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Birds of a feather need not flock together

Mitigating common pool problems and status quo bias with fiscal constraints

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Abstract

The ability of decision-makers to cooperate with one another is a controversial topic in political economy. It is often hypothesized that governments and parliaments, which are fragmented in terms of size and ideology will have worse economic outcomes than their less fragmented counterparts. This paper offers an empirical analysis of the impact of fiscal constraints on collective action problems using a panel data set for Swedish municipalities covering the period 1995 to 2012. The study finds some evidence of common pool problems as well as status quo bias, but their effects on economic outcomes are mitigated by the introduction of a balanced budget rule in 2000. However, since the balanced budget rule does not constrain all types of spending, it is likely to induce a behavioral change towards the types of spending that are not bound by the policy reform. Interestingly, this paper finds (weak) evidence of fragmented municipalities starting to channel more spending through their corporations following the policy reform. Thus, while fiscal constraints may eliminate the "direct" negative side effects of political fragmentation, they may also trigger non-expected responses in other parts of the economy.

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1 Introduction

Game theory suggests that cooperation becomes difficult when there are many players. Applied to political economy, this means that coalitions should find cooperation more difficult than single-party governments. After all, coalitions consist of a greater range of opposing interests and are often *fragmented* both in terms of size and ideology.[1]

Two influential theories relating to political fragmentation and collective action problems are the *common pool theory* and *veto player theory*. In short, common pool theory predicts that fragmented governments will issue higher levels of debts and expenditure, while veto player theory suggests that the size of policy adjustment will be smaller the more fragmented a government is. Conservatively estimated, more than fifty articles related to these theories have been published in the past two decades, and an appealing aspect of undertaking such research are the straightforward predictions offered by economic theory.

Yet, the predictions from economic theory do not match Swedish data. For example, the number of single-party municipal governments decreased by two thirds during the time period under consideration in this paper, while the number of five-party coalitions increased almost four-fold. Political fragmentation saw an unprecedented increase in the 1998 election, after which the number of municipal coalitions increased by 35 per cent. Yet, at the same time as fragmentation started to increase, fiscal performance improved.[2] Deficits were greatly reduced in the early 2000s after the financial crisis of the 1990s; from more than half of all municipalities running deficits to less than a third in only a couple of years, followed by further stabilizations in the pursuant years. Theory, however, predicts worse fiscal performance following increased political fragmentation.[3] Interestingly, fiscal improvements happened around the same time as the introduction of a balanced budget rule in 2000, which restricted the potential for municipalities to run deficits and spend excessively.

The institutional context - which theory effectively leaves out - is therefore a plausible explanation for why fragmented Swedish municipalities do not manifest collective action problems on an aggregate level, at least not at first sight. Even Primo and Snyder (2008, p. 485), criticizing and modifying the results of Weingast et al. (1981), state that "under full cost-sharing, total spending is *always* increasing in the number of districts".[4] Yet, binding jurisdiction and fiscal frameworks may help to eliminate the scope for common pool behavior and may furthermore increase fragmented governments' abilities to cooperate on policy adjustments.

In this paper, common pool and veto player theory are therefore extended to include a fiscal constraint in the form of a balanced budget rule. From theory we can infer testable hypotheses that the effects of political fragmentation will be mitigated by fiscal constraints. The mitigating effects of fiscal constraints are, however, not obvious. While it is theoretically plausible that fiscal constraints are positively related to sustainable public finances, there exist several reasons for why this need *not* be the case in reality. For example, governments that fear losing the next election may use fiscal constraints strategically to tie their political opponents. Technically, a fragmented government may budget such that the sanctions from not having obeyed to the balanced budget rule will affect the *next* government negatively. Further, fiscal

[1] According to Poterba and von Hagen (1999, p. 3), fragmentation can "arise when there are many actors involved in the budget process, and when the decision-making processes in which these decision makers interact diffuses power".

[2] Fiscal performance improved even when taking account of the budget cycle.

[3] It should, however, be noted that crises are special events and that improvements in financial outcomes may simply be the result of mean reversion. However, the current strong fiscal situation in many Swedish municipalities is rare from a historical perspective.

[4] Where districts are equivalent to political interests.

constraints are rarely strictly binding, and they often leave space for changes in behavior. For example, Swedish municipalities enjoy a relatively large degree of freedom in terms of accounting, and may more or less structure their finances as they please. Therefore, it is possible that fiscal constraints may give rise to a relocation of revenue and expenditure, rather than having a "true" impact on finances. Thus, it is essential to also look for less obvious effects of fiscal constraints than those directly derived from theory.[5]

The aphorism "birds of a feather flock together" means that people with similar characteristics tend to stay together. To a large extent, the moral of political economy is that similar people *should* stay together in order to facilitate cooperation. This paper shows that this need not be the case since the side effects of fragmentation may effectively be mitigated by fiscal constraints. However, the study also finds some support that fiscal constraints may lead to behavioral changes when a given fiscal constraint only *partially* constrains the budget. Therefore, while balanced budget rules may mitigate the effects of political fragmentation for some variables, they may exacerbate the effects of fragmentation for other variables.

How does this add to the current state of knowledge? A majority of the OECD member countries have a balanced budget rule, and further fiscal constraints are being introduced in the aftermath of the 2008 financial crisis. Thus, a natural point of interest is to improve the understanding of how regulation ties to two of the most researched theories in political economy. While most previous literature shows either the effectiveness of budget rules in improving economic outcomes *or* the role of political fragmentation affecting fiscal outcomes, we merge the two approaches. Moreover, the paper is policy-relevant in the sense that it illuminates potential side effects of balanced budget rules that have not been explicitly analyzed previously.

The paper is structured as follows. Section 2 includes a review of relevant previous research. Section 3 provides a theoretical background to common pool and veto player theory. Section 4 presents the hypotheses of the paper. Section 5 gives an account of the institutional setting. Section 6 elaborates on the data used and section 7 motivates the empirical strategy. Section 8 presents the results. Lastly, section 9 summarizes the conclusions of the paper.

2 Literature review

There exists a rich literature on the relationship between common pools and institutions (see for example Ostrom 1990) as well as extensive empirical research relating to political fragmentation and economic outcomes (see Appendix *A*). Furthermore, there is a vast policy-related literature examining the effectiveness of fiscal policies.[6] However, there exists little research specifically evaluating collective action problems and fiscal outcomes in light of fiscal constraints.

In a well-cited paper, Roubini and Sachs (1989) suggest that in several OECD countries the slow rate at which the post-1973 fiscal deficits were reduced resulted from the difficulties of political management in coalition governments. Since then, a great number of studies using similar approaches have been undertaken, both to test common pool as well as veto player theory. In order to save space, we refrain from presenting

[5]Moreover, other factors such as government ideology may be more important determinants than fiscal constraints in terms of affecting general budget outcomes. If so, the effects of more important determinants may counteract the hypothesized effects of fiscal constraints.

[6]The typical findings are that centralization of budget procedures and balanced budget rules are effective in improving fiscal discipline. These empirical studies are almost exclusively geared towards evaluating policies and leave out the relationship with economic theory. Kirschgässner (2003) provides a, somewhat outdated but exhaustive, summary of studies on effects of institutional rules.

previous studies that do not specifically consider fiscal frameworks. Summaries of approximately thirty relevant works since Roubini and Sachs (1989) can be found in the appendix. To a large extent, the results of these studies are dependent on variable specifications and the data sets used. Moreover, the studies are typically situated in different institutional environments, which can probably partly explain their conflicting findings. For example, Elgie and McMenamin (2008) first successfully replicate the results of Volkerink and de Haan (2001) on an OECD sample. However, when they add ten non-OECD countries, the effect of political fragmentation disappears.

As previously mentioned, there are very few studies specifically considering the institutional context when evaluating common pool and veto player theory. To the best of our knowledge, only Schaltegger and Feld (2009) evaluate how balanced budget rules address the problems that economic theory suggests result from political fragmentation. Their study is, however, very different from ours.[7] Moreover, a range of studies analyzes centralized budgetary procedures relating to political fragmentation. However, these studies are generally cross-country, where issues of heterogeneous settings as well as potential endogeneity of the institutions and the number of decision-makers arise.[8]

Tovmo (2007) argues that centralized budgetary procedures have an advantage in overcoming common pool problems in the decision-making process. Moreover, Tovmo and Borge (2009) find that municipalities with more fragmented councils are more "consumption sensitive" under a balanced budget rule. Similarly, Hagen and Vabo (2005) find support for the hypothesis that strong political leadership improves fiscal performance and de Haan and Sturm (1994) conclude that countries with unstable governments may have more difficulties in controlling their public debt, but that introducing tight fiscal criteria may compensate for the internal political instability.[9] Alesina and Perotti (1995) argue that budget procedures and budget institutions influence budget outcomes. Grisanti et al. (1998) find that fragmented governments tend to have higher spending and larger deficits unless they are constrained by institutional rules leading to greater centralization of the budget process. Feld and Kirschgässner (2006) also find that fiscal constraints significantly reduce budget deficits. Velasco (1997), on the other hand, argues that deficits resulting from common pool problems can be eliminated through a fiscal reform, but that such a reform may only take place after a delay during which government debt is built up.

From the text above, it is evident that there is a dearth of knowledge in the literature in terms of merging research on political fragmentation with fiscal constraints that are likely to affect what impact fragmentation has on economic outcomes, while not simultaneously affecting the degree of fragmentation.

[7] It is different, for example, in terms of fragmentation measures and dependent variables. Moreover, they exclude the theoretical underpinnings for why fiscal frameworks should affect economic outcomes through fragmentation.

[8] For example, when a study analyzes the number of decision-makers in presidential and proportional systems, the observed differences in fragmentation are likely to be a result of the institutional setting, which may itself be endogenous. Persson et al. (2007) set-up a model in which the electoral rule is endogenous. They find that the distinction between single-party and coalition governments is indeed central to the size of public spending. The central mechanism is that voters can discriminate between the parties of a coalition government, while they cannot do so between different factions making up a single-party government.

[9] Coalitions generally have shorter tenures, and they may therefore be considered unstable.

3 Theory

Below, we provide a more detailed account of the two main theoretical strands connecting political fragmentation to economic outcomes. Moreover, we explain why the predictions of the theories change upon introducing a fiscal constraint such as a balanced budget rule.

3.1 Common pool theory

The standard interpretation of the common pool problem is that coalition governments will be less willing or able to resist pressures for increased spending. Common pool theory is often attributed to Weingast et al. (1981). However, it can also be traced in Buchanan and Tullock (1962) as well as in Olson (1965). By deriving conditions under which a representative legislature will select a number of projects, each of which exceeds the efficient scale, Weingast et al. (1981) formalized a model in which the Nash equilibrium leads to overspending. Weingast et al. (1981) argue that the political agents will adopt logrolling norms where the decision-makers vote for each other's projects to get their own passed. With this behavior, total public revenue will be a common pool for all political agents. However, the overdrawing of resources will only be possible as long as the common pool of resources *can* actually be used for this purpose. Fiscal constraints will limit the scope for how much public expenditure can increase as the number of political agents increases.

More specifically, assume that there is a publicly provided project (X) with benefits $B(X)$ concentrating in a district i.[10] For example, this may represent a rural part of an otherwise diverse municipality and assume that in district i, most voters vote for party p. Let n be the number of districts (which can be interpreted as the number of political interests), and let $C(X)$ be the cost of the project. Next, assume that the benefits of spending in i increase with spending, but that there are diminishing returns. Moreover, assume cost sharing of all projects, and that taxes are equal for the citizens. A representative of party p will choose the optimal project-size by maximizing the net benefit. This is done by equating the marginal benefit to district i with the marginal cost, which is $\frac{1}{n}$. Following this line of reasoning, legislators' incentives to overspend on distributive projects increase proportionally with the number of districts or special interests, since it will be sufficient only with small marginal benefits of a project for it to be "worthwhile" when n increases.

Formally, if district i receives a project X_{it}, and all other districts receive projects X, then the payoff for the legislator representing district i is defined as[11]:

$$\prod_i(X_i, n) = B(X_i) - C(X_i)/n - C(X)(n-1)/n \tag{1}$$

If the representative of party p chooses the size of her district's project X_i, taking all other districts' projects as fixed, then differentiating with respect to X_i yields the first-order condition $B'(X_i^*) = C'(X_i^*)/n$. Next, differentiating this first-order condition totally with respect to n yields:

$$\frac{\partial X_i^*}{\partial n} = \frac{B'(X_i^*)}{C''(X_i^* - nB''(X_i^*)} > 0 \tag{2}$$

In the equations above, it is evident that each district's project size is increasing in proportion to the number of districts. The number of projects is also increasing in

[10] Note that the denotations used in the following example are largely the same as in Primo and Snyder (2008) and Franzese (2010).

[11] Given that each legislator's payoff is equal to the total payoff of all citizens in her district.

proportion to the number of districts, since each district receives a project. Therefore, total government spending, $nC(X_i^*)$, is increasing in proportion to the number of districts.

If one adds a budget constraint for the level of public expenditure, government spending will consequently decrease, given the assumption that the government previously had higher expenditure than revenue. Decision-makers still equate the marginal benefit to district i with the marginal cost; however, a constraint decreases the inefficiency in spending since the logrolling of projects will only be possible to the extent that $revenue \geq expenditure$.[12]

Negative side effects of political fragmentation can be measured by the extent to which decision-makers fail to internalize the full costs of projects. With balanced budget rules in place, decision-makers still fail to internalize these costs. However, they will not be able to increase spending *beyond* the limits of the balanced budget rule.[13] Thus, restrictions on spending should mitigate the negative side effects of common pools.

While common pool theory is often taken for granted in the literature, several other objections to its validity can be made. For example, Primo and Snyder (2008) demonstrate that the "law of $1/n$" with respect to project sizes and total spending is dependent on several factors, including the type of good being provided, the costs of raising revenues and whether the local government has to share in the project's cost with the central government. In the case of, for example, deadweight costs of taxation a "reverse law of $1/n$" may even hold. Moreover, the common pool hypothesis suggests that general taxes resemble common goods and that they may be overutilized in fragmented party systems. Yet, politicians do not necessarily maximize the provision of welfare services but may instead aim to minimize taxes. Furthermore, the common pool hypothesis assumes that a large single party representing several groups in society behaves differently from a coalition of smaller parties. There is little theoretical foundation for such an assumption.[14]

3.2 Veto player theory

A different take on the effects of government fragmentation on economic outcomes is provided by veto player theory. Veto player models imply that coalition governments will find it more difficult to "take action", since the parties in the coalition are effectively veto players who may block project proposals of other coalition partners. Therefore, the number and/or interest-ideological polarization of policymaking actors whose approval is required to alter the policy status quo reduces the probability of policy change and/or the size of policy change (Franzese 2010).[15] Coalition governments are expected to find it more difficult to reduce budget deficits since the different members of the coalition are expected to veto spending cuts or tax increases that impinge upon the interests of

[12]This is the case with balanced budget rules, and therefore the example that is provided given that this is the type of fiscal constraint of interest in this study.

[13]Or other fiscal constraints in place, see for example Feld and Kirschgässner (2006) for an analysis on debt brakes.

[14]Bawn and Rosenbluth (2006) examine whether a given coalition of groups would be represented differently in government by a single "long coalition" party or by a transient "short" coalition government of narrow-interest parties. Their answer is that electoral accountability indeed differs between long and short coalitions since a party maximizes its marginal contribution to its support groups' welfare, and externalizes costs not borne by its support groups. They argue that "short coalitions of multiple parties in government negotiate less efficient logrolls than long coalitions because policy decisions, which reflect the preferences of the coalition partner that cares most about the policy area, externalize more costs than would occur within single-party government" (p. 262).

[15]The two terms "status quo bias" and "veto player problems" are henceforth used interchangeably.

their respective constituencies (de Haan et al. 1999).[16]

Tsebelis (1995) argues that a rise in the number of parties results in a potential for status quo bias in policy. He defines veto players as political parties that are actual or potential members of the governing coalition and which may block specific policy proposals from passage. The potential for policy change decreases with the number of veto players and the dissimilarity of policy positions among veto players.

Alesina and Drazen (1991) model veto player problems by a "war of attrition" (also see Bulow and Klemperer 1999) in which the number of agents affects the time it takes to make a decision. More precisely, decisions are delayed until the costs of the delay become too big for one of the decision-makers. When stabilization has significant distributional implications, different socioeconomic groups will attempt to shift the burden of stabilization onto other groups. Essentially, a party that by delaying the decision forces the other parties to give in will be able to pass the majority of the negative effects of a decision onto the other parties. There may therefore be an agreement on the need for fiscal consolidation, but a political stalemate over how the burden of policy adjustments should be allocated (Huber et al. 2003).

Fiscal constraints may, however, limit the time it takes to stabilize deficits, where the main argument is that fiscal constraints generally imply costs for noncooperation. The costs inevitably incurred by all parties if failing to stabilize a deficit in the presence of fiscal frameworks may be higher than the expected gains of waiting each other out. In many countries, failure to meet the balanced budget rule is met with sanctions. For example, the central governments in Denmark and Norway may even seize control of the sub-central governments if they do not adhere to the fiscal constraints in place. If all policy-makers run the risk of losing power and thus "failing" their constituencies anyway, giving in from the beginning is likely to be a preferred choice. Moreover, fiscal frameworks generally imply higher "waiting costs", further increasing the costs of noncooperation. For example, in Sweden there may be adjustments to the discretionary grants from the central government, naming and shaming in media, as well as "time and/or administrative costs" related to the extensive paperwork required, after failing to adhere to the fiscal framework.

More precisely, the problem can be modeled in an n-multiperson prisoner's dilemma (see e.g. Schelling 1978; Taylor 1987; Cremer 1986), where noncooperation is usually modeled as the dominant strategy in the finitely repeated game. However, here it is hypothesized that the possible sanctions of breaking the fiscal constraints change the payoff matrix, implying costs to noncooperation such that cooperation instead becomes the dominant strategy. Moreover, in the model by Alesina and Drazen (1991), each party has in the presence of an information asymmetry an incentive to wait and see whether the others give in first. However, if there is a binding jurisdiction some of this information asymmetry disappears, decreasing the incentives for waiting each other out.

Furthermore, one may also view the problem as an infinite game where political agents in the decision-making body will play the "delay game" forever. We know from the repeated prisoner's dilemma that if the future is sufficiently important, i.e. the discount factor is low enough, efficient cooperation is possible in infinitely repeated noncooperative games, even if there is no possibility of binding agreements (see e.g. Jehle and Reny 2011; Osborne 2003).

[16] One should note that by the same logic, governments with veto players may also find it harder to agree on where to increase spending in response to a windfall in revenue, which could have the reverse effect on budget deficits.

