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## Vertical Effects of Fiscal Rules – The Swiss Experience

Heiko T. Burret und Lars P. Feld  
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# VERTICAL EFFECTS OF FISCAL RULES

## THE SWISS EXPERIENCE

December 2015

### Abstract

Formal fiscal rules have been introduced in many countries throughout the world. While most studies focus on the intra-jurisdictional effects of fiscal rules, vertical effects on the finances of other levels of government have yet to be explored thoroughly. This paper investigates the influence of Swiss cantonal debt brakes on municipal finances during the years 1980-2011 by examining aggregated and disaggregated local data. A Difference-in-Differences estimation (two-way fixed effects) provides little evidence that budget constraints at the cantonal level affect average municipal finances and fiscal decentralization.

**Keywords** Fiscal Rule; Vertical Effect; Fiscal Shock; Decentralization, Sub-national Finances

**JEL Classification** H60, H77, H74, H72

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## 1. INTRODUCTION

National and sub-national fiscal rules have been widely implemented in the hope that they would reduce incentives to overspend and thus ensure sustainability of public finances. However, politicians are tempted to circumvent these constraints in order to regain fiscal discretion. Those unintended consequences of fiscal rules have been primarily discussed with respect to window-dressing and creative accounting. Among other things, the accumulation of tax arrears, the reclassification of expenditures, off-budget activities and the use of non-restricted debt instruments may help to disguise public deficits. Consequently, political decision-makers follow the rule only superficially as all they do is temporarily embellish the targeted headline indicators and hardly improve the overall fiscal position.

In addition to such intra-jurisdictional effects, there are good reasons to assume that fiscal rules also have vertical effects in federations with multiple government layers. Fiscal rules on an upper government level may influence lower government finances even though the latter are not formally covered by the constraints. Unlike accounting gimmicks, vertical effects of fiscal rules might change recurring costs on the upper and lower government levels and affect the extent of fiscal decentralization. From a theoretical standpoint, there could be vertical effects in both directions, such as the improvement or the burdening of finances at a lower level of government. Thus, whether and how fiscal rules constraining fiscal deficit at an upper level of government affect finances at a lower level of governments is a question to be answered empirically.

Although little research exists on the vertical effects of fiscal rules, German municipalities complained that the new budget constraints at the state level (Laender) would burden their finances (German Association of Cities, 2011; Lenk et al., 2012).<sup>1</sup> Swiss municipalities expressed similar concerns, notably claiming that the canton of Zurich would consolidate its budget on their back and shift the fiscal burden to the local level (NZZ, 2004; Steiner et al., 2012).

Against this background, this paper analyzes the influence of cantonal debt brakes on local finances in Switzerland. To reveal vertical effects, we examine a sample of all municipalities aggregated at the cantonal level (1980-2011) and a sample of disaggregated data of the 139

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<sup>1</sup> Petra Roth (then President of the German Association of Cities and mayor of Frankfurt am Main) stated in 2011 that communities needed a protective shield, which prevented the German states (Laender), in complying with their debt brakes, from shifting public debt to their municipalities, thus deteriorating local finances.

largest cities (1982-2007).<sup>2</sup> A Difference-in-Differences estimation (two-way fixed effects) provides little evidence that budget constraints at the cantonal level affect average local finances or decentralization. This holds even in situations that make vertical effects most likely, i.e., in times of fiscal shocks at the cantonal level.

The remainder of this paper is organized as follows: Section 2 reviews empirical studies. Section 3 presents the Swiss institutional framework alongside with the discussion of vertical transmission channels and ends with testable hypotheses. The data and empirical strategy are presented in Section 4. The baseline results are reported in Section 5 and the robustness checks in Section 6. Finally, Section 7 discusses implications of the main findings and offers concluding remarks.

## 2. LITERATURE REVIEW

The first empirical studies on the effects of sub-national fiscal rules on budget outcomes have presumably been conducted in the late 1960s for the US (Mitchell, 1967; Pogue, 1970). More recent studies provide evidence that strong budget limitations support fiscal discipline among US states (for a survey see Burret and Feld, 2014). Similar evidence exists for other countries, such as Canada (Imbeau and Tellier, 2004), Latin America (Alesina et al., 1999), Africa (Gollwitzer, 2010), OECD countries (Guichard et al., 2007), EU countries (Ayuso-i-Casals et al., 2007; De Haan et al., 1999; Debrun et al., 2008; Foremny, 2014; Hallerberg et al., 2007; Marneffe et al., 2011) and various other states (Blume and Voigt, 2013; Lavigne, 2011; Singh and Plekhanov, 2006). Interestingly, Reuter (2015) shows that, even though they are often violated, fiscal rules are effective as they act as a kind of benchmark for good fiscal policy in EU countries.<sup>3</sup>

Empirical studies on Switzerland consistently find that cantonal deficit restrictions, i.e., debt brakes, trigger sound cantonal finances. In an initial study on cantonal debt brakes, Feld and Kirchgässner (2001a) construct a fiscal rule index that ranges from 0 to 3 for the period 1986-1997. Their results suggest that strong debt brakes significantly depress cantonal debt and deficits. Schaltegger (2002) draws a similar conclusion for the years 1980-1998. Feld and Kirchgässner (2008) provide further evidence for the same period. Krogstrup and Wälti (2008) confirm the previous findings after controlling for voter preferences. A new fiscal rule index

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<sup>2</sup> To be precise, the first sample covers municipalities from all cantons except for Basel-City.

<sup>3</sup> For the relationship between fiscal rules and macroeconomic stability refer to, e.g., Sacchi and Salotti (2015).

that assigns values between 0 and 100 is constructed by Yerly (2013). Again, in line with previous results, she finds that harder budget rules reduce cantonal deficits. Finally, Burret and Feld (2015) exploit a rich dataset and show that cantonal debt brakes have the most pronounced impact on the current budget (rather than the total budget) and mitigate political budget cycles.

A somewhat different question is analyzed by Luechinger and Schaltegger (2013). They find evidence that debt brakes significantly reduce the probability of projected and realized cantonal deficits. While Chatagny (2015) shows that revenue projection errors are significantly related to the ideology of the cantonal finance minister, debt brakes tend to reduce the impact of ideology. In a related strand of literature, Feld et al. (2013) provide evidence that cantonal yield spreads are significantly decreased by strong debt brakes and by the no-bailout regime established subsequent to the Leukerbad Supreme Court decision in 2003. Similar findings are obtained for fiscal rules in the EU (Iara and Wolff, 2014). Interestingly, a recent study on Swedish municipalities suggests that bailouts may favor fiscal discipline if the granted payments are conditional on measures that improve the situation (Dietrichson and Ellegård, 2015).

However, the overall effect of budget rules on fiscal outcomes might be lower than indicated by the targeted headline variables if the constraints are avoided by means of fiscal gimmickry.<sup>4</sup> Empirical analyses focus either on US states (Bennett and DiLorenzo, 1983; Briffault, 1996; Bunch, 1991; Costello et al., 2012; GAO, 1993; Mitchell, 1967; von Hagen, 1992, 1991) or EU member countries (Balduzzi and Grembi, 2011; Bernoth and Wolff, 2008; Buti et al., 2007; Dafflon and Rossi, 1999; Koen and van den Noord, 2005; Milesi-Ferretti and Moriyama, 2006; von Hagen and Wolff, 2006). More recently, Burret and Feld (2015) showed that Swiss cantonal debt brakes can hardly be associated with a shift of expenditures from the (constrained) current budget to the (unconstrained) investment budget.

While a few studies mention the possibility of vertical effects of fiscal rules (Briffault, 1996; Heins, 1963; Mitchell, 1967; New, 2001; Sørensen et al., 2001), empirical evidence is scarce. Stansel (1994) provides anecdotal evidence that tax and expenditure limitations in US states lead to a shift of costs to local governments. To the best of our knowledge, so far only five empirical studies touch upon the issue of vertical effects of fiscal rules (Table 1). Nice (1991)

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<sup>4</sup> For various guises of fiscal gimmickry refer to Irwin (2012).

shows that fiscal rules in US states are, if anything, associated with decreased debt at the local level. Unlike Nice (1991), von Hagen (1992) finds that the ratio between municipal and state debt tends to be larger in states with fiscal rules. Similarly, Kiewiet and Szakaly (1996) conclude that restrictive provisions at the state level lead to more debt issuance at the local level.

Burret and Feld (2015) and Feld and Kirchgässner (2008) provide some evidence for Switzerland. The two studies examine the effect of cantonal debt brakes on cantonal deficits and combined cantonal and local deficits. As the estimated debt brake coefficient is highly significant and of almost equal size in both equations, the studies consistently conclude that debt brakes have little impact on local deficits. If anything, evidence suggests that cantonal debt brakes can be associated with decreased local deficits.

Table 1 Summary of studies on vertical effects of fiscal rules

Study	Period	Jurisdictions	Dependent variable (selection)	Fiscal rule	Vertical effect?
Nice (1991)	1982	US states	- State debt - State and local debt	- Legally permitted amount of debt - Budget rules	<b>NO</b> (if anything local debt decreases)
Von Hagen (1992)	1985	US states	- Debt ratio between local and state debt	- Debt limitations - Fiscal rule index	<b>YES</b> (local debt increases)
Kiewiet and Szakaly (1996)	1961-1990	US states	- State debt - State and local debt	- Limitations on bond issuance	<b>YES</b> (strict limits increase local debt)
Feld and Kirchgässner (2008)	1980-1998	Swiss cantons (5 with a fiscal rule)	- Cantonal deficit - Cantonal and local deficit	- Fiscal rule index	<b>NO</b>
Burret and Feld (2015)	1980-2011	Swiss cantons (18 with a fiscal rule)	- Cantonal (current) deficit - Cantonal and local (current) deficit	- Debt brake dummy and fiscal rule index	<b>NO</b> (if anything local deficit decreases)

A related strand of literature discusses the vertical effects of direct democracy. Matsusaka's findings (1995) suggest that local spending is higher in US states with popular initiatives. While Feld et al. (2008) show that centralization of expenditure is less likely in Swiss cantons with direct democracy, Funk and Gathmann (2011) find only little evidence for an impact of cantonal direct democracy on local spending and decentralization. Finally, Galletta and Jametti (2015) find that cantonal fiscal referenda increase local spending particularly in municipalities without fiscal referenda. In a companion paper, Galletta (2015) shows that decentralization decreases once mandatory referenda are introduced at the local level. Another related field of research

focuses on vertical tax externalities. Several studies provide evidence in favor of a significant relation between national and sub-national tax rates (see Brülhart and Jametti, 2006, and for a survey on the effects of decentralization Baskaran, 2011, and more recently Ligthart and van Oudheusden, 2015).

In sum, the few existing studies on vertical effects of fiscal rules either only elude to the issue, are inconclusive, use combined data of the regional and local levels, or analyze meagre datasets. The only existing papers on vertical effects of fiscal rules in Switzerland (Burret and Feld, 2015; Feld and Kirchgässner, 2008) fall short of the mark since the conclusions are derived by simply comparing the debt brake coefficient in the cantonal deficit equation with the one in the cantonal and local deficits equation. This is not sufficient for identification, if effects of cantonal debt brakes on local expenditure (revenue) are compensated by equivalent adjustments of local revenue (expenditure). Such a compensation is not unlikely given the large autonomy that the local level has on the expenditure and revenue side in combination with broad local fiscal responsibility. Unlike previous studies, we exploit rich panel datasets that cover debt brakes in 17 Swiss cantons, various fiscal indicators and other covariates at the municipal level. Furthermore, we are, to the best of our knowledge, the first to analyze the effects of fiscal rules on decentralization.

### **3. INSTITUTIONAL FRAMEWORK AND VERTICAL TRANSMISSION CHANNELS**

The federal framework of Switzerland is characterized by a strong tradition of fiscal autonomy and fiscal responsibility of the federal level as well as of the 26 cantons (states) and the 2,353 municipalities. Despite amalgamations, half of all municipalities still count less than 1,000 residents. While the structure of the local level is subject to cantonal provisions, municipalities de facto constitute the third level of government and enjoy substantial autonomy ever since (Meyer, 2011).<sup>5</sup> In order to coordinate the provision of public services, the cantons frequently award/withdraw mandates to/from their municipalities. For instance in 2004, the fiscal restructuring program of the canton of Zurich mandated the duty to contribute a basic amount to the funding of inpatient treatment to the local level. Since 2012, the canton assumes full hospital funding, while the funding of home care and nursing home is fully dependent on

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<sup>5</sup> Article 50 as set out in the Swiss Constitution of 1999 states: "The autonomy of the communes shall be guaranteed in accordance with cantonal law. The Confederation shall take account in its activities of the possible consequences for the communes. In doing so, it shall take account of the special position of the cities and urban areas as well as the mountain regions." (Translation of the authors.)



municipalities (VZF, 2011). Besides their executive functions related to the implementation and administration of cantonal mandates, the municipalities carry out legislative and judicial tasks. The environment and culture and recreation are by far the most important areas of local responsibility as measured by their spending share (Table 2).

