

**GRIPS Discussion Paper 19-27**

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Evidence from Swiss Communes**

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**November 2019**



**GRIPS**

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FOR POLICY STUDIES

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# Policy Choices in Assembly versus Representative Democracy: Evidence from Swiss Communes

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## Abstract

This paper investigates whether the form of the legislative institution - citizen assembly versus elected parliament - affects the level and composition of local public expenditure. Our empirical analysis focuses on medium-sized and mostly German-speaking communes in Switzerland that switched from assembly to parliament between 1945 and 2010. Event study estimates suggest that parliament adoption increases total spending by about 6 percent and that this increase is driven mostly by general administration and education spending. To understand potential mechanisms at play, we run a survey among assembly participants and document a sizeable under-representation of 20- to 40-year-olds, as well as of women in assemblies compared to both voters in elections and to the electorate at large. Since these two demographics have relatively strong preferences for public spending on education in our setting, switching from citizen assembly to parliament likely increased their representation in the political process.

Keywords: legislative institutions, government spending, event study

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# 1 Introduction

Whether the form of the legislative institution at the local level matters for collective choices is an open and important question. Citizen assemblies (also called town meetings) are the form of government in which ordinary citizens gather to legislate and decide budget priorities. Local parliaments, on the other hand, are characterized by principals (citizens) delegating decision-making power to their agents (politicians). This may create well-known agency problems, where politicians pursue their own interests instead of maximizing the principals' welfare (e.g. Persson and Tabellini, 2000; Besley, 2006). In order to improve governance, the World Bank and several aid-organizations have been actively promoting citizen participation in local budgeting decisions for several decades (e.g. World Bank, 1996). Similarly, Bryan (2004) praises the virtues of town meetings in New England. Both legislative forms are prevalent around the world today after a surge of participatory democracy in several developing countries such as Brazil, Venezuela and India.

While citizen assemblies seem appealing because of their deliberative character and the lack of agency problems, (Ban et al., 2012; Wantchekon et al., forthcoming), one potential worry is about unequal participation and representation (Lijphart, 1997). Because attending assembly meetings is time-consuming, theory predicts potentially low and non-representative participation in assembly democracies (Osborne, Rosenthal and Turner, 2000). Voting in elections on the other hand only requires a trip to the ballot box once every four years or so. Policies may therefore differ across legislative institutions simply because median voters differ. While there is a sizeable literature on how direct democratic instruments (voter initiative and budget referendum) *within* representative systems affect policy, to date very little is known about the causal effect of assembly versus representative legislative forms on collective decisions (see Tyrefors-Hinnerich and Pettersson-Lidbom, 2014, for a notable exception).

This paper provides some of the first evidence on the effects of legislative form on the level and composition of public expenditure. The setting is one of a mature federal system (Switzerland), composed of cantons (states) and communes, where representative and assembly democracy coexist at the commune level. Our analysis focuses on cantons where communes have the authority to determine the form of their legislative power. To get information on the commune's current and past legislative forms, we sent our own "legislative survey" to all communes in these cantons. Based on our survey, canton-level administrative data and prior surveys on local governance, we identified a "switcher sample" - 77 communes that changed the form of their legislative institution at least once between 1945 and 2010, most of them abolishing the assembly and introducing a parliament. The most common reasons for the switch were low participation in assemblies, space problems due to population growth and women suffrage, and the desire to professionalize the legislative process. We focus on such switcher communes because communes that always had an assembly or a parliament are likely different from each other in partly unobservable dimensions. We did our own data collection in local archives of switcher communes in order to recover historical public expenditure information.

Our event study in the switcher sample suggests that adopting a parliament increases total spending per capita by about 6 percent.<sup>1</sup> The overall spending increase is mostly driven by administrative and education spending. For other spending categories, such as welfare, law enforcement, and traffic and environment, we find typically smaller and statistically insignificant effects. The causal interpretation of these estimates hinges on the assumption that time-varying unobservables are uncorrelated with parliament adoption within communes over time. Although this assumption is not directly testable, we show that results are robust to including time-varying controls for demographic composition, as well as major determinants of parliament adoption (population and

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<sup>1</sup>We also conducted a fuzzy regression discontinuity analysis in canton Vaud that turned out to be under-powered.

local women suffrage). Introducing a commune-specific time trend leaves our results unaffected or increases the size of estimated impacts. Perhaps the most important validation comes from the event study graph: pre-adoption effects are small and insignificant, while the post-adoption period is marked by a sharp and persistent increase of effect size estimates.<sup>2</sup>

The positive impact of representative democracy on administrative spending is consistent with rent-extraction. Representative democracies are subject to a principal-agent problem, and elected politicians may deviate from voter preferences in order to pursue their own interests (e.g. Persson and Tabellini, 2000; Besley, 2006). An alternative plausible explanation is that newly paid salaries for members of parliament and their staff mechanically increased administrative spending. The positive impact on education spending suggests that preferences for this type of spending were systematically under-represented in the assembly system, which is consistent with predicted low and potentially non-representative assembly turnout due to participation costs (Osborne, Rosenthal and Turner, 2000). Voting costs for Swiss elections in contrast are particularly low because many cantons introduced postal voting over the course of our study period (Funk, 2010).

To better understand the socio-demographic characteristics of the median voter in assemblies and elections, we also conducted an "assembly survey" investigating assembly participants' gender, age, education, family status and working hours in communes of canton Zürich. Results suggest a sizeable under-representation of 20- to 40-year-olds as well as of women in assemblies compared to both election participants and to the electorate at large. We corroborate these results using a nation-wide survey of commune secretaries who were asked about the representation of different groups at assembly meetings.

Since these two sources document an under-representation of women and relatively young citizens in assemblies (relative to elections), it is key to understand whether and how these groups

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<sup>2</sup>While the switch from assembly to parliament leads to a higher likelihood of introducing referendum rights for citizens on average, impacts on spending composition are present even when the switch in legislative form involved no change of referendum rights.

differ from male and older citizens in terms of policy preferences. Results from a recent post-election survey (SELECTS, 2011) that explicitly asked about policy preferences suggest that Swiss 20- to 40-year-olds and women are more strongly in favor of local public childcare provision than older citizens and men. Similarly, Cattaneo and Wolter (2009) confirm (using another Swiss survey) that elderly people are less supportive of education spending more generally.<sup>3</sup> Switching from citizen assembly to parliament in our setting therefore seems to increase the representation of two demographics with relatively strong preferences for public spending on education. At the same time, relatively young citizens and women are not more favorable to increased spending on welfare and law enforcement than older citizens and men. Our findings that parliament adoption increases spending on education - but not on law enforcement or welfare spending - are thus remarkably consistent with the evidence on policy preferences of under-represented groups in assemblies.

Our paper most closely relates to Tyrefors-Hinnerich and Pettersson-Lidbom (2014) who compare welfare spending under assembly and representative democracy in early 20th century Sweden after the introduction of universal and equal suffrage using a regression discontinuity design based on local population. The main result is that parliaments spend 40 to 60 percent more on public welfare and the authors argue that this is due to higher elite capture under the assembly system. The fact that early 20th century Sweden was a rural society, and that voting in assemblies (in contrast to elections) was typically not anonymous, likely facilitated capture by elites. Introducing secret ballots has also been shown to reduce elite capture in other rural settings. In Chile, for example, the de facto control over voters by landlords was a key argument for introducing secret ballots in rural elections (Baland and Robinson, 2008). Similar elite capture has been documented in citizen assemblies in Indonesia (Olken, 2010) and Afghanistan (Beath et al., 2017). The authors

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<sup>3</sup>Analogous results have also been documented for the U.S. (Figlio and Fletcher, 2012; Bertocchi et al. 2017). In a similar vein, Carruthers and Wanamaker (2015) summarize mostly U.S. evidence on women's greater preference for both private and public goods and services that enhance child welfare. While less relevant for us, the link between gender and preferences for education spending is less clear-cut in developing countries (Chattopadhyay and Duflo, 2004).

compare the types of projects chosen under citizen assemblies and secret ballot referenda and show that referenda tend to diminish the influence of elites on chosen projects (more clearly so in Beath et al.).

Even though voting in Swiss assembly meetings is typically not anonymous, the type of narrow elite capture described in the studies above is unlikely to explain our results. First, we find no effect on welfare spending, which is inconsistent with elites holding back this kind of spending under the assembly system. Second, by 1945 Switzerland already was a highly industrialized country. Particularly in our switcher sample communes, agriculture was practically inexistent according to the 1950 census. Since factory owners likely had less control over their workers than landlords over their tenants (Baland and Robinson, 2008), elite capture of assembly decisions seems less plausible compared to more rural settings. Third, elite capture was never mentioned as a reason for introducing a parliament among our switcher communes. Fourth, the rich and highly educated are if anything under-represented in assemblies according to the 2009 Ladner survey of commune secretaries. Last but not least, using recent post-election survey data (SELECTS, 2011), we document that while the rich are slightly less favorable to higher welfare spending in Switzerland, they are actually slightly more favorable to public provision of childcare than the not-so-rich. Similarly, the more highly educated actually prefer higher public spending on education (Cattaneo and Wolter, 2009). As such, if the elite were capturing the process in assemblies, introducing a parliament should reduce education spending - the opposite of what we find.

A more recent paper from a developed country setting compares aggregate budgetary outcomes of assembly and representative democracies for very small communes in Spain. Sanz (forthcoming) employs a regression discontinuity design with population cutoff at 100 and quite convincingly deals with the fact that the density of the population distribution is discontinuous at the cutoff. His results suggest that representative democracy increases total spending by about 8 percent, which is

quantitatively close to what we find. Our paper goes further by decomposing the spending increase and by providing the first direct evidence of systematic demographic differences between assembly participants, voters and the electorate at large, which plausibly explain observed differences in local public budget choices.

Our paper is also related to an earlier literature that shows how adding elements of direct democracy (voter initiative and budget referendum) *within* representative systems reduces government spending (e.g. Feld and Matsusaka, 2003; Funk and Gathmann, 2011). In contrast to our comparison between assembly and parliament, this literature looks at cantons or states under parliamentary systems but varying degrees of direct democratic instruments. More recent studies, such as Hainmueller and Hangartner (2019), focus on more specific questions, such as whether direct democracy affects immigrant naturalization decisions.

