Taxation under Direct Democracy*

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Abstract

Direct democracy sweeps the globe but its causal impacts are still widely unknown. We investigate direct democracy in its "purest" form where citizen lawmaking entirely substitutes decision-making by parliaments. Town meetings (popular assemblies) replace local councils in German municipalities below a specific population threshold. We show that tax policies of local governments change at the threshold. Property tax rates, which apply to all residents, decrease by 9 to 17% under direct democracy, and expenditures on investment projects come down. Business tax rates, by contrast, do not change. We conclude that direct democracy entails incentives to reduce spending and to share the dividend equally among citizens.

JEL-Classification: D71, D72, H71

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1 Introduction

Direct democracy is literally popular. Many US states introduced initiatives and referendums in past decades, and not a single state ever abolished direct democratic instruments (Matsusaka, 2005a). Parliaments delegate decision making back to the people and submit important laws to referendums. Important examples at the national level are Brexit (2016), the new Turkish constitution (2017) or same-sex marriage and abortion laws in Ireland (2015, 2018). Also initiatives aiming at reversing or substituting parliamentary decisions emerge at all levels of government around the globe. An evident key question therefore is whether direct democracy yields different policy outcomes than public choice by parliaments as predicted by theoretical models (Romer and Rosenthal, 1979; Noam, 1980; Frey, 1994; Gerber, 1996; Maskin and Tirole, 2004; Matsusaka, 2018).

Scholars have extensively studied direct democratic instruments *complementing* representative democracy: initiatives and referendums (for a survey, see Matsusaka (2018)). Referendums tend to be associated with less spending, tax cuts and deficit reductions (Feld and Kirchgaessner, 2001a,b; Feld and Matsusaka, 2003; Nguyen-Hoang, 2012; Lewis *et al.*, 2015). Evidence on initiatives is less conclusive showing decreases in spending, public employment and taxes (Matsusaka, 1995, 2009; Funk and Gathmann, 2011), mixed findings (Besley and Case, 2003; Blume *et al.*, 2009), and also higher levels of expenditures and tax rates under direct democracy (Asatryan, 2016; Asatryan *et al.*, 2017a,b). Reliable causal evidence however remains scarce because endogeneity issues apply in many studies. Few randomized field experiments in developing countries show that policies barely change under direct democracy (Olken, 2010; Beath *et al.*, 2017).[1]

Against the background of limited causal evidence, scholars have also hardly paid attention to settings where direct democracy *substitutes* rather than complements decision making by parliaments. In the "purest" form of direct democracy, citizens instead of elected councils gather in popular assemblies to legislate policies. This town meeting form of government is widespread in New England local governments in the US but also in some

[1]The experimental studies however strongly suggest that satisfaction with policy making increase under direct democracy in developing countries. The survey study by Besley *et al.* (2005) shows that direct democracy also improves targeting resources to the poor.

Swiss cantons such as Glarus or the Appenzells.[2] Few studies so far have investigated full citizen lawmaking. Salvino *et al.* (2012) find no fiscal effects of town meetings in a cross-section of New England local governments. Hinnerich and Pettersson-Lidbom (2014) show that welfare spending decreases in Swedish local governments which opted for a town meeting form of government in the interwar period. Sanz (2019) investigates small Spanish municipalities and shows that expenditures and revenues decrease in town meeting municipalities. Previous studies, however, have to deal with concerns regarding sorting into treatment and focus on budget aggregates.

In this paper, we estimate the causal effect of *pure* direct democracy on tax policies and on further budget and political outcomes. We exploit a quasi-experimental setting in Germany where municipalities autonomously decide on tax rates on property and business. In the German federal state of Schleswig-Holstein, town meetings (popular assemblies) replace local councils in municipalities which have 70 and fewer inhabitants at a specific cut-off day some 30 months before a local election. All other rules are equal, for example, fiscal grants by the state government. We investigate whether tax policies of local governments change at the threshold. Our results show that property tax rates, which apply to all residents, decrease under direct democracy. Effects are economically substantial and amount to 0.4 to 0.8 standard deviations in property tax rates. Also capital expenditures and debt decrease in town meeting municipalities. By contrast, we hardly find evidence that business tax rates change. Direct democracy seems to entail incentives to run policies for "the masses" rather for than special interest groups (see also, Gerber (1999); Lewis *et al.* (2015); Asatryan *et al.* (2017b)) and to break the "cartel of politicians directed against voters and taxpayers" (Frey, 1994, p. 338).

What about the external validity of our findings? An obvious issue is whether tiny municipalities with a population of around 70 allow to draw any general conclusion. We believe that our findings may well have implications for larger jurisdictions. Columns (1) and (2) of Table 1 compare tiny municipalities (population between 3 and 140) with the average municipality of Schleswig-Holstein. As expected, sample municipalities are

[2] Around 5% of US municipalities have a town meeting form of government. 53% have a council-manager system (council appoints manager), 40% have a mayor-council system with directly elected mayors. See the ICMA Form of Government Statistics – Municipalities (2014), April 02, 2018.

by far smaller than the average municipality in terms of population (86 versus 2,509) and area (550 versus 1,415 hectare). Beside scaling, however, small municipalities fairly resemble the state average. Population shares regarding sex, nationality, age, family status, marital status, and religion barely differ between our small sample municipalities and the state average. Traffic and water areas cover around 3 to 4% of the total area and around three quarters are agriculture; only the share of the settlement area is somewhat smaller in tiny municipalities. In both groups, around one-third of the resident population are employees. 2% of the population are unemployed of which roughly 50% for more than one year (long-term unemployed). Small municipalities also remarkably resemble state average firm size (5.2 versus 6.2 employees). In conclusion, the municipalities under investigation are admittedly small in scale but highly representative in structure and composition.

[Table 1 about here]

Our paper contributes to several further strands of literature beside papers on the effects of direct democracy. First, there is an ongoing discussion whether direct democracy substitutes or complements representative democracy (Matsusaka, 2005b; Blume *et al.*, 2009). In Switzerland, direct democracy ensures checks and balances among voters and the all-party government. However, direct democracy could also undermine representative decision making and the authority of parliaments. We investigate whether voting behavior in national elections changes in town meeting municipalities. We do not find that town meeting constitutions spill over to national elections. Direct democracy neither affects voter turnout, invalid vote shares nor vote shares for right-wing populist parties including the *Alternative für Deutschland* (AfD).[3] We conclude that direct democracy does not come at the cost of representative decision making.

Second, our results suggest that voters prefer smaller public sectors than politicians and are more conservative in budgeting (Peltzman, 1992; Frey, 1994; Lowry *et al.*, 1998; Brender, 2003; Brender and Drazen, 2008; Potrafke, 2013). For example, Swiss citizens frequently decide on expenditures and tax rates in referendums and Switzerland ranks among the smallest OECD public sectors in terms of GDP. We document substantially lower levels of

[3]The AfD is a populist anti-establishment party and campaigns for direct democracy.

tax rates, debt and (capital) expenditures under direct democracy. Even in very small municipalities, voters seem to be more fiscally conservative than councils. This result is well in line with theories, for example, on budget maximization by public officials (Niskanen, 1968).

Third, we contribute to the discussion on government forms and constitutions (Persson and Tabellini, 2003). Empirical studies so far have compared presidential systems and parliamentary systems (MacDonald, 2008; Egger and Koethenbuerger, 2010; Coate and Knight, 2011; Saha, 2011; Whalley, 2013; Ade, 2014; Enikolopov, 2014; Garmann, 2015; Koeppl-Turyna, 2016; Hessami, 2018).[4] Both systems mainly differ in the executive branch. Presidential systems have direct elections of the head of government. In parliamentary systems, councils appoint the head of government. New England-style town meetings, by contrast, are a hardly explored third form of government beyond presidentialism and parliamentarism (Maskin and Tirole, 2004). Previous findings show that policy outcomes vary between an elected and an appointed executive branch. Our setting allows isolating the effects of changes in the legislative branch: citizens instead of councils make laws. We show that legislative institutions well matter to policies. Council decisions deviate from decisions by the general public. Town meetings legislate in a careful and responsible way favoring "the masses".

2 Theory

Why should citizens tax different than councils? In a conventional median voter model, preferences of a proportionally elected parliament should well mirror preferences of the electorate. However, theoretical studies have shown that politicians and voters can diverge in cases of information asymmetries, pressure groups or multidimensional issue spaces (Matsusaka, 2018). We briefly outline three different mechanisms resulting from these distortions which have been discussed in the theoretical literature: unbundling of political decisions, incentives of representatives to overspend, and the size of the legislature (for

[4] At the local level, the mayor-council form of government competes with the council-manager form of government.

overviews, see Matsusaka (2005a, 2018)). Later, we provide some empirical evidence which mechanism is likely to drive the results in our case (see, section 6).

