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Decision-making Institutions and Voters' Preferences for Fiscal Policies[☆]

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Abstract

This paper examines the effect of local political decision-making institutions (i.e., direct democracy vs. representative democracy) on citizens' preferences toward public spending. Exogenous variation in institutions comes from a regression discontinuity design, which exploits a discrete change in the probability that a municipality has representative democracy based on a legally stipulated population threshold in the Swiss canton (state) of Vaud. Fiscal policy preferences by municipality are measured by vote shares on Swiss national referendums and initiatives that, if approved, would have increased public spending. Relative to direct democracy, representative democracy reduces vote shares in favor of spending by around 5 percentage points. The effect is not due to sorting on other observables or to feedback from changes in local policies. These findings demonstrate the importance of preferences as a channel through which political decision-making institutions can affect public policies.

JEL Classification: D7, H7

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1. Introduction

There is abundant evidence to suggest that political decision-making institutions (i.e., direct vs. representative democracy) affect local public spending (Feld and Matsusaka, 2003; Funk and Gathmann, 2011; Funk and Litschig, 2018; Hinnerich and Pettersson-Lidbom, 2014; Galletta and Jametti, 2015).¹ In contrast, very little is known about the effects of these kinds of institutions on people’s preferences.² The main goal of this paper is to provide causal evidence on the relationship between political decision-making institutions and citizens’ support for fiscal policies.

This is a relevant open question for two main reasons. First, in recent years, several articles have demonstrated the importance of society’s formal institutions in determining people’s preferences.³ Most of these studies compare individuals’ preferences across countries with different statewide politico-economic frameworks. But little is known about the role of less wide-ranging, and more local, institutions in affecting preferences.

Second, the existing empirical literature relating political decision-making institutions and public policies implicitly relies on the assumption that preferences are not affected by such institutions. This could be problematic because “if preferences are affected by the policies or institutional arrangements we study, we can neither accurately predict nor coherently evaluate the likely consequences of new policies or institutions without taking account of preference endogeneity” (Bowles, 1998, p. 75). Highlighting a causal effect of the decision-making process on preferences suggests that previous reduced-form estimates of how institutions affect policies can be decomposed into direct effects and indirect preference effects. This might challenge the reliability, or at least the interpretation, of existing results.

The empirical context is municipalities in the Swiss canton of Vaud. These municipalities have two forms of local government. The traditional form is direct democracy via citizen assemblies (i.e., town meetings), while the alternative one is representative democracy via local parliaments (i.e., city council). Cantonal law mandates that municipalities with more than 800 inhabitants adopt a city council. Municipalities with fewer than 800 inhabitants utilize a town meeting by default but have the option to switch to representative democracy. Exploiting this population threshold, the main results of this

¹Matsusaka (2018) provides a survey of recent findings.

²An important exception is the correlational study by Funk and Gathmann (2013).

³Alesina and Giuliano (2015) provide a survey of recent findings.

paper are estimated using a (fuzzy) regression discontinuity design (RD).⁴

To measure voters' fiscal policy preferences, I use municipality-level results of national referendums and initiatives for the years 1970 through 2001.⁵ These votes provide an informative overview of Swiss opinion across a broad set of topics, and importantly, these are national issues unrelated to local politics.⁶ Specifically, I follow Funk and Gathmann (2013) and select 39 popular votes on issues that, if approved, would have implied an increase in the public budget.

The RD estimates indicate that, relative to direct democracy, representative democracy is associated with fiscal conservatism. Specifically, in municipalities governed by representatives, the share of voters in favor of federal proposals that increase public spending is, on average, 5 percentage points lower compared to municipalities ruled directly by citizens participating in town meetings. The effect is substantiated through a series of robustness checks.⁷

A number of mechanisms could explain this effect of institutions on preferences. First, there could be differential sorting of voters based on preferences in response to the decision process. However, I can rule this out. For one thing, local socio-demographic covariates are balanced at the RD threshold. Also, the results are robust to inclusion of covariates optimally selected via double LASSO (Belloni et al., 2014). Moreover, I formally exclude the possibility that decision-making institutions affect electorate participation in federal propositions.

Next, there could be feedback effects from the implementation of policies. As different decision-making institutions favor different public policies, these might also indirectly affect individual preferences (Chen et al., 2016). In particular, higher public spending locally would likely reduce the marginal value of public spending nationally. Alternately, higher public spending locally could be due to capture by local public officials. This may be more likely to occur under delegation to a city council than under direct democracy. As municipalities learn about these hazards, they might become more skeptical of public

⁴Funk and Litschig (2018) and Hinnerich and Pettersson-Lidbom (2014) use a similar estimation strategy by studying the effect of town meetings on public policies, rather than preferences for public expenditure.

⁵To be more specific, voting behavior is a measure of revealed preferences, and therefore it is an expression of preferences and/or beliefs.

⁶A similar strategy has been previously employed in many other studies. For example: Eugster et al. (2011), Funk (2016), Funk and Gathmann (2011), Funk and Gathmann (2013), and Stutzer and Lalive (2004).

⁷In the online Appendix B, I provide complementary results using an event-study design.

spending. But in this empirical context, I can again rule this out: as previously reported by Funk and Litschig (2018), there is no effect at the RD threshold on major enacted fiscal policies (e.g., spending on education, welfare, and health).

Therefore, the results are most consistent with a direct effect of decision-making process on citizens' attitudes (Beath et al., 2017; Dal Bó et al., 2010; Olken, 2010). For instance, the experience or right to participate in town meetings might itself have a positive effect on individuals' satisfaction with public policy or policymakers which, in turn, drives people to favor a larger public sector (Frey et al., 2004; Frey and Stutzer, 2005). Alternatively, communal participation in town meetings might affect people's feelings or trust towards others, increasing altruism or social capital (Putnam et al., 1993; Putnam, 2001). With high social capital, or high valuing of others' utility, individuals may place a higher value on public goods that benefit everyone (Daniele and Geys, 2015; Yamamura, 2012). Moreover, the institutional setting might affect citizens' information about policymaking. People from municipalities with town meetings may be more informed about the functioning of the public sector and therefore have a higher understanding of the implications of either an increase or decrease of public spending (Benz and Stutzer, 2004; Frey, 1994; Mendelsohn and Cutler, 2000). Distinguishing between these three- or other- mechanisms is not possible with the available data.

This paper contributes to two distinct branches of literature. First, it relates to the behavioral political economy literature on the effect of institutions on people's preferences.⁸ Alesina and Giuliano (2015) provide an extensive survey of recent findings and point to the empirical challenge of dealing with the endogenous nature of culture and institutions. To estimate the causal effect of institutions on preferences, authors should take advantage of exogenous institutional reforms that occurred in areas sharing a homogeneous cultural background. Examples include the collapse or rise of socialist regimes in countries such as Germany and North Korea. Combining these events with answers from opinion surveys has made it possible to show the effect of communism on individuals' preferences for redistribution (Alesina and Fuchs-Schündeln, 2007), on women's attitudes toward work (Campa and Serafinelli, 2016), and on social policies (Kim et al., 2016). In contrast, Fuchs-Schündeln and Schündeln (2015) show that preferences depend on an individual's cumulative experience with political institutions. By using individual-level observations from more than 100 countries over two decades, they find that individuals'

⁸This is part of a broader literature on endogenous preferences (Bowles, 1998)

preferences for democracy are positively affected by time spent living under a democratic system.

The second area of contribution examines the effects of the decision-making process on public policies. Matsusaka (2018) provides a survey of recent findings from the U.S. and Switzerland, which points to a systematic negative effect of direct democracy on public spending and revenue. For instance, Feld and Matsusaka (2003) show that Swiss cantons that avail themselves of a mandatory referendum and popular initiative institutions have lower public expenditures. Funk and Gathmann (2011), using a fixed-effects model over a period of 100 years, find similar results and extend them by also showing a reduction in government revenues.⁹ In both studies, the authors account for the endogeneity of direct democratic institutions using an instrumental variable approach. Differently, Hinnerich and Pettersson-Lidbom (2014) use a regression-discontinuity design with population thresholds, similar to the one used in the present paper, to study the effects of town meetings in Swedish municipalities from 1919 to 1938. They find that municipalities with town meetings spent 40–60 percent less on public welfare than municipalities with city councils.¹⁰

Finally, Funk and Litschig (2018) and Funk and Gathmann (2013) are the closest papers to this work. Funk and Litschig (2018) apply an RD design to the case of canton Vaud, similarly to the present research, and also use an event-study approach to 77 Swiss municipalities that switched from a town meeting to a city council in the period 1945–2010. Their primary goal is to estimate the effect of town meetings on local public finance. Their RD results on canton Vaud, in contrast with the evidence from Swedish municipalities, show that town meetings do not affect either the size or composition of the public budget. Yet in the DD analysis on switchers, coherently with the literature, representative democracy increases public spending and revenues. This is mainly driven by an increase in administrative and education spending. Funk and Gathmann (2013) analyze the association between direct democratic institutions and Swiss people’s preferences regarding public expenditure using the results of national referendums and initiatives. They compare cantons with a mandatory budget referendum to those without, showing that citizens in cantons with strong direct democracy are less

⁹Comparable results have been highlighted by recent studies on German local jurisdictions (Arnold et al., 2016; Asatryan, 2016; Asatryan et al., 2017).

¹⁰Sanz (2017) uses an RD model but focuses on Spanish localities.

in favor of public spending. These results are the opposite of mine. Moreover, the authors argue that changes in preferences over time eventually trigger institutional reforms. But they cannot disentangle the causal direction. The present research improves on these findings thanks to a clearer identification of the direction of causality by focusing on a different form of direct democracy and a more local institutional setting.

