Does Voters' Veto Power Curb Regulatory Activity?

Simon Luechinger* and Mark Schelker†

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Abstract

We investigate how an institution granting voters veto power over legislation affects regulatory activity. In a difference-in-differences design with unique panel data from the Swiss cantons in 1908-2020, we estimate the impact of the mandatory legislative referendum on the number of changes to statutory enactments. We find that its abolishment raises regulatory activity by about 50 percent. The increase is especially pronounced in legal areas with substantial scope for favoring narrow interests. Our results suggest that voters' veto power can limit special interest regulation.

Keywords: regulation; institutions
JEL codes: H11; D7; K20

* Simon Luechinger, University of Lucerne, Frohburgstrasse 3, 6002 Lucerne, simon.luechinger@unilu.ch; † Mark Schelker, University of Fribourg, Bd. de Pérolles 90, 1700 Fribourg, mark.schelker@unifr.ch.
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1. Introduction

There are large discrepancies in the regulatory activity across jurisdictions. For example, the number of laws adopted by national parliaments in 2020, ranged from a low of 32 in Ireland, 41 in Switzerland, and 54 in Spain, to an intermediate level of 156 in Germany and Norway and 239 in the United States, to a high of 469 in Sweden and 690 in Finland.[1]

What is the normative assessment of such regulatory activity? There are two polar positions (for example, Djankov et al. 2002; Botero et al. 2004): According to one position, regulation corrects market failures and increases efficiency (Stiglitz 2009)[2]. According to the other position, regulation often serves the interests of narrow groups, leads to misallocation, and reduces efficiency (Stigler 1971)[3]. Such regulation can only prevail due to the "nature of the political process in a democracy", in which "voters must employ representatives", while it would not pass a "direct and informed vote" (Stigler 1971, 10). For example, in the parliamentary process, legislators can pursue their interests and those of their supporters by forming coalitions, and typically must not fear a voter veto at the ballot box.

Our setting allows us to pit the two polar positions against each other: We can directly test if voters' veto power reduces regulatory activity. We analyze how the abolishment of mandatory legislative referendums – Stigler's "direct and informed vote" – affects regulatory activity using panel data from the Swiss cantons from 1908 to 2020. The mandatory legislative referendum gives voters veto power by requiring that any change to the body of legal statutes passes a binding popular vote. These changes include new enactments, amendments, and repeals. Mandatory legislative referendums prevent legislation without a chance of gaining a majority among voters (Feld and Matsusaka 2003). Direct democratic instruments more generally have been shown to curb special interest influence (Matsusaka 2009 on public employment; Matsusaka 2023 on stock market reactions to ballot decisions).

We capture regulatory activity by the number of changes to legal statutes enacted by cantonal parliaments. To estimate the causal effect of mandatory legislative referendums on regulatory activity, we employ a difference-in-differences design. We show that the abolishment of mandatory legislative referendums strongly increases regulatory activity. Exploratory evidence

[1] All figures except for Switzerland are from the Inter-Parliamentary Union, https://data.ipu.org/, accessed August 19, 2022; the Swiss figure is based on the authors' data (see Section 2).
[2] The general position goes at least as far back as Pigou (1932). However, Stiglitz (2009) makes a most forceful argument regarding regulation.
[3] For an important early extension of Stigler's (1971) contribution, see Peltzman (1976).

suggests that this increase is concentrated in topic areas with substantial scope for special interest regulation and little opportunity for correcting market failures.

The findings are consistent with a Stiglerian view of regulation. An alternative view holds that the mandatory legislative referendum simply increases the fixed costs of legislating. In this case, we would expect politicians to change laws less frequently, but make more extensive changes when they do. We do not find more extensive changes if the mandatory legislative referendum is in place. A simple fixed-cost argument would also imply that the abolishment of the mandatory legislative referendum increases regulatory activity across all topic areas. This is not what we find in our exploratory analysis: We find no increases in topic areas without substantial scope of special interest regulation.

We contribute to the literature on the assessment of regulation. While there are vast bodies of literature related to the two polar positions represented by Stigler (1971) and Stiglitz (2009), only very few papers empirically contrast these positions. Prominent examples are Djankov et al. (2002) and Botero et al. (2004), who examine regulation in the specific areas of market entry and labor in a cross-country setting. They test the competing positions by investigating in what contexts such regulations are more pervasive and by studying their consequences. Another example is Peltzman (2022), who investigates public attitudes toward regulation. These papers provide evidence generally more in line with Stigler's (1971) position.

We also add to the literature on regulatory quantity and activity. Some papers estimate the growth effects of regulatory activity. Most of these papers find adverse growth effects (Dawson and Seater 2013; Coffey, McLaughlin, and Peretto 2020), with a recent exception of Ash, Morelli, and Vannoni (2025). Closer to our paper, others investigate the determinants of regulatory quantity and activity. Previous research documents positive effects of population size (Mulligan and Shleifer 2005), the length of legislative terms (Dal Bó and Rossi 2011), political instability (Gratton et al. 2021), and British Columbia's one-in-two-out rule (Coffey and McLaughlin 2021).

2. Empirical strategy and data

We analyze the effect of the mandatory legislative referendum in two steps. First, we illustrate the development of regulatory activity around the abolishment of the mandatory referendum in a before-and-after comparison, in a standard event study, and in event-type analyses accounting for effect heterogeneity (de Chaisemartin and D'Haultfœuille 2020; Wooldridge 2023, 2025). This allows us to assess the effect of the mandatory legislative referendum, effect dynamics,

and pre-trends. Second, we estimate the average effect of mandatory legislative referendums in a difference-in-differences setup. The event-type estimates from the first step provide the necessary evidence to evaluate the parallel trend and the no-anticipation assumptions.

