

Geschwind, Stephan; Roesel, Felix

Working Paper

Taxation under Direct Democracy

CESifo Working Paper, No. 9166

Provided in Cooperation with:

Ifo Institute – Leibniz Institute for Economic Research at the University of Munich

Suggested Citation: Geschwind, Stephan; Roesel, Felix (2021) : Taxation under Direct Democracy, CESifo Working Paper, No. 9166, Center for Economic Studies and Ifo Institute (CESifo), Munich

This Version is available at:

<https://hdl.handle.net/10419/236708>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

Taxation under Direct Democracy

Stephan Geschwind, Felix Roesel

Impressum:

CESifo Working Papers

ISSN 2364-1428 (electronic version)

Publisher and distributor: Munich Society for the Promotion of Economic Research - CESifo GmbH

The international platform of Ludwigs-Maximilians University's Center for Economic Studies and the ifo Institute

Poschingerstr. 5, 81679 Munich, Germany

Telephone +49 (0)89 2180-2740, Telefax +49 (0)89 2180-17845, email office@cesifo.de

Editor: Clemens Fuest

<https://www.cesifo.org/en/wp>

An electronic version of the paper may be downloaded

- from the SSRN website: www.SSRN.com
- from the RePEc website: www.RePEc.org
- from the CESifo website: <https://www.cesifo.org/en/wp>

Taxation under Direct Democracy

Abstract

Do citizens legislate different tax policies than parliaments? We provide quasi-experimental evidence for causal effects of direct democracy. Town meetings (popular assemblies) replace local councils in small German municipalities below a specific population threshold. Difference-in-differences, RD and event study estimates consistently show that direct democracy comes with sizable but selective tax cuts. Property tax rates, which apply to all residents, decrease by some 10 to 15% under direct democracy. We do not find that business tax rates change. Direct democracy allows citizens to design tax policies more individually than voting for a high-tax or low-tax party in elections.

JEL-Codes: D710, D720, H710, R510.

Keywords: direct democracy, town meeting, popular assembly, constitution, public finance, taxation.

Stephan Geschwind
University of Passau, School of Business,
Economics and Information Systems
Innstrasse 41
Germany – 94032 Passau
stephan.geschwind@uni-passau.de

*Felix Roesel**
ifo Institute Dresden
Einsteinstrasse 3
Germany – 01069 Dresden
roesel@ifo.de

*corresponding author

June 30, 2021

We thank Zareh Asatryan, Ivo Bischoff, Sebastian Blesse, Reiner Eichenberger, Lars P. Feld, Sebastian Garmann, Kai A. Konrad, Christian Lessmann, Max Löffler, Niklas Potrafke, Christian Ochsner, Carlos Sanz, Vassilis Sarantides, Mark Schelker, Marcel Thum, Lars Vandreï, and the participants of the EPCS 2019 in Jerusalem, the XXXVI Tax Day 2019 in Munich, the 9th ifo Dresden Workshop on Regional Economics 2019 in Dresden, the Workshop for the Yearbook of Public Finance 2019 in Leipzig, the ifo Christmas Conference 2019 in Munich, the PEDD 2020 in Münster, and the ZEW Workshop on Current Topics in Political Economy 2021 for helpful comments. Jaqueline Hansen provided excellent research assistance. We are grateful to Florian Neumeier for sharing German house price data. Felix Roesel gratefully acknowledges funding by the German Research Foundation (DFG grant number 400857762).

1 Introduction

Direct democracy is literally popular. More and more parliaments delegate fundamental decisions back to the people. Brexit (2016), the Turkish constitution (2017) or same-sex marriage and abortion laws in Ireland (2015, 2018) are widely discussed examples. The number of bottom-up initiatives by citizens aiming at reversing parliamentary decisions is growing as well in many villages, cities and regions.¹ In light of these trends, an evident question is whether citizens legislate different policies than parliaments—as predicted by many theoretical models (see, for example, Romer and Rosenthal, 1979; Noam, 1980; Frey, 1994; Gerber, 1996; Maskin and Tirole, 2004; Matsusaka, 2018). Taxes are a key government policy and among the most relevant candidates for popular votes. At a global scale, direct democracy seems to go hand in hand with higher tax rates (see Figure 1 for evidence from around 100 countries).² Moving from zero direct democracy (e.g., the US federal level) to the global average of direct democracy (Portugal or Spain) implicates an increase of government revenues of around 0.8 to 1.3 % of GDP. However, causality may also run from taxes to direct democracy, for example if citizens launch initiatives against overtaxing. Governments have also been shown to tailor institutions according to fiscal and political needs (Lorz and Willmann, 2005; Robinson and Torvik, 2016; Correa-Lopera, 2019). Thus, the direction of causality is not clear.

[Figure 1 about here]

In this paper, we use a quasi-experimental setting to overcome the endogeneity of direct democracy. A sharp and binding population threshold determines institutions in small German municipalities in the federal state of Schleswig-Holstein. Town meetings (popular assemblies) replace local councils in municipalities which have 70 or less inhabitants at a specific cut-off day for a full election term of five years. All other rules are equal for

¹For German local governments, see the “Bürgerbegehrensbericht 2020 für Deutschland”. Less than 300 initiatives at the local level were counted for the full period 1956 to 1989; nowadays there are more than 300 per year.

²When regressing government revenues in % of GDP on the V-Dem Direct popular vote index by Coppedge *et al.* (2019) while controlling for GDP per capita in real terms, the direct democracy coefficient is 13.31 ($t = 2.52$) for a cross section of 166 countries in 2016 and 8.39 ($t = 2.25$) for two-way fixed effects regression using a balanced panel of 102 countries observed from 1994 to 2018. Results are very similar when we use central government tax revenues only.

municipalities above and below the direct democracy threshold. German municipalities autonomously decide on property and business tax rates. This setting allows us to estimate the causal effect of direct democracy on tax policies and other political outcomes. We investigate whether tax policies of local governments change at the institutional threshold. To do so, we track tax policies and institutions in more than 1,100 German local governments over a period of 40 years and apply difference-in-differences, regression discontinuity (RD), and event studies regressions.

We find that citizens adopt *lower* tax rates than elected politicians, but do so selectively. The most likely mechanism is that direct democracy allows to unbundle tax policies: citizens can design tax policies more individually and gradually than simply voting for a high-tax or a low-tax party. We derive this insight from effect heterogeneity across different categories of taxes. Property tax rates which apply to all residents decrease under direct democracy. The effects are economically substantial and amount to around 10 to 15% of the average property tax rate. By contrast, our results do not suggest that business tax rates change significantly under direct democracy. Other mechanisms are less likely to explain our results. We analyze protocols (including all names of participants) of more than 200 sessions of town meetings and councils and do not find that representation, agendas, or session complexity differ between both forms of democracy. We can also rule out that legislature size drives the result by comparing councils and town meetings of the same size. In conclusion, unbundling of policies via direct democracy seems to entail incentives to deliver policies for the many and not for special interest groups (see also, Gerber, 1999; Lewis *et al.*, 2015; Asatryan *et al.*, 2017b).

Our findings suggest that direct democratic institutions matter *even* in very small groups where social control of elected representatives is expected to be strong. The implication is that direct democracy makes a difference, even if we can shut down conventional agency concerns which apply to representative democracy. An obvious concern is whether tiny municipalities with a population of around 70 allow to draw general conclusions. For two reasons, we believe that our findings may hold also for larger groups. First, our municipalities under investigation are representative in structure and population composition despite their small size. Table A.1 in the Online appendix compares tiny

municipalities with a population of ± 70 around our institutional population threshold with the average municipality in Schleswig-Holstein. As expected, tiny municipalities are by far smaller than the average in terms of population (85 versus 2,600) and area (550 versus 1,400 hectare). Beside scaling, however, tiny municipalities fairly resemble the state average in socio-demographics. Population shares regarding employment, sex, nationality, age, family status, marital status, and religion barely differ between our small sample municipalities and the state average.³ Home ownership which has been shown to influence popular votes (Ahlfeldt and Maennig, 2015) does also not differ. Second, we collect historical evidence from former town meetings in another German state corroborating our results at higher population thresholds (see, section 5.4).⁴

Our paper contributes new findings to several strands of the literature. First, we are the first to show that popular assemblies design different tax policies than parliaments. Previous studies have investigated direct democratic instruments *complementing* representative democracy: initiatives and referendums (for a survey, see Matsusaka, 2018). Referendums are found to come with less spending, tax cuts, deficit reductions, better services, and decentralization (Feld and Kirchgaessner, 2001a,b; Feld and Matsusaka, 2003; Feld *et al.*, 2008; Nguyen-Hoang, 2012; Lewis *et al.*, 2015; Sances, 2018). Evidence on initiatives is less conclusive and ranges from decreases in spending, public employment and taxes (Matsusaka, 1995, 2009; Funk and Gathmann, 2011) over mixed or context-dependent findings (Besley and Case, 2003; Blume *et al.*, 2009; Galletta and Jametti, 2015) to higher levels of expenditures and tax rates under direct democracy (Schelker and Eichenberger, 2010). The paper series by Asatryan (2016), Asatryan *et al.* (2017a) and Asatryan *et al.* (2017b) studies indirect spill-over effects of referendums and initiatives on local public finance. In town meetings, direct democracy entirely substitutes rather than complements decision making by parliaments. Citizens legislate policies instead of elected councils. This form of government is widespread in New England local governments in the US and

³Traffic 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).

⁴We treat these findings with caution because municipalities in other German states were able to self-select into direct democracy.

in some Swiss cantons.⁵ Very few studies have investigated citizen lawmaking in town meetings.⁶ 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) and Funk and Litschig (2020) show that welfare and education spending decreases in Swedish and Swiss local governments which opted for a town meeting form of government.⁷ Sanz (2020) finds that expenditures but also revenues decrease in small Spanish town meeting municipalities. However, previous studies have not yet analyzed tax policies under town meeting direct democracy.

Second, we investigate spill-overs of town meetings to political participation at other levels of government. There is an ongoing discussion whether direct democracy substitutes or complements representative democracy (Matsusaka, 2005b; Blume *et al.*, 2009). In Switzerland, direct democracy guarantees checks and balances among voters and the government where all four major political parties are represented simultaneously since 1959. However, there are also concerns that direct democracy may 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 statistically significant effects on national election outcomes. Direct democracy neither seems to change voter turnout, invalid vote shares, nor vote shares for far-right populist parties including the *Alternative für Deutschland* (AfD).⁸

Third, our tax-specific results suggest that direct democratic institutions are useful tools to complement fiscal policies of representatives in individual cases. In general however, parliaments are authentic agents of the median voter. Previous studies suggested that voters prefer a smaller public sector than politicians and are more conservative in budgeting (Peltzman, 1992; Frey, 1994; Lowry *et al.*, 1998; Brender and Drazen, 2008; Potrafke,

⁵Around 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. See also the seminal work on New England town meetings by Bryan (2004).

⁶Randomized field experiments in developing countries show that policies barely change under direct democracy (Olken, 2010; Beath *et al.*, 2017). 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.

⁷Galletta (2021) shows that spending preferences in national referendums decrease when Swiss local governments operate under town meetings.

⁸The AfD is a populist anti-establishment party and campaigns for direct democracy.

2013). Our results are more nuanced. We do not find a uniform decrease over all tax rates. Instead, if citizens have a say they alter fiscal policies very selectively. Property taxes decrease, but business taxes do not change.⁹

Fourth, our study is one of the first showing effects of institutional changes in the legislative branch on fiscal outcomes. We therefore contribute to the discussion on economic effects of constitutions (Persson and Tabellini, 2003; Persson, 2004). Empirical studies so far have compared presidential systems and parliamentary systems (MacDonald, 2008; Egger and Koethenbueger, 2010; Coate and Knight, 2011; Saha, 2011; Whalley, 2013; Ade, 2014; Enikolopov, 2014; Garmann, 2015; Koepl-Turyna, 2016; Hessami, 2018; Gaebler and Roesel, 2019). 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 yet hardly explored third form of government beyond presidentialism and parliamentarism (Maskin and Tirole, 2004).

2 Theory

Why should citizens and councils tax differently? 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 overviews, see Matsusaka, 2005a, 2018).

First, direct democracy allows unbundling of political issues. This idea was proposed by Besley and Coate (2008) and alludes to the difference between multi-issue parties (standing in elections) and single-issue referendums. In elections, voters can only select from parties

⁹The channel through property is well in line with Diamond (2017) who shows that citizens use direct democracy to combat overtaxing leviathan governments when housing supply is less elastic.

offering menus of policies which may widely reflect preferences but also include at least some special-interest policies. Direct democracy unbundles decisions; voters are able to opt out special-interest policies. In town meetings, citizens have full control over the entire set of policies at any point of time, and not only over a bundle of policies on one single election day. If special interests are less served under direct democracy, we may expect tax rates for the general public to decrease 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 friends or even relatives (Kauder and Potrafke, 2015). Frey (1994) and others argue that the ‘cartel of politicians’ may lead to oversized public sectors with spending and tax levels beyond voters’ preferences. Such overtaking is more likely when housing supply is less elastic (Diamond, 2017). Information asymmetries are a second source of overspending. If voters are rational but imperfectly informed or information costs are very high, increasing expenditures before elections is a reasonable strategy to bolster re-election (Rogoff, 1990). Politicians use large-scale 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. Tax rates and spending for ‘monumental projects’ should come down under direct democracy.