The remainder of this article is structured as follows. Section 2 provides the institutional background, while the data are described in Section 3. The estimation strategy is explained in Section 4. Results and further identification checks are provided in Section 5. Section 6 discusses the potential mechanisms and Section 7 concludes.

2. Institutional background

Switzerland is a federation of 26 cantons divided into nearly 2,200 municipalities. Swiss federalism assigns cantons high autonomy in setting both taxes and public spending, as well as in most public tasks. Moreover, cantons determine the fiscal and institutional autonomy of their local governments. For this reason, the political decision-making institutions of Swiss municipalities are heterogeneous both between and within cantons.

A unique aspect of Switzerland is the widespread use of direct democratic institutions. Indeed, direct democracy is extensively used in all three levels of government. At the federal and cantonal levels, citizens' direct participation mainly plays a complementary role to the existing representative bodies that usually make decisions.¹¹ Citizens can overturn such decisions through referendums (mandatory or optional). Specifically, any change to the federal constitution by the parliament needs the approval of the majority of citizens as well as of the majority of cantons. Further, any new laws approved by the national parliament can be abolished by the population with an optional referendum. To hold such a referendum requires 50,000 signatures from eligible voters within 100 days. Finally, citizens can also directly amend the federal constitution by proposing a popular initiative. In this case, proponents have to collect 100,000 valid signatures within 18 months. Figure 1 reports the number of national ballots by year from 1970 to 2017. There was at least one proposition a year, with a maximum of 16, while on average there were 8 votes a year.¹² Therefore, all Swiss who have reached majority age are called

¹¹There are still two cantons (Glarus and Appenzell Innerrhoden) where there are no representative bodies and decisions are taken directly by citizens in public meetings (Landsgemeinde).

¹²However, most of the time, people vote about multiple propositions on the same day.

to express their opinions on a regular basis in favor of or against federal propositions. In the empirical analysis, I take advantage of this characteristic to arrive at a precise measure of revealed preferences for public spending in each community.

At the local level, in addition to referendums and initiatives, direct democracy also takes in the form of the town meeting. This is often considered the purest form of direct democracy, as there are no elected representatives who mediate citizens' preferences. In fact, all resident citizens can vote in town meetings. This type of decision-making institution is an alternative to city councils, where elected citizens make decisions. This paper focuses on these two different political decision-making institutions, and to provide causal estimates, it exploits the institutional framework of canton Vaud. Canton Vaud is the third-largest canton in Switzerland, with roughly 800,000 inhabitants it is also the third-largest canton in terms of economic activity.¹³ In 2000, there were 383 municipalities, while today there are 309 municipalities (January 2017).¹⁴ As in other Swiss cantons, local governments have high autonomy in setting both taxes and public spending. Importantly for the empirical analysis, the type of legislative body depends on a municipality's population size. A cantonal law passed in 1814 established that municipalities with 500 or more inhabitants had to operate under a representative democracy (conseil communal/city council), while those with fewer than 500 inhabitants could choose between representative and assembly democracies (conseil général/town meeting). This law has been modified a few times since then. In 1845, with the approval of a new constitution, the population threshold was raised to 600. In 1895, a new reform set the threshold at 800 inhabitants. This threshold was in place for more than a 100 until it was raised to 1,000 in 2005. Importantly, no other institutions or policy arrangements change at this threshold.

The threshold refers to the municipality's population size in the year before an election. Therefore, for example, if elections are held in 2001, a city council would be required if a municipality had a population higher than 800 in 2000. Similarly, a municipality with fewer than 800 inhabitants can still introduce a city council, but this determination has to be made the year before the election. Both city councils and town meetings have the same tasks, and both pass laws with a simple majority. The most important act is the approval of the annual budget. The size of a city council depends on the number of

¹³The canton of Vaud produces roughly 8% of the national GDP.

¹⁴This is the result of a wave of municipal amalgamations.

inhabitants, with a minimum of 30 and a maximum of 100. In town meetings, all resident citizens can participate in the vote. In addition to the legislative body, all municipalities have an executive branch (*municipalité*) elected every four years.¹⁵

To provide more insight into the differences in political life between the two institutional settings, I report statistics from a survey conducted in 1988 by Prof. Andreas Ladner on a large sample of Swiss municipalities (Ladner, 1991).¹⁶ First, in canton Vaud, the average turnout for town meetings is around 35%. This is low compared with the turnout in the executive municipal election (68%) but not too distant from turnout in cantonal (43%) and national (42%) elections.

Second, political parties and other groups of interest usually have limited roles in affecting town meeting decisions. Only 2% of the municipalities said that external influences often affect the outcomes of town meetings, in contrast to 24% declaring that it occasionally happens and 72% claiming that it never occurs. This is in contrast with what reported by Hinnerich and Pettersson-Lidbom (2014) on the Swedish experience.

Moreover, in 93% of the municipalities with town meetings, the personal characteristics of the candidates matter more than party affiliation in the election of the local executive. This is true for 68% of municipalities with a city council. Further, a significant share of municipal executive members are not affiliated with political parties. In around 80% of municipalities, fewer than one-third of executive representatives belong to a party. There are no differences when comparing municipalities with and without town meetings. This implies a generalized limited role of parties in local public administration in Canton Vaud.

Finally, during town meetings, policy decisions are typically approved with a large majority. Only 1% of respondents said decisions were often or very often approved by a narrow margin. The situation is relatively different in city councils, where 10% of municipalities reported the same answer.

¹⁵Starting in 2005, elections are held every five years. The executive has from three to nine members.

¹⁶The questions were addressed to municipal clerks. Around 70% of the municipalities from canton Vaud answered the survey.

3. Data

3.1. *Decision-making institutions in canton Vaud*

To have a complete overview of the institutional arrangements in local administrations in canton Vaud, I have gathered institutional information on 380 municipalities for 9 electoral terms from 1966 to 2001.¹⁷ This information has been manually collected from the Cantonal Archive (Archives Cantonales Vaudoises).¹⁸ As reported in Table 1, at the beginning of the period of interest, 272 municipalities used town meetings and 108 municipalities had a city council. In the latter category, there are 31 municipalities with a population below 800 that decided to introduce a city council even though it was not mandatory. Of the 272 municipalities that had a town meeting in 1966, 41 had switched to a city council by 2001. Half of these reforms (20 cases) took place because of an exogenous increase in population that eventually forced the local administration to introduce a representative body. There were no cases of a municipality that had introduced a city council, either voluntarily or not, reverting to town meetings afterward. From this data, I create the dummy variable *representative democracy* which equals one for the terms when a municipality has a city council.

3.2. *A proxy for voters' preferences*

As already emphasized in the previous section, Swiss citizens are called to votes in favor or against federal propositions on a regular basis. These votes provide an informative overview of Swiss opinions about public policies. Citizens have easy access to information, as they receive ballot documents at home by mail. These documents summarize the arguments for and against the propositions (e.g., parliamentary debates and opinions from interest groups). The option of voting by mail, available since 1995, has made it less “costly” to vote. The average voter turnout is around 40%.

The Swiss statistical office has information about the number of votes in favor of and against each federal proposition, from 1950 until today for each Swiss municipality. For this paper, I follow Funk and Gathmann (2011, 2013) and analyze only the results of 39 popular votes that took place between 1970 and 2001 on issues that would have implied

¹⁷The period of analysis stops in the year 2001 mainly because from that year many municipalities have merged, making it difficult to follow institutional arrangements that changed within a political term.

¹⁸I am also grateful to Patricia Funk and Stephan Litschig for having provided a subset of the municipality institutional data used in this paper at the early stage of this research.

higher government spending. From this data I create the dependent variable *percentage yes-votes*, which measures for each municipality the percentage of voters in favor of the proposition. Also, I calculate the municipal *turnout* for each ballot. Table 2 reports the average value of these two variables for each of the propositions used in the empirical analysis.

3.3. Further data

Besides yearly municipal *population* size, I have collected municipal demographic measures coming from the 1970 decennial Swiss census. These municipal characteristics include: *share of active population*, *share of female active population*, *share of self-employed active population*, *share of active population in agriculture*, *share of active population in industry*, *share of active population in services*, *share of German speakers*, *share of Italian speakers*, *share of Protestants*, *share of adults (age > 19 and age < 65)*, *share of old (age > 64)*, *share of foreigners*, *share of married*, and *share of males*. I complete this set of variables with geographical characteristics such as *surface* and *altitude*. Finally, I add the *share of votes for left-wing political parties* in the 1971 national elections and the *federal tax on income per capita* paid on average by each municipality in 1973.¹⁹

4. Estimation strategy

I provide the main set of results about the effects of political decision-making institutions on citizens' preferences by estimating the following RD model:

$$Y_{kit} = \alpha + \gamma_k + \beta Repr. Democracy_{it} + f(Pop_{it}) + \epsilon_{it} \quad (1)$$

where Y_{kit} denotes the percentage of votes in favor of proposal k in municipality i at time t ; $repr. democracy_{it}$ is a dummy variable taking the value of 1 if a municipality has a city council, at time t . γ_k are referendum fixed effects. Pop_{it} is the municipality population (running variable) the year before the election, while $f()$ is a first- or

¹⁹Note that these variables are predetermined with respect to the dependent variable, but cannot be considered as pre-treatment covariates. Indeed, for most municipalities the treatment status has remained fixed for decades before the period of analysis. Table OA.1 in the online Appendix reports the summary statistics of all the variables used in this paper.