First, direct democracy allows "unbundling" political issues. This idea was proposed by Besley and Coate (2008) and alludes the difference between multi-issue parties (standing in elections) and single-issue referendums. In elections, voters can only select from menus which may widely reflect their preferences but also include at least some special-interest policies. Direct democracy unbundles decisions and may curb special interests because voters are able to opt out. In town meetings, citizens have full control over the entire set of policies in any point of time, and not only over a bundle of policies (parties) in one point of time (election day). If special-interest spending is lower under direct democracy, we would also expect tax rates for the general public to decrease as well but not tax rates for minority groups.

Second, representative decision making induces all kinds of principal-agent complexities. Frey (1994) outlines a model of a "political class" tending to overspend at the cost of taxpayers and voters. Similar to bureaucrats trying to relocate a maximum of resources to their office (Niskanen, 1968), the "political class" as a whole might be tempted to expand their sphere as far as possible. Large budgets allow representatives to run expressive and monumental projects and to extract resources, for example, to hire fellows or even relatives (Kauder and Potrafke, 2015). A "cartel of politicians" may therefore lead to oversized public sectors with spending and tax levels beyond voters' preferences. Information asymmetries are second source of overspending. If voters are rational but imperfectly informed, increasing expenditures before elections is a reasonable strategy to bolster re-election (Rogoff, 1990). Politicians use large-scaled projects to signal power and competence to the electorate. The availability of referendums or initiatives can internalize incentives for both sources of expressive expenditures. This should hold even more true if the median voter *herself* decides on policies as in the case of town meetings. We therefore expect lower levels of tax rates and spending under direct democratic rule. In particular, capital spending which covers expenditures for "monumental projects" should come down under direct democracy.

Third, the size of the legislature may play a role. For decades, the theory of a "law of 1/n" developed by (Weingast *et al.*, 1981) dominated in the literature proposing that expenditures increase in the number of councilors and electoral districts. New theories on legislature size however have drastically challenged the conventional "law of 1/n". Primo and Snyder (2008) show that the law only holds under specific conditions. Pettersson-Lidbom (2012) even argues that larger parliaments are better able to monitor and control budget maximizing administrations and therefore produce smaller public sectors. Another issue are transaction costs which are the general argument for representative decision making. Delegating powers to councils reduces total information costs because only a fraction of the population has to gather information before deciding on political issues. Under direct democracy, by contrast, transaction costs increase. For this reason, projects which yield at least marginal returns under councils may become unprofitable under direct democracy. We would therefore expect less projects and spending if citizens instead of councils legislate. Paralleling discussions in the theoretical literature, recent empirical findings are also ambiguous. Egger and Koethenbuerger (2010) provide evidence in favor of the "law of 1/n", but neither Baskaran (2013), Hankins (2015), nor Bel *et al.* (2018) can confirm effects of the size of legislatures or cabinets. Pettersson-Lidbom (2012) and Hoehmann (2017) find even lower levels of government spending in larger councils. Thus, recent theories and evidence seem to favor an "inverted law of 1/n": larger legislatures are better able to control public expenditures than small legislatures. If public sectors decrease in legislature size, town meetings which represent a larger share of the population than councils may dampen tax rates and expenditures.

Altogether, if anything, theories on unbundling political decisions, overspending by representatives and legislature size let us expect that town meeting constitutions are associated with a smaller public sector and lower tax rates in general. In particular, "expressive" spending on large-scale projects and special-interest policies are likely to decrease under direct democracy.

3 Institutional background

3.1 Municipalities in Germany

Germany has two layers of local government similar to the US: counties (*Landkreise*) and municipalities (*Gemeinden*). Tasks of local governments vary to some extent among the 16 German federal states. The around 300 counties are responsible for social care, county roads, economic development, and public transport. Public safety and order, waste disposal, water supply, culture, and local schools and kindergartens are assigned to the around 11,000 German municipalities. Around 100 consolidated city-counties (*kreisfreie Städte*) have both counties' and municipalities' responsibilities (Roesel, 2017).

According to the German constitution, local governments enjoy a great deal of fiscal autonomy. Local councils at the municipality and at the county level legislate local by-law and decide on tax rates and budgets. Annual budgets are proposed by a usually directly elected head of local government (mayor or county administrator). Fiscal autonomy includes designing expenditures, debt, and local taxes. By 2017, local governments spent some Euro 250 billion ($ 290 billion) which is around 20% of German total public expenditures.[5]

3.2 Local property and business taxes

Fiscal autonomy of German municipalities includes the right to levy local taxes. The most important taxes are the local property tax (*Grundsteuer*) and the local business tax (*Gewerbesteuer*). Technically, local governments do not design taxes but local tax rates. The federal law defines the tax base and a basic rate (*Steuermesszahl*). Municipalities only decide on local multiplier rates (*Hebsätze*) which are multiplied with the basic rate. For example, a local business tax multiplier rate of 200 translates into an effective tax rate of $200 \times 3.5\%$ (federal basic rate) $= 7\%$. Because federal basic rates do not vary across Germany, local multiplier rates directly translate into tax rates on property and businesses. In this paper, we therefore refer to local multiplier rates as tax rates.

[5]Figures do not include expenditures of the three city states of Bremen, Hamburg, and Berlin.

The tax base of the property tax is the value of land and buildings at a specific cut-off day. There is a property tax rate for agriculture and forestry (property tax A) and a tax rate for all other property (property tax B). Property taxes affect all citizens. Tax bills are paid by the owner, and hirers are allowed to pass taxes on to renters. Business taxes, by contrast, are levied on the income of local firms.[6] Since 2004, a minimum tax rate of 200 applies to the business tax, there is no upper cap. Property tax rates have no limits at all. By 2017, average property and business tax rates are at around 400 in Germany.

Apart from local taxes, municipalities share income and value added taxes with the state and the federal government. Tax rates are set at the federal level. State governments also grant transfers to local governments. However, tax rates on property and businesses are the most important instruments of local finance in Germany. By 2017, property taxes generate total revenues of Euro 14 billion, local business taxes yield around Euro 53 billion. Revenues from property and business taxes account for around one-half of total tax revenues of local governments in Germany.

3.3 Town meetings in Schleswig-Holstein

Schleswig-Holstein is a federal state in the very North of Germany dominated by agriculture and fishing (see Figure 1). Schleswig-Holstein has around 1,100 comparably small municipalities representing 10% of all German municipalities but only 4% of total area and 3% of the German population. Municipalities vary substantially in size; population ranges from 4 to around 250,000. Responsibilities of municipalities in Schleswig-Holstein are similar to other German states.

[Figure 1 about here]

Because tiny municipalities can hardly cope with carrying out day-to-day administrative tasks, 3 to 34 municipalities form joint administrations (*Ämter*). Political decisions, however, remain at the municipality level and are allocated to mayors and two types of

[6] Income is defined as net profits, adjusted, for example, regarding interest payments. For details on the German business tax, see Baskaran (2014); Fuest *et al.* (2018)

legislative bodies: local councils and town meetings. Local councils legislate all kinds of local law including by-law, local tax rates, and the annual local budget. Councils have between 7 and 49 members, depending on population size. Local councils are elected on the basis of a mixed majoritarian-proportional system every five years.[7] Schleswig-Holstein municipalities with a population 4,000 and less have a council-manager system.

According to the German constitution, town meetings can replace local councils.[8] After WWII, several German states allowed tiny municipalities to opt for town meetings (Franke, 1996; Wollmann and Roth, 1999). However, very small municipalities disappeared in the course of large-scales municipal mergers in almost all German states in the 1970s. The states of Rhineland-Palatinate and Schleswig-Holstein are exceptions and hardly amalgamated municipalities. Rhineland-Palatinate, however, does not allow to substitute councils by town meetings. Today, Schleswig-Holstein is the only German state with municipal town meetings with legislative power.

In Schleswig-Holstein municipalities with 70 and less inhabitants, town meetings (popular assemblies) formed by all citizens eligible to vote replace the local council. All other rules and institutions are equal. Municipalities do *not* have a choice – the population threshold of 70 is sharp. Population does not refer to the population on the election day but to the 31st December three years prior to election year. Around 30 out of a total of around 1,100 municipalities in Schleswig-Holstein have a population of 70 and less. Few are located on islands, most small municipalities are on the main land (see, Figure 1). Because population varies over time, some municipalities frequently switch from local councils to town meeting, and vice versa (see, Figure A.1 in the Appendix). In our analysis, we will exploit both variation in population across municipalities and time variation within municipalities.

Table 2 reports some illustrating key facts on town meetings. We hand-collected publicly available protocols of 167 town meetings in Schleswig-Holstein. Protocols usually report the names of the citizens attending, the beginning and the end of the meeting and the agenda. Our data do not claim to be representative for the universe of town meetings but may give some feeling. On average, 13.5 citizens attend town meetings which is around

[7] Before 1995, local elections were scheduled every four years.

[8] "In municipalities a local assembly may take the place of an elected body.", see Article 28 of the German constitution.

