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TAXATION AND TURNOUT IN SWE- DISH MUNICIPALITIES

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ABSTRACT

The link between taxation and representation is considered foundational to the emergence of democratic governance. Nevertheless, the empirical relationship between taxation and the extent to which citizens actually exert representation by turning out to vote remains virtually unexplored. Using a long panel, from 1979 to 2018, drawn from a relatively difficult case, Swedish municipalities, I find that hikes in local tax rates are linked to increased municipal voter turnout. Accounting for a wide range of confounders, including turnout in concurrent parliamentary elections, these results indicate an important untapped explanation for differences in turnout, while offering a rare explicit test of the taxation-representation argument drawn from a mature democracy.

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In the summer of 2009, the municipal assembly in Boxholm—a small Swedish town facing a looming deficit and the broader consequences of the Great Recession—passed an increase of 0.95 percentage points to its municipal tax rate for the coming year, following a raise of 0.35 points the year before. Although the hike was subsequently reduced to 0.84 points (leaving the new, flat, local tax rate at 32.19 % of earned income), it was enough to place the municipality among the country’s three top hikers for 2010, an election year. Come September, municipal voter turnout, which had been dwindling during the last few cycles, rebounded by over three percentage points. In this study, I describe why, and empirically demonstrate that, episodes like this one are not only interlinked but indicative of a general relationship between taxation and turnout, thus far chronically underexplored but of high relevance for several areas of political analysis.

Indeed, the link between taxation and representation has been a central tenant of political discourse for well over two centuries, and scholars have empirically investigated the relationship from several angles.¹ Still, likely a consequence of its historical origins, the taxation-representation argument has only received limited attention in works on modern-day developed states and consolidated democracies (cf. Martin and Gabay (2018)). At the other end, the literature predicting turnout—which derives plenty of evidence from developed and mature democracies—is all but void of taxation.

In this study I contribute to both of these literatures; most immediately by conducting a straightforward investigation into whether taxation drives turnout in the first place. By extension, I explore the scope conditions of the general taxation-representation argument by drawing on data from a mature democracy: Modern-era (1979-2018) Swedish municipalities. This empirical context provides for both a stark contrast to the bulk of related existing studies, as well as a difficult case in itself. Arguably most important for the latter point is the fact that Sweden employs same-day voting for local (municipal- and county-level) and national (parliamentary) elections—with the latter consistently dominating attention during campaigns (Erlingsson and Oscarsson 2015)—thus presenting a challenge for any local factor to affect turnout rates.

¹ Including a substantial literature exploring the impact of democracy, turnout, and political participation on taxation (Martinez 1997; Cheibub 1998; Gould and Baker 2002; Esteller-Moré 2005; Timmons 2010; Aggeborn 2016).

The concurrency of elections furthermore facilitates isolating the focal relationship, along with the fact that, of the three conventional sets of factors explaining differences in turnout (Blais and Dobrzynska 1998)—institutional, socioeconomic, and party-centered—the unitary state of Sweden displays virtually no variation in the former aspect and limited variation in the latter two.

The result of the empirical analysis reveals a positive link between local tax rates and turnout in Swedish municipal elections, indicating that the taxation-representation link continues to exist deep into the realm of modern-day democracies. This relationship holds both under control for a wide range of potential rivaling factors and after considering participation at the concurrently held parliamentary and county-level elections. The municipal level of analysis prohibits from a full inquiry into the causal mechanisms suggested to underpin this link, herein summarized in terms of three distinct channels respectively based on a stakes-, reminder-, and conflict-effect. However, supplementary tests are indicative of taxes at least in part affecting voters' stakes to participate in local politics, as well as spurring political conflict during election season.

Further analysis into the influence of the surrounding national political landscape does divulge potential limits to the tax-turnout link, as the empirical relationship is absent in nationally demobilizing elections, as well as in municipalities where local taxes make up a comparatively smaller share of total direct taxes paid by individuals. This suggests that, while taxation does carry the potential to make people exert accountability locally, the surrounding national political landscape sets the stage for such a process.