second-order polynomial in Pop_{it} . Because municipalities with fewer than 800 inhabitants are free to choose between representative and direct democracy, I use a fuzzy RD. Therefore, the variable $repr. democracy_{it}$ will be instrumented by $Z_{it} = 1[Pop_{it} \geq 800]$. Importantly, as in the case of Hinnerich and Pettersson-Lidbom (2014), because of a one-sided compliance problem, the identifying assumption of the described RD approach is the same as that of a sharp RD model (Battistin and Rettore, 2008). The continuity of the distribution of the potential outcome at the threshold is satisfied if 1) municipalities did not sort around the threshold and 2) no other institutional arrangements change at the threshold.²⁰ If these conditions are satisfied, the estimated β represents the treatment effect on the treated.²¹

Formally, I follow the most recent guidelines by Calonico et al. (2014) and Cattaneo et al. (2017), and the preferred specifications are nonparametric local linear regressions (Hahn et al., 2001). The bandwidths are selected with a data-driven procedure suggested by Calonico et al. (2014), and I use a triangular kernel to give more weight to observations closer to the threshold (Cheng et al., 1997). To account for serial correlation, I cluster the standard errors at the municipality level.²²

5. Results

5.1. Covariates selection

Before presenting the results of the paper, I briefly describe the methodology used to select covariates. While not required for identification, studies using RD design commonly add covariates to the specification to reach a more precise estimate of the treatment effect. However, not all the variables that one collects are a priori good controls, and

²⁰I will empirically test for point 1) in Section 5.4, while point 2) is satisfied as there are no other cantonal or federal laws that vary in application depending on whether a community has more or less than 800 inhabitants.

²¹The RD exploits the cross-sectional variation in the data and does not account for the time each municipality has spent under a certain institutional framework. For completeness, section B of the online Appendix reports the results of an analysis in which I have exploited the time variation in the data with an event-study design using the 41 switcher municipalities. After the introduction of a city council, citizens tend to be less in favor of new spending, yet the coefficients of interest are not always statistically significant. The absence of significance could be due to either the limited statistical power of the test or because institutions would need several decades in order to influence preferences.

²²Specifically, all the RD estimates are implemented with the package `rdrobust` in STATA, which uses robust bias-corrected inference (Calonico et al., 2014).

the inclusion of many covariates might actually bias the estimates.²³ Therefore, I apply the post-double-selection procedure suggested by Belloni et al. (2014) to select those variables I will use as controls in the following sections. Specifically, I run two different LASSO regressions that include as independent variables the complete set of municipal covariates described in Section 3. In the first case, I use as outcome variable *percentage yes-votes* and in the second case *representative democracy*. The variables selected by the LASSO regression when the dependent variable is *percentage yes-votes* are: *share of active population*, *share of active population in agriculture*, *share of active population in services*, *share of foreigners*, *share of Protestants*, *surface*. However, when the dependent variable is *representative democracy*, only *share of active population in agriculture* is selected as a relevant regressor. The union of these two sets of covariates represents my control variables.

5.2. Main results

In this section, I report the results from the RD estimates by showing the reduced-form (Table 3 and Figure 2), the first stage (Table 4 and Figure 3), and the IV estimates (Table 5) separately. In each of the tables, I apply the following format: results without covariates are shown in the first panel, while the second panel shows results with covariates; the first two columns use a linear polynomial ($p=1$) and the last two a quadratic polynomial ($p=2$); in the first and third columns, the RD uses the optimal bandwidth suggested by Calonico et al. (2014) ($h=h_{cct}$), while in the second and fourth columns, the results are computed considering a bandwidth twice the optimal ($h=2h_{cct}$).

Figure 2 provides a first visual representation of the reduced-form effect. This figure reports the scatterplot of the residual from a regression of the *percentage yes-votes* on the control variables and referendum fixed effects (averaged over 25-inhabitant intervals), together with a local linear smoother using a uniform kernel with a bandwidth of 100. There is a visible discontinuity at the threshold, suggesting lower preferences for public spending in municipalities governed by representative democracy. Table 3 confirms this evidence. If we concentrate on the estimates from the first-order polynomial, the reduced-form effect is between -1.128 and -1.471, while if we look at the second-order polynomial, the effect varies between -0.962 and -1.541. The coefficients seem to be stable over the different sizes of the bandwidth, the order of the polynomial, and the inclusion of

²³Relevant covariates are typically correlated both with the dependent variable and the treatment status.

covariates. Still, controlling of covariates helps in obtaining more precise estimates and a higher level of significance.

Next, Figure 3 shows that the probability of having a city council increases the closer the local population is to the threshold. The estimates reported in Table 4 suggest that the gap in the probability of having a city council at the threshold is around 25 to 30 percentage points. Again, the effect is stable across the different specifications, and it always reaches the conventional level of significance. Indeed, the required relevance of the instrument is satisfied.

Finally, Table 5 displays the IV estimates, which are the principal results of the paper. Consistent with the sign of the reduced-form analysis, the IV estimates confirm that municipalities with a city council are less in favor of public spending compared to municipalities with a town meeting. All the coefficients are statistically significant once covariates are introduced, and this has little effect on the point estimates, particularly in the linear specification.²⁴ In this case, the effect ranges between -5.256 and -5.742 percentage points when a first-order polynomial is used, and between -6.179 and -4.072 percentage points when a second-order polynomial is used.²⁵ In summary, these results suggest that people living under representative democracy are more fiscally conservative than those living under direct democracy. Specifically, the linear estimation shows that the share of voters in favor of federal propositions aimed at increasing public spending is 5 percentage points lower in municipalities governed by representatives compared to municipalities where decisions are made directly by citizens in town meetings.

5.3. Effect on voter turnout

It has been suggested both theoretically and empirically that institutional settings can influence the level of political participation (Hajnal and Lewis, 2003; Osborne et al., 2000; Tolbert et al., 2001).²⁶

For example, one might expect people living in municipalities with town meeting democracy to turn out in lower numbers in federal ballots, as they are already facing

²⁴Note that when all the covariates I collected are included, the coefficients are also statistically significant and with negative sign. However, the point estimates are much larger compared to the ones computed without covariates (between 100% and 50% larger) which might be due to over-fitting.

²⁵The use of a rectangular kernel does not affect the size of the coefficient and would provide smaller standard errors, increasing the level of significance.

²⁶Bechtel et al. (2018) show that the exposure to compulsory voting for around two decades in the first half of the twentieth century in canton Vaud had no long-term impact on participation in federal referendums.

high participation cost in local politics. I check this possibility by employing the same RD strategy of the main analysis but using as a dependent variable the level of *turnout* in federal voting. Figure 4 and Table 6 report the results. Both the graphical representation and the estimates reveal a null effect of representative democracy on voter participation in national ballots. Thus, in the case of canton Vaud it appears that voter turnout is similar between the two different decision-making institutions. The absence of an effect is a crucial evidence to validate the main results, as it would otherwise be difficult to exclude the possibility that heterogeneous participation in elections drove the differences in voters' preferences.

5.4. Check of RD strategy

I provide evidence of the validity of the RD approach. Figure 5 emphasizes the absence of sorting at the cut-off by showing that there no a discontinuity in the density of the forcing variable at the threshold (McCrary, 2008). Specifically, the point estimate of the discontinuity is -0.101 (s.e.=0.116).

Further, Table 7 shows reduced form estimates, with both linear and quadratic polynomials, where the outcome variable is each time one of the municipal covariates I collected as measured at the beginning of the period of analysis.²⁷ Importantly, there is no significant discontinuity at the threshold for any of these variables, which supports the continuity assumption.

Finally, Table 8 shows that the results are consistent across bandwidths. It displays the estimates from 21 regressions, each time considering a different bandwidth. In more detail, I have run estimates using from 50% to 150% of the optimal bandwidth size with a 5% gap. As reported in the second column of Table 8, all coefficients have negative signs; however, they start being statistically significant only for regressions that apply a bandwidth higher than 87 (i.e., 75% of the optimal bandwidth). Importantly, the coefficients are stable for all estimates that use a bandwidth from 95% to 150% the optimal one.

²⁷Figures OA.1 and OA.2 in the online Appendix display a graphical representation of the balance tests.

6. Mechanisms and implications

6.1. Potential mechanisms

In this section, I discuss some of the potential mechanisms explaining the effect of political decision-making institutions on preferences.

A first consideration to take into account is that voters with specific characteristics, potentially correlated with preferences for public spending, decide to locate in a municipality because of the institutional setting (i.e., voter sorting). The estimates reported in Table 7 help in reducing this concern, as there are not substantial differences in voters' observable socio-demographic and political characteristics that might be an indicator of selective migration.

Another possible explanation of the results is a feedback effect coming from the implemented policies. Several papers have shown, for example, that local jurisdictions with direct democracy have a lower levels of public expenditure compared to pure representative democracy (Asatryan, 2016; Feld and Matsusaka, 2003; Funk and Gathmann, 2011; Galletta and Jametti, 2015). Therefore, it could be that citizens' preferences for public spending shifted in the opposite direction of the local defined policy. For example, if high spending under representative democracy was the consequence of local politicians' misbehavior or rent-seeking, it is reasonable to expect citizens to reassess their attitudes toward the public sector. However, Funk and Litschig (2018) show that in the case of canton Vaud the decision-making process has no effect either on the level or composition of the public budget. This makes the presence of a feedback effect from policies unlikely.

