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Fiscal rules, Local Fiscal Policies and Households' Mobility

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"*The truth is never pure and rarely simple.*"
Oscar Wilde

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Chapter 1

Fiscal Rules and Fiscal Performance: Evidence from Switzerland[1]

We study the dynamic effect of fiscal rules on fiscal performance, making use of the introduction of different balanced budget requirements at the cantonal and municipality levels in Switzerland over the past 50 years. We exploit the different timing of canton-level and municipality-level reforms of fiscal rules to control for time-invariant confounding factors as well as canton-specific trends. Results from a distributed-lag model show that more stringent fiscal rules at the cantonal level improve cantonal surplus through increased revenues, while expenditures are unaffected. Debt stabilizes to a lower level. We rule out that cantonal fiscal rules had a direct impact on the finances of their municipalities, but rather indirectly through transfers reduction.

JEL classification: H62, H64, H72, H77
Keywords: Fiscal rules, Deficits, Debt

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1.1 Introduction

Fiscal rules have been introduced at national or sub-national levels in many countries around the world with the aim of reducing public deficits and public debt, strengthening fiscal discipline and fostering economic performance. The recent financial and economic crises have put these fiscal rules (or the lack thereof) at the center of the policy debate.[2] Yet, empirical evidence on the effect of stringent fiscal rules on fiscal performance, while large, is plagued by challenging identification issues.

In this paper, we make use of the introduction of different balanced budget requirements at the cantonal and municipal level in Switzerland over the past 35 years. We exploit the different timing of canton-level and municipality-level reforms of fiscal rules to control for time-invariant confounding factors as well as canton-specific trends. We find that more stringent fiscal rules at the cantonal level reduce total cantonal debt through the generation of yearly surpluses via increased revenues.

We also investigate the vertical interdependencies between municipality-level and canton-level fiscal rules. These vertical interdependencies can be crucial, as the effects of institutions of one level of government could potentially be canceled by the lack thereof at another level. Upper-level governments could for example circumvent debt limits by transferring some tasks to lower-level governments.[3] Any effect of institutions at one level of government might therefore be canceled by the lack thereof at another level (Galletta and Jametti, 2015). While we do control for task decentralization reforms, we still find evidence of an indirect vertical effect: cantons are likely to perform larger cuts in transfers to the lower level if their municipalities are not subject to stringent fiscal rules.

Our paper contributes to the large literature investigating the effect of fiscal institutions on public finances outcomes. Contributions about the effect of budget balance restrictions are found in, among others, von Hagen (1991), Kiewiet and Szakaly (1992), Poterba (1994, 1996), Bohn and Inman (1996) with focus on the United States. This literature exploiting institutional variations among US states is summarized in Rose (2010). Overall, balanced budget rules are found to be associated with smaller deficits and lower debt.

[2] In this context, the so-called "Swiss debt brake" accepted in 2001 at the national level has often been discussed as a model for other countries. See Beljean and Geier (2013).

[3] Other mechanisms include creative accounting and other fiscal gimmickries. Commitment problems have been also extensively discussed in the literature. See, e.g., von Hagen (1991) or Alesina and Perotti (1996).

Switzerland, as the US, has been the research field of various studies on the effects of fiscal rules and other institutions, such as the fiscal referendum, that have the aim of strengthening the fiscal discipline of sub-federal governments. Feld and Kirchgässner (2001) construct an index of stringency of fiscal rules at the cantonal level and find that more stringent fiscal rules are associated with less public debt and deficits. This negative relationship is confirmed in a later study by Feld and Kirchgässner (2008) for cantonal deficits and for aggregated cantonal and local deficits. Krogstrup and Wälti (2008) also find that fiscal rules have a significant impact on real budget balances, even when voter preferences are controlled for. Feld et al. (2010) find, however, no effect of fiscal rules on cantonal revenues. Chatagny and Soguel (2012) investigate how fiscal deficits depend on budgeting errors and show that underestimating tax revenue is associated with lower deficits. A paper by Chatagny (2015) studies the link between fiscal rules and political ideology. His results indicate that, first, left-wing finance ministers tend to produce more conservative revenue forecasts to compensate for the negative signal related to their political ideology. Second, the paper shows that fiscal rules reduce the signaling power of tax revenue projections accuracy as measure of competence of finance ministers. As a result, left-wing ministers have less incentive to act conservatively in a fiscal rules regime compared to their right-wing counterparts. Luechinger and Schaltegger (2013) analyze the effect of fiscal rules on projected and realized budget deficits. Their results suggest that fiscal rules have an economically and statistically significant negative effect on the probability of a projected budget deficit, whereas the effect on realized budget deficits is smaller. Yerly (2013) proposes an alternative index for the stringency of fiscal rules based on Dafflon (2002). She finds that more stringent rules reduce indebtedness but have no effect on the balance of current accounts. Burret and Feld (2018a) investigate the effects of cantonal debt brakes on public deficits and debt, showing that fiscal rules are beneficial for deficit and debt. In particular, their effect is proportional to how well the rule targets the studied variable. Burret and Feld (2018b) address the topic of vertical effects between fiscal rules of upper and lower administrative levels in Switzerland. Their findings suggest that fiscal rules at an upper administrative level (cantons) could have, if any, a positive spillover effect on lower level finances.

Importantly, these four last studies use a fixed-effect panel model for their estimation, controlling for possible time-invariant and canton-specific confounding factors and identifying the effect through the different timing of introduction of fiscal rules at the cantonal level. We complement these studies by investigating the vertical interactions be-

tween municipality-level and canton-level fiscal rules on cantonal fiscal performance.

A closely related literature concentrates on the effects of direct democratic instruments. It discusses specifically the vertical effects of canton-level political institutions on decentralization and municipality-level outcomes. Feld et al. (2008) examine how canton-level direct democratic instruments affect the level of decentralization. Their results suggest that direct democracy indeed fosters decentralization. Funk and Gathmann (2011), using a long panel of cantons from 1890 to 2000 and exploiting different changes in political institutions over time, find no effect of direct democracy at the cantonal level on lower-level expenditure and decentralization.

This literature concentrates on the effects of canton-level fiscal rules on cantonal fiscal performance. Fiscal rules at an upper level of government might however have an influence on the fiscal performance of lower-level governments. The vertical interdependence of fiscal rules has not yet been fully analyzed in the literature. Most papers on fiscal institutions address superficially this issue by comparing the effects of fiscal rules on state and on the aggregate state plus local debt and deficits, see, e.g., Feld and Kirchgässner (2008). Fiscal rules at municipality level have no received great attention either.[4] The exception for Switzerland is Feld and Kirchgässner (1999, 2001a) who, building upon von Hagen (1992), construct an index of the stringency of budgetary procedures at the municipality level for a sample of 131 municipalities. They find that fiscal institutions such as formal fiscal restraints have no effect on local public debt, whereas political institutions, in particular the existence of fiscal referendum, are negatively correlated with debt. Feld et al. (2011) repeat their analysis for another year and confirm only partially the results.[5] Their empirical analysis is however limited by the use of cross-sectional variation among a small sample of municipalities, exposing their results to omitted variable bias.

The paper is organized as follows. Section 1.2 provides background on fiscal rules in Switzerland. Section 1.3 describes the dataset used in the analysis. Section 1.4 discusses the identification strategy and presents the main results. Section 1.5 is dedicated to heterogeneous effects, and section 1.6 concludes.

[4] A notable exception is Grembi et al. (2016) who use policy reforms in Italy as a quasi-experimental design to investigate the effect of relaxing fiscal rules on the budget of municipalities. They find that relaxing fiscal rules increased the deficits of Italian municipalities.

[5] They also describe in more details municipality-level fiscal rules for some cantons but do not include this information in their empirical analysis.

1.2 Fiscal Rules in Switzerland

Switzerland is an ideal setting to study the effects of fiscal rules at the sub-national level because of its long history of political and fiscal decentralization.

Since 1848 Switzerland has a federalist structure with three administrative levels: central government, cantons, and municipalities. A guiding principle is that the central government level fulfills only the duties that are explicitly assigned by the constitution, or for which a supra-cantonal regulation deems necessary. If tasks can be fulfilled efficiently and in financial autonomy by cantons, then no transfers to the central government should occur. To reach this goal, each canton has its own government, a parliament, and the autonomy to decide on its own political and tax system. Municipalities also enjoy a substantial fiscal autonomy: they can influence their own tax revenues through the choice of a "tax multiplier" to the cantonal tax base.

Cantons are responsible for all main public services while municipalities independently manage and finance a number of local public services, mainly schools, social services, energy supplies, and roads. About 50% of cantonal and municipal revenue are raised through their own taxes. At both levels, the personal income tax is the main fiscal instrument, accounting for roughly 70% of tax revenues at the cantonal level and 80% at the municipal level.

Switzerland has also a long history of statutory fiscal constraints. The first fiscal rule at the cantonal level has been introduced in 1929 by the canton of St. Gallen. This canton also has the most stringent rules at the municipal level. Since then, the majority of cantons have introduced several measures for the management of public finances at the cantonal and at the municipality level. The fiscal rules' sets are a collection of legal articles. Most articles for the cantonal fiscal rules are introduced in the canton-specific law for the financial management[6], sometimes complemented with constitutional articles stating the overall principals aimed with the fiscal rules, e.g. debt break. Often, specific accounting requirements are further laid down in by-laws[7], e.g. in the cantons of Basel-Landschaft, Glarus, Graubünden, Schwyz, Thurgau, Uri, Zürich. The approval by the majority of the cantonal voters is required for any change in the constitution, as well as for laws with op-

[6] Usually named "Finanzhaushaltsgesetz" in German, "Loi sur les finances" in French, "Legge sulla gestione finanziaria" in Italian, or similar.

[7] "Verordnung über den Finanzhaushalt" in German, "Ordonnance sur les finances" in French, "Ordinanza sulla gestione finanziaria" in Italian, or similar.

tional referendum. By-laws are not subject to popular vote. Fiscal rules for the municipal level are usually stated in the canton-specific law for the management of municipalities[8]. The timing of their introduction is decided by the cantonal parliament. For this reason, we consider the fiscal rules' implementation as exogenous to municipalities.[9] On top of the cantonal regulation, municipalities have the legal ability to adopt further rules, but this occurs in very limited circumstances.[10] Due to the democratic process, the implementation of (new) fiscal rules takes on average 2 years of discussions at political level, and their entry into force is usually certain one year in advance. These political aspects are indeed very important for the policy implications.

1.3 Data and sources

We exploit a longitudinal panel database consisting of publicly available data on the 26 Swiss Cantons from 1970 to 2016. The outcome variables of interest are cantonal gross debt, surplus, current expenditures and current revenues. From year 1990 onwards, we source our data from the Swiss Federal Finance Administration.[11] These data are subject to intensive harmonization efforts to ensure data comparability over the years and across cantons. Cantons are subject to minimal accounting principles, but in practice the detailed accounting procedures may vary. The harmonization consists in translating the accounting details into a common and slightly simplified accounting model. Depending on the cantonal characteristics, specific administrative units may be added (e.g. universities) or removed (e.g. churches). Time and cross-sectional variation should not suffer from the harmonization because the latter is consistent over time and units.
We try to recreate backward the time series by self-collecting data from the Swiss historical statistical yearbooks, which are, however, non subject to the same meticulous harmonization. The variables and their summary statistics are listed in Table 1.1.[12]

[8] "Gemeindegesetz" in German, "Loi sur les communes" in French, "Legge organica comunale" in Italian, or similar.

[9] A note for transparency: municipalities may be consulted by the cantons in a preliminary phase before the rules introduction, and we cannot rule out the presence of municipal mayors in cantonal parliaments either. However, with an average of around 80 municipalities per canton, the timing of implementation remains exogenous to the vast majority of local jurisdictions.

[10] Municipalities with own fiscal rules were identified in the cantons of Basel-Landschaft (municipality of Binningen), Graubünden (Domat/Ems), Lucerne (city of Lucerne, Schlierbach), Neuchâtel (Val-de-Travers, Val-de-Ruz), Schaffhausen (Hallau), Solothurn (city of Solothurn, Holderbank), list not exhaustive.

[11] https://www.efv.admin.ch/efv/en/home/themen/finanzstatistik/daten.html

[12] Summary statistics based on the harmonized data only can be found in Table 1.B.3.

The main variable of interest is the Fiscal Rules Stringency Index (FRSI) score applied at cantonal/municipal level, described in Section 1.3.1. Interestingly enough, there is great variation in the canton-level but also the municipality-level stringency index between and within cantons over time, as depicted in Figure 1.1 of Section 1.4. This is the variation we will exploit empirically. In the estimation we will consider only index increases because we are interested in the effect of *more stringent* fiscal rules.

The estimation model controls for socio-demographic characteristics of a canton as the share of population under 15 years, the share over 65 years and the share of foreign population. In addition, we control for unemployment and criminality rates, features that affect cantonal attractiveness, tax revenues and demand for public financing. Net income distribution differences are controlled for with the cantonal Gini coefficient.[13] Total number of firms and full-time equivalent employment (FTE) in the secondary and tertiary sectors measure economic activity. The political orientation of the cantonal Parliament is controlled for with the share of left-wing seats.[14] We also control for salient differences in the housing markets with a housing prices index.[15] Changes in the cantonal accounting system may influence the level of our outcome variables: for this reason, we also include in our regressions a dummy indicating the accounting system applied in a given canton and year.[16] Last, but not least, we control for reforms of task allocation between the canton and its municipalities. Historically, the sectors mostly confronted with such reforms are school, health, and social affairs. The last two decades have been marked by an intensive reform activities (Flèche, 2021): overall the tendency is to centralize decision making and decentralize financing. Failing to control for these reforms may lead to biased results. For this reason, we include in our estimations a dummy variable indicating whether a cantonal reform occurred, as identified and exploited in Flèche (2021).[17].

[13] The measure is sourced from the Swiss Federal Tax Administration and is computed on all natural persons, whether they pay the Federal income tax or not, https://www.estv.admin.ch/estv/de/home/allgemein/steuerstatistiken/fachinformationen/steuerstatistiken/direkte-bundessteuer.html.

[14] Parties' orientation is sourced from the Manifesto Project for Switzerland https://manifesto-project.wzb.eu/. Political preferences are assigned by analyzing the parties' electoral manifestos.

[15] The index is at macro-region. Cantons are assigned to macro regions equivalently to Brülhart and Parchet (2014): Zurich area (Zürich, Schaffhausen), Eastern Switzerland (Appenzell-Innerrhoden, Appenzell-Ausserrhoden, St. Gallen, Thurgau, Glarus, Luzern, Nidwalden, Obwalden, Schwyz, Uri, Zug), Northwestern Switzerland (Aargau, Basel-Landschaft, Basel-Stadt, Solothurn), Southern Switzerland (Graubünden, Ticino, Valais), Lake Geneva area (Genève, Vaud), Western Switzerland (Fribourg, Jura, Neuchâtel).

[16] Not included in the summary statistics. The primary source of information on the introduction of the Harmonized Accounting Model 2 (HAM2) is the Swiss Public Sector Financial Reporting Advisory Committee (SRS-CSPCP), https://www.srs-cspcp.ch/en. The year of introduction of the Harmonized Accounting Model 1 (HAM 1) is self-collected, with information from the cantonal administrations.

[17] Not included in the summary statistics.

Table 1.1: Summary statistics, 1970-2016

	Mean	SD	Min	Max	Obs.
Panel A: cantonal outcome variables					
Debt	5,385	5,075	405	35,529	1,213
Surplus	-38	441	-4,212	2,215	1,213
Expenditures	6,518	3,829	1,007	29,906	1,213
Revenues	6,480	3,842	904	29,392	1,213
Panel B: municipal outcome variables					
Debt	6,100	4,018	584	26,783	702
Surplus	62	191	-892	878	1,038
Expenditures	3,906	1,717	45	8,677	1,113
Revenues	4,150	1,643	155	8,749	1,038
Panel C: Fiscal Rules Stringency Index					
Cantonal level	1	1	0	6	1,213
Municipal level	2	2	0	7	1,213
Panel D: cantonal covariates					
Population share < 15y (%)	19	3	12	29	1,213
Population share > 65y (%)	15	2	9	22	1,213
Foreigners share (%)	17	7	7	41	1,213
Unemployment rate (%)	2	2	0	8	1,213
Criminality rate (‰)	3	1	0	9	1,213
Gini coeff. of net income	40	7	29	66	1,213
Firms in II sector	3,191	3,170	208	15,544	1,213
Firms in III sector	11,139	12,972	418	101,294	1,213
FTE in II sector	44,570	43,593	1,849	239,368	1,213
FTE in III sector	86,940	115,827	1,417	851,068	1,213
Share of left-wing seats (%)	24	14	0	53	1,213
Housing price index	99	36	26	224	1,213
Panel E: total cantonal population					
Total population	272,518	286,652	12,924	1,487,969	1,213

Notes: Table 1.1 provides summary statistics for our sample of cantons. Not reported are the dummy variables on the accounting systems and on the tasks centralization reforms. All outcome variables are expressed in per capita terms. Municipal outcome variables in Panel B are the aggregate value for all municipalities within a canton. The lower number of these observations is explained by restricted data availability: municipal revenue data start in 1977 (affecting also the availability of municipal surplus), while municipal expenditure data start in 1976. Municipal debt is available from 1990 onwards from the panel of harmonized variables (equivalent to Table 1.B.3). The canton of Jura was founded in 1979, no data exist for this canton prior to that year. We employ total cantonal debt as measure of municipal debt for the canton of Basel-Stadt.[a] The Gini coefficient was multiplied by 100 for reporting purposes. The housing price index reflects offer prices, indexed as 100 at year 2000.

[a] The canton of Basel-Stadt is a special case: it has only 3 municipalities, including the capital and largest city Basel. Parliament and government are responsible for both the canton and the city of Basel.

1.3.1 Fiscal Rules Stringency Index

We design a Fiscal Rules Stringency Index (FRSI) to measure the stringency of the legal framework for public finances management. The index structure aims at simplifying the sophisticated evaluation tool by Yerly (2013), making it applicable to both the cantonal and the municipal level. Indices that are similar in spirit can be found for the US and the European Union in Advisory Commission On Intergovernmental Relations (1987, exploited in Poterba, 1994), von Hagen (2005), Feld and Kirchgässner (2008), Xavier Debrun (2008). Applications to the Swiss institutional context can be found, in addition to the cited Yerly (2013), also in Locher (2015), Burret and Feld (2018a).

Our index aims at simplifying the evaluation of a set of fiscal rules by breaking them down into the three categories presented in Table 1.2.[18] For each category, only one option is possible. The score within each category ranges from 0 to 3, where 0 means that the category is not part of the fiscal rules' set and 3 means that the most stringent option within that category is implemented. The total FRSI score is the sum of scores across the three categories, with a maximum possible score of 9. We compute a FRSI score separately for the municipal and the cantonal level for each year.[19]

Table 1.2: Fiscal Rules Stringency Index design

Object of balance	**Re-balancing mechanism**	**Fiscal rule violation**
3 - Account and budget	3 - Automatic tax adjustment	3 - Autonomy restriction
2 - Account	2 - Deficit compensation in $t+2$	2 - Automatic sanction
1 - Budget	1 - Deficit compensation $> t+2$	1 - Plan corrective measures
0 - Not object defined	0 - No rebalance mechanism defined	0 - No sanction defined

[18] The choice of the categories follows a logical sequence of events from the political point of view. In $t-1$ the Parliament votes a budget for the next fiscal year t. In the first months of $t+1$ the financial statements for fiscal year t are available, revealing if the accounting year closed with a deficit or a surplus. While preparing the next budget for $t+2$, fiscal rules can, in principle, start affecting political decision making, for example if they require a deficit of year t being compensated in $t+2$ already. Sanctions in case of violation of a fiscal rule can occur at different stages of the political process, most commonly during the budget preparation.

[19] To prevent significant deviations of index scores due to subjective evaluation, we checked the correlation of our FRSI score at cantonal level with existing indices applied to the Swiss framework. Correlation measures range from 64% for Yerly (2013) to 75% for Locher (2015), with a maximum of 79% for the index used by Burret and Feld (2018a).

The first category is the *Object of balance*, i.e. if the law[20] requires the budget, and/or the end-of-year account to be balanced. The highest score (3) is assigned if the legislative framework requires to balance both account and budget. If only the account has to be balanced, the score is 2. If only the budget has to be balanced, the score is 1. The second category is the *Rebalance mechanism*, i.e. how public finances should be rebalanced to compensate for a deficit. The most severe adjustment is an automatic tax increase, for which we assign a score of 3. If general deficit compensation measures are required, we distinguish the score by the time horizon stated. If the laws state that in $t+2$ already, then we assign a score of 2. When the time horizon is specified, but further away than $t+2$, the score is 1. Note that if the law does not specify a time horizon, e.g. it requires a deficit compensation "in the medium term", no score is assigned because it is considered as a rather loose criterion. The third category is the *Fiscal rule violation*, which specifies any sanctions occurring if a fiscal rule (or a set thereof) is violated. The most severe sanction, applicable only to municipalities in case of repeated violation of legal dispositions, is the autonomy restriction. In such an extreme case, the canton takes over the municipality's management. If some automatic sanction is foreseen, e.g. a mandatory amortization rate for the balance sheet deficit, the score is 2. A score of 1 is granted for the more common but soft requirement of a financial plan with corrective measures.

Largest changes in the index at cantonal level are driven by rules on both budget and end-of-year accounts (e.g. canton of Ticino) and by automatic tax adjustments (e.g. cantons of Fribourg and St. Gallen). The most frequent combination is some mild rules regulating the budget without a specific time horizon to rebalance public finances. For the local level a similar picture arise, with the most stringent cantons being those with automatic tax adjustments to rebalance finances (e.g. cantons of Fribourg and St. Gallen). A restriction of local autonomy in case of fiscal violation occurs in almost half of the cantons.

[20] Note that our index does not discriminates between rules stated in a constitution vs other legal documents, our evaluation considers all existing rules for a given administrative level and year.

1.4 Assessing fiscal rules' impact on cantonal and municipal fiscal performance

As of end of 2020, 24 out of 26 Swiss Cantons had a system of fiscal rules in force at both administrative levels[21], confirming Switzerland as an ideal natural laboratory to study the dynamics of public finances. This institutional variety and heterogeneity in the timing of introduction of fiscal rules are key to the identification strategy of this paper. Figure 1.1 depicts the heterogeneity in application of fiscal rules' set and in their timing. For estimation purposes, we will focus on the effect of more stringent fiscal rules, i.e. strictly positive increases in the FRSI values.

1.4.1 Identification strategy and econometric method

Cantons typically introduce fiscal rules (often labeled as "debt brakes" measures) to reshape their own public finances. We therefore expect cantons with higher debt level or lower surplus to introduce more stringent fiscal rules, which would lead to biased estimates. Other factors such as e.g. preferences could shape both public finances and the fiscal rules. To alleviate these concerns, we design a model with both canton and year fixed effects that control for canton-specific time-invariant confounding factors, as well as common shocks that would affect all cantons equally. Our identification strategy exploits therefore the *timing* of fiscal rules reforms and assume that such timing is exogenous, conditional on the control variables listed in Table 1.1, Panel (D).[22] It is important to stress that, on one hand, fiscal reforms are the outcome of legislative procedures that can last several years until their entry into force.[23] On the other hand, also their impact should be considered as a dynamic process rather than a one-off occurrence. This rationale motivates the choice of a distributed lag model.[24]

The current literature on cantonal fiscal rules has focused on measuring a unique effect of fiscal constraints on fiscal performance with a static difference-in-difference (DID) ap-

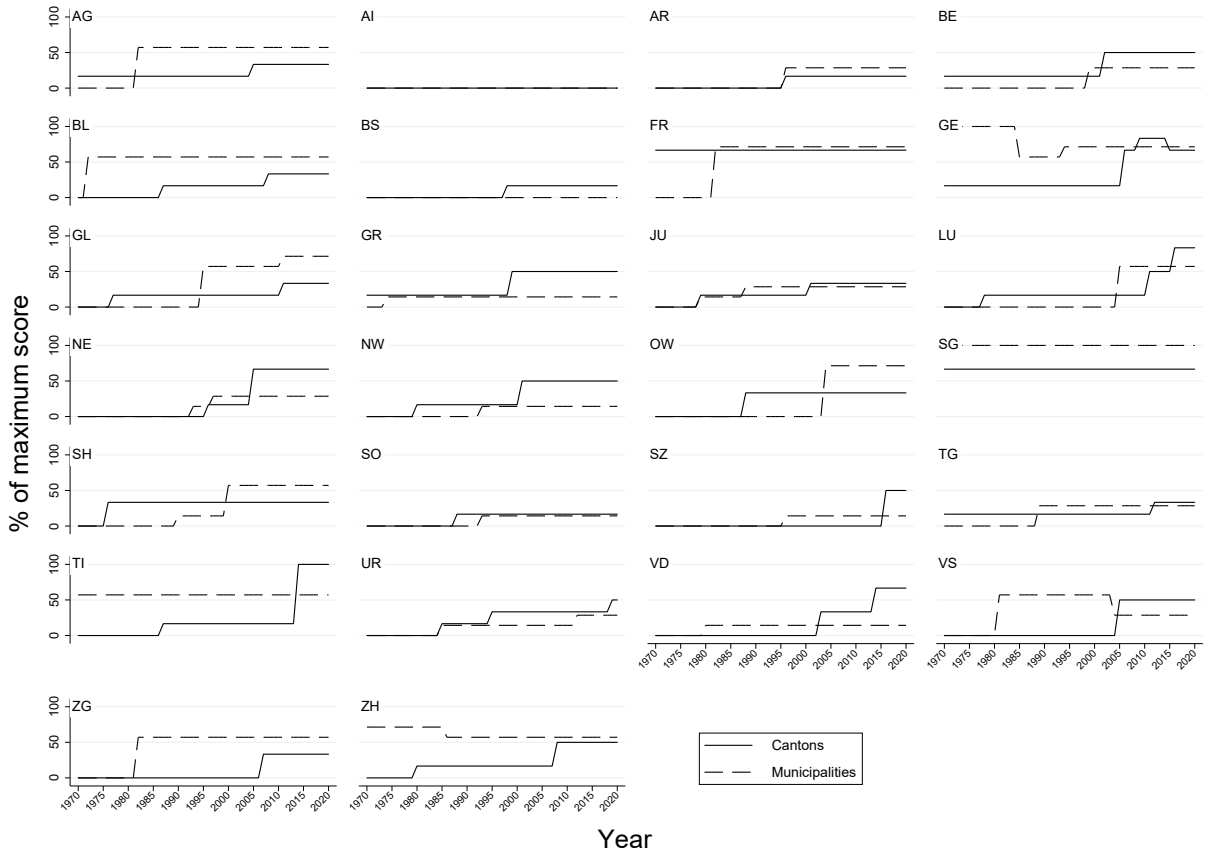
[21] Appenzell Innerrhoden (AI) does not define any fiscal rules for the cantonal level. AI and Basel-Stadt (BS) do not have any fiscal rules in place for the municipality level.

[22] Including accounting systems and tasks centralization reform dummies.

[23] Not only the legislative and the executive body have to agree on the reform, but often a popular vote is required if changes affect the cantonal constitutions.

[24] A distributed lag model is a re-parametrized version of an event-study with binned endpoints (Schmidheiny and Siegloch, 2020).

Figure 1.1: Cantonal and municipal fiscal rules, 1970 - 2020



Notes: Note: The y-axis reports the value of the Fiscal Rules Stringency Index for cantons and municipalities separately, as percentage of the overall maximum score attained during the 1970 - 2020 period. A higher score implies more stringent fiscal rules. Cantonal index decreases seldom occurred (GE) and are disregarded in the empirical estimation.

proach. For our research, we aim at moving one step further by adding *dynamics*. The distributed lag model provides a framework to plot regression results in a very intuitive way, showing how average treatment effects from a reform evolve over time.[25] In addition, it allows to check the validity of the underlying assumption of parallel trend between control and treatment units before the introduction or changes to fiscal rules.

The model is constructed as a two-way fixed effects panel regression, where we regress the dependent variable on a set of non-parametric reform indicators.[26] The timing of the re-

[25] A static DID model assumes that the effect is immediate after the reform and stays constant afterwards.

[26] Sometimes the indicators are defined as "event" indicators, i.e. dummy variables taking the value of 0 before and 1 after a certain treatment occurred.

forms is identified by any year with a positive change in $FRSIC$, i.e. $\Delta FRSIC > 0$, for each canton. The counterfactual is represented by each canton's pre-treatment period and by all not-yet-treated cantons[27]. Our model exploits all identified reforms, potentially more than one for each canton, and takes into account the reforms' magnitude.[28] In our baseline estimation, we consider a dynamic effect between 5 years before and 10 years after a reform. We assume that the effect is constant outside this window, and that any effect from a reform is homogeneous across cantons (and cohorts). The importance of explicitly stating these assumptions is highlighted by the current discussion in the event-study literature on how to correctly estimate an average treatment effects if treatment is staggered and the treatment effect heterogeneous (Athey and Imbens, 2021; Borusyak and Jaravel, 2018; Callaway and Sant'Anna, 2021; Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2020). One issue in particular is that an incorrect specification of the dynamics of the effect leads to biased estimates in standard two-way fixed effects models, as already treated units serve as control group for reforms happening at a later point in time.

1.4.2 Estimating equation

We estimate the following distributed lag model (Schmidheiny and Siegloch, 2020):

$$\Delta y_{it} = \sum_{j=-5}^{10} \gamma_j \Delta FRSIC_{i,t-j} + \beta' \Delta \mathbf{X_{it}} + \alpha_i + \lambda_t + \Delta \epsilon_{it} \tag{1.1}$$

where Δy_{it} a first-differenced fiscal performance indicator for canton i and year t. At cantonal level we will look at total surplus (defined as total expenditures - total revenues), total expenditures, total revenues, tax and non-tax revenues, as well as expenditures for transfers.[29] $\Delta FRSIC_{i,t-j}$ is the change in $FRSIC$ for canton i $j \in [-5, 10]$ years before or after each reform. We then report the cumulative effect β_j for each year before and after the reform, using $t-2$ as the reference year. Specifically, β_j is computed as the running sum of the γ_j parameters starting from the reference year. For the pre-treatment period, we

[27] This group includes also never-treated cantons (permanent control group).

[28] This corresponds to a model of "multiple events of different intensities" in Schmidheiny and Siegloch (2020).

[29] At a later stage we will run estimations on the same set of outcome variables at aggregate municipal level (sum of all municipalities of canton i in year t), except expenditures for transfers which are substituted with revenues from transfers.

cumulate downwards away from the reference period (negative sign) instead:

$$\beta_j = \begin{cases} -\sum_{k=j+1}^{-2} \gamma_k & \text{if } -5 \leq j \leq -3 \\ 0 & \text{if } j = -2 \\ \sum_{k=-1}^{j} \gamma_k & \text{if } -1 \leq j \leq 10 \end{cases}$$

The vector **X** includes the cantonal covariates listed in Table 1.1, Panel (D), as well as accounting systems and tasks centralization reforms dummies. Parameters α_i and λ_t capture cantonal linear time trend[30] and year fixed effects, respectively. The first-difference takes into account time-invariant unobserved factors at unit-level, i.e. cantonal fixed effect.

Our panel of dependent variables runs from 1970[31] to 2019, and we want to generate an effect window of $j \in [-5, 10]$. Let's denote $\underline{j} = -5$ and $\overline{j} = 10$, $\underline{t} = 1970$ and $\overline{t} = 2019$. Following Schmidheiny and Siegloch (2020) we note the following technical requirements: our coefficients measure a change in the treatment ($FRSIC$), therefore we need to include leads until $\underline{j}+1$, and that we need to observe treatment status from $\underline{t} - \overline{j}$ to $\overline{t} - |\underline{j}| - 1$. Any reform occurring outside the panel years can impact the dependent variable as any reform occurring within the panel years. For this reason, we need to observe reforms that occurred up to 10 years before the start of the panel to test for after-event trends, and up to 4 years after the end of the panel to test for pre-event trends. i.e. from 1960 (1970 - 10) to 2023 (2019 + 4). Since we observe treatment status only until 2020 we lose three years of observations at the end of the panel. In addition, due to the first-differenced model, we lose the first year of observation of the dependent variable. In total, our estimation will be exploiting the period 1971-2016.

1.4.3 Results

1.4.3.1 Cantonal level

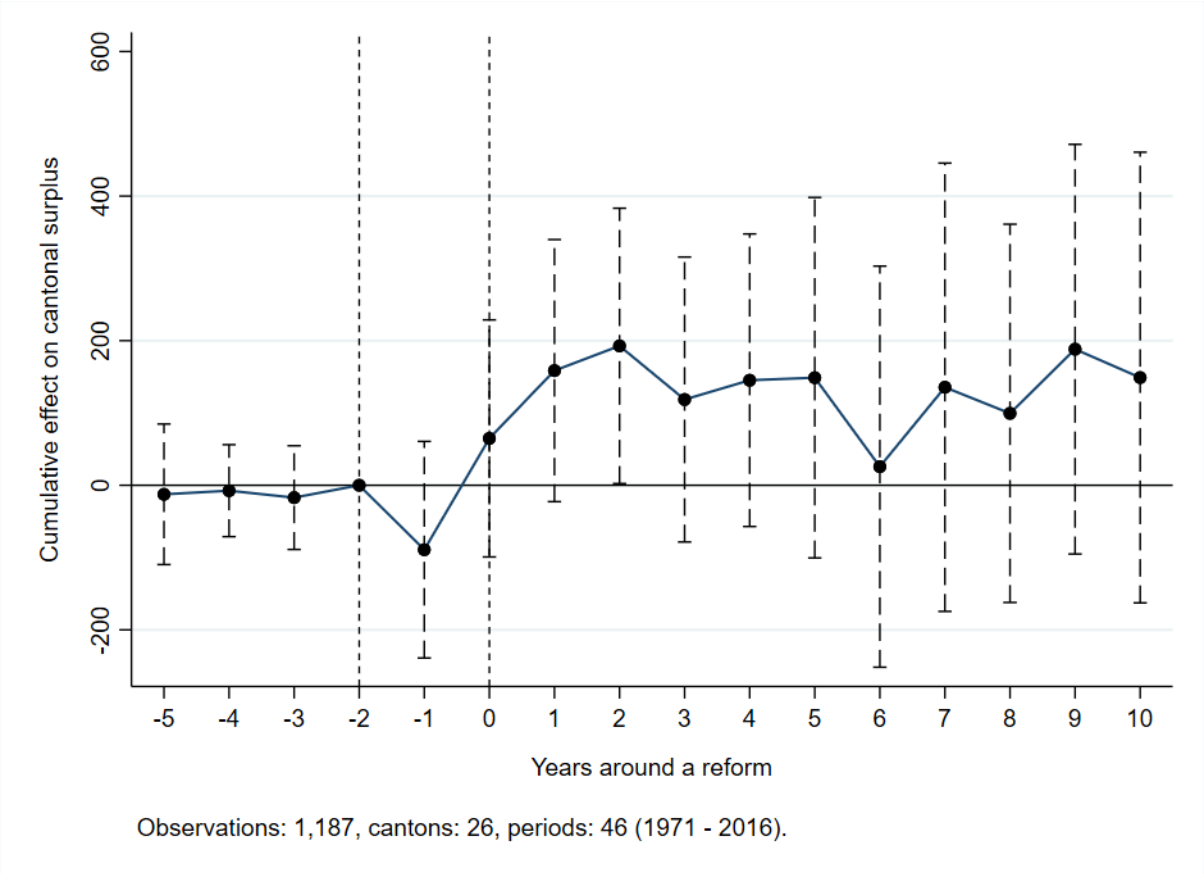
The vast majority of the cantonal fiscal rules in our setting require balancing budgeted expenditures and revenues, decreasing the yearly deficit. In Figure 1.2 we investigate the effect of fiscal rules on cantonal surplus. Results show that, indeed, cantons respond to fiscal rules' introduction (or strengthening) by increasing surplus (i.e. reducing their deficit). The estimated average effect reaches a maximum of CHF 193 per person 2 years after a fiscal rules reform. This positive effect is persistent, despite a slowdown around 6 years after

[30] In a model in level this would be equivalent to $\alpha_i \times t$.

[31] With exceptions for municipal outcome variables, see Table 1.1.

the reform, but completely offset the next year. After 10 years, the cumulated effect is of CHF 149 per person. Importantly, there is no effect prior of the reform, alleviating endogeneity concerns. Figure 1.A.1 in the appendix reproduces and confirms the result using a panel of harmonized cantonal data starting in 1990.

Figure 1.2: Cantonal surplus

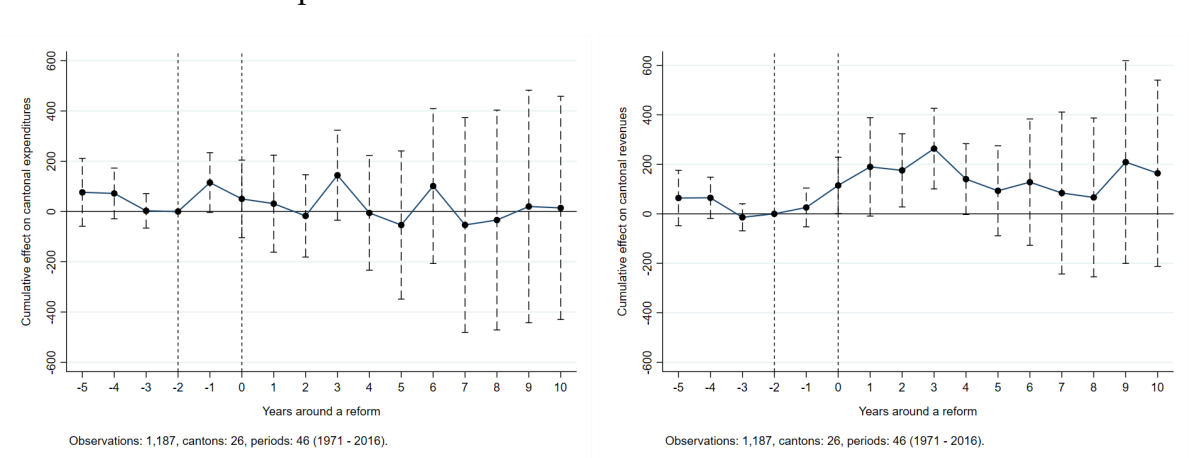


Notes: The figure shows the average cumulative effect on cantonal per capita surplus for an event window of -5 to +10 years around each fiscal rules reform from estimating equation 1.1. We consider a fiscal rules reform as any positive change in our Fiscal Rules Stringency Index at cantonal level, that is $\Delta FRSIC > 0$. Surplus is defined as total cantonal revenues - total cantonal expenditures. The regression includes cantonal fixed effect, year fixed effect, and a cantonal linear time trend. Control variables are: population share < 15 years, population share > 65 years, foreigners share, unemployment rate, criminality rate, Gini coefficient of net income, number of firms and number of jobs (full-time equivalent) in the secondary and tertiary sectors, share of left-wing seats in the Cantonal Parliament, housing prices index, accounting model and tasks centralization reforms dummies. Standard errors are clustered at canton and year level. Vertical dashed lines report 95% confidence intervals.

We now focus on the two components of surplus in isolation, namely expenditures and revenues. Estimation results are reported in Figure 1.3 below and show that adjustment

margin is on the revenue rather than the expenditure side. Over the first 3 years after a reform, cantons have, on average, improved their total revenues by CHF 263 per person compared to the pre-reform period. After 10 years, the cumulated effect is of CHF 164 per person. On the expenditures side, there is no cumulated statistically significant effect, and we lack statistical precision due to increasing variability in the long-run. Appendix Figure 1.A.2 shows a highly comparable behavior based on the panel of harmonized data. For the interested reader, in Appendix Figure 1.A.3 we report baseline results for cantonal surplus, expenditures and revenues by splitting reforms into two waves (before and after year 1995) and in Appendix Figure 1.A.4 we report estimation results for investment expenditures rather than current expenditures.

