically significant.

Table 7. *Group specific ratio effects*

Dep. Var: $\Delta Debt_{i,t}/\Delta Debt_{i,t}$	[1] OLS	[2] IV	[3] FE	[4] IV-FE
$A_i Ratio_i/\Sigma_Y \beta_{ARatioY}$	-0.122	0.530	-0.350	-2.158
	(0.175)	(0.556)	(0.311)	(1.972)
$B_i Ratio_i/\Sigma_Y \beta_{BRatioY}$	0.170	1.717	0.133	-4.552
	(0.193)	(1.706)	(0.276)	(6.295)
$C_i Ratio_i/\Sigma_Y \beta_{CRatioY}$	1.613	5.129	2.277	-9.048
	(1.576)	(3.522)	(2.102)	(13.851)
$A_i Ratio_i Y48_t$			-0.021	-1.007
			(0.086)	(0.633)
$A_i Ratio_i Y49_t$			0.013	-0.416
			(0.110)	(0.653)
$A_i Ratio_i Y50_t$			-0.103	-0.836
			(0.075)	(0.672)
$A_i Ratio_i Y51_t$			-0.238	0.101
			(0.159)	(0.693)
$B_i Ratio_i Y48_t$			-0.061	-2.868
			(0.088)	(2.056)
$B_i Ratio_i Y49_t$			0.102	-0.856
			(0.114)	(2.147)
$B_i Ratio_i Y50_t$			0.043	-2.179
			(0.114)	(2.204)
$B_i Ratio_i Y51_t$			0.049	1.350
			(0.100)	(2.274)
$C_i Ratio_i Y48_t$			0.795	-5.511
			(0.636)	(4.636)
$C_i Ratio_i C_i Y49_t$			0.135	-3.973
			(0.809)	(4.774)
$C_i Ratio_i Y50_t$			0.154	-5.446
			(0.794)	(4.972)
$C_i Ratio_i Y51_t$			1.193	5.882
			(0.952)	(5.279)
Controls	Yes	Yes	Yes	Yes
Years	No	No	Yes	Yes
Districts	Yes	Yes	No	No
R^2	0.320	-	0.019	-

Notes: Robust standard errors in parentheses.* significant at 10%; ** significant at 5%; *** significant at 1%. A_i, B_i and C_i are group dummies.

When evaluating the size of the ratio effect, we need to have the mean *Ratio* for each group in mind, which is 14.8 for group A, 5.3 for group B and 1.8 for group C as shown in Table 2. The average ratio effect which is $\beta_{Ratio} Ratio_{Mean}$ is therefore only -5.2 SEK per capita for group A, 0.7 SEK per capita for group B and 4.1 SEK per capita for group C when using the FE estimates. Compared to the intercept effect or the ATE, these effects are very small. The ratio effect appears to be of relatively low importance.

To speculate, a negative ratio effect for small units could be due to reduced activity when small units will be merged with populous units, since the chance of remaining in office is reduced with the size of the amalgam. A positive ratio effect for large units could possibly be explained by organizational differences which influence the ability to exploit the common pool. Although speculation about such finer details can be exciting, our results mostly indicate that the ratio effect is either small or absent. The exploitation of the common pool is largely non-systematic with respect to common pool size. The treated units increase their debt in face of amalgamation, but without much consideration of how large the optimal increase is. It might be that the municipalities simply spend extra resources on available projects as much as they could when facing an amalgamation and finance this by taking loans.

7. CONCLUSIONS

We argue that the Swedish amalgamation reform of 1952 is ideally suited for studying the common pool model of political decision making. We find that the treated (i.e. later amalgamated) municipalities increase their debt considerably more than the untreated municipalities before amalgamation. For the treated units, the common pool effect is of the same size as the increase in debt during the years before the reform, which is a sizeable share of total income (2.8 percent). However, we find no support for the common pool model as it is formulated by Weingast et al. (1981), which predicts the effect to increase with common pool size. Our results show that the common pool size has a small and mostly statistically insignificant effect on the change in debt (in contrast to Tyrefors, 2006). Possibly, this could be caused by limited opportunities for exploiting the common pool, resulting in less than optimal exploitation from each unit's opportunistic perspective.

The reform of 1952 gives rise to a situation very much like the basic common pool model. Failing to find any treatment effect in such an ideal case would have provided rather strong evidence against the common pool model. Interestingly, we do not find complete support for the model even in this clear case. Caution is therefore called for when applying the common pool model to other less typical political situations. However, we do find evidence of free-riding when a common pool is present. The power to tax was used to shift the burden of taxation to people in other political units, and our results indicate that such exploitation can be quite sizeable.

As argued by Weingast et al. (1981) the common pool problem is likely to permeate extensive parts of political decision making. Our results underscore such concerns even though fiscal exploitation before the Swedish amalgamation reform of 1952 appears to have been rather crude and unsophisticated when compared with their theory. Further empirical studies should therefore allow for a very imprecise reflection of the law of 1/n in political outcomes. In any respect, it ought to be an issue of constitutional importance to neutralize incentives for fiscal exploitation.

