Direct Democracy, Coalition Size, and Public Spending*

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August 17, 2020

Abstract

This article contributes to the literature on direct democracy and public spending in two ways. First, we explore how direct democratic institutions interact with a specific aspect of the respresentative system, the size of the governing coalition, to influence public spending. Second, based on newly collected data, we examine the relationship between three different direct democratic institutions, coalition size, and public spending over the period from 1860 to 2015. Empirically, we find that initiatives increase the size of the public sector under single-party governments, but this positive relationship disappears as coalition size increases. In contrast, we find that financial referendums slow down the growth of public spending, while law referendums are not systematically associated with public spending. Finally, we find that the relationship between direct democratic institutions, coalition size, and public spending does not change over time despite the long period under investigation.

*Drafts were presented at the ECPR Joint Sessions in Mons 2019 and EPSA 2019 in Belfast. We are grateful to Lucy Barnes, the session organizers, and participants for valuable comments. In addition, we thank the reviewers and the editor for their feedback and suggestions. Finally, we thank Nadine Golinelli, Anja Guidici, Ruben Hackler, Katharina Klung, Stefan Sandmeier, Julia Sifringer, Chiara Valsangiacomo, Linda Wanklin, and Sylvain Wenger for their research assistance. This research is being generously supported by the Swiss National Science Foundation (grant #:100017_184969).

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1 Introduction

Direct democratic institutions change how political decisions are reached. A classic question is whether the presence of direct democratic institutions affects the extent of state intervention (e.g. Wagschal, 1997; Matsusaka, 2004; Funk and Gathmann, 2013). In a recent comprehensive literature review, Matsusaka (2018) shows that effects differ between direct democratic institutions and depend on features of the representative system. Most notably, while financial referendums are consistently observed to have a negative effect on state intervention, the evidence is more mixed in case of popular initiatives. Finally, little is known about the effect of law referendums and earlier periods more generally (for the latter, an exception is Matsusaka, 2000).

We add to this literature in two ways. First, echoing Hug (2009), we argue that to better understand how direct democratic institutions work, we need to analyze how they interact with the representative system. While the literature has shown the effect of direct democractic institutions to depend on features of the representative system (e.g. Gerber, 1996*b*; Boehmke et al., 2015; Matsusaka, 2018), there has been little research focused specifically on understanding which features of the representative system matter. We explore an aspect of the respresentative system that is particularly important in shaping decision-making processes: the size of the governing coalition (Bawn and Rosenbluth, 2006; Persson et al., 2007). We thus formulate theoretical expectations on how different direct democratic institutions interact with coalition size in affecting public spending.

Second, based on newly collected data, we examine the relationship between three different direct democratic institutions, coalition size, and public spending over the period from 1860 to 2015. Empirically, we explore subnational variation in Switzerland, which is one of the most prominent cases relying on direct democracy (including at subnational level). Importantly, Switzerland's high level of fiscal decentralization means that most public spending occurs at subnational level. Switzerland also features a diversity of direct democratic institutions. We focus on three in particular.

Popular initiatives[1] allow voters to put policy proposals to popular votes (subject to signature requirements and collection periods) without consulting or requiring the approval of legislative or executive bodies. If approved at the ballot box, proposals must be implemented. In contrast, law referendums[2] allow citizens to challenge laws previously passed by parliament (again subject to signature requirements and collection periods). If law referendums are approved at the ballot box, laws are null and void. Finally, financial referendums[3] set a monetary threshold for public spending. If policies surpass this threshold, popular votes are either mandatory or can be held if enough signatures are collected in the required period. If financial referendums are approved at the ballot box, public money will not be spent.

Our empirical analysis finds the effect of popular initiatives to depend on the size of the governing coalition. Bawn and Rosenbluth (2006) show that single-party governments spend less than multi-party governments. We observe that when only few parties are in government, voters respond to the undersupply of public goods by launching initiatives that aim at expanding public spending. However, this positive effect on public spending disappears as coalition size increases.

In addition, in line with the existing literature (e.g. Funk and Gathmann, 2011), we

[1] Sometimes also referred to as *citizens' or voters' initiative*.

[2] Sometimes also referred to as *optional referendum*, *popular referendum*, *veto referendum*, *petition referendum*, or *abrogativo*.

[3] Sometimes also referred to as *budget refrendums* or *fiscal referendums*.

find that financial referendums slow down the growth of public spending. However, the effect does not vary with coalition size.

In contrast, unlike earlier research that emphasized the veto player effect of law referendums (e.g. Wagschal, 1997), we find no negative effect of law referendums on public spending. Instead, we argue that law referendums affect public spending in possibly unexpected ways, as they operate as a sword of Damocles, in response to which governments may forge oversized coalitions for laws where they fear defeat at the ballot box (for an early formulation of this argument, see Neidhart, 1970). Empirically, the anticipation of such direct democratic challenges leads to more public spending, which compensates for the more direct veto player effect of law referendums, ultimately resulting in a null effect on public spending.

Finally, we find that the relationship between direct democratic institutions, coalition size, and public spending does not change over time. Employing three different empirical strategies to identify time-variant effects, we observe the mechanisms to be stable and robust despite the long period under investigation (1860-2015).

This paper is organized as follows. The next section develops our theoretical expectations about how the different direct democratic institutions interact with coalition size in affecting public spending. Subsequently, we discuss our case selection, the research design, and the data. We then turn to the empirical examination of our theoretical expectations. A final section concludes.

2 Direct Democracy and the Representative System

The relationship between direct democracy and public spending is the subject of a rich literature. According to Vatter et al. (2019, 177-178), there are basically two schools of thought. Highlighting the veto player function of direct democracy, especially in the form of financial referendums (cf. Hug and Tsebelis, 2002), several scholars observe a negative effect on public spending (e.g. Romer and Rosenthal, 1979; Matsusaka, 1995; Wagschal, 1997; Obinger, 1998; Feld and Matsusaka, 2003; Funk and Gathmann, 2011, 2013). Other scholars, in the tradition of Downs (1957), argue that more direct participation, especially in the form of popular initaitives, may lead to more state intervention due to democracies' inherent tendency to redistribute (e.g. Freitag and Vatter, 2006; Asatryan, 2016; Asatryan et al., 2017; Blume et al., 2009; Blume and Voigt, 2012; Walter, 2019). In an encompassing literature review, Matsusaka (2018) finds that financial referendums are consistently observed to have a negative effect on state intervention. In contrast, the evidence is more mixed in case of popular initiatives (see also Morger and Schaltegger, 2018). Finally, comparatively little is known about the effect of law referendums.

In this section, we develop a series of theoretical expectations about how popular initiatives, law referendums, and financial referendums relate to public spending. Most notably, we explore how these direct democratic institutions interact with the representative system. The literature has repeatedly shown the effect of direct democractic institutions to depend on features of the representative system (e.g. Gerber, 1996*b*; Hug, 2009; Boehmke et al., 2015; Leemann and Wasserfallen, 2016; Matsusaka, 2018). Yet, there is comparatively little research focused specifically on understanding which features of the representative system matter. We examine a specific feature, which is of particular importance in shaping political decision-making processes: the size of the governing coalition, here understood as the number of parties forming the government.

The existing literature has shown coalition size to matter for public expenditure (e.g. Volkerink and de Haan, 2001; Perotti and Kontopoulos, 2002; Braeuninger, 2005; Bawn and Rosenbluth, 2006; Persson et al., 2007). Conceiving of the political space as being composed of a limited number of societal groups, this literature argues that the groups' welfare is a function of public spending. Government participation provides political parties with the possibility to channel resources to their constituencies. Yet, spending decisions are made under cost constraints that change with the size of a party's support base. In small coalitions, parties represent the interest of multiple societal groups that constitute a large part of the population. As a result, they have to balance the demand for different public goods with limited scope to externalize costs (assuming that the financial burden for public spending is borne by all societal groups). In contrast, parties in large coalitions depend on smaller constituencies, respectively, with more homogeneous preferences for spending, allowing them to externalize costs to a larger share of the population. This simple but powerful argument implies that public spending increases with the number of parties partaking in the governing coalition.[4]

How does coalition size interact with the relationship between direct democratic insti-

[4]We follow Bawn and Rosenbluth (2006) in arguing that this logic applies to all coalition governments and that governments – rather than legislatures – are the relevant decision makers on the public budget. However, it should be noted that this fiscal commons argument applies best to classic parliamentary democracies. In contrast, Swiss cantonal governments are directly elected, although they can virtually always rely on a majority coalition in parliament. There is thus no single majority coalition that determines *all* political outcomes. However, it would certainly be wrong to argue that the creation of ad hoc coalitions for each spending decision occurs randomly. Instead, we strongly expect a relationship between government and spending coalitions. For instance, in case of a coalition government consisting of two parties, we should expect these two parties to form the large majority of these spending coalitions. In contrast, in case of a coalition government consisting of four parties, we should expect spending coalitions to be on average larger and often consist of these very four parties. Hence, although Swiss cantonal governments are hardly the textbook example for the fiscal commons literature, we expect the logic to apply nonetheless.

tutions and public spending? In principle, we can distinguish between direct and indirect effects of direct democratic institutions (Neidhart, 1970; Gerber, 1996*a*; Matsusaka and McCarthy, 2001; Matsusaka, 2014). Consider the case of law referendums, which allow citizens to collect signatures against a newly legislated law. By definition, this institution's direct effect is to block new legislation and by doing so limit the creation of new government action with spending implications (e.g. Immergut, 1990; Hug and Tsebelis, 2002). Yet, such institutions might also have indirect effects. In the case of law referendums, indirect effects may result from the anticipation of referendum challenges. If governments and legislators are strategic, governments may be enticed to forge oversized coalitions to avoid defeat at the ballot box (e.g. Neidhart, 1970; Gerber, 1996*a*). In this case, the indirect effect of law referendums might in fact increase public spending.

We expect direct democratic institutions to differ in the way they interact with the representative system. Therefore, we discuss the interaction between direct democratic institutions and coalition size for each institution separately. We start with popular initiatives. Subsequently, we discuss law referendums and, finally, financial referendums.

2.1 Popular Initiatives

Popular initiatives extend proposal power to groups outside the governing coalition (Smith and Tolbert, 2004; Leemann, 2015; Morger and Schaltegger, 2018). The direct effect of popular initiatives is thus to give groups outside the governing coalition the possibility to put their spending preferences on the political agenda. In Switzerland, popular initiatives allow proposing a bill without consulting or requiring the approval of legislative or executive bodies. Therefore, popular initiatives provide the proposer with great flexibility to engineer an optimal coalition (Leemann, 2015, 601). The direct effect of popular initiatives should thus lead to more public spending (e.g. Freitag and Vatter, 2006; Asatryan, 2016; Asatryan et al., 2017).

