Political conflicts over public policy in local governments

Ellegård, Lina Maria

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Lina Maria Ellegård

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To Hans and Alf

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When I entered the PhD program, my best guess about the likely program effect was that I would eventually print a thesis with a dull white-and-blue cover. Other aspects, such as the topic of the thesis, the amount of economics I would learn and the lot of nice people I would get to know were still behind a veil of ignorance. The thesis is now written and my time as a PhD student is coming to an end; what is left is to acknowledge the people that have contributed to the individual-specific program effect.

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Contents

Chapter 1

Introduction

The characterization of Sweden as a welfare *state* conceals that the authority over most public consumption is delegated to local governments. The 21 county councils are responsible for the public health care system, while the 290 municipalities have authority over areas such as pre- to upper secondary schooling, elderly care and social assistance. Though national regulation of service provision and tax collection places restrictions on the local governments' authority,[1] the national regulation leaves plenty of room for regional variation and the local governments' authority over unregulated dimensions is constitutionally protected.[2]

This thesis attempts to explain some of the observed municipal variation in economic outcomes such as operating costs and surpluses. The starting-point is that outcomes partly reflect voters' preferences, and partly reflect the incentives of agents that are supposed to articulate and implement voters' preferred policies – that is, local politicians and bureaucrats. The thesis shares this starting-point with the field of political economics. In the introduction of their seminal book, Persson and Tabellini describe this field as a synthesis between political science and economics:

> As in political science (⋯) we want to understand (⋯) what shapes the incentives and constraints of the policymakers taking those decisions, and how conflicts over policy are resolved. But as in economics, we are ultimately interested in the outcomes of policy decisions. (pp. 1f)

[1] For instance, *Skollag 2010:800* regulates education, *Socialtjänstlag* (2001:453) regulates social assistance and elderly care, and *Hälso- och sjukvårdslag* (1982:763) regulates health care. *Kommunalskattelag* (1928:370) restricts the local tax authority to setting the rate of a proportional income tax, though direct user fees is another source of revenue.

[2] Regeringsformen (1974:152)

The quote summarizes the common thread running through the four chapters of this thesis. Chapter 2 concerns how policymakers' incentives and constraints, and thereby policy outcomes, are affected by the composition of voter groups in society. Specifically, the chapter investigates how local politicians adjust the generosity of elderly care as the share of elderly voters increases. The following three chapters instead focus on conflicts of interests between different levels of the governmental hierarchy. Chapter 3 relates to a specific conflict of interests between local governments on the one hand and the national government on the other. If local governments view the national government as ultimately responsible for their solvency, local governments have weaker incentives to economize with their resources than the central government would prefer. The chapter examines the development of costs and operating surpluses in municipalities that actually received extraordinary financial assistance from the national government in the beginning of the 21^{th} century. Chapters 4-5 seek to identify organizational features that resolve, or dampen the consequences of, conflicts of interests *within* a local government; that is, conflicts between politicians at the center of the municipal hierarchy and politicians and bureaucrats at the lower hierarchical level. The remaining part of this introductory chapter gives a more comprehensive background for and summarizes the contributions of each of the four thesis chapters.

1.1 Population aging and public systems for old-age support

Mental and physical disabilities are associated with difficulties to raise income and to perform activities of daily life such as shopping, cooking, getting dressed etc. As the risk for disability increases with age, the elderly in a society are relatively likely to depend on others' support. Historically, such support has mainly been provided by the family. However, during the 20^{th} century, most developed countries shifted part of the responsibility to the public sector. Public pension systems ensure the elderly a basic income regardless of their current labor supply (even regardless of their disability level), and long-term care of disabled elderly is often subsidized or even publicly provided (OECD, 2011, 2005). Clearly, the elderly are also among the prime beneficiaries of publicly financed health care (e.g. Payne et al.,

2007).

Declining fertility and mortality rates imply a change in the demographic structure of the very same countries that were first to expand public welfare systems. In Sweden, the share of inhabitants above 65 years of age was 12 percent in 1960, 18 percent in 2010, and is projected to rise to 24 percent in 2040 (Statistics Sweden, 2013). If the present public systems for old-age support are left untouched, a demographic development with fewer contributors and more benefit claimants implies that a larger share of society's resources will be allocated to the elderly in the future. The effect of population aging on public systems of old-age support is however more than a matter of projections of current benefit levels. The reason is that population aging also changes the composition of the electorate. Clearly, politicians may find it optimal to adjust the parameters of old-age support systems to better reflect the preferences of an electorate with a different age profile than the current.

Most theoretical models of population aging and public old-age support are framed within the context of pay-as-you-go pension systems. To gain or stay in power, politicians adjust the parameters of the pension system in accordance with the preferences of the median voter; thereby, they ensure that the system is acceptable for a majority of the electorate. Population aging has two opposite effects on the preferred policy of the median voter, who will typically be of middle-age (that is, a contributor to the system). On one hand, the increasing population share of beneficiaries means that the rate of return from contributions decreases. This economic effect tends to reduce the attractiveness of the pension system and thus reduces the median voter's support for the system. On the other hand, population aging shifts the identity of the median voter to someone of higher age, who has fewer years as a contributor left and therefore would prefer a higher benefit (e.g. Galasso and Profeta, 2007, 2004; Browning, 1975).

The intuition from these models applies not only to pension systems, but carries over to other systems of transfers from the young and middle-aged to the elderly – given that voters perceive a linkage between today's contributions and tomorrow's benefits. Empirically, the other systems of old-age support – public health care and elderly care systems – rarely have explicit linkages between contributions and benefits. As recognized by e.g. Strömberg (2006), altruism towards the elderly is one way to save the median voter model in the absence of a benefit-contribution link. An alter-

native modelling approach is to assume that politicians assign political importance to several different voter groups, rather than only to voters in the middle of the preference distribution. One such framework is offered by the probabilistic voting model, in which competing political parties assign extra weight to voters that have weak ideological preferences. The votes of these "swing voters" are thus easily won, but also easily lost (Lindbeck and Weibull, 1987; Dixit and Londregan, 1996). Rattsø and Sørensen (2010) discuss the relation between population aging and publicly financed long-term care in such a setting. Though the model allows for altruism towards elderly parents (and encompasses Strömberg's results), altruism is not necessary to rationalize the existence of public long-term care in the model. The effect of population aging on the generosity of long-term care – that is, spending per elderly – depends on whether the elderly are sufficiently politically important to overturn the negative effect that arises because the fixed total budget has to be divided on more individuals when the share of elderly increases.

Gerdtham et al. (2005) use a similar model (though without altruism) to analyse the relationship between public health care policy and population aging. In contrast to Rattsø and Sørensen, Gerdtham et al. allow the size of the total budget to be affected by aging. The model suggests a positive relationship between the population's share of elderly and the size of the health care budget. Whether the increase of the overall budget is sufficient to increase the level of health care spending *per elderly* however depends on the political importance of the elderly voters.

The predicted effect of population aging on old-age support thus depends on the political importance of the elderly, a matter on which it is difficult to have a prior. This means that empirical studies are necessary to sign the effect. Studies on OECD countries show that aging is associated with an increasing share of *pension* spending to GDP. The generosity of pension benefits (i.e., pension per retiree) does not show a strong correlation with the demographic structure (Breyer and Craig, 1997; Bryant, 2003), though Tepe and Vanhuysse (2009) find indications that there has been a negative association between the share of elderly and benefit generosity since the 1990's. Cross-country studies of *health care* spending do not generally find an association between the share of elderly and total spending (Bech et al., 2011; Zweifel et al., 2005). Likewise, the empirical investigation of Swedish county councils in Gerdtham et al. (2005) yields little support for

their hypothesized relation between aging and total health care spending (as measured by the county council tax rate). However, the authords do find a negative relation between the share of elderly and health care consumption (measured as the number of bed days) *per elderly*. In contrast to the results for health care, Karlsson and Klohn (2011) find positive effects of the share of elderly on total *long-term care* spending in Sweden. Studies on Norway and Denmark however find a negative effect on long-term care *generosity*, measured as spending per elderly (Rattsø and Sørensen, 2010; Borge and Rattsø, 2008).

Chapter 2 complements and contributes to the analysis of population aging and long-term care with more evidence from the Swedish local governments. Like Rattsø and Sørensen (2010) and Borge and Rattsø (2008), I study spending per elderly rather than total spending. I extend the analysis by taking into account that the elderlys' demand for long-term care may change simultaneously with their population share. The justification for this is that if population aging is due to an expansion of the time spent in bad health – i.e., a lowering of the lowest attainable health level (Olshansky et al., 1991) – the elderly as a group will demand more long-term care, which makes long-term care an increasingly important policy tool.[3] I control for municipality-level mortality to examine the importance of such demand effects, but find that the estimated importance of aging is not very sensitive to the inclusion of this health proxy.

My results conform with previous findings from other Scandinavian countries: spending per elderly declines when the share of elderly increases. The decline is implemented through a reduction in the proportion of care users, which indicates that the decline in spending per elderly is not merely a signal of economies of scale in long-term care production. Though aging has no statistically significant effect on spending per actual care user, the restricted access to long-term care likely applies to relatively healthy applicants. It thus seems reasonable to conclude that users at a given level of disability receive less help as a consequence of aging. To summarize the findings from Sweden, local politicians seem to direct additional resources to the increasing group of potential long-term care users, but not enough to retain the previous level of generosity. In other words, the burden of aging is spread across all inhabitants.

[3]Conversely, long-term care may become less politically important if aging is coupled by a postponement or compression of morbidity (Manton, 1982; Fries, 1980).

1.2 Conflicts of interests between central and local governments: implications for fiscal discipline

It is generally viewed as efficiency-enhancing to delegate authority to local governments, as delegation facilitates the adjustment of policy to local preferences and conditions (Oates, 1972). The national governments of decentralized countries however rarely leave local governments full discretion over neither revenue collection nor service provision. Most national governments transfer part of nationally collected tax revenues to local governments as targeted grants, and some federal countries redistribute locally collected taxes between regions (Boadway and Shah, 2007). Such interventions are intrinsically linked to the central government's sensitivity to the local governments' policy choices. If the central government is unconcerned about the local governments' choices, it has no reason to provide inter-governmental grants or to place national regulation on locally provided services. By contrast, the national government may have reason to intervene if the local governments' policy choices threaten the attainment of national policy goals (Inman, 2003; Goodspeed, 2002).

Local governments may take advantage of the national government's sensitivity to local decisions. If a local government runs into financing problems, it can threaten to e.g. declare bankruptcy or to lay off staff, and then hope that these actions would have sufficiently adverse consequences for the national government to make it prefer to bail the local government out rather than to watch the threat being effectuated. Though part of the bailout would be financed by the inhabitants of the troubled local governments itself – they too pay national taxes – part of the bailout would be financed by tax-payers in other regions; that is, a tax base which would have been inaccessible for the local government if the central government was insensitive to local decisions. The central government's sensitivity thus softens the local government's budget constraint: it can run a less fiscally disciplined policy than if it did not implicitly have access to the national tax base (Inman, 2003; Goodspeed, 2002; Rodden and Eskeland, 2003a).[4]

The soft budget constraint problem is especially pertinent for countries

[4]See Kornai et al. (2003) and Kornai (1979) for a more general treatment of the problem of soft budget constraints.

where the local governments have little tax autonomy, as they may then rightly view the central government as utterly responsible for the provision of sufficient funds (Rodden and Eskeland, 2003b). From this perspective, the far-reaching tax autonomy of Swedish local governments suggests that the soft budget constraint problem may be less concerning in Sweden. On the other hand, locally provided public services are important channels to achieve an equitable standard of living across the country, which is an important policy goal for the Swedish national government. As noted above, national sensitivity to local policy suggests that the municipal budget constraint is soft (Dahlberg and von Hagen, 2004).

Empirically, the Swedish national government has revealed softness of several occasions. In the 1970's and 80's, the local governments could apply for grants to cover deficits annually. The availability of these grants appear to have weakened the fiscal discipline of certain municipalities: specifically, the municipalities that expected to receive deficit grants accumulated more debt than other municipalities (Pettersson-Lidbom, 2010). After a reform of the intergovernmental grant system in 1993, the opportunity to seek deficit grants was abolished. The national government however again demonstrated its softness in the late 1990's, this time in the form of a temporary grant program for municipalities that for structural reasons had problems to achieve budgetary balance. Unlike the earlier deficit grants, the grant promised within the new program was conditional: the full grant would not be paid out until the municipalities had reduced costs according to a pre-specified plan and managed to achieve budgetary balance. How did the 36 affected municipalities perceive the conditional grant program – did they primarily interpret the grant as a signal of softness, or did they infer from the conditions that bailouts are not pain-free, and therefore something one might want to avoid? In chapter 3, which is co-authored with Jens Dietrichson, we examine the development of operating costs and surpluses during the decade following the launch of the program. We find that operating costs per capita have been largely unaffected by the program in most of the 36 municipalities. However, a non-negligible number of municipalities have held back costs more than expected, and operating surpluses have been higher than expected for the group as a whole. We conclude that participation in the conditional grant program did not undermine local fiscal discipline and propose that the conditions even may have served to harden the budget constraint.

1.3 Conflicts of interests in the municipal hierarchy: causes and remedies

When consumption decisions are made by agents that enjoy all the benefits from consumption but only contribute to part of the costs, the agents have incentives to increase consumption above the level that would be chosen if the they had to bear the full costs. The opportunity of a sub-group of the population to shift part of their consumption costs to the rest of the population is at the core of the soft budget constraint problem discussed in the previous section. In legislatures whose representatives are elected in different geographical districts, the representative of each of the n districts may rationally internalize only the part of the costs for financing of public projects that falls on the inhabitants of its own district; Weingast et al. (1981) provide a seminal treatment of this so called *fiscal common pool problem*. von Hagen and Harden (1995) note that another instance of the fiscal common pool problem may arise within a cabinet if, as is commonly the case, different policy portfolios are delegated to different ministries. If each ministry is unconcerned about the tax burden that falls on people outside its own policy domain, it will fail to internalize the full costs of increasing its budget appropriation and will thus advocate an inefficiently high spending level.

von Hagen and Harden (e.g. 1995), and later Hallerberg and von Hagen (1999) and Hallerberg et al. (2007), argue that in order to overcome the fiscal common pool problem, the budget process – that is, the drafting, approval and implementation of the budget – must be governed by a proper institutional structure. Either strategic powers must be centralized to an agent that internalizes costs fully – typically the finance minister – or the process must be bounded by pre-specified spending targets negotiated among the spending ministries. At the implementation stage, balanced-budget rules have been proposed as a way to avoid budget deficits (e.g. Bohn and Inman, 1996). There are by now plenty of empirical studies consistent with virtuous effects on fiscal performance of centralization and fiscal rules (see Eslava, 2011, for a review).

The last two thesis chapters consider a municipal version of the fiscal common pool problem pictured by von Hagen and Harden. The municipalities are typically hierarchically organized; the center of hierarchy comprises

the directly elected municipal council and the executive committee, while the lower hierarchical level comprises several local committees responsible for different policy areas.[5] Drawing an analogy to von Hagen and Harden's paper, the local committees may be thought of as ministries for different portfolios, and the council and executive committee as the unbiased finance minister. If the local committees fail to internalize the full costs of providing benefits to their clients, they will demand a higher aggregate level of spending than the center, who thus needs enforcement mechanisms to make the local committees comply to the budget.

In chapter 4, which is co-authored with Jens Dietrichson, we set up a theoretical model of the budget process and empirically examine a set of institutions that may help the central level to enforce the budget. We collect survey data on budget institutions in 265 of the 290 Swedish municipalities and examine how these institutions associate with fiscal performance, measured by the fiscal surplus (revenues net of costs). In contrast to previous studies on budget institutions, we acknowledge that the severity of the common pool problem may vary between contexts. If the local committees are not particularly biased, the budget institutions have no role to play. Our survey therefore also contains a measure of the prevalence of conflicts of interests between the central and local level regarding the importance of fiscal discipline. As expected, the estimated correlations between institutions and performance depend on the reported strength of conflicts between the two levels of hierarchy. A centralized budget process, a credible threat of replacement of local-level managers following systematic deficits, and a rule allowing local committees to carry over surpluses to the following fiscal year are all associated with higher surpluses, but only in municipalities that report that there is a substantial conflict of interests between the central and local level with regards to the importance of fiscal discipline. In municipalities where the conflict of interests is reported to be small, a deficit carry-over rule is positively correlated to net revenues. As municipalities with high surpluses and carry-over rules in practice almost always have centralized budget processes, it remains to be explored whether the carry-over rules are important also in the absence of a centralized budget process.

The variation in prevalence of conflicts of interests found in the survey data is worth emphasizing. Though it is plausible that local committee politicians would be biased towards their own policy area due to self-

[5] A handful of municipalities also employ a geographical division of committees.

selection (c.f. Weingast and Marshall, 1988), it is likewise plausible that the center would seek to counteract self-selection by appointing relatively unbiased politicians for the local committes (c.f. Gilligan and Krehbiel, 1990; Krehbiel, 1990). Obviously, the observed variation in conflicts of interests mean that neither of these two arguments give a complete description of the preference structure in Swedish local committees. The fact that there is variation suggests that there may be ways to resolve conflicts of interests and thereby to overcome the fiscal common pool problem. In chapter 5, I explore some potential determinants of the reported conflicts of interests. The main findings are that *(i)*, conflicts between the two hierarchical levels are less likely if members of the executive committee are appointed as chair persons for the local committees; *(ii)*, conflicts are less common in municipalities with fewer committees (for a given population size); and *(iii)*, conflicts are more likely in municipalities with recent experience of deteriorations of the general economic conditions. The first two findings suggest that the center may resolve the conflict of interests, and thereby mitigate the common pool problem, as it has discretion over appointments and the number of committees. However, as the general economic environment is beyond the control of the center, it is difficult to fully prevent conflicts between the central and local level.

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Chapter 2

Making Gerontocracy Work: Population Aging and the generosity of Public Long-Term Care

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2.1 Introduction

The ongoing aging of populations presents a challenge for Western countries, where spending on the elderly constitutes a significant share of public expenditures. Should the tax rate and benefit structure remain unchanged, the decreasing tax base and the increasing share of beneficiaries implied by aging obviously put pressure on public finances. However, the size and composition of the welfare state are determined in a policy process, which might respond to demographic changes. The direction of the policy response is theoretically ambiguous: on one hand, shrinking tax bases due to a larger share of retirees may lead policy makers to cut back on public spending. On the other hand, the elderly's increasing importance as a voter group may lead policy makers to increase spending on this group. In favor of this latter possibility, Sanz and Velazquez (2007) find that aging is a prominent driver of aggregate spending on health and social protection in member countries of the Organisation for Economic Co-operation and Development (OECD). Importantly, the authors also find that the increase in spending is partly financed by cuts in spending on activities that do not directly benefit the elderly, such as education. Such crowding-out of educational spending due to aging has also been found in a number of single-country studies.[1]

[1] Poterba (1997); Harris et al. (2001); Ladd and Murray (2001) consider U.S. states, Borge and Rattsø (1995); Strömberg (2006); Borge and Rattsø (2008); Rattsø and Sørensen

A related issue is the influence of aging on the generosity of public elderly benefits, that is, the probability of becoming eligible for benefits and the quantity of benefits granted. If spending per elderly increases in times of aging, due to increasing political importance of the elderly, costs increase more than proportionally. Conversely, if the elderly crowd themselves out, aggregate spending on the elderly does not increase as much as predicted by straightforward projections. From the point of view of the individual elderly, the generosity of benefits is arguably more of interest than the aggregate level of spending. However, the empirical evidence so far indicates that there are limitations to the importance of the elderly as a voter group. Regarding old-age pensions in OECD countries, Breyer and Craig (1997) and Sanz and Velazquez (2007) find no effect of aging on the per capita benefit, while Tepe and Vanhuysse (2009) find that the generosity of per capita pensions even decreases as response to aging. With regards to hospital care, Gerdtham et al. (2005) find a negative correlation between the number of bed days per those aged 75+ and the share of those aged 75+ in the Swedish population.

The Nordic systems of public long-term care (LTC) provide an ideal field for further exploration, as LTC provision is decentralized to the multiplicity of local governments, that is, the municipalities.[2] Specifically, while national laws regulate the lower age limit for eligibility to public long-term care (e.g. 65 in Sweden, 67 in Norway) and the obligation to deliver care to those in need, each municipality decides upon the exact criteria for eligibility, as well as service levels. Thus, the municipal context provides the heterogeneity in policy needed to further investigate the issue of "grey power".

While recent research suggests that aging is positively related to *aggregate* LTC spending (Karlsson and Klohn, 2011, Sweden), Rattsø and Sørensen (2010, Norway) and Borge and Rattsø (2008, Denmark) confirm that the elderly's political influence does not extend to the *generosity* of benefits; according to the latter two studies, LTC costs per inhabitant over 80 years of age decrease when the population share of 80+ increases. The present paper adds to the literature on generosity using panel data on LTC spending and demographic characteristics of the 290 Swedish municipalities during the period 1999-2007.[3] As opposed to previous studies on LTC gen-

(2010) study Scandinavian municipalities and Grob and Wolter (2007) study Swiss cantons.

[2]The two terms are used interchangeably in this paper.

[3]The data is collected from Kommun- och landstingsdatabasen (www.kolada.se) and Statistics Sweden (http://www.ssd.scb.se/databaser/makro/start.asp).

erosity, this study takes into account that a negative correlation between LTC generosity and the population share of elderly may be due to falling demand for LTC: to the extent that population aging is due to better health among the elderly, individual demand for LTC may decrease simultaneously with population aging. The study moreover acknowledges that retirees below 80 years of age also are potential LTC clients and therefore to some extent influence LTC policy.[4] This is relevant because the younger old have a relatively stronger political voice than the 80+; in the last three Swedish elections, 85 percent of those aged 65-79 voted, but less than 70 percent of the 80+ voted. An older study by Borge and Rattsø (1995) also uses spending per potential client as outcome measure. While these authors find no evidence that aging increases LTC generosity, short-run policy inertia may possibly have biased the estimations towards decreasing generosity as the dataset only covered four years. The present study covers a longer and more recent period. As a final contribution, the analysis explores the two channels through which aging may affect LTC policy: through the probability of becoming eligible, or through the generosity per actual client, or both.

The next two sections present institutional facts about Swedish longterm care and elaborates on the relation between population aging and LTC policy with reference to the empirical model. Thereafter, the data, estimation strategy, and results are presented. The final section discusses the interpretation of the results and considers them in a wider context.

2.2 Institutional background

The Swedish Social Service Act (*Socialtjänstlag* 2001:453) stipulates that the country's 290 municipalities, the most decentralized level of government, should guarantee their citizens a reasonable standard of living. Inhabitants above the age of 65 who are unable to perform normal activities of daily life (for example, getting in and out of bed, dressing, cooking, eating, cleaning) are entitled to assistance from the municipality they reside in. Municipalities should actively investigate the needs of elderly inhabitants, but the decision to seek LTC can be initiated by the individual elderly. Although

[4] Rattsø and Sørensen (2010) do control for the younger old in their estimations. But this variable will be positively related to the ratio of aggregate LTC costs to the population of 80+ simply because it affects the nominator but not the denominator.

their general responsibility is stated by law, the municipalities have considerable discretion concerning eligibility criteria, service levels and the exact mix of services (home care, transport service, institutional care) to provide. Municipalities are also free to choose between public or private provision, although publicly provided care remains the main option in most municipalities. It is noteworthy that the municipalities are not responsible for health care provision (nursing health care for elderly in LTC institutions excepted). Primary care, acute care and hospital care is the responsibility of another governmental layer: the 21 county councils.[5]

LTC provision is mostly financed by the municipalities, whose main sources of revenue are local income taxes and open-ended grants from the two national equalization systems. Each municipality decides upon its own income tax rate. The equalization grant is calculated based on the municipality's socio-demographic composition (spending needs equalization system) and its tax base size (tax equalization system).[6] Apart from these general sources of revenues, the municipalities can also charge LTC clients a user fee. On the aggregate, this is a subordinated source of revenue; in 2007, fees covered only about 4 percent of LTC costs (SALAR, 2009). This does not mean that fees are non-negligible from the point of view of individual clients, however. To prevent individuals from having to pay high fees, there is a nationally set cap on LTC fees since 2002.

2.3 Population aging and LTC generosity

The municipalities set their tax rates and the proportion of revenues directed to LTC annually; there is no pre-defined link between contributions paid and benefits received. The lack of long-run commitments characterizing the LTC system differs from the design of the national pay-as-you-go pension system (Rattsø and Sørensen, 2010): in comparison, the LTC system is static in nature.[7] It is therefore useful to consider the annual budget constraint,

[5] The municipality of Gotland is an exception for which the two levels coincide.

[6] The redistributive nature of the two equalization systems implies that some municipalities are net payers, although this is only the case for a small share of municipalities (for instance, 6 municipalities were net payers in 2007). In effect, the central government takes on most of the financing responsibility for the tax equalization and it is thus mostly a system of grants (Statistics Sweden, 2006).

[7] A particular implication of the absence of a benefit-contribution link is that aging does not affect the working population's demand for LTC benefits. Compare to the case of

which makes explicit the options available to cope with demographic change:

$$N_{old} \times \frac{LTC}{N_{old}} + \text{other spending} = T + G \tag{2.1}$$

The left-hand side of the budget constraint represents expenditures on LTC and other services, and the right-hand side represents tax revenues (T) and grant income from the two redistribution systems (G).[8]

As noted in the introduction, the precise definition of "elderly" – that is, the population above the nationally set minimum age for LTC eligibility – is exogenous to the local government. The empirical question addressed in this paper is what happens to the endogenous policy variable $\frac{LTC}{N_{old}}$ when the group of potential LTC clients grows relatively to other the rest of the population, that is when N_{old}/N increases. Local politicians can be expected to accommodate LTC generosity to population aging for two reasons. First, there is the direct effect that the elderly become a more important voter group. Politicians will thus become more responsive to the wishes of the elderly population.[9] *Second*, there is possibly an indirect effect through the LTC demand of the elderly; if population aging is driven by health improvements among the elderly (Fries, 1980; Manton, 1982), the average elderly individual will demand less LTC. A generous LTC policy will in that case become relatively less effective as a way to attract votes. Conversely, if aging results from an expansion of morbidity (Olshansky et al., 1991), the average elderly individual will demand more LTC and LTC generosity thus becomes a more important tool for vote-maximizing politicians.

