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Income tax schedule and redistribution in direct democracies – the Swiss case

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Abstract This study examines redistribution policy through personal income taxes in Swiss cantons over the period 1995–2011. In a first step, redistribution measures are estimated with the help of exhaustive administrative data. Redistribution is decomposed into average tax rate and tax progression. In a second step, we investigate the impact of direct democratic institutions and their usage on tax policy and redistribution. The results suggest that the effect of direct democracy on income tax redistribution is a multilayered process. First, the theoretical availability of direct democracy tools does not seem to have the same impact as the effective use of them. Second, fiscal referendums may – in the short term –reduce redistribution through lower tax rates and lead to less tax progression. Third, an increasing number of ballots on initiatives leads to more tax progression and more redistribution in the long run. It seems that the short-term dampening effects of fiscal referendums on redistribution may be overridden in the long run by the expansive effect of popular initiatives.

Keywords Direct democracy · Income redistribution · Spatial econometrics · Tax competition

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1 Introduction

In the majority of developed economies, income inequality has grown since the mid-1980s (OECD 2011) alongside considerably increased political and academic interest in redistribution policy. Numerous studies have investigated the redistributive effect of personal income tax (PIT) and, in a more general setting, of the whole national tax benefit system.[1]

In addition to the descriptive analysis of redistribution measures, a diversity of political economy research tries to explain the political process of redistribution. Most importantly, the empirical literature concludes that town meetings and fiscal referendums keep revenues and expenditures small. However, until now, the question of through which channels direct democracy affects the redistribution process has, from an empirical perspective, remained unanswered.

The contribution of this paper to the empirical literature is that more insights are given into the redistribution process of direct democracies. Specifically, we ask: which tax instruments (average tax level or tax progression) does direct democracy use to manage the redistribution process?

Switzerland has strong direct democratic institutions at both the subnational and national levels. Due to both historical and cultural aspects, institutions of direct democracy and tax systems differ substantially among the Swiss states, known as cantons. This provides an opportunity to empirically investigate the impact of democratic institutions on tax policy and redistribution outcomes. To answer the research questions, we divide our analysis into two parts. In the first part, redistribution parameters are estimated with the help of exhaustive tax data. Redistribution is decomposed by following the methodology of Aronson et al. (1994), who separate redistribution into its components of vertical redistribution, classical horizontal inequality, and the reranking of unequals. In the second part, we run regression analysis to investigate the impact of direct democracy on redistribution policy.

We find two interesting results. First our descriptive indicators for Switzerland show that it is important to look carefully at the combination of tax policy instruments when investigating the redistribution policies of states. The decomposition indicators suggest that even in jurisdictions with an overall low tax rate, redistribution can be substantial if the tax schedule is progressive. Second, regression results suggest that the impact of direct democracy on cantons depends on the instrument. Fiscal referendums may reduce redistribution – over the short-run – through lower tax rates and lower tax progression. However, an increasing number of ballots on initiatives leads to more redistribution over the long-run. In cantons with a high frequency of initiatives, tax progression and redistribution are more pronounced. Our results suggest that the short-term dampening effects of fiscal referendums on redistribution may be overridden in the long run by the expansive effect of popular initiatives.

The rest of this paper is organized as follows. Section 2 presents the relevant literature, introduces the political institutions in Switzerland, and discusses the methodology used to measure redistribution. In Section 3, the descriptive results are presented, and the used data as well as the empirical model are described. Section 4 discusses the findings of the regression analysis. Section 5 concludes.

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[1]For income tax studies see Berliant and Strauss (1985), Aronson et al. (1994), Kakwani and Lambert (1998), Wagstaff et al. (1999), Smith (2001), Wagstaff and van Doorslaer (2001), Hyun and Lim (2005), Čok and Urban (2007), Urban and Lambert (2008), and Lambert et al. (2010). Relating national tax-benefit systems see Kim and Lambert (2009), Immervoll and Richardson (2011), and Caminada et al. (2012).

2 Redistribution in democracies and decentralized jurisdictions

2.1 Literature review

A broad field of research examines the determinants of government revenue and expenditure from a political economy viewpoint. According to the traditional theory of Romer (1975), Roberts (1977), and Meltzer and Richard (1981), the median voter decides the amount of income redistribution through taxes. Rich individuals prefer lower taxes and poor voters favor higher taxes. Consequently, if income distribution becomes more skewed, income redistribution increases.[2]

In *representative democracies*, political outcomes may deviate substantially from the preferences of median voters. The most important arguments why effective revenue, expenses, or redistribution are far away from median voter preferences are pork-barrel politics (Weingast et al. 1981; Dixit and Londregan 1998), partisan politics Cameron 1978, Blais et al. 1993, 1996, Cussak 1997), logrolling (Weingast and Marshall 1988), political fragmentation (Roubini and Sachs 1989; Persson and Tabellini 2004), incomplete information between representatives and voters (Matsusaka 1992), or agenda setting (Romer and Rosenthal 1979).

Whether, and how, *democracy in general* leads to more or less redistribution, and whether it decreases economic inequality is both theoretically and empirically an open question, full of contradictory results (Acemoglu et al. 2013). For example – by analyzing former studies – Gradstein and Milanovic (2004) find that formal exclusion from the political process causes a high degree of economic inequality, whereas more recent research indicates an inverse relationship between democracy and inequality. Timmons (2010), among others, even finds no clear relationship between democracy/civil liberties and aggregate measures of economic inequality.

Instead, the literature on *direct democracy* finds that institutions do matter with respect to tax rates, expenditures, and revenues. Hinnerich and Pettersson-Lidbom (2014) analyze direct democracy in Swedish municipalities between the two world wars. They find that municipalities with town meetings spend about 40–60 percent less on public welfare than representative democracies.

Even in highly democratized countries, town meetings as the purest form of direct democracy are a very rare phenomenon. Other than town meetings, two main instruments are available, each with a different impact on political outcomes. One is the popular initiative, which allows people as citizens of municipalities, states, or a nation to make propositions addressing policy changes. Therefore, popular initiatives are instruments for citizens to act directly as a legislator. Other important instruments of direct democracy are optional and mandatory referendums. With a referendum, the population can veto political decisions. Thus, a referendum is a tool to control legislation.

Concerning *fiscal referendums*, there is broad evidence that voters are indeed successful at vetoing excessive governmental activity. It is widely accepted that this instrument reduces government revenue, spending, and deficit and that a referendum leads to a higher weight of user charges (Romer and Rosenthal 1979; Feld and Kirchgässner 2001; Feld and Matsusaka 2003; Freitag and Vatter 2006; Feld et al. 2010; Funk and Gathmann 2011). Gerber (1996) shows that the possibility of threatening the government at the poll may induce legislators to be self-restricting, which brings the policy nearer to the median voter. This can even be observed in jurisdictions where these instruments are not or only seldom used.

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[2]For a deeper discussion of the Romer–Roberts–Meltzer–Richards model and implications of the relaxation of its assumptions, see Borck (2007).

On the contrary, whether *voter initiatives* constrain public spending is unclear. It is often argued that initiatives work like a fiscal accelerator. An important argument is the so-called Robin Hood effect (Freitag and Vatter 2006), which assumes that the population has stronger preferences for redistributive policies than the government. Another argument is that initiatives are innovative proposals which, if they are accepted at the poll, in most cases induce new expenditures.

Feld and Kirchgässner (2001) as well as Feld et al. (2010) find evidence that the availability of a popular initiative may increase spending. According to their results, in cantons with a mandatory fiscal referendum, citizens use the initiative to increase spending, whereas in cantons without a mandatory fiscal referendum, such initiatives reduce spending and revenue, possibly because voters in these cantons have to use the popular initiative as a substitute for the missing referendum. Asatryan (2016) and Asatryan et al. (2017a, b) find that after the right of local initiatives was introduced in the German state of Bavaria in 1995, there was an expansion in the size of local government and an increase in tax rates.

On the contrary, Matsusaka (1995) finds that spending is significantly lower in US states with voter initiatives than in purely representative states. Concerning governmental revenue, he concludes that voter initiative states place a greater weight on user charges. Consistent with these results, Funk and Gathmann (2011) find that lowering signature requirements for the voter initiative by 1% reduces spending by 0.6% in Swiss cantons. Freitag and Vatter (2006), Besley and Case (2003), as well as some of the cited literature therein, however, find inconclusive results for the impact of voter initiatives on tax rates and spending.

