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Compulsory voting, turnout, and government spending: Evidence from Austria☆

Mitchell Hoffman[a,*], Gianmarco León[b,c], María Lombardi[c]

[a] University of Toronto, Canada
[b] Barcelona GSE, Spain
[c] UPF, Spain

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ABSTRACT

We study a unique quasi-experiment in Austria, where compulsory voting laws are changed across Austria's nine states at different times. Analyzing state and national elections from 1949 to 2010, we show that compulsory voting laws with weakly enforced fines increase turnout by roughly 10 percentage points. However, we find no evidence that this change in turnout affected government spending patterns (in levels or composition) or electoral outcomes. Individual-level data on turnout and political preferences suggest that these results occur because the impacts of compulsory voting on turnout are larger among those who are non-partisan, who have low interest in politics, and who are uninformed.

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1. Introduction

Despite the centrality of elections to democracy, in elections around the world many people fail to vote. Many European countries have seen a steep decline in turnout rates in the past 30 years, with record low rates in the past two (2009 and 2014) elections for the European Parliament.[1] Ethnic minorities, immigrants, and poor voters in Europe are significantly less likely to vote, potentially distorting the political process (e.g., Gallego, 2007). In the US, turnout also exhibits large disparities along socioeconomic and racial lines (e.g., Linz et al., 2007; Timpone, 1998). Such disparities in turnout are believed to cause disadvantaged groups to be under-served by government (e.g., Lijphart, 1997; Meltzer and Richard, 1981).

One policy to help address these issues is to make voting mandatory. As of 2008, 32 countries had a compulsory voting (CV) law in place (Chong and Olivera, 2008), and a higher number had CV at some point during the last 50 years. In March 2015, US President Barack Obama proposed the possibility of CV, arguing "If everyone voted, then it would completely change the political map of this country. The people who tend not to vote are young, they're lower income, they're skewed more heavily towards immigrant groups and minority groups.... There's a reason why some folks try to keep them away from the polls."[2] However, there is limited empirical evidence about how CV affects turnout, party vote shares, and government policy.

We provide empirical evidence on the impact of CV laws on turnout, electoral outcomes, and fiscal policy using a unique natural experiment in Austria. Since World War II, Austria's nine states

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* Corresponding author.
E-mail addresses: mitchell.hoffman@rotman.utoronto.ca (M. Hoffman), gianmarco.leon@upf.edu (G. León), maria.lombardi@upf.edu (M. Lombardi).

[1] From http://www.europarl.europa.eu/elections2014-results/en/turnout.html, last accessed March 16, 2016.

[2] See, e.g., http://www.cnn.com/2015/03/19/politics/obama-mandatory-voting/, accessed March 16, 2016.

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have changed their CV laws at different times for different types of elections. Austria provides a compelling case study for multiple reasons. First, the variation in CV laws is significant across states and over time, providing rich variation for quasi-experimental analysis. Second, like the US and many other countries, Austria exhibits socioeconomic disparities in turnout, with poor people being less likely to vote than the rich (Mahler et al., 2014). In addition, as noted by Ferwerda (2014), with the exception of one Swiss canton (Vaud), Austria is the sole modern democracy to have within-country variation in CV for national elections.

Using state-level voting records on state and national elections from 1949 to 2010, we find that CV increases turnout from roughly 80% to 90%. Impacts on turnout vary somewhat across the three types of elections (parliamentary, state, and presidential), but are sizable. Interestingly, however, CV does not appear to affect state-level spending. These "zero effects" are reasonably precisely estimated and robust to different specifications that deal with concerns regarding possible endogenous changes in CV laws.

How could it be that CV had large impacts on turnout, but did not affect policy outcomes? Our analysis shows that despite the large increase in turnout, CV did not affect electoral outcomes: vote shares for liberal parties did not change significantly, nor did the number of parties running for office or the victory margin in state or parliamentary elections.

To complement our main aggregate analysis and dig further into mechanisms, we use repeated cross sections of individual level data to analyze interaction effects of CV laws with voter characteristics. While our statistical power is more limited compared to our main analyses, we find suggestive evidence of larger CV impacts on turnout among women and those with lower income. Impacts also seem larger among those who have low interest in politics, who have no party affiliation, and who are relatively uninformed (as proxied by newspaper reading). While suggestive, these results are consistent with a story where voters who vote or abstain due to the introduction or repeal of CV do not have strong policy or partisan preferences (on average), thereby having little or no effect on electoral outcomes. Furthermore, if such voters are unresponsive to policy in deciding which party to support, parties may have little incentive to shape policies to suit those voters' preferences.

Our paper relates to three main literatures. First, an important literature analyzes how changes in turnout and electorate composition affect public policy (Persson and Tabellini, 2000), often looking at the impacts of enfranchising particular groups of people. For example, the enfranchisement of women in the US led to increases in government health expenditures (Miller, 2008), as did the adoption of electronic voting in Brazil, which effectively enfranchised illiterate voters (Fujiwara, 2015). Similarly, Naidu (2012) shows that post-Civil War laws restricting voting for blacks in the US South had sizable impacts on public policy.[3] Our findings do not contradict this literature, but complement it, suggesting that the extent to which changes in turnout affect policy depends importantly on whether these policies affect a group of the population with specific policy preferences.

Second, our paper speaks to the literature on the determinants of voter turnout. As discussed in Gerber and Green (2012), scholars have analyzed numerous interventions aimed at increasing turnout, often using randomized experiments. In non-experimental studies, a significant literature examines the impact of voting costs, often reaching different results from different changes in costs.[4] For example, Farber (2009) shows that election holidays and "time-off" have little impact on turnout in the US, whereas Brady and McNulty (2011) show that an increase in voting costs (due to unexpected changes in the location of polling stations) reduces turnout. We complement this literature by simultaneously analyzing turnout and government policy.

Two noteworthy recent studies follow this tradition, analyzing how changes in voting costs affect turnout and policy outcomes. Hodler et al. (2015) propose a model of government where higher-skill individuals are more likely to vote. A reduction in voting costs leads to some lower-skill individuals choosing to vote. On one hand, these lower-skill individuals like government spending because a greater share of it is paid for by the rich (Meltzer and Richard, 1981). On the other hand, they invest less in political information than high-skill voters, making them more likely to be impressionable and thereby driven to choose candidates based on lobby-funded campaign spending instead of government spending. To test the model, they study the staggered introduction of postal voting across Swiss cantons. Postal voting led to increased turnout, lower education of participants, lower political information, and lower welfare spending. Godefroy and Henry (2015) analyze the impact of voting cost shocks on the selection of politicians and discretionary expenditures. Using digestive infections as a shock to voting costs, they find that unanticipated increases in voting costs lead to lower turnout, higher candidate quality, and higher infrastructure expenditures in French cities. We discuss differences between our results and these two studies in Section 4.2.

Third, it relates to a small but burgeoning literature analyzing CV. Among a number of theory papers, Börgers (2004) and Krishna and Morgan (2011) argue that CV reduces welfare, whereas Krasa and Polborn (2009) show that compulsory voting (or costly voting) allows an aggregation of preferences that can increase welfare. In empirical work, Funk (2007) finds that abolishing CV significantly decreased turnout in Switzerland despite the fact that fines were small and not enforced. Her results highlight the expressive value of CV, an interpretation that could also apply to our setting, given the low levels of enforcement of the fines. However, this study does not investigate further the effects of changes in turnout on public policy.[5] In a cross-country study, Chong and Olivera (2008) show that countries with CV have lower income inequality. Fowler (2013) exploits the staggered introduction of CV across Australian states, finding that CV led to large increases in turnout. De Leon and Rizzi (2014) analyze students in Brazil, where voting is voluntary between ages 16 and 18, but mandatory afterward. They find that CV increases turnout, but does not affect political information. Using a field experiment in Peru providing information about changes in abstention fines, León (2015) shows that a reduction in the fines decreases turnout, and consistent with our findings, that the reduction is driven by uninformed, uninterested, and centrist voters. However, León (2015) can only analyze *policy preferences* and cannot analyze *actual policies* as our paper does.

[3] Other papers in this literature show mixed results of the extension of the voting franchise on redistributive policies (e.g., Cascio and Washington, 2014; Gradstein and Milanovic, 2004; Husted and Kenny, 1997; Rodriguez, 1999; Timpone, 2005). A common message from this literature is that efforts to extend the voting franchise can significantly affect public policy, making it more aligned with voters' preferences. This literature often analyzes episodes in which groups with specific policy preferences are *de jure* or *de facto* enfranchised, leading elected officials to cater policies toward them.

[4] Weather shocks have been used as exogenous shifts in the cost of voting (e.g., Fraga and Hersh, 2010; Gomez et al., 2007; Hansford and Gomez, 2010; Knack, 1994), as have general rules of governance (Herrera et al., 2014; Hinnerich and Pettersson-Lidbom, 2014), candidates' ethnicity (Washington, 2006), and availability of certain information technology (Enikolopov et al., 2010; Gavazza et al., 2014; Gentzkow, 2006; Gentzkow et al., 2011; Stromberg, 2004). Some of these voting cost shifters are unexpected shocks, whereas others could be anticipated by politicians.

[5] Also focusing on Switzerland, in recent work contemporaneous with our own, Bechtel et al. (2016b) find that CV increases electoral support in referendums for leftist policy positions. Note, however, that Bechtel et al. (2016b) study voters' support for policies as opposed to the behavior/policies of representative government, which we study. Still using Switzerland, Bechtel et al. (2016a) find that CV does not lead to habit formation in voting.