40% of the total number of eligible voters. This is in line with Franke (1996) who reports that in around 80% of all town meeting municipalities, between 25% to 50% of eligible voters are present. However, presence varies substantially ranging from 6% to 89% of the total voting population. Town meetings last slightly less than 1.5 hours and are held in the evening. 86% of all meetings start at 7:00, 7:30 or 8:00 p.m. (not shown in Table 2). The earliest meeting in our sample started at 6:00 p.m., the latest at 9:20 p.m. In about 40% of all meetings, citizens legislate the local budget. Local tax rates are an issue in one third of all meetings.

[Table 2 about here]

Franke (1996) reports further facts. Usually, town meetings in Schleswig-Holstein are small enough to gather in local pubs, barns or even in the sitting room of the mayor. Meetings take place one to four times a year. One third of all meetings are completely held in Lower German (*Plattdeutsch*) which is a local language different from German. Family clans do not play a major role and (ideological) conflicts are rare. Franke (1996) also mentions that citizens in town meeting municipalities are highly interested in sound local finances because spending decisions map into local property taxes paid by all residents. We take this as first anecdotal evidence that town meeting municipalities may somewhat differ from council legislation when it comes to taxation.

4 Identification strategy

4.1 Regression design

Constitutions are likely to be endogenous. Robinson and Torvik (2016) show that strategic reasons predict the choice between parliamentary and presidential systems. Similar concerns apply to direct democracy. Regressing outcome variables on direct democracy measures therefore usually yields biased estimates and is not sufficient to claim causality (Matsusaka, 2018).

Our identification strategy is to compare German municipalities closely around a specific population threshold which quasi-randomly determines the form of government. Municipalities in the state of Schleswig-Holstein with a population of 70 and less at a specific cut-off day have town meetings and thus exercise "pure" direct democracy. Municipalities with 71 and more inhabitants elect a local council. We assume that at the sharp threshold, assignment into forms of government is as good as random (Lee and Lemieux, 2010). The two main conditions for this assumption are the absence of sorting and compound treatments. We will show that these conditions are met (see section 4.2 below). In our baseline specification, we restrict the sample to municipalities closely around the threshold of 70 and estimate a difference-in-differences model using OLS which takes the following form:

$$Taxrate_{it} = \alpha_i + \delta_t + \beta Townmeeting_{it} + \epsilon_{it} \tag{1}$$

$Taxrate_{it}$ is the dependent variable and describes the average in one out of three local tax rates (*Hebesätze*) of municipality i in election term t. α_i and δ_t are municipality and election term fixed effects eliminating systematic time-invariant differences across municipalities (e.g., due to local yardstick competition within counties, Buettner and von Schwerin (2016)) and general time trends and shocks. ϵ_{it} decribes the error term. We cluster standard errors at the level of municipalities. Our coefficient of interest is β. It refers to the dummy variable $Townmeeting_{it}$ which takes on the value of one for municipalities with a town meeting constitution (*Gemeindeversammlung*) and zero otherwise. The dummy variable depends on the local population at a specific cut-off day some 30 months in advance of the election. If population at the cut-off day is smaller or equal to 70, town meetings replace the local council for the entire election term.

In the baseline specification, we use a bandwidth of ± 10 inhabitants around the threshold of 70. We take this as a reasonable bandwidth balancing the power of inferences and the comparability of municipalities. However, we later show that our results do not depend on a specific bandwidth. Because we limit our baseline sample to municipalities closely around the discontinuity of 70 inhabitants, our difference-in-differences strategy is equivalent to a

regression discontinuity (RD) panel approach.[9] This allows us to take full advantage of the temporal and spatial variation in our dataset. To test the robustness of our findings, we also estimate more sophisticated RD models where we drop time and municipality fixed effects and apply a local-linear procedure including a data-driven optimal bandwidth choice (Calonico *et al.*, 2017). We take the results as supportive evidence because these models are mainly designed for cross-section analyses and abstract from the time dimension.

4.2 Excluding sorting and compound treatments

Population is our crucial parameter determining the form of government. Using (self-reported) population as forcing variable in RD settings can induce specific problems. Eggers *et al.* (2018) discuss two main concerns: sorting and compound treatments. First, municipalities may manipulate population figures to achieve a specific treatment. In our case, municipalities may strategically report too low or too high population figures and thus self-sort into government forms. Second, effects are likely to be biased if more than one institution changes at the population threshold. For example, if fiscal transfers granted by higher layers of government change at the threshold of 70 inhabitants, effects overlap and become hardly separable. In our case, however, we have good reasons to believe that neither sorting nor compound treatment issues apply.

First, we take advantage of the specific nature of the cut-off day. Local elections take place in spring every five years. The population cut-off day, however, is the 31st December some 30 months before the local election. For example, municipalities with a population of 70 and less on 31st December 2015 do not hold municipal council elections on 6th May 2018. Population on the election day and during the election term may well exceed the threshold of 70 (see, Figure 2). We later replace cut-off day population by actual population as a pseudo treatment; effects only hold under the former.

[Figure 2 about here]

[9] We therefore follow Gelman and Imbens (2018) advising parsimonious RD polynomials.

Second, political debates about the threshold induce further uncertainty. One prominent example is the 2013 local election. In March 2012, a liberal-conservative majority in the state parliament of Schleswig-Holstein increased the threshold population for town meeting municipalities from 70 to 100. However, a left-wing coalition took over after the state parliament election in May 2012 and restored the initial threshold of 70 inhabitants in December 2012 – just right before the election in May 2013. Municipalities between 70 and 100 inhabitants could not be sure about their local constitution.

Third, we argue that quasi-exogenous events leverage population in very small municipalities to even larger extent than in cities. In municipalities around the threshold, population by 31st December becomes hardly predictable because events such as a single road accident with multiple fatalities, a mother giving birth to twins, or one couple with children moving in or out may easily shift municipality population over or under the threshold of 70.

Fourth, even if actual population randomly fluctuates around the threshold of 70, local officials may misreport population strategically in order to achieve a specific treatment (either direct democracy or a local council). If local officials favor one form of government, bunching would lead to an asymmetrical distribution of observations at the threshold. We therefore test whether observation density is biased towards one side of the cut-point. However, neither eye-ball inspection (Figure A.2 in the Appendix) nor manipulation tests as suggested by McCrary (2008) (Table 3) let us suspect strategic actions in favor of one form of government.[10] Altogether, we are rather confident to exclude sorting into treatment.

[Table 3 about here]

Overlapping effects could also bias the results if there are compound treatments at the threshold of 70 inhabitants. However, we have also good reasons to believe that *only* the form of government changes at the threshold. First, we carefully screened federal and state law and did not find any other institution changing at the threshold of 70 inhabitants. For

[10]By contrast, Sanz (2019) report a bias toward local councils in Spanish municipalities.

example, fiscal transfers from the Schleswig-Holstein state government do not depend on population size at all.

Second, we can also rule out that the presence or absence of elections makes any difference. If local elections were completely absent, attitudes towards democracy and public affairs may evolve differently. However, in our setting, voters in small municipalities do also participate in local elections. In the state of Schleswig-Holstein, municipality and county council elections are held at the same day. Voters in municipalities larger than 70 inhabitants cast two ballots – one ballot for the municipality council and one ballot for the county council. In municipalities with a population of 70 and less, voters cast only the ballot for the county council election (but no ballot for the municipality elections because direct democracy applies). Therefore, elections are held in all municipalities and the presence or absence of elections cannot drive the results.

Third, covariates vary smoothly across threshold of 70. The right-hand side in Table 1 provides evidence on several observable characteristics. We perform local-linear RD regressions testing for discontinuities in variables at the population threshold of 70.[11] Table 1 reveals no significant discontinuities regarding population, land use, or the local economy. In conclusion, neither compound treatments nor manipulative sorting should bias our results. We are therefore confident that our estimates allow a causal interpretation.

4.3 Data

We collect annual data on tax rates, population, and further fiscal and political outcomes for all around 1,100 municipalities of Schleswig-Holstein between 1978 and 2017. The dataset covers nine local election periods (see, Table A.1 in the Appendix). We compute election term averages from annual data in local tax rates (*Hebesätze*) and other budget outcomes between the first year of the election term and the last full year ahead of the local election (e.g., 2003 to 2007). Election term averages avoid inflating inferences. However, we later show that our results do not change when we use annual data. We end up with one

[11] The number of observations measuring the share of long-term unemployed and firm size (employees) is too low to perform local-linear RD estimations.

observation for each municipality and election term totaling around 10,000 observations (see, Table 4, column (1)).