We implement this analysis using regressions that include canton and year effects for an unbalanced panel of 26 cantons spanning 1908-2020. Given this long sample period, time-varying covariates and differential time trends may play an important role. For this reason, we implement a sequence of increasingly rich specifications: (1) We start with a specification only including canton and year fixed effects. (2) We augment this previous specification with time-varying institutional covariates, namely dummy variables for second-reading requirements, mandatory fiscal referendums, citizens' assemblies, and female suffrage, (3) other control variables such as the log of the cantonal population, the shares of people aged 20 to 39, 40 to 64, and 65 and above, the share of foreigners, the shares of Catholics and Protestants, and the shares of German and French speakers. To account for canton-specific time trends, we include (4) canton-specific linear and (5) quadratic time trends. These specifications are variants of Equation (1):

$$R_{it} = \beta A_{it} + \boldsymbol{X}_{it}\boldsymbol{\gamma} + \boldsymbol{\chi}_i + \boldsymbol{\theta}_t + \boldsymbol{\lambda}_i f(t) + \varepsilon_{it}, \tag{1}$$

where R_{it} denotes regulatory activity in canton i and year t, A_{it} the abolishment of the mandatory legislative referendum, $\boldsymbol{X}_{it}$ institutional and other controls, $\boldsymbol{\chi}_i$ canton effects, $\boldsymbol{\theta}_t$ year effects, $\boldsymbol{\lambda}_i f(t)$ canton-specific linear and quadratic trends, and ε_{it} the error term. We focus on the impact of the abolishment of the mandatory legislative referendum β. We allow standard errors to cluster at the level of cantons.

Our measure of regulatory activity captures all changes to legal statutes, that is new enactments, amendments, and repeals (see Luechinger and Schelker 2016 for a detailed description of the regulation data).[4] We manually collected the relevant information from regularly published cantonal law collections and official journals. For most cantons, we have consistent data for the years 1908 to 2020. Gaps exist for the cantons of Appenzell Ausserrhoden 1908 to 1914, Glarus 1908 to 1923, and Appenzell Innerrhoden 1908 to 1973.

[4] Based on an earlier and very preliminary version of our data, our research assistant Christian Marti explored the effect of facultative legislative referendums on electoral cycles in legislative activity without reporting any direct effects of referendums (Marti 2016).

In our main analysis, we use the log of the number of changed enactments.[5] It is possible to capture changes at different levels within the hierarchy of legal norms. It ranges from the constitution to legal statutes to legislative and executive decrees. A legal expert assigned the enactments to the different categories. Mandatory legislative referendums only apply to legal statutes. Hence, our main analysis focuses on this level. In an analysis of potential substitution effects, we also consider changes to the number of pages per enactment and changes to other enactments.

There is substantial institutional variation across cantons and over time. Over our sample period, the number of cantons with a mandatory referendum decreased from 16 to three. During our sample period, we only observe abolishments but no introductions. Therefore, our treatment is the abolishment of the mandatory legislative referendum. We code a dummy variable with value one starting with the year when the abolishment becomes effective (and for all cantons that never had the mandatory legislative referendum) and zero otherwise. Abolishments can happen at any point during the year. For the coding, we rely on current and historical cantonal constitutions. In a few cantons, the parliament can bypass the mandatory legislative referendum with a qualified majority. In these cases, we code the cantons as not having a mandatory legislative referendum. Some small cantons hold citizens' assemblies. In these cases, we code the cantons as having a mandatory legislative referendum.

To understand why the 13 cantons abolished the mandatory legislative referendum, we consulted the official leaflets for the corresponding popular votes. The dominant argument that featured prominently in all votes was the aim to avoid unnecessary votes on undisputed statutory changes.[6] This should save time and costs and allow voters to focus on disputed changes that can still be challenged through a facultative legislative referendum.

In our analysis, we control for four institutional characteristics: second reading requirements, mandatory fiscal referendums, citizens' assemblies, and female suffrage. Second reading requirements come in various forms. As we are interested in institutional arrangements that necessitate stable majorities to pass legislation, our coding requires that parliaments hold at

[5] We use the log of the number of changes + 1 because there are some cantons and years without any changes. Poisson regressions in Table A2 of the Appendix yield very similar results. Figure A1 of the Appendix presents the results of a Poisson event-type analysis based on Wooldridge (2023), corroborating our results.

[6] Variants of this argument from two cantons are: "Unnecessary votes on uncontroversial legislative proposals should, however, be avoided" (Grosser Rat des Kantons Bern 1972, 5; own translation). "The voters should be called to the polls only for contentious legislative proposals. […] The flood of votes will be curbed, and the number of ballots can be reduced" (Staatskanzlei des Kantons Solothurn 1998, 2; own translation with emphasis removed).

least two rounds of debates and vote on separate days. If there is any possibility of overruling these requirements, we do not code this canton as having a second reading requirement. We observe both introductions and abolishments. Again, we code the year in which the change becomes effective. We base our coding on cantonal constitutions as well as statutes and decrees on parliamentary rules and procedures – both current and historical.

