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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. Little is known however about whether such tendencies can be dampened if assistance is conditional on the local governments' own fiscal efforts. We examine a case in which the Swedish central government provided conditional grants to 36 financially troubled municipalities. To deal with the obvious selection problem related to participation in such a program, we use the synthetic control method (Abadie and Gardeazabal, 2003; Abadie et al., 2010) to identify suitable comparison units for each of the 36 municipalities. We then estimate fixed effects regressions on the resulting sample to compare the cost development of admitted municipalities to that of their most similar counterparts during the decade after the program. For most of the admitted municipalities, costs seem to be largely unaffected by the program. However, a non-negligible number are able to hold back costs more than expected, and the development of net revenues is favourable for the group as a whole. We conclude that participation in a conditional bailout program need not erode fiscal discipline, and may even induce a greater concern for fiscal discipline.

Keywords: bailouts, local governments, fiscal performance, synthetic control method
JEL codes: D230, H61, H62, H72, H77

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1 Introduction

Whenever a central government faces a sub-unit in financial distress, the unpleasant question that arises is whether to assist the unit or not. On the one hand, neglecting to bail out the unit may lead to default or bankruptcy, which could be very costly both economically and politically. On the other hand, bailouts may create problems of soft budget constraints: noting that the central government steps in in times of trouble, sub-units may come to expect that bailouts will be available when needed. Thereby, their incentive for fiscal discipline is eroded (Kornai, 1979; Wildasin, 1997; Goodspeed, 2002; Inman, 2003). The current situation in regions and countries within the EMU provides a clear illustration of the dilemma, but the empirical relevance of the problem is also backed up by more systematic evidence from studies of fiscally decentralized countries.¹

A possible way out of the dilemma may be to grant the sub-unit assistance, but condition payment on actions that lay the ground for fiscal discipline. We investigate a case in which the Swedish central government provided conditional bailouts to 36 municipalities in fiscal distress.² The 36 municipalities were granted extra funds, but payment was contingent on them first cutting certain costs and achieving budgetary balance. At the closure of the program, it was evident that there was a short-term effect on fiscal performance, as all admitted municipalities managed to meet the conditions. But the more interesting question is whether this newly acquired fiscal discipline was retained after the program, when there was no longer an explicit incentive for such behavior. To address this question, we analyze the evolution of per capita costs as well as revenues net of costs (henceforth referred to as *net revenues*) during the decade after the launch of the program.

To draw firm conclusions about the program effect, 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, by design, such programs are directed to a selected sample of units, namely those in fiscal distress. In the current context, this is illustrated by the fact that all 290 municipalities had the option to apply to the program, but only 36 of the 59 that chose to apply were judged to be eligible. The experience of being denied participation in the program is a kind of treatment in its own, and we analyze the fiscal performance also of the rejected municipalities.³

Instrumental variable estimation would overcome the selection problem in principle.

¹See Rodden (2002); Rodden et al. (2003); Plekhanov (2006); Bordignon and Turati (2009); Pettersson-Lidbom (2010); Baskaran (2012); Fink and Stratmann (2011), and Lusinyan and Eyraud (2011). Kornai et al. (2003) survey the theoretical literature and provides further empirical examples.

²The 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)).

³As most municipalities do not end up in fiscal distress, we are interested in the (conditional) average treatment effect on the treated for both groups (Imbens and Wooldridge, 2009).

As the program was explicitly directed to municipalities with poor fiscal performance, it is difficult to envision variables that are correlated to program status, but uncorrelated to our outcome variables, and even harder to come up with separate instruments for admission and rejection. Instead, we use the synthetic control method for case studies, developed in Abadie and Gardeazabal (2003) and Abadie et al. (2010), to identify appropriate comparison units for each of the municipalities affected by the program. This algorithm constructs a synthetic control municipality for each affected municipality as a weighted average of untreated municipalities. The weights are chosen to make the synthetic control match the actual municipality in terms of observable pre-program characteristics, including the pre-program development of costs.

Two assumptions are needed to interpret differences in the fiscal performance of actual and synthetic municipalities as causal effects of the program. *First*, program participation must be independent of potential outcomes, conditional on covariates (Imbens and Wooldridge, 2009).⁴ That is, a causal interpretation assumes that all post-program differences derive from the program, rather than from differences in unobservable characteristics, in the reaction to post-program shocks, or in the set of shocks experienced. To increase the credibility of this assumption, we estimate fixed effects regressions on the samples of admitted and rejected municipalities and their synthetic controls. Thereby, we explicitly control for time-invariant unobservables and can include covariates to capture post-program changes in observables.⁵

The *second* assumption is the Stable-Unit-Treatment-Value assumption (SUTVA) (e.g. Rubin, 2005); that is, the comparison units should be unaffected by the existence of the program. In this regard, we are most concerned about the municipalities that are neighbours to the admitted. Pettersson-Lidbom (2010) used the frequency of deficit grants to neighbouring municipalities as an instrumental variable for expectations of future grants, and showed that such expectations led to higher debt levels during an earlier regime of discretionary transfers in Sweden. However, neighbouring municipalities are also likely to be similar to the treated municipalities in many important dimensions and to experience the same shocks. In a nutshell, the comparison group that would make the first assumption most likely to hold is exactly the group for which the second assumption is most questionable. We therefore run the synthetic control algorithm twice, once including and once excluding neighbours in the "donor pool" of possible comparison units.

We use per capita costs of services as our main measure of fiscal performance and let the synthetic control algorithm search for comparison units based on this variable.

⁴The assumption is often called "unconfoundedness" in the program evaluation literature. Another assumption needed for selection on observables to work is that there should be overlap between the distribution of covariates for treated and untreated units (Imbens and Wooldridge, 2009). We see the synthetic control method as a way to increase the chances that this assumption holds as well.

⁵See e.g. Fitzpatrick (2008); Hudson (2010) for similar estimation strategies.

For the rejected municipalities, costs appear to be unaffected by the program regardless of whether neighbours are included in the donor pool or not. For the admitted municipalities, we find permanent cost reductions on average when neighbours are allowed to contribute to the synthetic controls, whereas the estimated average effects are insignificant when neighbours are excluded from the donor pool. An examination of the actual-synthetic cost difference for each municipality further reveals that the average cost reduction found when neighbours are included in the donor pool is driven by a third of the admitted municipalities; the remaining two-thirds show no divergence from their synthetic control. A tentative exploration of this heterogeneity suggests that the incumbent politicians in the former group were initially more certain to be re-elected; they could thus afford to hold back costs without fear of losing the next election. The latter group on the other hand increased their revenues more, which indicates that they chose another strategy to deal with their fiscal problems.

In accordance with these findings, we find positive, significant and large average effects on the net revenues of admitted municipalities for many post-program years when estimating similar fixed effects specifications on the sample of actual and synthetic municipalities. For net revenues, we find positive effects regardless of whether neighbours are included in the sample or not. For the rejected municipalities, the estimates for net revenues are often positive but less often significant.

Taken together, our results indicate that the program has not undermined the fiscal discipline of municipalities participating in the program; it may even have had a beneficial impact. The two identifying assumptions are basically untestable though; we cannot rule out that the results reflect differences in (time-variant) unobserved motivation for improving fiscal discipline that is unrelated to the participation in the program. However, the fact that the turn towards more fiscal discipline coincides with the initiation of the program suggests that the experience of being in program *had* a beneficial effect *per se*.

To the best of our knowledge, this is the first attempt to investigate the impact of conditional bailouts on the fiscal performance of local governments. Our results stand in contrast to findings from settings with unconditional bailouts (see footnote 1), which suggests that conditions may be key to dampening the soft-budget effect of central government bailouts.

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

2 Institutional background

The 290 Swedish municipalities are responsible for the financing and 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 (Statistics Sweden, 2012b). Revenues mainly derive from a proportional income tax, with the tax rate set freely by each municipality. On average, about 12 percent of revenues come from a rule-based equalization system.⁶ Central government *discretionary* transfers, 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 unconditional grants to cover deficits each year. Since a major reform of the grant system in 1993, the central government has been considerably more restrictive with discretionary transfers. 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 national 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.⁷ 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 for the program, municipalities had to apply in November 1999 at the latest; in all, 59 municipalities applied.⁸

Compared to the municipalities that did not apply, the applicants had higher costs, higher debt and a lower equity ratio in 1998, and had witnessed a larger population decline between 1994-1998 (see Appendix A, tables A.1-A.3). They and their neighbours

⁶In 2010, revenue from income taxes made up approximately 65 percent of total municipal revenues, fees 21 percent, and government grants from the equalization system 12 percent (Statistics Sweden, 2010).

⁷Nevertheless, 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).

⁸Two more municipalities initially applied but withdrew their application before the government made its decision. These two are not included in the rejected group in our specifications.

moreover received more discretionary transfers before 1993; they may thus have had higher expectations about receiving the grant (Pettersson-Lidbom, 2010).

During the spring of year 2000, the delegation held an initial meeting with each applicant and discussed its situation. According to the official report, the delegation used the following criteria to decide whether each applicant should be considered further or not (SOU, 2003):

- Structural problems, e.g. demographic changes and low employment rates.
- 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 were asked to come up with a proposal of cost reductions. These proposals 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, in accordance with the delegation's proposal (SOU, 2003, Appendix 1). Surprisingly, given the above criteria, there are no significant differences between the admitted and the rejected with regards to the cost structure, debt level and demographic changes (Tables A.1 and A.2). This suggests that projected future revenues was the most important of the selection criteria and the official motivations for rejection support this interpretation (Swedish Ministry of Finance, 2000).⁹

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 achieve budgetary balance. According to the committee's report to the government, the actions of the municipalities were continuously monitored during the program period (SOU, 2003).¹⁰

⁹The three committee members were politicians; two were social democrats and the third was from the Centre party. As Dahlberg and Rattsø (2010) note, political factors such as key voter districts or party concerns do not seem to explain selection into the program.

¹⁰Whether 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, an audit report

In 2002, the admitted municipalities received 25 percent of the grant given that they could show 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.

Though all 36 sooner or later fulfilled the conditions, a follow-up study from 2004 points at relatively large cost increases in the admitted municipalities between 2002 and 2003 (Siverbo, 2004) (i.e. after most of them had received the whole grant). Interviews with representatives from some of the admitted municipalities moreover suggest that the program succeeded to make a substantial change in only some municipalities, while other indicated that they had not succeeded to make the turn towards fiscal responsibility (Siverbo, 2004; SOU, 2003).

A related program complicates the story somewhat. In several of the Swedish municipalities, the real estate boom-and-bust in the beginning of the 1990s left the publicly owned housing companies highly indebted and with a large over-supply of apartments. In the late 1990s, several municipalities called for help from the central government, which installed a committee (*Bostadsdelegationen*) to assist with the reconstruction of insolvent housing companies. Together with each municipality in the housing program, this committee decided on the number of apartments that would be phased out,¹¹ and a cost-sharing arrangement between the central and local government, typically a 50-50 split. Other conditions forced municipalities to increase equity in housing companies to balance write-downs of assets and prohibited dividends for several years.

During 1998-2005, as many as 52 municipalities were in the housing program at some time. In fact, 23 out of the 36 in *Kommundelelegationen* also received assistance from the housing program (Swedish National Board of Housing, Building and Planning, 2005).¹² For these 23 cases, we can only estimate the combined effect of the two programs. We do not view this as very problematic, as the two programs were similar in spirit, but discuss the issue more in sections 4.2 and 6.¹³

from 2000 raises concerns about the central government's toughness and encourages the government to terminate the program (Swedish National Audit Office, 2000, p. 9).

¹¹In several cases phasing out implied tearing down fully functional houses.

¹²Of these 23, 6 entered the housing program in 1999, before they were admitted by *Kommundelelegationen*, and 4 entered the housing program after 2002.