[Table 4 about here]

However, missing population data before 1998 are a severe data constraint. Table A.1 in the Appendix shows cut-off days determining the form of government for all local elections since 1978. Since the local election in 1998, state law defines a clear cut-off day (31st December three years ahead of the election). We are therefore able to reconstruct town meeting municipalities for four election terms between 1998 and 2017 from official population figures. Before 1998, by contrast, cut-off days were not defined by law and varied substantially. Neither state ministries nor the statistical office were able to provide us with historical population figures for cut-off days before 1998. Fortunately, Franke (1996) summarizes town meeting municipalities before 1998. However, because we do not have population figures before 1998, we are not able to identify municipalities closely around the threshold of 70. Therefore, we can only use municipalities with 70±10 inhabitants in the period from 1998 to 2017, which is therefore our baseline sample. The full sample without any population restriction over the years 1978 to 2017 delivers additional evidence but should be treated with caution.[12]

Data on expenditures and debt are not available for years before 2008. We use per capita total expenditures, staff expenditures, administrative expenditures (including, for example, expenditures on material, maintenance, and interest rates), capital expenditures,[13] and total debt in core budgets. All budget variables are election term averages and in logs.[14] Finally, we collect political economy data on voter turnout and shares of invalid votes and votes for right-wing populist parties[15] in all eleven national elections between 1980 to 2017.

[12]Moreover, in 1998, the election term increased from four to five years. The baseline sample between 1998 to 2017 is also more homogeneous in this regard.

[13]18 observations drop out because we cannot take logs of non-positive values.

[14]We add one to per capita debt before taking logs.

[15]These are the NPD, DVU, REP and AfD.

Table 4 reveals some first interesting differences between town meeting municipalities and other municipalities. Comparing columns (1) and (6), town meeting municipalities have substantially lower property tax rates (around 250 versus around 235) on average but business tax rates hardly differ (around 300). Also expenditures and election outcomes are fairly similar but debt per capita is substantially lower in town meeting municipalities. The descriptive statistics therefore already suggest that property tax rates and debt may differ in municipalities with town meetings. In the next section, we test whether differences turn out to be statistically significant.

5 Results

5.1 Baseline

Table 5 reports our baseline difference-in-differences regression results for all three local tax rates. We start with the the full sample of all municipalities observed over the period 1978 to 2017 (columns (1) to (3)). Our results let us clearly reject that business tax rates change when a municipality comes under town meeting rule (column (3)). However, estimates for property tax rates (column (1) and (2)) are less clear, p-values are at around 0.20. Keeping in mind that the full sample includes a rather heterogeneous sample of municipalities reaching from 4 to 250,000 inhabitants and town meeting municipalities account only for 2% of all observations (Table 4), we cannot reject effects of direct democracy with certainty. We therefore turn to columns (4) to (6) which present our preferred specification of a close bandwidth of 10 inhabitants around the cut-off day population of 70. Observations shrink drastically but point estimates hardly differ from the full sample. We now obtain statistically significant results for property tax A (5% level) and property tax B (1% level) but, again, null results for business taxes. We discuss the implications of this asymmetry across taxes in the mechanisms section (see, section 6).

[Table 5 about here]

Local-linear RD specifications corroborate all baseline findings. Columns (10) to (12) in Table 5 report RD estimates using a data-driven optimal bandwidth procedure. We remove time and municipality fixed effects in this specification. Property taxes are significantly smaller in municipalities under direct democratic rule, but business taxes do not differ. Finally, columns (7) to (9) combine the optimal bandwidth identified by the local-linear procedure with difference-in-differences estimates including time and municipality fixed effects. The results well support all previous findings. Different specifications therefore uniformly show that property tax rates tend to be lower in municipalities with town meetings. The effects are economically substantial and amount to 0.4 to 0.8 standard deviations in property tax rates. Figure A.3 the Appendix provides further "eyeball evidence". On average, property taxes seem to be discontinuous at the population threshold of 70 while business tax rates vary smoothly across the threshold. Albeit the local-linear RD specification in Table 5 and Figure A.3 do not account for time and municipality fixed effects, results are well in line with difference-in-differences results.

5.2 Robustness

We submit our results to several robustness tests. First, we use annual data instead of election term averages. Table A.2 in the Appendix shows that point estimates hardly change. We find significant negative effects of town meetings on general property tax rates (columns (4), (5), (8), (10) and (11)) but no effects for business tax rates in specifications other than local-linear RD (column (12)).

Second, we challenge our default bandwidth of 10 inhabitants around the threshold population of 70. We have already shown that our effects are robust when we use data-driven optimal bandwidths (see, columns (10) to (12) in Table 5). However, we also systematically test different bandwidths between 4 and 16 inhabitants and plot the resulting coefficients (Figure 3). Each dot represents the point estimate for town meetings from one separate regression where we limit the bandwidth to ± 4, ± 6, ..., ± 16 inhabitants. Vertical solid lines are 90% confidence intervals. The specification in dashed lines is our baseline specification of ± 10 (columns (4) to (6) in Table 5). The figures for property

taxes show that confidence intervals hardly include the zero, property tax B in particular. Our results therefore do not depend on a specific bandwidth choice. In contrast, effects for business tax rates are not statistically significant in any specification.

[Figure 3 about here]

Second, we perform donut regressions where we omit observations very close to the threshold (Figure 4). The 2011 census revealed a difference in total German population of around 2% between recent census data and updated earlier census data.[16] We assume that municipality officials are able to misreport population by around 2 to 3%. We therefore omit municipalities with 69 to 71 and 68 to 72 inhabitants. The results do not differ from our baseline findings (specification in dashed lines).

[Figure 4 about here]

Third, we assign pseudo thresholds. We pretend that the threshold was at a population other than 70. We compute pseudo town meeting dummies and distances to the threshold for populations of 64, 67, 70 (which is real treatment), 73 and 76 re-run our regressions. Effects for a population of 70 correspond with the real treatment. Figure 5 shows the results of this procedure. We do not detect any significant effect for business tax rates (right-hand side). Neither confidence intervals for pseudo nor for real population thresholds exclude the zero. By contrast, we observe striking patterns for property taxes. Effects hardly turn out for any pseudo threshold below or above 70 inhabitants. Only the *real* population threshold of 70 yields significant results for both property tax A and B. We conclude that effects at the threshold of 70 are unlikely to be random.

[Figure 5 about here]

Fourth, and finally, we compare our baseline results to two further pseudo analyses. We use actual population as a pseudo cut-off day population instead of the real cut-off day and

[16] Statistical Office of Germany, Press release 188/2013-05-31, "2011 Census: 80.2 million inhabitants lived in Germany on 9 May 2011 – About 1.5 million fewer inhabitants than assumed".

compute town meeting dummies accordingly. This is probably among the most challenging robustness exercises to validate our findings because actual population and cut-off day population are strongly correlated ($r = 0.92$; see Figure 2). However, if we replace cut-off day population by actual (pseudo) population, we do not observe any statistically significant effect for property taxes (see, Figure 6). Second, we use data on population and tax rates from another German state, Rhineland-Palatinate. This is the only German state with small local governments very similar to Schleswig-Holstein. However, small municipalities in Rhineland-Palatinate do *not* have town meetings. We use the same cut-off days and population thresholds which apply to Schleswig-Holstein between 1998 and 2017 in the case of Rhineland-Palatinate. As expected, the results in Figure 6 do not show that Rhineland-Palatinate municipalities slightly below 70 inhabitants differ from municipalities above 70. We take both pseudo analyses as strong evidence that a population size of 70 itself does not drive the results.

[Figure 6 about here]

5.3 Further outcomes

We investigate further outcomes beside tax rates: expenditures, debt, and voting behavior in national elections. Table 6 show the results. We now arrange all difference-in-differences and RD specifications in vertical panels. First, we turn to expenditures and debt. We find a significant negative effect of town meetings on logged total expenditures per capita in most specifications (column (1)). Point estimates vary little. However, the standard error in the local-linear RD specification becomes large; the coefficient lacks statistical significance. Columns (2) to (4) show that decreases in total expenditures are mainly driven by reductions in capital expenditures. Neither staff nor administrative expenditures (including, for example, costs for materials, maintenance, interest rates) change significantly in town meeting municipalities. Running direct democracy does not lead to higher or lower operating costs. Finally, we also find that in almost all specifications, public debt decreases substantially under direct democracy (column (5)).

[Table 6 about here]

Finally, we test whether political outcomes change in town meeting municipalities. Executing a strong form of direct democracy may change attitudes towards representative democracy. We use data on eleven national elections. First, we test voter turnout. In theory, effects are ambiguous. On the one hand, participation in elections may decline because citizens started favoring direct democracy over representative democracy. On the other hand, citizens which experience the caveats, trade-offs and difficulties in public choice may even more respect politicians, and voter turnout may increase. However, we neither find evidence for the former nor for the later theory. Coefficients do not turn out to be significant (column (6) in Table 6). We also test the share of invalid ballots and the vote share for right-wing populist parties including the *Alternative für Deutschland* (AfD). The 2013 founded AfD is the main right-wing populist anti-establishment party and campaigns for more direct democracy. Again, however, we do not observe any significant effect of town meeting constitutions. We conclude that direct democracy does not come at the cost of representative decision making corroborating findings by Sanz (2019) for Spain.