For a moment, disregard the indirect effect and assume that the relative size of the group of elderly is unrelated to the individual demand for LTC. Assuming that the political importance of the elderly is an increasing function of their population share and that all elderly place positive

PAYG systems, in which increasing life expectancy translates into lower rates-of-return for the young population (Galasso and Profeta, 2007; Sanz and Velazquez, 2007; Persson and Tabellini, 2000).

[8] The possibility to finance current consumption by loans is disregarded here, as municipalities are not allowed to borrow for consumption.

[9] This claim assumes that population shares are key determinants of political influence, as in the voter group decision model of Craig and Inman (1986) (see also Borge and Rattsø, 1995). However, it is not certain that the population share is a key determinant of political influence. For instance, the median voter theorem would suggest that the population share of elderly is unimportant in the static LTC setting, as the median voter will not be elderly and thus places little or no value on LTC. But with such a model, it is hard to explain why there is LTC in the first place.

value on LTC (because they all run a positive risk, possibly increasing in age, of becoming disabled), the level of LTC per potential client ($\frac{LTC}{N_{old}}$) becomes a more important policy variable when the share of elderly increases. Whether the level of generosity actually is retained in times of aging depends on how politicians weigh the importance of the elderly relative to that of other voters. Politicians that care for all citizens will balance the demographic burden between the different population groups, to keep other spending and T at reasonable levels. Thus, LTC generosity may possibly decrease as response to population aging *despite* that the political importance of the elderly is rising. To relate this reasoning to the empirical model, the model tests whether the importance of elderly, and their interest in LTC, is sufficiently large to make the net effect of aging on LTC generosity positive.

Let us next relax the assumption that individual LTC demand is unrelated to the population's share of elderly. The aging of Western populations is partly driven by increasing life expectancy, which suggests that the policy preferences of the elderly change as their population share increases. For instance, if population aging is accompanied by a postponement of morbidity, the average elderly individual's demand for LTC can be expected to fall: due to better health, she will be less likely to demand LTC in the first place, more willing to substitute home care for more expensive institutional care, and more concerned about other features of the local policy (e.g. the tax burden). In other words, the indirect effect of aging, which works through the effect on demand, is in this case a second reason why LTC generosity may decrease in times of aging despite the increased political importance of the elderly. Conversely, if aging is accompanied by an expansion of morbidity, the average individual's LTC demand will increase. In this case, both the increasing importance of the elderly and their increasing demand work towards greater LTC generosity.

Whatever the sign of the indirect effect, the total effect of population aging on LTC generosity is the sum of this effect and the direct effect of the change in the age composition of the population: policy makers accommodate to the simultaneous changes in the importance of the elderly as voters and in this group's view of optimal policy. But population aging does not automatically imply that the health, and thereby LTC demand, of the elderly population changes. For instance, aging can be driven by fertility decline or by emigration of younger individuals. Moreover, although population aging so far seems to be driven by postponement of morbidity

(Payne et al., 2007), the future development of morbidity is an open question. It is therefore interesting to study the effect of aging net of its effect on individual demand. To accomplish this in an empirical model, individual demand for LTC must be held constant by controlling for elderly health in the regressions. This is an issue we return to in the Data section below.

A second aim of the paper is to investigate the specific ways through which population aging affects LTC generosity. Therefore, let us next decompose LTC generosity as follows:

$$\frac{LTC}{N_{old}} = \frac{N_{client}}{N_{old}} \times \frac{LTC}{client} = Pr(LTC) \times \frac{LTC}{client} \tag{2.2}$$

In words, LTC generosity is composed of the probability of becoming a LTC client ($Pr(LTC) = \frac{N_{client}}{N_{old}}$) and the generosity per actual client ($\frac{LTC}{N_{client}}$). The aging effect on LTC policy can be channeled through the probability of becoming a client, the generosity per client, or some combination thereof. Politicians may, for instance, react to aging by extending LTC services to relatively less disabled retirees (e.g. increasing the supply of cleaning and transport services), in an attempt to increase the number of votes. Then, aging implies an increase in $Pr(LTC)$ but a decrease in $\frac{LTC}{client}$. Alternatively, politicians may wish to concentrate resources on the severe cases, retaining $\frac{LTC}{client}$ but reducing $Pr(LTC)$. Theoretically however, there is no reason for a trade-off between the two variables; population aging could in principle have a positive (negative) effect on both.

2.4 Data and estimation strategy

2.4.1 Data

The data consists of official statistics for 1999-2007. The panel is unbalanced, due to i) the creation of a new municipality in 2003 (yielding 290 municipalities in total) and ii) missing values regarding LTC in some municipalities. Since the data is collected for administrative purposes, sample selection is however likely not a problem.[10]

[10]There can be differences between municipalities regarding whether expenditures are accounted for as assistance to the disabled or as LTC. These differences are likely constant over the study period and thus captured by the municipality effect in the estimations. In a few cases, the recorded variable values are obviously incorrect: in

The LTC data covers long-term care provided either as home care or as institutional care. Table 2.1 shows some summary statistics. The mean level of spending per potential client ($LTC/65+$) is 53 000 SEK (about 7 600 USD), but due to their discretion over LTC policy, the variation between municipalities is quite large, ranging from 20 000 to 90 000 SEK. Nine out of ten municipalities increased their level of spending between 1999 and 2007, but the mean difference is only about 7 000 SEK. Thus, in comparison to the overall panel variation, the time-series variation between 1999 and 2007 is relatively low. Counted per *actual* client, the mean level of spending in the panel is 350 000 SEK, with a variation ranging from 100 000 to about 600 000 SEK. On average, 15 percent of the 65+ population use public LTC services, but the range is very wide, with a minimum of only 8 percent and a maximum of 37 percent.

The between-municipality variation in the share of actual clients is well matched by the range of the share of potential clients: the population share of 65+ is on average 19 percent in the panel, the minimum is 8 percent and the maximum 30 percent. Population aging shows up in the next row of the table: the mean change in the share of 65+ between 1999 and 2007 is a 1 percentage point increase. However, not all municipalities experienced aging during this period: the share of 65+ decreased in one out of five municipalities. Notably, this was the case in the three metropolitan municipalities. This fact gives a hint about the sources of the time-series variation in the data: besides reflecting the effects of increasing life expectancy and decreasing fertility, it reflects the migration patterns of the young and middle-aged, for whom the local labour market is of concern.

To hold individual LTC demand constant, the mortality rate among the 65 + group is controlled for. Although not a perfect measure of population health, mortality can be sufficiently encompassing given that the panel only covers 9 years; within a decade, health should not have changed dramatically over and above the changes captured by the decline in mortality (see Table 2.1). The national Survey of Living Conditions conducted annually by Statistics Sweden indeed suggests the health of the 65+ did not change drastically between 1999 and 2006: for instance, 50 percent reported re-

Skövde, the share of long-term care clients drops from 14% in 1998 to 3% in 1999, and then returns up to 15%. In Hällefors, the reported *LTC/65+* drops to 20 000 in 2007 from about 50-56 000 in preceeding years. The estimation results reported below are robust to the exclusion of these outliers.

Table 2.1: Descriptive statistics

Variable[a]	**Observations**[b]	**Mean**	**SD**	**Min**	**Max**
LTC/65+[c]					
overall	N = 2 469	52 676	8 135	20 032	89 626
diff 2007-1999	n = 232	7 049	6 362	-27 722	27 607
within	$\overline{T}_i = 8.5$		3 507	22 459	66 757
LTC/client[c]					
overall	N = 2 467	350 360	57 914	101 148	612 869
diff 2007-1999	n = 230	2 945	68 411	-213 193	255 234
within	$\overline{T}_i = 8.5$		40 459	114 469	536 745
LTCshare					
overall	N = 2 467	0.153	0.027	0.080	0.370
diff 2007-1999	n = 281	0.021	0.033	-0.190	0.170
within	$\overline{T}_i = 8.5$		0.017	0.101	0.361
sh65+					
overall	N = 2 469	0.192	0.037	0.081	0.302
diff 2007-1999	n = 287	0.010	0.012	-0.026	0.045
within	$\overline{T}_i = 8.5$		0.005	0.168	0.215
mortality65+					
overall	N = 2 469	0.0516	0.00663	0.0315	0.0887
diff 2007-1999	n = 286	-0.0023	0.00665	-0.0278	0.0196
within	$\overline{T}_i = 8.5$		0.00458	0.0350	0.0801
sh0-19					
overall	N = 2 469	0.245	0.022	0.169	0.314
within	$\overline{T}_i = 8.5$		0.005	0.224	0.264
shImm					
overall	N = 2 469	0.108	0.064	0.023	0.489
within	$\overline{T}_i = 8.5$		0.009	0.063	0.171
G[c]					
overall	N = 2 469	8 342	4 955	-16 404	24 702
within	$\overline{T}_i = 8.5$		1 217	3 003	14 987
TInc[c]					
overall	N = 2 469	129 179	19 811	86 598	268 924
within	$\overline{T}_i = 8.5$		11 362	88 278	152 021
femlab					
overall	N = 2 469	0.702	0.037	0.522	0.819
within	$\overline{T}_i = 8.5$		0.013	0.626	0.786

[a] Overall *refers to the whole panel* (x_{it}), diff *refers to the difference between 2007 and 1999*, within *refers to deviations from the municipality mean over time* ($x_{it} - \overline{x}_{i.}$).
[b] $\overline{T}_i$ = Mean number of years in panel
[c] 2007 prices.

stricted mobility at the start of the period, compared to 47 percent at the end. The share reporting long-standing illness likewise changes little; 78 percent reported illness in the beginning of the period as compared to 80 percent at the end; here, one can however note that the change goes in the opposite direction of the mortality trend.[11]

The description of the municipalities' allocation problem in Equation 2.1 motivates a set of control variables. To hold the share of competitors over public funds constant, the share of children/young (0-19 year olds, *sh0-19*) and the share of foreign born (*shImm*) are included in the estimations.[12] Taxable income per capita (*TInc*) is also included to separate the political effect of aging from the economic effect that pension income, on average, is lower than labour income. In addition to these variables, grant income per capita (G) and the female labour force participation rate in the 16-64 age group (*femlab*) are also controlled for. Women are overrepresented as providers of formal as well as informal long-term care. Thus, what sign to expect on the coefficient of *femlab* is not evident.

Family altruism has been brought up in previous studies (Rattsø and Sørensen, 2010; Strömberg, 2006). However, because it is not the focus of the current analysis, it is not accounted for in the estimations. Although Strömberg (2006) found that family altruism towards elderly parents crowds out educational spending, most of the total effect of aging in his study is attributed to the increase in the share of elderly. Moreover, Rattsø and Sørensen (2010) find no evidence that altruism towards elderly parents is important in neighboring Norway.

2.4.2 Estimation strategy

The above considerations result in the following general estimation equation:

$$\log(y_{it}) = \beta_0 \log(sh65+_{it}) + \beta_1 \log(mort65+) + \beta_2 \log(sh0-19_{it}) + \beta_3 \log(shImm_{it}) + \\ + \beta_4 G_{it} + \beta_5 \log(TInc_{it}) + \beta_6 \log(femlab_{it}) + \alpha + \lambda_t + \mu_i + \varepsilon_{it} \quad (2.3)$$

[11] To attenuate the influence of sampling variation, these figures are 3-year averages. Descriptives from the Survey of Living Conditions (ULF) are available at Statistics Sweden's web page, www.scb.se.

[12] Foreign-borns are overrepresented as recipients of social assistance (Puide, 2000) and are included in the calculations regarding social assistance in the spending equalization system (Statistics Sweden, 2006) They are thus relevant for "other spending" and G in the budget restriction.

where y_{it} is either LTC costs per potential client (65+), LTC costs per actual client, or the share of actual LTC clients among the 65+. The variable α is a common constant, λ is a set of year dummies capturing time-specific shocks, μ is a vector of municipality-specific effects and ε_{it} is an idiosyncratic error. Variables are logged, when possible, to allow for elasticity interpretations.

As noted in related studies (Rattsø and Sørensen, 2010; Ladd and Murray, 2001), there may be reversed causality: cutbacks on LTC may induce outward migration of the elderly, and conversely attract families with children, as there is larger scope for spending on child care and education when the LTC policy is less generous. To account for such "Tiebout bias", previous studies use lagged population shares as instruments for the present demographic structure. The same strategy is employed here: the share of upper middle-aged (55-64 year-olds) in time $t-10$ is used as instrument for the share of 65+ at time t. Arguably, the share of upper-middle aged ten years ago correlates positively with the present share of elderly (consider the transmission of a baby-boom cohort through a demographic pyramid evaluated at two points in time), and it is reasonably not (i.e. given less than perfect foresight) affected by policy changes that occur ten years later.

However, simply estimating the model (2.3) using the fixed effects (FE) estimator, that is, conditioning on the municipality-specific effect μ_i, reduces the Tiebout bias to some extent. For Tiebout bias to be a problem in FE estimation, geographical mobility must react instantly to changes in LTC generosity (correlation between $\log(sh65+_{it})$ and ε_{it}). Thus, even without the use of instruments, Tiebout bias should be a minor concern in the FE specification (Ladd and Murray, 2001). The drawback of using FE is that the estimation completely relies upon time-series variation, which is comparably low in the data. It would therefore be preferable to be able to use the random effects (RE) estimator, which also uses cross-sectional information. The RE model is augmented with county council-specific fixed effects to control for unobserved heterogeneity that can be suspected to be correlated to the municipality-specific error term (which makes RE inconsistent (Baltagi, 2008)). Notably, the generosity of the health care system, which to some extent is a substitute to LTC, is captured by the county council fixed effects. The applicability of the RE model is tested by the Hausman (1978) test.[13]

[13]The model is also estimated with the regular IV estimator on the pooled cross-section,

2.5 Results

Table 2.2 shows the estimates of the relationship between aging and LTC generosity (*LTC/65+*). Column (1) shows the estimate of the total effect, including both the direct effects of the change in the age composition of the population (increasing political influence, and more pressure on public finances), and the potential indirect effect of changes in LTC demand among the elderly. The share of 65+ turns out to be negatively correlated to LTC generosity, with an elasticity of -0.26. Despite the low time-series variation, the effect is highly significant. Note that the estimated elasticities are smaller than one in absolute values; the estimations thus reproduce the familiar result that aggregate spending on the elderly increases in response to aging, although not sufficiently much as to retain generosity.

The estimate in (1) does not take into account the potential Tiebout bias, but the result is also similar when applying the instrumental variable for the share of elderly (column (2)). As expected by the Tiebout argument, the estimate increases somewhat in absolute terms (becomes more negative): the elasticity is now estimated at -0.30.[14] According to the estimate in (2), a one standard deviation increase in the share of 65+ implies a 4.3 percent, or 2 300 SEK, reduction in the overall LTC generosity. Employing a longitudinal perspective, consider the official projection that the share of 65+ will increase from 18 percent in 2008 to 23 percent in 2030 (Statistics Sweden, 2009). This 28 percent increase translates to a reduction of 8.3 percent (4 400 SEK) of the average level of generosity.

However, as highlighted previously, if population aging is driven by better health among the elderly, they will demand less LTC and so part of the reduction should be attributed to changing demand rather than to politicians reacting to the demographic burden. In column (3), I try to disentangle the demand effect by adding the mortality rate among the 65+ as a control variable. The estimates suggest that part of the negative effect found in the previous specification is indeed due to decreasing demand among the elderly, as the coefficient on the share of 65+ decreases from

without either FE or RE. The RE and IV estimators yield qualitatively similar results, and the RE estimate is closer to that of the (consistent) FE estimator, so only the results of the RE estimation are presented.

[14] The high F statistic (F=1 085) suggests that weak identification is not a concern; the instrument is highly correlated to the endogenous variable, and significantly so. For first-stage results, see Table 2.A.1.

Table 2.2: Estimates of overall generosity (LTC/65+)

Dependent variable	(1)	(2)	(3)	(4)
log(*LTC/65+*)	FE	FE-IV	FE-IV	RE-IV
log(*sh65+*)	-0.259***	-0.302***	-0.254***	-0.357***
	(0.0996)	(0.0845)	(0.0860)	(0.0688)
log(*mortality65+*)			0.0850***	0.119***
			(0.0176)	(0.0141)
log(*sh0-19*)	0.498***	0.493***	0.470***	-0.233***
	(0.180)	(0.112)	(0.110)	(0.0694)
log(*shImm*)	0.0400	0.0194	0.0286	-0.0409***
	(0.0391)	(0.0291)	(0.0295)	(0.0147)
G	0.00596*	0.00576**	0.00509**	0.0137***
	(0.00343)	(0.00231)	(0.00229)	(0.00179)
log(*TInc*)	0.701***	0.728***	0.717***	0.428***
	(0.189)	(0.119)	(0.117)	(0.0785)
log(*femlab*)	0.274**	0.230**	0.238**	-0.241**
	(0.131)	(0.0995)	(0.0972)	(0.0946)
Constant	3.102			4.790***
	(2.150)			(0.977)
Year FE	Yes	Yes	Yes	Yes
County council FE	N/A	N/A	N/A	Yes
Observations	2 469	2 443	2 443	2 443
Model significance (F/χ^2)	54.54	119.1	117.7	1 940 (χ^2)
R^2	0.442	0.447	0.458	0.419
First-stage F		1 085	1 077	246

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

-0.30 to -0.25. But considering that this means that the predicted reduction to 2030 is adjusted downwards from 8.3 to 7.1 percent, it does not make much difference.

The mortality rate is in itself positively and significantly related to the generosity of LTC. Although this may seem to be a trivial result, one should keep in mind that individual demand does not meet supply at a market in this institutional setting, but is channelled through a political layer.

In column (4), model (3) is re-estimated with the random effects estimator, to determine if the result also holds when cross-sectional information is used. The coefficient on the share of elderly remains significantly negative and is even larger in magnitude in the RE estimation; however, the Hausman test rejects that the FE and RE coefficient vectors are equivalent, which indicates that the estimate is exaggerated. Nonetheless, although it may be poorly measured, the RE estimate still suggests that the sign of the overall effect is negative. This conclusion is further supported by the following robustness checks: first, although the estimates in columns (2)-(4) are from IV models, the same pattern appears if the Tiebout bias is disregarded. Second, the result is robust to the incorporation of an instrument for the share of children/young, the main competitors over public funds. Third, the result remains when specifying a dynamic model (Arellano and Bond, 1991), which takes into account that the short-run reaction may be negative due to policy inertia.[15]

It is interesting to see how the reduction in generosity is reflected in actual LTC policy. Table 2.3 shows the estimated relationship between aging and the share of actual LTC clients (1) and the generosity per actual client (2). In both cases, Tiebout bias is taken into account, but this is not crucial for the results. Moreover, the aging effect is estimated net of the effect on demand, but the estimates are not very different when the mortality variable is omitted from the regressions. From the table, it seems that the reduction in generosity goes through harsher eligibility criteria: the share of 65+ is negatively associated with the share of LTC clients (1), while the generosity per actual client is not affected at all (2). To rule out that the insignificance in (2) is due to the low within-variation, column (3) shows the results of an RE estimation, which also suggests that the per client generosity does not react to population aging. Arguably though, harsher eligibility criteria affect those would-be clients that are relatively

[15]These robustness checks are available from the author upon request.

Table 2.3: Estimates of share of LTC clients and generosity per client

	(1)	(2)	(3)
	FE-IV	FE-IV	RE-IV
Dependent variable (log):	LTCshare	LTC/client	LTC/client
log(*sh65+*)	-0.272**	-0.00309	-0.109
	(0.138)	(0.157)	(0.110)
log(*mortality65+*)	0.0406*	0.0527**	0.0496**
	(0.0238)	(0.0260)	(0.0241)
log(*sh0-19*)	-0.143	0.603***	0.257**
	(0.165)	(0.177)	(0.106)
log(*shImm*)	0.0364	0.0386	-0.0458**
	(0.0428)	(0.0494)	(0.0215)
G	-0.0110***	0.00960**	0.0109***
	(0.00319)	(0.00411)	(0.00287)
log(*TInc*)	-0.179	0.527***	0.276**
	(0.148)	(0.198)	(0.118)
log(*femlab*)	-0.0233	0.145	0.0440
	(0.155)	(0.180)	(0.159)
Constant			9.571***
			(1.537)
Year FE	Yes	Yes	Yes
County council FE	N/A	N/A	Yes
Observations	2 838	2 441	2 441
Model significance (F/χ^2)	49.50	90.05	1,511 (χ^2)
R^2	0.268	0.403	0.400

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

healthy. Together with an unchanged level of spending per client, such a change in composition of the client group implies that clients at a given disability level do receive a lower service level in times of population aging.

In the longitudinal perspective, the estimates imply that the increase in the share of 65+ to 2030 implies a reduction in the share of LTC clients from 15% to 14%. Viewed from this angle, the reduction in generosity does not seem dramatic.

2.6 Concluding remarks

To summarize, the estimations show that the pattern found in other Scandinavian countries also appears in Swedish municipalities: population aging is positively correlated to aggregate LTC spending, but negatively correlated to the generosity of LTC benefits. Part of the latter result seems to be due to decreasing demand for LTC among the 65+, but most of the effect remains even when demand is held constant (i.e, controlling for mortality). Politicians reduce generosity by adjusting the share of elderly entitled to LTC and, given that this adjustment mainly affects relatively healthy individuals, the generosity per actual client. The magnitude of the negative correlation between aging and generosity is modest, however: holding mortality constant, the model predicts that the 30% increase in the share of 65+ projected to 2030 is accompanied by a 7% reduction (3 700 SEK) of LTC spending per elderly. Thus, while they do not manage to defend the level of generosity, the Swedish elderly seem to resist drastic cutbacks.

The overall negative correlation suggests that local politicians do not let the increasing political importance of LTC override their concern for the aggregate cost level (Rattsø and Sørensen, 2010). Moreover, no inhabitants are exempt from sharing the demographic burden; the elderly and the non-elderly, and LTC clients as well as non-clients are negatively affected. At least two competing interpretations of the findings can be made, however. First, the elderly may trade off LTC generosity towards the tax burden and find increases in LTC generosity unworthy its tax price (as mentioned, the elderly pay local income taxes): if so, local governments cannot attract the elderly's votes by increasing the generosity of LTC. The plausibility of this argument is however limited by the opportunities to crowd out other spending (see Strömberg, 2006, for evidence of such crowding-out). Second, if the mortality variable does not comprehensively control for health improve-

ments among the elderly, the negative correlation between LTC spending and the population share of elderly reflects the residual negative effect of health on LTC demand. Against this objection, one can argue that the mortality rate should be a sufficiently encompassing measure of health during the relatively short time period under study. Indeed, the Swedish Survey of Living conditions does not point at striking changes in elderly health over the panel period.

The model controls for elderly health and tax base size to ensure that the elderly share only captures the effect of demographic change. To receive a complete picture of the effects of population aging, one must acknowledge that these factors are likely to change simultaneously with the demographic composition of the population. Unfortunately for the elderly population in a world with more retirees, the estimates suggest that the correlation between the tax base size and LTC generosity is positive. With regard to health, it is notable that the there is a positive correlation between the mortality rate and the generosity of LTC policy. Together with the main result, this finding indicates that politicians are willing to extend generosity, but only if the elderly are perceived to "need" it in some sense; large cohorts should not hope to accomplish general increases in the LTC standard (for instance, higher frequency of home care visits, better food quality) by virtue of their cohort size alone.

The evidence from this and other studies of Scandinavian countries thus paints a picture of a municipal policy process that directs extra resources towards subpopulations in growth; however, the political importance of the elderly is not sufficiently strong to increase levels of benefits. Is this a feature of the municipal political organization, or does it extend to other political levels? An answer is provided by the fact that studies of population aging and pension system generosity in OECD countries generally tell the same story. Importantly, in the case of pensions, there is no reason for a negative bias related to the health level of retirees. So it seems to be a general result that although the grey power of an increasing share of elderly increases aggregate spending, it is not sufficiently strong to extend the generosity of public elderly benefits. Tentatively, this suggests that savings, and, with regard to LTC, private arrangements (formal as well as informal) will come to play a greater role for the elderly in an aging society.

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2.A First-stage estimates

Table 2.A.1: First-stage regression of share 65+

Dependent variable	(1)	(2)	(3)
log(*sh65+*)	FE (direct+indirect)	FE (direct)	RE (direct)
log($sh55\text{-}64_{t-10}$)	0.466***	0.462***	0.465***
	-0.0142	0.0141	0.012
log(*sh0-19*)	-0.453***	-0.444***	-0.639***
	-0.0268	0.0265	0.021
log(*shImm*)	-0.0311***	-0.0326***	-0.088***
	-0.0083	0.008	0.004
G	0.00162**	0.0017**	0.009***
	-0.00071	0.0007	0.0006
log(*TInc*)	-0.0973**	-0.0942**	0.0578**
	-0.0355	0.0354	0.028
log(*femlab*)	-0.305***	-0.304***	-0.629***
	-0.0305	0.0306	0.0240004
log(*mortality65+*)		-0.0164***	-0.003
		0.004	0.005
Year FE	Yes	Yes	Yes
County council FE	N/A	N/A	Yes
Observations	2 443	2 443	2 443
R^2	0.802	0.804	.
F-test	1 085	1 077	246

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

Chapter 3

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

with Jens Dietrichson

3.1 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

[1] See 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.

[2] The transfers were not last minute rescue attempts in the face of imminent defaults. We

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. 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.

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)).

[3] As 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).

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. 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

[4]The 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.

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

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 chapter is structured as follows: section 3.2 outlines the institutional background. Section 3.3 presents the data and discusses the choice of fiscal performance measure. Section 3.4 describes our estimation strategy and introduces the synthetic control method, while section 3.5 contains the estimation results. Section 3.6 explores potential sources of the

heterogeneity in program effects. Section 3.7 concludes.

3.2 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.

[6] In 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).

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

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 3.A, tables 3.A.1-3.A.3). They and their neighbours 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 3.A.1 and 3.A.2). This suggests that projected future revenues was the most important of

[8]Two 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.

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]

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).

[9] The 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.

[10] Whether the central government would actually be tough and apply the conditions, or give in and pay the whole sum anyway, was uncertain at the beginning of the program. For example, an audit report 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).