Table 2 Share of public expenditures in different categories by levels of government, 2010

<i>in %</i>	Federal level	Cantonal level	Municipal level
Administration	35.9	32.2	31.9
Security	36.1	46.2	17.7
Education	14.2	57.5	28.3
Culture and recreation	8.0	31.5	60.5
Health care	3.2	83.9	12.8
Social welfare	41.1	38.8	20.1
Transportation and communication	44.8	32.4	22.8
Environment	14.9	22.2	63.0
Economy	43.4	41.3	15.3
Finances and taxes	68.4	18.9	12.6
All areas	33.6	42.3	24.1

Note: Social security sector is not shown. Therefore, values may not add up to 100%. Source: Swiss Federal Department of Finance.

While annual local spending accounts for almost one quarter of total public spending, Switzerland has the lowest share of municipal funding through transfers (around 13%) across the whole of Europe (Council of Europe, 1997: 25; Rühli, 2013). The communities can set surcharges on income taxes relatively autonomously, but tax rates and bases are defined at the cantonal level (Feld et al., 2011). The variation of local tax burdens is mitigated by complex cantonal schemes of fiscal equalization among communities. Besides the fiscal autonomy of tax and expenditure, municipalities have the right to borrow and issue debt.

As cantons face – at least partial – responsibility for local finances, all cantons have legal provisions regarding control, supervision, approval and regulation of municipal finances. For instance, all municipalities are required to submit their annual accounts to a cantonal supervisory institution. Local accounting is subject to cantonal authorization in half of all cantons. As reported by Finances Publiques (2004), the cantons do indeed exercise their responsibility and reject cantonal budgets or impose local draft budgets in case of large fiscal imbalances. The canton of Zurich has even set a municipal law that requires local tax adjustments if the municipal budget is not balanced in the medium term.

Still, loopholes and deficiencies in cantonal monitoring became obvious in the case of Leukerbad. In 1999, the municipality of Leukerbad was unable to finance its accumulated CHF 346 million in debt (around CHF 200,000 per capita) and was placed under forced

administration and compulsory execution (Beiratschaft) of its canton Valais. Subsequent to a lawsuit by several creditors, the Swiss Supreme Court judged in 2003 that a bailout by the canton of Valais was not justified. The Court nevertheless recommended broadening and intensifying cantonal supervision (Geschäftsprüfungskommission des Grossen Rates, 1999; Swiss Supreme Court 2c.4/2000/mks, 2C.1/2001/mks).

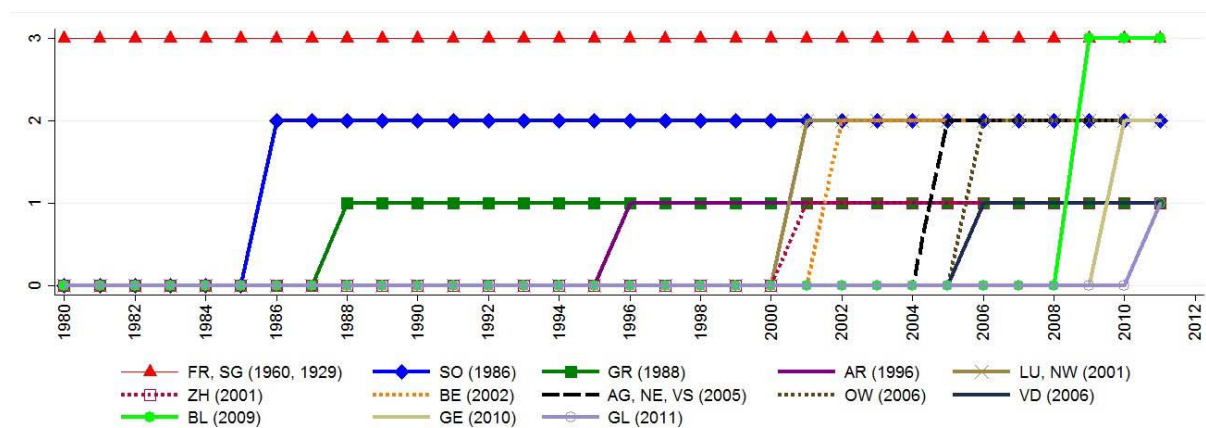
The 26 Swiss cantons enjoy a much larger extent of fiscal autonomy than their municipalities. To secure fiscal sustainability and transparency, the Conference of Cantonal Finance Ministers agreed on a role model law for cantonal budgeting in 1981. Amongst other things, the law requires the current budget to be balanced in the medium term and restricts debt increases in the investment account.<sup>6</sup> By the end of 2012 all cantons but Appenzell Inner-Rhodes had adopted some kind of budget rule in their constitution or budget law. The 25 cantonal budget rules vary substantially with respect to their design and date of introduction. Feld and Kirchgässner (2008, 2001a) and Feld et al. (2013) exploit this large variation in cantonal fiscal regulations in order to construct a fiscal rule index. They assign an index value between zero and three according to the number of requirements the fiscal rule fulfils. The evaluated components are: (I) the strong link between budget planning and execution, (II) the numeric deficit limit and (III) automatic sanctions. Despite the prevalence of fiscal rules today, we still assign an index value of zero to nine cantons, as their constraints do not meet any of the three criteria. Remarkably, all three requirements are only satisfied by the relatively old debt brakes of St. Gall (1929, revised 1997) and Fribourg (1960, revised 1996) and since 2008 Basel-County. While debt brakes have also been in place for some time in Solothurn (1986, revised 2005), Grisons (1988) and Appenzell Outer Rhodes (1996), most other cantons only followed after the turn of the millennium (Figure 1).<sup>7</sup>

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<sup>6</sup> For the latest amendment see Art. 33 and Art. 34 role model law for cantonal budgeting 2015, available from <http://www.srs-cspcp.ch/srscspcp.nsf/go/a78ff96571bb620bc1257afe006b3fdb?OpenDocument&lng=de>.

<sup>7</sup> The dates in parentheses indicate the year the law became effective. Stauffer (2001) and more recently Conference of Cantonal Ministers of Finance (2012) provide a broad overview of cantonal budget rules. Since the construction of a stringency index is always based on subjective judgments, other studies classify some cantons differently across time (e.g., Feld and Kirchgässner, 2008; Feld et al., 2013; Kirchgässner, 2013; Luechinger and Schaltegger, 2013; Yerly 2013).

Figure 1 Stringency index of cantonal fiscal rules, 1980-2011



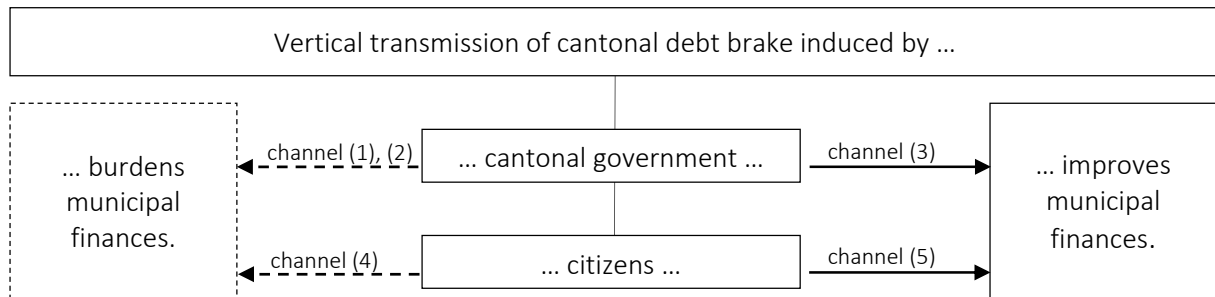
Note: AG Aargau, AR Appenzell Outer-Rhodes, BL Basel-County, BE Bern, FR Fribourg, GE Geneva, GL Glarus, GR Grisons, LU Lucerne, NE Neuchâtel, NI Nidwalden, OW Obwalden, SG St. Gall, SO Solothurn, VD Vaud, VS Valais, ZH Zurich. Appenzell Inner-Rhodes (AI), Basel-City (BS), Jura (JU) Schaffhausen (SH), Schwyz (SZ), Thurgau (TH), Ticino (TI), Uri (UR) and Zug (ZG) are not depicted since their fiscal rules have not been eligible to classify as debt brakes in any year during the period 1980-2011. Thus, an index value of "0" is assigned to them. Illustration based on Feld et al. (2013) and own research.

Although the cantonal debt brakes exclusively target the cantonal level, the rules might still influence municipal finances. Such a vertical transmission could be induced by measures taken by the cantonal government or citizens (Figure 2). In light of a budget constraint, politicians might be tempted to consolidate public budgets through spending cuts that alienate the electorate as little as possible. With this in mind cantonal governments could (1) *mandate the provision of public services to their municipalities* or (2) *modify laws to reduce transfers to the local level* and thereby burden municipal finances. Conversely municipal finances could improve as cantons might (3) *work towards a consolidation of local finances* in order to hedge the risk of higher transfers or future bailouts. The cantonal solicitude might have been less pronounced prior to the debt brake, as cantons could have financed transfers (bailouts) by incurring higher debt. To improve local finances, the canton could take implicit measures such as broadening and intensifying the supervision of local finances or take explicit measures such as a strengthening of the regulatory regime or a withdrawal of mandates from their municipalities.

Alternatively, vertical effects could be provoked by citizens. If fiscal rules prevent cantons from responding to citizen demands, citizens might (4) *shift their demands to the municipal level of governments*, resulting in a burdening of local finances. However, it also seems possible that citizens would be more willing to (5) *devolve responsibilities to the cantonal government*,

believing that cantonal overspending is effectively controlled for by budget constraints. Thereby the fiscal burden of local governments would be reduced.<sup>8</sup>

Figure 2 Vertical transmission channels of fiscal rules



Source: Own illustration.

As Figure 2 indicates, vertical effects could basically work in opposite directions. However, we cast doubt on the possibility of a notable transmission of debt brakes through channels (1), (2) and (4), since such a transmission is likely to be accompanied by decreased or at least non-increasing cantonal spending. However, Burret and Feld (2015) report evidence that debt brakes increase cantonal spending (by less than revenues). Moreover, a notable transmission through channel (2) is additionally questionable as cantons transfer only minor amounts to their municipalities.

Thus, a transmission through channels (3) and (5), i.e., debt brakes induce cantons to work towards a consolidation of local finances or increase citizens' willingness to delegate responsibilities to the canton (rather than to the local level), seems to be more likely. Regardless of which of the two channels dominates, we expect the municipal response to imply lower municipal expenditures, deficits and debt. Against this background, we will test the following hypotheses:

H(1) *The introduction of a (strong) debt brake in a canton is associated with decreased expenditures, deficits and debts in the municipalities located within that canton.*

H(2) *Given H(1), a (strong) debt brake in a canton should lead to a lower level of fiscal decentralization in that canton.*

<sup>8</sup> For transmission channel (4) see Nice (1991). The channel (5) is based on vertical effects of direct democratic institutions as stated by Funk and Gathmann (2011). While cantonal debt brakes could also influence federal finances through similar transmission channels, we expect downward effects to dominate, given the design of the Swiss federal framework.

To conclude, Switzerland provides an almost ideal institutional setting to examine the effects of fiscal rules on the finances of lower levels of government. First, despite institutional differences between Swiss cantons, the common political, cultural and constitutional framework implies less heterogeneity across municipalities than across countries making spurious correlation due to omitted variables less likely (Luechinger and Schaltegger, 2013). Second, time and canton fixed effects can be applied since debt brakes have been implemented at different points in time. Third, seven cantons have yet to introduce a (credible) debt brake, giving us a treatment and a control group. Fourth, empirical evidence suggests that debt brakes are strong enough to trigger fiscal consequences at the cantonal level. Fifth, cantonal debt brakes do not formally cover the local level. And sixth, the fiscal framework allows for vertical transmission channels.

#### **4. DATA AND EMPIRICAL STRATEGY**

In order to test our hypotheses, we use two datasets. The first sample covers harmonized data of the Swiss municipalities aggregated at the cantonal level for each year between 1980 and 2011. We include all cantons except Basel-City, as it is not possible to distinguish between the budget of the canton and that of its capital. The second sample comprises harmonized disaggregated fiscal indicators and covariates of large Swiss cities and communities that were members of the Swiss Association of Cities during the period 1982-2007.<sup>9</sup> This second dataset covers around 40% of the total Swiss population in up to 139 cities with a population size between 2,272 (Arosa) and almost 400,000 (Zurich). This sample includes cantonal capitals and large cities of all cantons but Basel-City and Obwalden.