Mandatory fiscal referendums constitute an additional instrument of direct democracy. They typically trigger a vote whenever a political decision entails expenditures above a certain threshold. In coding this variable, we proceed analogously to the coding of the mandatory legislative referendum. Five cantons had citizens' assemblies at the start of the sample period, and two remain at the end. We capture these institutional changes with the corresponding variable. We consult current and historical cantonal constitutions and other sources (for example, Trechsel and Serdült 1999; Funk and Gathmann 2011) to code the variables for mandatory fiscal referendums and citizens' assemblies. For the variable on female suffrage, we rely on the introduction dates reported by the Federal Commission for Women's Issues.

Other control variables refer to population characteristics. The variable on the cantonal population contains estimates of the annual average population as provided by the Federal Statistical Office. We calculate the population shares referring to age, foreigners, religion, and language based on the decennial census until 2000 and the official annual survey from 2010 onwards. We linearly interpolate the data for missing years.[7] The data are from the Historical Statistics of Switzerland for the years until 1990 and later from the Federal Statistical Office.

For the analysis of potential substitution effects, we collect data on cantonal expenditures from the Historical Statistics of Switzerland for the years until 1932, from the Federal Statistical Office (Statistical Yearbooks of Switzerland) for the years 1933-1949, and from the Swiss Federal Finance Administration from 1950 onwards. The data from 2008 onwards are based on new accounting rules. To link the different data series, we extrapolate the 1950-2007 data with annual growth rates from the other series. Table A1 of the Appendix provides summary statistics.

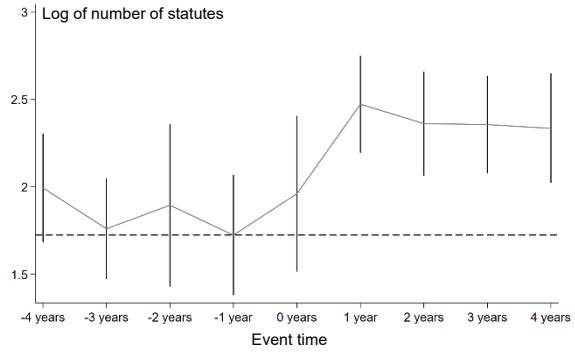
[7] We extrapolate the number of foreigners for the cantons of Bern and Jura in 1979, the year of their separation.

3. Results

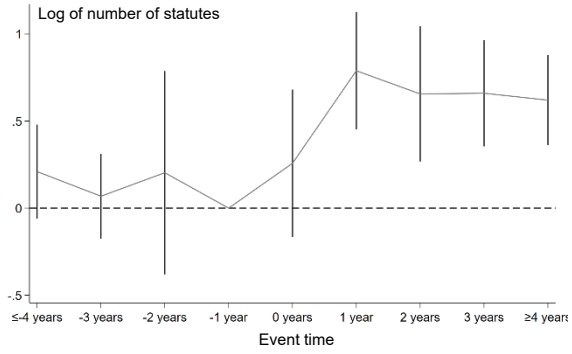
Figure 1 depicts the evolution of regulatory activity around the abolishment of the mandatory legislative referendum. It shows that the mandatory legislative referendum serves as an important regulatory brake: Its abolishment entails a large increase in regulatory activity.

Figure 1. Abolishment of mandatory legislative referendums and regulatory activity

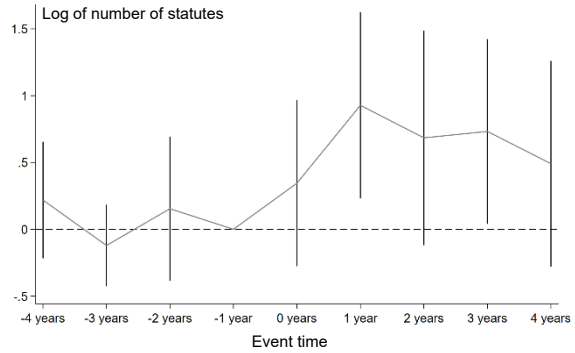
Panel A. Event-type analysis based on stacked raw data



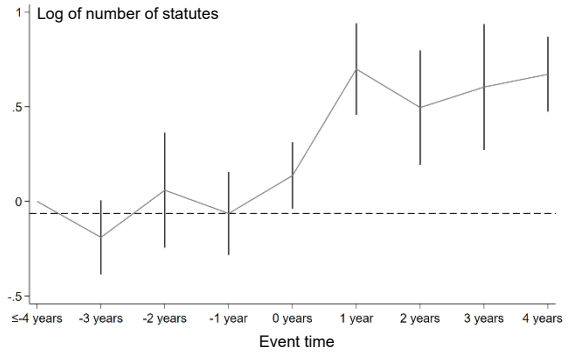
Panel B. Event-type analysis with standard two-way fixed effects approach



Panel C. Event-type analysis based on de Chaisemartin and D'Haultfœuille (2020)



Panel D. Event-type analysis based on Wooldridge (2023, 2025)



Notes: The graphs in the four panels show the effects of the abolishment of mandatory legislative referendums in the years before and after the abolishment. The x-axis depicts the event time relative to the year of the abolishment, the y-axis the means (Panel A) or the coefficients (Panels B to D) measured in the log of the number of statutes (+1) and the corresponding 95-percent confidence intervals. Panel A shows stacked raw data, that is the means of the log of the number of statutes by event time. Panel B reports estimates of a standard event-study regression based on a two-way fixed effects approach with binned endpoints (as suggested by Schmidheiny and Siegloch 2023). Panels C and D depict the results of event-type analyses that account for effect heterogeneity based on estimators of de Chaisemartin and D'Haultfœuille (2020) and Wooldridge (2023, 2025), the latter implemented by Rios-Avila, Nagengast, and Yotov (2024). The regressions in Panels B to D control for time-varying institutional and other covariates as well as canton and time effects (analogous to the specification in Column 3 in Table 1 below), and they allow standard errors to cluster at the level of cantons. The dashed line refers to the year before the abolishment.