Moreover, this direct effect should be particularly strong in case of small governing coalitions. Given that the level of public spending is lower in case of small governments, societal groups' spending preferences are least served in such situations. As a result, they have a strong incentive to employ popular initiatives to increase public spending (in their favor). In addition, it implies that the smaller the number of parties in government, the more likely that societal groups not represented in government will, ceteris paribus, succeed in forming a winning coalition (Walter, 2019). In contrast, in case of large governing coalitions, parties have a narrower voter base and, thus, can externalize a higher share of the costs, which should lead to higher levels of public spending (e.g. Bawn and Rosenbluth, 2006). This means, in turn, that there are fewer incentives to employ initiatives to increase public spending and there is a more limited potential to form a winning coalition.

Yet, popular initiatives also have indirect effects. With a view on the interaction with coalition size, we want to emphasize their role in splitting government coalitions. Popular initiatives can make it more difficult for coalition governments to externalize costs, because outside groups can use popular initiatives to propose alternatives to parts of the governing coalition. This indirect, coalition-splitting effect of popular initiatives makes credible commitments within coalitions more difficult (cf. Bäck and Lindvall, 2015). In doing so, popular initiatives can break up package deals in coalitions and prevent excessive spending that is externalized to groups outside the governing coalition.[5] The

[5]These reflections build on the agenda control literature associated with Romer and Rosenthal (1979).

indirect effect of popular initiatives should thus reduce public spending.

We expect this indirect, expenditure-reducing effect of popular initiatives to become more relevant with coalition size. It is easy to see why. The smaller the governing coalition (understood as the number of parties forming the coalition), the more difficult it becomes to break the coalition. At the most extreme, the government consists of only one party. In contrast, in case of large governing coalitions, there is also a large number of potential defectors, thus increasing the likelihood that popular initiatives have the discussed indirect effect.

Based on these mechanisms, we expect popular initiatives to have a positive effect on public spending in presence of small coalition governments (the direct effect prevails), while we expect popular initiatives to have a negative effect on public spending in case of large coalition governments (the indirect effect prevails).

2.2 Law Referendums

Law referendums have a direct effect on the policy process by being successfully employed. For instance, successful law referendums prevent the adoption of new laws or amendments that would have been implemented in the absence of a popular vote. If every legislative act is potentially subject to a law referendum, political decision-making processes are confronted with additional veto players, which affects the law-making process globally (Hug and Tsebelis, 2002). This direct effect of referendums is likely to slow down the growth of the state.

Yet, given our focus on how direct democratic institutions interact with governing coalitions, these arguments differ from contributions such as Matsusaka (2014), which also emphasize direct and indirect effects of popular initiatives, but focus on how the initiative process can change policy indirectly by providing a threat that induces the governing coalition to change policy (compared to the actual use of popular initiatives).

This negative effect should increase with coalition size (i.e. become more negative), because large coalitions tend to increase public spending, but this public spending does not necessarily benefit groups *outside* the coalition government. Hence, when large coalition governments are in power, outside groups are even more incentivized to challenge laws passed by parliament, because public spending is likely to strongly deviate from their spending preferences.

Yet, law referendums can also influence the policy process indirectly (Hug, 2004; Leemann and Wasserfallen, 2016). The credible threat of powerful interest groups or political parties to employ referendums can push legislators to change or abandon policy proposals. Given that outcomes in popular votes are to a certain extent unpredictable, members of parliament have an incentive to accommodate the interests of all relevant groups. The goal is the anticipation or withdrawal of referendum challenges. As a result, law referendums incentivize political decision-makers to create oversized coalitions in support of specific policies to avoid defeat at the ballot box. Ultimately, this anticipation effect implies that governments try to accommodate the policy preferences of all (or most) societal groups capable of launching a referendum (for an early formulation of this argument, see Neidhart, 1970).[6] As these policy preferences of societal groups typically result in further spending, this anticipation effect should lead to more public spending.

Coalition size is likely to accentuate this positive effect on public spending. Larger government coalitions consist of parties with relatively small, but homogeneous constituencies, which allow them to externalize costs to a larger share of the population (e.g. Bawn and Rosenbluth, 2006). Hence, in the case of large coalitions, the interests of an

[6]For related arguments in the US context and with regard to popular initiatives, see Gerber (1996*a*), Matsusaka and McCarthy (2001), and Matsusaka (2014).

even larger number of small groups must be accommodated in order to avoid defeat at the ballot box, which is likely to increase public spending even further.[7]

Whether the direct or indirect effect of law referendums on public spending prevails is an empirical question. Yet, we believe the indirect effect of law referendums to be more important because the large majority of political decisions are made exclusively within the representative system, while only a few policies are challenged in the people's court. Put differently, *all* political decisions are made in the "shadow" of a referendum challenge. As a consequence, the number of political decisions that are subject to the indirect effect clearly outnumbers the number of decisions subject to the direct effect of law referendums.

2.3 Financial Referendums

Like law referendums, financial referendums are subject to direct and indirect effects. Yet, empirical research has consistently found a negative effect on public spending (cf. Matsusaka, 2018). In the case of financial referendums, politicians propose a policy, resulting in a certain amount of additional public spending. If the monetary threshold triggering a financial referendum is crossed, citizens have the opportunity to approve the policy. If citizens accept the proposal, the policy will be implemented and lead to the expected costs. In contrast, if citizens decline, the policy will not be implemented and the expected costs will not materialize. Hence, with regard to direct effects, financial referendums should have a negative effect on public spending. In addition, for reasons discussed above, this negative effect should become stronger as governments are increas-

[7]This argument assumes that coalition size reflects a more fragmented party system more generally.

ingly spendthrift. As a consequence, the negative effect should increase with government coalition size.

Of course, governments might anticipate the possibility of a financial referendum (indirect effect). For instance, decision-makers might be incentivized to create oversized coalitions in support of specific policies to avoid defeat at the ballot box. Yet, for two reasons, such anticipation effects are less likely in the case of financial referendums.

First, financial referendums are often *mandatory* once a certain monetary threshold is crossed, which implies that financial compensation cannot prevent the referendum, but only tilt the outcome of the popular vote in one's favor. In addition, such anticipation effects leading to higher levels of public spending also increase the likelihood of crossing the monetary threshold triggering a financial referendum in the first place.

Second, in the case of financial referendums, anticipation strategies also include attempts to structure policies in an attempt to stay below the monetary threshold that triggers financial referendums (e.g. by splitting policy projects into several smaller ones, which all stay below the spending level triggering a financial referendum). If they succeed, the financial referendum can be avoided and the money can be spent as planned. Yet, there are often clear limits with regard to how policies can be restructured to such an end.

Hence, similar to law referendums, we expect direct and indirect effects to work against each other in the case of financial referendums. However, given these additional difficulties in anticipating referendum challenges, we expect the direct effect to prevail in the case of financial referendums.

3 Case Selection, Data, and Research Design

We examine the conditional effect of direct democratic institutions on public spending using subnational (cantonal level) data from Switzerland. Two features make Switzerland a good choice for our analysis. First, all Swiss cantons know direct democratic institutions. Yet, there is considerable variation across time and space. While some cantons introduced these instruments early, others were laggards (Leemann, 2019). Similarly, while some cantons make the recourse to direct democratic instruments easy, others feature more restrictive rules.

Second, most public spending is located at subnational level. Despite several decades of fiscal centralization, only 32% of public spending is located at federal level (data for 2016 from the Eidgenössische Finanzverwaltung 2017). In contrast, 41% of public spending is located at cantonal level (the remaining 27% is primarily located at municipality level). In addition, there is considerable variation across time and space, as we show in Figure 1 below.

We are of course not the first to focus on Swiss cantons to examine the link between direct democratic institutions and public policies. Several researchers have used similar research designs to examine the effect of direct democracy on public spending (e.g. Kirchgässner et al., 1999; Feld and Matsusaka, 2003; Freitag and Vatter, 2006; Funk and Gathmann, 2011) or other socio-economic outcomes (e.g. Frey, 1994; Feld and Savioz, 1996; Freitag and Vatter, 2000).

We have engaged in a considerable data collection effort. First, we have collected public spending data in 5-year intervals from the public budget reports of the cantonal governments, covering the period 1830-1930 (Emmenegger et al., 2019).[8] Subsequently, we have merged our data with that provided by Funk and Gathmann (2011), which covers the period 1890-2000 and were obtained from federal records (most notably the Federal Office of Statistics). Note that no federal records are available before 1890. Finally, we have extended the dataset to cover the period 2000-2015, again based on federal records.

Both data collection strategies have their specific strengths and weakenesses. Cantonal budget reports are closer to the "source". Yet, there is a risk that cantonal definitions differ as to what belongs to public spending (e.g. separate budgets for cantonal monopolies on the salt trade). Federal records might provide more comparable information. Yet, federal records are more valid only if the data provided by the cantons correspond to the federal guidelines. The overlap of 40 years (1890-1930) in our data and that provided by Funk and Gathmann (2011) allowed us to explore such differences. In some cases, we went back to the cantonal archives to find additional budgets that were not part of the official cantonal budget reports (e.g. separate budgets for public schools in Obwalden). In other cases, we found that cantons reported incomplete data to the federal administration (e.g. Appenzell Innerrhodes reported data only for its finance department, but not public spending by other cantonal departments). Once we had all discrepancies cleared, we merged the data to obtain a dataset covering the period 1830-2015. For the sake of consistency, we use all data in 5-year intervals.