Third, the size of the legislature may play a role. The theory of a ‘law of $1/n$ ’ developed by Weingast *et al.* (1981) proposes that expenditures increase in the number of councilors and electoral districts.¹⁰ However, Primo and Snyder (2008) have shown that this prediction only holds under specific conditions. Pettersson-Lidbom (2012) argues that larger parliaments are even better able to monitor and control budget maximizing administrations and

¹⁰A related strand of the literature explores the number and size of jurisdictions and tax competition; see, for example, Breuille *et al.* (2018).

therefore produce smaller public sectors. A related issue are information and transaction costs which are the general argument for representative decision making. Delegating powers to councils reduces 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. In this case, we would expect less projects and spending if citizens instead of councils legislate. Not only theoretical predictions but also recent empirical findings are ambiguous when it comes to legislature size. 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 lower levels of government spending in larger councils, implicating 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 (where usually more citizens are present than in councils) may reduce tax rates and expenditures.

Altogether, theories on unbundling of political decisions, overspending by representatives, and the ‘inverted law of $1/n$ ’ in legislature size let us expect that town meetings might be associated with a smaller public sector and lower tax rates. We later show that the channel of unbundling is the most likely mechanism driving the results in our case (see section 6).

3 Institutional background

3.1 Municipalities in Germany

Germany has two layers of local government similar to the US: counties (*Landkreise*) and municipalities (*Gemeinden*). 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 tasks of the around 11,000 German municipalities. Some 100 consolidated city-county governments (*kreisfreie*

Städte) have both counties' and municipalities' responsibilities (Roesel, 2017). According to the German constitution, local governments enjoy some fiscal autonomy. Municipality councils legislate local law, budgets as well as local property and business tax rates, but there are no local income taxes. Budgets are proposed by a usually directly elected head of local government but need approval by the local council. In 2017, local governments spent some Euro 250 billion (\$ 290 billion) which is around 20% of German total public expenditures, or 8% of GDP.¹¹

3.2 Local property and business taxes

Fiscal autonomy of German municipalities includes the right to levy local taxes. The most important local taxes beside some fringe taxes and fees (e.g., dog license fees) are the local property tax (*Grundsteuer*) and the local business tax (*Gewerbesteuer*). Local governments are limited to design effective tax rates. Federal law defines the tax base and a 'basic rate' (*Steuermesszahl*). Municipalities only decide on local multipliers (*Hebsätze*), which are multiplied with the uniform 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 the federal basic rate is uniform all across Germany nowadays,¹² local multiplier rates proportionally translate into tax rates on property and businesses. In this paper, we therefore refer to local multiplier rates as tax rates.

The tax base of the property tax is the value of land and buildings at a specific census day. There is a property tax rate for agriculture and forestry (property tax A) and a general tax rate for all other kinds of property (property tax B). Property taxes affect all citizens. Tax bills are paid by the owner, but landlords are allowed to pass taxes on to tenants. Business taxes, by contrast, are basically levied on the profits of local firms.¹³ Farms are exempted from paying business taxes. Since 2004, a minimum tax rate of 200 applies to the business tax and there is no upper cap. Property tax rates have no limits at all. By 2017, average property and business tax rates (multipliers) were at around 400 in

¹¹Figures do not include expenditures of the three city states of Bremen, Hamburg, and Berlin.

¹²The basic rates increased in the taxable amount until the late 2000s when a flat rate was introduced.

¹³There are some adjustments of the tax base, for example, regarding interest payments. For details on the German business tax, see Buettner (2003), Baskaran (2014) and Fuest *et al.* (2018).

Germany. The main business tax payers in our small municipalities under investigation are hotels and B&B, sometimes also craftsmen, barbers, and garages.

Apart from local taxes, municipalities share income and value added taxes with the state and the federal government. These taxes are exclusively designed at the federal level. State governments also grant transfers to local governments. However, property and businesses taxes are important sources to fund local expenditures in Germany. In 2017, property taxes generated some Euro 14 billion in tax revenues, local business taxes yield Euro 53 billion. Revenues from both taxes we investigate account for around one-half of total own tax revenues of local governments in Germany.

3.3 Town meetings in Schleswig-Holstein

Schleswig-Holstein is one of the 16 German federal states, located in the very North of Germany (see Figure 2). The state is dominated by agriculture and fishing. Schleswig-Holstein has around 1,100 comparably small municipalities which represent 10% of all German municipalities but only 4% of total area and 3% of the German population. Municipalities vary substantially in size; total population ranges from 4 to around 250,000. Responsibilities of municipalities in Schleswig-Holstein are similar to other German states.

[Figure 2 about here]

Small municipalities in Schleswig-Holstein usually pool resources in joint administrations (*Ämter*) which carry out administrative routine tasks like registry offices. *Ämter* have between 3 and 34 member municipalities. However, political decisions remain at the municipality level and are allocated to mayors and legislative bodies. Local legislatures design all kinds of local law including 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.¹⁴ Schleswig-Holstein municipalities with a population of 4,000 and fewer have a council-manager

¹⁴Before 1995, local elections were scheduled every four years.

system. The mayor in small municipalities serves on an honorary basis, chairs the local council or town meeting, and sets the agenda of the legislature.

Popular votes on local fiscal policies in referendums or initiatives are not allowed in Germany—town meetings are the only exception. According to the German constitution, town meetings can replace local councils in municipalities.¹⁵ After World War Two, several German states allowed tiny municipalities to opt for town meetings (Franke, 1996; Wollmann and Roth, 1999, see also Table A.2 in the Online appendix). However, very small municipalities disappeared in the course of large-scales municipal mergers in almost all German states in the 1960s and 1970s. The states of Rhineland-Palatinate and Schleswig-Holstein are exceptions and hardly amalgamated municipalities. Instead of forming larger municipalities, both states facilitated joint administrations of municipalities.¹⁶ Rhineland-Palatinate, however, never allowed to substitute councils by town meetings. Today, Schleswig-Holstein is the only German state with town meetings of legislative power.

In Schleswig-Holstein municipalities with 70 and less inhabitants on a specific cut-off day, town meetings (popular assemblies) formed by all citizens eligible to vote replace the local council for the full local election term of five years. All other rules and institutions are equal. Municipalities do *not* have a choice—the population threshold of 70 is sharp and binding. Population does not refer to the population on the election day but to the 31st December three years before the year of the local election. Today, around 30 out of 1,100 municipalities in Schleswig-Holstein have a population of 70 and less. Some are located on islands, but the majority of small municipalities is on the main land (see Figure 2). Because population varies over time, some marginal municipalities at around 70 inhabitants switched several times from local councils to town meeting, and vice versa (see Figure A.1 in the Online appendix). Table A.3 in the Online appendix reports the number of switches and switching municipalities for all local election terms since 1978. We count 46 switches in total, 27 from councils to town meetings and 19 switches from town

¹⁵See Article 28 of the German constitution: ‘In municipalities, a local assembly may take the place of an elected body.’

¹⁶One reason could be that Rhineland-Palatinate and Schleswig-Holstein were among the first German states merging local governments at the county level. County mergers already provoked a lot of protest and local resistance which may have stopped the state governments of Rhineland-Palatinate and Schleswig-Holstein to proceed with mergers at the municipality level. Other states like Saarland merged municipalities and counties simultaneously avoiding multiple periods of protest.

meetings to councils. Out of 20 municipalities involved in those 46 switches, 6 switched only once, 7 twice and the remainder up to five times between 1978 and 2013. In our analysis, we will exploit both variation across municipalities and time variation within municipalities.

Table 1 reports some additional illustrative key facts on town meetings in Schleswig-Holstein from hand-collected protocols of 167 town meeting sessions. Protocols usually report the names of the citizens attending, when meetings begin and end, and the agenda. Our data do not claim to be representative for the universe of town meetings but may provide some intuition for our setting. On average, 13.5 citizens attend town meetings which is around 40% of the total number of eligible voters. This is in line with Franke (1996) who reports that in the vast majority of all Schleswig-Holstein town meeting municipalities, between 25% to 50% of eligible voters are present. However, presence varies substantially between 6% to 89% of the total voting age population. We will present evidence in section 6.2 that town meetings are not biased towards a specific subgroup, corroborating the findings by Leininger and Heyne (2017) who have shown that Swiss referendums are representative.¹⁷ Town meetings in our sample last some 1.5 hours on average 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 1). 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 local budgets. Local tax rates are an issue in one third of all meetings and therefore a prominent and important policy.

[Table 1 about here]

Franke (1996) reports some additional facts worth to note. 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, we corroborate this in section 6.2. Regarding the mechanisms we have in mind, Franke

¹⁷Funk and Litschig (2020) find that town meetings reduce the representation of young and female voters in Switzerland, Kim (2019) presents evidence for the opposite.

(1996) already 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.

4 Identification strategy

4.1 Regression design

Constitutions are typically endogenous. For example, Robinson and Torvik (2016) show that strategic reasons predict the choice between parliamentary and presidential systems. Very similar concerns apply to direct democracy. Regressing outcome variables on direct democracy is very likely to produce biased estimates and is not sufficient to claim causality (Matsusaka, 2018). Our identification strategy is to compare German municipalities closely around the specific population threshold of 70 which quasi-randomly determines the form of government. Municipalities with a population of 70 and less at a specific cut-off day have town meetings. Municipalities with 71 and more inhabitants elect a local council. We assume that at the sharp threshold, the form of government is as good as random (Lee and Lemieux, 2010). The two main conditions for this assumption are the absence of both sorting and compound treatments. We will show that these conditions seem to be met (see section 4.2).

One first intuitive way of exploiting our variation in direct democracy over time and space are difference-in-differences models where we regress tax rates on institutions but restrict the sample to municipalities closely around the population threshold of 70:¹⁸

$$Taxrate_{it} = \alpha_i + \delta_t + \beta Townmeeting_{it} + \epsilon_{it} \quad (1)$$

$Taxrate_{it}$ is the dependent variable and describes the election term t average in one out of three local tax rates (*Hebesätze*) of municipality i . α_i and δ_t are municipality and election term fixed effects eliminating systematic time-invariant differences across municipalities

¹⁸Regressions are estimated with OLS, standard errors are clustered at the municipality level.

(e.g., due to local yardstick competition within counties; see Buettner and von Schwerin, 2016) and general time trends and shocks. ϵ_{it} describes 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 (*Gemeindeversammlung*), and zero otherwise (which means local councils). The dummy variable depends on the local population at a specific cut-off day some 30 months ahead 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 difference-in-differences specification, we use a bandwidth of ± 10 inhabitants around the threshold of 70. We assume that municipalities in a small population range of 60 to 80 are highly homogeneous. However, we later show that our results are very robust when we use smaller and larger bandwidths.

As a second intuitive way to exploit the variation from the population threshold, we estimate regression discontinuity (RD) design models. Equation 2 describes a RD model where we interact $Townmeeting_{it}$ with $\Delta Population_{it}$ measuring the distance to the population threshold of 70. However, RD estimations do also not depend on a specific bandwidth or polynomial choice. We vary bandwidths and polynomial orders and also estimate models where we drop time and municipality fixed effects and apply the local-linear procedure with a data-driven optimal bandwidth choice as proposed by 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.

$$Taxrate_{it} = \alpha_i + \delta_t + \beta Townmeeting_{it} + \gamma \Delta Population_{it} + \eta (Townmeeting_{it} \times \Delta Population_{it}) + \epsilon_{it} \quad (2)$$

Finally, a third strategy to exploit our setting are event study estimations. We observe 46 switches of government forms during our period of interest: 27 changes to town meetings and 19 to councils (see Figure A.1 and Table A.3 in the Online appendix).¹⁹ Event studies allow to check whether trends are parallel before changes of government forms,

¹⁹Another 4 switches from town meetings to councils are because of municipality mergers. In this case, however, we cannot observe the municipality after the change. We do not consider those exits.

and to assess the timing of the effects. Because population fluctuates around 70, some municipalities switched the form of government multiple times (Table A.3). In those cases, pre- and post-town meeting periods overlap. The debate is still unsettled how to deal with such multiple event settings in event study specifications.²⁰ We follow Schmidheiny and Siegloch (2019) proposing binning of effect window endpoints and normalizing to the last year before the treatment. We replace the difference-in-differences dummy $Townmeeting_{it}$ by a vector of 15 dummy variables taking on the value of 1 if a municipality is in year $-4, -3, \dots, +3, +10$ after its first town meeting. The endpoints -4 and 10 sum up all maximum lag and lead events beyond the window. We denote the year of switching from councils to town meetings by 0 , the omitted reference year is -1 . A similar coded second set of dummies $Council$ is included to the same regression equation to estimate the effects of the reverse switch, i.e., from town meetings to councils. This allows us to test whether switching *to* and switching *from* direct democracy makes a difference. To balance control and treatment group, we use only observations within the optimal bandwidth derived for the RD procedure. Our event study estimation equation takes the following form:

$$Taxrate_{it} = \alpha_i + \delta_t + \sum_{k=-4}^{k=10} \beta_k Townmeeting_{i(t-k)} + \sum_{j=-4}^{j=10} \beta_j Council_{i(t-j)} + \epsilon_{it} \quad (3)$$

4.2 Excluding sorting and compound treatments

Population is our crucial parameter determining the form of government. Using (self-reported) population as allocation rule comes with two main concerns: sorting and compound treatments (Eggers *et al.*, 2018). 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-select into government forms. Second, effects might be biased if more than one institution changes at the population threshold. For example, if fiscal transfers granted by higher layers of government or other policies change at the threshold of 70 inhabitants, effects overlap and become inseparable.

²⁰See, for example, Lafortune *et al.* (2018) or Sun and Abraham (2020).