Taxation, representation, and turnout

Evidence from at least three distinct literatures gives cause to expect covariation between taxation and voter turnout. First, in what is commonly referred to as the fiscal sociology-account of state development, taxation is attributed with spurring a wide range of transformational processes of importance to democracy throughout (particularly European) history (North and Weingast 1989; Kiser and Barzel 1991; Schumpeter 1991), like the founding of parliaments (Van Zanden, Buringh, and Bosker 2012), as well as playing a key role in both the American and French Revolutions. This “classic” iteration of the taxation-representation narrative is relatively narrow and primarily focuses on autocratic executives sharing power with domestic elites in order to facilitate and legitimize increased

tax-raising, most commonly in order to fund intra-state wars (Bates and Lien 1985; Levi 1989; Tilly 1990). Inherently historical, this account rests on a number of idiosyncratic factors; Herb (2003) even argues that the taxation-representation link was a uniquely early-modern European phenomenon, based on political representatives' role in collecting taxes, and by extension unlikely to be reproduced elsewhere in time and space.

Nevertheless, the general argument has subsequently left the confines of historical analysis and been expanded to apply to democracy on a more general level in modern-day developing states (Ross 2004; Brautigam, Fjeldstad, and Moore 2008; Eubank 2012; Prichard 2015; Rakner 2017; Moore et al. 2018; Kato and Tanaka 2019). For example, Ross (2004) finds partial support for the notion that increased taxation is associated with more democracy on the macro-level, while both observational (Broms 2015) and experimental (Paler 2013; Weigel 2017) evidence on the micro level shows that taxpaying spurs monitoring, political interest, and civic activity in developing contexts.

Third, the literature on the political resource curse (Ross 2001; Aslaksen 2010; Haber and Menaldo 2011; McGuirk 2013) incorporates absence of taxation to explain an observed lack of accountability and representation in polities rich in natural resources. A simplified version of this notion is that rulers of such "rentier" states can afford to avoid taxing their citizens, thereby eschewing the need to offer transparency, accountability, and representation in exchange. Although this mechanism is more or less consistently cited as a key reason for low levels of democracy in such states, taxation is routinely left implicit in empirical analysis of this type.²

Taken together, these interrelated fields provide plenty of fodder for expecting a tax-effect on democracy, democratization, and civic activity in both historic and current contexts where the state structure and a social—or "fiscal" (Timmons 2005)—contract either has not yet calcified or remains absent altogether.

In stark contrast, the body of work focusing on explaining differences in voter turnout is strikingly void of taxation. For instance, in a number of recent and/or widely cited reviews (Blais 2006; Jackman 1987; Cancela and Geys 2016; Wass and Blais 2017) taxation remains wholly absent. This gap is rather

² Although anecdotal, the lopsided nature of scholarly focus devoted to oil in favor of taxation as a predictor of democracy is neatly illustrated through the citation counts of Michael Ross's near-concurrently published articles on the respective topics: While his (2004) article in the *British Journal of Political Science* exploring the relationship between taxation and democracy has of April 2019 been cited 103 times, according to the Web of Science, the corresponding number for his (2001) piece focused on oil and democracy (published in *World Politics*) is 949.

paradoxical, not only given the evidence cited above regarding the potential of taxation to activate citizens politically, but also considering the central place voting holds in most conventional notions of democracy; as Manin, Przeworski, and Stokes (1999, 29) note, widespread electoral participation is a condition for government "to act in the best interest of the people," while Aldrich (1993, 246) goes on to claim that "[t]urning out to vote is the most common and important act of political participation in any democracy." Accordingly, turnout is a crucial case of de facto representation in a democracy.

