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Assist or desist? Conditional bailouts and fiscal discipline in local governments

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Abstract

Central government bailouts of local governments are commonly viewed as a recipe for local fiscal indiscipline, as local governments learn that the center will come to the rescue in times of trouble. However, little is known about the consequences of bailouts granted conditional on local governments first making efforts to improve the situation. We examine a case in which the Swedish central government provided conditional grants to 36 financially troubled municipalities. We use the synthetic control method to identify suitable comparison units for each of the 36 municipalities. To compare the development of costs and the fiscal surplus of admitted municipalities to that of their most similar counterparts during the decade after the program, we then estimate fixed effects regressions on the resulting sample. The analysis suggests that conditional bailouts did not erode, and may even had induced greater fiscal discipline.

1. Introduction

Should central governments bail out local governments in financial distress? On the one hand, refusing to bail out local governments may lead to defaults, which can be very costly both economically and politically. On the other hand, bailouts may create problems of soft budget constraints: noting that the central government comes to the rescue in times of trouble, local governments may come to expect that bailouts will be available when needed. Thereby, their incentive for fiscal discipline is eroded.[1] Several empirical studies of fiscally decentralized countries indicate that bailouts lead to lower fiscal discipline.[2] The relevance of the dilemma is further illustrated by the

[1] For theoretical explanations of the existence and effects of soft budget constraints, see e.g. Kornai (1979); Wildasin (1997); Goodspeed (2002); Inman (2003); Desai and Olofsgård (2006), and Sas (2014).

[2] See Rodden (2002); Rodden et al. (2003); Plekhanov (2006); Bordignon and Turati (2009); Pettersson-Lidbom (2010); Fink and Stratmann (2011); Lusinyan and Eyraud

development in the Euro area and the US after the financial crisis in 2008, with recent examples both of Euro countries (e.g. Greece) and regions (e.g. Andalusia and Valencia) receiving bailouts and of US cities (e.g. Detroit) going bankrupt.

A possible way out of the dilemma may be to bail out the local government, but condition payment on actions that lay the ground for future fiscal discipline. We investigate a program run during 2000-2002 in which the Swedish central government provided conditional bailouts to 36 municipalities in fiscal distress: the municipalities were granted extra funds, but payment was contingent on them first cutting costs and achieving budgetary balance.[3] The conditions were enforced: no municipality received the full grant until both conditions were met. As all municipalities admitted to the bailout program eventually managed to meet the conditions, their fiscal discipline evidently improved in the short run. But the more interesting question is whether their newly acquired fiscal discipline was retained after the program had ended. To examine the long run effects, we analyze the evolution of operating *costs of services* and revenues net of costs (henceforth referred to as the *fiscal surplus*) during the decade after the launch of the program.

To draw firm conclusions about whether and how the bailout program affected fiscal discipline, we would ideally have wanted municipalities to be randomly assigned to the program. However, non-random assignment is an inescapable feature of bailout programs since they, by design, are directed only to units in fiscal distress. In the current context, this is illustrated by the fact that out of the 290 Swedish municipalities, only 59 chose to apply to the program and no more than 36 of these were admitted. The experience of applying to but being denied to participate in the program is also a kind of treatment: a signal that the budget constraint is hard, or at least harder than expected. We therefore examine the 23 rejected municipalities as well.[4]

To deal with the selection problem, which pertains to both the admitted and the rejected municipalities, we combine matching with fixed effects (FE) estimations. For each municipality that applied to the program, we use the synthetic control method – developed in Abadie and Gardeazabal (2003) and

(2011); Sorribas-Navarro (2011), and Baskaran (2012). Kornai et al. (2003) survey the theoretical literature and provide further empirical examples.

[3]The transfers were not last minute rescue attempts in the face of imminent defaults. We use the term "bailout" to comply with the terminology in the literature on soft budget constraints, where the term is also used to denote discretionary transfers to cover deficits (see e.g. Fink and Stratmann (2011, p. 367)).

[4]As most municipalities do not end up in fiscal distress, we are interested in the average treatment effect on the treated (Imbens and Wooldridge, 2009).

Abadie et al. (2010) – to construct a synthetic municipality from the set of municipalities not affected by the program. We then estimate FE regressions on the samples of actual municipalities and their synthetic controls. Our FE model identifies the effect of the program if the average outcomes of the actual and synthetic municipalities would have followed a parallel path in the absence of the program. We believe this assumption is more likely to hold for the matched sample than for an unmatched one, as both the levels and trends of costs for actual and synthetic municipalities are very close during the pre-program period, and they are similar with respect to important predictors of costs and fiscal surplus.

According to our preferred specifications, the admitted municipalities have on average reduced costs permanently and increased the fiscal surplus most years after the program. As the synthetic control method enables us to examine the difference between the actual and synthetic costs of each admitted municipality, we moreover analyze each case separately. We find that the average cost reduction is driven by a third of the admitted municipalities; most of the others have not reduced costs significantly, while only two seem to have increased costs. The "cost-reducers" do not drive the positive average effect found for the fiscal surplus, however. This suggests that most of the admitted municipalities sought to deal with their fiscal problems, though by different strategies. As for the rejected municipalities, we find little that indicates important or lasting effects on their fiscal discipline.

One concern for the policy implications of our results is that we can only estimate how the applicants fare relative to municipalities that did not apply to the program. As all Swedish municipalities were most likely aware of the program, their fiscal discipline may in principle have been indirectly affected by it. Such spillover effects may imply that it would have been better for the central government to abstain from granting bailouts, even if the admitted municipalities compare favorably to municipalities that only heard about the program. It should however be noted that the average fiscal surplus of the Swedish municipalities have increased quite sharply in the period after the program ended (Persson, 2013). This development suggests that the program has not had detrimental effects on the fiscal discipline of municipalities in general.

Spillover effects may be especially likely to pertain to municipalities that are neighbors to the admitted municipalities (c.f. Pettersson-Lidbom, 2010). This presents us with a dilemma, as neighboring municipalities share many features with the treated municipalities and thus figure prominently in the synthetic controls. Upon excluding the neighbors from the comparison group, the estimated impact of the program on the admitted municipalities becomes insignificant, while the estimations indicate adverse effects on the fiscal discipline of the rejected municipalities. As the exclusion of neighbors imply considerably worse fit of the synthetic controls, it is however difficult to know how much confidence to put in these estimates. Moreover, the program was not a clear-cut signal of a softened budget constraint to municipalities that were not admitted: it was clearly delimited in time, employed relatively clear selection criteria, and rejected a large share of applications (almost 40 percent). Additionally, a substantial share (36 percent) of the neighbors to the admitted municipalities are also neighbors to at least one rejected municipality. These neighbours received a mixed signal about the availability of bailouts.

Importantly, none of our estimates support the idea that the bailout program has undermined the fiscal discipline of the admitted municipalities. The admitted municipalities furthermore compare favorably to the rejected municipalities, despite that the rejected municipalities received a signal of hard(er) budget constraints. Even a cautious interpretation of our results thus stands in contrast to findings from settings with bailouts that were not given conditional on local governments first making efforts to improve their situation (see the examples in footnote 1). This suggests that conditions of the type used in the Swedish case may be key to dampening the soft-budget effect of central government bailouts.

A plausible interpretation of our findings is that the conditions reduce the attractiveness of future bailouts – put differently, conditions increase the attractiveness of retaining fiscal discipline. The lack of variation in conditions prevents us to test this explanation directly, but our paper is, to the best of our knowledge, the first attempt to empirically investigate the impact of this type of conditional bailouts on the fiscal discipline of local governments. While we certainly do not advocate bailouts in general,[5] for a policy maker contemplating *how*, rather than whether, to assist local governments in financial trouble, the type of conditional assistance used by the Swedish government seems like a better option than the unconditional bailouts used in many other cases.

The rest of the paper is structured as follows: Section 2 outlines the institutional background, Section 3 presents the data, Section 4 describes our estimation strategy and introduces the synthetic control method, while Section 5 contains the estimation results. Section 6 explores heterogeneity in the program effects. Section 7 discusses potential explanations for our

[5]It would in many cases be preferable to put in place institutional arrangements that prevent bailouts in the first place. The empirical results in Foremny (2014) suggest some ways to mitigate the soft budget problem in fiscally decentralized countries.

results, and Section 8 concludes.

2. Institutional background and description of the program

The 290 Swedish municipalities are responsible for the provision of several important public services such as primary to upper secondary schooling and elderly care. Municipal expenditures accounted for approximately 14 percent of Swedish GDP in 2010, almost half of the public sector's total expenditures for final consumption and investments. On average, about 12 percent of revenues come from a rule-based equalization system. *Discretionary* central government grants, which are more likely to lead to soft budget constraint problems (Rodden and Eskeland, 2003), have varied in prevalence over time. Before 1993, municipalities could apply for grants to cover budget deficits every year (see Pettersson-Lidbom, 2010, for a discussion about these grants in relation to local fiscal discipline). Since a major reform of the grant system in 1993, the central government has been considerably more restrictive with discretionary grants. Still, it is unlikely that municipalities view their budget constraints as binding under all circumstances. Equal access to public services in the whole country is an important objective for the central government and municipalities are prohibited by law to default on debt; thus, the central government would likely step in if a municipality was threatened by insolvency (Dahlberg and von Hagen, 2004).