Figure 1.3: Cantonal expenditures and revenues

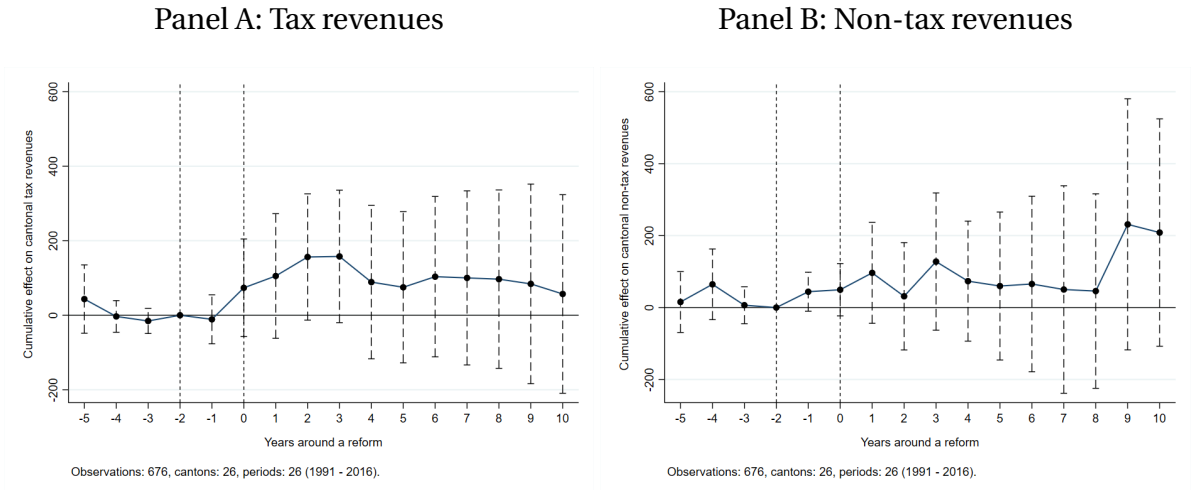


Notes: The figure shows the average cumulative effect on cantonal per capita total expenditures (Panel A) and per capita total revenues (Panel B) for an event window of -5 to +10 years around each fiscal rules reform from estimating equation 1.1. We consider a fiscal rules reform as any positive change in our Fiscal Rules Stringency Index at cantonal level, that is $\Delta FRSIC > 0$. The regression includes cantonal fixed effect (first-difference model), year fixed effect, and a cantonal linear time trend. Control variables are: population share < 15 years, population share > 65 years, foreigners share, unemployment rate, criminality rate, Gini coefficient of net income, number of firms and number of jobs (full-time equivalent) in the secondary and tertiary sectors, share of left-wing seats in the Cantonal Parliament, housing prices index, accounting model and tasks centralization reforms dummies. Standard errors are clustered at canton and year level. Vertical dashed lines report 95% confidence intervals.

Through which channels did governments succeed in increasing revenues in the short-term? To answer this policy-relevant question, we break down total revenues in two main categories: tax revenues and non-tax revenues in Figure 1.4. Note that due to data limi-

tations, we can generate this result only with the set of harmonized data. Results indicate that cantons increased their revenue mostly through their tax system rather than non-tax revenue. This opens the discussion on whether the effect on tax revenue is most likely driven by adjustments in the cantonal tax rates or adjustments in the tax base. Based on preliminary unreported tests, we believe that it is rather the first option, but an appropriate proof is left for future research. Our estimation strategy controls for possible confounders that may rather speak for a tax base adjustment, e.g. periods of economic growth and as the end of the '90s.

Figure 1.4: Cantonal tax revenues and non-tax revenues, harmonized data



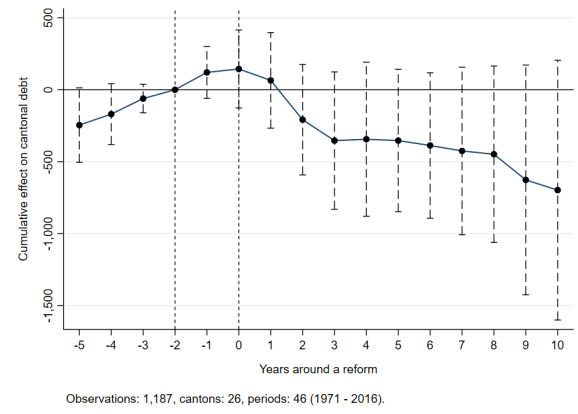
Notes: The figure shows the average cumulative effect on cantonal per capita tax revenues (Panel A) and per capita non-tax revenues (Panel B) for an event window of -5 to +10 years around each fiscal rules reform from estimating equation 1.1 on a panel of harmonized cantonal data from 1990-2019. We consider a fiscal rules reform as any positive change in our Fiscal Rules Stringency Index at cantonal level, that is $\Delta FRSIC > 0$. The regression includes cantonal fixed effect (first-difference model), year fixed effect, and a cantonal linear time trend. Control variables are: population share < 15 years, population share > 65 years, foreigners share, unemployment rate, criminality rate, Gini coefficient of net income, number of firms and number of jobs (full-time equivalent) in the secondary and tertiary sectors, share of left-wing seats in the Cantonal Parliament, housing prices index, accounting model and tasks centralization reforms dummies. Standard errors are clustered at canton and year level. Vertical dashed lines report 95% confidence intervals.

A next question rises naturally: is the surplus generated with additional tax revenues used to decrease outstanding public debt? The answer is probably yes, fiscal rules seems to act as effective "debt brake" mechanisms. Figure 1.5 reports the results for the full dataset (Panel A) and for harmonized data (Panel B). Results are close to a precisely estimated effect of a reduction in cantonal debt after fiscal rules reforms (confidence intervals cross by little the 0-effect line). The decrease occurs realizes during the first 3 years after a reform, with a

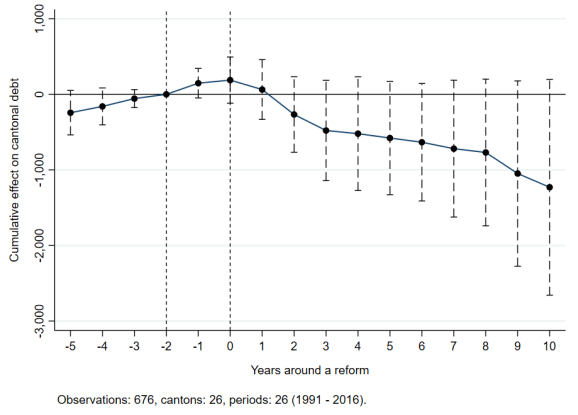
cumulated effect of CHF -353 per person compared to the pre-reform period. Afterwards, additional small reductions are reached, until a cumulated effect after 10 years of about CHF -697 per person (Panel A). Similar results, but larger in magnitude, are attained with the harmonized dataset (Panel B).

Figure 1.5: Cantonal debt

Panel A: Debt



Panel B: Debt, harmonized data

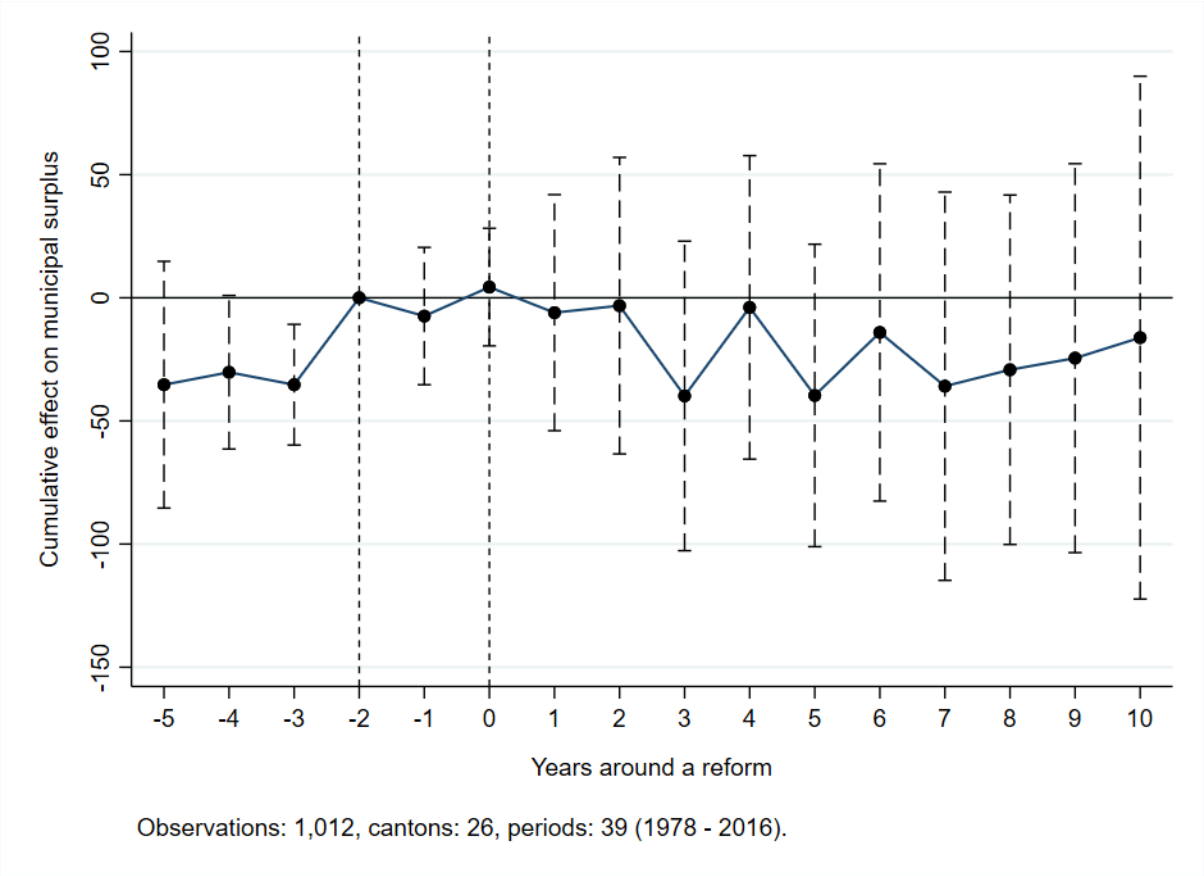


Notes: The figure shows the average cumulative effect on cantonal per capita total gross debt (Panel A) and per capita total debt using a 1990-2019 panel of harmonized cantonal data (Panel B) for an event window of -5 to +10 years around each fiscal rules reform from estimating equation 1.1. We consider a fiscal rules reform as any positive change in our Fiscal Rules Stringency Index at cantonal level, that is $\Delta FRSIC > 0$. The regression includes cantonal fixed effect (first-difference model), year fixed effect, and a cantonal linear time trend. Control variables are: population share < 15 years, population share > 65 years, foreigners share, unemployment rate, criminality rate, Gini coefficient of net income, number of firms and number of jobs (full-time equivalent) in the secondary and tertiary sectors, share of left-wing seats in the Cantonal Parliament, housing prices index, accounting model and tasks centralization reforms dummies. Standard errors are clustered at canton and year level. Vertical dashed lines report 95% confidence intervals.

1.4.3.2 Municipal level

The coexistence of and the connections between different government layers suggests that fiscal decisions at one government level may impact another level, even if the latter is not directly targeted by the decision. For this reason it is worth investigating also the effect of cantonal fiscal rules on municipal finances. We start by looking at municipal surplus in Figure 1.6. Based on the empirical results, we rule out that the cantonal fiscal rules reforms had a direct impact on the surplus/deficit of their municipalities. This is in line with the results from Burret and Feld (2018b).

Figure 1.6: Municipal surplus

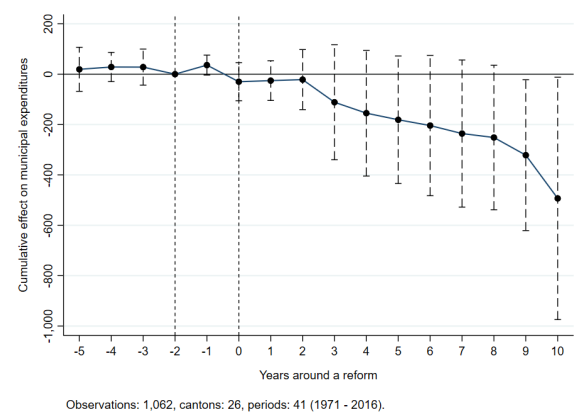


Notes: The figure shows the average cumulative effect on aggregated municipal per capita surplus for an event window of -5 to +10 years around each fiscal rules reform from estimating equation 1.1. We consider a fiscal rules reform as any positive change in our Fiscal Rules Stringency Index at cantonal level, that is $\Delta FRSIC > 0$. Surplus is defined as total municipal revenues - total municipal expenditures. The regression includes cantonal fixed effect (first-difference model), year fixed effect, and a cantonal linear time trend. Control variables are: population share < 15 years, population share > 65 years, foreigners share, unemployment rate, criminality rate, Gini coefficient of net income, number of firms and number of jobs (full-time equivalent) in the secondary and tertiary sectors, share of left-wing seats in the Cantonal Parliament, housing prices index, accounting model and tasks centralization reforms dummies. Standard errors are clustered at canton and year level. Vertical dashed lines report 95% confidence intervals.

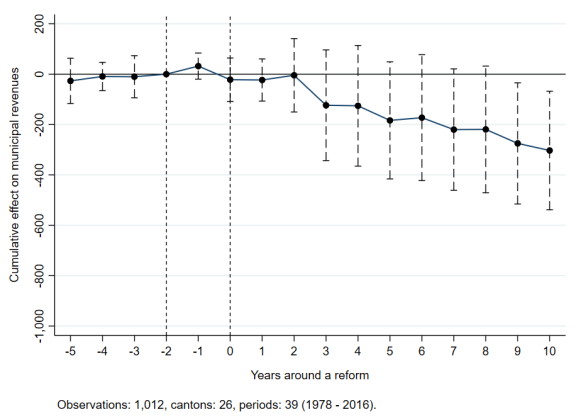
Decomposing the surplus into revenues and expenditures in Figure 1.7 reveals however another pattern. We find that both revenues and expenditures decreased at the municipality level after more stringent fiscal rules were adopted at the cantonal level. The negative effect on expenditures rules out a transfer of tasks from the canton to the municipalities, and the positive effect on canton-level tax revenues should not lead to a negative effect on municipalities (absent strong strategic vertical interactions in their tax rates).

Figure 1.7: Municipal expenditures and revenues

Panel A: Expenditures



Panel B: Revenues



Notes: The figure shows the average cumulative effect on aggregated municipal per capita total expenditures (Panel A) and per capita total revenues (Panel B) for an event window of -5 to +10 years around each fiscal rules reform from estimating equation 1.1. We consider a fiscal rules reform as any positive change in our Fiscal Rules Stringency Index at cantonal level, that is $\Delta FRSIC > 0$. The regression includes cantonal fixed effect (first-difference model), year fixed effect, and a cantonal linear time trend. Control variables are: population share < 15 years, population share > 65 years, foreigners share, unemployment rate, criminality rate, Gini coefficient of net income, number of firms and number of jobs (full-time equivalent) in the secondary and tertiary sectors, share of left-wing seats in the Cantonal Parliament, housing prices index, accounting model and tasks centralization reforms dummies. Standard errors are clustered at canton and year level. Vertical dashed lines report 95% confidence intervals.

To shed more light on this result, we investigate the effect of fiscal rules on municipal transfer vs non-transfer revenues in Figure 1.8. Note that this information is only available for harmonized data.[32]

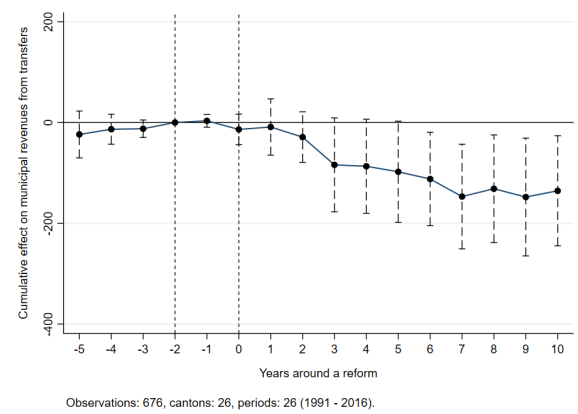
Results indicate that the decrease in municipal revenue is driven mainly by a decrease of transfers. The precisely estimated long-term decrease is of CHF 135 per person (Figure 1.8, Panel A) while non-transfer revenues seem largely unaffected by the cantonal fiscal rules reforms (Figure 1.8, Panel B). This finding suggests that cantons reduced their transfers to municipalities after adopting more stringent fiscal rules, and that municipalities adjusted downwards their expenditures as a result.[33]

[32] Results on municipal surplus, revenues and expenditures using harmonized data are fully in line with results using the full panel. See Figures 1.A.6 and 1.A.7 in the Appendix. For the interested reader, we report in Appendix Figure 1.A.8 estimation results for investment expenditures rather than current expenditures.

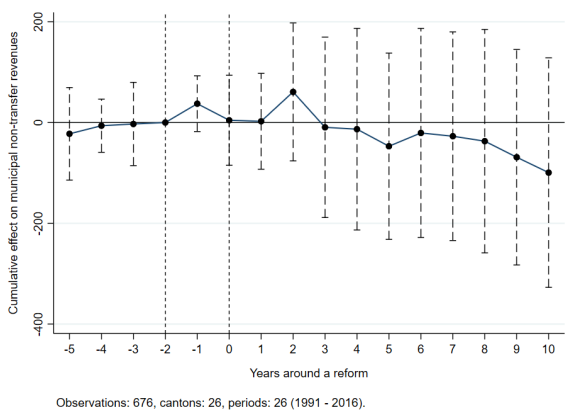
[33] The reduction in transfer revenues at the municipality level is confirmed by the reduction in cantonal transfer expenditures. See Figure 1.A.5 in the Appendix. Note that transfers to the municipal level represent around 12% of total cantonal transfer expenditures, but as much as 40% of municipal transfer revenues.

Figure 1.8: Municipal transfer and non-transfer revenues, harmonized data

Panel A: Transfer revenues



Panel B: Non-transfer revenues



Notes: The figure shows the average cumulative effect on aggregated municipal per capita total transfer revenues (Panel A) and per capita total non-transfer revenues (Panel B) for an event window of -5 to +10 years around each fiscal rules reform from estimating equation 1.1 on a panel of harmonized cantonal data from 1990-2019. We consider a fiscal rules reform as any positive change in our Fiscal Rules Stringency Index at cantonal level, that is $\Delta FRSIC > 0$. The regression includes cantonal fixed effect (first-difference model), year fixed effect, and a cantonal linear time trend. Control variables are: population share < 15 years, population share > 65 years, foreigners share, unemployment rate, criminality rate, Gini coefficient of net income, number of firms and number of jobs (full-time equivalent) in the secondary and tertiary sectors, share of left-wing seats in the Cantonal Parliament, housing prices index, accounting model and tasks centralization reforms dummies. Standard errors are clustered at canton and year level. Vertical dashed lines report 95% confidence intervals.

1.5 Heterogeneous effects

In this section, we test whether the effect of canton-level fiscal rules on municipality-level public finances differ depending on the presence (or not) of stringent fiscal rules applied to the municipal level.

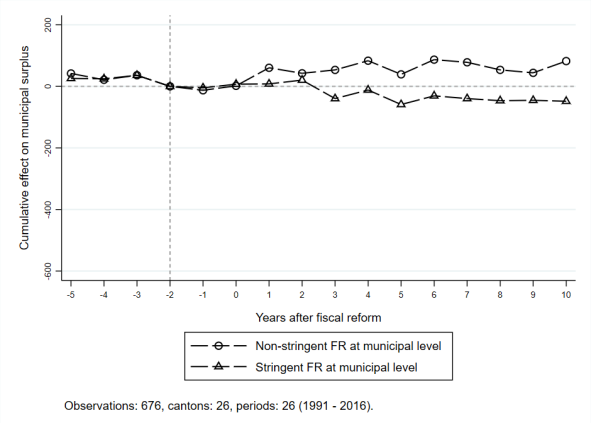
In order to do so, we exploit the FRSI score we compute for the aggregate municipal level, i.e. $FRSIM$. Based on this index value, we split our sample of cantons in two: one group is made of cantons where municipalities were subject to stringent fiscal rules at the time of the fiscal rule reform. The other group is made of cantons in which municipalities were never subject to stringent fiscal rules, or they were later than the canton. We define as stringent a level of $FRSIM$ equal or larger than the median $FRSIM$ score over the period 1970-2019, i.e. $FRSIM \geq 4$.

Results for the two subgroups are reported in Figure 1.9. Graphical evidence suggests that the main effects on municipal expenditures (Panel B), revenues (Panel C), and transfer revenues (Panel D) are stronger if the municipalities are not subject to stringent fiscal rules. Cantons seem more likely to reduce their transfers if municipalities are not subject to stringent fiscal rules.[34] The effect on surplus (Panel A) is very small in magnitude. Heterogeneous effects on the cantonal finances (surplus, expenditures, revenues, transfer expenditures) are reported in Appendix Figure 1.A.11.

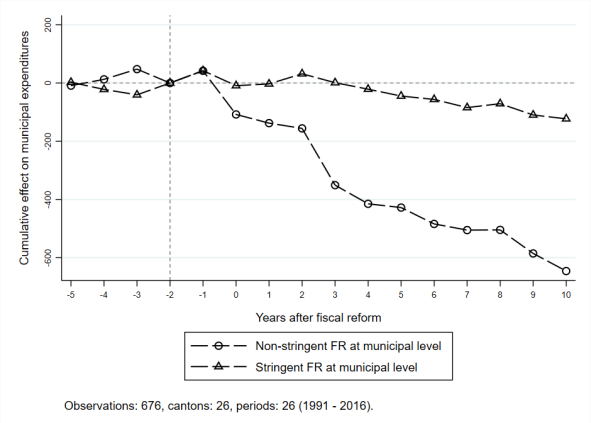
[34]We report here results for the harmonized data because revenues from transfers are not available for the full dataset. Results on the full dataset for expenditures and revenues can be found in Appendix Figure 1.A.10.

Figure 1.9: Municipal expenditures and revenues: heterogeneous effects

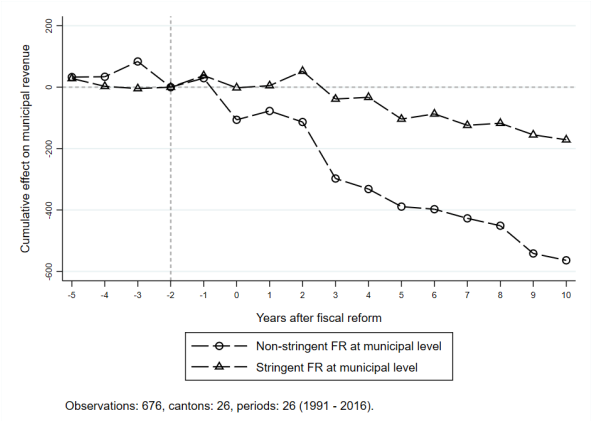
Panel A: Surplus



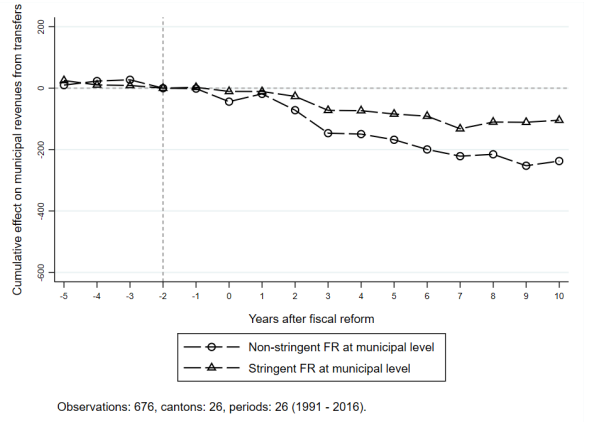
Panel B: Expenditures



Panel C: Revenues



Panel D: Transfer revenues



Notes: Coefficients are retrieved from estimating equation 1.1 interacting $\Delta FRSIM > 0$ with a dummy for the subgroup. The figure shows the average cumulative marginal effect in each subgroup on aggregated municipal per capita variables using a 1990-2019 panel of harmonized cantonal data for an event window of -5 to +10 years around each fiscal rules reform. We consider a fiscal rules reform as any positive change in our Fiscal Rules Stringency Index at cantonal level, that is $\Delta FRSIC > 0$. The regression includes cantonal fixed effect (first-difference model), year fixed effect, and a cantonal linear time trend. Control variables are: population share < 15 years, population share > 65 years, foreigners share, unemployment rate, criminality rate, Gini coefficient of net income, number of firms and number of jobs (full-time equivalent) in the secondary and tertiary sectors, share of left-wing seats in the Cantonal Parliament, housing prices index, accounting model and tasks centralization reforms dummies. Standard errors are clustered at canton and year level. Confidence intervals not reported.

1.6 Conclusion

Sound public finances are a key factor for governments' resilience. The popularity of fiscal constraints in most economies highlights their relevance as regulating mechanisms, not only at the moment of the moment of their introduction, but also in the long term. We look at the dynamic effect of cantonal fiscal rules in Switzerland by exploiting a distributed lag model, broadly confirming the literature results: fiscal rules are effective in improving cantonal finances. Interestingly, cantons benefit from an increase in revenues in the after-reform years. We think this should be an interesting starting point for more in-depth investigation on the reaction of cantonal tax instruments.

Our research is the first to take into consideration the relationship between cantonal and municipal fiscal rules on fiscal outcomes, uncovering indirect effects on aggregate municipal revenues through transfers reduction. This highlights the relevance of the vertical dimension when evaluating policy effects in federal states.

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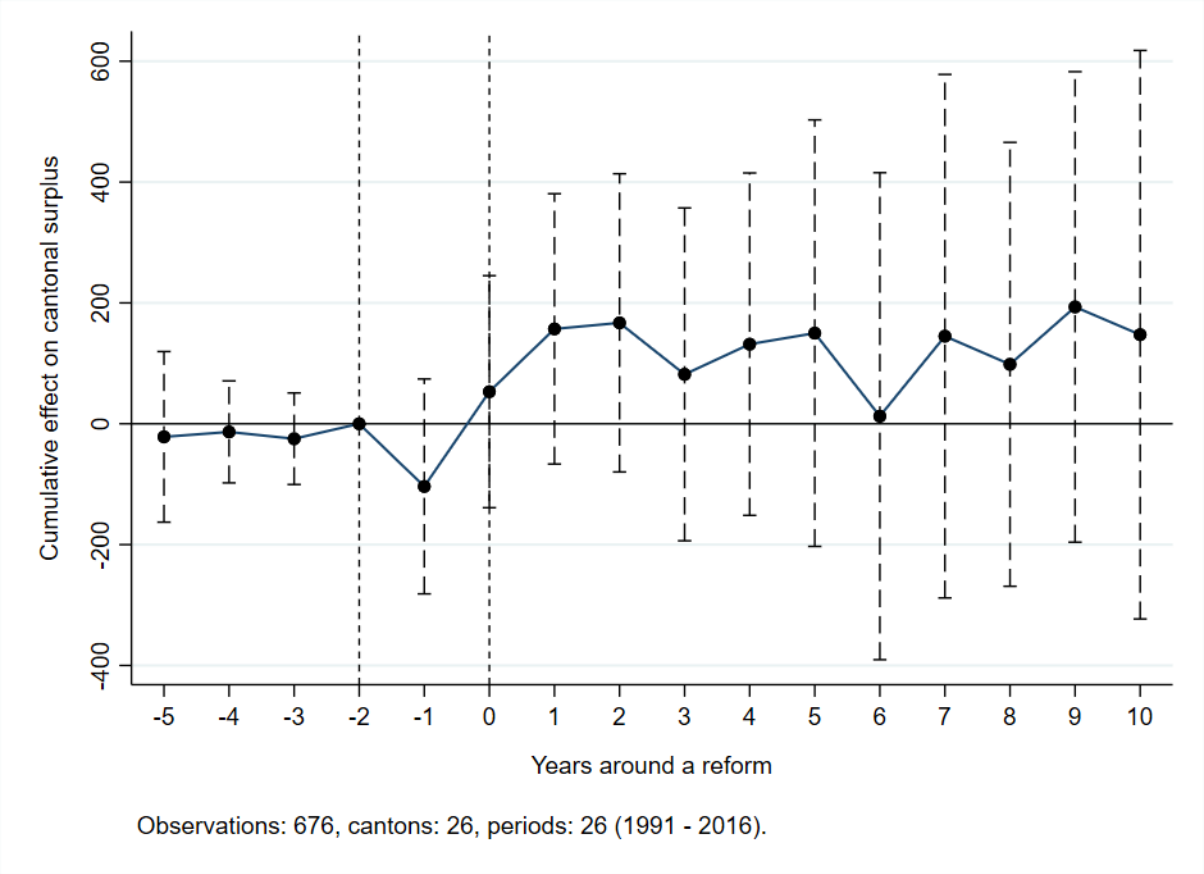
Appendix

1.A Supplementary Figures

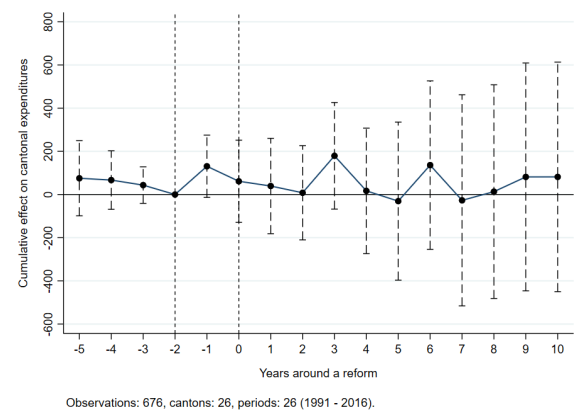
Figure 1.A.1: Cantonal surplus, harmonized data



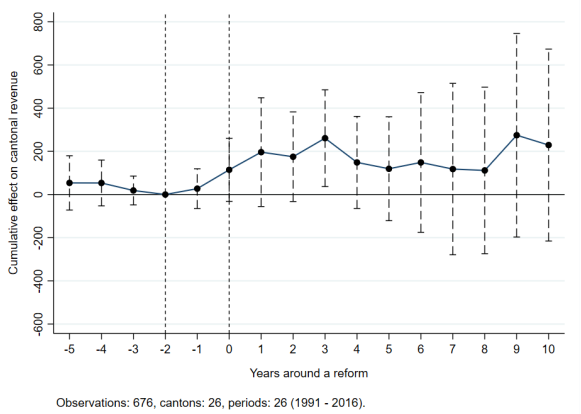
Notes: The figure shows the average cumulative effect on cantonal per capita surplus for an event window of -5 to +10 years around each fiscal rules reform from estimating equation 1.1 on a panel of harmonized cantonal data from 1990-2019. We consider a fiscal rules reform as any positive change in our Fiscal Rules Stringency Index at cantonal level, that is $\Delta FRSIC > 0$. Surplus is defined as total cantonal revenues - total cantonal expenditures. The regression includes cantonal fixed effect (first-difference model), year fixed effect, and a cantonal linear time trend. Control variables are: population share < 15 years, population share > 65 years, foreigners share, unemployment rate, criminality rate, Gini coefficient of net income, number of firms and number of jobs (full-time equivalent) in the secondary and tertiary sectors, share of left-wing seats in the Cantonal Parliament, housing prices index, accounting model and tasks centralization reforms dummies. Standard errors are clustered at canton and year level. Vertical dashed lines report 95% confidence intervals.

Figure 1.A.2: Cantonal expenditures and revenues, harmonized data

Panel A: Expenditures



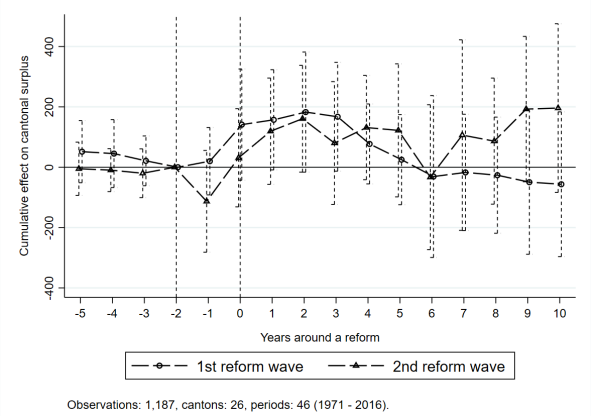
Panel B: Revenues



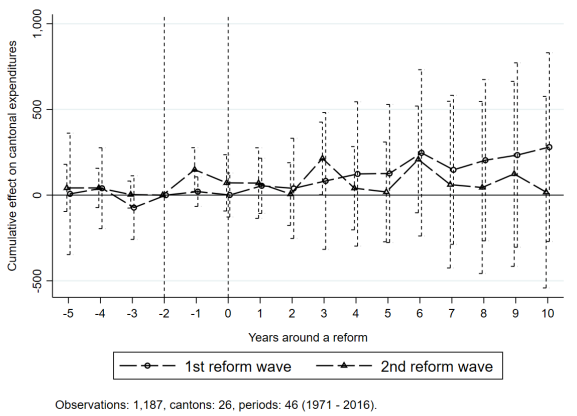
Notes: The figure shows the average cumulative effect on cantonal per capita total expenditures (Panel A) and per capita total revenues (Panel B) for an event window of -5 to +10 years around each fiscal rules reform from estimating equation 1.1 on a panel of harmonized cantonal data from 1990-2019. We consider a fiscal rules reform as any positive change in our Fiscal Rules Stringency Index at cantonal level, that is $\Delta FRSIC > 0$. The regression includes cantonal fixed effect (first-difference model), year fixed effect, and a cantonal linear time trend. Control variables are: population share < 15 years, population share > 65 years, foreigners share, unemployment rate, criminality rate, Gini coefficient of net income, number of firms and number of jobs (full-time equivalent) in the secondary and tertiary sectors, share of left-wing seats in the Cantonal Parliament, housing prices index, accounting model and tasks centralization reforms dummies. Standard errors are clustered at canton and year level. Vertical dashed lines report 95% confidence intervals.

Figure 1.A.3: Cantonal surplus, expenditures and revenues: comparing reform waves

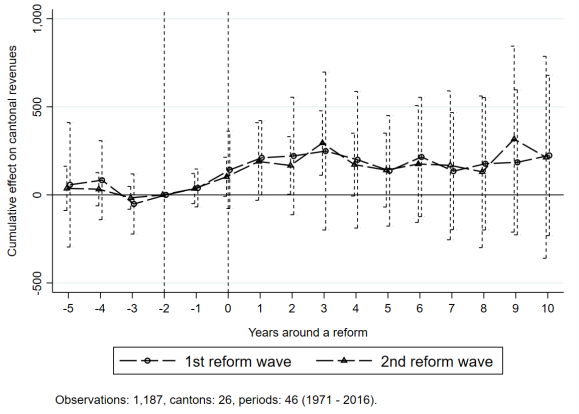
Panel A: Surplus



Panel B: Expenditures

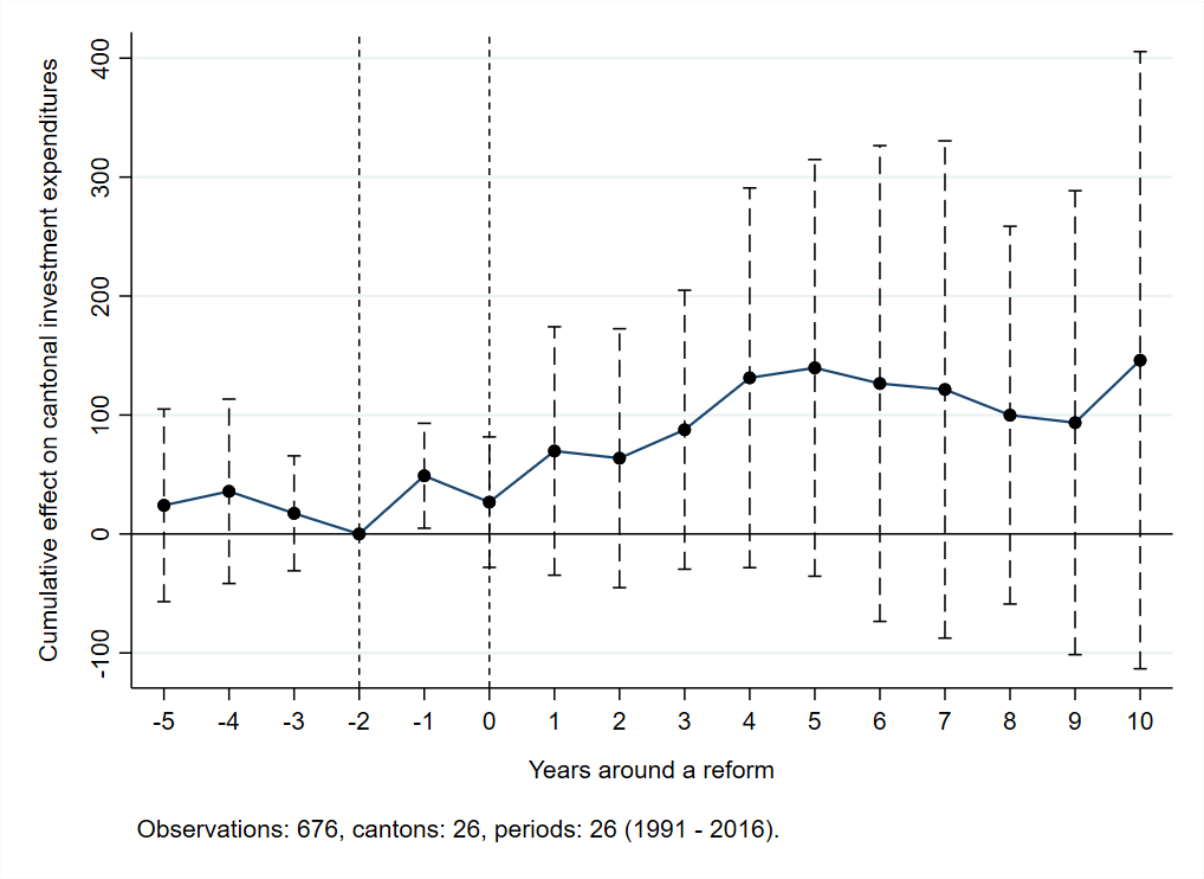


Panel C: Revenues



Notes: The figure shows the average cumulative effect on cantonal per capita surplus (Panel A), expenditures (Panel B), and revenues (Panel C) for an event window of -5 to +10 years around each fiscal rules reform from estimating equation 1.1. We consider a fiscal rules reform as any positive change in our Fiscal Rules Stringency Index at cantonal level, that is $\Delta FRSIC > 0$. We split cantonal reforms in two waves, i.e. those occurring before 1995 are considered as first wave, those occurring on or after 1995 are considered as second wave. Empirically, we create two samples: the first sample ignores any index change occurring on or after 1995 (the index value stays artificially constant starting this year onwards), the second sample ignores any index change occurring before 1995 (the index value stays artificially constant before this year). Year 1995 is selected because halfway between 1970 and 2020, start and end of the panel. Surplus is defined as total cantonal revenues - total cantonal expenditures. The regression includes cantonal fixed effect, year fixed effect, and a cantonal linear time trend. Control variables are: population share < 15 years, population share > 65 years, foreigners share, unemployment rate, criminality rate, Gini coefficient of net income, number of firms and number of jobs (full-time equivalent) in the secondary and tertiary sectors, share of left-wing seats in the Cantonal Parliament, housing prices index, accounting model and tasks centralization reforms dummies. Standard errors are clustered at canton and year level. Vertical dashed lines report 95% confidence intervals.