4 Hypotheses

Given the theoretical implications derived in the previous section, the purpose of this paper is to test whether balanced budget rules mitigate common pool behavior and status quo bias.[17] Moreover, the paper also aims to evaluate whether the introduction of a balanced budget rule induces behavioral changes among policy-makers. More formally, the following hypotheses are tested:

- **Hypothesis 1**: *Political fragmentation has a positive and significant effect on the level of debt and public expenditure, and a negative and significant effect on the level of net income.*
- **Hypothesis 2**: *Political fragmentation has a negative and significant effect on the ability to stabilize deficits.*
- **Hypothesis 3**: *The impact of political fragmentation on the level of debt, public expenditure and net income is mitigated by fiscal constraints.*
- **Hypothesis 4***: Partially binding fiscal constraints will induce fragmented policy-makers to channel expenditure to areas that are not constrained by jurisdiction.*

Hypothesis 1 tests whether common pools induce overdrawing of public resources.[18] Hypothesis 2 tests a simple model inspired by the veto player perspective, suggesting that the more fragmented a government is, the harder it should be to stabilize a deficit. Hypothesis 3 tests whether fiscal constraints, in terms of a balanced budget rule, are particularly binding for fragmented governments. Hypothesis 4 is tested in conjunction with testing hypothesis 1. In order to test hypothesis 4, this study makes use of the fact that the introduction of the balanced budget rule did not directly constrain the level of investments nor the municipal corporations, leaving scope for behavioral change.

5 Institutional setting

Prior to specifying the empirical strategy for testing the hypotheses, it is necessary to understand the contextual setting in which Swedish municipalities operate. This section reviews the Swedish political landscape, electoral rules and the municipal budget process.

5.1 Elections and jurisdiction

Similar to many other countries, Swedish municipalities are charged with the responsibility for the delivery of most public services. The local government sector accounts for more than twenty per cent of gross domestic product and it employs a quarter of the labor force.[19] Swedish law mandates a large share of municipal activities.[20] In order for

[17] Note that we do not study the notion of government fragmentation *over time*. Proportional election systems are more likely to have coalition governments, which in turn are more likely to have shorter tenures. Shorter tenures and/or political competition may induce parties to use debt strategically. For more on political competition and strategic use of debt, see e.g. Pettersson-Lidbom (2001).

[18] It is hypothesized that the effects of fragmentation on debt will be larger than for public expenditures since there is more space for discretion regarding debt levels. Law often mandates expenditures on welfare services and when the costs differ, they are often capturing structural differences such as population density rather than the degree of ambition of the politicians. Debt on the other hand also captures, for example, discretionary debt financed investments.

[19] The local government sector consists of *both* county councils and municipalities. However, this paper only considers municipalities.

[20] The responsibilities of county councils primarily revolve around health care, while the municipalities are responsible for a greater range of welfare services, including education.

the local governments to be able to fulfill their obligations, Sweden has a relatively far-reaching grant system for the equalization of taxpaying and income differences between the municipalities.[21] There are currently 290 municipalities in Sweden.[22]

Municipal elections are held in September every fourth year. Turnout is generally high.[23] In each municipality there is a council as well as an executive body. The council has the final say on important matters in the municipality, such as the budget and the tax rate, however, the government delivers the budget proposal.

Until recently, municipal councils were required to comprise of at least 31 representatives. The average council size during 1995 to 2012 comprised of less than 50 members, however, variation was large.[24] A type of highest average method, called the modified Saint-Laguë method, is used to allocate seats in Swedish municipalities. The basic principle is that seats are distributed one by one in consecutive rounds using a series of divisors.[25] Unlike the national parliament, there is no vote share threshold to enter the municipal council.

Municipalities are mandated by law to have sustainable public finances. The yearly financial result must cover needs for re-investments, pensions etc. Specific targets for meeting the general requirements of sustainable public finances vary between the municipalities.[26]

The municipal government is required to deliver a budget proposal for year $t+1$ by the end of October in year t. By law, the municipal budget must include a plan for the municipal activities as well as a plan for the municipal finances for the coming year. The budget plan must give an account of how municipal activities will be financed, as well as provide a forecast for the economic outlook at the end of the following year. Moreover, the municipalities also need to specify financial goals for sustainable public finances. If the municipal council decides on expenses outside of the budget during year $t+1$, the decision needs to be accompanied by specifications of how the expense is to be financed.

As already mentioned, a balanced budget rule was introduced in 2000 to strengthen the budget process. The balanced budget rule decrees that all municipalities must ensure that revenue exceed expenditure. If a municipality runs a deficit, the deficit needs to be recovered within three years. There are generally few circumstances in which municipalities are allowed to deviate from the rule. Municipalities are allowed to issue as much debt as they please, however, they may not finance the expenditure of their regular activities through debt. While the balanced budget rule itself is enforced

[21] *The income equalization grant* is based on the principle that all municipalities are guaranteed a taxable income per capita equivalent to the national average. The municipalities that have a lower taxable income per capita than the average is compensated with a grant, and those municipalities that have a higher taxable income than the average pay a fee to the system. *The cost equalization grant* is aimed at compensating municipalities for a number of factors, which may affect their chances of meeting their obligations. Differences to the national average in factors such as demography, population and unemployment will affect whether a municipality is a net payer or net recipient. *General grants* are given to municipalities based on population size. All municipalities receive the same per capita amount, after which a fixed amount is added for each inhabitant in younger and older age cohorts respectively. Moreover, since 1993 there exists a rule such that if the central government decides on a measure that will directly affect municipal activities, the municipalities are reimbursed/compensated through government grants.

[22] The current number of municipalities is mainly a result of a large number of municipality mergers, which were initiated in 1952 and completed in 1974. During this period, Sweden went from having 2,498 municipalities with an average population of 2,900 to 278 municipalities with an average population of 29,000.

[23] Turnout may be high because the local elections coincide with national elections. Roughly 80 per cent of eligible voters usually vote.

[24] For example, the Stockholm municipality has more than one hundred council members.

[25] The modified Saint-Laguë method uses 1.4 as the first divisor, then 3, 5, 7 etc.

[26] A common goal is, however, a net income that constitutes two per cent of taxes and general grants.

ex ante in terms of budgeting, there are many control mechanisms to ensure that it is followed. Moreover, the time limit for balancing the budget following a deficit gives further weight to it.

Since the introduction of the balanced budget rule the number of municipalities with deficits has decreased.[27] While some of this improvement may be a result of the budget cycle, comparisons with other macroeconomic variables imply that the budget cycle is not the sole determinant. For example, the development of net incomes does not follow the same pattern as municipal tax revenues, investments or consumption. These variables are all highly pro-cyclical and they are furthermore important determinants of net incomes (see figures $A1$ and $A2$ in Appendix A).

5.2 Political landscape

There has been a shift towards softened bloc politics in terms of larger and more fragmented coalitions in recent years.[28] Coalitions comprising three, four and five parties have seen the greatest relative increase over time (see Table 1).[29]

Table 1. Number of parties in the municipal coalitions

	1994	1998	2002	2006	2010
1 party	99	34	45	33	34
2 parties	66	75	73	48	39
3 parties	51	55	66	51	74
4 parties	45	69	64	98	97
5 parties	13	33	27	47	42
6 parties	1	9	11	11	3
7 or more parties	3	3	3	1	1
Shifting majorities	10	11	0	1	0
Total	288	289	289	290	290

Source: SALAR and own calculations.

Note: The table presents the coalitions that were formed after the elections held in 1994, 1998, 2002, 2006 and 2010.

Following the standard of the Swedish Association of Local Authorities and Regions (SALAR), we classify a coalition to be right-wing if it comprises the Conservative Party, the Christian Democrats, the Liberal Party and/or the Center Party but not the Social Democrats or the Left Party. A coalition is defined as a "rainbow coalition" if any of the right-wing coalition parties is included as well as the Social Democrats and/or the Left Party. The Environmental Party and special interest parties may be included in any of the coalition types. Moreover, while the number of cross-ideological coalitions has seen few changes, there has been a shift in the representation on the left-right scale. For example, in 1994 there were 59 right-wing coalitions and 176 left-wing coalitions.

[27] For example, more than 90 per cent of all municipalities presented surpluses in 2012. This can be compared to, for example, more than 65 per cent of all municipalities presenting deficits in 1999, the year before the introduction of the balanced budget rule.

[28] Wångmar (2006) studies the development of the forms of governments in Swedish municipalities during the period 1952-2002. Interestingly, he argues that government formation can be classified into three main phases. In the first phase (1950-1970), the majority and the minority shared chairmanships of local administrations. In the second phase, one of the traditional political blocs generally had all the chairmanships in a municipality. The third phase, starting in the 1990s and continuing onwards, is characterized by softened bloc politics.

[29] For example, after the election in 1994, there were 99 single-party governments and 45 four-party coalitions. In 2010, however, there were only 34 single-party governments and instead 97 four-party coalitions.

However, in 2010 there were 141 right-wing coalitions and 109 left-wing coalitions (see Table 2).[30]

Table 2. Types of coalitions over time, based on ideology

	1994	1998	2002	2006	2010
Left-wing	176	127	125	90	109
Rainbow	53	61	61	43	40
Right-wing	59	101	104	157	141

Source: SALAR and own calculations.

Additionally, it is relatively common that coalitions break up during the mandate period (see Table 3).

Table 3. Number of coalition break-ups over time

1994-1997	1998-2001	2002-2005	2006-2009	2010-2013
11	33	9	32	28
4%	11%	3%	11%	10%

Source: SALAR and own calculations.

Finally, it is important to note that the formation of coalitions in Sweden largely works contrary to the theory of minimum-winning coalitions (MWC) proposed by Riker (1962). He suggests that coalitions should consist only of those parties that are just about able to pass the 50 per cent seat threshold together, since including superfluous parties would undermine their power. In Sweden, however, "oversized" coalitions are common.[31]

6 Data

This paper uses panel data for the years 1995-2012. The data set consists of data for all municipalities for all years in the time period under consideration. Due to municipal splits a few municipalities have been excluded from the analysis. The excluded municipalities are Nykvarn, Södertälje, Uppsala and Knivsta. Further, the Gotland municipality has been excluded since it also handles county council tasks.

6.1 Fragmentation data

Election data on vote shares, the total number of votes and assigned seats come from the Swedish Election Authority and Statistics Sweden. The data are publicly available on their websites. The data cover the elections held in 1994, 1998, 2002, 2006 and 2010.

[30] In 79 municipalities, the same "political ideology" has governed since 1994. Out of these municipal governments, 32 have been right-wing and 47 have been left-wing. One municipality has had a rainbow coalition since 1994.

[31] One reason for this is that national politics impact on municipal politics. For example, the central party offices of the right-wing parties encouraged their municipal branches to form "the same type of coalition" as in national politics to come across as more unified, even when this implied including superfluous parties into the coalition. For example, in Nacka municipality, the ruling coalition comprised of the Conservative Party, the Christian Democrats and the Liberal Party, which were sufficient for a majority. Yet, this coalition invited the Center Party to join, despite the fact that the Center Party made the coalition oversized.

Coalition data come from SALAR.[32] SALAR's coalition data set was initially compiled using data from Statistics Sweden and the Swedish Agency for Public Management. The data are publicly available on SALAR's website. SALAR have double-checked the validity of the data with the municipalities.[33]

6.2 Dependent variables

Data for the dependent variables come from Statistics Sweden. Aggregated data for the years 1998-2012 are publicly available on Statistics Sweden's website. However, the detailed economic data that are used for this paper are, while being public, only available upon request. We received unprocessed data from the Institute for International Economic Studies (IIES) in Stockholm. Due to the low quality of some of the 1995-1997 data from IIES, a separate file was obtained from Statistics Sweden with complementary statistics on key variables of interest. The complementary Statistics Sweden data were used for the balance sheet and financial statement of income variables for the years 1996 and 1997.[34]

The economic data is based on the yearly financial accounts (FA) of the Swedish municipalities. FA is an annual collection of data conducted by Statistics Sweden where data on approximately 3,000 variables are collected. The data set includes information on both the municipalities as well as their corporations. All variables have been deflated to 2012 prices. Consumer price index (CPI) from Statistics Sweden has been used as deflator.[35]

Prior to 1998, the quality of the municipal financial accounts is generally low and the accounting definitions of the variables are often not consistent over time.[36] However, this study mainly uses variables that are concordant, and variables where the accounting principles have changed have been taken into consideration. While data are available for 1994 it is not used since, for example, the accounting principles of relevant variables in the operation accounts changed significantly between 1994 and 1995.

Importantly, accounting principles for the pension debt changed in 1998, where some additional costs (such as write-offs) were added to the annual financial result. While this has been taken into account, pre- and post-1998 data for the annual financial result are still not entirely concordant. While the differences are expected to be relatively small, there is a tendency to overestimate the fiscal situation prior to 1998,

[32]Note that there will be some differences in the coalition data that this paper presents and the coalition data that SALAR present in e.g. reports available on their website, since SALAR generally use the coalitions at the *end* of the mandate period while we use the coalitions created directly following the election.

[33]Using the SALAR coalition data, two almost identical data sets were created. The first takes account only of what coalition formed directly after the election, while the second takes into account whether there were any changes made to the coalition during the mandate period. In some cases, it is stated in the data from SALAR whether the coalition broke up and which parties replaced it, but not *when* the coalition ended. In such cases, we have made the assumption that the new coalition was installed halfway into the mandate period. This seems like a reasonable assumption given the timings of the coalition break-ups that we do have data for.

[34]Data for the municipal corporations were missing in the complementary data. Therefore, it is not possible to test the common pool hypothesis on corporate debts and deficits in section 8.

[35]Note, however, that the Swedish Ministry of Finance sometimes uses a different deflator for municipal expenditures, arguing that local government expenditures develop differently from the rest of the economy. In line with previous literature, we chose to use the standard CPI. Estimations using the deflator from the Swedish Ministry of Finance is, however, performed in the robustness checks in section 8.3.

[36]For a more exhaustive discussion on the quality and validity of the data along with information on the concordance over time, see the FA data set documentation available on Statistics Sweden's website.

since not all relevant costs are included in the annual financial result by this time.[37] The deficits in the early 1990s will therefore not appear as severe as they actually were by current accounting standards.

Finally, in an ideal world the hypotheses would also be tested using reliable investment data.[38] While we do have detailed investment data for the whole time period under consideration, several aspects contribute to making this data unreliable. Apart from quality issues, there exist no official investment data for the municipal corporations.[39] Yet, the municipal corporations make a significant share of all municipal investments.

6.3 Control variables

Data for the control variables come from Kolada and Statistics Sweden and are all publicly available.[40] The data used for the control variables are all official Swedish data and validity can be considered to be of good accuracy.

7 Empirical strategy

In this section the empirical strategy is motivated. Firstly, estimation issues and the model specifications are presented and discussed. Secondly, the variables included in the models are described.

7.1 Economic model

In short, we want to test the hypotheses of the paper using variants of the following dynamic model[41]:

$$Y_{i,t} = \beta_0 + \beta_1 Y_{i,t-1} + \beta_2 FRAGMENTATION_{i,t} + \beta_3 CONTROLS_{i,t} + v_{i,t} \quad (3)$$

where Y is the fiscal variable of interest (net income, debt or public expenditure), $FRAGMENTATION$ is a vector of political variables, $CONTROLS$ is a vector of control variables (such as age distribution, taxpaying power, population etc.) and v is the composite error term. While fragmentation is expected to affect the *levels* of the economic variables according to the common pool hypothesis, veto player theory instead predicts an impact on (absolute) *changes* between time periods. The use of lagged dependent variables is motivated both by economic theory as well as autocorrelation tests.[42] However, a number of estimation issues are associated with Equation 3, the

[37] This will, at worst, lead us to *underestimate* the effect of the balanced budget rule.

[38] Moreover, apart from quality issues there are normally long lags between investment proposals and implementation, making it hard to differentiate whether investments carried out during year i were the consequence of the current coalition or the initiative of previous decision-makers.

[39] Kommuninvest collect municipal corporation investment data from 2007 and onwards.

[40] Kolada is an online database that contains data related to the Swedish local government sector.

[41] Previous studies (see for example Roubini and Sachs 1989; Blom-Hansen et al. 2006; Perotti and Kontopoulos 2002; Hagen and Vabo 2005; Wehner 2010; Jochimsen and Nuscheler 2011; Borge 2005; Tovmo 2007; Ashworth et al. 2005; Schaltegger and Feld 2009; de Haan and Sturm 1997; Le Maux et al. 2011; Baskaran 2013; Eslava and Nupia 2010; Riciutti 2004; Volkerink and de Haan 2001; Coate and Knight 2011; Geys 2007; de Haan et al. 1999; de Haan and Sturm 1994; Bräuninger 2005; Huber et al. 2003) have used similar approaches.

[42] For example, we use the Wooldridge (2002) test for autocorrelation in panel-data models. Drukker (2003) shows that this test has good size and power properties in reasonably sized samples. The test can be applied under general conditions, and it does not make as specific assumptions about the nature

majority of which are related to the fact that *both* lagged dependent variables and fixed effects should be used.[43]

In Equation 3, the composite error v_{it} comprises fixed effects (a_i) and time varying factors (u_{it}), where a_i can be differenced out. First differencing indeed removes the fixed effects, but there remains a correlation between the lagged dependent variable and the error term. The issue arises because the differenced residual, $\triangle\varepsilon_{it}$, is necessarily correlated with the lagged dependent variable since both are a function of $\triangle\varepsilon_{it-1}$. More precisely, if a case of fixed effects is considered with $\tilde{y}$ and $\tilde{\varepsilon}$ as the centered y and the error, we get:

$$\tilde{y}_{i,t-1} = y_{i,t-1} - \frac{1}{T_i}\sum_{t=1}^{T_i} y_{i,t-1}$$

$$\tilde{\varepsilon}_{i,t-1} = \varepsilon_{i,t} - \frac{1}{T_i}\sum_{t=1}^{T_i} \varepsilon_{i,t}$$

Looking at the two equations above, it is evident that the error term, $\varepsilon_{i,t-1}$ is contained with weight $1 - \frac{1}{T_i}$ in $\tilde{y}_{i,t-1}$ and with weight $\frac{1}{T_i}$ in $\tilde{\varepsilon}$. Then, clearly $E[\tilde{y}_{i,t-1}\tilde{\varepsilon}_{t,t}] \neq 0$, which will bias the estimate (Beck and Katz 2009). Nickell (1981) first noted that the panel data estimates of regression equations such as Equation 3 do not yield consistent estimates (see also Hurwicz 1950).

There exist remedies to the above-mentioned problem of biased estimates. The instrumental variable (IV) estimator (Anderson and Hsiao 1982) and the generalized method of moments (GMM) estimator (Arellano and Bond 1991) are widely used for dynamic panel models in order to get consistent estimates. The basic intuition is that one instruments the lagged dependent variable and similarly endogenous variables with longer lags. Holtz-Eakin et al. (1988) found ways to improve the efficiency of the Anderson-Hsiao (AH) estimator by building a set of instruments from the lag, one for each time period, and substituting zeros for missing observations. Similarly, Arellano and Bond (1991) argued that additional instruments can be obtained in a dynamic panel data model if one utilizes the orthogonality conditions that exist between lagged values of y_{it} and the disturbances v_{it}, yielding Difference-GMM (DGMM) or the AB estimator.

The Arellano-Bover/Blundell-Bond (BB) estimator augments the AB estimator by making an additional assumption that first differences of instrument variables are uncorrelated with the fixed effects (Arellano and Bover 1995; Blundell and Bond 1998). The BB estimator is known as System-GMM (SGMM) since it builds a system of two equations, the original equation and the transformed one. More instruments are used in SGMM than in DGMM, which may improve efficiency.[44]

of the individual effects, or test for the individual-level of test for the individual-level effects jointly, as many other tests (see Baltagi (2001) for a discussion of different tests). The null hypothesis is that there is no serial correlation, and we use preliminary specifications without any lagged dependent variables. The null hypothesis of no serial correlation is rejected.