Partial exceptions to this lacuna are contingent and focus on an Anglo-saxon context. Percival et al. (2007), show that turnout in off-year U.S gubernatorial elections in the 1990s were higher in states with higher tax burdens, Merrifield (1993) finds tax progressivity to be positively associated with state-level turnout in the 1982 U.S. midterm elections, and, presenting contrasting evidence, McCormick (1996) fails to find a turnout-effect stemming from the introduction of a poll tax in Scotland prior to the 1990 elections. Instead, the most common mention of taxation in works on turnout refers to the important, but far more specific, issue of poll taxes designed to *inhibit* turnout (Aldrich 1993; Jackman 1987; Highton 2004; Harder and Krosnick 2008).

The Argument: Bringing in the tax—getting out the vote

There are at least three fundamentally distinct ways through which taxation could affect the propensity for individuals to vote. First, higher taxes increase citizens' incentives to monitor how their common resources are spent. If a larger share of peoples' wealth goes to the public coffer, it stands to reason that they will take a more active role in its management and/or work to reclaim their investment in the shape of public goods, increasing the potential material benefits from turning out to vote (Horiuchi 2005; Andersen, Fiva, and Natvik 2014). Simply put, stakeholders are more likely to participate in a process when the perceived stakes of its outcome are higher (Downs 1957; Lefevre and Van Aelst 2014). As a concrete iteration of this notion, Weigel (2017) notes that an increase in tax revenues may serve as a signal to citizens that there are more public goods to go around, increasing their incentives to take action in order to receive greater benefits. Similarly, Revelli (2016) finds that municipal turnout in Italy is positively affected by municipalities' power to raise taxes, while a flipped version of this argument has been extended directly to goods provision by Hajnal and Lewis (2003),

who find that outsourcing is associated with a decrease in turnout, linking this observation to a reduction in voter influence over service delivery.

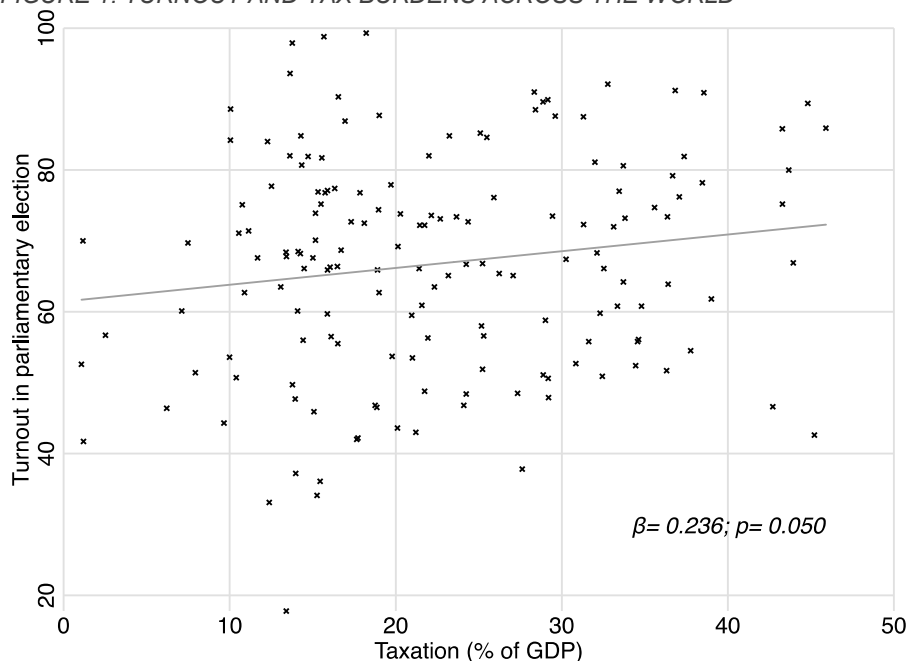
Second, it is reasonable to assume that the very act of taxpaying can act as an effective *reminder* of the salience of government itself. In contrast to the stake-based channel, which similarly revolves around salience but assumes a highly informed and rational set of actors, the impact of such a reminder-based channel can be expected to vary between contexts to a relatively high degree. In particular—and in adherence to recent calls for making explicit differences in turnout between national and local elections (Cancela and Geys 2016)—the potential pool of voters is likely disproportionately reminded of local government following tax changes at this level, considering its status as second-order level of government, to which citizens often give comparatively limited attention. As a point of comparison, Lefevere and Van Aelst (2014) show how perhaps the most explicit "reminder" to vote—political campaigning—follows this dynamic, with a larger effect in second- than in first-order elections.