The effect materializes in the year after the abolishment. In the year of the abolishment, we typically find no effect. This one-year lag is plausible. First, some of the observed abolishments take place towards the end of the year. Second, the institutional setting affects decisions already at the initiation stage of the legislative process. Because the drafting and decision-making

process takes time, the effect of the abolishment is only reflected in the number of enacted legislations with a lag. We observe a slight decrease and a stabilization of the effect after a peak in the year after the abolishment. We find no evidence for notable pre-trends.

Panel A illustrates these patterns in a before-and-after comparison with means of stacked raw data, Panel B with a standard event study regression, and Panels C and D with event-type analyses accounting for effect heterogeneity. The broken lines serve as visual support to facilitate the comparison to the last pre-treatment year. Depending on the method, the coefficient (in log points) for the first year after the abolishment (relative to the last pre-treatment year) lies between 0.75 (Panel A) and 0.93 (Panel C), the average of coefficients for the three later years between 0.63 (Panel A) and 0.65 (Panels B and D).[8]

Table 1. Abolishment of mandatory legislative referendums and regulatory activity

Dependent variable: Log of number of statutes	(1)	(2)	(3)	(4)	(5)
Mandatory legislative referendum (abolishment)	0.339** (0.078)	0.400** (0.076)	0.411** (0.067)	0.457** (0.070)	0.392** (0.064)
Canton effects	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes
Institutional controls	No	Yes	Yes	Yes	Yes
Other controls	No	No	Yes	Yes	Yes
Linear canton-specific trends	No	No	No	Yes	Yes
Quadratic canton-specific trends	No	No	No	No	Yes
Number of observations	2778	2778	2778	2778	2778

Notes: Institutional controls in Columns (2) to (5) include dummy variables for second reading requirements, mandatory fiscal referendums, citizens' assemblies, and female suffrage. Other controls in Columns (3) to (5) include the log of the population, the shares of people aged 20 to 39, 40 to 64, and over 64, the share of foreigners, the shares of Catholics and Protestants, the shares of German and French speakers. The sample period is 1908-2020. We allow standard errors (in parentheses) to cluster at the level of cantons. Significance levels: $^{+}$ $P < .10$, * $P < .05$, ** $P < .01$. For detailed results including control variables, see Appendix Table A2.

Table 1 reports the average long-term impact of the abolishment of the mandatory legislative referendum.[9] The estimates suggest the abolishment increases regulatory activity at the statutory level by 0.339 log points or 40 percent (Column 1) to 0.457 log points or 58 percent (Column 4) in the long run. The estimates differ little across the increasingly rich specifications.[10] We find similar, but slightly stronger, effects with methods that account for effect heterogeneity. The average effect based on de Chaisemartin and D'Haultfœuille (2020)

[8] See Figure A1 of the Appendix for a Poisson event-type analysis.
[9] Table A2 of the Appendix reproduces the estimates together with the estimates for all covariates.
[10] See Table A3 of the Appendix for a summary of results based on Poisson regressions.

amounts to 0.636 (SE: 0.338), the one based on Wooldridge (2023, 2025) amounts to 0.559 (SE 0.073).

Some abolishments took place in the context of a total revision of the constitution. Concurrent total revisions do not drive our results. Table 2 presents results distinguishing the effect of abolishments without and with concurrent total revision of the constitution. All estimates are positive and statistically significant. Estimates without a concurrent total revision are slightly higher than those with concurrent total revisions. Further, the results are robust to the exclusion of each canton that abolished the mandatory legislative referendum in turn (see Figure A2 of the Appendix).

Table 2. Abolishment of mandatory legislative referendums with and without concurrent total revision of the cantonal constitution

Dependent variable: Log of number of statutes	(1)	(2)	(3)	(4)	(5)
Mandatory legislative referendum (abolishment)					
Without concurrent total revision of cantonal constitution	0.356**	0.467**	0.495**	0.535**	0.463**
	(0.093)	(0.099)	(0.067)	(0.080)	(0.076)
With concurrent total revision of cantonal constitution	0.289**	0.266**	0.244**	0.323**	0.267*
	(0.072)	(0.078)	(0.071)	(0.091)	(0.113)
Canton effects	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes
Institutional controls	No	Yes	Yes	Yes	Yes
Other controls	No	No	Yes	Yes	Yes
Linear canton-specific trends	No	No	No	Yes	Yes
Quadratic canton-specific trends	No	No	No	No	Yes
Number of observations	2778	2778	2778	2778	2778

Notes: Institutional controls in Columns (2) to (5) include dummy variables for second reading requirements, mandatory fiscal referendums, citizens' assemblies, and female suffrage. Other controls in Columns (3) to (5) include the log of the population, the shares of people aged 20 to 39, 40 to 64, and over 64, the share of foreigners, the shares of Catholics and Protestants, the shares of German and French speakers. The sample period is 1908-2020. We allow standard errors (in parentheses) to cluster at the level of cantons. Significance levels: $^{+}$ $P < .10$, * $P < .05$, ** $P < .01$.