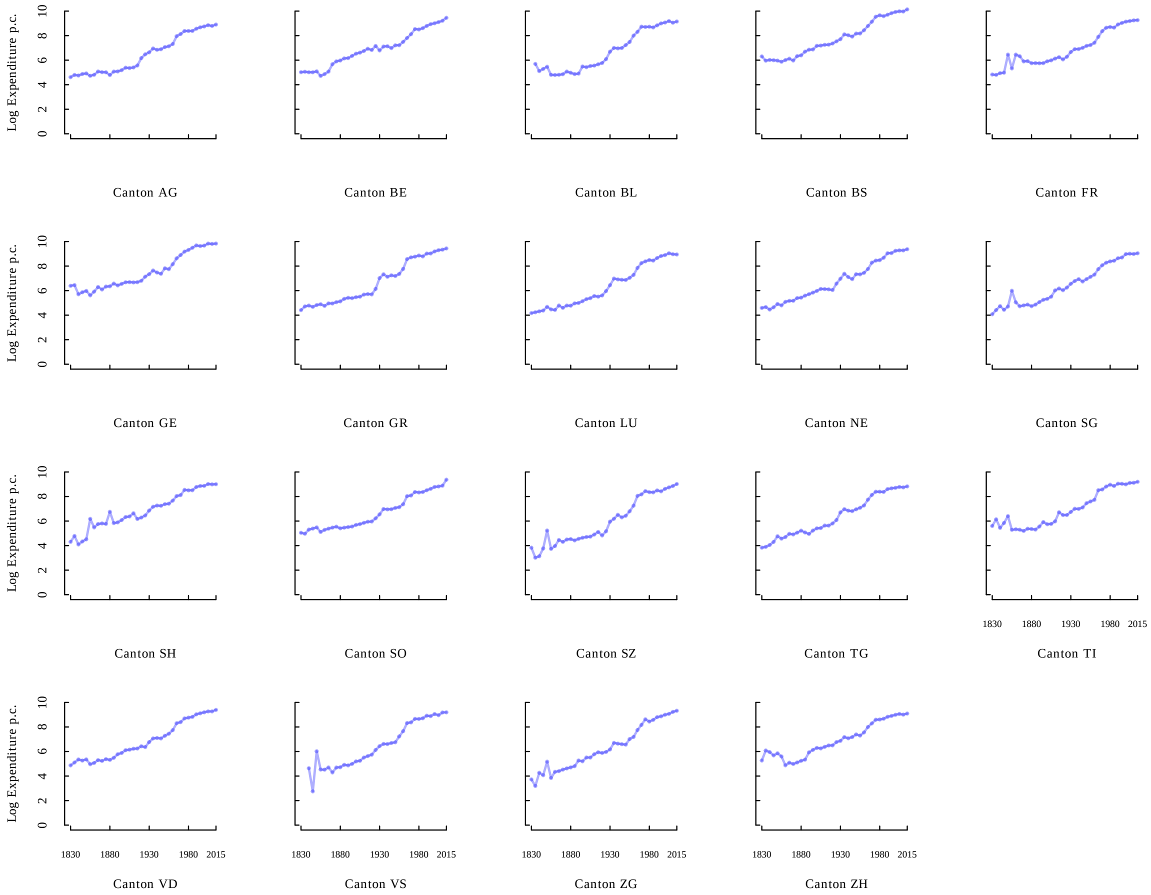
To make the data comparable over time and across cantons, we employ official exchange rates and the consumer price index (prices in 2005 Swiss francs).[9] In addition, we

[8]We have not collected data for the canton of Jura, which seceded from Bern only in 1979. Jura is thus excluded from the analysis.

[9]The Swiss franc was officially introduced in 1850. We use the official exchange rates in 1850 to convert earlier currencies into Swiss francs.

divided public spending by the cantonal population to account for different canton sizes. Finally, we transformed the data with the natural logarithm. In Figure 1, we plot the growth of spending in all cantons with representative systems. Unsurprisingly, the figure shows a clear upward trend in the period 1830-2015. Yet, we also observe considerable variation across time and space, which we will explore in the empirical section.

Figure 1: The Development of Public Spending in Swiss Cantons, 1830-2015



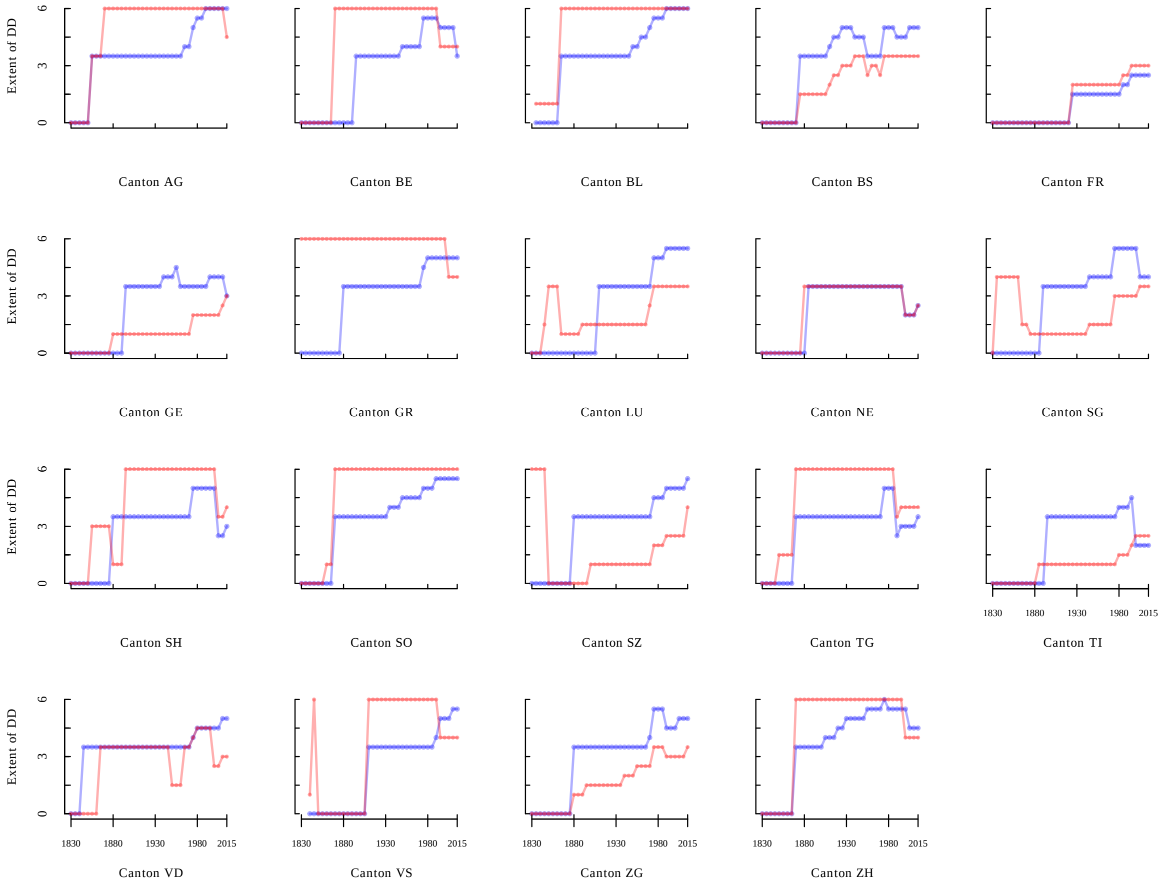
Second, we rely on data on the institutionalized rules of direct democratic instruments for the period 1830-2015. For the purpose of the paper, we distinguish between popular initiatives, law referendums, and financial referendums. We rely on all cantons that had a representative system throughout the period of investigation. We therefore exclude the six small cantons that rely on citizen assemblies (Glarus, Nidwalden, Obwalden, Uri and both Appenzell).

Direct democratic institutions vary with regard to the ease to which they can be put to use. The existing literature has used this variation to identify the effect of direct democratic institutions on political outcomes. Researchers have employed proxies for the economic costs of using direct democratic institutions related to variations in signature requirements and collection periods to capture these institutions' effects (e.g. Stutzer, 1999; Fehr and Gächter, 2000; Frey and Stutzer, 2010; Torgler, 2002). More precisely, the higher the signature requirements (relative to the population of the relevant political unit) and the shorter the collection period, the higher the economic costs of using direct democratic institutions.

Our measures for popular initiatives and law referendums are composite indices and come from Leemann (2019). Both indices range from 0 to 6 with higher values displaying lower "costs" of using direct democratic instruments, i.e. lower signature requirements and longer signature collection periods. Figure 2 shows the extent to which cantonal constitutions grant the citizenry participatory rights. Red lines capture law referendums, while blue lines capture popular initiatives. Higher values indicate that it is easier to force a ballot vote on a policy that has been passed in parliament (law referendum) or force a popular vote on a specific proposal (popular initiative). We provide more detailed information on the indices in the appendix (see subsection 6.2).

We also examine the effect of financial referendums on public spending. Existing analyses have often used rather simple measures of financial referendums, typically a dummy variable capturing its existence (e.g. Funk and Gathmann, 2011). Instead, we use the deflated monetary threshold per capita for financial referendums (for a similar

Figure 2: The Extent of Direct Democratic Institutions in Swiss Cantons, 1830-2015



Note: Red lines show values for law referendums and blue lines show values for popular initiatives.

approach, see Feld and Matsusaka, 2003). Put differently, we take advantage of the fact that the "stinginess" of financial referendums varies across cantons and time, with some cantons enabling financial referendums in the case of small expenditures, while others rule out the financial referendum even for relatively large expenditures. Figure 3 shows how the deflated monetary thresholds per capita developed in the period 1830 to 2015. In line with the literature, we expect the financial referendum to have a stronger effect in the case of low monetary thresholds (deflated and per capita). As a consequence, we expect the coefficient of our indicator of financial referendums to be positive (because high thresholds indicate a comparatively permissive financial referendum).

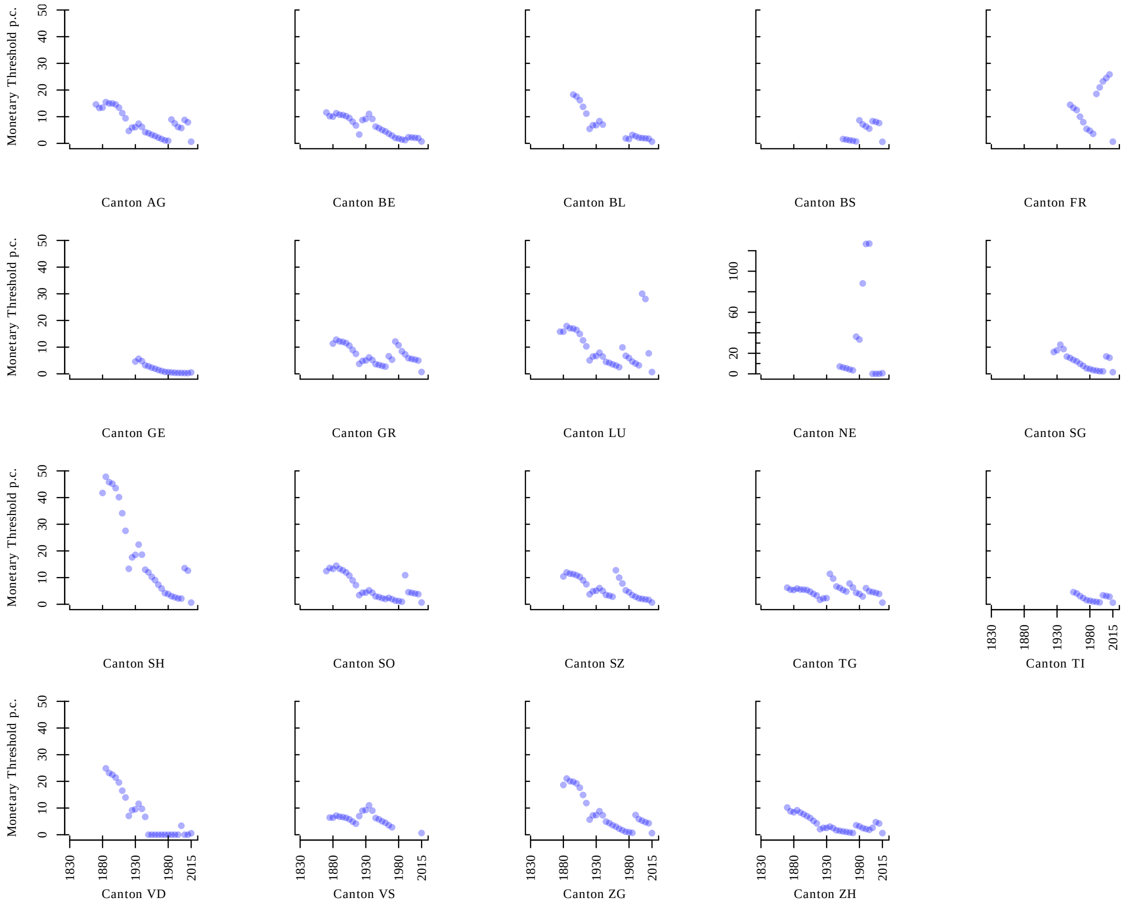
This operationalization implies that we are not working with an index comparable to the ones used for popular initiatives and law referendums. The reason is that in many Swiss cantons, a popular vote is mandatory if a policy surpasses a certain predefined monetary threshold. As a consequence, in these cases, there are no signature requirements and collection periods. Instead, we capture variation across cantons and over time by means of the deflated monetary threshold per capita triggering a financial referendum.