¹³We focus on *Kommundelelegationen* as it was directly connected to the overall fiscal performance of the municipalities. Housing is just one part of municipal services and far from the largest in terms of operating costs; it is also a non-obligatory part. *Kommundelelegationen* in principle addressed all of the municipal administration. For a short term evaluation of the housing program, see Swedish National Board of Housing, Building and Planning (2005).

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. 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.

3.1 Dependent variable

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 two reasons. First and most importantly, there were substantial differences among municipalities in the accounting of debt before the Municipal Accounting Act came into effect in 1998. Some important differences still remain today, notably in regard to the accounting of pensions. Second, balance sheet measures are heavily influenced by extraordinary historical events, such as sales of e.g. public companies and real estate. 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* as the main dependent variable.¹⁴ We also provide results with revenues net of costs (henceforth referred to as *net revenues*) as the outcome variable. A technical reason to focus on costs rather than net revenues is that the latter variable fluctuates a lot from year to year (for idiosyncratic reasons), which makes the synthetic control method more difficult to apply.

3.2 Covariates

The dataset contains several potential cost predictors which are used as inputs in the synthetic control matching algorithm and covariates in the fixed effects regressions. The ability to raise revenues is accounted for by the *tax base size* (taxable income per capita), *per capita central government grants*, and the *employment rate* (for the population +16 years). We account for the demographic structure by the *population size*, the *share of children* (0-14 years) and the *share of elderly* (+65 years). We moreover account for dif-

¹⁴We log costs to obtain better fit in the regressions and for interpretational ease. All economic variables are in 2010 prices. Financial costs are not included in the cost measure, partly because this item fluctuates a lot from year to year, and partly because financial costs are to some extent beyond the control of the municipalities.

ferences in policy preferences and political landscape by the *share of right-wing parties*,¹⁵ the *Herfindahl index of political concentration*,¹⁶ and the *number of seats in the municipal council*.¹⁷ Summary statistics for the year 1999 can be found in Appendix A. Tables A.4 and A.5 show that the differences between the groups of admitted and rejected municipalities in terms of the covariates are small (and not significant). On the other hand, compared to those who did not apply (Table A.6), all of the variables 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, and a municipal council that was less fragmented and had fewer seats.

The data also contains two proxies for initial bailout expectations: (i) the number of deficit grants from the central government received during 1979-1992, and (ii) the average share of each municipality's neighbours that received discretionary grants over the period 1979-1992.¹⁸ In accordance with the results in Pettersson-Lidbom (2010), both the number of discretionary grants and the share of neighbours with grants is significantly higher for applicants than for non-applicants. The former variable is not significantly different between the admitted and rejected groups, while the latter is; a larger share of neighbours of admitted municipalities received transfers during the earlier regime.

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 (Angrist and Pischke, 2008; Dahlberg et al., 2008). As high costs and poor fiscal performance in general were reasons to apply for the program, it is difficult to envision an instrumental variable that would be correlated to program status but uncorrelated to 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, which is described in more detail in Section 4.1, to select a comparison group that contains only units that are similar to the affected municipalities from the larger group of municipalities that did not apply to

¹⁵Pettersson-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).

¹⁶Defined as $H = \sum_i (\text{vote share of party } i)^2$ (see e.g. Borge, 2005).

¹⁷In the political economy literature, the size of the decision making body has been argued to influence costs (Weingast et al., 1981). See e.g. Perotti and Kontopoulos (2002) and Pettersson-Lidbom (2011) for (conflicting) empirical evidence.

¹⁸Neighbours are defined as sharing land borders.

the program (the "donor pool"). To study the average effects of the program, we then estimate fixed effects (FE) regressions on the resulting samples of admitted or rejected municipalities and their respective synthetic controls for the period 1999-2010 (see section 4.3 for details). The FE framework has some advantages over a simple comparison of the developments in actual and synthetic municipalities:¹⁹ *First*, it allows us to explicitly control for time-invariant unobservables when comparing the actual and synthetic costs in the post-program period. In particular, since we include the year 1999 in the sample, the fixed effects capture unobserved initial motivation for fiscal discipline, which is otherwise one of the key confounders. *Second*, the FE framework allows us to include a set of covariates to examine to what extent the actual-synthetic differences are driven by post-program changes in observables.

For a causal interpretation, we need to assume that comparison units are not affected by the program; i.e. that the Stable-Unit-Treatment-Value assumption (SUTVA) holds (Rubin, 2005). The validity of this assumption depends crucially on the choice of donor pool, which we discuss further in section 4.2.

As the synthetic control algorithm estimates the yearly actual-synthetic difference in costs for each municipality affected by the program, we lastly take the opportunity to explore the heterogeneity in responses to the program. To draw inference on the significance of each municipality's average difference, i.e. to classify the change in costs as a reduction, no change, or an increase, we create empirical distributions of placebo effects by estimating synthetic controls for the municipalities in the donor pool as well (see Section 4.4 for a fuller description).

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).²⁰ 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 so as to make the synthetic control similar to the program municipality in terms of some relevant characteristics (cost predictors in our case) during the pre-program period, and to make the synthetic control reproduce the pre-program outcome path for the program municipality. 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

¹⁹The potential drawbacks are stronger assumptions on functional form and the distribution of residuals. We provide estimates of the "raw" actual-synthetic differences as well as inference from a method based on the empirical distribution of placebo tests in Appendix B.

²⁰For earlier applications, see also e.g. Moser (2005); Fitzpatrick (2008); Hudson (2010); Hinrichs (2012).

synthetic control algorithm searches for weights w that make

$$\begin{cases} Z_i = Z^{dp}w \\ y_{i,t} = \sum_j w_j y_{j,t}^{dp} \end{cases} \quad \forall t < T_0 \quad (1)$$

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 several different cut-offs for the pre-program root MSPE (RMSPE). For municipalities whose pre-RMSPE exceeds each threshold, the effect is classified as indeterminate at the given threshold. Note that the RMSPE can be interpreted as a difference in percent (because the dependent variable is logged); 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.²¹ In Z , we include the debt level and equity ratio in 1998, population growth between 1994 and 1998, the average share of neighbours receiving a discretionary transfer in 1978-1992, and the average over the whole pre-treatment period (the default option in *synth*) of the following variables: taxable income per capita, central government grants per capita, employment rate, population size, share of population of age 0-14 and over 65, share of right-wing parties, Herfindahl index and the number of seats in the municipal council. These characteristics are statistically significant in initial regressions of costs for the whole sample of municipalities (results available on request). 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.²² Recalling the descriptive statistics (Appendix A), the municipalities applying for the program are quite likely to be extreme. Importantly, though,

²¹Unlike the September 2012 version of this paper, we now use the *nested allopt* option of the algorithm. This reduces the pre-program RMSPE's, especially when using the donor pool excluding neighbours.

²²More formally, this may be the case if the set of pre-program predictors of a unit falls far from the

the relevance of these two concerns can be judged after the estimation, as it is possible to examine 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. First, we exclude the admitted and the rejected municipalities from the donor pool, as they were directly affected by the program and thus violate SUTVA. A case can be made that the rejected should be included in the donor pool for the admitted – or even that they should constitute the whole donor pool. As seen from Tables A.1- A.2, the admitted and rejected are very similar in many dimensions and we also know that they both showed the intention to be treated. However, given that rejection is a kind of treatment in its own, it is uncertain to what extent a difference between the admitted and rejected would reflect the effect of being in 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 labour 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 neighbouring (to the admitted) municipalities in the donor pool. As neighbours 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 and thus make the assumption of unconfoundedness more likely to hold. However, if neighbours keep track of what is going on in bordering municipalities, it is possible that the neighbours of admitted municipalities interpreted the admission of their neighbours as a general softening of the municipal budget constraint and thus relaxed their fiscal efforts. If so, SUTVA does not hold. The results in Pettersson-Lidbom (2010) provide a reason for such suspicions, though we would argue that spillover effects on neighbours are less likely in the current context: in contrast to what was the case for the earlier deficit grants, the program studied here was limited in time, employed relatively clear selection criteria and rejected a large share of applications

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).

(almost 40 percent). It is therefore far from obvious that other municipalities, including neighbours, interpreted the program as a significant softening of the budget constraint.

To sum up, if we could prove that there was no spillover effect of the program on the neighbours, we would most definitely want to include them in the donor pool. Since it is impossible to prove this, we estimate synthetic controls twice: once including and once excluding the neighbours of admitted municipalities from the donor pool. The donor pool consists of 136 municipalities when neighbours are included, and 103 when neighbours are excluded.²³

4.3 Fixed effects estimations

Our general estimation equation is

$$y_{it} = \alpha + \beta X_{it} + \sum_{t=2000}^{2010} \gamma_t D_{it} + \lambda_t + \mu_i + \varepsilon_{it} \quad (2)$$

where X_{it} is a vector of cost determinants²⁴ and 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 $t \geq 2000$ and are zero for all other observations – in particular, it is always zero for the synthetic municipalities. 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. To compute the values of the covariates and the dependent variable for the synthetic municipalities, we use the weights obtained from the synthetic control estimation. For each variable, the value for the synthetic control is the weighted sum of the values for the municipalities that comprise the synthetic control.

The chosen specification, with separate program dummies for each post-program year, has two advantages over a specification with only one 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.

y_{it} is either the log of per capita costs or the per capita net revenues. It should be noted that we then assume that the municipalities contributing to the synthetic control for *costs* are also suitable comparison units for net revenues. This seems like a reasonable

²³The number of neighbours, defined as sharing a land border with an admitted municipality, is larger than 33, but many neighbours are already excluded for the other reasons mentioned above.

²⁴We include the time-variant controls used in the synthetic control estimation. This includes the central government grants variable, though the program grant may have ended up in this post for the admitted municipalities. However, the estimates of the coefficients of interests are not much affected by leaving this variable out.

assumption given that they are similar in terms of cost structure as well as political, economic and demographic characteristics.

4.4 Heterogeneity and placebo tests

In our exploration of the heterogeneity in responses to the program, we use placebo tests to classify each affected municipality’s average effect (computed over 2000-2010) as a cost increase, a cost reduction or no change.

To obtain a placebo distribution of effects, we follow Abadie et al. (2010) and construct synthetic controls for each municipality in the donor pool. The average effect for each admitted (or rejected) municipality is then compared to this distribution of placebo effects. A municipality’s average effect is classified as significant if either one or both 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.

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

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.

5 Results

5.1 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 suspect that there was not much time to implement changes due to the announcement in 1999. Therefore, we let the *synth* algorithm minimize the MSPE over 1993-1999.

The donor pool contains more than 100 municipalities, but the synthetic controls generally consist of only a handful of municipalities.²⁵ A comparison of the pre-program predictor values within each actual-synthetic pair shows that the algorithm generally produces controls that are similar to their actual counterparts, although the equity ratio

²⁵For the admitted, the median number of contributing donors is 6. 75 percent of the admitted have more than 4 but fewer than 9 contributing donors.

and the share of right-wing parties seem to have been relatively difficult to match (results available on request).²⁶ A visual inspection of the pre-program evolution of costs in actual and synthetic municipalities also suggests that the algorithm yields controls with adequate fit for most municipalities, though large pre-program fluctuations in actual costs are a complicating factor in some cases.

Table 1 shows the average pre-program RMSPE in each of the four estimations (admitted vs. rejected, including vs. excluding neighbours in donor pool) at different cut-off levels.²⁷ 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. The number of municipalities passing the cut-off criterion ($\text{pre-RMSPE} < \text{cut-off}$) naturally decreases as the cut-off becomes stricter. The decrease is especially drastic in the estimations where neighbours are excluded from the donor pool, which confirms that neighbours are important contributors to the synthetic controls.

Table 1: Average pre-RMSPE per synthetic control estimation, by pre-RMSPE cut-off level.

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

In parentheses: number of municipalities whose $\text{pre-RMSPE} < \text{cut-off}$.

5.2 Average program effects

Figures 1 and 2 present the results from the synthetic control estimations for admitted and rejected municipalities, respectively. The figures show, for each of the years 1993-2010, the average of the raw differences between actual and synthetic log costs per capita. The dashed vertical line indicates the start of the post-program period, i.e. year 2000. The

²⁶We were unable to construct synthetic controls for admitted municipality Älvdalen and rejected municipality Gullspång, due to missing data for some years.