Finally, the estimated results could be due to the decision-making process per se, without any mediation by the actual policy selected. Although I cannot perform additional empirical tests to validate this channel because of the lack of data, other studies have uncovered evidence that closely relates to the main results of the paper. Indeed, most of the research supporting this hypothesis emphasizes how citizens who live under direct legislation have a higher satisfaction with public intervention than to those living under representative democracy. This argument relates closely to the idea of procedural utility described by Frey et al. (2004). For instance, Olken (2010) presents results from a field experiment conducted in Indonesian villages emphasizing that people are more satisfied with local policies when they are selected via plebiscites compared to representative-based meetings, even after controlling for the kinds of policies chosen. Moreover, in line with my findings, citizens choosing policies via direct democracy are

more willing to contribute to public projects. Similar results are reached by Beath et al. (2017) in a field experiment across 250 villages in Afghanistan. In fact, they find that direct legislation raises citizens' appreciation of the local governance, even after controlling for policies. Likewise, Frey and Stutzer (2005) look at the Swiss context and, by using survey data from thousands of Swiss residents, highlight a positive effect of the right to vote and participate in political decision-making on individuals' reported well-being.²⁸

6.2. *Implications for the literature*

The main result of the paper indicates that political decision-making institutions have a causal effect on preferences. This is important evidence because it invalidates the assumption that individuals' preferences are exogenous to institutions, which is standard in the empirical literature linking decision-making institutions and public policies. Because of this assumption, it is common to include in the analysis control variables to proxy for people' preferences (e.g., voting behavior in political elections). In light of my results, even if one could avail of a perfect measure of citizens' preferences, it should not be included in the regression because it could bias the estimate, given its endogenous nature.

More generally, my findings hint at the possibility that the existing evidence on the effect of the decision-making process on public policies is indirectly affected by changes in voters' preferences. In other words, the estimates are identifying both the effect of institutions on public policies and the indirect effect coming from differences in preferences. Although the results of this paper have the standard external validity issues of the empirical approach used, if one assumes that they apply to other frameworks, existing findings showing a positive impact of representative democracy on public spending could be considered as a lower bound of the actual effect of such institutions.²⁹ Overall, the literature might be underestimating the direct effect of political decision-making processes on the public budget.

²⁸Instead, Dal Bó et al. (2010) use an experimental framework to emphasize that when decisions are taken directly by individuals, in contrast to when they are exogenously assigned, there is a stronger preference for cooperation. The same result shows up even when the two different decision-making processes select the same policy.

²⁹For instance, the null effect in Funk and Litschig (2018) when studying how decision-making institutions affect public policies in the case of canton Vaud, could be explained by the sum of two opposite effects of representative democracy: a positive on expenditure and a negative on citizens' preferences for public spending.

7. Conclusions

I show that political decision-making institutions affect preferences towards public spending. Focusing on municipalities in the Swiss canton of Vaud, I find that people living under representative democracy are more fiscally conservative than those living under assembly democracy. On the one hand, this evidence suggests that even local institutions do affect preferences. On the other hand, the result speaks to the literature on direct democracy as it indicates that preferences are not something “to control for” but rather one of the potential channels through which decision-making institutions could affect policy outcomes.

In future research, I intend to examine in further detail the channels through which decision-making processes affect preferences and to extend the analysis to preferences regarding other kinds of policies (e.g., immigration, environmental, or social policies).

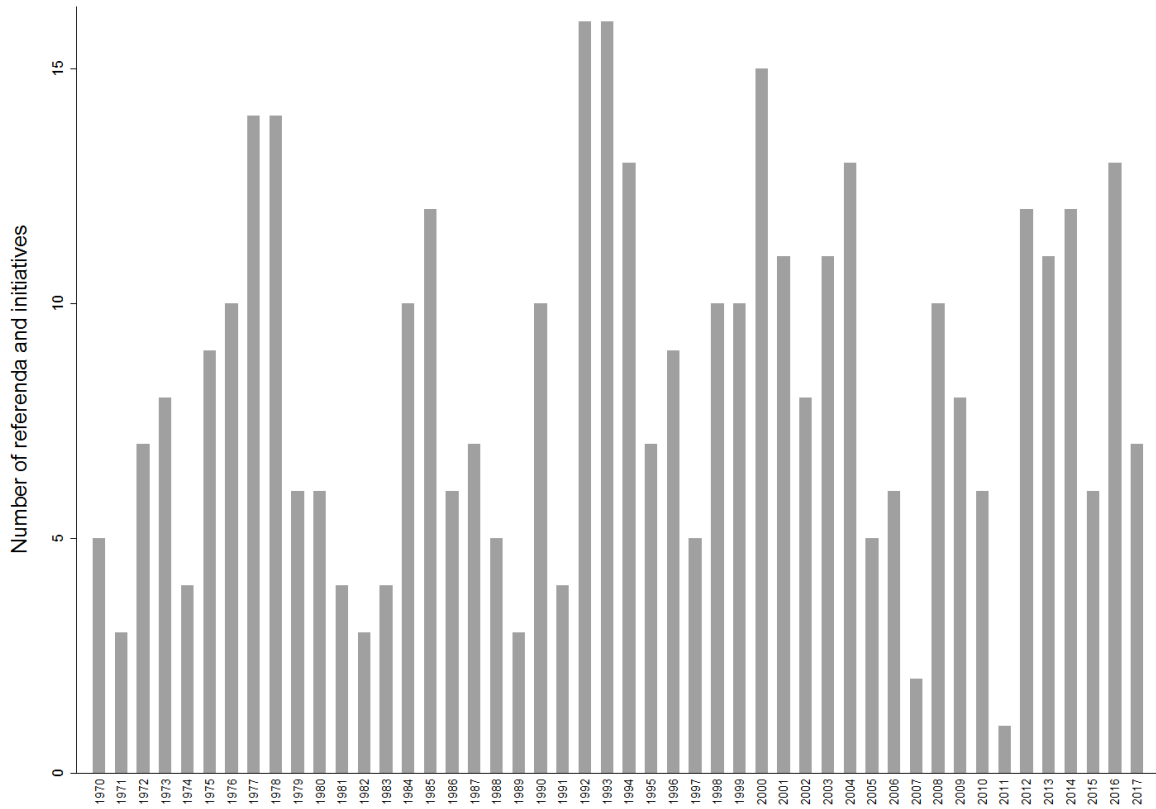


Figure 1: Number of propositions, by year

Notes: This figure displays the total number of Swiss national referendums and initiatives by year.

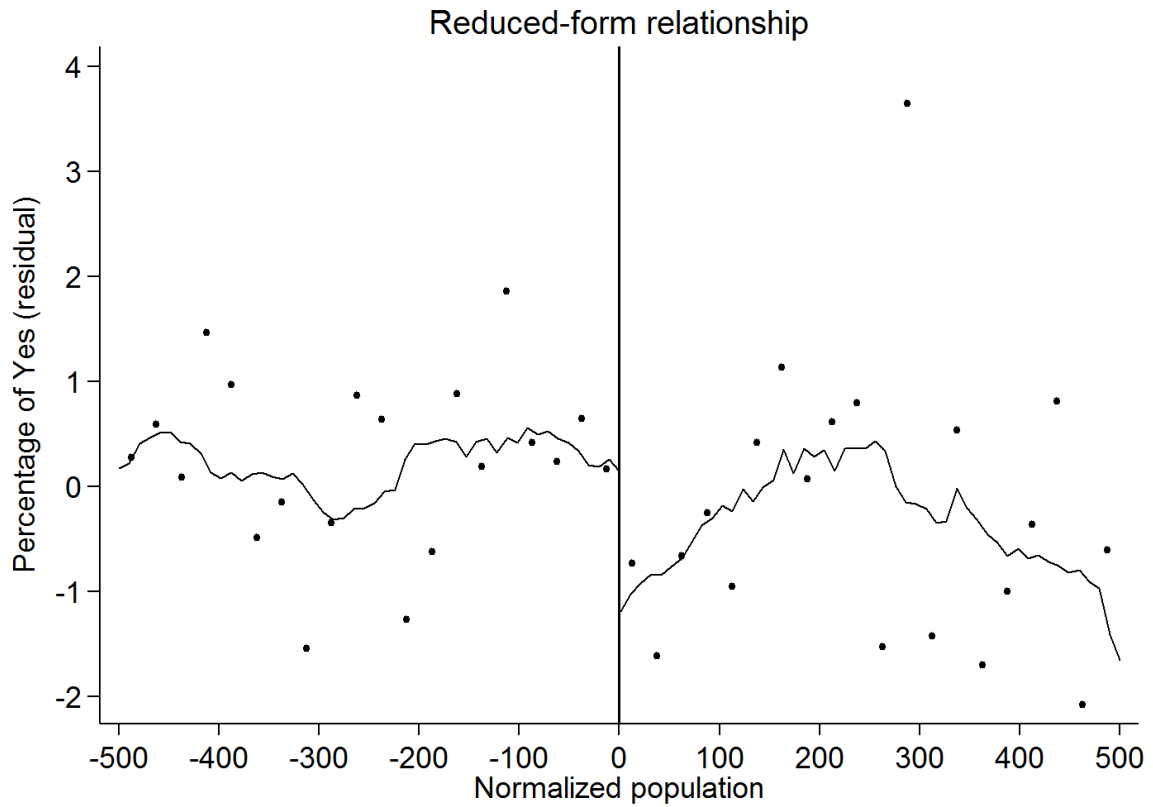


Figure 2: Reduced-form

Notes: This figure displays the reduced-form relationship. The points are the residuals from a regression of the *percentage yes-votes* on the selected control variables and referendum fixed effects (averaged over 25-inhabitant intervals). The solid line is the predicted values of a local linear smoother using a uniform kernel with a bandwidth of 100.