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 *Kommundelegationen* 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 3.4.2 and 3.6.[13]

3.3 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

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

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

[13] We focus on *Kommundelegationen* 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. *Kommundelegationen* 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).

in 1992. Comparisons further back in time may thus be misleading.

3.3.1 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.

3.3.2 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 differences in policy preferences and political landscape by the *share of*

[14] We 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.

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 3.A. Tables 3.A.4 and 3.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 3.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.

3.4 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

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

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

[17] In 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.

[18] Neighbours are defined as sharing land borders.

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 3.4.1, 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 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 3.4.3 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 frameworks 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 3.4.2.

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 3.4.4 for a fuller description).

[19] The 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 3.B.

3.4.1 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 synthetic control algorithm searches for weights w that make

$$\begin{cases} Z_i = Z^{dp} w \\ y_{i,t} = \sum_j w_j y_{j,t}^{dp} \qquad \forall t < T_0 \end{cases} \tag{3.1}$$

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.

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

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 3.A), the municipalities applying for the program are quite likely to be extreme. Importantly, though, the relevance of these two concerns can be judged after the estimation, as it is possible to examine the pre-program fit of each synthetic control.

3.4.2 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

[21] Unlike 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.

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

donor pool for the admitted – or even that they should constitute the whole donor pool. As seen from Tables 3.A.1- 3.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 3.2) 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 (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 syn-

thetic 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]

3.4.3 Fixed effects estimations

Our general estimation equation is

$$y_{it} = \alpha + \beta X_{it} + \sum_{t=2000}^{2010} \gamma_t D_{it} + \lambda_t + \mu_i + \varepsilon_{it} \tag{3.2}$$

where X_{it} is a vector of cost determinants[24] 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

[23] The 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.

[24] We 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.

to the synthetic control for *costs* are also suitable comparison units for net revenues. This seems like a reasonable assumption given that they are similar in terms of cost structure as well as political, economic and demographic characteristics.

3.4.4 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.

$$average_i = \frac{1}{T} \sum_{t=2000}^{2010} (y_{it}^{actual} - y_{it}^{synthetic}); \tag{3.3}$$

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.

3.5 Results

3.5.1 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 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 3.1: Average pre-RMSPE per synthetic control estimation

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

In parentheses: number of municipalities whose pre-RMSPE<cut-off.

Table 3.1 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 (pre-RMSPE<cut-off) naturally decreases as the cut-off becomes stricter. The decrease is especially drastic in the estimations where neighbours are ex-

[25] For 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.

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

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

cluded from the donor pool, which confirms that neighbours are important contributors to the synthetic controls.

3.5.2 Average program effects

Figures 3.1 and 3.2 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 black (dashed) 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 3.1 (Figure 3.2), 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 3.1 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 3.2) 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.

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

Figure 3.1: Average actual-synthetic difference, admitted

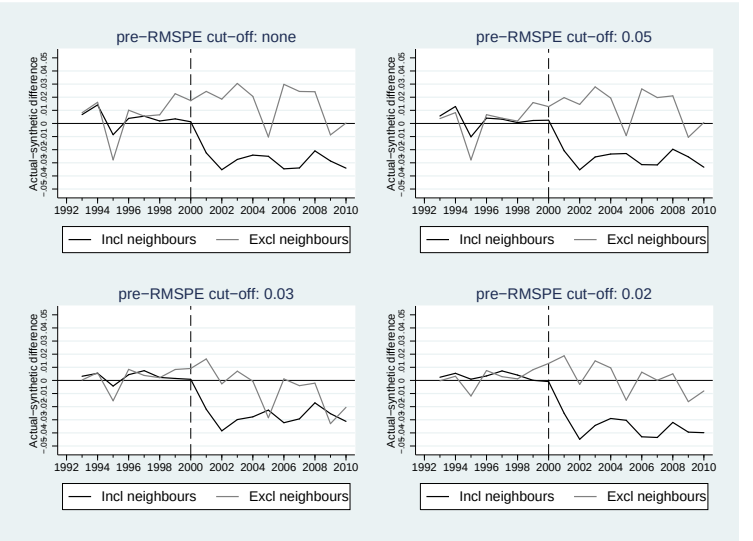


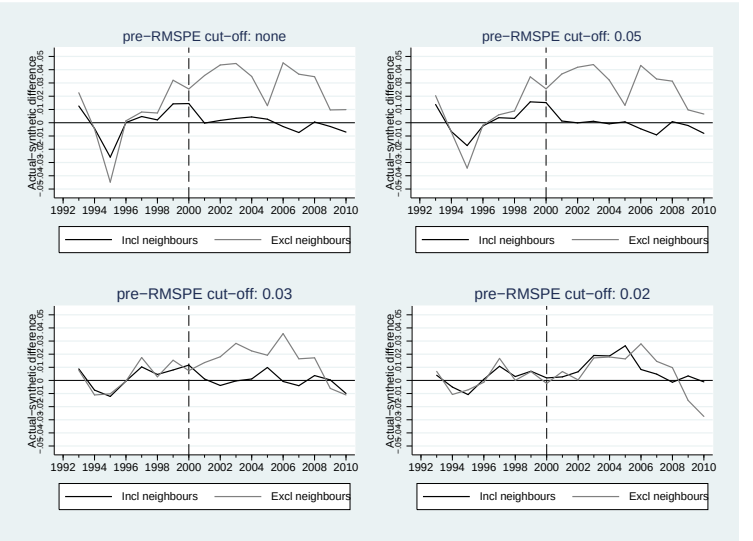
Table 3.1) as much of this deterioration arises due to bad fit in 1999. The sensitivity to the inclusion of neighbours motivates a further investigation. In Appendix 3.C, we therefore estimate synthetic controls for the 33 neighbours as well. In brief, we get a very poor fit for three of the municipalities 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 3.2 and 3.3 show the results for the samples of admitted and rejected, respectively. All actual-synthetic pairs enter the estimations reported in the table; i.e. no pre-RMSPE cut-off is applied. However, our conclusions do not change if we instead include only munici-

[29] Note 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.

[30] See Appendix 3.B for results for covariates.

Figure 3.2: Average actual-synthetic difference, rejected



palities with pre-RMSPE < 0.03 (results available on request). Neighbours 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 3.2), the admitted municipalities show a significantly lower cost level than their synthetic counterparts from 2001, the first full year of the program,[31] 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 3.1. Apparently, the inclusion of municipality-fixed effects entails a downward adjustment of the differences.[32]

For the rejected (Table 3.3), there are almost no significant differences between actual and synthetic costs, regardless of whether neighbours are

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

[32] The actual-synthetic differences shown in Figure 3.1, 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 3.B.

included in the donor pool or not. As for the admitted, the fixed effects seem to erase the seemingly positive effects in Figure 3.2 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 3.2 and 3.3); the changes in the magnitude and significance of the coefficients are mostly small for both groups.

In columns (5)-(8) of Tables 3.2 and 3.3, 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. These estimations yield three remarkable results. *First*, the magnitudes of the yearly differences in Table 3.2 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,[33] and the coefficients are highly significant most years. *Second*, we find little indications of a similar effect on the rejected municipalities (Table 3.3), 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, we find decreased costs for admitted municipalities when neighbours are included in their comparison group, but unchanged costs when neighbours are excluded from the comparison group. The rejected municipalities have similar cost levels as their comparison group, regardless of whether this group includes neighbours or not. Regarding net revenues, the consistently positive estimates for the admitted, as well as the difference between the estimates for the admitted and the rejected, suggest that program participation is associated with a relatively favourable development of net revenues.

In Appendix 3.B, we show that similar results are obtained when we estimate fixed effects models on samples without applying the synthetic control algorithm, and when we do not use the weights from the synthetic controls. We also include results that indicate that the results in the sample

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

Table 3.2: Fixed effects estimations of program effects, admitted municipalities

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

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

Table 3.3: Fixed effects estimations of program effects, rejected municipalities

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

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.

excluding neighbours are sensitive to the chosen length of the period: when we use the whole period 1993-2010, the estimates for net revenues become smaller for the admitted group and the cost estimates become larger for the rejected group. Importantly though, per capita costs are rarely significantly different from zero and net revenues are still positive and significant most years 2002-2010 for the admitted group. Thus, these results do not change our conclusions about the average effect for admitted municipalities, and the difference to the rejected group becomes more marked.

Regarding the interpretation of the results, a concern is that the findings may not be caused by the program, but by something else that we are not able to control for. One potential explanation is that the patterns do not reflect causal phenomena, but instead reflect mean reversion: that costs (net revenues) were high (low) for random reasons in the period leading up to the initiation of the program, and are just reverting back to the mean thereafter. As documented here and in Pettersson-Lidbom (2010), the financial troubles of both admitted and rejected municipalities date back to the late 1970's. In most of our estimations, the admitted group display a visible turn towards more discipline starting in 2001; that is, the first year when they had time and explicit incentives to react to the content of the program (rather than just to its announcement). It seems rather unlikely that a random process would generate above-average costs in two decades and then start to exhibit mean reversion exactly the same year when one could expect a program effect to kick in.

Other factors may have simultaneously forced the admitted municipalities to restrain spending and run surpluses though. The Balanced Budget Act is one such potential factor. However, it is unclear why the implementation of this act in 2000 should have affected the admitted group differently from the rejected group. In addition, the incentives to conform to the act were in place already in the year 2000, a year for which we do not see higher net revenues and lower costs for the admitted municipalities. Another possibility, emphasized theoretically by for example Battaglini (2011), is that at high levels of debt (the argument extends to costs if the ability to raise revenues is limited, as is the case here), debt service costs overshadow the utility to politicians of being able to spend by taking on more debt instead of using tax revenues. We do not think that this mechanism is the explanation of the results though, as the levels of total debt, as well as short term and long term debt, are very similar in the admitted and rejected groups

– both before and after the program (there are no significant differences for any year 2000-2010, results available on request). If debt service costs was the main explanation, we would expect this mechanism to affect the rejected group as well.

3.5.3 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 (3.3)). 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 3.4.4.

In the estimations where neighbours are *included* in the donor pool (Table 3.4, 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.[34] 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 3.4, 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,

[34] The 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.

we believe that the fixed effects pick up important unobserved heterogeneity and thus do not view the 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.[35] 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 3.4: 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

3.6 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)

[35] We 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.

to the 23 municipalities that do not reduce costs (the non-reducer group) according to the placebo analysis.[36] As the sample size is very small, we foremost interpret differences between the two groups as potentially fruitful directions for future investigations.

Table 3.D.1 in Appendix 3.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.[37] 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 compa-

[36] We 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 3.4.

[37] Interviews 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).

nies. In Appendix 3.B, we show however that costs were not only reduced in areas where such transfers would be recorded.[38] 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 cost-reducers and non-reducers relates to the size of the grants received within the bailout program (*Kommundelegationen*): 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 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 mo-

[38] Moreover 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).

tivation.[39] 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 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

[39] The 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.

however; thus, we do not rule out that institutions may be a channel for the differences between the groups.

3.7 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 pro-

gram 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.[40]

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

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

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 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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3.A Descriptive statistics

This section shows descriptive statistics for the municipalities, divided into admitted, rejected, and others. Table 3.A.1-3.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 3.A.4-3.A.6 display summary statistics for the time-varying covariates in 1999. Economic variables are in 2010 prices.

Table 3.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 3.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 3.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 3.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 3.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 3.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

3.B Sensitivity tests and covariate estimates

This appendix includes estimates from FE specifications on the full sample of municipalities, i.e. samples where we do not use any information from the synthetic control estimations. 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 3.B.1 shows results from fixed effects regressions where we do not make any use of 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 restrict the samples to 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 3.5, 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

Table 3.B.1: Fixed effects specifications, 1993-2010

	(1) Full sample	(2) Prog. 1999	(3) Incl neigh.	(4) Excl neigh.	(5) Full sample	(6) Incl neigh.	(7) Excl neigh.
Dependent	*costs*	*costs*	*costs*	*costs*	*net rev*	*net rev*	*net rev*
admitted	-0.021**	-0.015	-0.020**	-0.014	0.493***	0.554***	0.437**
	(0.010)	(0.010)	(0.010)	(0.011)	(0.152)	(0.172)	(0.181)
rejected	0.006	0.012	0.004	0.007	0.235	0.267	0.194
	(0.010)	(0.010)	(0.010)	(0.011)	(0.205)	(0.210)	(0.220)
$log(taxbase)$	0.652***	0.657***	0.483***	0.509***	-0.465	-0.249	-2.260
	(0.093)	(0.093)	(0.084)	(0.099)	(1.323)	(1.734)	(1.844)
eq.grant	0.0066***	0.0065***	0.0034**	0.0027	0.117***	0.105***	0.0794***
	(0.0020)	(0.0021)	(0.0017)	(0.0016)	(0.024)	(0.027)	(0.025)
$eq.grant^2$	0.0001	0.0001	0.0003***	0.0003***	-0.0013	-0.0021	-0.0011
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0011)	(0.0013)	(0.0014)
employment	-0.0039***	-0.0040***	-0.0030**	-0.0035**	0.055**	0.049	0.058*
	(0.0015)	(0.0015)	(0.0014)	(0.0016)	(0.022)	(0.030)	(0.031)
$log(pop.)$	-0.041	-0.033	0.061	0.075	4.25***	3.62***	3.31**
	(0.064)	(0.064)	(0.073)	(0.084)	(0.89)	(1.19)	(1.36)
share 0-14	0.0065	0.0065	0.0029	0.0034	-0.0058	0.050	0.012
	(0.0041)	(0.0041)	(0.0043)	(0.0049)	(0.057)	(0.069)	(0.073)
share 65+	0.011***	0.010***	0.0058**	0.0065**	-0.0077	0.032	0.035
	(0.0032)	(0.0032)	(0.0025)	(0.0027)	(0.035)	(0.047)	(0.049)
rightwing	-0.00013	-0.00013	-0.00033	-0.00061	0.012**	0.018***	0.020***
	(0.00037)	(0.00037)	(0.00044)	(0.00049)	(0.0056)	(0.0068)	(0.0074)
herfindahl	0.148***	0.147***	0.105*	0.0891	1.168	1.781	1.668
	(0.0509)	(0.0507)	(0.0579)	(0.0641)	(0.842)	(1.094)	(1.175)
seats	0.00025	0.00031	-0.0000	-0.0000	-0.00851	-0.00555	0.00533
	(0.0006)	(0.0006)	(0.0008)	(0.0008)	(0.0105)	(0.0130)	(0.0146)
Constant	0.861	0.760	0.813	0.567	-43.43***	-39.92***	-27.68*
	(0.714)	(0.716)	(0.791)	(0.884)	(10.22)	(14.08)	(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): 35 admitted, 22 rejected, and the donor pool of 136 municipalities.

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

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

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.[41]

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).

We have also estimated the same fixed effects specification on samples where we do not use the weights to compute the synthetic controls, but only to restrict the comparison group. That is, we include in the comparison group all municipalities that are given higher than zero weight in at least one synthetic control estimation. Interestingly, these estimations serve to strengthen the case for a disciplining program effect on costs, as the most striking difference from the baseline estimations reported in main text (columns (3) and (4) of Table 3.2) is that the cost estimates in the sample excluding neighbours become more negative and more significant. We can also see a similar tendency for the rejected municipalities, although for this group there are still only a few years towards the end of the period where costs are significantly lower. We refrain from showing these results as they are other similar to the results presented in the main text (results are available on request of course).

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

[41] To 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 3.B.2: Fixed effects on longer samples and housing/non-housing related costs

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

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 3.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 3.2). 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 3.2). 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 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 3.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 3.6, we want to examine whether the cost reductions of admitted municipalities are only a mechanical implication of having reconstructed their troubled housing companies.[42] 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);[43] 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 3.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,

[42] It 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.

[43] *Infrastruktur* and *Affärsverksamhet*.

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 program.

3.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.B.1 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 3.B.3.

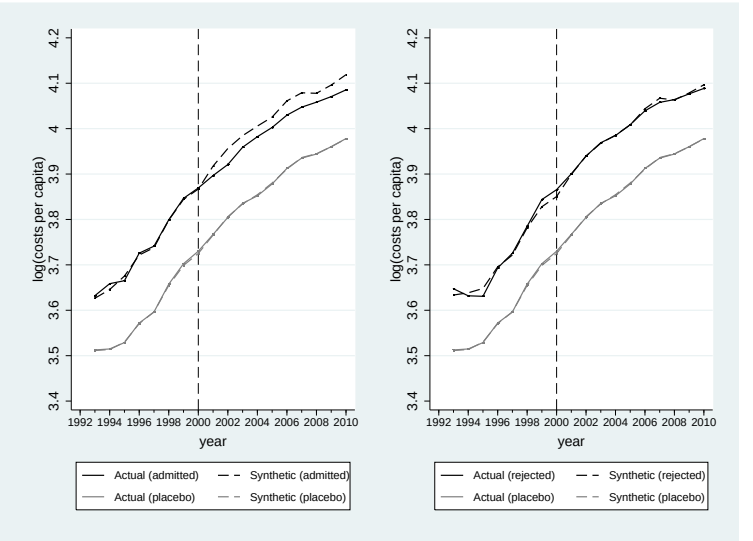
The inference on the yearly average program effects in Section 3.5 relies on standard errors from the fixed effects estimations. As an alternative way to evaluate the statistical significance of the yearly average program effect, we use a variant of the method recently suggested by Cavallo et al. (2011). 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 control in year t is then

$$\bar{\alpha}_t = \frac{\sum_{i=1}^{N^p} y_{it} - y_{it}^{synth}}{N_p}. \tag{3.4}$$

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 $t \geq T_0$,

Figure 3.B.1: Actual and synthetic average per capita (log) costs of services for admitted, rejected, and placebo municipalities, pre-RMSPE < 0.05 (incl. neighbours in donor pool)



i.e. the probability to observe such a large/small effect in the absence of program, as

$$p - value_t = \frac{\sum_{dp=1}^{10000} \mathbf{1}\left(\bar{\alpha}_t^{dp} < \bar{\alpha}_t\right)}{10000} \tag{3.5}$$

where $\bar{\alpha}_t$ is defined as in equation (3.4), $\bar{\alpha}_t^{dp}$ 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 3.B.2 (again for municipalities with pre-RMSPE < 0.05) and Table 3.B.4 show the results when neighbours are *excluded* from the donor

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

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

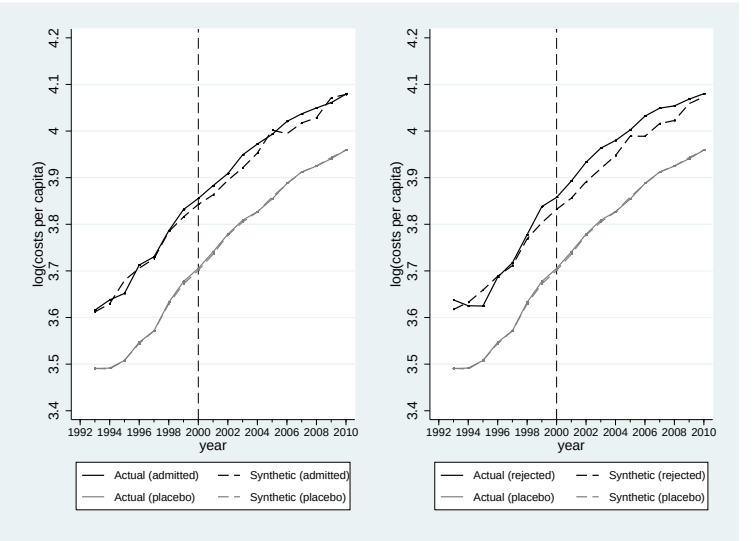
p-values in parentheses.
N_a = number of accepted municipalities with pre-RMSPE < cut-off
N_r = number of rejected municipalities with pre-RMSPE < cut-off

Table 3.B.4: Average program effects by year ($\bar{\alpha}_t$) excl neighbours from donor pool

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



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 3.2 and 3.3 when we exclude neighbours from the donor pool. This is not surprising: it becomes more important to control for unobservable, time-invariant characteristics 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.

3.C Synthetic controls 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 3.4.2. 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.

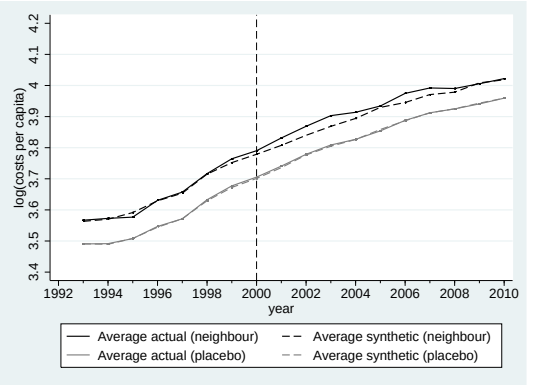
Figure 3.C.1 shows that the average of neighbours' actual costs are higher than the average of synthetic costs for several of the post-program years (as well as for 1999). Only the 31 municipalities with a pre-RMSPE < 0.05 were included in the computation of the average shown in the figure. An examination 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.

3.D Tests of equal means and equal proportions

Table 3.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

Figure 3.C.1: Actual and synthetic average log costs per capita, neighbours and placebo group



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 *Kommundelegationen* and, respectively, the total grants from the bailout program (Kommundelegationen) *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 3.3.2; 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 3.3.2); here, Δ-variables measure the relative change over 2000-2010. The mean (over 2000-2010) of population density (inhabitants/km^2) 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 signifi-

cantly 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 3.D.1: Sources of heterogeneity

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

Chapter 4

Institutions promoting fiscal discipline: evidence from Swedish municipalities

with Jens Dietrichson

4.1 Introduction

How to achieve satisfactory fiscal performance is a persistent challenge at all levels of government. The importance of this challenge has been all the more evident in the aftermath of the financial crisis in 2008, as the recession has severely strained the finances of many countries, regions, and municipalities, and even resulted in bailout-programs and defaults. One suggested response to the challenge, reflected in the European Union's fiscal pact for example, is to improve budget institutions – that is, the formal rules and informal norms related to the formulation, approval and implementation of the budget. Previous research indicates that institutional features such as transparent budget documents (e.g. Eslava, 2011), centralized budget processes (e.g. von Hagen and Harden, 1995; Hallerberg and von Hagen, 1999), and balanced budget rules (e.g. Bohn and Inman, 1996) are conducive to satisfactory fiscal performance.[1]

However, the literature is still far from a consensus on best practice in several respects. We contribute by addressing three issues: *First*, although conflicts of interest between agents within government are at the core of the political economy literature on fiscal performance, few empirical studies have tried to quantify such conflicts. To the best of our knowledge, none have done so using field data.[2] Omitting the degree of conflict be-

[1] Poterba (1996); Alesina and Perotti (1999) and Eslava (2011) survey this literature.

[2] Ehrhart et al. (2007) tests predictions of the Ferejohn and Krehbiel (1987) models of top-down and bottom-up budgeting in a laboratory experiment, and show that there is

tween agents makes it harder to detect the effect of budget institutions, since these should only play a role when there is a conflict for them to solve. *Second*, due to the often small number of observations, many studies represent budget institutions by index measures. The index formulation implies that the effect of particular institutions is obscured and thus precludes straightforward policy recommendations (Poterba, 1996). Index measures moreover preclude the study of interdependence between different institutions, while the specific combination of institutions has been argued to be of importance (e.g. von Hagen, 2006; Eslava, 2011). *Third*, although the need to control the whole budget process – from formulation to implementation – has been previously acknowledged (e.g. von Hagen and Harden, 1995; von Hagen, 1998; Hallerberg and von Hagen, 1999), empirical studies have largely overlooked the implementation stage, or considered it in index measures capturing features of the whole budget process (von Hagen and Harden, 1995; Fabrizio and Mody, 2006). Knowledge is therefore scant regarding how the incentives of local-level agents, to whom the responsibility of implementing the budget is delegated, can be aligned to the interests of the central level, that formulates the budget.

Our study addresses these three issues in an analysis of budget institutions and fiscal performance in the Swedish municipalities, thus adding to the literature on sub-national budget institutions.[3] Besides the fact that the municipalities – like local governments in general – constitute a large part of the national economy, certain attributes make them attractive study objects. They all operate under the same legal system and in the same cultural context, which mitigates the risk of confounding the effect of these factors with the effect of institutions – a prominent concern in cross-country studies. Moreover, all municipalities have the same fundamental areas of responsibility, which dampens the influence of differences in ambition. Still, the municipalities have considerable freedom to choose how activities should

no straightforward relationship between the sequence of the budget decisions and the size of the budget; the outcome also depends on the preferences of players.

[3]See e.g. Poterba (1994), Bohn and Inman (1996), Strauch and von Hagen (2001), and Krogstrup and Wälti (2008) who find that self-imposed balanced-budget rules are correlated to lower deficits; Foremny (2011) and Grembi et al. (2012) who find positive effects of fiscal rules imposed by the central government on fiscal performance; and Feld and Kirchgässner (1999), Hagen and Vabo (2005), Tovmo (2007), and Jochimsen and Nuscheler (2011) who find that centralization of the budget process is positively associated to (some) measures of fiscal performance.

be organized and financed, so there is heterogeneity to study.

We develop a simple model of the municipal budget process as a motivating framework for our empirical investigation. The model illustrates that to reach its desired fiscal outcome, the central level needs institutions that curb the local level's bargaining power in the budget formulation stage (i.e., institutions that *centralizes* the budget process) as well as institutions that align the incentives of the local level to the central's preferences at the implementation stage. Moreover, budget institutions may have to be strengthened if the conflict of interests between the central and local level becomes intensified. To obtain data on budget institutions and conflicts of interests, we construct a survey and collect a unique dataset covering 265 out of 290 municipalities. The survey explicitly measures fiscal conflicts of interest between the central level, which is responsible for the municipality's overall fiscal performance, and the local-level committees, who are responsible for their respective sub-fields only. The survey data indicates that substantial conflicts of interest regarding the importance of fiscal discipline prevail in roughly half of the municipalities.