²⁷Lowering the cut-off even further to 0.01 reduces the number of placebo municipalities substantially (from 97 when $\text{pre-RMSPE} < 0.02$ to 37) and 26 out of 36 program municipalities are categorized as indeterminate. Using 0.04 as a cut-off yields results that are in between the results for 0.03 and 0.05.

black (gray) line represents the average actual-synthetic cost difference when neighbours are included in (excluded from) the donor pool.²⁸ In the upper right part of Figure 1 (Figure 2), the yearly averages are computed over all 36 (22) admitted (rejected) municipalities, regardless of pre-program fit; in the other parts of the figure, the averages are computed over the municipalities that pass the pre-program RMSPE cut-offs of 0.05, 0.03 and 0.02, respectively.

For both admitted and rejected municipalities, the estimated average differences are sensitive to whether neighbours are included in the donor pool or not. Starting with the admitted, the upper part of Figure 1 shows that the average actual-synthetic differences are positive most years from 1999 and onwards when neighbours are excluded from the donor pool. For the municipalities passing the lower RMSPE cut-offs (bottom row of figure), there is more or less no difference between actual and synthetic costs. When neighbours are allowed to enter the donor pool, the admitted municipalities have *lower* costs than their synthetic controls from 2001 onwards for all RMSPE cut-offs. The rejected (Figure 2) show roughly the same pattern as the admitted; unexpectedly high costs when neighbours are excluded from the donor pool disappear when neighbours are included in the donor pool (as well as when applying lower cut-offs). However, unlike the admitted, the rejected never show any sign of *reducing* their costs in relation to their synthetic controls.

The figures give us a hint of the reason for the deterioration of pre-program RMSPE when neighbours are excluded from the donor pool (c.f. Table 1) as much of this deterioration arises due to bad fit in 1999. The sensitivity to the inclusion of neighbours motivates a further investigation. In Appendix C, we therefore estimate synthetic controls for the 33 neighbours as well. In brief, we get a very poor fit for three of the municipalities that figure prominently in the synthetic controls 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. The average effect is positive; however, most neighbours follow their synthetic controls closely during the post-program period so neighbours in general do not seem to be affected by the program.²⁹

We next turn to the fixed effects (FE) estimations on the samples including admitted (rejected) municipalities and their synthetic controls over the period 1999-2010.³⁰ Tables 2 and 3 show the results for the samples of admitted and rejected, respectively. All actual-synthetic pairs enter the estimation; i.e. no pre-RMSPE cut-off is applied.³¹ Neighbours

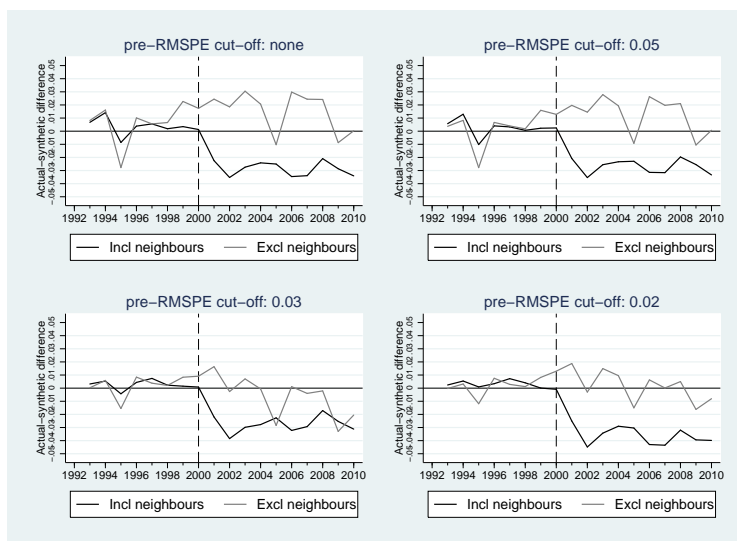
²⁸The point estimates and bootstrapped p-values for the raw differences in 2000-2010 are also shown in Appendix B, Table B.3 (including neighbours) and Table B.4 (excluding neighbours) respectively.

²⁹Note that our identifying assumptions carry over to the estimation for neighbours: i.e., just because some of the neighbours increase their costs unexpectedly after the program, we cannot be sure that it is due to the program rather than to something else.

³⁰See Appendix B for results for covariates.

³¹Our conclusions do not change if we instead include only municipalities with pre-RMSPE < 0.03, the

Figure 1: Average actual-synthetic difference, admitted



are allowed to contribute to the synthetic controls in columns (1)-(2), but not in columns (3)-(4). Columns (1) and (3) show the yearly average cost differences conditional only on municipality-specific and year-specific effects, while columns (2) and (4) show the results conditional also on covariates.

When neighbours are included in the donor pool (column 1 of Table 2), the admitted municipalities show a significantly lower cost level than their synthetic counterparts from 2001, the first full year of the program,³² and onwards. When neighbours are excluded from the donor pool (column 3), the estimates are much closer to zero and only significantly negative a few years. None of the coefficients are *positive* and significant though, contrary to what may be expected from the upper row of Figure 1. Apparently, the inclusion of municipality-fixed effects entails a downward adjustment of the differences.³³

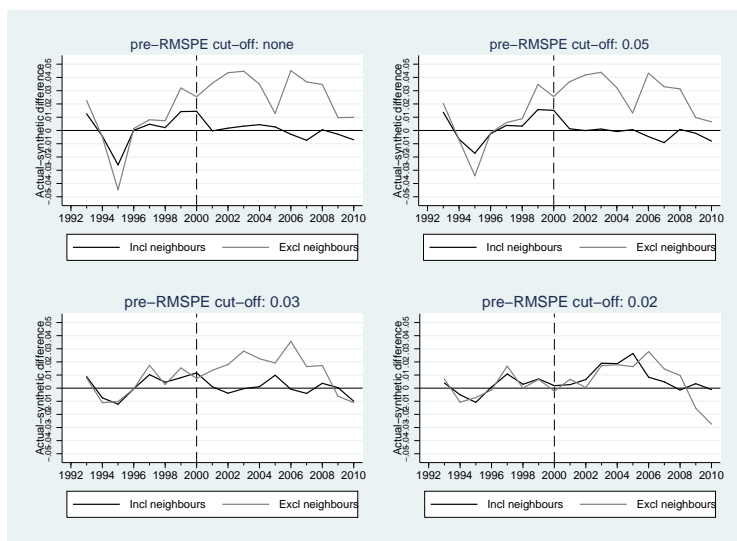
For the rejected (Table 3), there are almost no significant differences between actual and synthetic costs, regardless of whether neighbours are included in the donor pool or not. As for the admitted, the fixed effects seem to erase the seemingly positive effects in Figure 2 for the sample excluding neighbours.

Changes in the included covariates do not appear to drive the detected differences, as seen from a comparison of column (1) with column (2) and column (3) with column (4) (for each of Tables 2 and 3); the changes in the magnitude and significance of the results are in general very similar (results available on request).

³²Recall that applications were not approved/rejected until late 2000.

³³The actual-synthetic differences shown in Figure 1, i.e. the differences not accounting for municipality-specific effects or covariates, appear to be significantly positive according to the bootstrap p-values in Appendix B.

Figure 2: Average actual-synthetic difference, rejected



coefficients for both groups are mostly small for both groups.

In columns (5)-(8) of Tables 2 and 3, we use *net revenues per capita* as the dependent variable; column (5) corresponds to the specification used in column (1) etc. It can be noted that the coefficients now are expressed in thousands of SEK per capita, so a coefficient of 1 implies that admitted municipalities had 1 000 SEK higher net revenues per capita that year.

Three things stand out regarding the estimates for net revenues. *First*, the magnitudes of the yearly differences in Table 2 are very large. The estimated marginal effects for admitted municipalities amount to about 1000 SEK per capita, which is a little bit less than one standard deviation of the average for the period,³⁴ and the coefficients are highly significant most years. *Second*, we find little indications of a similar effect on the rejected municipalities (Table 3), though there are a few positive significant years (especially at the end of the period). A *third* and final observation is that the estimates are more or less insensitive to the exclusion of neighbours.

To sum up, the estimates for costs show decreased cost levels for admitted municipalities when neighbours are included in the sample, and hardly any differences to the comparison group when they are excluded. Rejected municipalities are close to the cost level of their comparison group, regardless of sample. The consistently positive estimates for the admitted, as well as the difference in the estimates for admitted and rejected, suggest that program participation is associated with a relatively favourable development

³⁴The standard deviation is about the same in the group of actual and synthetic as for the whole group of 290 municipalities.

of net revenues. In Appendix B, we show that similar results are obtained also when we estimate fixed effects models on the unweighted sample of municipalities (i.e. not applying the weights from the synthetic control algorithm). We also include results that indicate that the results in the sample *excluding* neighbours are sensitive to the chosen length of the period, especially for net revenues and for the rejected group. Importantly, per capita costs are still not significantly different from zero and net revenues are positive and significant most years 2002-2010 for the admitted group when we use the whole period 1993-2010. Thus, these results do not change our conclusions about the average effect for admitted municipalities, and the difference to the rejected group becomes, if anything, more marked.

5.3 Heterogeneous effects

The yearly average cost differences discussed in the previous section may hide substantial variation between municipalities. To examine this possibility, we investigate the actual-synthetic cost differences of each affected municipality (averaged over 2000-2010, see Equation (3)). We restrict our attention to the municipalities passing pre-program RMSPE cut-off of 0.05, to strike a balance between fit on the one hand and representativeness with respect to the whole group of affected (admitted or rejected) municipalities on the other. In order to classify each of the average cost differences as positive (cost increase), negative, or zero, we perform the placebo tests described in Section 4.4.

In the estimations where neighbours are *included* in the donor pool (Table 4, Panel A), admitted municipalities are over-represented in the lowest decile of a placebo distribution: out of the 34 municipalities passing the RMSPE criterion, 32 percent (11 municipalities) are classified as having reduced costs. The average cost reduction of these 11 municipalities is 7 percent, which can be compared to their average pre-program RMSPE of 2 percent.³⁵ 6 percent of the admitted appear to increase costs. For the rejected, the distribution is pretty similar to the placebo distributions: of 22 rejected municipalities, 14 percent (3 municipalities) are classified as having increased and 14 percent as having reduced their costs.

According to the estimates *excluding* neighbours from the donor pool (Table 4, Panel B), 8 out of 30 admitted and 6 out of 20 rejected are classified as having increased their costs, while the number reducing costs are fewer (4 admitted, 1 rejected). However, we would like to stress that the fit of the synthetic controls decrease noticeably with this donor pool and that the incorporation of fixed effects thus makes a large difference for the estimated average effects. Given the relatively poor fit with this donor pool, we believe that the fixed effects pick up important unobserved heterogeneity and thus do not view the

³⁵The one admitted municipality not passing the pre-RMSPE criterion of 0.05 (its pre-RMSPE is 0.0503) is also in the lowest decile of the placebo distribution. Its reduction amounts to 8 percent.