A few political science papers involve the specific case of CV in Austria. The first paper to explore it was Hirczy (1994), who compared overall voting rates between Austrian states over time; the graphical evidence presented suggests that turnout is significantly higher under CV. The paper closest (and contemporaneous) to ours is by Ferwerda (2014), who analyzes how the 1992 federal repeal of CV in Austria affected turnout and party vote shares. Although his analysis period is much shorter, the magnitude of the effects found on electoral participation and party vote shares are broadly consistent with ours.[6,7] Our paper goes beyond these studies in three main ways. First and foremost, not only do we analyze the political consequences of CV, but also impacts on spending, thereby providing the first micro study (for Austria or any other country) to examine how CV affects government spending. Second, we complement the analysis of aggregate data with individual level information on political preferences and voting behavior, allowing us to study the shift in the composition of the pool of voters resulting from CV. Finally, we analyze all elections from 1949 to 2010 instead of just a subset; this enables us to implement a fixed effects analysis allowing for different state linear trends, ruling out the concern that the effects are only valid in the short term and that we should expect a reversion to the mean.

Section 2 provides background on democratic institutions and CV in Austria. Section 3 describes the data. Section 4 discusses our estimation strategy and shows the results. Section 5 analyzes mechanisms for our results. Section 6 concludes and discusses external validity.

2. Institutional background

2.1. Democratic institutions and budgeting processes in Austria

Austria is a federal and parliamentary democracy, composed of nine autonomous states. The National Parliament is composed of two chambers, the National Council (*Nationalrat*) and the Federal Council (*Bundesrat*), with legislative authority vested mostly in the former. National Council members are directly elected by proportional representation, whereas members of the Federal Council are elected by the state legislatures. Austria's executive branch is composed of the Federal President (*Bundespräsident*), the Federal Chancellor (*Bundeskanzler*), and the Federal Cabinet. The Federal President is elected by simple majority in a popular election, and the candidates are nominated by party coalitions. The president holds the mostly ceremonial position of head of state. The Federal Cabinet is composed of the Federal Chancellor (the head of government) and a group of ministers, all of whom are appointed by the president. Austrian states are ruled by their own regional parliament (*Landtag*), a state government (*Landesregierung*), and a governor (*Landeshauptmann*). State parliament representatives are directly elected. Unlike the federal government, state governors are elected by the state parliament.

Over 95% of taxes in Austria are collected at the federal level (Blöndal and Bergvall, 2007). Taxes are distributed across the three levels of government (federal, state, and local) according to Fiscal Equalization Laws, which last for short periods of time (~4 years) and are established by a consensus between the federal and regional governments (Blöndal and Bergvall, 2007). Within the two lower levels of government, tax revenues are distributed across the different units according to a formula, which takes into account demographic and revenue criteria. Federal transfers to state governments are classified into two broad categories: (i) funds earmarked for a precise purpose and (ii) discretionary funds (1948 Constitutional Law, Sections II and III).[8] Throughout our period of analysis, discretionary funds tended to account for about half of the total transfers (Lehner, 1997), giving state governments considerable fiscal autonomy. States' spending autonomy comes across in the substantial variation in state spending percentages on different budget categories.[9] Although the largest portion of tax revenues are allocated to the central government, state governments receive a significant portion of the total budget, and are responsible for providing a wide array of public goods and services. In 2006, for example, spending by state governments accounted for 17% of total spending, with 70% and 13% of spending carried out by the central and municipal governments, respectively (IMF, 2008). State government responsibilities include primary education, regional infrastructure, hospitals, transportation, social welfare, and pensions for state civil servants (IMF, 2008).[10]

In the postwar period, there were two major parties in Austria, as well as several smaller parties. The two major parties were the center-right People's Party (ÖVP) and the center-left Social Democratic Party (SPÖ). In addition, there were the right-wing populist Freedom Party of Austria (FPÖ), the Communist Party of Austria (KPÖ), and the Green Party, as well as the Alliance for the Future of Austria, the Liberal Forum, and others.

2.2. Compulsory voting in Austria

Fig. 1 summarizes the process by which CV was introduced and later repealed in Austria. The mandate to vote was changed a number times during 1949–2010; whether voting was compulsory varied substantially both across and within states, and depending on the type of election, as seen in Fig. 2. CV was first introduced in Austria in the 1929 Constitution. In particular, voting became mandatory for all citizens in presidential elections, but it was up to each state to determine whether voting was mandatory or voluntary in parliamentary and state elections (see Appendix B.1 for further details).

The first presidential election with CV was held in 1951. Up until 1980, there were seven presidential elections, and all of them had CV. However, a 1982 amendment to the Austrian Constitution made voting in presidential elections compulsory only in the states that decided so. In the 1986 presidential elections, the states of Vorarlberg, Tyrol, Styria, and Carinthia decided to keep CV. Furthermore, Carinthia enacted a law establishing CV for parliamentary and state elections. The remaining five states abolished CV in presidential elections after the 1982 amendment.

In 1992, a Federal Constitution amendment withdrew the power of establishing mandatory voting in the national parliament elections from the states (Federal Law Gazette No. 470/1992). Starting in the 1994 parliamentary elections, voting was optional in all states. After this constitutional amendment, the states which still had CV in presidential and state parliament elections started repealing their state laws one by one. In 1993, Carinthia and Styria eliminated CV

[6] Ferwerda (2014) also uses municipal-level data instead of state-level data.

[7] Another contemporaneous paper, Shineman (2014), also uses Austria as a case study to demonstrate the effects of CV on individual-level political sophistication, finding that both recent and long-term exposure to CV increase voters' information.

[8] We use "discretionary" to refer to non-earmarked transfers. From the discussion in Lehner (1997), much of the earmarked funds are meant to be spent on wages, particularly wages of teachers. Earmarked funds are also used for infrastructure expenses, housing subsidies, residential dwelling projects, local transport, disaster control, environmental/agricultural expenses, and health.

[9] During 1980–2012, for example, the government of Burgenland devoted 66% of its budget to welfare expenditures and only 13% to infrastructure spending, whereas the neighboring state of Lower Austria spent 43% of its resources on welfare, and 40% on infrastructure. Section 3 describes the budget categories further.

[10] In some of these areas, the responsibilities of the central and state governments overlap and are thus co-financed or managed jointly (IMF, 2008). There is also overlap of responsibilities between state and municipal governments (IMF, 2008). OECD (1997) notes for Austria that "the revenue-sharing process is rather complex" (pp. 99–100). For further details on state spending, see Appendix C.

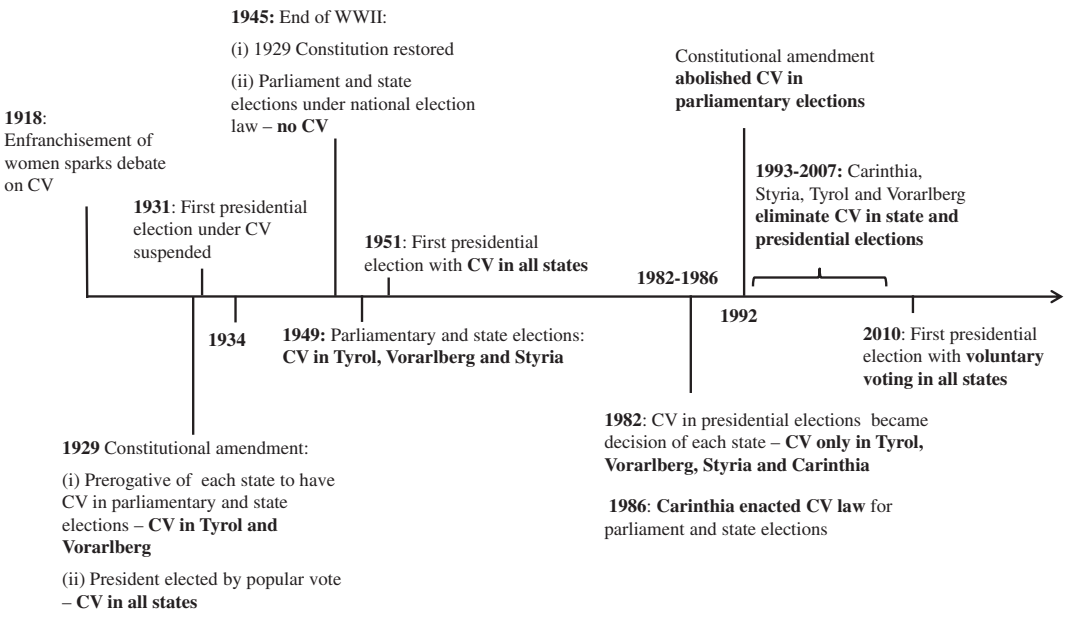
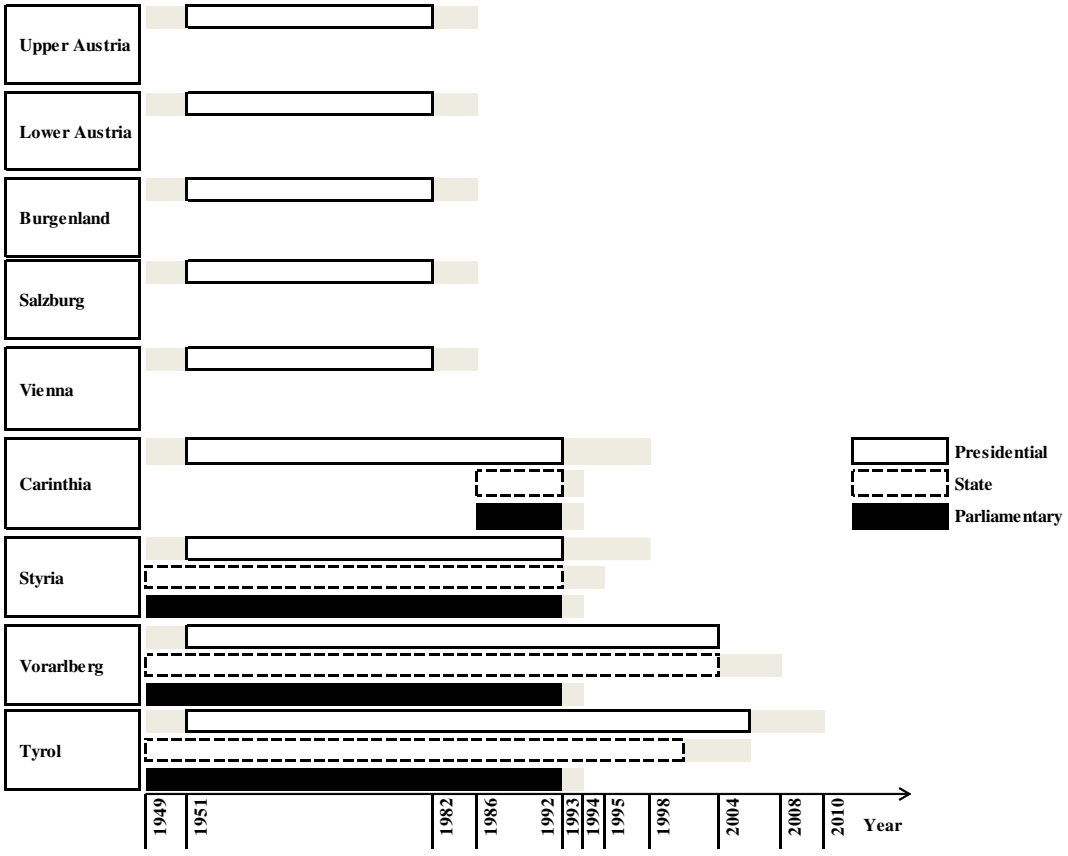