[43]Preliminary tests, such as the Hausman tests, indicate that fixed effects should be used. This makes sense, since fixed effects, such as location of the municipality, are likely to be important determinants of economic outcomes. Interestingly, Angrist and Pischke (2009) and Guryan (2004) note that fixed effects and lagged dependent variables have a useful bracketing property. If just using lags is a correctly specified model, but one mistakenly uses fixed effects, estimates of a positive treatment effect will tend to be too big. On the other hand, if a fixed effects model is correct but we mistakenly also include lags, estimates of a positive treatment effect will tend to be too small. Thus, it has been suggested that one may consider fixed effects and lagged dependent variables as bounding the causal effect of interest (given some assumptions about the nature of selection bias).

[44]See Roodman (2009a) for a full description of DGMM and SGMM.

While IV and GMM estimators may appear ideal at first sight[45], they also imply estimation issues. The AB estimator instruments differences with levels, yet, past levels do not seem to be very good predictors of current changes for the variables that this paper considers. Moreover, a central problem with any IV estimator is that it may increase the mean squared error if the instrument is not highly correlated with the variable. Furthermore, given that variation is already relatively low, the differencing of variables in DGMM eliminates important variation. We include a fairly large number of variables as well as time periods, and GMM generally becomes weak when using many instruments. For example, the Hansen overidentification test often fails to detect when the instruments as a group are invalid when there are many instruments (Roodman 2009b).[46]

Next, the BB estimator instruments levels with differences, and for the budget variables under consideration it is plausible that past changes are predictive of current levels. However, the assumptions required for SGMM are quite restrictive. For SGMM estimates to be valid, it is required that throughout the study period deviations from long-run means are not systematically related to the fixed effects. SGMM is consistent *only* if the data-generating process is such that the fixed effect and the autoregressive process governed by α, the coefficient on the lagged dependent variable, offset each other in expectation across the *whole* panel (Roodman 2009a).[47]

Thus, all available methods have drawbacks. While estimates from least squares dummy variable (LSDV) regressions may suffer from endogeneity bias, estimates from SGMM will not be consistent should the required assumptions not hold. Angrist and Pischke (2009) and Beck and Katz (2009) are generally critical of using GMM estimators instead of LSDV as T increases, since the difference in performance is not always great and computational issues may become more important. Beck and Katz (2009) argue that, in general, there is little reason not to prefer LSDV over the Kiviet estimator (further discussed below) when T is twenty or more, and they discourage from using the AH estimator when T is quite large. Indeed, a range of previous research refers to the property that endogeneity bias diminishes as T gets larger. For example, Ricciuti (2004), referring to the works of Bun and Kiviet (1999) and Judson and Owen (1999), conclude that for the panel of the size that they consider (where $T = 20$, only two more years than in this paper), the gains obtained using more complex methods are very small compared with the LSDV.

A number of Monte Carlo studies have been performed to evaluate which estimator that has the best performance, but the findings depend on the relative sizes of T and N. Judson and Owen (1999) find that bias-corrected LSDV estimates generally are preferred. However, the main drawback of the Kiviet (1999; see also Kiviet 1995) bias approximation is that it, unlike the GMM estimators, assumes that *all* other right-hand side variables apart from the lagged dependent variables are strictly exogenous to the dependent variable.

Given the discussion above, we use the LSDV estimator as our main approach. The main motivation is that it is the computationally most straightforward method. The LSDV estimator is also the standard in previous literature, and therefore enables comparisons.[48] To take potential endogeneity bias of the LSDV estimator into account,

[45]They are well-suited for data sets/models with 1) few time periods and many individuals, 2) a linear functional relationship, 3) one left-hand side variable that is dynamic, depending on its past realizations, 4) independent variables that are not strictly exogenous, 5) fixed effects and 6) heteroskedasticity.

[46]When there are many instruments they also tend to overfit the instrumented variables and bias the results toward those of OLS/GLS.

[47]Basically, the process should work much like investment and depreciation in a Solow growth model steady state.

[48]The most common approach in previous literature is LSDV. Furthermore, most previous specifi-

we also compute bias-corrected least squares dummy variable (LSDVC) estimates and their bootstrap variance-covariance matrices.[49] For all LSDV estimates that are presented in the main body of the text, we also present corresponding tables including the bias-corrected estimates in the appendix.

Bun and Kiviet (2003) show that the Kiviet (1999) approximation often accounts for virtually 100 per cent of the bias, and never less than 90 per cent. In order to correct the bias, we use both the AB and BB estimators as the initial consistent estimate of the coefficients. The standard errors were bootstrapped.[50]

Further, we also use the bracketing property of the LSDV-OLS range. Estimates from LSDV and pooled OLS effectively work as upper and lower values of the true estimates, and Bond (2002) points out that these bounds provide a useful check on results from theoretically superior estimators. Finally, further controls of the plausibility of the estimates by comparing the LSDV estimates with those obtained when using SGMM and DGMM were done.[51]

Therefore, while there is likely to remain some inaccuracy with respect to the exact point estimates, our empirical strategy should be well-suited in terms of estimating the bias, variance and direction of the effects, and therefore give valid approximations of the effects of interest.

7.1.1 Heteroskedasticity

Some preliminary tests are conducted to understand the properties of the data better. First, the Breusch-Pagan Lagrange Multiplier (LM) test is used. Trying several functional forms, the tests indicate heteroskedasticity, which indicates a need to construct a robust covariance matrix estimator. Robust standard errors are therefore used.

7.1.2 Stationarity

There are several panel cointegration tests, and some related literature test for data non-stationarity (see for example Ashworth et al. 2005).[52] Using the Im, Pesaran and Shin (2003) test for panel stationarity for the main economic variables, we indeed find numbers close to one, indicating cointegration. However, there are several reasons why we choose not to proceed with, for example, an error correction model (ECM). First, $T = 18$ and as pointed out by Beck and Katz (2009), during short time spans we often

cations are linear, however see for example Franseze (2010) and Tornell and Lane (1999) for non-linear models. A few studies use an IV approach, and there are also a few studies evaluating common pool problems using regression discontinuity design (see for example Freier and Odendahl 2012; Garmann 2013), methods that are however not suitable to the institutional setting that this study examines. Some studies also include both GMM and FE. For example, Perotti and Kontopolous (2002) re-estimate their results with the Arellano-Bond estimator and the Anderson-Hsiao estimator. The same thing goes for Borge (2005). Baskaran (2013) uses LSDV, but reports estimates for DGMM and SGMM as well as the Anderson-Hsiao estimator in the appendix. Ashworth et al. (2005) also try a direct estimation using GMM. Similarly, Bawn and Rosenbluth (2006) report OLS regressions as well as Arellano-Bond GMM estimates.

[49] Judson and Owen (1999) find, using a Monte Carlo approach, that the bias of LSDV for dynamic panel data models can be quite sizeable, even when $T = 20$. In fact, Judson and Owen (1999) argue that even with a time dimension of $T = 30$, the bias may be equal to as much as 20 per cent of the true value of the coefficient of interest.

[50] Due to the very large number of estimations and tables generated, the number of repetitions (first set to 100) were relaxed to speed up estimation time.

[51] Moreover, in line with Roodman (2009a), orthogonal deviations are used to maximize sample size when estimating with GMM.

[52] A stationary series is a series such that $E(y_{i,t}) = \mu$, $Var(y_{i,t}) = \sigma^2$ and $E(y_{i,t}, y_{i,t-k}) = \sigma_k$. Integrated data do not have equilibria. Instead, shocks to the series will accumulate forever. If the data is integrated of order one, $I(1)$, the data is not stationary but the first difference of the data is.

observe very few cycles. Thus, a series that seems persistent during a short time period may be either non-stationary or stationary in the long run.

Moreover, Beck and Katz (2009) note that while it is relatively common to find evidence of a unit root, they rarely make sense for political economy data. Making an argument similar to that of Alvarez and Katz (2000), if our series had unit roots, there would be a tendency for them to diverge far from their means and the variance of the observations would grow larger and larger over time. Both the proportion of, for example, the budget spent on providing welfare services and political vote shares are by definition confined between zero and hundred per cent, which bounds how large their variances can become. Finally, if either series in our data were $I(1)$, then we would be equally likely to see an increase or decrease in either variable regardless of its present value. Yet, it does not seem plausible that there is no tendency for e.g. investment expenditure to rise when it is low and to fall when it is high.

7.1.3 Reverse causality

It is also essential to address the issue of reverse causality. The fiscal situation in the municipalities may affect how the municipal inhabitants vote. For example, worse financial outcomes may give rise to fragmented governments being elected. However, we evaluate the financial outcomes of municipality i at the end of year t, where the policy-makers are given at the onset of year t. Therefore, the scope for reverse causality should be small given that, for example, the aggregated expenditure levels at the *end* of year t do not affect the degree of political fragmentation at the *beginning* of year t. To furthermore deal with the potential issue of reverse causality, we make controls with System-GMM estimations in which the fragmentation variables are treated as endogenous.

7.1.4 Confounding factors

As will be discussed more in section 7.2.3, the balanced budget rule is measured by a dummy variable that is equal to 1 from 2000 and onwards, and 0 otherwise. Since the balanced budget rule variable is effectively a time dummy it will, if not using appropriate controls, pick up the effects of developments of other fiscal determinants. Therefore, if not considering the developments of relevant variables over time, the balanced budget rule dummy will capture *more* things than just the effect of the balanced budget rule.

We have carefully chosen our control variables, presented in section 7.3, in order to account for the time developments of variables that, if omitted, mistakenly may be interpreted as effects of the introduction of the balanced budget rule. Below we elaborate on some important developments as well as our efforts to try to solve the estimation issues associated with these developments.

Importantly, municipal investments have increased since the balanced budget rule was introduced. According to SALAR (2013), one contributing factor to the surge in investments in recent years is increased urbanization. For example, many municipalities with positive population growth have seen the need to replace municipal buildings built in the 1960-70s with new schools and infrastructure. Moreover, SALAR (2013) discuss demographic change as a driver of investments. For example, more elderly homes have been built in recent years.

Municipal investments are strongly correlated with long-term debt levels, and increased investments are generally considered to be an important explanation to why municipal long-term debts have increased during the 2000-2012 time period. Furthermore, knowing that investments will be required in the future may be associated with

higher surpluses. If municipalities save in order to make investments, this would be a plausible explanation to why municipal net incomes have increased over time.

The relative importance of municipal corporations has also increased over time, which is crucial to account for when testing hypothesis 4. Generally, there is little research on *why* the relative importance of municipal corporations has increased. A common explanation is that since municipal corporations make a significant share of municipal investments, their relative importance has increased as the total level of investments has increased.

The tasks of municipal corporations have also increased. Previously, municipal corporations were mainly responsible for areas/activities relating to energy, real estate and infrastructure. However, in recent years it has become more common for municipal corporations to also provide, for example, cultural services. A possible explanation for this development may be political preferences. As discussed in section 5, there has been an increase in the number of right-wing coalitions in Sweden, and right-wing coalitions are often larger in terms of the number of participating parties. If it is the case that right-wing politicians prefer to run their activities through corporations, the development towards a greater relative importance of municipal corporations may simply be explained by current policy-makers having different preferences than previous policy-makers.

Additionally, the strong financial results in recent years have been affected by temporary incomes, such as the repayments of municipal insurance premia from AFA Insurance, which greatly increased many municipalities' net incomes in 2012. Moreover, the 2008 financial crisis led the Swedish central government to temporarily increase government grants quite significantly. If not taking account of such events, we are likely to falsely overestimate a positive effect of the balanced budget rule on fiscal discipline.

In order to account for the critical developments discussed above, we have included e.g. population density, age distribution of the population, political ideology and government grants etc. in our analysis. These variables are meant to control for the main drivers of investments as well as additional trends in sources of revenue and expenditure, which are likely to affect how the dependent variables under consideration change over time.

7.2 Model specifications

The models for testing the hypotheses of the paper are presented below. Firstly, two models for testing the common pool hypothesis (hypothesis 1 and 4) and status quo bias (hypothesis 2) are defined. Next, in order to test hypothesis 3, the models are extended to include the introduction of a balanced budget rule.[53] All variables are presented and discussed in section 7.3.

7.2.1 Hypothesis 1 and 4

To test hypothesis 1 and 4 the following equation is specified:

$$Y_{i,t} = \beta_0 + \beta_1 Y_{i,t-1} + \beta_2 FRAGMENTATION_{i,t} + \beta_3 CONTROLS_{i,t} + v_{i,t} \quad (4)$$

[53]Note that the balanced budget rule dummy is included also in model 1 and 2, and that the main difference when testing hypothesis 3 is the additional interaction terms.

where $CONTROLS$ is a vector of control variables, $FRAGMENTATION$ is the fragmentation variable/variables and Y is either debt, net income or public expenditure. The set-up is similar to that of Equation 3 presented in section 7.1. The specification and control variables differ depending on which dependent variable is evaluated.[54] Lagged values of the dependent variables are used in all specifications. Following the standard of previous literature, we use one lag.[55] It is expected that political fragmentation will have a positive and significant effect on debt and public expenditure, and a negative and significant effect on net income.

We use municipality fixed effects, but mainly refrain from including year fixed effects. The reason is that year specific variation between the elections is important when considering the behavior of policy-makers; the behavior of fragmented governments is likely to be exacerbated in election years, given that policy-makers are expected to be opportunistic. For example, Jochimsen and Nuscheler (2011) also refrain from adding year fixed effects in order to be able to test for political opportunism, where variation over time is essential. Thus, rather than using year fixed effects we mainly use dummies for election year effects as well as a linear time trend.[56]

7.2.2 Hypothesis 2

Secondly, to test hypothesis 2 the following equation is specified:

$$\Delta NET\ INCOME_{i,t} = \beta_0 + \beta_1 NET\ INCOME_{i,t-1} + \beta_2 FRAGMENTATION_{i,t} + \beta_4 CONTROLS_{i,t} + v_{i,t} \quad (5)$$

Similar to Equation 4 in section 7.2.1, $FRAGMENTATION$ is the fragmentation variable/s, $CONTROLS$ is a vector of control variables and $NET\ INCOME$ is the annual financial result.

Equation 5 is a variant of the model of Roubini and Sachs (1989), which has been used in several subsequent studies.[57] Roubini and Sachs (1989, p. 920) state that the purpose is to "describe the (basic) dynamic response of budget deficits to the major macroeconomic shocks in semi-reduced form equation", rather than to present a structural model. Roubini and Sachs (1989) use the change in the annual deficit, measured as the change in the public debt-GDP ratio, as their dependent variable. To test whether fragmented governments imply slower stabilization of deficits, they include an interaction term between a power dispersion index and a post-1974 dummy variable (since many countries encountered fiscal problems during the 1973 financial crisis, and subsequently needed stabilizations).

Similarly, this paper aims to evaluate whether the stabilizations of deficits following the 1990s financial crisis were slower in municipalities with fragmented policy-makers. However, the accounting principles for debts and financial results in Swedish municipalities differ, and since we are interested in the recovery of municipal deficits, the change in debt is not a preferred measure. For example, if there are debt financed investments in year t, debt will consequently increase, but the deficit will be unchanged,

[54] See the regression outputs for exact specifications.

[55] Preliminary tests indicate that the second and third lags of the dependent variables are generally insignificant or have a small effect. In the robustness checks of the paper, discussed in section 8.3, the lag lengths are increased.

[56] However, the Arellano-Bond GMM control estimates are *all* specified using year fixed effects. Roodman (2009a) encourages the use of year dummies as the autocorrelation test and the robust estimates of the coefficient standard errors assume no correlation across individuals in the idiosyncratic disturbances, and time dummies make this assumption more likely to hold.

[57] For example, Huber et al. (2003), de Haan et al. (1999) and de Haan and Sturm (1997) use a similar set-up of the model where the debt-to-GDP ratio is used as the dependent variable.

other things being equal. Thus, unlike the central government case, the yearly change in debt does not translate into the deficit in the case of municipalities.

Therefore, this paper uses the change in net income between years as a proxy for fiscal improvements in Swedish municipalities. The term "status quo bias" imply that changes in, for example, expenditure and net income between years should be smaller in municipalities where there are fragmented policy-makers, since it is expected to be harder for fragmented policy-makers to agree on policy-changes.[58] As most municipalities have transitioned over time from having small or negative net incomes to having positive net incomes, we hypothesize that the changes in net incomes between years are generally smaller for fragmented policy-makers. When there is a deficit (negative net income) in year $t-1$, we expect a positive change in net income in year t, i.e. we expect that expenses are cut or revenues increased such that the deficit is smaller in the following year. It is important to note that this is an *assumption* that we make. Since year 2000, this assumption is, however, supported in the sense that municipalities must recover deficits *within* three years. Following a deficit, surpluses in subsequent years are required and therefore reasonable to expect.

A relevant and valid objection to Equation 5 is that it fails to differentiate between deficits and surpluses. If one just uses the change in net income, it will capture differences over years *both* in terms of surpluses and deficits. While there have been years where a large share of all municipalities run deficits, particularly in the late 1990s, there are for *all* years always at least some municipalities with surpluses in our data set. Consider a case in which municipality i has a net income of D in year t and S in year $t-1$, while municipality j has a net income of S in t and D in $t-1$, where $S > D$. The absolute change of net income between t and $t-1$ is the same in the two municipalities, where municipality i has seen a negative change in the financial result whereas j has seen a positive change. Since the absolute change is the same, status quo bias (if it exists) is, by definition, the same in the two municipalities.

Yet, if using data including both surpluses and deficits, the implication (given the above explanation of us being interested in negative changes) will be that municipalities in which the net income has decreased are *more* subject to status quo bias. However, if net income generally decreases by large amounts in fragmented municipalities, the decreases may might as well be due to common pool behavior, where the fragmented governments increase their spending in a year, thus negatively affecting net income. If so, the negative changes in net incomes are not a result of fragmented governments not being able to agree on policy adjustments to recover their deficits, but rather an indication of them indeed cooperating, but on expenditure increases as opposed to spending cuts.

The motivation for still using the approach of analyzing changes in net income, despite the estimation issues associated with such an approach, rests on some key assumptions. Firstly, a majority of municipalities have historically had large deficits, which have been stabilized over time. As already mentioned, we argue that it is reasonable to expect that the municipalities will aim to increase net incomes (which has also been the case in the time period under consideration), but that the changes over years should be smaller as fragmentation becomes larger. Given the large number of deficits in the sample, we do not expect the surpluses to be of great concern. Moreover, as explained in section 3.2, veto players are likely to block policies when they fear that negative outcomes will impinge upon the interests of their respective constituencies. While this is relevant in terms of surpluses as well, we believe that the prevalence of veto players blocking policies will be larger in tough fiscal contexts. Additionally,

[58] Importantly, it should be noted that changes over years are not necessarily the consequence of policy-makers, but may rather be the result of other unforeseen events. Therefore, we have done our best to control for factors that are likely to affect the dependent variables under consideration.

municipal expenditure often tend to increase more than revenue.[59] Therefore, if a municipality leave things "as they are" in a year, net incomes are more likely to decrease than increase. Therefore, we argue that positive changes in net incomes over years generally requires more cooperation than decreases in net incomes do. Given the, more or less, "automatic" increases in expenditure, we believe that negative changes in net income better capture municipalities that have not been able to make necessary policy adjustments by firing people etc.