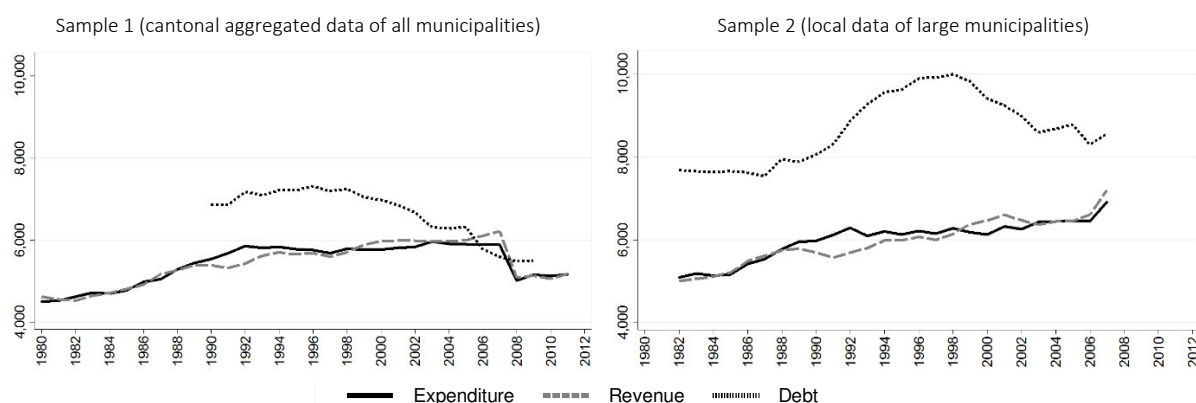
While cantonal debt brakes might affect local finances through multiple channels, we examine the consequential (indirect) effects on real local expenditure, revenue, debt and deficits. The budget balance variable results from subtracting revenues from expenditures such that a deficit has a positive sign and a surplus a negative one. Additionally, real local spending is divided into nine categories, and both spending and revenue decentralization are separately presented as left-hand side variables. To illustrate the development of the main dependent variables, Figure 3 depicts sample-aggregated data. While local debt per capita peaked in the mid-1990s and

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<sup>9</sup> We refrain from including observations after 2007 due to a profound revision in reporting standards. Furthermore, while the Swiss Federal Statistical Office defines municipalities (communities) with a population size above 10,000 as cities, we do not differentiate between the terms.

subsequently decreased, local spending and revenue increased until the early 1990s and somewhat stabilized thereafter. The developments on the left-hand panel of Figure 3 are partly influenced by a major revision in accounting standards in 2008 with some retroactive effects from 1990. In comparison to the average values of all communities (left), the cantonal capitals and large cities covered by the second sample (right) have substantially higher debt, revenue and expenditure per capita and generate less budget surpluses.

Figure 3 Development of Swiss municipal finances in real Swiss Francs per capita



Note: The first sample covers municipalities from all cantons except for Basel-City. The second sample covers between 123 and 139 municipalities from all cantons except for Basel-City and Obwalden. Own illustration based on data from Swiss Federal Finance Administration and Statistical Yearbooks of the Swiss Association of Cities.

Drawing on the literature (e.g., De Haan and Sturm, 1994; Feld and Kirchgässner, 2001a, 2001b; Roubini and Sachs, 1989a, 1989b; Shadbegian, 1996), we use institutional, economic, socio-demographic and political variables to explain local fiscal outcomes.<sup>10</sup> The main institutional variables measure the presence and stringency of cantonal fiscal rules. The local institutional setting, such as town meetings, municipal parliaments or mandatory fiscal referenda either does not vary sufficiently across time to be taken into account, or data is not continuously available. However, indicators of direct democracy at the cantonal level enter the robustness checks. While we could not gather information on budget rules on the local level during the period of interest, evidence suggests that local fiscal constraints do not have a significant impact on municipal finances (Feld and Kirchgässner, 2001a).

To capture (macro-) economic conditions, we include the unemployment rate<sup>11</sup>, taxable income, indicators of inter-governmental grants (i.e., own revenues) and relative local income. Socio-demographic indicators map the age structure of the population, cultural idiosyncrasies

<sup>10</sup> Refer to this literature for a broader discussion of our control variables.

<sup>11</sup> A direct influence of the level of unemployment on municipal finances is unlikely since unemployment insurance is financed by a federal payroll tax and benefits are regulated by state and cantonal authorities. However, the unemployment rate can be used as a proxy for welfare spending (partly) paid for by the local level of government.

(approximated by the language) and the number of citizens. Political variables measure the ideology of the government and the number of parties in the municipal executive. Table A.1 reports a summary of the descriptive statistics and the mean values and standard deviations separately for municipalities in cantons with and without a debt brake. As indicated by a simple t-test in the last column of Table A.1, the difference of means between the two groups is significant with respect to our main dependent variables in both samples. However, spending and revenue decentralization are not significantly different in the two groups. The definition and the source of each variable are provided in Table A.2. Due to data constraints, some controls could only be employed in the second model:

Sample/Model 1: Cantonal aggregated local data of all municipalities

$$(1) \quad Y1_{j,t} = \beta_0 + \beta_1 \text{Rule}_{c,t} + \beta_2 \text{RelativeIncome}_{j,t} + \beta_3 \text{Income}_{j,t} + \beta_4 \text{Unemployment}_{c,t} + \beta_5 \text{Old}_{c,t} + \beta_6 \text{Young}_{c,t} + \beta_7 \text{German}_{c,t} + \beta_8 \text{Pop}_{c,t} + \gamma_c + \tau_t + \epsilon_{j,t},$$

Sample/Model 2: Disaggregated local data of 139 large municipalities

$$(2) \quad Y2_{i,t} = \beta_0 + \beta_1 \text{Rule}_{c,t} + \beta_2 \text{RelativeIncome}_{i,t} + \beta_3 \text{Income}_{i,t} + \beta_4 \text{Unemployment}_{i,t} + \beta_5 \text{Old}_{i,t} + \beta_6 \text{Young}_{i,t} + \beta_7 \text{German}_{c,t} + \beta_8 \text{Pop}_{i,t} + \beta_9 \text{OwnRev}_{i,t} + \beta_{10} \text{Ideology}_{i,t} + \beta_{11} \text{Coalition}_{i,t} + \gamma_c + \tau_t + \epsilon_{i,t},$$

where:

Y1	Per capita real local expenditure, revenue, debt, deficit, spending in nine categories or spending and revenue decentralization (natural log except for deficit and decentralization variables)
Y2	Per capita real local expenditure, revenue, debt, deficit, spending in nine categories (natural log except for deficit variable)
Rule	Either a dummy variable that equals one if a cantonal debt brake is in place and zero otherwise or a fiscal rule index that measures the stringency of cantonal debt brakes
RelativeIncome	Taxable income per capita as share of average taxable income per capita in the sample
Income	Real taxable income per capita (natural log)
Unemployment	Unemployment rate
Old	Share of population older than 65 years of age
Young	Share of population younger than 25 years of age
German	Share of German speaking population
Pop	Population (natural log)
OwnRev	Share of own local revenues on total revenues
Ideology	Share of left-wing parties in the municipal government
Coalition	Number of political parties in the municipal government

and  $t$  indicates the year,  $i$  the municipality,  $j$  the municipalities within one canton and  $c$  the canton.

The models are estimated using OLS with canton ( $\gamma$ ) and time ( $\tau$ ) fixed effects to control for unobserved heterogeneity and unobserved time-specific factors affecting all entities. The two-way fixed effects estimator can be seen as a generalization of the Differences-in-Differences estimator as both techniques basically eliminate time trends affecting all units and time-constant differences between the units.<sup>12</sup> A key assumption for such a research design is that the treatment group (municipalities in a canton that is constrained by a debt brake) and control group (municipalities in a canton that is not constrained by a debt brake) would follow a common trend in the absence of treatment. While this is obviously not observable for the treated, similar trends before the treatment can strengthen the validity of the Differences-in-Differences estimates. Figure A.1 illustrates the development of local finances in cantons prior to the introduction of a debt brake (treatment) and in the control groups. The graphs suggest that local finances followed rather similar trends in all groups. In addition, the common political, cultural and constitutional Swiss framework adds to the credibility of the common trend assumption.

The use of two-way fixed effects and the common framework in Switzerland make spurious correlation due to omitted variables less likely. Nevertheless, the effect of cantonal debt brakes might not be the same for all treated units across time. Treatment heterogeneity might be an issue as we examine a long time-period of up to 32 years. For instance, once a debt brake passes it enters a pre-existing (explicit and implicit) framework that could change over time such that even similarly designed rules may have a different impact if much time passes between their statutory introductions. Similarly, vertical effects might be observed particularly during times of fiscal stress at the cantonal level and be less pronounced when the economy is running smoothly. Moreover, the common trend assumption is hard to defend for long periods with the consideration of pre-treatment trends. Therefore the robustness analysis separately examines the effect of “early” and “late” debt brake adopters and studies the influence of fiscal stress at

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<sup>12</sup> The robustness analysis provides the results of models without fixed effects. Simplified, the Differences-in-Differences estimator can be written as:

$$\hat{\beta} = (\bar{y}^{\text{treatment units after treatment}} - \bar{y}^{\text{treatment units before treatment}}) - (\bar{y}^{\text{control units after treatment}} - \bar{y}^{\text{control units before treatment}}).$$



the cantonal level. Besides, the investigation of sub-periods helps to cope with structural breaks due to a major revision in accounting standards.

On the one hand, endogeneity of the cantonal debt brakes is less of an issue since the municipalities enjoy large autonomy. On the other hand, the rules reflect the voters' preference since they are commonly subject to referenda. Thus, it is questionable whether the estimated effect is causal on the debt brake or on voters' preferences. To clarify whether debt brakes induce structural breaks, we calculate standard Chow breakpoint tests. The test has the null hypothesis that parameters (slopes and the intercept) of municipalities located in cantons with a debt brake are not different from those of the other group. If the null hypothesis is rejected, cantonal debt brakes induce a break in the regression coefficients. In addition, we follow Poterba (1997, 1996) and address potential endogeneity of fiscal institutions by controlling for voter preferences. Dafflon and Pujol (2001) suggest that the voters' fiscal preferences are largely time-invariant and are, thus, already captured by the fixed effects. Nevertheless, we adopt a frequent approach and use the share of left-wing parties in the municipal executive as an indicator of voter preferences in the second sample. The issue is further investigated in the robustness analysis. The endogeneity of our economic controls is less of an issue as they are unlikely to be influenced by the dependent variables within the same year.

Panel data frequently results in biased standard errors due to autocorrelation and cross-sectional dependence of the error terms that arise from common shocks and unobserved components. In fact, the Pesaran (2004) pre-estimation test rejects error cross-section independence for almost all outcome variables at the 1% level (Table A.5).<sup>13</sup> While cluster-robust standard errors are a common solution, a small number of clusters can lead to a substantial downward bias of estimated standard errors and, thus, an overstatement of statistical significance (Angrist and Pischke, 2009; Cameron et al., 2008). For accurate inference, the data should have at least 50 clusters of roughly equal sizes or at least 20 balanced clusters (Kézdi, 2004; Nichols and Schaffer, 2007). Rogers (1993) suggests that a cluster should not contain more than five percent of the sample data. As the first (aggregated) dataset comprises 25 cantons and data is almost equally distributed among cantons, we adjust the errors for clustering on the cantonal level and correct for heteroscedasticity following Luechinger and

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<sup>13</sup> Results could only obtain for the first sample, as the highly unbalanced second sample provides too few common observations across the panel to perform the test.

Schaltegger (2013) who examined a similar dataset and concluded that clustering at the cantonal level does not imply a substantial bias with reference to simulations by Bertrand et al. (2004) and Cameron et al. (2008).

Unlike the first sample, the second one comprises 139 municipalities that are considerably differently distributed among the 24 cantons. For instance, 24 communities are located in Zurich and 18 in Berne and only one municipality in Uri, Nidwalden, Glarus and both Appenzells. Since the cantonal cluster sizes would be largely unbalanced in this case, the cure of cluster-robust standard errors could be worse than the disease (Nichols and Schaffer, 2007). Thus, cluster-robust standard errors are only reported for the first sample. To further improve inference in both datasets we calculate robust standard errors based on the wild-cluster bootstrap-t procedure. The resampling method relaxes the restriction of equally sized clusters and has been found to work well in cases with few clusters (Cameron and Miller, 2013; Cameron et al., 2008).<sup>14</sup> In addition, it is quite robust to differences in the number of units in the treatment and control groups (MacKinnon and Webb, 2015). Thus, statistical inference is based on cluster-robust standard errors and bootstrapped p-values in the first sample and solely on bootstrapped p-values in the second sample.