Fig. 1. Evolution of CV laws (1918–2010).

for both types of elections. Tyrol repealed CV for state parliament elections in 2002, and Vorarlberg got rid of it before the 2004 elections. After these elections, Tyrol finally repealed CV for presidential elections. Thus, the 2010 presidential elections (the last presidential elections in our sample) were the first in which voting was voluntary throughout the country.

During the period when voting was compulsory, local authorities were responsible for issuing fines against non-voters failing to



Notes: The legend marks the period in which elections with CV were held for each type of election. Shaded bars indicate that the enactment/abolition of the CV law was already in place, although no elections were held in that period.

Fig. 2. Elections under compulsory and voluntary voting (1949–2010).

provide a reasonable excuse for abstaining.[11] Abstention penalties were extremely rare, as the law allowed for a wide range of excuses for not voting, such as illnesses, professional commitments, urgent family matters, being outside the state during the election, or "other compelling circumstances" due to which the voter could not go to the polls. Importantly, voters who excused themselves were not required to provide documentation justifying their absence.[12] After qualitative work with Austrian citizens and elites, Shineman (2014) concludes that fines rarely had real consequences and were almost never enforced. Appendix B.2 gives additional details supporting that fines were weakly enforced.

3. Data sources and descriptive statistics

In the empirical analysis, we draw upon three main sources of information. First, to analyze how CV affects turnout and political competition, we collected election data. Our initial sample consists of all parliamentary, presidential, and state elections held since the end of World War II until 2010.[13] For these elections, we hand-collected data on voter turnout, invalid ballots, and party vote shares from the Austrian Federal Ministry of the Interior's yearbooks. We define turnout as the share of registered voters who show up to the polls.[14]

Table 1 gives descriptive statistics. Average turnout in our sample is relatively high, ranging from 86% in state elections to 90% in parliamentary elections. The average incidence of invalid ballots in these elections is 2%. We define "right-wing" parties as ÖVP and FPÖ and "left-wing" parties as SPÖ and KPÖ.[15] Both in state and parliamentary elections, the right-wing vote share (52%–53 %) exceeds the left-wing vote share (around 40%).

Second, to analyze spending, we draw upon detailed annual information on expenditures by each of the state governments, which we obtained from the Austrian Statistical Agency. Unfortunately, this information is only available since 1980.[16] The expenditure data include 10 expenditure groupings by year. To simplify exposition and improve statistical precision, we combined the groupings into three broad categories: Administrative, Welfare, and Infrastructure. We define "administrative expenditures" as spending on elected representatives/general administration and finance. We define "welfare Expenditures" as spending on education, sports, and science; health; arts and culture; and social welfare and housing. We define "infrastructure Expenditures" as those for roads/transport,

Table 1
Summary statistics (1949–2012).

	Observations	Mean	Std. Dev	Min	Max
Panel A: Election data (1949–2010)					
Parliamentary elections					
Turnout (%)	171	0.90	0.07	0.70	0.98
Invalid votes (%)	171	0.02	0.01	0.01	0.05
Votes right (%)	171	0.52	0.09	0.22	0.75
Votes left (%)	171	0.40	0.10	0.15	0.62
Votes minor parties (%)	171	0.09	0.09	0.00	0.49
Number of parties	171	6.96	2.94	4.00	13.00
Vote share winner (%)	171	0.47	0.08	0.29	0.65
Margin of victory (%)	171	0.12	0.09	0.00	0.40
State elections					
Turnout (%)	121	0.86	0.09	0.61	0.98
Invalid votes (%)	121	0.02	0.01	0.01	0.08
Votes right (%)	121	0.53	0.11	0.21	0.76
Votes left (%)	121	0.39	0.11	0.10	0.62
Votes minor parties (%)	121	0.07	0.08	0.00	0.50
Number of parties	121	5.98	1.05	4.00	8.00
Vote share winner (%)	121	0.49	0.06	0.36	0.65
Margin of victory (%)	121	0.10	0.07	0.00	0.34
Presidential elections					
Turnout (%)	132	0.88	0.13	0.38	1.00
Invalid votes (%)	135	0.04	0.02	0.01	0.11
Panel B: Yearly state data (1980–2012)					
Unemployment rate (%)	297	0.06	0.02	0.00	0.10
Population (in thousands)	297	890	492	269	1717
Administrative expenditures	**297**	**898.67**	**779.60**	**118.98**	**4303.17**
Representatives and gen. admin	297	423.67	431.13	74.81	2280.97
Finance	297	475.00	485.26	42.20	3699.91
Welfare expenditures	**297**	**1977.86**	**1415.89**	**341.19**	**6916.49**
Education, sports and science	297	636.42	399.04	138.80	1774.97
Social welfare and housing	297	701.01	521.13	106.37	2315.46
Health	297	569.51	548.88	81.82	2977.10
Arts, culture and religion	297	70.92	62.89	9.91	288.36
Infrastructure expenditures	**297**	**763.11**	**1141.22**	**72.16**	**4818.05**
Roads and transport	297	230.64	211.70	47.55	1010.88
Public order and security	297	26.03	39.55	1.21	163.66
Promotion of the economy	297	121.33	64.87	21.13	318.33
Services	297	385.11	933.29	0.98	4055.52

Notes: State-level election data covers all elections held from 1949 to 2010. *Turnout* measures the percentage of registered voters who issued a vote, and *invalid votes* is the proportion of ballots considered invalid. Vote shares for the *right* and *left* are the percentage of valid votes that went to ÖVP+FPÖ and SPÖ+KPÖ, respectively. Votes shares for *minor parties* are the percentage of valid votes received by other smaller parties. The *vote share of the winner* is the percentage of valid votes obtained by the highest ranking party in each state, and *margin of victory* is the difference in vote shares between the highest ranking party and the runner-up. Expenditure, unemployment, and population data at the state-level cover all the years from 1980 to 2012. All state expenditures are expressed in millions of 2010 euros.

[11] Federal Presidential Election Law, Article 23 and Federal Parliament Election Law, Article 105 (4). We provide details on the maximum fine amounts specified in the law and their evolution in different states in Appendix B.2. While there is information on maximum fine amounts in states, we have limited information on actual fine amounts (for the cases where fines were actually enforced), due to the involvement of local authorities in setting actual fines.

[12] Abstainers needed to provide evidence of reasons for not voting only when an administrative penal procedure was initiated against them. Appendix B.2 provides further details on abstention sanctions.

[13] We exclude the 1945 election. This election was unusual in multiple respects, coming just after World War II and banning former Nazis (about one-tenth of the voting age population) who were not allowed to vote until the 1949 election (Bischof and Plasser, 2008) . In the period under consideration, there were 19 parliamentary elections, 12 presidential elections, and around 11–15 state elections in each of the nine Austrian states.

[14] As discussed by Geys (2006), studies of turnout have used different denominators in defining turnout, including voting age population, eligible voters, and registered voters. Geys (2006) argues that there is no single correct measure of turnout, but that using eligible voters is preferred over voting age population when data on eligible voters are available, since it excludes those who are legally forbidden from voting (Geys, 2006, p. 639). For Austria, registration is automatic for all citizens with a permanent residence in the country; thus, "registered voters" and "eligible voters" are approximately equivalent in Austria. In addition, we prefer using registered voters over voting age population because we were only able to gather state-specific voting age population data beginning in 1982. Finally, an advantage of using registered voters is that the data are based on administrative registration records.