The program under study was announced in August 1999, in connection to the approaching implementation of the Balanced Budget Act (which would come into effect in the year 2000). The act states that municipalities have to attain budgetary balance each year, and if deficits occur, they have to be recovered within the subsequent three years.[6] However, in 1999 the central government noted that quite a few municipalities would have substantial problems with achieving budgetary balance on time, due to structural factors perceived to be beyond the control of local politicians. In the fall of 1999, the government therefore decided to install a committee, *Kommundelegationen*, to investigate whether some municipalities should be granted financial assistance to mitigate their problems. To be considered by the committee, municipalities had to apply to the bailout program by November 1999 at the latest; in total, 59 municipalities applied.[7]

[6] Nevertheless, the law allows for exceptions, for example if the deficit is caused by unconverted losses in stocks and bonds, or if the municipality has previously amassed large amounts of wealth. It is in practice not enforced by any sanctions either (Swedish Government, 2004).

[7] Two more municipalities initially applied but withdrew their application before the

According to an official report, the committee used the following criteria to decide whether each applicant should be considered further or not (SOU, 2003):

- Structural problems, e.g. unfavorable demographics and low employment rate.
- Projected deficits over the coming three years.
- Weak balance sheet, in particular a high level of debt.
- Limited possibilities of increasing revenues.

The municipalities whose applications were not rejected by the committee were asked to come up with a proposal of cost reductions. The proposal formed the basis for a discussion of the necessary conditions to be fulfilled in order to receive the grant. The resulting agreements were approved by the respective municipal councils (SOU, 2003).

In early October 2000, the government took the formal decision about admission into the bailout program. The government's decision fully accorded with the committees's proposal (SOU, 2003, Appendix 1). Surprisingly, given the above criteria, there were no significant differences between admitted and rejected municipalities with regards to the cost structure, debt level and demographics (see Table 1 in Section 3). Instead, projected future revenues was the most important selection criterion according to the official motivations for rejection (Swedish Ministry of Finance, 2000). It is important to note that although the three committee members were politicians (two were social democrats and the third was from the centrist party), political factors (such as key voter districts or party concerns) do not seem to explain selection into the program (Dahlberg and Rattsø, 2010). The size of the grant was non-negligible; on average, it amounted to four percent of the program municipalities' cost level in the year 2000. The grant was supposed to be set as a fixed (i.e., same for all admitted municipalities) share of the cost reductions in the agreement; however, it is not entirely clear from the official documentation whether this practice was strictly applied (SOU, 2003).

To receive the full grant, the 36 admitted municipalities had to meet two conditions by the end of year 2002. First, they would have to cut the costs specified in their agreement with the government. Second, they would have to balance their operating budget; that is, have gross total revenues at least as high as total costs.

Due to separate operating and capital budgets, most municipalities need to run surpluses on average to be able to finance investments without taking

government made its decision. These two are not included in the group of rejected municipalities in our estimations.

on more debt. A balanced operating budget is therefore not such a harsh requirement for fiscal discipline as it may sound, and almost all municipalities have currently some form of surplus target (Brorström et al., 2009). Still, given the size and the frequency of deficits in the admitted group during the late 1990's, the condition was probably not trivial to fulfill.

The municipalities were continuously monitored throughout the program (SOU, 2003), but whether the central government would actually be tough and apply the conditions, or give in and pay the whole sum anyway, was uncertain at the beginning of the program. For example, a government audit report from 2000 raised concerns about the central government's toughness and encouraged the government to terminate the program (Swedish National Audit Office, 2000, p. 9). Other short-run studies of the program question whether the program succeeded to make a substantial change toward fiscal discipline in more than a few municipalities (Siverbo, 2004), or argue that the fiscal problems of the admitted municipalities were not primarily structural (Pettersson-Lidbom and Wiklund, 2002).

In 2002, the admitted municipalities received 25 percent of the grant after having shown that they had started to cut costs in 2001. Ten municipalities succeeded to fulfil all conditions in their agreements already in 2001, and therefore received the whole grant in 2002. Of the remaining 26, all but two municipalities fulfilled the program conditions in 2002 and thus received the remaining part of their grants in 2003. The last two received the remaining part of their grants in 2004, after having achieved budgetary balance in 2003.

The rejected municipalities were in general left to manage their finances on their own, but a related program complicates the story for both groups somewhat. In several Swedish municipalities, a real estate boom-and-bust in the beginning of the 1990's left the publicly owned housing companies highly indebted and with a large over-supply of apartments. In the late 1990's, the central government installed a committee (*Bostadsdelegationen*) to assist with the reconstruction of insolvent housing companies. During 1998-2005, as many as 52 municipalities were in the housing program at some time. In fact, 23 out of the 36 in the bailout program under study also received assistance from the housing program (Swedish National Board of Housing, Building and Planning, 2005).[8] For these 23 cases, our estimates may be interpreted as the combined effect of the two programs. Five of the rejected municipalities were also in the housing program. Importantly though, estimates shown in the supplementary material (Section 2.4.2) to this paper indicate that the

[8]Of these 23, six entered the housing program in 1999, before they were admitted by Kommundelegationen, and four entered the housing program after 2002.

housing program is not driving our results. Housing is moreover a small part of municipal services in terms of operating costs (although real estate may well be a large part of the balance sheet): the areas where costs related to housing could be accounted for made up less than 3 percent of total municipal costs in 2010 (Statistics Sweden (2012a) and own calculations).

3. Data

We obtain municipality-level data on a set of economic, political and structural variables for all 290 municipalities and for each year between 1993-2010 from Statistics Sweden.[9]

Of the available measures of fiscal performance, we find the two prime candidate measures from the balance sheet – the debt level and the equity ratio – unsatisfactory for three reasons. First, there were substantial differences among municipalities in the accounting of debt before the Municipal Accounting Act came into effect in 1998 and some important differences still remain (notably in regard to the accounting of pensions). Second, the debt level and the equity ratio are heavily influenced by extraordinary historical events, such as sales of e.g. public companies and real estate. Third, even changes in the debt level and the equity ratio are not straightforwardly interpreted in terms of fiscal discipline. For example, investments financed by borrowing increase the debt level and decrease the equity ratio but may be fiscally sound if they decrease future costs or increase future revenues enough. However, for large municipal investments such outcomes may need several decades to materialize.

We therefore delimit our choice set to the items on the revenues and costs statement, and settle for the (log of) *per capita operating costs of services* and *per capita fiscal surplus* as dependent variables.[10] As the conditions of the program were stated in terms of costs and the fiscal surplus, these variables were evidently prioritized by the Swedish government.

We use the term "costs" rather than expenditures as operating costs do not include direct capital outlays. In 2010, costs for employees made up the largest category (about 54 percent) followed by costs of buying services from

[9]The reform of the intergovernmental equalization grant system is the prime reason why we do not collect data further back than 1993. Besides, there were other major reforms put in place about the same time; specifically, the school system and the provision of long-term care to the elderly and disabled came under municipal responsibility in 1992. Comparisons further back in time may thus be misleading.

[10]We log costs to obtain better fit in the regressions and for interpretational ease. All economic variables are in 2010 prices.

external providers (about 18 percent). The largest service areas in terms of costs were (in order): elderly care, education, child care, infrastructure, and social services (Statistics Sweden, 2011). In our preferred measure of costs, we exclude some categories of costs (write offs, and financial costs such as interest rate payments on debt and pensions), which are to a large extent generated by historical decisions and can only be marginally influenced by the current leadership of a municipality. We also exclude so-called extraordinary costs. Generally, almost all revenues and costs are regarded as ordinary; extraordinary is reserved for e.g. natural disasters and sales of firms owned by the municipality (Council for Municipal Accounting, 2006). It should be emphasized that the excluded categories constitute a small share of total costs (4.4 percent on average in 2010), and that our results do not change when we run FE regressions with total costs per capita as the dependent variable instead (see Section 2.4 in the supplementary material).

The fiscal surplus is defined as gross total municipal revenues minus total costs (total costs includes the cost categories we exclude above). In 2010, the three largest sources of revenues were: proportional income taxes (65 percent ; the tax rate is set freely by each municipality), fees (21 percent), and central government grants from the rule-based equalization system (12 percent) (Statistics Sweden, 2012b). The measure excludes extraordinary revenues/costs. In the supplementary material (Section 2.4), we describe results from FE regressions using a measure of the fiscal surplus that do include such revenues and costs. The point estimates are very similar to those obtained with our preferred measure.