## 5. BASELINE RESULTS<sup>15</sup>

### 5.1. VERTICAL EFFECTS ON EXPENDITURE, REVENUE, DEBT AND DEFICIT

Table 3 presents the baseline regressions for the **first model** of cantonal aggregated local expenditure, revenue, debt and deficits. Following the Wald test results, we include canton and year fixed effects. While the estimated baseline model explains around 60% of the variance of the expenditure and revenue equations, it has notably less explanatory power regarding debt and deficits. The results suggest that the introduction of a cantonal debt brake induces local expenditure, revenue, deficits, and debt to decrease. According to these estimates, the presence of a cantonal fiscal rule reduces per capita local spending by almost 3%, local revenue by around 2.2% and local debt by around 1%. While the findings are largely in line with our

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<sup>14</sup> The method uses the wild bootstrap to resample clusters of residuals obtained from regressions which impose the null hypothesis ( $\beta = 0$ ) and re-estimates the original equation with the newly generated residuals. The pseudo samples of clusters are formed by multiplying the residuals with 1 and -1 with a probability of 0.5. The so-called Rademacher weight provides asymptotic refinement. See Cameron et al. (2008) for details. We employ the Stata post-estimation command `bootwildct` provided by Malde (2012) with 1000 repetitions.

<sup>15</sup> All estimates are performed with Stata 13. The discussion is primarily restricted to the main variables of interest. A reduced baseline model of each sample shows on the one hand that our controls matter and on the other hand, that statistical significance crucially depends on the standard errors under consideration (Tables A.6 and A.7).

hypothesis (1), neither the debt brake dummy nor the fiscal rule index is statistically significant in any equation (which is confirmed by the bootstrapped p-values, Table 3 in square brackets). As for controls, population, unemployment and language differences turn out to be significant in at least some equations.

Table 3 Baseline regression of sample 1: Local finances aggregated at the cantonal level, 1980-2011

	Expenditure		Revenue		Debt		Deficit	
Debt brake	-0.029 (-1.008) [0.358]		-0.022 (-0.852) [0.410]		-0.008 (-0.202) [0.843]		-48.654 (-0.990) [0.346]	
Fiscal rule index		-0.013 (-0.725) [0.555]		-0.007 (-0.433) [0.687]		-0.008 (-0.347) [0.747]		-34.561 (-1.297) [0.254]
Unemployment	-0.373 (-0.346)	-0.378 (-0.351)	-0.438 (-0.430)	-0.428 (-0.420)	-3.216* (-1.817)	-3.215* (-1.826)	-375.481 (-0.209)	-464.573 (-0.255)
Relative income	-9.266 (-0.631)	-9.046 (-0.602)	-19.596 (-1.364)	-18.939 (-1.314)	-18.111 (-0.523)	-19.434 (-0.579)	2,3587.218 (0.925)	2,1284.505 (0.805)
Income	0.427 (0.611)	0.422 (0.593)	0.961 (1.320)	0.936 (1.287)	0.375 (0.218)	0.433 (0.258)	-1,335.729 (-1.155)	-1,229.828 (-1.026)
Population	0.403** (2.511)	0.403** (2.463)	0.311* (1.806)	0.310* (1.775)	-1.728*** (-3.497)	-1.725*** (-3.500)	296.935 (0.770)	303.010 (0.785)
Share old	1.104 (1.502)	1.211 (1.528)	0.680 (0.809)	0.746 (0.828)	-1.815 (-0.641)	-1.721 (-0.609)	1,429.810 (0.893)	1,703.651 (1.097)
Share young	0.529 (0.734)	0.516 (0.714)	0.463 (0.616)	0.446 (0.591)	2.692 (1.604)	2.711 (1.618)	527.184 (0.377)	538.765 (0.384)
Share German	-0.058 (-0.111)	-0.089 (-0.177)	-0.437 (-0.880)	-0.462 (-0.970)	0.761 (0.596)	0.746 (0.586)	1,435.144** (2.354)	1,380.639** (2.221)
Adj. R2	0.58	0.58	0.60	0.60	0.44	0.44	0.23	0.23
N	800	800	800	800	550	550	800	800
Cluster	25	25	25	25	25	25	25	25
Wald test: FE	897.21***	597.98***	639.96***	588.47***	2,289.92***	7,374.84***	69.80***	135.58***
Chow test	4.82***		1.99*		3.40***		6.48***	

Note: Due to data limitations, public debt is analyzed during the years 1990-2009. Canton and year fixed effects included. Constant not shown. The numbers in parentheses indicate the estimated t-statistics for standard errors adjusted for clustering at the cantonal level and corrected for heteroscedasticity. These values are used to determine statistical significance: \*p<0.1 (significance at the 10% level), \*\*p<0.05 (significance at the 5% level), and \*\*\*p<0.01 (significance at the 1% level). The numbers in square brackets indicate the estimated p-values for the fiscal rule variables using the wild-cluster bootstrap-t procedure. The Wald test has the null hypothesis that the fixed effects are jointly equal to zero. The Chow test has the null hypothesis that the parameters of municipalities located in cantons with a debt brake are equal to those of the other group. For Wald and Chow tests, we report test statistics based on regressions with cluster-robust standard errors.

The **second sample**, that covers large cities only and includes additional controls, largely confirms the previous sample's findings (Table 4). Like in sample one, we follow Wald test results and apply cantonal and year fixed effects. As suggested by the first sample, cantonal debt brakes reduce local spending, revenue and debt. Conversely, they have a positive impact on local deficits. The statistical significance of the estimated effects depends on the standard errors under consideration. The default standard errors indicate a statistically significant impact of debt brakes in all but the debt equation (Table 4 in braces). As discussed in Section 4, the default standard errors may, however, be erroneous. Thus, we base statistical inference in the second sample on p-values computed by the wild-cluster bootstrap-t procedure (Table 4 in square brackets). In compliance with the first sample's findings, the bootstrapped p-values

reveal that neither the debt brake nor the fiscal rule index reaches statistical significance in any equation.

Table 4 Baseline regression of sample 2: Local finances of 139 large municipalities, 1982-2007

	Expenditure		Revenue		Debt		Deficit	
Debt brake	-0.030		-0.049		-0.008		82.010	
	{-2.090}		{-3.516}		{-0.222}		{1.991}	
	[0.713]		[0.442]		[0.959]		[0.386]	
Fiscal rule index	-0.033		-0.040		-0.002		24.710	
	{-3.694}		{-4.686}		{-0.089}		{0.978}	
	[0.677]		[0.536]		[0.983]		[0.466]	
Unemployment	-0.060	-0.135	-0.315	-0.390	1.884	1.887	1'036.639	1'012.187
	[0.957]	[0.871]	[0.719]	[0.565]	[0.438]	[0.422]	[0.565]	[0.597]
Relative income	0.150	0.146	0.122	0.119	0.432	0.433	86.082	79.753
	[0.655]	[0.645]	[0.705]	[0.665]	[0.308]	[0.290]	[0.609]	[0.665]
Income	0.161	0.166	0.224	0.227	-0.544	-0.545	-257.972	-249.239
	[0.639]	[0.579]	[0.514]	[0.496]	[0.216]	[0.240]	[0.394]	[0.398]
Population	0.087*	0.086*	0.079	0.078	0.214*	0.214*	53.535**	53.167**
	[0.074]	[0.054]	[0.114]	[0.128]	[0.078]	[0.078]	[0.014]	[0.012]
Share own revenue	-1.126***	-1.139***	-1.061***	-1.070**	-1.404***	-1.401***	-464.224*	-486.143
	[0.006]	[0.004]	[0.002]	[0.010]	[0.004]	[0.004]	[0.084]	[0.102]
Share young	-2.882**	-2.897**	-2.814**	-2.827**	-2.644	-2.642	-65.346	-81.601
	[0.036]	[0.026]	[0.036]	[0.016]	[0.310]	[0.356]	[0.817]	[0.821]
Share old	1.138*	1.134	1.000	0.995	0.943	0.942	712.226	718.954
	[0.094]	[0.116]	[0.128]	[0.130]	[0.587]	[0.595]	[0.240]	[0.218]
Share German	-0.391	-0.407	-0.600	-0.599	0.849	0.859	188.125	94.058
	[0.528]	[0.520]	[0.132]	[0.110]	[0.484]	[0.458]	[0.919]	[0.978]
Ideology gov't	-0.104	-0.103	-0.055	-0.054	0.091	0.091	-284.768***	-283.189***
	[0.290]	[0.314]	[0.635]	[0.615]	[0.691]	[0.663]	[0.002]	[0.006]
Coalition gov't	-0.020	-0.020	-0.022	-0.021	-0.035	-0.035	7.255	6.010
	[0.274]	[0.342]	[0.282]	[0.316]	[0.382]	[0.420]	[0.617]	[0.660]
Adj. R2	0.45	0.445	0.46	0.46	0.17	0.17	0.07	0.07
N	3,329	3,329	3,329	3,329	3,329	3,329	3,329	3,329
Wald test: FE	62.65***	64.44***	67.35***	69.60***	22.54***	22.61***	6.35***	6.34***
Chow test	15.53***		18.08***		12.50***		3.80***	

Note: Canton and year fixed effects are included. Constant not shown. The numbers in square brackets indicate the estimated p-values using the wild-cluster bootstrap-t procedure. These values are used to determine statistical significance: \*p<0.1 (significance at the 10% level), \*\*p<0.05 (significance at the 5% level), and \*\*\*p<0.01 (significance at the 1% level). Number in braces indicate estimated t-statistics for default standard errors. The Wald test has the null hypothesis that the fixed effects are jointly equal to zero. The Chow test has the null hypothesis that the parameters of municipalities located in cantons with a debt brake are equal to those of the other group. For Wald and Chow tests, we report test statistics based on regressions default standard errors.

Once we base statistical inference on the bootstrapped p-values only few controls are statistically significant. Like in the first sample, statistical significance obtains with respect to socio-demographic variables. In addition, own revenues are (highly) significant in almost all equations. As commonly assumed, a larger share of own local revenues on total local revenues reduces municipal spending, revenue, deficits and debt. Political controls provide some noteworthy insights: While previous evidence suggests that expenditure and debt increase if more parties are involved in the executive (e.g., Feld et al., 2010; Volkerink and de Haan, 2001), our coalition variable indicates an opposite effect – though p-values are far from indicating significance. The ideology variable that measures the share of left-wing parties in the municipal executive is significant at the 1% level in the deficit equation but does not have the expected sign.

## 5.2. VERTICAL EFFECTS ON DIFFERENT SPENDING CATEGORIES

Based on recent evidence that budget rules have a stronger impact on state finances when the underlying budget variable is defined more narrowly (e.g. Burret and Feld, 2015; Hou and Smith, 2010; Mahdavi and Westerlund, 2011), we investigate whether the effect of fiscal rules is more pronounced if more narrowly defined expenditure categories are considered instead of total spending. Municipalities might react to a debt brake induced spending change in a certain area by reallocating their spending among different categories, leaving total spending largely unaffected. To investigate this possibility we examine nine spending categories during the years 1990-2011 (sample 1) and 1982-2007 (sample 2).

Similar to the previous estimates, the results suggest that budget rules reduce local spending at least in some categories (Table A.3 and A.4). However, in most spending areas the estimated signs of the debt brake coefficients contradict each other in the two datasets. This might be due to deviations in the definition of each spending area between the two samples. A similar effect in both samples obtains with respect to spending on education (negative) and other areas (positive). While the coefficients are not statistically significant in almost all cases, transportation spending is significantly depressed by both debt brake variables in the first sample. In the second sample, a significant negative effect obtains once we base our statistical inference on default standard errors (Table A.4 in braces) rather than the more reliable bootstrapped p-values (in square brackets). In sum, we find little evidence that cantonal debt brakes influence local expenditure neither in its entirety nor in specific spending areas. If anything, evidence suggests that local expenditure would decrease.