To sum up, most studies investigating the impact of political institutions on fiscal policy outcomes focus on tax rate, government revenue, spending, or deficit. With respect to redistribution, there is little evidence. One of the few studies is from Feld et al. (2010), who analyze the impact of direct democracy in Switzerland on redistribution over 1981–1997. They find that in cantons with more direct democratic institutions, fewer public funds are used to redistribute income. However, they argue that income inequality is reduced to the same extent as in cantons with fewer direct democratic institutions, which leads them to conclude that direct democracies redistribute more efficiently compared to more representative democracies.

To investigate redistribution through PITs, we follow a comparable approach to that proposed by Feld et al. (2010). However, in contrast to them, we decompose redistribution through PITs into an average tax effect and a tax progression effect to gain more insight into the redistribution strategy of democracies.

2.2 Political and fiscal institutions in Switzerland

Switzerland has strong institutions of direct democracy that share a long historical tradition. In the regions of central Switzerland (Uri, Schwyz, Obwalden, Nidwalden, Zug, Glarus, Appenzell), direct democracy dates back to the 13th century, where the most important decisions were decided in town meetings (so-called *Landsgemeinden*). In the larger jurisdictions of Switzerland such as Bern and Zurich, the system of the public opinion poll was implemented. In these states, more binding referendums increased in relevance at the beginning of the 16th century (Curti 1885). In 1848, Switzerland agreed its constitution. With it, the constitutional initiative was mandated for all cantons. At the same time, the mandatory constitutional referendum was implemented at the federal level. In 1874, the optional referendum on laws followed, and since 1921, a referendum on state treaties has existed (Gross 2014).

Therefore, all cantons in Switzerland have direct democratic institutions that are older than 150 years. Nevertheless, the characteristics of cantonal systems differ substantially. For example, not all cantons adopted mandatory fiscal referendums, and for those that did, the monetary amount that triggers the referendum differs. All but one canton (Vaud) use mandatory and/or optional fiscal referendums. The number of signatures required to bring an optional referendum to the poll also differs considerably between cantons (see Online Appendix A1).

The institution of town meetings only continues to exist in Appenzell Inner Rhoden and Glarus. Appenzell Ausser Rhoden as well as Obwalden and Nidwalden abolished town meetings between 1996 and 1998 in favor of a parliament. Nevertheless, all cantons still use legislative initiatives. To sum up, not only is the history of the democratic institutions in Swiss cantons diverse but so are the bodies implemented today.

In combination with direct democratic institutions, the federalist structure of Switzerland supports heterogeneous outcomes in the local provision of public goods and income tax burden. In terms of revenue, PIT is the single most important tax in Switzerland, generating about 58 billion Swiss francs (CHF) in 2011 (equivalent to 9.3% of GDP). Overall, more than one-third of the total budget is from PIT earnings. All three levels of government have the authority to tax personal income; 45% of PIT revenue goes to cantons, 31% to municipalities, and only 24% to the federal government. Cantons have full sovereignty to set their own tax schedules.[3] Municipalities can generally apply a multiplier to the cantonal income tax, or participate in other ways (e.g., share tax earnings or apply extraordinary schedules). As only a relatively small share of total PIT earnings goes to the federal government, overall income tax rates differ substantially among and within cantons.

2.3 Measuring redistribution

A widely used method to measure the redistribution effect of taxes is to compare the before- and after-tax distribution of income. The difference between those two Gini coefficients is known as the Reynolds and Smolensky (1977) index of redistribution, although it dates back to Musgrave and Thin (1948). Kakwani (1976) shows that this redistribution effect (RE) can be decomposed into an average tax effect ($\bar{t}$) and departure from proportionality or progressivity effect P_K :

$$RE = G\left(Y^G\right) - G\left(Y^N\right) = \frac{\bar{t}}{1-\bar{t}} P_K \qquad (1)$$

where $G(\cdot)$ is the Gini coefficient of the before-tax (Y^G) and after-tax (Y^N) income distribution. The Kakwani index of progression P_K is defined as

$$P_K = C(t) - G\left(Y^G\right) \qquad (2)$$

with $C(t)$ as the tax concentration index. Therefore, the Kakwani index compares the distribution of the tax burden with the distribution of before-tax income and shows to what

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[3]However, the Federal Supreme Court of Switzerland restricted the autonomy of the cantons by declaring that regressive tax schedules violate the constitution. Furthermore, the tax base is widely harmonized by federal law.

degree a tax system departs from proportionality (Kesselman and Cheung 2004). A positive value means that the tax system is globally progressive, whereas a negative value indicates a globally regressive tax system. However, Kakwani's decomposition approach does not hold in the presence of the unequal treatment of equals (horizontal inequality H) and reranking of unequals (R). Aronson et al. (1994) show that the correct decomposition of the redistribution effect in the presence of reranking and horizontal inequality of group i with equal pre-tax income is as follows:

$$G\left(Y^G\right) - G\left(Y^N\right) = \frac{\bar{t}}{1-\bar{t}}P_K - \sum_i \alpha_i \beta_i G_i\left(Y^N\right) - \left[G\left(Y^N\right) - C\left(Y^N\right)\right]$$
$$RE \equiv V \qquad - H \qquad - R, \tag{3}$$

where α_i, β_i, and $G_i\left(Y^N\right)$ are the share of the population, the share of post-tax income, and the Gini coefficient of the post-tax income of group i, respectively. The reranking term is defined by Kakwani (1984) as the difference between the after-tax Gini and the post-tax concentration index $C\left(Y^N\right)$. Both H and R can have only zero or positive values, and therefore, the presence of horizontal inequality and reranking reduces the overall redistributive effect of a tax system.

Because the income tax burden differs between communities and cantons, the personal tax bill depends on place of residence. From a national or cantonal perspective, households with equal pre-tax income are treated unequally, which results in positive values of H and R. At the same time, these two measures provide a first indication of the impact of tax competition on redistribution. However, one important limitation has to be made when interpreting these figures: H and R show only the outcome of the redistribution process.

Married couples have to submit a joint PIT declaration, including minors if they obtain no income from employment. It is widely accepted that household income has to be adjusted when analyzing income distribution and redistribution in order to obtain a measure of household well-being (Lambert 2001). Therefore, we follow the majority of studies of income distribution and redistribution by deflating both household income and tax payments by using the equivalence scale, E, proposed by Cutler and Katz (1992):

$$E = (A + cK)^e, 0 \leq c, e \leq 1, \tag{4}$$

where A and K are the number of adults and children in the household, respectively, c is a parameter value giving weight to the children, and e is an economy-of-scale parameter. Widely applied parameter choices are $c = 1$ in combination with $e = 0.5$ (e.g., Atkinson et al. 1995) and $c = e = 0.5$ (e.g., Aronson et al. 1994; Wagstaff et al. 1999). By setting both values equal to 0.5, Aronson et al. (1994) argue that these parameters minimize their horizontal inequality measures. We thus apply these parameter values.

3 Data and empirical model

3.1 Tax data

The database of the Swiss Federal Tax Administration (SFTA) on PIT includes individual-level data on all taxpayers in Switzerland. Before 1995, the database excludes households with a zero federal tax bill (taxable income is below the tax exemption limit). Because

of this, we start with the tax period 1995/1996. Overall, the dataset includes roughly four million tax units in 1995/1996. This number steadily increases from year to year up to nearly five million tax units in 2011. At the end of the 1990s, income tax declaration was on a biannual basis; however, by 2001, cantons had shifted to an annual declaration system. Therefore, our database includes tax declarations for the biannual periods 1995/1996, 1997/1998, and 1999/2000 as well as the single years of 2001–2011.

Tax collection falls completely to the cantons, which levy federal PIT and transmit revenues to the confederation. This fact implies that the federal database includes only a few variables (taxable income and some but not all tax deductions). Therefore, gross household income is unknown. The procedures and assumptions that underlie the calculations of the redistribution indicators are discussed in Online Appendix A2. Despite certain data limitations, a full sample of administrative data has the advantage that very high-income classes are fully reconsidered in redistribution measures. Instead, survey data rarely have reliable data for very high incomes and/or these are underrepresented. However, it is extremely important to reconsider very high-income households properly in redistribution analysis. In 2010, the top 1% of income earners contributed 39.3% to total federal PIT earnings.

3.2 Explanatory variables

To select the exogenous variables, we largely follow the relevant literature discussed in Section 2.1. We include (1) different proxies for direct democratic institutions, (2) other control variables for political institutions, (3) structural and economic factors, and (4) variables that capture cultural differences between cantons.