The dataset contains several potential cost predictors which are used as inputs in the synthetic control matching algorithm. The ability to raise revenues is accounted for by the *tax base* (taxable income per capita), *equalization grants* (per capita), and the *employment rate* (for the population +16 years). We account for the demographic structure by the *population size*, the share of children (*share 0-14*) and the share of elderly (*share +65*). We moreover account for differences in policy preferences and the political landscape by the *share of right-wing seats* in the municipal council,[11] the Herfindahl index of political concentration (*herfindahl*),[12] and the *number of seats* in the municipal council.[13] Table 1 shows descriptive statistics for

[11]Pettersson-Lidbom (2008) find that municipalities with left-wing governments have higher levels of spending. However, in line with the model of Persson and Svensson (1989), right-wing municipal governments accumulate more debt when their probability of electoral defeat is high (Pettersson-Lidbom, 2001).

[12]Defined as $H = \sum_i (\text{vote share of party } i)^2$ (see e.g. Borge, 2005).

[13]In the political economy literature, the size of the decision making body has been

these variables (unless otherwise stated, the data refers to 1999 values). The table also describes a set of variables measured in 1998; these are intended to reflect the committee's selection criteria.

The data also contains two proxies for initial bailout expectations: (i) the number of discretionary central government grants received during 1979-1992, and (ii) the average share of each municipality's neighbors that received such discretionary grants over the period 1979-1992.[14] Using a ten percent significance level, the latter variable is the only one that differs significantly between the admitted and rejected municipalities: a larger share of the admitted municipalities' neighbors received transfers during the earlier regime. Compared to those that did not apply, all variables in Table 1 are significantly different on at least the 10 percent level for both groups of applicants. Applicants on average had smaller tax bases, received larger equalization grants, had lower employment rates, had smaller and older populations, more left-wing voters, a municipal council that was less fragmented and had fewer seats, higher debt and lower equity ratio in 1998, lower population growth between 1994 and 1998, and more own bailouts as well as a higher share of neighors with bailouts in 1972-1992.

4. Empirical strategy

The non-random selection into the program means that a simple regression of per capita costs on program status on the sample of all municipalities is unlikely to capture the causal effect of the program. As high costs and poor fiscal performance were reasons to apply for the program, it is difficult to envisage an instrumental variable that would be correlated with program status but uncorrelated with performance (conditional on program status). Consequently, it is even more difficult to find two separate instruments for admission and rejection.

Instead, we use the synthetic control method – described in Section 4.1 and 4.2 – to select a comparison group containing only units that are similar to the municipalities applying to the program. To study the average effects of the program, we then estimate fixed effects regressions on the samples of actual municipalities and their synthetic controls (see Section 4.3 for details). The foremost advantage of the FE framework, relative to a simple comparison of how the outcome variables have developed in actual and synthetic municipalities after the program, is that it allows us to explicitly control for

argued to influence costs (Weingast et al., 1981). See e.g. Perotti and Kontopoulos (2002) and Pettersson-Lidbom (2011) for (conflicting) empirical evidence.

[14] Neighbors are defined as sharing land borders.

Table 1: Descriptive statistics for admitted, rejected, and other municipalities

Variables	Mean Adm.	Mean Rej.	Mean Others	Min. All	Max. All
tax base	112	111.7	117	90.4	215.7
	(10.1)	(11.3)	(15.6)		
equalization grants	10.3	9.2	6.8	-7	23.2
	(5.1)	(4.6)	(3.9)		
employment rate, 16+ (%)	50.5	52.1	55.9	37.6	69.9
	(5.4)	(4.4)	(5)		
population size (in thousands)	12.2	14.7	35.2	2.7	743.7
	(6.5)	(15.8)	(63.5)		
share 0-14 (%)	17.9	18.5	19.1	13.5	24.2
	(1.5)	(1.5)	(1.7)		
share +65 (%)	21.7	20.9	18.4	8.1	28.8
	(3.9)	(2.5)	(3.7)		
share of right-wing seats (%)	35.5	39.7	45.9	8.6	67.7
	(13.8)	(13.9)	(11.4)		
herfindahl	0.26	0.26	0.24	0.17	0.51
	(0.05)	(0.05)	(0.04)		
number of seats	40.1	40.6	47.9	31	101
	(7.4)	(9.3)	(11.9)		
total costs 1998	45.5	43.8	39.9	29.9	57.5
	(5.7)	(4.6)	(4.6)		
debt 1998, excl pensions	19.3	23.1	15.9	4.1	73.6
	(8.5)	(13.9)	(10.5)		
equity ratio 1998 (%)	50.4	47.3	59.1	-5.5	92.7
	(17)	(21.7)	(17.9)		
population growth 1994-98 (%)	-4.7	-4.8	-1.2	-8.4	13.3
	(1.9)	(2.5)	(3.3)		
number of bailouts 1979-92	7.9	7.7	4.2	0	14
	(4.1)	(3.3)	(3.8)		
share neighbors w bailout 1979-92 (%)	50	40.8	30.3	0	1
	(16.6)	(11.8)	(19.7)		

Note: The table shows 1999 variable values unless otherwise stated. Economic variables are all in thousands of SEK per capita and 2010 prices. The mean refers to the 36 admitted, 23 rejected, and all other municipalities, respectively. Standard deviation of mean in parentheses. Minimum and maximum refers to the sample of all municipalities.

time-invariant unobservables. The FE frameworks also allows us to include a set of covariates to examine the extent to which detected difference between actual and synthetic municipalities are driven by post-program changes in observables. However, it turns out that adding covariates in most cases does not change our estimates of the program effect at all (see Section 2.4 in the supplementary material). As it is difficult to rule out that the program may have affected some of the covariates, we view these specifications as inferior to the ones without covariates and only comment on these in the few instances where covariates make a difference.

As the fixed effects turn out to have little impact on the estimates for our preferred sample, we lastly use the estimated difference between each municipality's actual and synthetic costs to explore heterogeneity in responses to the program. To draw inference on the significance of the actual-synthetic difference for each municipality (i.e. to classify the change as a reduction, as no change, or as an increase) we create empirical distributions of placebo effects by estimating synthetic controls also for the municipalities that never applied to the program (see Section 6 for a fuller description). The ability to perform this kind of heterogeneity analysis on a case-by-case basis is a strong advantage of the synthetic control method compared to other matching methods, and the prime reason why we chose this method to augment our FE sample.

4.1. The synthetic control method

The synthetic control method for case studies was first used in Abadie and Gardeazabal (2003) and further developed in Abadie et al. (2010). For each municipality i affected by the program, a synthetic control municipality is constructed as a weighted combination of the j municipalities not affected by the program (the "donor pool"). The weights are chosen to make the synthetic control similar to the program municipality in terms of pre-program characteristics, and to make the synthetic control reproduce the pre-program outcome path for the actual municipality. To increase chances that the algorithm succeeds to reproduce the outcome path, we only run the algorithm for one of our dependent variables, namely costs. Our second dependent variable, the fiscal surplus, fluctuates a lot from year to year for idiosyncratic reasons which presents a challenge for the algorithm. We find it plausible to assume that municipalities that are similar in terms of costs, demographics and political characteristics also are similar in terms of fiscal surplus, and thus that the synthetic municipalities developed for costs also are reasonable comparison units when it comes to the fiscal surplus.

Technically, let the donor pool be of size j, let w denote a $j \times 1$ vector of weights, Z^{dp} a $k \times j$ matrix of k cost predictors and y_t^{dp} a $j \times 1$ vector of pre-program outcomes at time t. Let T_0 denote the period when the program starts. The synthetic control algorithm searches for weights w that make

$$\begin{cases} Z_i = Z^{dp} w \\ y_{it} = \sum_j w_j y_{jt}^{dp} \qquad \forall t < T_0 \end{cases} \tag{1}$$

hold, where Z_i are the cost predictors and y_i is the time-t pre-program outcome for a municipality affected by the program. In case there is no w that make these equations hold exactly, the weights are chosen to make the synthetic control as similar to the actual municipality as possible. To do this, the algorithm minimizes the Mean Square Prediction Error (MSPE) over the pre-program period.

A large pre-program MSPE implies that the pre-program similarity of the actual and the synthetic unit is poor. As the method then has failed to construct a valid counterfactual, using such estimates for inference can be questioned (Abadie et al., 2010). However, there is no convention developed regarding the MSPE cut-off of a "sufficiently good" synthetic control. We evaluate our results at different cut-offs for the pre-program root MSPE (RMSPE). Note that the RMSPE can be interpreted as a difference in percent (because the dependent variable is in logarithms); thus, if pre-RMSPE is below 0.05, the absolute difference between actual and synthetic unit costs is lower than 5 percent on average during the pre-program period.

Estimation is performed by the *synth* package for Stata. In Z, we include the debt level and equity ratio in 1998, population growth between 1994 and 1998, the average share of neighbors receiving a discretionary transfer in 1978-1992, and the average over the whole pre-treatment period (the default option in *synth*) of the other variables shown in Table 1. These characteristics are statistically significant in initial regressions of costs for the whole sample of municipalities (see Section 1.1 in the supplementary material). We also include three lags of the dependent variable (1993, 1996 and 1998) in Z.