In our case, however, we have good reasons to believe that neither sorting nor compound treatments bias our results. First, and most importantly, the key population figure is not self-reported by local authorities but population as computed by the State Statistical Office. Both figures may well diverge and are subject to permanent revisions by censuses. Very small municipalities can hardly intervene into decisions by state authorities. Second, political debates about the threshold induced uncertainty. One prominent example is the 2013 local election. In March 2012, a right-wing 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 have no evidence for bunching around the threshold. Local officials may misreport population strategically in order to achieve a specific constitution (either direct democracy or a local council). If local officials favor one particular 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, eyeball inspection (Figure A.3 in the Online appendix) and manipulation tests as suggested by McCrary (2008) do not detect strategic actions in favor of one particular form of government. Fourth, we take advantage of the specific nature of the cut-off day. The population cut-off day is the 31st December some 30 months before the local election to be held in spring every five years. For example, municipalities with a population of 70 and less on 31st December 2015 had no 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 A.2 in the Online appendix). We later replace cut-off day population by actual population on the election day as a pseudo treatment; effects only hold under the former. Altogether, we are rather confident that sorting into treatment does not play a role.

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 institutions changing at the threshold of 70 inhabitants. For example, per-capita fiscal transfers from the Schleswig-Holstein state government do not change in population size. 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 also participate in local elections. In the state of Schleswig-Holstein, elections for both layers of local government (municipalities and counties) 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. Therefore, elections are held in all municipalities. Thus, the presence or absence of elections cannot drive the results. Third, covariates vary smoothly at the threshold of 70. The right-hand side in Table A.1 in the Online appendix provides evidence on several observable characteristics. We perform local-linear RD regressions testing for discontinuities in various variables at the population threshold of 70.²¹ Table A.1 reveals no significant discontinuities regarding population, land use, local economy, and housing. 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 approximately 1,100 municipalities of Schleswig-Holstein between 1978 and 2017. Data before 1992 are manually digitized from hard-cover copies. The dataset covers nine local election periods (see Table A.4 in the Online appendix). We use annual data of local tax rates (*Hebesätze*) and other budget outcomes. Results do not change when we use election term averages. We end up with one observation for each municipality and year

²¹The number of observations measuring the share of long-term unemployed and firm size (employees) is too low to perform RD estimations.

totaling to around 45,000 observations for each of the three tax rates (see Table 2, column (1)).

Additional data from other sources complement our dataset. Data on expenditures are from the statistical authorities of Germany and Schleswig-Holstein but are not available for years before 2003 and after 2014. We use total expenditures, current expenditures, and capital expenditures—all in per capita terms.²² All expenditure variables are in logs. Finally, we also collect political economy data on voter turnout and shares of invalid votes and votes for far-right populist parties²³ in all eleven national elections between 1980 to 2017.

[Table 2 about here]

Missing population data before 1998 are a severe data constraint. Table A.4 in the Online 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. However, before 1998, 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. Franke (1996) reports town meeting municipalities before 1998 but does not offer population data. We are therefore able to identify town meeting municipalities, but not cut-off day population for years before 1998. Thus, we can use empirical strategies which depend on population size only in the period from 1998 to 2017, which is our baseline sample. The full sample without any population restriction over the years 1978 to 2017 delivers additional evidence but should be interpreted with caution.²⁴

²²Few observations drop out because we cannot take logs of non-positive values of capital expenditures.

²³We code NPD, DVU, REP and AfD as populist parties.

²⁴Moreover, 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.

5 Results

The descriptive statistics in Table 2 reveal some first remarkable differences between town meeting municipalities and other municipalities. Comparing columns (1) and (6), town meeting municipalities have lower property tax rates (approximately 235 versus 250) on average but business tax rates hardly differ (around 300). Simple group means therefore already suggest that property tax rates may differ in municipalities with town meetings. We test now whether differences turn out to be statistically significant in our three different regression specifications (difference-in-differences, RD, event studies).

5.1 Baseline

Table 3 reports 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)). The results do not indicate that business tax rates change when a municipality comes under town meeting rule (column (3)). The point estimate of -1.58 is very small given the sample mean of around 300 and the standard error of 8.94. However, estimates for property tax rates (column (1) and (2)) are less clear. The point estimates are substantially larger than zero and p-values are at around 0.17 to 0.19. 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 (see Table 2), we cannot rule out effects of direct democracy with certainty.

[Table 3 about here]

Therefore, we reduce the sample to smaller and more homogeneous municipalities. Columns (4) to (6) in Table 3 present a difference-in-differences specification where we restrict the observations to a small bandwidth of ± 10 around the population threshold, which means including only municipalities between 60 and 80 inhabitants. The number of observations shrinks 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).

Again, we do not find results that are statistically or economically different from zero for the business tax. The effects for the property taxes are economically relevant, amounting to around 25 points which is a substantial 10% of mean tax rates or half a standard deviation. The point estimates for the property taxes increase when we use a the larger optimal bandwidths which we derive in the RD specifications (see below) Columns (7) to (9) in Table 3 report that the effect for the property taxes is still significant at the 10% level. The robustness checks (section 5.2) show that our results also hold when using other bandwidths.

We now move to the results of our second identification strategy: the RD estimates where we test for discontinuities at the population threshold of 70. RD plots shown in the upper panel of Figure 3 provide corresponding eyeball evidence in line with our difference-in-differences results. Property taxes seem to be clearly discontinuous at the population threshold of 70. Also business tax rates tend to be somewhat lower under town meetings, but effects are imprecisely estimated—albeit the variation in business tax rates is lower than the variation in property tax rates (Table 2).

[Figure 3 about here]

The effects hold under various RD specifications. The lower panel in Figure 3 shows different RD specifications. If we use a bandwidth of ± 10 around the population threshold instead of 70, effects barely change. Point estimates decrease somewhat when we use a quadratic or a cubic polynomial instead of the linear baseline version with a bandwidth of 70 or the data-driven optimal bandwidth (Calonico *et al.*, 2017). Effects are marginally statistically significant at the 10% level. When we remove time and municipality fixed effects and use the local-linear specification with the optimal bandwidth, point estimates effects are similar to the linear specifications but somewhat more precisely estimated. Thus, neither different estimation techniques, bandwidths nor including/excluding fixed effects alter the results. All specifications yield very similar outcomes: property taxes are significantly smaller at the threshold in municipalities under direct democratic rule, but confidence intervals for the business tax include the zero.

Event study estimations based on annual data are our third estimation strategy. We use the optimal bandwidth from the RD estimations. The results are also fully in line with our previous findings. Figure 4 shows that in the year after changing the form of government, property tax rates start to decline steadily. This is true for switches to direct democracy (upper panel) where the point estimate for property taxes decreases in year one after the first town meeting. Similar to Table 3, the coefficients however marginally lack statistical significance at the conventional levels. The effects for switches from town meetings to councils are more robust and statistically significant (lower panel). The maximum effect size corresponds with the difference-in-difference estimate of some 10 to 15% of the sample mean tax rate. Thus, the institutional reference point seems to matter. Business tax rates, by contrast, do never seem to change to a significant extent in any specification. Figure A.4 in the Online appendix shows that the results hardly change when we apply a smaller bandwidth.

[Figure 4 about here]

5.2 Robustness

We submit our results to several robustness tests. First, we use election term averages instead of annual data. Table A.5 in the Online appendix shows that point estimates perfectly reproduce our baseline difference-in-differences with annual observations.

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 a bandwidth of 70 instead of 10 and when we use data-driven optimal bandwidths. However, we also test other bandwidths between 5 and 70 inhabitants and plot the resulting coefficients in the upper panel of Figure 5. Each dot represents the point estimate for town meetings from one separate difference-in-differences regression where we limit the bandwidth to ± 5 , ± 10 , ..., ± 70 inhabitants. Vertical solid lines are 90% confidence intervals. Our baseline specification refers to a bandwidth of ± 10 (columns (4) to (6) in Table 3). The figures for property taxes show that confidence intervals hardly include the

zero, for the general property tax B in particular. In contrast, effects for business tax rates are close to zero and not statistically significant in any specification. Our results therefore do not depend on a specific bandwidth choice.

[Figure 5 about here]

Third, we perform donut regressions where we omit observations closely around the threshold (Figure 5, lower panel). The 2011 census revealed a difference in total German population of around 2% between recent census data and updated earlier census data.²⁵ We assume that municipality officials are able to misreport population by around 2 to 3% and omit municipalities with 69 to 71 and 68 to 72 inhabitants. The results do not differ from our baseline findings (no exclusion).

Fourth, we exclude ‘tax havens’, i.e., municipalities with tax rates of zero. Some small municipalities in Schleswig-Holstein were famous for their business tax policies which attracted many letter box companies. For example, the famous village of Norderfriedrichskoog had a business tax rate of zero until 2004 when a minimum business tax rate was introduced. However, excluding tax rates of zero does not qualitatively change our results (Table A.6 in the Online appendix).

5.3 Pseudo treatments

We also test different pseudo treatments. First, we pretend that the threshold was at a population threshold other than 70, and compute pseudo town meeting dummies and distances to the threshold for populations of 40, 64, 67, 70, 73, 76, and 100, and re-run our regressions. Effects for a population of 70 correspond with the real treatment. Figure 6 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 to be significant for any pseudo threshold below or above 70

²⁵Statistical 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’.

inhabitants. Only the *real* population threshold of 70 yields statistically significant results for both property tax A and B. We conclude that effects at the threshold of 70 are unlikely to be random.

[Figure 6 about here]

Second, we replace real cut-off day population by actual population and recompute 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 A.2 in the Online appendix). However, if we replace cut-off day population by actual population, we do not observe any statistically significant effect for property taxes (see Figure 6).

Third, we use data on population and tax rates from another German state, Rhineland-Palatinate. This is the only West German state with very small municipalities similar to Schleswig-Holstein. However, small municipalities in Rhineland-Palatinate *never* had town meetings. We use the same cut-off days and population thresholds which apply to Schleswig-Holstein between 1998 and 2017 for 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.

5.4 Other German states

To bolster the external validity of our findings, we collect historical data from another German state, Brandenburg, which used to have voluntary town meetings for small municipalities below a population threshold of 100. The stylized evidence we derive is very much in line with our previous findings. Figure A.5 in the Online appendix compares average tax rates for municipalities with a population of less than 100 to municipalities with 100 to 200 inhabitants in the East German state of Brandenburg. Before 1993, all Brandenburg municipalities had local councils. By 1993, Brandenburg introduced voluntary town meetings for municipalities with a population of less than 100.

In contrast to Schleswig-Holstein, the threshold was not binding and municipalities below 100 inhabitants could opt for councils.

Our figures cover the period from 1992 to 1997 for which data on tax rates are available and municipality mergers were rare.²⁶ The center figure shows that municipalities below and above a population of 100 had very similar general property tax rates B of around 300 before 1993. After 1994, many municipalities below 100 inhabitants switched to town meetings and average tax rates dropped a great deal. Tax rates in municipalities with a population of 100 to 200 remain almost constant. Switching to town meetings came with a reduction of some 6 to 7 % in property tax rates on average. Figures might be lower than in Schleswig-Holstein because not all Brandenburg municipalities have switched. We do not observe similar patterns for agriculture property tax rates (property tax A) and business tax rates. Those two tax rates fluctuate but trends in municipalities below 100 inhabitants very much follow the trend of municipalities with more than 100 inhabitants. Thus, stylized effects in Brandenburg municipalities resemble our baseline findings for the more general property tax B in Schleswig-Holstein.

5.5 Other outcomes

We also investigate other outcomes which may have been affected by direct democracy. If tax rates decrease under direct democracy, expenditures may also come down. However, results for expenditures turn out to be rather inconclusive (see Table A.7 in the Online appendix). Estimates for current expenditures are all close to zero; operating town meetings seem not to be more or less cost efficient compared to councils. Also specifications for total and capital expenditures do not reveal a statistically significant effect. One main reason could be the very limited number of observations as we do not observe expenditures for years before 2003. We are also limited to few expenditure categories; tax cuts may translate into reductions in specific expenditure categories we do not observe.

Second, we test whether political outcomes in national elections change in town meeting municipalities (Table A.8 in the Online appendix). Executing a strong form of direct

²⁶Between 1992 and 1997, the number of municipalities dropped by some 5% from 1,793 to 1,696. After multiple waves of merger reforms in the 1990s and 2000s, Brandenburg has 417 municipalities by 2019.

democracy may change attitudes towards representative democracy. We use data on eleven national elections. First, we test voter turnout. Effects are ambiguous from a theoretical point of view. 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 and value politicians, and voter turnout may increase. However, we neither find robust evidence for the former nor for the later theory. The town meeting estimate is positive in the full sample but negative when using smaller bandwidths. However, all coefficients are very small in size (compared to the average turnout rate of 75%) and imprecisely estimated. We also test the share of invalid ballots and the vote share for far-right populist parties including the *Alternative für Deutschland* (AfD). The AfD, founded in 2013, is the main far-right populist anti-establishment party and heavily campaigns for more direct democracy. Again, however, we do not observe any significant effect of town meetings on national elections. We therefore do not find compelling evidence that direct democracy comes at the cost of representative decision making, corroborating findings by Sanz (2020) for Spain.

Third, we investigate house prices. If property taxes decrease under direct democracy, real estate values may capitalize tax cuts. We use data on house price offers for the period 2005 to 2017 which were collected by Dolls *et al.* (2019). We observe annual average house prices on the municipality level if there are at least three offers. Table A.9 in the Online appendix does not show statistical significant effects when we regress logged house prices on town meetings in a difference-in-differences setting. Point estimates range between a zero and one percent increase in house prices but standard errors are large. Several reasons may explain why we do not observe significant effects on house prices. First, we have only very few observations and our dataset might be somewhat underpowered. Second, both theoretical predictions and empirical findings on capitalization of property taxes are ambiguous (Guilfoyle, 2000). In a recent study on Finland, Elinder and Persson (2017) do not find evidence for a capitalization of property tax cuts. Lutz (2015); Löffler and Siegloch (2018) find significant capitalization effects of local property tax changes only for larger cities. Third, the incidence of property taxation depends on various price elasticities

on the housing, land, and labor market. Only a part of the burden of the local property tax in Germany is borne by the landlord; substantial parts are shifted on to renters and local businesses. Fourth and finally, the German property tax is often considered underutilized compared to other countries, and may therefore hardly leverage into prices. By 2018, real estate values in Germany sum up to around Euro 11,800 billion while property taxes raise some Euro 14 billion, or 0.1% of total real estate values. Property taxes in the US are more than ten times higher.²⁷ Moderate changes of comparably low property tax rates may therefore hardly translate into substantial price changes.