More broadly, our paper contributes to a large literature relating institutional reforms, such as suffrage extensions or compulsory voting laws, to policies. For instance, Aidt et al. (2006) document how lifting socio-economic restrictions on the right to vote contributed to economic growth in Europe, while Miller (2008) shows how granting females the right to vote in the U.S. contributed to a decrease in child mortality. Hoffman et al. (2017) show that while making voting compulsory increased election turnout in Austria, the level and composition of public spending remained unaffected. In addition to these papers, which analyze changes in de jure power or legal requirements, there are contributions studying changes in de facto power. For instance, poor voters were effectively enfranchised when poll taxes and literacy tests were abolished in the U.S. (Husted and Kenny, 1997), or when Brazil switched from paper ballot voting to electronic voting (Fujiwara, 2015). Our paper is more closely related to this last strand of papers, even though the context is entirely different. We show that the citizen assembly may lead to under-representation of certain groups, which in turn affects their de facto power in the political decision-making process.



The paper is organized as follows. Section 2 presents institutional background on the Swiss federal system. Section 3 describes the data. Section 4 discusses identifying assumptions and the estimation approach for our event study. Section 5 presents the main results. Section 6 discusses robustness checks. Section 7 provides available evidence on mechanisms. Section 8 concludes.

## 2 Institutional background

### 2.1 Communal autonomy

Switzerland is a federal state with three layers of government: the federal level, the cantonal level, and the commune level. Political responsibilities remain with the cantons unless they were granted to the federal government in a national referendum. As a consequence, cantons have a lot of autonomy in the provision of public goods and the choice of political institutions. The degree of commune autonomy is regulated by cantonal laws, which leads to substantial heterogeneity across cantons. For instance, some cantons mandate political institutions at the commune level, while other cantons let the communes choose freely.

For our switcher sample analysis we focus on the fourteen cantons that allow local choice of the legislative institution.<sup>4</sup> We exclude communes from canton Ticino, since most of the local institutional variation was generated by commune mergers. Some cantons, such as Neuchâtel and Geneva, prescribe a parliament for all communes, while other cantons mandate that legislative decisions at the local level be made at the assembly or at the ballot box (cantons Appenzell Innerrhoden, Appenzell Ausserrhoden, Glarus, Nidwalden, Obwalden, Schwyz, Uri). Canton Vaud prescribes a parliament for communes with more than 800 inhabitants and allows local choice between parliament and assembly for communes with up to 800 inhabitants.<sup>5</sup> Population thresholds

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<sup>4</sup>In all but one canton (Schaffhausen) there were actual switches of legislative institutions during our sample period.

<sup>5</sup>The resulting fuzzy regression discontinuity analysis is unfortunately underpowered. Please see our earlier

also exist for communes in cantons Fribourg, Vallis and Zürich, which are included in our switcher sample analysis. The number of communes around these cutoffs is small and the assignment rule sometimes differs, mandating an assembly below the cutoff and allowing choice above.

## 2.2 Commune responsibilities

In addition to the heterogeneity in communal autonomy across cantons, the distribution of responsibilities for communal and cantonal public service provision also differs across cantons. Typically, however, commune responsibilities include preschool and primary education (grades 1 through 5 or 6), welfare, law enforcement, and traffic, among others. For the medium-sized communes in our switcher sample, responsibilities typically also include lower secondary education (grades 6 or 7 through 9). The bulk of communal spending is on education, welfare, traffic and general administration as shown in Table 1. Communal budgets need not be balanced every period. As for total communal spending relative to cantonal and federal spending, communes undertook 24% of total spending, cantons 42%, and the federal level the remaining 34% in 2010. A large share of local expenditures is financed through a local income tax.

## 2.3 Commune organization and political rights

Decision-making bodies at local level include the executive (usually called *Gemeinderat*), the legislative, organized as either assembly or parliament, the electorate, and special committees for example for financial affairs. The exact division of powers in the budget process varies across communes but typically it is characterized as follows. The executive implements approved expenditures and drafts the budget proposal in consultation with the finance committee. The legislative votes on the budget proposal and controls the execution of past expenditures. Participants at

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working paper for details.

assemblies can propose budgetary items for deliberation. Budgetary decisions are taken by simple majority in an open vote, except if a secret vote is requested and approved. Under both legislative forms, the final say on the budget may rest with the electorate, either through mandatory or facultative referendum - that is, when a sufficient number of citizens ask for a vote at the ballot box.

## 3 Data

### 3.1 The switcher sample

To gather information on the institutional history of Switzerland's 1,821 communes in cantons granting autonomy over the choice of local legislative form, we designed our own legislative survey and sent it to municipal secretaries in April 2011. We then combined information from our own survey with several prior surveys conducted by Prof. Ladner and his team, as well as with administrative information on local parliaments from four cantons. Based on these three data sources, we identified 77 communes that had changed the form of their legislative power between 1945 and 2010, mostly abandoning the citizen assembly in favor of introducing a parliament. To the best of our knowledge these 77 communes represent the universe of switchers over this period (see the online appendix for details). As shown in Figure 1 below, the switcher sample is spread out all over Switzerland. Furthermore, there is substantial heterogeneity in the time of institutional change across communes as shown in Figures 2 and 3.

What were the main reasons for the system change? According to municipal secretaries in the switcher sample (response rate 56%), the key rationale for introducing a parliament was low turnout at assemblies, coupled with potentially unrepresentative decisions.<sup>6</sup> Especially in large

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<sup>6</sup>See also Ladner (2016) for more recent examples and discussion.

communes, turnout in assemblies was very low (often less than 10 percent), which raised concerns about representation. Space problems (due to population growth or the introduction of women suffrage) were another frequently mentioned argument. In our empirical analysis we therefore control for population size and women suffrage. About one third of commune secretaries also cited the desire to professionalize the legislative process as a reason for switching to a parliament. Frequent arguments against having a parliament were a potentially stronger influence of political parties and lobbyists as well as higher costs and difficulties to find candidates. Importantly, none of the commune secretaries related the choice of legislative form to specific budget components.

### **3.2 Local budgetary data for the switcher sample**

In our switcher sample we collected historical data on total revenue and total expenditure, as well as expenditures broken down by spending category. Accounting systems varied across cantons and time, and even slightly within cantons. We coded every change in the commune's accounting system and control for these structural breaks using dummy variables in the regressions. Details are in the online appendix. Table 1 below presents summary statistics. Total revenue and spending in the switcher sample were about 3,700 Swiss Francs per capita on average from 1945 to 2010.<sup>7</sup> The most common categories include administrative, education, welfare, law enforcement and traffic and environment spending. Together, these account for about two-thirds of total spending. Other spending categories, such as health, economy, or finances, were less common at the commune level during our sample period.

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<sup>7</sup>The maximum spending and revenue of about 30,000 Swiss Francs per capita seem unrealistically high compared to the second-highest spending level of 19,000 Swiss Francs per capita. The corresponding year is also an outlier within the commune itself and thus likely reflects a one-time revenue windfall.

### 3.3 Control variables

Most control variables (commune population, demographic structure, labor force participation rate, and share foreigners) are from the Swiss Federal Statistical Office (*Bundesamt für Statistik*). We interpolate control variables between census years, except for commune population which is based on yearly administrative data between 1981 and 2010. Another key control variable is the indicator for local woman suffrage, which is from our legislative survey. As shown in Table 1, switcher communes tend to be medium-sized (about 8,500 inhabitants on average) with a labor force participation rate of about 62 percent and a share of foreigners of about 15 percent on average.

## 4 Identification and estimation approach

Let  $Y_{ct}$  denote spending per capita in a given category or overall in commune  $c$  and period  $t$ ,  $D_{ct}$  the indicator for parliament (1) or assembly (0),  $\beta$  the (constant) effect of parliament relative to assembly,  $X_{ct}$  a vector of time-varying covariates including commune population, demographic controls (share of population in age brackets 20-39, 40-64, 65 and above), labor force participation rate, share foreigners and the indicator for woman suffrage,  $\alpha_c$  commune fixed effects,  $\gamma_t$  time fixed effects, and  $U_{ct}$  the influence of unobserved additional factors that affect outcomes. The baseline specification is as follows:

$$\ln(Y_{ct}) = \beta D_{ct} + \delta X_{ct} + \alpha_c + \gamma_t + U_{ct}. \quad (1)$$

Causal interpretation of fixed effects estimates hinges on the assumption that time-varying unobservables are uncorrelated with parliament adoption, conditional on the commune and time fixed effects and time-varying controls. We control for population and local women suffrage since

these were mentioned as key reasons for parliament adoption in our survey. Similarly, whether the electorate includes women or consists only of men is an important determinant of public spending (Miller, 2008). We also control for commune demographics since the age profile of the population is a potential determinant of public spending priorities, as are labor force participation and the share foreigners in the commune. We do not include controls for tax rates or political competition since these might be affected by parliament adoption. For example, tax rates may well have increased to finance a spending increase under the representative system. Similarly, party politics may have become more important and elections more competitive because of the switch to parliament. Rather than being confounders, including such potentially bad controls would lead to biased estimates of parliament adoption. Our second specification additionally controls for commune-specific linear trends  $\theta_c t$ . The third specification in addition controls for commune-specific breaks in the local accounting system.

We also conduct an event study in order to assess the validity of the research design. Let  $E_c$  denote the year commune  $c$  switched for the first time from assembly to parliament between 1945 and 2010. As shown in Figures 2 and 3, all communes except for two started out with an assembly and switched to parliament at least once during this period. 12 communes switched back to assembly after a few years. Define time from parliament adoption in commune  $c$  as  $K_{ct} = t - E_c$ , and let  $D_{ct}^k = I\{K_{ct} = k\}$  denote a dummy variable equal to 1 for the  $k$ th year from parliament adoption and zero otherwise. We look at 8 years prior and 8 years post-adoption so  $k$  runs from -8 to 8 and we include a dummy for 9 or more years prior to adoption and another dummy for 9 or more years post-adoption. Focusing on a 16-year window around adoption ensures that the coefficients are identified from a similar set of communes. Expanding the pre- or post-adoption periods would lead to shifting sample compositions as some communes transitioned to parliament less than 8 years into the sample period while others transitioned too late (see Figure 2

in the online appendix for the distribution of years from the switch to parliament in our sample). Moreover, 12 communes switched back to the assembly form after a few years under parliament as shown in Figures 2 and 3. The omitted base category is the year prior to parliament adoption so that coefficients on the time from adoption dummies reflect differences compared to the year prior to adoption. Our main event study specification also includes the time-varying controls, commune-specific trends, and structural break dummies as above. Results without these controls are quantitatively similar but less precise as shown in our online appendix. The event study model is then as follows:

$$\ln(Y_{ct}) = \sum_k \beta_k D_{ct}^k + \delta X_{ct} + \alpha_c + \gamma_t + \theta_c t + U_{ct}, \quad (2)$$

$$D_{ct}^k = \begin{cases} I\{K_{ct} \leq k\} & k \leq -9 \\ I\{K_{ct} = k\} & -8 \leq k \leq 8 \\ I\{K_{ct} \geq k\} & k \geq 9 \end{cases} . \quad (3)$$

Intuitively, introducing a parliament in the future should probably not affect spending today and thus large and significant pre-adoption effects might signal that parliament adoption is endogenous. For example, a remaining unobserved confounder could be local wages or local income more generally, since the local income tax contributes substantially to local government revenues. Moreover, an increase in local wages might make time-consuming assembly meetings less attractive and thus lead to an increased likelihood of parliament adoption. Under this scenario, a gradual increase in wages should manifest itself already prior to parliament adoption. On the other hand, no impact of parliament on spending in the pre-adoption period but fairly rapid and persistent impacts after adoption would support a causal interpretation of the event study estimates.