Our results are consistent with the notion that without voters' veto power, the scope for special interest regulation expands. Regulatory changes become more frequent as they are no longer restricted to what is widely perceived as desirable by voters. An alternative view could be that the mandatory legislative referendum simply increases the fixed costs of legislating. In the following, we provide additional evidence to discriminate between the two interpretations.

First, we focus not only on the frequency of regulatory changes, but also on their extent. Second, we look at the effects of the abolishment of the mandatory legislative referendum across different legal topics.

Table 3. Potential substitution effects

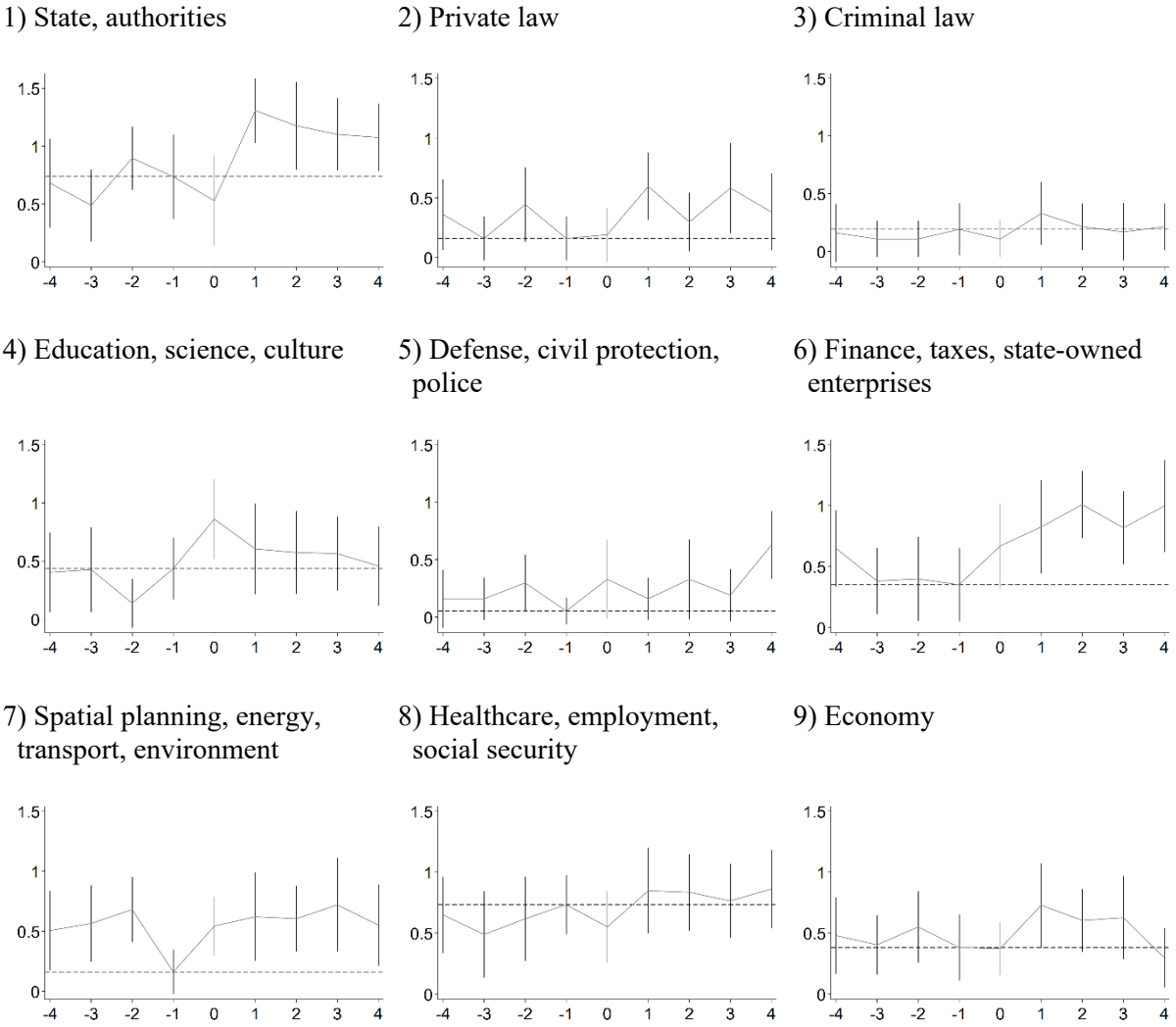
	(1)	(2)	(3)	(4)	(5)
Panel A – Dependent variable: Log of number of pages per statute					
Mandatory legislative referendum	0.109	0.074	0.103	-0.128	-0.083
(abolishment)	(0.151)	(0.145)	(0.134)	(0.142)	(0.152)
Number of observations	2574	2574	2574	2574	2574
Panel B – Dependent variable: Log of number of other enactments					
Mandatory legislative referendum	0.018	-0.033	-0.097	-0.143	-0.002
(abolishment)	(0.167)	(0.185)	(0.152)	(0.107)	(0.082)
Number of observations	2778	2778	2778	2778	2778
Panel C – Dependent variable: Log of cantonal expenditures					
Mandatory legislative referendum	-0.005	-0.126	-0.074	0.015	0.158**
(abolishment)	(0.112)	(0.107)	(0.057)	(0.048)	(0.041)
Number of observations	2867	2867	2867	2867	2867
Canton effects	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes
Institutional controls	No	Yes	Yes	Yes	Yes
Other controls	No	No	Yes	Yes	Yes
Linear canton-specific trends	No	No	No	Yes	Yes
Quadratic canton-specific trends	No	No	No	No	Yes