Finally, persistently different tax rates may induce some Tiebout sorting. Citizens can move to cities which meet their preferences in spending and taxes best. In Table A.9 in the Online appendix we investigate immigration and outmigration as a share of total population. We find that immigration does not change in the form of government. Point estimates are small and not statistically significant. By contrast, outmigration is lower when direct democracy applies. Town meetings halve the sample mean in outmigration. This effect is substantial and statistically significant at the 10% level. Thus, direct democracy does not seem to attract new ‘outsiders’ but to attach resident ‘insiders’ to their community.

6 Mechanisms

6.1 Unbundling of policies

We now return to unbundling of policies as the first of our three different channels through which direct democracy may influence public finances (see section 2). Our results on differences between property and business taxes indicate that unbundling of policies is a likely mechanism. Town meetings cut taxes selectively: the more taxes target the full population, the more robust our evidence for tax cuts under 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?

²⁷State and local governments in the US raise \$ 0.5 trillion in property taxes or 1.5% of total property values which stand around \$ 33.3 trillion in 2018.

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 2). Second, farming plays an important role in Schleswig-Holstein. Agriculture covers around three-quarters of total area; farmers are employers and local opinion leaders. At least in our small sample municipalities, agriculture represents a substantial share of the local economy.²⁸ This may map into the asymmetric tax rate effects we document for agriculture and businesses in general.²⁹ However, the direct democracy effects for the general property tax are always more robust and substantial than for the agriculture property tax.

6.2 Overspending by representatives

Reducing overspending tendencies by representatives could be a second mechanism. The overspending hypothesis implicitly assumes a deviation between the median citizen and the median council member due to agency problems or representation biases. Compared to 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’.³⁰ However, from a principal-agent perspective, in communities of slightly more than 70 inhabitants, monitoring and social control of council members by the voters is arguably very strong. It is not very likely that few councilors can effectively form a coalition to exploit the remainder population for a long time. Our results for expenditures do also not

²⁸The agricultural property tax accounts for 6% of total tax revenues in small municipalities with a population of less than 140, but only for 2% of total tax revenues in the full sample. The share of the general property tax B is very similar in small municipalities compared to the full sample (7% each).

²⁹Note that local business taxes do not apply to agriculture businesses.

³⁰There are some anecdotes from town twinning municipalities. 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’ (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>). 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 raised the rhetorical question ‘Well, nobody here has a shovel?’ and closed the session. (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>.)

support the overspending hypothesis. Table A.7 in the Online appendix has shown that main expenditure categories hardly change in town meeting municipalities.

Representation biases could be a second source of overspending tendencies. To analyze differences between citizens in town meetings and representatives in councils, we collect and digitized protocols of 229 sessions from five selected municipalities between 2008 and 2020. The protocols cover three election terms. All municipalities are somewhat equally sized with a population around the threshold of 70.³¹ Two out of five municipalities switched to town meetings for one election term, and back to councils afterwards during this period. Two municipalities always had councils. One municipality had town meetings only. This allows us to exploit within-variation using fixed-effects regression models; identification comes from temporary differences in local constitutions in two of the five municipalities. The protocols include the name list of all participants, the duration, and the agenda for all sessions.

Table A.10 in the Online appendix shows that town meetings and councils differ in size but not in legislator composition or agenda. We conduct panel regressions at the level of 229 sessions using year and municipality fixed effects. Column (1) shows that, as expected, more legislators (around 10 in our case) participate in town meetings compared to councils. We will come back to legislature size in section 6.3 in more detail. The composition of town meetings, however, does not differ from councils. Column (2) and (3) show that the share of female and of ‘clan’ participants (participants with a name which appears more than twice on the participation list) are not statistically different in town meetings. In column (4) and (5), we analyze the agenda and again do not detect significant differences: tax policies are not more pronounced and the complexity of the sessions, measured by the duration of the session, does not seem to deviate. Therefore, the overspending channel as a result of re-election motives or principal-agent complexities is not a likely mechanism explaining differences between direct democracy and councils in our case.

³¹Our sample includes the municipalities of Dunsum, Hingstheide, Rade, Süderende, and Witsum.

6.3 Legislature size

Finally, legislature size could play a role. We have shown in Table A.10 in the Online appendix that more legislators gather after municipalities switched from councils to town meetings. As outlined in section 2, some theories expect larger assemblies to be associated with tougher monitoring and lower expenditures. Our setting allows for some tentative evidence. 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 1). 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 A.11 in the Online appendix, 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 assume transaction and information costs to be somewhat comparable 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 A.11), results barely change. Town meeting municipalities still have lower property tax rates than council municipalities with a similar number of the legislators. Thus, differences between direct democracy and councils remain even when we shut down legislature size.

7 Conclusion

We have shown that citizens implement different tax policies than councils, but do so selectively. Property tax rates decrease by some 10 to 15% when citizens design tax policies. By contrast, we do not find that business taxes change. We conclude that direct democracy comes with sizable but selective tax cuts: the more taxes target the entire population, the more robust our evidence for tax cuts under direct democracy. Thus, direct democracy seems to entail incentives to deliver policies for the many and not for special interest groups.

Our results are derived from very small communities which allows us to discuss different working mechanisms. The most likely explanation for the robust differences in tax rates between direct and representative democracy is the unbundling of policies. In town meetings, citizens can design tax policies more individually than voting for a high-tax or a low-tax party in elections. By contrast, other theories such as agency issues between voters and representatives or differences in legislature size can hardly explain our results. Our results suggest that direct democratic institutions are useful complements to representative decision-making in individual cases, but there is no need for a full replacement of parliaments by popular votes.

Further research may elaborate more on the role of community size. Informal norms and repeated interactions may well differ in larger groups. For example, the public discourse, media influence, and political campaigning work differently in nations than in small rural communities. Another interesting topic for new studies is delegation. Town meetings may decide to delegate subordinate decisions to committees or advisory boards. We investigated two clear concepts: full direct democracy and full representative decision making. However, if town meetings delegate parts of decision making, institutions move towards parliamentary democracy. Future research may investigate how fiscal policies change in the ‘mix’ of representative and direct democracy.

References

- ADE, F. (2014). Do constitutions matter? Evidence from a natural experiment at the municipality level. *Public Choice*, **160** (3-4), 367–389.
- AHLFELDT, G. M. and MAENNIG, W. (2015). Homevoters vs. leasevoters: A spatial analysis of airport effects. *Journal of Urban Economics*, **87**, 85–99.
- ASATRYAN, Z. (2016). The indirect effects of direct democracy: Local government size and non-budgetary voter initiatives in Germany. *International Tax and Public Finance*, **23** (3), 580–601.
- , BASKARAN, T., GRIGORIADIS, T. and HEINEMANN, F. (2017a). Direct democracy and local public finances under cooperative federalism. *Scandinavian Journal of Economics*, **119** (3), 801–820.
- , — and HEINEMANN, F. (2017b). The effect of direct democracy on the level and structure of local taxes. *Regional Science and Urban Economics*, **65**, 38–55.
- BASKARAN, T. (2013). Coalition governments, cabinet size, and the common pool problem: Evidence from the German states. *European Journal of Political Economy*, **32**, 356 – 376.
- (2014). Identifying local tax mimicking with administrative borders and a policy reform. *Journal of Public Economics*, **118**, 41–51.
- BEATH, A., CHRISTIA, F. and ENIKOLOPOV, R. (2017). Direct democracy and resource allocation: Experimental evidence from Afghanistan. *Journal of Development Economics*, **124**, 199–213.
- BEL, G., RAUDLA, R., RODRIGUES, M. and TAVARES, A. F. (2018). These rules are made for spending: testing and extending the law of $1/n$. *Public Choice*, **174** (1), 41–60.
- BESLEY, T. and CASE, A. (2003). Political institutions and policy choices: Evidence from the United States. *Journal of Economic Literature*, **41** (1), 7–73.
- and COATE, S. (2008). Issue unbundling via citizens’ initiatives. *Quarterly Journal of Political Science*, **3** (4), 379–397.

- , PANDE, R. and RAO, V. (2005). Participatory democracy in action: Survey evidence from South India. *Journal of the European Economic Association*, **3** (2-3), 648–657.
- BLUME, L., MUELLER, J. and VOIGT, S. (2009). The economic effects of direct democracy—a first global assessment. *Public Choice*, **140** (3), 431–461.
- BRENDER, A. and DRAZEN, A. (2008). How do budget deficits and economic growth affect reelection prospects? Evidence from a large panel of countries. *The American Economic Review*, **98** (5), 2203–20.
- BREUILLE, M.-L., DURAN-VIGNERON, P. and SAMSON, A.-L. (2018). Inter-municipal cooperation and local taxation. *Journal of Urban Economics*, **107**, 47–64.
- BRYAN, F. M. (2004). *Real Democracy: The New England Town Meeting and How it Works*. University of Chicago Press.
- BUETTNER, T. (2003). Tax base effects and fiscal externalities of local capital taxation: evidence from a panel of German jurisdictions. *Journal of Urban Economics*, **54** (1), 110–128.
- and VON SCHWERIN, A. (2016). Yardstick competition and partial coordination: Exploring the empirical distribution of local business tax rates. *Journal of Economic Behavior & Organization*, **124**, 178–201.
- CALONICO, S., CATTANEO, M. D., FARRELL, M. H. and TITIUNIK, R. (2017). rdrobust: Software for regression-discontinuity designs. *Stata Journal*, **17** (2), 372–404.
- COATE, S. and KNIGHT, B. (2011). Government form and public spending: Theory and evidence from US municipalities. *American Economic Journal: Economic Policy*, **3** (3), 82–112.
- COPPEDGE, M., GERRING, J., KNUTSEN, C. H., LINDBERG, S. I., TEORELL, J., ALTMAN, D., BERNHARD, M., FISH, M. S., GLYNN, A., HICKEN, A., LÜHRMANN, A., MARQUARDT, K. L., MCMANN, K., PAXTON, P., PEMSTEIN, D., SEIM, B., SIGMAN, R., SKAANING, S.-E., STATON, J., CORNELL, A., GASTALDI, L., GJERLØW, H., MECHKOVA, V., VON RÖMER, J., SUNDTRÖM, A., TZELGOV, E., UBERTI, L.,

- TING WANG, Y., WIG, T., and ZIBLATT, D. (2019). *V-Dem Codebook v9*. Varieties of democracy (v-dem) project, University of Gothenburg.
- CORREA-LOPERA, G. (2019). Demand of direct democracy. *European Journal of Political Economy*, **60**, 101813.
- DIAMOND, R. (2017). Housing supply elasticity and rent extraction by state and local governments. *American Economic Journal: Economic Policy*, **9** (1), 74–111.
- DOLLS, M., FUEST, C., KROLAGE, C. and NEUMEIER, F. (2019). *Who bears the burden of real estate transfer taxes? Evidence from the German housing market*. ifo Working Paper Series 308, ifo Institute, Munich.
- EGGER, P. and KOETHENBUERGER, M. (2010). Government spending and legislative organization: Quasi-experimental evidence from Germany. *American Economic Journal: Applied Economics*, **2** (4), 200–212.
- EGGERS, A. C., FREIER, R., GREMBI, V. and NANNICINI, T. (2018). Regression discontinuity designs based on population thresholds: Pitfalls and solutions. *American Journal of Political Science*, **62** (1), 210–229.
- ELINDER, M. and PERSSON, L. (2017). House price responses to a national property tax reform. *Journal of Economic Behavior & Organization*, **144**, 18 – 39.
- ENIKOLOPOV, R. (2014). Politicians, bureaucrats and targeted redistribution. *Journal of Public Economics*, **120**, 74–83.
- FELD, L. P. and KIRCHGAESSNER, G. (2001a). Does direct democracy reduce public debt? Evidence from Swiss municipalities. *Public Choice*, **109** (3), 347–370.
- and — (2001b). The political economy of direct legislation: Direct democracy and local decision-making. *Economic Policy*, **16** (33), 330–367.
- and MATSUSAKA, J. G. (2003). Budget referendums and government spending: Evidence from Swiss cantons. *Journal of Public Economics*, **87** (12), 2703–2724.
- , SCHALTEGGER, C. A. and SCHNELLENBACH, J. (2008). On government centralization and fiscal referendums. *European Economic Review*, **52** (4), 611–645.