## 5 Main results

Table 2 shows estimation results for total spending and the most common spending categories. Each panel shows three impact estimates, corresponding to specifications with time and commune fixed effects and time-varying controls, additional commune-specific linear trends, and additional dummies for structural breaks in the local accounting system. Results in panel A show that having a parliament increases total spending per capita by about 7 percent compared to an assembly. This effect is significant at 10 percent even with commune-specific linear trends and when controlling for accounting system changes. Importantly, the R-squared reaches 91 percent, leaving little room for omitted variable bias. Total revenue also increases by about 7 percent (results available on request). Unfortunately we did not collect data on revenue composition and thus are unable to pinpoint which revenue categories increased.

Turning to the decomposition of the spending increase, panels B and C of Table 2 show that the parliament system increases administrative spending and education spending per capita by about 12 to 13 percent in the most demanding specification with commune-specific time trends and structural break dummies. Impact estimates for welfare, law enforcement, and traffic and environment spending, shown in panels D through F respectively, are small and statistically not significant. Similarly, estimates for other spending are also generally small and not significant (results available on request).

Figure 4 plots event study impact estimates for spending per capita and 95 percent confidence interval bars from 8 years prior to adoption until 8 years post-adoption based on equation (2). Panels A through C display estimated impacts on total, administrative and education spending per capita, respectively. The three panels show a pattern of pre-adoption effects that bounce around zero, followed by a sharp and persistent increase of the effect estimate at the time of parliament adoption. For total spending, the post-adoption estimates average about 6 percent and they are



not significantly different from zero due to the disaggregation. Since the post-adoption effect estimates show little variability, the corresponding pooled estimates from panel A in Table 2 are appropriate and these are significant at 10 percent. For administrative and education spending shown in panels B and C respectively, the post-adoption estimates average about 13 and 15 percent, exhibit little variability, and are mostly statistically different from zero even at 5 percent.

In contrast, event study estimates for welfare, law enforcement, and traffic and environment spending shown in panels D through F exhibit no clear pattern around the time of parliament adoption and are statistically not significant almost without exception. For example, while effect estimates for welfare spending in the post-adoption period average about 10 percent, similar-sized "impacts" are already present several years prior to adoption as shown in panel D of Figure 4. Event study estimates for other spending categories also look similar and are available on request.

Overall, the econometric evidence suggests that adopting a parliament increases total spending per capita by about 6 percent and that this increase is mostly driven by administrative and education spending. Indeed, since administrative spending accounts for about 10 percent of the budget, while education accounts for about 20 percent as shown in Table 1, together these categories account for a total spending increase of about  $0.1 \times 0.13 + 0.2 \times 0.15 = 4.3$  percent.

## **6 Robustness checks**

### **6.1 Referendum rights**

The switch from citizen assembly to parliament entails a delegation of legislative powers to elected officials. In order to counterbalance this delegation, some communes introduced a compulsory referendum on substantial financial matters, leaving the final decision to voters at the ballot box. Other communes, however, did not introduce additional referendum rights or already had

them in place under the assembly system (and kept them even after the switch to parliament). We run separate regressions for those switches of legislative form that held compulsory financial referendum rights constant and those that were accompanied by an introduction of such rights. Table 3 reports the results. While splitting the sample leads to a loss of precision, impacts on total, administrative and education spending are of similar magnitudes as in Table 2 even when the switch in legislative form involved no change of referendum rights (panels A, B and C). For switches involving a change in referendum rights, there are also similar-sized impacts on total and administrative spending and an attenuated estimate for education spending (panels D, E and F).

## 6.2 Political and school mergers

Over the course of our sample period, ten of the switcher communes experienced at least one merger with another commune. Even though the effect of a merger on spending *per capita* and spending composition is theoretically ambiguous, it could lead to a spurious correlation if the merger were also correlated with parliament adoption. But since in all cases except one the merger involved a much larger commune absorbing a smaller one (technically an amalgamation), it is a priori unlikely that the merger itself induced a change of legislative form. In another six switcher communes, previously autonomous school communities were integrated into the political commune. Such school community mergers would mechanically increase overall and education spending per capita of the political commune after the merger and lead to biased estimates if correlated with parliament adoption. We address these concerns by including separate dummy variables that switch to one for all periods after a political or school community merger. Table 4 shows that the magnitude and precision of nearly all estimates are quantitatively invariant to the inclusion of these additional controls.<sup>8</sup>

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<sup>8</sup>When we drop the communes involved in a political merger from the sample, results remain quantitatively unchanged in terms of both magnitude and precision. When we drop those experiencing a school community

### 6.3 Women suffrage

Women's right to vote in local affairs was introduced gradually during our sample period. In some cases, commune secretaries cited space problems associated with doubling the size of the electorate as reasons for switching from assembly to parliament. All our regressions therefore control for a women suffrage dummy. However, if increased women representation under parliament is indeed driving the observed increase in education spending, we should observe a similar increase when women get the right to vote irrespective of the legislative form. Table 2 in the online appendix shows that this is indeed the case. Women suffrage increases both total spending and education spending per capita by about 10 percent. The evidence on women suffrage is thus very much in line with increased female representation under the parliament system at least partially driving changes in budget priorities.

### 6.4 Placebo reform and randomization inference

A hypothetical switch from citizen assembly to parliament 10 years prior to the actual switch should not have any effect on budget outcomes. We implement this falsification test by restricting the sample to assembly periods and replacing the parliament dummy with a placebo dummy that switches from zero to one 10 years before actual parliament adoption. Then we run the exact same regressions as we did in the full sample with the actual switch to parliament. Table 3 in the online appendix shows small and insignificant estimates for 5 out of our 6 main spending outcomes (welfare spending being the exception). As an additional robustness check, we create a dataset with fake parliament adoptions - one switch from assembly to parliament per commune as in most of our switcher sample - at random points in time between 1945 and 2010. We then use the recent Stata command "ritest" to conduct randomization inference by drawing on these fake parliament

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merger, the effect estimates again remain similar but become insignificant for total and education spending.

adoptions to estimate our baseline specification equation (1). We run 2000 replications. None of the placebo estimates are larger than the respective actual estimates for total, administrative, and education spending per capita (p-values=0.000).

## **6.5 Time-varying controls and functional form**

Figure 3 in the online appendix plots event study estimates from a specification that only controls for commune and time fixed effects. The patterns of estimates are quantitatively similar to those in Figure 4 in the paper but less precise, suggesting that potentially endogenous time-varying controls are not driving our results. Similarly, Table 4 in the online appendix shows that the pooled estimates do not vary much when time-varying covariates are added one by one. Figure 4 in the online appendix shows that the results are also robust when the dependent variable is the natural logarithm of spending (instead of spending per capita). When spending is in per capita terms or raw (without logs), results are still qualitatively similar but substantially more noisy (online appendix Figures 5 and 6, respectively).

# **7 Mechanisms**

## **7.1 Political participation in assemblies and elections**

One key difference between assembly and representative democracy is the level of political participation or turnout. Indeed, our leading hypothesis is that turnout increases when parliamentary elections are introduced, which in turn may alter the identity and preferences of the pivotal voter. Ideally, we would therefore like to provide direct evidence on political participation from our switcher sample at different points in time. Unfortunately however, there are no historical data on turnout in assemblies or in local legislative elections. We draw instead on a recent country-wide

survey of commune secretaries (Ladner 2009) that inquired about patterns of participation in assemblies and local executive elections. Figure 5 documents that turnout in communal executive elections is indeed an order of magnitude higher than participation in assemblies.<sup>9</sup> While turnout is only available for local executive - not legislative - elections, we verified for recent elections in our switcher sample that local executive and legislative turnout are highly correlated (see online appendix Figure 1). We therefore think it is reasonable to assume that similar participation patterns also characterized earlier periods.

## 7.2 Characteristics of assembly participants

In order to understand whether assembly participants differ from voters in elections and from the electorate at large, we conducted our own survey in canton Zürich communes during the fall of 2016. Out of the 154 communes in canton Zürich with a citizen assembly, 62 agreed to participate in the survey. We decided to gather assembly participants' characteristics at the "budget" assembly, which is when the upcoming year's budget is decided. At the start of the assembly, the commune secretary explained to participants that the survey was part of a study financed by the Swiss national science foundation investigating the functioning of citizen assemblies. The secretary also encouraged assembly participants to fill out the survey, explaining that anonymity was guaranteed and that the survey would take less than five minutes to fill out. The survey itself consisted of two pages and asked about gender, age, family status, education and labor market status. 3,574 assembly participants filled out the survey. The secretary counted the total number of assembly participants so that we could assess the response rate, which was 66 percent on average across communes. As a robustness check, we also consider only communes with a response rate larger

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<sup>9</sup>A few communes appear to have zero election turnout, which seems unlikely. We checked that these zeros are not due to coding errors. Perhaps they are due to approximations given by the commune secretaries responding to the survey.

than 70 percent.

### 7.3 Electorate characteristics

From the statistical office of canton Zürich we obtained information on the set of individuals eligible to vote (Swiss citizens, aged 18 years and above) in each commune. Data on age are administrative and cover the entire population of the canton, while data on education, family structure and hours worked are collected as part of an annual survey (“Strukturerhebung”) run jointly by the federal government and cantonal authorities.<sup>10</sup> We aggregate each variable across all individuals living in the 62 communes that participated in our assembly survey.