6 Mechanisms

6.1 Unbundling policies

We now return to section 2 where we have outlined three different mechanisms how direct democracy may influence public choice: unbundling policies, overspending by representatives and legislature size. Our results on differences between property and business taxes suggest that unbundling policies is a likely mechanism. Town meetings do not cut taxes *in general* but selected taxes which target the "masses". These findings imply that the broad population rather than minorities benefit from direct democracy. However, significant effects on agriculture property tax rates (property tax A) seem to be somewhat puzzling: why do farmers benefit from direct democracy but not businesses in general? We have two explanations. First, general and agriculture property tax rates are

tightly correlated ($r = 0.93$). For reasons of equity, municipalities often change property tax rates A and B simultaneously and by same extent. In our sample, levels as well as changes are fairly similar across property taxes (see, Table 4). Second, farming plays a key role in Schleswig-Holstein. Agriculture covers around three-quarters of total area; farmers are important employers and local opinion leaders. At least in our small sample municipalities, agriculture does not represent a minority but the majority. This may map into the asymmetric tax rate effects we document for agriculture and businesses in general.[17] Our results suggest that curbing special interests is a likely mechanism through which direct democracy reduces taxation.

6.2 Overspending by representatives

Reducing overspending tendencies by representatives is a second mechanism. Table 6 provides evidence in favor of this channel. Expenditure reductions are almost entirely driven by lower levels of capital spending in town meeting municipalities. Contrasting politicians in local councils, popular assemblies consisting of all citizens do not have re-election motives and therefore have little incentives to engage in "monumental projects". As a result, capital spending comes down under direct democracy. Anecdotal evidence supports this mechanism. A new family which has moved to the municipality of Hohenfelde (population of around 50 to 60) reports that the local government "hardly spends anything, for example, there is no street lighting"[18]. Another article covers a town meeting in the same municipality in 2013. Citizens discussed whether the local government may provide resources to dig a new ditch. The mayor responded the rhetorical question "Well, nobody here has a shovel?" and closed the session.[19]

In conclusion, both the mechanisms of curbed overspending and of unbundling issues seem to interact: citizens generally seem to opt for a smaller public sector than politicians,

[17] Note that local business taxes do not apply to agriculture businesses.

[18] See, the article by Alexandra Schulz: "Die Neuen" – angekommen im 60-Einwohner-Dorf, Hamburger Abendblatt, 04 August 2014, https://www.abendblatt.de/region/stormarn/article130847366/Die-Neuen-angekommen-im-60-Einwohner-Dorf.html.

[19] Jana Luck, In Hohenfelde darf jeder mitregieren, Hamburger Abendblatt, 20 June 2013, https://www.abendblatt.de/region/stormarn/article117287788/In-Hohenfelde-darf-jeder-mitregieren.html.

leading to less "expressive" expenditures under town meeting constitutions. Citizens then return their "dividend" from avoided overspending to themselves via property tax reductions. Town meetings therefore seem to entail incentives to run policies for the general public rather than for specific minorities. However, so far, we cannot fully rule out that effects work the other way round: representative decision making corrects inefficiently low expenditure levels under direct democracy because councils decrease information and transaction costs. In the next section, we compare town meetings to similar sized councils to address this issue.

6.3 Legislature size

Finally, legislature size may play a role. As outlined in section 2, recent theories expect larger assemblies to be associated with tougher monitoring and lower expenditures. Our setting allows for some suggestive evidence testing this mechanism. To do so, we compare town meetings with local councils of around the same size. We compute the "effective" size of the legislative in town meeting municipalities as follows: Around 85% of the municipality population is eligible to vote (citizens above the age of 16). 40% of eligible voters attend town meetings on average (see, Table 2). We therefore multiply population with 0.85 and 0.40 to achieve an "effective" average size of the legislature of town meeting municipalities. For municipalities without town meetings (larger than 70 inhabitants), we use the numbers of local councilors which increase in population size according to Schleswig-Holstein local government law.

We split our sample at the median number of effective legislators in town meeting municipalities which is 17. In the upper panel of Table 7, we compare town meeting municipalities with an effective legislature of up to 17 members (small legislature) to local councils with up to 17 members. Column (2) shows that both groups of municipalities have very similar numbers of legislators on average (10.9 and 10.3). We conclude that also transaction and information costs are similar in both groups. In columns (3) to (5), we compare simple means of tax rates in both samples. Reproducing our regression analyses, property taxes are somewhat lower in the town meeting sample but business taxes hardly differ. When

we turn to larger legislatures of 18 to 24 members (lower panel of Table 7), results also barely change. Town meeting municipalities have lower property tax rates than council administered municipalities with the *same* number of the legislators. Thus, differences between direct democracy and councils remain even when we shut down legislature size. We conclude that neither legislature size nor information costs should drive the results. Direct democracy seems to correct inefficient overspending by councils – inefficient underspending by town meetings compared to councils is less likely.

7 Conclusion

We have shown that "pure" direct democracy where citizen lawmaking entirely replaces parliaments entails incentives for policies targeting "the masses" rather than well-organized minorities. Small municipalities in the German state of Schleswig-Holstein with a population below a specific threshold hold town meetings and do not elect councils. In town meeting municipalities, capital expenditures and property tax rates decrease. Popular assemblies consisting of all citizens do not have re-election motives and may therefore abstain from "monumental projects". The "dividend" from lower levels of capital spending is equally shared among citizens via property tax cuts. Business taxes, by contrast, do not change. Unbundling policies therefore seems to curb special interests.

Our results strongly suggest that direct democracy is able to internalize inefficiencies resulting from agency problems in representative decision making. However, we have also shown that direct democracy does not come at the cost of representative decision making. Local constitutions do not change voting behavior in national elections. For example, voter turnout or populist votes do not change when a municipality holds town meetings.

We are confident that our results also have implications for larger communities because our small sample municipalities well reproduce state average land use and economic structure. However, future studies may explicitly investigate direct democratic policy making in larger jurisdictions. Topics, public debates, media influence and political campaigning may well deviate at the national level compared to very small communities. Further

comparative case studies of larger popular assemblies (for example, the *Landsgemeinde* in the Swiss canton of Glarus) might be a promising avenue to investigate whether effects of citizen lawmaking change in population size.

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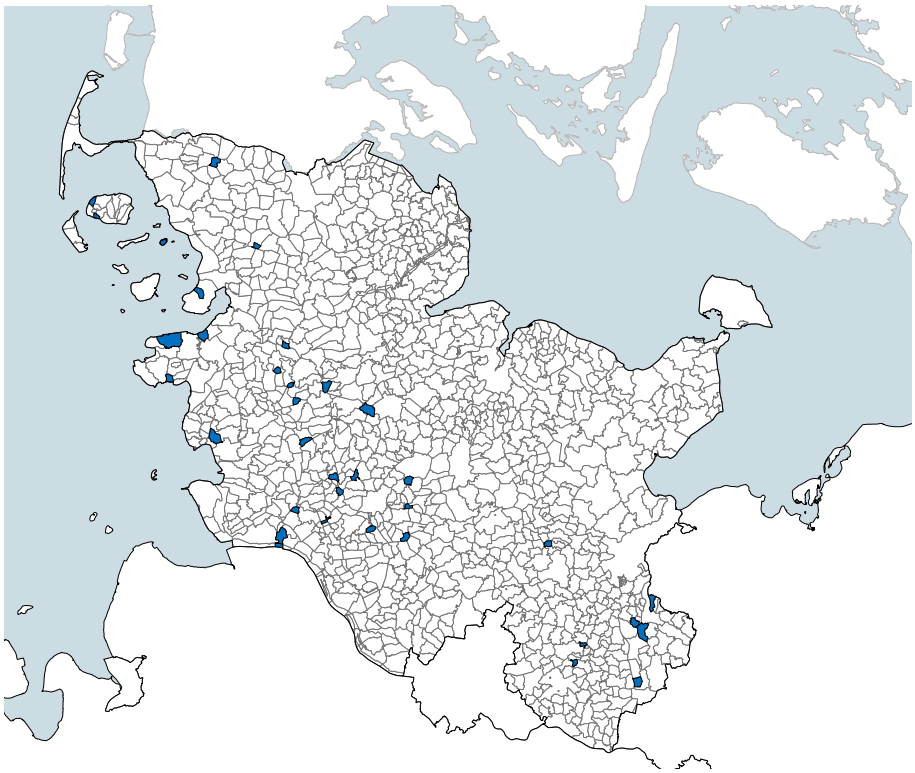
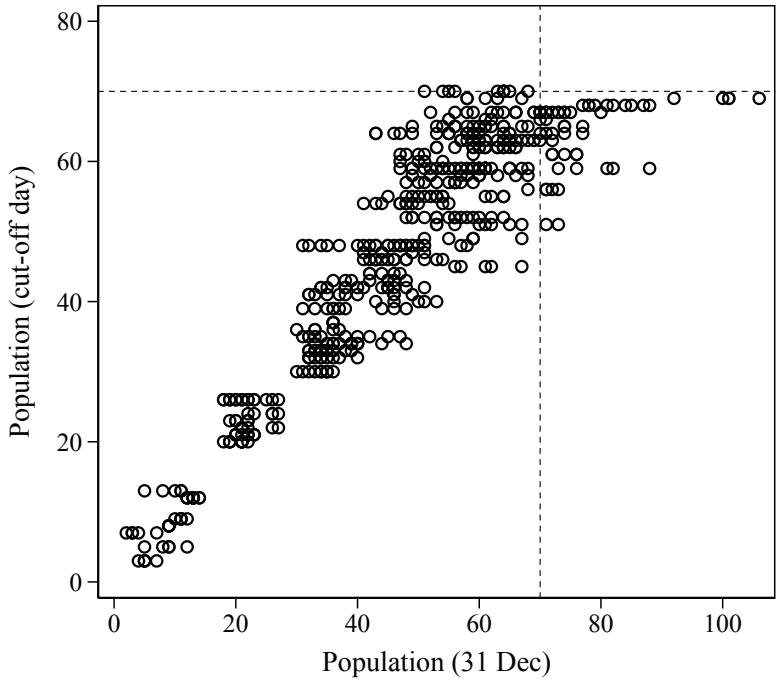
Figure 1: Municipalities of Schleswig-Holstein