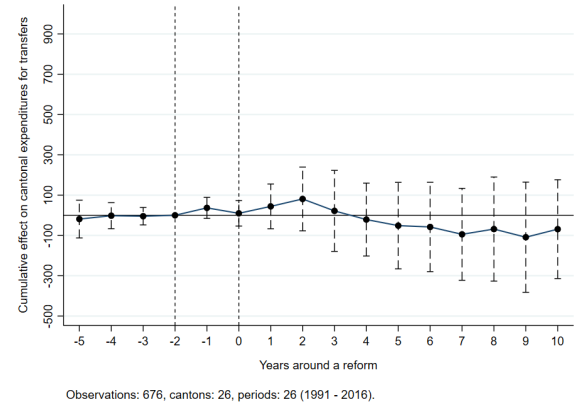
Figure 1.A.4: Cantonal investment expenditures, harmonized data



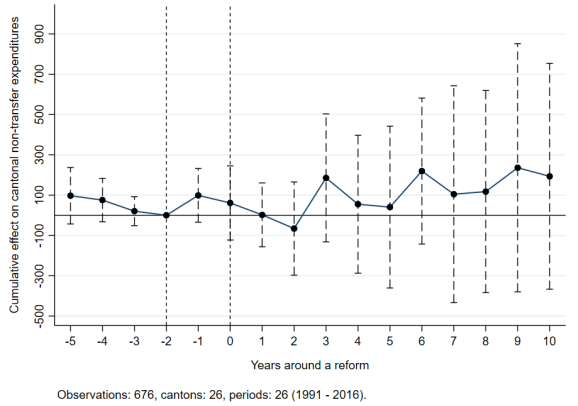
Notes: The figure shows the average cumulative effect on cantonal per capita total investment expenditures for an event window of -5 to +10 years around each fiscal rules reform from estimating equation 1.1 on a panel of harmonized cantonal data from 1990-2019. Yearly cantonal investment expenditures are, on average, CHF 1,200 per capita, or 10% relative to yearly cantonal current expenditures per capita over the same period of time. We consider a fiscal rules reform as any positive change in our Fiscal Rules Stringency Index at cantonal level, that is $\Delta FRSIC > 0$. The regression includes cantonal fixed effect (first-difference model), year fixed effect, and a cantonal linear time trend. Control variables are: population share < 15 years, population share > 65 years, foreigners share, unemployment rate, criminality rate, Gini coefficient of net income, number of firms and number of jobs (full-time equivalent) in the secondary and tertiary sectors, share of left-wing seats in the Cantonal Parliament, housing prices index, accounting model and tasks centralization reforms dummies. Standard errors are clustered at canton and year level. Vertical dashed lines report 95% confidence intervals.

Figure 1.A.5: Cantonal transfer and non-transfer expenditures, harmonized data

Panel A: Transfer expenditures

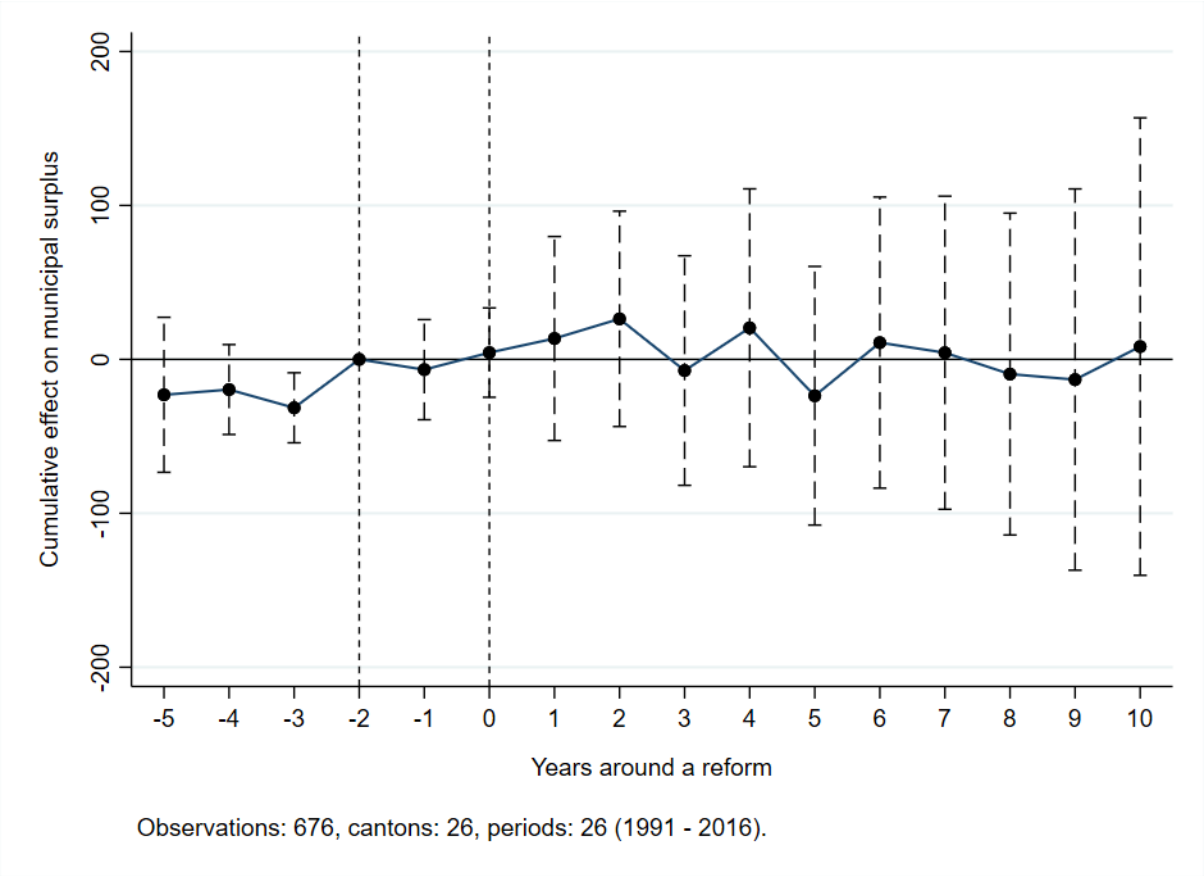


Panel B: Non-transfer expenditures



Notes: The figure shows the average cumulative effect on cantonal per capita total transfer expenditures (Panel A) and per capita total non-transfer expenditures (Panel B) for an event window of -5 to +10 years around each fiscal rules reform from estimating equation 1.1 on a panel of harmonized cantonal data from 1990-2019. We consider a fiscal rules reform as any positive change in our Fiscal Rules Stringency Index at cantonal level, that is $\Delta FRSIC > 0$. The regression includes cantonal fixed effect (first-difference model), year fixed effect, and a cantonal linear time trend. Control variables are: population share < 15 years, population share > 65 years, foreigners share, unemployment rate, criminality rate, Gini coefficient of net income, number of firms and number of jobs (full-time equivalent) in the secondary and tertiary sectors, share of left-wing seats in the Cantonal Parliament, housing prices index, accounting model and tasks centralization reforms dummies. Standard errors are clustered at canton and year level. Vertical dashed lines report 95% confidence intervals.

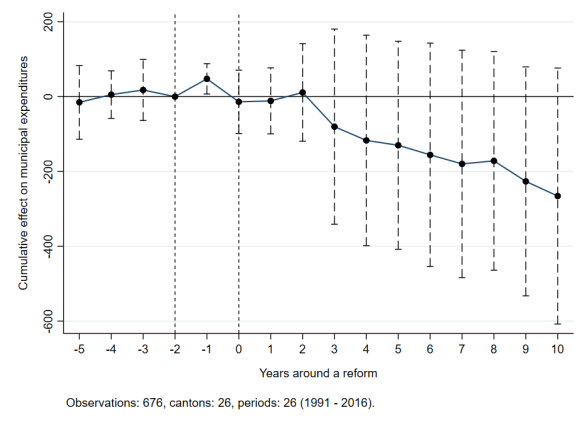
Figure 1.A.6: Municipal surplus, harmonized data



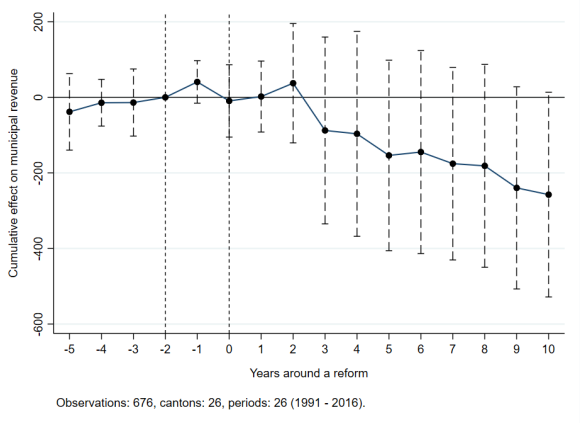
Notes: The figure shows the average cumulative effect on aggregated municipal per capita surplus for an event window of -5 to +10 years around each fiscal rules reform from estimating equation 1.1 on a panel of harmonized cantonal data from 1990-2019. We consider a fiscal rules reform as any positive change in our Fiscal Rules Stringency Index at cantonal level, that is $\Delta FRSIC > 0$. Surplus is defined as total municipal revenues - total municipal expenditures. The regression includes cantonal fixed effect (first-difference model), year fixed effect, and a cantonal linear time trend. Control variables are: population share < 15 years, population share > 65 years, foreigners share, unemployment rate, criminality rate, Gini coefficient of net income, number of firms and number of jobs (full-time equivalent) in the secondary and tertiary sectors, share of left-wing seats in the Cantonal Parliament, housing prices index, accounting model and tasks centralization reforms dummies. Standard errors are clustered at canton and year level. Vertical dashed lines report 95% confidence intervals.

Figure 1.A.7: Municipal expenditures and revenues, harmonized data

Panel A: Expenditures

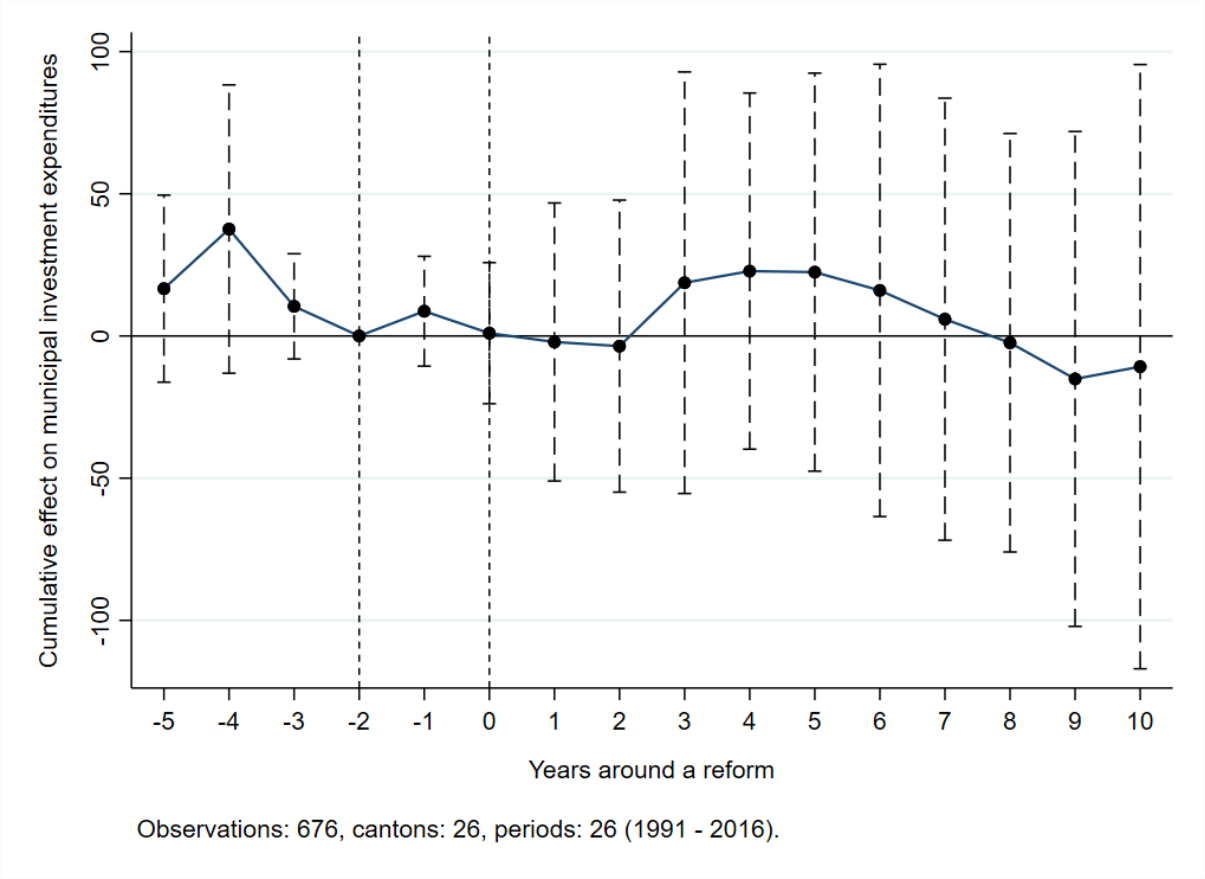


Panel B: Revenues



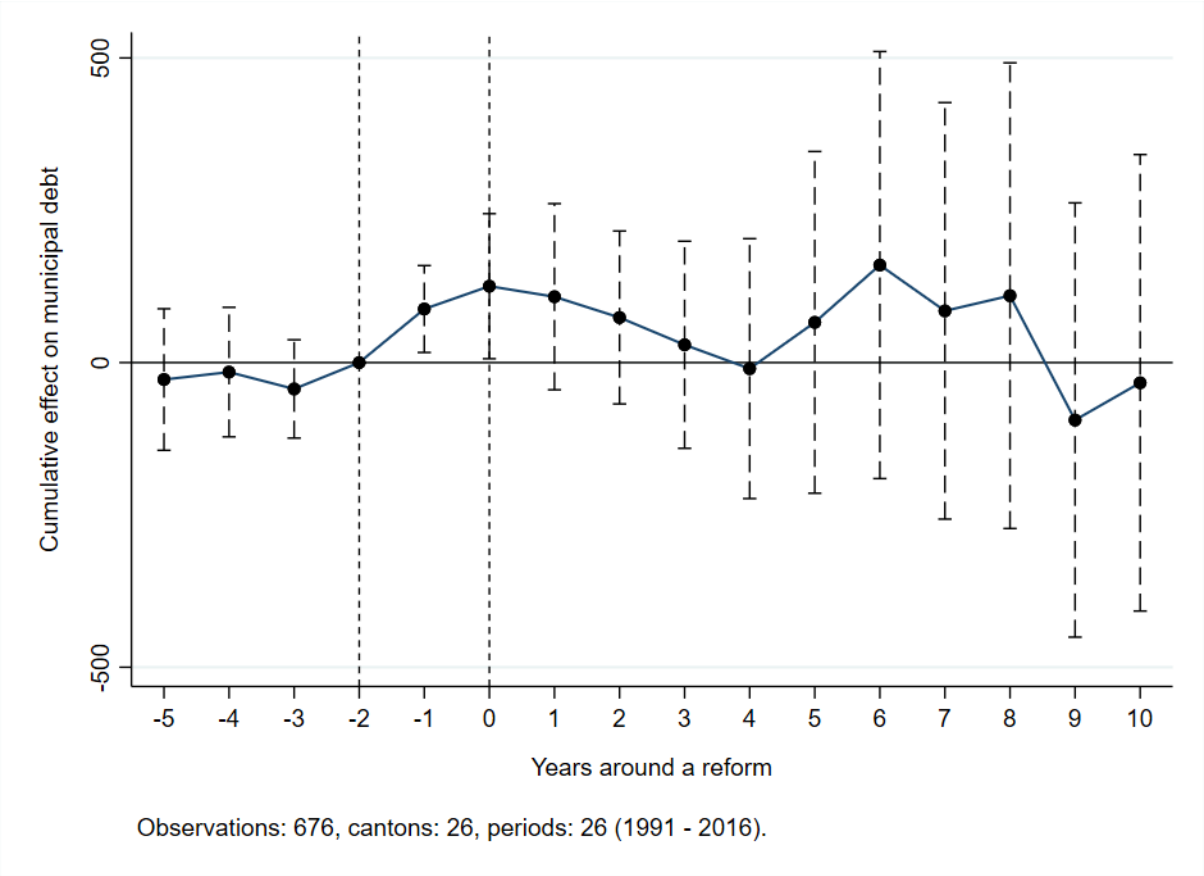
Notes: The figure shows the average cumulative effect on aggregated municipal per capita total expenditures (Panel A) and per capita total revenues (Panel B) for an event window of -5 to +10 years around each fiscal rules reform from estimating equation 1.1 on a panel of harmonized aggregated municipal data from 1990-2019. We consider a fiscal rules reform as any positive change in our Fiscal Rules Stringency Index at cantonal level, that is $\Delta FRSIC > 0$. The regression includes cantonal fixed effect (first-difference model), year fixed effect, and a cantonal linear time trend. Control variables are: population share < 15 years, population share > 65 years, foreigners share, unemployment rate, criminality rate, Gini coefficient of net income, number of firms and number of jobs (full-time equivalent) in the secondary and tertiary sectors, share of left-wing seats in the Cantonal Parliament, housing prices index, accounting model and tasks centralization reforms dummies. Standard errors are clustered at canton and year level. Vertical dashed lines report 95% confidence intervals.

Figure 1.A.8: Municipal investment expenditures, harmonized data



Notes: The figure shows the average cumulative effect on aggregated municipal per capita total investment expenditures for an event window of -5 to +10 years around each fiscal rules reform from estimating equation 1.1 on a panel of harmonized aggregated municipal data from 1990-2019. Yearly municipal investment expenditures are, on average, CHF 800 per capita, or 17% relative to yearly municipal current expenditures per capita over the same period of time. We consider a fiscal rules reform as any positive change in our Fiscal Rules Stringency Index at cantonal level, that is $\Delta FRSIC > 0$. The regression includes cantonal fixed effect (first-difference model), year fixed effect, and a cantonal linear time trend. Control variables are: population share < 15 years, population share > 65 years, foreigners share, unemployment rate, criminality rate, Gini coefficient of net income, number of firms and number of jobs (full-time equivalent) in the secondary and tertiary sectors, share of left-wing seats in the Cantonal Parliament, housing prices index, accounting model and tasks centralization reforms dummies. Standard errors are clustered at canton and year level. Vertical dashed lines report 95% confidence intervals.

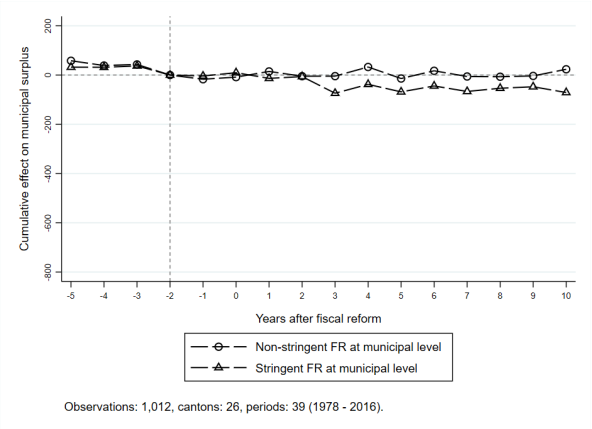
Figure 1.A.9: Municipal debt, harmonized data



Notes: The figure shows the average cumulative effect on aggregated municipal per capita total gross debt using a 1990-2019 panel of harmonized cantonal data for an event window of -5 to +10 years around each fiscal rules reform from estimating equation 1.1. We consider a fiscal rules reform as any positive change in our Fiscal Rules Stringency Index at cantonal level, that is $\Delta FRSIC > 0$. The regression includes cantonal fixed effect (first-difference model), year fixed effect, and a cantonal linear time trend. Control variables are: population share < 15 years, population share > 65 years, foreigners share, unemployment rate, criminality rate, Gini coefficient of net income, number of firms and number of jobs (full-time equivalent) in the secondary and tertiary sectors, share of left-wing seats in the Cantonal Parliament, housing prices index, accounting model and tasks centralization reforms dummies. Standard errors are clustered at canton and year level. Vertical dashed lines report 95% confidence intervals.

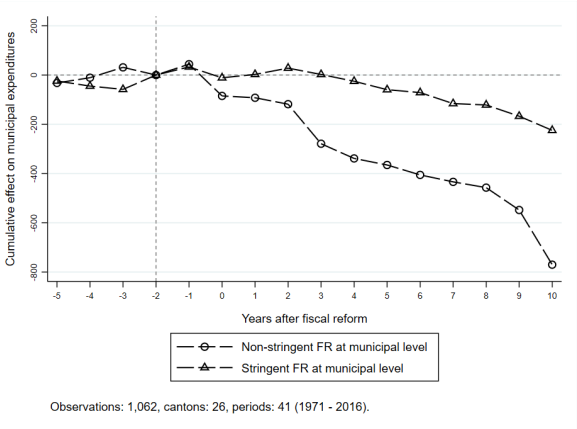
Figure 1.A.10: Municipal finances: heterogeneous effects

Panel A: Surplus



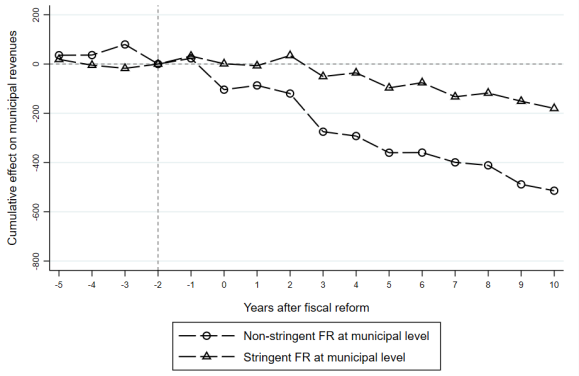
Observations: 1,012, cantons: 26, periods: 39 (1978 - 2016).

Panel B: Expenditures



Observations: 1,062, cantons: 26, periods: 41 (1971 - 2016).

Panel C: Revenues

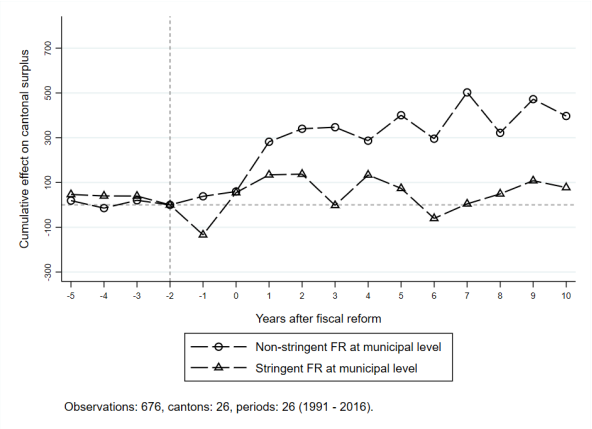


Observations: 1,012, cantons: 26, periods: 39 (1978 - 2016).

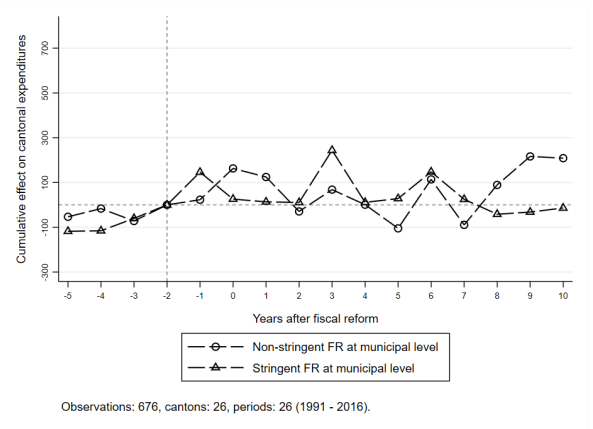
Notes: Coefficients are retrieved from estimating equation 1.1 interacting $\Delta FRSIM > 0$ with a dummy for the subgroup. The figure shows the average cumulative marginal effect in each subgroup on aggregated municipal per capita variables for an event window of -5 to +10 years around each fiscal rules reform. We consider a fiscal rules reform as any positive change in our Fiscal Rules Stringency Index at cantonal level, that is $\Delta FRSIC > 0$. The regression includes cantonal fixed effect (first-difference model), year fixed effect, and a cantonal linear time trend. Control variables are: population share < 15 years, population share > 65 years, foreigners share, unemployment rate, criminality rate, Gini coefficient of net income, number of firms and number of jobs (full-time equivalent) in the secondary and tertiary sectors, share of left-wing seats in the Cantonal Parliament, housing prices index, accounting model and tasks centralization reforms dummies. Standard errors are clustered at canton and year level. Confidence intervals not reported.

Figure 1.A.11: Cantonal finances: heterogeneous effects

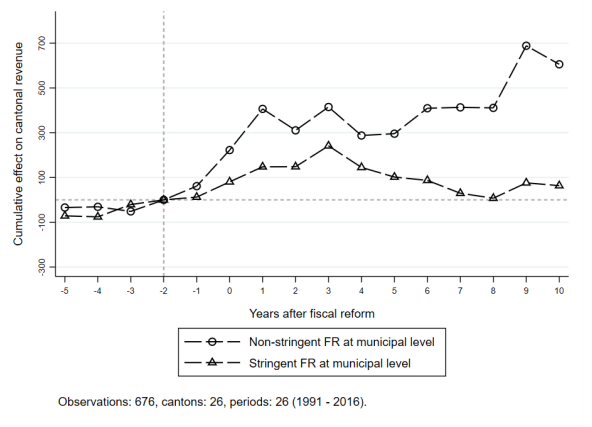
Panel A: Surplus



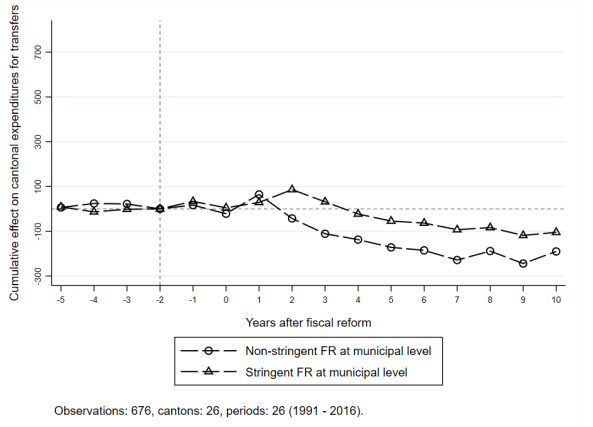
Panel B: Expenditures



Panel C: Revenues



Panel D: Transfer expenditures



Notes: Coefficients are retrieved from estimating equation 1.1 interacting $\Delta FRSIM > 0$ with a dummy for the subgroup. The figure shows the average cumulative marginal effect in each subgroup on cantonal per capita variables using a 1990-2019 panel of harmonized data for an event window of -5 to +10 years around each fiscal rules reform. We consider a fiscal rules reform as any positive change in our Fiscal Rules Stringency Index at cantonal level, that is $\Delta FRSIC > 0$. The regression includes cantonal fixed effect (first-difference model), year fixed effect, and a cantonal linear time trend. Control variables are: population share < 15 years, population share > 65 years, foreigners share, unemployment rate, criminality rate, Gini coefficient of net income, number of firms and number of jobs (full-time equivalent) in the secondary and tertiary sectors, share of left-wing seats in the Cantonal Parliament, housing prices index, accounting model and tasks centralization reforms dummies. Standard errors are clustered at canton and year level. Confidence intervals not reported.

1.B Supplementary Tables

Table 1.B.1: Fiscal reforms in Switzerland - Cantons

Canton	Year	FRSIC before reform	FRSIC after reform	Max age of reform
Zürich	1980	0	1	40
	2008	1	3	12
Bern (BE)	2002	1	3	18
Luzern	1978	0	1	42
	2011	1	3	9
	2016	3	5	4
Uri	1985	0	1	35
	1995	1	2	25
	2019	2	3	1
Schwyz	2016	0	3	4
Obwalden	1988	0	2	32
Nidwalden	1980	0	1	40
	2001	1	3	19
Glarus	1977	0	1	43
	2011	1	2	9
Zug	2007	0	2	13
Fribourg	1961	1	4	59
Solothurn	1988	0	1	32
Basel-Stadt	1998	0	1	22
Basel-Landschaft	1987	0	1	33
Basel-Landschaft	2008	1	2	12
Schaffhausen	1976	0	2	44
Appenzell-Ausserrhoden	1996	0	1	24
Graubünden	1999	1	3	21
Aargau	2005	1	2	15
Thurgau	1961	0	1	59
	2012	1	2	8
Ticino	1987	0	1	33
	2014	1	6	6
Vaud	2003	0	2	17
	2014	2	4	6
Valais	2005	0	3	15
Neuchâtel	1996	0	1	24
	2005	1	4	15
Genève	2006	1	4	14
	2009	4	5	11
Jura	1979	0	1	41
	2001	1	2	19

Notes: The table lists all cantons and all fiscal rules reforms identified by changes in the $FRSIC$. Maximum age of reform is calculated from the year of each cantonal fiscal reform, defined as any year where $\Delta FRSIC > 0$, until and included 2020. We considered as year of fiscal reform the year of entry into force of the legal documents (or specific articles) determining the $FRSIC$. If the entry into force of a legal document (or specific articles) was set on or after July 1st of a given year, then the following year was recorded, the effective duration in the original year being less than 6 months. Based on the historical legal documents analyzed, we identified a $FRSIC > 0$ prior to 1960 in the cantons of Bern, Fribourg, Graubünden, Aargau, St. Gallen.

Table 1.B.2: Fiscal reforms in Switzerland - Municipalities

Canton	Year	FRSIM before reform	FRSIM after reform	Max age of reform
Zürich	1926	0	5	94
Luzern	2005	0	4	15
Zug	1982	0	4	38
Basel-Landschaft	1972	0	4	48
Aargau	1982	0	4	38
Ticino	1951	0	4	69
Valais	1981	0	4	39
Genève	1955	0	7	65

Notes: The table lists all cantons whose municipalities were subject to above-median stringent fiscal rules $FRSIM \geq 4$ before the cantonal fiscal reforms occurred, i.e. where municipalities were "first-movers". If several reforms occur at cantonal level, we compare the introduction date of municipal stringent fiscal rules to the oldest cantonal reform. Maximum age of reform is calculated from the year where $FRSIM \geq 4$, until and included 2020. The control group is composed of cantons that never implemented fiscal reforms at municipal level (Appenzell-Innerrhoden, Basel-Stadt), cantons that never reached a $FRSIM \geq 4$ (Appenzel-Ausserrhoden, Bern, Graubünden, Jura, Neuchâtel, Nidwalden, Solothurn, Schwyz, Thurgau, Uri, Vaud), or cantons where municipalities did implement a $FRSIM \geq 4$ after the first cantonal reform (Fribourg, Glarus, Obwalden, St. Gallen, Schaffhausen). We considered as year of fiscal reform the year of entry into force of the legal documents (or specific articles) determining the $FRSIM$. If the entry into force of a legal document (or specific articles) was set on or after July 1st of a given year, then the following year was recorded, the effective duration in the original year being less than 6 months.

Table 1.B.3: Summary statistics, 1990-2016

	Mean	SD	Min	Max	Obs.
Panel A: cantonal outcome variables					
Debt	6,602	6,009	405	35,529	702
Surplus	-44	548	-4,212	2,215	702
Expenditures	8,618	3,528	3,309	29,906	702
Revenues	8,574	3,556	3,400	29,392	702
Panel B: municipal outcome variables					
Debt	6,100	4,018	584	26,783	702
Surplus	90	212	-903	1,036	702
Expenditures	4,704	1,498	321	8,677	702
Revenues	4,794	1,504	374	8,749	702
Panel C: Fiscal Rules Stringency Index					
Cantonal level	2	1	0	6	702
Municipal level	2	2	0	7	702
Panel D: cantonal covariates					
Population share < 15y (%)	17	2	12	24	702
Population share > 65y (%)	16	2	11	22	702
Foreigners share (%)	19	7	7	41	702
Unemployment rate (%)	3	2	0	8	702
Criminality rate (‰)	3	1	0	9	702
Gini coeff. of net income	44	6	31	66	702
Firms in II sector	3,228	3,110	216	13,486	702
Firms in III sector	13,192	14,937	524	101,294	702
FTE in II sector	41,649	38,927	1,918	207,529	702
FTE in III sector	105,593	133,406	2,783	851,068	702
Share of left-wing seats (%)	25	13	0	53	702
Housing price index	122	24	93	224	702
Panel E: total cantonal population					
Total population	287,927	299,921	13,768	1,487,969	702

Notes: The table provides summary statistics for outcome variables of the harmonized panel of cantons, and all other variables starting from the year 1990 for comparison purposes. All outcome variables are expressed in per capita terms. Municipal outcome variables in Panel B are the aggregate value for all municipalities within a canton. Note that municipal debt is equivalent to the outcome variable reported in Table 1.1. We employ total cantonal debt as measure of municipal debt for the canton of Basel-Stadt.[a] The Gini coefficient was multiplied by 100 for reporting purposes. The housing prices index reflects offer prices, indexed at 100 at year 2000.

[a] See footnote *a.* on page 8

Table 1.B.4: Data aggregation level and sources

	Aggregation level	Source
Cantonal outcome variables		
Debt	Canton	Statistisches Jahrbuch der Schweiz, Öffentliche Finanzen der Schweiz, Swiss Federal Finance Administration
Surplus	Canton	Statistisches Jahrbuch der Schweiz, Öffentliche Finanzen der Schweiz, Swiss Federal Finance Administration
Expenditures	Cantons	SStatistisches Jahrbuch der Schweiz, Öffentliche Finanzen der Schweiz, Swiss Federal Finance Administration
Revenues	Canton	Statistisches Jahrbuch der Schweiz, Öffentliche Finanzen der Schweiz, Swiss Federal Finance Administration
Municipal outcome variables		
Debt	Municipalities (aggregate)	Swiss Federal Finance Administration
item Surplus	Municipalities (aggregate)	Swiss Federal Finance Administration
Expenditures	Municipalities (aggregate)	Swiss Federal Finance Administration
Revenues	Municipalities (aggregate)	Swiss Federal Finance Administration
Fiscal Rules Stringency Index		
Cantonal level	Canton	Own calculation
Municipal level	Canton	Own calculation
Cantonal covariates		
Population share < 15 years (%)	Canton	Swiss Federal Statistical Office
Population share > 65 years (%)	Canton	Swiss Federal Statistical Office
Foreigners share (%)	Canton	Swiss Federal Statistical Office, own calculation
Criminality rate (‰)	Canton	Swiss Federal Statistical Office
Unemployment rate (%)	Canton	State Secretariat of Foreign Affairs (Amstat)
Gini coeff. of net income	Canton	Swiss Federal Tax Administration, extrapolation
Firms in II sector	Canton	Swiss Federal Statistical Office
Firms in III sector	Canton	Swiss Federal Statistical Office
FTE in II sector	Canton	Swiss Federal Statistical Office
FTE in III sector	Canton	Swiss Federal Statistical Office
Share of left-wing seats (%)	Canton	Swiss Federal Statistical Office, own calculation
Housing prices (index)	Canton	Swiss National Bank
Total population and other covariates		
Total population	Canton	Swiss Federal Statistical Office
Harmonized accounting model 1	Canton	Own research
Harmonized accounting model 2	Canton	Swiss Public Sector Financial Reporting Advisory Committee (SRS-CSPCP), own research
Harmonized accounting model 1	Municipalities (aggregate)	Own research
Harmonized accounting model 1	Municipalities (aggregate)	Swiss Public Sector Financial Reporting Advisory Committee (SRS-CSPCP), own research

Chapter 2

Municipalities and Local-level Fiscal Rules: Evidence from Switzerland[1]

Fiscal rules are widely used at central and sub-central administrative levels to strengthen fiscal discipline, but little is known about the effect of these rules when applied to the local level. This paper focuses on the lowest tier of government in Switzerland by evaluating the effect of these rules on local public finance outcomes over the past four decades. I exploit the staggered and exogenous introduction of fiscal rules to control for time-invariant confounding factors, and take into account the reform dynamics in a distributed-lag framework. First, estimation suggests that more stringent fiscal rules decrease per capita expenditures by 8% and per capita revenues by 8.3% in the long-term, de facto restricting government's size. Second, failing to control for the dynamic effects leads to a substantial effect underestimation for both expenditures and revenues. Third, the effect is heterogeneous and largest in jurisdictions with above-average population.

JEL classification: H62, H72
Keywords: Fiscal rules, Deficits

[1]I thank conference participants at the Swiss Workshop on Local Public Finance and Regional Economics 2019 in Lugano for the fruitful discussions, as well as the Swiss Federal Finance Administration for inviting me to present this work at their Brown Bag seminars in June 2019. I am very grateful to the cantonal administrations for their support during data collection. I acknowledge funding from the Swiss National Science Foundation (Grant No. 159348) through my supervisor Prof. Parchet Raphaël.

2.1 Introduction

Are fiscal institutions and decentralized tax and spending authority a powerful combination to constrain fiscal policy and foster economic stability within a country? The introduction of sub-national fiscal rules in Australia, Belgium, Czech Republic, Denmark, Finland, France, Germany, Greece, Italy, Japan, Korea, Norway, Portugal, Spain, Switzerland and Turkey (Sutherland et al., 2018) suggests a positive answer. Yet, the evidence on the effects of fiscal rules on surplus has been mixed.[2]

This chapter investigates the effects of fiscal rules on fiscal performance at the municipality level in Switzerland. The empirical analysis of both national and sub-national fiscal rules typically requires either exploiting rare quasi-experiments or designing an identification strategy that can address endogeneity concerns. The advantage of my setting is threefold. First, the introduction of fiscal rules at the local level is taken by cantons. The decision is therefore exogenous to the municipalities.[3] Second, different set of rules with different intensities have been implemented in a staggered fashion among municipalities (cantons), giving rise to the identifying variation I will exploit in this paper. Third, Swiss municipalities have a substantial degree of autonomy over both expenditure allocation and revenue collection, such that fiscal rules have a binding effect on the decisions of municipalities.

Empirically, I exploit the introduction of different balanced budget requirements at the municipality level in Switzerland on municipal surplus[4], expenditures and revenues over the last four decades (1984-2019). My empirical strategy is based on a distributed lag model to determine the dynamic effect of fiscal rules on municipal fiscal performance, taking advantage of the different timing of fiscal rules reforms to control for time-invariant municipality-specific confounding factors as well as time-varying common factors. The main results indicate that more stringent fiscal rules at the municipal level may reduce surplus by CHF 22 per capita in the long term. Both expenditures and revenues per capita

[2] Grembi et al. (2016) shows with a difference-in-discontinuity model that relaxing fiscal restraint for Italian municipalities below 5,000 inhabitants increased municipal deficit by about EUR 20 per person and lowered taxes. Foremny (2014) analyzes regional and local fiscal restraints for EU-15 countries over the 1995-2008 period and shows that the effectiveness of sub-national fiscal rules is conditional on the constitutional structure of a country. Interestingly, deficits are reduced by fiscal rules in unitary countries only.

[3] As mentioned in the first chapter: prior to the rules' introduction, municipalities may be consulted by the cantons. In addition, we cannot rule out the presence of municipal mayors in cantonal parliaments, i.e. in the political institution voting on municipal fiscal rules either. However, with an average of around 80 municipalities per canton, the timing of implementation remains exogenous to the vast majority of local jurisdictions.

[4] Surplus is computed as revenues - expenditures.

seem to be negatively affected: I estimate a reduction in expenditure of CHF 427 per capita against a reduction in revenues of CHF 457 per capita, de facto restricting the size of the government. Failing to control for the dynamics of the effects, for example by exploiting simple difference-in-differences, induces a clear underestimation of the effect on both expenditures and revenues. A heterogeneity test suggests that the reforms' effect is predominantly driven by large municipalities.

Chapter 1 of this Thesis focused on *canton-level* fiscal rules and their direct and indirect effects on municipal *aggregate* fiscal measures. I showed that the effect is heterogeneous depending on the fiscal rules applied to the local level. In this chapter, I focus on *municipal-level* fiscal rules instead, and shed light on their impact on municipal *own* outcomes. To the best of my knowledge, this research is the first that combines a measure of local fiscal rules stringency to municipal fiscal outcome on a Swiss-wide set of existing municipalities. This chapter contributes to the small literature investigating the effects of fiscal institutions on local public finances outcomes. Grembi et al. (2016) use policy reforms at local level in Italy as a quasi-experimental design to investigate the effect of relaxing fiscal rules on the budget of municipalities. They find that relaxing fiscal rules increased the deficits of Italian municipalities. Christofzik and Kessing (2018) exploit a temporary relaxation of fiscal control on municipalities in the German state of North Rhine-Westphalia, and show that municipalities used this time window to substantially increase debt.