### 7.4 Voter characteristics

Local parliament elections take place every four years and voting is either done by mail or at the ballot box. Because there are no commune-level surveys of voter characteristics, we rely on post-national-election surveys (SELECTS) that are representative at the cantonal, not local level. Respondents were contacted in the weeks following an election and were asked about their gender, age, education, income, and civil status.<sup>11</sup> We combine the 2011 and 2015 survey rounds to obtain a total sample of 1,127 respondents from canton Zürich who participated in the respective preceding national elections.

One natural question is whether voters in national elections differ from voters in cantonal or local elections. To address this concern, we exploit an earlier post-national-election survey from 2007 that asked about participation in both federal and cantonal elections. Turnout was only three percentage points lower in cantonal elections compared to federal elections (63 percent

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<sup>10</sup>See <https://www.bfs.admin.ch/bfs/de/home/statistiken/bevoelkerung/erhebungen/se.html> for further information.

<sup>11</sup>A description of the surveys and all the data can be found here: <http://forscenter.ch/en/our-surveys/selects/>.

versus 66 percent). Unsurprisingly, the characteristics of voters in federal and cantonal elections in terms of age, gender, education and income were almost identical. Similarly, even though turnout in local elections tends to be even lower (about 41 percent in our switcher sample), an even earlier survey from 2003 asked about respondents' interest in local and cantonal politics on a scale from one to four. Among voters in cantonal elections, average interest in local politics was 2.88, while for cantonal politics it was 2.86. It therefore seems reasonable to expect socio-demographic characteristics of voters in cantonal and local elections to be similar.

## 7.5 Comparison of characteristics

Panel A of Figure 6 documents that assembly participants are substantially older than the electorate in canton Zürich communes that took part in our assembly survey. As is evident from that figure, 20- to 40-year-olds are particularly under-represented in those communes. While the average Swiss citizen is 50.9 years old, average age of assembly participants is 57.1. Panel B of Figure 6 shows that 20- to 40-year-olds are under-represented in assemblies also compared to canton Zürich voters who participated in national elections (the average voter is 52.8 years old). The age distribution of assembly participants is also statistically different from the age distributions of the electorate and of voters (Kolmogorov-Smirnov p-values=0.000).

In addition, Panel A of Figure 7 shows that women are under-represented in assemblies compared to their proportion in the electorate in the set of communes that participated in our assembly survey. While the proportion of females among Swiss citizens is about 0.51, the proportion of female assembly participants is only 0.40. Similarly, Panel B of Figure 7 shows that women are also under-represented in assemblies when compared to canton Zürich voters in national elections in which about 48 percent are female. The proportion of women in assemblies is also statistically different from the proportion of women in the electorate or among voters (p-values=0.000). Fig-

ures 7 and 8 in the online appendix show that these age and gender distribution differences are robust to restricting the sample to communes with an assembly survey response rate above 70 percent. Results for other characteristics are less clear-cut. For example, Figure 9 in the online appendix shows that while average education is similar among assembly participants and canton Zürich voters in national elections, the variance is higher among assembly participants.

A natural concern is whether these results generalize beyond canton Zürich. We again take advantage of the 2009 Ladner survey of commune secretaries, who were asked about their subjective opinion regarding which groups of people are over- or under-represented at assemblies in their commune. Reassuringly, Figure 8 shows that commune secretaries tend to view women and especially young people as being under-represented at assemblies. Similarly, the rich and highly educated are if anything under-represented in assemblies, which goes counter to the idea of reduced elite capture under parliament driving increased education spending.

## 7.6 Policy preferences

The above results document sizeable differences in terms of age and gender between assembly participants, voters, and the electorate at large. In order to assess whether policy preferences differ by age and gender, we rely again on a post-election survey from 2011, which is representative at the cantonal level. We consider all respondents, irrespective of whether they participated in the election or not. One of the key questions in the survey asks whether it should be the responsibility of the state to provide affordable childcare for parents who would like to combine work and family. Childcare provision is primarily a commune responsibility. The survey also asks whether respondents are in favor of higher or lower government spending on several items, including welfare benefits and police and law enforcement. While welfare benefits are again mainly provided by local governments, police and law enforcement tend to be provided by both commune



and cantonal authorities (Rüthli, 2012).

Panel A of Figure 9 shows that women are 10 percentage points more likely to favor local public childcare provision than men. Moreover, Panel D documents that Swiss 20- to 40-year-olds are 15 percentage points more likely to be in favor of local public childcare provision than older citizens. These age and gender differences in policy preferences are not only economically but also statistically significant (p-values=0.000). Similarly, results from another survey by Cattaneo and Wolter (2009) confirm that elderly people in Switzerland are less supportive of education spending more generally. Switching from citizen assembly to parliament in our setting therefore seems to increase the representation of two demographics with relatively strong preferences for public spending on education. At the same time, relatively young citizens and women are not more favorable to increased spending on welfare and law enforcement than older citizens and men, as shown in Panels B, C, E, and F of Figure 9. Our findings that parliament adoption increases spending on education - but not on law enforcement or welfare spending - are thus remarkably consistent with the evidence on policy preferences of under-represented groups in assemblies.

## 8 Conclusion

This paper empirically investigates whether the choice of legislative institution matters for the level and composition of local government spending in Switzerland. We find that for medium-sized communes that all switched their legislative form at least once between 1945 and 2010, introducing a parliament increases total spending per capita by about 6 percent. The spending increase is mostly driven by general administration and education spending. While rent seeking and the cost of running a parliament can explain the increase in administrative spending, they are unlikely to account for the increase in education spending. A more likely mechanism is a change in the identity and preferences of the pivotal voter. Legislative elections (compared to assemblies)

increase the representation of relatively young citizens and women, two groups that tend to be relatively favorable to public spending on education. Overall, these results suggest that the form of the local legislative institution matters for budget allocation and that the benefits of direct citizen participation may come at the cost of selective representation. Future research might therefore investigate ways to give under-represented groups more voice in the assembly decision-making process.

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Table 1: Summary statistics for the switcher sample

	Obs.	Mean	Std. D.	Min	Max
<u>Form of the local legislative power (authors' data collection)</u>					
Parliament (1), Assembly (0)	5,082	0.506	0.500	0	1
<u>Local budgetary data (authors' data collection)</u>					
Total revenue per capita	4,762	3,702	2,562	134	30,273
Total spending per capita	4,790	3,659	2,520	164	30,273
Administrative spending per capita	4,797	370	287	16	2,620
Welfare spending per capita	4,285	437	495	0	3,543
Law enforcement spending per capita	4,329	149	126	0	1,234
Education spending per capita	4,502	755	507	3	2,848
Health spending per capita	3,400	150	151	0	1,056
Traffic and environment spending per capita	4,680	632	650	0	7,587
<u>Control variables (Bundesamt für Statistik)</u>					
Resident population	5,082	8,532	6,052	404	29,006
Labor force participation rate (%)	5,082	61.8	4.6	46.5	79.1
Share of 0- to 19-year-olds (%)	5,082	28.8	6.1	14.9	45.7
Share of 20- to 39-year-olds (%)	5,082	30.5	4.2	13.9	53.0
Share of 40- to 64-year-olds (%)	5,082	28.9	3.9	16.9	45.5
Share of at least 65-year-olds (%)	5,082	11.8	4.4	2.7	30.0
Share foreigners (%)	5,082	15.4	9.5	0	53.8
Woman suffrage	5,082	0.617	0.486	0	1

*Notes:* The unit of observation is a commune-year. There are 77 switcher communes and the sample period ranges from 1945 to 2010. Budgetary data are in year 2010 Swiss Francs based on the consumer price index. Control variables are from the Swiss Federal Statistical Office (Bundesamt für Statistik), except for the woman suffrage indicator, which is from our legislative survey. Resident population is based on administrative data from 1981 to 2010 and interpolated from census data between 1945 and 1980. The other control variables are interpolated based on census data.

Table 2: Impact estimates on ln(spending per capita)

	A. ln(total spending per capita)	B. ln(administrative spending per capita)	C. ln(education spending per capita)
Parliament (0/1)	0.071* (0.040)	0.079* (0.044)	0.072* (0.043)
Observations	4,790	4,790	4,790
R-squared	0.87	0.91	0.93
	D. ln(welfare spending per capita)	E. ln(law enforcement spending per capita)	F. ln(traffic/environment spending per capita)
Parliament (0/1)	-0.006 (0.073)	0.008 (0.064)	-0.001 (0.062)
Observations	4,282	4,282	4,282
R-squared	0.83	0.89	0.82
Year fixed effects	Y	Y	Y
Commune fixed effects	Y	Y	Y
Population	Y	Y	Y
Other covariates	Y	Y	Y
Commune linear trend	N	Y	Y
Structural break dummies	N	N	N

*Notes:* OLS estimations. The unit of observation is a commune-year. The sample period is from 1945 to 2010. The number of communes is 77. Commune-level-clustered standard errors are in parentheses. Population adds linear and quadratic terms in resident population to the regression. Other covariates are: labor force participation, share of 20- to 39-year-old residents, share of 40- to 64-year-old residents, share of at least 65-year-old residents, share of foreigners and an indicator for commune-level woman suffrage. The coding of structural break dummies is described in the main text. (\*\*\*, \*\*, and \*) denote significance at the 1 percent, 5 percent and 10 percent levels, respectively.