Moreover, we also include an interaction term between net income and fragmentation and try both specifications, with and without the interaction term. The reason for including the interaction term is that the level of net income may matter for the impact of political fragmentation in a veto player setting. The intuition is the following: the smaller net incomes per capita, the worse the fiscal situation in the municipality and the larger policy adjustments may be required. The larger the policy adjustments required, the more likely it is that cuts will affect several different political interests, and therefore that more parties will use their veto powers and oppose the necessary cuts.

The use of Equation 5 thus requires making several assumptions, which are not entirely in line with veto player theory. To account for these assumptions, we also make control estimations where the "standard" veto player approach is used. In the spirit of veto players, fragmented governments are expected to find *any* policy changes more difficult than their less fragmented counterparts, irrespective of whether there are surpluses or deficits. Thus, Equation 5 was also evaluated using the *absolute change* in net income. We found that the larger political fragmentation is, the smaller are the absolute changes in net income over the years. The coefficients when using absolute values of net incomes were, however, smaller than in the first case. The estimates are available upon request.

Finally, it is worth noting that one should view Equation 5 as a *complementary* approach to consider collective action problems rather than a robust approach for measuring status quo bias. We want to emphasize the fact that this approach is *inspired* by veto player reasoning, raher than a veto player model on its own. Thus, if the sole purpose of the analysis was to evaluate the exact impact of status quo bias on economic outcomes, one should opt for another method (more on this in section 8.4).

7.2.3 Hypothesis 3

In order to test hypothesis 3 minor changes are introduced to Equation 4 and Equation 5. To test hypothesis 3 in terms of the common pool hypothesis, Equation 4 is extended to include an interaction term between political fragmentation and a balanced budget rule dummy variable.[60] The effect of political fragmentation on the dependent variables is expected to be positive (negative for the deficit). The interaction term is expected to be negative (positive for the deficit), since the balanced budget rule is hypothesized to mitigate the effects of political fragmentation in line with the theoretical implications derived in section 3.1.

For testing hypothesis 3 in terms of status quo bias (hypothesis 2) we do not include an additional interaction term, but rather use Equation 5 and separately test it for the years 1995-1999 and 2000-2012.[61] Status quo bias is expected to be smaller upon the

[59] This can be noted by analyzing, for example, the financial statements of the municipalities.

[60] Variation *over* time is used since the the balanced budget rule was introduced in all municipalities at the same time.

[61] A three-way interaction would imply including all three independent variables, all three pairs of two-way interaction terms, and the three-way interaction term. We do, however, include an interaction term between the balanced budget rule dummy and fragmentation in specifications where the interaction term from Equation 5 has been excluded.

introduction of the balanced budget rule. Therefore, it is expected that there will be effects of political fragmentation on the changes in net income using the 1995-1999 sample, but smaller or no effects using the 2000-2012 sample.

Individual coefficients may not always have a straightforward interpretation. When they do, it is important to remember that in a regression equation like the following:

$$Y_i = \beta_0 + \beta_1 X_i + \beta_2 Z_i + \beta_3 Z_i * X_i + \varepsilon_i$$

β_1 is *not* the average effect of X_i on Y_i, which is instead given by $\frac{\partial Y}{\partial X} = \beta_1 + \beta_3 Z_i$. Thus, sometimes it may initially appear as if individual coefficients are counterintuitive. However, if the interaction effects are accounted for, the individual coefficients have the expected signs (as shown in section 8).

7.3 Variables

All variables used in the analysis are described below. The exact accounting definitions of the economic variables are available upon request. They are also publicly available on SALAR's website.

7.3.1 Fragmentation

The central variable of interest is political fragmentation, where the main point of focus is government fragmentation.[62] The notion of political fragmentation is quite broad, and it differs in common pool and veto player theory. In common pool theory, the absolute number of participants in the decision-making process is more important, whereas in veto player theory it is primarily the relative importance of the participating agents that matters.

Furthermore, as shown in the literature review, measures of political fragmentation differ from study to study. Apart from some of the voting power indices used in previous literature, this paper considers most previous fragmentation measures in order to evaluate whether the results are robust to different measures of fragmentation.[63] While the hypotheses are tested for all fragmentation measures, this study mainly reports estimates of the Herfindahl index (described below). The motivation for choosing the Herfindahl index is that it is a relatively good proxy of fragmentation in *both* common pool and veto player theory.[64]

Fragmentation encompasses the following variables in our analysis:

RAINBOW is a dummy variable that equals 1 if the municipality has a "rainbow coalition" in which different ideologies are represented. Rainbow coalitions are considered to be more fragmented than either right-wing or left-wing coalitions, and it is thus expected that rainbow coalitions will find it more difficult to find common grounds.

IDEOLOGICAL FRAGMENTATION is the maximum absolute ideological distance between the members of the coalition. In line with previous studies, the parties

[62] Council fragmentation is also evaluated and is expected to have similar effects.

[63] Consider a municipality i in election year t. Assume that i has been governed by coalition a since year $t-4$, and that a new coalition b replaces a in the election held in year t. Should one use the fragmentation of coalition a or b when considering the economic outcomes in year t? Coalition a indeed prepared the budget bill for t in $t-1$, and they were in power during most of t. Yet, coalition b may decide to make significant changes once they get to power and strongly affect the economic outcomes in year t. Following the standard in the budget cycle literature, it makes most sense to use fragmentation in the year prior to the election since the newly elected government has relatively little time to affect spending levels during the election year and since parties seeking re-election are expected to "act strategically" *prior* to the election. In our case, however, the decision between the two is quite arbitrary given that new governments may actually have an impact also after the election.

[64] However, the Herfindahl index is a worse measure for evaluating status quo bias than it is for evaluting the common pool hypothesis, since it fails to capture the actual presence of veto players.

in the coalition have been assigned a number on a $1-8$ scale depending on their policies.[65] For example, if the Conservative party (7) and the Left party (1) are in a coalition together, the absolute distance between them will yield an index value of 6.

Additionally, dummies for the number of parties in the municipal government are included. $P2$ is a dummy variable that equals 1 where there are two parties in the coalition, 0 otherwise. $P3$ is a dummy variable that equals 1 where there are three parties in the coalition, 0 otherwise, etc. Comparisons are made with single-party governments.

The hypotheses are also tested using the Herfindahl index (see Laakso and Taagepera 1979). The Herfindahl index is defined as:

$$HERFINDAHL = \sum_{p=1}^{P} SH_p^2$$

where SH_p is the share of representatives from party p. The index takes the maximum value of 1 when a single party holds all the seats in the municipal council, while the minimum value of $1/P$ is obtained when the seats are equally divided.[66] Similar to for example Persson et al. (2007), $1 - HERFINDAHL$ is used in the estimations.[67] The Herfindahl index depends both on the number of parties in the coalition/council as well as on their relative seat shares (see Table 4).

Table 4. Sample values of the Herfindahl index

Calculation	1-Herfindahl
$1-(0.3^2+0.4^2+0.3^2)$	0.66
$1-(0.1^2+0.4^2+0.5^2)$	0.58
$1-(0.9^2+0.1^2)$	0.18
$1-(0.5^2+0.5^2)$	0.5

Note: For example, the top row illustrates the Herfindahl index of a government that comprises of three parties with 30, 40 and 30 per cent of the seats respectively.

The inverse of the Herfindahl index yields $ENOP$, which represents the effective number of parties in the coalition. Unlike the dummies for the number of parties in the coalition, this measure also takes account of the size inequality of the participating parties in the coalition. Following, for example, Volkerink and de Haan (2001) the variables $ENOP\_OPPOSITION$ and $ENOP\_COUNCIL$ are also used, where consideration is taken of the fragmentation of the opposition and the council.[68] Moreover, the squared values of $ENOP$ are included to allow for possible non-linear effects of fragmentation.

[65] Ideological standpoints of special interest parties are unknown. The categorization of the other parties follows the left-right scale.

[66] Since no single party ever holds all seats in the council during the time period under consideration, the Herfindahl index is never equal to 1 in our data set. Morover, in order to calculate the Herfindahl index of the coalition, we need to use the relative seat shares in the council as weights since this is the only way to get information on their approximate "importance/strength" in the coalition. If considering a central government, one could instead have looked at e.g. the number of ministers from each party. Therefore, even when there are single-party governments, the Herfindahl index of the coalition will not be equal to 1 either.

[67] Due to the fact that interpretation is facilitated by this formulation; larger values on the Herfindahl index then implies *more* fragmentation.

[68] This type of analysis is performed for the normal Herfindahl index as well. Note that estimates for $ENOP\_OPPOSITION$ are not presented.

Finally, $MINORITY$ is a dummy variable that equals 1 if the municipality has a minority government, 0 otherwise.[69]

The two most frequently used power indices are the Banzhaf index (Banzhaf 1965) and the Shapley-Shubik index (Shapley and Shubik 1954), which first appear suitable for testing status quo bias. However, the additional effort to calculate them is expected to yield little added value to this paper. These indices consider the ability *to make or break* a coalition (thus suitable for identifying veto players), giving weight to small parties in coalitions that are able to break the coalitions. This may, however, not be the best measure since coalitions in Sweden are often oversized and the "threat" of one party breaking the coalition loses its straightforward intuition provided in the case of MWCs.

7.3.2 Dependent variables

$DEBT$ measures municipal long-term debts. Long-term debts are used since short-term debts mainly reflect temporary imbalances (see Ashworth et al. 2005 for similar reasoning).[70]

$PUBLIC\ EXPENDITURE$ measures expenditure, both using aggregate public expenditure as well as expenditure for individual spending categories such as infrastructure, education, health care, culture and political activities. The inclusion of spending categories is made in case coalition governments spend more or less on certain categories, which does not necessarily have any effect on aggregate expenditure.

$NET\ INCOME$ measures the annual financial result.[71] It is expected that more fragmented governments will have worse financial results, although it has been noted by Perotti and Kontopoulos (1999) that the theoretical case for an effect of fragmentation on expenditure is much stronger than for the effects of the deficit since common pool theory is less clear on predicting revenues.[72]

7.3.3 Control variables

BBR is a dummy variable for the balanced budget rule. BBR takes on the value 1 from 2000 onwards, 0 otherwise.

$LEFT-WING\ IDEOLOGY$ is a dummy variable that takes on the value 1 if the coalition is left-wing, 0 otherwise. It is often expected that left-wing parties are more tolerant of larger deficits, which results from the assumption that the political left is generally more spendthrift (see Hibbs 1977; Schmidt 1996).[73]

$POPULATION$ equals the municipal population, measured in logs in order to reduce the effects of outliers.[74] Larger municipalities are likely to have higher demands for public expenditure, leading to higher levels of public debt. Municipalities

[69]The effects of minority governments are however not theoretically founded, and this measure is thus included mainly to enable comparisons with previous studies.

[70]The cost of debt financing has been excluded from the analyses. Many municipalities pay the same debt costs (loaning from e.g. Kommuninvest), i.e. the costs are not tied to their respective financial risks, and debt costs will thus mainly reflect the size of the debt. Including the cost of debt financing in preliminary analyses turned out to be very similar to when including the actual debt level.

[71]Net income before extraordinary items.

[72]Velasco (2000) develops a dynamic extension of a common pool model where the deficit increases as the number of decision-makers increase. However, this is the result of the specific functional forms assumed in the model. Thus, there is no general intuition for the overall sign of the relationship between the number of policymakers and the deficit.

[73]There is, however, a lack of theoretical and solid empirical foundation for this proposition. For instance, de Haan and Sturm (1994) conclude that the ideology of government does not affect budget deficits in European Union countries.

[74]Several studies use population to capture structural differences (see for example Borge 1995; Allers et al. 2001; Blom-Hansen et al. 2006).

with positive population growth are moreover expected to invest more, which is likely to affect debt levels positively. Population is also included to control for possible economies of scale in providing public services.

DENSITY measures the population density in the municipalities and is meant to control for the cost of providing social services.[75]

YOUNG and *OLD* measure the share of the population in each municipality that are below or above 19 and 65 years. These variables are included to control for the demand for social services.[76]

TAX INCOME equals real taxable income earned by the municipalities' inhabitants. If public goods are normal goods, expenditure is expected to increase with higher income. Income also affects the extent to which debt is "needed" to finance expenditures and/or investments.

GOVERNMENT GRANT represents the income that municipalities are given (need to pay in) from (to) the central government.[77] The expected effect is similar to that of *TAX INCOME*.

SPECIAL INTEREST PARTY is a dummy variable indicating whether the coalition includes a special interest party or not. Such parties rarely have a comprehensive platform, and they may be more concerned about single issues than the fiscal sustainability in their municipalities.[78]

EXCESS SEAT SHARE is defined as the share of seats in excess of a majority that the coalition has.[79] Governments with larger electoral margins may find it easier to remain in power after the next election, which should lower the incentive to e.g. strategically use debt. On the other hand, in a veto player model, excess seats may imply "less" veto power, i.e. the larger the majority, the easier it is for the government to put in place fiscal consolidation programs after a negative shock or tighter constraints on government expenditure.

NET COSTS measures the costs of different municipal operating activities. Higher costs in a given year are expected to have a positive effect on debt and a negative effect on net income.[80]

ELECTION YEAR EFFECTS is a political control variable. It measures the time before the election (in years). It is meant to pick up the effect of potential political budget cycles. Decision-makers, motivated by chances of re-election, are expected to increase expenditures before elections, which negatively affects net income.

TTREND equals a linear time trend. It is included to capture time trends in the development of the variables over time.[81]

[75] For example, it is more expensive to provide welfare services in rural areas.

[76] See for example Persson et al. 2007 for similar variables.

[77] This variable is referred to as "exogenous income" in the tables that are presented in section 8.

[78] There are special interest parties in almost 15 per cent of the coalitions in the data set.

[79] It is similar to measures in, for example, Volkerink and de Haan (2001).

[80] The control variable *NET COSTS* includes *different/other* costs than the costs that are used when evaluating the common pool hypothesis for public expenditure. The latter include a wider range of costs, including more discretionary costs, whereas *NET COSTS* mainly includes costs that are related to the municipalities' mandatory activities.

[81] For example, SALAR (2014) and the Confederation of Swedish Enterprise (2014) find that municipal public expenditures have increased by one per cent more than what is required by demographic change over time.

8 Results

Since hypothesis 3 is an extension of hypotheses 1 and 2, the results of our empirical analysis are presented accordingly. Thus, the results from testing hypotheses 1 and 2 are presented together with the results from testing hypothesis 3. Hypothesis 4 simply implies using the model for testing hypothesis 1, but with other dependent variables, and is thus presented in conjunction with testing hypothesis 1 on public expenditure.

To save space, relatively few results are presented in the main body of the paper.[82] For illustrative purposes, we start with a wide approach where estimates of *all* fragmentation measures are discussed. Once the main relationships of interest have been established, we however only present estimates of the Herfindahl index. Coefficients should be interpreted as the change in the dependent variable in real per capita Swedish krona (SEK) following a one-unit change in the relevant explanatory variable. For some variables, it is difficult to interpret unit changes meaningfully. In such cases, we make clarifying interpretations in standard deviations or percentage points. Relevant fragmentation estimates are marked in bold in the tables.

Relatively similar estimates are generally obtained when using DGMM and SGMM as when estimating with LSDV.[83] However, the DGMM estimates are almost always insignificant. Moreover, using the BB or the AB estimator as the initial consistent estimator for the LSDVC estimator barely changes the results. However, according to Bruno (2005) the AB estimator appears more robust than the BB estimator.[84] All relevant LSDVC estimates can be found in the appendix. The LSDV estimates are often close to the bias-corrected estimates.

8.1 Hypotheses 1, 3 and 4

8.1.1 Long-term debt

Prior to testing the effect of the balanced budget rule on collective action problems, we test the existence of common pool behavior among fragmented governments (hypothesis 1). The full 1995-2012 sample is used for these estimations.

Positive and significant coefficients are obtained for most fragmentation measures. Minority governments and ideological fragmentation are both positively and significantly related to long-term debt levels (see Table $A5$ for the LSDV estimates and Table $A7$ for LSDVC estimates in Appendix A). While rainbow coalitions are positively related to long-term debt levels, the relationship is not significant. Additionally, coalitions with more parties have higher levels of long-term debts, but the relationship is non-monotonic. For example, three-party coalitions have, on average, higher debts than four- and five-party coalitions. At first, the effect of one additional party in a coalition may appear small. However, one should keep in mind that a coefficient on the fragmentation variable of approximately $SEK\ 1,000$ per capita implies a total of SEK 30 million in long-term debts for an averaged sized municipality.

Moreover, the effective number of parties in the coalition as well as in the council is positively and significantly related to long-term debt levels, but the effects are non-linear (see Table 5 on the next page). Furthermore, the coefficients on the Herfindahl index of the coalition and the council are in line with expectations. A one standard deviation increase in the Herfindahl index of the coalition and the council are associated

[82] Moreover, the balanced budget rule dummy is often abbreviated as BBR in the tables in order to save space. Further, in some cases, estimates for a few control variables have been excluded in order to save space (information is provided when this is the case).

[83] The GMM results were, however, very sensitive to the different specifications of the models and less robust than the LSDVC results.

[84] The AB estimator is therefore our preferred choice.

with increases in debt levels of approximately SEK 750 and SEK 900 per capita, which is broadly in line with the magnitude of the estimates of the other relevant fragmentation measures. The signs of the control variables are mainly of the expected sign and magnitude. For example, population has a positive and significant effect on real long-term debt levels and there are positive and significant election year effects.[85]

Table 5. Fragmentation, common pools and debt

VARIABLES	(1)	(2)	(3)	(4)
Lagged debt	0.731***	0.743***	0.737***	0.743***
	(0.0185)	(0.0191)	(0.0188)	(0.0191)
Excess seat share	-182.8	1,235	3,656***	1,219
	(985.1)	(861.2)	(1,082)	(858.5)
Exogenous income	0.204**	0.194**	0.207**	0.196**
	(0.0865)	(0.0863)	(0.0855)	(0.0865)
Time trend	-324.9***	-340.7***	-329.8***	-344.6***
	(121.6)	(119.3)	(119.9)	(119.2)
Special interest party	20.00	-237.3	-359.6	-268.7
	(257.5)	(254.0)	(266.9)	(254.7)
Share under 19	-23,710	-29,413	-27,615	-28,033
	(19,647)	(19,782)	(19,728)	(19,766)
Share over 65	23,361*	21,652*	23,771*	21,732*
	(12,664)	(12,514)	(12,586)	(12,519)
Population	15,407**	15,994**	16,396**	15,748**
	(7,219)	(7,658)	(7,298)	(7,601)
Density	-6,973	-7,530	-7,382	-7,519
	(5,264)	(5,842)	(5,397)	(5,788)
Real taxable income	0.0571***	0.0608***	0.0569***	0.0616***
	(0.0198)	(0.0195)	(0.0196)	(0.0194)
Net costs	-0.000373	-0.000439	-0.000405	-0.000450
	(0.000719)	(0.000696)	(0.000669)	(0.000701)
Left-wing ideology	-289.6	-8.470	328.4	-15.48
	(244.2)	(215.3)	(261.1)	(215.2)
Election year effects	598.1***	599.0***	597.1***	600.2***
	(41.71)	(41.35)	(41.58)	(41.36)
Balanced budget rule	1,265***	1,028***	1,197***	1,040***
	(195.7)	(194.6)	(193.6)	(194.4)
Coalition ENOP	**223.9****			
	(93.69)			
Non-linear coalition ENOP	-11.09***			
	(4.233)			
Council ENOP		**4,660*****		
		(1,096)		
Non-linear council ENOP		-444.9***		
		(126.2)		
Coalition Herfindahl			**9,804*****	
			(2,303)	
Council Herfindahl				**16,764*****
				(2,815)
Constant	-132,878**	-145,936***	-147,972***	-145,153***
	(53,734)	(55,889)	(54,139)	(55,330)
Observations	4,844	4,844	4,844	4,844
R-squared	0.556	0.560	0.558	0.560
Number of municipalities	285	285	285	285

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

[85]Population growth requires more investments, which generally are debt financed.