- FRANKE, F. (1996). *Gemeindeversammlung in Recht und Verwaltungspraxis in Schleswig-Holsteins Kleinstgemeinden*. Berlin: Verlag Dr. Köster.
- FREY, B. (1994). Direct democracy: Politico-economic lessons from Swiss experience. *The American Economic Review*, **84** (2), 338–342.
- FUEST, C., PEICHL, A. and SIEGLOCH, S. (2018). Do higher corporate taxes reduce wages? Micro evidence from Germany. *The American Economic Review*, **108** (2), 393–418.
- FUNK, P. and GATHMANN, C. (2011). Does direct democracy reduce the size of government? New evidence from historical data, 1890–2000. *The Economic Journal*, **121** (557), 1252–1280.
- and LITSCHIG, S. (2020). Policy choices in assembly versus representative democracy: Evidence from Swiss communes. *Journal of Public Economics*, **182**, 104122.
- GAEBLER, S. and ROESEL, F. (2019). Do direct elections matter? Quasi-experimental evidence from Germany. *International Tax and Public Finance*, **26** (6), 1416–1445.
- GALLETTA, S. (2021). Form of government and voters’ preferences for public spending. *Journal of Economic Behavior & Organization*, **186**, 548–561.
- and JAMETTI, M. (2015). How to tame two Leviathans? revisiting the effect of direct democracy on local public expenditure in a federation. *European Journal of Political Economy*, **39**, 82–93.
- GARMANN, S. (2015). Elected or appointed? How the nomination scheme of the city manager influences the effects of government fragmentation. *Journal of Urban Economics*, **86** (3), 26–42.
- GERBER, E. R. (1996). Legislative response to the threat of popular initiatives. *American Journal of Political Science*, **40** (1), 99–128.
- (1999). *The Populist Paradox: Interest Group Influence and the Promise of Direct Legislation*. Princeton University Press.
- GUILFOYLE, J. (2000). The effect of property taxes on home values. *Journal of Real Estate Literature*, **8** (2), 109–127.

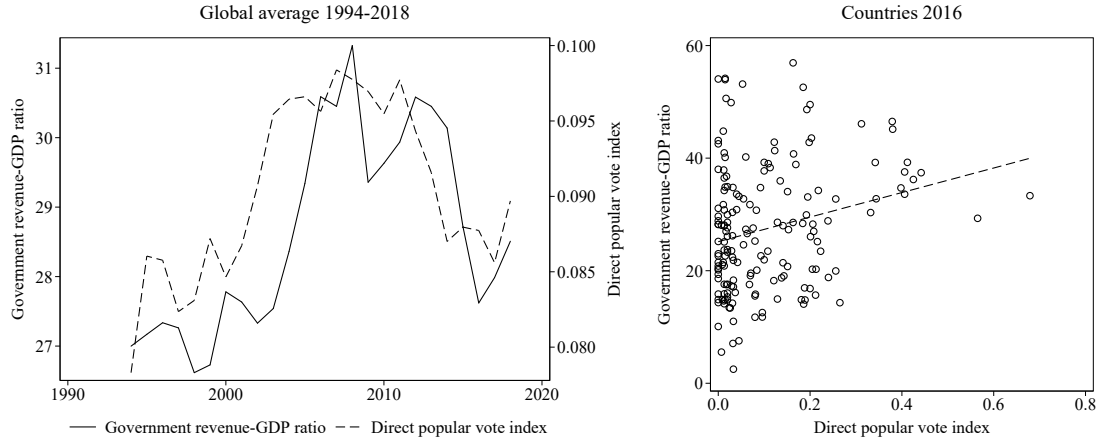
- HANKINS, W. B. (2015). Government spending, shocks, and the role of legislature size: Evidence from the American states. *Social Science Quarterly*, **96** (4), 1059–1070.
- HESSAMI, Z. (2018). Accountability and incentives of appointed and elected public officials. *The Review of Economics and Statistics*, **100** (1), 51–64.
- HINNERICH, B. T. and PETTERSSON-LIDBOM, P. (2014). Democracy, redistribution, and political participation: Evidence from Sweden 1919-1938. *Econometrica*, **82** (3), 961–993.
- HOEHMANN, D. (2017). The effect of legislature size on public spending: evidence from a regression discontinuity design. *Public Choice*, **173** (3), 345–367.
- INTERNATIONAL MONETARY FUND (2019). *World Economic Outlook Database, April 2019*. International Monetary Fund.
- KAUDER, B. and POTRAFKE, N. (2015). Just hire your spouse! Evidence from a political scandal in Bavaria. *European Journal of Political Economy*, **38**, 42 – 54.
- KIM, J. H. (2019). Direct democracy and women’s political engagement. *American Journal of Political Science*, **63** (3), 594–610.
- KOEPPL-TURYNA, M. (2016). Opportunistic politicians and fiscal outcomes: The curious case of Vorarlberg. *Public Choice*, **168** (3-4), 177–216.
- LAFORTUNE, J., ROTHSTEIN, J. and SCHANZENBACH, D. W. (2018). School finance reform and the distribution of student achievement. *American Economic Journal: Applied Economics*, **10** (2), 1–26.
- LEE, D. S. and LEMIEUX, T. (2010). Regression discontinuity designs in economics. *Journal of Economic Literature*, **48** (2), 281–355.
- LEININGER, A. and HEYNE, L. (2017). How representative are referendums? evidence from 20 years of Swiss referendums. *Electoral Studies*, **48**, 84–97.
- LEWIS, D. C., SCHNEIDER, S. K. and JACOBY, W. G. (2015). The impact of direct democracy on state spending priorities. *Electoral Studies*, **40**, 531–538.

- LÖFFLER, M. and SIEGLOCH, S. (2018). *Property Taxes, Housing, and Local Labor Markets: Evidence from Germany*. Working paper, University of Mannheim, Mannheim.
- LORZ, O. and WILLMANN, G. (2005). On the endogenous allocation of decision powers in federal structures. *Journal of Urban Economics*, **57** (2), 242–257.
- LOWRY, R. C., ALT, J. E. and FERREE, K. E. (1998). Fiscal policy outcomes and electoral accountability in American states. *American Political Science Review*, **92** (4), 759–774.
- LUTZ, B. (2015). Quasi-experimental evidence on the connection between property taxes and residential capital investment. *American Economic Journal: Economic Policy*, **7** (1), 300–330.
- MACDONALD, L. (2008). The impact of government structure on local public expenditures. *Public Choice*, **136** (3-4), 457–473.
- MASKIN, E. and TIROLE, J. (2004). The politician and the judge: Accountability in government. *The American Economic Review*, **94** (4), 1034–1054.
- MATSUSAKA, J. G. (1995). Fiscal effects of the voter initiative: Evidence from the last 30 years. *Journal of Political Economy*, **103** (3), 587–623.
- (2005a). Direct democracy works. *Journal of Economic Perspectives*, **19** (2), 185–206.
- (2005b). The eclipse of legislatures: Direct democracy in the 21st century. *Public Choice*, **124** (1), 157–177.
- (2009). Direct democracy and public employees. *The American Economic Review*, **99** (5), 2227–46.
- (2018). Public policy and the initiative and referendum: a survey with some new evidence. *Public Choice*, **174** (1), 107–143.
- MCCRARY, J. (2008). Manipulation of the running variable in the regression discontinuity design: A density test. *Journal of Econometrics*, **142** (2), 698 – 714.
- NGUYEN-HOANG, P. (2012). Fiscal effects of budget referendums: Evidence from New York school districts. *Public Choice*, **150** (1), 77–95.

- NISKANEN, W. A. (1968). The peculiar economics of bureaucracy. *The American Economic Review*, **58** (2), 293–305.
- NOAM, E. M. (1980). The efficiency of direct democracy. *Journal of Political Economy*, **88** (4), 803–810.
- OLKEN, B. A. (2010). Direct democracy and local public goods: Evidence from a field experiment in Indonesia. *American Political Science Review*, **104** (2), 243–267.
- PELTZMAN, S. (1992). Voters as fiscal conservatives. *The Quarterly Journal of Economics*, **107** (2), 327–361.
- PERSSON, T. (2004). Consequences of constitutions. *Journal of the European Economic Association*, **2** (2-3), 139–161.
- and TABELLINI, G. (2003). *The Economic Effects of Constitutions: What Do the Data Say*. Cambridge, Massachusetts: MIT Press.
- PETTERSSON-LIDBOM, P. (2012). Does the size of the legislature affect the size of government? Evidence from two natural experiments. *Journal of Public Economics*, **96** (3), 269–278.
- POTRAFKE, N. (2013). Evidence on the political principal-agent problem from voting on public finance for concert halls. *Constitutional Political Economy*, **24** (3), 215–238.
- PRIMO, D. M. and SNYDER, J. M. (2008). Distributive politics and the law of $1/n$. *The Journal of Politics*, **70** (2), 477–486.
- ROBINSON, J. and TORVIK, R. (2016). Endogenous presidentialism. *Journal of the European Economic Association*, **14** (4), 907–942.
- ROESEL, F. (2017). Do mergers of large local governments reduce expenditures? – Evidence from Germany using the synthetic control method. *European Journal of Political Economy*, **50**, 22–36.
- ROGOFF, K. (1990). Equilibrium political budget cycles. *The American Economic Review*, **80**, 21–36.

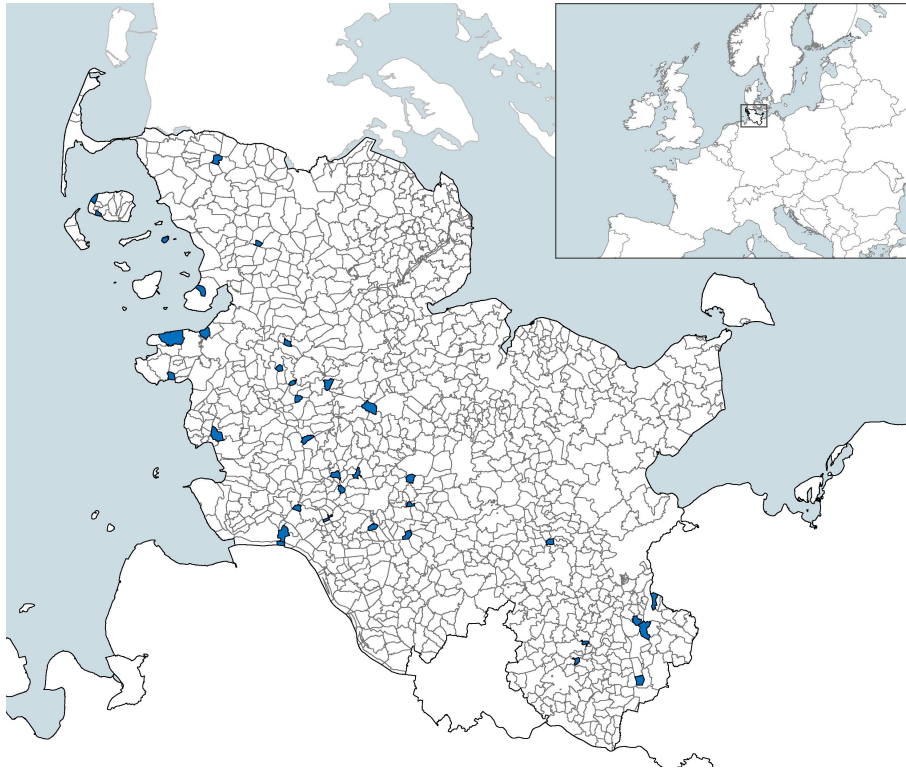
- ROMER, T. and ROSENTHAL, H. (1979). Bureaucrats versus voters: On the political economy of resource allocation by direct democracy. *The Quarterly Journal of Economics*, **93** (4), 563–587.
- SAHA, S. (2011). City-level analysis of the effect of political regimes on public good provision. *Public Choice*, **147** (1), 155–171.
- SALVINO, R., TASTO, M. T. and TURNBULL, G. K. (2012). A direct test of direct democracy: New England town meetings. *Applied Economics*, **44** (18), 2393–2402.
- SANCES, M. W. (2018). Something for something: How and why direct democracy impacts service quality. *Quarterly Journal of Political Science*, **13** (1), 29–57.
- SANZ, C. (2020). Direct democracy and government size: evidence from Spain. *Political Science Research and Methods*, **8** (4), 630–645.
- SCHELKER, M. and EICHENBERGER, R. (2010). Auditors and fiscal policy: Empirical evidence on a little big institution. *Journal of Comparative Economics*, **38** (4), 357–380.
- SCHMIDHEINY, K. and SIEGLOCH, S. (2019). *On Event Study Designs and Distributed-Lag Models: Equivalence, Generalization and Practical Implications*. CESifo Working Paper Series 7481, CESifo.
- SUN, L. and ABRAHAM, S. (2020). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, **forthcoming**.
- WEINGAST, B. R., SHEPSLE, K. A. and JOHNSON, C. (1981). The political economy of benefits and costs: A neoclassical approach to distributive politics. *Journal of Political Economy*, **89** (4), 642–664.
- WHALLEY, A. (2013). Elected versus appointed policy makers: Evidence from city treasurers. *Journal of Law and Economics*, **56** (1), 39–81.
- WOLLMANN, H. and ROTH, R. (eds.) (1999). *Kommunalpolitik: politisches Handeln in den Gemeinden*. Opladen: Leske + Budrich, 2nd edn.

Figure 1: Direct democracy and government revenues



Notes: The figures compare a direct democracy index defined between 0 and 1 (V-Dem project popular vote index by Coppedge *et al.*, 2019) to general government revenues in % of GDP (International Monetary Fund, 2019). The left-hand figure plots the global average of the popular vote index and government revenues in % of GDP for a balanced panel of 103 countries for which both variables are available since 1994. The right-hand figure shows the year 2016 for which data for 171 countries are available.