In the case of financial referendums, we are, however, confronted with a challenge. If the financial referendum has not been adopted (yet), we have no data for the monetary threshold. This is not a problem with our indices of popular initiatives and law referendums, which conceptualize the recourse to popular initiatives and law referendums in terms of associated "costs". In this operationalization, the absence of popular initiatives and law referendums, respectively, corresponds to the highest possible costs on the index (score is 0). In the case of financial referendums, we follow two strategies to deal with such cases. First, in the absence of a financial referendum, we assign a value of 0 to the monetary threshold. Second, we add a binary indicator that takes on the value of 1 when a canton does not have the financial referendum institution. This is the modified zero-order regression described by Greene (1993) and Maddala (1977) as a solution to partial missingness.

Third, we rely on data on the political composition of cantonal governments for the period 1848-2017 (Walter and Emmenegger, 2019).[10] In line with the previous literature (e.g. Bawn and Rosenbluth, 2006), we use the number of parties in government as theoretically relevant predictor of public spending. In addition, we control for the seat share

[10]For two cantons, data is missing for parts of the 19$^{\text{th}}$ century (Argovia 1848-1885 and Schaffhouse 1848-1876).

Figure 3: Deflated Monetary Thresholds of Financial Referendums (per capita) in Swiss Cantons, 1830-2015



Note: The canton of Neuchatel (NE) has a different scale on the y-axis.

of left parties to control for partisan effects on public spending (Schmitt and Zohlnhoefer, 2019). Finally, the previous literature has linked parliamentary fragmentation to higher government spending, especially in political systems in which governments are directly elected (Eslava and Nupia, 2017). To proxy parliamentary fragmentation, we use a binary variable to capture whether proportional representation is employed to elect representatives for the legislative.

In addition, we use a number of control variables to account for socio-economic influences on government spending. Given that GDP data is not available, we use the size of the first and second sectors, infant mortality, and physician density as proxies. Further-more, we measure demographic pressures with the dependency ratio, i.e. the share of the population below the age of 15 and above 64. We also include the size of the population, transformed by the natural logarithm, to capture changes in the denominator of our dependent variable. Lastly, we use the logarithm of federal subsidies per capita. A table with summary statistics is presented in the appendix (see Table 3).

The combined data covers the period 1860-2015 and is measured in 5-year intervals. To estimate the effect of direct democratic instruments on public spending, we rely on an error correction model and employ the following specification. First, we include canton and year fixed effects (FE) to control for unobserved constant heterogeneity and common shocks. Second, we add a lag of the outcome variable (public spending) to all specifications (LDV). Finally, standard errors are clustered by cantons.

4 Empirical Tests

In this section, we examine our expectations. All models that we employ can be described by the following equation:

$$\Delta y_{i,t} = \beta_{LDV} y_{i,t-1} + \boldsymbol{\beta} \boldsymbol{X}_{i,t} + \alpha_i + \gamma_t + \varepsilon_{i,t},$$

whereas α_i are unit fixed effects, γ_t are time fixed effects, and $\varepsilon_{i,t}$ is a normally distributed error term. Our main independent variables as well as our control variables are captured by the stacked matrix $\boldsymbol{X_{i,t}}$ with a vector of coefficients $\boldsymbol{\beta}$. This specification helps to isolate the relationship between cantonal spending and the institutional setting. To shield against spurious correlations due to common trends, we add unit as well as time fixed effects. General leveling effects are absorbed by lagged per capita spending $\beta_{LDV} y_{i,t-1}$.[11]

Model 1 in Table 1 includes all variables as well as the interaction of the popular initiative with the number of parties in government. In Model 2, the interaction is set up with the law referendum. Model 3 looks at the interaction with the financial referendum. Finally, Model 4 includes all three interactions simultaneously. The findings are consistent across all specifications displayed in Table 1.

Three observations can made based on Table 1. First, there is clear empirical support for the argument that the association of popular initiatives and public spending is dependent on the number of parties in government. When only few parties are in government, popular initiatives have a positive effect on public spending, but this positive effect becomes weaker as the number of parties in government increases. Second, we cannot observe any statistically significant relationship between law referendums, coalition size, and public spending. Third, the models suggest that the effect of financial referendums is not dependent on the number of parties in government. In all models, the association is significant with the expected (positive) sign, whereas the interaction terms in Models 3 and 4 do not reach conventional levels of statistical significance.

In the appendix, we present a number of robustness checks. In Table 6 we re-estimate the same models but also include the small citizen assembly cantons. The inclusion of these small cantons – which also have very extensive direct democratic rights – does not alter the substantive results. In Table 7, we drop the highly urbanized cantons Basel and

[11] Please note that even though our model specification is restrictive, it does not ensure that we can identify causal relationships. Previous approaches have therefore used temporal or spatial lags as instruments to address such endogeneity concerns (Feld and Matsusaka, 2003; Funk and Gathmann, 2011). However, recent methodological contributions have shown that these strategies are build on problematic assumptions and do not solve the problem that they are trying to address (Bellemare et al., 2017; Betz et al., 2018). As a result, we rely on this more conventional approach.

Table 1: Direct Democracy and Government Spending, 1860-2015

	Model 1	Model 2	Model 3	Model 4
Number of Parties	0.07**	0.03	0.04*	0.06*
	(0.03)	(0.02)	(0.02)	(0.03)
Popular Initiatives	0.04·	0.02	0.01	0.05*
	(0.02)	(0.01)	(0.01)	(0.02)
Financial Referendum (Threshold)	0.14*	0.16*	0.33*	0.31·
	(0.06)	(0.07)	(0.16)	(0.18)
With Financial Referendum	−0.03	−0.01	−0.02	−0.02
	(0.03)	(0.03)	(0.03)	(0.03)
Law Referendum	0.01	0.00	0.01	−0.01
	(0.01)	(0.02)	(0.01)	(0.02)
Lag Dependent Variable	−0.29***	−0.30***	−0.30***	−0.29***
	(0.04)	(0.04)	(0.04)	(0.04)
Share First Sector	−0.40·	−0.39	−0.38	−0.39
	(0.24)	(0.24)	(0.24)	(0.25)
Share Second Sector	−0.23	−0.18	−0.14	−0.23
	(0.30)	(0.30)	(0.30)	(0.31)
Dependency Ratio	0.96*	0.99·	0.98*	1.04*
	(0.47)	(0.51)	(0.50)	(0.46)
Infant Mortality	−0.30	−0.30	−0.29	−0.32
	(0.53)	(0.52)	(0.51)	(0.54)
Share Left Parties	−0.06	−0.07	−0.07	−0.05
	(0.07)	(0.08)	(0.08)	(0.07)
Proportional Representation	−0.08**	−0.07**	−0.07**	−0.08**
	(0.03)	(0.03)	(0.03)	(0.03)
Physician Density	−0.10*	−0.10**	−0.10**	−0.10*
	(0.04)	(0.04)	(0.04)	(0.04)
ln Population Size	0.03	0.02	0.02	0.03
	(0.06)	(0.05)	(0.05)	(0.06)
ln Federal Subsidies	0.03*	0.03*	0.03*	0.03*
	(0.01)	(0.01)	(0.01)	(0.01)
Num. Par. * Initiative	−0.01*			−0.01**
	(0.00)			(0.01)
Num. Par. * Law Referednum		0.00		0.01
		(0.00)		(0.01)
Num. Par. * Fin. Referendum			−0.07	−0.07
			(0.06)	(0.07)
R^2	0.52	0.51	0.51	0.52
Adj. R^2	0.45	0.45	0.45	0.46
Num. obs.	628	628	628	628
RMSE	0.15	0.15	0.15	0.15

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\cdot}p < 0.1$

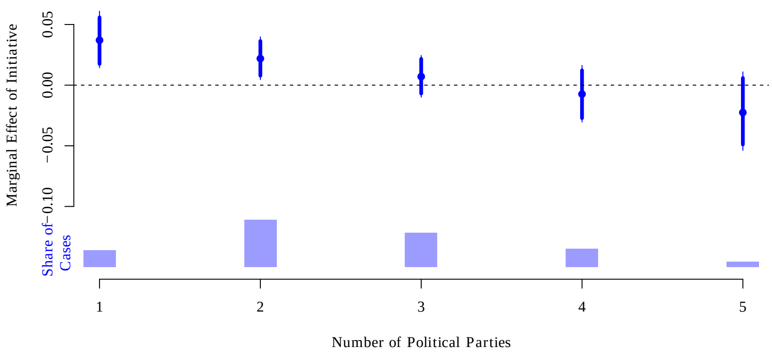
Geneva from our sample. In addition, we exclude all observations for Neuchâtel after 1970 (see Table 8) because the canton displays unusually high values for the financial referendum (see Figure 3). These additional model specifications yield substantively identical results. Put differently, the estimation results are in line with the results presented here.

In addition, we also explore whether these estimates are stable over 155 years or whether there is any temporality to them (Matsusaka, 2004). Below, we show that these results remain stable in the face of three independent modeling strategies all aimed at uncovering temporal change in the estimates. However, first, we take a closer look at the interaction terms.