[15] Appendix C gives further discussion and details on this grouping.

[16] This restricts our analysis to 10 parliamentary elections, 6 presidential elections, and 6–7 state elections in each state. In Appendix Table A.3, we repeat our main results on turnout, invalid votes, and political competition for the restricted period of 1979–2010, and they are qualitatively similar to those in the main text covering 1949–2010.

security, economy promotion, and services.[17] The yearly expenditure data is expressed in millions of 2010 euros. In the 1980–2012 period, a majority of expenses (54%) were devoted to welfare, while 25% of all resources were spent on administration, and the remaining 21% were devoted to infrastructure. In all of our main specifications, we also include state-specific, time-varying covariates (namely, total population and unemployment rates) obtained from the Austrian Statistical Agency.

Third, to analyze heterogeneity in CV turnout effects according to individual characteristics, we use the Austrian Social Survey (ASS), a nationally representative survey conducted in 1986, 1993, and 2003. The 1993 round did not include questions on turnout, so we exclude it from our analysis. Haller et al. (1987) and Haller et al. (2005) give details on the 1986 and 2003 rounds. The ASS asks respondents standard questions on demographics, socioeconomic status, education, and importantly, it inquires about voting behavior, and political and social preferences. Table 2 shows summary statistics from our individual level data.[18]

3.1. Comparing Austria to other countries

Before providing our results, we seek to provide context by comparing Austria to other countries in terms of political behavior. Fig. 3 compares turnout rates in Austria and other OECD countries. While Austria has high turnout, it is not an extreme outlier and there are a number of other countries with broadly similar turnout levels. While the median turnout in this sample is 75.7%, turnout in Austrian parliamentary (state) elections under voluntary voting is 83.8% (77.5%), which places these elections in the 76th (56th) percentile of the turnout distribution.[19]

Table A.2 compares Austria to other rich countries on three non-turnout measures of political engagement: interest in politics, information acquisition (proxied by newspaper reading), and political party membership. Austrians are politically engaged, but seem broadly similar to other rich countries. In the 2003 ASS, 26% of Austrians reported being "Very Interested in Politics," which is comparable with Switzerland (26.6%) or Germany (21%), but slightly higher than other OECD countries participating in the World Value Survey.[20] While 68.7% of Austrians in the ASS read the newspaper regularly, 74.8% of people from other OECD countries in the World Value Survey report having read the newspaper the week before they were surveyed. The level of information acquisition in Austria is below countries like Switzerland (91.3%), Sweden (94.5%), and Japan (88.8%), and only above less developed democracies like Hungary (56.8%), Poland (55.1%), and Spain (62.7%). Likewise, 12.4% of Austrians are members of a political party, comparable to 13.4% of respondents in other OECD countries. These statistics provide reassurance that our results seem unlikely to be due to an unusual institutional context or by political behaviors that are highly specific to Austria.

Table 2
Descriptive statistics: 1986 and 2003 Austrian Social Survey.

	Observations	Mean	Std. Dev	Min	Max
Turnout					
Voted in last parliamentary elections (%)	3726	0.87	0.34	0.00	1.00
Political party of preference					
Left (%)	3670	0.29	0.45	0.00	1.00
Right (%)	3670	0.34	0.47	0.00	1.00
Minor parties (%)	3670	0.07	0.25	0.00	1.00
No party preference (%)	3670	0.30	0.46	0.00	1.00
Not member of a political party (%)	3693	0.82	0.39	0.00	1.00
Interest in politics and Informedness					
Uninterested in politics (%)	3723	0.36	0.48	0.00	1.00
Mildly interested in politics (%)	3723	0.40	0.49	0.00	1.00
Very interested in politics (%)	3723	0.24	0.43	0.00	1.00
Does not read newspaper regularly	3705	0.31	0.46	0.00	1.00
Socioeconomic variables					
Age	3726	46.04	16.71	18.00	92.00
Female	3726	0.59	0.49	0.00	1.00
Household income (in 2003 euros)	2918	1797.95	963.54	180.00	4341.90
Number of members in household	3726	2.85	1.57	1.00	9.00
Employed (%)	3726	0.49	0.50	0.00	1.00
Unemployed (%)	3726	0.03	0.17	0.00	1.00
Retired (%)	3726	0.27	0.44	0.00	1.00
Educational attainment					
Compulsory schooling (%)	3726	0.65	0.48	0.00	1.00
Vocational middle school (%)	3726	0.13	0.34	0.00	1.00
High school (%)	3726	0.16	0.37	0.00	1.00
College (%)	3726	0.06	0.24	0.00	1.00

Notes: The sample includes all individuals in the 1986 and 2003 Austrian Social Survey who reported whether they voted in the last parliamentary elections (1983 and 2002) and were of voting age. *Political party of preference* specifies the party the respondent identifies with (left if SPÖ or KPÖ, right if ÖVP or FPÖ, no party preference if the individual does not identify with any party, and minor parties otherwise), and not a member of a political party is a dummy for whether the individual has no party affiliation. Individuals are separated into three categories according to whether they manifest to be uninterested, mildly or very interested in politics. The Austrian Social Survey separates household income into 21 different categories. To make the figures comparable across periods, we imputed household income as the mid-point of the category into which individuals fell, and converted the 1986 mid-point into 2003 euros. Educational variables are mutually exclusive dummies for the maximum educational attainment. The educational category of "Vocational middle school" corresponds with "berufsbildende mittlere Schule" (or "bms") in the data.

4. Empirical strategy and results

We estimate the effect of CV laws (in different elections) on turnout, invalid ballots, electoral outcomes, and state-level public spending. Using a difference-in-difference model, we compare states with and without CV at different points in time. Our baseline specification is:

$$y_{st} = \alpha_0 + \beta_1 CV_{st} + X_{st}\beta_2 + \delta_s + \nu_t + \gamma_{st} + \epsilon_{st}$$

[17] Table A.1 provides a detailed description of expenditure areas falling into each of the 10 groupings. Our conclusions are substantively unchanged if we analyze the 10 groupings individually.

[18] Our sample restricts to individuals (i) who were of voting age in the previous parliamentary elections and (ii) who reported whether or not they voted in the previous parliamentary elections. Only 3% of people failed to provide information about whether they voted, and missingness is not correlated with whether there is CV in their state. In the 1980s, people were allowed to vote if they were 19 or older, while the voting age was lowered to 18 in 1992 (Kritzinger et al., 2013) . For analysis of the ASS data, we do not restrict the sample based on voter eligibility because information on voter eligibility is absent from the 2003 wave. Thus, the turnout denominator (voting age population, which is 19 or older in the 1986 round and 18 or older in the 2003 round) in our analysis of the ASS data (presented later in Table 6) differs slightly from the one used in our main turnout results (Table 3). Our main focus in the heterogeneity analysis is on differences across subgroups, so this should not be a central concern. Further, the Table 3 results are similar whether registered voters or voting age population is used as the denominator.

[19] The percentile numbers are calculated relative to elections with voluntary voting between 1979 and 2010 for the OECD countries in Fig. 3. Being in the 76th percentile means that average turnout in Austrian parliamentary elections is higher than turnout in 76% of OECD elections. The state election percentile should be taken with some care because it reflects a comparison of state elections in Austria to national elections in other countries. In Fig. 3, turnout is defined as a percentage of registered voters.

[20] Note that Austria did not participate in the WVS, so the numbers are not strictly comparable, but they do give us a broad sense of how Austrians' political opinions compare to those in other countries.