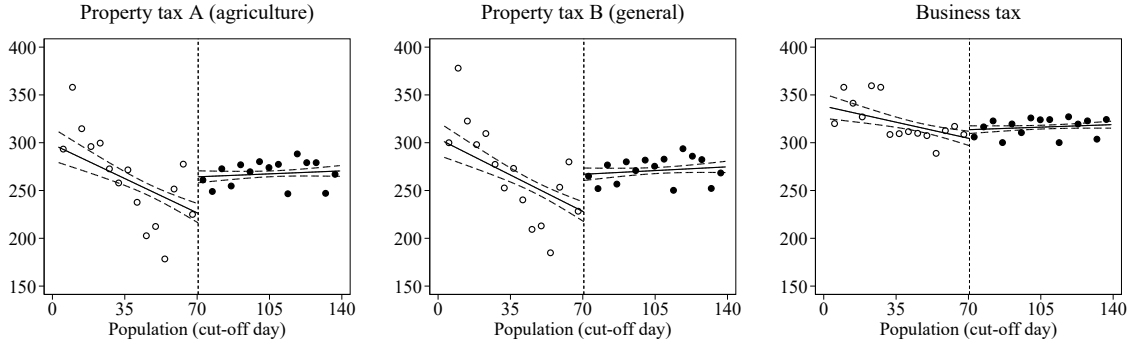
Figure 2: Municipalities of Schleswig-Holstein



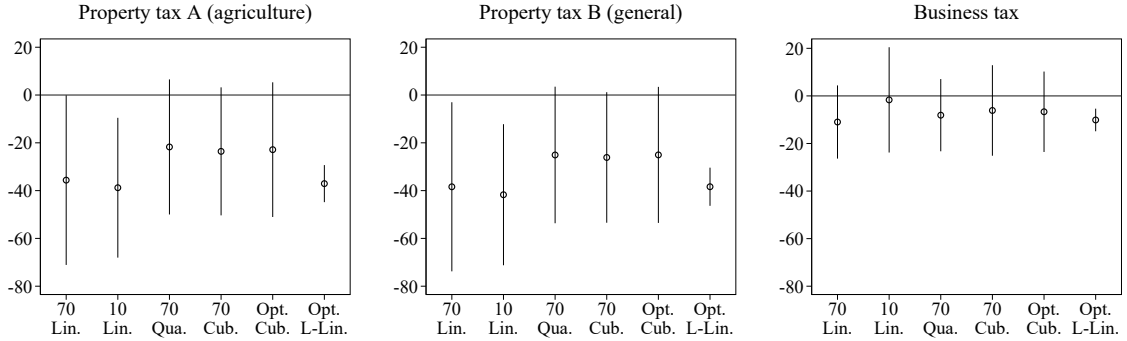
Notes: The map shows the German state of Schleswig-Holstein bordering Denmark (North), and the German states of Lower Saxony (West), Hamburg (South), and Mecklenburg-West Pomerania (East). Black lines are federal state boundaries, gray lines within Schleswig-Holstein describe municipality boundaries. 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 Online appendix traces all town meeting municipalities over time.

Figure 3: RD estimates

Baseline RD

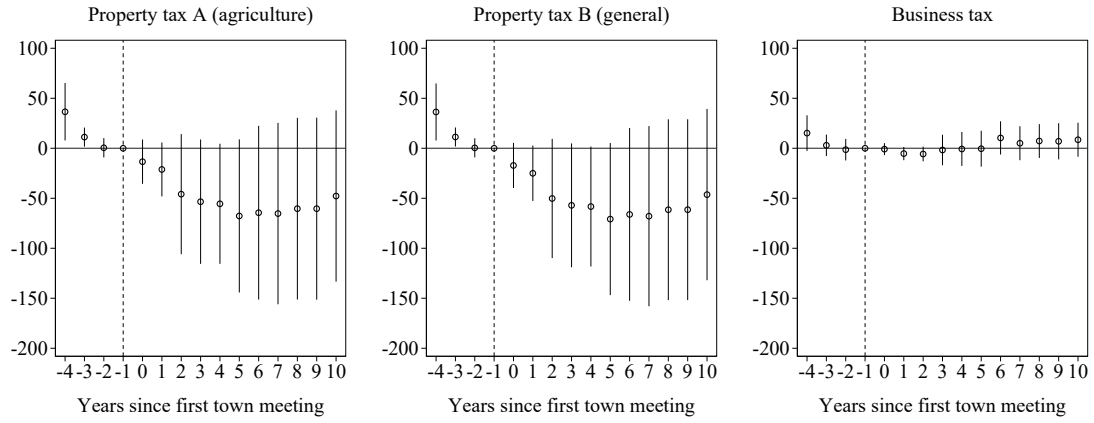


Different bandwidths and polynomial orders

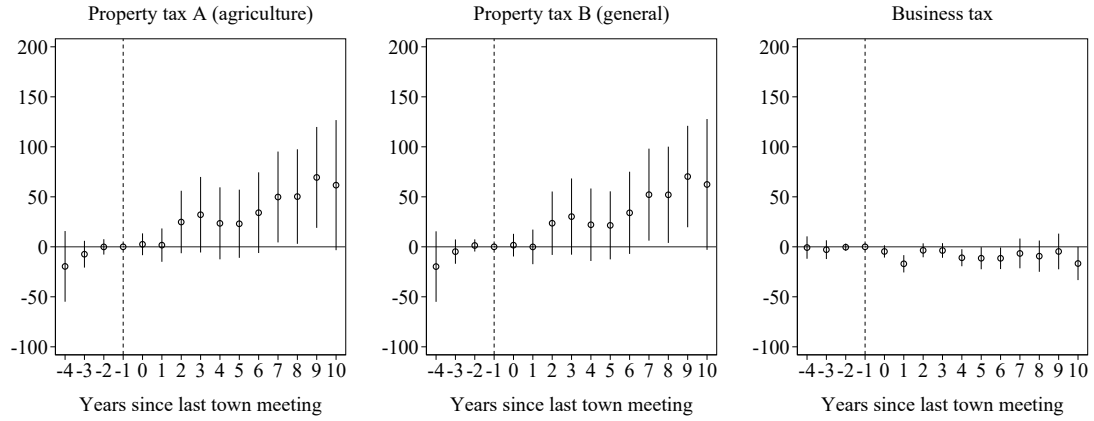


Notes: The upper figures show binned local tax rates and a linear fit (property tax rate A for agriculture, general property tax rate B, and business tax rate) for a bandwidth of ± 70 inhabitants around the population threshold of 70. Municipalities of 70 and less inhabitants have town meetings. Data are averaged in 15 bins on both sides of the threshold. The lower figures report RD estimates for different bandwidths (10, 70, optimal data-driven bandwidth) and polynomial fits (linear, quadratic, cubic, local-linear); the first specification in each figure refers to the corresponding baseline RD plot.

Figure 4: Event study estimates
Switching from councils to town meetings



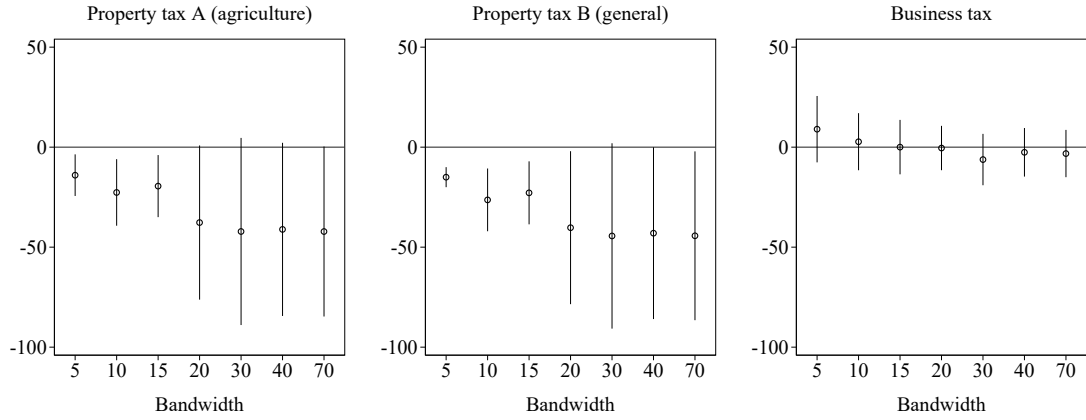
Switching from town meetings to councils



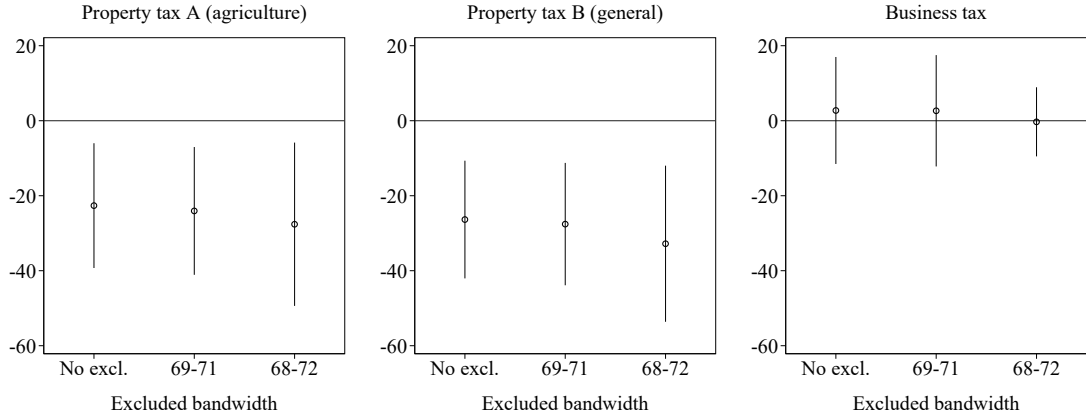
Notes: The figures report regression point estimates from (difference-in-differences) event study specifications. Dummy variables measure the time distance to the year where municipalities switch from councils to town meetings or switch from town meetings to councils (year 0). Vertical solid bars represent 90% confidence intervals. The optimal bandwidth derived in the RD procedures apply (see, columns (7) to (9) in Table 3).

Figure 5: Sensitivity to bandwidths

Bandwidth variation

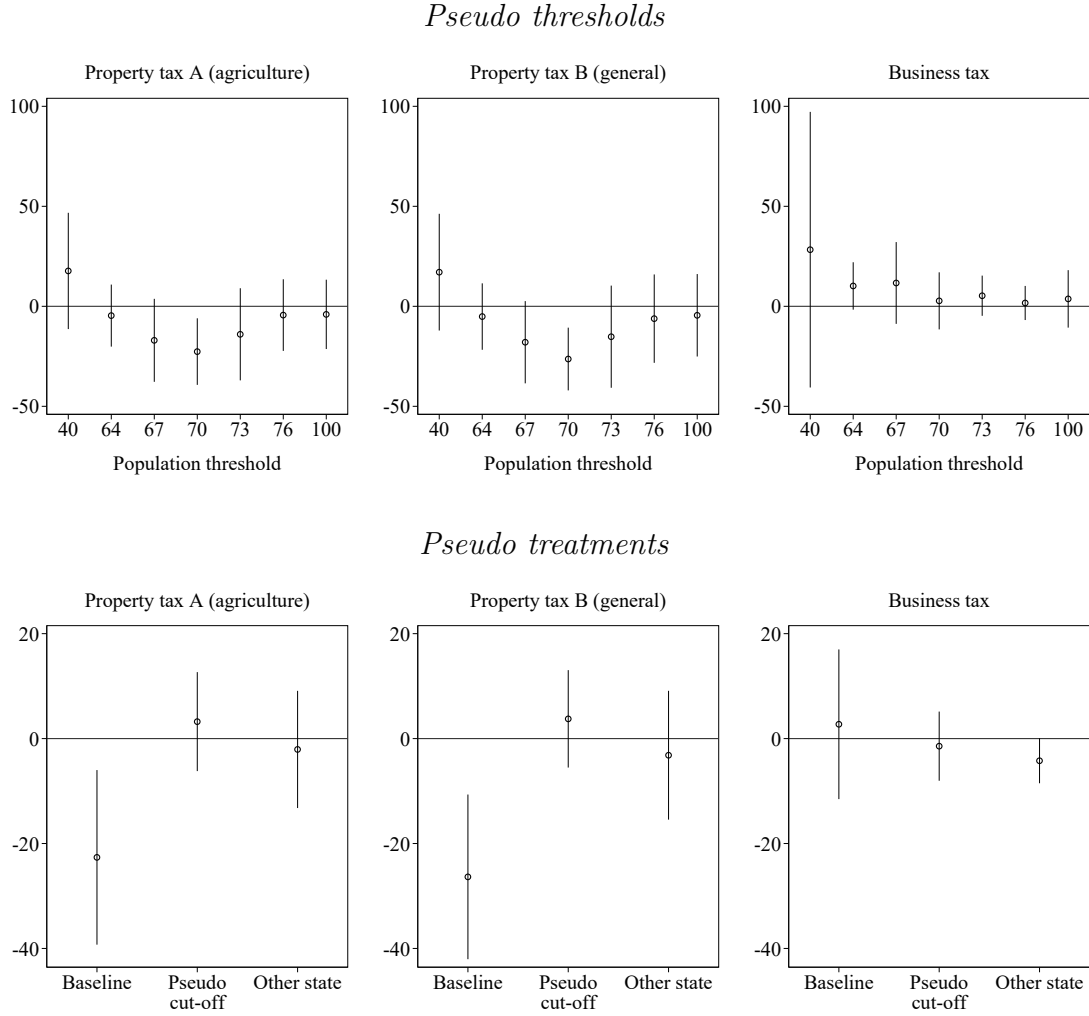


Donut regressions



Notes: Top figures report difference-in-differences regression estimates of town meetings for different bandwidths from ± 5 to ± 70 around the threshold. Bottom figures report regression point estimates when we exclude observations around the threshold (none, 67–71 inhabitants, 68–72 inhabitants). Vertical solid bars represent the 90% confidence interval. The baseline bandwidth is ± 10 inhabitants and excludes no observations around the threshold of 70 (specifications in columns (4) to (6) in Table 3).