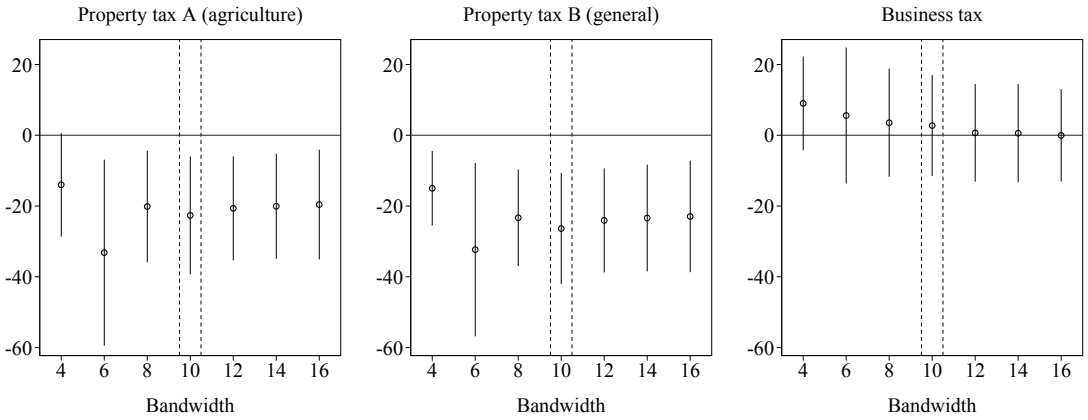
Notes: The map shows the German state of Schleswig-Holstein bordering Denmark (North), and the German states of Lower Saxony (West), Hamburg (South), and Mecklenburg-West Pomerania (East). Black lines are federal state boundaries, gray lines within Schleswig-Holstein describe municipality boundaries. Dark highlighted municipalities had a town meeting form of government in at least one out of nine local election periods between 1978 and 2017 (36 municipalities in total). Figure A.1 in the Appendix traces all town meeting municipalities over time.

Figure 2: Population in town meeting municipalities



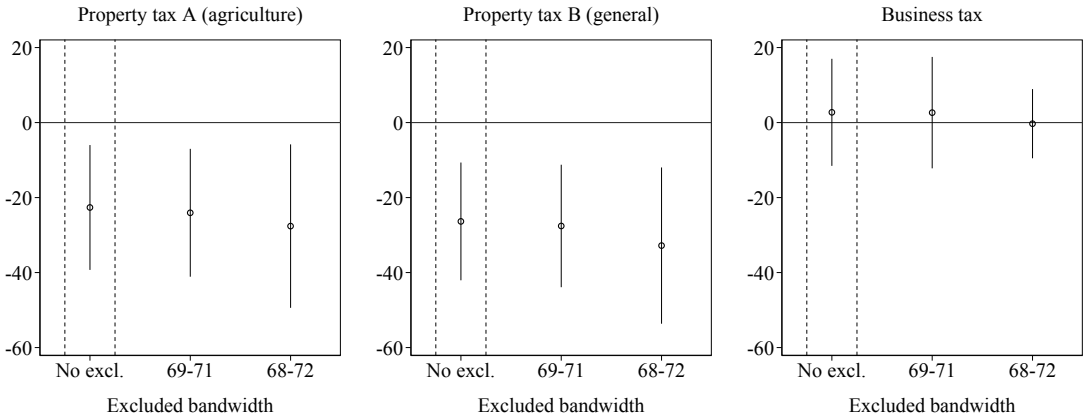
Notes: The figure compares population at the cut-off day determining the form of government (31st December, three years before the election year) with the actual annual population (31st December) for town meeting municipalities in our baseline sample. We use population at the cut-off days for the local elections in 1998, 2003, 2008, and 2013 when population data are available. Dashed lines represent the cut-off population of 70. The correlation coefficient is $r = 0.92$.

Figure 3: Sensitivity to bandwidths



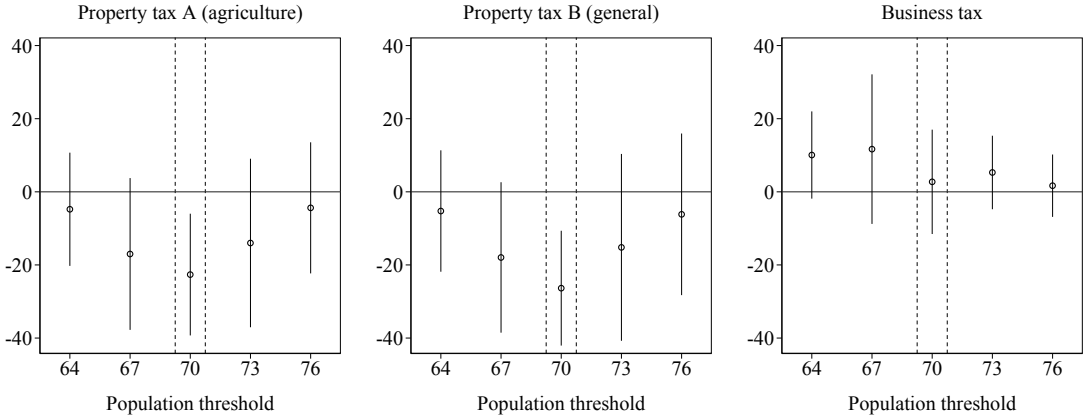
Notes: The figures report regression point estimates under different bandwidths from ±4 to ±16 around the threshold. The specifications refer to *Small bandwidth* (columns (4) to (6) in Table 5). Vertical solid bars represent 90% confidence intervals. The baseline bandwidth is ±10 inhabitants around the threshold of 70 (vertical dashed lines).

Figure 4: Donut regressions



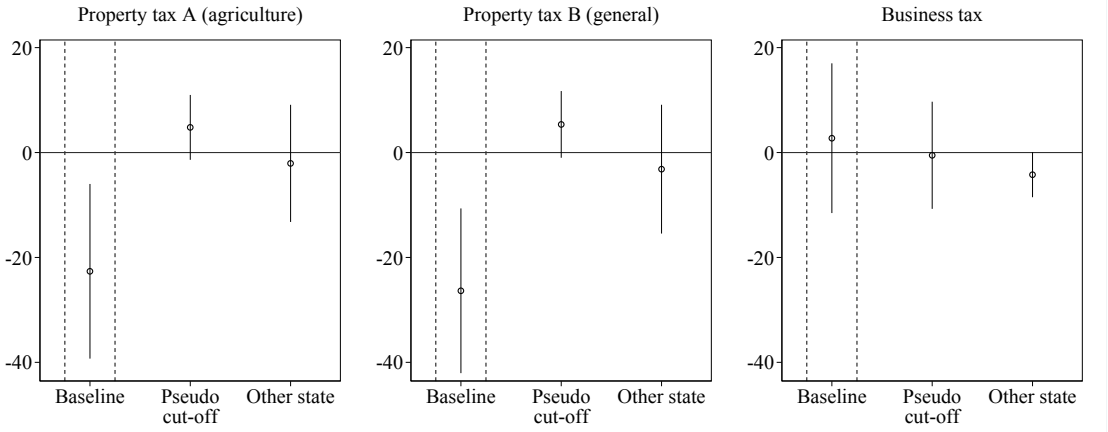
Notes: The figures report regression point estimates when we exclude observations around the threshold (none, 67–71 inhabitants, 68–72 inhabitants). The specifications refer to *Small bandwidth* (columns (4) to (6) in Table 5). Vertical solid bars represent the 90% confidence intervals. The baseline bandwidth excludes no observations around the threshold of 70 (vertical dashed lines).

Figure 5: Pseudo thresholds



Notes: The figures report regression point estimates when we shift the actual threshold (70 inhabitants) to pseudo thresholds (64, 67, 73, and 76 inhabitants). The specifications refer to *Small bandwidth* (columns (4) to (6) in Table 5). Vertical solid bars represent the 90% confidence intervals. The baseline bandwidth is the threshold of 70 (vertical dashed lines).