Next, the interaction term between government fragmentation and the balanced budget rule dummy is included in order to test hypothesis 3 on long-term debt levels (see Table 6).[86] The estimates of the fragmentation variables are still positive and significant, but there is a negative sign on the interaction term between the balanced budget rule dummy and the fragmentation variable. This indicates that the effect of fragmentation on long-term debt levels *diminishes* with the introduction of the balanced budget rule. The finding also holds for fragmentation measures other than the effective number of parties and the Herfindahl index (see Table *A*9 in Appendix *A*).

Table 6. Common pool behavior, the balanced budget rule and debt

VARIABLES	(1)	(2)	(3)	(4)
Lagged debt	0.734***	0.748***	0.742***	0.748***
	(0.0186)	(0.0197)	(0.0190)	(0.0196)
Left-wing ideology	-331.9	-10.68	293.1	-24.26
	(243.5)	(215.1)	(260.2)	(215.0)
Exogenous income	0.205**	0.205**	0.215**	0.208**
	(0.0861)	(0.0860)	(0.0847)	(0.0862)
Net expenditure	-0.000345	-0.000414	-0.000373	-0.000431
	(0.000715)	(0.000686)	(0.000651)	(0.000690)
Real taxable income	0.0602***	0.0655***	0.0605***	0.0660***
	(0.0197)	(0.0191)	(0.0193)	(0.0190)
Time trend	-346.3***	-384.5***	-364.4***	-386.3***
	(120.8)	(116.8)	(117.7)	(117.0)
Special interest party	35.28	-237.2	-335.5	-237.9
	(258.7)	(252.2)	(267.1)	(253.1)
Share under 19	-32,552*	-43,516**	-41,045**	-42,559**
	(19,719)	(19,880)	(20,139)	(20,122)
Share over 65	20,207	17,601	19,229	17,223
	(12,573)	(12,611)	(12,609)	(12,637)
Population	16,196**	17,742**	17,973**	17,575**
	(7,140)	(7,614)	(7,259)	(7,592)
Density	-6,458	-6,910	-6,786	-6,793
	(5,138)	(5,750)	(5,280)	(5,722)
Election year effects	601.1***	606.2***	602.4***	607.1***
	(41.67)	(41.08)	(41.44)	(41.14)
Balanced budget rule	2,328***	4,383***	7,435***	9,396***
	(350.0)	(913.2)	(1,714)	(2,403)
Coalition ENOP	**322.9*** **			
	(98.99)			
Coalition ENOP * BBR	**-165.9*** **			
	(41.99)			
Non-linear coalition ENOP	-9.739**			
	(4.170)			
Council ENOP		**4,168*** **		
		(1,068)		
Council ENOP * BBR		**-824.0*** **		
		(214.7)		
Non-linear council ENOP		-317.4**		
		(123.3)		
Coalition Herfindahl			**14,581*** **	
			(2,761)	
Coalition Herfindahl * BBR			**-7,527*** **	
			(2,035)	
Council Herfindahl				**22,998*** **
				(3,620)
Council Herfindahl * BBR				**-11,209*** **
				(3,184)
Observations	4,844	4,844	4,844	4,844
R-squared	0.557	0.562	0.559	0.562
Number of municipalities	285	285	285	285

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

[86] Net costs have been excluded in Table 6 to fit the table on one page.

8.1.2 Net income

To evaluate the common pool hypothesis (hypothesis 1) using net income as the dependent variable, we again start by establishing the existence of common pool behavior *without* interaction terms. In line with a priori expectations, the results indicate that net income decreases with increased fragmentation (see Table 7).[87] Moreover, the coefficients of the control variables are mainly as expected. For example, election year effects are negatively related to net income, which is likely to be related to excessive spending prior to elections in order to increase the probability of being re-elected. A confounding finding in Table 7 is, however, that the coefficients on the balanced budget rule are insignificant and have negative signs. Comfortingly to the specification of our model, the estimates of the balanced budget rule dummy are *both* significantly and positively related with net income when using the LSDVC estimator. Further, fragmentation estimates from LSDV are otherwise similar to those that have been approximated with Kiviet bias-correction, but the LSDV estimates appear to underestimate the effects (see Table $A11$ in Appendix A).

Table 7. Common pool behavior and net income

VARIABLES	(1)	(2)	(3)	(4)
Lagged net income	0.249***	0.234***	0.243***	0.235***
	(0.0429)	(0.0417)	(0.0423)	(0.0416)
Excess seat share	60.22	-248.2	-936.5***	-258.1
	(232.2)	(236.9)	(277.6)	(234.1)
Exogenous income	0.117***	0.118***	0.116***	0.118***
	(0.0185)	(0.0192)	(0.0186)	(0.0192)
Time trend	-183.4***	-170.8***	-178.5***	-170.7***
	(29.87)	(31.39)	(29.88)	(31.25)
Special interest party	-72.95	6.079	23.52	3.287
	(63.85)	(62.64)	(63.56)	(61.17)
Share under 19	-5,891	-4,660	-4,942	-4,657
	(3,981)	(4,173)	(4,093)	(4,251)
Share over 65	8,985***	9,519***	8,855***	9,600***
	(2,593)	(2,790)	(2,692)	(2,791)
Population	-1,006	-1,164	-1,314	-1,147
	(1,914)	(1,784)	(1,864)	(1,786)
Density	1,676	1,972	1,846	1,942
	(1,642)	(1,458)	(1,564)	(1,460)
Real taxable income	0.0456***	0.0427***	0.0449***	0.0427***
	(0.00538)	(0.00556)	(0.00538)	(0.00555)
Net costs	-0.000842***	-0.000816***	-0.000831***	-0.000817***
	(0.000299)	(0.000303)	(0.000309)	(0.000301)
Left-wing ideology	114.7	31.82	-53.33	36.83
	(86.06)	(81.39)	(86.60)	(81.51)
Balanced budget rule	-111.6	-23.70	-85.25	-24.97
	(99.48)	(98.32)	(100.1)	(96.92)
Coalition ENOP	**-36.94**			
	(27.24)			
Non-linear coalition ENOP	1.625			
	(1.375)			
Council ENOP		**-1,122*** **		
		(324.7)		
Non-linear council ENOP		93.67**		
		(40.21)		
Coalition Herfindahl			**-2,816*** **	
			(565.6)	
Council Herfindahl				**-5,328*** **
				(754.9)
Constant	-1,415	1,860	3,184	2,789
	(13,540)	(12,959)	(13,323)	(12,943)
Observations	4,844	4,844	4,844	4,844
R-squared	0.174	0.184	0.178	0.184
Number of municipalities	285	285	285	285

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

[87] However, while estimates are of the right sign and magnitude when using GMM, they were mainly insignificant.

Next, the interaction term between the balanced budget rule and political fragmentation is included in order to test hypothesis 3 on net income (see Table 8). As can be seen in Table 8, more fragmented governments have, on average, lower net incomes than non-fragmented governments, but the effect of political fragmentation on net income diminishes with the introduction of the balanced budget rule.[88] Furthermore, in order to see why the individual effect of the balanced budget rule has the expected effects (we expect it to always have either a neutral or positive effect on net income), one must consider the size of the fragmentation measures that are used. As discussed in section 7.2.3, the values of the $1 - Herfindahl$ index are always quite high. For example, the average $1 - Herfindahl$ index value of the council is 0.75.[89] Therefore, even for seemingly low values of government fragmentation, when multiplying the fragmentation values with the coefficient on the interaction term in order to get the individual effect of the balanced budget rule, we tend to get coefficients that are indeed in line with a priori expectations.

Table 8. Common pool behavior, the balanced budget rule and net income

VARIABLES	(1)	(2)	(3)	(4)
Lagged net income	0.238***	0.215***	0.228***	0.216***
	(0.0433)	(0.0401)	(0.0423)	(0.0404)
Excess seat share	241.4	-306.5	-1,338***	-343.6
	(246.7)	(245.1)	(294.2)	(242.1)
Exogenous income	0.0835***	0.0854***	0.0814***	0.0852***
	(0.0169)	(0.0183)	(0.0171)	(0.0180)
Time trend	-148.4***	-122.3***	-138.2***	-125.4***
	(29.65)	(31.43)	(29.46)	(30.91)
Special interest party	-88.69	17.80	28.31	-4.704
	(65.38)	(64.01)	(64.62)	(62.23)
Share under 19	-1,431	4,641	2,574	4,342
	(4,284)	(4,563)	(4,523)	(4,572)
Share over 65	12,431***	13,750***	13,010***	13,955***
	(2,662)	(2,954)	(2,834)	(2,938)
Population	-1,156	-2,088	-1,977	-2,041
	(2,040)	(1,884)	(1,952)	(1,866)
Density	1,534	1,768	1,669	1,690
	(1,744)	(1,546)	(1,587)	(1,509)
Real taxable income	0.0359***	0.0327***	0.0354***	0.0330***
	(0.00527)	(0.00544)	(0.00523)	(0.00541)
Net costs	-0.000855***	-0.000825***	-0.000848***	-0.000823***
	(0.000299)	(0.000307)	(0.000318)	(0.000305)
Left-wing ideology	153.9*	34.78	-59.39	45.43
	(89.73)	(82.94)	(89.89)	(83.24)
Election year effects	-88.82***	-94.69***	-90.70***	-94.24***
	(17.36)	(17.53)	(17.39)	(17.44)
Balanced budget rule	-371.5**	-1,797***	-2,780***	-4,280***
	(168.1)	(338.0)	(561.6)	(728.7)
Coalition ENOP	**-108.4*** **			
	(33.93)			
Coalition ENOP * BBR	**63.88*** **			
	(18.62)			
Non-linear coalition ENOP	2.548			
	(1.658)			
Council ENOP		**-1,077*** **		
		(332.7)		
Council ENOP * BBR		**470.9*** **		
		(71.32)		
Non-linear council ENOP		36.17		
		(43.61)		
Coalition Herfindahl			**-5,882*** **	
			(773.2)	
Coalition Herfindahl * BBR			**3,428*** **	
			(642.8)	
Council Herfindahl				**-10,140*** **
				(943.5)
Council Herfindahl * BBR				**5,903*** **
				(939.8)
Constant	650.8	10,668	11,600	14,271
	(14,412)	(13,629)	(14,117)	(13,592)
Observations	4,844	4,844	4,844	4,844
R-squared	0.147	0.171	0.156	0.169
Number of municipalities	285	285	285	285

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

[88] Moreover, when including the interaction term, the individual effects of the balanced budget rule are in line with a priori expectations also for the LSDV estimates. The obtained LSDVC estimates are always in line with expectations.

[89] Neither will the $1 - Herfindahl$ index be 0 in the case of single-party governments (as in some other analyses), due to the coalition Herfindahl index having been weighted with the relative seat share in the council. We therefore always have positive values for all fragmentation measures.

8.1.3 Public expenditure

After having obtained results that are indicative of common pool behavior in terms of debt levels and net incomes, a similar analysis is performed with public expenditure as the dependent variable. It is expected that political fragmentation will be positively related to levels of public expenditure, but that the balanced budget rule will mitigate the effects of fragmentation. Since the balanced budget rule does not constrain *all* types of spending, it is furthermore plausible that fragmented governments start channeling shares of spending to those expenditure areas that are not constrained by the policy reform.

In order to understand this, one must take into consideration what the balanced budget rule actually constrains. The Swedish balanced budget rule *only* restricts spending for operating activities, and does not constrain the level of investments nor the activities of the municipal corporations. Municipalities are free to organize their activities in certain corporations, and these are only indirectly affected by the balanced budget rule. While municipalities are required to maintain "sustainable public finances" in all their activities, whether performed by their corporations or not, there is more discretion possible in terms of budgeting as well as expenditures for the corporations. There are also fewer demands on, for example, specifying financial goals and monitoring for the corporations. Moreover, municipalities are generally "evaluated" on the net incomes of the municipality and not that of their corporations.

As mentioned in section 6.2, there exists no official investment data for the municipal corporations. However, general corporate expenditure data, which can be compared to that of the municipalities, are available. In the case of corporate expenditures, it is expected that the interaction term between political fragmentation and the balanced budget rule dummy will have a positive sign, i.e. that the effect of fragmentation on corporate expenditure increases with the introduction of a balanced budget rule constraining the expenditure on "normal" municipal activities. Estimates, using the Herfindahl index of fragmentation, are presented in Table 9 on the next page.[90] Columns 1 and 2 show estimates using municipal costs, while columns 3 and 4 show estimates using corporate costs as the dependent variable.[91]

At first, the individual effect (negative and significant) of fragmentation on corporate costs appears counterintuitive. After all, fragmented governments are expected to spend either the same or more through the corporations prior to the reform. However, if one considers the size of the fragmentation variables along with the interaction effects, the individual effects of the fragmentation variables are of the right size and sign. Moreover, F-tests indicate that the interaction term and the individual effect are jointly significant. Similar estimates for the council as well as LSDVC estimates can be found in the appendix (see Table $A6$ and Table $A14$ in Appendix A).[92]

Taken together, the results indicate that fragmented coalitions and councils spend more, but that the introduction of the balanced budget rule diminishes the positive effect of fragmentation on spending. Moreover, the results indicate that fragmented governments spent roughly the same as their less fragmented counterparts on their corporations prior to the policy reform (given that one takes the interaction effect into

[90] When testing the common pool hypothesis for very detailed accounting levels, such as certain types of infrastructure and the provision of services related to different cultural activities, we do not find significant effects of fragmentation. The reason is probably that there is little variation in the data, and that a majority of municipalities always declare null values for the different spending categories. Thus, we are unable to infer whether common pool behavior matters more for certain types of expenditure.

[91] The columns show estimates for LSDV and pooled regressions.

[92] In some other estimations, there were no significant effect of fragmentation on corporate expenditures prior to the reform but a positive interaction effect, which is in line with what one would expect.

account when considering the main effect), but that the introduction of the balanced budget rule was associated with an increase in fragmented governments' spending on their corporations. Yet, tightening the fiscal constraint to include *all* aspects of the municipal budget is not a viable policy option. For example, such constraints would be likely to increase pro-cyclicality and make consumption smoothing difficult. Nevertheless, the finding is important since it implies effects and dynamics of political fragmentation as well as balanced budget rules that have previously not been considered.

Table 9. Common pool behavior and public expenditure

	(1)	(2)	(3)	(4)
VARIABLES	FE-MU	Pooled-MU	FE -MC	Pooled-MC
Lagged net costs	0.440***	0.813***	0.287***	0.735***
	(0.0875)	(0.0483)	(0.0661)	(0.0472)
Excess seat share	1,870***	330.5	397.9	42.82
	(407.7)	(263.0)	(502.2)	(399.0)
Time trend	418.5***	49.28	466.4***	94.40***
	(87.82)	(33.98)	(75.19)	(33.32)
Special interest party	45.93	52.28	172.6*	39.63
	(84.96)	(73.29)	(102.2)	(91.56)
Share under 19	31,576***	5,208**	39,001***	14,902***
	(6,978)	(2,203)	(8,340)	(2,892)
Share over 65	-4,939	9,307***	2,437	17,315***
	(5,186)	(1,865)	(5,785)	(3,083)
Population	-8,377**	78.90*	-9,059**	3.740
	(3,362)	(47.48)	(3,541)	(68.93)
Density	415.8	-433.3***	1,784	-608.2***
	(1,877)	(95.86)	(2,349)	(104.6)
Real taxable income	0.0370***	0.0120***	0.0578***	0.0159***
	(0.00633)	(0.00300)	(0.00749)	(0.00377)
Left-wing ideology	-18.16	309.5***	-76.58	387.7***
	(116.3)	(105.3)	(117.2)	(127.2)
Election year effects	-22.53	-100.8***	28.02	-130.1***
	(19.42)	(20.74)	(25.79)	(27.23)
Balanced budget rule	2,741***	1,617***	-2,291**	-3,457***
	(749.8)	(619.7)	(1,044)	(996.4)
Coalition Herfindahl	**5,904*** **	**2,179*** **	**-2,665***	**-3,169****
	(992.4)	(643.5)	(1,564)	(1,572)
Coalition Herfindahl*BBR	**-2,330*** **	**-1,876*** **	**3,203*** **	**3,289*** **
	(817.5)	(633.6)	(1,193)	(1,111)
Constant	82,481***	2,075	90,631***	6,538***
	(28,230)	(1,555)	(27,886)	(1,710)
Observations	4,843	4,843	4,275	4,275
R-squared	0.929		0.863	
Number of municipalities	285	285	285	285

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

8.2 Hypotheses 2 and 3

Following having evaluated the common pool hypothesis (hypothesis 1) and the impact of fiscal constraints (hypothesis 3 and 4) on long-term debt, net income and public expenditure in section 8.1, we move on to test the existence of status quo bias (hypothesis 2) and the impact of fiscal constraints on status quo bias (hypothesis 3).

Firstly, we test whether political fragmentation is negatively related to stabilizations of deficits. Using several different fragmentation measures, estimates indicate that this is the case (see Table 10 as well as $A15$ in Appendix A). Estimates of the effective number of parties (council) and the Herfindahl indices are negative and significant. The results indicate that fragmented governments are more likely to have negative changes in the financial results between years. As can be seen in Table 10, a one standard deviation increase in the coalition Herfindahl index is associated with a negative change in the annual financial result by roughly SEK 280 per capita.

Table 10. Status quo bias, 1995-2012

VARIABLES	(1)	(2)	(3)	(4)
Lagged net income	-0.757***	-0.774***	-0.765***	-0.776***
	(0.0438)	(0.0425)	(0.0430)	(0.0419)
Balanced budget rule	34.90	107.4	63.64	124.7
	(98.89)	(94.98)	(98.96)	(95.97)
Share under 19	-4,313	-4,486	-3,612	-3,324
	(4,026)	(4,307)	(4,169)	(4,374)
Share over 65	11,458***	11,363***	11,031***	11,727***
	(2,579)	(2,807)	(2,713)	(2,834)
Population	-785.6	-885.7	-1,226	-1,037
	(2,051)	(1,872)	(1,950)	(1,847)
Density	1,638	2,074	1,907	2,033
	(1,791)	(1,569)	(1,651)	(1,513)
Real taxable income	0.0380***	0.0351***	0.0369***	0.0350***
	(0.00513)	(0.00539)	(0.00524)	(0.00541)
Exogenous income	0.0823***	0.0885***	0.0842***	0.0900***
	(0.0168)	(0.0178)	(0.0172)	(0.0179)
Left-wing ideology	141.8	45.70	-73.62	42.24
	(89.41)	(81.55)	(90.29)	(83.39)
Net costs	-0.000858***	-0.000804***	-0.000830***	-0.000812***
	(0.000297)	(0.000298)	(0.000311)	(0.000300)
Excess seat share	-64.23	-205.1	-1,163***	-297.7
	(234.7)	(241.5)	(277.5)	(244.3)
Time trend	-161.7***	-146.9***	-153.9***	-147.0***
	(28.21)	(30.46)	(29.12)	(30.71)
Election year effects	-85.93***	-90.09***	-87.82***	-90.00***
	(17.29)	(17.48)	(17.36)	(17.48)
Coalition ENOP	**-12.42**			
	(7.942)			
Council ENOP		**-469.3*** **		
		(56.53)		
Coalition Herfindahl			**-3,647*** **	
			(564.2)	
Council Herfindahl				**-6,900*** **
				(774.1)
Constant	-3,073	-1,423	3,401	3,061
	(14,364)	(13,401)	(13,895)	(13,375)
Observations	4,844	4,844	4,844	4,844
R-squared	0.427	0.438	0.432	0.440
Number of municipalities	285	285	285	285

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

Secondly, we use an approach similar to that in section 8.1.2, in which an interaction term between the balanced budget rule dummy and relevant fragmentation measures is included.[93] Again, estimates indicate that the balanced budget rule mitigates the effects of fragmentation; the introduction of the balanced budget rule diminishes the degree to which fragmented governments have a negative impact on stabilizations of deficits (see Table 11 and Table $A16$ in Appendix A).