Switzerland has been the research field of various studies on the effects of fiscal rules and other institutions, such as the fiscal referendum, that aim at strengthening the fiscal discipline of sub-federal governments. Feld and Kirchgässner (2001b) construct an index of stringency of fiscal rules at the cantonal level and find that more stringent fiscal rules are associated with less public debt and deficits. This negative relationship is confirmed in a later study by Feld and Kirchgässner (2008) for cantonal deficits and for aggregated cantonal and local deficits. Krogstrup and Wälti (2008) also find that cantonal fiscal rules have a significant impact on real budget balances of the same administrative level, even when voter preferences are controlled for. Feld et al. (2010) find, however, no effect of cantonal fiscal rules on cantonal revenues. Chatagny and Soguel (2012) investigate how cantonal fiscal deficits depend on budgeting errors and show that underestimating tax revenue is associated with lower deficits. Yerly (2013) proposes an alternative index for the stringency of fiscal rules based on Dafflon (2002). She finds that more stringent cantonal rules reduce indebtedness but have no effect on the balance of current accounts. Burret and Feld (2018a) investigate the effects of cantonal debt brakes on public deficits and debt, showing that fis-

cal rules are beneficial for deficit and debt. In particular, their effect is proportional to how well the rule targets the studied variable. The local level has been analyzed in Burret and Feld (2018b) by considering the vertical effect of cantonal fiscal rules on municipal fiscal outcomes, showing the existence of a positive spillover effect. Despite the growing interest, this paper is one of the very few exploiting concrete measures of stringency of fiscal rules at local level. To the best of my knowledge, only Feld and Kirchgässner (1999, 2001a), building upon von Hagen (1992), have constructed an index of the stringency of budgetary procedures for the 134[5] largest municipalities in Switzerland. Their cross-sectional analysis shows that fiscal institutions such as formal fiscal restraints have no effect on local public debt, whereas political institutions, in particular the existence of fiscal referendum, are negatively correlated with debt. Feld et al. (2011) repeat their analysis for another year and confirm only partially the results.[6] Their empirical analysis is however limited by the use of cross-sectional variation among a small sample of municipalities, exposing their results to omitted variable bias.

Section 2.2 discusses fiscal rules at municipal level in Switzerland. The dataset is explained in Section 2.3, while Section 2.4 discusses the identification strategy, reports baseline results and a heterogeneity test. Section 2.5 concludes.

2.2 Fiscal rules at local level

Budget regulation at local level is widely used in Switzerland as well as in many other countries (e.g. Argentina, Austria, Brazil, Estonia, Germany, Indonesia Italy, Poland, Spain, United States). Local-level regulation pursues similar targets as upper government level regulation: expenditures stabilization and debt sustainability. As of 2020, 25 out of 26 cantons in Switzerland have a set of fiscal rules for their municipalities. Early examples of fiscal rules' introduction[7] date back to 1926 with the canton of Zurich, followed by St. Gallen in 1948, Ticino in 1951 and Fribourg in 1955.[8] Most cantons have adopted municipal finance regulation's principles in the '80s and '90, while Obwalden and Lucerne did so in 2004 and 2005, respectively. The only canton that has no fiscal rules for the local level (nor for the

[5] 131 in the 1999 paper.

[6] They also describe in more details municipality-level fiscal rules for some cantons, but do not include this information in their empirical analysis.

[7] Fiscal rules' introduction and reforms are identified thanks to the $FRSIM$ score.

[8] $FRSIM$ scores' time series starts in 1950. However, older fiscal rules' introduction years can be retrieved thanks to the entry-into-force date of historical legal documents analyzed.

cantonal level) is Appenzell Innerrhoden, while the cantons of Neuchâtel and Uri have experienced two regulation changes each.
As anticipated in Chapter 1, the set of rules for the municipal level are usually included in the so-called "Gemeindegesetz" in German, or similar[9], i.e. the law concerning the municipal level. The cantonal law articles are included in the "Finanzhaushaltsgesetz" instead, i.e. the law about the cantonal financial management, and sometimes they are complemented with constitutional articles stating the guiding principles of the fiscal rules' set. In principle, each municipality has also the autonomy to set further rules on top of the canton-wide rules[10], but according to my research these are very few and they are assumed not to be a confounding factor for the estimation.[11]

Municipal regulation typically requires balancing the income statement in the medium term. Few cantons (OW, UR) go one step further and state explicitly the medium term duration (6-10 years). A popular component is also the request of planning corrective measures if the medium-term balance principle cannot be fulfilled (Appenzell-Ausserrhoden, Bern, Fribourg, Jura, Neuchâtel, Valais). Two cantons require an automatic tax adjustment as a compensation measure. Interestingly, these cantons have the same requirement also for the cantonal level (Fribourg, St. Gallen).

2.3 Dataset

This paper exploits the longest and most comprehensive dataset on public finances of Swiss municipalities to my knowledge. Data collection took place between 2010 and 2016 by the members of the project "The Swiss Confederation: A Natural Laboratory for Research on Fiscal and Political Decentralization", funded by the Swiss National Science Foundation (Sinergia programme).[12] The project, supported by the cantonal administrations, delivered a very rich panel on municipal finances, mainly covering expenditures, revenues and their functional breakdown up to the year 2012.[13] The extension of the municipal data

[9] "Loi sur les communes" in French, "Legge organica comunale" in Italian, or similar.
[10] This was confirmed by each cantonal administration.
[11] Municipalities with own fiscal rules were identified in the cantons of Basel-Land (Binningen), Graubünden (Domat/Ems), Lucerne (city of Lucerne, Schlierbach), Neuchâtel (Val-de-Travers, Val-de-Ruz), Schaffhausen (Hallau), Solothurn (city of Solothurn, Holderbank), list not exhaustive.
[12] Some of these data are accessible via the project website www.fiscalfederalism.ch.
[13] Balance sheet and investment account items are unfortunately scarce. No historical data could be retrieved for the canton of Jura, nor Appenzell Innerrhoden. Output of the project are also historical series on personal income and wealth tax rates and tax bases, corporate tax rates and taxable income and can-

of interest until the last available year was my first step to carry out this research project.[14] Dependent variables are retrieved from the end-of-year statements of each municipality and represent totals based on current account items (no investment items)[15].

This project is also the first to exploit a comprehensive stringency score for fiscal rules at municipal level in Switzerland. The index design has been described in Chapter 1, section 1.3.1. As mentioned, all municipalities within a canton are subject to a common set of fiscal rules that varies over time, and this is the measure I exploit empirically. The advantage, is that the decision on the rules' implementation is taken by the cantonal parliaments and can therefore been seen as exogenous from the point of view of the individual municipality.

For empirical purposes, I will consider all fiscal rules reforms occurring between 1974 and 2020, to allow the inclusion of a sufficient number of leads and lags, see section 2.4.1. A fiscal rules reform is, in analogy to chapter 1, defined as any positive change in the fiscal rules' stringency index score, $\Delta FRSIM > 0$.[16] The full list of municipal reforms exploited in the estimation is reported in Appendix Table 2.B.1.[17]

The dataset is completed with a set of covariates for the period 1984 to 2019, see summary statistics in Table 2.3.1 and the data sources in Appendix Table 2.B.5. I first include several socio-economic variables at municipal level, in particular the share of population aged < 20 years, the share of population over 65, the share of foreign population and the unemployment rate, to control for different population features that are likely to be reflected in the local public finance level and development. In addition, I include tax-base (and local revenue-determining) controls: the median net income and the Gini coefficient of net income distribution. The political orientation of the municipality is controlled for

tonal public finances. Information on income and wealth tax bases is available as early as of the fiscal year 1947/1948, while most of the other variables start in the '60/'70s.

[14] Online availability has sharply increased only in the second half of 2010s. A comprehensive collection would have never been possible without the collaboration of the cantonal administrations that kindly followed up on my requests for data.

[15] It is important to acknowledge that the lack of harmonization among them and across years mechanically reduces the comparability power.

[16] Index score decreases are excluded from the analysis. The number of occurrences is extremely low: Geneva (1985), Valais (2004), and Zurich (1986). As I will explain later, the canton of Valais is completely dropped from the sample due to data inconsistencies.

[17] Note that there exists a permanent control group of 4 cantons that never implemented a fiscal rules reform in the observed period (Graubünden, Solothurn, Schwyz, Vaud). This table differs from table 1.B.2 in Chapter 1, where only municipal reforms leading to a $FRSI \geq 4$ occurring before cantonal reforms were reported, i.e., subsequent time variation at local level was not relevant. For example, the following reforms mentioned in table 1.B.2 are not exploited in Chapter 2, because the variation occurred prior to the first observation year: Zürich (1926), Ticino (1951), Genève (1955), Basel-Landschaft (1972). The canton of Valais is dropped for data inconsistency reasons, as mentioned earlier.

by including the share of votes for left and for right-wing parties at the Federal Parliament elections.[18] Location competitiveness for mobile high-income taxpayers is taken into consideration with the cantonal effective personal income tax rate, in addition to economic attraction potential, measured with cantonal statutory corporate capital and profits tax rates. Based on the results from Chapter 1, the vector of control variables is completed with a full set of 10 lags of cantonal-level fiscal rules stringency score, total per capita cantonal expenditures and total per capita cantonal revenues. The purpose is to model adjustments at the cantonal level that can vertically influence the municipal level.[19] Last but not least, dummy variables for the cantonal accounting systems in place at cantonal and municipal level[20] and for task centralization reforms (Flèche, 2021) are included.[21]

[18]The two categories are not mutually exclusive. The excluded category is the share of votes for center parties. Parties are assigned according to the Manifesto Project Database for Switzerland https://manifesto-project.wzb.eu/, which is based on the parties' electoral manifestos content analysis.

[19]Leads are not included based on the absence of pre-trends from the results in Chapter 1.

[20]Harmonized Accounting Model 1 (HAM1) or Harmonized Accounting Model 2 (HAM2). I do collect information on pilot municipalities implementing HAM2 in each canton, therefore the dummy variable for the harmonized accounting model is time-varying and specific for each municipality.

[21]See Appendix Table A1 of Flèche (2021). The author reports all years with known reforms of tasks distribution between cantonal and municipal levels, occurring in 19 out of 26 Swiss Cantons between 2000 and 2014. She considered the years when each reform was effectively implemented. On average, local decentralization decreased by 6 percentage points because of these reforms. For this reason, the author defines them as "centralization" reforms in her paper.

Table 2.3.1: Summary statistics

	Mean	SD	Min	Max	Obs.
Panel A: municipal outcome variables					
Surplus	101	710	-29,629	40,018	55,980
Expenditures	5,401	5,391	772	160,827	56,166
Revenues	5,502	5,442	126	163,193	56,132
Panel B: Fiscal rules stringency index					
Municipality level	3	2	0	7	59,486
Cantonal level	2	1	0	6	59,486
Panel C: municipal covariates					
Population share < 20y (%)	23	4	0	44	58,907
Foreigners share (%)	13	9	0	62	59,452
Population share ≥ 65y (%)	15	5	2	89	58,907
Unemployment rate (%)	2	1	0	20	59,002
Median net income	54,891	12,379	3,900	132,500	59,468
Gini coeff. of net income	35	8	2	96	59,468
Share of left-wing seats (%)	23	10	0	67	59,291
Share of right-wing seats (%)	71	12	0	100	59,291
Panel D: cantonal covariates					
Expenditures	8,376	2,730	3,052	29,237	59,289
Revenues	8,380	2,649	3,077	19,779	59,289
Statutory corporate capital tax rate (‰)	1	1	0	4	59,443
Statutory corporate profits tax rate (%)	8	3	2	15	59,443
Personal income tax rate (%)	6	2	2	12	59,133
Panel E: total municipal population					
Total population	3,512	11,910	9	420,217	59,449

Notes: The table reports summary statistics for dependent variables and covariates over the period 1984-2019. Excluded cantons: Basel-Stadt (city-canton), Appenzell-Innerrhoden and Nidwalden (missing data), Glarus (in 2011 full restructuring of the canton-municipalities relationship, including fiscal rules and task redistribution, with the consolidation from 25 to 3 municipalities), Valais (data consistency issues). The total number of municipalities in the sample is 2,063. The cantonal personal income tax rate is computed for a representative married person without children in top 10% of the national income distribution. Total population is measured by the number of permanent residents as of 31.12 each year, i.e. Swiss nationals with main residence in the country, and foreign nationals with a residence permit (permanent/non-permanent) since at least 12 months. The full panel of FRSI values runs form 1950 to 2020, the historical $FRSIM$ median score being 2.

2.4 Empirical and identification strategy

I study the impact of municipal-level fiscal rules on municipal finances with an event-study design that exploits multiple events of heterogeneous sizes. This setting is the same as the one presented in chapter 1.

2.4.1 Estimating equation

I apply the distributed lag model (Schmidheiny and Siegloch, 2020) as follows:

$$\Delta y_{mt} = \sum_{j=-5}^{10} \gamma_j \Delta FRSIM_{c,t-j} + \sum_{k=-5}^{10} \gamma_k \Delta FRSIC_{c,t-k} + \beta' \Delta \mathbf{X_{mt}} + \lambda_t + \Delta \epsilon_{mt} \tag{2.1}$$

where Δy_{mt} is the first difference of a public finance variable (surplus, expenditures or revenues, all in per capita terms) for municipality m and year t. The model supports multiple events of different intensities, measured by the magnitude of $\Delta FRSIM$ for each municipality (canton). From the first chapter, we know that cantonal fiscal rules have an indirect effect on municipal finances. Based on this result, I include also control for the set of leads and lags of the cantonal fiscal rules stringency index, $FRSIC$. This distributed lag model is identical to an event study design with binning of the first and the last event indicator (Schmiedheiny and Siegloch, 2020). Results report the cumulative effect of a change in $FRSIM$, i.e. $\sum_{j}^{\tau} \gamma_j$ re-parametrized to be expressed relative to $t-2$ that serves as the reference year.[22]

The growing popularity of this empirical method is owed to the transparent graphical visualization of its estimates. Figures will report the cumulative effect β_j, computed as the running sum of the γ_j parameters starting from the reference year, each year before and after the reform.[23]

$$\beta_j = \begin{cases} -\sum_{\delta=j+1}^{-2} \gamma_h & \text{if } -5 \leq j \leq -3 \\ 0 & \text{if } j = -2 \\ \sum_{h=-1}^{j} \gamma_h & \text{if } -1 \leq j \leq 10 \end{cases}$$

[22] Appendix Figure 2.A.3 includes main results using -1 as reference year.

[23] Cumulated estimates in the pre-treatment period are computed by cumulating away from the reference period, which explains the negative sign.

The vector **X** includes covariates at municipal and cantonal level as described in section 2.3. The model corrects for unobservable time-invariant factors at municipal level by first-differencing all variables, in addition to a year fixed effect λ_t to take into account any nationwide shocks occurring during the years. Identification comes from the heterogeneity in fiscal rules reforms' sizes and timing. As already mentioned, the estimation of their effect is less prone to endogeneity concerns compared to the cantonal level. Unobserved factors that might influence treatment assignment and outcome variables at the same time are less unlikely to contaminate the estimation, because all municipalities within a given canton are assigned the same treatment at the same time by the upper government level (no self-assignment to treatment). Finally, clustering of the standard errors is at canton and year level. The additional estimation tables reported in the Appendix report standard errors and p-values with clustering at cantonal and year level as well as at municipal and year level, mainly for transparency reasons. We look at individual municipalities, but the reforms are in common within each canton, the error correlation unit is a priori not so clear-cut.[24]

I observe the dependent variables for the period 1984 to 2019 and the treatment status $FRSIM$ starting from 1974, so I do not lose any observations at the start of the panel due to the introduction of the 10 lags. The inclusion of 5 leads reduces the number of observations in the estimation instead. Since I observe the treatment only up to the year 2020, the last year of dependent variable exploited in the estimation is 2016.[25] The first year of the panel, 1984, is not exploited in the estimation because of the first-difference approach.

2.4.2 Main results

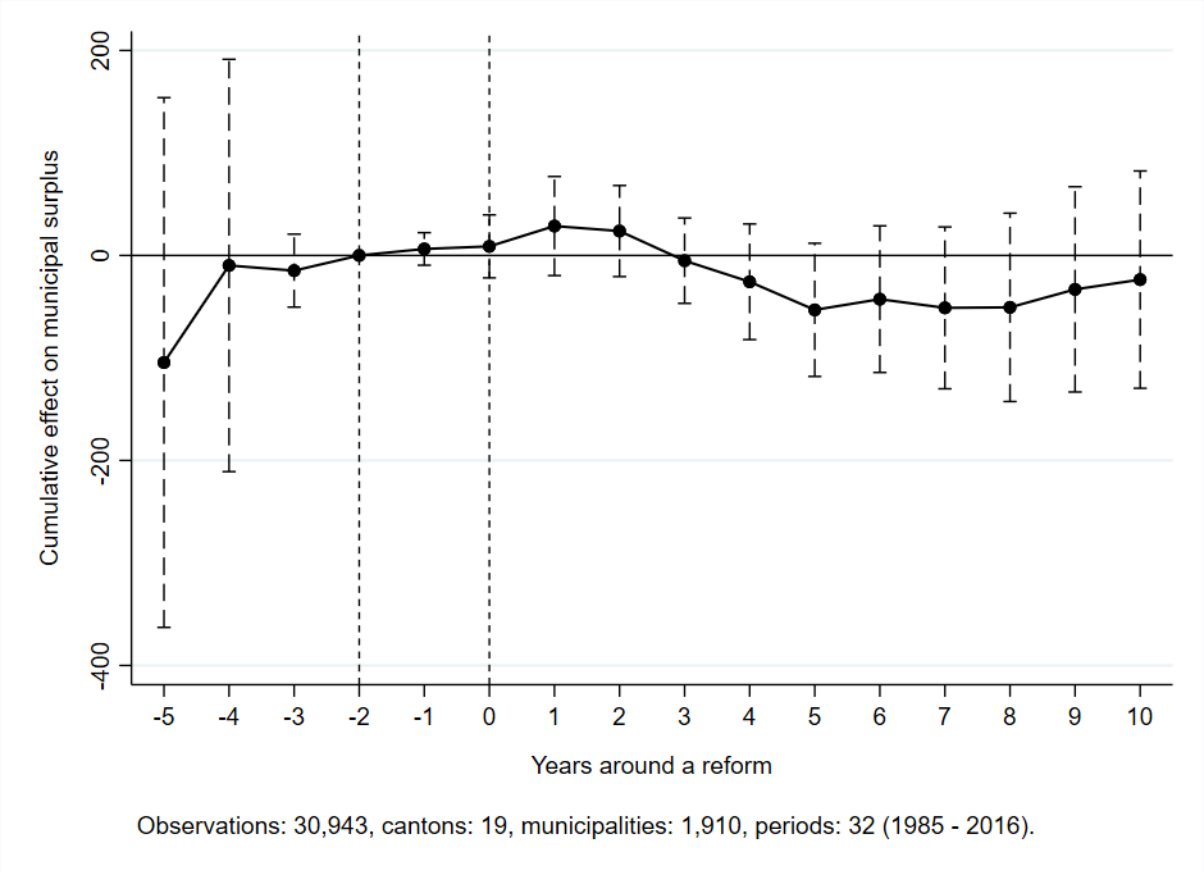
Figure 2.4.1 shows the cumulated dynamic effect of fiscal rules for the municipal level on the municipal surplus. Each data point is interpreted as the cumulated effect of a fiscal rules reform. The comparison is performed for each municipality with itself before against after the reform, as well as against the not-yet-treated municipalities and against the permanent control municipalities. Overall, surplus at municipal level seems to follow

[24] Not reported event-study graphs with clustering of the standard errors at municipality and year level shows minimal difference compared to the clustering at canton and year level.

[25] The distributed lag model is a re-parametrization version of the event study, with binned endpoints. As reported in Remark 5 of Schmiedheiny and Siegloch (2020): "[...] distributed-lag coefficients measure treatment effect changes, such that one fewer lead has to be estimated [compared to the conventional event-study approach]". Since I observe the $FRSIM$ value only up to the year 2020, the last year of dependent variable exploited in the estimation is 2016, i.e. 2020-5 leads + 1.

a slightly downward trend starting in year 1 after a reform, and to stabilize to a lower level going further. Statistical precision is not remarkable, but there is no presence of pre-trends. In the long term, municipal surplus may suffer a reduction by roughly CHF 22 per capita. Compared to a mean surplus of around CHF 100 per capita, a reduction of the surplus of CHF 22 per capita would not lead to a deficit.

Figure 2.4.1: Municipal surplus

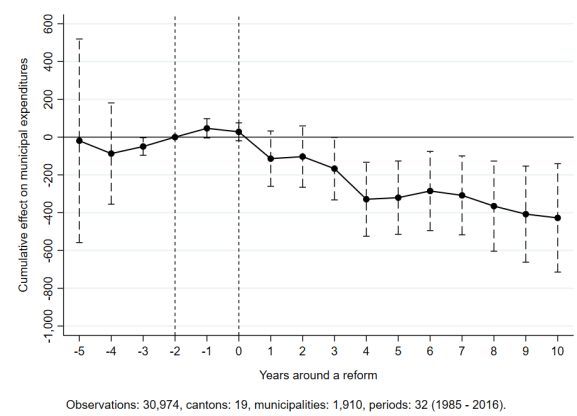


Notes: The figure shows the average cumulative effect on municipal per capita surplus for an event window of -5 to +10 years around each fiscal rules reform at municipal level. Data points represent the cumulative sum of the γ_j from equation 2.1. A fiscal rules reforms is identified by any (positive) change in our Fiscal Rules Stringency Index at municipal level, i.e. $\Delta FRSIM > 0$. Surplus is computed as total municipal revenues - total municipal expenditures. The estimation includes municipality and year fixed effect, municipal covariates (share of population < 20 years, share of population ≥ 65 years, share of foreign population, unemployment rate, median net income, Gini coefficient of net income, share of votes for left parties and for right parties at national parliament elections) and cantonal covariates (statutory tax rate on firms' capital, statutory tax rate on firms' profit, tax rate for a representative married person without children in top 10% of the national income distribution, accounting model dummies, centralization reforms dummy). Standard errors are clustered at canton and year level. Vertical dashed lines report 95% confidence intervals.

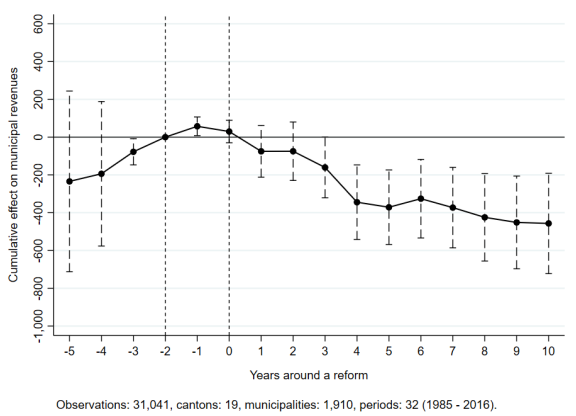
The null effect on surplus is explained by a parallel reduction of both expenditures and revenues, by CHF 427 per person and CHF 457 per person in the long term, respectively. Both are comparable to an 8% reduction in per capita expenditure and to an 8.3% reduction in per capita revenues, computed against the mean values over the period. Interestingly, the reduction is realized in 2 steps, of about equal magnitude: the first step is one year after, the second around 4 years after a fiscal rules reform. Overall, results on municipal surplus, expenditure and revenue suggest that municipality-level fiscal rules do not change municipal surplus but lead to a reduction in the size of the local government. Note that, for expenditure and revenue, I cannot rule out with certainty that treated and non-treated units do not differ prior to the reform, which would invalidate the parallel trend assumption and the causal interpretation of the effect. Differences in pre-trends are however not statistically significant. If anything, it suggests that municipalities with more stringent fiscal rules where the one with an increasing size of government (both revenue and expenditure) and that fiscal rules stopped and reverted this trend.

Figure 2.4.2: Municipal expenditures and revenues

Panel A: Expenditures



Panel B: Revenues



Notes: The figure shows the average cumulative effect on municipal per capita total expenditures (Panel A) and per capita total revenues (Panel B) for an event window of -5 to +10 years around each fiscal rules reform at municipal level. Data points represent the cumulative sum of the γ_j from equation 2.1. A fiscal rules reforms is identified by any (positive) change in the Fiscal Rules Stringency Index at municipal level, i.e. $\Delta FRSIM > 0$. The estimation includes municipality and year fixed effect, municipal covariates (share of population < 20 years, share of population ≥ 65 years, share of foreign population, unemployment rate, median net income, Gini coefficient of net income, share of votes for left parties and for right parties at national parliament elections) and cantonal covariates (statutory tax rate on firms' capital, statutory tax rate on firms' profit, tax rate for a representative married person without children in top 10% of the national income distribution, accounting model dummies, centralization reforms dummy). Standard errors are clustered at canton and year level. Vertical dashed lines report 95% confidence intervals.

To test the long-term estimates against the predictions of static models, I compare in Table 2.4.1 the effects of fiscal rules on municipal surplus, expenditures and revenues for three specifications. Specification (1) is a static model (no leads nor lags) with only municipality and year fixed effects. Specifications (2) and (3) add the vector of controls and canton-level financial variables ($FRSIC$, total per capita expenditures and revenues), respectively. Specification (4) reports my preferred estimate, which includes a full set of leads and lags of the reform timing to control for dynamics. This specification provides the long-term estimate, equivalent to the results reported graphically. Here is the interesting insight: if I were to estimate the reform's effects with a standard two-way fixed effects without consideration of the dynamics, as predominant in the literature (specification (3)), I would be heavily underestimating the effect on expenditures and revenues. As an example: a conventional regression would (imprecisely) estimate a reduction in expenditures by about CHF 11 per person, in revenues by about CHF 37 per person. They both represent only 2.6% resp. 8% of the long-run effect I estimate in my baseline results (specification (4)).

Table 2.4.1: Effect of municipal fiscal rules reforms on local public finances

	Surplus per capita				Expenditures per capita				Revenues per capita			
	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
$\hat{\gamma}$	-5.39	-7.04	-15.38		-17.96	-17.31	-11.09		-30.03	-30.73	-36.75	
	(9.64)	(10.51)	(18.74)		(20.29)	(25.62)	(29.00)		(20.08)	(29.62)	(41.18)	
	[0.5825]	[0.5106]	[0.4220]		[0.3867]	[0.5070]	[0.7065]		[0.1505]	[0.3120]	[0.3834]	
	(9.11)	(6.84)	(18.74)		(6.21)	(11.07)	(29.00)		(7.44)	(10.96)	(41.18)	
	[0.5584]	[0.3101]	[0.2823]		[0.0066]	[0.1271]	[0.6265]		[0.0003]	[0.0083]	[0.1492]	
Long-term effect ($\sum_{j=-1}^{10} \hat{\gamma}_j$)				-21.61				-427.48				-456.87
				(50.78)				(146.43)				(135.57)
				[0.6755]				[0.0092]				[0.0034]
				(44.64)				(124.75)				(130.18)
				[0.6318]				[0.0017]				[0.0014]
YearFE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
MunFE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Controls	NO	YES	YES	YES	NO	YES	YES	YES	NO	YES	YES	YES
Cantonal variables	NO	NO	YES	YES	NO	NO	YES	YES	NO	NO	YES	YES
μ	104	104	130	32	5,434	5,394	5,470	5,378	5,538	5,502	5,601	5,408
N	53,582	52,519	36,953	30,943	53,706	52,643	36,984	30,974	53,764	52,697	37,051	31,041
R^2	0.02	0.03	0.14	0.01	0.01	0.01	0.01	0.02	0.01	0.01	0.03	0.02
Start	1985	1985	1985	1985	1985	1985	1985	1985	1985	1985	1985	1985
End	2019	2019	2019	2016	2019	2019	2019	2016	2019	2019	2019	2016

Notes: Specifications (1) to (3) report estimates from running a conventional two-way fixed effect equation on surplus, expenditures and revenues (all in per capita terms), with stepwise augmentation:

$$\Delta y_{mt} = \gamma \Delta FRSIM_{ct} + \beta' \Delta \mathbf{X_{mt}} + \sum_{j=-5}^{10} \delta_j \Delta V_{c,t-j} + \lambda_t + \Delta \epsilon_{mt} \tag{2.3}$$

Each coefficient represents the average effect on the municipal outcome variable from fiscal rules reforms at local level. A fiscal rules reforms r is identified by any (positive) change in our Fiscal Rules Stringency Index at municipal level, i.e. by $\Delta FRSIM > 0$. Specification (1) includes only municipality and year FE. Specification (2) is augmented with the vector $\mathbf{X_{mt}}$ with the usual covariates (share of population < 20 years, share of population ≥ 65 years, share of foreign population, unemployment rate, median net income, Gini coefficient of net income, share of votes for left parties and for right parties at national parliament elections) and cantonal covariates (statutory tax rate on firms' capital, statutory tax rate on firms' profit, tax rate for a representative married person without children in top 10% of the national income distribution, accounting model dummies, centralization reforms dummy). Specification (3) is further augmented with the full set of 10 lags of the following cantonal variables: $FRSIC$, total cantonal expenditures and revenues per capita, $\sum_{j=-5}^{10} \delta_j \Delta \mathbf{V_{c,t-j}}$, $\mathbf{V}$ being a vector. Specification (4) is the preferred specification and is the equivalent to the distributed lag model in the event-study graphs, i.e. the full equation 2.3 is run with $\Delta FRSIM_{ct}$ replaced by the full set of leads and lags, i.e. by $\sum_{j=-5}^{10} \gamma_j \Delta FRSIM_{c,t-j}$. The long-term effect is computed by summing the coefficients for $t-1$, t, and all lags (such that the pre-event period is $t-2$ and further leads, as in the event-study graphs). Standard errors clustered at canton and year level in brackets at line 2, p-values in squared brackets at line 3 (line 7 for specification (4)). Standard errors clustered at municipality and year level in brackets at line 4, p-values in squared brackets at line 5 (line 8 for specification (4)).

To test robustness of the results, I repeat the estimation of the dynamic model but substituting the $FRSIM$ with an indicator variable which takes the value of 1 from the year when a municipality starts being subject to a median or above-median stringent fiscal rules, i.e. $FRSIM \geq 2$. Results are reported in Appendix Figures 2.A.1 for surplus and 2.A.2 for ex-

penditures and revenues. The graphical analysis indeed confirms the main results.

Second, as per model construction, any effect is assumed to be constant more than 5 years prior to the reform and more than 10 years after. To test whether the event window is appropriate, I estimate the same distributed lag model with a municipality fixed-effect (demeaning all variables) and plot the estimates against the first-difference estimates, see Appendix Figures 2.A.4 and 2.A.5. The two sets of estimates are very similar, with few exceptions in the pre-event period. According to Schmidheiny and Siegloch (2020) a small deviation between the two sets denotes a correct modelling of the dynamics. If the effect were to unfold after the last event indicator considered, then the two estimation sets would diverge because the fixed effect estimator would pick up part of the delayed effect.

Last, to complete the analysis, I report in Appendix Table 2.B.2 the long-term estimates for the set of all 10 functional expenditure categories. I find precisely estimated reductions in public administration expenditures, education, and social security. The significant increase in healthcare expenditures may be related to a secular increase in this sector's costs that my model cannot capture. Appendix Table 2.B.3 includes the long-term effect of fiscal rules on municipal tax revenues, municipal multiplier, municipal tax rate and non-tax revenues. Results indicate a decrease in non-tax revenues, but also a decrease in the municipal tax rate, suggesting that local governments decreased their tax burden after the introduction of more stringent fiscal rules. Additional research could be focused on understanding more precisely what drives the effect of fiscal rules on local governments' size.

2.4.3 Heterogeneous effects

Are large and small municipalities affected in the same way by the fiscal rules reforms? In order words, does the *size* of the municipality matter in this regard? Municipalities of different sizes face different production functions and productive efficiency (Balaguer-Coll et al., 2007). In turn, this may impact the effectiveness of fiscal rules, but the direction of the effect is a priori unknown. To investigate this question, I split the sample of municipalities into two, comparing the 75% of the smallest municipalities to the quartile of largest municipalities[26] and run estimating equation 2.1 for each subgroup independently. Results for surplus are reported in Figure 2.4.3 while Figure 2.4.4 reports the heterogeneous effect for expenditures (Panel A) and revenues (Panel B). The effect of fiscal rules on municipal sur-

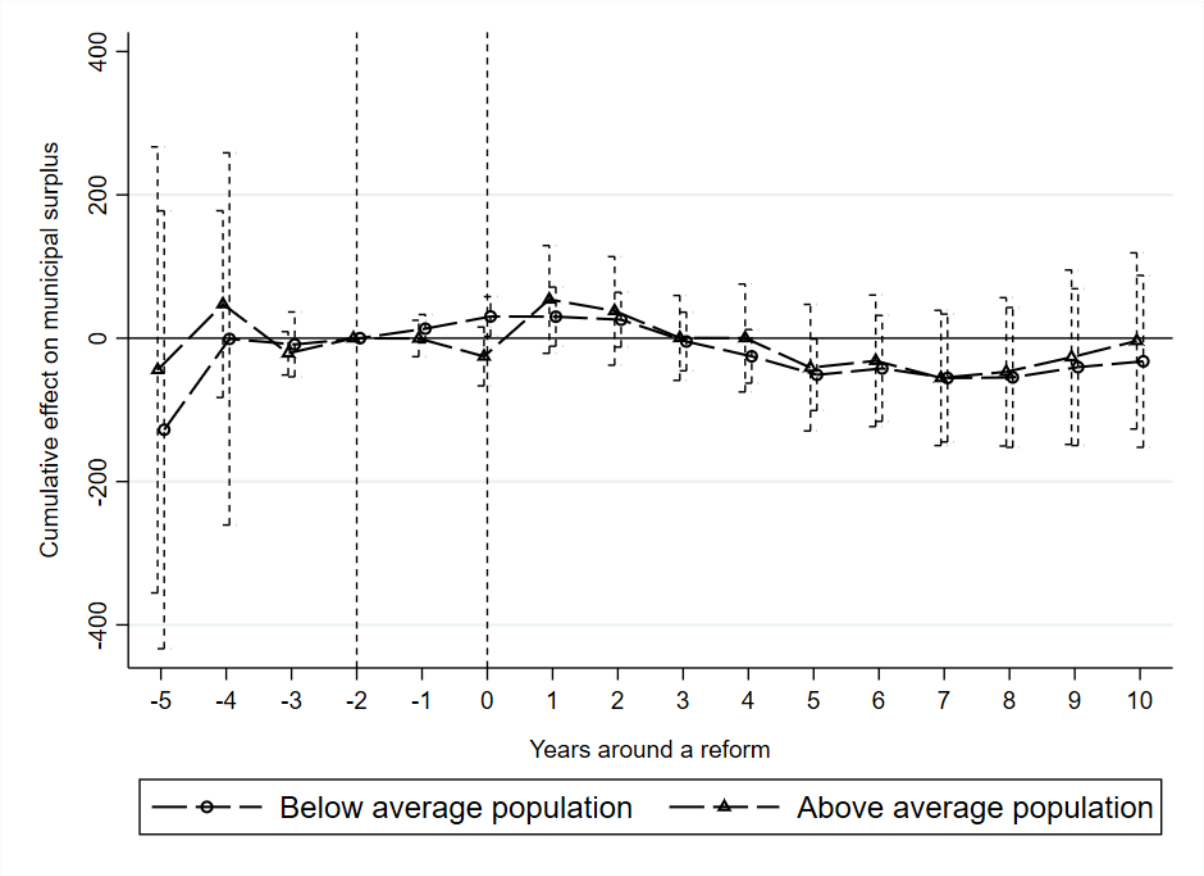
[26]The population threshold is 3,308 inhabitants. It is computed on total municipal population as of the year 2000 and is time-invariant.

plus seems quite homogeneous across the two groups. However, the effect on expenditure and on revenue is larger, the larger is the municipality. Hence, fiscal rules reforms affect municipalities differently depending on their size, but those most strongly impacted are home to the vast majority of the population residing in Switzerland. Appendix Table 2.B.4 reports differences in means and medians for outcome variables and regressors. Interestingly enough, above-median population municipalities have both larger expenditures and revenues per capita, and cantons with large cities have also, on average, a more stringent set of fiscal rules compared to cantons with rather small municipalities. Therefore, more stringent fiscal rules seem to create a convergence toward a more similar level of per capita expenditures and revenues across the two groups of municipalities.[27] For the interested reader, I also report in Appendix Figure 2.A.6 estimation results with an alternative definition of city/non-city. I consider as "city" the 128 major municipalities in Switzerland, an approach similar to Feld et al. (2011).[28]

[27] Exploiting an indicator variable for the fiscal rules or the index itself does not alter the conclusion (Figures not reported).

[28] Selected municipalities are the current members of the Swiss Cities Association, https://staedteverband.ch/de/info/stadteverband/mitglieder. They have at least 5,000 inhabitants each and have a "city" character.

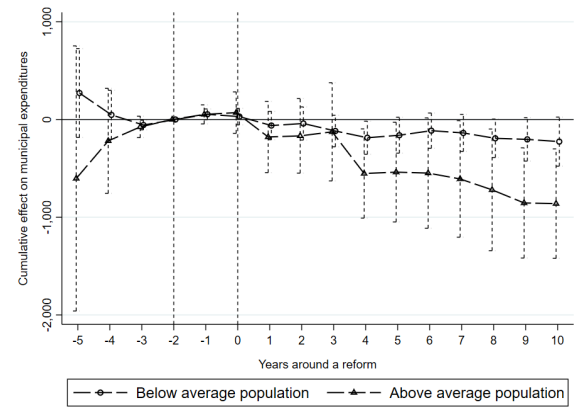
Figure 2.4.3: Municipal surplus - heterogeneous effects by size



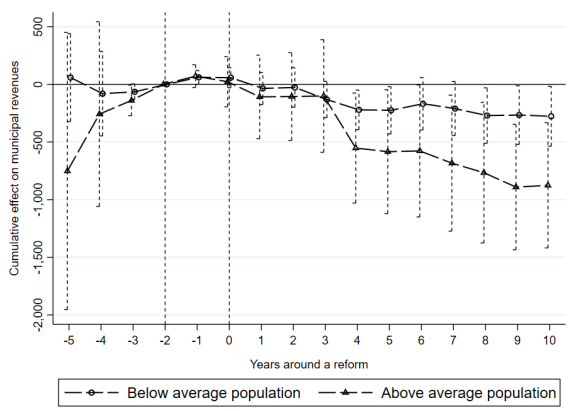
Notes: The figure shows the average cumulative effect on municipal per capita surplus for an event window of -5 to +10 years around each fiscal rules reform at municipal level. Data points represent the cumulative sum of the γ_j from equation 2.1, run on each population subgroup independently. A fiscal rules reforms is identified by any (positive) change in our Fiscal Rules Stringency Index at municipal level, i.e. by $\Delta FRSIM > 0$. Surplus is computed as total municipal revenues - total municipal expenditures. The estimation includes municipality and year fixed effect, municipal covariates (share of population < 20 years, share of population ≥ 65 years, share of foreign population, unemployment rate, median net income, Gini coefficient of net income, share of votes for left parties and for right parties at national parliament elections) and cantonal covariates (statutory tax rate on firms' capital, statutory tax rate on firms' profit, tax rate for a representative married person without children in top 10% of the national income distribution, accounting model dummies, centralization reforms dummy). Standard errors are clustered at canton and year level. Vertical dashed lines report 95% confidence intervals.

Figure 2.4.4: Municipal expenditures and revenues - heterogeneous effects by size

Panel A: Expenditures



Panel B: Revenues



Notes: The figure shows the average cumulative effect on municipal per capita total expenditures (Panel A) and per capita total revenues (Panel B) for an event window of -5 to +10 years around each fiscal rules reform at municipal level. Data points represent the cumulative sum of the γ_j from equation 2.1, run on each population subgroup independently. A fiscal rules reforms is identified by any (positive) change in our Fiscal Rules Stringency Index at municipal level, i.e. by $\Delta FRSIM > 0$. The estimation includes municipality and year fixed effect, municipal covariates (share of population < 20 years, share of population ≥ 65 years, share of foreign population, unemployment rate, median net income, Gini coefficient of net income, share of votes for left parties and for right parties at national parliament elections) and cantonal covariates (statutory tax rate on firms' capital, statutory tax rate on firms' profit, tax rate for a representative married person without children in top 10% of the national income distribution, accounting model dummies, centralization reforms dummy). Standard errors are clustered at canton and year level. Vertical dashed lines report 95% confidence intervals.