Table 2: Fixed effects estimations of program effects, admitted municipalities

Dependent:	(1) Costs	(2) Costs	(3) Costs	(4) Costs	(5) Net rev.	(6) Net rev.	(7) Net rev.	(8) Net rev.
<i>admitted</i> × 2000	-0.00221 (0.00517)	0.000653 (0.00533)	-0.00525 (0.00423)	-0.000214 (0.00458)	-0.387 (0.325)	-0.322 (0.331)	-0.317 (0.287)	-0.246 (0.290)
<i>admitted</i> × 2001	-0.0259*** (0.00649)	-0.0232*** (0.00684)	0.00176 (0.00624)	0.00665 (0.00673)	0.796** (0.396)	0.877** (0.405)	0.784** (0.375)	0.907** (0.389)
<i>admitted</i> × 2002	-0.0388*** (0.00786)	-0.0476*** (0.00730)	-0.00411 (0.00815)	-0.0135* (0.00682)	2.381*** (0.365)	2.443*** (0.382)	1.694*** (0.351)	1.808*** (0.379)
<i>admitted</i> × 2003	-0.0309*** (0.00954)	-0.0420*** (0.0105)	0.00785 (0.00987)	-0.00453 (0.0111)	1.103*** (0.272)	1.184*** (0.294)	1.020*** (0.268)	1.170*** (0.323)
<i>admitted</i> × 2004	-0.0276*** (0.00988)	-0.0383*** (0.0104)	-0.00202 (0.0102)	-0.00813 (0.0103)	0.516* (0.286)	0.599* (0.300)	1.412*** (0.276)	1.609*** (0.290)
<i>admitted</i> × 2005	-0.0285*** (0.0102)	-0.0339*** (0.0117)	-0.0330*** (0.0106)	-0.0376*** (0.0103)	1.027*** (0.281)	1.166*** (0.315)	2.560*** (0.341)	2.719*** (0.383)
<i>admitted</i> × 2006	-0.0381*** (0.0109)	-0.0415*** (0.0135)	0.00715 (0.0114)	0.00452 (0.0123)	1.332*** (0.248)	1.488*** (0.302)	0.840*** (0.216)	1.019*** (0.295)
<i>admitted</i> × 2007	-0.0374*** (0.0113)	-0.0444*** (0.0141)	0.00173 (0.0119)	0.000818 (0.0126)	1.634*** (0.298)	1.773*** (0.336)	1.382*** (0.266)	1.534*** (0.297)
<i>admitted</i> × 2008	-0.0244** (0.0116)	-0.0282* (0.0159)	0.00152 (0.0123)	0.00253 (0.0132)	1.840*** (0.331)	2.019*** (0.370)	1.194*** (0.262)	1.389*** (0.322)
<i>admitted</i> × 2009	-0.0321** (0.0124)	-0.0318* (0.0175)	-0.0314** (0.0120)	-0.0280** (0.0139)	1.397*** (0.303)	1.636*** (0.361)	1.939*** (0.265)	2.143*** (0.291)
<i>admitted</i> × 2010	-0.0376*** (0.0130)	-0.0370* (0.0186)	-0.0224* (0.0132)	-0.0205 (0.0153)	1.556*** (0.307)	1.738*** (0.365)	1.244*** (0.269)	1.416*** (0.312)
Constant	3.875*** (0.00413)	0.135 (1.890)	3.863*** (0.00426)	-0.338 (1.849)	-0.147 (0.123)	-76.78** (37.75)	-0.363*** (0.0927)	-64.24* (34.17)
Covariates	N	Y	N	Y	N	Y	N	Y
Neighbours in d.p.	Y	Y	N	N	Y	Y	N	N
Observations	840	840	816	816	840	840	816	816
Nr of municipalities	70	70	68	68	70	70	68	68
R ²	0.917	0.930	0.912	0.923	0.440	0.451	0.447	0.458
F	365.5	444.9	820.3	556.8	53.47	53.63	142.6	102.7

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Year fixed effects included in all models. Ålvdalen is excluded in all samples. Dorotea is excluded in samples excluding neighbours from the donor pool.

Table 3: Fixed effects estimations of program effects, rejected municipalities

Dependent:	(1) Costs	(2) Costs	(3) Costs	(4) Costs	(5) Net rev.	(6) Net rev.	(7) Net rev.	(8) Net rev.
<i>rejected</i> × 2000	0.000225 (0.00737)	0.00217 (0.00788)	-0.00660 (0.00725)	-0.00225 (0.00776)	0.0238 (0.439)	0.0459 (0.442)	0.136 (0.417)	0.117 (0.426)
<i>rejected</i> × 2001	-0.0145 (0.00893)	-0.0122 (0.00911)	0.00356 (0.00889)	0.00493 (0.00870)	0.513 (0.422)	0.526 (0.426)	0.594 (0.415)	0.562 (0.411)
<i>rejected</i> × 2002	-0.0125 (0.0111)	-0.0153 (0.0108)	0.0115 (0.0116)	0.00393 (0.0104)	0.581 (0.534)	0.899* (0.534)	0.209 (0.540)	0.363 (0.517)
<i>rejected</i> × 2003	-0.0109 (0.0114)	-0.0127 (0.00991)	0.0126 (0.0117)	0.00495 (0.00908)	0.251 (0.396)	0.541 (0.384)	0.124 (0.387)	0.217 (0.350)
<i>rejected</i> × 2004	-0.00985 (0.0146)	-0.0200 (0.0137)	0.00288 (0.0145)	-0.00628 (0.0124)	0.0957 (0.404)	0.391 (0.348)	0.838* (0.416)	0.916** (0.354)
<i>rejected</i> × 2005	-0.0116 (0.0144)	-0.0173 (0.0119)	-0.0192 (0.0142)	-0.0290*** (0.0107)	0.468 (0.365)	0.798** (0.350)	1.640*** (0.437)	1.696*** (0.424)
<i>rejected</i> × 2006	-0.0171 (0.0150)	-0.0145 (0.0124)	0.0131 (0.0157)	0.00863 (0.0123)	0.581 (0.374)	0.750* (0.373)	0.197 (0.338)	0.242 (0.373)
<i>rejected</i> × 2007	-0.0216 (0.0153)	-0.0189 (0.0128)	0.00455 (0.0160)	0.00519 (0.0136)	0.570 (0.412)	0.783* (0.410)	0.410 (0.387)	0.476 (0.455)
<i>rejected</i> × 2008	-0.0137 (0.0160)	-0.00521 (0.0126)	0.00266 (0.0166)	0.00498 (0.0134)	0.662 (0.451)	0.904** (0.424)	0.310 (0.408)	0.368 (0.450)
<i>rejected</i> × 2009	-0.0171 (0.0180)	-0.00682 (0.0136)	-0.0224 (0.0179)	-0.0197 (0.0141)	0.712** (0.349)	0.953*** (0.307)	1.111*** (0.332)	1.189*** (0.370)
<i>rejected</i> × 2010	-0.0213 (0.0175)	-0.0185 (0.0143)	-0.0222 (0.0174)	-0.0239 (0.0147)	1.891** (0.719)	2.256*** (0.756)	1.762** (0.667)	2.007*** (0.836)
Constant	3.849*** (0.00544)	-2.716* (1.474)	3.846*** (0.00563)	-3.578** (1.516)	0.274 (0.181)	-5.479 (48.36)	0.0874 (0.159)	43.64 (56.15)
Controls	N	Y	N	Y	N	Y	N	Y
Neighbours in d.p.	Y	Y	N	N	Y	Y	N	N
Observations	528	528	528	528	528	528	528	528
Number of municipalities	44	44	44	44	44	44	44	44
R ²	0.919	0.941	0.915	0.937	0.316	0.363	0.339	0.386
F	275.9	560.9	379.2	421.0	22.97	18.82	55.88	60.77

Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Year fixed effects included in all models. Gullspång excluded from all samples.

raw actual-synthetic differences as equally reliable as for the sample including neighbours in the donor pool. With this caveat in mind, it may however be noted that the raw actual-synthetic differences for *neighbours* show a similar pattern of heterogeneity, with 20 percent (6 municipalities) in the highest decile of a placebo distribution and 6 percent (2 municipalities) in the lowest decile.

This analysis reveals great heterogeneity in the post-program differences.³⁶ In particular, the average negative cost differences for admitted municipalities when neighbours are included in the donor pool appear to be driven by a subset of this group, while two thirds of the admitted show no indication of a program effect. Regardless of which donor pool one prefers, it seems reasonable to conclude that *for most municipalities, there is little evidence that the program implies increased costs in the long run.*

Table 4: Distribution of individual program effects

Panel A	Donor pool: including neighbours (130 municipalities)		
	(1)	(2)	(3)
Group	Cost reduction	No change	Cost increase
Admitted	11	21	2
Rejected	3	15	3
Panel B	Donor pool: excluding neighbours (98 municipalities)		
	(1)	(2)	(3)
Group	Cost reduction	No change	Cost increase
Admitted	4	18	8
Rejected	1	13	6

6 Exploring sources of response heterogeneity

We finally examine whether certain structural characteristics, institutions, and attitudes can explain why some of the admitted municipalities managed to hold back costs more than others. Restricting our attention to the estimations where neighbours are included in the donor pool, we compare the 12 municipalities that appear to have reduced costs (the cost-reducer group) to the 23 municipalities that do not reduce costs (the non-reducer group) according to the placebo analysis.³⁷ As the sample size is very small, we foremost interpret differences between the two groups as potentially fruitful directions for future investigations.

³⁶We cannot perform the same analysis for net revenues, but looking at the raw averages over the period 2000-2010 for the admitted, these range from -1.2 to 2.6 percent of gross tax revenues. Thus, there seems to be great heterogeneity also for this variable.

³⁷We do not apply a pre-RMSPE cut-off; hence there are 12 instead of 11 cost-reducers. The twelfth municipality has a pre-RMSPE of 0.0503, which is not strikingly larger than the 0.05 cut-off applied in section 4.

Table D.1 in Appendix D shows (two-sided) t-tests for equal means (or equal proportions, where applicable) between the cost-reducer and non-reducer groups for a set of candidate explanatory variables. In the interest of space, we delimit the discussion here to variables that differ significantly between the groups or are of particular interest for other reasons.

As a primarily methodological check, we examine whether the different developments of costs in the two groups relate to the importance of neighbours in their respective synthetic controls. For each synthetic control, we compute the share of the total weight that derives from neighbours to the admitted municipalities. This share is rather large for most of the 35 municipalities – the mean is 0.64 and the median is 0.74. Though the mean share is higher in the group of reducers than in the group of non-reducers (0.74 vs. 0.60), the difference between the two means is not statistically significant (p-value=0.41). Moreover, the correlation between the share of neighbours and the average actual-synthetic cost difference ($average_i$) is small (-0.093) and insignificant (p-value=0.59).

A notable difference between the groups is that the share receiving assistance from the contemporary housing program is higher in the group of cost-reducers (83 percent) than in the non-reducer group (52 percent) (p-value=0.070). This difference may indicate that participation in two programs – both of which coupled grants with costly efforts – was necessary to enable a turn towards fiscal discipline. It may likewise mean that the general program did not affect fiscal discipline at all, but that the housing program was the real wake-up call.³⁸ Another possibility is that the cost reductions only capture that the municipalities whose housing companies had been reconstructed no longer had to transfer funds to their housing companies. In Appendix B, we show however that costs were not only reduced in areas where such transfers would be recorded.³⁹ Furthermore, there is no indication that the municipalities admitted *only* to the housing program reduce costs in other areas. Thus, for whatever reason, the cost-reducers appear to have engaged in a rather broad cost reduction effort.

Another significant difference between the cost-reducers and non-reducers relates to the size of the grants received within the bailout program (*Kommundelelegationen*): on average, the grant amounted to 6 percent of total costs for the cost-reducers in 2000, but to 4 percent for the non-reducers (p-value=0.067). As the cost-reducers are over-represented in the housing program, there is also a large difference in the ratio of grants received

³⁸Interviews with representatives from a few of the admitted municipalities shortly after they received their grant give some support for the idea that the housing program was a wake-up call; some express that it was no longer possible to ignore the severity of the municipality's financial problems when fully functional apartments were destroyed as part of the housing program (SOU, 2003).

³⁹Moreover and importantly, a majority of book-keeping posts in these two areas are also unrelated to housing (Statistics Sweden, 2012a) and the areas are small in comparison total costs (on average for all municipalities, the two categories amount to 13 percent of total costs in 2010).

from *both* programs to total costs; on average, total grants amount to 17 percent of total costs for the cost-reducers but to 8 percent for the non-reducers (p-value=0.011). These findings may relate to between-group differences in motivation and/or ability to reduce costs, as the size of the grant was positively related to the size of the cost reductions in the agreement (SOU, 2003).