Table 11. Status quo bias, incl. balanced budget rule, 1995-2012

VARIABLES	(1)	(2)
Lagged net income	-0.772***	-0.784***
	(0.0423)	(0.0404)
Share under 19	2,532	4,347
	(4,514)	(4,569)
Share over 65	13,003***	13,958***
	(2,829)	(2,936)
Population	-1,974	-2,040
	(1,949)	(1,866)
Density	1,666	1,691
	(1,584)	(1,510)
Real taxable income	0.0354***	0.0330***
	(0.00523)	(0.00541)
Exogenous income	0.0815***	0.0852***
	(0.0170)	(0.0180)
Left-wing ideology	-58.49	45.89
	(90.05)	(82.32)
Net costs	-0.000846***	-0.000824***
	(0.000319)	(0.000305)
Excess seat share	-1,307***	-345.9
	(279.3)	(238.8)
Time trend	-138.3***	-125.4***
	(29.45)	(30.89)
Election year effects	-90.68***	-94.23***
	(17.39)	(17.43)
Balanced budget rule	-2,786***	-4,279***
	(561.5)	(729.3)
Coalition Herfindahl	**-5,822*** **	
	(747.7)	
Coalition Herfindahl * BBR	**3,435*** **	
	(642.5)	
Council Herfindahl		**-10,145*** **
		(930.9)
Council Herfindahl * BBR		**5,901*** **
		(940.4)
Constant	11,546	14,265
	(14,098)	(13,587)
Observations	4,844	4,844
R-squared	0.437	0.445
Number of municipalities	285	285

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

[93]However, note that first differences are used instead of levels in order to capture the expected veto player dynamics.

Thirdly, an interaction term between political fragmentation and lagged net income (described in section 7.2.2) is included. Table 12 shows LSDV and pooled regression estimates, using the Herfindahl index of the coalition (columns 1 and 2) and that of the council (columns 3 and 4). We previously hypothesized that the interaction term should be negative, since a negative coefficient on the interaction term implies that the lower the financial result, the more negative the effect of fragmentation on the change in next year's result. As expected, there is a negative and significant effect on the interaction term as well as the fragmentation measures (see Table 12 and Table $A17$ in Appendix A). However, while the interaction effect is significant and of the expected sign, its magnitude is almost negligible. This indicates that the level of the deficit in year t is not very useful for predicting the magnitude of the change in the annual financial result between t and $t+1$ with respect to the effect of fragmentation.

Table 12. Status quo bias, incl. interaction term, 1995-2012

	(1)	(2)	(3)	(4)
VARIABLES	FE.C.	Pooled.C.	FE.A.	Pooled.A.
Lagged net income	-0.000722	0.0427	0.506	0.550
	(0.257)	(0.259)	(0.339)	(0.373)
Share under 19	-2,790	2,086	-1,973	2,674*
	(4,091)	(1,383)	(4,312)	(1,415)
Share over 65	10,604***	203.4	10,842***	440.0
	(2,807)	(1,117)	(2,872)	(1,131)
Population	-1,056	171.0***	-1,068	192.3***
	(1,771)	(35.62)	(1,650)	(35.73)
Density	1,590	61.32***	1,699	64.19***
	(1,477)	(22.89)	(1,333)	(23.67)
Real taxable income	0.0363***	0.00168	0.0353***	0.000804
	(0.00530)	(0.00196)	(0.00542)	(0.00200)
Exogenous income	0.0794***	0.0242***	0.0849***	0.0221***
	(0.0170)	(0.00843)	(0.0173)	(0.00842)
Left-wing ideology	-60.89	-126.9**	31.98	-93.98*
	(92.53)	(60.44)	(80.60)	(49.17)
Net costs	-0.000838***	3.25e-05	-0.000814***	4.13e-05
	(0.000314)	(3.36e-05)	(0.000306)	(3.12e-05)
Excess seat share	-1,069***	-423.4*	-308.6	-107.2
	(295.3)	(222.7)	(236.4)	(173.2)
Time trend	-142.0***	40.58***	-131.5***	48.02***
	(28.87)	(10.56)	(29.89)	(10.66)
Election year effects	-94.17***	-123.1***	-98.33***	-126.5***
	(17.61)	(15.95)	(17.66)	(16.10)
Coalition Herfindahl	**-2,700*** **	**-899.2***		
	(623.9)	(490.2)		
Coalition Herfindal * Lagged net income	**-0.936*** **	**-0.882*** *		
	(0.344)	(0.344)		
Council Herfindahl			**-5,095*** **	**-1,129***
			(714.1)	(604.7)
Council Herfindahl * Lagged net income			**-1.740*** **	**-1.670*** **
			(0.483)	(0.530)
Constant	1,952	-1,849*	2,947	-2,118**
	(12,768)	(1,014)	(12,073)	(1,047)
Observations	4,844	4,844	4,844	4,844
R-squared	0.436		0.447	
Number of municipalities	285	285	285	285

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

Finally, in order to further evaluate the introduction of the balanced budget rule (hypothesis 3), Equation 5 is separately tested on data for the years 1995-1999 and 2000-2012. Estimation is somewhat problematic due to the small number of years in the data set prior to the introduction of the balanced budget rule in 2000. For example, the sample almost vanishes when using GMM with more than one lag.[94] Moreover, endogeneity bias may be sizeable in LSDV estimates when the panel is short. Therefore, it is expected that the LSDVC and the LSDV estimates will differ more than in the previous estimations. While the effects are still small, the direction of the effect is in line with expectations.

The LSDV and LSDVC estimates both indicate significant and negative effects on the interaction term for the 1995-1999 sample (see Table 13). The LSDVC estimates moreover imply relatively large and significant negative individual effects of the council Herfindahl index using the 1995-1999 sample (see Table $A18$ in Appendix A). However, the effect of political fragmentation entirely disappears when evaluating the effects on the 2000-2012 sample, a finding that holds for a range of other specifications as well.[95]

Table 13. Status quo bias, 1995-1999 and 2000-2012

	(1)	(2)	(3)	(4)
VARIABLES	FE.C.95-99	Pooled.C.95-99	FE.C.00-12	Pooled.C.00-12
Lagged net income	0.308	0.216	-0.937***	-0.742***
	(0.318)	(0.478)	(0.150)	(0.232)
Share under 19	27,227	-1,253	-5,090	-1,638
	(24,650)	(4,413)	(5,460)	(1,684)
Share over 65	14,373	-3,361	1,696	-2,549*
	(19,859)	(3,107)	(3,537)	(1,341)
Population	20,340***	191.7**	-635.8	142.0***
	(7,248)	(95.82)	(1,446)	(42.61)
Density	478.3	90.07*	-285.7	74.78**
	(5,747)	(48.76)	(910.6)	(30.61)
Real taxable income	-0.151***	-0.0119**	0.0506***	0.00849***
	(0.0381)	(0.00496)	(0.00620)	(0.00241)
Exogenous income	-0.0994**	0.0259	0.165***	0.0474***
	(0.0446)	(0.0231)	(0.0230)	(0.0108)
Left-wing ideology	324.7	-54.40	77.83	-88.76
	(305.5)	(174.4)	(78.76)	(60.74)
Net costs	0.000649	0.000169	-0.00117**	6.34e-05*
	(0.00128)	(0.000108)	(0.000581)	(3.30e-05)
Excess seat share	-900.9	-361.8	292.4	-17.07
	(549.0)	(433.5)	(335.0)	(212.9)
Time trend	-70.85	-551.4***	-162.9***	44.43***
	(130.7)	(95.38)	(29.77)	(12.53)
Election year effects	-20.85	162.0***	-122.8***	-191.7***
	(51.53)	(46.61)	(18.67)	(18.42)
Coalition Herfindahl	**-24.27**	**-46.79**	**698.6**	**-163.0**
	(1,986)	(1,731)	(707.9)	(412.8)
Coalition Herfindahl *	**-1.735*** **	**-1.135***	**0.0587**	**-0.0172**
Lagged net income				
	(0.426)	(0.683)	(0.178)	(0.269)
Constant	-191,980***	1,868	750.3	-1,973**
	(56,612)	(3,497)	(11,531)	(916.2)
Observations	1,139	1,139	3,420	3,420
R-squared	0.622		0.496	
Number of municipalities	285	285	285	285

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

[94] An alternative would be to use the full sample but interact all variables with the balanced budget rule dummy, allowing all coefficients to differ across time periods.

[95] This finding holds when trying different estimators, such as GMM.

In summary, the results from the empirical analysis in section 8.1 indicate that political fragmentation has a positive and significant effect on debt levels and expenditure, and a negative and significant effect on net income. Taken together, these findings indicate common pool behavior. Moreover, the introduction of the balanced budget rule is associated with fragmented municipalities increasing their corporate expenditures. In terms of status quo bias, analyzed in section 8.2, the results indicate that political fragmentation is significantly and negatively associated with deficit stabilizations (however, given quite strong assumptions about the developments of deficits). Therefore, the results support the hypotheses of the paper, even though it should be noted that the evidence is relatively weak.

8.3 Robustness checks

Next, we perform sensitivity analyses to evaluate whether changes to the models and the data yield similar estimates as those obtained in section $8.1 - 8.2$. For example, we perform re-estimations using coalition data where consideration has been taken to coalition splits during the mandate period.[96] The main findings remain the same.[97] We also re-estimate where the deflator for municipal expenditures from the Swedish Ministry of Finance is used instead of CPI. Moreover, longer lag lengths for the dependent variables are also included, where the obtained results generally remain similar to those obtained in section $8.1 - 8.2$. Some re-estimations are also done where the economic variables and the fragmentation data are matched differently.[98]

8.4 Validity of estimates

It is important to discuss the validity of the estimates. In short, we present credible ranges of the effects rather than exact point estimates. Given that similar effects are found when using different specifications and models, the reader can (if accepting the various assumptions that this paper has made) be quite comfortable that the *direction of the effects* is estimated correctly. However, we discourage further interpretation of the magnitude of the estimates.[99] While the obtained GMM estimates often are in the range of pooled OLS and LSDV, the ranges are sometimes large. However, the LSDV estimates are always comfortably close to the LSDVC estimates.

Furthermore, as is often the case, it is impossible to account for *all* mechanisms that may have an impact on the variables of interest. For example, we are unable to capture the potential effects of "expectations". While the balanced budget rule was implemented in 2000, it was announced in 1998. Decision-makers may therefore have already started to behave differently before its introduction. If this is the case, the study has probably underestimated the effects of fragmentation as well as the effects of the introduction of the balanced budget rule.

Moreover, as already explained, there are some econometric weaknesses associated with our approach for evaluating status quo bias. In retrospect, we believe that using

[96] As stated in section 5.3, between some elections approximately ten per cent of all coalitions were changed during the mandate period.

[97] However, we do not have perfect information regarding the timing of some of the break-ups, but have rather assumed that they happened halfway into the mandate period. Therefore, these estimates are not necessarily more valid than those that have already been obtained.

[98] This is done in order to take account of the fact that "lagged fragmentation" may be more important for the current economic variables.

[99] Adding to this, since this study is mainly dealing with indices and weighted measures, interpretation is rarely intuitive. For example, consider the case where the effective number of parties increases by one party. Even when interpreting this in standard deviations, interpretation of the estimates is difficult since a rise in the effective number of parties may be caused by a range of different changes.

survival analysis would have yielded more valid estimates when testing hypothesis 2. For example, a better approach would have been to start from the year of a deficit and evaluate what time it takes to reach a surplus again, and how this in turn depends on political fragmentation.

As far as external validity is concerned, the institutional setting in Sweden is similar to that of the other Nordic countries, but it differs from some federal countries where local governments are generally less bound by the central government. Furthermore, the extent to which fiscal constraints are expected to mitigate the effects of collective action problems depends on whether the constraints are binding. In a country where there is a balanced budget rule (imposed ex ante), but there exist no penalties for breaking the rule, the same effects cannot be expected.[100]

8.5 Limitations

As discussed in section 7.1.4, a limitation of the paper is the inability to take account of all factors that may be confounded with the introduction of the balanced budget rule.

Another main limitation of the paper is that of data inaccuracy. Compiling concordant panel data sets where full consideration is taken of changed accounting principles over time is difficult. While the key variables in the financial statement of income are often relatively straightforward to use (and subsequently have been used in this paper), they are "black boxes". To further understand the behavior of policy-makers, it would be interesting to make analyses using more detailed economic panel data. Unfortunately, ensuring that many smaller, disaggregated variables were concordant over time was beyond the scope of this paper.

Similar to the reasoning above, while this study delineates patterns that are relevant both to research on political fragmentation and fiscal constraints, it does not dig deeper into the actual dynamics of things. In future research, it would therefore be interesting to include a range of other dimensions. For example, while we examine ideological fragmentation and size fragmentation separately, future analyses would benefit from combining them, since the impact of size fragmentation may depend on ideological fragmentation and vice versa. Moreover, in future research more subtle indices and advanced models can be used. For example, models that combine different collective action models into one as opposed to testing them separately would constitute an interesting extension of our analysis.

[100] Further, unlike in national accounting, there exists no standardized international system for local government accounting. Therefore, variables (such as measures of debt) will necessarily vary between countries if performing similar analyses in the future.

9 Conclusion

This paper examines the impact of the introduction of a balanced budget rule on collective action problems in Swedish municipalities during 1995-2012. The results indicate that the effects of common pool behavior and status quo bias are mitigated by the introduction of a balanced budget rule. Since fiscal constraints appear effective in mitigating the problems related to political fragmentation, the common conception of political fragmentation as disadvantageous should be reconsidered. By extending the logic of common pool and veto player theory, the paper shows how two influential theories for problems of collective action are relevant also in an institutional context with fiscal constraints. However, the study also finds that the introduction of a balanced budget rule is associated with fragmented governments increasing spending for some types of expenditure that are not constrained by the policy reform. Therefore, institutional contexts and fiscal constraints matter, but not necessarily in the way that one would first think. Indeed, this paper has many limitations that need to be acknowledged, but hopefully the study can serve as an inspiration for future research on institutional contexts.

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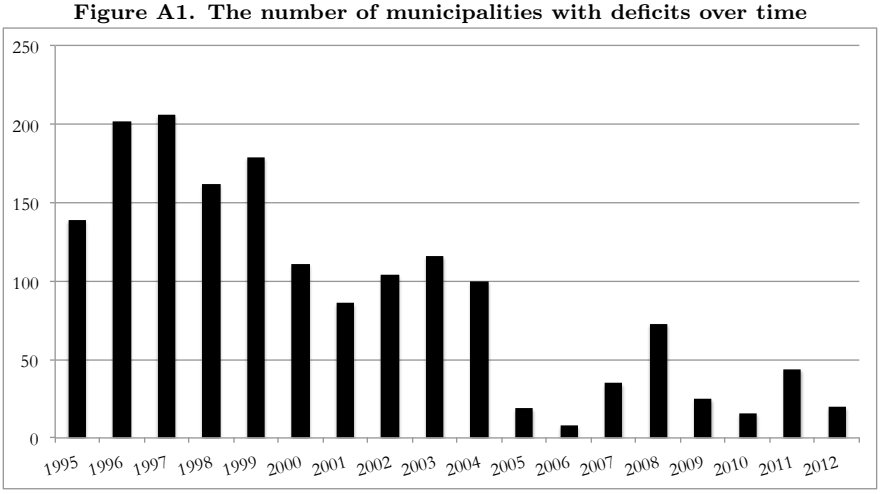
A Appendix

A.1 Development of economic variables over time

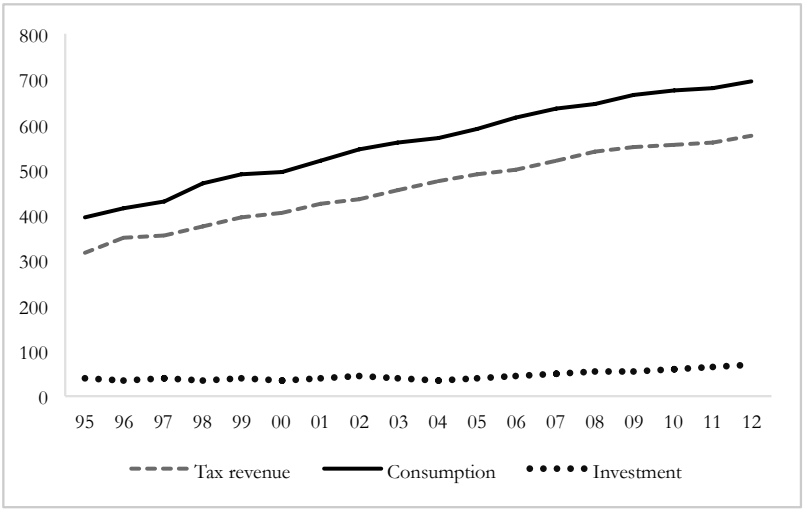
Figure A1. The number of municipalities with deficits over time



Source: Statistics Sweden and own calculations.

Figure A2. Municipal tax revenues, consumption and investments over time

Real 2012 prices, billions of SEK



Source: National Institute of Economic Research and own calculations.