Notes: Institutional controls in Columns (2) to (5) include dummy variables for second reading requirements, mandatory fiscal referendums, citizens' assemblies, and female suffrage. Other controls in Columns (3) to (5) include the log of the population, the shares of people aged 20 to 39, 40 to 64, and over 64, the share of foreigners, the shares of Catholics and Protestants, the shares of German and French speakers. In Panel B, we use the log of other enactments + 1. The sample period is 1908-2020. There are fewer observations in Panel A than in Panel B because the ratio of pages per enactment is not defined for cantons and years with zero statutes. We allow standard errors (in parentheses) to cluster at the level of cantons. Significance levels: $^{+}$ $P < .10$, * $P < .05$, ** $P < .01$.

If the mandatory legislative referendum simply increased fixed costs of legislating, policymakers would change laws less frequently but make more changes when they do. According to Panel A in Table 3, we observe no decrease in the number of pages per enactment after the abolishment. In Panels B and C, we explore additional substitution effects. We find no evidence that politicians circumvent voters' veto by regulating through other types of enactments (for example, executive and legislative decrees), which are not subject to the

mandatory legislative referendum, or by switching to fiscal policy instruments such as subsidies.[11]

Figure 2. Abolishment of mandatory legislative referendums and regulatory activity by legal topic



Notes: The graphs show the effects of the abolishment of mandatory legislative referendums on regulatory activity in nine topic areas for four years before and after the abolishment. The x-axis depicts the event time relative to the year of the abolishment, the y-axis the means of the log of the number of statutes (+1) and the corresponding 95-percent confidence intervals.

The scope for special interest regulation differs across legal topics. There is little scope in some areas because it is either impossible to favor specific powerful economic groups through general rules or because the federal level regulates the most important aspects. An example for the former is criminal law, examples for the latter are social security as well as defense and civil

[11] According to the specification with canton-specific quadratic time trends, expenditures even increase with the abolishment of the mandatory legislative referendum.

protection. Therefore, it is informative to know in which area of law the effect of the voter veto is strongest. We manually code the legal topics for four years before the abolishment to four years after. Again, we rely on our legal expert to assign the enactments to one of the nine official legal topic areas. It is prohibitively costly to manually code the legal topics or to digitize the relevant information for all cantons and years.

Figure 2 shows a striking pattern: The increase in regulatory activity is strongly concentrated in the legal areas 1) "state, authorities" and 6) "finance, taxes, state-owned enterprises". In these two legal areas, the scope for special interest regulation is substantial. The former topic area is where politicians regulate themselves, their bureaucracy, and judges and lawyers. Interestingly, laws in this area do not typically focus on correcting market failures. In the latter topic area, especially tax laws allow politicians to target specific groups. Our finding that regulatory activity increases in those categories prone to special interest regulation, and not across the board, is consistent with a Stiglerien view of regulation and speaks against a simple fixed-cost interpretation.

A few examples from the years immediately after the abolishment of the mandatory legislative referendums illustrate the scope for special interest regulation in the two legal areas.[12] In the canton of Aargau in 2005, legislators increased subsidies to party groups in parliament by more than 40 percent in real terms. In the canton of Solothurn in 2000, they substantially strengthened public sector unions by designating unions as official representatives of public employees and by granting them consultation and co-determination rights. In the canton of Graubünden in 2006, the parliament extended the scope of legal cases in which parties must be represented by licensed attorneys, especially to lucrative divorce cases. In the same year, the parliament also halved the top tax rate on corporate profits. In the canton of Bern in 1974, legislators exempted the two dominant retailers from a tax rate increase by reclassifying their very specific type of cooperative into another tax category; this reclassification even resulted in a tax rate reduction (Der Bund 1974).

This additional evidence is inconsistent with the notion that mandatory legislative referendums simply increase the fixed costs of regulation: We fail to detect a shift from extensive to more limited changes with the abolishment of mandatory legislative referendum and we see the

[12] Even after the abolishment of the mandatory legislative referendum, strong institutions, including citizens' options to collect signatures and to demand a facultative legislative referendum, continue to prevent outrageous cases of preferential treatment.

largest reactions in areas that are especially prone to special interest capture. Thus, in our view this additional evidence favors a Stiglerian view of regulation.

4. Conclusion

We document that institutional constraints that give voters a veto over legislation reduce regulatory activity. In our view, this suggests that large parts of the population and of legislators perceive important portions of regulations as undesirable.

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Appendix

Table A1. Summary statistics

Variables	Mean	SD	Min	Max
Dependent variables				
Regulatory activity: statutes (log)	1.891	1.037	0	5.497
Regulatory activity: pages per statute (log)	1.577	0.769	0	5.215
Regulatory activity: other regulation (log)	3.806	0.946	0	5.778
Cantonal expenditures (log)	11.633	2.481	5.689	16.546
Institutional variables				
Mandatory legislative referendum (abolishment)	0.475	0.499	0	1
Second reading requirement	0.316	0.465	0	1
Mandatory fiscal referendum	0.644	0.479	0	1
Citizens' assembly	0.179	0.383	0	1
Female suffrage	0.452	0.498	0	1
Other controls				
Cantonal population (log)	11.769	1.118	9.467	14.251
Ages 20-39 (%)	29.307	2.763	21.940	39.213
Ages 40-64 (%)	28.889	4.080	19.387	38.588
Ages 65+ (%)	11.462	4.362	4.229	23.081
Foreigners (%)	13.053	8.027	1.532	40.950
Catholics (%)	55.522	27.640	11.403	98.348
Protestants (%)	35.673	26.760	1.633	88.361
German speakers (%)	70.064	33.173	2.958	99.685
French speakers (%)	16.584	29.301	0.029	89.962