2.5 Conclusion

In a federal state, the performance of public finances starts from the bottom. Fiscal rules are widely enforced by sub-central governments to foster fiscal discipline at the local level. In this paper, I rely on a distributed-lag model to show that municipal finances' regulation constrains government size by reducing both per capita expenditures and revenues, but not significantly affecting surplus. The effect is strongest in large-sized municipalities.
The empirical evidence provides answers to a research question, but answers raise new research questions: after all, why were FR at municipal implemented in the first place? Are stringent fiscal rules reforms a (cheaper) moral hazard prevention rather than a cure? Treated and control municipalities did not differ before the reforms, regardless of their size. Summary statistics suggest that, on average, municipalities' finances were already balanced.
I see two options for the unexpected results. First option, there may be a mis-identification issue due to other reforms with a confounding effect, e.g. transfers reduction after cantonal fiscal rules (Chapter 1), reforms of tasks allocation between cantons and municipalities, and general accounting principles that are implemented together with accounting system. They are modeled as covariates, maybe this is not sufficient to disentangle the true effect of fiscal rules. Second option, let's take results at face value from a political-economy perspective. Elected conservative politicians may exploit the fiscal rules imposed from the canton as a spurious argument to cut the size of the government by reducing the municipal multiplier. Testing this option requires a more precise measure than the political orientation of the municipality as a whole, for example the political affiliation of the mayor. This is left for future research.

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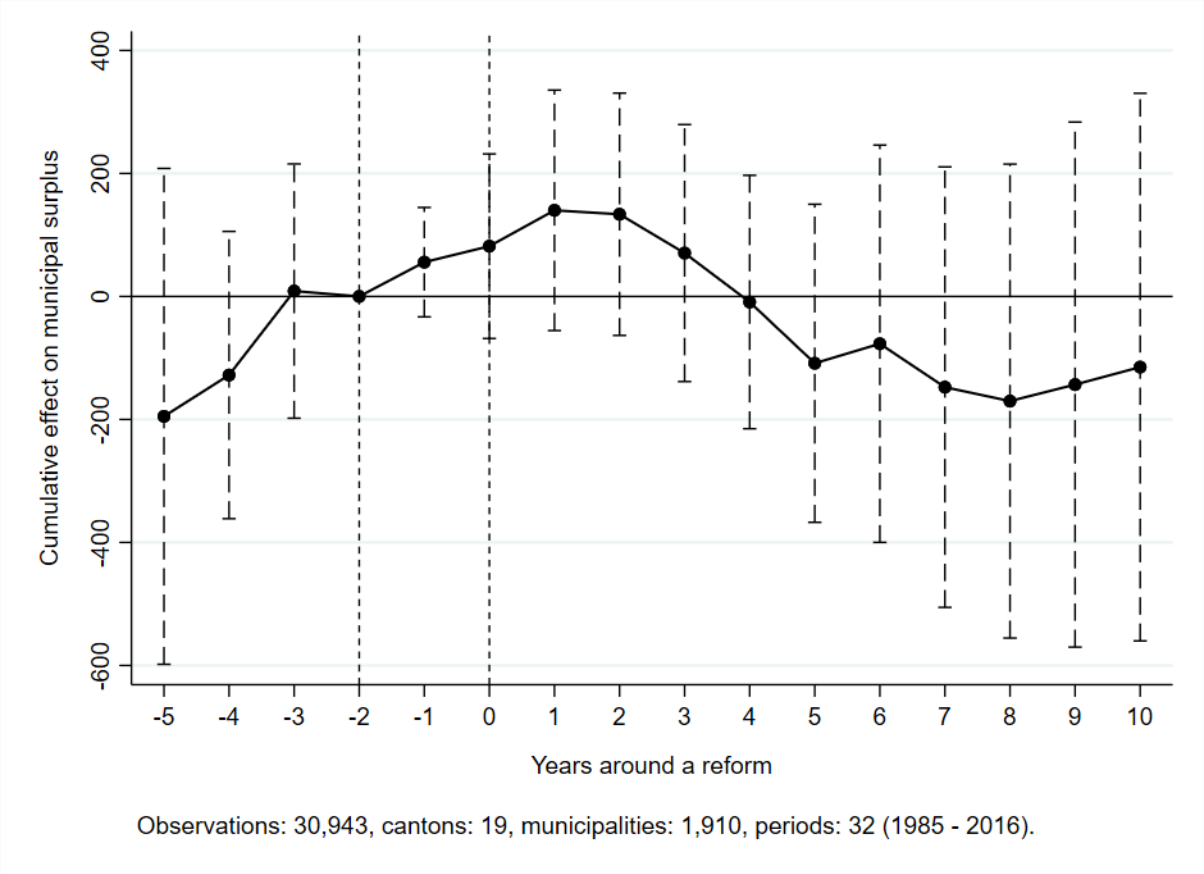
Appendix

2.A Robustness checks

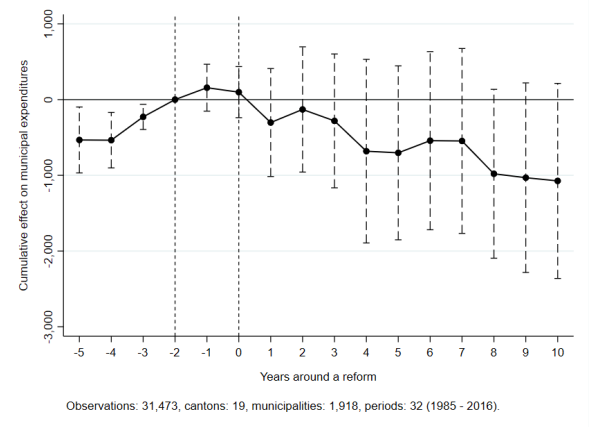
Figure 2.A.1: Municipal surplus



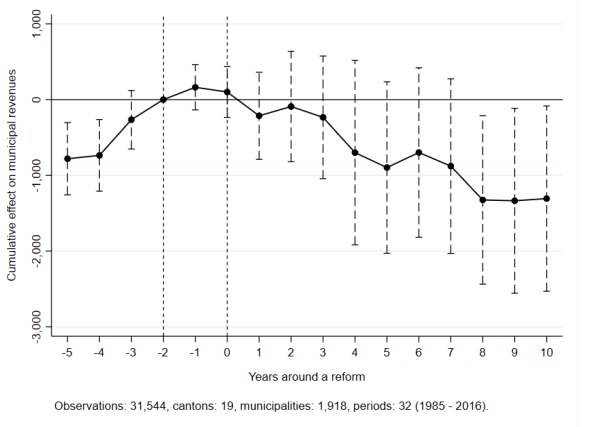
Notes: The figure shows the average cumulative effect on municipal per capita surplus for an event window of -5 to +10 years around each fiscal rules reform at municipal level. Data points represent the cumulative sum of the γ_j from equation 2.1. A fiscal rules reforms is identified by the attainment of a median or above-median Fiscal Rules Stringency Index at municipal level, i.e. by $FRSIM \geq 2$. The estimation exploits an indicator variable equal to 1 from the year where $FRSIM \geq 2$. The treatment group that contributes to the estimation includes 14 cantons (Aargau, Appenzell-Ausserrhoden, Bern, Basel-Landschaft, Fribourg, Genève, Luzern, Neuchâtel, Obwalden, St. Gallen, Schaffhausen, Ticino, Uri, Zürich). Surplus is computed as total municipal revenues - total municipal expenditures. The estimation includes municipality and year fixed effect, municipal covariates (share of population < 20 years, share of population ≥ 65 years, share of foreign population, unemployment rate, median net income, Gini coefficient of net income, share of votes for left parties and for right parties at national parliament elections) and cantonal covariates (statutory tax rate on firms' capital, statutory tax rate on firms' profit, tax rate for a representative married person without children in top 10% of the national income distribution, accounting model dummies, centralization reforms dummy). Standard errors are clustered at canton and year level. Vertical dashed lines report 95% confidence intervals.

Figure 2.A.2: Municipal expenditures and revenues

Panel A: Expenditures



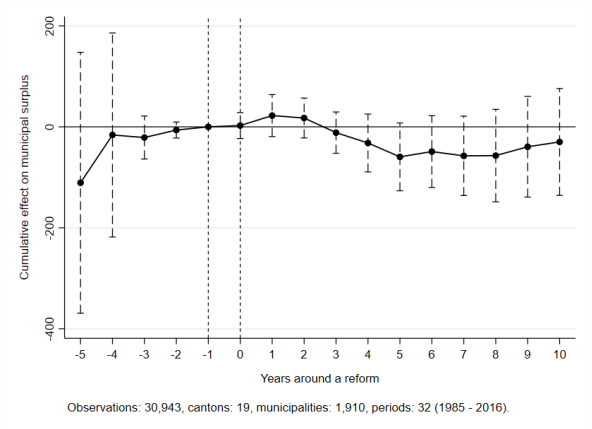
Panel B: Revenues



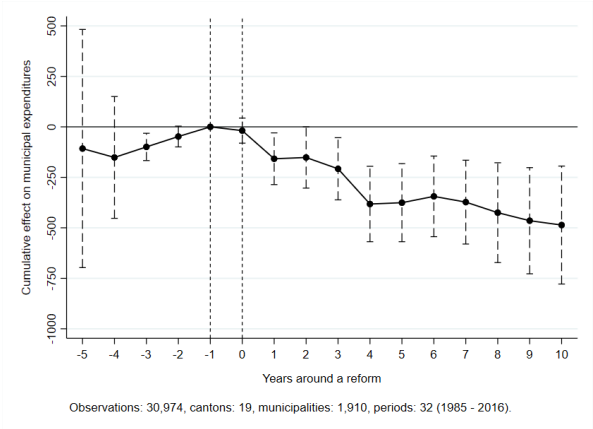
Notes: The figure shows the average cumulative effect on municipal per capita total expenditures (Panel A) and per capita total revenues (Panel B) for an event window of -5 to +10 years around each fiscal rules reform at municipal level. Data points represent the cumulative sum of the γ_j from equation 2.1. A fiscal rules reforms is identified by the attainment of a median or above-median Fiscal Rules Stringency Index at municipal level, i.e. by $FRSIM \geq 2$. The estimation exploits an indicator variable equal to 1 from the year where $FRSIM \geq 2$. The treatment group that contributes to the estimation includes 14 cantons (Aargau, Appenzell-Ausserrhoden, Bern, Basel-Landschaft, Fribourg, Genève, Luzern, Neuchâtel, Obwalden, St. Gallen, Schaffhausen, Ticino, Uri, Zürich). The estimation includes municipality and year fixed effect, municipal covariates (share of population < 20 years, share of population ≥ 65 years, share of foreign population, unemployment rate, median net income, Gini coefficient of net income, share of votes for left parties and for right parties at national parliament elections) and cantonal covariates (statutory tax rate on firms' capital, statutory tax rate on firms' profit, tax rate for a representative married person without children in top 10% of the national income distribution, accounting model dummies, centralization reforms dummy). Standard errors are clustered at canton and year level. Vertical dashed lines report 95% confidence intervals.

Figure 2.A.3: Municipal surplus, expenditures and revenues, -1 as reference year

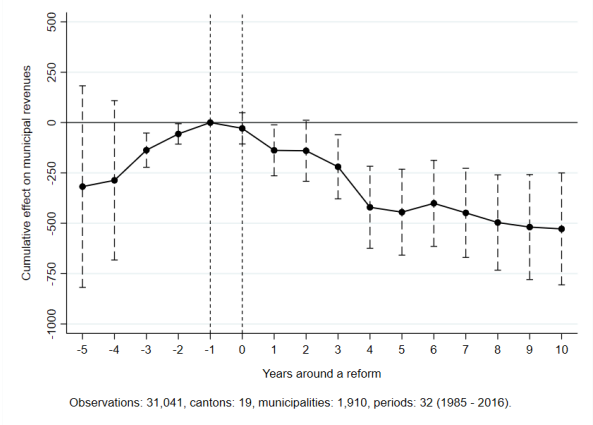
Panel A: Surplus



Panel B: Expenditures

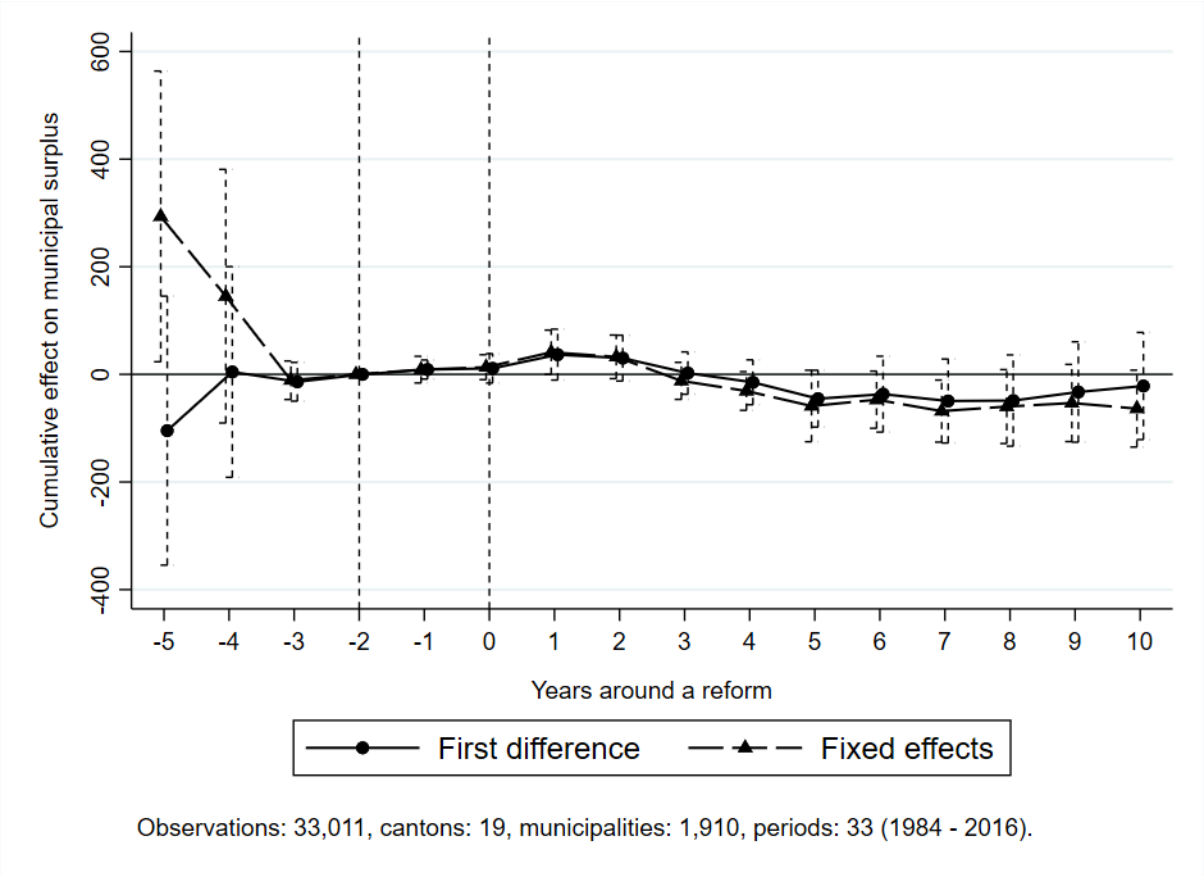


Panel C: Revenues



Notes: The figure shows the average cumulative effect on municipal per capita total expenditures (Panel A) and per capita total revenues (Panel B) for an event window of -5 to +10 years around each fiscal rules reform at municipal level. Data points represent the cumulative sum of the γ_j from shifting the reference year in equation 2.1 by one period, i.e. using the year prior to the reform as reference. A fiscal rules reforms is identified by the attainment of a median or above-median Fiscal Rules Stringency Index at municipal level, i.e. by $FRSIM \geq 2$. The estimation exploits an indicator variable equal to 1 from the year where $FRSIM \geq 2$. The treatment group that contributes to the estimation includes 14 cantons (Aargau, Appenzell-Ausserrhoden, Bern, Basel-Landschaft, Fribourg, Genève, Luzern, Neuchâtel, Obwalden, St. Gallen, Schaffhausen, Ticino, Uri, Zürich). The estimation includes municipality and year fixed effect, municipal covariates (share of population < 20 years, share of population ≥ 65 years, share of foreign population, unemployment rate, median net income, Gini coefficient of net income, share of votes for left parties and for right parties at national parliament elections) and cantonal covariates (statutory tax rate on firms' capital, statutory tax rate on firms' profit, tax rate for a representative married person without children in top 10% of the national income distribution, accounting model dummies, centralization reforms dummy). Standard errors are clustered at canton and year level. Vertical dashed lines report 95% confidence intervals.

Figure 2.A.4: Municipal surplus: First-difference vs fixed-effects



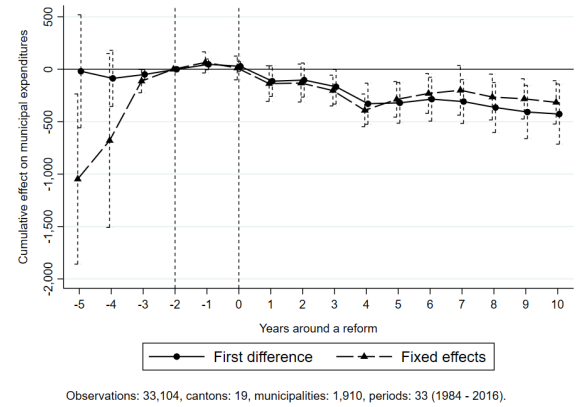
Notes: The figure shows the average cumulative effect on municipal per capita surplus for an event window of -5 to +10 years around each fiscal rules reform at municipal level. Data points for the model in first-difference represent the cumulative sum of the γ_j from equation 2.1. For the model with fixed-effects, data points are the cumulative sum of the δ_j from running the following estimating equation:

$$y_{mt} = \sum_{j=-5}^{10} \delta_j \Delta FRSIM_{c,t-j} + \sum_{j=-5}^{10} \delta_k \Delta FRSIC_{i,t-j} + \beta' \mathbf{X_t} + \alpha_m + \lambda_t + \epsilon_{mt} \tag{2.5}$$

A fiscal rules reforms is identified in both models by any (positive) change in our Fiscal Rules Stringency Index at municipal level, i.e. by $\Delta FRSIM > 0$. Surplus is computed as total municipal revenues - total municipal expenditures. Surplus is computed as total municipal revenues - total municipal expenditures. The estimation includes municipality and year fixed effect, municipal covariates (share of population < 20 years, share of population ≥ 65 years, share of foreign population, unemployment rate, median net income, Gini coefficient of net income, share of votes for left parties and for right parties at national parliament elections) and cantonal covariates (statutory tax rate on firms' capital, statutory tax rate on firms' profit, tax rate for a representative married person without children in top 10% of the national income distribution, accounting model dummies, centralization reforms dummy). Standard errors are clustered at canton and year level. Vertical dashed lines report 95% confidence intervals.

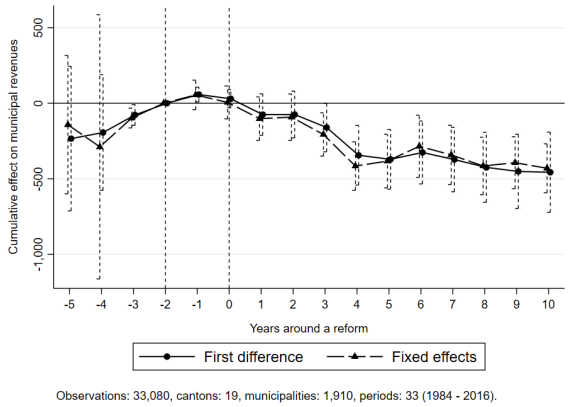
Figure 2.A.5: Municipal expenditures and revenues: First-difference vs fixed-effects

Panel A: Expenditures



Observations: 33,104, cantons: 19, municipalities: 1,910, periods: 33 (1984 - 2016).

Panel B: Revenues

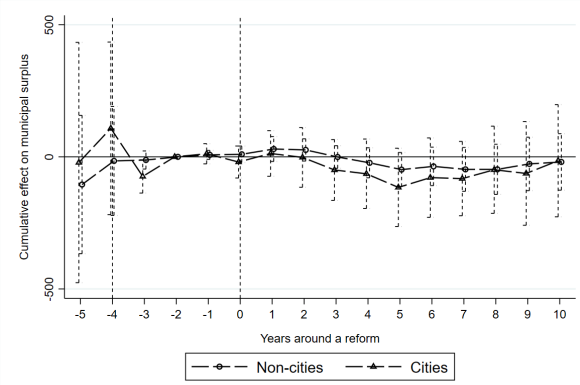


Observations: 33,080, cantons: 19, municipalities: 1,910, periods: 33 (1984 - 2016).

Notes: The figure shows the average cumulative effect on municipal per capita total expenditures (Panel A) and per capita total revenues (Panel B) for an event window of -5 to +10 years around each fiscal rules reform at municipal level. Data points for the model in first-difference represent the cumulative sum of the γ_j from equation 2.1. For the model with fixed-effects, data points are the cumulative sum of the δ_j from running estimating equation 2.5. A fiscal rules reforms is identified in both models by any (positive) change in our Fiscal Rules Stringency Index at municipal level, i.e. by $\Delta FRSIM > 0$. The estimation includes municipality and year fixed effect, municipal covariates (share of population < 20 years, share of population ≥ 65 years, share of foreign population, unemployment rate, median net income, Gini coefficient of net income, share of votes for left parties and for right parties at national parliament elections) and cantonal covariates (statutory tax rate on firms' capital, statutory tax rate on firms' profit, tax rate for a representative married person without children in top 10% of the national income distribution, accounting model dummies, centralization reforms dummy). Standard errors are clustered at canton and year level. Vertical dashed lines report 95% confidence intervals.

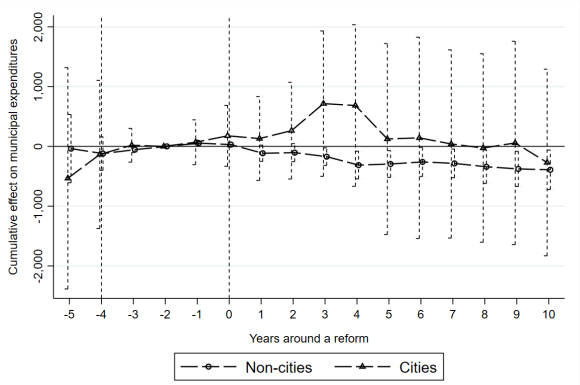
Figure 2.A.6: Municipal finances: heterogeneous effects cities vs non-cities (128 largest cities)

Panel A: Surplus



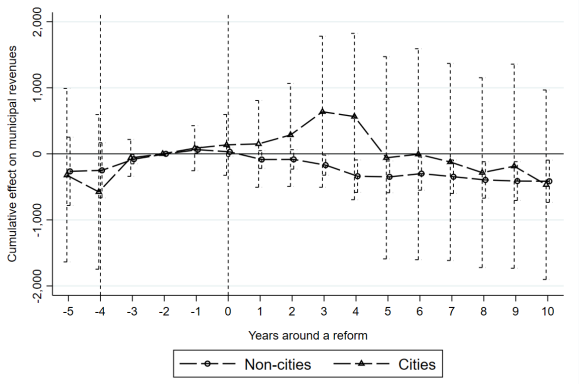
Observations: 1,921, cantons: 19, non-cities: 1,801, cities: 109, periods: 32 (1985 - 2016).

Panel B: Expenditures



Observations: 1,924, cantons: 19, non-cities: 1,801, cities: 109, periods: 32 (1985 - 2016).

Panel C: Revenues



Observations: 1,930, cantons: 19, non-cities: 1,801, cities: 109, periods: 32 (1985 - 2016).

Notes: The figure shows the average cumulative effect on municipal per capita surplus (Panel A), total expenditures (Panel B) and per capita total revenues (Panel C) for an event window of -5 to +10 years around each fiscal rules reform at municipal level. Cities are defined as the 128 major municipalities in Switzerland, current members of the Swiss Cities Association. Data points for the model in first-difference represent the cumulative sum of the γ_j from equation 2.1. For the model with fixed-effects, data points are the cumulative sum of the δ_j from running estimating equation 2.5. A fiscal rules reforms is identified in both models by any (positive) change in our Fiscal Rules Stringency Index at municipal level, i.e. by $\Delta FRSIM > 0$. The estimation includes municipality and year fixed effect, municipal covariates (share of population < 20 years, share of population ≥ 65 years, share of foreign population, unemployment rate, median net income, Gini coefficient of net income, share of votes for left parties and for right parties at national parliament elections) and cantonal covariates (statutory tax rate on firms' capital, statutory tax rate on firms' profit, tax rate for a representative married person without children in top 10% of the national income distribution, accounting model dummies, centralization reforms dummy). Standard errors are clustered at canton and year level. Vertical dashed lines report 95% confidence intervals.

2.B Supplementary Tables

Table 2.B.1: Fiscal reforms in Switzerland - Municipalities

Canton	Year	FRSI M before reform	FRSI M after reform	Max age of reform
Bern	1999	0	2	21
Luzern	2005	0	4	15
Uri	1985	0	1	35
	2012	1	2	8
Obwalden	2004	0	5	16
Zug	1982	0	4	38
Schaffhausen	2000	1	4	20
Graubünden	1974	0	1	46
Aargau	1982	0	4	38
Neuchâtel	1993	0	1	27
	1997	1	2	23
Genève	1994	1	5	26

Notes: The table reports details on the fiscal rules reforms for the municipal level exploited in the baseline event-study design with the reform definition $\Delta FRSIM > 0$. We considered as year of fiscal reform acceptance the year in which the set of fiscal rules set was accepted by either the cantonal Parliament or popular vote. If the entry into force of a legal document (or specific articles) was set on or after July 1st of a given year, then the following year was recorded, the effective duration in the original year being less than 6 months.

Table 2.B.2: Effect of municipal fiscal rules reforms on expenditure categories

	Public administration	Justice and public security	Education	Culture and activities	Healthcare	Social security	Transportation and communication	Environment	National economy	Interests and fiscal equalization
Long-term effect ($\sum_{j=-1}^{10} \hat{\gamma}_j$)	-56.97	-6.57	-220.13	-2.65	93.29	-165.27	-3.88	5.57	-0.65	-132.82
	(14.11)	(9.56)	(52.81)	(5.26)	(23.84)	(26.13)	(10.38)	(24.23)	(22.54)	(76.51)
	[0.0020]	[0.5063]	[0.0016]	[0.6240]	[0.0024]	[0.0001]	[0.7158]	[0.8224]	[0.9775]	[0.1105]
	(7.68)	(4.94)	(51.68)	(4.33)	(28.00)	(28.08)	(9.12)	(22.18)	(11.69)	(83.54)
	[0.0000]	[0.1952]	[0.0003]	[0.5457]	[0.0027]	[0.0000]	[0.6743]	[0.8037]	[0.9561]	[0.1244]
YearFE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
MunFE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Cantonal variables	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
μ	616	224	1,535	135	192	726	324	486	228	834
N	19,401	19,401	19,401	19,401	19,401	19,401	19,401	19,401	19,401	19,401
R^2	0.02	0.01	0.05	0.01	0.11	0.08	0.02	0.03	0.01	0.04
Start	1991	1991	1991	1991	1991	1991	1991	1991	1991	1991
End	2016	2016	2016	2016	2016	2016	2016	2016	2016	2016

Notes: The table reports estimates as in specification (4) of Table 2.4.1 for the 10 functional categories of expenditures. Note that cantonal public finance control variables are substituted with the cantonal per capita level of expenditures for each expenditure category (e.g. for the first category, "Public administration", I include as control the total per capita expenditures at cantonal level for the same category). Each coefficient represents the long-term effect on each expenditure category (in per capita terms) from fiscal rules reforms at local level. Due to data limitations on detailed expenditures categories' figures, the panel starts in 1990 and, to improve comparability across observations, it is restricted to the municipalities for which all variable values in the respective estimating equation are available. Standard errors clustered at canton and year level in brackets at line 2, p-values in squared brackets at line 3. Standard errors clustered at municipality and year level in brackets at line 4, p-values in squared brackets at line 5.

Table 2.B.3: Effect of municipal fiscal rules reforms on revenue-related variables

	Tax revenues	Municipal tax rate	Municipal multiplier	Non-tax revenues
Long-term effect ($\sum_{j=-1}^{10} \hat{\gamma}_j$)	-437.54	-0.25	-0.11	-624.33
	(293.34)	(0.11)	(0.07)	(304.18)
	[0.1553]	[0.0392]	[0.1360]	[0.0569]
	(242.25)	(0.12)	(0.06)	(260.55)
	[0.0830]	[0.0434]	[0.0484]	[0.0244]
YearFE	YES	YES	YES	YES
MunFE	YES	YES	YES	YES
Controls	YES	YES	YES	YES
Cantonal variables	YES	YES	YES	YES
μ	1,751	5	0	1,707
N	28,145	28,145	27,594	28,145
R^2	0.06	0.30	0.32	0.04
Start	1991	1991	1991	1991
End	2016	2016	2016	2016

Notes: The table reports estimates as in specification (4) of Table 2.4.1 for four revenue-related variables: total municipal tax revenue, municipal tax rate, municipal multiplier, and non-tax revenue. Note that cantonal public finance control variables are substituted with the cantonal per capita amount of each outcome variable (e.g. for "Tax revenues", I include as control the total per capita tax revenues at cantonal level). Each coefficient represents the long-term effect from fiscal rules reforms at local level on the above variables. Due to data limitations on detailed revenue figures, the panel starts in 1990 and, to improve comparability across observations, it is restricted to the municipalities for which all variable values in the respective estimating equation are available. Multipliers and tax rates are sourced from Parchet (2019). The tax rate is computed as a share of the municipal tax liability. Multipliers have different scales (units or percentages) and are therefore standardized by canton, such that all level changes are relative to the own scale. Its coefficient is interpreted as the long-term effect of the reforms on the standard deviation of the municipal multiplier. The municipal (personal) tax rate is computed for a representative married person without children in top 10% of the national income distribution. Standard errors clustered at canton and year level in brackets at line 2, p-values in squared brackets at line 3. Standard errors clustered at municipality and year level in brackets at line 4, p-values in squared brackets at line 5.

Table 2.B.4: Summary statistics for small vs large municipalities

	Median		Mean			
	Small	Large	Small	Large	Δ	P-value
Panel A: municipal outcome variables						
Surplus	10.7	12.8	98.1	111	13	.177
Expenditures	4,165	4,920	5,059	6,469	1,410	0
Revenues	4,240	5,040	5,161	6,569	1,408	0
Panel B: Fiscal rules stringency index						
Municipality level	2	4	2.65	3.48	1	0
Cantonal level	2	2	2.01	2.07	0	.193
Panel C: municipal covariates						
Population share < 20y (%)	23.8	22	23.6	22.3	-1	0
Foreigners share (%)	8.93	18.8	10.9	19.9	9	0
Population share > 65y (%)	15	15.4	15.4	15.5	0	.647
Unemployment rate (%)	1.69	2.36	1.9	2.61	1	0
Median net income	54,150	55,200	54,631	55,646	1,015	.0179
Gini coeff. of net income	32.5	33.8	34.8	36	1	0
Share of left-wing seats (%)	22.4	25.7	22.7	25.9	3	0
Share of right-wing seats (%)	71.9	67.2	71.8	67.3	-5	0
Panel D: cantonal covariates						
Expenditures	8,192	7,849	8,506	8,004	-502	0
Revenues	8,143	7,859	8,514	7,994	-519	0
Statutory corporate capital tax rate (‰)	1.5	1.5	1.43	1.33	0	.0019
Statutory corporate profits tax rate (%)	8.5	8	7.65	7.22	0	.0008
Personal income tax rate (%)	6.34	5.01	6.42	5.66	-1	0
Panel E: total municipal population						
Total population	918	5,873	1,147	10,364	9,218	0

Notes: The table reports median, mean and difference in means with p-value over 1984 to 2019 for the 75% smallest municipalities ("small") vs the top quartile ("large"), for the same set of variables as in the baseline Table 2.3.1. The estimated difference in means Δ is the β coeffcient of the regression with standard errors clustered at municipality level $y_{mt} = \alpha + \beta D_m + \epsilon_{mt}$ where: y_{mt} is the dependent/independent variable for municipality m in year t, and $D_m = 1$ if municipality is in the top quartile measured by population in the year 2000, 0 else.

Table 2.B.5: Data aggregation level and sources

	Aggregation level	Source
Municipal outcome variables		
Surplus	Municipality	SNF project Sinergia (Nr. 159348) and own research
Expenditures	Municipality	SNF project Sinergia (Nr. 159348) and own research
Revenues	Municipality	SNF project Sinergia (Nr. 159348) and own research
Other municipal outcome variables		
Tax revenues	Municipality	SNF project Sinergia (Nr. 159348) and own research
Municipal tax rate	Municipality	Parchet (2019)
Municipal multiplier	Municipality	Parchet (2019)
Non-tax revenues	Municipality	SNF project Sinergia (Nr. 159348) and own research
Fiscal rules stringency index		
Municipality level	Municipality	Own research
Cantonal level	Canton	Own research
Municipal covariates		
Population share < 20y (%)	Municipality	Swiss Federal Statistical Office
Foreigners share (%)	Municipality	Swiss Federal Statistical Office, own calculation
Population share ≥ 65y (%)	Municipality	Swiss Federal Statistical Office
Unemployment rate (%)	Municipality	Swiss Federal Statistical Office
Median net income	Municipality	Swiss Federal Tax Administration
Gini coeff. of net income	Municipality	Swiss Federal Tax Administration
Share of left-wing seats (%)	Municipality	Swiss Federal Statistical Office, Manifesto Project (parties classification), own calculation
Share of right-wing seats (%)	Municipality	Swiss Federal Statistical Office, Manifesto Project (parties classification), own calculation
Cantonal covariates		
Expenditures	Canton	Statistisches Jahrbuch der Schweiz, Öffentliche Finanzen der Schweiz, Swiss Federal Finance Administration
Revenues	Canton	Statistisches Jahrbuch der Schweiz, Öffentliche Finanzen der Schweiz, Swiss Federal Finance Administration
Statutory corporate capital tax rate (‰)	Canton	SNF project Sinergia (Nr. 159348) and own research
Statutory corporate profits tax rate	Canton	SNF project Sinergia (Nr. 159348) and own research
Personal income tax rate (%)	Canton	SNF project Sinergia (Nr. 159348) and own research
Cantonal tax revenues	Canton	Swiss Federal Finance Administration, own research
Cantonal tax rate	Canton	Parchet (2019)
Cantonal multiplier	Canton	Parchet (2019)
Cantonal non-tax revenues	Canton	Swiss Federal Finance Administration, own research
Total population and other covariates		
Total population	Municipality	Swiss Federal Statistical Office
Harmonized accounting model 1	Municipality (aggregate)	Own research
Harmonized accounting model 2	Municipality	Own research
Harmonized accounting model 1	Canton	Swiss Public Sector Financial Reporting Advisory Committee (SRS-CSPCP), own research
Harmonized accounting model 2	Canton	Swiss Public Sector Financial Reporting Advisory Committee (SRS-CSPCP), own research
Centralization tasks reform	Canton	Flèche (2021)

Chapter 3

Taxation, Public Spending and Internal Migratory Responses in Switzerland: Who Votes with Her Feet?[1]

This article investigates mobility reactions to tax rates and public goods in Switzerland. We match administrative data covering the whole population to income data from the social security earnings, and we analyze almost 1,500,000 households' relocations over eight years (2010-2017). We first show that migration profiles are similar across income groups and decrease in age. We then analyze migration responses to the net-of-tax rate and local spending by income groups using an aggregate model of migration flows between municipality pairs. We tackle policy endogeneity bias by including a set of geographical and time fixed effects. Our results show heterogeneous responses: the tax-base elasticity to net-of-tax rate seems positive and predominantly driven by households without children in the highest quartile of the income distribution. The estimated elasticity is around 3 for the top 10% incomes and stable across specifications. We proxy public good's provision by school expenditures and childcare subsidies. We find a negative relationship between school expenditures and mobility inflows. In contrast, subsidies have a positive impact.

JEL classification:H24, H31, H41, H73, J60, R23
Keywords: Migration, Mobility, Taxation, Public Goods

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3.1 Introduction

The concept that local policies distort location choice is the central idea of Tiebout's seminal paper (1956). Migration responses to tax rates and public policy shape the way households sort across jurisdictions, potentially leading to spatial segregation, but also undermine the ability of governments to redistribute. Understanding forces that govern household location choice is of particular relevance, given existing evidence on the effects of neighborhoods on individuals' long-term outcomes (Chyn, 2018).

Albeit important, there is still relatively little empirical evidence on mobility induced by local policies (see Kleven et al., 2020 for an overview). In our setting, this means tax-rate setting and public goods. First, data requirements for the tax-base elasticity are demanding: accurate measures of applicable tax rates and residential location changes are often available only for selected samples in a population. Second, a local services measure with plausibly exogenous variation is required. Our research tackles these challenges by exploiting rich administrative data covering the universe of residents in Switzerland over eight years, and by focusing on two measures of local services, namely school expenditures and childcare subsidies, to address the question: "What is the tax base elasticity to changes in tax rate and local public goods provision?".

Our results indicate heterogeneity in the migratory effect in response to changes in the net-of-tax rate differential between the origin municipality and a destination. Households in the upper quartile of the income distribution react the most: a 1% increase in the net-of-tax rate attracts around 3% of households in the top 10%. We investigate reactions to local services by using two proxies: public school spending and childcare subsidies. We find that higher school spending in a given municipality, if anything, discourages households from relocating to that municipality while childcare subsidies attract households, but the effect is rather small. The estimated elasticity of mobility are -0.04 and 0.0008, respectively. We investigate further underlying explanations for adverse reactions to public expenditures.

Switzerland offers an ideal setting to study mobility reactions as local jurisdictions, i.e., *municipalities*, are granted substantial freedom both in terms of revenue collection and budget allocation. We exploit precise information on Swiss households' location over eight years across 2,240 municipalities, and we relate changes in mobility flows within a given municipality pair to changes in the net-of-tax rate, school expenditures, and childcare subsidy differentials. The main issue with this identification approach is the non-randomness of local policies. For example, a destination may offer well-paid jobs and at the same time

being a low-tax jurisdiction. In such a scenario, the location choice could be driven by the job offer rather than the tax rates or public services. To address this endogeneity concern, we exploit variation over time and within a municipality pair, absorbing permanent household-flows due to unobserved factors.

Our paper's contribution is twofold. First, we contribute to the growing literature on migratory responses to tax rates by estimating mobility reactions to tax rates along the income distribution using administrative data. Previous works mainly focus on selected group of individuals: strong reactions have been found among top football players (Kleven et al., 2013a), foreigners (Kleven et al., 2013b; Schmidheiny and Slotwinski, 2018), high-income earners (Young et al., 2014; Martínez, 2017; Agrawal and Foremny, 2019) and superstar inventors (Akcigit et al., 2016; Moretti and Wilson, 2019).
Evidence for a more comprehensive group of individuals is remarkably scant. To the best of our knowledge, only a few studies analyze mobility reactions to tax rates within a country[2] along the income dimension. Liebig et al. (2007) using data from the Swiss Census and exploiting differences in tax rates across communities investigate internal migratory responses to tax rates. A comparison of 1995 with 2000 suggests that young and highly educated individuals were the most responsive group to tax rate changes. Our paper differs from this study considerably. First, we directly observe *actual* households' income, while Liebig et al. (2007) had to use predicted earnings. Second, we impute a precise measure of the tax burden by using our tax simulator instead of applying an approximate rate based on income brackets. Third, the panel structure of our data allows controlling for unobserved factors and aggregate shocks.[3] In a recent study, Brülhart et al. (2022) estimate the stock elasticity of low, middle, and high-income, showing that mainly above-median income households without children move in response to local tax rates.