4.1 Interaction Terms

We now turn to the interpretation of the interaction terms. In this section, we focus on illustrating the short-run effects of direct democratic institutions for different coalition sizes. Our discussion of the magnitude of the long-run effects under different scenarios can be found in the appendix (subsection 6.5).

Figure 4: Marginal Effect of Popular Initiatives

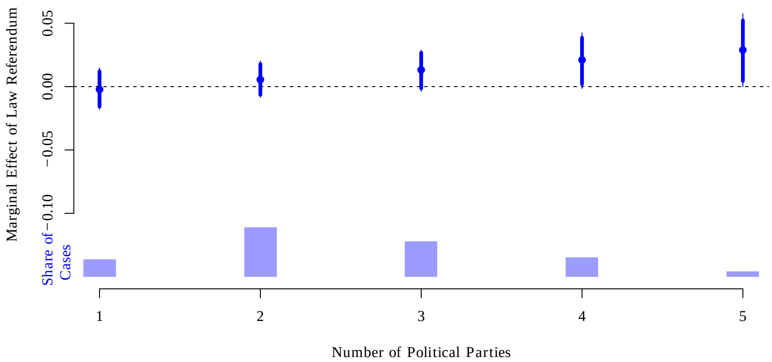


Note: Based on estimates from Model 4. Simulated marginal effect with 90% and 95% confidence intervals.

We first examine the effect of popular initiatives. Across all specifications presented in Table 1, we find a conditional effect of popular initiatives on public spending. Figure 4 displays the marginal effect of popular initiatives conditional on the number of parties in government. We find that for small governments consisting of only one or two parties, there is a positive marginal effect for the popular initiative. This positive effect turns negative as the number of parties in government increases. This result is in line with our theoretical argument.

Figure 5 displays the marginal effect of law referendums conditional on coalition size. The marginal effect increases for larger coalitions. Interestingly, the effect is significant for large coalition governments. These results provide some limited support to the argument that in anticipation of law referendums, governments try to foster large bill-specific coalitions, which in turn are linked to higher spending.

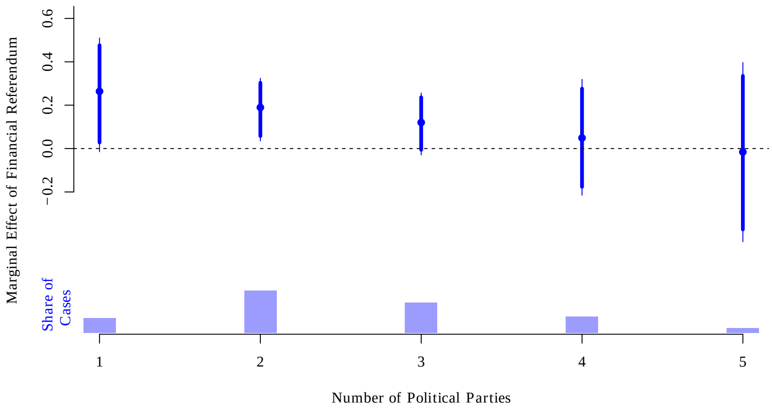
Figure 5: Marginal Effect of Law Referendums



Note: Based on estimates from Model 4. Simulated marginal effect with 90% and 95% confidence intervals.

Finally, Figure 6 displays the marginal effect of financial referendums conditional on coalition size. The marginal effect appears to be decreasing, but the interaction parameter is not distinguishable from 0. In addition, the marginal effect is straddling the threshold for significance. In case of two-party coalition governments, the effect is significantly different from zero. Together with results displayed in Table 1, this finding suggests that financial referendums are linked to lower public spending, which is consistent with our theoretical expectations (remember that we examine the monetary threshold triggering a referendum). However, we do not find strong evidence that the effect of financial referendums is conditional on coalition size.

Figure 6: Marginal Effect of Financial Referendums



Note: Based on estimates from Model 4. Simulated marginal effect with 90% and 95% confidence intervals.

Error correction models have a dynamic component. Yet, the shown estimates as well as the visualizations of the interaction terms display only the short-run effects. In the appendix, we therefore discuss long-run effects of all direct democratic instruments (see subsection 6.5).

4.2 Stability over Time

Analyzing data over more than 150 years immediately leads to the question whether these estimates are stable over time or whether one is missing part of the historical processes by imposing a too strict model specification (Wawro and Katznelson, 2014). The models presented so far all rely on fixed parameters over time, thus assuming that the effect of direct democratic institutions remained stable over time. At first, this might seem like a heroic assumption but it is in line with what we typically assume when we analyze the effects of electoral rules on strategic behavior (cf. Cox, 1997).

However, rather than just asserting this point, we also conduct a number of empirical tests that are likely to show if these effects vary over time. In total, we employ three different strategies to show this over-time stability. First, we explicitly interact explanatory factors with time variables (and the square thereof) to allow for different functional forms. Second, we estimate the model only on slices of the data and test whether estimates on early data are different from estimates on later data (Egger et al., 2019). Finally, we use hierarchical models and model parameters as random effects and inspect how their time disturbances cluster (Wawro and Katznelson, 2014).

4.2.1 Interacting with Year Variable

In the first step, we interact the interaction terms of the baseline specification in Model 4 with a binary indicator for the 19^{th} century. This simple test should allow us to see if there is any difference between the 19^{th} and the 20^{th} century. As an alternative and more flexible approach, we also interact the term with the year and in subsequent models with squared years.

None of these interactions with century dummies, years, or years squared are significant (see Table 2). There is thus no indication that the effects vary over time. While this is a somewhat restrictive approach, as we impose a clear functional form, we also present two more flexible approaches in the next two subsections.

4.2.2 Differences between Periods

Following Egger et al. (2019), we divide data into different folds and test whether the parameter estimates are different. We split the time up into an early and into a late

Table 2: Temporal Heterogeneity Tests (Controls and Constitutive Terms omitted)

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9
Init. * Num. Par. * 19th Cent.	-0.01								
	(0.03)								
Law Ref. * Num. Par. * 19th Cent.		0.00							
		(0.02)							
Fin. Ref. * Num. Par. * 19th Cent.			-0.06						
			(0.54)						
Init. * Num. Par. * Year				$0.00^{\cdot}$					
				(0.00)					
Law Ref. * Num. Par. * Year					-0.00				
					(0.00)				
Fin. Ref. * Num. Par. * Year						-0.00			
						(0.00)			
Init. * Num. Par. * Year sq.							0.00		
							(0.00)		
Law Ref. * Num. Par. * Year sq.								-0.00	
								(0.00)	
Fin. Ref. * Num. Par. * Year sq.									-0.00
									(0.00)
R^2	0.53	0.53	0.54	0.53	0.53	0.53	0.53	0.54	0.55
Adj. R^2	0.47	0.47	0.48	0.47	0.47	0.47	0.47	0.48	0.49
Num. obs.	628	628	628	628	628	628	628	628	628
RMSE	0.15	0.15	0.15	0.15	0.15	0.15	0.15	0.15	0.15

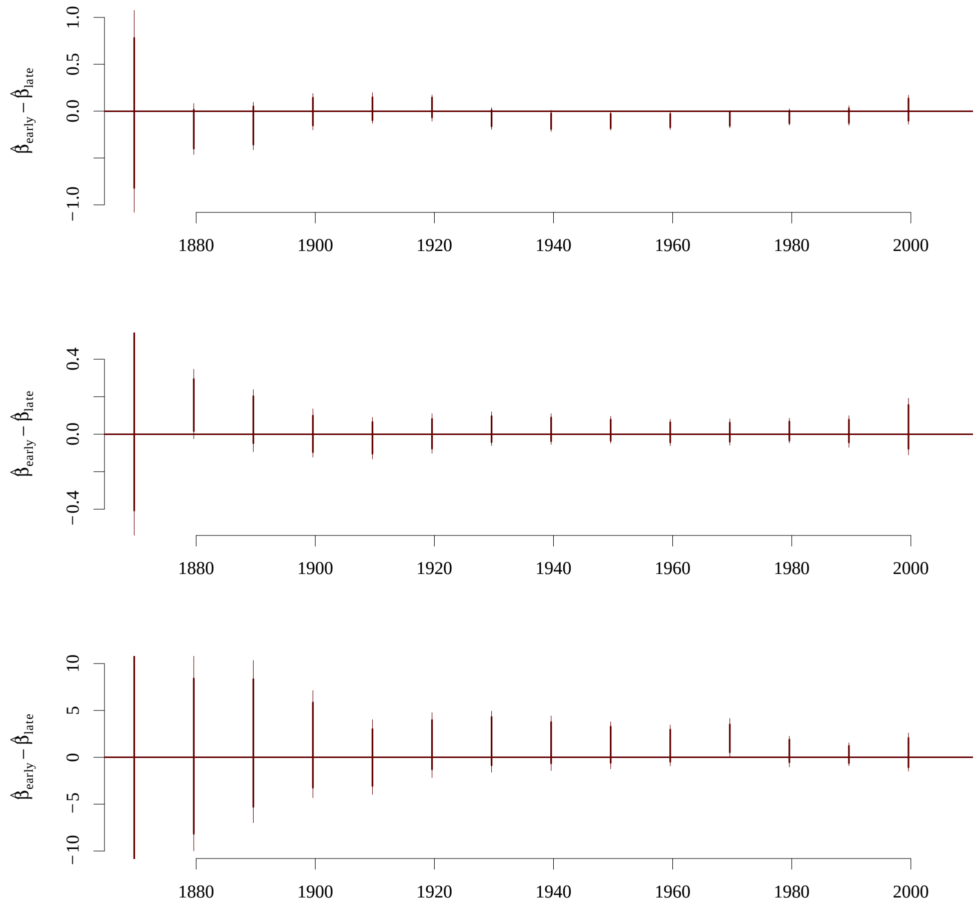
$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\cdot}p < 0.1$

period. We then re-estimate Model 4 from Table 1 on both folds and compare the two estimates for the interaction term (delivering $\hat{\beta}_{int,\text{early fold}}$ and $\hat{\beta}_{int,\text{late fold}}$). In a second step, we test whether these two parameters are different from each other. We repeat these two steps, while moving the cut-off between early and late periods from 1880 up to 2000, each time moving ten years forward per step.

For all three interactions, we analyze whether there are any differences between estimates based on early folds and late folds. The results of this explorative exercise are displayed in Figure 7. The top panel shows the results for popular initiatives, the middle panel shows the results for law referendums, and the bottom panel shows the results for financial referendums. Across all three panels, there is no clear evidence that the mechanism changes considerably over time.