The comparatively large number of cross-sectional observations enable us to analyze a diverse set of budget institutions without resorting to index measures. Besides the centralization of the budget formulation process, we examine two types of institutions that may allow the central level to influence the local level's spending decisions: result carry-over rules and threats to replace managers and politicians running systematic deficits.[4]

Our regression estimates confirm the importance of taking the interaction between institutions and conflicts of interest into account, as the estimated correlations depend on the degree of conflict. Like many previous studies, we find that a centralized budget process is beneficial for fiscal performance (measured as operating revenues net of costs), though only for municipalities where there is a substantial conflict of interest – that is, only in the circumstances where centralization should have a role to play. For this group of municipalities, we furthermore find that fiscal performance correlates positively to the use of a surplus carry-over rule and to a credible threat of replacement of local-level managers. For municipalities with less intense conflicts, the use of a deficit carry-over rule is positively correlated

[4]Dahlberg et al. (2005) find no correlation between result carry-over rules and fiscal performance in a study of the Swedish municipalities. To the best of our knowledge, replacement threats have not been studied before in the context of local governments.

to performance. While the data does not allow us to study the effect of either carry-over rule in the absence of a centralized budget process – most municipalities with carry-over rules also have a relatively centralized process – we do find that municipalities that combine carry-over rules with a centralized budget process have higher performance than municipalities that employ at most one of these institutions.

The next section gives some background information about the Swedish municipalities. We present our theoretical framework in Section 3. Section 4 describes the survey, the construction of our institutional variables and the other variables in the analysis, while Section 5 describes our empirical strategy. Section 6 is devoted to the presentation and interpretation of our results; and section 7 contains a discussion of identification issues and concluding remarks.

4.2 The Swedish municipalities

Sweden is divided into 290 municipalities: geographically separated units for local government. 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, 2011). All municipalities have the same fundamental responsibilities, e.g. the pre- to upper secondary school system, elderly care, social services, building and planning issues, environmental protection, and fire department services (Brorström et al., 1999). Nonetheless, the principle of municipal self-government, written into Sweden's constitutional laws, implies considerable freedom to choose how activities should be organized and financed (Berlin and Carlström, 2003). Revenues mainly derive from a proportional income tax, with the tax rate set freely by each municipality. In 2010, revenue from income taxes made up approximately 65 percent of total municipal revenues, fees 21 percent, and government grants from an equalization system 12 percent (Statistics Sweden, 2011).

Swedish law stipulates that each municipality must have a council and an executive committee. The council is appointed through general elections, held every four years, and the executive committee is elected by the council (Brorström and Siverbo, 2001). Most municipalities employ an organizational structure in which the council delegates the responsibility for different services to lower-level political committees, generally defined by

function (e.g schools) and/or by geography (e.g. a district). Administrative units with civil servants are connected to each political committee.

The municipalities are obliged to annually specify a budget, which should contain a plan for the coming year, and a long-term budget for the subsequent two years. The balanced budget law, enacted in 2000, moreover states that a budget deficit one year must be followed by an equally large surplus over the next three years. Nevertheless, the law allows for exceptions[5] and in practice is not enforced by any sanctions.

According to the bills preparing the legislation, the balanced budget requirement should be regarded as a minimal demand (Swedish Government, 2004). Empirically, nearly all municipalities have formulated more ambitious financial goals; a common target is to aim for net revenues of 1-2 percent of total tax and grant revenues (Dahlberg et al., 2005; Brorström et al., 2009). The main reason to strive for surpluses is that the municipalities have separate operating and capital budgets. Investments in capital generate expenditures immediately, but they only become costs in the form of write-offs. As investment expenditures normally are higher than write-offs, municipalities need to run surpluses to be able to finance investments without taking on more debt.

4.3 Theoretical framework

This section draws on the most relevant earlier literature to construct a motivating framework for our empirical investigation of what institutions, and what combinations of these are conducive to fiscal performance in situations characterized by conflicts of interest.[6] We sketch the budget process as a simple game, and, following North (1990; 2005), identify institutions with the (formal and informal) rules and enforcement characteristics of this game. We design the budget game with the budget process of the Swedish municipalities in mind, but the main features apply to public budget processes in general.

[5] E.g. if the deficit is caused by unconverted losses in stocks and bonds, or if the municipality has previously amassed large amounts of wealth (Swedish Government, 2004).

[6] We focus on budget institutions and thus disregard the large and related literature emanating from Roubini and Sachs (1989), that examines the effect of weak governments on fiscal performance. See e.g. Ashworth et al. (2005) for a review of the (mixed) results of this literature. We do however acknowledge strength of government in the empirical analysis, see section 4.4.

The budget game has two types of players, the central player (C) and the local player (L). Translated to the context of Swedish municipalities, C corresponds to the council and the executive committee and its administration, while each L corresponds to an operating branch at a lower level, for example the committee and administration of public schools. For simplicity, we assume only one single L in the game.[7] To focus on the relation between the central and the local level, we do not model how voter preferences influence the political game of deciding the tax rate and the level of fees.[8] In effect, we assume that the level of revenues is exogenous to the game.

The game focuses on two stages of the budget process: the planning stage, during which the budget is drafted and approved, and the implementation stage, during which it is executed. Previous research, as well as the features of the two stages, suggests that each stage has its own crucial institutional features. At the planning stage, the degree of centralization of the drafting and approval process is a crucial feature. At the implementation stage, the prevalence (or absence) of institutions that restrain the spending of the local level is important. As these latter institutions align the incentives of the local level to the interests of the central level, we henceforth refer to them as *incentive-aligning institutions*. In the game, we represent this type of institution with a possibility that C may punish L for not complying with the budget. The timing of the game is as follows:

1. The players receive information about the level of revenues.
2. In the planning stage, C and L bargain about the budget: L puts forward a budget proposal b_L and C determines the final budget b.
3. In the implementation stage, L chooses a spending level $x > 0$.
4. If the spending level exceeds the budget, C may punish L. Otherwise, the game ends after step 3.

The next three sections describe players' preferences and the planning and implementation stages in more detail. Throughout, information about pref-

[7] Treating the central and local levels as unitary players abstracts from the possibility that politicians and civil servants within each level have different preferences. For the purposes of this paper, we think that central-local conflicts of interests are more important.

[8] This choice precludes a theoretical treatment of the transparency of the budget process, suggested to be important by Eslava (2011). See section 4.7.1 for a discussion about this matter in relation to our results.

erences, payoffs, probabilities, and strategies are assumed to be common knowledge.

4.3.1 Players' preferences

Each player has preferences over fiscal performance; that is, the difference between revenues and spending. As the level of revenues is exogenous, we can translate, without any further loss of generality, preferences over fiscal performance into preferences over spending. For every level of revenue, we also assume that preferences are single-peaked; that is, there exists an optimal level of spending for each player, denoted x_C^* and x_L^* respectively. Due to the different roles and responsibilities of C and L, we assume that L's preferred level of spending is at least as high as C's, $x_L^* \geq x_C^*$, and say that conflicts of interest over spending increase as the difference between x_L^* and x_C^* increase. Although we assume that L is relatively spending-prone, L's optimal level of spending is not likely to be completely unrestrained. We rely here on the argument in Wildavsky (1975, p. 6-8) that there must be an element of cooperation and a shared understanding of the limits for budgetary proposals if an organization is to be able to function at all. Thus, L may but need not be a budget maximizer as the bureaus in for example Niskanen (1968).

4.3.2 The planning stage

Bargaining over the budget draft is a key feature of the planning stage. Weingast et al. (1981) were the first to suggest that excessively high (and Pareto-dominated) levels of spending can be explained by a common-pool problem present at the planning stage. von Hagen and Harden (1995) show that centralization of the budget process addresses the problem by changing the balance of bargaining power in favour of a centrally appointed finance minister (a player who, in contrast to ministers with specific portfolios, takes the full costs of each proposal into account).[9]

[9]Empirically, positive associations of centralization with fiscal performance have been found in the EU (von Hagen and Harden, 1995), Asia (Lao-Karaya, 1997), Latin America (Alesina et al., 1999; Stein et al., 1999), Africa (Gollwitzer, 2010), American states (Strauch and von Hagen, 2001), and in Norwegian municipalities (Hagen and Vabo, 2005; Tovmo, 2007). However, Dahlberg et al. (2005) and Perotti and Kontopoulos (2002) find no significance of centralization-type institutions in Swedish municipalities and OECD countries, respectively.

In our game, C bargains with L over a budgeted level of spending. We denote L's budget proposal b_L, and the approved budget, which is determined by C, is denoted b. To retain focus on the interplay between the institutions at the two stages, we refrain from explicitly modelling the bargaining process. Instead, to incorporate the insights from the earlier literature regarding centralization and bargaining power of L, we let C incur a cost, $(h \geq 0)$ if the final budget proposal is lower than L's proposed level. We also assume that h is increasing in L's bargaining power. Following von Hagen and Harden (1995), we call the planning stage *centralized* when the bargaining power of L is constrained in some way, for example by restrictions on the possibilities of proposing amendments,[10] or on the share of resources bargained over. That the possibility of making proposals is connected to the bargaining power of L can be rationalized by the fact that budget proposals of local committees are typically made publicly known through the media in Swedish municipalities. Thus, popular proposals are costly to decline for the central level. In other words, restricted possibilities of making proposals decrease the bargaining power of local committees and vice versa. Therefore, increasing centralization decreases h, and a fully centralized planning stage implies $h = 0$.

4.3.3 The implementation stage

If the fiscal preferences of the central level differ from those of the local level, which takes the actual spending decisions, incentive-aligning institutions at the implementation stage are necessary to prevent the local level from spending in excess of the budget (Hallerberg and von Hagen, 1999). Balanced budget rules and other numerical targets are examples of institutions intended to constrain agents. Poterba (1996) and Eslava (2011), who review the literature on numerical targets, underline that, although several studies find a positive correlation to fiscal performance, rules are only effective if enforced.[11] Acknowledging these results, we incorporate a generic

[10] Agenda-setting is often associated with bargaining power in political economy-models (e.g. Persson and Tabellini, 2000; Tovmo, 2007).

[11] Bohn and Inman (1996) find that balanced budget rules in American states that are enforced by the state supreme court have a positive impact on fiscal performance, and that the rule is more binding in appointed, as opposed to elected, supreme courts. The results in Debrun et al. (2008) for the countries in the European Union suggest that features such as statutory basis, independent monitoring and enforcement, automatic

incentive-aligning institution into the game – a threat of punishment for budget non-compliance – that varies in credibility and strength.

At the implementation stage, L first chooses the level of spending (x). After having learnt the realized level of spending and compared it to the budgeted level b, C decides whether or not to punish L. A punishing institution affects L's actions in the previous step by making deviations from the budgeted level of spending costly. The deterring effect of the institution depends on the size of punishment, $p \geq 0$ (the strength), and on L's subjective assessment of the probability that the punishment is carried out, $q \in [0, 1]$ (the credibility). We assume that this probability is known by C and that it is strictly increasing in the size of the deviation from the budgeted level. Furthermore, if $x = b$ then $q = 0$.

4.3.4 Payoffs and results

The payoffs (utility) for the two utility maximizing players are given by the following functions:

$$U_C = u_C(x) - h$$

$$E(U_L) = u_L(x) - q(x)p$$

where the utility of spending for each player i, $u_i(x)$, is a continuous and strictly concave function with a single optimum $x = x_i^* \in (0, \infty)$. Assume also that if $u_L(x' = b) = u_L(x) - q(x)p$, then L prefer to comply with the budget, i.e. choose x' rather than risk punishment. Within this setup, we look for the sub-game perfect equilibrium level of spending of the game (x^e) and state the following propositions (see Appendix 4.A for proofs):

Proposition 1: (i) $qp = 0 \Rightarrow x^e = x_L^*$, (ii) $x^e \in [x_C^*, x_L^*]$, (iii) If $x_L^* = x_C^*$, then $x^e = x_L^* = x_C^*$.

Part (i) shows that an incentive-aligning institution has to possess some credibility and strength ($qp > 0$) to be effective. It follows that h is unimportant if $qp = 0$; if the threat of punishment is not credible, L can simply disregard the budget and centralization becomes unimportant. Part (ii) captures that neither C nor L is interested in spending less than x_C^* or more than x_L^*. The interesting implication of part (iii) is that if there are

correction mechanisms, and media coverage are all important.

no conflicts of interest, then the institutional structure is not important for the level of spending, and in turn not for fiscal performance. In the following, we assume $x_L^* > x_C^*$.

Proposition 2: Suppose $h > 0$. Then, (i) $x^e > x_C^*$ for any $qp \geq 0$; and (ii) for any $qp > 0$, $x^e \in (x_C^*, x_L^*)$ strictly increases in h.

Part (i) shows that incentive-aligning institutions are not sufficient to reach the outcome desired by C; in fact, whenever the planning stage is not fully centralized, C's optimal level of spending is unattainable. The reason is that C then has to take the cost h of proposing a budget $b < b_L$ into account, which gives L enough bargaining power to force C to set $b > x_C^*$. Part (ii) states that for a given level of incentive-alignment, a more decentralized budget process always implies a higher level of spending. (ii) also implies that for sufficiently large h, a given incentive-aligning institution will not be able to curb spending at all. The reason is that when C experiences sufficiently high costs of deviating from L's proposal, L can make C propose $b = x_L^*$ at the planning stage and thus obviate the threat of punishment at the implementation stage.

Proposition 3: For any $h \geq 0$, (i) there is a lowest feasible level of spending $\underline{x} \in [x_C^*, x_L^*]$ and (ii) $x^e \in (\underline{x}, x_L^*]$ strictly decreases in q and p.

Part *(i)* highlights that the level of centralization implies a lower bound for the attainable level of spending C can force L to choose. Part (ii) adds that the equilibrium level of spending will be closer to x_C^*, the stronger and/or more credible the incentive-aligning institutions are (until the lower bound defined by L's bargaining power is reached).

In the last proposition, we take the point of view of C and show that when conflicts of interests over spending increase, i.e. when x_L^* increases, C may need stronger institutions to retain the earlier level of spending. Notably, increased conflicts of interest do not imply higher levels of spending if punishments are sufficiently credible and severe to start with.

Proposition 4: For given x_C^* and $h > 0$, (i) x^e is non-decreasing in x_L^*; and (ii) if the strength (p) and credibility (q) of the incentive-aligning institution is sufficiently weak, then x^e is strictly increasing in x_L^*.

If we instead take the opposite view and fix x_L^*, the equilibrium level of spending may similarly be decreasing in x_C^* (that is, decreasing in the level of conflict), but the strength and credibility of the institutions play the same crucial role in the determination of the equilibrium level of spending. Viewing C's bliss point as a measure of its motivation for high fiscal performance, henceforth referred to as its *fiscal motivation*, proposition 4 indicates that C has incentives to strengthen its budget institutions as its bliss point moves farther away from L's. This suggests that the realized level of spending and the institutional framework may be jointly determined, which is challenging for our empirical investigation. We discuss this matter further in section 4.7.1.

In sum, if there are differences in preferences over spending, both centralization and incentive-aligning institutions may be required to reach a level of spending that implies an outcome close to that desired by the central level. The need for stronger institutions also increases when conflicts of interests over spending increase. We use these results and the earlier literature to guide our data collection and econometric analysis, which we describe in the next sections.

4.4 Data

For the empirical investigation we require information on conflicts of interests, on the degree of centralization, and on candidates for incentive-aligning institutions. To obtain such data, we constructed a survey that was sent to all 290 Swedish municipalities in June 2010.[12] The electronic survey was addressed to the civil servant in charge of planning and implementing the overall budget, i.e. the budget manager. Respondents were promised confidentiality.

We modelled the survey after a similar survey conducted by the Swedish Association of Local Authorities and Regions (SALAR) in 2004.[13] Our survey differs from the 2004 survey in important respects though; in particular, the older survey does not record whether there are conflicts of interests between the central and local levels. To validate the survey questions, we discussed them with the budget manager and one of his close co-workers

[12] See Appendix 4.E for a translation of the survey questions.
[13] Dahlberg et al. (2005) analyze this survey.

in the municipality of Helsingborg (the 9th largest municipality), and with representatives of SALAR.

As many as 265 of the municipalities responded to the survey (91 percent). With regards to representativeness, it can be noted that the 25 non-responding municipalities are significantly smaller and have higher tax rates, smaller income tax bases and lower net revenues than the responding municipalities. For individual survey questions, the response rates are often lower than 91 percent (Appendix 4.B analyzes the differences in response rates between questions). This implies that our estimation sample consists of fewer than 265 observations.

Data on fiscal performance and additional control variables is obtained from Statistics Sweden (2011).

4.4.1 Measuring conflicts of interest

To measure conflicts of interests over fiscal matters between the central and local levels, the budget managers were asked to indicate the situation that best describes their municipality:[14]

1. the executive committee and the municipal council are more concerned about fiscal discipline than local committees;

2. the executive committee, the municipal council and the local committees do not differ significantly in their concerns about fiscal discipline;

3. local committees are more concerned about fiscal discipline than the executive committee and the municipal council.

The survey answers are translated into the dummy variable ci, which equals 1 if the executive committee/municipal council are more concerned about fiscal discipline (alternative 1) and 0 otherwise.[15] 56 percent of the 239 municipalities that responded to the question chose alternative 1, i.e. the budget manager estimated that there were conflicts of interest of some substance. To relate the survey question to our theoretical model, which considers preferences over spending levels, note that the level of revenues is

[14] The translation of the Swedish survey question into English is not perfect, the question uses an idiom ("en ekonomi i balans") in use in the municipalities, which does not literally translate as "fiscal discipline". We think that fiscal discipline conveys the meaning of the idiom better than the literal translation ("a balanced economy").

[15] Only two municipalities indicated alternative 3. The results are not affected by putting them in the same category as those who chose alternative 2.

fixed in the model. The preferences over spending levels in the model are therefore closely related to preferences regarding fiscal discipline as measured here. Fiscal discipline is of course a more long-term concept than the model reveals, but local committees that are concerned about overall discipline should also be more likely to respect their own short-term budget balance.

We are confident that the budget manager is the most suitable person to judge the situation, as the manager has a coordinating role in the budget process and closely follows the local level throughout the budget year. It is moreover important to note that the budget manager has little interest in stating a certain response in order to look better her-/himself, the question regards the committees.

The variable is a crude measure of the degree of conflicts though, as respondents' individual cut-off points for choosing one alternative over another are subjective and likely to differ. Therefore, some municipalities that according to an objective measure would be categorized as having substantial conflicts may choose alternative 2, and vice versa. Such mis-categorizations decrease the difference between the groups in terms of real conflicts of interest, which makes it more difficult to empirically detect between-group differences in how budget institutions work.

The concern for fiscal discipline likely differs somewhat between the two levels even in municipalities that chose alternative 2, as each local committee is responsible for only one part of the municipality's services and moreover partly functions as advocate for its own area. For the empirical analysis, this implies that the expected difference in the workings of the budget institutions becomes a matter of degree; effective institutions are not unthinkable in municipalities that chose alternative 2, but we expect them to be less important.

4.4.2 Incentive-aligning institutions

Our theoretical model considers a generic type of incentive-aligning institution, but empirically they can take various shapes. Monetary bonus schemes readily come to mind, but such schemes are virtually non-existent in Swedish municipalities.[16] We therefore examine a few other institutions,

[16] Only one municipality in our survey reports the use of bonus schemes related to surpluses, despite the nearly universal prevalence of surplus targets.

to see whether they possess incentive-aligning properties.

As a first candidate, we consider *result carry-over rules*: rules specifying that local level surpluses/deficits are to be transferred to the next budget year. Note that we do not mean rules regarding whether deficits are at all *allowed* or not, which is a common use of the term (see Alt and Lowry (1994) for a discussion of this in relation to US states). In the context of countries and states, result carry-over rules in our sense of the term have been hypothesized to decrease fiscal performance (e.g. Alt and Lowry, 1994; von Hagen and Harden, 1996; Fabrizio and Mody, 2006), but there are several reasons why we think such rules restrain local level spending within municipalities. The reward (punishment) of forwarding a surplus (deficit) increases (decreases) the autonomy of the local level, as it implies greater (smaller) possibilities of allocating its resources as it sees fit over time.[17] A surplus carry-over rule reduces the local level's incentives to spend its entire budget each year, as unspent resources one year does not equal "wasted money" if it can be carried over to the next year's budget. The surplus rule moreover sends a signal of trust and thus of respect.[18]

The variables *keep surplus* and *keep deficit* indicate the presence of either carry-over rule. *Keep surplus* equals 1 if local committees/administrations carry over surpluses (wholly or partly) from one fiscal year to another, and 0 otherwise. *Keep deficit* equals 1 if local committees/administrations carry deficits over to subsequent fiscal years, and 0 otherwise.

A second way to punish non-complying committees and managers is to replace them.[19] For example, the municipal council has the authority to dissolve or reorganize a local committee, or change its responsibilities. Two dummy variables measure the risk of dismissal: *committee risk* and *manager risk*. To construct *committee risk*, we ask respondents whether a scenario of non-incidental and repeated deficits would constitute a sufficient reason to replace the members of the largest local committee.[20] A positive

[17] Wilson (1989, pp. 179-195) argues that public organizations often value autonomy as much as, or more than, additional resources.

[18] See e.g. Ellingsen and Johannesson (2007) and the references therein for how esteem and respect may align interests between principals and agents.

[19] Hallerberg and von Hagen (1999, p. 218) write that "the ultimate punishment is dismissal from office". Although they discuss spending ministers, it should be equally true for civil servants.

[20] The "largest" administration/committee refers to the one with the highest level of spending. As spending levels vary greatly among the different local committees/administrations in a municipality, there is substantial heterogeneity in their im-

answer implies a value of 1 on the variable, which otherwise is coded as 0. *Manager risk* is constructed in a slightly different way.[21] We first ask whether a scenario of non-incidental and repeated deficits would constitute a sufficient, or a conducive but not sufficient, reason to replace the manager of the largest local administration. Respondents who answer that such a situation could be a conducive but not a sufficient reason are presented with a similar scenario, with the modification that the administration has made efforts to reduce the deficit. *Manager risk* equals 1 for those municipalities who answered that either of the two scenarios would constitute a sufficient reason to replace the manager, and 0 for the others.[22]

Table 4.1 shows descriptive features of our candidate incentive-aligning institutions. Almost 50 percent of municipalities employ a surplus carry-over rule, while one out of three employs the corresponding rule for deficits. The correlation between the two rules is quite high, $\rho = 0.64$. It can moreover be noted that the regulations of surpluses and deficits have changed in 25 (surplus rule) and 28 percent (deficit rule) of the responding municipalities between the 2004 and 2010 surveys.

68 percent of the respondents state that systematic deficits increase the risk that a local committee will be replaced. The risk is even higher for local managers – 78 percent of the respondents indicate the presence of such a risk. The two institutions are moreover highly correlated, $\rho = 0.68$. As seen in the table, the questions making up *committee risk* and *manager risk* have relatively low response rates (66 and 72 percent, respectively). Non-respondents are significantly different from respondents in some respects; for example, they have better fiscal performance (see Appendix 4.B). There were no corresponding questions about risk of replacement in the 2004 survey.

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pacts on the overall fiscal performance, and it is therefore unlikely that all committees/administrations are treated similarly with respect to deficits/surpluses. We restrict attention to the largest committee as the question would be difficult to answer if framed in a more general way, due to the heterogeneity.

[21] We would have preferred to construct the two variables in this way, but to limit the number of survey questions, we specified *committee risk* – which we ex ante believed to be less effective – in a simpler way.

[22] Note that our survey is not a direct measure of q, L's beliefs about C's propensity to punish in the budget game, as the respondents are centrally placed administrators. It was simply not possible to send the survey to 290×number of local administrators.

Table 4.1: Incentive-aligning institutions

Variable	N	Mean	S.d.	Min	Max
keep surplus	255	0.45	0.50	0	1
keep deficit	256	0.33	0.47	0	1
committee risk	174	0.68	0.47	0	1
manager risk	191	0.78	0.42	0	1

4.4.3 Centralization of the budget process

We use three survey questions to measure the degree of centralization during the formulation phase of the budget process. The first asks whether the budget process is initiated by the executive committee or by the local committees. If the executive committee initiates the process, a follow-up question asks whether the local committees have large, limited, or no possibilities of proposing adjustments to the executive committee's budget proposal. The third question asks whether demographic factors and pre-set unit costs (e.g. schooling costs per pupil) govern the resource allocation to a large extent, to some extent or to a small extent. This question is an attempt to measure the size of the resources that are bargained over (if bargaining possibilities exist). The possibility of making budget proposals should make little difference for the local committees, if they only bargain over a negligible share of total resources.

We divide the municipalities into four categories, summarized in Table 4.2. Category 1, which refers to the highest degree of centralization, contains municipalities where the local committees hardly influence the budget process at all: where *a*) the central level initiates the process, there is no room for adjustment proposals and/or only a small share of total resources is bargained over; or *b*), the local level initiates the process but the scope for bargaining is small. In category 2 we put municipalities where the executive committee initiates the budget process, the local committees have some limited possibilities of making adjustment proposals, and there are some resources to be bargained over. Category 3 contains municipalities where *a*) local committees initiate the budget process and there are some resources to be bargained over; *b*) the executive committee initiates the budget process, there are large possibilities of making adjustment proposals, and there are some resources to be bargained over; or *c*) the executive committee initiates the budget process, there are some possibilities of making adjustment

Table 4.2: Classification of degrees of centralization

Budget initiation	Adjustment proposal	Scope for bargaining	Centralization category
Central	Large	Large	4
Central	Large	Some	3
Central	Large	Small	1
Central	Limited	Large	3
Central	Limited	Some	2
Central	Limited	Small	1
Central	None	Large	1
Central	None	Some	1
Central	None	Small	1
Local	N.A.	Large	4
Local	N.A.	Some	3
Local	N.A.	Small	1

Table 4.3: Distribution of centralization variable

Degree of centralization	Frequency	Percent
1 (Most centralized)	45	18
2	35	14
3	111	45
4 (Most decentralized)	58	23
Total	249	100

proposals, and a large share of total resources is bargained over. Category 4, the most decentralized category, contains municipalities where a large share of total resources is bargained over and either the local committees initiate the budget process, or the executive committee initiates the budget process but local committees have large possibilities of making adjustment proposals.

Table 4.3 shows how the municipalities are distributed over the four categories. Of the 249 responding municipalities, 18 percent are categorized as highly centralized, 23 percent are highly decentralized and 59 percent lie in between.[23]

[23] Our measure of centralization is not directly comparable to any measure in the 2004 survey. The first two questions are similar to those used to measure centralization in Tovmo (2007). Tovmo does not include any measure of the share of resources open to bargaining though.