Figure 6: Pseudo treatments



Notes: The figures report regression point estimates when we use actual population instead of population at the cut-off day and when we use data from the German state of Rhineland-Palatinate where no town meetings are held. The specifications refer to *Small bandwidth* (columns (4) to (6) in Table 5). Vertical solid bars represent the 90% confidence intervals. The baseline specification uses cut-off day population in Schleswig-Holstein (vertical dashed lines).

Table 1: What can we infer from very small municipalities?

	Mean		Balancing test at threshold	
	Pop. < 140	State average	Local-linear RD	Band-width
	(1)	(2)	(3)	(4)
Population (2011 census)				
Population (total)	85.83	2,509.07	-5.62	212
Share of female population	48.61	50.13	-0.39	268
Share of foreign population	1.43	1.88	-1.49	242
Share of population age < 18	17.30	18.71	-3.02	254
Share of population age > 75	8.59	8.18	1.68	258
Share of married population	48.47	48.72	-3.07	232
Share of Protestant population	66.89	61.84	4.78	228
Geography (2016)				
Area (total)	550.11	1,414.85	-50.59	218
Share of settlement area	3.36	8.24	0.51	218
Share of traffic area	3.19	4.23	0.23	258
Share of agriculture area	78.55	71.96	-1.23	234
Share of water area	3.42	3.67	-0.41	192
Economy (2017)				
Population share employees	34.63	37.10	1.61	242
Population share unemployed	2.20	2.18	-0.61	226
Share of long-term unemployed	53.44	48.69	–	–
Firm size (employees)	5.23	6.23	–	–
Max. obs.	71	1,116	273	–

Notes: The table compares characteristics of 71 small municipalities in the German state of Schleswig-Holstein (maximum of 140 inhabitants at cut-off day) in column (1) with the state average in column (2). Columns (3) shows local-linear RD point estimates at the population threshold of 70, column (4) reports the corresponding data-driven optimal bandwidth. The number of observations on long-term unemployed and firm size is too small to perform RD regressions. Significance levels (RD standard errors): *** 0.01, ** 0.05, * 0.1 (but no statistically significant result to report).

Table 2: Key facts on Schleswig-Holstein town meetings

	All municipalities				
	Obs.	Mean	Std. Dev.	Min	Max
	(1)	(2)	(3)	(4)	(5)
Presence and duration					
Citizens attending	160	13.49	5.74	3	38
Share of eligible voters attending	160	0.40	0.18	0.06	0.89
Duration of the meeting in hours	166	1.38	0.57	0.25	3.50
Agenda					
Decisions on budget	167	0.41	0.49	0	1
Decisions on tax rates	167	0.34	0.48	0	1

Notes: The table reports some hand-collected key facts from publicly available protocols of 167 town meetings in 16 different municipalities of the state of Schleswig-Holstein between 2008 and 2018. Data cover around half of the municipalities with a town meeting constitution.

Table 3: McCrary RD manipulation test

	T	P>T
	(1)	(2)
Conventional	-1.30	0.20
Robust	-0.47	0.64
Kernel	Triangular	
Population bandwidth	*Optimal*	
Observations	4,484	
Effective number of obs.	325	

Notes: The table shows the results of the manipulation test suggested by McCrary (2008) comparing observation density at both sides of the threshold of 70 inhabitants. We use population at the cut-off days for the local elections in 1998, 2003, 2008, and 2013 when population data are available and apply the procedure proposed by Cattaneo *et al.* (2018). Significance levels (RD standard errors): *** 0.01, ** 0.05, * 0.1 (but no statistically significant result to report).

Table 4: Descriptive statistics

	All municipalities					Town meeting = 1
	Obs.	Mean	Std. Dev.	Min	Max	Mean
	(1)	(2)	(3)	(4)	(5)	(6)
Direct democracy						
Town meeting	10,123	0.02	0.15	0	1	1
Population	4,477	2,489.32	11,088.23	3	246,033	46.94
Population at cut-off day	4,479	2,534.10	11,147.20	4	247,943	47.66
Tax rates						
Property tax A (agriculture)	10,151	246.36	44.17	0	500	235.23
Property tax B (general)	10,151	252.85	44.92	0	504	236.60
Business tax	10,151	300.67	32.25	0	550	297.54
Expenditures (log, per capita)						
Total expenditures	2,226	7.20	0.41	6.59	13.33	7.76
Staff expenditures	2,226	4.17	0.97	2.24	7.84	4.40
Administration expenditures	2,226	5.15	0.58	2.84	9.05	5.20
Capital expenditures	2,208	5.38	1.05	-1.78	10.85	5.70
Public debt (log, per capita)						
Debt	2,226	4.30	2.72	0.00	10.58	0.86
National election outcomes						
Voter turnout	12,351	75.91	8.63	33.33	100.00	80.49
Invalid vote share	12,351	1.27	1.08	0	17.65	1.61
Right-wing populist vote share	12,351	1.85	2.71	0	38.10	1.49

Notes: The table shows the descriptive statistics of our dataset. The unit of observation are around 1,100 municipalities in the German state of Schleswig-Holstein. We have information on town meeting constitutions and tax rates for nine local election terms between 1978 and 2017. Population data are only available for the local elections in 1998, 2003, 2008, and 2013; other fiscal outcomes (expenditures, debt) are available after 2008. National election outcomes refer to eleven German national elections since 1980.

Table 5: Baseline results

	Difference-in-differences									Local-linear RD		
	1978–2017			1998–2017						1998–2017		
	No bandwidth			*Small bandwidth*			*Optimal bandwidth*			*Optimal bandwidth*		
	Prop. tax A (agr.)	Prop. tax B (gen.)	Business tax	Prop. tax A (agr.)	Prop. tax B (gen.)	Business tax	Prop. tax A (agr.)	Prop. tax B (general)	Business tax	Prop. tax A (agr.)	Prop. tax B (gen.)	Business tax
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Town meeting	-20.03	-20.77	-1.48	-22.64**	-26.35***	2.73	-43.71*	-45.48*	-3.36	-36.79***	-38.08***	-10.24
	(15.95)	(15.68)	(8.91)	(9.71)	(9.16)	(8.33)	(25.64)	(25.47)	(6.92)	(11.04)	(11.35)	(7.34)
Mun. fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No
Bandwidth	–	–	–	10	10	10	169	165	187	169	165	187
Mean of dep. var.	246.39	252.87	300.68	254.59	257.29	313.09	262.62	266.14	314.30	262.62	266.14	314.30
Municipalities	1,143	1,143	1,143	24	24	24	213	207	234	213	207	234
Observations	10,123	10,123	10,123	54	54	54	745	723	820	745	723	820
Within R^2	0.67	0.65	0.56	0.67	0.65	0.47	0.35	0.32	0.44	–	–	–

Notes: The table reports our baseline regression results using different difference-in-differences and RD specifications. The unit of observation are municipalities in the German state of Schleswig-Holstein. The dependent variable is the election term average in local tax rates (property tax rate A for agriculture, general property tax rate B, and business tax rate). Significance levels (standard errors clustered at the municipality level/RD standard errors): *** 0.01, ** 0.05, * 0.1.

Table 6: Budget and political outcomes

	Expenditures				Debt	Vote shares in national elections		
	Total	Staff	Admin-istration	Capital		Voter turnout	Invalid votes	Right-wing pop-ulists
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Difference-in-differences								
No bandwidth								
Town meeting	-0.31***	-0.02	0.11	-1.79***	-0.32***	0.86	0.54	-0.29
	(0.04)	(0.06)	(0.12)	(0.63)	(0.05)	(1.34)	(0.47)	(0.48)
Mun. fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	–	–	–	–	–	–	–	–
Mean of dep. var.	7.20	4.17	5.15	5.38	4.30	75.91	1.27	1.85
Municipalities	1,119	1,119	1,119	1,119	1,119	1,142	1,142	1,142
Observations	2,224	2,224	2,224	2,206	2,224	12,349	12,349	12,349
Within R^2	0.10	0.23	0.24	0.15	0.04	0.78	0.22	0.78
Small bandwidth								
Town meeting	-0.20**	-0.02	-0.35**	-1.51***	-1.28	-0.65	0.90	0.43
	(0.07)	(0.04)	(0.14)	(0.51)	(1.09)	(2.09)	(0.86)	(0.43)
Mun. fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	10	10	10	10	10	10	10	10
Mean of dep. var.	7.08	4.03	4.55	4.57	1.78	77.11	1.47	1.89
Municipalities	18	18	18	18	18	24	24	24
Observations	25	25	25	24	25	81	81	81
Within R^2	0.18	0.52	0.61	0.59	0.21	0.16	0.20	0.38
Optimal bandwidth								
Town meeting	-0.20***	0.04	0.08	-1.22*	-0.57***	-0.06	0.04	0.26
	(0.07)	(0.04)	(0.14)	(0.51)	(1.09)	(2.09)	(0.86)	(0.43)
Mun. fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	167	169	198	223	210	168	163	183
Mean of dep. var.	7.24	3.82	4.98	5.12	2.60	74.26	1.70	2.56
Municipalities	189	193	219	244	228	213	203	230
Observations	356	363	420	459	446	1,111	1,064	1,205
Within R^2	0.02	0.14	0.20	0.31	0.07	0.30	0.14	0.55
Local-linear RD								
Optimal bandwidth								
Town meeting	-0.18	-0.02	-0.11	-0.15	-2.31***	1.71	-0.08	0.39
	(0.17)	(0.17)	(0.24)	(0.56)	(0.72)	(1.46)	(0.44)	(0.54)
Mun. fixed effects	No	No	No	No	No	No	No	No
Year fixed effects	No	No	No	No	No	No	No	No
Bandwidth	167	169	198	223	210	168	163	183
Mean of dep. var.	7.24	3.82	4.98	5.12	2.60	74.26	1.70	2.56
Municipalities	189	193	219	244	228	213	203	230
Observations	356	363	420	459	446	1,111	1,064	1,205
Within R^2	–	–	–	–	–	–	–	–