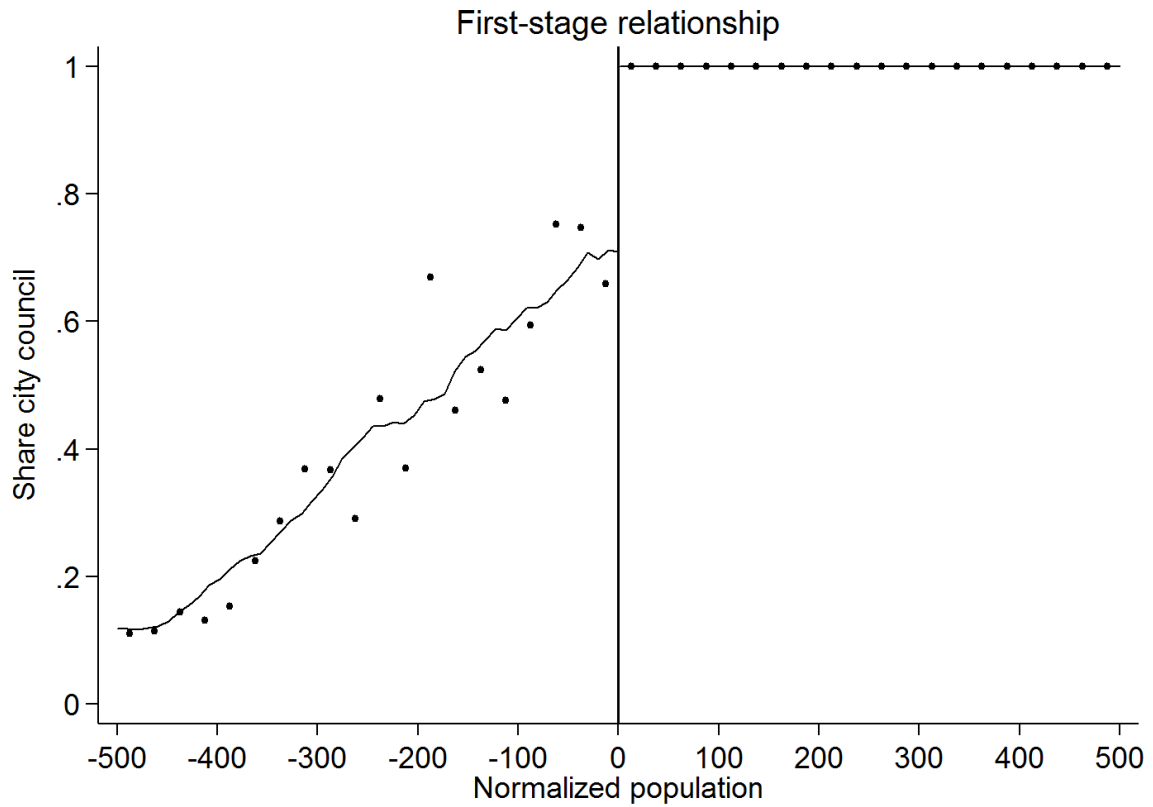


Figure 3: First-stage

Notes: This figure displays the first-stage relationship. The points are the share of municipalities with *representative democracy* (averaged over 25-inhabitant intervals). The solid line is the predicted values of a local linear smoother using a uniform kernel with a bandwidth of 100.

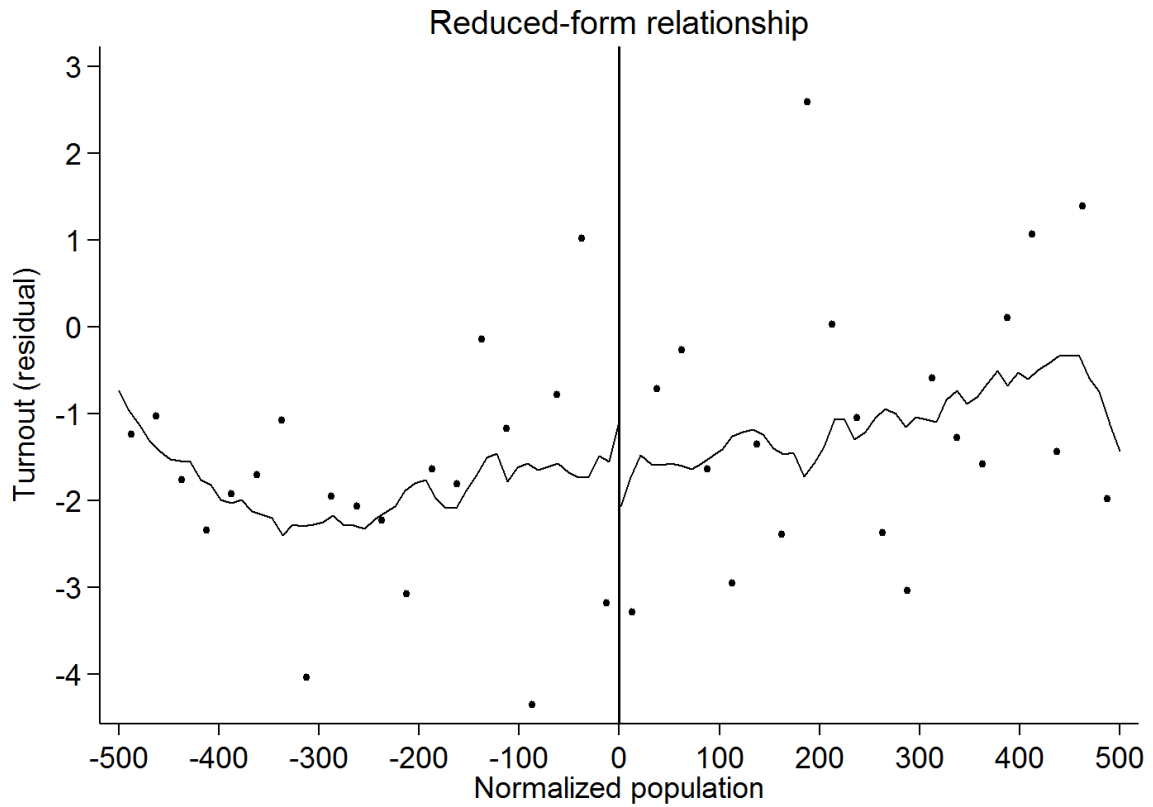


Figure 4: Reduced-form

Notes: This figure displays the reduced-form relationship. The points are the residuals from a regression of the *turnout* on the selected control variables and referendum fixed effects (averaged over 25-inhabitant intervals). The solid line is the predicted values of a local linear smoother using a uniform kernel with a bandwidth of 100.

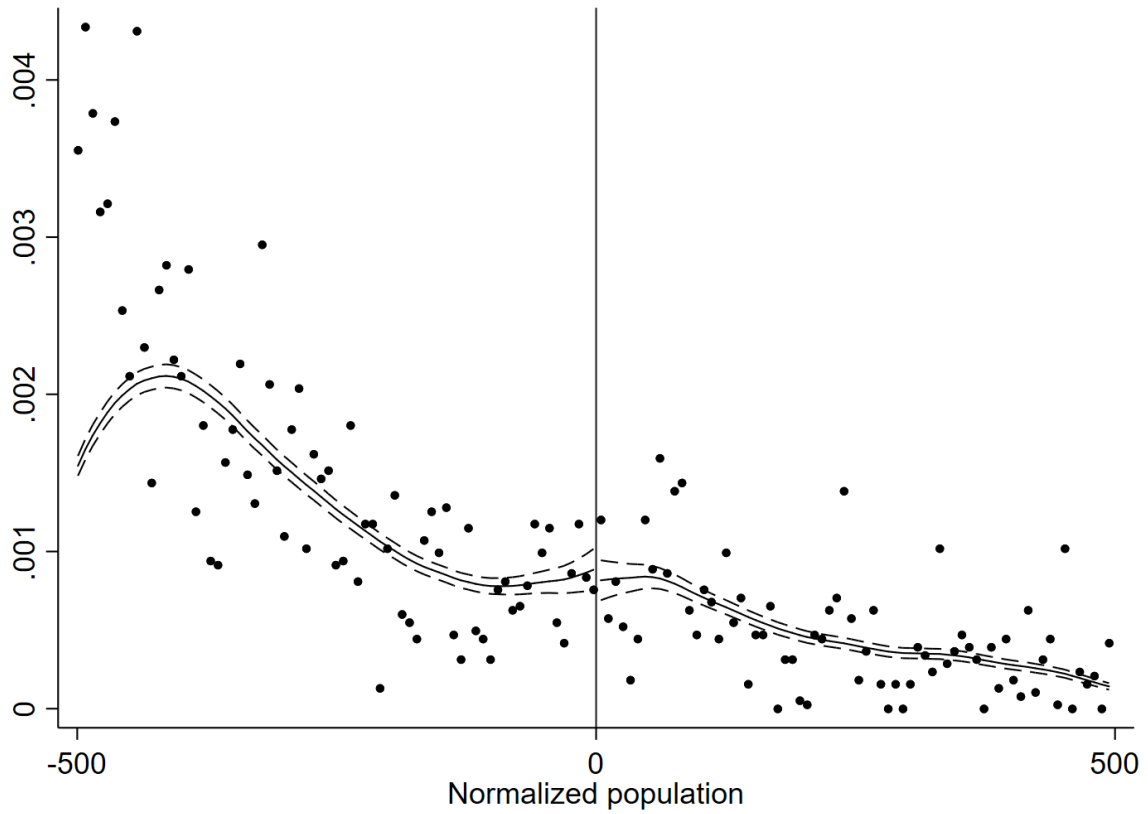


Figure 5: McCrary's (2008) test

Notes: This figure displays the graphical results of the McCrary's (2008) test. The point estimate of the discontinuity is -0.101 (s.e.=0.116).