## 5.3. VERTICAL EFFECTS ON FISCAL DECENTRALIZATION

While related studies reveal that cantonal debt brakes support fiscal discipline at the cantonal level (e.g., Burret and Feld, 2015; Feld and Kirchgässner, 2008, 2001a; Krogstrup and Wälti, 2008; Luechinger and Schaltegger, 2013; Schaltegger, 2002), our baseline findings provide little evidence that cantonal debt brakes affect average finances at the local level. This raises the related question of whether cantonal budget rules affect fiscal decentralization. Given the large autonomy of Swiss municipalities on the expenditure and revenue sides, we employ two measures of fiscal decentralization to investigate this question: spending (revenue)

decentralization is measured by the ratio of local spending (revenue) in a canton to local and cantonal spending (revenue) in that canton, i.e.  $(\text{Local} / [\text{Cantonal} + \text{Local}])$ .<sup>16</sup>

Table 5 presents similar findings for both decentralization indicators. In line with our hypothesis (2), the results suggest that cantonal debt brakes reduce fiscal decentralization, i.e., lead to a higher level of cantonal as compared to local spending or revenue. While the fiscal rule index turns out to be insignificant, the debt brake dummy reaches statistical significance at the 10% level in the expenditure decentralization equation (1) and at the 5% level if revenue decentralization (3) is considered. The more conservative p-values based on the wild-cluster bootstrap-t procedure confirm statistical significance, though only at the 10% level (Table 5 in square brackets). Thus, we find little evidence for an impact of cantonal debt brakes on fiscal decentralization. If anything, our results suggest that debt brakes are associated with fiscal centralization.

Table 5 Baseline regression of sample 1: Effect of fiscal rules on decentralization, 1980-2011

	Spending decentralization		Revenue decentralization	
	(1)	(2)	(3)	(4)
Debt brake	-0.017* (-1.909) [0.088]		-0.021** (-2.275) [0.064]	
Fiscal rule index		-0.009 (-1.623) [0.154]		-0.009 (-1.543) [0.178]
Unemployment	-0.052 (-0.122)	-0.060 (-0.142)	0.148 (0.331)	0.145 (0.319)
Relative income	-0.452** (-2.121)	-0.454** (-2.064)	-13.886** (-2.437)	-13.713** (-2.307)
Income	0.452* (1.865)	0.457* (1.824)	0.579** (2.243)	0.575** (2.136)
Population	0.148 (1.615)	0.148 (1.619)	0.136 (1.440)	0.136 (1.439)
Share old	0.288 (0.989)	0.359 (1.158)	0.342 (1.105)	0.420 (1.216)
Share young	0.052 (0.194)	0.046 (0.172)	-0.080 (-0.297)	-0.091 (-0.331)
Share German	0.130 (1.097)	0.111 (0.877)	0.072 (0.595)	0.049 (0.369)
Adj. R2	0.58	0.58	0.57	0.56
N	800	800	800	800
Cluster	25	25	25	25
Wald test: FE	1,203.30***	689.54***	82.71***	224.89***
Chow test	3.10**		1.81	

Note: Canton and year fixed effects are included. Constant not shown. The numbers in parentheses indicate the estimated t-statistics for standard errors that are adjusted for clustering at the cantonal level and corrected for heteroscedasticity. These values are used to determine statistical significance: \*p<0.1 (significance at the 10% level), \*\*p<0.05 (significance at the 5% level), and \*\*\*p<0.01 (significance at the 1% level). The numbers in square brackets indicate the estimated p-values for the fiscal rule variables using the wild-cluster bootstrap-t procedure. The Wald test has the null hypothesis that the fixed effects are jointly equal to zero. The Chow test has the null hypothesis that the parameters of municipalities located in cantons with a debt brake are equal to those of the other group. For Wald and Chow tests, we report test statistics based on regressions with cluster-robust standard errors.

<sup>16</sup> As the second dataset covers only few (in some cases only one) municipalities from each canton we lack an adequate decentralization measure and, thus, analyze the effect of fiscal rules on decentralization only with respect to the first sample that covers all municipalities within a canton.

This matches the above findings, i.e., the numerator – be it local expenditures or local revenues – is at least not increased by cantonal fiscal rules, and it matches recent evidence that cantonal expenditures and revenues, i.e., part of the denominator, are increased by cantonal fiscal rules (Burret and Feld, 2015).

## **6. ROBUSTNESS CHECKS**

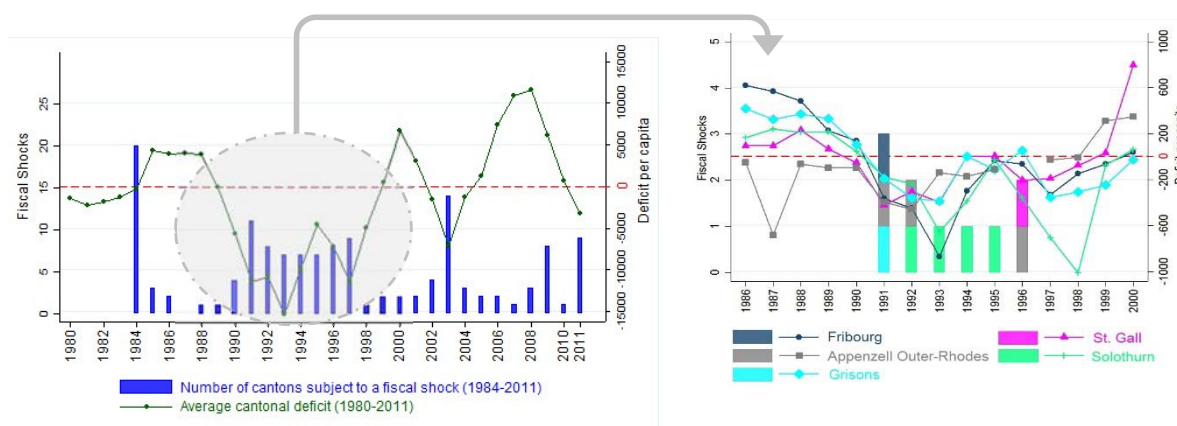
Given our baseline “non-findings” it seems crucial to carefully check whether vertical effects fail to be revealed due to methodological issues or whether they do not actually exist. With that goal in mind, we apply a wide range of robustness tests. In the interest of clarity, we only report results regarding the first sample and the debt brake dummy and base our statistical inference exclusively on the more conservative wild-cluster bootstrapped p-values. However, similar findings obtain if we consider the second sample, the fiscal rule index and cluster-robust standard errors (results upon request).

### **Part I: Vertical effects in times of fiscal shocks**

So far, the baseline findings provide little evidence of an average treatment effect, possibly because cantons regularly generate surpluses (Figure 4, left panel) with the result that budget rules only play a minor role. Thus, vertical effects of debt brakes might be more pronounced during times of fiscal stress, i.e., when shocks occur. To investigate this possibility, we follow two distinct approaches:

*First*, we restrict our analysis to the fiscally notable period between 1990 and 1998 (Figure 4). As this sub-period is characterized by economic turmoil and unbalanced cantonal budgets, vertical effects of cantonal debt brakes seem particularly likely. However, the validity of this intuitive approach remains fairly limited as we analyze quite a short period with only five fiscally constrained cantons (AR, FR, GR, SG, SO). Moreover, deficits are only a crude indicator of fiscal shocks as those are not necessarily unexpected and can be intentional.

Figure 4 Cantonal fiscal shocks (bars, left axis) and budget deficits (lines, right axis)



Note: In 1996 Appenzell Outer-Rhodes generated a large surplus by selling its Cantonal Bank to UBS. For scaling reasons, this surplus is not shown on the right hand graph.

A *second* approach mitigates these shortcomings by constructing a distinct measure for fiscal shocks and taking a much longer period into account (1984-2011). To be precise, we draw on, e.g., Lundberg (1997), Poterba (1994), Poterba and Rueben (2001, 1999), Rattsø (2004) and Rattsø and Tovmo (2002) and define a fiscal shock in canton  $c$  in year  $t$  as:

$$\text{Fiscal Shock}_{ct} = \left[ \left( \text{Expenditure}_{ct}^{\text{actual}} - \text{Expenditure}_{ct}^{\text{forecast}} \right) - \left( \text{Revenue}_{ct}^{\text{actual}} - \text{Revenue}_{ct}^{\text{forecast}} \right) \right] / \text{Population}_{ct}.$$

A positive fiscal shock indicates an unfavorable deficit shock, i.e., an unexpected shortfall of current revenue or an unexpected increase in current spending. Conversely, a fiscal shock takes a negative value in case of an unexpected favorable surplus shock.<sup>17</sup> Since cantons frequently generate surpluses, it is not surprising that the mean of fiscal shocks is -0.158 (corresponding to a surplus shock) and that 558 surplus shocks and only 242 deficit shocks have been recorded. Similar to cantonal budget deficits, deficit shocks cluster particularly during the 1990s (Figure 4, left). However, during the period 1990-1998, the five fiscally constrained cantons experienced only ten deficit shocks taken all together and each Fribourg, Grisons and St. Gall faced only one (Figure 4, right). This creates further doubt about the robustness of the above analysis of the period 1990-1998. In the second approach we interact the fiscal rule dummy with the newly constructed indicator for fiscal shocks, trying to clarify how changes in local finances and decentralization after a cantonal fiscal shock differ depending on the presence of a cantonal debt brake.

<sup>17</sup> Note that a canton might face a shock although its books are balanced. This could be the case if a canton expects a budget surplus (deficit) and faces a deficit (surplus) shock. Furthermore, the definition of a fiscal shock implicitly assumes that the fiscal year's budget forecasts are not (strategically) biased. We are grateful to Luechinger and Schaltegger (2013) for providing us with data on expected and actual current income and expenses for the years 1984-1998. From 1999 onwards, the data source is the Conference of Cantonal Ministers of Finance.



Table 6 briefly summarizes the findings. Both approaches broadly confirm our baseline estimates. Interestingly, debt brakes significantly improve municipal finances and reduce decentralization once we restrict the analysis to the sub-period 1990-1998 (Table 6, upper panel). While these findings have to be taken with a great deal of caution, the second approach reveals somewhat similar effects as it suggests that debt brakes mitigate shock-related local deficits (Table 6, lower panel). Regarding dependent variables other than budget deficits, neither the interaction term nor its constitutive terms are statistically significant.<sup>18</sup> We thus conclude that even in times of fiscal stress at the cantonal level, debt brakes do not lead to a burdening of local finances. If anything, the results indicate that debt brakes are associated with improved average municipal finances.

Table 6 Robustness tests part I: Vertical effects in times of fiscal shocks

<i>Sample 1</i>			Expenditure	Revenue	Debt	Deficit	Expend. Decentr.	Revenue Decentr.
<b>a)</b>	Sub-period, 1990-1998	Debt brake	-0.122*** [0.002]	-0.067*** [0.004]	0.076 [0.142]	-292.824*** [0.002]	-0.058*** [0.002]	-0.076*** [0.002]
		Controls	Yes	Yes	Yes	Yes	Yes	Yes
		Two-way fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
		Adj. R2	0.13	0.22	0.20	0.22	0.42	0.40
		N	225	225	225	225	225	225
		Cluster	25	25	25	25	25	25
<b>b)</b>	Fiscal shock, 1984-2011	Debt brake	-0.023 [0.462]	-0.014 [0.663]	-0.009 [0.823]	-65.447 [0.272]	-0.017 [0.130]	-0.020 [0.130]
		Deficit shock	-0.000 [0.987]	-0.013 [0.416]	0.003 [0.735]	73.363** [0.018]	-0.002 [0.336]	-0.000 [0.999]
		Debt brake* Fiscal shock	-0.002 [0.905]	-0.002 [0.859]	-0.002 [0.999]	-16.920 [0.787]	-0.000 [0.917]	0.001 [0.807]
		Controls	Yes	Yes	Yes	Yes	Yes	Yes
		Two-way fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
		Adj. R2	0.47	0.48	0.44	0.25	0.62	0.59
		N	700	700	700	700	700	700
		Cluster	25	25	25	25	25	25

Note: Besides the variables shown, we employ all controls as in the corresponding baseline regression of sample 1 (Table 3 and 5). Due to data limitations, public debt is only analyzed during the years 1990-2009. The numbers in square brackets indicate the estimated p-values using the wild-cluster bootstrap-t procedure. These values are used to determine statistical significance: \*p<0.1 (significance at the 10% level), \*\*p<0.05 (significance at the 5% level), and \*\*\*p<0.01 (significance at the 1% level). Full regression bodies available upon request.

## Part II: General modifications

In the second part, we investigate the robustness of the baseline results by taking into account breaks in the time series, additional institutional controls, voter preferences, statistical outliers and the role of fixed effects.

*First*, we examine several **sub-periods** in order to address structural breaks in the time series due to revised reporting standards, and to check whether the effect of cantonal debt brakes varies between “early” and “late” adopters. However, fixed effects and a low

<sup>18</sup> The interpretation of the interaction terms is based on marginal effects as suggested by Brambor et al. (2006).

institutional variation limits the validity of the results for the sub-period 1980-1989, respectively.