4.4.4 Dependent variable

As our measure of fiscal performance, we use the per capita operating revenues net of costs (*net revenues*). This and all other economic variables are measured in 2010 prices. We focus on surpluses/deficits rather than balance sheet measures such as debt per capita or the equity ratio because our institutions are only indirectly connected to the balance sheet through the level of net revenues. Positive net revenues increases equity, and if net revenues are higher, the need to borrow is lower so there is a clear relationship also to the change in the debt level. Moreover, as stock measures, debts and equity ratios are heavily influenced by extraordinary historical events (e.g. sales of large public companies) and can thus be misleading in a cross-sectional setting. There are also differences in accounting practices, notably in regards to the accounting of pensions. In addition, the balanced budget law shows the lawmakers' focus on the revenues and costs statement, rather than the balance sheet (Brorström et al., 1999, pp. 54-61).

The distribution of net revenues in 2010 (Figure 4.1, Table 4.5) is centered around 1 360 SEK per capita (approximately 160 EUR). This is about 2.9 percent of average tax and grant revenues and may seem high, but recall from section 3.2 that surplus targets are the norm due to separate operating and capital budgets. Moreover, to dampen the consequences of the concurrent recession, the central government made extra transfers (proportional to population size) to all municipalities in 2009 and 2010; therefore, the recession did not have a large impact on revenues these years. As many municipalities had already decided on the budget when the announcement of the transfers came, revenues became higher than budgeted for many. Notably though, despite the balanced budget law, 6 percent of the municipalities ran deficits in 2010. This is somewhat below the average share running deficits during 2003-2009, which is 18 percent.

The measure of fiscal performance does not include so-called *extraordinary revenues/costs*.[24] This suits our purposes well, as we want to capture systematic components of the municipalities' fiscal performance, rather than large exogenous shocks. However, the chosen measure is certainly not an indisputable measure of fiscal performance. For instance, discretion over

[24] Note that the extra transfer from the central government is *not* counted as extraordinary. Generally, almost all revenues and costs are regarded as ordinary; extraordinary is reserved for e.g. natural disasters and sales of firms owned by the municipality (Council for Municipal Accounting, 2006).

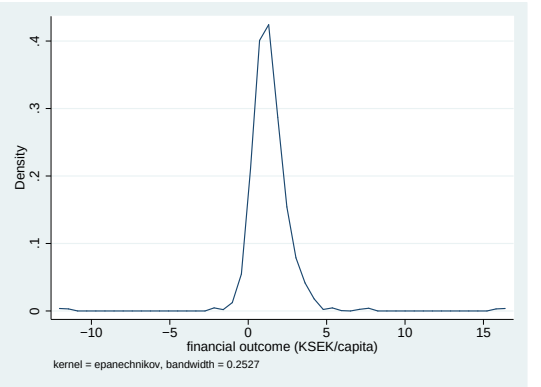
the timing of accounting for certain costs and revenues can be used to manipulate the reported figures to some extent, and there are reasons why municipalities might wish to do so: the balanced budget law creates incentives to avoid showing deficits and "too large" net revenues may cause unsustainable demands for spending and/or tax cuts. We therefore expect the distribution of the dependent variable to be "compressed" compared to what it would be if the municipalities had no discretion regarding the timing of accounting. As a robustness check, we also perform regressions with per capita operating costs (i.e. excluding financial costs) as the dependent variable, thus excluding some manipulable posts. In order not to classify municipalities that temporarily run deficits to reduce previous high surpluses as irresponsible, we moreover include the equity ratio and mean net revenues over the period 2000-2007 in the estimations (the chosen period corresponds approximately to the latest completed business cycle in Sweden).

A remaining drawback of our approach is that neither net revenues nor costs are unambiguous measures of "better" fiscal performance in a normative sense; municipalities are supposed to provide adequate services in a fiscally responsible way, not to maximize profits.[25] While these variables are indicative of fiscal performance, they need not be linearly related. One way to circumvent this problem would be to relate the actual net revenues of each municipality to the level specified in the budget (assuming that the budgeted level represents a fiscally sustainable level). By including the budgeted net revenue as a control variable, we do not completely avoid the problem that higher does not equal better, but we at least avoid comparing apples with oranges in terms of level of ambition.

We have manually collected information on the budgeted level of net revenues from the 2010 annual reports of almost all municipalities in our sample. The average difference between actual and budgeted level of net revenues is very large, 898 SEK per capita (66 percent of the average actual level of net revenues). This large difference likely reflects the extra transfers from the central government in 2010, which also suggests that many municipalities did not adjust their budgets after the transfer was announced.

[25] In short run analyses, this argument applies with even greater force to the debt level and the equity ratio (and changes in these). For example, investments in e.g. housing and roads increases the debt level and lowers the equity ratio, but it may of course be fiscally sound to invest in infrastructure.

Figure 4.1: Per capita net revenues 2010



The difference between budget and outcome may also reflect factors such as bad forecasting, caution, or a desire to surpass expectations, and may therefore not be strongly related to a fiscally sustainable level. It is moreover conceivable that the budgeted level depends on what is deemed feasible given the set of budget institutions and the degree of conflict of interests. Because of these issues, we do not include the budgeted level in our baseline specifications, though a specification including this variable is available in Appendix 4.D.

4.4.5 Control variables

Following e.g. Tovmo (2007) and Krogstrup and Wälti (2008), we acknowledge that some municipalities may be more likely to employ budget institutions than others. The carry-over rules are more common and the budget process more centralized the larger, richer (in terms of personal income), younger, better educated and more right-wing the population (significant at 10 percent level in t-tests).[26] Although these municipalities have a relatively strong income tax base, their per capita total municipal revenues are significantly lower. This is explained by the intergovernmental equalization system, which favours municipalities with smaller income tax bases and unfavourable demography. Nonetheless, the municipalities using carry-over rules and/or a centralized budget process have higher equity ratios than

[26] As the education level is highly collinear to the population size, we do not include the education level among the control variables.

other municipalities. The prevalence of *manager risk* and *committee risk* is higher in the same type of municipality, although there are no statistically significant differences in the share of right-wing voters, and the differences with regard to economic or political control variables are smaller and often statistically insignificant.

As these background variables are also likely to be correlated with the realized level of net revenues, we control for them in the analysis. Definitions of these control variables, as well as some structural controls, are found in Table 4.4 and Table 4.5 shows descriptive statistics for 2010.

Because of high collinearity, we cannot simultaneously include *total revenues* and *income tax base* in the analysis. In the choice between the two, we settle for the former, which makes our empirical model come closer to the theoretical model. One may argue that revenues, in contrast to the income tax base, are endogenously determined. However, almost all discretionary parameters (i.e. tax and fee rates) are fixed in the budget before the start of the fiscal year; during the fiscal year, local committees/administrations have little influence over revenues and mainly affect fiscal performance through their spending decisions.[27]

We also include *fixed asset revenues* and *financial costs* as control variables, not because we believe that these are related to the institutional structure, but to reduce variation in the dependent variable, which stems largely from rare events that do not say much about fiscal performance.

Importantly, the five budget institutions are more common in municipalities where the *long-term budget* is viewed as important according to the survey.[28] As *long-term budget* is a plausible proxy for central level fiscal motivation, we include it in the empirical specification to partly deal with the problem that budget institutions and net revenues may be simultaneously determined by such motivation (c.f. proposition 4 and Bohn and Inman, 1996; Eslava, 2011). Notably, *long-term budget* is the only significant variable in a similar analysis performed in Dahlberg et al. (2005).[29] As *mean fiscal performance 00-07* and *equity ratio* too reflect the central level's fiscal

[27] Revenue from income taxes make up approximately 65 percent of total municipal revenues; fees (21 percent), and government grants (12 percent) are the other two main sources of revenue (Statistics Sweden, 2011).

[28] Contrary to what its name suggests, this variable does not indicate whether the municipality employs long-term budgeting or not; all municipalities are obliged to.

[29] The survey question is a translation of an item in von Hagen's 1991 survey (von Hagen and Harden, 1995; de Haan et al., 1999).

Table 4.4: Description of control variables

Variable	Type	Description
total revenues	Economic	Per capita total revenues (KSEK)
relative change in total revenues	Economic	Change, tot. rev. between t and $t-1$ (%)
fixed asset revenues	Economic	Realization of fixed assets (% of tot. rev.)
financial costs	Economic	Per capita interest, asset write-downs etc
equity ratio	Economic	Private equity/total assets in $t-1$
mean net revenues 00-07	Economic	Mean net revenues 2000-2007 (KSEK)
share right-wing parties	Political	Right-wing seats in municipal council (%)
herfindahl	Political	$h = \sum_i$ (vote share of party i)^2
long-term budget	Political	LTB viewed as important
population	Demographic	Population (log)
population 20-79	Demographic	Population share in ages 20-79 (%)
cities	Structural	Dummy for larger cities
rural	Structural	Rural location
suburb/commuter/ manufacturing	Structural	Municipality either suburban, or large share commuters/manufacturing industries

Table 4.5: Descriptive statistics, dependent and control variables in 2010

Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**N**
*net revenues**	1.36	1.62	-11.78	16.13	265
*total revenues**	59.63	7.42	43.23	88.41	265
relative change in total revenues	2.32	2.82	-9.44	31.17	265
*fixed asset revenues**	2.34	8.61	0	90.0	265
*financial costs**	0.41	0.58	-0.09	6.32	265
equity ratio	50.08	17.72	-13.69	81.83	265
*mean fiscal performance 00-07**	0.53	0.53	-1.29	2.58	265
share right-wing parties	44.92	11.64	6.45	88.89	265
herfindahl	0.25	0.05	0.16	0.43	265
long-term budget	0.53	0.5	0	1	257
population (log)	9.87	0.95	7.81	13.65	265
population 20-79	70.80	1.62	64.44	77.17	265
cities	0.11	0.31	0	1	265
rural	0.13	0.34	0	1	265
suburb/commuter/manufacturing	0.41	0.49	0	1	265

*KSEK per capita

All data from 2010, except for *equity ratio* (2009).

motivation, the inclusion of these variables also addresses this omitted variables problem to some extent. We use the value in 2009 for *equity ratio*, as the ratio in 2010 year is directly affected by the net revenues the same year. It can lastly be noted that the variable capturing conflicts of interests, *ci*, shows no strong pairwise correlation to the mentioned background variables (although Ellegård (2013) shows that conflicts are slightly more common in smaller municipalities). We discuss issues of endogeneity and identification more in-depth in section 4.7.1.

4.5 Empirical strategy

We first explore whether any of the candidate incentive-aligning institutions (*keep surplus, keep deficit, manager risk* and *committee risk*) and/or *centralization* correlate positively to fiscal performance. All institutions are included in one regression, in order not to confound their effects.[30] Recalling that the expected positive effect of incentive-aligning institutions on fiscal performance depends on the degree of conflicts of interest, we interact each institution with the indicator for a substantial conflict of interest over fiscal discipline (*ci*). We thus estimate

$$\textit{fiscal performance}_i = \alpha + \mathbf{institution}_i'\boldsymbol{\beta}_0 + (ci_i \times \mathbf{institution}_i)'\boldsymbol{\beta}_1 + \beta_2 ci_i + \mathbf{x}_i'\boldsymbol{\gamma} + \varepsilon_i \tag{4.1}$$

where **institution** is a 5×1 vector including the four potentially incentive-aligning institutions and the centralization variable, $\mathbf{x}$ is the vector of control variables, and ε is a random error term. $\boldsymbol{\beta}_0$ and $\boldsymbol{\beta}_1$ are 5×1 vectors of parameters for the institutional variables and their interactions with *ci*. In the following, we refer to $\beta_n^j, n = 0, 1$ and $j = ks, kd, mr, cr, c$, when discussing the parameter estimates for *keep surplus, keep deficit, manager risk, committee risk* and *centralization*, respectively.

The theoretical framework suggests that the effectiveness of centralization depends on the incentive-aligning institutions and vice versa. After having estimated Equation 4.1 to find good candidates for effective incentive-aligning institutions, we next partition the municipalities into groups to

[30] As previously noted, non-response is relatively high for the two *risk* variables. Instead of dropping these observations – and thereby lose efficiency in the estimation of the effect of the carry-over rules – we include dummies for non-response to the *risk* questions.

explore whether municipalities that employ *both* centralization and effective incentive-aligning institutions perform better than municipalities that employ only centralization, only incentive-alignment, or neither.

There are some influential observations (Cook's distance $> 4/n$), typically characterized by extreme values in terms of *net revenues*. By investigating the annual financial report of each outlier, we detect whether their extreme outcomes can be explained by rare events and/or book-keeping technicalities. As this is not the kind of behavior we seek to explain, we estimate each model twice: first including and then excluding the outliers whose extreme outcomes can be explained by such factors (3 in 2010). Note however that rare events and book-keeping technicalities can be hidden behind the more "modest" fiscal performances of other municipalities as well. That is, the real basis for exclusion of the outliers is not the extreme events in themselves, but the fact that they result in overall extreme outcomes. Thus, the sample excluding the outliers is not unambiguously a more valid basis for conclusions.

4.6 Results

4.6.1 Institutions and fiscal performance

Table 4.6 shows our baseline estimates. To illustrate how the level of conflict (ci) influences the estimates of the institutions, the first two columns report the estimates from a model without interaction terms between ci and the institutional variables, while the last two columns show estimates for the interaction model specified in Equation 4.1.[31] The three outliers are included in the estimations reported in odd-numbered columns and excluded from the estimations reported in even-numbered columns.

It can first be noted that municipalities with substantial conflicts of interests have somewhat lower net revenues than municipalities with smaller conflicts. Though the estimates from the model without interactions are small and statistically insignificant, the coefficient of ci in the interaction specifications indicates that net revenues are about 500 SEK per capita lower

[31] See Appendix Table 4.C.1 for control variables. The indicators for non-response to the risk questions are left out of the table, as the coefficients for non-respondents are insignificant at conventional levels in most specifications and have no meaningful interpretation.

Table 4.6: Baseline results, OLS on 2010 sample

	(1)	(2)	(3)	(4)
Variables		Ex. outliers		Ex. outliers
keep surplus (ks)	0.463**	0.168	0.249	-0.063
	(0.195)	(0.164)	(0.280)	(0.236)
ci×keep surplus			0.353	0.332
			(0.341)	(0.306)
keep deficit (kd)	0.240	0.266	0.481*	0.602**
	(0.188)	(0.169)	(0.290)	(0.252)
ci×keep deficit			-0.390	-0.545
			(0.374)	(0.332)
manager risk (mr)	0.424*	0.407*	0.243	0.349
	(0.253)	(0.225)	(0.398)	(0.345)
ci×manager risk			0.357	0.105
			(0.406)	(0.363)
committee risk (cr)	-0.005	-0.113	0.012	-0.164
	(0.258)	(0.220)	(0.397)	(0.346)
ci×committee risk			-0.037	0.101
			(0.407)	(0.360)
cent123 (c)	0.194	0.220	0.054	-0.021
	(0.164)	(0.157)	(0.240)	(0.233)
ci×cent123			0.225	0.410
			(0.339)	(0.313)
ci	-0.118	-0.110	-0.507*	-0.499*
	(0.138)	(0.124)	(0.289)	(0.285)
Constant	-6.382*	-6.427**	-6.227*	-6.536**
	(3.598)	(3.197)	(3.615)	(3.248)
Incremental effects of institutions for municipalities where $ci = 1$				
$\beta_0^{ks} + \beta_1^{ks}$			0.602**	0.269
$\beta_0^{kd} + \beta_1^{kd}$			0.091	0.058
$\beta_0^{mr} + \beta_1^{mr}$			0.599**	0.454*
$\beta_0^{cr} + \beta_1^{cr}$			-0.025	-0.063
$\beta_0^{c} + \beta_1^{c}$			0.278	0.389*
Controls	Yes	Yes	Yes	Yes
Observations	225	222	225	222
R^2	0.567	0.256	0.574	0.275
F	4.300	3.710	3.661	3.294

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
See Appendix Table 4.C.1 for control variable estimates.
The number of observations is lower than 265 due to non-response to individual survey questions.

in municipalities with a substantial conflict of interest and weak institutions. This is a large difference in relation to the average net revenues (1 360 SEK). Importantly, the result indicates that the conflicts of interests measured by the survey do decrease fiscal performance.

The question is whether budget institutions are helpful in closing the gap. The estimates give some support to the idea that the reward for being able to carry surpluses over to the next fiscal year promotes fiscal performance, as the coefficient on *keep surplus (ks)* is positive and significant in column (1). Considering instead the interaction specification in (3), we note that the positive and significant incremental effect derives from the group of municipalities that report substantial conflicts of interest. That is, β_0^{ks} is not significantly different from zero but $\beta_0^{ks}+\beta_1^{ks} > 0$ (see lower part of table). This is in line with our expectation that the institution should only make a difference where there is a problem to be solved. However, columns (2) and (4) reveal that the magnitude as well as the statistical significance is partly driven by the three outliers. This calls for some caution in drawing conclusions regarding the surplus rule – though the results in Section 4.6.2 below seem to indicate that there is more than an outlier effect.

We find no significance of the corresponding carry-over rule for deficits (*keep deficit, kd*) in the specifications without interactions. The interaction specifications entail a challenge for our theoretical framework, as we find a reversed result from what we expect: *keep deficit* is significantly and positively related to fiscal performance (with large magnitude, 480-600 SEK per capita), but not in municipalities with a substantial conflict of interest. For them, the incremental effect ($\beta_0^{kd} + \beta_1^{kd}$) is not distinguishable from zero in any specification. One possible interpretation is that where there are major conflicts, the employment of a punishment rule sends out a signal that reinforces the conflict and increases non-compliance, while where conflicts are small, the rule simply serves its restraining purpose.

manager risk (mr) is strongly and positively related to net revenues according to the specification without interactions in columns (1) and (2). Moving to the interaction specification reveals that the results for *manager risk* are in line with the theoretical framework: the incremental effect is insignificant for municipalities that do not report substantial conflicts of interests ($\beta_0^{mr} = 0$), but it is positively related to the fiscal performance of municipalities with a substantial conflict to be resolved ($\beta_0^{mr} + \beta_1^{mr} > 0$). A credible threat that local managers who misbehave will be replaced is

associated with approximately 450-600 SEK higher per capita net revenues, which implies substantial economic significance.

By contrast, none of the estimations suggest that *committee risk (cr)* is useful, as the coefficient for this variable is insignificant in all specifications. Thus, we find no support for the idea that the threat of replacement deters politicians of local boards from overspending. Multicollinearity with *manager risk* may cause the insignificance, but we also see another plausible explanation: the risk is connected to more severe consequences for the managers, for whom the risk applies to their main occupation, than to local politicians, who usually only have part time commissions or devote leisure time to politics.

The *centralization* variable enter the regressions as a dummy variable with category 4 as reference. This decision is guided by a Wald test on a model including categories 1, 2 and 3 separately, which suggests that their coefficients are indistinguishable from each other.[32] The coefficient of *cent123 (c)* is insignificant in all samples, so, according to expectations, there is no evidence that a centralized process influences outcomes in municipalities where central and local levels agree on the importance of fiscal discipline. However, the incremental effect of centralization is positive and significant for municipalities that report substantial conflicts of interest in the sample excluding outliers ($\beta_0^c + \beta_1^c > 0$; the coefficients imply 275-390 SEK per capita higher net revenues). Thus, although not as clear as in other studies, we do find indications of a beneficial effect of centralization in circumstances where it should make a difference.

Of the control variables (Appendix Table 4.C.1), we restrict our discussion to the political variables and the emphasis put on long term budgets, as these factors are most related to the institutional variables and the previous literature. Neither the *share of right-wing parties* nor the fragmentation of the municipal council (*herfindahl*) are significant in any specification. The negative coefficient of *herfindahl* contrasts with the theoretical predictions of fragmentation (Tovmo, 2007; Eslava, 2011); still, its statistical insignificance suggests that our focus on the interaction between central and local levels is more important for performance than the composition of the council. The importance assigned to the *long-term budget* is significantly associated with higher fiscal performance, just as found in the 2004 data by Dahlberg et al. (2005). A reasonable interpretation is that the variable captures the

[32] In Appendix 4.D we examine a less broad classification.

emphasis on fiscal discipline at the central level.

In Appendix 4.D we examine the robustness of the baseline results to 1) using *costs of services per capita* as the dependent variable; 2) removing and adding control variables; 3) using alternative, less endogenous, revenue measures; 4) other categorizations of the centralization variable and 5) including budgeted net revenues as control variable. We also run a first-difference regression for the carry-over rules, on which we have information for 2004. In sum, the baseline results are rather robust; the key issue seems to be that the results for *keep surplus* and *cent123* are sensitive to outliers. We conclude that all institutional variables except *committee risk* may be beneficial for net revenues. In the next section, we examine the theoretical suggestion that fiscal performance is even higher when centralization and incentive-aligning institutions are combined.

4.6.2 Combinations of budget institutions

The previous estimations suggest that *keep surplus, manager risk* and *cent123* are important for municipalities where there is a conflict of interest (ci=1), and that *keep deficit* is important for the group where ci=0, whereas *cent123* is mostly positive, but insignificant for this group. To examine whether municipalities that employ combinations of good institutions (according to the above results) are relatively well-performing, we partition the municipalities into four groups – A, B, C and D – as follows. Groups A (76 municipalities) and D (24 municipalities) contain the municipalities where ci=1; group A consists of those who also employ both *cent123* and either of *keep surplus* and *manager risk* (or both), and group D consists of the complementary subset that employs at most one type of institution (centralization or incentive-aligning). Similarly, groups B (24 municipalities) and C (50 municipalities) contains the municipalities where ci=0; group B consists of those who employ both *cent123* and *keep deficit*, while group C consists of those who employ at most one of these two institutions. As before, odd-numbered columns report estimates for the sample including the three outliers and even-numbered columns report estimates excluding these municipalities.

Using group D as the reference category, columns (1) and (2) of Table 4.7 show that well-performing municipalities are overrepresented in the groups

that combine several beneficial institutions (A and B).[33] For example, the coefficient on A is positive and significant both statistically and economically (the coefficients imply 450-560 SEK per capita higher net revenues). Similarly, the coefficient on group B is positive and larger than the coefficient on group C (and larger than the reference group D) with magnitudes of 670-730 SEK per capita. The difference between B and C is significant at the five percent level according to Wald tests (shown in the lower part of the table).

It would certainly be interesting to also examine whether centralization, for example, is more effective in the presence of certain incentive-aligning institutions. We are unable to address this question for the combination of carry-over rules and centralization, as almost all municipalities that employ a carry-over rule also have a centralized budget process.[34] In addition to preventing us from estimating a meaningful interaction model, this implies that the baseline estimates for the carry-over rules (Table 4.6) by and large capture their influence conditional the budget process being relatively centralized.

Collinearity with centralization is less of an issue for *manager risk*. In a specification with the *manager risk* interacted with centralization (results available on request), *manager risk* is positive and significant regardless of whether centralization is employed or not, but the interaction of *manager risk* and *cent123* is never significant. Thus, while the risk of replacement seems influential in itself, it neither affects nor is affected by centralization.

To see whether the results for group A are entirely driven by *manager risk*, we create a new group a, which contains the municipalities that report a substantial conflict of interest ($ci = 1$) and also employ both *cent123* and *keep surplus*. The results in column (3) and (4) of Table 4.7, where groups B and C are kept the same and we include *manager risk* as a control variable, show that these fears are unwarranted. The coefficient on group a is positive,

[33] The number of observations is decreases because the partition implies that we cannot include non-respondents to the *manager risk*-question.

[34] For instance, only 7 municipalities employ *keep surplus*, have a substantial conflict of interest ($ci = 1$) *and* are centralized to the lowest degree (*cent123*$= 0$), and only 3 municipalities that use *keep deficit* have small conflicts of interests ($ci = 0$) *and* lack a centralized process. The high prevalence of municipalities that combine result carry-over rules and centralization is not surprising from the point of view of our theoretical results. If the game approximates the municipal budget process and these institutions are effective, the central level of municipalities should be expected to employ both.

Table 4.7: Combinations of institutions, 2010 sample

	(1)	(2) Excl outliers	(3)	(4) Excl outliers
Group A	0.566**	0.458**		
	(0.250)	(0.223)		
Group B	0.741**	0.680**	0.693**	0.565**
	(0.290)	(0.269)	(0.302)	(0.250)
Group C	0.222	0.212	0.159	0.0948
	(0.231)	(0.220)	(0.204)	(0.185)
Group a			0.727***	0.440**
			(0.242)	(0.214)
manager risk			0.432**	0.326*
			(0.175)	(0.169)
Constant	-6.618	-8.478**	-7.063	-8.416**
	(4.937)	(4.254)	(4.656)	(4.179)
Test B≠C	p=0.040	p= 0.035	p= 0.049	p= 0.044
Control variables	Yes	Yes	Yes	Yes
Observations	174	171	174	171
R^2	0.620	0.334	0.641	0.348
F	7.682	6.803	8.208	6.958

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
Group *A*: *ci*=1, *cent123*=1 and either *keep surplus*=1.
or *manager risk*=1, or both.
Group *B*: *ci*=0, *cent123*=1 and *keep deficit*=1.
Group *C*: *ci*=0, at most one of *cent123* and *keep deficit* = 1.
Group *a*: *ci*=1, *cent123*=1 and *keep surplus*=1.
Group *D*: reference category.

significant, and of comparable size to the coefficient on group *A*. Notably, the estimates for group *a* strengthen our belief in the importance of the surplus rule and centralization, especially since the coefficient is significant regardless of whether outliers are included or not.

4.7 Discussion and conclusions

4.7.1 Causality and identification

In her review, Eslava (2011) mentions several shortcomings of the empirical literature on political and institutional determinants of fiscal performance. In short, due to reverse causality and omitted variables, most studies fail to discriminate between competing explanations for observed phenomena. How does this study fare in these dimensions?