Two features of the synthetic control method are potentially problematic in our setting. As the risk of bias decreases with the number of pre-program periods (Abadie et al., 2010), there may be too few pre-program years to produce good controls. Moreover, the method may fail to construct good controls for units that are extreme in terms of pre-program characteristics, as it is difficult (or even impossible) to find suitable combinations of the donors for such units.[15] Recalling the descriptive statistics (Table 1), the

[15] More formally, this may be the case if the set of pre-program predictors of a unit falls far from the convex hull of the set of predictors of the units making up the synthetic control, in which case the identifying assumptions of the synthetic control method may not even hold approximately (Abadie et al., 2010).

municipalities applying for the program are quite likely to be extreme. Importantly, though, the relevance of these two concerns can be judged after the estimation, by examining the pre-program fit of each synthetic control.

4.2. Selection of donor pool

One advantage of the synthetic control method is that it implies a data-driven choice of comparison group (Abadie et al., 2010). Nevertheless, this does not imply that any municipality should be included in the donor pool. We exclude the admitted and the rejected municipalities from the donor pool, as they were directly affected by the program.

Because the concurrent housing program (see Section 2) may have affected costs directly as well as indirectly (through bailout expectations), we exclude municipalities that were admitted to or rejected from the housing program. We also exclude large cities (as defined by the official classification from Statistics Sweden), which, due to their different cost structure and labor market, are unlikely to be suitable comparison units, and the municipality of Gotland, which has a broader set of responsibilities than the other municipalities. Other municipalities are excluded for more technical reasons, namely municipalities that were formed during or after the pre-program period and two municipalities that were formed in 1992 (for which we lack data on some matching variables).

A particularly difficult choice is whether or not to include neighboring (to the admitted) municipalities in the donor pool. As neighbors are likely to share the same economic, political, and structural characteristics, and experience similar shocks, they are likely to be important contributors to the synthetic controls. However, if neighbors keep track of what is going on in bordering municipalities, it is possible that the neighbors of admitted municipalities interpreted the admission of their neighbors as a general softening of the municipal budget constraint and thus relaxed their fiscal efforts. The results in Pettersson-Lidbom (2010) provide a reason for such suspicions: using the frequency of central government discretionary grants to neighboring municipalities as an instrumental variable for expectations of own future grants, Pettersson-Lidbom showed that such expectations led to higher debt levels during the 1980's in Sweden. We would argue that spillover effects on other municipalities – neighbors or not – are less likely in the present context: in contrast to what was the case during the earlier regime of discretionary deficit grants, the program studied here was limited in time, employed relatively clear selection criteria and rejected a large share of applications (almost 40 percent). It is therefore far from obvious that other municipalities interpreted the program as a significant softening of the budget constraint. Adding to this point, a substantial share (36 percent) of the neighbors to admitted municipalities are also neighbors to at least one rejected municipality. These municipalities thus received a mixed signal.

If we could prove that there was no spillover effect of the program on the neighbors, we would most definitely want to include them in the donor pool. Since it is impossible to prove this, we estimate the synthetic controls twice: once including and once excluding the neighbors of admitted municipalities from the donor pool. The donor pool consists of 136 municipalities when neighbors are included, and 103 when neighbors are excluded.[16]

4.3. Fixed effects estimations

The fixed effects models are estimated on two types of samples: one containing the admitted municipalities and their synthetic controls, one containing the rejected municipalities and their synthetic controls. To compute values of the dependent variables for the synthetic municipalities, we use the weights obtained from the synthetic control estimation: the variable value for the synthetic control is the weighted sum of the variable values for the municipalities that comprise the synthetic control.

Our main estimation equation is

$$y_{it} = \alpha + \sum_{t=T_0}^{2010} \gamma_t D_{it} + \lambda_t + \mu_i + \varepsilon_{it} \tag{2}$$

where y_{it} is either the log of per capita costs of services or the per capita fiscal surplus, D_{it} is a dummy variable that capture the year-specific program effect; i.e. the t'th dummy equals 1 for admitted (rejected) municipalities all years $t \geq T_0$ and are zero for all other observations – in particular, it is always zero for the synthetic municipalities. We return to the issue of which year that should be regarded as T_0, i.e. the starting year for the program, in Section 5.1. λ_t is a vector of time dummies, μ_i is a vector of fixed effects for each municipality – note that the actual and synthetic versions of municipality i have separate fixed effects – while ε_{it} is an idiosyncratic error term.

A specification with separate program dummies for each post-program year has two advantages over one with a single program dummy for the post-program period. First, we can compare the average effect for each year with the raw difference from the synthetic control estimations. Second, Laporte and Windmeijer (2005) show that if the yearly effects differ, then a single-dummy version may be biased.

[16]The number of neighbors, defined as sharing a land border with an admitted municipality, is larger than 33, but many neighbors are already excluded for the other reasons mentioned above. Of these 33, 12 are also neighbors to the rejected municipalities.

This FE specification is essentially a difference-in-differences (DID) model, which identifies the effect of the program if the average outcomes of the admitted municipalities, the rejected municipalities and their respective synthetic control groups would have followed a parallel path in the absence of the program (e.g. Abadie, 2005). In the estimation of costs, the synthetic control approach increases the likelihood that the identifying assumption holds, as the synthetic controls match the actual municipalities in terms of both the pre-program trend and the level of costs. In the estimation of the fiscal surplus, it should again be noted that we assume that the municipalities contributing to the synthetic controls for costs are also suitable comparison units for the fiscal surplus. As shown below, our actual and synthetic municipalities show roughly parallel trends for the fiscal surplus during the years preceding the program, which adds to the plausibility of the assumption.

Bertrand et al. (2004) warn against the possibility of downward-biased standard errors due to serial correlation in DID models spanning many years. In the baseline estimations, we therefore use Stata's *cluster* command to cluster on municipality, a solution that performs satisfactorily in Bertrand et al.'s simulations for a comparable number of treated units. We also estimate a two-period specification. Here, the average outcome in the pre-program period is compared to the average outcome in the post-program period. This specification should be less sensitive to the problems discussed by Bertrand et al.

In the supplementary material, we show results from a large number of additional robustness checks. For instance, we employ a two-way clustering method developed by Cameron et al. (2011) to see if correlations between synthetic municipalities bias our standard errors downwards. None of the robustness checks results in changes to our main conclusions.

5. Results

In Section 5.1, we inspect the synthetic controls and their goodness of fit. The main results are then presented in Section 5.2, followed by some robustness checks in Section 5.3.

5.1. Synthetic control estimations and fit

As the program was announced in the fall of 1999 and the admission decision was not made until one year later, we let the *synth* algorithm minimize the MSPE over 1993-1999. Though the donor pool contains more than 100 municipalities, the synthetic controls generally consist of only a handful of

Table 2: Average pre-RMSPE per synthetic control estimation

	Admitted		Rejected	
pre-RMSPE	Incl neighbors	Excl neighbors	Incl neighbors	Excl neighbors
cut-off level	(1)	(2)	(3)	(4)
None	0.0189	0.0261	0.0251	0.0323
	(35)	(34)	(22)	(22)
0.05	0.0180	0.0218	0.0222	0.0285
	(34)	(30)	(21)	(20)
0.03	0.0140	0.0159	0.0184	0.0228
	(28)	(22)	(16)	(10)
0.02	0.0117	0.0137	0.0128	0.0134
	(23)	(17)	(9)	(7)

Note: The left-most column shows the cut-off level in terms of pre-RMSPE. The number in parentheses shows the number of municipalities whose pre-RMSPE<cut-off. Even without a cutoff, the numbers are lower than the full 36 admitted and 23 rejected. We were unable to construct synthetic controls for admitted municipality Älvdalen and rejected municipality Gullspång, due to missing data for some years. When we exclude neighbors, we also fail to construct a synthetic control for admitted municipality Dorotea.

municipalities.[17]

A comparison of the pre-program variable values of each municipality and its synthetic control shows that the algorithm generally succeeds to construct good matches in terms of costs as well as cost predictors, though large fluctuations in actual costs are a complicating factor in some cases (full results available on request). Table 2 shows the average pre-program RMSPE in each of the four estimations (admitted or rejected; donor pool including or excluding neighbors) at different cut-off levels.[18] The pre-program RMSPEs are in the order of 0.01-0.03, i.e. the prediction errors during 1993-1999 typically amount to 1-3 percent of the yearly cost level. At most cut-offs, the synthetic controls of admitted municipalities have a better fit than those of the rejected municipalities. The number of municipalities passing the cut-off criterion (pre-RMSPE<cut-off) naturally decreases as the cut-off becomes stricter. The decrease is especially drastic in the estimations where neighbors are excluded from the donor pool, which confirms our anticipation that neighbors are similar to the municipalities and therefore important contributors to the synthetic controls.

[17]For the admitted municipalities, the median number of contributing donors is 6. 75 percent of the admitted municipalities have more than 4 but fewer than 9 contributing donors.

[18]Lowering the cut-off even further to 0.01 reduces the number of placebo municipalities substantially (from 97 when pre-RMSPE < 0.02 to 37).