*Second*, **additional institutional controls** are employed. To capture the extent of direct democracy, three indicators are used: a dummy which equals one if cantonal spending projects require an approval by a majority of voters, a measure of the spending thresholds (in millions of Swiss Francs) that trigger cantonal mandatory referenda and a variable indicating the number of signatures per capita required to launch a cantonal initiative. In a next step, we employ the share of own local revenues from total local revenues to control for the influence of grants. Due to data restrictions, the two modifications apply to the sub-periods 1980-2005 and 1990-2011.

*Third*, to address the influence of **voter preferences** we exploit the information on voters' fiscal preferences revealed through the nationwide referendum on the federal debt brake in 2001 to divide the sample into two sub-panels: one panel with fiscally conservative voters (municipalities with approval rates above average) and one panel with voters that revealed low preferences for fiscal consolidation (municipalities with approval rates below average). If the estimated vertical effect in the two sub-panels is similar, the impact seems independent from voters' preferences.

*Fourth*, the influence of **outliers** is examined. The presence of outliers might lead to erroneous estimates of the standard errors as OLS weighs larger residuals more heavily and assumes normally distributed error terms. Due to federal asymmetries and inter-jurisdictional differences in areas such as geography, urbanization, industrialization and population size, the issue of outliers is likely to be of relevance in our case. While this problem is already mitigated by our large sample and the log transformation, we further address the issue by estimating a median regression and by discarding observations with a large Cook's distance.<sup>19</sup>

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<sup>19</sup> The median regression estimates the 0.5 quantile of the dependent variable rather than the mean in ordinary regression. This minimizes the sum of the absolute residuals. Median regression is, thus, more robust to outliers in observations and the results are valid even if the errors are not independent and identically distributed. The Cook's distance is the accumulated change in the estimated coefficients if a particular observation is excluded. We follow a rule of thumb and discard observations with a Cook's distance greater than  $4/N$  (where  $N$  is the number of observations in the sample).

*Fifth*, we test the robustness of baseline estimates to the **exclusion of fixed effects** as these might hide the impact of an institutional variable and render it insignificant. However, the debt brake dummy varies widely across time and cantons (Figure 1), making the exclusion of fixed effects problematic. On the one hand, the issue of omitted variables arises; on the other hand, cantonal asymmetries are not adequately taken into account. Moreover, fixed effects seem necessary to mitigate the impact of block concentrated outliers and to control for the voters' preferences. This is supported by the Wald tests as they suggest including two-way fixed effects (see baseline regressions).

The results are briefly summarized in Table A.8. In short, the robustness tests produce some variation in the results, while the baseline findings still seem robust to most specifications. In fact, almost all robustness checks confirm a negative though statistically insignificant impact of cantonal fiscal rules on local spending, revenue, deficit and fiscal decentralization. The sign of the debt brake dummy changes frequently in the debt equations but statistical significance hardly obtains. Interestingly enough, the effect of cantonal budget rules on spending and revenue decentralization becomes (highly) significant once outliers weigh less heavily through the application of median regressions or if we additionally control for the extent of cantonal direct democracy. Since direct democratic institutions are key elements of the fiscal framework in Switzerland, this is a highly important piece of evidence.

The results for jurisdictions with fiscally conservative and non-conservative voters are close to the baseline results. Yet, in fiscally non-conservative jurisdictions, cantonal budget rules have a significantly negative effect on local deficits (5% level) and on debt (10% level). This suggests that debt brakes spill over from the cantonal to the municipal level if local voters exercise little consolidation pressure. It should, however, be noted that the validity of this specification is restricted as the regression covers only eight fiscally non-conservative cantons, none of which had a debt brake in place before 2005. Finally, the exclusion of fixed effects – that should be taken with a great deal of caution – hardly reveals any further insights as debt brake coefficient remains insignificant in almost all equations.

## 7. DISCUSSION AND CONCLUSION

Most empirical studies on fiscal rules focus on effects within the constrained entity such as the rules' impact on intra-jurisdictional finances, fiscal sustainability, macroeconomic stabilization and creative accounting. However, there are good reasons to assume that fiscal rules also have

vertical effects on the finances of other levels of government even though those other levels are actually not covered by the rule. While municipal governments have contended that fiscal rules at a higher level of government were burdening their finances, opposite effects could also be justified. Thus, whether and how fiscal rules constraining fiscal deficits at an upper level of government affect finances at a lower level of governments is a question to be answered empirically.

In order to do so, this paper investigates the vertical effects of Swiss cantonal debt brakes on municipal expenditure, revenue, debt, deficits and fiscal decentralization. As the cantonal fiscal rules vary substantially across time and cantons, a Difference-in-Differences design can be used. We exploit a sample of cantonal aggregated data of all municipalities (1980-2011) and a sample of disaggregated data of the 139 largest cities (1982-2007). This paper adds to both the existing literature on fiscal rules and to research on vertical effects of institutions. While previous studies suggest that debt brakes are strong enough to trigger fiscal consequences at the cantonal level (e.g., Burret and Feld, 2015; Feld and Kirchgässner, 2008; Krogstrup and Wälti, 2008), we find little evidence that Swiss cantonal debt brakes affect average local finances or decentralization. This holds even in situations that make vertical effects most likely, i.e., in times of fiscal shocks at the cantonal level. If anything, evidence suggests that cantonal debt brakes cushion the impact of fiscal shocks on local finances.

Our “non-findings” raise the question whether vertical effects are masked by methodological issues or do simply not exist. Therefore it is important to emphasize that we apply a wide range of robustness checks that largely confirm our “non-findings”. However, as we estimate the average treatment effect, we cannot dismiss the possibility that municipalities respond in heterogeneous ways that compensate each other. Nevertheless, we suppose that municipalities react in similar ways since we do not find considerably different results if we take only a selection of municipalities into account. In fact, the findings are similar for both datasets and for the sub-samples of fiscally conservative and non-conservative municipalities. Still, vertical effects might be masked if municipalities respond in a way that compensates the initial effect. For instance, municipalities might react to a debt brake-induced spending change in a certain area by reallocating their spending among different categories with the result that total spending is largely unaffected. However, we dismiss this possibility as we do not find vertical effects of cantonal debt brakes in any spending category. Finally, vertical effects could be offset

by transmission channels that work in opposite directions. Again, we cast doubt on this possibility as the domination of transmission channels that improve local finances seems most likely given the Swiss institutional framework (see Section 3). In line with this, our findings suggest that cantonal debt brakes are, if anything, associated with improved local finances.

While our findings are broadly in line with cursory evidence for Switzerland provided by Burret and Feld (2015) and Feld and Kirchgässner (2008), earlier studies for the US suggest that fiscal rules in US states burden local finances (e.g., Kiewiet and Szakaly, 1996; von Hagen 1992). We take this as a sign that our results are, at least partly, due to a well-designed and incentive-based fiscal framework in Switzerland. Yet, much research remains to be done on the exact vertical transmission mechanisms in different institutional settings.

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## APPENDIX

Table A.1 Descriptive statistics in total and by institutional regime, whole periods

	Total					No debt brake <sup>1)</sup>			Debt brake <sup>1)</sup>			
	Obs	Mean	SD	Min	Max	Obs	Mean	SD	Obs	Mean	SD	p-value <sup>2)</sup>
<b>Dependent variables SAMPLE 1</b>												
Expenditure per capita (log)	800	8.469	0.253	7.782	9.106	588	8.442	0.258	212	8.546	0.221	0.000
Revenue per capita (log)	800	8.466	0.250	7.430	9.057	588	8.436	0.254	212	8.550	0.217	0.000
Debt per capita (log)	550	8.522	0.493	6.439	9.350	364	8.482	0.558	186	8.601	0.316	0.007
Deficit per capita	800	18.106	261.200	-924.965	2,536.192	588	32.321	264.464	212	-21.321	248.295	0.010
Expenditure decentralization	800	0.401	0.085	0.169	0.541	588	0.399	0.091	212	0.406	0.066	0.309
Revenue decentralization	800	0.400	0.084	0.184	0.543	588	0.399	0.089	212	0.403	0.065	0.641
Spending categories per capita (log)	----- For reasons of clarity statistics for the nine spending categories are not shown. -----											
<b>Explanatory variables SAMPLE 1</b>												
Debt brake dummy <sup>#</sup>	800	0.265	0.442	0.000	1.000							
Debt brake index <sup>#</sup>	800	0.541	0.985	0.000	3.000							
Relative income	800	1.000	0.168	0.766	1.685	588	1.014	0.182	212	0.961	0.113	0.000
Income per capita (log)	800	3.340	0.189	2.871	3.984	588	3.331	0.202	212	3.365	0.146	0.026
Unemployment rate	800	0.021	0.017	0.000	0.078	588	0.020	0.019	212	0.023	0.014	0.010
Population (log)	800	11.965	1.142	9.470	14.140	588	11.805	1.171	212	12.409	0.923	0.000
Share own revenue	550	0.841	0.079	0.540	0.987	364	0.834	0.088	186	0.853	0.582	0.008
Share old	800	0.148	0.018	0.103	0.198	588	0.147	0.019	212	0.151	0.014	0.005
Share young	800	0.247	0.032	0.184	0.341	588	0.252	0.033	212	0.235	0.027	0.000
Share German speaking <sup>#</sup>	800	0.690	0.354	0.039	0.980	588	0.685	0.374	212	0.706	0.292	0.460
Signature requirement for initiatives	650	0.014	0.010	0.000	0.039	528	0.014	0.105	122	0.015	0.001	0.302
Spending threshold for referenda	650	3.231	6.106	0.000	25.000	528	2.808	6.077	122	5.062	5.916	0.000
Mandatory referendum dummy	650	0.677	0.468	0.000	1.000	528	0.625	0.485	122	0.902	0.299	0.000
Cantonal fiscal shock per capita	700	-0.158	0.484	-4.323	5.900	496	-0.141	0.502	204	-0.199	0.436	0.155
<b>Dependent variables SAMPLE 2</b>												
Expenditure per capita (log)	3,329	8.472	0.351	6.955	9.562	2,500	8.414	0.356	829	8.648	0.264	0.000
Revenue per capita (log)	3,329	8.472	0.345	7.035	9.781	2,500	8.412	0.347	829	8.653	0.266	0.000
Debt per capita (log)	3,329	8.538	0.614	5.299	10.148	2,500	8.496	0.634	829	8.665	0.529	0.000
Deficit per capita	3,329	7.975	578.274	-6,990.880	3,465.496	2,500	21.492	551.374	829	-32.791	651.384	0.019
Spending categories per capita (log)	----- For reasons of clarity statistics for the nine spending categories are not shown. -----											
<b>Explanatory variables SAMPLE 2</b>												
Relative income	3,329	1.000	0.282	0.231	4.629	2,500	1.007	0.290	829	0.982	0.256	0.028
Income per capita (log)	3,329	10.294	0.227	8.877	11.92	2,500	10.290	0.230	829	10.307	0.218	0.065
Unemployment rate	3,329	0.028	0.023	0.000	0.123	2,500	0.028	0.024	829	0.029	0.018	0.068
Population (log)	3,329	9.646	0.691	7.728	12.816	2,500	9.659	0.674	829	9.608	0.737	0.063
Share own revenue	3,329	0.839	0.089	0.273	0.995	2,500	0.846	0.091	829	0.821	0.078	0.000
Share old	3,329	0.147	0.037	0.038	0.243	2,500	0.144	0.037	829	0.155	0.034	0.000
Share young	3,329	0.231	0.039	0.146	0.362	2,500	0.235	0.038	829	0.221	0.037	0.000
Ideology government	3,329	0.262	0.163	0.000	1.000	2,500	0.268	0.161	829	0.245	0.167	0.001
Coalition government	3,329	3.780	0.968	1.000	7.000	2,500	3.800	0.996	829	3.721	0.881	0.044

Note: <sup>#</sup> indicates that the variable is employed in both samples. The corresponding descriptive statistics for sample 2 might differ due to deviating time period. 1) Municipalities are recorded in the debt brake group only from the moment on in which the corresponding canton implements a debt brake. 2) Two-tailed p-value for the difference in means between the two groups of municipalities. The null hypothesis being that the difference between the means is zero.