In our view, reverse causality from fiscal performance to institutions is not very plausible in our setting: budget institutions are unlikely to be reformed very often and we control for previous fiscal performance by several variables. We moreover argue that reverse causality mainly would serve to strengthen our case. In their search for ways to reduce deficits, high-deficit municipalities should be more likely to experiment with the institutional structure, while low-deficit municipalities have no reasons to rock the boat.[35] According to this argument, deficit-prone municipalities are over-represented in the pool of observations with "good" institutions, thus contributing *negatively* to the correlation between our conjecturally good institutions and fiscal performance. However, the opposite case can also been made; in particular, Fabrizio and Mody (2010) find that countries with higher deficits are less likely to reform their budget institutions, and argue that a war of attrition between different policy fields impedes institutional reforms. It can be noted from Table 4.8 below that the raw correlations between our institutions and the measures of previous fiscal performance – *mean net revenues 00-07* and *equity ratio* – are positive (though only significantly so for the surplus rules). In any case, since we control for exactly these variables in the analysis and institutions infrequently change, reverse causality is no prominent ground for concern.

The same control variables also decrease the risk of reverse causality from performance to *ci*, which otherwise may be suspected to reflect respondents' explanations for observed unsatisfying fiscal performance. But notably, even if the negative correlation between *ci* and fiscal performance is due to reverse causality, the correlation is evidently weaker in municipalities that employ

[35] Alesina and Perotti (1999) argue that as institutions are costly to change, they have to be unsatisfactory to be changed. Alt and Lassen (2006) and de Haan et al. (1999) also note that fiscal crises often precede institutional reform.

some of the budget institutions.

A more relevant concern is that the budget institutions may proxy for omitted factors that affect both fiscal performance and the institutional structure. Voter preferences over fiscal discipline is one often mentioned factor (e.g. Poterba, 1996; Krogstrup and Wälti, 2008; Eslava, 2011); for instance, fiscally responsible politicians may implement balanced budget rules to win the votes of fiscally conservative voters. There are three reasons to believe that voter preferences are sufficiently taken into account in our estimations: *first*, we control for voter preferences to some extent through the variable *share of right-wing parties*; *second*, voters' preferences for fiscal discipline are likely correlated to the equity ratio (a long-run measure) rather than to the yearly fiscal performance, and the equity ratio is included as a control variable; *third*, the details of governance captured by our institutions are unlikely to buy many votes; for instance, we suspect that few voters know whether their municipality employs result carry-over rules.[36]

The transparency of the budget process is another much-discussed factor (Alt and Lassen, 2006; Eslava, 2011). For politicians, a transparent budget process increases the risk of being punished at the polls due to fiscally irresponsible behaviour (Eslava, 2011). Budget transparency also relates to the institutional structure, specifically to centralization; more transparency may make the central level more adherent to local level budget proposals, as information about deviations from popular proposals becomes more widespread. However, as all budget documents must be made publicly available, there are national standards for municipal accounting and almost all municipalities publish their annual reports on their websites, there are reasons to believe that the between-municipality variation in transparency is low.

In our view, insufficient control for the central level's fiscal motivation is the key impediment to a causal interpretation of our results. It is conceivable – although far from indisputable – that conflicts of interests are more likely if the central level is relatively prudent. Fiscal motivation is moreover likely to be positively related to the achieved level of net revenues *and*

[36] With regard to other features of the political landscape, it can be noted that the within-country setting rules out any confounding of the influence of budget institutions with the influence of the electoral system (Eslava, 2011), and that the *herfindahl* variable accounts for confounding effects of political fragmentation (Hallerberg and von Hagen, 1999).

to the propensity to use budget institutions that are believed to improve fiscal performance. For sure, it is difficult to explain why fiscally successful municipalities would bother to use these institutions if unmeasured fiscal motivation accounts for *all* of the positive association between institutions and performance – using ineffective rules seems rather pointless (especially if they are costly to implement). Moreover, the problem should be somewhat dampened by the inclusion of *equity ratio*, the *mean net revenues* 2000-2007 and *long-term budget* – all of which can be thought of as proxies for fiscal motivation (these proxies are indeed correlated to net revenues as well as the institutional structure, see Table 4.8). Nevertheless, we cannot rule out the possibility that we fail to exhaustively control for fiscal motivation, and we thus abstain from making causal claims.

In fact, the estimated significance of *manager risk* cannot be explained without making reference to central level fiscal motivation – for *manager risk* to be greater than zero, it is *necessary* that someone with the authority to replace managers is concerned about fiscal performance. The case for *manager risk* having an effect in itself is nevertheless rather strong; facing a conflict, the central level needs to apply some incentive-aligning measure in order to enforce the budget, and replacement of non-complying agents seems like a plausible choice.

This omitted variables problem is shared by most of the related literature. More generally, central level fiscal motivation is intrinsically connected to the enforceability of budget institutions. Thus, credible causal claims are more likely to be possible in settings with super-imposed budget institutions (e.g. fiscal rules imposed by the central government, as in Grembi et al. (2012) and Foremny (2011)), than in settings like ours where local governments themselves choose institutions.[37]

[37] Note that the few studies finding positive correlations between institutions and fiscal performance when using fixed effects, e.g. Fabrizio and Mody (2006), do not fully circumvent the omitted variables problem. Since politicians and party majorities change over time, it is quite likely that fiscal motivation is not fully captured by the fixed effects. Attempts to correct the problem using lags of the institutional structure as instrumental variables (Debrun et al., 2008; Hallerberg et al., 2007) rest on the assumption that fiscal motivation show no persistence at all. See Acemoglu (2005) for an enlightening discussion of the feasibility of IV in the analysis of institutions.

Table 4.8: Correlations for *ci*, institutions and proxies for central level fiscal motivation

Variables	*ci*	*keep surp.*	*keep def.*	*man. risk*	*cent123*	*mean...00-07*	*equity ratio*	*long-term b.*
ci	1.000							
keep surplus	0.120	1.000						
	(0.065)							
keep deficit	0.005	0.644	1.000					
	(0.936)	(0.000)						
manager risk	0.084	0.098	0.082	1.000				
	(0.197)	(0.118)	(0.193)					
cent123	-0.008	0.271	0.125	0.155	1.000			
	(0.900)	(0.000)	(0.049)	(0.014)				
mean...00-07	-0.012	0.215	0.200	0.094	0.078	1.000		
	(0.853)	(0.001)	(0.001)	(0.125)	(0.222)			
equity ratio	0.002	0.213	0.078	-0.037	0.003	0.473	1.000	
	(0.975)	(0.001)	(0.214)	(0.546)	(0.967)	(0.000)		
long-term budget	0.054	0.157	0.126	0.105	0.086	0.151	0.043	1.000
	(0.412)	(0.013)	(0.045)	(0.092)	(0.178)	(0.015)	(0.495)	

p-values in parentheses.

4.7.2 Concluding remarks

Our estimations underline the importance of controlling for conflicts of interest between central and local levels, as the relationship between budget institutions and fiscal performance depends on the degree of such conflicts. For instance, the positive correlation between a centralized budget process and the level of net revenues is concealed when we do not take into account our measure of conflicts of interest between central and local levels.

Apart from centralization, our analysis points out other specific institutions that may improve fiscal performance. As one of few studies examining carry-over rules individually, rather than as part of an index, we find that total net revenues are higher if the local committees are allowed to carry over surpluses between fiscal years. The detected correlation is not entirely robust though and requires further investigation. A natural next step would be to relate the rule to the outcomes of actual local committees, for whom the rule is more likely to be exogenous. We also find that systematic carry-over of deficits correlates positively to fiscal performance, though only in municipalities that report small conflicts of interest. While the data does not allow us to conclude that the carry-over rules are also influential in the absence of a centralized budget process, it should be noted that municipalities combining carry-over rules with centralization have higher net revenues than municipalities employing centralization only. Furthermore, it is interesting to note that our findings run counter to the argument that carry-over rules weaken fiscal performance, which has been put forward in studies of European countries and US states (e.g. Alt and Lowry, 1994; von Hagen and Harden, 1996; Fabrizio and Mody, 2006).

Net revenues are higher in municipalities where managers face a relatively high risk of dismissal as a consequence of budget deficits. Though this is an informal institution, its implementation goes hand in hand with a strong commitment to fiscal discipline at the central level. This result also has interesting policy implications for the national government. For instance, to alleviate soft budget constraint problems (e.g. Kornai, 1979), the government may condition grants and bailouts on a strict treatment of local managers in the face of repeated deficits.

Like most researchers in this area, we cannot make convincing causality claims due to the possible endogeneity of budget institutions. Nonetheless, the results clearly suggest that conflicts of interests, as well as centralization

and incentive-aligning institutions, ought to be considered when examining the causes of variability in fiscal performance.

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4.A Proofs of propositions

Proposition 1: (i) $qp = 0 \Rightarrow x^e = x_L^*$ (where x^e is the equilibrium level of spending), (ii) $x^e \in [x_C^*, x_L^*]$, (iii) If $x_L^* = x_C^*$, then $x^e = x_L^* = x_C^*$.

Proof $qp = 0 \Rightarrow E(U_L) = u_L$ and by definition, x_L^* solves $\max u_L(x)$. This proves (i). To prove part (ii), we have to show that each of the proposed bounds is a feasible realized level of spending and that it is a bound. Suppose L chooses $x' > x_L^*$. Then, $u_L(x') - q(x')p > u_L(x_L^*) - q(x_L^*)p$, which implies that $p(q(x') - q(x_L^*)) < 0$ as $u_L(x') - u_L(x_L^*) < 0$ by definition. Consequently, $q(x') < q(x_L^*)$. In turn, this implies that $|b - x'| < |b - x_L^*|$ and that $b > x_L^*$. As $x' > x_L^* \geq x_C^*$ and thus $u_C(x') < u_C(x_L^*) \leq u_C(x_C^*)$, for C to choose $b > x_L^*$, it must hold that $u_C(x') > u_C(x_L^*) - h$ and therefore that $b_L > x_L^*$. But this is a contradiction to x_L^* being L's optimal level, as if h is large enough to make C set $b = b_L > x_L^*$, then $b'_L = b' = x_L^*$ is also feasible as well as preferred by L. Thus, x_L^* is the upper bound.

For the lower bound: suppose L chooses $x' < x_C^*$. Then, $q(x') < q(x_C^*)$ because $u_L(x_C^*) > u_L(x')$ as $x' < x_C^* \leq x_L^*$; which implies that $|b - x'| < |b - x_C^*| \Rightarrow b < x_C^* \Rightarrow u_C(x') > u_C(x_C^*) - h \Rightarrow b_L < x_C^*$. But this is a contradiction because if p is large enough to make L choose x', then L can set $b_L = x_C^*$ and guarantee a higher payoff, as C then sets $b = b_L$. Finally, it follows from the players' utility functions that x_C^* is feasible for $h = 0$ and a sufficiently large qp, such that $u_L(x_C^*) - u_L(x) > p(q(x_C^*) - q(x))\forall x > x_C^*$. (iii) follows directly from (ii).■

Proposition 2: Suppose $x_L^* > x_C^*$ and $h > 0$. Then, (i) $x^e > x_C^*$ for any $qp \geq 0$; and (ii) for any $qp > 0$, $x^e \in (x_C^*, x_L^*)$ strictly increases in h.

Proof First note that when $qp = 0$, (i) holds by part (i) of proposition 1.

The following proves the proposition for $qp > 0$. At step 3, L chooses the x that solves $\frac{\partial u_L}{\partial x} = \frac{\partial q}{\partial x}p$, unless $\frac{\partial q}{\partial x}p > \frac{\partial u_L}{\partial x}$ at $x = b$, in which case L chooses $x = b$. As u_L, p and $q(x)$ are common knowledge, C knows the threshold level of spending x' for which L rather complies to a budget $b' = x'$ than chooses another level of spending:

$$u_L(x') \geq u_L(x) - q(x)p \quad \forall x \neq b' = x' \tag{4.2}$$

L complies to any budget $b > x'$; $b' = x'$ is simply the lowest budget that C can enforce. It is easy to see that Proposition 2*i)* holds in case $x' > x^*_C$. For the case where $x' \leq x^*_C$, we must show that $h > 0$ implies that C will propose a budget $b > x^*_C$ *despite* that C is able to enforce a smaller budget in this case.

As $h > 0$ and $u_C(x)$ is continuous, there are $x'' \in (x^*_C, x^*_L]$ such that

$$u_C(x'') > u_C(x^*_C) - h. \tag{4.3}$$

As $u_C(x)$ is common knowledge, L can identify the largest proposal $\overline{b_L} = \overline{x''}$ from which C will not deviate at step 2. By setting $b_L = \overline{b_L}$, L will make C set the final budget to $b'' = \overline{b_L} = \overline{x''} > x^*_C$. As $\overline{x''} > x^*_C \geq x'$, L will comply to this budget at Step 3. Thus, $x^e > x^*_C$ also in this case. Finally, (ii) follows from C's utility function: larger h implies that (4.3) holds for larger x''.■

Proposition 3: For any $h \geq 0$, (i) there is a lowest feasible level of spending $\underline{x} \in [x^*_C, x^*_L]$ and (ii) $x^e \in (\underline{x}, x^*_L]$ strictly decreases in q and p.

Proof By proposition 1, $x^e \in [x^*_C, x^*_L]$, so any feasible level of spending belongs to this interval. The concavity of u_C together with the inequality in equation (4.3) in the proof of proposition 2 shows that there is a lowest feasible level that depends on h. (ii) follows from $E(U_L)$ being decreasing in qp.■

Proposition 4: For given x^*_C and $h > 0$, (i) x^e is non-decreasing in x^*_L; and (ii) if the strength (p) and credibility (q) of the incentive-aligning institution is sufficiently weak, then x^e is strictly increasing in x^*_L.

Proof First, note that since $u_L(x)$ has a single optimum, equation (4.2) holds for larger x' if x^*_L increases. In words, the minimal budget to which

C can make L comply increases when L's bliss point moves further away from C's bliss point. Whether this affects the equilibrium level of spending x^e or not depends on whether x'' in equation (4.3), the maximal budget proposal b_L from which C will not deviate, is larger than x' or not. As long as $x'' \geq x'$, the logic behind the optimality of $b_L = x'', b = x''$, and $x = x''$ explained in the proof of proposition 2 holds. Thus, the equilibrium level of spending is neither increasing nor decreasing in x_L^* when $x'' \geq x'$. To prove (ii), note that $x' > x''$ implies that L's bargaining power is to weak to make C set a larger budget than x'. Also, for all $b < x'$, L would choose $x > x'$ by equation (4.2), which is worse for C. Thus, it is optimal for L propose $b_L = x'$ and for C to choose $b = x'$ in step 2, as then no cost h is incurred for C and L does not get punished for choosing $x = x'$ (this assumes that $x' \geq x_C^*$, which is true because $x'' > x_C^*$ when $h > 0$ as shown in proposition 2). Thus, $x' > x'' \Rightarrow x^e = x'$, which is increasing in x_L^* by equation (4.2).■

4.B Analysis of response rates

Many municipalities replied to some, but not all, of the survey questions. Table 4.B.1 summarizes the response rates for the central survey questions.[38] Regarding the carry-over rules and centralization, we do not consider the levels of non-response to be a problem. For *manager risk*, *committee risk*, and *ci*, which have lower response rates, we perform a series of Wilcoxon rank sum tests with respect to the independent variables in the baseline estimations. The rank sum tests compare those that responded to the specific survey question to those that did not respond to this question, but have responded to other questions. Applying 10 percent as the significance level yields the following results: 1) There are no significant differences between respondents and non-respondents regarding the question that we base the *ci* variable on; 2) Non-respondents to *committee risk* have lower financial costs and higher equity ratios; and 3) Non-respondents to *manager risk* have lower financial costs, higher equity ratios, and are over-represented in the municipalities categorized as suburban, dominated by commuters or by manufacturing industries.

[38] The denominator is 265, i.e. the number of municipalities who did respond to at least one question. That is, these figures overestimate the "real" response rates. However, as we already know that the drop-outs differ from the respondents, we leave the out of the comparison so the table gives the relevant rates.

Table 4.B.1: Response rates

Variable	Response rate
committee risk	66%
manager risk	72%
ci	90%
centralization	95%
keep surplus	96%
keep deficit	97%

The similarity between respondents and non-respondents with regard to *ci* is reassuring.[39] For *manager risk* and *committee risk*, we include dummy variables for non-response to these questions to increase precision. Reassuringly, leaving out these dummies does not substantially affect the results. The only noteworthy difference to the baseline estimation is that the incremental effect of *cent123* $\times$ *ci* is no longer significant in the specification including outliers. The sign and magnitude of the coefficients are still similar though, so we interpret this as indicative of low precision (results available upon request).

[39] It may be noted that *ci* is negatively associated to population in a multiple regression setting, see Ellegård (2013).

4.C Control variable estimates

In table 4.C.1, we show the coefficients for the control variables included in the baseline estimation shown in table 4.6.

Table 4.C.1: Baseline results, control variables

	(1)	(2) Ex. outliers	(3)	(4) Ex. outliers
total revenues	0.0114	0.00622	0.0100	0.00503
	(0.0122)	(0.0121)	(0.0120)	(0.0121)
change in revenues	0.171***	0.100***	0.174***	0.100***
	(0.0501)	(0.0303)	(0.0499)	(0.0305)
fixed asset revenues	0.0815***	-0.0152	0.0818***	-0.0177
	(0.0233)	(0.0112)	(0.0237)	(0.0115)
financial costs	0.229*	0.177	0.250*	0.197
	(0.136)	(0.134)	(0.141)	(0.138)
equity ratio	0.00546	0.00771	0.00703	0.00972
	(0.00584)	(0.00564)	(0.00611)	(0.00589)
mean fiscal perf. 00-07	0.103	0.152	0.0677	0.114
	(0.142)	(0.129)	(0.146)	(0.127)
share right-wing	-0.00506	-0.00182	-0.00442	-0.000410
	(0.00686)	(0.00584)	(0.00709)	(0.00610)
herfindahl	-1.554	-1.649	-1.276	-1.227
	(1.457)	(1.485)	(1.484)	(1.506)
long-term budget	0.312**	0.278**	0.290**	0.251*
	(0.139)	(0.128)	(0.138)	(0.128)
log(*population*)	0.0164	0.0125	0.0361	0.0213
	(0.0988)	(0.0888)	(0.0994)	(0.0908)
share 20-79	0.0758*	0.0847**	0.0724	0.0857**
	(0.0435)	(0.0390)	(0.0448)	(0.0411)
cities	-0.271	-0.135	-0.319	-0.133
	(0.284)	(0.251)	(0.280)	(0.248)
rural	0.506*	0.417	0.500*	0.384
	(0.272)	(0.264)	(0.259)	(0.251)
suburb/commuter/manufactural	0.519***	0.534***	0.551***	0.564***
	(0.170)	(0.144)	(0.177)	(0.153)
Observations	225	222	225	222
R^2	0.567	0.256	0.574	0.275
F	4.300	3.710	3.661	3.294

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

4.D Robustness checks

In Table 4.D.1, we examine the robustness of the results presented in column (3) and (4) of Table 4.6. Columns (1)-(5) of Table 4.D.1 show estimates of Equation (4.1) from the full sample (including outliers), but the results excluding outliers are commented on in the text.[40]

In column (1) we change the dependent variable to per capita costs of services. Note that the expected signs are reversed, e.g. that negative coefficients imply lower costs in municipalities that employ a certain institution. The results are therefore qualitatively similar to the baseline estimations (the incremental effect $\beta_0^c + \beta_1^c$ even becomes more significant). However, the cost regression is sensitive to changes in the control variables; in particular *total revenues* explains a very large share of the variation in costs (the high R^2 value mainly derives from this variable).

A second concern is that our large set of control variables may influence the estimates. In column (2), we show that the results are similar when we only control for *relative change in revenues*.[41] This holds when outliers are excluded as well, with the exception that the incremental effect of *cent123* $\times$ *ci* (i.e. $\beta_0^c + \beta_1^c$) becomes insignificant (though with a p-value of 0.13). The estimates for the institutions are moreover robust to the inclusion of only controls that are significant at the 10 percent level (results not shown).

We have also tested specifications where the following variables are added one at a time to our baseline control variables (results available on request): the number of committees, indicators for whether the local committees are chaired by members of the executive committee; an indicator for whether the executive director (highest ranked civil servant) is the manager of local administration managers (instead of local committees doing the hiring and firing of managers); indicators for whether the political majority changed from left to right or vice versa in the election of 2006; an indicator for having no shift of political majority in the three elections 1998, 2002, and 2006; and an indicator for whether the municipality reports that it may not put forward a balanced budget in a recession. With the exception of the

[40] Control variables are included in the regressions but are suppressed in the table. For the first-difference estimation, the control variables consist of all time-varying controls in the baseline cross-sectional estimation.

[41] The results for *manager risk* and *keep deficit* also remain in a specification without *any* controls. The results for *keep surplus* and *cent123* are qualitatively similar but lose significance when all controls are removed.

indicator for having had no shift of political majority (which is positive and significant in the sample including outliers) none of these variables come out significant on conventional levels. More importantly, their inclusion leaves the institutional variables largely unchanged.

In column (3), we exclude *total revenues*, *changes in net revenues*, *fixed asset revenues*, and *financial costs*, and instead include the tax base size and the level of government grants, which are exogenous in the short run. The results are qualitatively similar, but the incremental effect of *keep surplus* $\times$ *ci* and *cent123* $\times$ *ci* are no longer significant in any sample.

In column (4), we examine a less broad classification of centralization by separating category 3 from categories 1 and 2. First of all, we note that there are no important implications for the other four institutional variables when we change the categorization. However, although none of the centralization coefficients are significant, the correlation between centralization and fiscal performance is quantitatively different for category 3 than for category 12, and the magnitude depends on *ci*. For the most centralized category (*cent12*), the correlation is positive regardless of the value of *ci* and slightly larger for those where *ci*=1. For category 3, the correlation is negative if *ci*=0 but positive if *ci*=1. A Wald test of equality of the coefficients on *cent12* and *cent3* suggests that the correlations differ between the categories (p=0.0398), although neither coefficient is distinguishable from zero. But the interesting question according to our framework is whether the influence of centralization is positive when there are conflicts of interest. Looking at the municipalities that report *ci*=1, there is notably no significant difference (p=0.438) between the incremental effect of centralization for category 12 ($\beta_0^{cent12} + \beta_1^{cent12} = 0.405$) and for category 3 ($\beta_0^{cent3} + \beta_1^{cent3} = 0.210$). When excluding the three outliers, the estimated incremental effects in the two categories are even more similar. Thus, as the incremental effects are indistinguishable from zero when *ci*=0 and similar for category 12 and 3 when *ci*=1, it seems reasonable to merge the two categories as done in the baseline.

Column (5) contains results where we control for *budgeted net revenues*. The coefficient for *keep deficit* becomes smaller and is no longer significant (although almost so in the sample excluding outliers, p-value = 0.116). The results for the other institutions are qualitatively similar to the baseline. The coefficient on *budgeted net revenues* is positive (250-360 SEK per capita) and significant at the 5 percent level in the sample excluding outliers, and

Table 4.D.1: Robustness estimations

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
keep surplus (ks)	-0.324	0.000485	0.136	0.351	0.383	0.370
	(0.327)	(0.249)	(0.403)	(0.291)	(0.265)	(0.298)
ci×keep surplus	-0.320	0.441	0.151	0.224	0.216	
	(0.397)	(0.346)	(0.410)	(0.357)	(0.324)	
keep deficit (kd)	-0.307	0.683**	0.772**	0.449	0.294	0.496*
	(0.344)	(0.273)	(0.344)	(0.287)	(0.283)	(0.287)
ci×keep deficit	0.159	-0.469	-0.682	-0.350	-0.286	
	(0.434)	(0.383)	(0.428)	(0.377)	(0.366)	
manager risk (mr)	-0.218	0.515	0.644	0.237	0.294	
	(0.541)	(0.398)	(0.420)	(0.377)	(0.386)	
ci×manager risk	-0.403	0.0725	-0.152	0.342	0.331	
	(0.566)	(0.414)	(0.438)	(0.397)	(0.397)	
committee risk (cr)	0.241	-0.0322	-0.0338	-0.0168	-0.0625	
	(0.549)	(0.417)	(0.401)	(0.375)	(0.400)	
ci×committee risk	-0.226	0.0508	-0.0475	0.0111	0.0209	
	(0.569)	(0.433)	(0.397)	(0.397)	(0.411)	
cent123 (c)	-0.227	0.109	0.117		0.0520	
	(0.295)	(0.225)	(0.264)		(0.232)	
ci×cent123	-0.328	0.288	0.216		0.215	
	(0.421)	(0.317)	(0.364)		(0.321)	
ci	0.758**	-0.445	-0.227	-0.473	-0.517*	
	(0.355)	(0.292)	(0.408)	(0.293)	(0.288)	
cent12				0.364		
				(0.291)		
ci×cent12				0.0403		
				(0.411)		
cent3				-0.124		
				(0.250)		
ci×cent3				0.333		
				(0.351)		
budgeted net rev.					0.255	
					(0.156)	
Constant	7.668*	0.0668	-8.760*	-6.360*	-4.651	-17.19
	(4.447)	(0.330)	(4.420)	(3.627)	(3.490)	(26.12)
Incremental effect of institution j for municipalities where $ci = 1$						
$\beta_0^{ks} + \beta_1^{ks}$	-0.644**	0.442*	0.286	0.575**	0.600**	
$\beta_0^{kd} + \beta_1^{kd}$	-0.148	0.213	0.090	0.099	0.007	
$\beta_0^{mr} + \beta_1^{mr}$	-0.621**	0.587**	0.492**	0.579**	0.625**	
$\beta_0^{cr} + \beta_1^{cr}$	0.014	0.019	-0.081	-0.006	-0.042	
$\beta_0^{c} + \beta_1^{c}$	-0.555*	0.397*	0.334		0.267	
Controls	Baseline	Reduced set	Reduced set	Baseline	Baseline	Time variant
Observations	225	227	225	225	219	456
R^2	0.974	0.397	0.452	0.584	0.604	0.521
F	365.9	2.520	3.666	3.673	4.667	14.84

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1. Column (1) use per capita costs of services as dependent variable. Column (2) include only *relative changes in revenues* as control variable. Column (3) include *tax base* and *government grants* while excluding *total revenues*, *relative changes in net revenues*, *fixed asset revenues*, and *financial costs*. Column (5) adds *budgeted net revenues* to the baseline control variables. Column (6) show a first-difference specification on the years 2004 and 2010, including *keep surplus*, *keep deficit*, and the time-variant baseline control variables. N.o. observations < 265 due to non-response to individual survey questions.

close to significant (p-value 0.104) in the sample including outliers.