To examine whether the parallel trend assumption appears plausible, we look at the pre-program trends of costs and the fiscal surplus for the admitted municipalities, the rejected municipalities and their synthetic controls. Figure 1 shows logged costs per capita and Figure 2 shows the per capita fiscal surplus (in thousands of SEK). The solid black lines represents the averages for the admitted (left panel) and the rejected municipalities (right panel), while the dashed gray lines represent the averages for their respective synthetic controls. For reference, we have also included the averages of the respective donor pools (solid gray lines). All relevant municipalities are included in the computation of the averages, irrespective of the pre-program RMSPE of the synthetic controls. The solid vertical line in the figures marks the year when the government took the decision about admission into the program (i.e. year 2000).

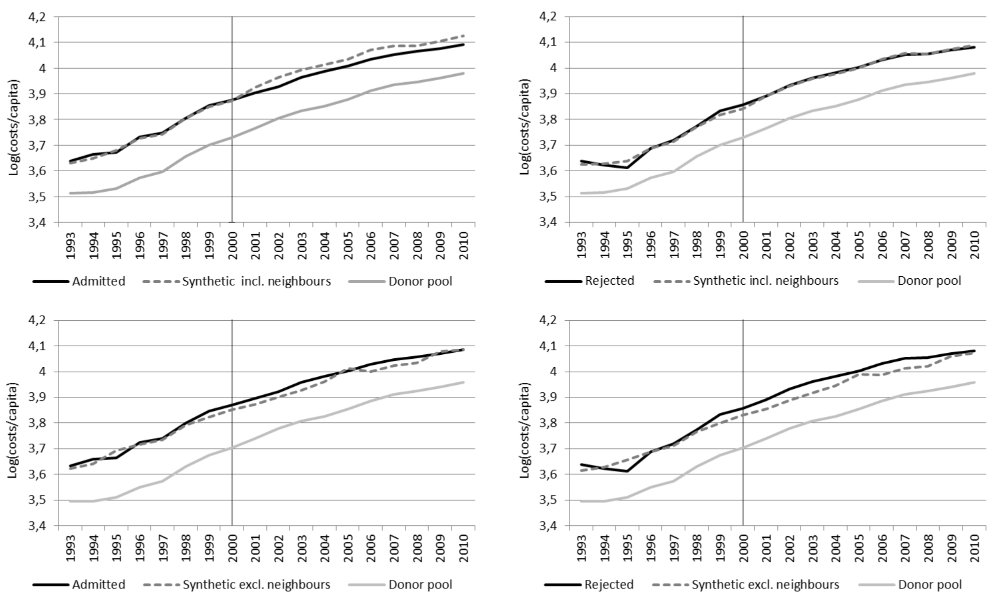

Figure 1: Actual and synthetic costs

Starting with costs, the admitted as well as the rejected municipalities are very close to their synthetic controls both in levels and in trends during the pre-program period when neighbors are included in the donor pool (top panels, Figure 1). As indicated by Table 2, the fit deteriorates when neighbors are excluded (bottom panels, Figure 1). Much of this deterioration arises due to bad fit in 1999, when both groups diverge from their synthetic controls.[19]

[19]The sensitivity to the inclusion of neighbors motivates a further investigation. We

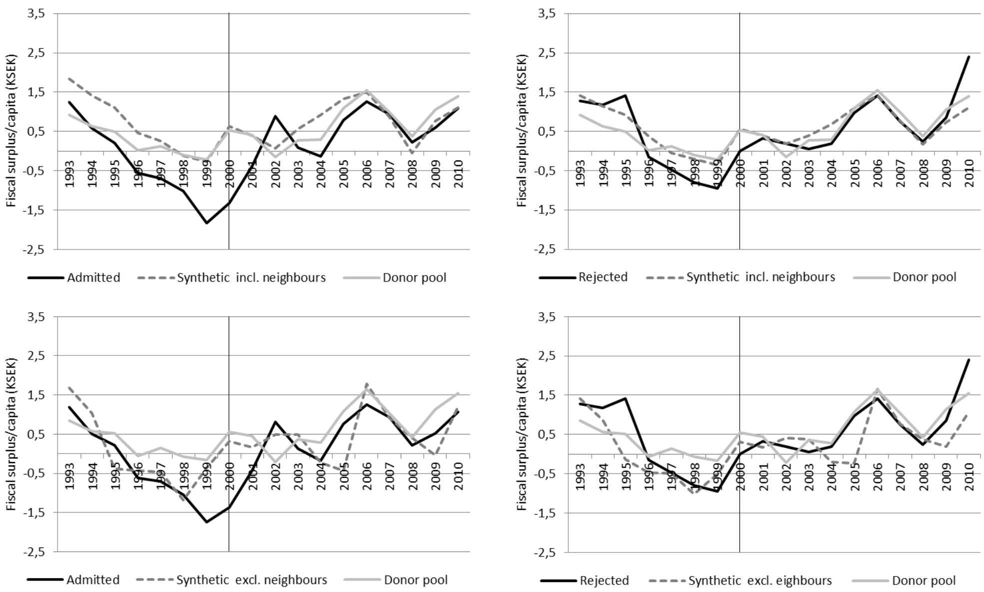

Figure 2: Actual and synthetic fiscal surplus

The trend for the donor pool is overall similar in all panels, although it is difficult to make out exactly how well the trends match over shorter periods as the level of costs is clearly lower.

The level of fiscal surplus (Figure 2) differs somewhat between actual and synthetic municipalities in the pre-program period, especially when neighbors are included in the donor pool. More importantly with respect to our FE estimations, the actual and synthetic municipalities follow quite similar trends before the program. The most notable exception is that both the admitted and the rejected municipalities diverge from their synthetic controls already

have therefore also estimated synthetic controls for the 33 neighbors (see Section 1.3 in the supplementary material). We get very poor fit for three of the neighbors that figure prominently in the synthetic controls including neighbors mentioned above. We are unable to sign the effect for two of these, while the third has higher costs than its synthetic control during the post-program period. A FE model estimated on the neighbors and their synthetic controls points at cost increases on average; importantly though, the FE estimates are not driven by the three municipalities that were important in the synthetic controls for the admitted municipalities. Moreover, a large majority of neighbors follow their synthetic controls closely during the post-program period. Furthermore, the deterioration does not appear to have lasted, as the average effects on costs are insignificant for the years 2008-2010, with very small or negative coefficients in 2009-2010, and the fiscal surplus estimates are insignificant from 2007 and onwards. Note that we do not know whether the cost increases of some neighbors starting in 1999 is due to the program or to other factors.

in 1999 when neighbors are excluded from the donor pool. This may be interpreted as *a*) deteriorating economic conditions, unrelated to the program, in the applicant municipalities; or *b*) as an announcement effect of the program. As we do not know the correct interpretation, we decide to treat 1999 as a part of the program period in the subsequent FE estimations. That is, we let D_{it}, the indicator for program participation, be equal to one from 1999 onwards.[20] We moreover see that the average fiscal surpluses of all four donor pools follow different pre-program trends than the actual and the synthetic municipalities. This suggests that the synthetic municipalities are strictly better comparison units than the donor pools.

Figures 1 and 2 also provide a first look at the development in the post-program period, though it should be noted that the averages presented in these figures are *not* adjusted for municipality fixed effects. Figure 1 indicates that the admitted municipalities on average have had lower costs than their synthetic controls all years 2001-2010 in the sample including neighbors, while they instead appear to have had higher costs than their synthetic controls since 1999 when neighbors are excluded from the donor pool. The average costs of the rejected municipalities and their synthetic controls are almost exactly the same in the sample including neighbors, while the rejected municipalities have higher costs when neighbors are excluded.

As for the fiscal surplus, the top panel of Figure 2 indicates that the pre-program gap between actual and synthetic levels is closed in the post-program period, i.e. that the actual municipalities have increased their fiscal surplus. There is little difference between actual and synthetic fiscal surplus in either period when neighbors are excluded from the donor pool (bottom panel).

5.2. Average effects: Fixed effects estimates

Table 3 show the FE estimates for the admitted and the rejected municipalities respectively. The *program*$\times$*year* coefficients show the yearly effects on the *admitted* municipalities in columns (1)-(4), and on the *rejected* municipalities in columns (5)-(8). All actual-synthetic pairs enter the estimation, i.e. no pre-RMSPE cut-off is applied. However, the results are very similar if we instead include only the municipalities with pre-RMSPE < 0.03 (see Section 2.1 in the supplementary material). In columns (1)-(2) we use the

[20]Letting the indicators be equal to one from 2000 does not change our results much. Our results are strengthened if we use *only* 1999 as a comparison year by truncating the sample period to 1999-2010 (i.e. if we assume that the differences in 1999 do not reflect announcement effects and exploit that 1999 in that case is the most valid reflection of pre-program differences between actual and synthetic controls).

logarithm of *per capita costs of services* as the dependent variable; a coefficient of 0.01 should therefore be interpreted as a difference of 1 percent. Neighbors are allowed to contribute to the synthetic controls in column (1), but not in column (2). In columns (3)-(4), we use the *per capita fiscal surplus* as the dependent variable; column (3) corresponds otherwise to the specification used in column (1), and column (4) to column (2). As the fiscal surplus is in thousands of SEK per capita, a coefficient of 1 implies that admitted municipalities had 1 000 SEK higher per capita fiscal surplus that year. For the rejected municipalities, column (5) corresponds to the specification in column (1), and so on.