Table A.2 Definition and source of variables\*

	Level	Cantons	Period	Source	Description
<b>Dependent variables SAMPLE 1</b>					
Expenditure per capita (log)	Cantonal aggregated local data	25	1980-2011	Swiss Federal Finance Administration	Total expenditure per capita, adjusted for double counting.
Revenue per capita (log)	Cantonal aggregated local data	25	1980-2011	Swiss Federal Finance Administration	Total revenue per capita, adjusted for double counting.
Debt per capita (log)	Cantonal aggregated local data	25	1990-2011	Swiss Federal Finance Administration	Total debt per capita, adjusted for double counting.
Deficit per capita	Cantonal aggregated local data	25	1980-2011	Swiss Federal Finance Administration	Total revenues less total expenditures. A fiscal deficit is, thus, indicated by a positive and a surplus by a negative sign.
Expenditure decentralization	n/a	25	1980-2011	Own calculation	Total expenditure of all municipalities in a canton divided by the sum of local and cantonal spending in that canton.
Revenue decentralization	n/a	25	1980-2011	Own calculation	Total revenue of all municipalities in a canton divided by the sum of local and cantonal revenues in that canton.
Spending categories per capita (log)	Cantonal aggregated local data	25	1990-2011	Swiss Federal Finance Administration	Nine spending categories: Administration, Security, Education, Culture and recreation, Health care, Environment, Social welfare, Transportation and Others. Others includes expenditures on the economy and finances and taxes and Transportation includes communication.
<b>Explanatory variables SAMPLE 1</b>					
Debt brake dummy <sup>#</sup>	Cantonal	25	1980-2011	Feld et al. (2013) and own research	Dummy which equals one if a canton is legally constrained by a debt brake and zero otherwise.
Debt brake index <sup>#</sup>	Cantonal	25	1980-2011	Feld et al. (2013) and own research	Measures the stringency of cantonal debt brakes on a scale between zero (none) and three (strong).
Relative income	Cantonal	25	1980-2011	Own calculation	Cantonal taxable income per capita as share of average cantonal taxable income per capita of all municipalities in the sample.
Income per capita (log)	Cantonal	25	1980-2011	Swiss Federal Finance Administration	See note (1).
Unemployment rate	Cantonal	25	1980-2011	Swiss Federal Statistical Office	
Population (log)	Cantonal	25	1980-2011	Swiss Federal Statistical Office	Mean residential population.
Share own revenue	Cantonal aggregated data	25	1990-2011	Swiss Federal Finance Administration	Share of own revenues of all municipalities within a canton on total revenues in the same municipalities. Own revenues are calculated by subtracting total transfer receipts from total revenues.
Share old	Cantonal	25	1980-2011	Own calculation	Share of population aged 65 and above according to the midyear (2011 end of the year) permanent residential population.
Share young	Cantonal	25	1980-2011	Own calculation	Share of population aged 20 and below according to the midyear (2011 end of the year) permanent residential population.
Share German speaking <sup>#</sup>	Cantonal	25	1980-2011	Swiss Federal Statistical Office	Share of population speaking German (data changes only once in ten years).
Signature requirement for initiatives	Cantonal	25	1980-2005	Feld et al. (2013)	Number of signatures per capita required launching a statutory initiative process.
Spending threshold for referenda	Cantonal	25	1980-2005	Feld et al. (2013)	Expenditure threshold (in million Swiss Francs) that triggers mandatory referenda if exceeded. It equals zero if no mandatory referenda is in place.
Mandatory referendum dummy	Cantonal	25	1980-2005	Feld et al. (2013)	Dummy that equals one if spending projects require an approval by the majority of voters and zero otherwise.
Cantonal fiscal shock per capita	Cantonal	25	1984-2011	Own calculation	Measured in million Swiss Francs. Calculation based on realized and forecasted current cantonal income and expenses as provided by Luechinger and Schaltegger (2013) and Conference of Cantonal Finance Ministers.
<b>Dependent variables SAMPLE 2</b>					
Expenditure per capita (log)	Local	24	1982-2007	Yearbooks Swiss Association of Cities	Total expenditure per capita.
Revenue per capita (log)	Local	24	1982-2007	Yearbooks Swiss Association of Cities	Total revenue per capita.
Debt per capita (log)	Local	24	1982-2007	Yearbooks Swiss Association of Cities	Sum of current liabilities, short-, medium- and long-term debts and liabilities for special accounts.
Deficit per capita	Local	24	1982-2007	Yearbooks Swiss Association of Cities	Total revenues less total expenditures. A fiscal deficit is, thus, indicated by a positive and a surplus by a negative sign.
Spending categories per capita (log)	Local	24	1982-2007	Yearbooks Swiss Association of Cities	The nine categories are: administration, Security, Education, Culture and recreation, Health care, Environment, Social welfare, Transportation and Others. Security indicates expenditures on judicature, police and fire department. Transportation includes transportation and energy until 1989 and only transportation thereafter. Definitions might be different from sample 1.
<b>Explanatory variables SAMPLE 2</b>					
Relative income	Local	24	1982-2007	Own calculation	Local taxable income per capita as share of average local taxable income per capita of all municipalities in the sample.
Income per capita (log)	Local	24	1982-2007	Swiss Federal Finance Administration	See note (1).
Unemployment rate	Local	24	1982-2007	Yearbooks Swiss Association of Cities	
Population (log)	Local	24	1982-2007	Yearbooks Swiss Association of Cities	
Share own revenue	Local	24	1982-2007	Yearbooks Swiss Association of Cities	Share of own municipal revenue on total revenue in that municipality. Own revenues are compiled of local taxes, regalia and concessions ("Regalien und Konzessionen"), asset earnings ("Vermögenserträge") and administrative assets and fees ("Verwaltungsvermögen und Entgelte").
Share old	Local	24	1982-2007	Yearbooks Swiss Association of Cities	Share of population aged 65 and above (data changes only once in ten years in most cases).
Share young	Local	24	1982-2007	Yearbooks Swiss Association of Cities	Share of population aged 25 and below (data changes only once in ten years in most cases).
Ideology government	Local	24	1982-2007	Yearbooks Swiss Association of Cities	Share of left wing parties in municipal executive. See also note (2).
Coalition government	Local	24	1982-2007	Yearbooks Swiss Association of Cities	Number of different political parties in municipal executive.

Note: \* All monetary variables are deflated to the year 2000, based on the Swiss consumer price index. <sup>#</sup> indicates that variable is employed in both samples. (1) Taxable income of natural persons, including special cases (e.g. in-between and temporary assessment, flat tax, residents with overseas income and foreigner with domestic income). Until 1998, cantonal taxes were collected on basis of the average income of the previous two years (praenumerando). Therefore, income data do not change for two consecutive years. In 1999 some, and two years later all, cantons moved towards a system of postnumerando taxation, i.e. tax collection according to same year's income, providing us with annual income data. Due to the transition from praenumerando to postnumerando taxation, income data is missing for two consecutive years in most cases. Missing income data was derived through interpolation. Data for 2011 was calculated by extrapolation. (2) The following Swiss parties are considered as left wing orientated: Green Party (GPS), Evangelical People's Party (EVP), Social Democratic Party (SPS), Swiss Party of Labour (PDA) and Progressive Organizations of Switzerland (POCH). See Swiss Federal Chancellery (2007, 2011).

Table A.3 Baseline regression of sample 1: Expenditure categories in all municipalities, 1990-2011

	Admin		Security		Education		Culture and recreation		Health care		Environment		Social Welfare		Transportation		Others	
Debt brake	0.032		0.013		-0.104		-0.014		0.243		0.062		0.032		-0.064*		0.044	
	(1.171)		(0.343)		(-1.176)		(-0.226)		(0.681)		(1.356)		(0.342)		(-1.963)		(0.563)	
	[0.271]		[0.727]		[0.304]		[0.837]		[0.595]		[0.236]		[0.825]		[0.074]		[0.615]	
Fiscal rule index	0.012		0.002		-0.049		-0.004		0.156		0.046*		0.024		-0.045**		0.007	
	(0.814)		(0.071)		(-1.071)		(-0.120)		(0.859)		(2.007)		(0.483)		(-2.718)		(0.169)	
	[0.448]		[0.965]		[0.456]		[0.929]		[0.428]		[0.084]		[0.623]		[0.020]		[0.871]	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Two-way FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R2	0.15	0.15	0.26	0.26	0.16	0.15	0.08	0.08	0.40	0.40	0.13	0.14	0.20	0.20	0.28	0.29	0.35	0.35
N	550	550	550	550	550	550	550	550	549	549	550	550	550	550	550	550	550	550
Cluster	25	25	25	25	25	25	25	25	25	25	25	25	25	25	25	25	25	25
Wald test: FE	197***	234***	69***	56***	37***	207***	535***	818***	2,220***	2,745***	2,280***	4,998***	41,500***	96,249***	4,189***	55***	124***	450***

Note: Same controls included as in the baseline regression of sample 1 (Table 3). The numbers in parentheses indicate the estimated t-statistics for standard errors that are adjusted for clustering at the cantonal level and corrected for heteroscedasticity. These values are used to determine statistical significance: \*p<0.1 (significance at the 10% level), \*\*p<0.05 (significance at the 5% level), and \*\*\*p<0.01 (significance at the 1% level). The numbers in square brackets indicate the estimated p-values using the wild-cluster bootstrap-t procedure. The Wald test has the null hypothesis that the fixed effects are jointly equal to zero. For the Wald tests, we report test statistics based on regressions with cluster-robust standard errors.

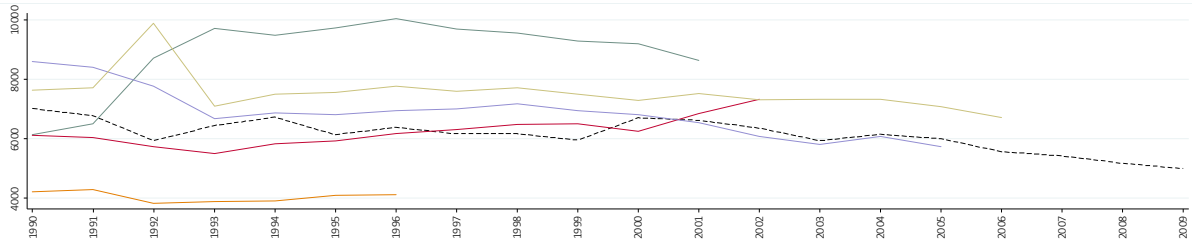
Table A.4 Baseline regression of sample 2: Expenditure categories in 139 large municipalities, 1982-2007

	Admin		Security		Education		Culture and recreation		Health care		Environment		Social Welfare		Transportation		Others	
Debt brake	-0.055		-0.118*		-0.142		0.001		-0.381		-0.058		-0.030		0.003		0.043	
	{-2.821}		{-4.473}		{-6.268}		{0.014}		{-5.633}		{-2.096}		{-1.099}		{0.104}		{1.250}	
	[0.292]		[0.080]		[0.569]		[0.921]		[0.460]		[0.450]		[0.811]		[0.973]		[0.478]	
Fiscal rule index	-0.027		-0.053		-0.123		-0.037		-0.315		-0.021		-0.016		-0.001		0.017	
	{-2.215}		{-3.284}		{-8.918}		{-1.319}		{-7.641}		{-1.218}		{-0.988}		{-0.070}		{0.793}	
	[0.476]		[0.144]		[0.687]		[0.667]		[0.593]		[0.585]		[0.711]		[0.965]		[0.741]	
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Two-way FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R2	0.28	0.28	0.48	0.48	0.14	0.15	0.57	0.57	0.12	0.13	0.10	0.10	0.51	0.51	0.22	0.22	0.42	0.42
N	3329	3329	3329	3329	3329	3329	3329	3329	3169	3169	3329	3329	3329	3329	3329	3329	3329	3329
Wald test: FE	25***	26***	84***	85***	85***	86***	31***	30***	65***	66***	33***	33***	89***	89***	32***	33***	42***	42***

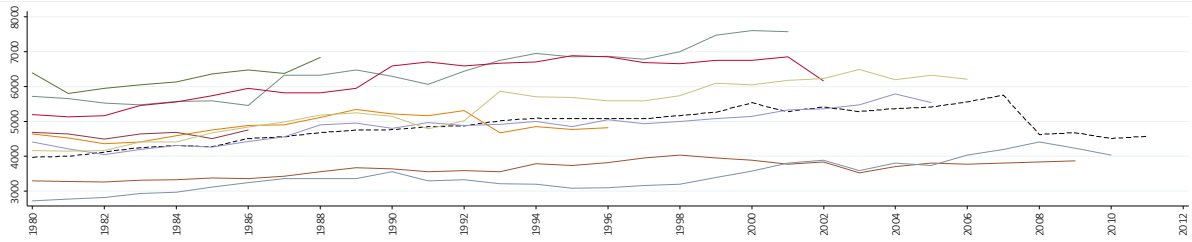
Note: Same controls included as in the baseline regression of sample 2 (Table 4). The numbers in square brackets indicate the estimated p-values using wild-cluster bootstrap-t procedure. These values are used to determine statistical significance: \*p<0.1 (significance at the 10% level), \*\*p<0.05 (significance at the 5% level), and \*\*\*p<0.01 (significance at the 1% level). Numbers in braces indicate estimated t-statistics for default standard errors. The Wald test has the null hypothesis that the fixed effects are jointly equal to zero. For the Wald tests, we report test statistics based on regressions with default standard errors.