As the use of carry-over rules was surveyed both in 2004 and 2010, we also run a regression in differences (column (6)). The main virtue of this first-difference (FD) model is that it controls for time-invariant omitted factors at the municipality level, but for several reasons its usefulness as a robustness check is limited. First, as we lack information on *ci* for 2004, insignificant coefficients may result from the failure to model the need for incentive-alignment rather than from ineffectiveness of the rules. Second, as we cannot control for either *manager risk* or centralization in the FD regression, these institutions are omitted. Third, and perhaps most important, identification of the coefficients comes only from those who switched rules between 2004 and 2010. But there must be a reason why rules are changed, and this reason is likely related to changes in the importance assigned to fiscal discipline or in the central-local relation – thus, the municipality fixed effects, that only control for time-invariant factors, do not wipe out the confounding heterogeneity. Finally, it is uncertain whether it is appropriate to model the effect of introducing the rules as quantitatively similar to the effect of abolishing the rules, as implied by the FD model. With those caveats in mind, we interpret the FD model in (6) as follows. First, we cannot determine whether the insignificant (but positive) coefficient on *keep surplus* is due to lack of relevance (lack of conflicts of interests) or due to the rule being ineffective. The FD model thus does not overturn our previous tentative conclusion that *keep surplus* may be effective. Second, the positive and significant coefficient on *keep deficit* cannot conclusively be interpreted as a causal effect, as there may well be unobserved changes related both to the change in rules and to the change in fiscal performance.

Except for the FD estimation, all estimations use the sample from 2010. Running regressions on a pooled sample over the period 2009-2011 or using the mean of the variables over the same period yields results that are in general qualitatively similar to the baseline, while the results are less stable for the single years 2009 and 2011. As we do not know whether the institutions have changed between the years, we think that these specifications are less reliable and refrain from showing them (they are available on request).

4.E Survey questions

The survey was constructed with the help of the electronic survey program Easyresearch. A link to the survey was sent to the official e-mail address of every municipality with a note asking for the survey to be forwarded to the chief financial officer/budget manager. Note that several of the questions below were not directly used in the econometric analysis in the paper. For completeness, we have included all questions here. The original survey in Swedish and (anonymous) data over the municipalities' answers are available upon request. All questions included a 'Do not know'-alternative, which we have omitted below for brevity.

1. *When does the council decide on the overall budget for the coming fiscal year?*

   - In spring before the fiscal year
   - In fall before the fiscal year

2. *Which of the following alternatives bear most resemblance to the beginning of the budget process in your municipality? (The concept of committees is intended to include all governing bodies that consist of politically elected representatives and are placed organizationally directly under the municipal council. The executive committee and as well as other boards with responsibilities for tax- and fees financed activities are thus included in the concept).*

   - The budget process begins with a budget proposal from each committee regarding their own activities
   - The budget process begins with a budget proposal from the executive committee for all committees

[Question 3 was only posed to municipalities that indicated the second or the 'Do not know' alternative in Question 2.]

3. *Which of the following alternatives bear most resemblance to the continued participation of the committees in the budget process?*

   - The committees have relatively large possibilities to propose changes to the executive committees budget proposal
   - The committees have limited possibilities to propose changes to the executive committees budget proposal
   - The committees have no possibilities to propose changes to the executive committees budget proposal

4. *Indicate the alternative below that best describe how the municipality allocates its resources:*

   - The resource allocation is to a large extent governed by centrally established unit costs for different services (SEK/student etc) and demographic variables

- The resource allocation is partly governed by centrally established unit costs for different services (SEK/student etc) and demographic variables
- The resource allocation is to a small extent or not at all governed by centrally established unit costs for different services (SEK/student etc) and demographic variables

5. *How are forecasts of tax revenues produced in the municipality?*

- The municipality uses the Swedish Association of Local Authorities and Regions' forecasts
- The municipality uses the Swedish Association of Local Authorities and Regions' forecasts as a point of departure, but produces an independent assessment of the tax revenues as well
- The municipality does not use the Swedish Association of Local Authorities and Regions' forecasts, but produces an independent assessment of the tax revenues

[Question 6 was only posed to municipalities that indicated the second, third and the 'Do not know' alternative in Question 5.]

6. *Indicate the alternative below that best describe the municipality's independent assessment of the tax revenues:*

- The municipality's independent assessment is in general higher than the Swedish Association of Local Authorities and Regions'
- There is in general no or a small difference between the municipality's independent assessment and the Swedish Association of Local Authorities and Regions'
- The municipality's independent assessment is in general lower than the Swedish Association of Local Authorities and Regions'

7. *Which alternative does best describe the situation in your municipality regarding long-term budgets?*

- Long-term budgets are lacking entirely
- Long-term budgets have the character of a pure forecast
- Long-term budgets are indicative decisions
- Long-term budgets constitute important political commitments

8. *How often during the fiscal year are follow-ups of the overall financial outcome performed by the executive committee?*

- 8-12 times/year
- 5-7 times/year
- 3-4 times/year
- 1-2 times/year

9. *Are the committee chairmen members of the executive committee?*
(Yes, all/Yes, some/No)

10. *How many of the last 5 years has one/some committees been given extra appropriations during the year, over and above their budgeted resource allocation?*
(0/1/2/3/4/5)

11. *How are budget surpluses handled?*

- The committees can carry-over the surplus to the next fiscal year
- The committees can carry-over some of the surplus to the next fiscal year
- The committees have no possibility to carry-over the surplus to the next fiscal year

[Question 12 was only posed to municipalities that indicated the first alternative in Question 11.]

12. *This question concerns only the municipality's, in terms of gross costs, largest committee. Could the committee, in violation of the principle, be deprived of some of the surplus if it amounted to 3-5% of total resources allocated to the committee?*
(Yes/No)

[Question 13 was only posed to municipalities that indicated the first alternative in Question 12.]

13. *This question concerns only the municipality's, in terms of gross costs, largest committee. Could the committee, in violation of the principle, be deprived of some of the surplus if it amounted to 1-2% of total resources allocated to the committee?*
(Yes/No)

[Question 14 was only posed to municipalities that indicated the second or the 'Do not know' alternative in Question 12.]

14. *This question concerns only the municipality's, in terms of gross costs, largest committee. Could the committee, in violation of the principle, be deprived of some of the surplus if it amounted to 6-10% of total resources allocated to the committee?*
(Yes/No)

15. *Does your municipality have a principle of forcing committees to carry over budget deficits from one year to another?*
(Yes/No)

[Question 16 was only posed to municipalities that indicated the first alternative in Question 15.]

16. *This question concerns only the municipality's, in terms of gross costs, largest committee. Could the committee, in violation of the principle, be remitted some of the deficit if it amounted to 3-5% of total resources allocated to the committee?*
(Yes/No)

[Question 17 was only posed to municipalities that indicated the first alternative in Question 16.]

17. *This question concerns only the municipality's, in terms of gross costs, largest committee. Could the committee, in violation of the principle, be remitted some of the deficit if it amounted to 1-2% of total resources allocated to the committee?*

(Yes/No)

[Question 18 was only posed to municipalities that indicated the second or the 'Do not know' alternative in Question 16.]

18. *This question concerns only the municipality's, in terms of gross costs, largest committee.Could the committee, in violation of the principle, be remitted some of the deficit if it amounted to 6-10% of total resources allocated to the committee?*
(Yes/No)

19. *This question concerns only to the municipality's, in terms of gross costs, largest committee. Consider a scenario where the committee for some years has run budget deficits, which are not caused by incidental circumstances. In this situation, which of the alternatives below best describe your municipality?*

- The deficits would possibly be a sufficient reason to replace the leadership of the committee
- The deficits would possibly be a contributing but not a sufficient reason to replace the leadership of the committee
- The deficits would not be a reason to replace the leadership of the committee

20. *Is the chief executive officer in your municipality the head over the managers for the respective administrations?*
(Yes/No)

21. *Does it occur in your municipality that managers of the administrations receive some form of bonus if the administration runs surpluses?*
(Yes/No)

22. *This question concerns only the municipality's, in terms of gross costs, largest administration. Consider a scenario where the administration for some years has run budget deficits, which are not caused by incidental circumstances. In this situation, which of the alternatives below best describe your municipality?*

- The deficits would possibly be a sufficient reason to replace the to replace the manager of the administration
- The deficits would possibly be a contributing but not a sufficient reason to replace the to replace the manager of the administration
- The deficits would not be a reason to replace the to replace the manager of the administration

[Question 23 was only posed to municipalities that indicated the second or the 'Do not know' alternative in Question 22.]

23. *This question concerns only to the municipality's, in terms of gross costs, largest administration. Consider a similar scenario as in the previous question: the administration has for some years run budget deficits not due to incidental circumstances. Furthermore, the administration has to a large extent planned and carried out measures to come to terms with the deficit, but these measures have not succeeded in reducing*

the deficit. Would this situation be a sufficient reason to replace the manager of the administration?
(Yes/No)

[Question 24 was only posed to municipalities that indicated the second or the 'Do not know' alternative in Question 23.]

24. *This question concerns only the municipality's, in terms of gross costs, largest administration. Consider a similar scenario as in the previous question: the administration has for some years run budget deficits not due to incidental circumstances. Furthermore, the administration has to a small extent planned and carried out measures to come to terms with the deficit, but these measures have not succeeded in reducing the deficit. Would this situation be a sufficient reason to replace the manager of the administration?*
(Yes/No)

25. *Suppose that the forecasted revenues in your municipality decreases due to a considerable recession. Is it possible that such a scenario would imply that the municipal council would decide on an underbalanced budget?*
(Yes/No)

[Question 26 was only posed to municipalities that indicated the first or the 'Do not know' alternative in Question 25.]

26. *Would the municipal council decide on an underbalanced budget if the forecasted revenues decreased by 3-5%?*
(Yes/No)

[Question 27 was only posed to municipalities that indicated the first alternative in Question 26.]

27. *Would the municipal council decide on an underbalanced budget if the forecasted revenues decreased by 1-2%?*
(Yes/No)

[Question 28 was only posed to municipalities that indicated the second or the 'Do not know' alternative in Question 26.]

28. *Would the municipal council decide on an underbalanced budget if the forecasted revenues decreased by 6-10%?*
(Yes/No)

29. *Which alternative best describe your municipality?*

- The executive committee and the municipal council are more concerned about fiscal discipline than the local committees
- The executive committee, the municipal council and the local committees do not

differ significantly in their concerns about fiscal discipline

- The local committees more concerned about fiscal discipline than the municipal council and the executive committee

Chapter 5

Divided we fall. Conflicts of interests regarding fiscal discipline in municipal hierarchies.

5.1 Introduction

The recent fiscal crises experienced by some European countries and by several U.S. municipalities highlight the importance of understanding causes and remedies for fiscal indiscipline, i.e. excessive spending and budget deficits. It has long been suggested that fiscal indiscipline may arise if the political system provides incentives for irresponsible behavior on part of the government (e.g Nordhaus, 1975; Rogoff, 1990; Tabellini and Alesina, 1990; Persson and Svensson, 1989). However, even if the government itself is prudent, fiscal discipline may be undermined if the agents in charge of policy implementation are less responsible. After all, the submission and approval of a balanced budget is only a prerequisite for fiscal discipline; the actual outcome is determined during the fiscal year, when policy is implemented by a multiplicity of ministries, authorities and street-level bureaucrats further down in the governmental hierarchy. As long as policy is not exclusively rule-based, the implementing agents have the opportunity to affect fiscal outcomes (von Hagen and Harden, 1996).

One reason why fiscal indiscipline may arise during the implementation stage relates to fact that agents at the lower hierarchical level are responsible only for sub-fields of policy. If the subdivisions care particularly much for their own policy fields, they may consider fiscal discipline to be the responsibility of other parts of the organization and thus pay little attention to their own, as well the overall, budget constraint. Policy-biased subdivisions may moreover have incentives to engage in excessive spending, as

each subdivision enjoys all the benefits from spending on its field but does not fully internalize the costs, which are partly borne by taxpayers that the subdivision does not care directly about (cf. Weingast et al., 1981; von Hagen and Harden, 1995).

These arguments beg the question of whether and why agents at the lower hierarchical level are biased towards their own policy fields. Non-political agents, i.e. bureaucrats, often display a special interest in their own field (Prendergast, 2007; Brehm and Gates, 1997; Lipsky, 1980). Likewise, some political scientists have argued that standing committee members are biased in relation to the policy preferences of the legislature as a whole, because of self-selection into committees according to field of interest (e.g. Weingast and Marshall, 1988; Shepsle and Weingast, 1987). The view that committees are biased is not uncontested, however. Gilligan and Krehbiel (1990) argue that the main reason why legislatures delegate authority to committees is to enhance the flow of information upwards in the hierarchy. From this perspective, the legislature would be irrational if it appointed politicians with extreme preferences as committee representatives, as such politicians would likely manipulate the flow of information to their own advantage.

Empirical studies of the U.S. congress and state legislatures give support for both arguments, as committee bias appears to prevail in some but not all committees (e.g. Battista, 2006; Overby et al., 2004; Frisch and Kelly, 2004; Londregan and Snyder, 1994). Though it certainly would be preferable to be able to conclude that committees are perfect agents of the legislature, the fact that committee bias varies between contexts is hopeful insofar as it suggests that bias is not written in stone. For the centrally placed principal, the natural question is then how a currently biased lower hierarchical level can be influenced to adopt a more holistic view. In the present paper, I address this question using unique survey data from the Swedish municipalities. The survey contains a direct measure of conflicts of interests between the two levels of hierarchy with regards to the importance of fiscal discipline; by definition, such conflicts of interests can only prevail if the agents at the lower level are biased in relation to the preferences of the center. Additional data allows me to examine how organizational practices and features correlate with the prevalence of conflicts of interests, which may give some clues about how the central level can mitigate committee bias.

The municipalities play an important role in the Swedish welfare state:

they are responsible for the financing and provision of public services such as schooling and elderly care, and their total level of spending amounts to about 14 percent of GDP. Though they all operate in the same broad institutional context and have the same areas of responsibility, the municipalities have large freedom in organizing their services. The municipal context thus provides variation to study, while limiting the institutional heterogeneity plaguing cross-country studies.

The center of the municipal hierarchy comprises a directly elected council and an executive committee (appointed by the council), while the lower level of hierarchy comprises a set of standing committees, henceforth referred to as local committees.[1] According to the survey, committee bias is a concern to a varying degree: roughly half of the survey respondents report that the two levels of hierarchy have differing views on the importance of fiscal discipline. Where there are such conflicts of interests, the center is, as expected, the one that assigns greater importance to fiscal discipline. With regards to actual fiscal performance, it may be noted that the municipalities reporting a conflict of interests have lower operating surpluses and higher costs (Dietrichson and Ellegård, 2012b).

The empirical analysis offers some tentative suggestions for municipalities striving to eliminate committee bias in order to promote fiscal discipline: conflicts of interests are *less* common in municipalities where the local committees are chaired by members of the executive committee, and *more* common in municipalities with a relatively fragmented committee structure. Notably, these organizational features are under the discretion of the centrally placed politicians. I also find that the conflicts are more likely to appear in conjunction with deteriorations in the general economic conditions of the municipality. This finding can be understood as reflective of a 'war of attrition', i.e. that local committees try to shift the burden of fiscal adjustment onto other parts of the organization (Alesina and Drazen, 1991). As economic conditions are exogenous and some delegation of policy implementation is inevitable, it may be difficult to fully eliminate this latter mechanism by organizational redesign. Thus, it seems that a prudent principal also needs enforcement mechanisms to restrain the agents at the lower hierarchical level.

[1] The relation between politicians and bureaucrats is not directly studied in this paper, though I recognize that bureaucrats can play a crucial role in the determination of conflicts.

The next section describes the municipalities' responsibilities and organizational structure. Section 5.3 provides the empirical hypotheses, while the data and econometric model are presented in section 5.4. The estimation results are presented in section 5.5 and discussed in section 5.6. Section 5.7 concludes.

5.2 Institutional background

The 290 municipalities, Sweden's third layer of government, are responsible for the financing and delivery of several important public services such as pre- to upper secondary education, elderly care, social services, building and planning issues, environmental protection, and fire department services. Service provision is mostly financed by a proportional income tax, with the tax rate set freely by each municipality. Intergovernmental grants and fees are other sources of funding.[2]

According to Swedish legislation,[3] each municipality must have a council and an executive committee. The council is appointed in general elections, and the executive committee is appointed by the council. The executive committee can be thought of as the municipal analogue of a national government, with the distinction that the distribution of committee seats between parties mirrors the distribution of seats in the council. Despite that most parties are represented in the executive committees, there is usually a stable coalition of parties forming a majority, thus in practice functioning as the governing coalition (or party, in case of single-party majority) of the municipality. It is the responsibility of the executive committee to prepare a budget proposal, which is to be approved by the municipal council; the budget will thus reflect the policy preferences of the parties collecting a majority of votes.

Although all municipalities have the same fundamental responsibilities, the principle of municipal self-government, written into Sweden's constitutional laws, implies considerable freedom to choose how activities should be organized and financed (Berlin and Carlström, 2003). Most municipalities employ a hierarchic organization with the executive committee at the top

[2] In 2010, tax receipts made up about two thirds of total revenues; general intergovernmental grants made up about 12 percent and fees and directed grants approximately 20 percent (Statistics Sweden, 2010).

[3] *Kommunallag* 1991:900

and several local committees at the lower level. Local committees are generally defined by policy area, though a few municipalities define committees by geography. At the very least, each municipality must have two committees in addition to the executive committee: an election committee and a chief guardian committee.

The local committees consist of politicians appointed by the council. Committee members may, but are not required to be, members of the the municipal council. It is not uncommon that the central level is directly represented in the local committees: in 2010, members of the executive committee chaired the local committees three out of four municipalities.

5.3 Determinants of inter-level conflicts

To understand the circumstances under which the local committees are less concerned about fiscal discipline than the center is, a fruitful starting point is to consider factors that may affect the local committee's bias towards their own policy area – that is, their policy bias from the viewpoint of the council and the executive committee. This is partly because such bias is a necessary condition for conflicts of interests in general (and regarding fiscal discipline in particular), and partly because such bias implies an incentive for excessive spending. There are at least two reasons why local committee representatives may be biased towards their own policy areas. *First*, politicians may self-select into committees according to field of interest (e.g. Weingast and Marshall, 1988; Shepsle and Weingast, 1987). *Second*, even if newly appointed local committee members initially are unbiased, the bureaucrats operating in their policy field may influence their view of optimal policy (Wildavsky, 1975; Niskanen, 1971). It is generally accepted that bureaucrats self-select into agencies that they are particularly interested in (Prendergast, 2007; Brehm and Gates, 1997; Lipsky, 1980), and the possibility to influence politicians has been argued to be an important reason behind the over-representation of policy-motivated agents in bureacracy (Gailmard and Patty, 2007; Gailmard, 2010).

Nonetheless, as pointed out by Gilligan and Krehbiel (1990), the legislature (here, the council) should be able to use its appointment authority to counteract tendencies for bias. For instance, by appointing members of the executive committee as chair persons for the local committees, the center's opinion can be advocated directly at the local committees' meetings. When

the chair has one foot at each hierarchical level, the possible influence from local-level bureaucrats is moreover balanced by influence from bureaucrats at the center.

Hypothesis 1: Inter-level conflicts of interests regarding fiscal discipline are less likely when members of the executive committee chair the local committees.

The hypothesis may appear trivially true, as the appointment of central-level agents as chairs implies that the center's view gets more representation at the local level. Still, just because the center ensures more representation by appointing a central-level player as chair, it does not mean that the chair manages to influence the other committee members. It is thus interesting to examine the hypothesis empirically.

Previous research on national governments suggests that fiscal discipline is sensitive to the number of spending ministries, i.e. the size of the cabinet (e.g. Perotti and Kontopoulos, 2002; Schaltegger and Feld, 2009; Wehner, 2010). A proposed explanation is that each ministry fails to internalize the full costs of spending on its policy area, as some of the costs are borne by individuals for whom the ministry is not particularly concerned. This externality induces excessive spending (and deficits, in a dynamic context). The larger the number of ministries, the narrower is each ministry's policy field and the smaller is thus the population share for which each ministry cares about – that is, the more aggravated is this *fiscal common pool problem* (see e.g. Weingast et al., 1981; von Hagen and Harden, 1995). The same line of reasoning can be applied to the municipal context, where the number of local committees correspond to the number of ministries.

Hypothesis 2. Conflicts of interests regarding fiscal discipline are less strong in municipalities with fewer local committees.

Of course, Hypothesis 2 can only be true if the local committees *may* be policy biased, i.e. if they are not always the rationally appointed unbiased agents hypothesized by Gilligan and Krehbiel (1990). This remark suggests that the chosen committee structure may be endogenous: perhaps the governing majority only chooses to use a heavily specialized committee structure (i.e. a lot of committees) in case it has confidence in its ability to

appoint unbiased representatives to the committees. If so, there should be no relation between conflicts of interests and the number of committees.[4]

The fiscal common pool problem has earlier been brought up in comparative studies of single-party majority governments and coalition governments. In this case, the externality arises because each coalition party only internalizes the part of the costs for spending that fall on its own voters. Though the empirical literature is inconclusive about the relevance of the fiscal common pool problem in coalition governments (Roubini and Sachs, 1989; Edin and Ohlsson, 1991; Persson et al., 2007; Freier and Odendahl, 2012), the possibility of such a problem suggests that the number of parties in the governing majority may be negatively correlated to the likelihood of an *inter-level* conflict about the importance of fiscal discipline: with more parties in the governing coalition, the central level is itself more likely to be fiscally indisciplined; thus, there is no breeding ground for an *inter-level* conflict about the importance of fiscal discipline.

Hypothesis 3. Conflicts of interests regarding fiscal discipline are less likely when there are more parties in the governing majority.

The opposite relation can also be advocated, though. In a coalition, several parties share the authority to appoint committee representatives. If the coalition parties engage in strategic trading of appointments to different committees (c.f Weingast and Marshall, 1988), committees consisting of preference outliers may well be the outcome. An alternative hypothesis to H3 is therefore that more parties in the governing coalition leads to more room for inter-level conflicts.

[4]In a study of Norwegian municipalities, Hagen and Vabo (2005) find that municipalities that have one committee for each bureaucratic agency have higher operating surpluses than municipalities where the committee structure is not parallel to the bureaucratic structure. Hagen and Vabo interpret their finding as supportive of Gilligan and Krehbiel's argument: a specialized (i.e. parallel) committee structure allows the central level to extract informational gains, which translate into a higher surplus. Evidently, this informational advantage is not fully overturned by policy biased committee representatives. Note that Hagen and Vabo's finding does not reject the validity of Hypothesis 2. The effect of a more specialized committee structure (which supposedly implies a larger number of committees) on fiscal performance (the outcome in Hagen and Vabo's study) is the sum of two counteracting effects: a positive effect due to better the informational flow in a more specialized structure and a negative effect due to the common pool problem.

It seems plausible to conjecture that the broader economic environment affects the probability of conflicts of interests within the municipal hierarchy. Specifically, conflicts of interests may be activated if the general economic conditions deteriorate and the municipality has to make fiscal adjustments to retain budgetary balance. Alesina and Drazen (1991) argue that although a collective would gain from rapid fiscal adjustments, adjustments may be delayed because different groups try to shift the burden of adjustment onto each other.[5] The idea of such a "war of a attrition" can be straightforwardly applied to the case of local committees (again given that committees *may* be biased towards their own fields). In bad times, each local committee continues with business as usual and argues that the required spending cuts should be shifted onto other policy fields. Thereby, the executive committee appears more concerned for fiscal discipline than the local committees.[6]

Hypothesis 4. Conflicts of interests regarding fiscal discipline are more likely when the general economic conditions deteriorate.

5.4 Data and econometric specification

5.4.1 Dependent variable

There are several ways to measure inter-level conflicts of interests. Previous studies (e.g. Battista, 2006) use opinion polls of individual legislators to compare committee members' opinions to the preferences of the legislature as a whole. With regards to the specific issue of fiscal discipline, another possibility is to use a measure of fiscal performance, e.g. the budget deficit, as a proxy variable for inter-level conflicts of interests. However, low fiscal performance reflects many other factors than inter-level conflicts. In particular, it is impossible to disentangle the contribution from inter-level conflicts of interests from the contribution of bad luck or from low ambitions on part of the central level itself. Instead, I use data from a survey sent to the budget managers in all 290 municipalities in June 2010 (see Dietrichson and Ellegård, 2012b, for further documentation of the survey).

[5] See also Alesina et al. (1998, 2006).

[6] See Fabrizio and Mody (2010) for an empirical analysis in similar spirit.

Each respondent was asked to indicate which situation that best described its municipality:

1. the executive committee and the municipal council are more concerned about fiscal discipline[7] than local committees;

2. the executive committee, the municipal council and the local committees do not differ significantly in their concerns for fiscal discipline;

3. local committees are more concerned about fiscal discipline than the executive committee and the municipal council.

According to the survey responses, politicians at the central level are in general more concerned about fiscal discipline than local politicians: of the 239 respondents, 56 percent indicate alternative 1 and only two indicate alternative 3. In the following analysis, I use a dummy variable, ci, which equals 1 if the executive committee and municipal council are more concerned about fiscal discipline (alternative 1) and 0 otherwise.

There are always reasons to be sceptic about the informational quality of subjective judgements. It may however be noted that the budget managers likely are the most reliable source of information about inter-level conflicts of interests, given their active role in the budget process and their close contact with committees and administrations during the fiscal year. It should also be emphasized that the survey question does not invite the budget managers to answer strategically (e.g. to put themselves in a better light), as the question refers to other people's preferences. Moreover, a study of fiscal performance in 2010 (see Dietrichson and Ellegård, 2012b) showed that operating surpluses were lower and costs higher in municipalities where the two levels of hierarchy had earlier been reported to disagree about the importance of fiscal discipline (ci=1); this finding may be interpreted as an indication that the survey question does indeed pick up inter-level conflicts of interests that worsen fiscal performance. However, an alternative interpretation is that budget managers in low-performing municipalities attribute their fiscal performance to an inter-level conflict, despite that the high costs and low surpluses are explained by other factors. In the

[7]The translation of the Swedish survey question into English is not perfect, the question uses an idiom ("en ekonomi i balans") in use in the municipalities. Though the idiom does not literally translate as "fiscal discipline", this term conveys the meaning of the idiom better than the literal translation ("a balanced economy").

estimations below, I perform some additional checks to further examine the adequacy of the measure.