Table 3: Impact estimates on  $\ln(\text{spending per capita})$  holding referendum rights constant

	I. Subsample of switches between assembly and parliament forms holding referendum rights constant			C. $\ln(\text{education spending per capita})$					
	A. $\ln(\text{total spending per capita})$	B. $\ln(\text{administrative spending per capita})$							
Parliament (0/1)	0.081 (0.066)	0.090 (0.065)	0.083 (0.065)	0.118 (0.088)	0.126 (0.077)	0.109 (0.077)	0.206 (0.180)	0.268* (0.134)	0.186* (0.093)
Observations	2,208	2,208	2,208	2,202	2,202	2,202	2,041	2,041	2,041
R-squared	0.87	0.91	0.91	0.73	0.86	0.87	0.80	0.91	0.92
	II. Subsample of switches between assembly and parliament legislative forms with changing referendum rights			E. $\ln(\text{administrative spending per capita})$			F. $\ln(\text{education spending per capita})$		
	D. $\ln(\text{total spending per capita})$								
Parliament (0/1)	0.107 (0.070)	0.092 (0.068)	0.104** (0.041)	0.019 (0.109)	0.106 (0.106)	0.115 (0.106)	-0.027 (0.076)	0.045 (0.032)	0.043 (0.034)
Observations	2,032	2,032	2,032	2,046	2,046	2,046	1,996	1,996	1,996
R-squared	0.88	0.92	0.94	0.84	0.90	0.90	0.91	0.96	0.96
Year fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Commune fixed effects	Y	Y	Y	Y	Y	Y	Y	Y	Y
Population	Y	Y	Y	Y	Y	Y	Y	Y	Y
Other covariates	Y	Y	Y	Y	Y	Y	Y	Y	Y
Commune linear trend	N	Y	Y	N	Y	Y	N	Y	Y
Structural break dummies	N	N	Y	N	N	Y	N	N	Y

*Notes:* OLS estimations. The unit of observation is a commune-year. The sample period is from 1945 to 2010. The number of communes is 77. Commune-level-clustered standard errors are in parentheses. Population adds linear and quadratic terms in resident population to the regression. Other covariates are: labor force participation, share of 20- to 39-year-old residents, share of 40- to 64-year-old residents, share of at least 65-year-old residents, share of foreigners and an indicator for commune-level woman suffrage. The coding of structural break dummies is described in the main text. Referendum rights are constant when switching from an assembly with(out) compulsory financial referendum to parliament with(out) compulsory financial referendum or vice versa. Referendum rights are changing when the switch is from assembly without compulsory financial referendum to parliament with compulsory financial referendum or vice versa. (\*\*\*, \*\*, and \*) denote significance at the 1 percent, 5 percent and 10 percent levels, respectively.

Table 4: Impact estimates on ln(spending per capita), controlling for mergers

	A. ln(total spending per capita)	B. ln(administrative spending per capita)	C. ln(education spending per capita)
Parliament (0/1)	0.164* (0.089)	0.070* (0.041)	0.062 (0.041)
Observations	4,790	4,790	4,790
R-squared	0.64	0.91	0.92
	D. ln(welfare spending per capita)	E. ln(law enforcement spending per capita)	F. ln(traffic/environment spending per capita)
Parliament (0/1)	0.129 (0.120)	-0.002 (0.062)	-0.009 (0.063)
Observations	4,282	4,282	4,282
R-squared	0.57	0.89	0.89
Year fixed effects	Y	Y	Y
Commune fixed effects	N	Y	Y
Population	N	Y	Y
Other covariates	N	Y	Y
Commune linear trend	N	Y	Y
Structural break dummies	N	Y	Y
Post-merger dummy	N	Y	Y
Post-school comm. integration	N	N	N

*Notes:* OLS estimations. The unit of observation is a commune-year. The sample period is from 1945 to 2010. The number of communes is 77. Commune-level-clustered standard errors are in parentheses. Population adds linear and quadratic terms in resident population to the regression. Other covariates are: labor force participation, share of 20- to 39-year-old residents, share of 40- to 64-year-old residents, share of at least 65-year-old residents, share of foreigners and an indicator for commune-level woman suffrage. The coding of structural break dummies is described in the main text. Post-merger dummy equals 1 in all years following a commune merger. Post-school comm. integration equals 1 in all years following the integration of a school community into the political commune. (\*\*\*, \*\*, and \*) denote significance at the 1 percent, 5 percent and 10 percent levels, respectively.

Figure 1: Geographical distribution of the switcher sample

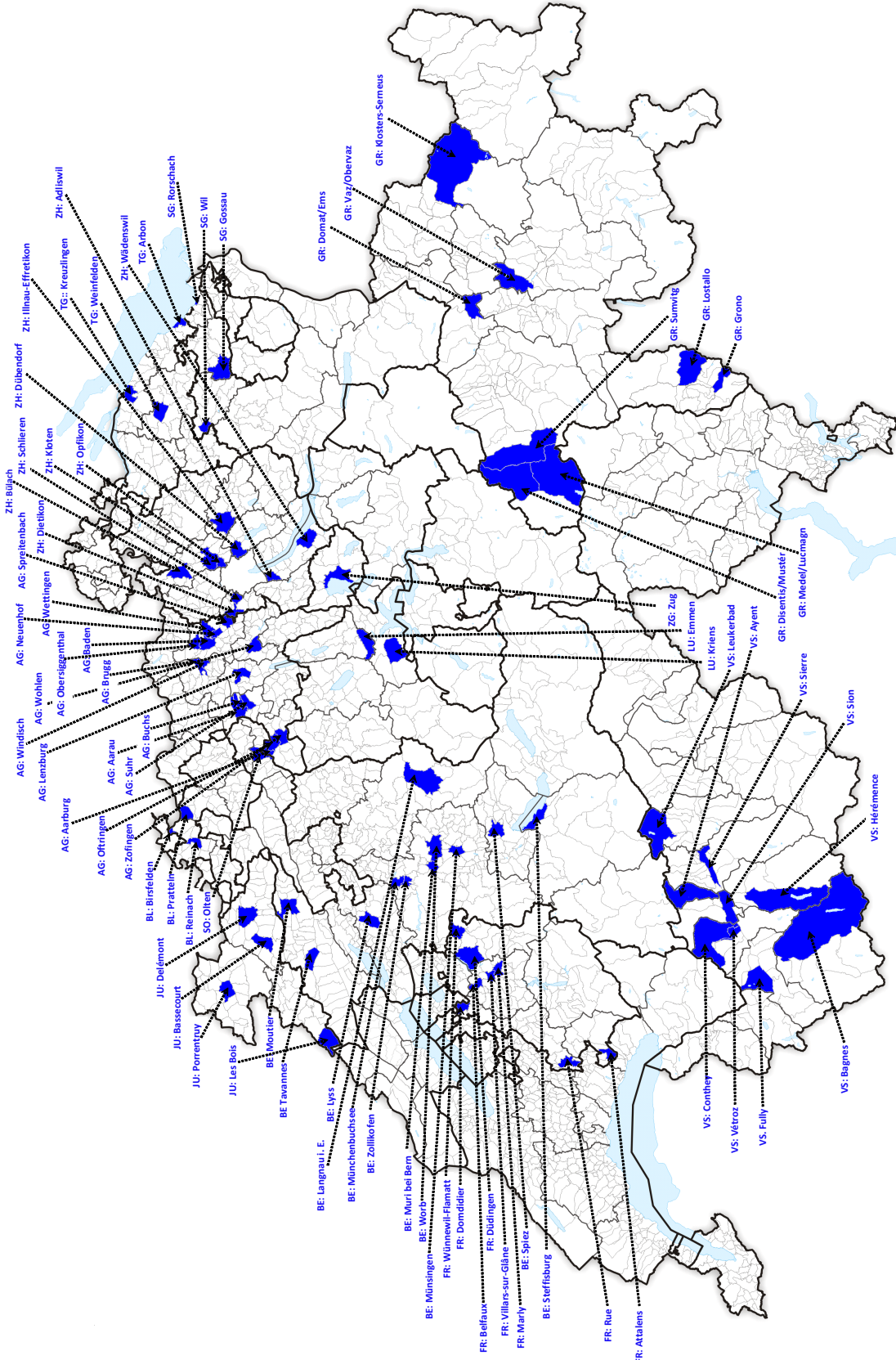
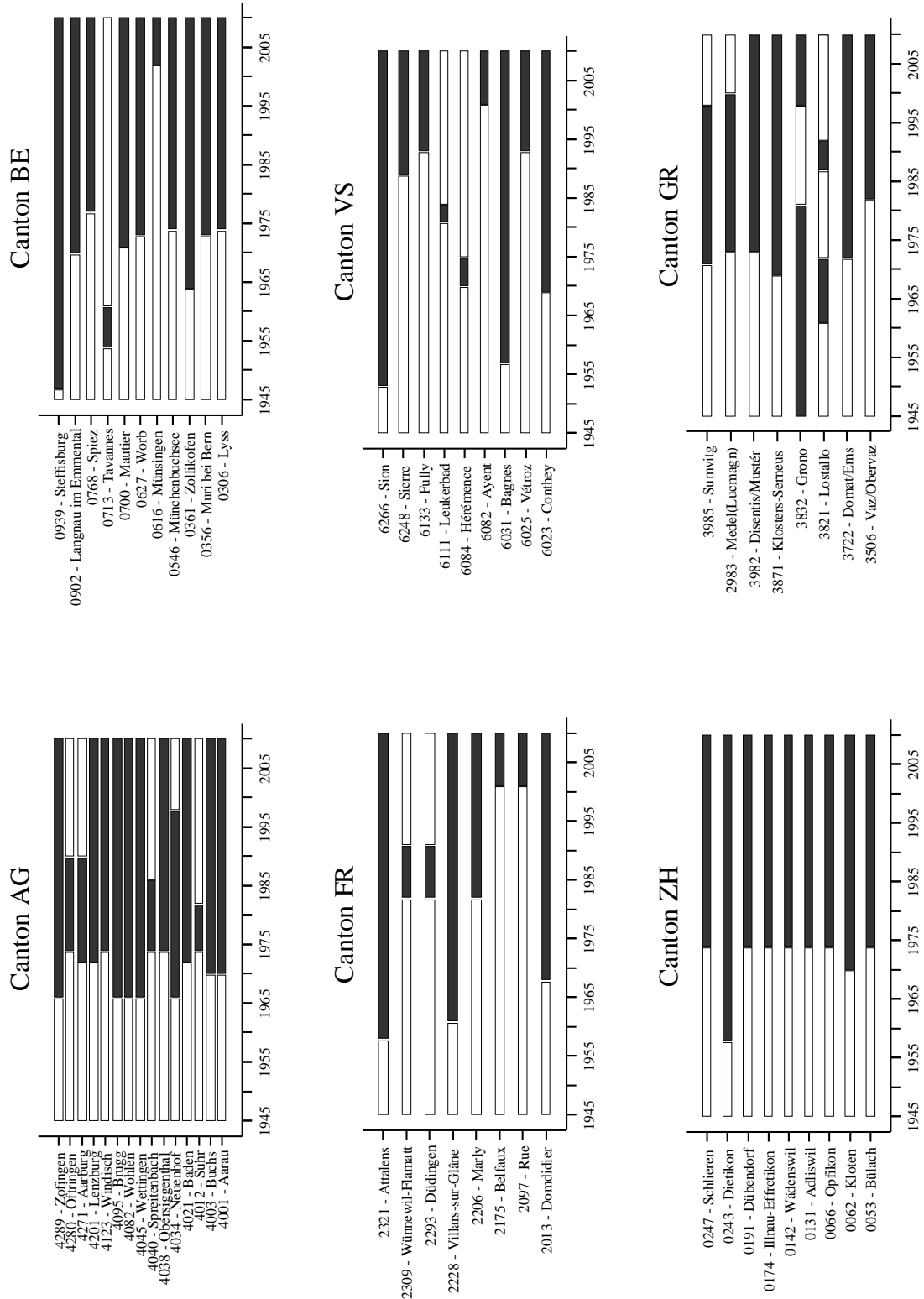
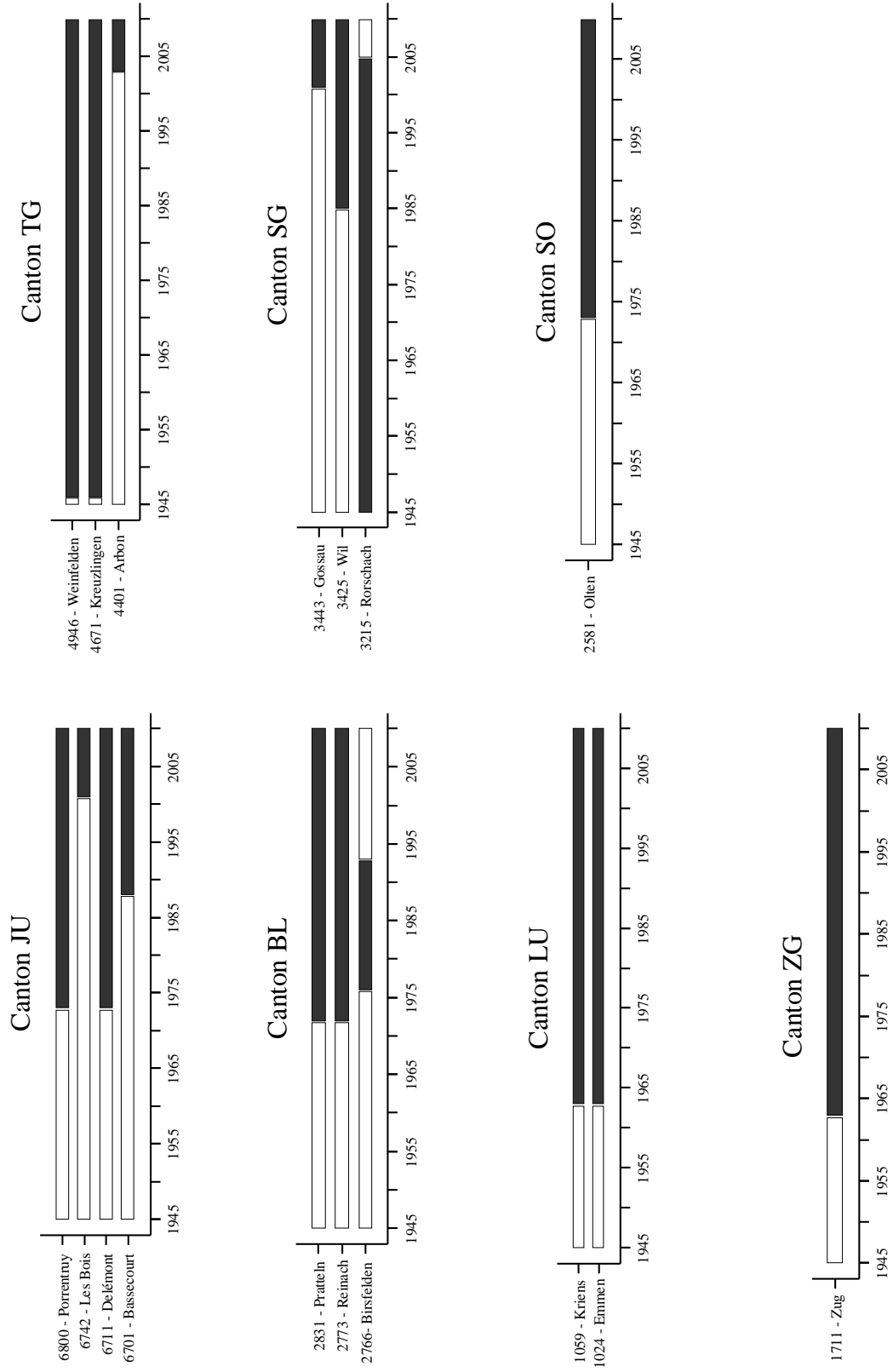