For the initiative there might be a difference depending on the applied α level threshold. However, as soon as we exclude the three WWII time periods, no single comparison is significant (see Figure 13 in the appendix). From this second approach, which is less

Figure 7: Overtime Stability



Note: Top panel: popular initiative, middle panel: law referendum, bottom panel: financial referendum. The cut-off year (separating early and late fold) is shown on the $x-$axis. For each test we take 1,000 draws from the posterior vector and then compare the two draws. The figure shows the 95% and the 99% confidence interval of the difference of the two coefficients (early vs late fold).

restrictive than the first one, we take that there is no empirical evidence that this mechanism changes over time. We move next to an alternative non-restrictive approach, where we try to impose a less functional form.

4.2.3 Random Coefficient Modeling

Our final test for stability builds on a hierarchical modeling strategy, where we include the relevant parameters as random effects. Following Wawro and Katznelson (2014), we use a hierarchical modeling approach to see if there is a temporal drift in the disturbance estimates of the relevant parameters. Specifically, rather than estimating the interaction term as a fixed β_{int}, we model it as a random coefficient β^*_{int}, whereas $\beta^*_{int,t} = \beta^0_{int,t} + \nu_t$ and ν_t follows a normal distribution with mean 0 and a variance that is estimated by the model.

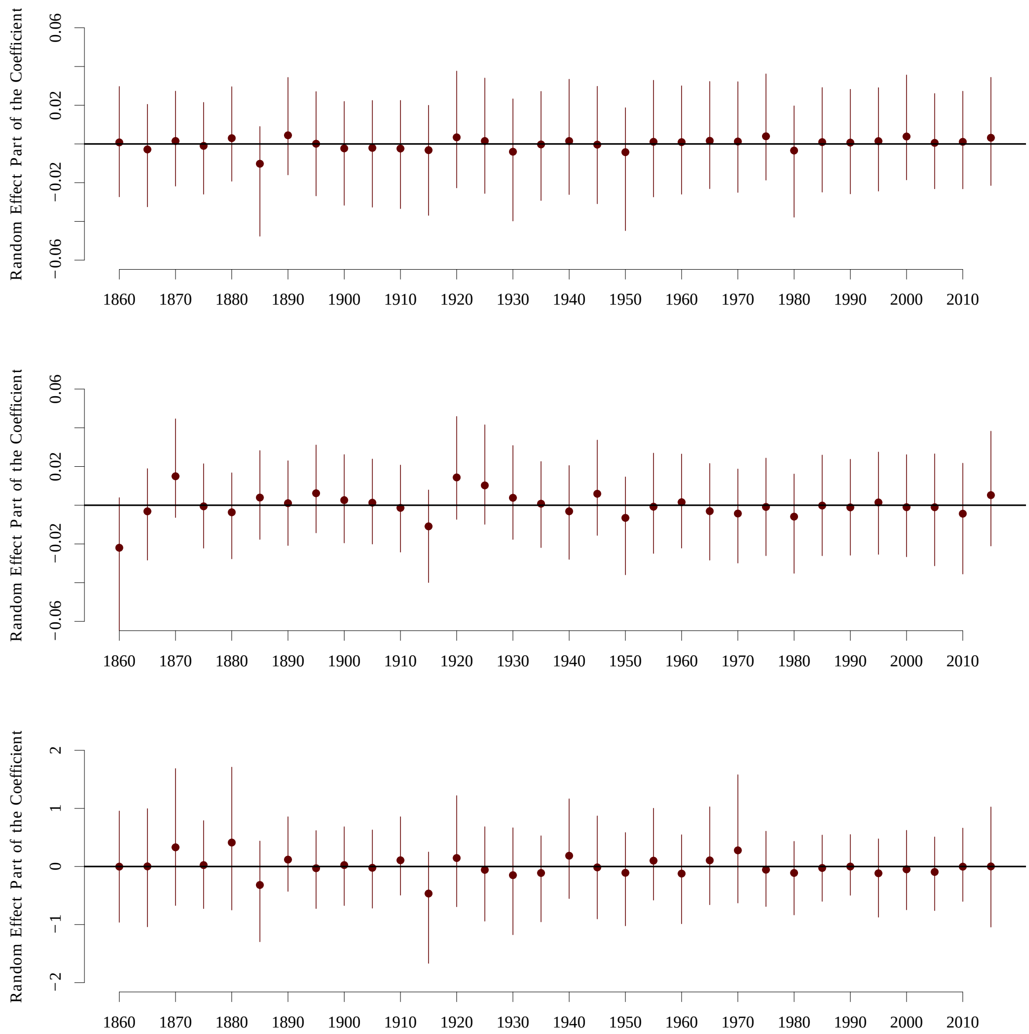
Figure 8 shows the realization of the random component of the random coefficient of the interaction term for the three institutions. The top panel shows effects for popular initiatives, the middle panel for law referendums, and the bottom panel for financial referendums. We see neither any shock nor any drift over time.

In this section, we have provided three tests on whether the main findings of this paper change over time. We have employed three different strategies that vary in how much the model imposes on the data. None of these tests indicates that there is a change over time. The observed effects remain stable from the mid-19$^{\text{th}}$ century over the 20$^{\text{th}}$ century into the 21$^{\text{st}}$ one.

5 Conclusion

How do direct democratic institutions affect public spending? In this paper, we have tried to contribute to this literature in two ways. First, we have explored how three direct democratic institutions (popular initiatives, law referendums, and financial referendums)

Figure 8: Overtime Stability in Random Shocks to Parameters



Note: Top panel: Initiative; middle panel: Law referendum; bottom panel: financial referendum. Plot shows realization of random component of the interaction term when estimated as a hierarchical model using the BRMS package (Bürkner, 2018).

interact with a specific aspect of the representative system, the size of the governing coalition, to influence public spending. Second, based on newly collected data, we have examined this relationship between three different direct democratic institutions, coalition size, and public spending over the period from 1860 to 2015.

This paper thus takes into account the diversity of direct democratic institutions and acknowledges that they do not operate in a vacuum but within a representative system that may condition the effect of direct democratic institutions. While we have only focused on the institutionalized rules of direct democracy instead of their actual use, previous research has shown that lower economic costs are strongly linked to a higher frequency of initiatives and referendums (Leemann, 2015; Asatryan, 2016).

Our results show that the effect of popular initiatives depends on the size of the governing coalition. In the case of small coalitions, extensive initiative rights are linked to higher public spending. However, this positive effect on public spending disappears as coalition size increases. In addition, we find that financial referendums are linked to lower public spending, which – contrary to our theoretical expectations – does not seem to be affected by coalition size. In contrast, we find no negative effect of law referendums on public spending. If anything, law referendums seem to have a positive effect on public spending, which grows with coalition size, although the effect is significantly different from zero only for large coalition governments. The reason for this possibly surprising finding can be found in anticipation effects. Law referendums operate as a sword of Damocles, which force governments to forge oversized bill-specific coalitions to avoid defeat at the ballot box (Neidhart, 1970). As governments try to accommodate the policy preferences of groups capable of launching a law referendum, spending is likely to increase – in particular if the resulting bill-specific coalition consists of a large number of small groups with homogeneous preferences, which are capable of externalizing costs to a larger share of the population (Bawn and Rosenbluth, 2006).

Finally, we find that the interaction between coalition size and direct democratic institutions does not change over time and, therefore, persists in a number of different socio-economic and political contexts. Apart from different levels of economic development, Swiss cantons started out with limited male suffrage and majoritarian electoral systems and switched to proportional representation and universal suffrage over the last two centuries. As a result, we are confident that our results will also hold in other contexts beyond the Swiss cantons.

In sum, these heterogenous effects demonstrate that when studying the relationship between direct democracy and public spending, researchers have to be attentive to differences between direct democratic institutions and take into account their embeddedness in the representative system (cf. Hug, 2009). Yet, certainly more research is needed to understand which features of the representative system matter and how these different features relate to each other when moderating the effect of direct democratic institutions on public spending. In addition, future research should investigate how the use of direct democratic institutions alters the composition of governing coalitions and the design of policies in detail. For instance, under what conditions do direct democratic institutions help to integrate minority interests in the policy process? To answer such questions, the interaction between representative institutions and direct democracy must be examined at the micro level.

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6 Appendix

6.1 Summary Statistics of the Data

Table 3: Summary Statistics

Variable	vars	n	mean	sd	min	max	range	se
Δ ln public spending (p.c.)	1	724	0.1	0.3	-1.9	3.3	5.2	0.0
Year	2	741	1926.4	54.8	1830.0	2015.0	185.0	2.0
Number of parties	3	671	2.7	1.0	1.0	5.0	4.0	0.0
Lagged logarithm of spending (p.c.)	4	724	6.8	1.6	2.8	10.0	7.2	0.1
Initiative	5	741	2.9	1.9	0.0	6.0	6.0	0.1
Law referendum	6	741	3.2	2.3	0.0	6.0	6.0	0.1
Financial referendum	7	741	0.1	0.1	0.0	1.3	1.3	0.0
Share second sector (employees)	8	638	0.4	0.1	0.1	0.7	0.6	0.0
Share first sector (employees)	9	638	0.2	0.2	0.0	0.8	0.8	0.0
Dependency ratio (younger than 20, older than 64)	10	638	0.4	0.0	0.3	0.5	0.2	0.0
Child mortality	11	709	0.1	0.1	0.0	0.4	0.4	0.0
Share of left parties	12	671	0.1	0.2	0.0	0.6	0.6	0.0
Proportional representation	13	741	0.5	0.5	0.0	1.0	1.0	0.0
Population density	14	666	0.9	0.6	0.2	4.2	4.1	0.0
Logarithm population density	15	741	5.0	0.9	2.9	7.3	4.4	0.0
Logarithm of federal subsidies	16	741	9.2	4.0	0.0	15.6	15.6	0.1

6.2 Measuring Direct Democracy

We rely on indicator from Leemann (2019) who in turn builds on Stutzer (1999). The subnational direct democracy index (snDDI) is a composite measure and we use here three disaggregated elements from it.

6.2.1 Initiative

The initiative is coded on the number of days one is granted to collect the necessary amount of signatures as well as the amount of signatures. Table 4 shows how these measures are translated into index points:

Table 4: Index Rules for Initiative

Absolute Numbers of Signatures	Points	Relative Share of Signatures	Points	Allowed Collection Period	Points
0-2,500	6	0-1%	6	more than 300 days	6
2,500-5,000	5	1-2%	5	241-300 days	5
5,000-7,500	4	2-3%	4	181-240 days	4
7,500-10,000	3	3-4%	3	121-180 days	3
10,000-12,500	2	4-5%	2	61-120 days	2
more than 12,500	1	more than 5%	1	less than 60 days	1

The score for the initiative is then the average value across these three dimensions.