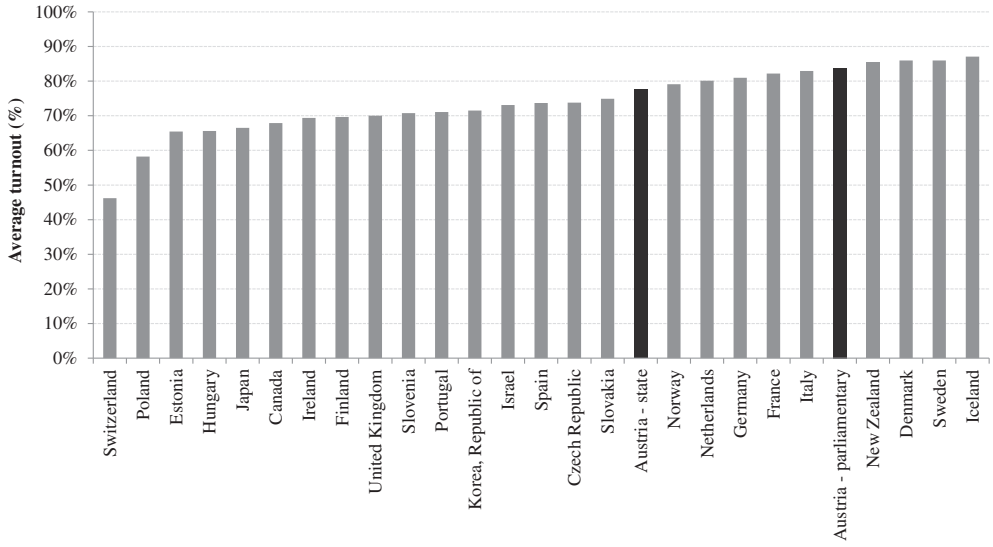


Fig. 3. Average turnout in OECD countries with voluntary voting (1979–2010).
Notes: Average turnout (as a % of registered voters) for all elections held in 1979–2010 in OECD countries with voluntary voting. We consider parliamentary elections in Canada, Czech Republic, Denmark, Estonia, Finland, Germany, Hungary, Iceland, Ireland, Israel, Italy, Japan, Netherlands, New Zealand, Norway, Poland, Portugal, Slovakia, Slovenia, Spain, Sweden, Switzerland, and the United Kingdom, and presidential elections in France and South Korea. In the case of Austria, we display the average turnout in parliamentary and state elections considering only elections in which voting was voluntary. Voter registration is generally compulsory and/or automatic (broadly speaking, e.g., it may be broadly opt out) in these countries, making turnout as a % of registered voters comparable to turnout as a % of the voting age population (VAP). Details about each of the countries' voter registration procedures are provided by the ACE Electoral Knowledge Network at http://aceproject.org/epic-en/research/VR/spreadsheet_country. We have excluded the United States, where registration is not automatic or mandatory. Canada created a new voter registration system in 1997, and individuals can opt out of the national register (see "Description of the National Register of Electors" from Elections Canada). For Ireland, the ACE database does not list registration as automatic; however, Pew Research lists 2016 turnout as 58% among the VAP and 65% among registered voters, indicating a high registration rate. Data on turnout in OECD countries was obtained from the Institute for Democracy and Electoral Assistance. While France is listed on the ACE Electoral Knowledge Network as having compulsory voter registration, Powell Jr (1986) lists France as not having compulsory registration during the 1970s, though registration was still high in the 1970s at 91% (Powell Jr, 1986, p. 38). Italy had CV until 1993 (www.idea.int/vt/compulsory_voting.cfm, accessed July 30, 2016), but we use data from all Italian elections in 1979–2010.

where y_{st} is an outcome variable in state s and year/election t;[21] CV_{st} is a dummy for whether voting was compulsory in year/election t and state s; X_{st} is a vector of state–year covariates (population and the unemployment rate); δ_s are state fixed effects; ν_t are year fixed effects; γ_{st} are state-specific linear trends (at the election level); and ϵ_{st} is an error. We run these regressions separately for different types of elections (parliamentary, state, and presidential). We also do a pooled specification including the three types of elections together — when we do this, we also include election type dummies, as well as all three sets of state-specific trends.

We allow for arbitrary within-state correlation of the errors by clustering our standard errors at the state level. Given the small number of clusters, our standard errors might be inconsistently estimated (Bertrand et al., 2004) . Following Cameron et al. (2008), we also report wild-bootstrap p-values.[22]

In these specifications, our "treatment group" are statesXelectoral periods subject to CV, while the "control group" comprises those in which there is voluntary voting (VV). At any given period in time, we compare states under CV vs. VV (leveraging the time fixed effects), and at the same time, we make within-state comparisons, comparing electoral terms with and without CV (using state fixed effects). Using state level data, we analyze the effect of CV on: (i) turnout and valid ballots; (ii) left/right vote shares, number of parties, vote shares and margin of victory of the winning party; and (iii) government expenditures in social services, administration, and infrastructure. For (i) and (ii), the unit of analysis is state×election; when analyzing the impact of CV on expenditures, the unit of analysis is a state×year.

4.1. Turnout and invalid votes

Even with weak enforcement, as is the case for Austria, CV can affect turnout through the signaling value of enacting a law, as argued in Funk (2007). Panel A in Table 3 shows the effects of CV on turnout within and across Austrian states in the 1949–2010 period. The introduction of CV causes statistically and economically significant increases in turnout in parliamentary, state, and presidential elections.

When independently considering each type of election, we find that CV increases turnout by 6.5 percentage points in parliamentary elections, by 17.2 percentage points in state elections, and by 9.5 percentage points in presidential elections. However, we gain additional power by pooling all types of elections together, as doing so allows more precise estimation of the year and state fixed effects. In column 4 of Panel A in Table 3, we pool the three types of elections together, and analyze the impact of CV on each type of election (our preferred specification). CV now increases turnout by 6.6, 8.1, and 9.1 percentage points for parliamentary, state, and presidential elections, respectively. Note that these results show slightly lower point estimates than in the previous regressions, and this is particularly the case for state elections, for which we have a smaller sample size. The results are highly significant based on standard errors clustered

[21] For the case of government expenditures, the timing of the dependent variable is slightly different. We analyze spending as a function of CV in the current "electoral period" with a one year lag. We define an electoral period as lasting from the year of an election until the year before the next election. Thus, for example, if elections takes place in years t and $t+4$, we consider expenditures in years $t+1$ to $t+4$ as a function of CV in year t. The idea is that the potential impact of CV on spending may come through elections, and we want to allow an election to take place in order to allow the possible consequences of CV on spending to occur. Also, for our spending analysis, the state-specific trends are by year instead of by election.

[22] We calculated the wild-bootstrap p-values using the *cgmwildboot* program created by Judson Caskey, and imposed the null hypothesis, as recommended by Cameron et al. (2008).

Table 3
Effect of CV on turnout and invalid votes (1949–2010).

	Parliamentary	State	Presidential	Pooled
Panel A: Turnout (%) as dependent variable				
CV	0.065**	0.172**	0.095***	
	(0.020)	(0.059)	(0.022)	
	[0.002]	[0.000]	[0.012]	
CV*Parliamentary				0.066**
				(0.021)
				[0.006]
CV*State				0.081**
				(0.024)
				[0.000]
CV*Presidential				0.091***
				(0.021)
				[0.006]
Observations	171	121	132	424
R squared	0.956	0.953	0.961	0.934
Mean turnout (if CV=0)	0.879	0.833	0.763	0.844
Panel B: Invalid votes (%) as dependent variable				
CV	0.005	0.019*	0.019**	
	(0.004)	(0.008)	(0.007)	
	[0.200]	[0.000]	[0.000]	
CV*Parliamentary				0.009***
				(0.002)
				[0.018]
CV*State				0.013***
				(0.003)
				[0.054]
CV*Presidential				0.018***
				(0.005)
				[0.000]
Observations	171	121	135	427
R squared	0.787	0.834	0.835	0.831
Mean invalid votes (if CV=0)	0.015	0.018	0.038	0.020

Notes: Standard errors clustered by state are in parentheses, and cluster-robust wild-bootstrap *p*-values in square brackets. All regressions control for state population and unemployment rate, and include state fixed effects, year fixed effects, and state-specific linear trends (at the election level). An observation in these regressions is a state-election. The sample in columns (1)–(3) includes all parliamentary, state, and presidential elections from 1949 to 2010, respectively. The pooled regressions in column (4) include all three types of elections from 1949 to 2010, with their corresponding election type dummies. In column (4), we include three different election-numbered state linear trends. *Turnout* measures the percentage of registered voters who issued a vote, and *invalid votes* is the proportion of ballots considered invalid. *CV* is a dummy for whether voting was mandatory in the state and election. For three of the presidential elections, turnout in the data is listed at over 100%, so they are excluded. Results are qualitatively robust to including these points.

* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

by state (in parentheses) or based on wild bootstrap *p*-values (in brackets).[23]

CV can increase turnout by drawing uninterested voters, or those who might not be familiar with the voting process. If this is the case, we might expect the proportion of invalid ballots to rise. As shown in Panel B of Table 3, the increase in turnout from CV is paired with a statistically significant increase in invalid votes. In elections without CV, the share of invalid votes ranges between 1.5% and 3.8%. Based on the results in the preferred specification (column 4), CV increases the share of invalid votes by 0.9–1.8 percentage points, depending on the type of election. Even though the increase in turnout associated with CV is also conducive to a higher proportion of invalid votes, there is certainly not a one-to-one relation. That is, for every 10 people who are driven to vote due to CV, only 1.5–3 of them issue an invalid ballot, while the others correctly vote for a party or candidate. Hence, an increase in turnout of this magnitude could very well result in a shift in election results and public policies.[24]

4.2. Public spending

An increase in participation rates could potentially affect government spending in multiple ways. If preferences for public goods in the participating electorate are now different, the government might change the distribution of public spending, keeping the size of the overall budget constant, but shifting it between sectors. Alternatively, the overall size of the budget could change by pushing the local government or local parliamentarians to change deficits or to negotiate larger budgets from the federal government.

As discussed in Section 2.1, given the ceremonial role of the federal president in Austria, we do not expect to see effects of CV in presidential elections on spending — as a placebo test, we run regressions estimating the effect of CV in presidential elections on fiscal behavior at the state level, and as expected, we do not find any effects (Appendix Table A.4). On the other hand, the national parliament decides on the resources that each state government gets. In addition, state parliaments are in charge of preparing the state's budget, and thus laws that affect this level of government may also affect spending. In this section, we turn our attention to the effects of CV on fiscal policy at the state level.

In the subsequent analysis, we study total state expenditures, as well as their composition: administrative, welfare, and infrastructure expenditures. For each spending category, we analyze three different measures of fiscal policy, which are intended to capture the different mechanisms described above: (i) the log levels, (ii) the log per-capita, and (iii) as a percentage of the total budget. We use a similar estimation framework as in Section 4.1.