Second, we analyze the role public services play for households' location choice. As noted by Kleven et al. (2020), controlling for non-tax determinants is critical in the interest of correctly estimating mobility responses to tax rates themselves. In this work, we address this gap by including local public goods provision as a determinant of location choice in addition to tax rates. With this approach, we aim at minimizing the bias potential in the estimates of the tax rate elasticity. Previous studies provide mixed evidence on the effect of local services on residential choice. Friedman (1981) was among the first authors to study

[2] Schmidheiny (2006) and Schaltegger et al. (2011) analyze within canton mobility.
[3] Liebig et al. (2007) use the difference between the 1995 and the 2000 cross-sectional datasets.

how local public services influence households' location choice, showing that the role of public service is only marginal. In contrast, Quigley (1985) finds that households tend to relocate, by small magnitudes, to municipalities with lower per-pupil expenditures. Other studies demonstrate that school expenditures are a relevant factor with substantial attractive power. Nechyba and Strauss (1998) provide evidence from a logit model of a large and positive elasticity of mobility to local per-pupil school expenditures in New Jersey. Their study sample concentrates on non-retirees homeowner households with children in school-age located in a concentrated area (less than 5 miles relocation radius). Nechyba (1999) confirmed the results and shed light on the relative importance of per capita spending in education. Results are subsequently confirmed by Bayoh et al. (2006) who studied relocation within Ohio. Dahlberg et al. (2012), using Swedish data, shows that childcare spending plays a significant role, while education spending matters only for families with school-age children. Furthermore, recent results by Brülhart et al. (2022) demonstrate that households have different valuations of public goods depending on their income level. Indeed, the lack of consensus can be attributed to differences in the institutional setting, the sample, and the methodological approach. An additional question is whether schooling expenditures are an appropriate measure for local services and if their variation is sufficiently exogenous for a valid estimation. Basically all previous studies use any variation in school spending, and as noted by Jackson (2018) and Jackson et al. (2015), this variation should be transparent in order to disentangle the influence of spending from other factors. We re-examine whether local public services matter using a pairwise approach and two measures of local public services: the more traditional school spending and an exogenous variation of childcare offer resulting from The Federal Act on Financial Aid for Child-Care.

The paper proceeds as follows. In the next section 3.2 we present the Swiss institutional setting. Next, in 3.3 we describe the data and present stylized facts of migration in Switzerland. We layout the empirical strategy in 3.4. In section 3.5 we present our preliminary results and show their robustness to different specifications in section 3.6.1. Section 3.7 concludes.

3.2 Institutional setting

Switzerland is a federal country with three tiers of government, and roughly 2,300 municipalities located in 26 cantons. Cantons and municipalities have substantial autonomy in

terms of revenue collection and public expenditures. Together, they raise about 53% of the consolidated (federal, cantonal, and municipal) tax revenue. Income tax is the main cantonal and municipal tax instrument, alone it accounts for almost 60% of the total tax revenue.[4] In contrast, corporate tax and wealth tax represent only a minor share of the fiscal revenue, 16% and 9%, respectively.[5] The federal income tax is regulated by the central government, and it is homogeneous across sub-federal levels. Local income taxes are highly heterogeneous instead and are due to the cantonal government, to the municipality, and often also to the church.[6] Cantons enjoy near-complete autonomy over tax-base definition, tax-rate setting, and on how to spend the tax revenue. As a result, in Switzerland, 26 different systems of tax regulations and fund allocations coexist.

Income tax is levied annually and according to the primary place of residence. Starting at the age of 18, residents are legally obliged to submit a tax file every year to determine income and (less often) wealth tax burden according to mostly progressive schedules. The taxable income includes all kinds of income, i.e. any remuneration from employment and self-employment, capital income, earnings from the pension scheme, and replacement incomes (e.g unemployment income), net of social contributions, deductions and debt. Married couples return a single tax file (i.e the income tax base is the sum of spouse and partner income) and are subject to a different tax regime than single households.[7] In most cantons, joint taxation applies a splitting factor[8] or using a special tax schedule[9].

Deductions reduce by definition the taxable income and thus the tax rate. Typical deductions are granted for children, childcare, double earners, work-related expenses, and retirement savings. They are all capped, and the maximum and minimum amount vary across cantons. Individuals receive a deduction for children in 25 out of 26 cantons[10], ranging from a minimum of CHF 750 in the canton of Basel-Landschaft to a maximum of CHF

[4] Or, roughly 38% of the total revenue.

[5] The percentages are calculated over the period 2010-2017, data sourced from the Swiss Federal Finance Administration: https://www.efv.admin.ch/efv/de/home/themen/finanzstatistik/daten.html.

[6] A church tax is collected, autonomously, in most Swiss cantons.

[7] In many cantons single households with children are also subject to joint taxation. In our tax calculator, we apply the joint taxation regime to all household heads living with a minor, even if the individual is non legally married.

[8] The income that determines the tax schedule is divided by a factor, commonly 2.

[9] For example, in the canton of Ticino.

[10] Canton Vaud applies a family quotient.

12,000[11] in the canton of Zug. Few cantons provide social deductions[12]: the amount varies across household types (single, married single with children) and ranges from CHF 3,200 in the canton of Schwyz to CHF 18,000 in the canton of Basel-Stadt.[13] In every canton taxpayers may also benefit from a double-earners deduction if both household members work and are either married or in a registered partnership. The deductions amount varies from CHF 500 in the canton of St. Gallen to CHF 9,300 CHF in the canton of Bern.[14]

Most municipalities inherit the tax base and the tax schedule from the canton and apply the so-called "tax multiplier" to the cantonal tax burden. Tax multipliers are set every year by the municipal council or the municipal assembly[15]. As a result, tax rates may vary across all municipalities. Overall, tax burden variation occurs within each canton because of the local tax multiplier, and across cantons because of differences in cantonal tax systems.

Figure 3.2.1 shows the income tax rate across 2,240 Swiss municipalities in 2017. Using our tax simulator, we calculated the consolidated (cantonal and municipal) tax rate for a hypothetical single household with a yearly gross income of CHF 100,000, with no children and with two children. In our setting, we define the tax rate as an "effective" tax rate, i.e. we divide the amount due as tax burden by the total taxable income. The map shows substantial variation across areas with tax rates varying by a factor of around three. Indeed, a single household with children faces a tax rate of 7.71% (~ CHF 6,070) in the municipality of Walchwil in the canton of Zug, and of 22% (~ CHF 18,233) in the municipality of Schelten in the canton of Bern. Figure 3.A.1 in the appendix shows that even at a short distance there is a considerable number of municipality pairs with a tax rate differentials of at least 4%[16]. Figure 3.2.2 reports local total per pupil spending in CHF for Swiss municipalities in 2017. The geographical variation is highly interesting, and occurs both across as well as within cantons. We cannot distinguish a clear spatial pattern, except for a tendency to lower per pupil expenditures in the western parts of the country.

Out of all sample municipalities, 80% of them run an own kindergarten and a primary school. The Swiss schooling system is predominantly financed with public funds, the main role being played by cantons and municipalities. Overall, mandatory schooling (we con-

[11] The amounts refer to the year 2017, data sourced from the Swiss Federal Tax Administration: https://www.estv.admin.ch/estv/fr/home/allgemein/steuerinformationen/fachinformationen/schweizerisches-steuersystem/steuermaeppchen.html.

[12] In the large majority of cantons a social deduction implicitly occurs using different tax schedules.

[13] Single household without children in 2017.

[14] The deductible amount may also depend on income.

[15] Municipal assembly is a form of direct democracy and is relatively rare.

[16] The average tax rate differential across pairs is 3.55%.

sider here kindergarten and primary school, i.e. up to the age of 11) is financed at 55% by the municipalities, 45% by the cantons.[17] Average local expenditures are for roughly one third devoted to primary and secondary schooling. The yearly investment amounts to 5 to 6% of national GDP, a rate very similar to the U.S.[18] The financial flows for the schooling system vary by canton, e.g. in some cantons, schooling services are carried out by municipalities but they are predominantly financed with transfers of lump sums by pupil or by teacher from the cantonal level.[19] Out of all sample municipalities, 80% of them run an own kindergarten and a primary school. The remaining two-thirds of local expenditures are dedicated to other items, like roads and underground infrastructure. While average per capita expenditures for schooling largely varies, public school quality is high and fairly homogeneous across the country.[20] , [21] Central government co-financing is limited to targeted subsidy programs, e.g. for early childcare and after-schooling activities.

The Swiss schooling system is less close to a Tiebout model for example than U.S. school districts because it is less obvious who pays for what, financing being mixed between cantons and municipalities. The tax rate employed in our model is a consolidated tax rate, the ratio of municipal to cantonal funds is irrelevant for the migration decision, only total funds devoted to schooling. Our model follows the literature on the implications of Tiebout's principles, i.e that residential mobility is reactive to the public goods' price (taxes) and performance (Oates, 1969; Tullock, 1971).

[17] Swiss Federal Office of Statistics,
https://www.bfs.admin.ch/bfs/de/home/statistiken/bildung-wissenschaft/bildungsfinanzen/oeffentliche-bildungsausgaben.assetdetail.14367430.html

[18] Swiss Federal Statistical Office, National Center for Education Statistics.

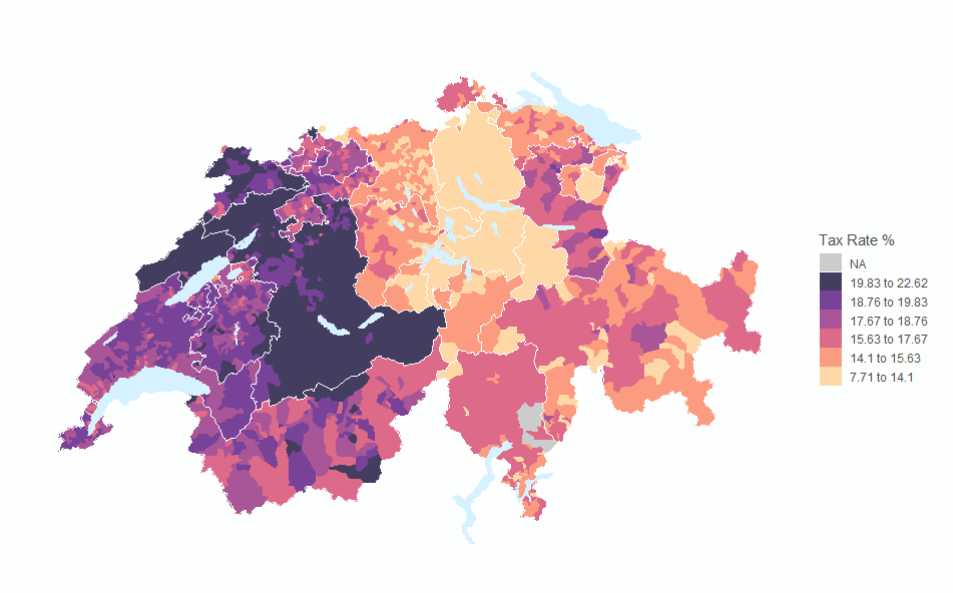
[19] For example, most municipalities in the French-speaking cantons are not responsible for the payment of teachers' salaries, while German-speaking cantons are. In the latter case, salaries are mostly financed with large transfers from the cantonal level, possibly complemented with a municipal contribution. Our data on schooling expenditures reflect the effective amount spent on education when it flows through the municipal accounting, be it financed from the local, the cantonal, or both levels. What our data does not include is the cantonal direct contribution to teachers' salaries in each municipality, if it runs outside of municipal books. This feature is constant across all municipalities in a given canton, and therefore also a constant difference, or similarity, in each municipality pair, and it can be factored out with the municipality-pair fixed effect. This will be explained more in detailed in Section 3.4.2, dedicated to the empirical strategy.

[20] We ignore the production function of municipalities: we acknowledge that we can only proxy school quality by school expenditures. We observe a range of per-pupil expenditures between a minimum of around CHF 11,000 for municipalities in the canton of Bern to more than CHF 30,000 for municipalities in the canton of Zug.

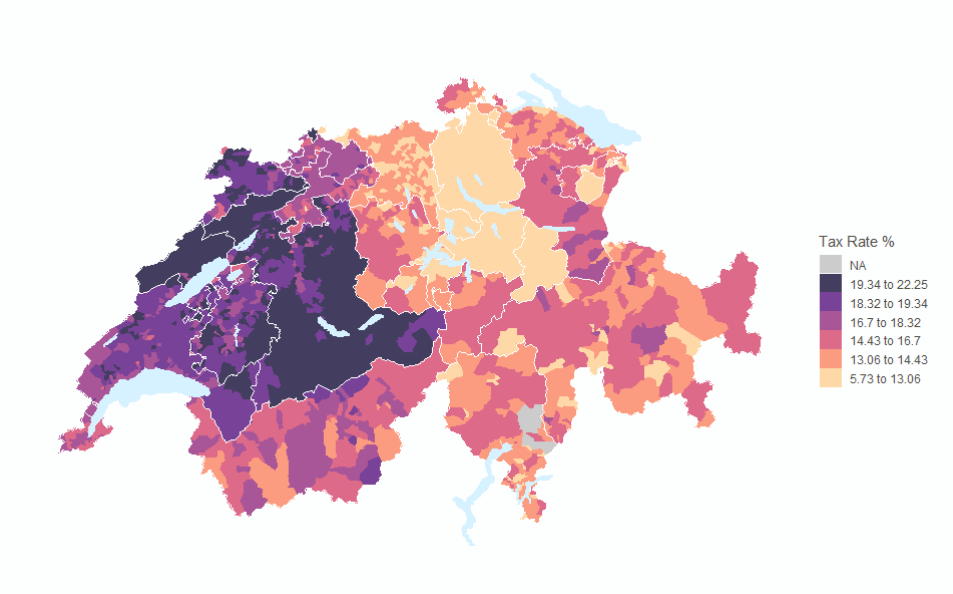
[21] Private schools cover only less than 10% of the market for mandatory schooling and are mainly concentrated in urban areas.

Figure 3.2.1: Tax rate on CHF 100,000 yearly gross income

(a) Single, no children

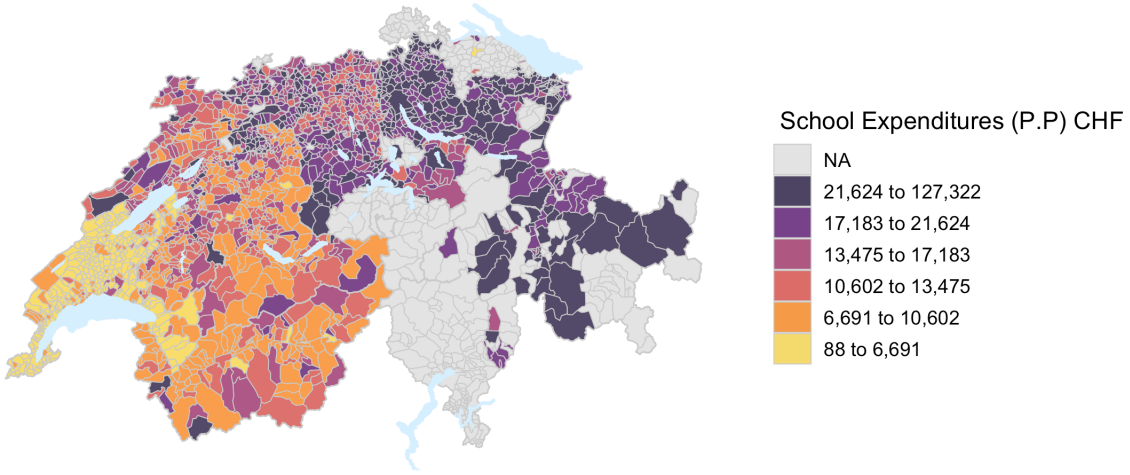


(b) Single, two children



Notes: This figure presents the cantonal and municipal tax rates across Swiss municipalities in 2017. The tax rates are computed for a single taxpayer with a net yearly income of CHF 100,000, it corresponds to the 88th percentile of the labor earnings distribution across all Swiss working population. The tax rate is defined as the tax burden over the taxable income (after deductions) and is calculated using our tax simulator. Panel (a) depicts the average tax rate for a single taxpayer without children, while Panel (b) shows the average tax rate for a single taxpayer with two children.

Figure 3.2.2: Local school spending variation



Notes: The figure depicts per pupil schooling expenditures in 2017 across Swiss municipalities. Amounts are in CHF. Swiss National Science Foundation, Sinergia project n. 147668 "The Swiss Confederation: A Natural Laboratory for Research on Fiscal and Political Decentralization" extended by Laura Fontana-Casellini under the supervision of Prof. Raphaël Parchet (USI).

The Federal Act on Financial Aid for Child-Care

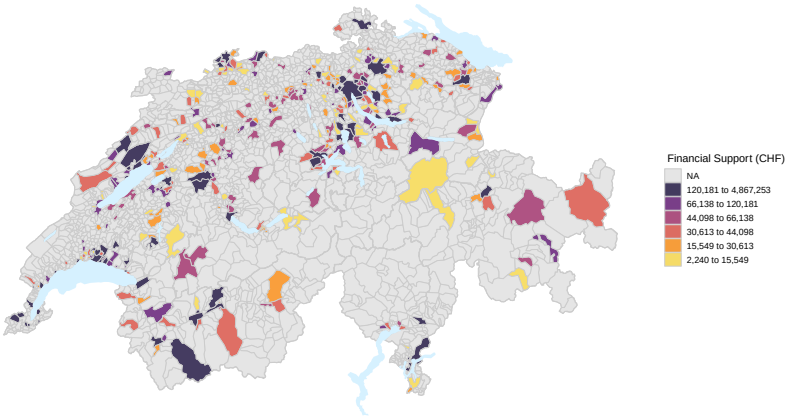
Schooling services need not be financed exclusively by the municipalities. We take the example of one of the largest subsidy programs run by the Swiss federal government starting 2003 that targeted the creation and extension of early childcare and after-schooling childcare to reconcile work and family.[22] Under this law, the Swiss government provides financial support to local governments or private associations to create new childcare facilities

[22] Federal Department of Home Affairs, https://www.bsv.admin.ch/bsv/de/home/finanzhilfen/kinderbetreuung/finanzhilfen-schaffung-betreuungsplaetze.html (in German).

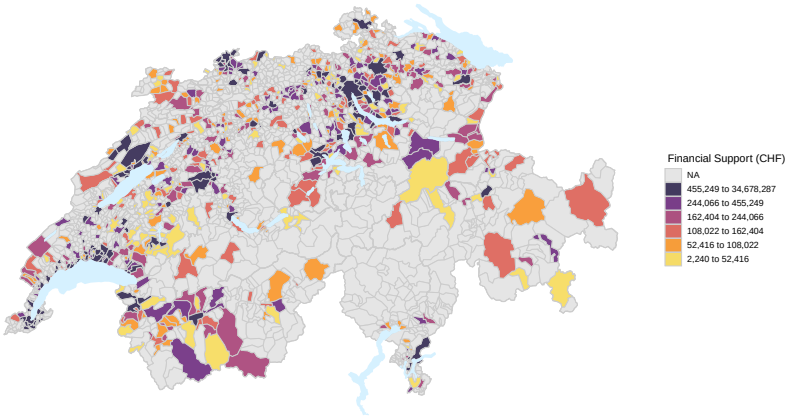
or expand existing ones. Between 2003 and 2021, 3,601 requests have been approved, the Confederation paid out CHF 408 million (around USD 445 million), contributing to the creation of 65,329 new childcare slots across 896 municipalities out of 2,240 (OFAS, 2021).

Figure 3.2.3: Spatial distribution of financial aid for childcare between 2010 and 2017

(a) 2010



(b) 2010-2017



Notes: The figure presents the spatial distribution of financial supports for childcare received by municipalities. Panel (a) depicts the situation in 2010, while Panel (b) shows the cumulative sum between 2010 and 2017. All amounts are expressed in CHF.

3.3 Data and Mobility Patterns in Switzerland

In this section we describe the data and present stylized facts about migration in Switzerland. This descriptive evidence shows how large is the heterogeneity in the propensity to move depending on income, and motivates us to investigate this empirically.

3.3.1 Data

We draw our data from three sources, the Swiss Federal Statistical Office (FSO), the Central Compensation Office (CCO), and a Swiss National Science Foundation (SNSF) Sinergia Project.[23] The final dataset combines matched individual-level information, obtained from a merge between the population and households statistics (STATPOP) and the social security earning records (SSER) with administrative municipality-level data from the Sinergia Project.

Matched STATPOP - SSER

Our data originates from the Population and Households Statistics (STATPOP) of the FSO. The data is based on administrative registers and provides us individual-level demographic information on the universe of residents in Switzerland for the years 2010 to 2017.[24] In particular, for every individual we observe, age, gender, marital status, municipality of residence in year t and $t-1$, nationality as well as other information about migration history (e.g., municipality of birth, arrival date in the municipality).[25] Additionally, we can relate every individual to his family: children, parents, spouse/partner. This important feature helps us to define households accurately. Individual observations are then matched to longitudinal (2010-2017) social security earnings records (CCO). The register covers the universe of labor earnings[26] legally obtained in Switzerland. This includes earnings from employment and self-employment as well as earnings from unemployment and disability benefits. Most important, these earnings are not top-coded in the registers, therefore we

[23] Swiss National Science Foundation, Sinergia project n. 147668 "The Swiss Confederation: A Natural Laboratory for Research on Fiscal and Political Decentralization" extended by Laura Fontana-Casellini under the supervision of Prof. Raphaël Parchet (USI).

[24] The FSO defines as resident all Swiss nationals with the main residence in Switzerland and all foreign nationals that have held a residence or a permanent residence permit for at least 12 months.

[25] All records refers to the end of the year (31st December).

[26] More than 90% of the working-age (18-65 years) population during our sample period 2010-2017.

observe the true labor income distribution.[27, 28]

In a first step, we define households according to marital status, common children and the municipality of residence. We consider children below 12 years of age because typically, at 11 years a child ends the primary school. Most Swiss municipalities have an own primary school, while the secondary school is, fairly often, in another municipality. Two individuals are considered a household if they are either married (or equivalently in a registered partnership) or not married, but they have their youngest child in common, and they all live in the same municipality. Unmarried partners without common children and individuals older than 18 years are considered as single households. We then restrict our sample to the individuals with the highest income in the households; we refer to this sample as household heads. Finally, we restrict the sample to Swiss household heads between 18 and 57 years of age because of two reasons. First, only permanent residents are subject to the Swiss tax regime. Foreign nationals are largely subject to different tax regimes, on which our tax simulator would perform poorly. Second, given that we observe only labor income (no pensions) we naturally choose to restrict the individual observations to those within the working age.[29] We will further refer to the observation unit as to a "household" rather than household head. We then rank household's income[30] relative to other households in the same distribution. Figure 3.A.3 in the appendix reports the Swiss households labor income distribution. In short, we are able to track over time and space a sample of (i) Swiss households, (ii) between the age of 18 and 57 years, (iii) with non-negative labor incomes.

Migration flows

We define a move at time t when we observe a change in the household's municipality of residence between t and $t-1$. We refer to *origin* and *destination* as the municipality at time $t-1$ and t, respectively. Appendix figure 3.A.4 shows the total number of households moves and municipality pairs with positive migration flows over the 2010-2017 period. We observe 1,438,383 (household) moves across 275,567 municipality pairs out of 5,015,360 pairs[31].

[27] However, estimation data are indirectly top-coded due to the lack of capital income.

[28] Negative incomes (less than 0.5% of the individual income dataset) and missing records have been imputed as 0.

[29] Earliest age to be eligible for retirement benefits is 58 years in Switzerland (from the so-called Pillar 2).

[30] Note that we call "income" the income of the household's head, while total household income is the mean income of all households members.

[31] 2,240× 2,240-2,240. We harmonize the set of municipalities as of year 2017.

Table 3.3.1: Summary statistics

	Sample						
	All	Top 1%	98-90th	89-75th	74-50th	49-25th	24-0th
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Individual:							
Female (%)	39.35	8.97	14.91	25.87	39.11	55.00	50.77
Age	36.89	48.28	46.27	43.02	37.38	33.61	29.22
Married (%)	28.91	80.50	67.87	50.46	28.00	13.72	5.19
Share Children < 12 (%)	8.78	25.54	20.02	13.68	8.14	5.21	2.56
Nr Children <12 (If any child)	1.45	1.60	1.59	1.57	1.49	1.36	1.31
Youngest Child Age	12.05	12.10	11.77	11.48	11.63	13.58	13.65
Years Municipality	7.58	11.10	10.96	9.36	7.05	6.39	6.07
Household Head Income	69,603	474,447	160,019	100,331	69,872	38,268	7,357
Spouse Income	33,840	53,099	40,033	34,676	30,848	18,530	5,056
Movers (%)	7.57	3.52	4.06	5.95	9.74	10.36	5.42
Movers:							
Intra-Cantonal Movers (%)	69.93	62.41	65.78	68.48	72.97	69.64	70.05
Inter-Cantonal Movers (%)	30.07	37.59	34.22	31.52	27.03	30.36	29.95
Intra-MS Region Movers (%)	43.23	40.03	39.45	41.72	46.21	42.90	43.08
Inter-MS Region Movers (%)	56.77	59.97	60.55	58.28	53.79	57.10	56.92
Distance if Move (km)	31.67	37.78	34.11	31.09	27.45	33.17	34.45
Distance if Move (min)	35.69	39.89	37.24	35.01	32.44	37.11	37.91
Obs.	18,519,125	223,743	1,990,951	3,221,389	5,026,545	3,891,016	4,000,447

Notes: The table provides summary statistics of our sample of households. A household is represented by Swiss household heads between 18 and 57 years old. Income refers to the yearly income of the household head. MS Region is a definition of common labor-market regions. There are 106 MS Regions in total in Switzerland.

Table 3.3.1 provides summary statistics of our sample of households for the years 2010-2017. Column (1) summarizes the full sample, while columns (2)-(7) show the means by income groups. There is substantial heterogeneity across income groups in terms of household composition. The table clearly shows a positive relationship between income, age, being male and married. In the upper tail of the distribution, household heads are more likely to be men, older and married. The propensity to move (the share of people who moves from a municipality o to a municipality d) is overall quite small. Interestingly, it is about three times higher for the middle-income groups (second and third quartile) than for the tails. In section 3.3.2 we investigate migration profiles more in detail. Movers seem to relocate within a radius of about 30 km from the origin municipality, as shown in Fig-

ure 3.A.2. 80% of the moves occur within a radius of 40 km. Roughly 2/3 of the moves are within the same canton, 2/5 within the same labor market-regions ("MS Region").[32]

Tax Rates

To compute tax rates, we collect information about cantonal tax regimes, and we build our own tax simulator[33], see Appendix 3.C for details. Specifically, we extended the tax calculator used by Bütler and Ramsden (2017) to our observation period. To impute a precise measure of the taxable income, we assign deductions based on working status and family composition. This includes deductions for i) social purposes, ii) dependent (i.e. children), iii) double-earners, and iv) work-related expenses.[34] Note that we apply the tax rate for married persons to legally unmarried couples who live with a child in common, as this is common practice in most cantons.

For the empirical analysis we compute a measure of the average tax rate for a representative household in each municipality and income class. Ideally, we want to capture tax rate changes due to changes in the legislation (i.e. tax reforms) rather than structural shifts in the population. To this end, we fix the households population as of 2010 (the first year of our panel) and then calculate the tax rate for different household types (by presence of children and marital status) at the maximum income of their respective income class as of 2010. As a result, we have a representative tax rate for each income class and household type across all panel years and municipalities. At this point we shall stress that we observe income from labor and from transfers, but no income from other sources like pensions or capital.[35] As shown by Martínez (2020), labor income is the most important income source, especially up to the 99th percentile. Overall, income composition follows an inverted U shape, with individuals in the lower tail relying more on transfers while those in the upper tail relying more on capital income. Due to the tax schedule progressivity, we may not accurately capture tax rates for the very top of the income distribution.

Public Expenditures

The largest dataset available to us has been collected by a SNSF Sinergia project. This data provides yearly figures on total municipal spending, including a breakdown by functional

[32] Labor market regions are not identified by institutional borders but by commuting patterns within a common labor market. See section 3.4.2.

[33] We collect information from several sources, including the Swiss Federal Tax Administration (ESTV), cantonal laws, cantonal tax offices and "Die Steuern der Schweiz" booklets.

[34] We impute the maximum amount allowed.

[35] Note that we do not observe imputed rental income of homeowners, a feature of the Swiss system that taxes the rental income an homeowner would have received if she was renting her property.

categories: public administration, public security, education, culture, healthcare, social security, transportation and communication, environment, economic development, interests, and fiscal equalization. Total expenditure figures are available for a set of 21 cantons.[36] A functional breakdown for the full panel 2010-2017 is available for a set of 14 cantons (Aargau, Appenzell-Ausserrhoden0, Bern, Basel-Landschaft, Fribourg, Graubünden, Luzern, St. Gallen, Solothurn, Schwyz, Thurgau, Vaud, Valais, Zürich). At least one third of local expenditures are absorbed by schooling services, and in closely located municipalities this represents the probably only non-excludable public good while the use of public facilities is easily accessible from other locations.

Financial support for childcare

We collect publicly available data on the amount paid out to local governments and private associations between the years 2010 and 2017 from the Federal Insurance Office [37]. The data reports the complete list of accepted requests including, the organization asking for financial support, the amount received, the duration (start and end date), and the municipality. We digitalize the information and aggregate the amount of financial support received by each municipality each year. Although we only observe the amount paid out at the end of the period rather than by year, we divided the total financial support received by the duration. The cantonal breakdown is presented in Figure 3.3.1. Between 2010 and 2017, more than CHF 43 million, on average CHF 123,021 per year (~ USD 134K) were paid out across 304 (out of 2,240) municipalities. The financial help ranges from a minimum of CHF 8,880 for the municipality of Rochefort in the canton of Neuchâtel to a maximum of CHF 1,138,461 paid out to the municipality of Tolochenaz in the canton of Vaud. The average duration was 2.5 years.

3.3.2 Who moves and when?

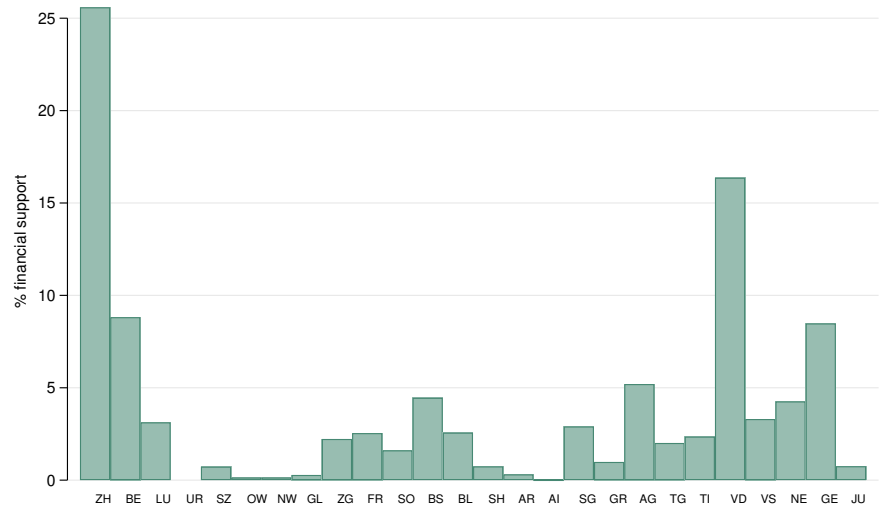
The propensity to move within Switzerland is highly influenced by income, but not only. The simple graphical analysis in Figure 3.3.2 shows that the decision to move residence is highly correlated with the life cycle, as Schmidheiny (2006) suggests.

Panel (a) in Figure 3.3.2 reports the propensity to move depending on age for five different income classes based on the income percentile. Interestingly enough, the bottom income

[36] All but Appenzell-Innerrhoden, Basel-Stadt, Nidwalden, Jura, Zug.

[37] Federal Social Insurance Office (FSIO), https://www.bsv.admin.ch/bsv/fr/home/finanzhilfen/kinderbetreuung.html

Figure 3.3.1: Cantonal distribution of financial support for childcare



Notes: The figure shows how the funancial support for childcare is distributed across cantons between 2010 and 2017.

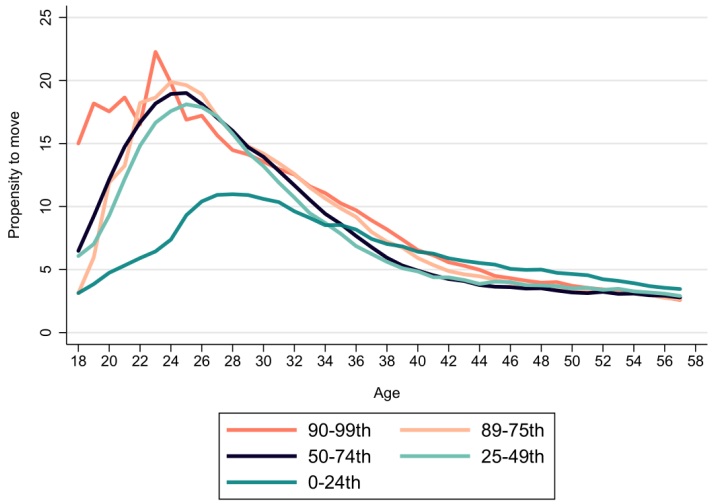
class from the 0 to the 24th percentile has almost a 50% lower propensity to move than the other three quartiles, with the maximum distance in behavior observed at age 24 to 26. After this turning point, the propensity to move starts declining across all income classes, in particular for the upper three quartiles. From the mid-thirties, the probability of a move is more than halved for the top three income classes compared to the period 24 to 26 years of age and aligned to the propensity of the bottom income class. The propensity to move monotonically decreases over the next two decades until 57 years of age, the maximum of our dataset.

Following the intuition in Giannone et al. (2020) we investigate further how the characteristics of destination municipalities compare to those of the origin municipalities. The central elements of the analysis are the tax burden liability of the household and the level of local public expenditures. These factors are key to the location choice model we describe in Section 3.4 and answer the question: how much does access to a certain bundle of local public goods cost to a household? To do that, we first compute a ratio of the two measures and look at the propensity to "move-up", i.e., to move to a destination with a higher ratio.[38] For this exercise, we employ a measure of total per capita public expenditures in each mu-

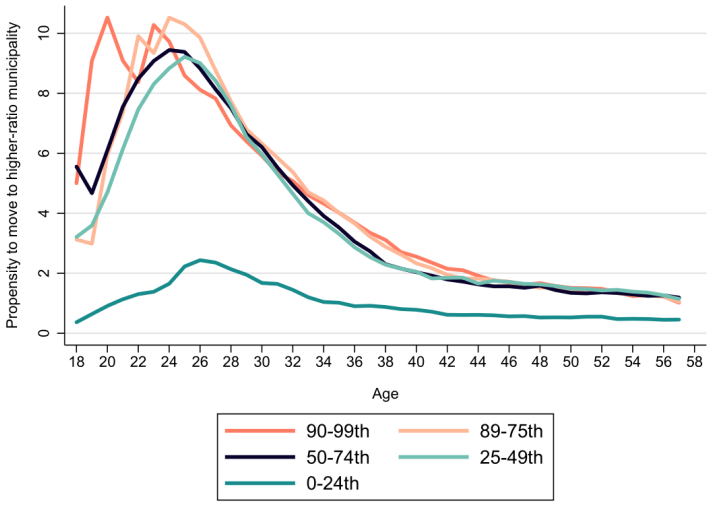
[38] A higher ratio can be attained by either paying a lower tax burden for the same level of local expenditures, or receiving more total local public expenditures per capita for the same price, or both.

Figure 3.3.2: Propensity to move by age and income class

(a) Moving



(b) Moving-up



Notes: The figure shows the propensity to move and to move to a location with a better deal by age and income.

nicipality. Graphically, the results are reported in Panel (b) of Figure 3.3.2. The first striking fact is that the bottom-income class has a propensity to move-up, close to 0 for most of the observed ages, and never reaches the other income classes' level. Our argument for this is that municipalities with higher ratios also have, on average, higher housing prices in which the ratios are capitalized. The higher housing prices are an obstacle to the bottom income class; therefore, they barely can afford to move-up to those municipalities at all stages in life. Households within the 25th and 99th percentiles experience the peak in realized moving-up around 25 years of age. The propensity to move-up steadily declines with age, similarly to the evolution of the overall propensity to move from Panel (b) of Figure 3.3.2.

3.4 Framework and empirical strategy

In this section, we present a simple model of location choice to guide the empirical analysis.

3.4.1 Location choice

We start from a simple location choice model[39] following mainly Moretti and Wilson (2017) and Agrawal and Foremny (2019). Links to similar models can be found in Gabriel et al. (1993), Sasser (2010), and Zhang and Hewings (2019).

Let's o denote the origin municipality, and d the municipality of destination. In any period, households choose the location that maximizes their (expected) utility V among a finite set of mutually exclusive destinations.[40] A household i moves from the origin o (residence in $t-1$) to the alternative municipality d (residence in t) if it can attain a higher utility in municipality d than at any other alternative municipality d', origin municipality included (Herger2013). If the household stays in the same municipality, then the utility at the current municipality of residence outperforms the utility at any other destination d different from the origin o. We define V_{iodt} the utility of a household i moving from o to d in t, V_{ioot}

[39] We rely on the assumption that there exists a fixed households' flow among each pair, reflecting the baseline differences, for example in amenities or different job opportunities, that motivate households to relocate regardless of local policy changes.

[40] The model features a lag between the moving decision (in $t-1$) and the actual move (in t). The household's valuation is implicitly based on expected values for year t.

is the utility from not moving:

$$m_{iodt} = \begin{cases} 1 \; if \; V_{iodt} > V_{iod't}, \forall d' \neq d \\ 0 \; if \; V_{ioot} \geq V_{iodt}, \forall d \neq o \end{cases} \tag{3.1}$$

We will further refer to "Movers" as to households for which $m_{iodt} = 1$. "Stayers" are households for which $m_{iodt} = 0$, instead.
Households maximize the following utility function V_{iodt}, with functions u and v assumed to be separable and suited for the log functional form:

$$V_{iodt} = u(c_{dt}) + v(s_{dt}) + Z_d - C_{od} + e_{iodt} \tag{3.2}$$

In the spirit of Agrawal and Foremny (2019), the utility components are the utility from private goods consumption $u(c_{dt})$, from public goods consumption $v(s_{dt})$, utility given by municipal amenities Z_d, and utility from idiosyncratic preferences for the municipality e_{iodt}. We can think of this term as a personal attachment to a location, e.g. the municipality of birth, or it can embed also unobserved personal taste for lake or mountain view, or closeness to specific cultural regions. Total utility is reduced by a factor $-C_{od}$, representing time-invariant moving costs.[41] We can think of this as the distance between the origin and the destination municipality.

Households spend by assumption all their after-tax wage $(1 - \tau_{dt})w_t$ in private consumption goods.[42] We denote with τ_{dt} the municipal tax rate of the destination municipality. We model public goods consumption as a function of the public goods offer, which we proxy with the per capita public expenditures s_{dt}. After substituting the terms, equation 3.2 reads:

$$V_{iodt} = u((1 - \tau_{dt})w_t) + v(s_{dt}) + Z_d - C_{od} + e_{iodt} \tag{3.3}$$

Utility is determined by structural factors that are subject to external shocks, like tax rates, wages and public goods provision, but also by idiosyncratic factors, allowing for personal shocks, as a change in family status or composition (children), to influence location choice. Equation 3.3 specifies $u(\cdot)$ and $v(\cdot)$ as a log-function to interpret estimated parameters as

[41] The cost factor is assumed to be 0 for stayers, i.e. if $d = o$ then $C_{od} = C_{oo} = 0$.

[42] Note that in our model we treat the variable wage w_t as being location-independent, w_t and not w_{dt}. We are interested in relocation choices that are not wage-driven. This is plausibly the case for the relatively close relocations we observe in the data.

elasticities.

$$\begin{aligned} V_{iodt} &= \ln\left[(1-\tau_{dt})w_t\right] + \ln s_{dt} + Z_d - C_{od} + e_{iodt} \\ &= \ln(1-\tau_{dt}) + \ln w_t + \ln s_{dt} + Z_d - C_{od} + e_{iodt} \end{aligned} \tag{3.4}$$

At this point we shall stress that our model is a non-standard random utility model. Namely, utility is based on a pairwise approach: the utility of the destination municipality is evaluated against the utility of the origin municipality. Hence, the decision to move depends on the origin location at time $t-1$, in line with the literature. A household moving from o to d experiences a loss or gain in utility equal to:

$$\begin{aligned} V_{iodt} - V_{ioot} &= \ln(1-\tau_{dt}) - \ln(1-\tau_{ot}) + \ln s_{dt} - \ln s_{ot} \\ &\quad + (Z_d - Z_o) - (C_{od} - C_{oo}) + (e_{iodt} - e_{ioot}) \\ &= \ln\left[\frac{(1-\tau_{dt})}{(1-\tau_{ot})}\right] + \ln\left(\frac{s_{dt}}{s_{ot}}\right) + (Z_d - Z_o) - C_{od} + (e_{iodt} - e_{ioot}) \end{aligned} \tag{3.5}$$

Equation 3.5 indicates that the probability of moving increases, for example, if the tax rate at destination is lower than at the origin, $1-\tau_{dt} > 1-\tau_{ot}$, or if local services are higher at destination than at origin, $s_{dt} > s_{ot}$, all else equal. The utility differential depends negatively on strong preference for the origin municipality in absence of other changes, i.e. if $e_{ioot} > e_{iodt}$.