6.2.2 Law Referendum

The structure of the law referendum is very similar to the initiative (see Table 5). The largest difference to the initiative lies in how the absolute signatures are counted - lower numbers count for more in the law referendum.

Table 5: Index Rules for Law Referendum

Absolute Numbers of Signatures	Points	Relative Share of Signatures	Points	Allowed Collection Period	Points
0-1,250	6	0-1%	6	more than 150 days	6
1,250-2,500	5	1-2%	5	121-150 days	5
2.500-3,750	4	2-3%	4	91-120 days	4
3,750-5,000	3	3-4%	3	61-90 days	3
5,000-6,250	2	4-5%	2	31-60 days	2
more than 6,250	1	more than 5%	1	less than 30 days	1

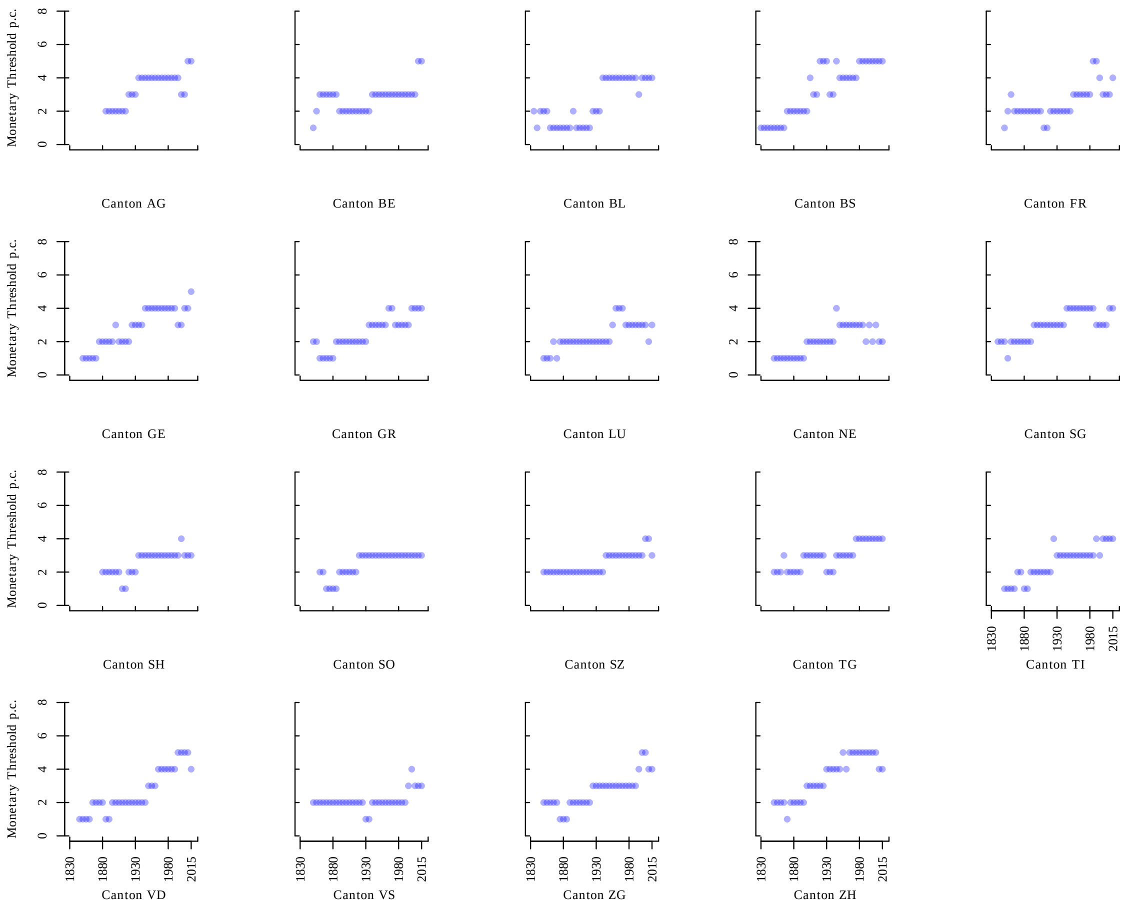
The score for the initiative is then the average value across these three dimensions.

6.2.3 Financial Referendum

For the financial referendum we rely on two measures. The first one accounts for whether such an institution was present in a given canton year observation. The second measure is the defined threshold that triggers a vote. We take the threshold and adjust it for inflation to create a measure that is comparable over time.

6.3 Number of Parties in Government

Figure 9: Number of Parties in Government over Time, 1830-2015



6.4 Robustness Tests

Table 6: Results with Citizen Assembly Cantons, 1860-2015

	Model 1	Model 2	Model 3	Model 4
Proportional Representation	-0.06	-0.05	-0.05	-0.06
	(0.04)	(0.04)	(0.04)	(0.04)
Popular Initiatives	0.04^{**}	0.01	0.01	0.04^{**}
	(0.01)	(0.01)	(0.01)	(0.01)
Financial Ref. (Threshold)	0.10	0.10	0.20	0.16
	(0.07)	(0.07)	(0.15)	(0.16)
With Financial Referendum	-0.01	-0.00	-0.00	-0.01
	(0.04)	(0.04)	(0.04)	(0.04)
Law Referendum	0.01	0.02	0.01	0.01
	(0.01)	(0.02)	(0.01)	(0.02)
Lag Dep. Variable	-0.47^{***}	-0.47^{***}	-0.47^{***}	-0.47^{***}
	(0.05)	(0.05)	(0.05)	(0.05)
Share First Sector	-0.46	-0.55	-0.55	-0.46
	(0.44)	(0.47)	(0.47)	(0.45)
Share Second Sector	-0.08	-0.05	-0.06	-0.08
	(0.37)	(0.37)	(0.37)	(0.38)
Dependency Ratio	0.69	0.63	0.61	0.68
	(0.61)	(0.61)	(0.61)	(0.60)
Infant Mortality	-0.47	-0.45	-0.47	-0.48
	(0.39)	(0.39)	(0.40)	(0.39)
Share Left Parties	0.05	0.03	0.03	0.05
	(0.07)	(0.07)	(0.07)	(0.07)
Physician Density	0.06	0.06	0.05	0.06
	(0.08)	(0.08)	(0.08)	(0.08)
ln Population Size	0.05	0.04	0.03	0.05
	(0.13)	(0.14)	(0.14)	(0.13)
ln Federal Subsidies	0.02^{*}	0.02^{*}	0.02^{*}	0.02^{*}
	(0.01)	(0.01)	(0.01)	(0.01)
Num. Par. * Initiative	-0.02^{**}			-0.02^{**}
	(0.01)			(0.01)
Num. Par. * Law Referendum		-0.00		0.00
		(0.01)		(0.01)
Num. Par. * Fin. Referendum			-0.04	-0.02
			(0.05)	(0.06)
R^2	0.56	0.55	0.55	0.56
Adj. R^2	0.50	0.49	0.49	0.50
Num. obs.	774	774	774	774
RMSE	0.16	0.16	0.16	0.16

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table 7: Direct Democracy and Government Spending without City Cantons

	Model 1	Model 2	Model 3	Model 4
Number of Parties	0.10^{***}	0.05^{*}	0.05^{*}	0.09^{**}
	(0.03)	(0.02)	(0.02)	(0.03)
Popular Initiatives	0.05^{**}	0.02	0.02	0.06^{**}
	(0.02)	(0.01)	(0.01)	(0.02)
Financial Ref. (Threshold)	0.16^{*}	0.18^{*}	0.40^{*}	$0.35^{\cdot}$
	(0.07)	(0.08)	(0.18)	(0.19)
With Financial Referendum	-0.01	0.01	0.01	-0.01
	(0.03)	(0.04)	(0.04)	(0.03)
Law Referendum	0.00	0.01	0.00	-0.00
	(0.01)	(0.02)	(0.01)	(0.02)
Lag Dep. Variable	-0.30^{***}	-0.30^{***}	-0.30^{***}	-0.30^{***}
	(0.05)	(0.04)	(0.04)	(0.05)
Share First Sector	-0.31	-0.28	-0.28	-0.31
	(0.29)	(0.29)	(0.29)	(0.30)
Share Second Sector	-0.29	-0.18	-0.14	-0.27
	(0.38)	(0.37)	(0.38)	(0.38)
Dependency Ratio	0.88	0.81	0.84	0.93
	(0.60)	(0.66)	(0.64)	(0.60)
Infant Mortality	-0.03	-0.01	-0.02	-0.06
	(0.56)	(0.56)	(0.55)	(0.57)
Share Left Parties	-0.07	-0.12	-0.11	-0.06
	(0.09)	(0.09)	(0.10)	(0.09)
Proportional Representation	-0.07^{*}	-0.07^{*}	-0.07^{*}	-0.07^{*}
	(0.03)	(0.03)	(0.03)	(0.03)
Physician Density	$-0.11^{\cdot}$	$-0.11^{\cdot}$	$-0.11^{\cdot}$	$-0.11^{\cdot}$
	(0.06)	(0.06)	(0.06)	(0.06)
ln Population Size	0.15^{*}	$0.10^{\cdot}$	0.09	0.14^{*}
	(0.06)	(0.06)	(0.06)	(0.06)
ln Federal Subsidies	0.04^{***}	0.04^{***}	0.04^{***}	0.04^{***}
	(0.01)	(0.01)	(0.01)	(0.01)
Num. Par. * Initiative	-0.02^{***}			-0.02^{**}
	(0.00)			(0.01)
Num. Par. * Law Referendum		-0.00		0.00
		(0.00)		(0.00)
Num. Par. * Fin. Referendum			-0.09	-0.08
			(0.07)	(0.07)
R^2	0.53	0.52	0.52	0.53
Adj. R^2	0.46	0.45	0.45	0.46
Num. obs.	564	564	564	564
RMSE	0.16	0.16	0.16	0.16