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APPENDICES

Appendix 1. Additional Descriptive Statistics

Table A1. *Detailed description of variables for the whole population*

Variable	Mean	Std. Dev.	Min	Max
Treat	0.868	0.338	0	1
Ratio	5.731	11.596	0.000	218.252
New	2.916	5.507	0.000	192.498
$\Delta Debt_i$	29.059	91.887	-287.476	1,100.804
$Debt_i$	45.574	56.913	0.000	508.786
Inc_i	1,051.892	299.084	261.741	2,583.301
ΔInc_i	47.693	49.044	-297.525	524.183
Pop_i	1.492	1.822	0.067	24.841
ΔPop_i	-0.019	0.064	-0.359	0.479
$Dens_i$	0.254	0.441	0.002	10.404
$\Delta Debt_{i,t}$	5.812	40.793	-403.555	557.003
$Debt_{i,t}$	53.757	71.791	0.000	966.903
$Inc_{i,t}$	210.378	70.308	31.126	689.051
$\Delta Inc_{i,t}$	9.539	43.883	-362.972	393.913
$Pop_{i,t}$	1.493	1.857	0.067	25.230
$\Delta Pop_{i,t}$	0.000	0.035	-0.310	0.420
$Dens_{i,t}$	0.254	0.447	0.002	10.521
Area	16.996	72.225	0.045	1,814.364
AreaCoun	0.010	0.013	0.00009	0.184
PopDist	23.701	32.443	0.000	483.641

Appendix 2. Additional Cross Section Results

Table A2. *OLS with subset of controls, Ratio with all obs., and First step IV estimates*

Dep. Var: $\Delta Debt_i$	[1] OLS:Inc	[2] OLS:Pop	[3] OLS:Ratio+	[4] IV:TreatI	[5] IV:RatioI
$Treat_i$	19.513***	17.981**			
	(6.618)	(8.259)			
$Ratio_i$	-0.248	-0.484***	-0.042		
	(0.170)	(0.185)	(0.153)		
$Area_i$				0.001***	
				(0.0003)	
$AreaCoun_i$				-8.201***	
				(1.087)	
$PopDist_i$					0.149***
					(0.009)
Inc_i	0.024***		0.044***	-0.0002***	-0.002*
	(0.009)		(0.016)	(0.00003)	(0.001)
ΔInc_i	0.682***		0.622***	-0.0001	-0.011**
	(0.079)		(0.070)	(0.0001)	(0.006)
Pop_i		4.749	2.079	-0.033***	-0.359*
		(3.039)	(2.543)	(0.005)	(0.187)
ΔPop_i		226.039***	48.457	-0.253***	0.475
		(46.891)	(45.191)	(0.096)	(4.040)
Den_i		14.188*	14.764**	-0.054***	0.248
		(7.282)	(7.181)	(0.014)	(0.582)
Districts	No	No	Yes	Yes	Yes
Units	2,280	2,280	2,280	2,280	1,980
R^2	0.159	0.050	0.327	0.577	0.371

Notes: Robust standard errors in parentheses.* significant at 10%; ** significant at 5%; *** significant at 1%.

Propensity score estimates:

$$\begin{aligned}\hat{P}(Treat=1) = \Phi[&-0.001 Inc_i + 0.0000002 Inc^2{}_i - 0.001\Delta Inc_i - 0.667 Pop_i + 0.027 Pop_i{}^2 \\ &\quad (0.0009) \quad (0.0000004) \quad (0.009) \quad (0.048)^{***} \quad (0.003)^{***} \\ &-0.006 Area_i - 4.697 AreaCoun_i + 3.622 \qquad Pseudo\, R^2 = 0.44 \\ &\quad (0.002)^{***} \quad (4.970) \quad (0.586)\end{aligned} \tag{A1}$$

Table A3. *Number and percentage (in parenthesis) correctly predicted from eq. (A1)*

Treat / Predicted Treat	0	1	Total
0	147	153	300
	(0.49)	(0.51)	(1.00)
1	53	1,927	,1980
	(0.03)	(0.97)	(1.00)
Total	200	2,080	2,280
	(0.09)	(0.91)	(1.00)