Table 4 shows no consistent evidence of CV affecting the amount or composition of public spending. Most coefficients are close to zero, and the clustered standard errors as well as the wild-bootstrap *p*-values indicate that there is no significant relationship between CV and total budget or its composition. Across the 12 regressions in Panel A of Table 4, the coefficients are sometimes positive and sometimes negative, but small in magnitude. They are also relatively precise. For example, the estimated coefficient in column 1 on total spending corresponds to a 95% confidence interval of $[-0.009, 0.071]$, meaning we can reject that CV decreases total spending by more than 0.9% or that it increases it by more than about 7.1%. Similarly, the point estimates on administrative, welfare, and infrastructure spending are relatively close to zero, at 0.6%, 3.5%, and 6.6%, respectively.[25] In contrast, electronic voting in Brazil (Fujiwara, 2015, p. 452) and US women's suffrage (Miller, 2008, p. 1289) are estimated to have each raised health spending by about one-third.

Our result on public spending contrasts with those of Hodler et al. (2015) and Godefroy and Henry (2015). Consistent with a model where decreases in voting costs increase the share of voters who are uninformed, Hodler et al. (2015) find that Swiss postal voting decreased welfare spending by 4–7 % and business taxation by 3–7 %. Why might our result differ? First, there are various

[23] Note that the wild bootstrap procedure does not deliver bootstrapped standard errors, but rather *p*-values. The *p*-values found using the clustered standard errors and the wild bootstrap procedure are generally similar in most cases. Throughout the paper, for ease of exposition, our interpretation of confidence intervals is based on the clustered standard errors.

[24] Given that the analysis of the effects of CV on fiscal behavior is performed only for those years for which expenditure data is available (1979–2010), we re-run the analysis from Table 3 on a comparable sample in Appendix Table A.3. The results shown for turnout and invalid votes are comparable to the ones for 1949–2012.

[25] Besides using the 3 broad expenditure categories, we also do the analysis using the 10 more disaggregated groupings in Appendix Tables A.5 and A.6. Given the more granular level of the data, some of the confidence intervals are relatively large (e.g., for finance and service expenditures). However, the conclusions are qualitatively robust, with coefficients mostly statistically insignificant and close to zero. Further, our zero results are also qualitatively robust under an instrumental variable approach, where we estimate the effect of turnout (instrumented by CV) on expenditures (Appendix Table A.7).

Table 4
Effect of CV on expenditures (1980–2012).

	Log-levels				Log-per capita				% of Total		
	Total	Admin.	Welfare	Infrast.	Total	Admin.	Welfare	Infrast.	Admin.	Welfare	Infrast.
Panel A: Parliamentary elections											
CV	0.031	0.006	0.035	0.066	0.008	−0.018	0.011	0.043	−0.008	0.001	0.007
	(0.017)	(0.112)	(0.034)	(0.097)	(0.037)	(0.121)	(0.048)	(0.088)	(0.027)	(0.025)	(0.015)
	[0.124]	[0.893]	[0.284]	[0.464]	[0.883]	[0.909]	[0.737]	[0.609]	[0.757]	[0.981]	[0.659]
R squared	0.991	0.949	0.990	0.979	0.930	0.792	0.895	0.942	0.590	0.791	0.875
Panel B: State elections											
CV	−0.019	−0.168	0.033	−0.029	0.023	−0.126	0.075	0.013	−0.040*	0.034	0.006
	(0.037)	(0.104)	(0.036)	(0.101)	(0.058)	(0.116)	(0.059)	(0.088)	(0.021)	(0.022)	(0.017)
	[0.633]	[0.096]	[0.324]	[0.775]	[0.649]	[0.396]	[0.202]	[0.825]	[0.040]	[0.130]	[0.701]
R squared	0.991	0.950	0.990	0.979	0.930	0.795	0.897	0.942	0.601	0.796	0.875
Observations	297	297	297	297	297	297	297	297	297	297	297
Mean dep. variable	7.878	6.449	7.342	5.937	−5.649	−7.078	−6.184	−7.590	0.248	0.592	0.160
p-Value (equality across election types)	0.316	0.304	0.956	0.581	0.870	0.585	0.478	0.836	0.360	0.260	0.965

Notes: Standard errors clustered by state are in parentheses, and cluster-robust wild-bootstrap *p*-values in square brackets. All regressions control for state population and unemployment rate, and include state fixed effects, year fixed effects, and state-specific linear trends (at the year level). An observation in these regressions is a state–year, and the sample is composed of all the years from 1980 to 2012. The dependent variable is yearly real spending by the state government, and the independent variable in Panels A and B is whether voting in the corresponding electoral period was compulsory for parliamentary or state elections, respectively. Spending totals and spending for each subcategory (administrative, welfare, and infrastructure) is in logs in columns (1)–(4) and in per capita logs in columns (5)–(8). Yearly spending for each subcategory is expressed as a percentage of total state spending in columns (9)–(11). **Significant at 5%; ***Significant at 1%.

* Significant at 10%.

political differences between Switzerland and Austria, including that Switzerland has much lower turnout (though other levels of political involvement and interest do not seem so different). For example, following the idea of the model of Hodler et al. (2015), it could be that there are few "impressionable" voters for politicians to take advantage of in Austria, and this could limit whether there are impacts on spending. Second, postal voting and CV may have different impacts on a political system. It is not clear that the population of voters who would respond to CV are the same as those would respond to postal voting. Even if both populations seem more uninformed on average, they may differ on unobservables. We note, though, that these explanations are speculative and cannot fully resolve why there are differences across papers. Further research is needed.[26]

Godefroy and Henry (2015) show that a decrease in turnout in French cities due to more digestive infections decreases harmful financial decisions, and leads to higher subsidies obtained by the municipality and more infrastructure expenditure, and they argue that this is due to the selection of better qualified politicians. In our paper, it is difficult to test this hypothesis directly because we do not observe direct information on the quality of politicians (such as whether a mayor contracted a toxic loan, which is observed by Godefroy and Henry, 2015). In addition, in contrast to us, Godefroy and Henry (2015) study unanticipated, unknown-in-advance shocks to voting cost.

4.3. Robustness checks

The identification assumption in our main regressions is that CV is uncorrelated with unobserved time-varying state characteristics once we have controlled for time invariant, state-specific factors, as well as year-specific, state invariant factors, and partialled out state-specific linear time trends. For example, if conservative states are more likely to support CV, this should be absorbed by our state fixed effects. On the other hand, if there is a national push for abolishing these types of laws (e.g., in 1982), this would be captured by the year fixed effects. One threat to our identification assumption is that, even though some of the changes in CV laws were issued at the federal level (e.g., the 1992 repeal of CV in parliamentary elections), and thus are unlikely to respond to state-specific political dynamics, others changes were issued at the state level, and these decisions might be related to voting trends. As in any difference-in-difference model, this is the same as assuming that, conditional on the set of observables and fixed effects, the trends in voting, political competition, and expenditures in states in which CV was introduced were the same as in states where voluntary voting was in place; that is, if the new voting regime had not been enacted, there would have been parallel trends in the pre-treatment period.

The parallel trends assumption would be violated if the states most likely to implement CV were those in which turnout was downward trending. In this case, an estimation relying on simple fixed effects will understate the effect of CV laws. Similarly, state governments might find it easier to enact CV laws when turnout is trending upward, since enforcement costs will be lower in these states. In this case, a fixed effects model would overestimate the results. The inclusion of state-specific time trends controls for any linear trends in our outcome variables, and thus partially addresses these concerns, but further tests are needed.

As mentioned in Section 2.2, in our study period there is one change in CV laws that is unrelated to any state–year specific characteristic, namely, the one introduced by the federal government in 1992.[27] This Federal Constitution amendment withdrew the prerogative of establishing mandatory voting in the national parliament elections from the states. Effectively, while some states already had voluntary voting in parliamentary elections, others (Vorarlberg, Styria, Tyrol and Carinthia) were forced to adopt it. Fig. 4 shows the evolution of turnout, invalid votes, and total, administrative, welfare, and infrastructure expenditures in the same analysis period (1986–2011), for states that never had CV and those that were mandated to eliminate it in 1992. States that had CV before 1992 had higher turnout and more invalid ballots, but importantly, before CV is abolished, the trends in these variables run parallel to the ones

[26] There are a number of other potentially significant political differences between Switzerland and Austria. These include that Swiss cantons seem more powerful than Austrian states (e.g., unlike Swiss cantons, Austrian states do not have significant tax powers; (Fuentes et al., 2006)); that Austria is more linguistically homogeneous; and that Switzerland and Austria have different historical experiences with democracy.

[27] Ferwerda (2014) uses this federal change to explore impacts on party vote shares; he argues that, given that it was issued at the federal level, it is independent of political dynamics at the local level.