Starting with the results for the admitted municipalities, the estimates where neighbors are included in the donor pool indicate a statistically significant 2-4 percent cost reduction from 2001, the first full year of the program,[21] and onwards (column 1 of Table 3). When neighbors are excluded from the donor pool (column 2), the estimates are instead positive and sometimes significant before, during and right after the program (though the magnitudes are generally smaller than the pre-program prediction error of the synthetic controls, see Table 2), but mostly insignificant and of varying signs from 2004 onwards. In relation to the raw post-program averages shown in the left panel of Figure 1, it is notable that the inclusion of fixed effects do not affect the conclusions for the sample including neighbors, but the positive effect on costs in the figure for the sample excluding neighbors vanishes after the program when we account for time-invariant unobserved characteristics.

The estimated effects on the fiscal surplus in the sample including neighbors (column 3) go from negative right before the program to positive and highly significant in 2002, the year when the admitted municipalities *had* to balance the budget to get the grant. Except for 2004, all post-program coefficients are positive and most of them are highly significant. The estimated marginal effects in the later years are large; many amount to around 1000 SEK per capita, which is a little bit less than one standard deviation of the average for the period.[22] The estimated coefficients from the sample excluding neighbors (column 4) are less stable in terms of sign, size and significance. The estimates for both samples are consistent with the picture from the left panel of Figure 2.

Looking instead at the rejected municipalities, there are no indications of a "program effect" on costs when neighbors are included in the donor

[21]Recall that applications were not approved/rejected until late 2000.

[22]The standard deviation is about the same in the group of actual and synthetic as for the whole group of 290 municipalities.

Table 3: Fixed effects estimations of program effects 1993-2010

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Group	Adm	Adm	Adm	Adm	Rej	Rej	Rej	Rej
Dependent:	Costs	Costs	Surplus	Surplus	Costs	Costs	Surplus	Surplus
program×1999	-0.000427	0.0195***	-0.696***	-1.258***	0.0160	0.0338***	-0.391	-0.819***
	(0.00612)	(0.00654)	(0.244)	(0.214)	(0.0109)	(0.0113)	(0.303)	(0.300)
program×2000	-0.00264	0.0143*	-1.083***	-1.575***	0.0162	0.0272**	-0.368	-0.683**
	(0.00755)	(0.00746)	(0.326)	(0.314)	(0.0110)	(0.0110)	(0.365)	(0.336)
program×2001	-0.0264***	0.0213**	0.100	-0.474	0.00151	0.0373***	0.122	-0.225
	(0.00889)	(0.00835)	(0.347)	(0.341)	(0.0113)	(0.0109)	(0.327)	(0.311)
program×2002	-0.0392***	0.0154	1.685***	0.436	0.00352	0.0453***	0.190	-0.610
	(0.00979)	(0.0101)	(0.283)	(0.318)	(0.0131)	(0.0136)	(0.424)	(0.450)
program×2003	-0.0314**	0.0274**	0.407	-0.238	0.00506	0.0464***	-0.140	-0.695**
	(0.0119)	(0.0124)	(0.291)	(0.278)	(0.0133)	(0.0137)	(0.295)	(0.292)
program×2004	-0.0281**	0.0175	-0.180	0.154	0.00616	0.0366**	-0.296	0.0189
	(0.0124)	(0.0125)	(0.254)	(0.269)	(0.0150)	(0.0149)	(0.278)	(0.289)
program×2005	-0.0290**	-0.0134	0.331	1.302***	0.00445	0.0146	0.0765	0.822**
	(0.0122)	(0.0122)	(0.218)	(0.287)	(0.0150)	(0.0145)	(0.308)	(0.363)
program×2006	-0.0385***	0.0267*	0.636***	-0.418**	-0.00108	0.0468***	0.189	-0.622*
	(0.0127)	(0.0136)	(0.203)	(0.178)	(0.0157)	(0.0168)	(0.341)	(0.324)
program×2007	-0.0378***	0.0213	0.938***	0.124	-0.00560	0.0383**	0.178	-0.409
	(0.0131)	(0.0141)	(0.270)	(0.239)	(0.0154)	(0.0167)	(0.348)	(0.315)
program×2008	-0.0248*	0.0210	1.144***	-0.0642	0.00232	0.0364**	0.271	-0.509
	(0.0135)	(0.0143)	(0.315)	(0.231)	(0.0156)	(0.0167)	(0.372)	(0.305)
program×2009	-0.0325**	-0.0119	0.701**	0.681**	-0.00106	0.0113	0.321	0.292
	(0.0145)	(0.0144)	(0.279)	(0.268)	(0.0179)	(0.0179)	(0.333)	(0.296)
program×2010	-0.0380**	-0.00285	0.860***	-0.0136	-0.00526	0.0116	1.499*	0.943
	(0.0146)	(0.0151)	(0.269)	(0.246)	(0.0178)	(0.0181)	(0.782)	(0.742)
Constant	3.853***	3.825***	-0.695***	-0.426***	3.818***	3.800***	-0.447***	-0.308***
	(0.00402)	(0.00450)	(0.0885)	(0.0851)	(0.00583)	(0.00619)	(0.121)	(0.110)
neighbors in d.p.	Y	N	Y	N	Y	N	Y	N
Observations	1,260	1,224	1,260	1,224	792	792	792	792
Nr of municipalities	70	68	70	68	44	44	44	44
R^2	0.960	0.953	0.399	0.435	0.955	0.950	0.323	0.360
F	750.3	859.8	57.18	150.9	329.9	454.7	45.00	52.74

Note: Standard errors clustered on municipality in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Year fixed effects included in all models. Älvdalen is excluded in all samples for the admitted municipalities. Dorotea is excluded in samples excluding neighbors from the donor pool for the admitted municipalities. Gullspång excluded from all samples for the rejected municipalities.

pool (column 5). When neighbors are excluded from the sample (column 6), the point estimates are by contrast consistently positive and many of the estimates are large in magnitude and statistically significant. We find little effects on the fiscal surplus for the rejected municipalities in either sample. All results for the rejected municipalities are broadly consistent with what would be expected from Figures 1 and 2.

In sum, our estimations indicate decreased costs and higher fiscal surplus for admitted municipalities if neighbors are included in their comparison group. When we exclude neighbors, we cannot rule out effects on costs of some substance before, during and right after the program, but there are no signs of lasting effects on fiscal discipline. The rejected municipalities have similar cost levels as their comparison group if the latter includes neighbors, but clearly higher if neighbors are excluded. There is no consistent pattern regarding the fiscal surplus for the rejected municipalities, regardless of comparison group.

The differences between the results including and excluding neighbors deserve a comment. Recalling the generally higher pre-program RMPE's of the synthetic controls excluding neighbors, it is clear that the donor pool including neighbors is superior in delivering a similar comparison group. In relation to this, consider the results for the rejected municipalities in the sample excluding neighbors. It seems unlikely that the *higher* cost level of the rejected municipalities compared to their synthetic controls is caused by them being rejected from the program. It is difficult to see why the experience of being rejected – which essentially signals that the local budget constraint is *hard* – would make them respond by increasing costs. Rather, we suspect that these results reflect that the synthetic controls excluding neighbors is a flawed control group. This suspicion carries over to the admitted municipalities, which are very similar to the rejected municipalities before the program, whose synthetic controls consist of neighbors to a similar extent,[23] and whose cost estimates are affected by the exclusion of neighbors in the same direction.

Nevertheless, our scepticism towards the results excluding neighbors does not imply that the results including neighbors tell the true story about what would have happened to the admitted municipalities in the absence of a program, as we cannot rule out indirect effects on neighbors.

5.3. Robustness checks

As a first robustness check we estimate another type of specification, in which we collapse the pre- and post-program years into two periods and

[23]The mean weight that derives from neighbors are 0.64 in the admitted group, and 0.57 in the rejected group.

use the group averages for these two periods in a DID comparison between actual and synthetic municipalities. The motivation for this type of specification is two-fold: first, averaging over periods yields one single estimate of the program effect, which is easier to interpret than the yearly estimates (whose signs differ in some samples); and second, this type of specification is less likely to suffer from biased standard errors caused by serially correlated outcome variables (Bertrand et al., 2004).[24]

We let the pre-program period run from 1993-1998 and the post-program period from 2005-2010; this conservative delimitation excludes the years when the program may have had an announcement effect, as well as the years when the program was directly affecting at least some of the admitted municipalities (recall that the last two grants were paid out in 2004).