Figure A.1 Development of local finances in the control and treatment groups

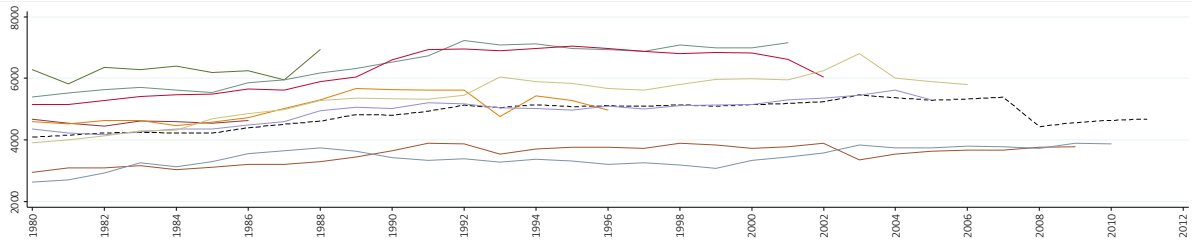
Sample 1: Debt per capita



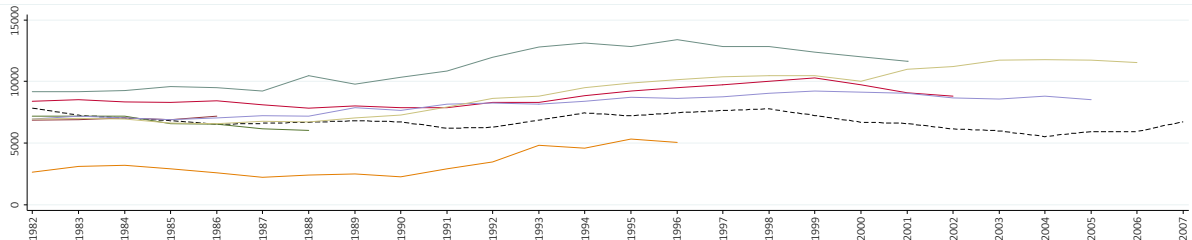
Sample 1: Revenue per capita



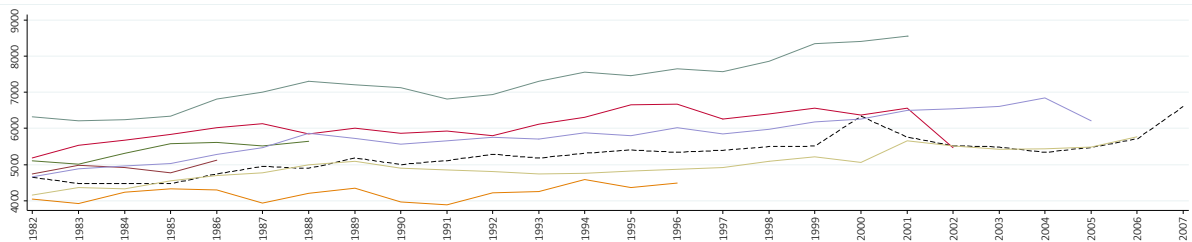
Sample 1: Expenditure per capita



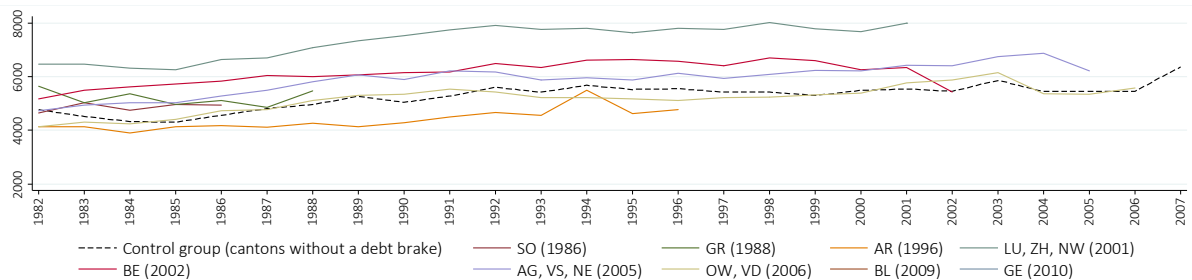
Sample 2: Debt per capita



Sample 2: Revenue per capita



Sample 2: Expenditure per capita



Note: In the interest of clarity, data of the 139 municipalities (sample 2) is aggregated at the level of the corresponding canton. The control group includes municipalities that are located in cantons without a debt brake during the whole period 1980-2010 (sample 1) and 1982-2010 (sample 2). Since the treatment (debt brake introduction) took place at different points in time, various treatment groups are depicted. The development of local finances in the treatment groups is only shown prior to treatment. All values are in real terms.

Table A.5 Pre-estimation test on cross-sectional dependence

Dependent variable (sample 1)	CD-test	p-value	Average correlation coefficient	Absolute correlation coefficient	Cross-sectional dependence?
Expenditures	17.29	0.000	0.213	0.372	✓
Revenue	31.23	0.000	0.384	0.448	✓
Debt	27.69	0.000	0.341	0.457	✓
Deficit	27.19	0.000	0.335	0.360	✓
Expenditure decentralization	56.00	0.000	0.572	0.578	✓
Revenue decentralization	53.93	0.000	0.550	0.563	✓
Expenditures on administration	-0.36	0.720	-0.004	0.282	-
Expenditures on security	14.96	0.000	0.184	0.345	✓
Expenditures on education	10.70	0.000	0.132	0.363	✓
Expenditures on culture	11.41	0.000	0.141	0.267	✓
Expenditures on health care	38.46	0.000	0.474	0.581	✓
Expenditures on social welfare	17.30	0.000	0.213	0.442	✓
Expenditures on transportation	16.61	0.000	0.205	0.316	✓
Expenditures on environment	4.93	0.000	0.060	0.302	✓
Expenditures on others	19.40	0.000	0.239	0.407	✓

Note: We report the average and absolute correlation coefficient across  $N \times (N-1)$  pairs of correlation. CD presents the Pesaran (2004) cross-section dependence statistic that is distributed standard normal and tests the null hypothesis of cross-section independence of error terms. We use the Stata routine xtcd.

Table A.6 Reduced form regression of sample 1: Local finances aggregated at the cantonal level, 1980-2011

	Expenditure	Revenue	Debt	Deficit
Unemployment	-0.299 {-0.559} (-0.279)	-0.381 {-0.702} (-0.371)	-3.208** {-2.383} (-1.802)	-251.104 {-0.159} (-0.142)
Relative income	-0.257 {-0.863} (-0.482)	-0.696** {-2.306} (-1.316)	-0.667 {-0.845} (-0.454)	1,135.228 {1.295} (1.088)
Income	0.310 {0.941} (0.485)	0.870*** {2.607} (1.281)	0.313 {0.351} (0.173)	-1,533.326 {-1.581} (-1.300)
Population	0.398*** {4.489} (2.291)	0.307*** {3.415} (1.724)	-1.731*** {-6.108} (-3.483)	288.531 {1.104} (0.714)
Share old	1.120*** {2.810} (1.506)	0.693* {1.713} (0.825)	-1.785 {-1.232} (-0.627)	1457.971 {1.241} (0.885)
Share young	0.481 {1.473} (0.656)	0.426 {1.284} (0.559)	2.653*** {2.732} (1.568)	445.876 {0.463} (0.324)
Share German	-0.088 {-0.391} (-0.179)	-0.461** {-2.013} (-0.984)	0.795 {1.215} (0.599)	1,383.581** {2.081} (2.205)
Adj. R2	0.56	0.58	0.41	0.20
N	800	800	550	800

Note: Due to data limitations, public debt is analyzed during the years 1990-2009. Canton and year fixed effects included. Constant not shown. The number in braces indicate the estimated t-statistics for default standard errors. These values are used to determine statistical significance: \* $p < 0.1$  {significance at the 10% level}, \*\* $p < 0.05$  {significance at the 5% level}, and \*\*\* $p < 0.01$  {significance at the 1% level}. The numbers in parentheses indicate the estimated t-statistics for standard errors adjusted for clustering at the cantonal level and corrected for heteroscedasticity.

Table A.7 Reduced from regression of sample 2: Local finances of 139 large municipalities, 1982-2007

	Expenditure	Revenue	Debt	Deficit
Unemployment	-0.370 {-0.906} [0.743]	-0.558 {-1.419} [0.701]	1.546 {1.609} [0.533]	665.108 {0.609} [0.649]
Relative income	0.204*** {3.761} [0.611]	0.179*** {3.435} [0.585]	0.514*** {4.026} [0.273]	75.125 {0.517} [0.368]
Income	-0.017 {-0.223} [0.925]	0.041 {0.569} [0.877]	-0.824*** {-4.678} [0.106]	-251.357 {-1.254} [0.052]
Population	0.084*** {13.343} [0.094]	0.078*** {12.913} [0.096]	0.222*** {14.944} [0.060]	40.161** {2.377} [0.617]
Share old	1.462*** {8.955} [0.064]	1.321*** {8.409} [0.088]	1.446*** {3.768} [0.386]	762.377* {1.746} [0.232]
Share young	-2.450*** {-13.058} [0.024]	-2.416*** {-13.379} [0.026]	-2.198*** {-4.983} [0.400]	171.193 {0.341} [0.147]
Share German	-0.276 {-0.970} [0.703]	-0.442 {-1.615} [0.290]	1.007 {1.506} [0.354]	-4.985 {-0.007} [0.955]
Adj. R2	0.39	0.40	0.14	0.06
N	3,329	3,329	3,329	3,329

Note: Canton and year fixed effects are included. Constant not shown. Number in braces indicate estimated t-statistics for default standard errors. These values are used to determine statistical significance: \*p<0.1 {significance at the 10% level}, \*\*p<0.05 {significance at the 5% level}, and \*\*\*p<0.01 {significance at the 1% level}. The numbers in square brackets indicate the estimated p-values using the wild-cluster bootstrap-t procedure.

Table A.8 Robustness tests part II: Further modifications

		Expend.	Revenue	Debt <sup>2)</sup>	Deficit	Exp. dec.	Rev. dec.
<i>Sample 1</i>							
<b>1) Sub-periods</b>	a) 1980-1989	Confirm	Confirm	n/a	Confirm	+/-	Confirm
	b) 1990-2011	Confirm	Confirm	n/a	*	Confirm	Confirm
	c) 1990-2008	Confirm	Confirm	+/-	Confirm	Confirm	Confirm
<b>2) Institutional controls</b>	a) Direct democracy 1980-2005	Confirm	Confirm	+/-*	Confirm	***	**
	b) Share own revenues 1990-2011	Confirm	Confirm	Confirm	*	Confirm	Confirm
<b>3) Voters' preferences</b>	a) Conservative cantons 1980-2011	Confirm	Confirm	+/-	Confirm	Confirm	Confirm
	b) Non-conservative cantons 1980-2011	Confirm	Confirm	*	**	Confirm	Confirm
<b>4) Outliers</b>	a) Median regression 1980-2011 <sup>1)</sup>	Confirm	Confirm	+/-	Confirm	**	*
	b) Exclusion of Cook outliers 1980-2011	Confirm	Confirm	+/-	Confirm	Confirm	*
<b>5) Exclusion of fixed effects</b>	a) Only canton fixed effects, 1980-2011	*	Confirm	Confirm	Confirm	+/-	**
	b) Only year fixed effects, 1980-2011	+/-	+/-	+/-	+/-	**	+/-
	c) No fixed effects, 1980-2011	+/-	+/-	+/-	Confirm	+/-	Confirm

Note: "Confirm" means that the debt brake dummy keeps the same sign as in the baseline regression (i.e. negative) and that it does not reach statistical significance at any conventional level based on p-values calculated by the wild-cluster bootstrap-t-procedure. If the variable has a different sign than in our baseline estimation it is indicated by +/- . If the variables reach statistical significance it is indicated by: \*(p<0.1), \*\* (p<0.05) and \*\*\* (p<0.01). OLS regression including two-way fixed effects and all controls as employed in the baseline regression (Table 3 and 5). Full regression bodies available upon request. 1) Inference based on cluster-robust standard errors. 2) Deviating periods since debt data is only available between 1990 and 2009.



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