(1) Proxies for direct democracy. Different variables that represent the degree of direct democracy are included. In the baseline specifications, we use proxies for institutions of direct democracy that are often used in the political economy literature. The first is the fiscal referendum variable, measured as the share of signatures required to trigger an optional fiscal referendum compared to the total population, which is set to 0 for those cantons with mandatory fiscal referendums. We use the fiscal referendum, as this variable seems to be the most important institution for controlling budgetary issues.[4] The second variable for direct democratic institutions is the number of per capita signatures required to bring a popular initiative to a vote. Because population follows a positive time trend in most cantons in 1995–2011 and because redistribution may also follow a positive time trend, to mitigate a spurious correlation between the two direct democracy variables and the redistribution parameter, we use the population average of 1995–2011 as the denominator rather than yearly values.

In a second step, we counted the number of fiscal referendums (separately for optional and mandatory fiscal referendums) and initiatives for each canton in each year under investigation. Such an indicator measures "active use of democracy" by the population instead of theoretical availability of democratic institutions. Clearly, these two groups of indicators do not measure exactly the same issue, but both handle the same question: How does direct democracy affect redistribution? Counting the use of democratic institutions has a substantial advantage over institutional variables – variability substantially increases. The variation

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[4]We follow the specification of Feld et al. (2010) who use, however, the broader legislative referendum. Funk and Gathmann (2011) used a dummy variable, which equaled zero for an optional fiscal referendum and one for a mandatory fiscal referendum.

is so high (in some years there are no referendums at all in some cantons, where in other years there are several) that we need to average the measures over different time periods (we check 3-year, 5-year, and 10-year averages). One disadvantage of using these variables in a regression is that the indicators cannot be used for the five cantons that implemented town meetings for two reasons. First, the policy making process is different in cantons with town meetings (every single person may veto a governmental decision or make a proposal). Second, the database seems to be incomplete for some of these cantons. Therefore, we decided to exclude the cantons which held a town meeting in one or all years under investigation. These cantons are Appenzell Ausserrhoden, Appenzell Innerrhoden, Glarus, Obwalden, and Nidwalden.

Concerning the institutional variables, one should note that between 1995 and 2011, the signature requirement for a fiscal referendum changed in only two cantons. This stability may result in an identification problem in panel regression. Table 1 gives some impressions about the low within-canton variability of direct democratic institutions.

(2) Other political and institutional factors. To control for institutional differences between cantons, we include, first, a variable representing tax competition among cantons, measured by the sum of the inverse of the top-income class, average tax burden in all other cantons. This variable is weighted by the spatial weight matrix *W* (discussed below) to give neighboring cantons more weight than other cantons. Second, an indicator of the degree of fiscal decentralization within a canton is reconsidered, which is measured by the share of the PIT revenues of the canton and its municipalities accruing to the municipalities. Third, we take into account the per capita payments from and to the fiscal equalization scheme (measured in millions of Swiss Francs). This variable might be of interest because some poor cantons use the payments from the equalization scheme to reduce their PIT rates. Fourth, the per capita revenues (2011 prices, measured in millions Swiss Francs) from other sources than PIT are included to control for the other fiscal preferences of the cantons. Fifth, we take into account political fragmentation, denoted by the number of parties, and sixth, the share of leftwing party members in the cantonal government.

Furthermore, we include a political variable consisting of the share of homeowners to reflect the influence of pressure groups on political outcomes. Both at the cantonal and at the federal levels, homeowners are one of the most important lobbying groups for reducing the tax burden. For example, in 2012 alone, three popular initiatives at the federal level aimed to reduce the PIT burden for homeowners by allowing more generous income tax deductions. Therefore, we expect that a higher share of homeowners is accompanied by lower PIT redistribution.

(3) Structural and economic factors. As further control variables, we include the pre-tax Gini coefficient, the log of the population, the share of the population younger than 20 years and older than 64 years, the share of the employed population (measured in full-time equivalents) working in the tertiary sector, and the unemployment rate. All these variables should control for demographic, economic, and structural differences among cantons, which may lead to differences in redistribution policy.

(4) Cultural differences. We reconsider four variables that capture cultural differences and differences in political ideology between cantons' populations. The first cultural variable consists of the share of the people in each canton that are German native speakers. Second, the share of the population who are Roman Catholic is included, and third, the share of the population that lives in an urban municipality is reconsidered. Last, we include the share of the people who voted for left-wing parties at national assembly elections.

Table 2 includes summary statistics and data sources for the control variables included in the model.

Table 1 Descriptive statistics of the direct democracy variables

Variable		Mean	s.d.	Min	Max	Obs.
Institution: fiscal referendum	overall	0.007	0.008	0.000	0.027	N =294
	between		0.008	0.000	0.022	n=21
	within		0.002	−0.015	0.011	T=14
Institution: Popular initiative	overall	0.016	0.007	0.005	0.036	N =294
	between		0.007	0.005	0.032	n=21
	within		0.001	0.008	0.019	T=14
Number of popular initiatives per year	overall	0.840	1.197	0.000	9.000	N =294
	between		0.676	0.143	2.500	n=21
	within		0.998	−1.660	7.340	T=14
Number of popular initiatives, 10–year average	overall	0.762	0.672	0.000	3.500	N =294
	between		0.648	0.107	2.386	n=21
	within		0.227	−0.023	1.877	T=14
Number of fiscal referendums per year	overall	0.602	0.950	0.000	6.000	N =294
	between		0.489	0.000	2.357	n=21
	within		0.821	−1.755	4.745	T=14
Number of fiscal referendums, 10–year average	overall	0.695	0.567	0.000	3.300	N =294
	between		0.451	0.043	1.921	n=21
	within		0.357	−0.220	3.080	T=14
N of mandatory fiscal referendums per year	overall	0.473	0.907	0.000	6.000	N = 294
	between		0.518	0.000	2.286	n=21
	within		0.753	−1.813	4.687	T=14
N if mandatory fiscal referendums, 10–year average	overall	0.537	0.572	0.000	3.300	N =294
	between		0.479	0.000	1.821	n=21
	within		0.329	−0.377	2.923	T=14
N of option fiscal referendums per year	overall	0.129	0.356	0.000	2.000	N =294
	between		0.162	0.000	0.500	n=21
	within		0.318	−0.371	1.986	T=14
N of option fiscal referendums, 10–year average	overall	0.157	0.267	0.000	1.800	N =294
	between		0.231	0.000	0.864	n=21
	within		0.144	−0.307	1.272	T=14

Note: All cantons except Appenzell Ausserrhoden, Appenzell Innerrhoden, Glarus, Obwalden, and Nidwalden Sources: Institutional variables: Trechsel and Serdült (1999); www.badac.ch and own compilations (constitutions and legislations of the cantons). Frequency variables: https://www.zdaarau.ch/en/research/direct-democracy/referendums-and-elections

Table 2 Descriptive statistics for control variables

	mean	sd	min	max	Source
Left-wing gov.	0.274	0.143	0.000	0.600	Swiss Federal Statistical Office, SFSO
Fragmentation	3.650	0.677	2.000	5.000	SFSO
Homeowner	0.386	0.112	0.120	0.614	SFSO (values for 1990; 2000; 2010; 2011*)
Tax competition	0.004	0.001	0.002	0.008	Swiss Federal Tax Administration (SFTA)
Decentralization	0.436	0.120	0.042	0.588	Federal Finance Administration (FFA)
Fiscal eq. scheme	0.000	0.001	–0.002	0.002	FFA
Other revenues	0.009	0.002	0.005	0.017	FFA
log(Population)	12.419	0.857	10.465	14.147	SFSO
<20 years	0.223	0.022	0.165	0.273	SFSO
>64 years	0.160	0.021	0.117	0.214	SFSO
Tertiary sector	0.632	0.088	0.452	0.838	SFSO
Unemployment	0.032	0.014	0.004	0.074	SFSO
German	0.619	0.356	0.039	0.945	SFSO (values for 1990; 2000; 2010; 2011*)
Catholics	0.483	0.208	0.158	0.901	SFSO (1990; 2000; 2010; 2011*)
Urbanity	0.684	0.242	0.000	1.000	SFSO and own calculations
Left-wing voters	0.297	0.105	0.000	0.523	SFSO (1995; 1999; 2003; 2007; 2011*)

* Linear interpolation for non-available years

3.3 Descriptive results on income redistribution

Over the long-term horizon, the literature finds that income distribution as well as redistri-
bution is exceptionally stable in Switzerland (Frey et al. 2016; Frey and Schaltegger 2016).
Having a smaller horizon, our measures for Switzerland show a slight increase in before-
and after-tax income inequality, while redistribution remained more or less stable from 1995
to 2011 (see Online Appendix A3). In most cantons (23 out of 26), there was a small increase
in before-tax income inequality over the period 1995/1996 to 2011 (see Online Appendix
A4). Similarly, the after-tax Gini coefficient grew in 22 out of 26 cantons.[5]

In all cantons, redistribution sharply decreased in the economic crisis years of 2008 and
2009 and returned to pre-crisis levels in 2010 (see Online Appendix A5). Over the whole
horizon of 1995/1996 to 2011, one can also find a systematic geographical divergence;
whereas in the eastern part of Switzerland redistribution decreased, redistribution increased
in the western part of Switzerland (see Fig. 1).