Figure 6: Pseudo analyses



Notes: Top figures report difference-in-differences regression estimates of town meetings when we shift the actual threshold (70 inhabitants) to pseudo thresholds (40, 64, 67, 73, 76, and 100 inhabitants). Bottom 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. Vertical solid bars represent the 90% confidence interval. The baseline specification uses cut-off day population of 70 in Schleswig-Holstein (specifications in columns (4) to (6) in Table 3).

Table 1: Key facts on Schleswig-Holstein town meetings

	All municipalities				
	Obs.	Mean	Std. Dev.	Min	Max
	(1)	(2)	(3)	(4)	(5)
<i>Presence and duration</i>					
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
<i>Agenda</i>					
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 key facts from hand-collected protocols of 167 town meetings in 16 different municipalities of the state of Schleswig-Holstein between 2008 and 2017. Data cover around half of the municipalities with a town meeting.

Table 2: Descriptive statistics

	All municipalities					Town meeting = 1
	Obs.	Mean	Std. Dev.	Min	Max	Mean
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Direct democracy</i>						
Town meeting	45,000	0.02	0.15	0	1	1
Population (Cut-off day)	22,398	2,489	11,084	3	246,033	46.88
Population (31st December)	22,398	2,521	11,103	2	247,943	47.46
<i>Tax rates</i>						
Property tax A (agriculture)	44,992	249.15	46.33	0	690	236.78
Property tax B (general)	44,992	255.46	47.19	0	700	238.37
Business tax	44,992	302.45	33.65	0	750	299.37
<i>Expenditures (log, per capita)</i>						
Total expenditures	13,427	7.08	0.46	5.38	13.96	7.54
Current expenditures	13,427	6.81	0.40	4.47	13.87	7.25
Capital expenditures	13,175	5.26	1.20	-5.81	13.26	5.46
<i>National election outcomes</i>						
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
Far-right populist vote share	12,351	1.85	2.71	0	38.10	1.49

Notes: The table shows the descriptive statistics of the dataset. The unit of observation are around 1,100 municipalities in the German state of Schleswig-Holstein. We have information on local constitutions (town meetings/councils) 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; expenditures are available after 2003. National election outcomes refer to eleven German national elections since 1980.

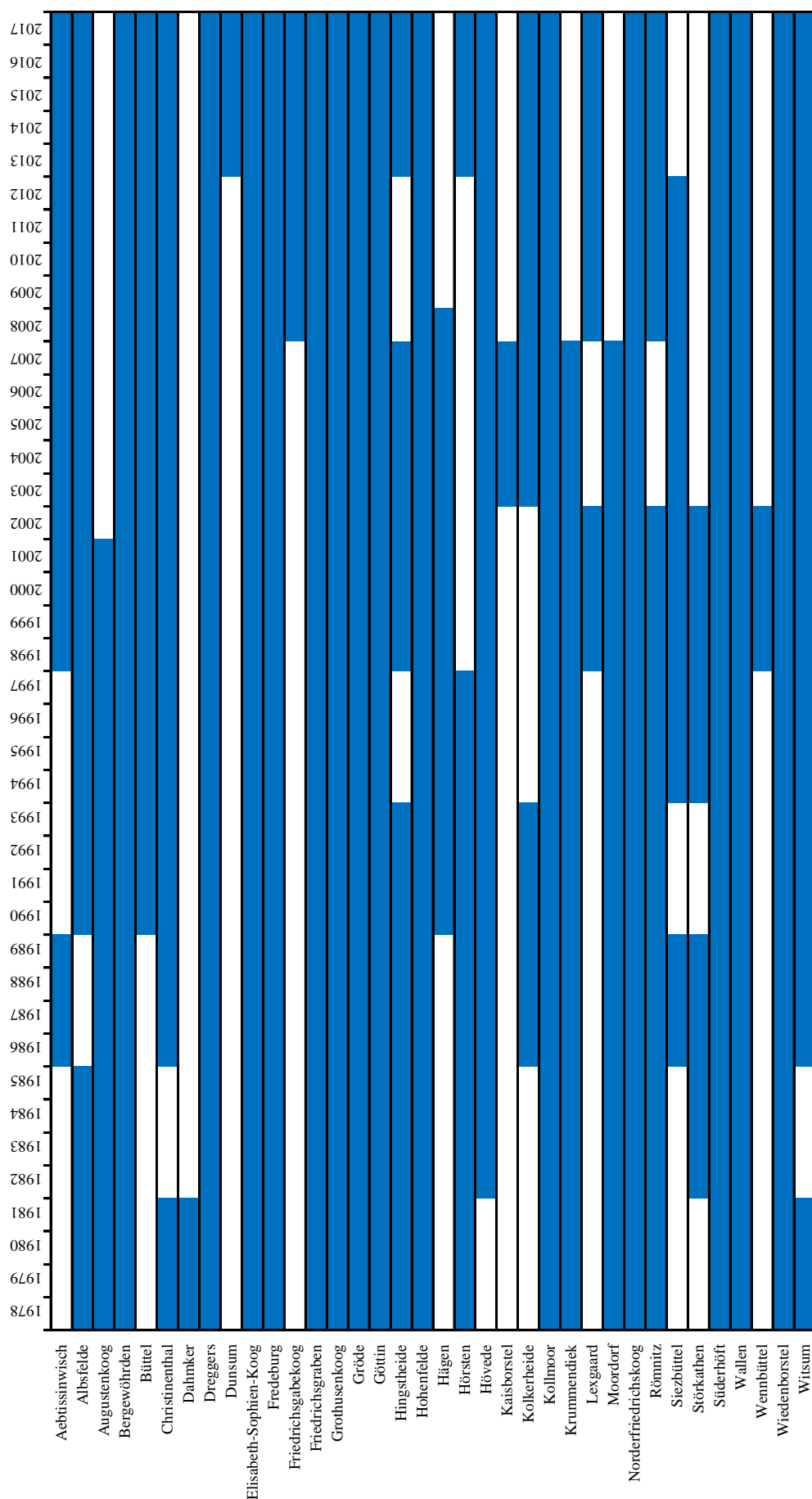
Table 3: Difference-in-differences estimates

	1978–2017				1998–2017			
	Prop. tax A (agr.)	Prop. tax B (gen.)	Busi- ness tax		Prop. tax A (agr.)	Prop. tax B (gen.)	Busi- ness tax	
	(1)	(2)	(3)		(4)	(5)	(6)	(7)
Town meeting	-21.81 (16.73)	-22.59 (16.46)	-1.58 (8.94)		-22.64** (9.71)	-26.35*** (9.16)	2.73 (8.32)	-43.37* (25.63)
Mun. fixed effects	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes		Yes	Yes	Yes	Yes
Bandwidth	–	–	–		10	10	10	154.61
Mean of dep. var.	249.16	255.46	302.45		254.59	257.29	313.09	261.89
Municipalities	1,145	1,145	1,145		24	24	24	191
Observations	44,956	44,956	44,956		270	270	270	3370
Within R ²	0.63	0.61	0.51		0.45	0.44	0.38	0.26
								0.23
								0.36
								178.39
								314.62
								222
								3930
								0.36

Notes: The table reports our baseline difference-in-differences regression estimates of town meetings. The unit of observation are municipalities in the German state of Schleswig-Holstein. The dependent variables are local tax rates (property tax rate A for agriculture, general property tax rate B, and business tax rate). Different bandwidths around the population threshold of 70 apply. Significance levels (standard errors clustered at the municipality level): *** 0.01, ** 0.05, * 0.1.

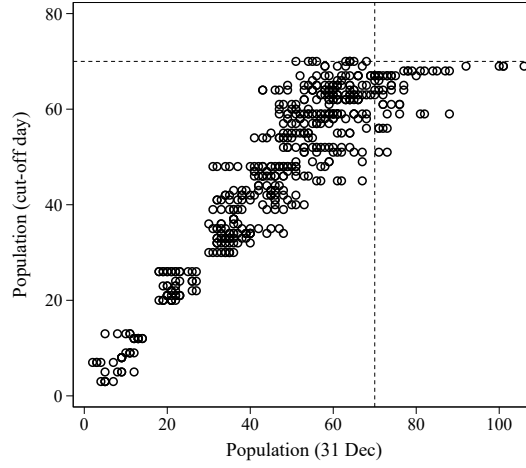
Online appendix (for online publication only)

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, Hågen, and Siezbüttel were merged with neighboring municipalities during the election term.

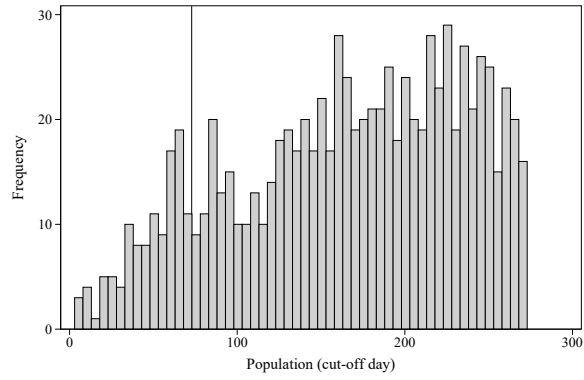
Figure A.2: Population in town meeting municipalities



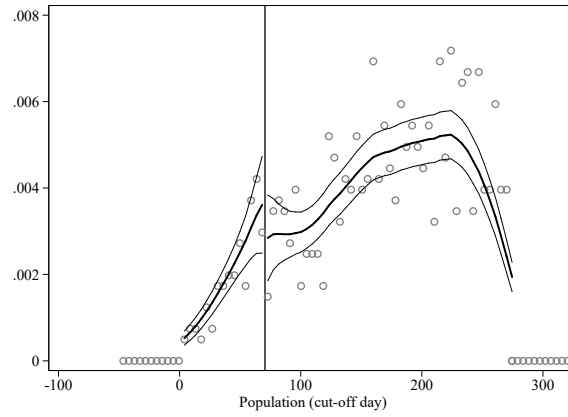
Notes: The figure compares population at the cut-off day determining the form of government (31st December, three years before the election year) with the actual annual population (31st December) for town meeting municipalities in our baseline sample. We use population at the cut-off days for the local 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 A.3: Manipulation tests

Histogram (bandwidth: 200, bin width: 5 inhabitants)



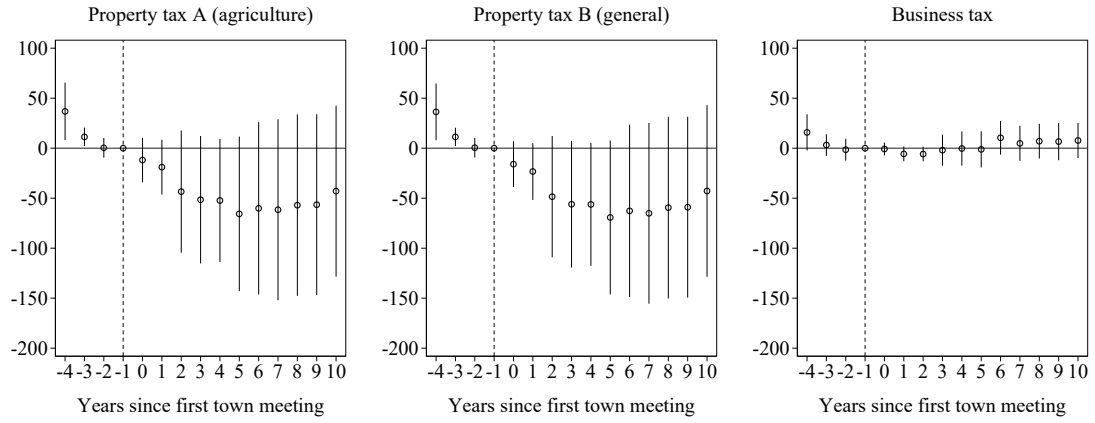
McCrary (2008) bunching test (bandwidth: 200)



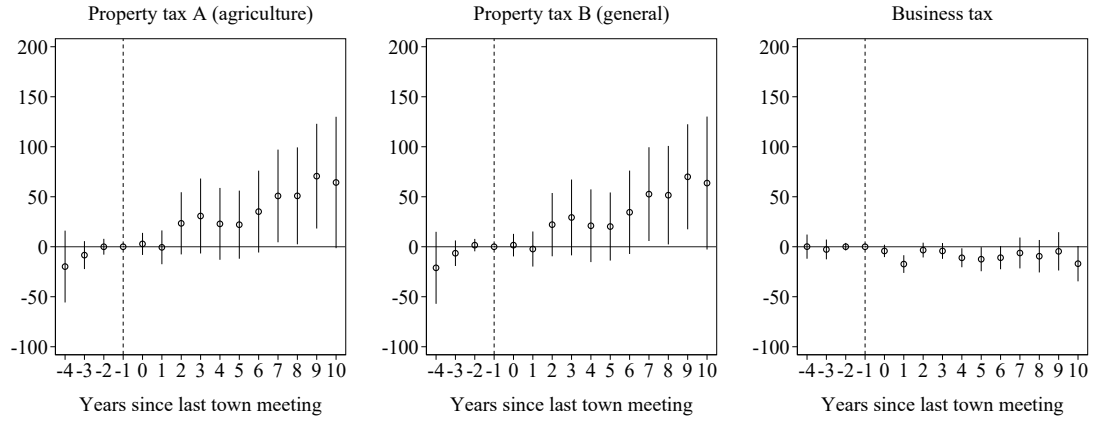
Notes: The the upper graph plots the number of observations; the bottom figure shows the density of observations at both sides of the threshold of 70 inhabitants. The corresponding discontinuity estimate and standard errors as suggested by McCrary (2008) are -0.30 (0.26) for when using a bandwidth population of 200. We use population at the cut-off days for the local elections in 1998, 2003, 2008, and 2013 when population data are available.