An argument in favour of differences in ability rather than motivation is that the cost-reducers historically have received relatively many deficit grants from the central government: on average, municipalities in this group received deficit grants from the central government in 10 of the years 1979-1992. The corresponding average is 6 in the non-reducer group and the difference is statistically significant (p-value=0.013). Moreover, the average proportion of neighbours receiving deficit grants (again during 1979-92) is higher for the cost-reducers (0.56) than for the non-reducers (0.46)(p-value of difference = 0.068). It certainly seems counter-intuitive that municipalities that are used to relying on the central government suddenly (i.e. at the time of application to the program) would be particularly motivated to increase fiscal discipline. In fact, Pettersson-Lidbom (2010) shows that municipalities that received many grants in the 1980s were more likely to apply for the program under study here, and interpret this result as a sign that the applicants were particularly likely to believe that the central government would come to their rescue – hardly a sign of pre-program motivation.⁴⁰ Moreover, both groups have bought consultant services from the Swedish Association of Local Authorities and Regions to a similar degree (SALAR has a special unit that, against a fee, helps municipalities to improve their fiscal situation), and the political commitment to long-term budgets is also not different. Both these variables are reasonable proxies for fiscal motivation.

There are on the other hand between-group differences that supports ability as an explanation for the heterogeneity. The average (over 2000-2010) share of right-wing parties in the municipal council is lower in the cost-reducer group, 30 percent versus 42 percent for the non-reducers (p-value=0.010). This difference also reflect differences in electoral uncertainty: in the most recent election before the program was initiated (held in 1998), the right-wing parties had between 45 and 55 percent of the votes in one third (8) of the non-reducer municipalities, while there were no such close elections in the cost-reducer group (p-value of difference = 0.020). The cost-reducers could thus implement cost reductions with less fear of losing the next election, while the situation was different in the other group.

The relative increase in fees and total revenues between 2000-2010 is significantly higher in the non-reducer group (p-value = 0.016 and 0.002 respectively). This group

⁴⁰The grant was reasonably the prime incentive to participate in the program. Any actions taken to increase fiscal discipline during the program would in principle be possible to implement without involvement of the central government or the program committee.

has also increased their tax rates more (although not significantly so, p -value = 0.137). These differences may be related to the differences in electoral uncertainty between the two groups. It may be less costly (in terms of votes) to raise taxes and fees than to cut spending on popular services; thus, municipalities with close elections may opt for the strategy to increase revenues, while municipalities with more certain majorities can afford to choose the cost-reducing strategy. In relation to this possibility, it can be noted that the positive and significant coefficients in the FE regressions on net revenues are not driven by the group of cost-reducers (results available on request). There seem to be less heterogeneity when it comes to net revenues than when it comes to costs.

Apart from these variables, we find no significant between-group differences for any of the examined demographic, economic, political, and institutional variables. Missing values for the institutional variables is a concern however; thus, we do not rule out that institutions may be a channel for the differences between the groups.

7 Conclusions

None of our main specifications indicate that the admitted municipalities on average have increased costs significantly, and all specifications indicate that they on average have increased net revenues significantly. There is heterogeneity behind the average results though; some are more prone to cut costs while others mainly increase revenues. A cautious interpretation is that conditional discretionary intergovernmental grants need not have negative effects on fiscal discipline. A stronger claim is that the program even increased fiscal discipline in several municipalities.

The assumptions needed to identify causal effects of the program are untestable, but we can discuss their validity in relation to the two interpretations. Of the municipalities in the comparison group, we believe that neighbours to the admitted are the most likely to be influenced by the program and we find evidence consistent with such spillover effects in a few cases. SUTVA is thus least likely to be violated when we exclude neighbours from the comparison group. In these estimations, we find no significant effects on the post-program costs of the admitted; thus, there is support for the more cautious of our interpretations. As the admitted have significantly higher net revenues in this sample, there is even support for the stronger claim. It should be pointed out though that the estimates for net revenues rely on the additional (and in our view reasonable) assumption that the synthetic control municipalities constructed for costs are valid also for net revenues.

The admitted and their neighbours are similar in many respects. While increasing the credibility of SUTVA, the exclusion of neighbours therefore simultaneously reduces the credibility of the unconfoundedness assumption. For the sample including neighbours, the admitted on average have significantly lower costs and higher net revenues than their

synthetic controls. If SUTVA holds, these results support the stronger claim. Notably though, even if SUTVA does not hold and the neighbours *are* affected by the program, the results suggest that fiscal discipline benefited less from, or was harmed more by, non-participation than from participation in the program. Whether fiscal discipline overall benefited from or was harmed by the program can however not be established in this case.

Though we compare very similar units and control for time-invariant characteristics, unconfoundedness may still be threatened by unobserved time-variant characteristics. In relation to the cautious interpretation, it is for instance conceivable that the program harmed the admitted municipalities' motivation for fiscal discipline and that they would have displayed even better outcomes if the program had not existed. In relation to the stronger claim, the most concerning confounder is that the admitted municipalities *for reasons unrelated to the program* have become more motivated to come to terms with their fiscal situation. We find unobserved fiscal motivation less worrying for two reasons:

First, in most samples and for both outcome variables, there is a visible turn towards more discipline in 2001. This was the first year when admitted municipalities had time and explicit incentives to react to the content of the program (rather than just to its announcement). Among all conceivable explanations for the timing of the turn, a program effect appears most plausible.

Second, we find little evidence of improvements for the municipalities that were denied to participate in the program, who were similar to the admitted in many respects and obviously were motivated enough to apply to the program. We cannot rule out that the program committee was able to discern and admit only the most motivated applicants. Motivation at the time of admission should however be captured by the fixed effects, and thus cannot explain the different results for the two groups. The most plausible explanation instead relates to participation in the program: while the admitted could use a pending grant to convince the opposition and/or the public about the necessity of improving discipline, the rejected had no such means at hand.⁴¹

We do not intend to downplay the importance of motivation for the establishment of fiscal discipline. As long as debt roll-over is possible, motivation is a prerequisite for fiscal discipline. It is also the only channel through which the program possibly may have affected the municipalities' behaviour after its closure. Our point is rather that it is hard to explain the change on average for the admitted without referring to their participation in the program. On balance, we think that the most plausible interpretation of our results is that the program did not reduce the fiscal discipline of the admitted, and that it even had beneficial effects on fiscal discipline in several cases.

Only some of the admitted municipalities reduce costs significantly compared to their synthetic controls. This group does not appear to drive the results for net revenues and

⁴¹We thank Magnus Henreksson for suggesting this explanation.

we find no differences in motivation between the two groups of admitted municipalities. A tentative explanation is instead that the incumbent politicians in municipalities opting for the cost reducing strategy had more certain majorities, and thus could afford to cut costs without fear of losing the next election.

The contrast between our results and the message from previous studies suggests that the conditions attached to the grants, a distinguishing factor of the program under study, may be a key component in dampening the soft-budget effect of discretionary intergovernmental grants. If the government clearly announces that harsh conditions will be applied, negative spillover effects on other units may moreover be mitigated. This is important as previous research (Pettersson-Lidbom, 2010) as well as our findings are consistent with a spillover interpretation. However, to claim more conclusively that conditions are crucial we would need larger samples and more variation in the conditions. This presents an interesting avenue for future research in other contexts.

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A Descriptive statistics

This section shows descriptive statistics for the municipalities, divided into admitted, rejected, and others. Table A.1-A.3 display variables corresponding to the selection criteria for the program, as well as the number of bailouts and share of neighbours with at least one bailout during the earlier regime of discretionary transfers. Table A.4-A.6 display summary statistics for the time-varying covariates in 1999. Economic variables are in 2010 prices.

Table A.1: Selection criteria and initial bailout expectations, admitted municipalities

Variable	Mean	Std. Dev.	Min.	Max.	N
Total costs 1998 (KSEK/capita)	45.5	5.7	29.9	57.5	36
Debt 1998, incl pensions (KSEK/capita)	37.3	9.4	24.9	65.7	36
Equity ratio 1998 (%)	50.4	17.0	12.7	78.6	36
Pop growth 94-98 (%)	-4.7	1.9	-8.2	1.8	36
Number of bailouts 79-92	7.9	4.1	0	14	36
Share neighbours with bailout 79-92 (%)	50.0	16.6	8.6	77.1	36

Table A.2: Selection criteria and initial bailout expectations, rejected municipalities

Variable	Mean	Std. Dev.	Min.	Max.	N
Total costs 1998 (KSEK/capita)	43.8	4.6	34.9	51.5	23
Debt 1998, incl pensions (KSEK/capita)	40.1	14.1	23.0	92.8	23
Equity ratio 1998 (%)	47.3	21.7	-5.5	82.2	23
Pop growth 94-98 (%)	-4.8	2.5	-8.3	4.7	23
Number of bailouts 79-92	7.7	3.3	0	13	23
Share neighbours with bailout 79-92 (%)	40.8	11.8	17.9	57.1	23

Table A.3: Selection criteria and initial bailout expectations, others

Variable	Mean	Std. Dev.)	Min.	Max.	N
Total costs 1998 (KSEK/capita)	39.9	4.6	30.8	57.3	229
Debt 1998, incl pensions (KSEK/capita)	31.7	11.7	11.4	84.8	229
Equity ratio 1998 (%)	59.1	17.9	-4.4	92.7	229
Pop growth 94-98 (%)	-1.2	3.3	-8.4	13.3	227
Number of bailouts 79-92	4.2	3.8	0	14	226
Share neighbours with bailout 79-92 (%)	30.3	19.7	0	100	224

Table A.4: Summary statistics, admitted municipalities

Variable	Mean	Std. Dev.	Min.	Max.	N
Tax base (KSEK/capita)	112.0	10.1	90.4	139.5	36
Central gov. grant (KSEK/capita)	10.3	5.1	-1.1	23.2	35
Employment rate, 16+ (%)	50.5	5.4	37.6	69.4	36
Population size	12177.8	6498.7	2746	28872	36
Share 0-14 (%)	17.9	1.5	15.6	23.0	36
Share +65 (%)	21.7	3.9	8.1	28.8	36
Share right-wing (%)	35.5	13.8	8.6	67.7	36
Herfindahl	0.26	0.05	0.18	0.36	36
Number of seats	40.1	7.4	31	61	36

Table A.5: Summary statistics, rejected municipalities

Variable	Mean	Std. Dev.	Min.	Max.	N
Tax base (KSEK/capita)	111.7	11.3	97.9	135.6	23
Central gov. grant (KSEK/capita)	9.2	4.6	1.0	21.6	23
Employment rate, 16+ (%)	52.1	4.4	41.3	64.5	23
Population size	14658.4	15755.4	4304	64096	23
Share 0-14 (%)	18.5	1.5	15.8	22.6	23
Share +65 (%)	20.9	2.5	13.4	26.1	23
Share right-wing (%)	39.7	13.9	22.6	66.7	23
Herfindahl	0.26	0.05	0.18	0.38	23
Number of seats	40.6	9.3	31	61	23

Table A.6: Summary statistics, others

Variable	Mean	Std. Dev.	Min.	Max.	N
Tax base (KSEK/capita)	117.0	15.6	94.5	215.7	230
Central gov. grant (KSEK/capita)	6.8	3.9	-7.0	20.7	230
Employment rate, 16+ (%)	55.9	5.0	45.0	69.9	230
Population size	35156.0	63524.9	3244	743703	230
Share 0-14 (%)	19.1	1.7	13.5	24.2	230
Share 65+ (%)	18.4	3.7	8.6	28.1	230
Share right-wing (%)	45.9	11.4	13.7	86.7	230
Herfindahl	0.24	.04	0.17	0.51	230
Number of seats	47.9	11.9	31	101	230

B Sensitivity tests and covariate estimates

For comparison, this Appendix includes estimates from FE specifications on the "raw" sample of municipalities, i.e. not applying the weights obtained from the synthetic control method. We also estimate similar FE specifications as in the main text, but include more pre-program years, and present estimates where the dependent variable is disaggregated into costs possibly related to housing and costs unrelated to housing. Finally, we present the raw actual-synthetic cost differences, as well as bootstrap estimates of the significance of these differences.