A.2 Tables summarizing previous literature[101]

Table A1. Common pools

Article	Type of sample	Measure	Dependent variable	Results
Kontopoulos and Perotti (1999)	20 OECD countries (1960-1995)	Number of parties, number of spending ministers and index for type of government	Deficit, expenditure and revenue	The effect of the number of parties and the number of spending ministers depends on the time period. Moreover, the effects are stronger in tough times
Volkerink and de Haan (2001)	22 OECD countries (1971-1996)	Effective number of parties in the council and the coalition, number of spending ministers	Deficit and expenditure	Positive and significant effect for deficit and expenditure
Padovano and Venturi (2001)	Italy (1948-1994)	Herfindahl index of the coalition + opposition and Banzhaf index	Deficit and expenditure	More fragmented governments significantly raises the deficit
Perotti and Kontopoulos (2002)	19 OECD countries (1970-1995)	Number of parties, number of spending ministers and index for type of government	Deficit, expenditure and revenue	Significant positive effect only for the number of spending ministers
Rattsø and Tovmo (2002)	275 Danish municipalities (1984-1996)	Herfindahl index of the coalition	Expenditure and revenue	No effect
Ricciuti (2004)	19 OECD countries (1975-1995)	Number of spending ministers and Rae fractionalization index	Deficit and expenditure	Reduced surplus and increased expenditures when the number of spending ministers increase
Borge (2005)	376 Norwegian municipalities (1991-1999)	Herfindahl index of the coalition	Deficit	Positive and significant effect on the deficit
Bawn and Rosenbluth (2006)	17 European countries (1970-1998)	Number of parties	Expenditure	The public sector is larger the more parties there are in the coalition
Jochimsen and Nuscheler (2011)	10 German Länder (1960-2000)	Number of parties and dummy for coalition government	Deficit	No effect
Franzese (2010)	20 OECD countries (1956-1990)	Number of parties	Deficit	No effect
Le Maux et al. (2011)	French departéments (1992-1999)	Own index of effective political power of both coalition as well as the opposition	Expenditure	Per capita social expenditure depend on the effective political power of the majority
Baskaran (2013)	16 German states (1975-2010)	Instrument for the likelihood of coalition governments	Expenditure	No effect

[101] A categorization has been made in Tables *A*1-*A*4 to differentiate between common pool and veto player theories. Note, however, that the studies often overlap in terms of which theory is being evaluated, and that the categorization is rough.

Table A2. Status quo bias

Article	Type of sample	Measure	Dependent variable	Results
Roubini and Sachs (1989)	13 OECD countries (1960-1985)	Type of government, index	Deficit	Positive effect, effect stronger in bad times
Edin and Ohlsson (1991)	13 OECD countries (1960-1985)	Type of government, dummies	Deficit	Effect only for minority governments
Alt and Lowry (1994)	48 U.S. states (1968-1987)	Divided government	Revenue and expenditure	Divided governments are less able to react to shocks in revenue
Poterba (1994)	27 U.S. states (1988-1992)	Divided government	Spending and taxes	Divided governments have smaller responses to deficit shocks
de Haan and Sturm (1994)	12 European countries (1981-1989)	Index as well as dummies for type of government	Deficit	No effect
Clingermayer and Wood (1995)	48 U.S. states (1961-1989)	Divided government	Debt	No effect
Borelli and Royed (1995)	16 OECD countries (1959-1990)	Index for strength of government	Deficit	Significant rise in debt for weak governments only when GDP-change is low
Volkerink (1999)	20 OECD countries (1965-1995)	Index as well as dummies for type of government, number of parties	Deficit	No effect
de Haan et al. (1999)	20 OECD countries (1979-1995)	Index as well as dummies for type of government, number of parties	Deficit	Positive significant effect for number of parties for gross central government data
Freitag and Sciarini (2001)	14 European countries (1978-1997)	Number of parties	Deficit	No effect
Huber et al. (2003)	21 OECD countries (1970-1999)	Banzhaf index	Deficit	Deficits are higher when parties are more equal in strength
Ashworth et al. (2005)	298 Flemish municipalities (1977-2000)	Several measures, such as the effective number of parties, actual number of parties, dummies for the number of parties and ideological fragmentaiton.	Debt	No evidence that having a coalition leads to larger indebtedness. However, multiparty governments are less able to respond to exogenous shocks.
Blais et al. (2010)	33 parliamentary democracies	Number of parties and ideological distance among coalition partners	Deficit	Coalition governments have a status quo bias

Table A3. Non-monotonic effects

Article	Type of sample	Measure	Dependent variable	Results
Geys (2007)	296 Flemish municipalities (1977-2000)	Dummies for government size	Debt	Two-party coalitions tend to witness a significantly higher growth rate of debt than other types of coalitions
Smart et al. (2011)	399 German municipalities (1992-2006)	Herfindahl index	Expenditures	The effect of a mayoral reform was significantly increased spending where fragmentation of councils was in the top and bottom quartile. However, in the middle quartiles of fragmentation the effect on expenditure is effectively zero, suggesting a non-monotonic effect.

Table A4. Institutions

Article	Type of sample	Measure	Dependent variable	Results
Hagen and Vabo (2005)	434 Norwegian municipalities (1991-1998)	Herfindahl index of the coalition	Deficit	Strong political leadership improves fiscal performance
Tovmo (2007)	Approx 280 Norwegian municipalities (1991-1999)	Herfindahl index of the coalition and Shapley-Shubik index	Deficit	Centralized budgetary procedures have an advantage in overcoming common pool-resource problems in the decision-making process
Schaltegger and Feld (2009)	26 Swiss cantons (1980-1998)	Size of the cabinet and size of the coalition	Budget variables	The fiscal commons problem can be mitigated by fiscal institutions. This particularly holds for balanced budget regimes.

A.3 Complementary LSDV estimates

Long-term debt

Table A5. Common pool behavior and debt I (LSDV)

VARIABLES	(1)	(2)	(3)	(4)
Lagged debt	0.729***	0.729***	0.731***	0.730***
	(0.0184)	(0.0182)	(0.0183)	(0.0186)
Excess seat share	1,575	440.4	-820.4	-1,311
	(1,032)	(859.6)	(1,060)	(1,195)
Exogenous income	0.201**	0.206**	0.203**	0.202**
	(0.0864)	(0.0857)	(0.0862)	(0.0860)
Time trend	-309.0**	-298.9**	-324.2***	-331.8***
	(122.3)	(122.6)	(122.2)	(120.0)
Special interest party	61.79	108.3	84.48	-24.11
	(246.8)	(238.3)	(244.9)	(270.7)
Share under 19	-24,467	-25,337	-25,345	-28,879
	(19,634)	(19,802)	(19,689)	(19,837)
Share over 65	23,360*	22,066*	24,309*	22,675*
	(12,713)	(12,658)	(12,702)	(12,359)
Population	15,187**	15,493**	15,910**	17,719**
	(7,208)	(7,204)	(7,163)	(7,156)
Density	-6,875	-6,691	-7,069	-8,081
	(5,226)	(5,202)	(5,152)	(5,160)
Real taxable income	0.0542***	0.0533***	0.0560***	0.0580***
	(0.0199)	(0.0199)	(0.0198)	(0.0193)
Net costs	-0.000349	-0.000375	-0.000353	-0.000374
	(0.000715)	(0.000729)	(0.000707)	(0.000688)
Left-wing ideology	-276.8		231.6	-138.0
	(213.9)		(325.9)	(255.8)
Election year effects	594.0***	591.9***	596.1***	597.8***
	(41.90)	(41.84)	(41.76)	(41.69)
Balanced budget rule	1,269***	1,282***	1,291***	1,228***
	(194.8)	(195.4)	(195.8)	(195.7)
P0				-266.7
				(845.5)
P2				**682.5****
				(307.3)
P3				**1,262*****
				(376.2)
P4				**790.5***
				(404.6)
P5				**789.9***
				(470.7)
P6				**1,889*****
				(580.5)
P7				**3,814****
				(1,696)
P8				**2,600*****
				(338.0)
Minority	**583.2****			
	(267.8)			
Rainbow coalition		304.6		
		(229.9)		
Ideological fragmentation			**251.5****	
			(104.2)	
Constant	-129,829**	-133,053**	-136,995**	-150,384***
	(53,656)	(53,612)	(53,576)	(53,473)
Observations	4,844	4,844	4,844	4,844
R-squared	0.556	0.555	0.556	0.558
Number of municipalities	285	285	285	285

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

Public expenditure

Table A6. Common pool behavior, the balanced budget rule and public expenditure (LSDV)

VARIABLES	(1) FE-MU	(2) Pooled-MU	(3) FE-MC	(4) Pooled-MC
Lagged net costs	0.434***	0.813***	0.286***	0.732***
	(0.0857)	(0.0481)	(0.0662)	(0.0477)
Excess seat share	718.4**	39.39	471.2	155.3
	(300.3)	(190.6)	(352.3)	(262.7)
Time trend	409.6***	49.08	470.3***	102.9***
	(88.25)	(34.76)	(76.42)	(35.12)
Special interest party	89.12	69.06	174.1*	61.15
	(81.46)	(72.40)	(97.68)	(87.96)
Share under 19	30,301***	5,044**	40,821***	15,677***
	(7,629)	(2,238)	(9,134)	(3,140)
Share over 65	-5,959	9,177***	3,409	17,522***
	(5,431)	(1,887)	(5,866)	(3,185)
Population	-8,497**	76.90	-9,312**	29.06
	(3,411)	(49.08)	(3,695)	(71.32)
Density	392.5	-434.0***	1,863	-611.3***
	(1,772)	(95.33)	(2,357)	(104.9)
Real taxable income	0.0403***	0.0119***	0.0572***	0.0144***
	(0.00639)	(0.00291)	(0.00739)	(0.00362)
Left-wing ideology	-159.3	265.8***	-85.91	359.4***
	(106.5)	(87.68)	(102.4)	(101.5)
Election year effects	-18.32	-100.6***	27.67	-130.5***
	(19.41)	(20.80)	(25.69)	(27.19)
Balanced budget rule	3,714***	2,752***	-4,262**	-5,261***
	(1,024)	(809.2)	(1,858)	(1,705)
Council Herfindahl	**9,942*** **	**3,313*** **	**-5,996** **	**-7,027** **
	(1,360)	(1,032)	(2,934)	(2,997)
Council Herfindahl * BBR	**-3,982*** **	**-3,629*** **	**6,182** **	**6,063** **
	(1,473)	(1,123)	(2,651)	(2,464)
Constant	81,736***	1,573	94,571***	8,920***
	(29,100)	(1,834)	(30,124)	(2,515)
Observations	4,843	4,843	4,275	4,275
R-squared	0.929		0.863	
Number of municipalities	285	285	285	285

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

A.4 Bias-corrected LSDV estimates

Long-term debt

Table A7. Common pool behavior and debt I (LSDVC)

	(1)	(2)	(3)	(4)
Balanced budget rule	1,820***	1,874***	1,893***	1,892***
	(189.8)	(182.7)	(184.5)	(166.7)
Excess seat share	-1,450	1,157***	-1,163	241.6*
	(1,492)	(142.1)	(757.1)	(126.9)
Exogenous income	0.255***	0.252***	0.254***	0.258***
	(0.0373)	(0.0375)	(0.0358)	(0.0335)
Time trend	-367.7***	-335.5***	-356.8***	-326.3***
	(60.93)	(53.82)	(46.36)	(53.97)
Special interest party	-51.20	20.47	61.72	92.82
	(572.5)	(477.9)	(517.9)	(528.8)
Share under 19	-25,090***	-18,265***	-19,866***	-19,562***
	(4,982)	(4,515)	(5,130)	(4,997)
Share over 65	15,213*	16,319*	17,316**	14,922*
	(9,220)	(8,688)	(8,769)	(8,496)
Population	16,993***	14,108***	15,100***	14,479***
	(469.5)	(272.2)	(663.9)	(300.3)
Density	-8,754***	-7,339***	-7,723***	-7,169***
	(1,080)	(1,200)	(973.7)	(1,137)
Real taxable income	0.0631***	0.0583***	0.0611***	0.0576***
	(0.0146)	(0.0157)	(0.0138)	(0.0152)
Net costs	-0.000285	-0.000251	-0.000260	-0.000290
	(0.000450)	(0.000449)	(0.000457)	(0.000433)
Left-wing ideology	-174.0**	-369.0***	212.9	
	(81.66)	(95.88)	(398.9)	
Election year effects	646.4***	641.8***	644.8***	640.2***
	(17.10)	(19.82)	(19.36)	(19.51)
P0	87.73			
	(1,269)			
P2	**804.6*** **			
	(185.8)			
P3	**1,430*** **			
	(307.7)			
P4	**1,026*** **			
	(238.4)			
P5	**941.3** **			
	(448.7)			
P6	**1,959*** **			
	(670.8)			
P7	**3,927*** **			
	(686.7)			
P8	2,425			
	(1,559)			
Minority		**471.5*** **		
		(151.0)		
Ideological fragmentation			**279.2** **	
			(137.9)	
Rainbow coalition				**313.2**
				(329.2)
Observations	4,845	4,845	4,845	4,845
Number of municipalities	285	285	285	285

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Long-term debt

Table A8. Common pool behavior and debt II (LSDVC)

VARIABLES	(1)	(2)	(3)	(4)
Balanced budget rule	1,860***	1,547***	1,762***	1,563***
	(180.1)	(157.4)	(179.4)	(149.5)
Excess seat share	-416.1**	1,126***	3,865***	1,105***
	(211.1)	(139.2)	(327.0)	(143.9)
Exogenous income	0.255***	0.247***	0.263***	0.249***
	(0.0358)	(0.0355)	(0.0356)	(0.0364)
Time trend	-357.1***	-379.5***	-367.2***	-382.6***
	(52.72)	(49.19)	(54.68)	(49.74)
Special interest party	-20.52	-323.9	-449.8	-348.7
	(590.8)	(449.1)	(518.2)	(469.0)
Share under 19	-17,946***	-24,245***	-22,348***	-22,757***
	(5,225)	(4,613)	(5,170)	(4,958)
Share over 65	16,348*	15,140*	17,260**	15,202*
	(9,086)	(8,510)	(8,696)	(8,416)
Population	14,463***	15,148***	15,579***	14,897***
	(175.6)	(218.4)	(233.8)	(311.4)
Density	-7,508***	-8,128***	-7,937***	-8,104***
	(1,211)	(1,018)	(1,185)	(1,084)
Real taxable income	0.0622***	0.0674***	0.0626***	0.0680***
	(0.0148)	(0.0142)	(0.0153)	(0.0144)
Net costs	-0.000274	-0.000351	-0.000317	-0.000361
	(0.000443)	(0.000463)	(0.000449)	(0.000456)
Left-wing ideology	-365.9**	-44.92	332.5**	-55.63
	(147.4)	(139.8)	(147.9)	(134.8)
Election year effects	646.6***	645.6***	645.3***	646.9***
	(19.67)	(18.83)	(18.86)	(18.96)
Coalition ENOP	**242.6*** **			
	(35.69)			
Non-linear coalition ENOP	**-11.89*** **			
	(2.783)			
Council ENOP		**4,996*** **		
		(411.7)		
Non-linear council ENOP		**-469.6*** **		
		(71.89)		
Coalition Herfindahl			**11,033*** **	
			(1,454)	
Council Herfindal				**18,647*** **
				(1,887)
Observations	4,845	4,845	4,845	4,845
Number of municipalities	285	285	285	285

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Long-term debt

Table A9. Common pool behavior, the balanced budget rule and debt I (LSDVC)

VARIABLES	(1)	(2)	(3)	(4)
P0				730.3
				(503.0)
P2				**1,472*** **
				(399.4)
P3				**2,192*** **
				(150.5)
P4				**1,881*** **
				(232.8)
P5				**1,496*** **
				(415.4)
P6				**5,028*** **
				(216.3)
P7				**5,526*** **
				(59.51)
P8				**3,955** **
				(1,892)
P0 * BBR				**-714.9*** **
				(162.6)
P2 * BBR				**-1,214*** **
				(462.9)
P3 * BBR				**-1,348*** **
				(505.0)
P4 * BBR				**-1,437*** **
				(308.0)
P5 * BBR				**-1,176***
				(691.4)
P6 * BBR				**-4,238*** **
				(17.74)
P7 * BBR				**-2,688***
				(1,542)
P8 * BBR				**-3,249**
				(4,551)
Balanced budget rule	2,013***	1,893***	2,614***	2,952***
	(288.0)	(184.5)	(256.9)	(86.10)
Share under 19	-17,501***	-19,866***	-32,687***	-38,443***
	(2,203)	(5,130)	(3,421)	(790.5)
Share over 65	16,537***	17,316**	14,655**	12,281*
	(4,912)	(8,769)	(6,123)	(6,456)
Population	13,979***	15,100***	16,996***	18,841***
	(256.9)	(663.9)	(1,021)	(160.4)
Density	-7,171***	-7,723***	-7,407***	-8,360***
	(1,528)	(973.7)	(1,105)	(1,127)
Election year effects	639.3***	644.8***	651.2***	649.2***
	(9.582)	(19.36)	(6.754)	(9.185)
Minority	**1,386*** **			
	(195.1)			
Minority * BBR	**-1,119*** **			
	(245.5)			
Ideological fragmentation			**530.9*** **	
			(194.8)	
Ideological fragmentation * BBR			**-379.3*** **	
			(3.775)	
Observations	4,845	4,845	4,845	4,845
Number of municipalities	285	285	285	285

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Note: Estimates for excess seat share, exogenous income, time trend, special interest party, real taxable income, net costs and left-wing ideology have been excluded from the table in order to save space.