Appendix 3. Additional Panel Results

Table A4. *Panel RE estimates*

Dep. Var: $\Delta Debt_{i,t}$	[1] RE:Treat	[2] RE:Ratio	[3] RE:Both	[4] IV:Treat	[5] IV:Ratio	[6] IV:Both
$\Sigma_Y \beta_{TreatY}$	24.955***		25.229***	89.035*		140.286***
	(8.260)		(8.254)	(47.124)		(53.103)
$\Sigma_Y \beta_{RatioY}$		-0.299*	-0.312*		-1.201**	-1.445**
		(0.180)	(0.180)		(0.595)	(0.574)
$Treat_i Y47_t$	0.244		-0.195	16.263		22.305*
	(2.220)		(2.219)	(10.380)		(12.442)
$Treat_i Y48_t$	7.059***		6.962***	14.638		27.101**
	(2.037)		(2.036)	(10.517)		(12.543)
$Treat_i Y49_t$	10.991***		10.467***	26.150**		33.580***
	(2.826)		(2.852)	(10.863)		(12.893)
$Treat_i Y50_t$	1.064		1.346	16.175		29.084**
	(3.425)		(3.455)	(10.905)		(12.949)
$Treat_i Y51_t$	5.597*		6.649**	15.809		28.215**
	(3.233)		(3.327)	(10.730)		(12.691)
$Ratio_i Y47_t$		0.021	0.022		0.138	
		(0.045)	(0.045)		(0.232)	
$Ratio_i Y48_t$		-0.033	-0.033		-0.460**	
		(0.051)	(0.051)		(0.232)	
$Ratio_i Y49_t$		0.027	0.025		-0.068	
		(0.049)	(0.050)		(0.231)	
$Ratio_i Y50_t$		-0.096**	-0.100**		-0.407*	
		(0.048)	(0.048)		(0.232)	
$Ratio_i Y51_t$		-0.218	-0.226		-0.403*	
		(0.146)	(0.145)		(0.237)	
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Years	Yes	Yes	Yes	Yes	Yes	Yes
Districts	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.064	0.061	0.065	-	-	-

Notes: Robust standard errors in parentheses.* significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix 4. Additional Results by Population Groups

Table A5. *Cross section OLS and IV estimates for each subgroup*

Dep. Var: $\Delta Debt_i$	[1] OLS:A	[2] OLS:B	[3] OLS:C	[4] IV:A	[5] IV:B	[6] IV:C
$Ratio_i$	-0.262	0.138	1.720	-1.317	-48.051	19.818*
	(0.299)	(0.150)	(2.186)	(1.954)	(268.836)	(10.383)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Districts	Yes	Yes	Yes	Yes	Yes	Yes
Units	559	632	789	559	632	789
R^2	0.518	0.457	0.583	-	-	-

Notes: Robust standard errors in parentheses.* significant at 10%.

Table A6. *Panel FE and IV-FE estimates for each subgroup*

Dep. Var: $\Delta Debt_{i,t}$	[1] FE:A	[2] FE:B	[3] FE:C	[4] IV:A	[5] IV:B	[6] IV:C
$\Sigma_Y\beta_{RatioY}$	-0.530	0.228	7.269*	-4.201	15.965	18.030
	(0.355)	(0.232)	(2.863)	(2.682)	(47.339)	(18.408)
$Ratio_iY48_t$	-0.072	0.028	1.262	-1.711**	0.660	0.629
	(0.089)	(0.076)	(0.897)	(0.822)	(15.202)	(5.404)
$Ratio_iY49_t$	-0.002	0.035	2.245**	-0.431	6.295	1.396
	(0.132)	(0.078)	(0.959)	(0.824)	(14.406)	(5.595)
$Ratio_iY50_t$	-0.107	0.051	1.184	-0.794	7.764	-0.887
	(0.080)	(0.115)	(0.997)	(0.831)	(14.889)	(5.911)
$Ratio_iY51_t$	-0.350*	0.115	2.578**	-1.264	1.245	16.892***
	(0.207)	(0.073)	(1.226)	(0.877)	(14.870)	(6.499)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Years	Yes	Yes	Yes	Yes	Yes	Yes
Districts	No	No	No	No	No	No
R^2	0.011	0.053	0.020	-	-	-

Notes: Robust standard errors in parentheses.* significant at 10%; ** at 5%; *** at 1%.

Interaction term specification, OLS on the cross section and FE on the panel:

$$\hat{\Delta}Debt_i = -\underset{(0.208)}{0.127}Ratio_i + \underset{(0.357)}{0.127}NewPop_i + \boldsymbol{\beta}_{\mathbf{X}}'\mathbf{X}_i + \boldsymbol{\beta}_{\mathbf{D}}'\mathbf{D}_i \quad R^2 = 0.32 \tag{A2}$$

$$\begin{aligned}\hat{\Delta}Debt_{i,t} = & -\underset{(0.105)}{0.092}Ratio_iY_{48} - \underset{(0.083)}{0.079}Ratio_iY_{49} - \underset{(0.071)^{***}}{0.220}Ratio_iY_{50} - \underset{(0.135)^{**}}{0.318}Ratio_iY \\ & + \underset{(0.171)}{0.142}NewPop_iY_{48} + \underset{(0.195)}{0.269}NewPop_iY_{49} + \underset{(0.175)^{*}}{0.320}NewPop_iY_{50} + \underset{(0.272)}{0.254}NewPop_iY_{51} \\ & + \boldsymbol{\beta}_{\mathbf{X}}'\mathbf{X}_{i,t} + \boldsymbol{\beta}_{\mathbf{Y}}'\mathbf{Y}_t + \boldsymbol{\beta}_{\mathbf{U}}'\mathbf{U}_i \qquad R^2 = 0.016\end{aligned} \tag{A3}$$

$\Sigma_Y\beta_{RatioY}$	-0.709 $(0.225)^{***}$	$\Sigma_Y\beta_{NewPopY}$	0.985 (0.423)**