Table A2. Abolishment of mandatory legislative referendums and regulatory activity

Dependent variable: Log of number of statutes	(1)	(2)	(3)	(4)	(5)
Mandatory legislative referendum (abolishment)	0.339**	0.400**	0.411**	0.457**	0.392**
	(0.078)	(0.076)	(0.067)	(0.070)	(0.064)
Institutional controls					
Second reading requirement		-0.091^{+}	-0.131**	-0.059	-0.047
		(0.052)	(0.042)	(0.071)	(0.091)
Mandatory fiscal referendums		0.004	0.035	-0.022	-0.014
		(0.055)	(0.047)	(0.064)	(0.075)
Citizens' assembly		0.330**	0.343**	0.200^{+}	-0.077
		(0.077)	(0.070)	(0.117)	(0.157)
Female suffrage		0.003	-0.047	0.052	0.096
		(0.066)	(0.063)	(0.086)	(0.093)
Other controls					
Cantonal population (log)			0.196	-0.350	-1.878**
			(0.138)	(0.481)	(0.550)
Ages 20-39 (%)			-0.016	-0.044^{+}	-0.041
			(0.020)	(0.023)	(0.025)
Ages 40-64 (%)			-0.023	-0.051**	-0.048*
			(0.017)	(0.016)	(0.018)
Ages 65+ (%)			-0.017	-0.032	-0.093*
			(0.019)	(0.027)	(0.034)
Foreigners (%)			0.009	0.006	0.036^{+}
			(0.008)	(0.017)	(0.018)
Catholics (%)			0.023**	-0.001	-0.015
			(0.007)	(0.008)	(0.014)
Protestants (%)			0.016*	0.035**	0.035**
			(0.007)	(0.012)	(0.010)
German speakers (%)			-0.006	-0.029^{+}	-0.030*
			(0.005)	(0.014)	(0.013)
French speakers (%)			-0.008	-0.015	-0.011
			(0.009)	(0.012)	(0.010)
Canton effects	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes
Linear canton-specific trends	No	No	No	Yes	Yes
Quadratic canton-specific trends	No	No	No	No	Yes
Number of observations	2778	2778	2778	2778	2778

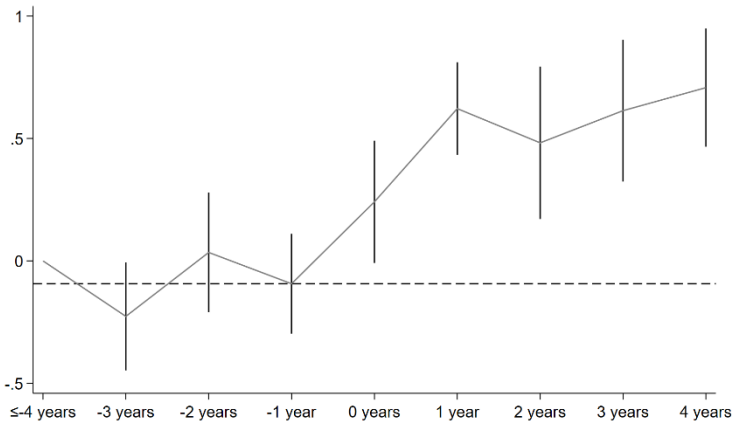
Notes: The sample period is 1908-2020. We allow standard errors (in parentheses) to cluster at the level of cantons. Significance levels: $^{+}$ $P < .10$, * $P < .05$, ** $P < .01$.

Table A3. Poisson regressions

	(1)	(2)	(3)	(4)	(5)
Panel A – Dependent variable: Number of statutes					
Mandatory legislative referendum	0.395**	0.427**	0.445**	0.435**	0.401**
(abolishment)	(0.081)	(0.080)	(0.061)	(0.064)	(0.064)
Number of observations	2778	2778	2778	2778	2778
Panel B – Dependent variable: Number of statutes					
Mandatory legislative referendum (abolishment)					
without concurrent total revision	0.421**	0.488**	0.540**	0.535**	0.441**
of cantonal constitution	(0.102)	(0.111)	(0.067)	(0.072)	(0.077)
with concurrent total revision	0.310**	0.294**	0.249**	0.237**	0.324**
of cantonal constitution	(0.045)	(0.050)	(0.048)	(0.088)	(0.109)
Number of observations	2778	2778	2778	2778	2778
Panel C – Dependent variable: Number of other enactments					
Mandatory legislative referendum	-0.008	-0.048	-0.132	-0.029	0.032
(abolishment)	(0.172)	(0.201)	(0.136)	(0.083)	(0.081)
Number of observations	2778	2778	2778	2778	2778
Canton effects	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes
Institutional controls	No	Yes	Yes	Yes	Yes
Other controls	No	No	Yes	Yes	Yes
Linear canton-specific trends	No	No	No	Yes	Yes
Quadratic canton-specific trends	No	No	No	No	Yes