Figure 2: Timeline of legislative forms in the switcher sample



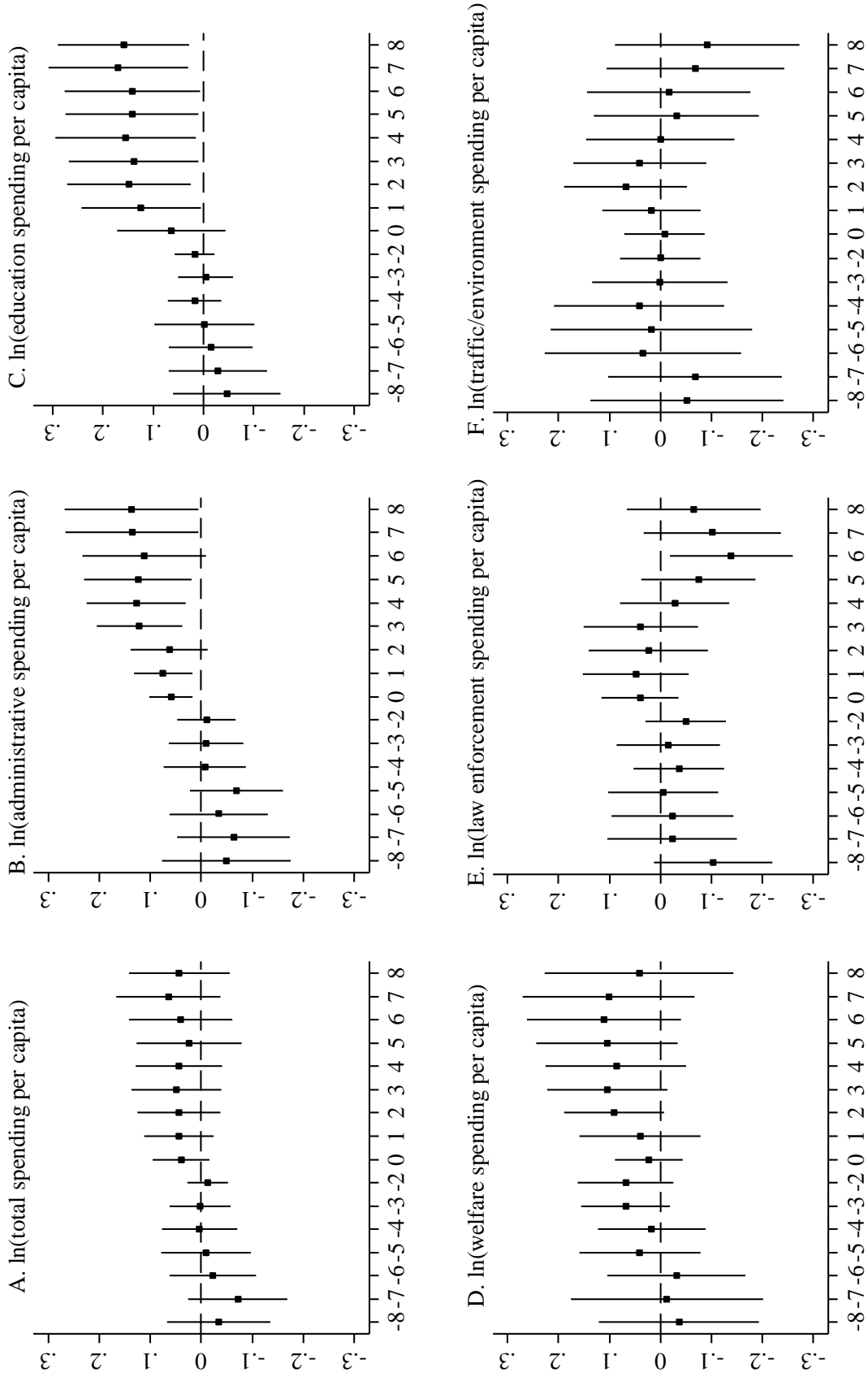
Notes: White bars indicate assembly periods and black bars indicate parliament periods.

Figure 3: Timeline of legislative forms in the switcher sample continued



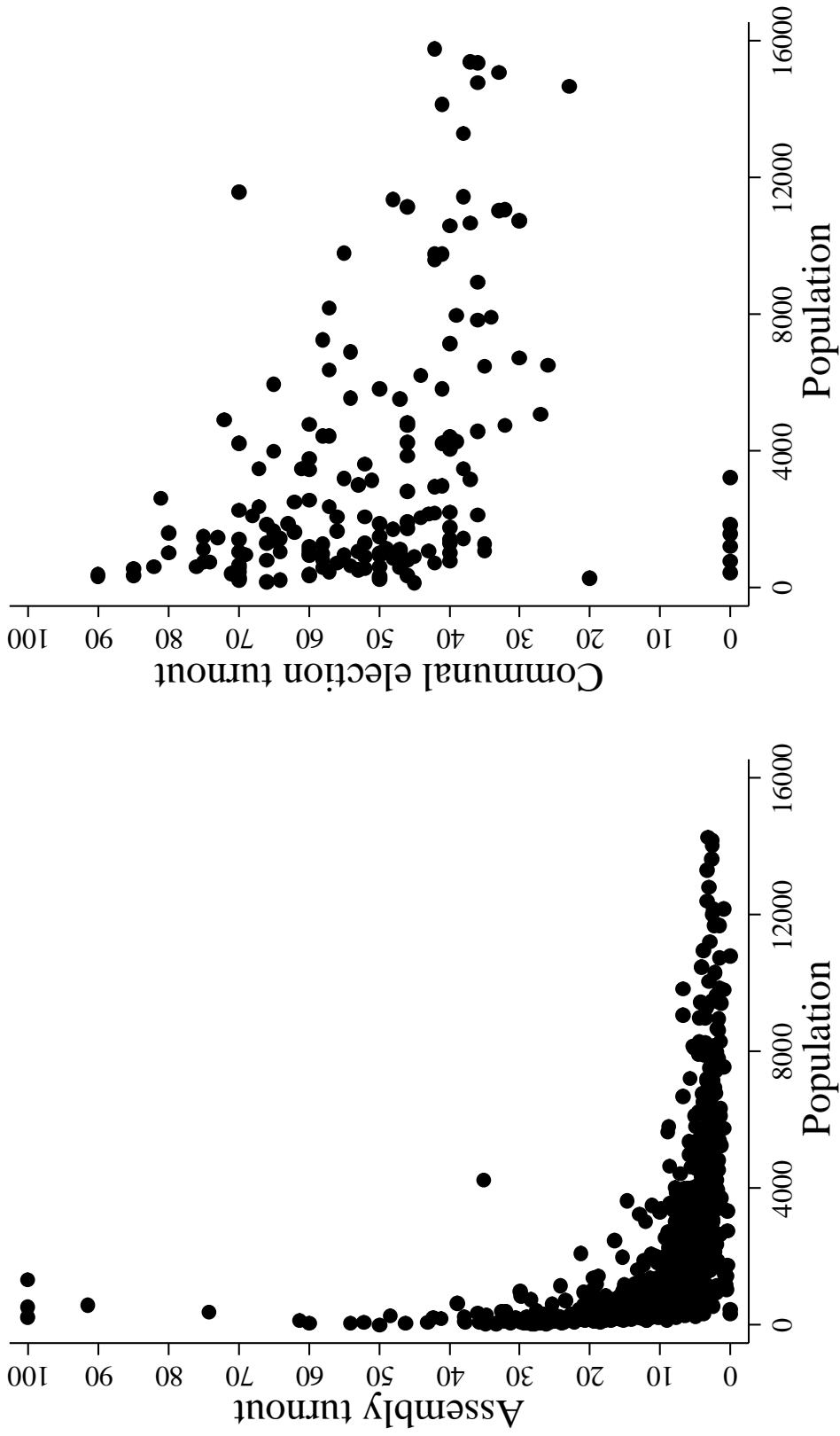
Notes: White bars indicate assembly periods and black bars indicate parliament periods.