Interestingly, the average tax rate decreased in all but three cantons: in Neuchâtel, Vaud,
and Zug, the tax burden as a share of adjusted gross income rose. With respect to Neuchâtel
and Vaud, the increase in the tax burden can be explained by the tax system itself (i.e.,
the absence of major tax reforms). For Zug, the explanation is completely different. Zug is
the canton with the lowest PIT rates, as explained in Section 2.2. It seems that there was a
substantial immigration of rich households into Zug over the period 1995/1996 to 2011 as
nominal adjusted gross incomes nearly doubled during this time. Because of this significant
increase in income, households in Zug fell into higher tax brackets. Consequently, owing

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[5]Only the values from 1995 and 2011 are shown. The full dataset including the annual values can be obtained
from the authors upon request.
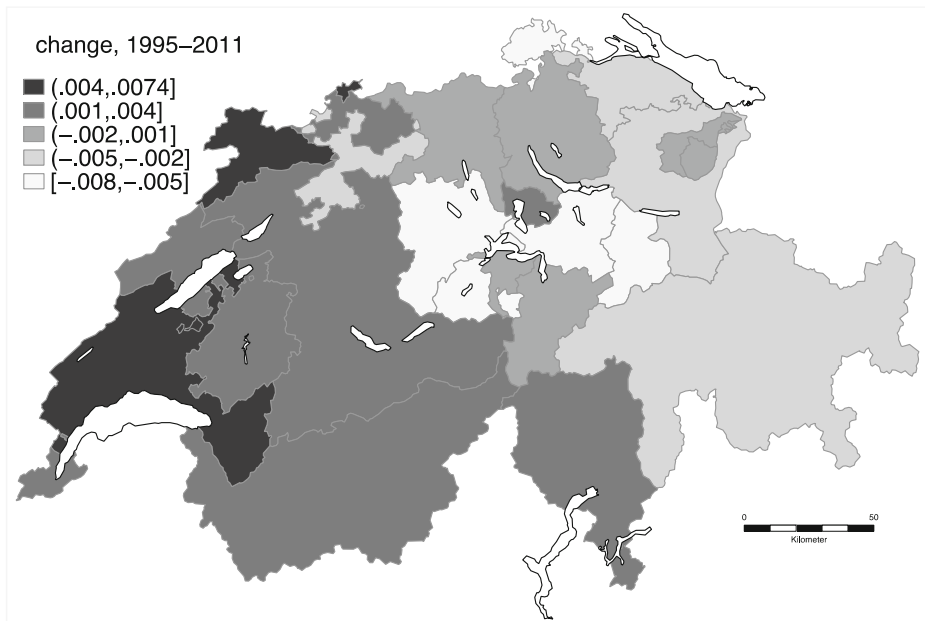
Fig. 1 Change in PIT redistribution, 1995/1996–2011

to very progressive federal PIT rates in particular, the average tax burden increased. In contrast to the average tax effect, progression increased in 23 out of 26 cantons. Overall, one may thus conclude that overall redistribution did not change substantially from 1995 to 2011, but how this redistribution was achieved (i.e., the design of the tax schedule) changed remarkably.

Interestingly, the horizontal inequality measures H and R are both very low. Low values of H can be explained by the fact that the width of equal income groups is very thin (CHF 100). Therefore, almost all horizontal inequality is absorbed by the reranking term. Consequently, it seems useful to investigate the horizontal inequality measures together $(H + R)$.

There are two potential sources of horizontal inequality. First, tax deductions that are not granted to all taxpayers equitably may lead to high values of H and R. However, we take into account only a few tax deductions that are, moreover, of minor importance. The second source of horizontal inequality is the tax schedule itself. Differentiated tax rates at the municipal level induce—from a cantonal or federal viewpoint—the unequal treatment of taxpayers in comparable economic situations. The decomposition of $H + R$ into the components of federal PIT and cantonal (including municipal) PIT demonstrates that roughly 95% of overall horizontal inequality is because of cantonal and municipal PIT, indicating that the overwhelming majority of horizontal inequality is due to tax competition.

The results show the importance for politics to consider the functions that the different state levels can pursue with their tax policy. According to Musgrave (1959), the central state should primarily enforce redistribution goals. Here, the Swiss tax system follows this guiding principle, as income tax at the federal level is strongly progressive. However, despite the observation that tax competition may induce horizontal inequality, our results suggest that overall horizontal inequality in Switzerland is quite low. Depending on the year under

consideration, $H + R$ in relation to vertical redistribution accounts for 1.5% to 2.4%, meaning that horizontal inequality reduces potential redistribution through PIT by this range. At the cantonal level for 2011, horizontal inequality reduces potential redistribution by between 0.3% (canton of Zug) and 2.8% (canton of Valais).

Comparing these figures with the results from other studies, overall horizontal inequality, including reranking, is low. For example, Hyun and Lim (2005) find that horizontal inequality reduces vertical redistribution in Korea by between 27% (1991) and 57% (1996). According to Čok and Urban (2007), horizontal inequality ($H + R$) reduces potential redistribution in Slovenia by 3% and in Croatia by 2.5%. By relying on survey data, Wagstaff et al. (1999) conclude that horizontal inequity rates range from the lower levels of 2% for Italy, 2.6% for the United States, and 4.9% for Spain to the higher levels of 20.7% for Switzerland and 23.8% for Denmark.

In comparison to Wagstaff et al. (1999), our significantly lower inequality measures can be explained by two factors. First, some tax deductions that we could not consider, but that are implicitly included by Wagstaff et al. (1999), may induce important horizontal inequality. Second, horizontal inequality may be overestimated with survey data, or redistribution may be underestimated, because of the underrepresentation of top-income earners. An indication of the relevance of the second hypothesis gives us a comparison of the redistribution parameters. While Wagstaff et al. (1999) find a redistribution parameter for Switzerland of 0.0174 in 1992, our data suggest a value of 0.038 in 1995/1996.

At first glance, one might conclude that regional tax differences are not necessarily an impediment for income redistribution at the cantonal or national level. Tax allowances seem to be much more unequitable in a horizontal sense. However, this statement is clearly a static one. Regional tax competition may lead to a "race to the bottom" with very low tax rates in general and low PIT redistribution. In such a world, tax rate differences would be small and, with it, the values of H and R.

3.4 Empirical model

To investigate redistribution through PITs, we follow a comparable approach to that proposed by Feld et al. (2010). However, by contrast, we analyze the main components of tax progression and the average tax rate to obtain a deeper insight into the policy process that precedes the redistribution outcome. A second modification to the literature is that we explicitly control for spatial correlations. This seems important, as different studies show that tax competition within Switzerland is regionally concentrated (Eugster and Parchet 2014). Therefore, income redistribution through PIT may be spatially correlated, too. Actually, Moran's I statistic detects a positive and strongly significant spatial correlation parameter at the 1% level in each of the 14 years.