The results from this condensed DID model mirror the previously shown results for both admitted and rejected municipalities. Panel A of Table 4 shows the results for the admitted municipalities. When neighbors are included in the sample, the admitted municipalities have significantly lower costs (about 3.5 percent) and significantly higher fiscal surplus (about 750 SEK per capita) than their synthetic controls. When neighbors are excluded, the effect on costs is close to zero, and the estimated effect on the fiscal surplus is positive but insignificant. These results reinforces the conclusion about no lasting negative effects on fiscal discipline in the sample excluding neighbors.

Panel B shows the results for the rejected municipalities. The estimated effect on costs is very close to zero when neighbors are included, while the effect on the fiscal surplus is positive but not significant. When neighbors are excluded, the estimated effect on costs is positive and significant at the 10 percent level, while the estimated effect on the fiscal surplus is very small and insignificant. Note that some of these models perform badly in terms of R^2- and F-statistics. Adding the time-varying covariates shown in Table 1 make all specifications pass an F-test of joint significance. Two more coefficients then become significant: the admitted municipalities have significantly higher fiscal surplus in the specification excluding neighbors (at the 10 percent level) and the rejected municipalities have significantly higher fiscal surplus in the

[24]As mentioned in Section 4.3, this should not be a major problem in our case as Stata's *cluster* command performs rather well in Bertrand et al. (2004) for a comparable number of treated units. Furthermore, we show in the supplementary material that we obtain similar standard errors when applying a method for inference developed by Cavallo et al. (2013), which uses bootstrapped standard errors from the empirical distribution of placebo effects obtained by developing synthetic controls for all municipalities in the donor pool. When the synthetic controls have good fit, both these standard errors and the estimates of the program effect are close to the ones in the corresponding fixed effects models.

Table 4: Two-period difference-in-differences

Panel A: Actual-synthetic, admitted				
	(1)	(2)	(3)	(4)
Variables	Costs	Costs	Surplus	Surplus
admitted	-0.0344**	0.0068	0.768***	0.269
	(0.0127)	(0.0130)	(0.206)	(0.168)
Constant	0.377***	0.338***	0.0916	0.598***
	(0.00487)	(0.00520)	(0.141)	(0.0679)
neighbors in d.p.	Y	N	Y	N
Observations	70	68	70	68
R^2	0.093	0.004	0.170	0.037
F	6.93	0.27	13.92	2.54

Panel B: Actual-synthetic, rejected				
	(1)	(2)	(3)	(4)
Variables	Costs	Costs	Surplus	Surplus
rejected	-0.00104	0.0265*	0.423	0.0861
	(0.0150)	(0.0155)	(0.322)	(0.287)
Constant	0.374***	0.346***	0.275	0.611***
	(0.00520)	(0.00708)	(0.185)	(0.111)
neighbors in d.p.	Y	N	Y	N
Observations	44	44	44	44
R^2	0.000	0.065	0.039	0.002
F	0.00477	2.923	2.312	1.719

Note: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. The pre-program period is 1993-98 and the post-program period is 2005-10. Due to problems of creating synthetic controls, Älvdalen is excluded in all samples in Panel A, Dorotea is excluded in samples excluding neighbors from the donor pool in Panel A, and Gullspång excluded from all samples in Panel B.

specification including neighbors (at the 5 percent level).

The standard errors of the estimates in Table 3 are clustered on municipality to control for correlations within municipalities over time. However, as there may also be correlations between municipalities in the same year, there is a risk that our standard errors still are underestimated (see e.g. Cameron and Miller, forthcoming). While this is a general problem, it could be especially pertinent for our sample, as some municipalities from the donor pool contribute to the synthetic controls of several municipalities.[25] We have therefore also estimated the same specification as in Table 3 using the proce-

[25]We thank an anonymous referee for drawing our attention to this issue.

dure described in Cameron et al. (2011) to estimate robust standard errors clustered on both municipality and year. The results, which are shown in Section 2.2 of the supplementary material, are in all cases very close to the ones reported here.

To go beyond the informal comparison between the effects of admitted and rejected municipalities above, we also contrast the two groups directly by estimating our FE model on a sample including only the admitted and rejected municipalities (i.e. no synthetic controls enter the estimation sample). The rejected municipalities provide a reasonable control group as they are very similar to the admitted municipalities (see Figure 2 in the supplementary material) and, importantly, also exhibited an intention to be treated. Moreover, when compared to synthetic controls with a good pre-program fit, the rejected municipalities do not seem to have been greatly affected by the program themselves. The results of this comparison, shown in Section 2.3 of the supplementary material, point at lower costs and, except for the earliest years and 2010, higher fiscal surplus for the admitted municipalities. However, the differences are not significant after 2008 for costs, and only significant in 2002 for the fiscal surplus. These results strengthen our belief that the program at least has not reduced the fiscal discipline of the admitted municipalities.

As shown in the supplementary material to this paper, we also obtain results that point in a similar direction as in Table 3 when we estimate the model on the same sample of municipalities but abstain from using the synthethic control weights, as well as with several other sample restrictions and other definitions of our dependent variables.

6. Heterogeneity

The average effects discussed in the previous section may hide substantial variation between municipalities. To examine this possibility, we compare the average post-program difference between each admitted municipality's actual costs and the costs of its synthetic control from the estimation including neighbors. Notably, here we leave the FE framework and therefore no longer control for time-invariant unobservable effects; however, as the incorporation of fixed effects makes little difference for the sample including neighbors, this seems like a minor sacrifice.

We use placebo tests to classify each of the average cost differences as positive (cost increase), negative, or zero. To obtain a distribution of placebo effects, we follow Abadie et al. (2010) and construct synthetic controls for each municipality in the donor pool. The average cost difference for each admitted municipality is then compared to the distribution of differences in the placebo group. We classify a municipality's average effect as significant if at least one of the following two statistics lie in the extreme deciles of their respective placebo distributions: (i) the average actual-synthetic difference in per capita costs 2000-2010, i.e.

$$average_i = \frac{1}{T} \sum_{t=2000}^{2010} (y_{it}^{actual} - y_{it}^{synthetic}); \tag{3}$$

and, (ii) the ratio between the post-program RMSPE and the pre-program RMSPE. The first statistic has the advantage of capturing the sign of the effect, while the other has the advantage that it acknowledges the effect size in relation to the fit of the synthetic control. An estimated effect of 0.03 (i.e. 3 percent) is arguably more indicative of a significant effect if the pre-program RMSPE is 0.01 than if it is 0.1.

Out of the 35 municipalities for which we have been able to construct synthetic controls, 12 (about 34 percent) are classified as having reduced costs according to the placebo analysis.[26] The average cost reduction of these 12 municipalities is 7 percent, which is a notable magnitude in relation to their average pre-program RMSPE (2 percent) and about twice the size of the estimated average effects reported in column 1 of Table 3. Only 2 admitted municipalities are classified as having increased costs significantly according to the placebo analysis.

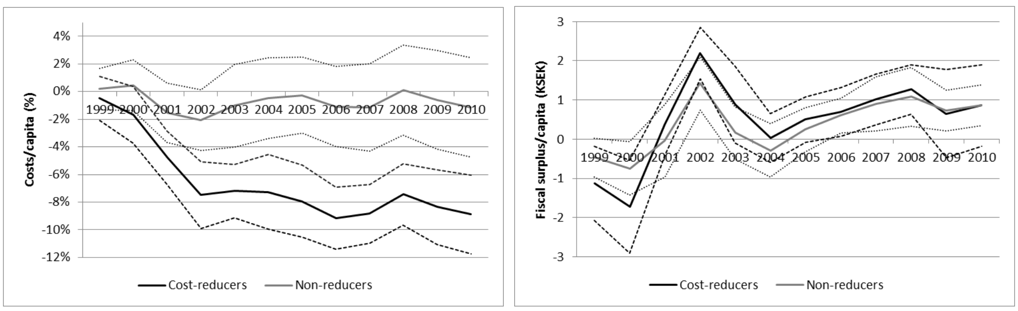

Figure 3: Costs and fiscal surplus estimates, cost-reducers (black) and non-reducers (gray). The left part of the figure shows cost estimates for the two groups, and the right shows the fiscal surplus estimates. Both parts contain the 95 percent confidence intervals (dashed lines are cost-reducers, dotted non-reducers).

The placebo analysis thus suggests that the cost reduction found on average for admitted municipalities when neighbors are included in the donor

[26]We do not apply any pre-program RMSPE cut-off. A cut-off of 0.05 would only exclude one admitted municipality: one that reduced costs, whose pre-RMSPE equals 0.0503.

pool is driven by a subset of the admitted municipalities. Re-running the FE specification in column (1) of Table 3 but dividing the sample into "cost-reducers" and "non-reducers" confirms this interpretation: the estimates, which are presented in Figure 3, suggest significantly lower costs for the cost-reducers, while the non-reducers' estimates are never significantly different from zero. Since we have not run the synthetic matching algorithm directly for the fiscal surplus, we cannot perform the placebo analysis for this variable. The right part of Figure 3 however shows that the average positive effects on the fiscal surplus including neighbors (column (3) of Table 3) are *not* driven by the cost-reducers. Thus, the cost-reducers are not the only admitted municipalities that improved their fiscal discipline.[27] This means that the cost-reducers and the non-reducers chose different strategies to deal with their fiscal problems.