Table 1: Institutional setting, by electoral term (380 municipalities)

Term	Legislative branch		
	Town meeting	City council (pop. < 800)	Switcher (pop. < 800)
1966-1969	272	108 (31)	0 (0)
1970-1973	266	114 (37)	6 (5)
1974-1977	262	118 (37)	4 (1)
1978-1981	261	119 (35)	1 (0)
1982-1985	256	124 (34)	5 (1)
1986-1989	249	131 (33)	7 (7)
1990-1993	240	140 (34)	9 (4)
1994-1997	238	142 (32)	2 (1)
1998-2001	231	149 (29)	7 (2)

Notes: This table reports the number of municipalities for each institutional setting as well as the number of municipalities introducing a city council, by electoral term. In parenthesis are reported the number of municipalities with less than 800 inhabitants that have a city council.

Table 2: Voting on public spending - Funk and Gathmann 2013

Term	Title of Proposition	Year	Percentage of Yes (mean)	Turnout (mean)	National Outcome
<i>Term 1970-1973</i>					
	Subsidies for Domestic Sugar Industry	1970	84%	50%	Yes
	Housing Guarantee and Protection of Families	1970	51%	29%	No
	Subsidies for Scientific Research	1973	68%	19%	Yes
<i>Term 1974-1977</i>					
	Loan to International Development Agency	1976	36%	33%	No
<i>Term 1978-1981</i>					
	Decrease Retirement Age	1978	15%	44%	No
	Subsidies for Universities/Technical Colleges	1978	38%	44%	No
	Milk Production	1978	55%	42%	Yes
	Federal Responsibility for Security	1978	19%	43%	No
	Subsidize Hiking Trails	1979	43%	44%	Yes
	For a New Immigration Policy	1981	21%	39%	No
<i>Term 1982-1985</i>					
	Energy Article	1983	55%	23%	No
	Protection Motherhood	1984	17%	36%	No
<i>Term 1986-1989</i>					
	Secure Vocational Training and Retraining	1986	16%	35%	No
	Protection of Renters	1986	50%	31%	Yes
	Railway 2000	1987	50%	44%	Yes
	Protection Motor	1987	19%	44%	Yes
	Reform Health Insurance	1987	50%	44%	No
	Decrease Retirement Age	1988	31%	36%	No
<i>Term 1990-1993</i>					
	Vine Cultivation	1990	72%	39%	No
	Energy Article	1990	63%	33%	Yes
	Traffic Law	1990	73%	33%	Yes
	Promoting Public Transport	1991	23%	27%	No
	Financing of Health Insurance	1992	23%	37%	No
	Protection of Waters	1992	47%	34%	Yes
	Saving the Waters	1992	67%	33%	No
	Building Railway through the Alps	1992	67%	41%	Yes
	Raise Salary of Parliamentary Members	1992	27%	40%	No
	Improve Infrastructure for Parliamentary Members	1992	58%	38%	No
<i>Term 1994-1997</i>					
	Promote Culture	1994	50%	37%	No
	Health Insurance	1994	61%	40%	Yes
	For a New Health Insurance	1994	24%	39%	No
	Securing Invalidity/Age Insurance	1995	25%	38%	No
	For an Environmentally Oriented Agriculture	1996	52%	34%	Yes
	Re-Organisation Administration	1996	49%	34%	No
<i>Term 1998-2001</i>					
	Reform of Age Insurance	1998	44%	56%	No
	Infrastructure for Public Transportation	1998	55%	36%	Yes
	Law on Insurance of Motherhood	1999	60%	40%	No
	For a Flexible Age Insurance	2000	50%	38%	No
	For a Flexible Retirement Age	2000	58%	38%	No

Notes: This table reports the results of 39 federal referendums and initiatives that would have increase the size of government spending if approved, as selected by Funk and Gathmann (2013).

Table 3: Reduced-form (dep. variable: *percentage yes-votes*)

	p=1, h_{cct}	p=1, $2h_{cct}$	p=2, h_{cct}	p=2, $2h_{cct}$
	without covariates			
Reduced-form Effect	-1.128 (0.771)	-1.382** (0.643)	-1.649** (0.894)	-0.962 (0.735)
bandwidth	130	260	146	292
n. obs.	[534, 583]	[1185, 898]	[592, 631]	[1462, 943]
n. municipalities	[70, 57]	[123, 66]	[77, 59]	[142, 67]
	with covariates			
Reduced-form Effect	-1.399*** (0.507)	-1.471*** (0.452)	-1.102*** (0.621)	-1.541*** (0.537)
bandwidth	116	233	184	368
n. obs.	[472, 533]	[1012, 805]	[778, 710]	[2159, 1095]
n. municipalities	[72, 57]	[127, 67]	[99, 62]	[171, 67]

Notes: The dependent variable is *percentage yes-votes*. The coefficients are constructed using local polynomial estimators with triangular kernel. All estimates include referendum fixed effects. The covariates are: *share of active population*, *share of active population in agriculture*, *share of active population in services*, *share of foreigners*, *share of protestants*, *surface*. Standard errors clustered at the municipality level in parenthesis * p < 0.1, ** p < 0.05 and *** p < 0.01.

Table 4: First-stage (dep. variable: *representative democracy*)

	p=1, h_{cct}	p=1, $2h_{cct}$	p=2, h_{cct}	p=2, $2h_{cct}$
	without covariates			
First-stage Effect	0.265*** (0.038)	0.269*** (0.033)	0.318*** (0.033)	0.265*** (0.041)
bandwidth	129	259	146	292
n. obs.	[534,583]	[1185,898]	[592, 631]	[1462, 943]
n. municipalities	[70, 57]	[123, 66]	[77, 59]	[142, 67]
	with covariates			
First-stage Effect	0.266*** (0.038)	0.256*** (0.033)	0.270*** (0.040)	0.249*** (0.040)
bandwidth	116	233	184	368
n. obs.	[472, 533]	[1012, 805]	[778, 710]	[2159, 1095]
n. municipalities	[72, 57]	[127, 67]	[99, 62]	[171, 67]

Notes: The dependent variable is *representative democracy*. The coefficients are constructed using local polynomial estimators with triangular kernel. All estimates include referendum fixed effects. The covariates are: *share of active population*, *share of active population in agriculture*, *share of active population in services*, *share of foreigners*, *share of protestants*, *surface*. Standard errors clustered at the municipality level in parenthesis * p < 0.1, ** p < 0.05 and *** p < 0.01.

Table 5: IV estimates (dep. variable: *percentage yes-votes*)

	p=1, h_{cct}	p=1, $2h_{cct}$	p=2, h_{cct}	p=2, $2h_{cct}$
without covariates				
Treatment Effect	-4.247 (2.867)	-5.123** (2.288)	-5.178** (2.801)	-3.618 (2.690)
bandwidth	130	260	146	292
n. obs.	[534, 583]	[1185, 898]	[592, 631]	[1462, 943]
n. municipalities	[70, 57]	[123, 66]	[77, 59]	[142, 67]
with covariates				
Treatment Effect	-5.256** (1.934)	-5.742*** (1.828)	-4.072* (2.524)	-6.179*** (2.173)
bandwidth	116	233	184	368
n. obs.	[472, 533]	[1012, 805]	[778, 710]	[2159, 1095]
n. municipalities	[72, 57]	[127, 67]	[99, 62]	[171,67]

Notes: The dependent variable is *percentage yes-votes*. The coefficients are constructed using local polynomial estimators with triangular kernel. All estimates include referendum fixed effects. The covariates are: *share of active population*, *share of active population in agriculture*, *share of active population in services*, *share of foreigners*, *share of protestants*, *surface*. Standard errors clustered at the municipality level in parenthesis * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$.

Table 6: IV estimates (dep. variable: *turnout*)

	p=1, h_{cct}	p=1, $2h_{cct}$	p=2, h_{cct}	p=2, $2h_{cct}$
without covariates				
Treatment Effect	0.452 (3.652)	-1.369 (2.796)	-2.098 (4.559)	1.798 (3.879)
bandwidth	146	293	114	229
n. obs.	[592, 631]	[1462, 943]	[466, 528]	[1007, 794]
n. municipalities	[82, 60]	[171, 67]	[67, 57]	[114, 66]
with covariates				
Treatment Effect	0.250 (3.427)	-1.745 (2.856)	1.265 (3.747)	-0.943 (3.230)
bandwidth	150	300	205	410
n. obs.	[623, 637]	[1530, 943]	[888, 725]	[2585, 1139]
n. municipalities	[85, 60]	[171, 67]	[101, 62]	[171, 67]

Notes: The dependent variable is *turnout*. The coefficients are constructed using local polynomial estimators with triangular kernel. All estimates include referendum fixed effects. The covariates are: *share of active population*, *share of active population in agriculture*, *share of active population in services*, *share of foreigners*, *share of protestants*, *surface*. Standard errors clustered at the municipality level in parenthesis * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$.