Table B.1 shows results from fixed effects regressions where we do not apply the weights obtained from the synthetic control method. The estimation samples cover the whole period 1993-2010. To capture the long-run effect for admitted and rejected municipalities in the same regression, we use two dummy variables (*admitted* and *rejected*) that take on the value 1 from 2000 and onwards for the respective groups.

In column (1)-(4) we use per capita operating costs as dependent variable. In column (1) the full sample of 290 municipalities is included. The admitted coefficient is negative, significant and amounts to about 2 percent lower cost level on average, while the rejected coefficient is positive and insignificant. In column (2), we let the dummy variables take the value 1 already in 1999. The admitted coefficient is still negative but now insignificant. The rejected coefficient becomes somewhat more positive, but is still insignificant. In column (3) and (4) we let the samples mimic the donor pools used in the synthetic control estimation: (3) includes the 33 neighbours of admitted municipalities that were not excluded for other reasons, while (4) excludes this group. In these two estimations, we also exclude the admitted and rejected municipalities that we were unable to develop synthetic controls for; i.e. column (3) excludes Älvdalen and Gullspång and column (4) excludes also Dorotea. In line with the baseline estimates presented in section 5, the coefficient for the admitted group is negative and significant when neighbours are included, and more or less of the same size as in the full sample, while less negative and insignificant when neighbours are excluded. The coefficient on rejected municipalities is positive, but small and insignificant in both columns which is also in line with our baseline estimates. Furthermore, the coefficient on admitted municipalities is significantly lower than the rejected coefficient on at least the 10 percent level in all columns (1)-(4).

Column (5)-(7) instead use per capita net revenues as dependent variable. In column (5) we again use the full sample, while column (6) and (7) corresponds to the sample used in columns (3) and (4) respectively. The admitted coefficient is positive, significant and large in all samples: the magnitude corresponds to 500 SEK per capita higher net revenues on average (for comparison, the mean and standard deviation for all municipalities 2000-2010 is 621 and 1,272 SEK per capita respectively). The rejected coefficient is positive,

insignificant and about half the size of the admitted coefficient. The difference between the two groups is not significant in any sample.⁴²

As in the baseline estimation, we include government grants and its square in these estimations, although this variable may have been directly affected by the program. There is however, just as in the baseline, hardly any effect on the admitted and rejected coefficients if we instead exclude these two variables (results available on request).

In our baseline FE estimations, we use a short sample from 1999-2010 to capture more of the unobserved heterogeneity (as more things should be fixed over a shorter period). This is especially important in the specifications where the fit of the synthetic controls is less good as in the samples excluding neighbours, but also for the estimations of net revenues. However, this approach may be problematic if the difference between actual and synthetic municipalities is large for some idiosyncratic reason in 1999.

To see if this is a problem, we re-run our baseline FE regressions with the samples of actual and synthetic municipalities but use the whole period 1993-2010. When neighbours are included in the sample, this yields similar results for both admitted and rejected – very much alike for costs, somewhat more attenuated coefficients for net revenues but still large and highly significant most years (results not shown). This is fully in line with the view that the fixed effects are less important in these samples. In column (1) and (2) of table B.2, we replicate the potentially more problematic specifications that excludes neighbours for the admitted group. Column (1) shows coefficients using per capita costs as dependent variable and including covariates (compare column (4) of Table 2). There are some significant and positive years for costs but most are insignificant, especially towards the end of the period where there are also some negative coefficients. In column (2), we show the coefficients for a similar specification with net revenues as dependent variable (compare column (8) of Table 2). These are smaller and less significant, but still positive all years except one during 2002-2010, and large and significant for several of these years. For both costs and net revenues we get closer to the coefficients from the estimation on the 1999-2010 sample as we progressively shorten the sample (results available on request). Thus, we do not think that these results should change our main conclusion that fiscal discipline for the admitted group have not deteriorated on average, and have increased for several municipalities.

The changes for the rejected group are larger when we exclude neighbours, especially for costs. The *rejected* \times *year* coefficients using costs as dependent variable are consistently positive, larger than in the baseline, and significant for a majority of the post-program years in the 1993-2010 sample. The results for 1999-2010 also seems more special compared

⁴²To save space, we do not include estimates with the program taking effect in 1999 for net revenues in the table, but both coefficients become smaller and are insignificant in this specification (results available on request).

Table B.1: Fixed effects specifications, 1993-2010

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Specification:	Full	Prog.	Incl neigh-	Excl neigh-	Full	Incl neigh-	Excl neigh-
Dependent var:	sample	1999	bours	bours	sample	bours	bours
	costs	costs	costs	costs	net rev	net rev	net rev
<i>admitted</i>	-0.021** (0.010)	-0.015 (0.010)	-0.020** (0.010)	-0.014 (0.011)	0.493*** (0.152)	0.554*** (0.172)	0.437** (0.181)
<i>rejected</i>	0.006 (0.010)	0.012 (0.010)	0.004 (0.010)	0.007 (0.011)	0.235 (0.205)	0.267 (0.210)	0.194 (0.220)
<i>log(taxbase)</i>	0.652*** (0.093)	0.657*** (0.093)	0.483*** (0.084)	0.509*** (0.099)	-0.465 (1.323)	-0.249 (1.734)	-2.260 (1.844)
<i>eq.grant</i>	0.0066*** (0.0020)	0.0065*** (0.0021)	0.0034** (0.0017)	0.0027 (0.0016)	0.117*** (0.024)	0.105*** (0.027)	0.0794*** (0.025)
<i>eq.grant</i> ²	0.0001 (0.0000)	0.0001 (0.0000)	0.0003*** (0.0000)	0.0003*** (0.0000)	-0.0013 (0.0011)	-0.0021 (0.0013)	-0.0011 (0.0014)
<i>employment rate</i>	-0.0039*** (0.0015)	-0.0040*** (0.0015)	-0.0030** (0.0014)	-0.0035** (0.0016)	0.055** (0.022)	0.049 (0.030)	0.058* (0.031)
<i>log(population)</i>	-0.041 (0.064)	-0.033 (0.064)	0.061 (0.073)	0.075 (0.084)	4.25*** (0.89)	3.62*** (1.19)	3.31** (1.36)
<i>share 0-14</i>	0.0065 (0.0041)	0.0065 (0.0041)	0.0029 (0.0043)	0.0034 (0.0049)	-0.0058 (0.057)	0.050 (0.069)	0.012 (0.073)
<i>share 65+</i>	0.011*** (0.0032)	0.010*** (0.0032)	0.0058** (0.0025)	0.0065** (0.0027)	-0.0077 (0.035)	0.032 (0.047)	0.035 (0.049)
<i>share right-wing</i>	-0.00013 (0.00037)	-0.00013 (0.00037)	-0.00033 (0.00044)	-0.00061 (0.00049)	0.012** (0.0056)	0.018*** (0.0068)	0.020*** (0.0074)
<i>herfindahl</i>	0.148*** (0.0509)	0.147*** (0.0507)	0.105* (0.0579)	0.0891 (0.0641)	1.168 (0.842)	1.781 (1.094)	1.668 (1.175)
<i>seats</i>	0.00025 (0.0006)	0.00031 (0.0006)	-0.0000 (0.0008)	-0.0000 (0.0008)	-0.00851 (0.0105)	-0.00555 (0.0130)	0.00533 (0.0146)
Constant	0.861 (0.714)	0.760 (0.716)	0.813 (0.791)	0.567 (0.884)	-43.43*** (10.22)	-39.92*** (14.08)	-27.68* (14.72)
Year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,198	5,198	3,474	2,862	5,198	3,474	2,862
Municipalities	290	290	193	159	290	193	159
F	483.8	473.2	403.5	298.0	44.86	36.05	33.23
R ²	0.929	0.929	0.944	0.942	0.237	0.244	0.252

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Column (1) and (2) includes all 290 municipalities.

Column (3) includes the donor pool of 136 municipalities, plus 35 admitted and 22 rejected municipalities.

Column (4) includes the donor pool of 103 municipalities, plus 34 admitted and 22 rejected municipalities.

Column (5)-(7) use the same sample as column (1), (3), and (4) respectively.

to the results for the admitted group, as there are still many positive and significant coefficients for the intermediate sample lengths as well. When we use net revenues as the dependent variable, the coefficients are also smaller and some are negative (although never significant), while there are still some large, positive and significant years in the 1993-2010 sample (all results available on requests). As the synthetic controls have worse fit for the rejected group, we are more reluctant to draw firm conclusions from these results, but the difference to the admitted group definitely seem to remain also in these specifications.

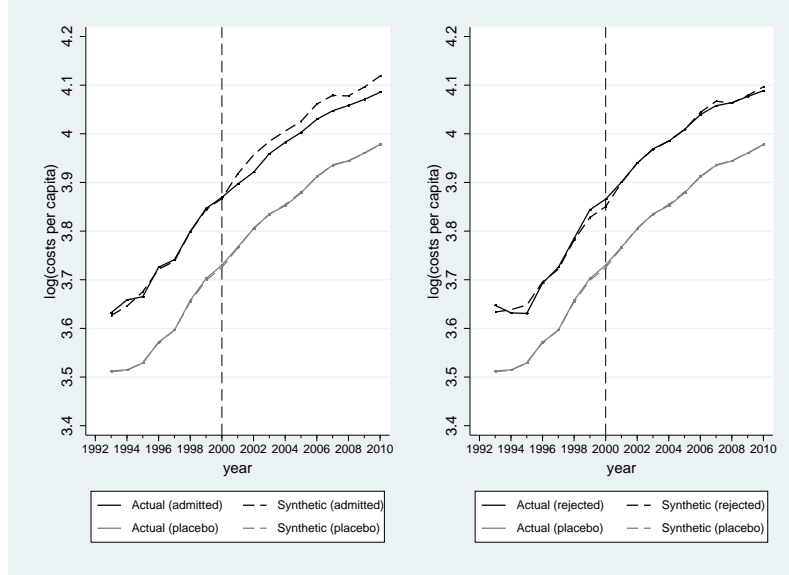
Columns (3)-(6) in Table B.2 show FE models with the per capita costs variable disaggregated into two: costs potentially related to housing and costs unrelated to housing. As discussed in Section 6, we want to examine whether the cost reductions of admitted municipalities are only a mechanical implication of having reconstructed their troubled housing companies.⁴³ This may be the case if municipalities made transfers to their troubled housing companies before the reconstruction began, but no longer have a reason to do so after the reconstruction. The cost reductions we find in our synthetic control estimations are then unrelated to changes in fiscal discipline. The dependent variable *housing* covers the bookkeeping posts where transfers to housing companies should be recorded (Statistics Sweden, 2012a, p. 41 and 50);⁴⁴ it should however be noted that these posts contain a lot more than just housing related costs. *non-housing* covers all other bookkeeping posts. In columns (3) and (4), the estimation sample consists of admitted municipalities and the donor pool including neighbours during the period 1998-2010 (we do not have data over the different areas of costs further back). The estimates show that the admitted municipalities have had significantly lower values of *non-housing* during most of the post-program period, while the level of (potentially) housing-related costs is not significantly different except in 2002 (although the point estimates are sometimes large).

In columns (5) and (6) of Table B.2, we estimate a FE model for the full sample of municipalities while including single-dummies for admitted and rejected. This allows us to study also the municipalities that were in the housing program but that did not apply to the bailout program. *housing program*, a dummy equal to one from the year a municipality was admitted to the housing program and onwards, is insignificant for both types of costs (although very close to significant for potentially housing related costs). The admitted dummy is negative but insignificant for housing related costs, while negative and significant for non-housing related costs. This result does not support the hypothesis that the program effect for the cost-reducers was only due to their participation in the housing

⁴³It is common practice to have municipally owned commercial real estate and apartments for rent in a separate limited liability company, and not as a part of the regular municipal administration. All municipalities admitted to both programs except one (a non-reducer) followed this common practice already before the two programs started, the cost reductions should thus not be caused by reducers simply moving housing costs off the revenues and cost statement and into a separate company.