Consider the following general specification for a regression model with spatial correlation (Elhorst 2010a, b):

$$y_{it} = \rho \sum_{j=1}^{N} w_{ij} y_{jt} + \mathbf{x}_{it} \beta + \sum_{j=1}^{N} w_{ij} \mathbf{x}_{ijt} \theta + \mu_i + u_{it}$$

$$u_{it} = \lambda \sum_{j=1}^{N} w_{ij} u_{jt} + \varepsilon_{it} \tag{5}$$

where y_{it} is an observation of the dependent variable of canton i ($i = 1, \ldots, N$) at time t ($t = 1, \ldots, T$). w_{ij} is an element of the spatial weight matrix W of order N that captures

the spatial interaction between regions i and j The elements of W are non-negative with $w_{ij} = 0$ for $i = j$ Here, the elements of our W matrix are the inverse of the road distances between the cantons capital cities.[6] $w_{ij} y_{jt}$ represents the endogenous spatial interaction effects of the dependent variable, $w_{ij} \mathbf{x}_{ijt}$ are the exogenous interaction effects, and $w_{ij} u_{jt}$ are the interaction effects of the disturbance terms. ρ the spatial lag parameter, λ the spatial autocorrelation parameter and θ are the corresponding unknown coefficients. β is a vector of parameters relating to the non-spatial exogenous variables matrix $\mathbf{x}_{it}$. Lastly μ_i is an individual-specific random or fixed effect and ε_{it} is an error term with zero mean and variance σ^2.

When the spatial lag of the dependent variable, the spatial autocorrelation, and the spatial exogenous variables are included simultaneously, then the parameters are not identifiable (Manski 1993). Therefore, at least one of the parameters ρ, θ or λ has to be constrained to 0. Most simply, if $\rho = \theta = \lambda = 0$, the general spatial model reduces to a simple random or fixedeffects model. Setting θ and λ equal to zero yields the socalled spatial lag or spatial autoregressive (SAR) model, in which the spatial interaction comes from the endogenous variable. Instead, excluding ρ and θ from (6) yields the spatial error model (SER). If λ equals 0, the spatial Durbin model (SDM) is obtained. Even if the SDM does not explicitly account for a potential spatial error, it is nonetheless robust to error dependence (LeSage and Pace 2009; Elhorst 2010a). Owing to the explicit consideration of two of the three forms of spatial correlation and robustness with respect to the third form, we do not investigate spatial models other than the SDM and its two special cases SAR and SEM.

Our variable of interest is the redistribution index discussed in Section 2. However, we investigate only the redistribution through cantonal and municipal PIT, ignoring that through federal PIT because federal PIT is strongly progressive and would lead us to wrong conclusions when regressing overall redistribution on a set of cantonal institutional factors and other cantonal control variables.

4 Regression results

4.1 First results: Insight into the redistribution process

In our baseline model we include the institutional proxies of direct democracy. Therefore regressions could be run with all 26 cantons. We start with the simple pooled OLS regression and compare this model with the random-effects model. The Breusch–Pagan test advocates that the simple OLS model has to be strongly rejected (at the 0.1% level), favoring the random-effects model. However, the Hausman test indicates that the random-effects model must be rejected against the fixed-effects model at the 0.1% level. As noted above, it seems important to control for potential spatial correlation. We estimate the SAR and SEM versions of the fixed-effects model, finding that no spatial correlation remains in the endogenous variable and error term, as both λ and ρ are insignificant. The likelihood ratio test on the SEM model against the simple fixed-effects model, however, proposes a weak superiority (p=0.089) of the SEM model. This leads to the SDM model. For this, one has to decide which exogenous coefficients may be spatially lagged. We rely on the economic theory of tax competition and reconsider the variables *fiscal decentralization*, *federal equalization scheme*, *other fiscal revenue*, and *income inequality (Gini)* as factors that may be

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[6] W is row-standardized (the sum of the elements in each row of the matrix equals 1).

spatially correlated. The variable *tax competition* is not reconsidered as, in its original form, it is already weighted with the *W* matrix. While the spatial lag variable is still insignificant, the parameters of the spatially lagged exogenous variables are jointly significant at the 0.1%-level. The LM tests lead us to reject SAR and SEM.[7]

We have now found our preferred model: the SDM. In a first specification (see column (1) in Table 3), we regress the redistribution parameter on the institutions of direct democracy. Results show that the fiscal referendum variable is significantly negative at the 1% level, meaning that a higher signature requirement leads to less redistribution. In other words, having strong institutions of direct democracy leads to more redistribution. As discussed above, the institutional variables rarely change between years, and the significant results may be completely random. Therefore, in specification (2), we apply the 3-year average of the number of initiatives and fiscal referendums. The 3-year average includes the values of the year under investigation and the two preceding years. The result for the fiscal referendum variable is still significant at the 1%-level; however, the relationship is completely different. The negative sign now means that a more intensive use of the fiscal referendum variable leads to lower redistribution. This is the classical relationship found in the empirical literature that relates to governmental expenditure and revenue. Disentangling the frequency of fiscal referendums into optional and mandatory fiscal referendums shows that the dampening impact of the referendum on redistribution comes solely from the cantons with a mandatory system (see specification (3)).

In addition to the 3-year average of the measure for the use of direct democracy, we also include 5- and 10-year averages. With these more smoothed variables, the measure for fiscal referendum gets less significant (5-year average, see specification (4) and (5)) and is even insignificant (10-year average, see specification (6) and (7)). In contrast, the measure for the use of the initiative becomes highly significant. The results suggest that a more intensive use of the initiative leads to significantly more redistribution. In other words, in the short run, fiscal referendums may dampen redistribution successfully. In the long run, however, initiatives may override the dampening effect.

The spatial lag of redistribution is highly significant and negative. This finding suggests a negative correlation of redistribution between neighboring cantons. A negative spatial lag can be explained by the possibility of external effects. Consider two neighboring cantons, one of which reduces income tax rates below the level of the other. Consequently, mobile households with above-average income would migrate from the high-tax canton to the low-tax canton. Migration then reduces the overall redistribution of the high-tax canton, while it increases the progressivity of the low-tax canton. Therefore, if migration-specific externalities are present, a negative spatial correlation between neighboring cantons exists. We can support our argument with the results of Frey et al. (2016), who find that lower tax rates in neighboring cantons reduce top-income shares in a canton.

Overall, our results fit well with previous outcomes of the empirical literature – as long as the variables of usage of direct democracy are implemented in the model. Instead, our institutional variables (signature requirement in order to trigger a referendum) give completely opposite results. We have two potential explanations for this phenomenon. First, the significance of the fiscal referendum variable may be spurious, as the proxy is almost a cross-sectional variable that only changes in two cantons over the period under investigation. There is some indication for the spurious relationship. The impact of the fiscal referendum variable becomes insignificant if the pooled OLS or random effects model is used instead

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[7]The results of the tested models can be found in Online Appendix A6.

Table 3 Explaining redistribution –regression results

Proxy of direct democracy	(1) Institution	(2) (3y)	(3) (3y)	(4) (5y)	(5) (5y)	(6) (10y)	(7) (10y)
Number of initiatives		0.003	0.004*	0.003	0.003	0.020***	0.017***
		(0.002)	(0.002)	(0.002)	(0.002)	(0.006)	(0.005)
Number of fiscal referendums		–0.005***		–0.005*		0.004	
		(0.002)		(0.003)		(0.007)	
N of optional fiscal ref.			0.007		–0.004		–0.010
			(0.006)		(0.010)		(0.008)
N of mandatory fiscal ref.			–0.006***		–0.005*		0.007
			(0.002)		(0.003)		(0.008)
Initiative	–0.038						
	(0.072)						
Referendum	–0.197***						
	(0.036)						
Gini	–0.029**	–0.025*	–0.024*	–0.024*	–0.024*	–0.025**	–0.026**
	(0.012)	(0.013)	(0.013)	(0.013)	(0.013)	(0.012)	(0.012)
Left–wing gov.	0.002	0.002	0.002	0.002	0.002	0.002	0.003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)
Fragmentation	–0.000	–0.000	0.000	–0.000	–0.000	–0.000	–0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Homeowner	–0.040**	–0.030*	–0.029*	–0.029*	–0.029*	–0.029*	–0.030*
	(0.016)	(0.016)	(0.017)	(0.017)	(0.017)	(0.017)	(0.017)
Tax comp.	–0.249	–0.878	–0.873	–0.920	–0.925	–0.933	–0.637
	(1.547)	(1.503)	(1.564)	(1.577)	(1.573)	(1.287)	(1.261)
Decentralization	–0.015***	–0.012**	–0.011**	–0.012***	–0.012***	–0.015***	–0.017***
	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)
Fiscal eq. scheme	0.849	1.133	1.011	1.193	1.187	0.996	0.918
	(0.788)	(0.851)	(0.844)	(0.830)	(0.850)	(0.788)	(0.765)
Other revenues	0.025	0.072	0.114	0.046	0.051	–0.056	–0.112
	(0.187)	(0.186)	(0.192)	(0.205)	(0.230)	(0.198)	(0.206)
log(Population)	0.008	0.011	0.010	0.012	0.012	0.013	0.013
	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)	(0.010)	(0.009)
<20 years	0.198***	0.201***	0.199***	0.203***	0.203***	0.214***	0.202***
	(0.046)	(0.043)	(0.043)	(0.043)	(0.043)	(0.041)	(0.038)
>64 years	0.052	0.066	0.055	0.063	0.063	0.084	0.086*
	(0.065)	(0.060)	(0.061)	(0.061)	(0.062)	(0.061)	(0.052)
Tertiary sector	–0.008	0.008	0.009	0.007	0.008	0.004	–0.005
	(0.022)	(0.017)	(0.017)	(0.016)	(0.018)	(0.017)	(0.019)
Unemployment	0.004	0.007	0.003	0.004	0.004	0.009	0.003
	(0.030)	(0.033)	(0.034)	(0.033)	(0.033)	(0.034)	(0.034)
German	–0.035	–0.035	–0.037*	–0.036	–0.036*	–0.053**	–0.051**
	(0.022)	(0.023)	(0.022)	(0.023)	(0.022)	(0.024)	(0.022)
Catholics	–0.001	–0.004	–0.005	–0.003	–0.003	0.023	0.022
	(0.021)	(0.021)	(0.020)	(0.021)	(0.021)	(0.024)	(0.022)