Table 7: Test of balance covariates

<i>Dependent variable</i>	p=1, h_{cct}	p=1, $2h_{cct}$	p=2, h_{cct}	p=2, $2h_{cct}$
Active population 1970	-0.005 (0.014)	0.003 (0.011)	-0.003 (0.017)	-0.005 (0.014)
Active female population 1970	0.001 (0.007)	0.006 (0.007)	0.005 (0.008)	0.001 (0.008)
Active self-employed population 1970	0.012 (0.014)	0.001 (0.011)	0.023 (0.017)	0.005 (0.013)
Active population in agriculture 1970	0.007 (0.024)	0.000 (0.019)	0.020 (0.029)	0.001 (0.023)
Active population in industry 1970	-0.022 (0.033)	0.002 (0.025)	-0.027 (0.035)	0.004 (0.027)
Active population in services 1970	-0.000 (0.016)	-0.009 (0.015)	0.007 (0.021)	-0.001 (0.017)
Adult population 1970	-0.010 (0.014)	-0.006 (0.012)	-0.014 (0.015)	-0.007 (0.013)
Old population 1970	0.009 (0.010)	0.010 (0.009)	0.010 (0.012)	0.009 (0.010)
Male population 1970	-0.001 (0.009)	-0.000 (0.007)	-0.001 (0.011)	-0.001 (0.009)
Foreign population 1970	-0.011 (0.032)	-0.016 (0.026)	-0.020 (0.038)	-0.011 (0.030)
German speaking population 1970	0.006 (0.011)	0.003 (0.009)	0.010 (0.013)	0.006 (0.010)
Italian speaking population 1970	0.001 (0.019)	0.001 (0.015)	-0.008 (0.028)	0.002 (0.022)
Protestant population 1970	0.017 (0.040)	0.006 (0.037)	0.059 (0.049)	0.016 (0.041)
Married population 1970	-0.013 (0.013)	-0.005 (0.011)	-0.015 (0.014)	-0.006 (0.012)
Federal tax on income 1973	-1.036 (49.953)	-21.520 (45.463)	-46.941 (70.014)	-0.967 (49.351)
Left wing 1971	-2.584 (3.483)	0.637 (2.680)	-3.693 (3.807)	0.698 (2.807)
Surface	54.696 (358.856)	191.570 (290.857)	1272.668** (572.958)	257.054 (421.752)
Altitude	11.002 (62.352)	51.620 (55.540)	84.420 (80.194)	9.857 (69.677)

Notes: Each coefficient is constructed using a local polynomial estimator with triangular kernel where the dependent variable is indicated in the first column on the left. Standard errors clustered at the municipality level in parenthesis. * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$.

Table 8: Bandwidth checks

Share of h_{cct}	coeff	st.err	Num. observations		Num. municipalities		bandwidth
			left	right	left	right	
0.50	-0.669	2.709	270	235	51	46	58
0.55	-1.780	2.525	298	290	51	48	63
0.60	-2.250	2.256	327	320	53	50	69
0.65	-2.587	2.115	338	345	56	50	75
0.70	-3.020	2.047	364	382	60	51	81
0.75	-3.609*	2.001	397	431	62	53	87
0.80	-4.244**	2.016	407	453	62	54	92
0.85	-4.581**	1.963	436	467	66	56	98
0.90	-4.814**	1.936	448	487	66	56	104
0.95	-5.018**	1.931	464	511	67	57	110
1	-5.231**	1.932	472	533	72	57	116
1.05	-5.289**	1.919	496	545	74	58	121
1.10	-5.161**	1.941	528	577	78	59	127
1.15	-5.080**	1.976	540	604	79	59	133
1.20	-5.011**	1.999	553	631	80	60	139
1.25	-5.046**	2.042	592	631	83	60	145
1.30	-5.123**	2.058	623	637	88	61	150
1.35	-5.108**	2.047	651	655	91	62	156
1.40	-5.125**	2.030	693	667	94	62	162
1.45	-5.163**	2.022	723	697	99	62	168
1.50	-5.243**	2.018	751	698	100	62	174

Notes: The dependent variable is *percentage yes-votes*. The coefficients are constructed using local linear estimators with triangular kernel. The first column on the left indicates the size of the bandwidth in comparison to the optimal one. Standard errors clustered at the municipality level in parenthesis. * $p < 0.1$, ** $p < 0.05$ and *** $p < 0.01$.

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Decision-making Institutions and Voters' Preferences for Fiscal Policies

ONLINE APPENDIX

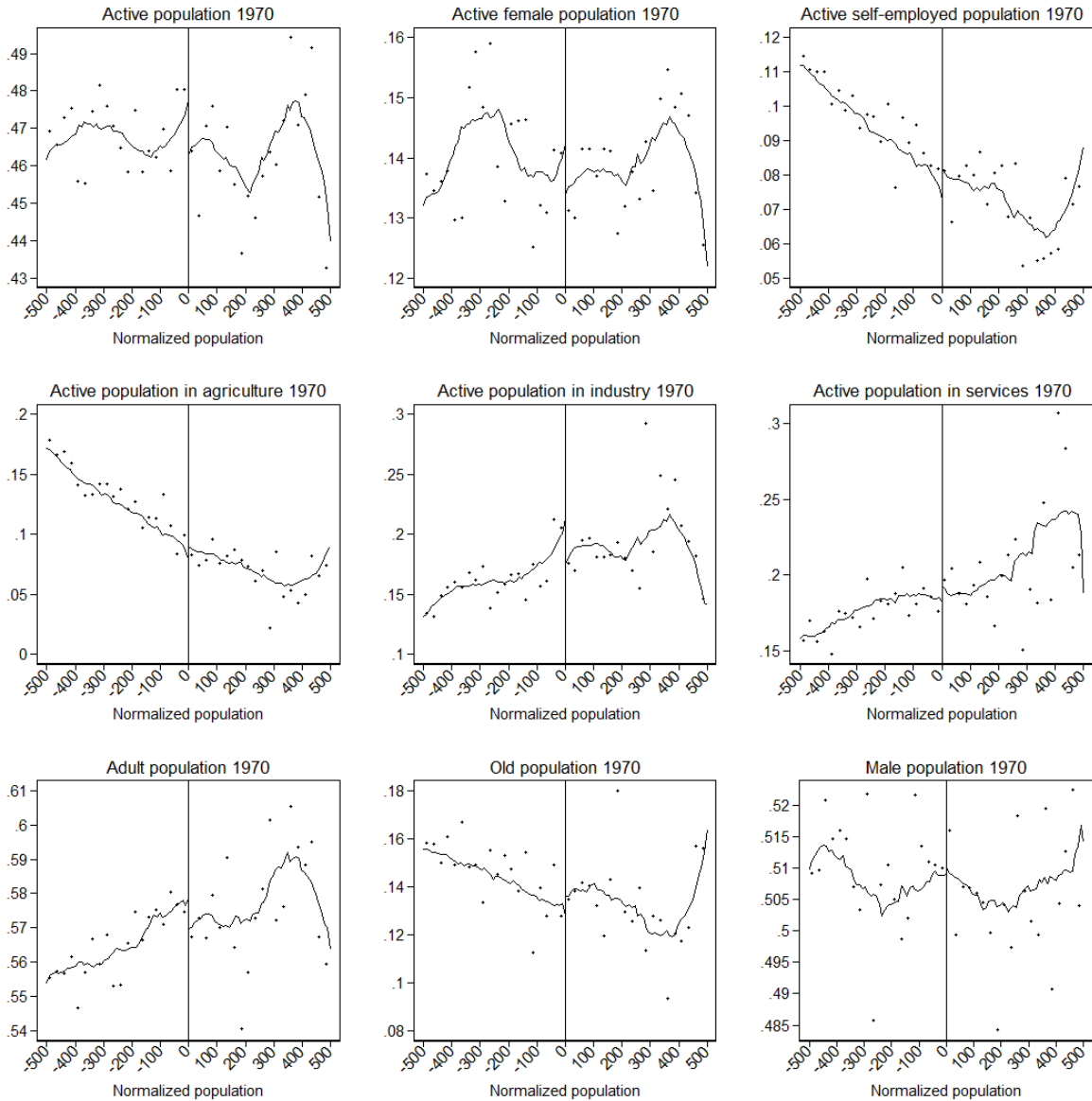


Figure OA.1: Balance of covariates

Notes: This figure displays graphical evidence of covariates balance. The points are the value of the variable indicated in the title of each sub-panel (averaged over 25-inhabitant intervals). The solid line is the predicted values of a local linear smoother using a uniform kernel with a bandwidth of 100.