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\cdot}p < 0.1$

Table 8: Direct Democracy and Government Spending without Neuchatel after 1970

	Model 1	Model 2	Model 3	Model 4
Number of Parties	0.08^{**}	0.04	0.05^{*}	0.07^{**}
	(0.02)	(0.02)	(0.02)	(0.03)
Popular Initiatives	0.04^{*}	0.02	0.02	0.05^{*}
	(0.02)	(0.01)	(0.01)	(0.02)
Financial Ref. (Threshold)	0.22^{*}	0.23^{*}	0.30	0.29
	(0.11)	(0.11)	(0.21)	(0.24)
With Financial Referendum	−0.03	−0.02	−0.03	−0.03
	(0.03)	(0.03)	(0.03)	(0.03)
Law Referendum	0.01	0.00	0.01	−0.01
	(0.01)	(0.02)	(0.01)	(0.02)
Lag Dep. Variable	-0.30^{***}	-0.30^{***}	-0.30^{***}	-0.30^{***}
	(0.05)	(0.04)	(0.04)	(0.05)
Share First Sector	−0.37	−0.36	−0.36	−0.37
	(0.25)	(0.25)	(0.24)	(0.25)
Share Second Sector	−0.20	−0.15	−0.13	−0.22
	(0.30)	(0.31)	(0.30)	(0.31)
Dependency Ratio	0.95^{*}	$0.98^{\cdot}$	$0.96^{\cdot}$	1.02^{*}
	(0.48)	(0.51)	(0.50)	(0.46)
Infant Mortality	−0.26	−0.26	−0.26	−0.28
	(0.53)	(0.52)	(0.52)	(0.55)
Share Left Parties	−0.09	−0.11	−0.11	−0.08
	(0.08)	(0.08)	(0.08)	(0.08)
Proportional Representation	-0.08^{**}	-0.07^{**}	-0.07^{**}	-0.08^{**}
	(0.03)	(0.03)	(0.03)	(0.03)
Physician Density	-0.10^{*}	-0.10^{*}	-0.10^{*}	-0.10^{*}
	(0.04)	(0.04)	(0.04)	(0.04)
ln Population Size	0.04	0.03	0.04	0.04
	(0.06)	(0.05)	(0.05)	(0.06)
ln Federal Subsidies	0.03^{*}	0.03^{*}	0.03^{*}	0.03^{*}
	(0.01)	(0.01)	(0.01)	(0.01)
Num. Par. * Initiative	-0.01^{*}			-0.01^{**}
	(0.00)			(0.00)
Num. Par. * Law Referendum		0.00		0.01
		(0.01)		(0.01)
Num. Par. * Fin. Referendum			−0.03	−0.03
			(0.08)	(0.08)
R^2	0.52	0.52	0.52	0.52
Adj. R^2	0.46	0.46	0.46	0.46
Num. obs.	619	619	619	619
RMSE	0.15	0.15	0.15	0.15

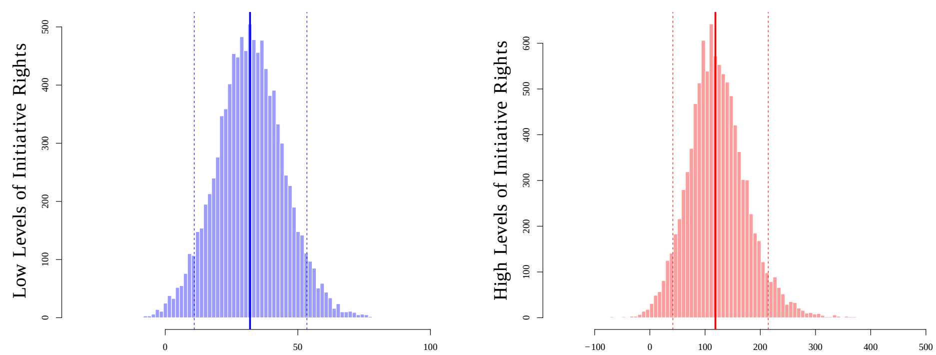
$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\cdot}p < 0.1$

6.5 Long-Run vs Short-Run Effects

Error correction models have a dynamic component. Yet, the shown estimates as well as the visualizations of the interaction effects display only short-run effects. In the following, we turn to the long-run total marginal effects based on the dynamic structure of the model. Following De Boef and Keele (2008), we compute the long-run total marginal effects as $e^{(\frac{\beta}{-\gamma})} - 1$, where β is the estimated coefficient of a variable X and γ is the coefficient of the lagged outcome variable. The effect is the percent change. We draw 1,000 simulations of a pseudo-posterior vector (assuming perfectly multivariate normally distributed coefficients) and use these simulations to compute the long-run effects. We compare two hypothetical cantons where one has a two-party coalition government and the other has a four-party coalition government.

How does the number of parties matter? We first look at long-run total marginal effects of popular initiatives. We do so for a hypothetical canton with low values on the popular initiative index and for a hypothetical canton with a high value. We take the range of values in the year 1930, i.e. the lowest value is 1.5 and the highest is 5. At low to moderate levels of initiative rights (index value of 1.5), we find that the long-term difference is about 32% more public spending in the two-party case. This is illustrated in the left panel of Figure 10. At high levels of initiative rights (index value of 5), the long-term difference is even more impressive (see the right panel of Figure 10). The difference between a canton with two parties in government and one with four parties in government is 118.7% in the long run. Substantively, these simulations of long-run total marginal effects show that in the long run, a canton with a large governing coalition will spend about twice as much as one with a small coalition when there are extensive initiative rights.

Figure 10: Long-Run Total Marginal Effects: Popular Initiatives

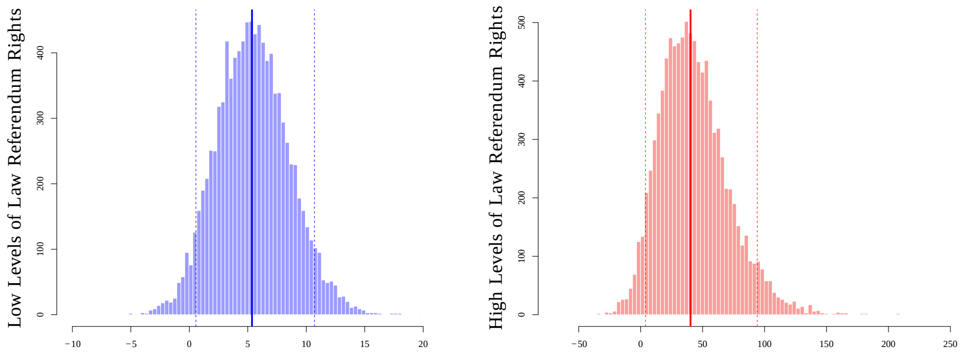


Note: Blue histogram is for low levels of initiative rights. Red histogram is for fairly developed initiative rights. Dashed lines indicate the 95% confidence interval.

We now turn to law referendums. The impact of law referendums depends on the size of the government. We look again at how a difference in the number of parties in government affects long-term spending. We do so for a hypothetical canton with low values on the law referendum index and for a hypothetical canton with a high value. As above, we take the range of values in the year 1930, i.e. the lowest value is 1 and the highest is 5.

Figure 11 shows the difference between a two-party government and a four-party government. In the long run, a large government will spend approximately 5.4% more. But this difference is much larger if there are extensive law referendum rights available. With extensive referendum rights the long-term difference is about 40.2%. Substantively, these simulations of long-run total marginal effects show that in the long run, a canton with a large governing coalition will spend almost one and half times as much as one with a small coalition when there are extensive law referendum rights.

Figure 11: Long-Run Total Marginal Effects: Law Referendums

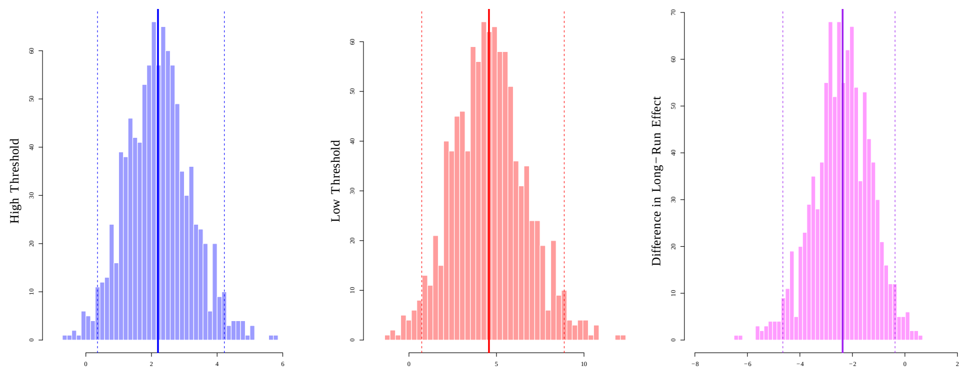


Note: Blue histogram is for low levels of referendum rights. Red histogram is for fairly developed referendum rights. Dashed lines indicate the 95% confidence interval.

The last institution is the financial referendum. It is measured as the threshold of public spending (p.c. and adjusted for inflation) that triggers a ballot vote on that spending. As Table 1 shows, the effect of the financial referendum does not depend on coalition size. We therefore resort to a simpler illustration of its long-term effect. To show the long-run impact in Figure 12, we simulate the effect it has when it moves from a high (0.09) to a lower value (0.04). These two values correspond to the first and third quartile of the threshold value in 1930.

Figure 12 shows that the difference in the long-run impact from slightly decreasing the threshold for the financial referendum is -2.4% in public spending. Substantively, these simulations of long-run total marginal effects show that in the long run, cantons with high monetary thresholds for financial referendums will spend between 2-3% more than cantons with low thresholds.

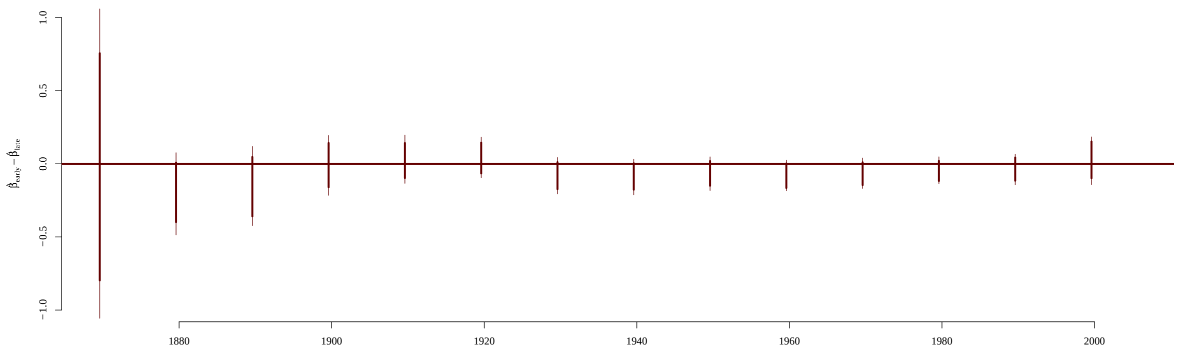
Figure 12: Long-Run Total Marginal Effects: Financial Referendums



Note: Blue and red histogram is for lower/higher threshold. The purple histogram shows the difference. Dashed lines indicate the 95% confidence interval.

6.6 Overtime Stability: Initiative without WWII Years

Figure 13: Overtime Stability: Initiative without WWII Years



Note: For each test we take 1,000 draws from the posterior vector and then compare the two draws. The figure shows the 95% and the 99% confidence interval of the difference of the two coefficients (early vs late fold).