3.4.2 Empirical strategy

Our main empirical strategy relies on an aggregate mobility analysis based on the yearly total number of movers within an origin-destination pair. This is particularly useful to benchmark our results against previous studies employing comparable strategies. The analysis is run at the origin-destination pair level, with yearly observations. For each pair, we compute the total number of movers in a given year:

$$Y_{odt} = \sum_{i=1}^{N} m_{iodt} \tag{3.6}$$

where N represents the total number of households in the dataset in year t. The count dependent variable is then regressed on the net-of-tax rate differential and on local public

services differential with a Poisson pseudo-likelihood estimation:

$$E(Y_{odt}|z_o, z_d) = exp(\mathbf{Z'}\beta) \tag{3.7}$$

Where:

$$\mathbf{Z'}\beta = \eta_1 \ln[\frac{(1-\tau_{dt})}{(1-\tau_{ot})}] + \eta_2 \ln\left(\frac{S_{dt}}{S_{ot}}\right) + \beta\prime\mathbf{X} + \gamma_{od} + \gamma_{MSod\times t} + \lambda_t + \varepsilon_{odt}$$

The left-hand side variable is the count of households that relocate in year t. The net-of-tax rate differential is $\frac{(1-\tau_{dt})}{(1-\tau_{ot})}$, while $\ln\left(\frac{S_{dt}}{S_{ot}}\right)$ denotes the local service differential. We add a vector $\mathbf{X}$ of time-varying municipal-level control variable differentials between destination and origin. The vector includes, total local public spending (in per capita, excluding schooling spending), left-wing and right-wing orientation, share of elderly (inhabitants $\geq$ 65 years of age), share of young people (inhabitants $<$ 15 years of age), share of foreign population and median wealth tax differentials. γ_{od} denotes municipality-pair fixed effects and capture unobserved characteristics specific to the municipality pair that are constant over time, such as mobility costs (say, distance between the origin and destination). Note that γ_{od} also absorbs unobserved features of the origin municipality and of the destination municipality, alone.[43] To capture labor market shocks we employ a local labor market-regions[44] pair fixed effect $\gamma_{MSod\times t}$. The latter fixed effect is crucial to exclude confounding effects on mobility from adjustment margins other than the local policy changes, e.g., moving in response to a shock in the labor market. To control for nationwide shocks, we include a year fixed effect λ_t. We are interested in the estimation of η_1, η_2. The two estimates can be directly interpreted as elasticities of aggregate mobility to net-of-tax rate differentials (η_1), and local services (η_2). Our model predicts a positive coefficient for η_1 and η_2. Previous studies often employ conditional logit approaches following (McFadden, 1977) for the estimation of aggregate location choice models. Following the recent developments of the trade (Silva and Tenreyro, 2006) and labor mobility literature (e.g.Brian and Morten, 2019) we prefer to estimate the aggregate analysis model with Poisson pseudo-maximum likelihood (PPML). First, the Poisson estimation exploits all available information from the observations, including the municipality pairs without any movers[45], while a logit model

[43] γ_{od} is equivalent to $\gamma_{od} + \gamma_o + \gamma_d$ in the estimation.

[44] The Swiss Federal Office of Statistics classifies Swiss municipalities according to 106 labor market regions ("MS regions"). Labor market regions are not identified by institutional borders but by commuting patterns within a common labor market. Around 30% of the regions cross between 2 and 5 cantonal borders, grouping between 21 and 69 different municipalities.

[45] We remove from the mobility matrices the combinations where origin equals destination because their cell values are by construction 0 and not as a result of a location choice.

would discard these observations. Second, logit models impose the fairly strong axiom of independence of irrelevant alternatives (IIA). According to this axiom, the probability of choosing a particular location is not influenced by unobservable characteristics the model does not control for Guimarães and Woodward (2004). In other words, IIA requires the error term to be independent across alternatives and households. Clearly, the idiosyncratic and unobservable component e allows for correlation among choices (this can be particularly strong for narrowly located destinations) and poorly fits the IIA, with the risk of producing biased estimates. Third, the computational burden of estimating a logit model is large the larger the set of alternatives (Guimarães and Woodward, 2003)[46], a feature of our mobility matrix for the aggregate analysis, with 2,240 alternatives (origin municipality included) for each of the 2,240 origin municipalities over eight years.[47]

3.4.2.1 Identification

In its simplicity, the aggregate analysis is an appealing way to study household responses to local policies and understand the impact, if any, of explicitly controlling for public goods provision. Our concern is to apply a valid identification strategy to uncover *causal effects* rather than correlations between local policies and household mobility. The pairwise approach has the advantage to consider municipal characteristics (and actions) *relative* to other municipalities rather than in isolation. The elasticity estimation can be more precise because we can take into account changes in local policies in all alternative municipalities, thus better fitting tax competition theory. Only in the pairwise approach changes in a municipality's tax base can be related to the exogenous changes in other municipalities' tax rates.

Following the inclusion of municipality-pair (γ_{dot}) and year fixed effects (λ_t) identification comes from variation in local policies that occurred at different points in time. The policy variation can arise from pairs of municipalities that lie within the same labor market region or across two different labor market regions. If the two tax rates or the local public expenditures are equivalent, there is no contribution to the identification as $ln(1) = 0$. If the differential of a given municipality pair is unchanged over time, e.g., because policies never changed, or because both origin and destination municipalities changed policy in the same year but their differential was unaffected, the pair/year observation does not con-

[46] In a conditional logit model: $P_{iodt} = \frac{\exp(V_{iodt})}{\sum_m \exp(V_{iodt})}$ where $d = 1, ..., D$. The main source of the computational burden is represented by the computation of the denominator for each household in observation.

[47] In total the mobility matrix has 2,240 (origin municipality) × 2,240 (destination municipality)× eight years= 40,140,800 observations, deducing the 2,240 × 8 where the origin is identical to the destination = 40,122,880 observations.

tribute either.

The key threat to identification is that the household's decision to move might be influenced by factors that we do not model explicitly and correlate with local policy changes (omitted variables). No one can deny that Geneva has a great public infrastructure by the lake to enjoy, whereas Basel is home to attractive and well-paid jobs in the pharmaceutical industry. Housing properties in St. Moritz are valued on average three times higher than in Visp, and in Switzerland often moving implies the cost of crossing a linguistic and cultural border. All these more or less observable factors, either specific of the origin, of the destination, or of pair, are taken into consideration by the term γ_{od}. Moreover, the fixed effect $\gamma_{MSod\times t}$ helps pin down mobility reactions that are independent of labor market shocks, because we capture time-varying confounding factors that might affect all municipalities in the same labor market region. Additionally, economic or demographic changes can hit Switzerland as part of international financial, job and trade markets. Such countrywide shocks that might affect local taxation, public goods provision, and relocation decisions are controlled for by λ_t. Overall, we are able to identify a causal effect to the extent that there are no time-varying confounding effects. Identification is improved compared to previous approaches found in the literature. Section 3.B reports additional tables reporting alternative estimations, i.e. a log-linear model applied to municipality pairs with the log count of movers as dependent variable (Table 3.B.6), and a two-way fixed-effects model at municipality level (Table 3.B.7). Our identification outperforms the log-linear model because it exploits variation from municipality pairs that changed from 0 to some positive number of movers or the opposite.[48] A second example is a municipality-level estimation with fixed effects as exploited in Brülhart et al. (2022), in Table 3.B.7. The key advantage compared to the municipality-level estimation is that we can consider changes in tax rates in all alternative municipalities, improving the precision of the elasticity estimation. Our estimation does not outperform the municipality-level in controlling for time-varying potential confounders at municipal level, but it does for pair-invariant confounders.

Our empirical approach minimizes the endogeneity concerns of local policy changes by assuming that migration flows are influenced by permanent and transitory components linked to a location (or location pairs). These components are heterogeneously distributed across locations (or location pairs) and can be factored out with the aggregate analysis.

[48] The log of 0 is unidentified, hence the pair/year observation is discarded from the log-linear model, while it is retained in the Poisson estimation.

3.5 Results

In Table 3.5.1 we report estimation results for equation 3.8. Even columns report results when proxying local services using school expenditures, while odd columns use the differential of financial childcare support. To preserve the sample, in this last model, we replaced each 0 with a symbolic amount of 1.
The estimating equation is run on the full sample and, separately, for each income class.[49,50] Column (1) and column (2) report coefficients for the full sample, columns (3)-(14) report the results for our six income groups. To interpret the results in column (3)-(14), the reader should keep in mind that tax rates and migration flows are group specific, therefore in column (3)-(14), specific migration flows are regressed on group-specific (log) net-of-tax-rate differentials. The last two rows show descriptive statistics of the estimation sample. In addition to the number of observations, we report the number of municipality pairs with positive flows as well as the average number of movers across pairs with positive flows. In line with our descriptive evidence in section 3.3.2, these statistics confirm that relocation patterns are similar across the three upper quartiles of the income distribution.
It is somehow reassuring that we do not find any significant effect for the whole population: overall mobility is not sensitive to net-of-tax rate changes (columns (1)-(2)). Indeed, total mobility flows are regressed on an overall average tax rate, which is by construction not representative. Out of 1,401,192 municipality-pair-year observations, 188,077 exhibits positive flows, and among them, we have on average 2.48 households that relocate each year. Despite the similarity of mobility patterns along the income distribution, mobility reactions to tax rates strongly increase in income. We find negative but not significant reactions at the bottom of the distribution, suggesting that below the median income households do not respond to tax rates.

In line with previous studies, we find positive effects at the top of the distribution. An increase in the net-of-tax rate between a given origin and destination increases the number of households moving from the origin to the destination. Intuitively, if a destination becomes more attractive, either because you pay lower tax at the destination or because you pay higher taxes at the origin (e.g., net-of-tax rate increases at the destination or decreases at the origin), you expect that more households will move from the origin to the destina-

[49] As economists, we give priority to income-analysis rather than age-analysis.

[50] The separate and independent run for each income class is motivated by the excessive computational burden that a unique dataset to run all estimations at once, for example with dummy variables for class assignment, would require.

tion. Taxpayers in the top 10% exhibit large mobility elasticities: a 1% increase in the net-of-tax rate differential increase the number of households moving from the origin to the destination by more than 7% and about 3% for households in the top percentile and in the 90-98th percentile, respectively (columns (3)-(4) and (5)-(6). The weighted average of this effect is slightly above 3; rich households (top 10%) have a mobility elasticity of around 3. Columns (7) to (10) shows that medium-high income classes are also sensitive to tax rates. The elasticity ranges from 1.3 to around 2%. Compared to previous studies, our mobility elasticity for the top 1% is three to four times higher. However, we argue that this can be explained by differences in the methodological approach and differences in the type of migration analyzed. Interestingly, coefficients for schooling expenditures (odd columns) are overall negative, even though very small. Thus, for example, a 1% increase in school expenditures reduces the number of households by 0.04%, which means that school spending, if any, deter households' moves. In contrast, childcare spending motivate households to move in when they increase with an overall small but positive elasticity of 0.0008%.[51]

For the interested reader wondering whether it is the policy changes at destination or at the origin impacting the elasticity estimates, we include an additional table (Table 3.B.5) with separated terms for destination and origin municipalities. This approach relaxes any requirement of symmetry between the two possible "identities" of a municipality. Results suggest that incentives to induce migration to the own municipality (as destination) seem stronger than incentives to reduce out-migration from the own municipality (as origin). The most notable exception is the lowest, and least relocation-prone, income class as we know from the descriptive statistics in section 3.3.2.

[51] Note that, relative to schooling expenditures, childcare spending represents only a small fraction, this could partially explain the small effect size.

Table 3.5.1: The effect of local policies on migration by income class - Baseline specification

	All		Top 1%		98-90th		89-75th		74-50th		49-25th		24-0th	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Δ Net-of-tax rate ($\hat{\eta}_1$)	0.862	0.821	7.290**	7.255**	2.875***	2.833***	2.083**	2.036**	1.405**	1.353**	-0.562	-0.576	-3.233	-3.307
	(0.5386)	(0.5392)	(2.9006)	(2.9080)	(1.0755)	(1.0751)	(0.9321)	(0.9311)	(0.6572)	(0.6575)	(0.6732)	(0.6739)	(2.4224)	(2.4179)
Δ School Expenditures ($\hat{\eta}_2$)	-0.0437***		-0.0295		-0.0649**		-0.0825***		-0.0495***		-0.0240		-0.0591***	
	(0.0105)		(0.0732)		(0.0302)		(0.0213)		(0.0139)		(0.0156)		(0.0222)	
Δ Subsidy ($\hat{\eta}_2$)		0.000805**		0.00209		0.00280***		0.000606		0.000681		0.000353		0.00116*
		(0.0003)		(0.0025)		(0.0009)		(0.0006)		(0.0004)		(0.0005)		(0.0006)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MS region pair × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municpality pair FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,401,192	1,402,373	21,306	21,315	205,831	206,039	410,450	410,917	731,551	732,219	631,960	632,513	354,598	355,017
Pairs with movers	188,077	188,468	3,437	3,440	29,094	29,154	56,447	56,589	99,455	99,657	87,072	87,256	50,137	50,254
Average movers	2.41	2.41	1.14	1.14	1.35	1.35	1.50	1.50	1.81	1.81	1.65	1.65	1.50	1.50

Notes: Each cell in this table reports the coefficient of a Poisson pseudo-likelihood regression of the number of movers from o to d on a measure of (category specific) net-of-tax rate and of local services differential (in log).
All specifications includes Municipality Pairs, Year and MS Region of Destination × MS Region of origin × Year Fixed Effects. Standard errors in parentheses, with three-way clustering by origin municipality × year, destination municipality × year, and municipality pair. Municipal covariates are: public spending (excluding school spending), unemployment, left-wing and right-wing orientation, share of people ≥ 65y, share of foreign population, share of people ≤15y, median wealth tax. Subsidy values of 0 have been replaced by a symbolic 1.
Column (1) and column (2) include all household heads and use a weighted tax rate. Column (2) -(14) limits the sample to household heads at a given level of the national income distribution and use the average net of tax rate of the specific group.
$^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$.

Building on the intuition that heterogeneous households have heterogeneous preferences for public goods (Brülhart et al., 2022), we further investigate the effect of children's presence in the households by analyzing responses of households with children in primary school age (younger than 12 years of age) and without children in primary school age. Because primary school is mostly provided by the municipality of residence, we believe that reactions to school expenditures can be heterogeneous across these two subgroups.

Panel A in Table 3.5.2 presents the results for households with children, while Panel B shows the results for households without children, as before migration flow and tax rates are group specific. Overall, the mobility elasticity to net-of-tax rate estimation (η_1) is primarily due to households without children, and this preliminary result is robust to the exclusion of public expenditures, see Table 3.B.4. Indeed, panel B displays quantitatively similar results as Table 3.5.1, Panel A. Column (5) reports in Panel A a precise estimate of counter intuitive magnitudes for η_2 compared to Panel B. We would have expected that households with children dislike public expenditures in schooling less than their counterparts without children, while the estimates suggest the opposite. Another unexpected result is reported in column (4) and column (12). As before, one would expect a positive effect for households with children in school-age and no effect for families without children in school-age. However, two remarks are worth here. First, Panel A investigates the behavior of households without children between 0 and 12 years of age. Hence, not having young children today does not exclude having children in the future. The effect may be influenced by households who are going to have children soon (anticipation effect). Second, the sample sizes are substantially different between Panel A and Panel B. Therefore, we argue that results in Panel A have to be interpreted with caution.

Table 3.5.2: The effect of local policies on migration by income class and parenting status - Baseline specification

	Top 1%		98-90th		89-75th		74-50th		49-25th		24-0th	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: without children < 12 **y.o**												
Δ Net-of-tax rate ($\hat{\eta}_1$)	0.300	0.422	3.898***	3.839***	2.539**	2.511**	1.386**	1.332*	-0.304	-0.305	-4.251	-4.335
	(3.9023)	(3.9010)	(1.3018)	(1.2984)	(1.0302)	(1.0289)	(0.7055)	(0.7054)	(0.7049)	(0.7054)	(2.7250)	(2.7202)
Δ School Expenditures ($\hat{\eta}_2$)	0.175		-0.0663*		-0.0652***		-0.0496***		-0.0245		-0.0661***	
	(0.1084)		(0.0363)		(0.0227)		(0.0146)		(0.0160)		(0.0229)	
Δ Subsidy ($\hat{\eta}_2$)		0.00346		0.00358***		0.000740		0.000684		0.000495		0.00145**
		(0.0035)		(0.0011)		(0.0007)		(0.0005)		(0.0005)		(0.0007)
Observations	13,001	13,010	156,562	156,714	351,260	351,628	684,207	684,817	597,634	598,159	325,853	326,251
Pairs with movers	2,222	2,225	22,402	22,448	48,591	48,703	93,105	93,294	82,517	82,691	46,269	46,381
Average movers	1.11	1.11	1.30	1.30	1.46	1.46	1.79	1.79	1.64	1.64	1.49	1.49
Panel B: with children < 12 **y.o**												
Δ Net-of-tax rate ($\hat{\eta}_1$)	15.39***	15.24***	0.745	0.730	-0.160	-0.254	2.279	2.224	-4.020	-4.119	4.205	4.210
	(4.9537)	(4.9757)	(1.8616)	(1.8602)	(1.6520)	(1.6535)	(1.8666)	(1.8657)	(2.8823)	(2.8781)	(7.6580)	(7.6554)
Δ School Expenditures ($\hat{\eta}_2$)	-0.149		-0.0517		-0.163***		-0.0678		-0.0210		-0.0177	
	(0.1260)		(0.0539)		(0.0446)		(0.0424)		(0.0578)		(0.0726)	
Δ Subsidy ($\hat{\eta}_2$)		-0.000725		0.00111		0.000265		0.000665		-0.00161		-0.00240
		(0.0044)		(0.0017)		(0.0014)		(0.0013)		(0.0017)		(0.0023)
Observations	8,287	8,287	71,241	71,331	110,785	110,972	129,621	129,778	77,329	77,440	46,787	46,842
Pairs with movers	1,392	1,392	10,513	10,543	15,798	15,854	18,430	18,481	11,634	11,668	7,529	7,547
Average movers	1.07	1.07	1.16	1.16	1.15	1.15	1.14	1.14	1.08	1.08	1.07	1.07
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MS region pair × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municpality pair FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Each cell in this table reports the coefficient of a Poisson pseudo-likelihood regression of the number of movers from o to d on a measure of (category specific) net-of-tax rate and of local services differentials (in log) for the set of households with children (Panel A) and without children (Panel B) under 12 years of age.
All specifications includes Municipality Pairs, Year and MS Region of Destination × MS Region of origin × Year Fixed Effects. Standard errors in parentheses, with three-way clustering by origin municipality × year, destination municipality × year, and municipality pair. Municipal covariates are: public spending (excluding school spending), unemployment, left-wing and right-wing orientation, share of people ≥ 65y, share of foreign population, share of people ≤15y, median wealth tax. Subsidy values of 0 have been replaced by a symbolic 1.

*$p < 0.10$,**$p < 0.05$, ***$p < 0.01$.

3.5.1 School spending: a tale of several factors?

Our preliminary results motivated us to have a closer look at local school spending. In the next section, we report the results of what we have learned from an additional investigation on the topic to figure out the mechanisms behind the negative elasticity.

First, we shall recognize that the crude measure of public expenditure in the baseline regression might not perfectly mirror the services perceived by the population. Indeed, input into the educational process does not necessarily reflect outcomes. Ideally, we should exploit a spatially comparable measure of public services representative of households' valuations. The lowest this ideal measure correlates with *our* measure, the largest the potential bias in the estimates. In addition to that, more than 90% of the schooling expenditures are current expenditures rather than investments, for example, into physical school infrastructure. They are largely devoted to teachers' salaries and rents for school buildings[52], and only partially observable by households.

Second, as recently highlighted by Jackson (2018), Jackson et al. (2015), the source of variation of school expenditures is often unclear and not well understood. A reverse causality problem might arise if households self-select into neighborhoods because of school spending, and if school spending is also a function of family background. Therefore, credible studies should use *exogenous* variation in spending rather than any variation available.

Distance between municipalities could represent a third factor that contaminates the relationship between local schooling expenditures and available public services. Spillover effects might arise for closely located municipalities. At short distances, a household does not need relocation to enjoy a different level or type of public service. To test this, we run the baseline empirical estimation with the full sample for different distances between origin and destination municipality does not support this claim, see Table 3.5.3.

[52] See report "Öffentliche Bildungsausgaben 2005-2014", FSO.

Table 3.5.3: Public spending elasticity for municipality pairs at different distances

	0-15 km		15-30 km		30-60 km		60 km and more	
Δ Net-of-tax rate ($\hat{\eta}_1$)	0.918	0.858	0.969	0.900	1.279	1.274	-0.233	-0.247
	(0.6709)	(0.6736)	(0.9876)	(0.9858)	(1.1947)	(1.1940)	(1.4394)	(1.4378)
Δ School Expenditures ($\hat{\eta}_2$)	-0.0481***		-0.0486***		-0.0240		-0.0498**	
	(0.0136)		(0.0185)		(0.0203)		(0.0231)	
Δ Subsidy ($\hat{\eta}_2$)		0.000766*		0.00167***		0.000646		-0.000747
		(0.0004)		(0.0006)		(0.0007)		(0.0008)
Observations	266,067	266,181	338,767	339,046	380,622	380,925	398,468	398,964
Pairs with movers	34,359	34,397	43,974	44,069	50,260	50,356	59,032	59,194
Average movers	4.14	4.14	2.13	2.13	1.55	1.55	1.33	1.33

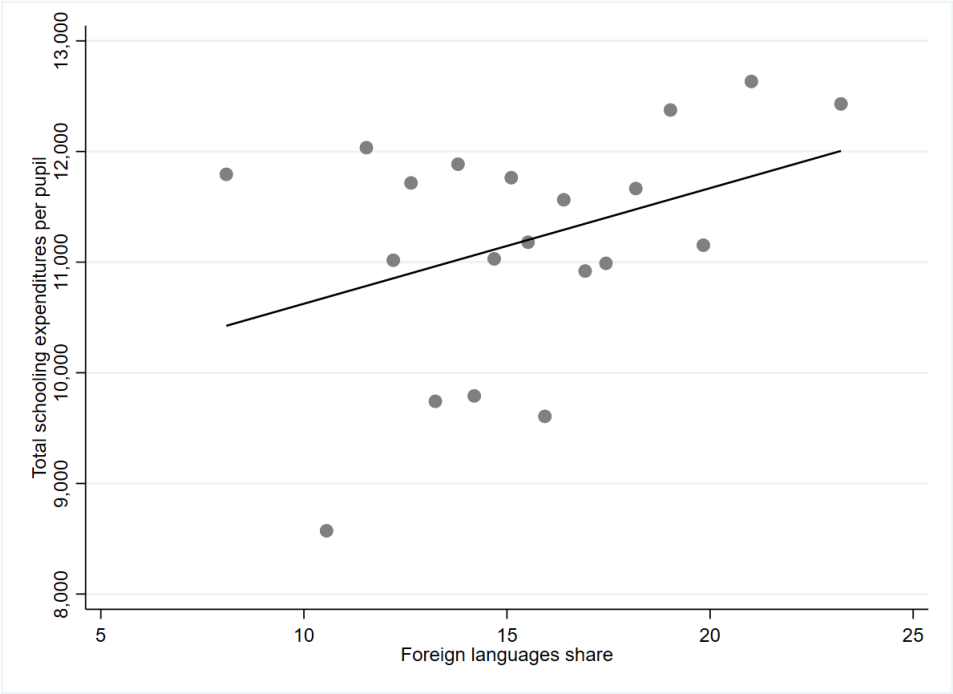
Notes: Each cell in this table reports the coefficient of a Poisson pseudo-likelihood regression of the number of movers from o to d on a measure of net-of-tax rate and of local services differentials (in log) for municipality pairs with origin and distance located at a distance between 0 and 15km (endpoint not included), 15 to 30km, 30 to 60km, and 60km and more. Results are reported for the full sample, i.e. we include all household heads and use a weighted tax rate.
All specifications include Municipality Pairs, Year and MS Region of Destination × MS Region of origin × Year Fixed Effects. Standard errors in parentheses, with three-way clustering by origin municipality × year, destination municipality × year, and municipality pair. Municipal covariates are: public spending (excluding school spending), unemployment, left-wing and right-wing orientation, share of people ≥ 65y, share of foreign population, share of people ≤15y, median wealth tax. Subsidy values of 0 have been replaced by a symbolic 1.

$^{*}p < 0.10$,$^{**}p < 0.05$, $^{***}p < 0.01$.

Fourth, spending variation - and spending levels - are likely to depend on the population's needs. Higher expenditures might reflect higher needs, for example, if there is a high share of pupils population that does not speak the local language.[53] Figure 3.5.1 suggests that the level of per-pupil expenditures in a jurisdiction (here, districts) positively correlates with the share of population that speaks a foreign mother tongue, i.e. different than German, French, Italian, or English.

[53] Information on the population share that speaks a different language than the local one is available only at district level. For this reason, we do not include this measure in the baseline estimation, but rather the share of foreign population.

Figure 3.5.1: School spending and foreign language population share



Notes: This figure presents the correlation between the schooling per pupil expenditure used in the estimation and the share of foreign languages, i.e. different than German, French, Italian, or English. All variables are at district level. Correlation is estimated after partialling out the effect of the covariates (in levels) from the baseline estimation.

An additional element that is worth exploring is whether the type of municipality, rural or city, plays a role. The municipality type strongly determines the associated services in the schooling sector, and therefore might confound the mobility estimates we are interested in. Table 3.5.4 reports a test of the baseline estimation on municipality pairs where at least one municipality is a city, and table 3.5.5 where both municipalities are rural[54]. Both results lead us to discard the idea that the effect is contaminated by the municipality type: the sign of local schooling services elasticity is not sensitive to this.

[54]We follow here the EUROSTAT classification of "Greater City", https://ec.europa.eu/eurostat/web/cities/spatial-units. In Switzerland, 12 municipalities are classified as cities: Zurich, Winterthur, Bern, Bienne, Thun, Luzern, Zug, Basel, St.Gallen, Lugano, Lausanne and Geneva. For the definition of rural municipalities we follow the classification by the FSO.

Table 3.5.4: Elasticity estimates by municipality type - urban

	All		Top 1%		98-90th		89-75th		74-50th		49-25th		24-0th	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Δ Net-of-tax rate ($\hat{\eta}_1$)	0.200	0.202	5.191	5.511	4.707**	4.713**	2.876	2.760	-1.116	-1.091	-1.647	-1.659	-2.470	-2.659
	(1.3933)	(1.4035)	(7.7539)	(7.7787)	(2.3418)	(2.3548)	(2.7888)	(2.7832)	(1.9057)	(1.9185)	(1.7058)	(1.7076)	(5.8374)	(5.8405)
Δ School Expenditures ($\hat{\eta}_2$)	-0.0346		-0.104		-0.0685		-0.125**		-0.0322		-0.0141		-0.0434	
	(0.0228)		(0.1482)		(0.0639)		(0.0519)		(0.0338)		(0.0361)		(0.0520)	
Δ Subsidy ($\hat{\eta}_2$)		0.00127*		0.00743		0.00690***		0.0000671		0.00152		-0.000245		-0.000643
		(0.0007)		(0.0059)		(0.0020)		(0.0015)		(0.0011)		(0.0010)		(0.0016)
Observations	97,978	98,020	3,422	3,422	22,374	22,394	39,002	39,020	55,933	55,954	58,417	58,444	37,002	37,014
Pairs with movers	5,559	5,563	496	496	2,227	2,229	3,256	3,258	3,904	3,908	3,963	3,967	5,559	5,563
Average movers	5.21	5.21	1.34	1.34	2.09	2.09	2.52	2.52	3.30	3.30	2.85	2.85	2.35	2.35

Notes: Each cell in this table reports the coefficient of a Poisson pseudo-likelihood regression of the number of movers from o to d on a measure of (category specific) net-of-tax rate and of local services differentials (in log) for municipality pairs where either origin or destination, or both, are classified as cities by EUROSTAT.
All specifications include Municipality Pairs, Year and MS Region of Destination × MS Region of origin × Year Fixed Effects. Standard errors in parentheses, with three-way clustering by origin municipality × year, destination municipality × year, and municipality pair. Municipal covariates are: public spending (excluding school spending), unemployment, left-wing and right-wing orientation, share of people ≥ 65y, share of foreign population, share of people ≤15y, median wealth tax. Subsidy values of 0 have been replaced by a symbolic 1.
Column (1) include all household heads and use a weighted tax rate. Column (2) -(7) limits the sample to household heads at a given level of the national income distribution and use the average net of tax rate of the specific group.
$^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$.

Table 3.5.5: Elasticity estimates by municipality type - rural

	All		Top 1%		98-90th		89-75th		74-50th		49-25th		24-0th	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Δ Net-of-tax rate ($\hat{\eta}_1$)	2.814***	2.666***	14.65	14.49	2.499	2.130	8.101***	7.932***	2.040*	1.910*	2.232*	2.168	-1.653	-2.347
	(0.9035)	(0.9079)	(12.7051)	(12.6506)	(3.1841)	(3.1751)	(1.7863)	(1.7893)	(1.1277)	(1.1301)	(1.3538)	(1.3607)	(4.8421)	(4.8565)
Δ School Expenditures ($\hat{\eta}_2$)	-0.103***		-0.231		-0.266**		-0.201***		-0.0405		-0.124***		-0.169***	
	(0.0233)		(0.8739)		(0.1261)		(0.0632)		(0.0341)		(0.0378)		(0.0617)	
Δ Subsidy ($\hat{\eta}_2$)		0.00180*		-0.00330		0.00470		0.00291		0.00274**		0.000191		0.00329
		(0.0009)		(0.0243)		(0.0040)		(0.0021)		(0.0012)		(0.0015)		(0.0022)
Observations	267,943	268,315	583	583	19,160	19,206	60,081	60,183	142,880	143,079	122,624	122,828	60,830	60,951
Pairs with movers	9,143	9,179	103	105	1,350	1,358	2,946	2,961	4,901	4,921	4,575	4,591	9,143	9,179
Average movers	1.79	1.79	1.00	1.00	1.07	1.07	1.18	1.18	1.46	1.46	1.33	1.33	1.21	1.21

Notes: Each cell in this table reports the coefficient of a Poisson pseudo-likelihood regression of the number of movers from o to d on a measure of (category specific) net-of-tax rate and of local services differentials (in log) for municipality pairs where both origin and destination are classified as rural by the FSO.
All specifications include Municipality Pairs, Year and MS Region of Destination × MS Region of origin × Year Fixed Effects. Standard errors in parentheses, with three-way clustering by origin municipality × year, destination municipality × year, and municipality pair. Municipal covariates are: public spending (excluding school spending), unemployment, left-wing and right-wing orientation, share of people ≥ 65y, share of foreign population, share of people ≤15y, median wealth tax. Subsidy values of 0 have been replaced by a symbolic 1.
Column (1) include all household heads and use a weighted tax rate. Column (2) -(7) limits the sample to household heads at a given level of the national income distribution and use the average net of tax rate of the specific group.
$^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$.

3.6 Robustness

3.6.1 Dynamic effects

Our estimates could be invalidated if local policies are not exogenous. Indeed, crucial for our purpose is the strict exogeneity of tax rates and public expenditures' variation. A common approach for diagnosing such an issue is to test for pre-trends. In this section, we use a distributed lag model[55] to uncover the timing of the effect and to further test our estimation strategy against possible anticipation effects. Table 3.6.1 and Table 3.6.2 report the baseline estimation with the inclusion of one lead and one lag of each of the regressors of interest, schooling expenditures in Table 3.6.1 and the federal subsidy in Table 3.6.2, for all income classes. Panel A reports the long-term effect, i.e. the sum of all three coefficients, whereas Panel B reports the single coefficients from the distributed lag model. Results reported in Table 3.6.1 do not indicate pre-treatment effects of the net-of-tax rate (coefficients of NTR_{t+1} (leads) are not significant).
The positive coefficients of local expenditures at $t-1$ (lags) suggest that mobility reactions to local expenditures are not immediate. This could be due to a lag between public spending shock and public goods' realization. The effect of an exogenous subsidy is predominantly positive across the periods around t. In the long term, our estimates suggest a small effect of opposite signs for local expenditures and federal subsidy.

Somebody might argue that pre-trends are undetected due to limited statistical power (Freyaldenhoven et al., 2019), we argue that this is not the case: tax multipliers are the main source of variation across municipalities, and they are usually set at the end of the year for the following year. Therefore, they are likely to be unknown far in advance.

[55] See Schmidheiny and Siegloch (2020) for equivalence of event studies and distributed lag.

Table 3.6.1: Dynamic and long-term effects

		Top 1%	98-90th	89-75th	74-50th	49-25th	24-0th
		(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Long Term Effect							
Δ Net-of-tax rate ($\hat{\eta}_1$)		8.276**	2.780**	2.579**	1.520*	-1.135	-4.306
		(3.879)	(1.337)	(1.148)	(0.800)	(0.838)	(3.190)
Δ Schooling Exp ($\hat{\beta}_2$)		0.078	-0.014	-0.103***	-0.057***	-0.054**	-0.024
		(0.128)	(0.046)	(0.030)	(0.019)	(0.021)	(0.032)
Panel B: Distributed Lag							
Δ NTR$_{t+1}$	t+1	1.420	1.248	0.730	0.400	-0.256	-3.535
		(3.874)	(1.381)	(1.028)	(0.762)	(0.872)	(3.578)
Δ Schooling Exp$_{t+1}$	t+1	0.142	0.00132	-0.0411	-0.0166	-0.0137	0.0156
		(0.092)	(0.039)	(0.026)	(0.018)	(0.019)	(0.030)
Δ NTR$_t$	t	3.857	2.177	2.734**	0.868	-0.0209	-2.245
		(3.962)	(1.494)	(1.267)	(0.957)	(1.046)	(3.712)
Δ Schooling Exp$_t$	t	-0.0273	-0.0862**	-0.0746***	-0.0658***	-0.0301	-0.0739**
		(0.083)	(0.037)	(0.027)	(0.019)	(0.020)	(0.031)
Δ NTR$_{t-1}$	t-1	2.999	-0.645	-0.886	0.252	-0.858	1.474
		(3.032)	(1.183)	(1.077)	(0.792)	(0.920)	(2.667)
Δ Schooling Exp$_{t-1}$	t-1	-0.0365	0.0711**	0.0123	0.0255	-0.0100	0.0338
		(0.097)	(0.035)	(0.024)	(0.016)	(0.016)	(0.026)
Observations		20,580	192,427	380,760	677,304	583,476	327,661

Notes: Each cell in this table reports the coefficient of a Poisson pseudo-likelihood regression of the number of movers from o to d on a measure of (category specific) net-of-tax rate and of schooling expenditures differential (in log) in a distributed lag model with 1 lead and 1 lag. The long-term effect reported in Panel A is the sum of all three coefficients.
All specifications includes Municipality Pairs, Year and MS Region of Destination × MS Region of origin × Year Fixed Effects. Standard errors in parentheses, with three-way clustering by origin municipality × year, destination municipality × year, and municipality pair. Municipal covariates are: public spending (excluding school spending), unemployment, left-wing and right-wing orientation, share of people ≥ 65y, share of foreign population, share of people ≤15y, median wealth tax.

*$p < 0.10$,**$p < 0.05$, ***$p < 0.01$.

Table 3.6.2: Dynamic and long-term effects

		Top 1%	98-90th	89-75th	74-50th	49-25th	24-0th
		(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Long Term Effect							
Δ Net-of-tax rate ($\hat{\eta}_1$)		10.061***	3.224**	2.225**	1.524*	-0.810	-4.500
		(3.800)	(1.304)	(1.116)	(0.793)	(0.815)	(3.106)
Δ Subsidy ($\hat{\beta}_2$)		0.002	0.003**	0.001	0.001*	0.000	0.001
		(0.004)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Panel B: Distributed Lag							
Δ NTR$_{t+1}$	t+1	2.321	1.094	0.656	0.408	-0.0644	-3.773
		(3.785)	(1.357)	(0.995)	(0.752)	(0.870)	(3.536)
Δ Subsidy$_{t+1}$	t+1	-0.000138	0.0000447	0.00104	0.000570	0.000133	-0.000228
		(0.003)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Δ NTR$_t$	t	4.215	2.293	2.402*	0.764	-0.194	-2.061
		(3.960)	(1.480)	(1.247)	(0.944)	(1.043)	(3.664)
Δ Subsidy$_t$	t	0.00295	0.00303***	0.000158	0.000413	0.000174	0.00146*
		(0.003)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Δ NTR$_{t-1}$	t-1	3.525	-0.163	-0.833	0.352	-0.552	1.334
		(3.000)	(1.176)	(1.050)	(0.786)	(0.906)	(2.646)
Δ Subsidy$_{t-1}$	t-1	-0.00100	0.0000580	0.0000707	0.000119	0.0000848	-0.000461
		(0.003)	(0.001)	(0.001)	(0.000)	(0.001)	(0.001)
Observations		21,160	204,069	406,590	724,337	624,319	351,391

Notes: Each cell in this table reports the coefficient of a Poisson pseudo-likelihood regression of the number of movers from o to d on a measure of (category specific) net-of-tax rate and of subsidy differential in a distributed lag model with 1 lead and 1 lag. The long-term effect reported in Panel A is the sum of all three coefficients.
All specifications includes Municipality Pairs, Year and MS Region of Destination × MS Region of origin × Year Fixed Effects. Standard errors in parentheses, with three-way clustering by origin municipality × year, destination municipality × year, and municipality pair. Municipal covariates are: public spending (excluding school spending), unemployment, left-wing and right-wing orientation, share of people ≥ 65y, share of foreign population, share of people ≤15y, median wealth tax. Subsidy values of 0 have been replaced by a symbolic 1.

*$p < 0.10$,**$p < 0.05$, ***$p < 0.01$.

3.6.2 Sensitivity tests

In this section we test the sensitivity of our results to different specifications. Table 3.6.3 and Table 3.6.4 confirm that the baseline results presented in Table 3.5.1 and Table 3.5.2, respectively, are not due to the choice of the fixed effects.

Table 3.6.3: The effect of local policies on migration: sensitivity to different specifications

	Top 1%															
	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)	
Δ Net-of-tax rate ($\hat{\eta}_1$)	3.550*	3.564*	4.957***	4.975***	3.931**	3.945**	6.128***	6.160***	7.290**	7.255**	4.916*	4.888*	3.451	3.470	6.472**	6.549**
	(1.8744)	(1.8759)	(1.8086)	(1.8122)	(1.9116)	(1.9174)	(1.9998)	(1.9977)	(2.9006)	(2.9080)	(2.7014)	(2.7068)	(2.6276)	(2.6317)	(2.8792)	(2.8574)
Δ School Expenditures ($\hat{\eta}_2$)	0.00520		0.0315		0.00956		0.0505		-0.0295		-0.0103		-0.0184		0.179	
	(0.0618)		(0.0671)		(0.0660)		(0.0686)		(0.0732)		(0.0690)		(0.1012)		(0.1299)	
Δ Subsidy ($\hat{\eta}_2$)		0.00157		0.00161		0.00132		0.00161		0.00209		0.00161		0.00234		-0.00177
		(0.0017)		(0.0023)		(0.0023)		(0.0023)		(0.0025)		(0.0024)		(0.0039)		(0.0042)
Origin, Destination FE	Yes		No		No		No		No		No		No		No	
Mun Pair FE	No		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Origin MS region × Year FE	No		No		Yes		No		No		No		No		No	
Destination MS region × Year FE	No		No		No		Yes		No		No		No		No	
MS Region Pair × Year FE	No		No		No		No		Yes		No		No		No	
Canton Pair × Year FE	No		No		No		No		No		Yes		No		No	
Origin Mun × Year FE	No		No		No		No		No		No		Yes		No	
Destination Mun × Year FE	No		No		No		No		No		No		No		Yes	
Obs.	6,640,116	6,657,594	31,372	31,387	30,817	30,832	30,971	30,986	21,306	21,315	29,605	29,617	20,706	20,713	20,608	20,608

Notes: Each cell in this table reports the coefficient of Poisson pseudo-likelihood regression of the number of movers from o to d on a measure of (category specific) net-of-tax rate and of local services differential (in log).
Standard errors in parentheses, with three-way clustering by origin municipality × year, destination municipality × year, and municipality pair. Municipal covariates are: public spending (excluding school spending), unemployment, left-wing and right-wing orientation, share of people ≥ 65y, share of foreign population, share of people ≤15y, median wealth tax. Subsidy values of 0 have been replaced by a symbolic 1.