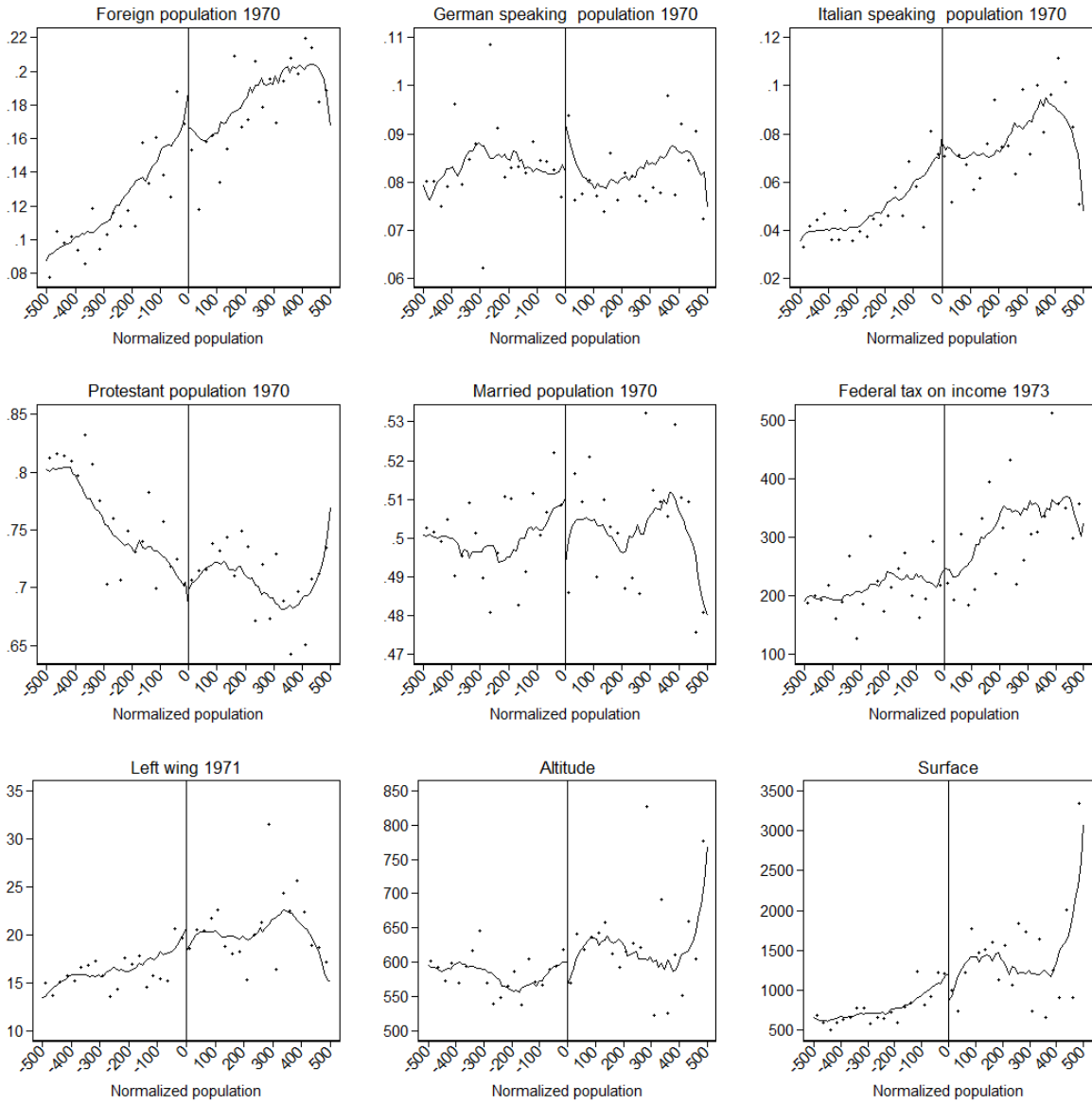


Figure OA.2: Balance of covariates

Notes: This figure displays graphical evidence of covariates balance. The points are the value of the variable indicated in the title of each sub-panel (averaged over 25-inhabitant intervals). The solid line is the predicted values of a local linear smoother using a uniform kernel with a bandwidth of 100.

Table OA.1: Summary statistics

Variable	Mean	Std. Dev.	Min	Max	N
Representative democracy	0.464	0.499	0	1	5393
Percentage yes-votes	46.815	19.869	4.124	100	5393
Turnout	35.162	9.279	5.822	78.843	5393
Population	596.602	260.574	301	1291	5393
Active population 1970	0.468	0.047	0.358	0.603	5393
Active female population 1970	0.14	0.04	0.063	0.439	5393
Active self-employed population 1970	0.095	0.033	0	0.171	5393
Active population in agriculture 1970	0.127	0.071	0	0.335	5393
Active population in industry 1970	0.163	0.064	0.023	0.383	5393
Active population in services 1970	0.177	0.067	0	0.626	5393
Adult population 1970	0.564	0.041	0.461	0.673	5393
Old population 1970	0.146	0.044	0.017	0.292	5393
Male population 1970	0.509	0.035	0.246	0.616	5393
Foreign population 1970	0.123	0.087	0	0.424	5393
German speaking population 1970	0.082	0.046	0.014	0.415	5393
Italian speaking population 1970	0.051	0.045	0	0.227	5393
Protestant population 1970	0.762	0.152	0.009	0.969	5393
Married population 1970	0.501	0.05	0.237	0.597	5393
Federal tax on income 1973	225.477	250.047	29.01	1560.95	5393
Left wing 1971	16.786	8.65	0.8	52.8	5393
Altitude	595.526	178.589	107	1137	5393
Surface	850.085	1015.403	32	6409	5393

Notes: This table reports summary statistics of all collected variables for municipalities with a population between 300 and 1300 (i.e., ± 500 inhabitants around threshold). The level of observation is municipality-federal proposition.

B. Online Appendix - Time variation

The RD mainly exploits the cross-sectional variation and does not account for the time each municipality has spent under a certain institutional framework. However, there is evidence that the time of exposure to political institutions is a relevant factor in explaining preferences (Fuchs-Schündeln and Schündeln, 2015). Therefore, for completeness, I exploit the time variation in the data to provide some insights on this issue.

I estimate the following model:

$$Y_{kit} = \alpha + \gamma_k + \lambda_i + \sum_{j=-7}^8 \beta_j RD_{it}^j + \epsilon_{kit} \quad (1)$$

where, in addition to the terms previously defined, λ_i are municipality fixed effects. RD_{it}^j is a set of dummies identifying whether a municipality has a city council j periods away from t . Therefore, as not all switcher municipalities introduced a city council in the same electoral term, the dummies could refer to different periods of time.

To rely on weaker identification assumptions, and to reduce potential selection bias, I use only the sub-sample of 41 municipalities that introduced a city council at some point in time during the period of analysis. Therefore, β_j , the coefficient of interest, mainly identifies the difference in the outcome of the votes between municipalities that still have a town meeting and those that left it since/from j term(s). The identifying assumption requires that, conditional on controls, the time of the reform is good as random. That is, one should rule out the possibility that preferences were statistically significantly different in periods preceding the institutional reform.

Figure OB.1 shows graphically the results from the estimates of equation (1).¹ The introduction of a city council reduces citizens' support for public spending, and this effect seems to slowly grow over time. The gap is maximum after the sixth term (i.e., 16 to 20 years) with a city council (coeff= -2.33 p.p.). However, with the exception of the dummy identifying the fourth term after the introduction of a city council, all coefficients do not reach the conventional level of significance. Overall, this test provides evidence of a link between cumulative experience with institutions and preferences. Nevertheless,

¹In addition to the fixed effects, I include municipal population size as a time-varying control, given its relevance in determining the treatment status. Yet, its inclusion has a marginal effect on the results.

the robustness of these results have some limitation given the small sample size.

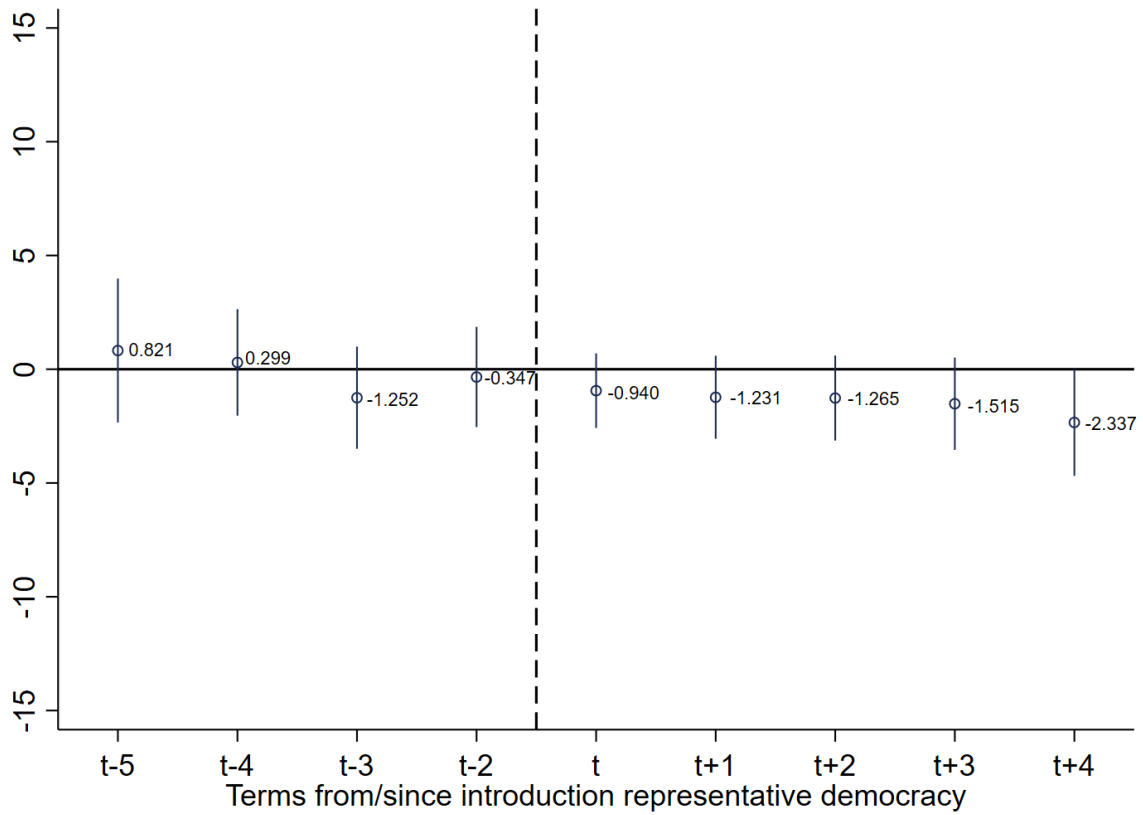


Figure OB.1: Event study

Notes: This figure presents the results from the event-study model. The dependent variable is *percentage yes-votes*. The term before the introduction of a city council (t-1) is the reference category. The coefficient estimates of the other terms are depicted here, with a 95% confidence intervals. Standard errors clustered at the municipality level.