Table 3 (continued)

Proxy of direct democracy	(1) Institution	(2) (3y)	(3) (3y)	(4) (5y)	(5) (5y)	(6) (10y)	(7) (10y)
Urbanity	–0.039	–0.021	–0.011	–0.014	–0.014	0.055	0.056
	(0.104)	(0.104)	(0.099)	(0.103)	(0.102)	(0.099)	(0.095)
Left–wing voters	0.000	–0.001	–0.001	–0.001	–0.001	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Spatial rho	–0.541**	–0.538**	–0.560**	–0.547**	–0.548**	–0.567**	–0.567**
	(0.250)	(0.266)	(0.274)	(0.271)	(0.272)	(0.269)	(0.263)
R^2	0.129	0.335	0.391	0.363	0.367	0.382	0.386

* $p<0.10$, ** $p<0.05$, *** $p<0.01$. N = 294; regressions performed with 21 cantons. Note: All estimations are performed with a fixed effects Spatial Durbin Model. Robust standard errors (clustered at cantonal level) are in parentheses. Time dummies and estimates for θ (coefficients for the spatial lag of decentralization, fiscal equalization scheme, other revenues, and Gini) are not shown

of the fixed effects model. Second, the institutional variable and the variables of usage of direct democracy measure two different things. The existence of institutional conditions has a disciplinary effect on politicians to discourage deviation from voter preferences.

4.2 Test for endogeneity and other robustness checks

Cantons that are more conservative tend to have stronger direct democratic institutions. Therefore, if there is no control for voters' preferences, this may induce omitted variable bias. Funk and Gathmann (2013) find that the correlation between direct democracy and government spending declines by roughly 20% when they control for voters' preferences.

Possible endogeneity is another problem that may arise in this empirical investigation. An increasing tax burden and redistribution may motivate voters to use the referendum against new expenditures and/or higher taxes more often.

To address the question of whether the cultural background of the cantons may influence both direct democracy and redistribution, we repeat different SDMs of Table 3 (see Table 4a) by excluding the following cultural factors: the share of German native speakers, the share of Roman Catholics in population, the share of the population that lives in an urban municipality, and the share of left-wing voters. There is virtually no change in either the magnitude of the coefficients or the significance levels of the proxies for direct democracy when excluding the cultural variables. We take this result as an indication that there is no fundamental multicollinearity problem between direct democracy variables and cultural factors.

We conduct two-stage least-squares to test whether endogeneity is prevalent. The beforementioned cultural variables are used as instruments for the direct democracy variables.

Table 4b shows the first- and second-stage results of the instrumental variable (IV) regression. Concerning the first stage, it can be seen that the instruments are not all good. In particular, the cultural variables only correlate significantly with the longterm proxies for direct democracy usage (the ten-year average of the number of initiatives and fiscal referendums).

In the second stage, all the direct democracy variables become insignificant. The test of overidentifying restrictions suggests that the instruments are exogenous in all model specifications. However, the under-identification test statistics (Lagrange multiplier (LM) test)

Table 4 Robustness checks

a) Basic regressions without controlling for cultural variables (reproduction of Table 3)

Corresponding model specification in Table 3	(1)	(3)	(5)	(7)
Proxy of direct democracy	Institutions	3y-average	5y-average	10y-average
Initiative	−0.028	0.004*	0.003	0.014***
	(0.07)	(0.00)	(0.00)	(0.00)
Referendum	−0.193***			
	(0.04)			
Number of optional fiscal ref.		0.006	−0.005	−0.015*
		(0.01)	(0.01)	(0.01)
Number of mandatory fiscal ref.		−0.006***	−0.005*	0.004
		(0.00)	(0.00)	(0.01)
R2	0.211	0.285	0.280	0.306

Table 4 (continued)

b) 2SLS-Estimations

	(a)			(b)			(c)		
	First stage referendum	First stage initiative	Second stage	First stage referendum (3y)	First stage initiative (3y)	Second stage (3y)	First stage referendum (10y)	First stage initiative (10y)	Second stage (10y)
Initiative	−0.016		−0.294			−0.007			−0.012
	(0.02)		(1.12)			(0.02)			(0.01)
Referendum			0.609			0.007			0.003
			(0.89)			(0.02)			(0.03)
German	0.001	0.001		0.204	−0.383		0.898**	0.480*	
	(0.02)	(0.02)		(0.63)	(0.53)		(0.38)	(0.28)	
Catholics	0.001	−0.010		−0.745**	0.289		−0.908***	−0.978***	
	(0.02)	(0.02)		(0.34)	(0.35)		(0.32)	(0.30)	
Urbanity	−0.015	0.017		0.106	0.269		−0.788	−2.484**	
	(0.09)	(0.09)		(2.50)	(1.71)		(1.18)	(1.11)	
Left-wing voters	0.003	0.004		−0.190**	−0.161***		−0.131***	−0.062**	
	(0.00)	(0.00)		(0.09)	(0.05)		(0.04)	(0.03)	
Hansen-J (p.-val.)			0.6995			0.4418			0.257
LM-Test (p.-val.)			0.473			0.185			1.277
Cragg-Donald F[a)]			0.6231			0.0007			2.901
C-Statistics (p.-val.)			0.4796			0.9638			0.023

* $p<0.10$, ** $p<0.05$, *** $p<0.01$. Robust standard errors (clustered at cantonal level) in parentheses [a)] Stock and Yogo (2005) critical value for 30%-IV-bias: 4.73. 2SLS are performed with the help of Fixed Effects Regression

Table 5 Explaining tax progression –regression results

Proxy of direct democracy	(1) Institutions	(2) 3-year average	(3) 3-year average	(4) 5-year average	(5) 5-year average	(6) 10-year average	(7) 10-year average
Number of initiatives		−0.020	−0.016	−0.021	−0.021	0.137***	0.116***
		(0.017)	(0.017)	(0.021)	(0.022)	(0.042)	(0.044)
N of fiscal referendums		−0.015		−0.009		0.046	
		(0.017)		(0.025)		(0.039)	
N of optional fiscal ref.			0.067		−0.001		−0.083
			(0.058)		(0.080)		(0.058)
N of mandatory fiscal ref.			−0.027		−0.011		0.073
			(0.018)		(0.026)		(0.046)
Initiative	−0.105						
	(0.517)						
Referendum	−1.481***						
	(0.292)						
Gini	−0.331***	−0.290***	−0.284***	−0.289***	−0.289***	−0.302***	−0.314***
	(0.065)	(0.072)	(0.074)	(0.072)	(0.073)	(0.066)	(0.064)
Left-wing gov.	−0.013	−0.014	−0.016	−0.014	−0.014	−0.013	−0.007
	(0.011)	(0.011)	(0.011)	(0.011)	(0.012)	(0.010)	(0.012)
Fragmentation	0.002	0.003	0.004	0.003	0.003	0.001	0.000
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
Homeowner	−0.167	−0.092	−0.081	−0.088	−0.087	−0.092	−0.102
	(0.128)	(0.123)	(0.127)	(0.120)	(0.122)	(0.119)	(0.114)
Tax comp.	−15.197	−20.834*	−20.695*	−20.699*	−20.733*	−19.884*	−17.218
	(11.902)	(12.443)	(12.562)	(12.454)	(12.310)	(11.224)	(11.609)
Decentralization	0.009	0.030	0.040	0.029	0.029	0.008	−0.011
	(0.041)	(0.048)	(0.046)	(0.047)	(0.045)	(0.040)	(0.038)
Fiscal eq. scheme	8.931	11.812*	10.866	11.328*	11.277*	10.117	9.405
	(6.405)	(7.003)	(7.167)	(6.619)	(6.710)	(6.357)	(6.397)
Other revenues	1.244	1.160	1.476	1.361	1.403	0.602	0.105
	(1.244)	(1.466)	(1.546)	(1.398)	(1.575)	(1.240)	(1.234)
log(Population)	0.121*	0.135*	0.132*	0.131*	0.130*	0.143*	0.147**
	(0.069)	(0.074)	(0.072)	(0.077)	(0.077)	(0.077)	(0.073)
<20 years	1.400***	1.427***	1.413***	1.410***	1.411***	1.505***	1.394***
	(0.312)	(0.327)	(0.321)	(0.323)	(0.323)	(0.300)	(0.296)
>64 years	0.666	0.760*	0.679	0.765*	0.760*	0.900**	0.911***
	(0.423)	(0.422)	(0.442)	(0.423)	(0.424)	(0.404)	(0.333)
Tertiary sector	0.077	0.215	0.221	0.209	0.214	0.181	0.095
	(0.187)	(0.176)	(0.168)	(0.178)	(0.188)	(0.168)	(0.182)