Long-term debt

Table A10. Common pool behavior, the balanced budget rule and debt II (LSDVC)

VARIABLES	(1)	(2)	(3)	(4)
Balanced budget rule	3,108***	5,793***	9,557***	12,031***
	(150.3)	(304.3)	(372.5)	(1,196)
Excess seat share	-729.7***	1,103***	4,319***	1,174***
	(238.1)	(7.199)	(218.1)	(58.62)
Left-wing ideology	-419.4***	-43.14	300.5	-68.56
	(108.4)	(193.1)	(187.3)	(187.9)
Exogenous income	0.254***	0.258***	0.277***	0.270***
	(0.00590)	(0.00786)	(0.00605)	(0.00979)
Net costs	-0.000229	-0.000320	-0.000284	-0.000350
	(0.000589)	(0.000606)	(0.000589)	(0.000601)
Real taxable income	0.0676***	0.0758***	0.0691***	0.0756***
	(0.0212)	(0.0196)	(0.0212)	(0.0208)
Time trend	-390.2***	-450.0***	-422.6***	-449.0***
	(47.18)	(37.94)	(44.79)	(47.59)
Special interest party	2.802	-319.0	-419.2	-303.4
	(469.0)	(359.4)	(411.8)	(388.1)
Share under 19	-31,299***	-47,454***	-42,086***	-44,248***
	(858.4)	(2,630)	(3,382)	(1,186)
Share over 65	11,967**	8,818*	11,210**	8,956*
	(5,235)	(4,767)	(4,693)	(5,175)
Population	15,650***	17,886***	17,995***	17,637***
	(135.7)	(193.0)	(392.6)	(16.22)
Density	-6,756***	-7,133***	-7,128***	-7,067***
	(1,428)	(1,314)	(1,455)	(1,257)
Election year effects	648.9***	651.5***	651.6***	655.0***
	(8.206)	(8.295)	(9.017)	(9.695)
Coalition ENOP	**356.0*** **			
	(39.71)			
Coalition ENOP * BBR	**-200.0*** **			
	(15.58)			
Non-linear coalition ENOP	**-10.31*** **			
	(3.073)			
Council ENOP		**4,578*** **		
		(553.1)		
Council ENOP * BBR		**-1,061*** **		
		(24.76)		
Non-linear council ENOP		**-333.4*** **		
		(99.33)		
Coalition Herfindahl			**17,003*** **	
			(846.4)	
Coalition Herfindahl * BBR			**-9,446*** **	
			(161.4)	
Council Herfindahl				**26,415*** **
				(1,601)
Council Herfindahl * BBR				**-14,100*** **
				(1,872)
Observations	4,845	4,845	4,845	4,845
Number of municipalities	285	285	285	285

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Net income

Table A11. Common pool behavior and net income II (LSDVC)

VARIABLES	(1)	(2)	(3)	(4)
Balanced budget rule	77.10***	161.4***	100.6***	161.3***
	(29.45)	(19.02)	(27.01)	(15.50)
Excess seat share	139.1**	-274.9***	-1,126***	-290.5***
	(61.93)	(21.21)	(69.36)	(39.39)
Exogenous income	0.0798***	0.0859***	0.0800***	0.0858***
	(5.53e-05)	(8.80e-05)	(0.000243)	(0.000222)
Time trend	-155.7***	-146.7***	-153.1***	-146.8***
	(11.39)	(10.17)	(12.02)	(10.69)
Special interest party	-75.41	20.44	38.36	13.25
	(111.4)	(82.16)	(95.19)	(90.00)
Share under 19	-3,910***	-2,907***	-2,908***	-2,777***
	(164.6)	(163.1)	(84.02)	(157.8)
Share over 65	11,486***	11,640***	11,150***	11,770***
	(2,289)	(2,347)	(2,314)	(2,277)
Population	-958.2***	-1,175***	-1,346***	-1,167***
	(10.78)	(21.79)	(18.41)	(30.05)
Density	1,616***	1,984***	1,812***	1,939***
	(503.2)	(458.2)	(504.8)	(476.6)
Real taxable income	0.0364***	0.0344***	0.0363***	0.0344***
	(0.00492)	(0.00468)	(0.00511)	(0.00476)
Net costs	-0.000848***	-0.000816***	-0.000838***	-0.000819***
	(0.000152)	(0.000160)	(0.000154)	(0.000157)
Left-wing ideology	133.6***	40.49	-61.86	47.51
	(21.19)	(44.19)	(43.63)	(44.05)
Election year effects	-93.26***	-95.01***	-93.40***	-94.96***
	(2.835)	(2.706)	(2.994)	(2.848)
Coalition ENOP	**-64.70*** **			
	(16.05)			
Non-linear coalition ENOP	**2.807*** **			
	(0.958)			
Council ENOP		**-1,249*** **		
		(138.8)		
Non-linear council ENOP		**98.85*** **		
		(24.82)		
Coalition Herfindahl			**-3,452*** **	
			(194.4)	
Council Herfindahl				**-6,461*** **
				(720.5)
Observations	4,845	4,845	4,845	4,845
Number of municipalities	285	285	285	285

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Net income

Table A12. Common pool behavior, the balanced budget rule and net income I (LSDVC)

VARIABLES	(1)	(2)	(3)	(4)
Balanced budget rule	39.73	29.95	-183.1***	-494.7***
	(38.97)	(35.94)	(22.38)	(86.10)
Excess seat share	-284.5***	-48.11***	46.56	-39.16
	(63.03)	(4.036)	(297.9)	(226.7)
Exogenous income	0.0810***	0.0760***	0.0778***	0.0818***
	(0.000288)	(0.00164)	(0.000647)	(1.84e-05)
Time trend	-163.2***	-162.0***	-146.3***	-145.0***
	(13.19)	(12.09)	(8.463)	(12.47)
Special interest party	-90.72	-126.8	-96.31	-112.8
	(102.9)	(112.7)	(120.7)	(99.46)
Share under 19	-3,805***	-2,872***	257.9***	2,270**
	(76.34)	(168.4)	(51.95)	(1,105)
Share over 65	11,676***	12,262***	12,708***	13,213***
	(2,318)	(2,419)	(2,689)	(2,745)
Population	-835.5***	-1,019***	-1,459***	-1,894***
	(11.08)	(52.24)	(157.2)	(43.02)
Density	1,522***	1,484***	1,421***	1,532***
	(510.2)	(470.5)	(412.9)	(375.8)
Real taxable income	0.0377***	0.0373***	0.0347***	0.0359***
	(0.00534)	(0.00504)	(0.00461)	(0.00522)
Net cost	-0.000873***	-0.000847***	-0.000879***	-0.000862***
	(0.000149)	(0.000144)	(0.000153)	(0.000153)
Left-wing ideology	159.2***		156.8	127.2***
	(31.78)		(156.5)	(25.58)
Election year effects	-91.51***	-91.83***	-94.23***	-93.91***
	(3.465)	(3.393)	(2.889)	(3.123)
P0				**-495.4*** **
				(172.2)
P2				**-570.4*** **
				(103.1)
P3				**-647.8*** **
				(59.95)
P4				**-598.6*** **
				(65.03)
P5				**-777.2*** **
				(106.7)
P6				**-1,712*** **
				(87.18)
P7				-125.9
				(124.1)
P8				**-2,404*** **
				(606.2)
P0 * BBR				**518.6*** **
				(53.04)
P2 * BBR				**593.6*** **
				(144.4)
P3 * BBR				**681.1*** **
				(179.1)
P4 * BBR				**547.4*** **
				(120.3)
P5 * BBR				**862.4*** **
				(225.0)
P6 * BBR				**1,878*** **
				(38.73)
P7 * BBR				510.9
				(369.5)
P8 * BBR				2,194
				(1,363)
Minority	**-301.8*** **			
	(71.58)			
Minority * BBR	**250.9*** **			
	(64.36)			
Rainbow coalition		**-184.3**		
		(134.5)		
Rainbow coalition * BBR		**159.3*** **		
		(48.42)		
Ideological fragmentation			**-104.1***	
			(54.29)	
Ideological fragmentation * BBR			**129.1*** **	
			(4.824)	
Observations	4,845	4,845	4,845	4,845
Number of municipalities	285	285	285	285

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Net income

Table A13. Common pool behavior, the balanced budget rule and net income II (LSDVC)

VARIABLES	(1)	(2)	(3)	(4)
Balanced budget rule	-291.7***	-1,544***	-2,433***	-3,779***
	(7.280)	(36.19)	(45.04)	(560.2)
Excess seat share	213.4***	-297.6***	-1,272***	-332.0***
	(55.13)	(21.36)	(68.05)	(33.85)
Exogenous income	0.0801***	0.0817***	0.0773***	0.0810***
	(5.93e-06)	(0.000114)	(0.000164)	(0.000906)
Time trend	-147.6***	-123.2***	-137.8***	-125.9***
	(12.05)	(10.74)	(12.33)	(13.40)
Special interest party	-81.44	17.29	29.24	-3.083
	(110.6)	(81.50)	(94.15)	(86.90)
Share under 19	-485.3	5,056***	2,897***	4,384***
	(482.8)	(341.9)	(58.88)	(848.4)
Share over 65	12,676***	13,827***	12,946***	13,769***
	(2,396)	(2,355)	(2,341)	(2,535)
Population	-1,279***	-2,211***	-2,086***	-2,160***
	(42.16)	(48.06)	(0.0537)	(105.6)
Density	1,431***	1,681***	1,571***	1,607***
	(486.4)	(446.0)	(496.8)	(425.3)
Real taxable income	0.0351***	0.0320***	0.0346***	0.0322***
	(0.00500)	(0.00469)	(0.00511)	(0.00500)
Net costs	-0.000860***	-0.000832***	-0.000853***	-0.000830***
	(0.000153)	(0.000158)	(0.000152)	(0.000156)
Left-wing ideology	148.3***	38.90	-52.68	46.76
	(22.59)	(43.72)	(44.09)	(43.87)
Election year effects	-94.01***	-97.49***	-95.16***	-97.62***
	(2.878)	(3.106)	(3.196)	(3.392)
Coalition ENOP * BBR	**57.90*** **			
	(5.718)			
Coalition ENOP	**-99.09*** **			
	(12.60)			
Non-linear coalition ENOP	2.349**			
	(1.001)			
Council ENOP		**-1,014*** **		
		(124.5)		
Council ENOP * BBR		**420.4*** **		
		(13.41)		
Non-linear council ENOP		36.14		
		(22.05)		
Coalition Herfindahl			**-5,415*** **	
			(145.5)	
Coalition Herfindahl * BBR			**3,059*** **	
			(86.54)	
Council Herfindahl				**-9,389*** **
				(304.8)
Council Herfindahl * BBR				**5,289*** **
				(772.5)
Observations	4,845	4,845	4,845	4,845
Number of municipalities	285	285	285	285

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Public expenditure

Table A14. Common pool behavior, the balanced budget rule and public expenditure (LSDVC)

	(1)	(2)	(3)	(4)
VARIABLES	MU.C	MC.C	MU.A	MC.A
Balanced budget rule	2,011***	-2,672***	2,659***	-4,506***
	(2.617)	(345.7)	(385.1)	(306.4)
Excess seat share	1,713***	379.3	681.8***	445.5
	(62.10)	(442.2)	(167.8)	(300.1)
Time trend	334.1***	399.3***	326.2***	402.6***
	(16.62)	(2.573)	(14.80)	(2.961)
Special interest party	34.36	160.9	73.41	162.0
	(140.5)	(172.9)	(126.5)	(169.6)
Share under 19	27,025***	32,149***	25,986***	33,527***
	(487.3)	(6,550)	(995.4)	(6,777)
Share over 65	-5,499***	-454.7	-6,465***	257.0
	(1,939)	(2,380)	(1,999)	(2,913)
Population	-7,048***	-7,988***	-7,168***	-8,241***
	(2.454)	(1,425)	(58.54)	(1,426)
Density	505.3	1,765	472.8	1,864
	(838.0)	(2,785)	(716.3)	(2,689)
Real taxable income	0.0334***	0.0533***	0.0366***	0.0529***
	(0.00682)	(0.00276)	(0.00584)	(0.00245)
Left-wing ideology	-54.47	-109.9***	-180.2**	-117.3***
	(87.24)	(12.39)	(86.33)	(10.52)
Election year effects	-28.02	2.704	-24.45	2.673
	(34.55)	(10.46)	(33.91)	(10.47)
Herfindahl	**4,974*** **	**-2,685*** **	**8,494*** **	**-5,699*** **
	(638.7)	(194.8)	(1,845)	(1,194)
Herfindahl * BBR	**-1,694*** **	**3,288*** **	**-2,837*** **	**6,098*** **
	(89.88)	(699.7)	(376.0)	(722.9)
Observations	4,843	4,275	4,843	4,275
Number of municipalities	285	285	285	285

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Note: MU represents general expenditures of the municipality, while MC represents general expenditures of the municipal corporations. Columns 1 and 2 show estimates for the coalition, while columns 3 and 4 show estimates for the council.

Status quo bias

Table A15. Status quo bias, 1995-2012 (LSDVC)

VARIABLES	(1)	(2)	(3)	(4)
Share under 19	-1,145	-1,451	-800.8	-537.5
	(2,967)	(2,913)	(2,959)	(2,904)
Share over 65	10,654***	10,706***	10,346***	11,110***
	(1,952)	(1,928)	(1,915)	(1,933)
Population	-1,135	-1,202	-1,510	-1,333
	(2,083)	(2,093)	(2,051)	(2,081)
Density	1,531	2,006	1,832	1,979
	(1,782)	(1,739)	(1,743)	(1,741)
Real taxable income	0.0348***	0.0320***	0.0337***	0.0320***
	(0.00540)	(0.00541)	(0.00546)	(0.00543)
Exogenous seat share	0.0818***	0.0834***	0.0820***	0.0850***
	(0.0177)	(0.0176)	(0.0176)	(0.0175)
Left-wing ideology	109.8*	35.17	-71.83	30.82
	(59.50)	(61.23)	(80.43)	(59.45)
Net costs	-0.000740***	-0.000708***	-0.000723***	-0.000715***
	(9.04e-05)	(9.39e-05)	(9.12e-05)	(9.30e-05)
Excess seat share	-77.00	-190.7	-1,031***	-277.7
	(185.6)	(183.6)	(157.1)	(179.7)
Time trend	-144.0***	-131.5***	-137.7***	-132.0***
	(28.74)	(28.53)	(28.95)	(28.58)
Election year effects	-92.18***	-93.03***	-92.75***	-92.92***
	(12.55)	(12.38)	(12.40)	(12.26)
Balanced budget rule	62.04	147.3***	98.03*	164.2***
	(50.84)	(53.13)	(51.83)	(53.97)
Coalition ENOP	**-11.54**			
	(7.359)			
Council ENOP		**-407.7*** **		
		(58.19)		
Coalition Herfindahl			**-3,178*** **	
			(595.8)	
Council Herfindahl				**-6,061*** **
				(760.2)
Observations	4,844	4,844	4,844	4,844
Number of municipalities	285	285	285	285

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Status quo bias

Table A16. Status quo bias, incl. balanced budget rule interaction, 1995-2012 (LSDVC)

VARIABLES	(1)	(2)
Share under 19	4,762	6,480**
	(2,970)	(2,803)
Share over 65	12,403***	13,648***
	(1,830)	(1,876)
Population	-2,087	-2,133
	(2,091)	(2,131)
Density	1,570	1,635
	(1,738)	(1,718)
Real taxable income	0.0323***	0.0301***
	(0.00547)	(0.00539)
Exogenous income	0.0783***	0.0800***
	(0.0175)	(0.0171)
Left-wing ideology	-61.21	37.89
	(82.11)	(60.29)
Net costs	-0.000739***	-0.000728***
	(9.18e-05)	(9.30e-05)
Excess seat share	-1,213***	-333.0*
	(156.6)	(182.7)
Time trend	-124.1***	-113.7***
	(28.98)	(28.03)
Election year effects	-94.09***	-95.50***
	(12.36)	(11.94)
Balanced budget rule	-2,361***	-3,695***
	(276.4)	(578.6)
Coalition Herfindahl	**-5,170*** **	
	(485.3)	
Coalition Herfindahl * BBR	**2,975*** **	
	(327.2)	
Council Herfindahl		**-9,062*** **
		(561.6)
Council Herfindahl * BBR		**5,187*** **
		(774.5)
Observations	4,844	4,844
Number of municipalities	285	285

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Status quo bias

Table A17. Status quo bias, incl. interaction term, 1995-2012 (LSDVC)

VARIABLES	(1)	(2)
Share under 19	-3,424	-3,071
	(5,222)	(2,880)
Share over 65	11,236***	12,049***
	(2,989)	(2,778)
Population	-961.9	-919.2
	(2,467)	(2,094)
Density	1,562	1,695
	(2,375)	(1,865)
Real taxable income	0.0358***	0.0344***
	(0.00726)	(0.00585)
Exogenous income	0.0805***	0.0877***
	(0.0238)	(0.0188)
Left-wing ideology	-61.73	33.08
	(112.6)	(97.19)
Net costs	-0.000851***	-0.000836***
	(0.000142)	(0.000204)
Excess seat share	-1,085***	-316.6
	(266.8)	(274.9)
Time trend	-146.9***	-141.3***
	(36.91)	(27.95)
Election year effects	-93.08***	-96.35***
	(18.52)	(16.25)
Balanced budget rule	68.85	132.6*
	(84.98)	(80.30)
Coalition Herfindahl	**-2,760*** **	
	(877.4)	
Coalition Herfindahl * Lagged net income	**-0.939*** **	
	(0.0201)	
Council Herfindahl		**-5,276*** **
		(844.7)
Council Herfindahl * Lagged net income		**-1.747*** **
		(0.0223)
Observations	4,844	4,844
Number of municipalities	285	285

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Status quo bias

Table A18. Status quo bias, incl. interaction term, 1995-1999 and 2000-2012 (LSDVC)

VARIABLES	(1) 95-99.C	(3) 00-12.C	(2) 95-99.A	(4) 00-12.A
Share under 19	22,460	-4,866	19,483	-4,925
	(14,156)	(9,805)	(13,994)	(10,080)
Share over 65	14,168	1,789	17,956	1,662
	(25,883)	(5,983)	(23,678)	(6,191)
Population	19,889***	-333.1	17,113**	-423.3
	(7,254)	(3,417)	(6,812)	(3,293)
Density	933.4	-701.1	1,255	-594.0
	(5,006)	(2,673)	(4,317)	(2,648)
Real taxable income	-0.158***	0.0502***	-0.144***	0.0502***
	(0.0287)	(0.00847)	(0.0227)	(0.00815)
Exogenous income	-0.119	0.164***	-0.0989	0.164***
	(0.0819)	(0.0410)	(0.0717)	(0.0419)
Left-wing ideology	287.8	82.71	384.9*	38.03
	(257.7)	(132.6)	(218.1)	(136.8)
Net costs	0.000683	-0.00124***	0.000611	-0.00123***
	(0.000805)	(0.000450)	(0.000759)	(0.000471)
Excess seat share	-1,169*	298.4	-518.1	85.25
	(668.4)	(802.9)	(524.0)	(621.4)
Time trend	-44.87	-161.2***	-13.45	-160.6***
	(164.9)	(44.57)	(135.4)	(44.33)
Election year effects	-41.46	-122.7**	-52.99	-123.1**
	(48.92)	(48.57)	(43.82)	(47.89)
Coalition Herfindahl	**-832.3**	**691.5**		
	(1,306)	(1,045)		
Coalition Herfindahl * Lagged net income	**-1.771*** **	**0.0574**		
	(0.0728)	(0.0519)		
Council Herfindahl			**-3,106****	**546.0**
			(1,392)	(1,224)
Council Herfindahl * Lagged net income			**-2.902*** **	**-0.223*** **
			(0.0582)	(0.0577)
Observations	1,139	3,420	1,139	3,420
Number of municipalities	285	285	285	285

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

A.5 Comments on GMM estimations

Below are some general comments regarding the GMM estimations that were done[102]:

- *Overidentification*: Too many instruments can be problematic since it may cause the significance of the variables to be overestimated. A weakness of the Hansen overidentification test for exogeneity of the instruments is that it will be more likely to "accept" the instruments as exogenous/valid the more instruments there are (Roodman 2009b). Therefore, if not including any restriction on the lag length used for the instruments, the Hansen test generally yields perfect p-values of 1.00, which is worrisome. Moreover, we have relatively many control variables, yielding many instruments, which calls for caution when evaluating the Hansen overidentification tests. Fortunately, the overidentification test for validity of the instruments generally fails to reject the null hypothesis for "satisfactory" p-values.

- *Lag length*: In line with the paragraph above, we generally restrict the lag length in the GMM estimations. However, relatively often we find that when restricting the lag length such that the Hansen test yields credible results, but while still keeping the lag length long, we reject the null hypothesis of instrument validity. This is not too surprising, it simply indicates that instruments using longer lags are quite weak, which makes sense considering the variables under consideration. Therefore, short lags are generally used for the validity of the instruments.[103] For example, we quite often choose a lag length using only $y_{i,t-2}$ and $\triangle y_{i,t-1}$ as instruments. In the estimations, the variables are instrumented according to their "characteristics". We have strictly exogenous variables (e.g. year dummies), predetermined variables (e.g. lag of y, which are potentially correlated with past errors) and endogenous variables (e.g. which are potentially correlated with past and present errors). Moreover, we also collapse the instrument matrix to reduce the number of instruments.[104]

- *AR-tests:* The standard following GMM estimations is to perform the Arellano-Bond (1991) test for autocorrelation in the residuals, for which the null hypothesis is that there exists no serial correlation. As expected from the dynamic structure of the models, we reject the null for $AR(1)$ in our estimations. However, we fail to reject the null hypothesis of no autocorrelation for $AR(2)$ and higher orders.

[102] These estimations were performed in order to check that the LSDV and LSDVC estimates obtained were similar to those obtained when using estimators where the explanatory variables could be treated as endogenous/predetermined.

[103] We rarely use more than five lags.

[104] By collapsing we create one instrument for each variable and lag distance, rather than one for each time period, variable, and lag distance. It divides the "GMM-style" moment conditions into groups and sums the conditions in each group to form a smaller set of conditions.