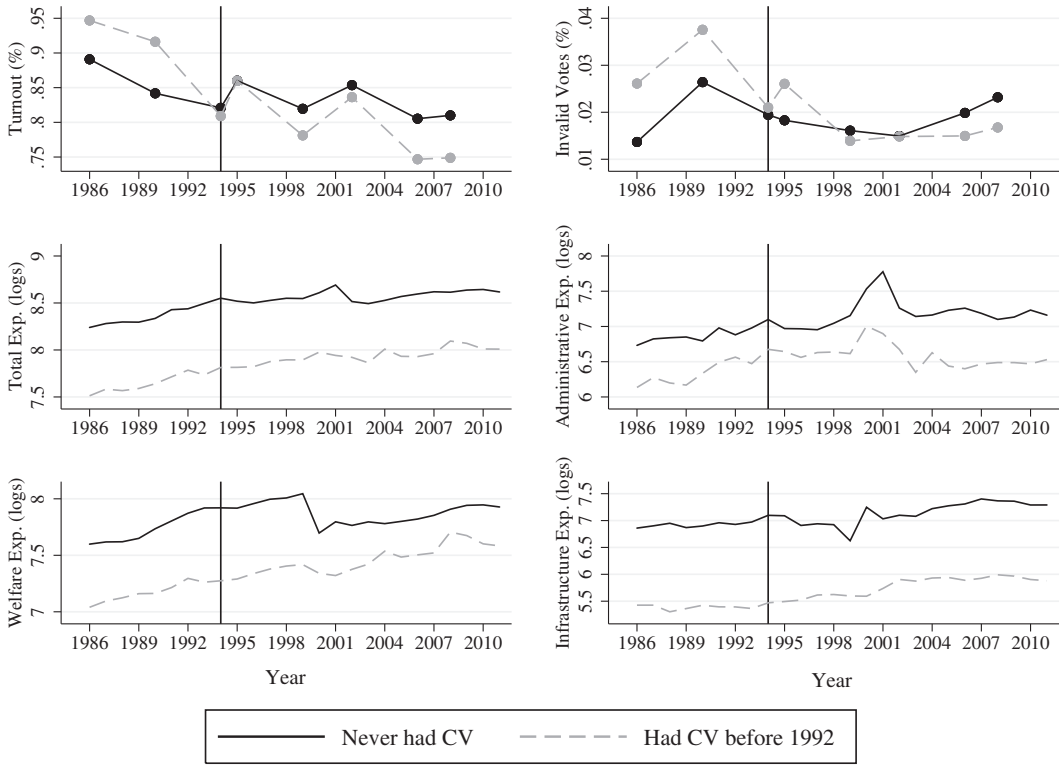


Fig. 4. Turnout, invalid votes and expenditures (1986–2012).
Notes: Turnout measures the percentage of registered voters who issued a vote, and invalid votes is the proportion of votes considered invalid. All state expenditures are expressed in millions of 2010 euros, in logs. The figures for turnout and invalid votes cover the parliamentary elections held in 1986–2008, and the figures for expenditures cover all the years in 1986–2012.

in states that did not have CV before 1992. Similarly, in our four expenditure variables, for which we do not observe an effect of the elimination of CV, the trends for both types of states run parallel during the whole study period.[28]

To further alleviate the concern that CV laws might have been introduced responding to changes in our dependent variables of interest, we include leads and lags of CV in our regressions. If it were the case that CV laws responded to changes in turnout, we would expect turnout in period t to be correlated to either CV in $t+1$ or CV in $t-1$. The results for our preferred specification in Table A.9 show that, besides the contemporaneous effect of CV on turnout and invalid votes, the introduction of CV in the previous election or next electoral period has no effect on our variables of interest. The estimated effects for the three types of elections generally show a "zero" of the lags and leads of our independent variable, i.e., there were no pre-trends, or anticipation effects. (One exception is the surprising significance of the coefficient of the lead of CV for presidential elections.)

A potential concern is that authorities anticipate the introduction/repeal of CV laws and alter the level or composition of public spending before the law change takes place. If this were the case, we would observe a correlation between public spending in year t and CV in $t+1$. Alternatively, any delays in the reaction of public spending to changes in CV laws would lead to a correlation between CV in $t-1$ and public spending in year t, which would not be captured in our baseline specification. As seen in Table A.10, spending is uncorrelated with CV in the past, current, or future electoral period for all types of elections.

Together, Fig. 4 and Tables A.8–A.10 provide evidence supporting the parallel trend assumption, and help rule out potential reverse causation between turnout and CV. As an additional robustness check, in Appendix C, we discuss heterogeneity in our spending (and turnout) results according to levels of turnout in different states.

5. Understanding the "null effect" on policy outcomes

How could it be that CV had sizable impacts on turnout, increasing the number of valid votes, but did not affect policy outcomes? One potential explanation for these results is that the political choices of people who turn out because of CV are, on average, similar to the ones of people who would have voted even in the absence of CV. Another explanation is that median voter preferences may have changed, potentially leading to changes in electoral outcomes, but government spending still does not change for some other reason (e.g., commitment or agency issues).[29]

Besides exploring electoral outcomes, we also attempt to shed light on identifying the voters affected by CV. Several recent studies analyze large increases in turnout due to *de jure* or *de facto* enfranchisement of specific groups of the electorate, such as women

[28] We also perform a difference-in-difference regression limiting our sample to the parliamentary elections in the electoral periods between 1986 and 2011. The magnitude and statistical significance of the results is similar to those shown in Tables 3 and 4, suggesting that any other changes in CV (besides the 1992 one) are unlikely to be correlated with trends in the main dependent variables. For further details turn to Table A.8 and Appendix C.

[29] For example, in citizen candidate models (Besley and Coate, 1997; Osborne and Silvinski, 1996), politicians may implement preferred policies that may differ from those of the median voter. In empirical work, Lee et al. (2004) (building on the model of Alesina, 1988) show that exogenous changes in party electoral strength do not affect the voting patterns of US congressmen. Another possibility is that uninformed or uninterested voters may have different preferences, but may be less responsive to policy than other voters; in such a scenario, politicians may have little incentive to change their policies.

Table 5
Effect of CV on electoral outcomes (1949–2010).

	Left (%)	Right (%)	Minor parties (%)	Num. of parties	Share winner (%)	Margin of victory
Panel A: Parliamentary elections						
CV	−0.014	0.018	−0.003	0.413	0.020	0.040
	(0.009)	(0.017)	(0.018)	(0.336)	(0.017)	(0.026)
	[0.130]	[0.468]	[0.845]	[0.312]	[0.296]	[0.118]
Observations	171	171	171	171	171	171
R squared	0.984	0.922	0.902	0.965	0.886	0.765
Mean dep. variable	0.400	0.515	0.085	6.959	0.465	0.120
Panel B: State elections						
CV	0.035	0.001	−0.036	−0.052	0.014	0.004
	(0.046)	(0.038)	(0.043)	(0.514)	(0.036)	(0.044)
	[0.623]	[0.977]	[0.352]	[0.915]	[0.643]	[0.891]
Observations	121	121	121	121	121	121
R squared	0.967	0.915	0.848	0.823	0.870	0.790
Mean dep. variable	0.393	0.535	0.072	5.975	0.494	0.101
p-Value (equality across election types)	0.210	0.720	0.491	0.368	0.881	0.551

Notes: Standard errors clustered by state are in parentheses, and cluster-robust wild-bootstrap *p*-values in square brackets. All regressions control for state population and unemployment rate, and include state fixed effects, year fixed effects, and state-specific linear trends (at the election level). In column (4), we include three different election-numbered state linear trends. An observation in these regressions is a state-election. The sample in Panels A and B includes all parliamentary and state elections from 1949 to 2010, respectively. Vote shares for the *left* and *right* are the percentage of valid votes that went to SPÖ+KPÖ and ÖVP+FPÖ, respectively. Vote shares for *minor parties* are the percentage of valid votes received by other smaller parties. *Number of parties* is the number of parties participating in each election and state. The *vote share of the winner* is the percentage of valid votes obtained by the highest-ranking party in each state, and *margin of victory* is the difference in vote shares between the highest-ranking party and the runner-up. *CV* is a dummy for whether voting was mandatory in the state in that particular election.

in Miller (2008); the poor and illiterate in Fujiwara (2015); and African-Americans in Naidu (2012). Unlike these studies, we do not necessarily have a strong prior that people who vote because of CV make significantly different political choices than those who vote even when voting is voluntary.

5.1. Electoral outcomes

Table 5 examines whether CV affects various electoral outcomes. We estimate similar regressions as in Section 4.1, but use as dependent variables the percentage of votes to the left or right wing parties; the number of parties; and the share of votes of the winning party and its margin of victory (i.e., the difference in vote share between the winning party and the runner-up). For both parliamentary and state elections, CV does not affect the share of votes going to the right or left wing parties.[30] Further, there is no response from the political supply: the number of parties remains constant at about 6.9 and 6 for parliamentary and state elections, respectively. Finally, the party that wins the election does not receive a significantly different proportion of votes under CV, compared to states and elections in which voting is voluntary.[31]

In a related paper, Ferwerda (2014) exploits the 1992 constitutional change to analyze the effects of political participation on electoral results. Using municipality level data from 1990 and 1994 (before and after the change), he finds modest impacts of the change on electoral outcomes. He finds a slight increase in votes for the left-wing SPÖ, coupled with a slight decrease in votes for minor parties, but his results are broadly similar to ours.

Overall, in Table 5, CV does not significantly affect party vote shares, the number of parties, or margin of victory. Under models with commitment issues, we might have expected CV to affect electoral outcomes, but we do not observe that in the data.

5.2. Composition of the electorate

We use individual data from two rounds of the ASS (1986 and 2003) to examine what type of voters were most affected by CV. The goal is to better understand the mechanisms underlying our main results. We have information on turnout in the previous parliamentary election (1983 and 2002), and exploit within and between state variation in CV introduced by the federal abolition of CV between the surveys (in 1992). While no states had CV in the 2002 parliamentary election, 3 states (Styria, Tyrol, and Vorarlberg) had it in the 1983 elections. Our dependent variable is whether an individual voted in the previous parliamentary election, and our main regressor is a dummy for whether voting was compulsory in that election in the state where the respondent lives. We control for a set of individual covariates, as well as state and survey year fixed effects.[32] To examine what type of voters are more likely to respond to CV, we interact CV with various individual characteristics.[33]

Table 6 shows 8 separate regressions. In the left panel, we observe that impacts of CV seem to be larger among females, those with a vocational middle school degree, and those in the lower two income quartiles.[34] In this table, the wild bootstrap *p*-values are larger than the implied *p*-values from the clustered standard errors, making the inference here more suggestive. In the right panel, we observe that the impact of CV is larger among people who are not party members,

[30] We do not perform these regressions for presidential elections because parties do not run as separate entities in those races. Additionally, we find no effects on individual party vote shares or voter polarization. See Appendix C for details. Somewhat relatedly in the literature, Martinez and Gill (2005) examine how turnout affects Democrat vote share in US presidential elections.