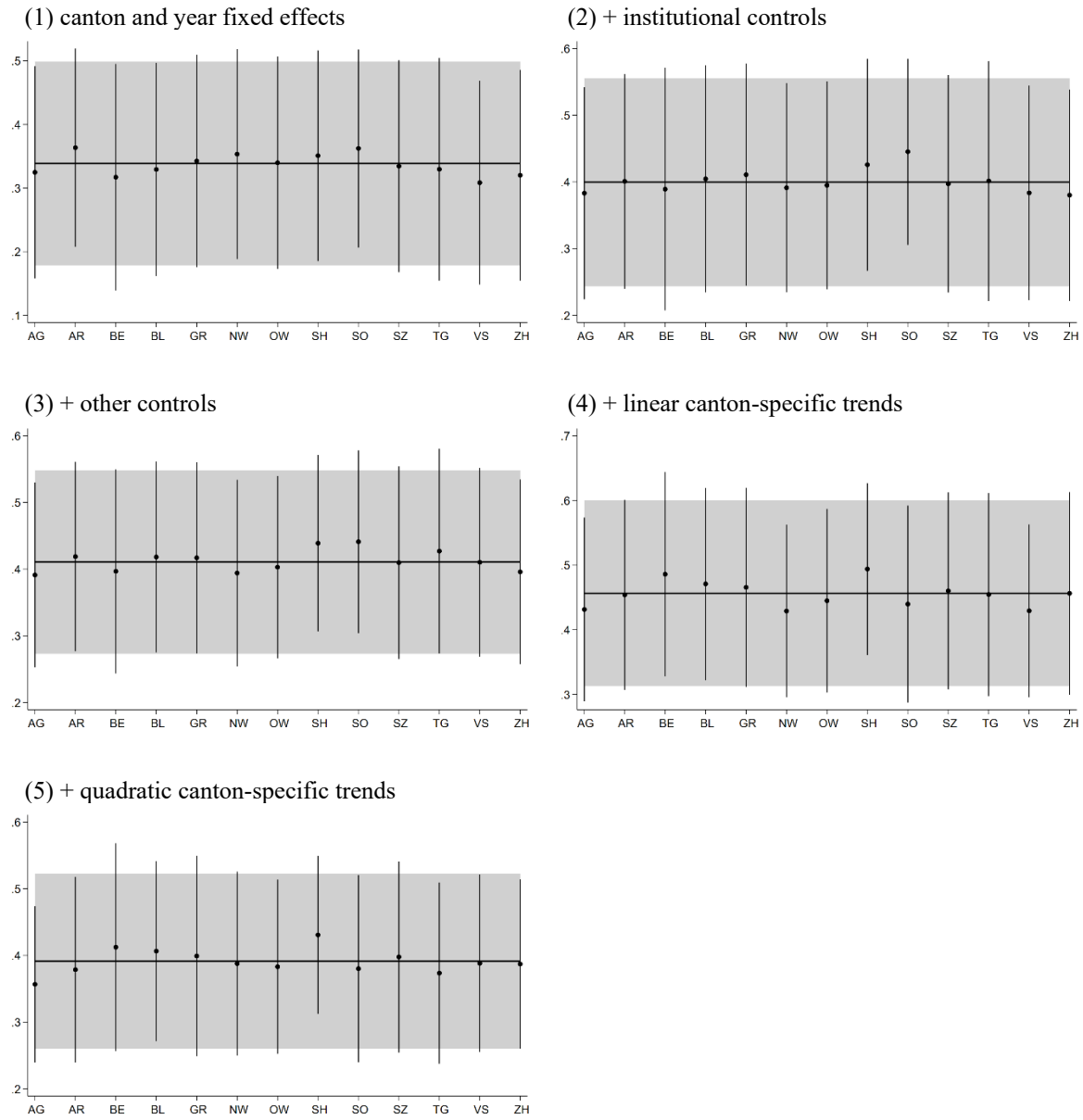
Notes: Institutional controls in Columns (2) to (5) include dummy variables for second reading requirements, mandatory fiscal referendums, citizens' assemblies, and female suffrage. Other controls in Columns (3) to (5) include the log of the population, the shares of people aged 20 to 39, 40 to 64, and over 64, the share of foreigners, the shares of Catholics and Protestants, the shares of German and French speakers. The sample period is 1908-2020. We allow standard errors (in parentheses) to cluster at the level of cantons. Significance levels: $^{+}$ $P < .10$, * $P < .05$, ** $P < .01$.

Figure A1. Poisson event-type analysis based on Wooldridge (2023)



Notes: The graph shows the effects of the abolishment of mandatory legislative referendums in the years before and after the abolishment, along with the corresponding 95-percent confidence intervals. The x-axis depicts the event time relative to the year of the abolishment. The y-axis reports the results (on the log-scale) of an event-type analysis using Poisson pseudo maximum likelihood regression, accounting for effect heterogeneity based on Wooldridge (2023) as implemented by Rios-Avila, Nagengast, and Yotov (2024). The regression controls for time-varying institutional and other covariates as well as canton and time effects (analogous to the specification in Column 3 in Table 1) and allows standard errors to cluster at the level of cantons. The dashed line refers to the year before the abolishment.

Figure A2. Exclusion of individual treatment cantons from the estimation



Notes: The graphs present the robustness estimates with the exclusion of individual cantons abolishing the mandatory legislative referendum. Each graph corresponds to one of the five increasinly rich specifications of Table 2. The black lines and shaded areas represent the corresponding point estimates and 95-percent confidence intervals for the full sample. The dots and vertical lines show point estimates and 95-percent confidence intervals for reduced samples excluding the canton indicated on the x-axes.