Notes: The table reports regression results for fiscal and political economy outcomes using different difference-in-differences and RD specifications in vertical panels. The unit of observation are municipalities in the German state of Schleswig-Holstein. The dependent variable are election term averages in fiscal outcomes since 2008 and election results in eleven national elections since 1980. Significance levels (standard errors clustered at the municipality level/RD standard errors): *** 0.01, ** 0.05, * 0.1.

Table 7: Size of the legislature

	Mean				
	Population	(Effective) Legislature	Prop. tax A (agriculture)	Prop. tax B (general)	Business tax
	(1)	(2)	(3)	(4)	(5)
Small legislature					
Town meeting	32.15	10.93	257.98	261.02	317.34
Council	899.39	10.32	272.26	276.33	316.81
in % of Council	*4%*	*106%*	*95%*	*94%*	*100%*
Large legislature					
Town meeting	61.20	20.81	242.53	245.22	314.07
Council	8,611.77	20.21	299.91	312.19	332.60
in % of Council	*1%*	*103%*	*81%*	*79%*	*94%*

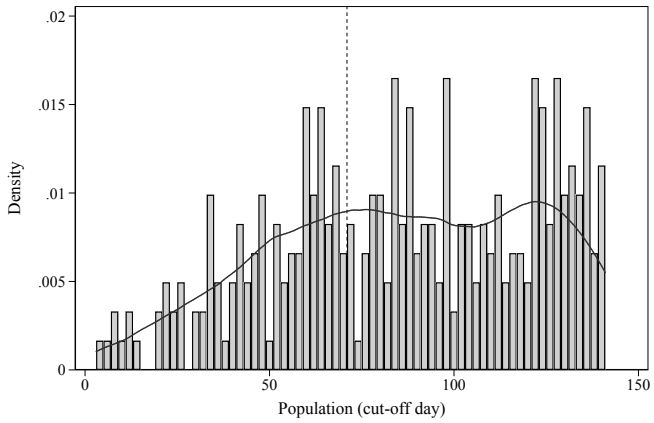
Notes: The table reports means of tax rates (property tax rate A for agriculture, general property tax rate B, and business tax rate) in municipalities with a comparable size of a legislature – either as town meeting or as local council. Small legislatures have up to 17 members (median in town meeting municipalities), large legislatures have 18 to 24 members.

Appendix (for online publication only)

Figure A.1: Municipalities with town meeting form of government

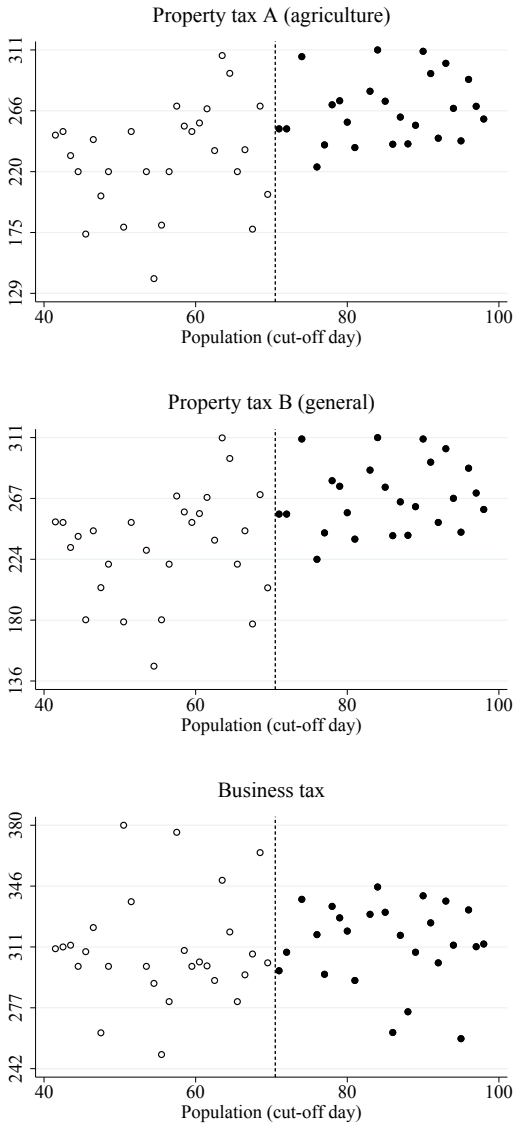
Notes: The figure shows government forms in municipalities of Schleswig-Holstein between 1978 and 2017. Election terms are 4 years (until 1998) and 5 years (after 1998). Dark shaded bars represent a town meeting form of government (36 municipalities in total). The municipalities of Augustenkoog and Hägen were merged with neighboring municipalities during the election term.

Figure A.2: Frequencies of cut-off day population



Notes: The figure shows frequencies of cut-off day population in our sample municipalities (31st December 1995, 2000, 2005, 2010, and 2015). The bin width is 2 inhabitants, the vertical dashed line represents the threshold of 70 inhabitants. The solid line is the kernel density.

Figure A.3: Tax rate averages around the population threshold



Notes: The figures show election term average in local tax rates (property tax rate A for agriculture, general property tax rate B, and business tax rate) for a bandwidth ±30 inhabitants around the population threshold of 70. Data are averaged in bins, the bin width is 1.

Table A.1: Local election dates

	Election day	Cut-off day	Information on town meetings	Population data
	(1)	(2)	(3)	(4)
1978–1982	05.03.1978	31.03.1977	Yes	No
1982–1986	07.03.1982	31.03.1981	Yes	No
1986–1990	02.03.1986	31.12.1984	Yes	No
1990–1994	25.03.1990	30.09.1988	Yes	No
1994–1998	20.03.1994	30.06.1992	Yes	No
1998–2003	22.03.1998	31.12.1995	Yes	Yes
2003–2008	02.03.2003	31.12.2000	Yes	Yes
2008–2013	25.05.2008	31.12.2005	Yes	Yes
2013–2018	26.05.2013	31.12.2010	Yes	Yes

Notes: The table reports local election terms in Schleswig-Holstein from 1978 to 2018, election days and cut-off days determining town meeting municipalities. Information on town meeting constitutions before 1998 are from Franke (1996), after 1998 information are self-compiled from cut-off day population data.

Table A.2: Annual data frequency

	Difference-in-differences									Local-linear RD		
	1978–2017			1998–2017						1998–2017		
	No bandwidth			*Small bandwidth*			*Optimal bandwidth*			*Optimal bandwidth*		
	Prop. tax A (agr.)	Prop. tax B (gen.)	Business tax	Prop. tax A (agr.)	Prop. tax B (gen.)	Business tax	Prop. tax A (agr.)	Prop. tax B (general)	Business tax	Prop. tax A (agr.)	Prop. tax B (gen.)	Business tax
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Town meeting	-22.78	-23.61	-2.50	-22.64**	-26.35***	2.73	-42.15	-44.36*	-3.15	-19.52***	-20.36***	-6.50**
	(17.07)	(16.78)	(8.84)	(9.71)	(9.16)	(8.32)	(25.58)	(25.36)	(6.98)	(6.03)	(6.14)	(3.14)
Mun. fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No
Bandwidth	–	–	–	10	10	10	67	69	82	67	69	82
Mean of dep. var.	249.99	256.31	302.99	254.59	257.29	313.09	261.38	264.62	315.54	261.38	264.62	315.54
Municipalities	1,145	1,145	1,145	24	24	24	94	94	103	94	94	103
Observations	43,827	43,827	43,827	270	270	270	1,465	1,495	1,760	1,460	1,495	1,760
Within R^2	0.63	0.61	0.51	0.45	0.44	0.38	0.18	0.18	0.28	–	–	–

Notes: The table reproduces regression results of Table 5 using annual observation instead of election term averages. The unit of observation are municipalities in the German state of Schleswig-Holstein. Significance levels (standard errors clustered at the municipality level/RD standard errors): *** 0.01, ** 0.05, * 0.1.