[31] Results for the 1979–2010 period shown in Appendix Table A.11 are comparable to those in Table 5. We find relatively larger point estimates for the effects of CV on party vote shares, but they are not statistically significant. Only for the case of votes for the left wing parties in parliamentary elections do we find a statistically significant impact of CV; however, the effect of CV in this case is relatively small.

[32] Controls included in all regressions in this section include: age, age squared, gender, educational attainment, parents' education, working status, household size, community size, party preferences, party membership, interest in politics, and information acquisition (read newspaper regularly).

[33] Although the ASS asks about which party respondents voted for, we do not analyze this outcome because 17% of our sample did not answer this question, and attrition is differential along individuals' self-reported political preferences.

[34] In regression 1, we examine the impact of CV on turnout without interaction effects. CV increases turnout by 5.5 percentage points, slightly lower than the effect in the aggregate, state-level data. We must bear in mind, however, that these regressions rely on self-reported data, which might measure turnout with error. A standard concern could be that people might lie about whether they voted. We take comfort from the fact that the self-reported voting shares of 92% and 82% in 1983 and 2002 are relatively close to the actual voting shares in our data (93% and 85%, respectively). The voting shares are not directly comparable across datasets because our administrative turnout measure uses registered voters as the denominator, whereas the survey implicitly uses voting age population as the denominator (see footnote 18), but the general similarity suggests that misrepresentation seems unlikely to be a central issue in the data.

Table 6
Individual-level impact of CV on turnout: heterogeneity by voter characteristics.

Dependent variable: Voted in last parliamentary elections			
1) Effect of CV on turnout		5) Party membership	
CV	0.055*	Not party member*CV	0.066*
	(0.028)		(0.029)
	[0.209]		[0.235]
		Party member*CV	0.011
			(0.028)
			[0.768]
2) Gender		6) Informed. vs uninformed citizens	
Female*CV	0.075*	Uninformed*CV	0.087**
	(0.035)		(0.033)
	[0.217]		[0.311]
Male*CV	0.027	Informed*CV	0.043
	(0.019)		(0.027)
	[0.255]		[0.225]
3) Educational attainment		7) Interest in politics	
Compulsory schooling*CV	0.049	Uninterested*CV	0.082*
	(0.028)		(0.039)
	[0.215]		[0.253]
Vocational middle school*CV	0.104**	Mildly interested*CV	0.045
	(0.044)		(0.024)
	[0.271]		[0.161]
High school or college*CV	0.038	Very interested*CV	0.017
	(0.039)		(0.029)
	[0.343]		[0.576]
4) Income quartile		8) Self-reported political preference	
Income Q1*CV	0.072*	Left*CV	0.039
	(0.034)		(0.030)
	[0.217]		[0.271]
Income Q2*CV	0.075*	Right*CV	0.034
	(0.039)		(0.022)
	[0.271]		[0.245]
Income Q3*CV	0.029	Minor parties*CV	−0.017
	(0.034)		(0.153)
	[0.467]		[0.928]
Income Q4*CV	0.037	No party preference*CV	0.148**
	(0.030)		(0.063)
	[0.321]		[0.077]

Notes: This table presents 8 separate regressions numbered 1–8. Except for regression 1, the coefficients shown are interactions of *CV* with individual characteristics. In regression 6, we use whether someone regularly reads the newspaper as a proxy for whether they are informed. Standard errors clustered by state are in parentheses, and cluster-robust wild-bootstrap *p*-valuesin square brackets. The dependent variable in all regressions is a dummy for whether the individual voted in the previous parliamentary elections. *CV* is a dummy for whether voting was compulsory for that election in the individual's state of residence. All regressions include baseline controls for age, age squared, gender, educational attainment, parents' education, working status, household size, community size, self-reported political preference, party membership, being informed, interest in politics, state fixed effects, and survey year fixed effects. Regression 4 includes income quartile controls (excluded from the other regressions because they sometimes have missing data). The sample includes all individuals in the 1986 and 2003 Austrian Social Survey who reported whether they voted in the last parliamentary elections (1983 and 2002) and were of voting age. Regression 4 has 2647 observations, whereas all other regressions have 3369 observations. ***Significant at 1%.

* Significant at 10%.

** Significant at 5%.

who are not informed (proxied by newspaper reading), who declare themselves uninterested in politics, and who report no party preference. For example, the coefficient of 0.148 in regression 8 indicates that CV increases turnout by 14.8 percentage points among individuals with no party preference. In contrast, for individuals who declare a party preference toward the main left or right parties in Austria, CV increases turnout by 3–4 percentage points.

Although CV may have affected the gender, educational attainment, and income of the median voter, our results indicate that it may not have shifted the median voter's political preferences. If those induced to vote by CV do not have strong political views, then such voters may not necessarily vote differently from the median voter in elections without CV. If the choices of the median voter are not shaped by CV (and if party platforms don't change), it would not be surprising if CV failed to affect what party wins and what policies are implemented. These results may also be consistent with a citizen candidate framework in which significant changes in the electorate are not necessarily accompanied by changes in policy outcomes. Unfortunately, we do not have enough data to precisely determine which of these two frameworks are more suited to explain the observed results.

6. External validity and conclusion

Although compulsory voting (CV) is often viewed as a way to foster voter turnout and consequently improve the representativeness of political processes, relatively little is known about how CV causally affects voter participation and, in particular, how it affects economic policy. We analyze these impacts by leveraging quasi-experimental variation in CV laws across Austria's nine states. We find that CV increased turnout from roughly 80% to 90%. This occurred even though penalties for not voting were rarely enforced. However, in our main results, the increase in turnout did not affect state-level spending (either in levels or shares of sectoral spending) or electoral outcomes. Effects of CV on turnout seem larger among individuals who are uninterested in politics, who do not have strong political views, and who are relatively uninformed (with informedness proxied by newspaper reading). This suggests that individuals swayed to vote by CV are more likely to exhibit these characteristics (compared to individuals not swayed by CV).

We view our results as consistent with a story where voters swayed to vote by CV do not cast different votes (on average) from those who vote regardless of the law. Our results may also be consistent with other explanations (e.g., citizen candidate models) where politicians implement policies that may not correspond to those preferred by the median voter. Ultimately, it is difficult to say definitively what theoretical mechanism explains our results. Our contribution, though, is to provide causal evidence (previously lacking) that CV laws need not significantly affect government spending. We believe this is important evidence for the policy debate regarding CV.

Overall, our results complement the literature documenting that extension of the voting franchise to specific population groups impacts policy. Our study suggests that policies aimed at increasing turnout (e.g., get-out-the-vote campaigns) need not necessarily affect public spending, and this seems particularly the case if these policies do not increase turnout among voters with specific policy preferences.

Our results are specific to Austria, so it is important to consider to what extent we think the results would extrapolate to other countries. As discussed earlier in Section 3.1, Austria has had relatively high turnout and political involvement even when CV was not in place, at least relative to the US and the OECD averages. At the same time, however, there are a sizable number of countries (e.g., Germany, the Scandinavian countries, and others) that share these features, particularly in Europe. Thus, we believe our findings may be relevant for these countries and other advanced democracies where reforms to increase political participation (such as CV laws) are being evaluated.

How might our results extrapolate for countries with lower levels of political involvement? In terms of turnout, one might imagine that countries with low initial turnout levels might experience even greater turnout increases (in absolute percentage levels) as a result of CV compared to countries like Austria with traditionally high turnout. Thus, our turnout findings could form a "lower bound" for impacts for reforms implemented in countries with low turnout.[35]

[35] However, one can also imagine situations where CV laws have smaller effects on turnout. For example, countries with low turnout may be reflective of citizens being generally distrustful of government and authority. Such characteristics could lead to CV laws having less of an effect on turnout.

Turning to government spending, as observed earlier in the paper, many countries without CV (such as the US) experience significant disparities in turnout along socio-economic lines, which are also correlated with levels of political interest. For Austria, our results suggest that CV induces low-interest or low-knowledge voters to participate. For countries with low initial turnout, while it is possible that voters induced to participate because of CV would have low interest in politics, it is also possible that a broader set of voters would be affected. This might cause CV to actually have a significant impact on government spending. However, as noted by Hodler et al. (2015), changes in voting costs could affect government spending in either direction, and it is challenging to make confident empirical predictions about possible impacts of CV in a setting very different from Austria. Thus, we urge significant caution in assessing the relevance of our results for countries like the US where turnout is much lower than in Austria.

Beyond political involvement, there are a host of other factors that may affect how CV affects government spending including whether there is a presidential or parliamentary system, and whether the population is relatively ethnically heterogeneous or homogenous. While we do not have strong priors on how such factors would influence the impact of CV, we cannot rule out that they may be at play. While our results provide the first quasi-experimental evidence on how CV affects government spending, they are certainly not the last word. We look forward to future research on how CV affects government spending, hopefully using data from additional countries.

Appendix A. Supplementary data

Supplementary data to this article can be found online at http://dx.doi.org/10.1016/j.jpubeco.2016.10.002.

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