⁴⁴*Infrastruktur* and *Affärsverksamhet*.

Figure 3: Actual and synthetic (including neighbours) average log costs per capita (pre-RMSPE < 0.05)



program.

B.1 Synthetic control estimates and inference

This section displays the yearly averages of the raw actual-synthetic difference in costs. Starting with the results when neighbours are *included* in the donor pool, the solid black lines in Figure 3 shows average per capita costs for admitted (left panel) and rejected (right panel) municipalities; the dashed black lines show the corresponding averages for the synthetic controls. The gray lines display the corresponding graphs for the placebo group, that is, the donor pool (note that admitted and rejected have the same placebo group); evidently and reassuringly, there are no signs of any program effect for the placebo group. Only observations with a pre-RMSPE lower than the 0.05 cut-off are included in the figure. Results for each RMSPE cut-off are shown in Table B.3.

The inference on the yearly average program effects in Section 5 relies on standard errors from the fixed effects estimations. As an alternative way to evaluate the statistical significance of the yearly average program effect, we use a variant of the method recently suggested by Cavallo et al. (2011). Let $N_p, p = a, r$ be the number of units affected by the program, where a denotes admitted municipalities and r denotes rejected. The average of the difference in per capita costs between each actual municipality and its synthetic

Table B.2: Fixed effects on longer samples and housing/non-housing related costs

	(1)	(2)	(3)	(4)	(5)	(6)
	<i>costs</i>	<i>net rev.</i>	<i>housing</i>	<i>non-housing</i>	<i>housing</i>	<i>non-housing</i>
<i>admitted</i> ×2000	0.0172** (0.00720)	-1.147*** (0.307)	0.0124 (0.0235)	0.00508 (0.00574)		
<i>admitted</i> ×2001	0.0245*** (0.00863)	-0.0762 (0.362)	-0.00536 (0.0249)	-0.0134* (0.00777)		
<i>admitted</i> ×2002	0.00683 (0.00876)	0.814** (0.339)	-0.0963** (0.0372)	-0.0315*** (0.00826)		
<i>admitted</i> ×2003	0.0216* (0.0123)	0.0206 (0.266)	-0.0606 (0.0468)	-0.0212*** (0.00805)		
<i>admitted</i> ×2004	0.0141 (0.0116)	0.490* (0.260)	-0.0745 (0.0704)	-0.0164* (0.00885)		
<i>admitted</i> ×2005	-0.0151 (0.0115)	1.648*** (0.286)	-0.0657 (0.0645)	-0.0213** (0.00956)		
<i>admitted</i> ×2006	0.0243* (0.0137)	-0.0675 (0.212)	-0.0572 (0.0670)	-0.0235** (0.0107)		
<i>admitted</i> ×2007	0.0180 (0.0138)	0.546** (0.241)	-0.0805 (0.0654)	-0.0205* (0.0116)		
<i>admitted</i> ×2008	0.0213 (0.0142)	0.336 (0.248)	-0.0826 (0.0671)	-0.0179 (0.0114)		
<i>admitted</i> ×2009	-0.00994 (0.0147)	1.124*** (0.268)	-0.0499 (0.0632)	-0.0217* (0.0122)		
<i>admitted</i> ×2010	-0.00145 (0.0159)	0.372 (0.259)	-0.0650 (0.0680)	-0.0215 (0.0133)		
<i>admitted</i>					-0.0397 (0.0388)	-0.0195*** (0.00688)
<i>housing program</i>					-0.0683 (0.0415)	0.00760 (0.00981)
<i>rejected</i>					-0.103** (0.0444)	0.00308 (0.00964)
Constant	1.163	-49.19***	-3.487 (5.724)	0.869 (0.774)	-6.423* (3.602)	1.354** (0.585)
Covariates	Y	Y	Y	Y	Y	Y
Observations	1,224	1,224	2,235	2,235	3,762	3,762
Nr, municipalities	68	68	172	172	290	290
F	1146.3	141.2	6.430	279.3	8.290	475.1
R ²	0.958	0.439	0.182	0.932	0.134	0.925

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

housing includes costs recorded as "infrastructure" (*Infrastruktur*) or "business activities" (*Särskilt riktade insatser*); *non-housing* includes all other costs.

control in year t is then

$$\bar{\alpha}_t = \frac{\sum_{i=1}^{N_p} y_{it} - y_{it}^{synth}}{N_p}. \quad (4)$$

Cavallo et al. ask how rare it is to encounter an average effect, computed over N_p units, amounting to the estimated program effect. They thus calculate the average effects for each possible combination of N_p -sized samples drawn from the donor pool, and check where the program effect ends up in this distribution.

We modify the method slightly because of our large donor pool. We choose to draw (with replacement) 10 000 bootstrap samples of size N_p from the donor pool for each of the eleven years during and after the program. We then compute the "p-value" of the average program effect in year $t \geq T_0$, i.e. the probability to observe such a large/small effect in the absence of program, as

$$p - value_t = \frac{\sum_{dp=1}^{10000} \mathbf{1}(\bar{\alpha}_t^{dp} < \bar{\alpha}_t)}{10000} \quad (5)$$

where $\bar{\alpha}_t$ is defined as in equation (4), $\bar{\alpha}_t^{dp}$ is the average placebo effect in bootstrap sample dp_t , and $\mathbf{1}(\cdot)$ is an indicator function taking the value 1 whenever an average from the donor pool is lower than the program average, if we are doing inference about negative point estimates (vice versa for positive estimates). The p-values can be interpreted as an estimate of whether a certain average program effect is large compared to the placebo effects and therefore also tells us if the effect is likely to be due to chance.

As would be expected given the small magnitudes, the actual-synthetic differences are rarely significant for the *rejected*. For the *admitted* municipalities, however, the bootstrap p-values suggest that the effects are unlikely to be due to chance: from 2001 and onwards, the p-values are well below 0.05.

Figure 4 (again for municipalities with pre-RMSPE < 0.05) and Table B.4 show the results when neighbours are *excluded* from the donor pool. As discussed in the main text, the estimates are not as stable over the different cut-offs as when neighbours were included in the donor pool. For the *admitted*, the average differences are now positive and significant until 2009 when looking at columns (1) and (3), where the relatively lax pre-RMSPE cutoffs are applied. For the observations with lower pre-program prediction error than 0.03 (column (5) and (7)), the estimates are positive and significant in the first years but turns towards zero already in 2004; the differences in 2005 and 2009 are even significantly negative in column (5). For the *rejected*, we see positive and significant effects until 2009 at most cut-offs, though it should be noted that more than half of the *rejected* municipalities fail to pass the lower pre-RMSPE cut-offs.

There are some discrepancies between the results reported here and the ones reported in Tables 2 and 3 when we exclude neighbours from the donor pool. This is not surprising: it becomes more important to control for unobservable, time-invariant characteristics

Table B.3: Average program effects by year ($\bar{\alpha}_t$), including neighbours in donor pool

Year	All		pre-RMSPE < 0.05		pre-RMSPE < 0.03		pre-RMSPE < 0.02	
	(1) Adm. $N_a = 35$	(2) Rej. $N_r = 22$	(3) Adm. $N_a = 34$	(4) Rej. $N_r = 21$	(5) Adm. $N_a = 28$	(6) Rej. $N_r = 16$	(7) Adm. $N_a = 23$	(8) Rej. $N_r = 9$
2000	0.001 (0.640)	0.015* (0.058)	0.003 (0.683)	0.015* (0.065)	0.001 (0.752)	0.012 (0.144)	-0.001 (0.256)	0.002 (0.547)
2001	-0.022*** (0.000)	-0.000 (0.472)	-0.021*** (0.000)	0.001 (0.515)	-0.022*** (0.000)	0.001 (0.495)	-0.025*** (0.000)	0.003 (0.381)
2002	-0.035*** (0.000)	0.002 (0.323)	-0.035*** (0.000)	-0.000 (0.567)	-0.039*** (0.000)	-0.004 (0.421)	-0.045*** (0.000)	0.007 (0.180)
2003	-0.027*** (0.000)	0.003 (0.354)	-0.026*** (0.000)	0.001 (0.499)	-0.030*** (0.000)	-0.000 (0.486)	-0.034*** (0.000)	0.019** (0.024)
2004	-0.024*** (0.004)	0.004 (0.215)	-0.023*** (0.002)	-0.001 (0.562)	-0.028*** (0.002)	0.001 (0.356)	-0.029*** (0.002)	0.019* (0.03)
2005	-0.025*** (0.002)	0.003 (0.255)	-0.023*** (0.002)	0.001 (0.384)	-0.023*** (0.001)	0.010 (0.101)	-0.030*** (0.001)	0.026*** (0.008)
2006	-0.035*** (0.000)	-0.003 (0.422)	-0.031*** (0.001)	-0.005 (0.334)	-0.032*** (0.000)	-0.001 (0.501)	-0.043*** (0.000)	0.008 (0.275)
2007	-0.034*** (0.000)	-0.007 (0.329)	-0.032*** (0.001)	-0.009 (0.268)	-0.029*** (0.000)	-0.004 (0.471)	-0.044*** (0.000)	0.005 (0.271)
2008	-0.021** (0.017)	0.001 (0.455)	-0.020** (0.024)	0.001 (0.440)	-0.017*** (0.006)	0.004 (0.305)	-0.032*** (0.006)	-0.001 (0.582)
2009	-0.029*** (0.002)	-0.003 (0.426)	-0.026*** (0.003)	-0.002 (0.442)	-0.025*** (0.000)	0.000 (0.434)	-0.039*** (0.000)	0.003 (0.341)
2010	-0.034*** (0.000)	-0.007 (0.298)	-0.033*** (0.000)	-0.008 (0.260)	-0.031*** (0.001)	-0.010 (0.255)	-0.040*** (0.001)	-0.001 (0.564)

p-values in parentheses.

N_a = number of accepted municipalities with pre-RMSPE < cut-off

N_r = number of rejected municipalities with pre-RMSPE < cut-off

Table B.4: Average program effects by year ($\bar{\alpha}_t$), excluding neighbours from donor pool