Figure 4: Event study impact estimates on ln(spending per capita)



Notes: This figure plots impact estimates of parliament adoption from 8 years prior to adoption to 8 years post-adoption. The base category is the year prior to parliament adoption. The model includes year and commune effects, a commune-specific linear trend, as well as an indicator for 9 or more years before adoption and one for 9 or more years post-adoption. Also included are population and demographic controls, as well as structural break dummies. Bars indicate 95% confidence intervals.

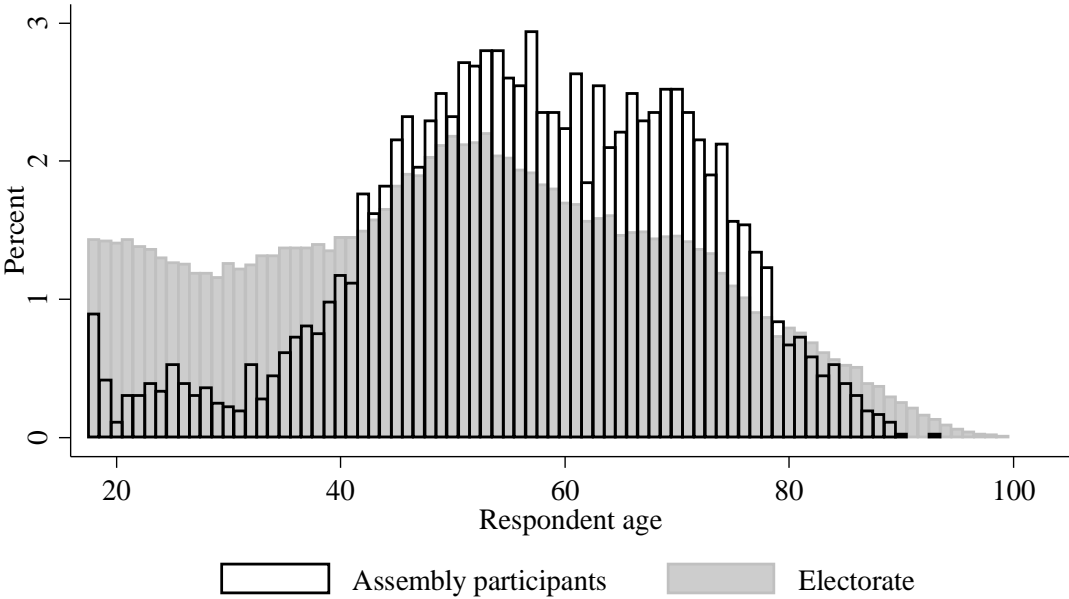
Figure 5: Turnout at assemblies and communal elections



*Notes:* Survey of communal secretaries (Ladner 2009). Each dot represents a commune. Assembly turnout is calculated as the average number of assembly participants as reported by the communal secretary divided by the size of the electorate. Communal election turnout refers to elections for executive office.

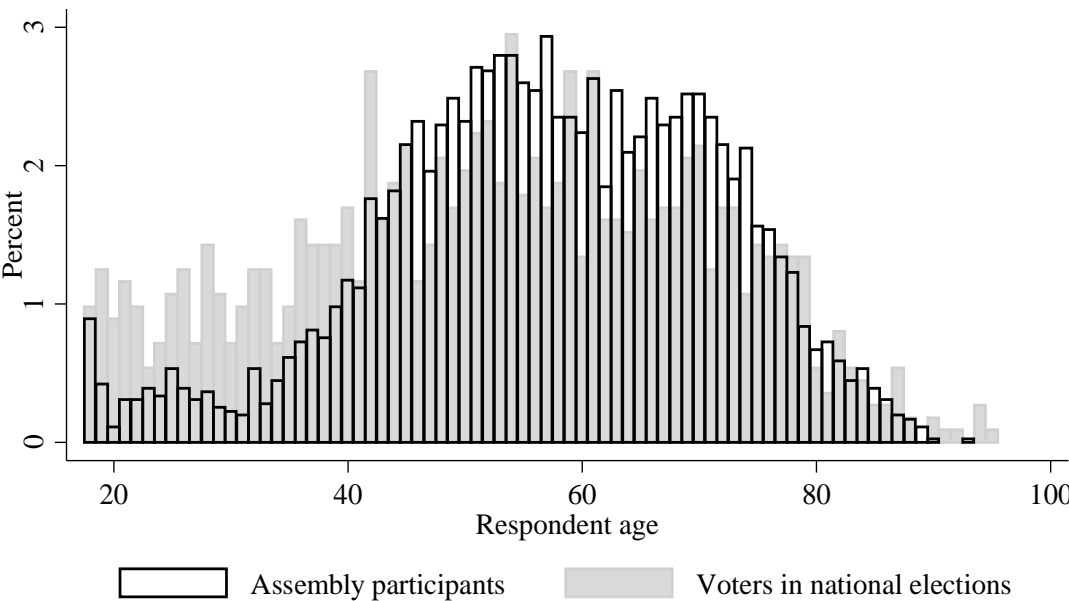
Figure 6: Age of assembly participants, the electorate and voters

Panel A: Assembly vs. Electorate



*Notes:* All respondents are from canton Zürich communes that took part in our 2016 assembly survey. Assembly participants responded to our survey. The electorate corresponds to Swiss citizens and is based on register data collected by the statistical office of canton Zürich.

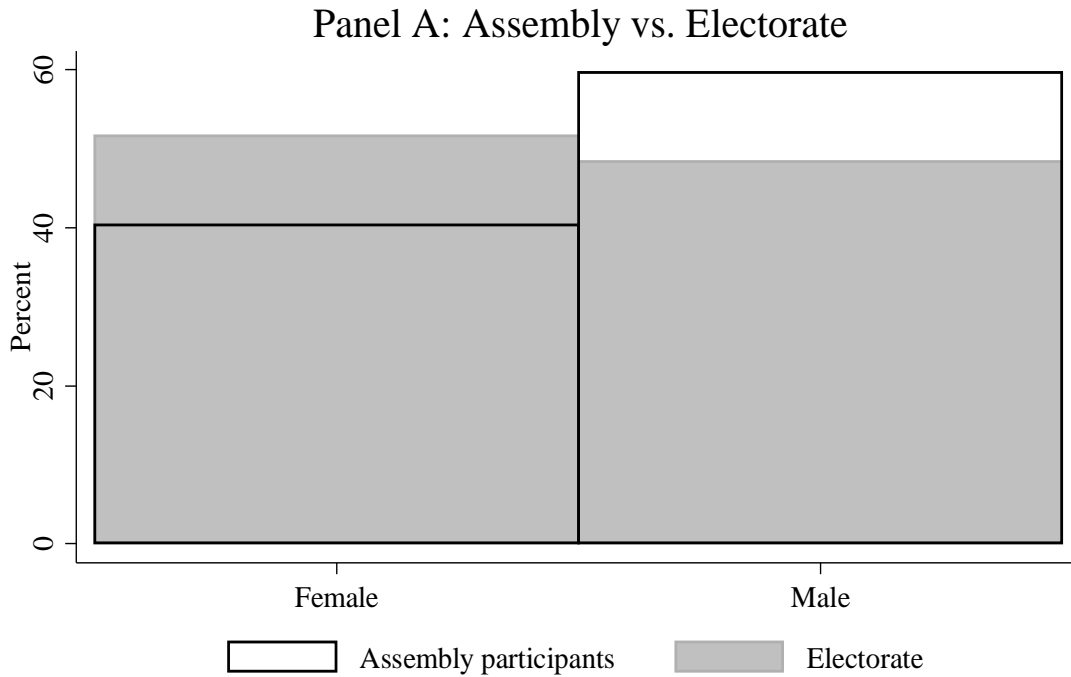
Panel B: Assembly vs. Voters in Elections