To explore this further, we have also examined the development of gross revenues (see Section 3.3 in the supplementary material). We do not find that gross revenues have increased on average; if anything, revenues have developed worse than in the synthetic municipalities. Dividing the admitted municipalities into cost-reducers and non-reducers reveals that this result is mainly driven by the cost-reducers, which have significantly lower gross revenues than their synthetic controls. The cost-reducers have in particular lower fees than their synthetic counterparts, which makes sense because if they have scaled down services, income from fees should decrease as well.

To reconcile these results with the finding that fiscal surpluses increase for both cost-reducers and non-reducers, we propose the following taxonomy for the admitted municipalities: 1) some mainly reduced costs, thus improving their fiscal surpluses (this group comprises the cost-reducers mentioned above); 2) some reduced costs insignificantly but also increased revenues somewhat, thus improving their fiscal surpluses significantly; 3) some failed to reduce costs but managed to increased revenues, thereby improving their surpluses; 4) some simply did not improve their fiscal surpluses.

7. Interpretations and potential explanations

Our analysis suggests that the admitted municipalities have improved their fiscal discipline, or at least that it has not deteriorated, after the program. As we are in a non-experimental setting, it is however possible that the findings reflect something else than the causal effect of the program. We start this section by evaluating some competing explanations for the findings,

[27] Another sign of this is that all admitted municipalities run surpluses on average during 2005-2010.

and then put forth reasons for why participation in the program may explain the results. We thereafter discuss why the admitted municipalities display heterogeneous responses to the program.

The selective nature of the program may imply that the central government was able to identify municipalities that would react favorably to the program. Nothing in the official documentation reveal that the government had such information – limited possibilities to increase revenues is the primary official reason for admission, as mentioned in Section 2 – but we do not expect such motivations to be written down either. In any case, the explanation begs the question of why the Swedish government succeeded with this task, where so many other governments have failed. For example, the earlier Swedish bailouts studied by Pettersson-Lidbom (2010), which affected fiscal discipline negatively, were also selective.

Mean reversion is a second competing explanation. As the admitted municipalities' costs (fiscal surplus) were unusually high (low) in the period leading up to the initiation of the program, they could be expected to revert back to the mean. Importantly, both the rejected municipalities and the synthetic controls are from the same part of the distribution, so these two groups should have similar developments as the admitted municipalities in all samples if mean reversion was driving the results. This is not what we find.

The implementation of the Balanced Budget Act is a third potential explanation for the improvements (or lack of deteriorations); however, there are no obvious reasons why the act should have affected the admitted group differently from the rejected group.[28] The incentives to conform to the act were moreover in place already in the year 2000, a year for which we do not see higher fiscal surplus or lower costs for the admitted municipalities.

A fourth possibility, emphasized theoretically by Battaglini (2011), is that at high levels of debt (the argument extends to costs if the ability to raise revenues is limited, as is the case here), debt service costs overshadow the utility to politicians of being able to spend by taking on more debt instead of using tax revenues. We do not think that this mechanism is the explanation of the results though, as the admitted and the rejected municipalities have very similar levels of total debt – there are no significant differences for any year 1998-2010 (see Table 1 and Section 2.3 in the supplementary material). If debt service costs was the main explanation, we would expect to find

[28]See Persson (2013) for a more in-depth analysis of the municipal reaction to the act, which indicates that the new balanced budget requirement did not affect fiscal discipline in general.

improvements for the rejected municipalities, contrary to what we find.

By contrast, the program is a plausible reason for the different outcomes of the admitted and the rejected municipalities: while the admitted municipalities could use a pending grant to convince the political opposition and the public about the necessity of improving discipline, the rejected municipalities had no such means at hand.[29]

There are also possible consequences of the program design, mainly connected to the conditions attached to the grants, that may explain the sustained fiscal discipline after the program. Allers and Merkus (2013) stress the importance of reducing the attractiveness of bailouts. They argue that a reason why bailouts are rare in the Netherlands, despite that local governments are guaranteed to be bailed out when in need, is that bailouts are associated with a significant loss of local fiscal autonomy. The program studied here reduced the attractiveness of bailouts by using conditions, requiring a certain amount of fiscal discipline to be shown before the transfers were made. The conditions may moreover have had a long-run impact, if local politicians have come to expect that central government bailouts are only available after complying with painful conditions, making retained fiscal discipline a more attractive choice. This interpretation seems more plausible if the governing majorities have been stable, that is if the political parties that once had to fulfil the conditions have continued to stay in power.[30] The admitted municipalities are very stable in this sense: in 32 out of the 35 municipalities for which we have managed to obtain information about governing majorities, at least one party participates in the governing coalition in both election periods 1998-2002 and 2002-2006. In 30 out of 35 cases, at least one party participates in the governing coalition in all three election periods 1998-2010.[31] Thus, a minimum requirement of institutional memory appears to be fulfilled, though these empirical observations say nothing about other important determinants of policy such as bargaining power of different parties.

A complementary reason why the conditions could lead to sustained fiscal discipline relates to a study by Knutsson et al. (2008), which documents that about 37 percent of the cost reductions within the program were "structural", meaning that they implied the removal of an existing branch or organization. For instance, if an admitted municipality cut costs by shutting down a nursing

[29] We thank Magnus Henreksson for suggesting this explanation.

[30] Tenure may also have a direct effect on fiscal discipline. Jochimsen and Thomasius (2014) show that finance ministers with longer tenure have lower deficits in the German Länder.

[31] We thank an anonymous referee for suggesting this test.

home in 2001, this implies a permanent cost reduction of a certain size the following years unless the home was later re-opened.[32]

To find reasons for why only some of the admitted municipalities reduced costs (see previous section), we have compared the cost-reducers and non-reducers with t-tests to examine differences in structural characteristics, institutions, and attitudes (results available in Section 3.2 in the supplementary material).[33] We find very few differences between the two groups, with two notable exceptions. First, the non-reducers had more room for increasing revenues at the beginning of the program: they had lower total revenues, lower tax rates, and lower levels of fees. Raising revenues might therefore have been more feasible for the non-reducers, than for the cost-reducers (recall that at least some of the non-reducers must have increased their revenues to explain the positive effect on fiscal surplus for this group, c.f. Figure 3). Second, although both groups are politically stable, the cost-reducers have clearly fewer close elections both before and after the program. Only in one out of the twelve cost-reducers did the right-wing parties get between 45 and 55 percent of the votes in an election held between 1998 and 2010. Among the non-reducers, this number varies between five to ten, and the difference is significant in the elections of 1998 and 2010, despite the test being clearly underpowered. For Swedish municipalities with a left-leaning electorate it may be less costly (in terms of votes) to raise taxes and fees than to cut spending on what is viewed as very important public services.[34] Thus, it is plausible that close elections made some municipalities opt for the alternative of increasing revenues, while municipalities with more certain majorities could afford to choose the cost-reducing strategy.

8. Conclusions

A cautious interpretation of our results is that conditional discretionary intergovernmental grants need not have negative effects on fiscal discipline. A bolder claim – based on stronger assumptions regarding indirect effects of the program on neighbors to the admitted municipalities – is that the bailout

[32]It can be noted that costs were reduced in most service areas, see our supplementary material, Section 3.3.

[33]As a methodological check, we also examine whether the different developments of costs in the two groups relate to the importance of neighbors in their respective synthetic controls. The correlation between the share of neighbors and the average actual-synthetic cost difference ($average_i$) is small (-0.093) and insignificant (p-value=0.59).

[34]Municipal income taxes are moreover not individually visible on tax receipts and the rates are often reported together with regional tax rates.

program under study improved the fiscal discipline of the admitted municipalities. However, only some of the admitted municipalities have reduced costs significantly compared to their synthetic controls, and the positive effect on the fiscal surplus is not significant in all specifications. The most balanced interpretation of our results may therefore be that the program did not reduce fiscal discipline in general, and that it improved the fiscal discipline of some of the admitted municipalities.

Notably, even the more cautious interpretation stands in contrast to the message from previous studies. This suggests that the conditions attached to the grants, a distinguishing factor of the program under study, might have been key to dampen the soft-budget effect. The efforts needed to cut costs and balance budgets may have made politicians in the admitted municipalities realize that even if central government bailouts are available, they come at a cost. Retaining fiscal discipline may therefore be a preferable choice.

This mechanism provides a complement to the argument presented in Besfamille and Lockwood (2008) for why soft(er) budget constraints may be efficiency-improving. Besfamille and Lockwood argue that hard budget constraints may lead local governments to underinvest, whereas the difference between admitted and rejected municipalities found here indicates that conditional grants may induce more fiscal discipline than a hard(er) budget constraint. However, to claim conclusively that the type of conditions is crucial and to see whether our findings generalize to other countries, more variation in the conditions of programs and in the context of bailouts would be needed. This presents interesting avenues for future research.

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