51 budget managers (18 percent) did not respond to the survey question.[8] Reassuringly, the municipalities for which conflict data is missing are similar to the other municipalities in most observable dimensions. An exception is that they tend be rather small: information on *ci* is missing for 20 of the 75 municipalities with fewer than 10 000 inhabitants (27 percent), as opposed to 31 of the 215 municipalities with larger populations (14 percent).

5.4.2 Independent variables

Appendix Tables 5.A.1 and 5.A.2 displays the definitions and summary statistics of the variables used in the analysis. With respect to Hypothesis 1, *LocalExecutive* is a dummy variable indicating whether or not the chair person of each local committee also is a member of the executive committee. The variable derives from the same survey as *ci*. Executive committee members chaired the local committees in three quarters of the (responding) municipalities in 2010.[9]

To examine Hypothesis 2 and 3, I use information from Statistics Sweden on the number of local *committees* of each municipality and the number of parties constituting the majority coalition (*majsize*). *committees* is measured in 2007 – that is, before the survey was sent out – and *majsize* refers to the situation during the mandate period ending in September 2010.

All municipalities have at least 4 committees and the maximum number of committees is 42. Only 4 municipalities, among them the two largest cities, have more than 25 committees though. As larger municipalities have good reasons to delegate the implementation to a larger number of committees than smaller municipalities, I control for *population* in the estimations.[10] Population size may also be related to the probability of conflict for reasons unrelated to the number of committees. All else equal, economies of scale in the production of municipal services may imply more slack in the organization and thus less reason for conflicts to arise. In addition, the

[8] The overall response rate to the survey was 91 percent.

[9] Unfortunately, I do not have a measure of to which extent the local committee members are seated in the municipal council.

[10] Indeed, the two largest cities, Stockholm and Göteborg, have the largest numbers of committees.

larger distance between politicians and citizens in larger municipalities may make it easier to implement fiscal adjustments.

To examine Hypothesis 4, I include a set of variables capturing the general economic conditions of the municipality. These variables reflect the possibility to raise revenues and the demand for municipal services. The level and change (between 2007 and 2009) of taxable income per capita in thousands of SEK (*taxbase* and *taxbase0709*) relate mostly to the revenue side, while the level and change in employment rate (*employment* and *employment0709*) captures the possibility to raise revenues as well as the need for social assistance transfers. For the differenced variables, the change between 2007 and 2009 is chosen to capture the economic downturn starting in 2008. I also include the level and change (between 2004 and 2009) of the population share in working ages (i.e. 20-64, *WorkingAge* and *WorkingAge0409*).[11] A larger share of working-aged implies lower demand for the municipalities services, which are mainly used by children and elderly. Lastly, I include the level and change (between 2004 and 2009) of grants from the intergovernmental equalization system (*cggrants* and *cggrants0409*), which is supposed to compensate for changes in need.

5.4.3 Econometric specification

I estimate the following linear probability model using OLS with robust standard errors:

$$ci_i = \alpha + \beta \times \mathbf{fragmentation}_i + \gamma \times \mathbf{economic}_i + \theta \times population + \varepsilon_i (5.1)$$

fragmentation is a vector including the variables related to the fragmentation of the organization (i.e. the variables related to Hypothesis 1-3: *LocalExecutive, committees* and *majsize*), and **economic** is a vector or variables capturing the economic conditions of the municipality (Hypothesis 4).

As a sensitivity check, I have also estimated probit specifications. The probit model is specially designed to handle binary outcome variables, however at the cost of stronger distributional assumptions (Cameron and Trivedi, 2005). As the probit estimates are similar to the linear probability estimates, I only display the latter.

[11] Demographic changes are more likely to be interpreted as trends (as opposed to noise), the longer they have continued. Therefore, I use the five-year change between 2004 and 2009 instead of the change between 2007 and 2009.

5.5 Results

Column 1 of Table 5.1 shows the estimates from a specification including only the variables capturing fragmentation and population size. The estimates are consistent with Hypotheses 1 and 2, as the coefficients on *LocalExecutive* and *committees* are of the expected sign and statistically significant. Hypothesis 3 receives no support, as the coefficient on *majsize* is positive and statistically insignificant. As suspected, conflicts of interests are negatively related to population size.

In column 2, I instead include only the economic variables and population size. While some of the differenced variables are significant at the 10 percent level, the economic variables in levels are all far from significant. I then enter the levelled and differenced variables in separate regressions, and find that the F statistic of the model as a whole is markedly higher in the estimation including only the differenced variables; moreover, three out of the four differenced variables become statistically significant at the 10 percent level or less (column 3).[12] Municipalities experiencing a stronger growth in employment between 2007 and 2009 – that is, municipalities less hit by the economic downturn starting in 2008 – are less likely to report conflicts; similarly, relatively large increases in the share of working-aged (*WorkingAge0409*) are less likely to report conflicts. Interestingly, growth in the amount of grants from the intergovernmental equalization system is positively related to the probability of conflict. While it appears unintuitive that conflicts would become more intense when revenues in fact increase, the finding may reflect that the equalization system does not completely compensate for growth in structural disadvantages (such as population ageing) – that is, according to the local committees.

Column 4 reports the results from the full model, including both the fragmentation variables and the differenced economic variables. The results are similar to those in the partial models, though the standard errors of *employment0709* and *WorkingAge0409* increase somewhat.[13] Before drawing conclusions, there are however reasons to go deeper into some of the

[12] Notably, the F test of joint significance of included variables is far from significant in the specification including only the variables in levels.

[13] This appears to partly be explained by that the estimation samples in column 3 and 4 are slightly different (because of missing values on the fragmentation variables): running the specification in column 3 on only the 226 municipalities in the sample for the full model yields higher standard errors for the mentioned variables.

Table 5.1: Results; all municipalities

Variables	(1) Frag.	(2) Ec.	(3) Ec. diffs	(4) All	(5) <25
LocalExecutive	-0.181**			-0.190**	-0.186**
	(0.0766)			(0.0760)	(0.0763)
committees	0.0320***			0.0276***	0.0280
	(0.00967)			(0.00990)	(0.0192)
majsize	0.0248			0.0378	0.0396
	(0.0238)			(0.0245)	(0.0250)
taxbase		-0.000351			
		(0.00296)			
taxbase0709		-0.00207	7.08e-05	0.00235	0.00135
		(0.0250)	(0.0221)	(0.0232)	(0.0233)
employment		0.00239			
		(0.0110)			
employment0709		-0.0415*	-0.0393*	-0.0395*	-0.0399*
		(0.0249)	(0.0213)	(0.0220)	(0.0221)
WorkingAge		0.00183			
		(0.0201)			
WorkingAge0409		-0.0368	-0.0378**	-0.0360*	-0.0386*
		(0.0256)	(0.0190)	(0.0204)	(0.0205)
cggrants		-0.00139			
		(0.0129)			
cggrants0409		0.00354***	0.00354***	0.00296***	0.00309***
		(0.00111)	(0.00109)	(0.00110)	(0.00111)
population	-0.00151***	-0.000139	-0.000132	-0.00131***	-0.00197
	(0.000434)	(0.000496)	(0.000383)	(0.000447)	(0.00149)
Constant	0.333***	0.175	0.389**	0.148	0.155
	(0.115)	(1.877)	(0.164)	(0.202)	(0.238)
Observations	226	239	239	226	222
R-squared	0.048	0.028	0.028	0.071	0.058
F	4.832	2.262	4.125	5.178	3.574

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
Column 5 excludes observations with $\geq$ 25 committees.

results. In particular, Hypothesis 2 deserves further discussion. The reason is that the variation in *committees* in large part derives from a handful of municipalities with an usually large number of committees: recall that only four observations have more than 25 committees. Column 5 shows that the results are somewhat sensitive to the removal of these observations. Though the estimated coefficient on *committees* is more or less unchanged, it is now only significant at the 15 percent level of significance, i.e. above conventional thresholds.

This sensitivity does not obviously mean that Hypothesis 2 should be rejected. It is of course no surprise that the statistical significance of a variable decreases when observations that increase its variance are removed.

Moreover, the difference between columns 4 and 5 may simply indicate that the relation between the number of committees and the likelihood of conflict is non-linear. Most importantly, the relationship between *committees* and *ci* is robust to exclusion of the four outliers when the model is estimated on only relatively large municipalities (for which the response rate of *ci* is much higher). Table 5.2 shows the estimates for the sample excluding municipalities with fewer than 10 000 inhabitants. Column 1 of Table 5.2 corresponds to column 4 of Table 5.1, i.e. the estimates for the full model with no restriction on the number of committees.[14] The coefficients are similar to those in column 4 of Table 5.1; the only substantial difference is that *WorkingAge0409* is insignificant at conventional levels (p=0.153). Column 2 of Table 5.2, which corresponds to column 5 of Table 5.1, shows that *committees* remains significant also when the four municipalities with more than 25 committees are excluded from the analysis.[15] It therefore seems reasonable to interpret the data as consistent with Hypothesis 2, at least for larger municipalities.

The result for *committees* may capture more than the effect of increasing the number of committees, however. For instance, *committees* may also pick up the effect of using a geography-based, rather than sector-based, division of committees: a geographical division may mechanically entail more committees, as some services (e.g. garbage collection) will likely remain centralized.[16] In column 3, I therefore check whether the estimate for *committees* changes when I include a dummy for geographical division *geo*. The estimate for *committees* does not appear to be overly sensitive to the

[14] 215 municipalities have more than 10 000 inhabitants. Of these, 184 have responded to the survey question on *ci*; thus 11 have missing values on other variables, mainly on *majsize*. The estimated coefficients on the other variables are very similar when excluding *majsize* from the analysis, so I retain the full model from Table 1.

[15] When estimating the model on only municipalities with fewer than 20 committees, the coefficient on *committees* is very similar to that found in column 2, but the p-value increases to 0.125 – i.e. above, but not very far from, the 0.10 threshold. Notably, when the model is estimated on a sample consisting of only the 34 municipalities with more than 50 000 inhabitants but fewer than 20 committees, the coefficient on *committees* more than doubles in magnitude and is significant at the 10 percent level.

[16] In line with this interpretation, three of the four municipalities with more than 25 committees use a geographical division. However, two of these – Göteborg and Stockholm – are large enough to likely have had a lot of committees anyway. Apart from Stockholm and Göteborg, Malmö, Umeå, Västerås, Borås, Kalmar, Södertälje, Eskilstuna and Köping also had at least one committee based on geographical division.

Table 5.2: Results; municipalities with *population* > 10 000

	(1)	(2)	(3)	(4)
Variables	All	<25	Geo	Proxies
LocalExecutive	-0.199**	-0.198**	-0.198**	-0.212**
	(0.0901)	(0.0900)	(0.0904)	(0.0931)
committees	0.0320***	0.0374*	0.0372*	0.0347***
	(0.0104)	(0.0223)	(0.0224)	(0.00980)
majsize	0.0319	0.0353	0.0348	0.0354
	(0.0288)	(0.0292)	(0.0293)	(0.0303)
taxbase0709	0.00102	-0.00223	-0.00137	0.00289
	(0.0312)	(0.0315)	(0.0317)	(0.0296)
employment0709	-0.0470*	-0.0478*	-0.0474*	-0.0517**
	(0.0252)	(0.0253)	(0.0255)	(0.0239)
WorkingAge0409	-0.0391	-0.0413	-0.0421	-0.0452*
	(0.0272)	(0.0272)	(0.0274)	(0.0272)
cggrants0409	0.00260**	0.00270**	0.00275**	0.00265**
	(0.00121)	(0.00122)	(0.00123)	(0.00113)
population	-0.00149***	-0.00259	-0.00279	-0.00142***
	(0.000479)	(0.00161)	(0.00170)	(0.000443)
geo			0.103	
			(0.230)	
consultant				0.151*
				(0.0863)
LTB				0.138*
				(0.0767)
bailout				0.258**
				(0.115)
Constant	0.109	0.0928	0.0986	-0.117
	(0.265)	(0.302)	(0.303)	(0.269)
Observations	173	169	169	171
R-squared	0.080	0.065	0.066	0.139
F	5.155	3.260	2.917	6.227

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1
Columns 2-3 excludes observations with $\geq$ 25 committees.

inclusion of *geo*, which itself is far from significant (p=0.667).[17]

A crucial aspect with regards to the interpretation of the results is that the dependent variable is reliable, i.e. that the measure manages to pick up conflicts of interests of substance. A feasible way to examine the reliability of *ci* is to check whether it is positively correlated to other variables that should correlate positively with the prevalence of inter-level conflicts of interests. To find such variables, I recall that inter-level conflicts of interests regarding fiscal discipline are expected to arise because the center is more concerned than the local committees, rather than because the local committees are more concerned than the center. This suggests that conflicts of interests are more likely in municipalities where the center cares a lot about fiscal discipline than in municipalities where the center cares little. Along these lines, column 4 of Table 5.2 shows the estimates from a model incorporating a set of proxies for the center's preferences for fiscal discipline. *consultant* is a dummy variable indicating that the municipality has bought consultant services from the Swedish Association of Local Authorities and Regions (SALAR) to get input on how to improve fiscal discipline. Obviously, there is no reason to buy such services if one is not interested in fiscal discipline (though the group of non-buyers also includes municipalities with similar interest in fiscal discipline, but less demand for consultant services). Almost a third of all municipalities bought such services during the first decade of the 21$^{\text{th}}$ century. The second proxy, *LTB*, is a dummy variable indicating that the long-term budget is viewed as an important commitment (according to the survey); the reference category (*LTB*=0), which comprises about 40 percent of the observations, contains municipalities that either view the long-term plan as a projection only, or even ignore the statutory requirement to prepare a long-term budget. *LTB* arguably reflects the central level's planning horizon, a concept that connects closely to preferences for fiscal discipline: with a short planning horizon, there is no reason to care about budgetary balance.

I also include an indicator variable for the municipalities that received a conditional bailout from the central government at the beginning of the 21$^{\text{th}}$ century (*bailout*). Previous research suggest that these municipalities are, or at least have been, motivated to conduct a fiscally disciplined

[17] These conclusions do not depend on the specific estimation sample used in column 3. *geo* lacks importance also when adding the 4 outliers to the estimation sample as well as when adding the municipalities with fewer than 10 000 inhabitants.

policy (SOU, 2003; Siverbo, 2004; Dietrichson and Ellegård, 2012a). The *bailout* variable is however not unambiguously indicative of higher-than-average concern for fiscal discipline. Obviously, many municipalities did not even apply for a bailout, simply because they did not need financial assistance. Thus, though the reference category *bailout*=0 clearly contains relatively unmotivated municipalities, it also contains municipalities with a relatively large concern for fiscal discipline but little need for assistance. The main argument to include *bailout* is instead that the conditions attached to the bailout may have directly induced inter-level conflicts of interests. To receive the transfer from the central government, the municipalities first had to reduce their operating costs. Following these initial cost reductions, it seems likely that policy biased local bureaucrats intensified their pressure on local committees to call for more funds, which may have intensified the conflict of interests between the two hierarchical levels.

As shown in column 5, all three proxies are positively and significantly related to the probability of *ci*=1 in the sample of larger municipalities. All proxies are still positive, but only *bailout* is significant when also the municipalities with fewer than 10 000 inhabitants are included in the estimation sample (not shown).[18] Though these findings do not prove that *ci* provides meaningful information on inter-level conflicts of interests, the results are at least consistent with what would be expected if *ci* is meaningful.

5.6 Discussion

The results can be summarized as follows. *First*, consistent with Hypothesis 1, conflicts are less common in municipalities where representatives from the executive committee are appointed as chairs of the local committees. In relation to the mean conflict rate of 55 percent, the estimates point at a 30-35 percent lower conflict risk in municipalities where *LocalExecutive*=1. *Second*, the data is consistent with Hypothesis 2, though not robustly so for small municipalities. At a given population size, the conflict risk increases with about 5 percent for each additional local committee; a one standard deviation increase in the number of local committees implies a 25 percent higher conflict risk. *Third*, contrary to what was expected from Hypothe-

[18] Of all 290 municipalities, 36 have *bailout*=1. 20 of these 36 have more than 10 000 inhabitants. Due to non-response to *ci*, one of the 20 is discarded in the estimation shown in column 4.

sis 3, the prevalence of inter-level conflicts is independent of the number of coalition parties in the governing majority. *Fourth*, consistent with Hypothesis 4, conflicts are more likely to appear in conjunction with shocks to the employment rate and the dependency ratio (proxied by equalization grants). However, none of the economic variables are correlated to the conflict risk when measured in levels.

It is highly plausible that conflicts of interests are less likely when executive committee members chair the local committees (Hypothesis 1). The key concern about this finding is whether the estimated correlation follows trivially from the survey respondents conception of the local and executive committees: if the respondent in a municipality where *LocalExecutive*=1 has the same person (the chair) in mind when thinking about the two hierarchical levels, there is by definition no room for inter-level conflicts of interests. As the committees consist of more members than only the chair, it seems reasonable to interpret the result as more than trivial, though the estimated correlation of course does not prove that there is a direct causal link from *LocalExecutive* to *ci*.[19]

With regards to fiscal discipline, it should be noted that the finding for *LocalExecutive* says nothing about which side that has to give in to resolve the conflict. The implications for fiscal discipline of *LocalExecutive*=1 are unclear, because a lack of conflict can reflect either that the two levels agree on the virtues of fiscal discipline (in which case the center is more influential), or that that neither cares (in which case the local committees are more influential).

The positive relation between *committees* and *ci* supports Hypothesis 2 and suggests that committees are more narrow-minded, and therefore perceive themselves as less responsible for fiscal discipline, the smaller is their area of responsibility. By contrast, in municipalities with relatively few – and therefore large –committees, local committee politicians recognize that their actions have implications for the municipality's ability to retain fiscal discipline and internalize more of the costs when making spending decisions.

As for alternative interpretations of the result for *committees*, it should be noted that reverse causality is not a very plausible driver, *first*, because *ci* was measured three years after the committee structure was recorded,

[19] At the very least, the plausible result is reassuring regarding the informational content of *ci*.

and *second*, because it would seem a more intuitive strategy to integrate the organization (i.e. reduce the number of committees) if the central level perceived that the local level was overly imprudent (i.e. if ci=1). However, it cannot be ruled out that the correlation is driven by omitted factors correlated to both the committee structure and the level of conflicts.

Why is the correlation between *committees* and *ci* stronger in larger municipalities? One reason can be that in small municipalities (i.e. fewer than 10 000 inhabitants), the closeness between politicians and taxpayers make all politicians feel responsible for the whole municipality's undertakings, regardless of the organizational structure. But it is also possible that the result relates to the larger non-response rate in smaller municipalities.

On grounds of the unexpected sign on *majsize*, it seems safe to reject Hypothesis 3. As can be recalled from section 5.3, the positive correlation between the number of parties in the majority and the likelihood of conflict may be explained by strategic trading during the appointment of committee members. It may be noted that the correlation is significant at the 15 percent level in some of the estimations. Rather than discarding the relation as irrelevant, it may therefore be worthy of future examinations.

The finding that conflicts are more common in municipalities where the employment rate has developed unfavorably can be understood as indicative of a "war of attrition", in which each local committee tries to protect its own area from the spending cuts necessitated by worsened conditions. Similarly, the significant positive relation between growth in equalization grants and the likelihood of conflict indicates that the additional grant revenues are not sufficient to compensate for the unfavourable demographic development – that is, according to the local committees.

When conflicts of interests arise as a consequence of the formal organizational structure, it is easy to see how they can be mitigated. It is arguably more difficult to prevent conflicts of interests that arise from deteriorations of the economic conditions, which are exogenous to the local government. The correlation between unfavourable economic conditions and conflicts instead suggests that the central level has to restrain the discretion of local-level agents to retain fiscal discipline in bad times. Previous research suggests that certain features of the budget process, e.g. a rela-

tively centralized process[20] or a comprehensive system of fiscal rules[21] can promote fiscal discipline, at least when applied to national governments or the local government itself. Dietrichson and Ellegård (2012b) draw similar conclusions for a set of rules and processes applied directly to local committees.

What about the insignificance of the economic variables in levels? Are the measured conflicts of interests really problematic for fiscal discipline, if they are not more common in municipalities that have structural disadvantages? Here, it should be noted that it is perfectly possible to run a balanced budget or even a surplus even with a relatively small tax base. Moreover, recall that *ci* is negatively related to fiscal performance according to Dietrichson and Ellegård (2012b).[22] Though the measured conflicts are unrelated to levels of some of the prerequisites for public spending, the conflicts thus seem relevant with respect to the fiscal performance of the municipality.

5.7 Concluding remarks

It should be emphasized that an inter-level conflict of interests does not *per se* lead to fiscal indiscipline; a conflict between the central level and the committees is merely a symptom that the organization possesses characteristics that may lead to fiscal discipline. I have used this symptom to examine some possible ways to enhance fiscal discipline, but it should be stressed that a lack of inter-level conflicts does not imply lack of fiscal indiscipline: both hierarchical levels can happily agree on pursuing a fiscally irresponsible policy, just as they can agree on the virtues of fiscal responsibility.

For local governments that are firmly determined to conduct a fiscally responsible policy, the estimates give two tentative policy suggestions: *(i)* use the appointment authority to influence the local committees and *(ii)* minimize the number of local committees. Still, though the findings are consistent with a causal relation running from these factors to the likelihood

[20] von Hagen and Harden (1995); Perotti and Kontopoulos (2002); Hallerberg et al. (2007)

[21] Alt and Lowry (1994); Poterba (1994); Bohn and Inman (1996); Fabrizio and Mody (2006); Hallerberg et al. (2007); Debrun et al. (2008); Foremny (2011); Grigoli et al. (2012)

[22] The results in Dietrichson and Ellegård (2012b) are robust to the inclusion of the significant predictors of *ci* detected in the present paper.

of inter-level conflicts, it would be overly confident to argue that the present analysis establishes causality. The fact that conflicts of interests are more likely to arise when the general economic conditions of the municipality deteriorates moreover suggests that the center only has a limited ability to mitigate conflicts of interests. To retain fiscal discipline in bad times, the center of the municipality may therefore want to restrict the discretion of the local committees.

Though the present study focuses on fiscal discipline, the analysis is also interesting in relation to the broader discussion of committee bias conducted in political science. It is especially notable that the very different measure of committee bias used here confirm previous findings that committees should not *ex ante* be assumed to be neither preference outliers nor perfect agents of the legislature (here: the council). To validate the analysis in the present study, it would be interesting to replace the dependent variable with the kind of bias measures used by other authors, i.e. measures comparing the distribution of policy preferences of different committees to the preference distribution of the council as a whole. This is left as a topic for future research.

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5.A Variable descriptions and summary statistics

Table 5.A.1: Variables

Variable name	Description
ci	conflict of interest in 2010
	=1 if executive committee and council more concerned for fiscal discipline
	than local committees, 0 otherwise
committees	Number of local committees in 2007
majsize	Number of parties in the majority coalition
LocalExecutive	= 1 if chairs of local committees also are members of the executive committee,
	0 otherwise
population	Population size/1000
taxbase	Taxable income per capita (KSEK)
taxbase0709	Percentage change in *taxbase* between 2007 and 2009
employment	Employment rate (population aged 20-64)
employment0709	Percentage change in *employment* between 2007 and 2009
WorkingAge	Share of population aged 20-64 (%)
WorkingAge0409	Percentage change in *WorkingAge* between 2004 and 2009
cggrants	Transfer from/to the redistribution system (KSEK per capita)
cggrants0409	Percentage change in t*cggrants* between 2007 and 2009
geo	= 1 if at least 1 committee defined by geography
	= 0 if all committees defined by policy area
consultant	1 if municipality has bought consultant services from SALAR, 0 otherwise
LTB	=1 if the long-term budget viewed as directive or strong commitment
	=0 if the long-term budget is viewed as a forecast, or if the municipality
bailout	1 if municipality received conditional bailout in early 2000's, 0 otherwise

Table 5.A.2: Summary statistics, all municipalities

Variable	Mean	Std. Dev.	Min.	Max.	N
ci	0.535	0.5	0	1	226
committees	9.628	4.3	4	42	226
majsize	3.372	1.368	1	7	226
LocalExecutive	0.752	0.433	0	1	226
population	35.322	72.441	2.46	847.073	226
taxbase	163.697	20.701	129.988	313.423	226
taxbase0709	5.154	1.503	-0.912	9.005	226
employment	77.462	4.103	61.3	86.400	226
employment_0709	-4.289	1.604	-10.742	-0.943	226
WorkingAge	56.151	2.409	48.173	66.389	226
WorkingAge0409	-0.109	1.706	-5.256	5.46	226
cggrants	10.686	5.425	-11.077	26.213	226
cggrants0409	-0.532	10.537	-151.566	16.61	226
geo	0.035	0.185	0	1	226
consultant	0.336	0.473	0	1	226
LTB	0.536	0.5	0	1	224
bailout	0.128	0.335	0	1	226

Table 5.A.3: Summary statistics, municipalities with *population* > 10 000

Variable	Mean	Std. Dev.	Min.	Max.	N
ci	0.549	0.499	0	1	173
committees	10.462	4.535	4	42	173
majsize	3.387	1.37	1	7	173
LocalExecutive	0.78	0.415	0	1	173
population	44.028	80.86	10.053	847.073	173
taxbase	167.085	21.839	140.438	313.423	173
taxbase0709	5.163	1.286	1.947	9.005	173
employment	77.519	4.093	61.3	85.400	173
employment_0709	-4.229	1.633	-10.742	-0.943	173
WorkingAge	56.644	2.372	52.255	66.389	173
WorkingAge0409	-0.408	1.559	-5.256	3.951	173
cggrants	9.462	4.571	-11.077	23.237	173
cggrants0409	-0.769	12.016	-151.566	16.61	173
geo	0.046	0.211	0	1	173
consultant	0.289	0.455	0	1	173
LTB	0.602	0.491	0	1	171
bailout	0.11	0.314	0	1	173