*$p < 0.10$,**$p < 0.05$, ***$p < 0.01$.

Table 3.6.4: The effect of local policies on migration: sensitivity to different specifications

	90-98 th															
	(1)		(2)		(3)		(4)		(5)		(6)		(7)		(8)	
Δ Net-of-tax rate ($\hat{\eta}_1$)	1.092	1.000	1.425*	1.302*	1.464*	1.324	1.480*	1.399*	2.875***	2.833***	2.529**	2.436**	2.003*	1.818*	-0.229	-0.334
	(1.5145)	(1.5370)	(0.7736)	(0.7734)	(0.8297)	(0.8292)	(0.8169)	(0.8143)	(1.0755)	(1.0751)	(1.0289)	(1.0292)	(1.0367)	(1.0383)	(1.0444)	(1.0372)
Δ School Expenditures ($\hat{\eta}_2$)	-0.0708		-0.0930***		-0.102***		-0.0749***		-0.0649**		-0.0738**		-0.143***		-0.0552	
	(0.0447)		(0.0270)		(0.0268)		(0.0256)		(0.0302)		(0.0292)		(0.0388)		(0.0379)	
Δ Subsidy ($\hat{\eta}_2$)		0.00226***		0.00231**		0.00212**		0.00261***		0.00280***		0.00235***		0.00421***		-0.000481
		(0.0005)		(0.0009)		(0.0009)		(0.0009)		(0.0009)		(0.0009)		(0.0013)		(0.0013)
Origin, Destination FE	Yes		No		No		No		No		No		No		No	
Mun Pair FE	No		Yes		Yes		Yes		Yes		Yes		Yes		Yes	
Origin MS region × Year FE	No		No		Yes		No		No		No		No		No	
Destination MS region × Year FE	No		No		No		Yes		No		No		No		No	
MS Region Pair × Year FE	No		No		No		No		Yes		No		No		No	
Canton Pair × Year FE	No		No		No		No		No		Yes		No		No	
Origin Mun × Year FE	No		No		No		No		No		No		Yes		No	
Destination Mun × Year FE	No		No		No		No		No		No		No		Yes	
Obs.	22,625,413	22,638,897	235,989	236,187	235,831	236,029	235,870	236,068	205,831	206,039	234,536	234,734	218,264	218,475	219,229	219,451

Notes: Each cell in this table reports the coefficient of Poisson pseudo-likelihood regression of the number of movers from o to d on a measure of (category specific) net-of-tax rate and of local services differential (in log).
Standard errors in parentheses, with three-way clustering by origin municipality × year, destination municipality × year, and municipality pair. Municipal covariates are: public spending (excluding school spending), unemployment, left-wing and right-wing orientation, share of people ≥ 65y, share of foreign population, share of people ≤15y, median wealth tax. Subsidy values of 0 have been replaced by a symbolic 1.

*$p < 0.10$,**$p < 0.05$, ***$p < 0.01$.

We carry out a second sensitivity check to test the baseline results on mobility elasticity to net-of-tax rate against the inclusion of local expenditures, results are reported in Table 3.B.4. We run equation 3.8 excluding η_2 and note that the most reactive income classes are again the top quartile (except the top 1% as we have already commented), and the driving subsample is made of households without children (see Panel B of Table 3.B.4). This reinforces the intuition that, in the long term, schooling are likely not consistently driving relocation decisions across most income classes.

3.7 Conclusion

This work analyzes mobility reactions to local policies in Switzerland. Using data on around 1,500,000 moves, we quantify mobility elasticities to local taxes and local expenditures. Our strategy relies on a flow model of migration: mobility flows at the municipality pair level are regressed on net-of-tax rate and net-of-local expenditures differentials, and we control for constant flows across pair and time and regional pair time trends. Additionally, we include a set of municipality-level controls to control for local shocks at pair level. We assume that absent tax and public expenditures changes, region pair mobility flows are fixed over time. We confirm results from previous literature; indeed, we find that wealthy taxpayers are highly sensitive to tax rates, with an estimated elasticity of 6.7. Furthermore, we also find substantial responses for the first quartile of the distribution. We find negative responses to schooling expenditure predominant to households without children in school age. We investigate possible explanations and acknowledge that the measure of local expenditures has some limits as proxy for local public goods provision.

Although the top 1% of taxpayers is the most reactive group, we provide evidence that tax-induced migration is not exclusive to wealthy taxpayers. Implications for the revenue collection are not yet investigated.

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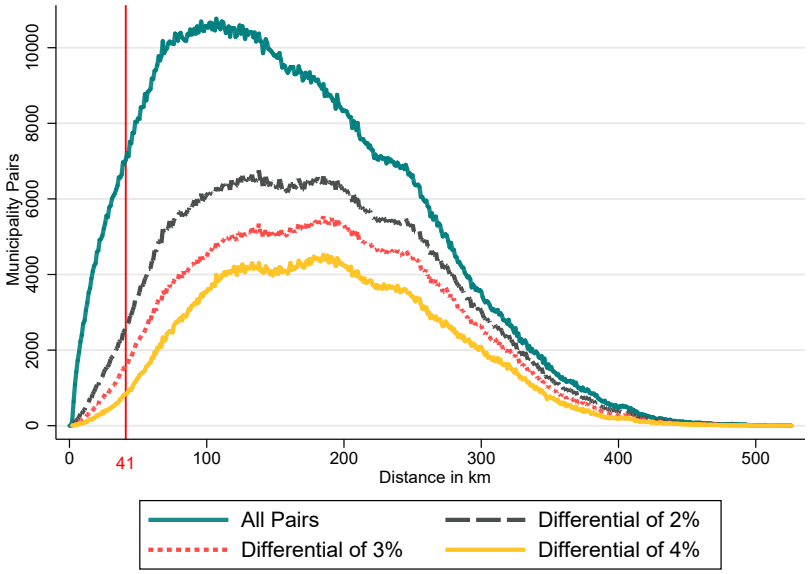
Appendix

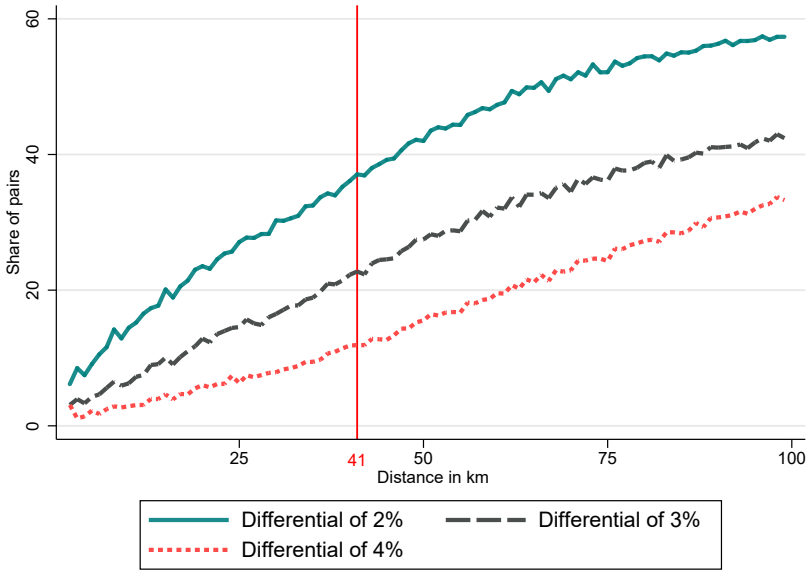
3.A Supplementary Figures

Figure 3.A.1: Tax rate variation and distance

(a) Pairs of municipalites



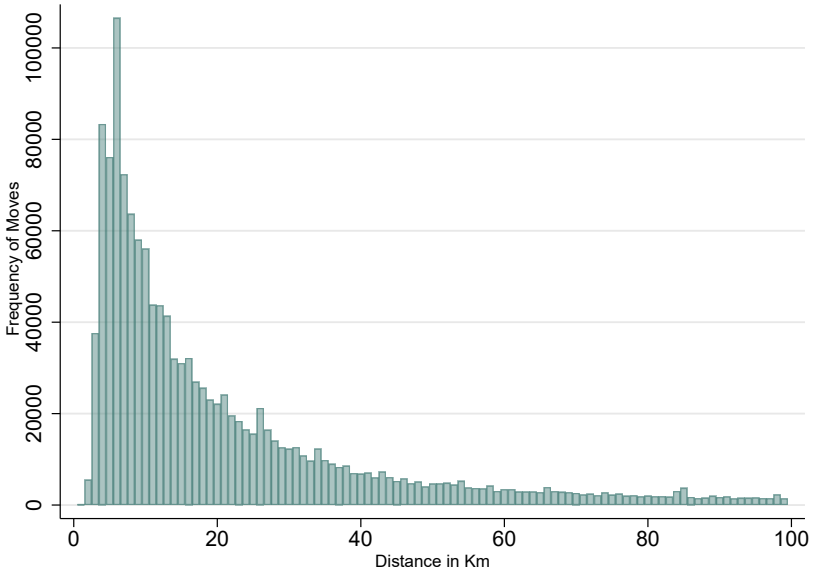
(b) Share of municipality pairs



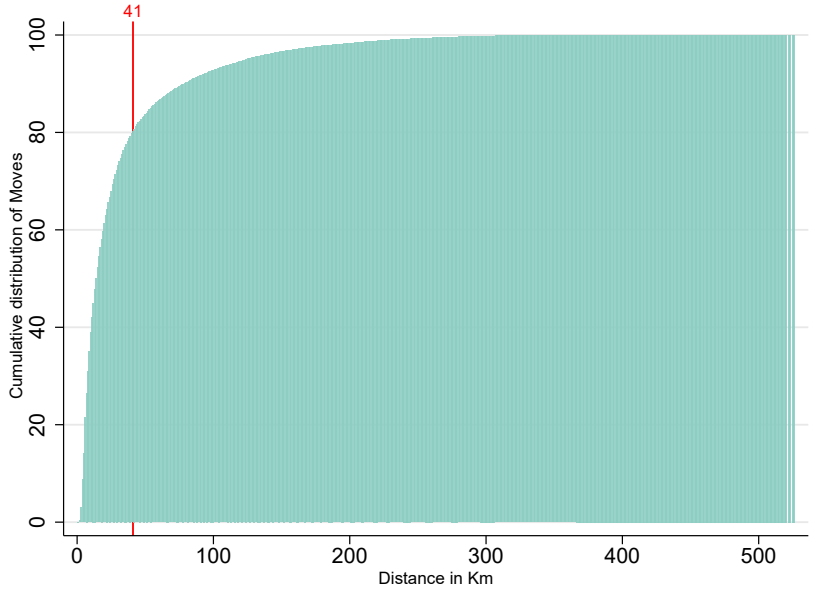
Notes: The total number of pairs is 2,240×2,240-2,240.

Figure 3.A.2: Movements and distance

(a) Distribution of moves across distance

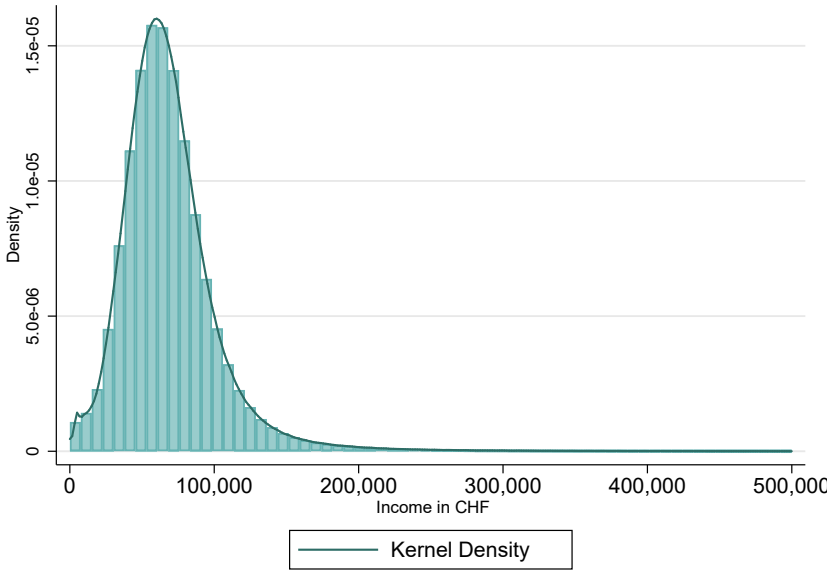


(b) Cumulative moves across distance



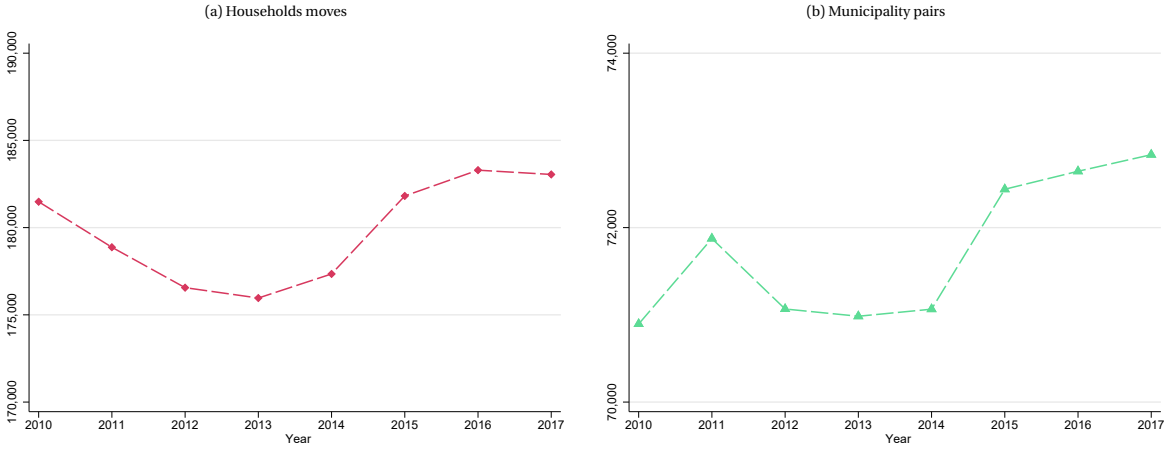
Notes: The figure presents the distribution of moves across distance over our period of observation.

Figure 3.A.3: Labor earnings distribution



Notes: The figure presents the labor earnings distribution among Swiss households heads in the working age group (18-57 years old). Individual observations are averaged over the years 2010-2017. Missing records have been replaced with 0.

Figure 3.A.4: Migration flows over time



Notes: The figure presents migration flows over time. Panel (a) shows the number of household moves over time and panel (b) depicts the total number of pairs with positive flows. Total number of households moves is 1,438,383 and the total number of pairs with non-zero flows is 275,567 (out of 5,015,360).

3.B Supplementary Tables

Table 3.B.1: The effect of Local policies on migration by income class - Net-of-tax rate only

	(1) All	(2) Top 1%	(3) 98-90th	(4) 89-75th	(5) 74-50th	(6) 49-25th	(7) 24-0th
Δ Net-of-tax rate ($\hat{\eta}_1$)	0.770	7.165**	3.065***	1.959**	1.324**	-0.652	-3.376
	(0.5412)	(2.8783)	(1.0727)	(0.9306)	(0.6575)	(0.6710)	(2.4295)
Observations	1,406,647	21,357	206,904	412,337	734,557	634,674	356,348
Pairs with movers	48,055	2,045	12,372	20,083	27,631	25,940	48,055
Average movers	2.48	1.15	1.37	1.53	1.85	1.68	1.52

Notes: Each cell in this table reports the coefficient of a Poisson pseudo-likelihood regression of the number of movers from o to d on a measure of (category specific) net-of-tax rate differentials (in log).
All specifications includes Municipality Pairs, Year and MS Region of Destination × MS Region of origin × Year Fixed Effects. Standard errors in parentheses, with three-way clustering by origin municipality × year, destination municipality × year, and municipality pair. Municipal covariates are: unemployment, left-wing and right-wing orientation, share of people ≥ 65y, share of foreign population, share of people ≤15y, median wealth tax.

*$p < 0.10$,**$p < 0.05$, ***$p < 0.01$.

Table 3.B.2: The effect of Local policies on migration by income class and parenting status - Net-of-tax rate only

	(1) Top 1%	(2) 98-90th	(3) 89-75th	(4) 74-50th	(5) 49-25th	(6) 24-0th
Panel A: with children						
Δ Net-of-tax rate ($\hat{\eta}_1$)	10.49	5.145	0.240	1.540	-0.423	3.438
	(7.7811)	(3.3855)	(3.0865)	(3.3024)	(4.0550)	(10.3050)
Observations	3,655	29,944	39,696	54,282	42,743	26,497
Pairs with movers	489	2,721	3,558	4,811	3,997	2,960
Average movers	1.03	1.05	1.05	1.06	1.05	1.04
Panel B: without children						
Δ Net-of-tax rate ($\hat{\eta}_1$)	4.813	2.894**	2.187**	1.278*	-0.659	-4.076
	(3.4133)	(1.1764)	(0.9651)	(0.6721)	(0.6806)	(2.6405)
Observations	17,348	187,817	392,334	716,044	615,570	339,495
Pairs with movers	1,766	11,646	19,549	27,349	25,557	48,055
Average movers	1.14	1.36	1.52	1.84	1.67	1.52

Notes: Each cell in this table reports the coefficient of Poisson pseudo-likelihood regression of the number of movers from o to d on a measure of (category specific) net-of-tax rate differentials (in log).
All specifications includes Municipality Pairs, Year and MS Region of Destination × MS Region of origin × Year Fixed Effects. Standard errors in parentheses, with three-way clustering by origin municipality × year, destination municipality × year, and municipality pair. Municipal covariates are: unemployment, left-wing and right-wing orientation, share of people ≥ 65y, share of foreign population, share of people ≤15y, median wealth tax.

*$p < 0.10$,**$p < 0.05$, ***$p < 0.01$.

Table 3.B.3: The effect of Local policies on migration by income class and parenting status - General public expenditures

	(1) All	(2) Top 1%	(3) 98-90th	(4) 89-75th	(5) 74-50th	(6) 49-25th	(7) 24-0th
Δ Net-of-tax rate ($\hat{\eta}_1$)	0.945*	7.151**	3.323***	1.979**	1.567**	-0.564	-3.334
	(0.5435)	(2.8901)	(1.0771)	(0.9351)	(0.6595)	(0.6745)	(2.4468)
Δ Public Exp ($\hat{\eta}_2$)	-0.0313***	0.114	-0.0851**	-0.00684	-0.0498***	-0.0177	-0.0295
	(0.0101)	(0.1104)	(0.0360)	(0.0223)	(0.0144)	(0.0148)	(0.0220)
Observations	1,406,647	21,357	206,904	412,337	734,557	634,674	356,348
Pairs with movers	46,457	2,023	12,208	19,703	26,979	25,359	46,457
Average movers	2.48	1.15	1.37	1.53	1.85	1.68	1.52

Notes: Each cell in this table reports the coefficient of a Poisson pseudo-likelihood regression of the number of movers from o to d on a measure of (category specific) net-of-tax rate differentials (in log) and of per capita public expenditures.
All specifications includes Municipality Pairs, Year and MS Region of Destination × MS Region of origin × Year Fixed Effects. Standard errors in parentheses, with three-way clustering by origin municipality × year, destination municipality × year, and municipality pair. Municipal covariates are: unemployment, left-wing and right-wing orientation, share of people ≥ 65y, share of foreign population, share of people ≤15y, median wealth tax.

*$p < 0.10$,**$p < 0.05$, ***$p < 0.01$.

Table 3.B.4: The effect of Local policies on migration by income class and parenting status - General public expenditures

	(1) Top 1%	(2) 98-90th	(3) 89-75th	(4) 74-50th	(5) 49-25th	(6) 24-0th
Panel A: without children < 12 **y**						
Δ Net-of-tax rate ($\hat{\eta}_1$)	0.429	4.238***	2.509**	1.572**	-0.343	-4.177
	(3.8679)	(1.3008)	(1.0336)	(0.7084)	(0.7057)	(2.7404)
Δ Public Exp ($\hat{\eta}_2$)	0.219	-0.0796*	-0.0111	-0.0488***	-0.0204	-0.0260
	(0.1381)	(0.0430)	(0.0241)	(0.0151)	(0.0154)	(0.0226)
Observations	13,023	157,418	352,897	687,020	600,218	327,500
Pairs with movers	1,420	10,250	18,055	26,290	24,724	46,457
Average movers	1.12	1.32	1.49	1.82	1.66	1.51
Panel B: with children < 12 **y**						
Δ Net-of-tax rate ($\hat{\eta}_1$)	15.27***	1.672	-0.504	2.275	-3.468	2.011
	(4.9312)	(1.8817)	(1.6479)	(1.8655)	(2.8740)	(7.5199)
Δ Public Exp ($\hat{\eta}_2$)	-0.153	-0.118*	0.0182	-0.0708*	0.0362	-0.0181
	(0.1996)	(0.0606)	(0.0452)	(0.0413)	(0.0600)	(0.0732)
Observations	8,309	71,608	111,391	130,237	77,746	47,021
Pairs with movers	927	5,435	7,948	9,152	6,263	4,543
Average movers	1.07	1.17	1.15	1.15	1.09	1.07

Notes: Each cell in this table reports the coefficient of Poisson pseudo-likelihood regression of the number of movers from o to d on a measure of (category specific) net-of-tax rate differentials (in log) and of per capita public expenditures.
All specifications includes Municipality Pairs, Year and MS Region of Destination × MS Region of origin × Year Fixed Effects. Standard errors in parentheses, with three-way clustering by origin municipality × year, destination municipality × year, and municipality pair. Municipal covariates are: unemployment, left-wing and right-wing orientation, share of people ≥ 65y, share of foreign population, share of people ≤15y, median wealth tax.

*$p < 0.10$,**$p < 0.05$, ***$p < 0.01$.

Table 3.B.5: Destination vs origin

	(1) All	(2) Top 1%	(3) 98-90th	(4) 89-75th	(5) 74-50th	(6) 49-25th	(7) 24-0th
Δ Net-of-tax rate destination	1.648*	8.140*	6.000***	3.510**	2.181**	-1.324	1.380
	(0.9189)	(4.8615)	(1.6909)	(1.5062)	(1.1007)	(1.0854)	(3.8882)
Δ Net-of-tax rate origin	0.533	-0.914	0.417	1.528	-0.858	0.223	-12.78***
	(0.7323)	(5.2627)	(1.6281)	(1.3584)	(0.9573)	(0.9024)	(4.1872)
Δ Schooling Exp destination	-0.111***	-0.248	-0.303***	-0.206***	-0.112***	-0.0486*	-0.114***
	(0.0222)	(0.1647)	(0.0562)	(0.0420)	(0.0290)	(0.0255)	(0.0372)
Δ Schooling Exp origin	0.00403	-0.303	0.0341	0.0172	0.00330	-0.00421	0.0357
	(0.0150)	(0.2208)	(0.0580)	(0.0355)	(0.0198)	(0.0243)	(0.0377)
Δ Non-Schooling Exp destination	-0.0326	-0.131	-0.0184	0.0356	-0.0503*	-0.0200	-0.0797*
	(0.0212)	(0.1737)	(0.0634)	(0.0419)	(0.0300)	(0.0290)	(0.0414)
Δ Non-Schooling Exp origin	-0.00241	-0.146	0.0112	-0.0256	-0.0320	0.0236	0.0262
	(0.0151)	(0.2050)	(0.0588)	(0.0383)	(0.0228)	(0.0262)	(0.0385)
Observations	1,092,708	14,352	152,415	313,751	570,447	490,979	286,986
Pairs with movers	167,543	2,643	24,491	49,164	88,421	77,061	46,064
Average movers	2.41	1.14	1.35	1.50	1.81	1.65	1.51

Notes: Each cell in this table reports the coefficient of Poisson pseudo-likelihood regression of the number of movers from o to d on a measure of (category specific) net-of-tax rate, schooling expenditures and non-schooling expenditures but separating the terms for destination and origin municipality. We regress $\mathbf{Z'}\beta = \phi_1 \ln(1-\tau_{dt}) + \phi_2 \ln(1-\tau_{ot}) + \phi_3 \ln(S_{dt}) + \phi_4 \ln(S_{ot}) + \phi_5 \ln(NS_{dt}) + \phi_6 \ln(NS_{ot}) + \beta\prime\mathbf{X} + \gamma_{od} + \gamma_{MSod\times t} + \lambda_t + \varepsilon_{odt}$ All specifications includes Municipality Pairs, Year and MS Region of Destination × MS Region of origin × Year Fixed Effects. Standard errors in parentheses, with three-way clustering by origin municipality × year, destination municipality × year, and municipality pair. Municipal covariates, entering the equation separately for origin and destination (not in differential), are: unemployment, left-wing and right-wing orientation, share of people ≥ 65y, share of foreign population, share of people ≤15y, median wealth tax. Column (1) includes all households and uses a weighted tax rate. Columns (2) -(7) limit the sample to households at a given level of the national income distribution and use the average net of tax rate of the specific group.
$^{*}p < 0.10$,$^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3.B.6: Log-count

	All		Top 1%		98-90th		89-75th		74-50th		49-25th		24-0th	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Δ Net-of-tax rate ($\hat{\eta}_1$)	1.222**	1.198**	7.075	6.364	1.848	1.536	-1.245	-1.259	2.339**	2.319**	-1.003	-1.047	2.684	2.500
	(0.5850)	(0.5846)	(12.6116)	(12.5244)	(2.4601)	(2.4682)	(1.7162)	(1.7153)	(0.9172)	(0.9169)	(1.2377)	(1.2366)	(5.5396)	(5.5020)
Δ School Expenditures ($\hat{\eta}_2$)	-0.00937		-0.00493		-0.123*		-0.0357		-0.0159		0.00609		-0.0307	
	(0.0123)		(0.1751)		(0.0673)		(0.0429)		(0.0207)		(0.0254)		(0.0496)	
Δ Subsidy($\hat{\eta}_2$)		0.000272		-0.00403		0.00275		0.000138		0.000859		0.000725		0.00129
		(0.0003)		(0.0084)		(0.0017)		(0.0010)		(0.0005)		(0.0007)		(0.0011)
Observations	196,216	196,519	633	633	10,792	10,816	31,088	31,157	82,226	82,396	58,094	58,233	23,684	23,743
Pairs with movers	42,164	42,279	155	155	2,497	2,507	7,080	7,105	17,843	17,907	12,855	12,910	5,617	5,640
Average movers	1.24	1.24	0.90	0.90	1.01	1.01	1.04	1.04	1.13	1.13	1.09	1.09	1.03	1.03

Notes: Each cell in this table reports the coefficient of a linear regression of the log number of movers from o to d on a measure of (category specific) net-of-tax rate and of local services differentials (in log). Count of movers with value 0 have been replaced with symbolic 1 to avoid observation loss due to the logarithmic transformation. All specifications includes Municipality Pairs, Year and MS Region of Destination × MS Region of origin × Year Fixed Effects. Standard errors in parentheses, with three-way clustering by origin municipality × year, destination municipality × year, and municipality pair. Municipal covariates are: public spending (excluding school spending), unemployment, left-wing and right-wing orientation, share of people ≥ 65y, share of foreign population, share of people ≤15y, median wealth tax. Subsidy values of 0 have been replaced by a symbolic 1.
$^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$.

Table 3.B.7: Municipality-level

	All		Top 1%		98-90th		89-75th		74-50th		49-25th		24-0th	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Net-of-tax rate ($\hat{\eta}_1$)	0.347	0.358	1.355*	1.338*	1.461*	1.466*	2.221***	2.249***	0.919*	0.925*	-0.273	-0.256	-1.763	-1.795
	(0.2857)	(0.2885)	(0.6919)	(0.6891)	(0.8339)	(0.8354)	(0.7868)	(0.7899)	(0.5202)	(0.5212)	(0.4253)	(0.4246)	(1.1177)	(1.1186)
School Expenditures ($\hat{\eta}_2$)	0.0372***		-0.0175		0.0703**		0.0833***		0.0170		0.0392***		0.0369***	
	(0.0093)		(0.0254)		(0.0313)		(0.0231)		(0.0147)		(0.0140)		(0.0130)	
Subsidy ($\hat{\eta}_2$)		-0.000120		0.00371***		-0.000868		-0.000211		-0.000277		0.0000447		-0.0000852
		(0.0002)		(0.0014)		(0.0007)		(0.0005)		(0.0003)		(0.0002)		(0.0002)
Observations	13,878	13,878	13,878	13,878	13,878	13,878	13,878	13,878	13,878	13,878	13,878	13,878	13,878	13,878
Average movers	6.12	6.12	1.53	1.53	2.83	2.83	3.71	3.71	4.67	4.67	4.95	4.95	4.96	4.96

Notes: Each cell in this table reports the coefficient of a regression of the log count of households on a measure of (category specific) net-of-tax rate and of local services at municipality level (in log). We regress $\ln TP_{it} = \beta_1 \ln(1-\tau_{it}) + \beta_2 \ln S_{it} + \beta \prime \mathbf{X} + \alpha_i + \gamma_{MSod \times t} + \lambda_t + \varepsilon_{it}$
where TP_{it} is the number of households in municipality i at time t. All specifications include Municipality, Year and MS Region × Year Fixed Effects. Standard errors in parentheses, with one-way clustering by municipality. Municipal covariates are: public spending (excluding school spending), unemployment, left-wing and right-wing orientation, share of people ≥ 65y, share of foreign population, share of people ≤15y, median wealth tax. Column (1) includes all households and uses a weighted tax rate. Columns (2) -(7) limit the sample to households at a given level of the national income distribution and use the average net of tax rate of the specific group.
$^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$.

3.C Supplementary Information on tax simulator and tax rates

In this section we describe the steps to compute our measure of the average tax rate for the household population as of 2010.

- We compute net household income (household head net income+spouse income) by subtracting compulsory salary contributions to Old-Age and Survivors' Insurance (OASI), Disability Insurance (DI), Income Compensation Insurance (IC). We also subtract non-mandatory Occupational Pension Insurance (LPP).

- We then compute the taxable income by subtracting deductions. The amounts each taxpayer can claim vary across cantons. Additionally, it depends on the number of children, marital status, and occupation status (single or double earner). We subtract the maximum amount a taxpayer can claim for children, double earnings, social purposes, and work-related expenses.

  Table 3.C.1, 3.C.2 and 3.C.3 report the mean and maximum gross income (1), net income (2) and taxable income (3) in each income group respectively.

- We then compute the average taxable income in each income group across cantons. We refer to this as the representative taxpayer and should capture the taxable income of the average taxpayer in Switzerland in each income group. Table 3.C.4 provides the mean and the maximum (baseline) in each income group.

- We then compute the tax rate our average taxpayer faces in each municipality (2,240) each year (2010-2017), using our tax simulator. The tax simulator is a do-file that contains cantonal tax schedules to obtain the simple tax rate. Then, it applies municipal and cantonal multipliers to calculate the total tax burden.

- We weigh the rates by the population. E.g., the tax rate for the top 1% is a weighted average between the non-married top 1% and married top 1%. In the baseline, the tax rate is expressed as a share of taxable income. Table **??** provides cantonal tax rates by income group for the baseline rate and the rate expressed as a share of the gross income. Table 3.C.5 reports results when expressing the tax rate as a share of gross income.

Table 3.C.1: Average gross, net and taxable labor earnings

	Mean (1)	S.D (2)	Min (3)	Max (4)
1a:HH head Gross Income	68,856	105,312	0	41,869,180
1b: Spouse Gross Income	32,719	34,220	0	2,014,500
1: HH Gross Income	77,640	112,659	0	41,878,172
2a: HH head Net Income	62,056	98,856	0	39,746,140
2b: Spouse Net Income	7,818	20,461	0	1,907,404
2: HH net Income (Before Deductions [a])	69,874	105,199	0	39,754,588
3: HH taxable Income (After Deductions)	60,715	103,514	0	39,750,688
Deductions				
Double Earners	2,512	2,301	0	130,233
Social	2,738	6,010	0	35,000
Child	6,169	2,620	0	12,000
Work	3,643	1,045	500	7,000
Obs.	2,344,573			

[a]Double Earners/Social/Child/Work

Table 3.C.2: Average gross, net and taxable labor earnings by categories

	All (1)	Top 1% (2)	98-90th (3)	89-75th (4)	74-50th (5)	49-25th (6)	24-0th (7)
1a:HH head Gross Income	68,856	490,469	158,372	98,504	68,362	37,036	7,179
1b: Spouse Gross Income	32,719	52,247	38,101	33,417	30,642	19,899	4,358
1: HH Gross Income	77,640	529,857	182,741	114,573	76,765	39,333	7,279
Average HH Gross Income	58,662	329,560	121,202	83,666	63,351	36,205	7,154
2a: HH head Net Income	62,056	458,751	143,754	87,651	60,891	33,469	6,744
2b: Spouse Net Income	7,818	35,528	21,677	14,264	7,474	2,055	95
2: HH net Income (Before Deductions [a])	69,874	494,279	165,432	101,915	68,365	35,524	6,839
3: HH taxable Income (After Deductions)	60,715	478,409	151,541	90,211	59,058	28,075	1,044
Deductions							
Double Earners	2,512	3,311	2,854	2,666	2,511	2,377	2,295
Social	2,738	2,197	2,377	2,493	2,790	2,942	2,885
Child	6,169	6,169	6,004	6,019	6,169	6,305	6,238
Work	3,643	3,687	3,657	3,659	3,690	3,655	3,547
Obs.	2,344,573	28,845	255,290	412,340	642,577	505,639	499,882

[a]Double Earners/Social/Child/Work

Table 3.C.3: Maximum gross, net and taxable labor earnings by categories

	Top 1% (1)	98-90th (2)	89-75th (3)	74-50th (4)	49-25th (5)	24-0th (6)
1a:HH head Gross Income	41,869,180	276,300	119,500	83,620	53,544	17,618
1b: Spouse Gross Income	2,014,500	276,133	119,500	83,616	53,544	17,617
1: HH Gross Income	41,878,172	552,266	239,000	167,232	107,088	35,234
Average HH Gross Income	33,182,280	530,300	235,724	165,651	106,215	35,134
2a: HH head Net Income	39,746,140	258,523	112,147	78,550	50,305	16,552
2b: Spouse Net Income	1,907,404	256,361	109,110	75,660	49,795	16,551
2: HH net Income (Before Deductions [a])	39,754,588	510,670	217,341	151,320	99,116	33,102
3: HH taxable Income (After Deductions)	39,750,688	497,146	210,767	147,108	93,501	30,883
Deductions						
Double Earners	130,233	10,000	10,000	10,000	9,459	7,600
Social	35,000	35,000	35,000	35,000	35,000	35,000
Child	12,000	12,000	12,000	12,000	12,000	12,000
Work	7,000	7,000	7,000	7,000	7,000	7,000
Obs.	28,845	255,290	412,340	642,577	505,639	499,882

[a] Double Earners/Work/Child/Social

Table 3.C.4: Representative taxpayer income

	Top 1% (1)	98-90th (2)	89-75th (3)	74-50th (4)	49-25th (5)	24-0th (6)
A: Weight						
Share Married	0.57	0.53	0.42	0.25	0.12	0.04
Share Single	0.43	0.47	0.58	0.75	0.88	0.96
B: Mean in each income group						
Taxable Income Married	484,348	166,734	103,107	72,920	33,858	-4,365
Taxable Income Single	470,520	134,103	80,706	54,427	27,279	1,296
C: Maximum in each income group (Baseline)						
Taxable Income Married	39,750,688	497,146	210,767	147,108	93,501	30,883
Taxable Income Single	37,695,088	256,163	108,833	77,948	49,773	16,052
D: Weights						
Share Double Earner Children	0.67	0.73	0.73	0.66	0.36	0.13
Share Single Earner Children	0.33	0.27	0.27	0.34	0.64	0.87
Share Single Earner No Children Married	0.23	0.14	0.09	0.05	0.08	0.06
Share Single Earner No Children Single	0.46	0.59	0.72	0.85	0.89	0.93
Share Double Earner No Children Married	0.31	0.27	0.19	0.09	0.03	0.01
E: Mean in each income group						
Taxable Income With Children	511,341	162,526	98,466	69,779	36,183	-6,176
Taxable Income Without Children	509,976	186,332	120,086	86,505	44,403	1,829
Taxable Income Double Earner Children ([a])	478,318	162,526	98,466	69,779	36,183	-7,473
Taxable Income Single Earner Children ([b])	511,341	132,475	73,784	46,959	20,057	-6,176
Taxable Income Double Earner No Children Married	509,976	186,332	120,086	86,505	44,403	1,691
Taxable Income Single Earner No Children Married	474,672	137,441	79,936	52,493	23,678	-1,847
Taxable Income Single Earner No Children Single	417,983	134,114	82,178	55,353	28,342	1,829
F: Maximum in each income group (Baseline)						
Taxable Income With Children	37,695,088	493,432	208,593	147,108	93,350	29,602
Taxable Income Without Children	39,750,688	497,146	210,767	147,016	93,501	30,883
Taxable Income Double Earner Children ([c])	23,314,518	493,432	208,593	147,108	93,350	29,602
Taxable Income Single Earner Children ([d])	37,695,088	253,393	107,382	74,149	48,244	16,035
Taxable Income Double Earner No Children Married	39,750,688	497,146	210,767	147,016	93,501	30,883
Taxable Income Single Earner No Children Married	18,985,916	255,283	107,290	74,455	47,638	16,046
Taxable Income Single Earner No Children Single	21,424,464	256,163	108,833	77,948	49,773	16,052

[a]Married tarif
[b]Married tarif
[c]Married tarif
[d]Married tarif

Table 3.C.5: Tax rate as gross income

	All		Top 1%		98-90th		89-75th		74-50th		49-25th		24-0th	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Δ Net-of-tax rate ($\hat{\eta}_1$)	0.0342	0.0495	7.077**	7.017**	2.877**	2.863**	2.304**	2.306**	1.162*	1.168*	-0.582	-0.574	-3.115	-3.105
	(0.1780)	(0.1788)	(2.9340)	(2.9314)	(1.1202)	(1.1207)	(0.9854)	(0.9851)	(0.6784)	(0.6781)	(0.7009)	(0.7008)	(2.5707)	(2.5810)
Δ School Expenditures ($\hat{\eta}_2$)	-0.0447***	-0.0435***	-0.0293	-0.0226	-0.0683**	-0.0647**	-0.0940***	-0.0933***	-0.0565***	-0.0554***	-0.0258	-0.0252	-0.0587**	-0.0567**
	(0.0105)	(0.0105)	(0.0766)	(0.0758)	(0.0319)	(0.0319)	(0.0230)	(0.0230)	(0.0148)	(0.0147)	(0.0164)	(0.0164)	(0.0234)	(0.0234)
Δ Subsidy[a] ($\hat{\eta}_2$)	0.000869***		0.00282		0.00292***		0.000543		0.000780*		0.000405		0.00137**	
	(0.0003)		(0.0025)		(0.0010)		(0.0007)		(0.0005)		(0.0005)		(0.0007)	
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
MS region pair × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municpality pair FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,401,192	1,401,192	20,185	20,185	193,592	193,592	385,803	385,803	686,339	686,339	594,240	594,240	332,618	332,618
Pairs with movers	188,077	188,077	3,279	3,279	27,695	27,695	53,940	53,940	95,072	95,072	83,384	83,384	47,946	47,946
Average movers	2.41	2.41	1.14	1.14	1.35	1.35	1.50	1.50	1.81	1.81	1.65	1.65	1.50	1.50

[a]zeros have been replaced by 1.