Figure A.4: Event study estimates (smaller bandwidth)

Switching from councils to town meetings

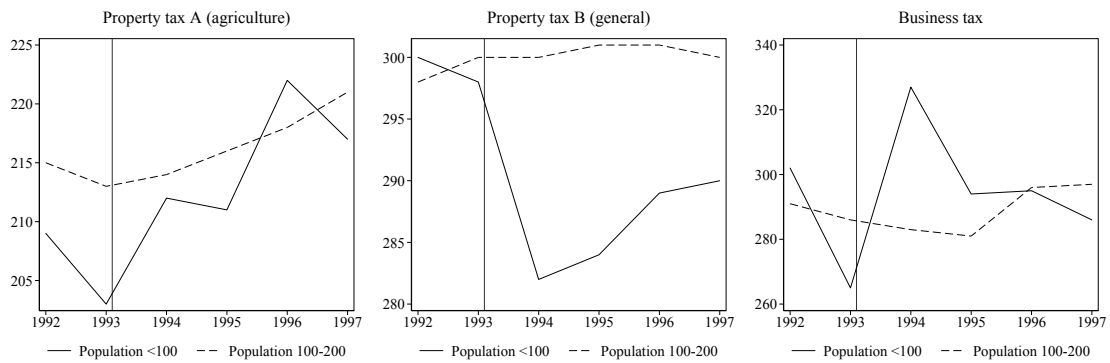


Switching from town meetings to councils



Notes: The figures report regression point estimates from (difference-in-differences) event study specifications. Dummy variables measure the time distance to the year where municipalities switch from councils to town meetings or switch from town meetings to councils (year 0). We exclude municipalities which always had a town meetings. Vertical solid bars represent 90% confidence intervals. A bandwidth of 70 applies.

Figure A.5: Evidence from Brandenburg municipalities



Notes: The figures show average tax rates for two groups of municipalities in the German state of Brandenburg. Solid lines represent tax rates in municipalities with a population of less than 100, which had local councils until 1993 and town meetings since 1994 (but municipalities could opt out). Dashed lines represent tax rates in municipalities with a population between 100 and 200 which had councils for the entire period.

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

			Balancing test at threshold	
	Pop. < 140	State average	Local- linear RD	Band- width
	(1)	(2)	(3)	(4)
<i>Population (2017)</i>				
Population (Cut-off day)	83.37	2,555.60	0.00	246
Population (31st December)	85.32	2,603.44	-2.19	224
<i>Census (2011)</i>				
Share of female population	48.61	50.13	-0.41	284
Share of foreign population	1.43	1.88	-1.48	256
Share of population age < 18	17.30	18.71	-3.06	268
Share of population age > 75	8.59	8.18	1.69	278
Share of married population	48.47	48.72	-3.06	244
Share of Protestant population	66.89	61.84	4.76	234
<i>Geography (2016)</i>				
Area (total)	550.11	1,414.85	-47.45	236
Share of settlement area	3.36	8.24	0.53	228
Share of traffic area	3.19	4.23	0.22	272
Share of agriculture area	78.55	71.96	-1.29	242
Share of water area	3.42	3.67	-0.32	208
<i>Economy (2017)</i>				
Population share employees	34.63	37.10	1.63	264
Population share unemployed	2.20	2.18	-0.57	241
Share of long-term unemployed	53.44	48.69	–	–
Firm size (employees)	5.23	6.23	–	–
<i>Housing (2011)</i>				
Share of owner occupied dwellings	66.59	63.24	0.49	288
Share of rented dwellings	25.35	30.95	-1.40	294
Share of holiday dwellings	5.20	3.03	1.27	260
Share of vacant dwellings	2.86	2.78	-0.44	274
Max. obs.	71	1,116	303	–

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 A.2: Population thresholds for town meetings in other German states

	Population size threshold		Abolished in
	Compulsory town meeting	Voluntary town meeting	
	(1)	(2)	(3)
Schleswig-Holstein	70	–	–
Brandenburg	–	100	2003
Lower Saxony	100	200	1977/1963
Hesse	100	–	1976
North Rhine-Westphalia	–	100	1975
Baden-Württemberg	–	200	1974

Notes: The table reports population thresholds for compulsory or voluntary town meetings in German states. Information are taken from Franke (1996) and state law. The voluntary town meeting threshold in Lower Saxony was already abolished by 1963. Voluntary town meetings in Baden-Württemberg and North Rhine-Westphalia were designed as opt-ins, in Brandenburg as opt-outs.

Table A.3: Transition of government forms

	Number of switches		Number of municipalities between 1978 and 2013				
	To town meeting	From town meeting	1 switch	2 switches	3 switches	4 switches	5 switches
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1978	3	2	—	—	—	—	—
1982	2	3	—	—	—	—	—
1986	5	1	—	—	—	—	—
1990	3	3	—	—	—	—	—
1994	2	2	—	—	—	—	—
1998	4	1	—	—	—	—	—
2003	2	4	—	—	—	—	—
2008	3	3	—	—	—	—	—
2013	3	0	—	—	—	—	—
Total	27	19	6	7	4	1	2

Notes: The table counts switches to or from town meetings following local election terms in Schleswig-Holstein from 1978 to 2017 (column (1) and (2)). Columns (3) to (7) reports the number of municipalities by the number of switches between 1978 and 2017. Information on town meetings before 1998 are from Franke (1996), information after 1998 are self-compiled from cut-off day population data.

Table A.4: 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 2017, election days and cut-off days determining town meeting municipalities. Information on town meetings before 1998 are from Franke (1996), information after 1998 are self-compiled from cut-off day population data.

Table A.5: Election term averages

	1978–2017			1998–2017					
	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 (gen.)	Business tax
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Town meeting	-19.73 (15.90)	-20.43 (15.64)	-1.27 (8.84)	-22.64** (9.71)	-26.35*** (9.16)	2.73 (8.33)	-43.77* (25.60)	-45.73* (25.50)	-3.44 (6.89)
Mun. fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	–	–	–	10	10	10	192.23	184.67	213.10
Mean of dep. var.	246.32	252.77	300.67	254.59	257.29	313.09	262.01	266.10	313.84
Municipalities	1,143	1,143	1,143	24	24	24	237	230	255
Observations	10,123	10,123	10,123	54	54	54	843	810	929
Within R ²	0.67	0.65	0.56	0.67	0.65	0.47	0.36	0.34	0.45

Notes: The table reproduces regression results of Table 3 using election term averages instead of annual observations. The unit of observation are municipalities in the German state of Schleswig-Holstein. Significance levels (standard errors clustered at the municipality level): *** 0.01, ** 0.05, * 0.1.

Table A.6: Excluding 'tax havens'

	1978–2017			1998–2017					
	Prop. tax A (agr.)	Prop. tax B (gen.)	Busi- ness tax	Prop. tax A (agr.)	Prop. tax B (gen.)	Busi- ness tax	Prop. tax A (agr.)	Prop. tax B (gen.)	Busi- ness tax
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Town meeting	-7.10 (7.84)	-7.98 (7.49)	-1.45 (8.92)	-22.64** (9.71)	-26.35*** (9.16)	2.73 (8.32)	-18.16** (7.06)	-20.03*** (7.10)	-3.45 (6.93)
Mfun. fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	–	–	–	10	10	10	154.61	146.67	178.39
Mean of dep. var.	249.86	255.96	302.80	257.45	260.18	313.09	264.32	267.46	315.02
Municipalities	1,142	1,143	1,143	24	24	24	191	178	222
Observations	44,829	44,868	44,904	267	267	270	3339	3122	3925
Within R ²	0.66	0.64	0.54	0.57	0.55	0.38	0.38	0.36	0.39

Notes: The table reports our baseline difference-in-differences regression estimates of town meetings, but we exclude tax rates of zero. The unit of observation are municipalities in the German state of Schleswig-Holstein. The dependent variables are local tax rates (property tax rate A for agriculture, general property tax rate B, and business tax rate). Different bandwidths around the population threshold of 70 apply. Significance levels (standard errors clustered at the municipality level): *** 0.01, ** 0.05, * 0.1.

Table A.7: Expenditure results

	2003–2014							
	Total expen.	Current expen.	Capital expen.	Total expen.	Current expen.	Capital expen.	Total expen.	Current expen.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Town meeting	0.02 (0.10)	0.07 (0.12)	-0.21 (0.21)	-0.01 (0.07)	-0.09 (0.07)	0.25 (0.26)	0.04 (0.09)	0.08 (0.12)
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	144.09	122.24
Mean of dep. var.	7.08	6.81	5.26	6.91	6.74	4.44	7.14	6.91
Municipalities	1,134	1,134	1,134	21	21	21	165	142
Observations	13,419	13,419	13,167	159	159	136	1,821	1,548
Within R ²	0.16	0.45	0.02	0.19	0.47	0.18	0.10	0.22
								0.06

Notes: The table reports difference-in-differences regression estimates of town meetings. The unit of observation are municipalities in the German state of Schleswig-Holstein. The dependent variable are logged expenditures per capita (total, current, capital) since 2003. Different bandwidths around the population threshold of 70 apply. Significance levels (standard errors clustered at the municipality level): *** 0.01, ** 0.05, * 0.1.

Table A.8: Election results

1980–2017									
	Voter turnout	Invalid votes	Far-right populists	Voter turnout	Invalid votes	Far-right populists	Voter turnout	Invalid votes	Far-right populists
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Town meeting	0.86 (1.34)	0.54 (0.47)	-0.29 (0.48)	-0.65 (2.09)	0.90 (0.86)	0.43 (0.43)	-0.04 (1.90)	0.04 (0.46)	0.22 (0.61)
Mun. fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bandwidth	–	–	–	10	10	10	222.78	209.56	235.85
Mean of dep. var.	75.91	1.27	1.85	77.11	1.47	1.89	73.93	1.67	2.61
Municipalities	1,142	1,142	1,142	24	24	24	268	249	287
Observations	12,349	12,349	12,349	81	81	81	1,453	1,366	1,532
Within R ²	0.78	0.22	0.78	0.16	0.20	0.38	0.34	0.16	0.59

Notes: The table reports difference-in-differences regression estimates of town meetings. The unit of observation are municipalities in the German state of Schleswig-Holstein. The dependent variable are national election outcomes (voter turnout, invalid vote share, far-right populist vote share) since 1980. Different bandwidths around the population threshold of 70 apply. Significance levels (standard errors clustered at the municipality level): *** 0.01, ** 0.05, * 0.1.

Table A.9: Effects on house prices and migration

	Real estate price (log)		Immigration rate		Outmigration rate	
	(1)	(2)	(3)	(4)	(5)	(6)
Town meeting	0.00 (0.22)	0.02 (0.24)	0.79 (0.69)	0.43 (0.95)	-4.30* (2.34)	-4.23* (2.43)
Mun. fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var.	12.12	12.13	8.90	8.94	9.13	9.15
Bandwidth	70	35	70	35	70	35
Municipalities	64	35	77	47	76	46
Observations	305	166	661	391	673	399
Within R ²	0.10	0.16	0.02	0.03	0.02	0.02

Notes: The table reports difference-in-differences regression estimates of town meetings. The unit of observation are municipalities in the German state of Schleswig-Holstein. The dependent variable in columns (1) and (2) are municipality average real estate price offers in logged 1,000 Euros. We observe only municipalities with at least three offers a year. We use immigration and outmigration in % of total population in columns (3) to (6). In columns (1), (3), and (5), we use municipalities with a population of ± 70 around the town meeting threshold. In column (2), (4), and (6) we use ± 35 . Significance levels (standard errors clustered at the municipality level): *** 0.01, ** 0.05, * 0.1.

Table A.10: Session protocols

	Partici- pants	Share of 'family clans'	Share of females	Taxes on agenda	Duration of session
	(1)	(2)	(3)	(4)	(5)
Town meeting	10.21** (2.72)	0.09 (0.05)	0.02 (0.08)	-0.04 (0.09)	4.63 (10.75)
Mun. fixed effects	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Mean of dep. var.	8.72	0.08	0.24	0.18	104.16
Municipalities	5	5	5	5	5
Observations	229	229	229	229	222
Within R ²	0.46	0.18	0.27	0.09	0.09

Notes: The table reports the results of fixed-effects regressions using 229 protocols from five municipalities with different forms of government (two switched to town meetings for one election term during this period, two municipalities had always councils, one municipality had always town meetings). Column (1) uses the number of participants as dependent variable. Column (2) uses the share of participants with a name that appears at least three times in the participation list. In column (3), the dependent variable is the share of female participants. Column (4) refers to a dummy variable taking on the value of 1 if the agenda of the session includes any debate about property or business tax rates. Column (5) uses the duration of the session (in minutes) as dependent variable. Significance levels (robust standard errors): *** 0.01, ** 0.05, * 0.1.

Table A.11: Size of the legislature

	Mean				
	Popu- lation	Effective legislation	Prop. tax A (agr.)	Prop. tax B (gen.)	Busi- ness tax
	(1)	(2)	(3)	(4)	(5)
<i>Small legislature</i>					
Town meeting	32.15	10.93	257.98	261.02	317.34
Council	899.39	10.32	272.26	276.33	316.81
<i>in % of Council</i>	<i>4%</i>	<i>106%</i>	<i>95%</i>	<i>94%</i>	<i>100%</i>
<i>Large legislature</i>					
Town meeting	61.20	20.81	242.53	245.22	314.07
Council	8,611.77	20.21	299.91	312.19	332.60
<i>in % of Council</i>	<i>1%</i>	<i>103%</i>	<i>81%</i>	<i>79%</i>	<i>94%</i>

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.