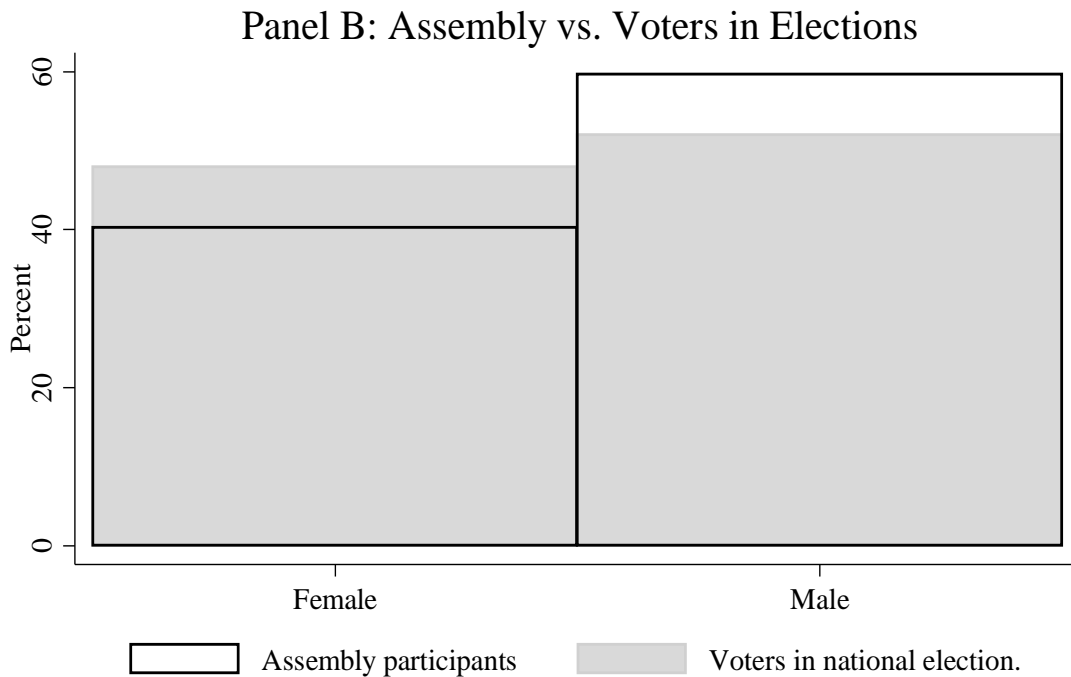
*Notes:* All respondents are from canton Zürich. Assembly participants are from those communes that took part in our 2016 assembly survey. Voters in national elections participated in the Swiss Electoral Studies surveys of 2011 or 2015 and are from the entire canton.



Figure 7: Gender of assembly participants, the electorate and voters

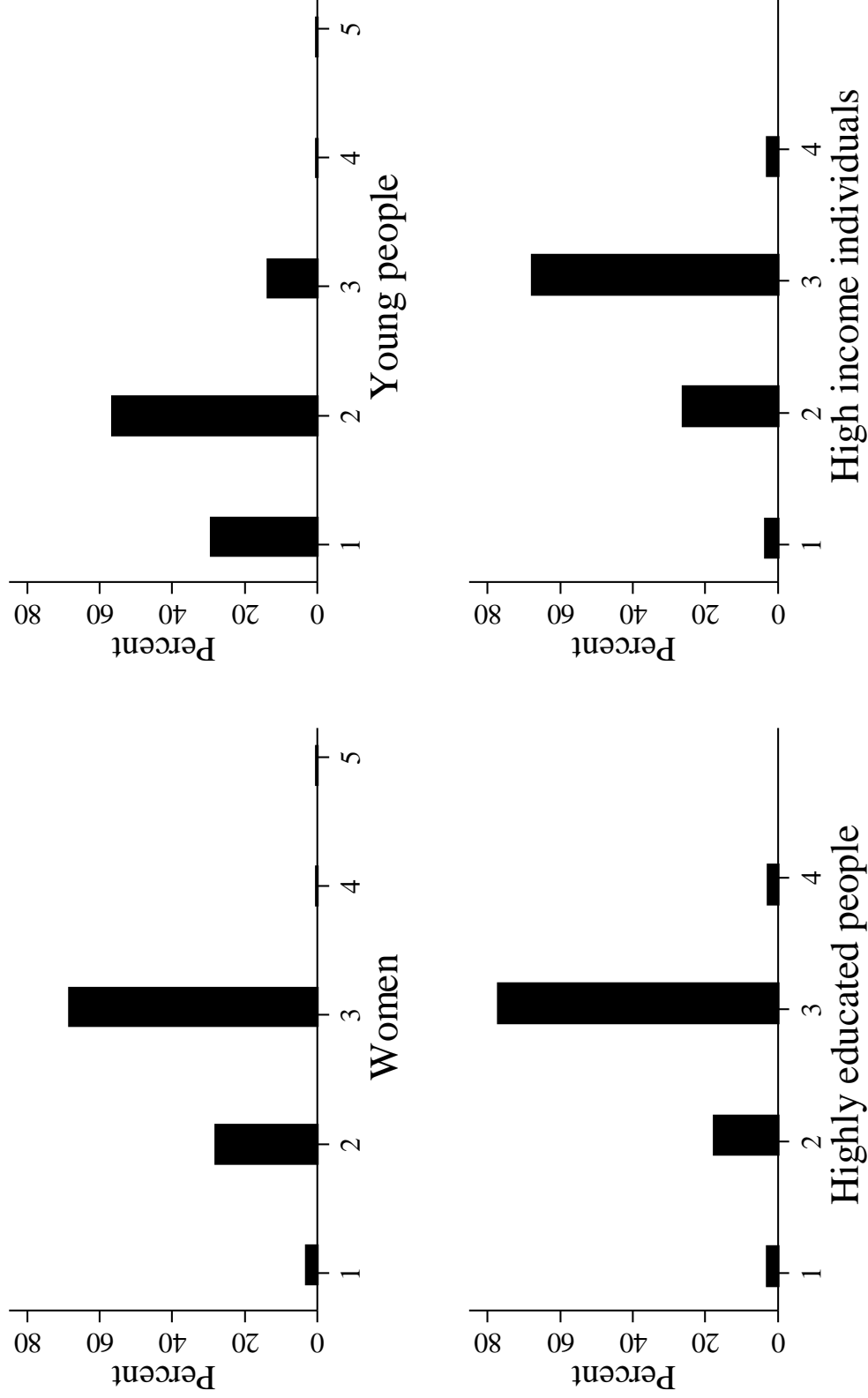


*Notes:* All respondents are from canton Zürich communes that took part in our 2016 assembly survey. Assembly participants responded to our survey. The electorate corresponds to Swiss citizens and is based on register data collected by the statistical office of canton Zürich.



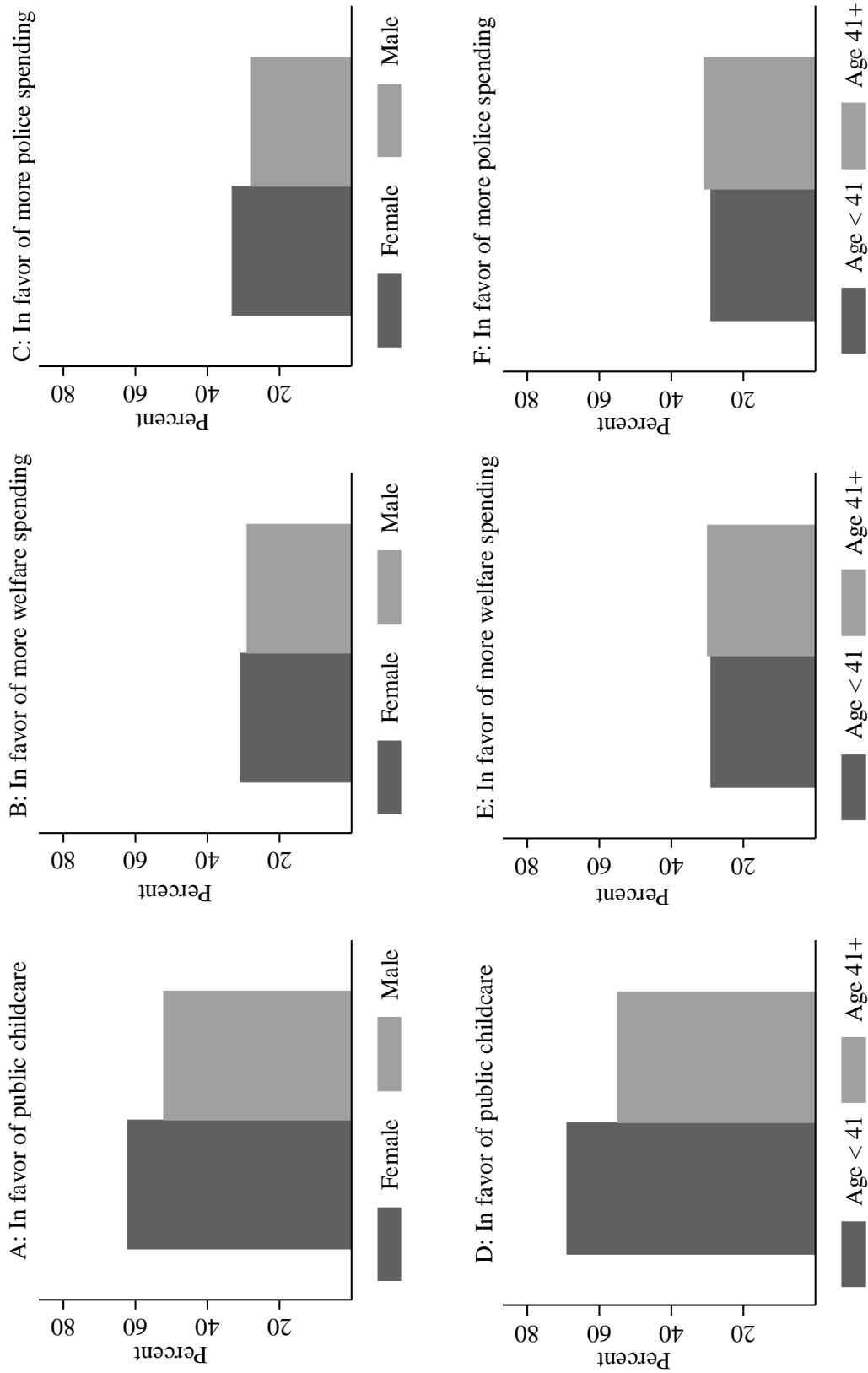
*Notes:* All respondents are from canton Zürich. Assembly participants are from those communes that took part in our 2016 assembly survey. Voters in national elections participated in the Swiss Electoral Studies surveys of 2011 or 2015 and are from the entire canton.

Figure 8: Representation of different groups at assemblies



Notes: Survey of commune secretaries (Ladner 2009). Representation of key groups at assemblies according to the commune secretary. 1: strongly under-represented, 2: under-represented, 3: adequately represented, 4: over-represented, 5: strongly over-represented.

Figure 9: Policy preferences by gender and age



Notes: The sample consists of respondents to the Swiss Electoral Studies survey of 2011. The survey question in panels A and D was: Do you think it should be the responsibility of the state to provide affordable childcare for parents who would like to combine work and family? For panels B and E, the question was: Are you in favor of higher or lower welfare benefits? For panels C and F, the question was: Are you in favor of higher or lower spending on police and law enforcement?