Table 5   (continued)

Proxy of direct democracy	(1) Institutions	(2) 3-year average	(3) 3-year average	(4) 5-year average	(5) 5-year average	(6) 10-year average	(7) 10-year average
Unemployment	–0.146	–0.165	–0.196	–0.159	–0.160	–0.095	–0.156
	(0.155)	(0.168)	(0.171)	(0.165)	(0.164)	(0.160)	(0.154)
German	–0.271*	–0.297*	–0.308*	–0.292*	–0.293*	–0.407**	–0.393**
	(0.163)	(0.168)	(0.171)	(0.169)	(0.166)	(0.187)	(0.180)
Catholics	–0.076	–0.085	–0.088	–0.088	–0.089	0.106	0.095
	(0.141)	(0.144)	(0.140)	(0.148)	(0.148)	(0.175)	(0.168)
Urbanity	0.095	0.215	0.290	0.221	0.226	0.787	0.788
	(0.746)	(0.754)	(0.763)	(0.776)	(0.779)	(0.763)	(0.733)
Left-wing voters	–0.007	–0.018	–0.016	–0.016	–0.016	0.003	0.003
	(0.023)	(0.019)	(0.019)	(0.021)	(0.021)	(0.025)	(0.024)
Spatial rho	–1.024***	–0.995***	–1.014***	–0.997***	–0.998***	–1.010***	–1.024***
	(0.211)	(0.206)	(0.214)	(0.206)	(0.210)	(0.212)	(0.210)
R^2	0.232	0.288	0.295	0.291	0.292	0.273	0.268

* $p<0.10$, ** $p<0.05$, *** $p<0.01$. N= 294. Note: All estimations are performed with a fixed effects Spatial Durbin Model. Robust standard errors (clustered at cantonal level) are in parentheses. Time dummies and estimates for θ (coefficients for the spatial lag of decentralization, fiscal equalization scheme, other revenues, and Gini) are not shown

show that the instruments are valid in one out of three model specifications. The Cragg-Donald Wald F-Statistics suggest that the instruments are correlated only very weakly with the endogenous regressors. According to Stock and Yogo's (2005) critical values, the IV relative bias may be far more than 30% in all three estimates. Finally, the hypothesis of exogeneity (C-test) can only be rejected in one of the three model specifications. Based on these test results, we assume that the problem of reverse causality must be ignored for further analysis.

4.3 Tax schedule

To get more insight into the redistribution process of direct democracies, we now analyze the driving forces behind the two main tools for redistribution policy: tax progression and the average tax rate. As before, the different tests lead us to the SDM model.

In the first model specification, we regress tax progression on the institutional variables (and the other control variables). As for the redistribution estimates, there is a negative relationship; a lower signature requirement for a fiscal referendum leads to higher tax progression. In contrast, the number of fiscal referendums carried out seems to have no impact on tax progression (see specifications (2) to (7) in Table 5). However, in the long run, an increasing number of initiatives leads to higher tax progression (see specifications (6) and (7)).

Next, we investigate the impact of direct democracy on the average tax burden. As before, it turns out that the SDM is the preferred model when using the average tax effect as the dependent variable (see Table 6).

Our results suggest that the fiscal referendum variable has no impact on average tax burden. The nonsignificant relationship is not implausible. First, one has to distinguish between

Table 6 Explaining average tax burden – regression results

Proxy of direct democracy	Institutions	3-year average	3-year average	5-year average	5-year average	10-year average	10-year average
Number of initiatives		0.023***	0.023***	0.021	0.021	–0.013	–0.010
		(0.006)	(0.007)	(0.015)	(0.016)	(0.016)	(0.016)
N of fiscal referendums		–0.009		–0.015		–0.023	
		(0.010)		(0.013)		(0.019)	
N of optional fiscal ref.			–0.015		–0.008		–0.004
			(0.030)		(0.038)		(0.034)
N of mandatory fiscal ref.			–0.008		–0.016		–0.027
			(0.011)		(0.013)		(0.021)
Initiative	–0.138						
	(0.277)						
Referendum	0.225						
	(0.211)						
Gini	0.081**	0.070**	0.070**	0.072**	0.073**	0.080***	0.082***
	(0.034)	(0.031)	(0.031)	(0.030)	(0.031)	(0.030)	(0.031)
Left–wing gov.	0.014**	0.014**	0.014**	0.013**	0.013**	0.014**	0.013**
	(0.005)	(0.006)	(0.006)	(0.005)	(0.006)	(0.006)	(0.005)
Fragmentation	–0.003**	–0.003**	–0.003**	–0.003*	–0.003*	–0.003**	–0.003**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Homeowner	–0.017	–0.030	–0.031	–0.027	–0.026	–0.017	–0.016
	(0.057)	(0.053)	(0.053)	(0.053)	(0.053)	(0.052)	(0.051)
Tax comp.	1.270	2.921	2.903	2.883	2.862	1.698	1.312
	(6.981)	(6.761)	(6.729)	(6.759)	(6.798)	(6.992)	(7.258)
Decentralization	–0.070**	–0.072**	–0.073**	–0.069**	–0.069**	–0.067**	–0.064**
	(0.029)	(0.029)	(0.029)	(0.030)	(0.030)	(0.030)	(0.029)
Fiscal eq. scheme	–1.164	–1.914	–1.836	–1.310	–1.355	–1.463	–1.361
	(2.846)	(2.827)	(2.862)	(2.876)	(2.877)	(2.975)	(3.008)
Other revenues	–0.903	–0.675	–0.701	–0.860	–0.824	–0.765	–0.694
	(0.645)	(0.632)	(0.653)	(0.711)	(0.723)	(0.691)	(0.701)
log(Population)	–0.045	–0.045	–0.045	–0.037	–0.038	–0.038	–0.039
	(0.030)	(0.031)	(0.031)	(0.033)	(0.033)	(0.032)	(0.031)
<20 years	0.025	0.028	0.029	0.047	0.047	0.019	0.035
	(0.141)	(0.144)	(0.142)	(0.145)	(0.145)	(0.143)	(0.149)
>64 years	–0.098	–0.102	–0.095	–0.115	–0.119	–0.126	–0.128
	(0.142)	(0.137)	(0.142)	(0.140)	(0.142)	(0.140)	(0.140)
Tertiary sector	–0.000	–0.030	–0.030	–0.033	–0.029	–0.025	–0.013
	(0.096)	(0.096)	(0.095)	(0.096)	(0.097)	(0.094)	(0.099)
Unemployment	0.022	0.050	0.052	0.032	0.031	0.006	0.015
	(0.164)	(0.158)	(0.160)	(0.159)	(0.159)	(0.156)	(0.163)
German	0.035	0.053	0.054	0.048	0.047	0.062	0.060
	(0.095)	(0.089)	(0.090)	(0.087)	(0.088)	(0.093)	(0.093)
Catholics	0.022	0.015	0.015	0.021	0.020	–0.010	–0.009
	(0.062)	(0.059)	(0.058)	(0.061)	(0.061)	(0.070)	(0.071)