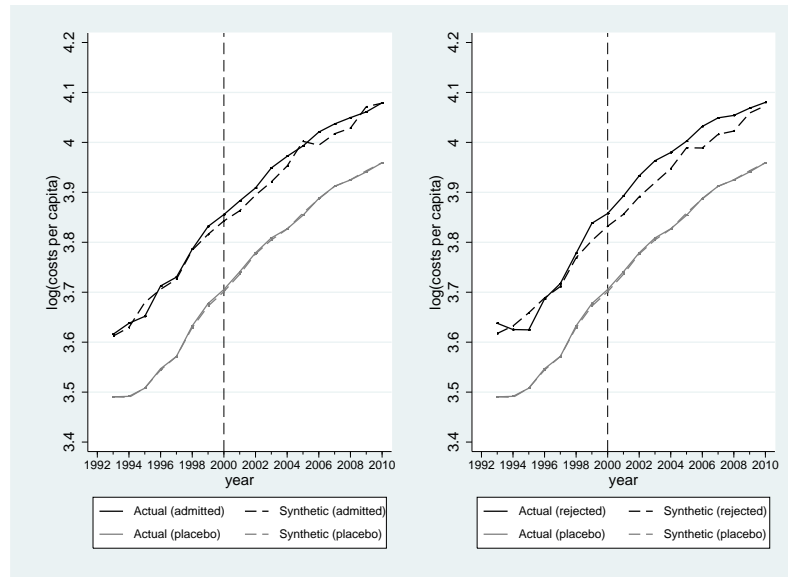
Year	All		pre-RMSPE < 0.05		pre-RMSPE < 0.03		pre-RMSPE < 0.02	
	(1) Adm. $N_a = 34$	(2) Rej. $N_r = 22$	(3) Adm. $N_a = 30$	(4) Rej. $N_r = 20$	(5) Adm. $N_a = 22$	(6) Rej. $N_r = 10$	(7) Adm. $N_a = 17$	(8) Rej. $N_r = 7$
2000	0.017*** (0.003)	0.025*** (0.000)	0.013* (0.061)	0.026*** (0.001)	0.009 (0.190)	0.007 (0.349)	0.013* (0.085)	-0.002 (0.298)
2001	0.024*** (0.000)	0.036*** (0.000)	0.020*** (0.010)	0.037*** (0.000)	0.016** (0.025)	0.014 (0.150)	0.019** (0.012)	0.007 (0.352)
2002	0.019*** (0.010)	0.044*** (0.000)	0.015* (0.056)	0.042*** (0.000)	-0.003 (0.436)	0.018* (0.060)	-0.003 (0.408)	0.000 (0.471)
2003	0.031*** (0.000)	0.045*** (0.000)	0.028*** (0.001)	0.044*** (0.000)	0.007 (0.234)	0.028** (0.014)	0.015** (0.045)	0.017* (0.100)
2004	0.021*** (0.007)	0.035*** (0.000)	0.019** (0.014)	0.032*** (0.002)	-0.001 (0.554)	0.022** (0.039)	0.010 (0.130)	0.018* (0.094)
2005	-0.010 (0.250)	0.013** (0.050)	-0.009 (0.285)	0.013** (0.044)	-0.028*** (0.004)	0.019** (0.038)	-0.015 (0.141)	0.016* (0.063)
2006	0.030*** (0.003)	0.045*** (0.001)	0.026** (0.013)	0.043*** (0.002)	0.001 (0.319)	0.036*** (0.005)	0.006 (0.222)	0.028** (0.049)
2007	0.024*** (0.008)	0.037*** (0.003)	0.020** (0.038)	0.033*** (0.009)	-0.004 (0.530)	0.016* (0.088)	0.000 (0.391)	0.015 (0.165)
2008	0.024** (0.012)	0.035*** (0.007)	0.021** (0.038)	0.031** (0.016)	-0.002 (0.604)	0.017 (0.103)	0.005 (0.262)	0.010 (0.250)
2009	-0.009 (0.329)	0.010 (0.177)	-0.011 (0.249)	0.010 (0.198)	-0.033** (0.017)	-0.006 (0.496)	-0.016 (0.208)	-0.015 (0.317)
2010	0.000 (0.433)	0.010 (0.204)	0.001 (0.467)	0.007 (0.309)	-0.021 (0.121)	-0.011 (0.383)	-0.008 (0.375)	-0.027 (0.138)

p-values in parentheses.

N_a = number of accepted municipalities with pre-RMSPE < cut-off

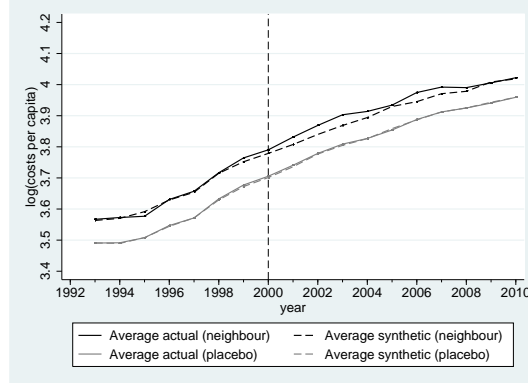
N_r = number of rejected municipalities with pre-RMSPE < cut-off

Figure 4: Actual and synthetic average per capita (log) costs of services for admitted, rejected, and placebo municipalities, pre-RMSPE < 0.05 (excl. neighbours in donor pool)



and observable time-variant characteristics when the match between actual and synthetic controls is worse. That the estimates are similar, especially for the admitted group, for the samples including neighbours are reassuring.

Figure 5: Actual and synthetic average log costs per capita, neighbours and placebo group



C Synthetic control estimates for neighbours

Here, we report results from the estimation of synthetic controls for the 33 municipalities that are neighbours to at least one municipality admitted to the program and not excluded from the donor pool for other reasons. The donor pool consists of 103 municipalities as described in Section 4.2. Apart from 1995 and 1999, pre-program fit is in general good for the neighbours (average pre-RMSPE is 0.020). However, there are some prominent exceptions for which the algorithm fails to find good controls, especially Lycksele (pre-RMSPE = 0.079), Vilhelmina (0.065) and Åmål (0.049). Notably, Lycksele contributes to the synthetic control (i.e. has a weight > 0) of 14 admitted municipalities, Vilhelmina contributes to 13, and Åmål to 4 (Lycksele’s average weight is 0.115, Vilhelmina’s is 0.337, and Åmål’s is 0.197). It is therefore unfortunate that we do not get very precise estimates of the “program effect” for these municipalities.

Figure 5 shows that the average of neighbours’ actual costs are higher than the average of synthetic costs for several of the post-program years (as well as for 1999). Only the 31 municipalities with a pre-RMSPE < 0.05 were included in the computation of the average shown in the figure. An examination of the average (over 2000-2010) difference of each individual neighbour suggests that the positive differences found on average are driven by 6 municipalities (including Åmål). 2 neighbours have instead reduced their costs relative to their synthetic controls. It is worth emphasizing that 23 of the 31 neighbours with pre-RMSPE < 0.05, i.e. an overwhelming majority, are quite close to their synthetic controls; in other words, seemingly unaffected by the program.

D Tests of equal means and equal proportions

Table D.1 shows the group means (or proportions) and two-sided tests of equal means (proportions) for a set of explanatory variables.

neighbours' weight indicate the proportion of a municipality's synthetic control that derives from neighbours. I.e. if two donors contribute to a synthetic control and one of them is a neighbour with weight 0.7, then *neighbours' weight* equals 0.7 for this synthetic control (recall that the total weight is normalized to 1). *housing program* is a dummy equal to one if the municipality was ever in the housing program, and zero otherwise. The next two variables relate the grant received from *Kommundelelegationen* and, respectively, the total grants from the bailout program (Kommundelelegationen) and (if applicable) the housing program, to the municipality's total costs of services in 2000. The variables *number of bailouts* and *share of neighbours bailouts* were presented in Section 3.2; note that they concern the period 1979-1992. Regarding the political variables, *close election in 1998* is a dummy equal to one if right-wing parties got between 45 and 55 percent of the council seats after the 1998 election and *years, left majority* counts the number of years (during 2000-2010) that the Leftist party and the Social Democrats together have had more than 50 percent of the council seats.

There are also some self-explanatory structural variables (see also Section 3.2); here, Δ -variables measure the relative change over 2000-2010. The mean (over 2000-2010) of population density (inhabitants/km²) is included because it may be more difficult to reduce costs if the population is more spread out (due to fixed costs).

We also set out to examine institutional features of the budget process and some measures of motivation for fiscal discipline, using survey data collected by the Swedish Association of Local Authorities and Regions (SALAR) in 2004 and by ourselves in 2010 (Dietrichson and Ellegård, 2012). From these surveys, we take some institutional variables that were significantly correlated with better fiscal performance in the Swedish municipalities in Dietrichson and Ellegård (2012). The third survey was conducted by Statistics Sweden in the election years 1998 and 2002. The variable *help from SALAR 2000-2010* tests for differences between the groups in their propensity to buy consultant services from SALAR that have a special unit that, against a fee, helps municipalities to improve their fiscal situation (personal communication).

The *surplus/deficit rules*-variable, measured in 1998 and 2002, indicate whether there are regulations regarding local committees' surpluses and deficits, but does not specify what type of regulation. *centralization*, which is available only for 2010, measures the presence of restrictions on the bargaining power of local committees and administrations in the budget process. *centralization* is an ordinal variable with four categories, where 1 implies most centralized and 4 implies least centralized. The dummy variables *keep*

surplus and *keep deficit*, measured in 2004 and 2010, indicate whether local committees are allowed to carry over surpluses/have to carry over deficits to the next fiscal year or not. *manager risk*, measured in 2010, is a dummy variable that equals 1 if managers of local administrations run a relatively high risk of being replaced if they repeatedly run deficits. The dummy *long term budget* indicates whether the multi-year budget is viewed as an important commitment by politicians or not. The last dummy variable, *conflicts of interests* (also this from 2010), equals 1 if a municipality reports that the executive committee and the municipal council assign higher importance to fiscal discipline than local committees.

Table D.1: Sources of heterogeneity

Variable	Reducers	Non-reducers	z/t	p-value
	(n=12) Mean/Prop.	(n=23) Mean/Prop.		
<i>neighbours' weight</i>	0.742	0.602	0.83	0.408
<i>housing program</i>	0.833	0.522	1.81	0.070
<i>grant Kommundelegationen/total costs in 2000</i>	0.055	-0.037	-1.97	0.067
<i>total program grants/total costs in 2000</i>	0.166	0.075	2.86	0.011
<i>nr of bailouts</i>	10.42	6.480	2.76	0.013
<i>share of neighbour bailouts</i>	0.565	0.465	1.90	0.068
<i>mean, share right-wing 2000-10</i>	30.40	41.70	-2.80	0.010
<i>years, left majority 2000-10</i>	8	6	1.23	0.230
<i>close election in 1998</i>	0	0.348	-2.33	0.020
<i>mean, herfindahl 2000-10</i>	0.277	0.275	0.15	0.880
<i>debt incl pensions 1998</i>	39.31	35.94	0.85	0.409
<i>fees mean 2000-10</i>	12.01	11.89	0.10	0.918
Δ <i>fees 2000-10</i>	0.104	0.369	-2.56	0.016
<i>total revenues mean 2000-10</i>	59.98	57.36	0.96	0.346
Δ <i>total revenues 2000-10</i>	0.218	0.298	-3.40	0.002
<i>tax rate mean 2000-10</i>	22.37	22.16	0.98	0.338
Δ <i>tax rate 2000-10</i>	0.012	0.023	-1.54	0.137
<i>tax base mean 2000-10</i>	138.4	134.7	1.09	0.287
Δ <i>tax base 2000-10</i>	0.350	0.356	-0.29	0.776
<i>employment rate mean 2000-10</i>	52.76	51.84	0.63	0.537
Δ <i>employment rate 2000-10</i>	0.022	0.012	0.47	0.641
<i>equalization grants mean 2000-10</i>	13.10	12.09	0.51	0.619
Δ <i>equalization grants 2000-10</i>	0.464	0.361	0.64	0.527
<i>population size mean 2000-10</i>	12047	11682	0.15	0.879
Δ <i>population size 2000-10</i>	-0.075	-0.057	-0.89	0.384
<i>mean, population density 2000-10</i>	11.76	20.66	-1.32	0.196
<i>share 0-14 mean 2000-10</i>	16.06	16.04	0.03	0.981
Δ <i>share 0-14 2000-10</i>	-0.180	-0.179	-0.03	0.974
<i>share 65+ mean 2000-10</i>	22.45	22.93	-0.38	0.705
Δ <i>share 65+ 2000-10</i>	0.114	0.146	-0.93	0.362
<i>help from SALAR 2000-10</i>	0.417	0.478	-0.24	0.810
<i>centralization</i>	3	2.94	0.17	0.863
<i>keep surplus 2004</i>	0.181	0.227	-0.30	0.763
<i>keep surplus 2010</i>	0.300	0.333	-0.18	0.856
<i>keep deficit 2004</i>	0.091	0	1.40	0.160
<i>keep deficit 2010</i>	0.200	0.111	0.64	0.520
<i>surplus/deficit rules 1998</i>	0.500	0.522	-0.12	0.903
<i>surplus/deficit rules 2002</i>	0.333	0.500	-0.94	0.350
<i>manager risk</i>	0.667	0.800	-0.73	0.465
<i>long-term budget 2004</i>	0.272	0.363	-0.522	0.601
<i>long-term budget 2010</i>	0.200	0.389	-1.03	0.305
<i>conflicts of interest</i>	0.800	0.611	1.03	0.305