Table 6 (continued)

Proxy of direct democracy	Institutions	3-year average	3-year average	5-year average	5-year average	10-year average	10-year average
Urbanity	–0.040	–0.047	–0.053	–0.025	–0.021	–0.129	–0.130
	(0.425)	(0.414)	(0.418)	(0.418)	(0.421)	(0.421)	(0.421)
Left–wing voters	0.014	0.017	0.016	0.014	0.014	0.010	0.011
	(0.011)	(0.012)	(0.012)	(0.013)	(0.013)	(0.012)	(0.012)
Spatial rho	–1.445***	–1.392***	–1.393***	–1.372***	–1.370***	–1.414***	–1.416***
	(0.274)	(0.249)	(0.251)	(0.255)	(0.255)	(0.270)	(0.272)
R^2	0.004	0.005	0.005	0.004	0.004	0.009	0.009

* $p<0.10$, ** $p<0.05$, *** $p<0.01$. N= 294. Note: All estimations are performed with a fixed effects Spatial Durbin Model. Robust standard errors (clustered at cantonal level) are in parentheses. Time dummies and estimates for θ (coefficients for the spatial lag of decentralization, fiscal equalization scheme, other revenues, and Gini) are not shown

Table 7 Explaining top income tax rate – regression results

Proxy of direct democracy	(1) Institutions	(2) (3y)	(3) (3y)	(4) (5y)	(5) (5y)	(6) (10y)	(7) (10y)
Number of initiatives		0.015	0.012	0.011	0.007	0.015	–0.000
		(0.011)	(0.012)	(0.023)	(0.024)	(0.034)	(0.037)
N of fiscal referendums		–0.015		–0.030		–0.088***	
		(0.013)		(0.023)		(0.023)	
N of optional fiscal ref.			–0.063		–0.105*		–0.181***
			(0.039)		(0.059)		(0.052)
N of mandatory fiscal ref.			–0.008		–0.020		–0.069**
			(0.013)		(0.022)		(0.028)
Initiative	–0.172						
	(0.649)						
Referendum	0.685**						
	(0.275)						
Gini	–0.031	–0.053	–0.057	–0.051	–0.054	–0.030	–0.039
	(0.045)	(0.045)	(0.046)	(0.044)	(0.045)	(0.040)	(0.041)
Left–wing gov.	0.007	0.007	0.008	0.005	0.007	0.007	0.010
	(0.011)	(0.011)	(0.011)	(0.011)	(0.012)	(0.011)	(0.012)
Fragmentation	0.000	0.000	0.000	0.001	0.000	0.001	–0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Homeowner	–0.187	–0.219*	–0.226*	–0.209*	–0.216*	–0.164	–0.171
	(0.124)	(0.122)	(0.123)	(0.123)	(0.122)	(0.116)	(0.116)
Tax comp.	21.867*	26.065*	25.933**	26.501**	26.827**	21.957*	24.138*
	(12.867)	(13.416)	(13.162)	(13.391)	(12.885)	(12.962)	(12.359)
Decentralization	–0.138***	–0.143***	–0.149***	–0.137***	–0.141***	–0.122**	–0.135***
	(0.049)	(0.047)	(0.048)	(0.048)	(0.047)	(0.048)	(0.049)

Table 7   (continued)

	(1) Institutions	(2) (3y)	(3) (3y)	(4) (5y)	(5) (5y)	(6) (10y)	(7) (10y)
Proxy of direct democracy							
Fiscal eq. scheme	−10.499*	−11.227**	−10.679*	−10.413*	−9.969*	−11.889**	−12.415**
	(5.656)	(5.723)	(5.701)	(6.019)	(5.937)	(5.705)	(5.793)
Other revenues	1.903*	2.146**	1.963**	2.171**	1.799*	2.363**	2.005*
	(1.050)	(1.050)	(0.970)	(1.089)	(0.988)	(1.112)	(1.090)
log(Population)	−0.018	−0.016	−0.014	−0.004	0.002	0.029	0.032
	(0.038)	(0.038)	(0.038)	(0.042)	(0.042)	(0.046)	(0.043)
<20 years	0.455	0.477	0.485	0.499	0.496	0.437	0.356
	(0.334)	(0.327)	(0.321)	(0.326)	(0.315)	(0.308)	(0.317)
>64 years	−0.012	−0.044	0.002	−0.061	−0.014	−0.090	−0.083
	(0.418)	(0.386)	(0.368)	(0.383)	(0.359)	(0.359)	(0.302)
Tertiary sector	−0.007	−0.081	−0.084	−0.090	−0.127	−0.086	−0.149
	(0.145)	(0.146)	(0.142)	(0.141)	(0.131)	(0.131)	(0.141)
Unemployment	0.196	0.220	0.237	0.204	0.220	0.130	0.085
	(0.221)	(0.209)	(0.214)	(0.205)	(0.213)	(0.205)	(0.207)
German	0.379**	0.402***	0.409***	0.402***	0.412***	0.440***	0.450***
	(0.161)	(0.155)	(0.153)	(0.154)	(0.149)	(0.159)	(0.151)
Catholics	−0.049	−0.058	−0.057	−0.062	−0.059	−0.116	−0.124
	(0.100)	(0.094)	(0.093)	(0.099)	(0.096)	(0.111)	(0.104)
Urbanity	0.736	0.681	0.633	0.713	0.669	0.663	0.672
	(0.595)	(0.575)	(0.577)	(0.567)	(0.568)	(0.551)	(0.541)
Left–wing voters	0.048***	0.049***	0.047***	0.046***	0.046***	0.038***	0.038***
	(0.012)	(0.013)	(0.013)	(0.015)	(0.015)	(0.012)	(0.011)
rho	−0.759***	−0.712***	−0.718***	−0.706***	−0.704***	−0.701***	−0.671***
	(0.183)	(0.180)	(0.184)	(0.181)	(0.187)	(0.174)	(0.178)
R^2	0.044	0.050	0.052	0.043	0.044	0.027	0.028

* $p<0.10$, ** $p<0.05$, *** $p<0.01$. N= 294. Note: All estimations are performed with a fixed effects Spatial Durbin Model. Robust standard errors (clustered at cantonal level) are in parentheses. Time dummies and estimates for θ (coefficients for the spatial lag of decentralization, fiscal equalization scheme, other revenues, and Gini) are not shown

statutory tax rates and effective average tax rates. Here, the effective average tax burden is investigated. However, political decision-making concerns statutory tax rates. For example, people may vote for lower tax rates, which may not lead to lower effective average tax rates. The effective average tax rate may even increase after a tax rate reduction in systems with a progressive schedule, if, for example, people increase their labor participation or if wealthy people immigrate.

If we replace the average tax burden with the top income tax rate (average tax rate for a household earning CHF 1 million per year), then we find the expected negative relationship for our long-term proxy of direct democracy (10-year-average); a higher number of optional and mandatory fiscal referendums reduces the top income tax rate significantly. Also, we find a positive relationship for the institutional proxy; a higher signature requirement for

a fiscal referendum increases the top income tax rate. However, the number of initiatives seems to have no impact on top income tax rates (see Table 7).

5 Conclusion

Our results show that it is important to look carefully at the combination of tax policy instruments when investigating the redistribution policy of states. In simple terms, policy has two methods of redistributing: by means of the average tax burden and through tax progression. As the decomposition indicators suggest, even in jurisdictions with an overall low tax rate, redistribution can be substantial if the tax schedule is progressive. As a side effect, the overall low tax burden attracts wealthy persons, thus raising the amount of redistribution.

The diversity of our regression results leads us to conclude that the effects of direct democracy on income tax redistribution is a multilayered process. First, the theoretical availability of direct democratic tools does not seem to have the same impact as the effective use of them. Second, as previous literature finds with respect to governmental revenue and expenditures, we conclude that the prevalence of popular initiatives increases tax progression and redistribution in the long term. Third, the use of fiscal referendums may reduce the top income tax rate. However, at the same time, fiscal referendums seem to have only short-term effects or even no effect on the average tax burden, tax progression, and redistribution. This is no contradiction, as our results show that there is a highly significant negative spatial correlation between nearby cantons, suggesting that there are negative spillover effects. There remains the question of whether voters are capable of assessing the impact of collective decisions on tax competition and tax mobility, a relevant question which may be addressed by future research.

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