Finally, and as discussed above, taxation is notorious for spurring political conflict; Alt (1983, 208) distinctly sums up this narrative, noting that “the history of taxation is to some extent the history of riots, uprisings, and revolution.” Since elections are the principal instruments for citizens to show discontent in a representative democracy, it makes sense that tax protests be reflected through increased electoral participation. Notably, and in contrast to the turnout-literature, works focusing on retrospective voting occasionally feature taxation as a variable of interest, generally under the assumption that the electorate punishes tax increases (Kone and Winters 1993; Niemi, Stanley, and Vogel 1995; Johnson, Lynch, and Walker 2005). While a different research question than the one at hand, these works do provide informative evidence in favor of a conflict channel: For example, while Madestam et al. (2013) show that increased tea party activity—a movement that fundamentally revolved around taxation—bolstered Republican turnout in the U.S. 2010 midterm elections, it did not decrease Democratic turnout. Indeed, increased political engagement among opponents to a tax hike is in turn likely to activate its supporters who, fearing a looming policy reversal, mobilize and thereby create a self-reinforcing circle of increased participation. As Niemi, Stanley, and Vogel (1995) note, taxation is distinct from many macroeconomic issues like unemployment or inflation in that it is not exclusively despised (i.e., a pure valence issue), but a policy issue with both supporters and opponents among the electorate (i.e., a position issue). Mörk and Nordin (2016) confirm this intuitive notion in a Swedish municipal setting, showing that voters in favor of a smaller public sector punish incumbents for raising taxes while voters who prefer a larger public sector reward such action.

Empirical strategy and case selection

The arguments laid out in the previous section collectively underpin the hypothesis that taxation increases turnout. Does available data actually support such a proposition? In figure 1, which provides a view of the widest possible angle using recent cross-sectional data of 160 countries, this most general and simple test does lend some credence to the notion of a positive tax-turnout link, with a percentage-point increase in taxation as share of GDP associated with a quarter-percentage-point increase in turnout.

FIGURE 1. TURNOUT AND TAX BURDENS ACROSS THE WORLD



Note. $N=160$. Tax includes social contributions. (ICTD/UNU-WIDER (2016), data collected at one point between 2012 and 2015). Voter turnout in most recent parliamentary election (2012-2018) (IDEA 2018). Data downloaded from Teorell et al. (2019).

Undoubtedly, although encouraging for the hypothesis at hand, this prima facie check leaves plenty to desire in terms of identifying a true relationship; neither does it account for a myriad of known confounders, nor is it informative of the direction of causality, nor does it capture temporal variation. In order to remedy these concerns, my analysis will employ sub-national data from a single country,

Sweden, between 1979 and 2018, essentially the entire era of modern municipal organization in the country. This approach will keep the most plausible confounders either constant (e.g., institutional factors like voting laws and other facets to the electoral system) or at a minimal level of variation (e.g., political or civic culture). Furthermore, the considerable time span—containing 39 years and 12 elections—allows for focusing on changes within units—in our case, the country’s 290 municipalities—over time. While these two approaches will reasonably alleviate the most pressing set of concerns to identifying the true focal relationship, I further take advantage of the fact that Sweden employs same-day voting for municipal, county, and national elections, and estimate the degree of roll-off in turnout between the different levels, similar to the approach of Andersen, Fiva, and Natvik (2014) and Horiuchi (2005). Taken together, any remaining errors, biases, and discrepancies are likely to be minuscule, or—when estimating roll-off—be more prone to *underestimate* the true magnitude of the tax-turnout relationship.

The focus on Sweden is motivated in part by contrasting the historical and developmental settings used in related works on taxation and democracy more generally. Sweden also stands out as an a priori difficult case for the hypothesis at hand. Considering the aforementioned narrative of how the construction of a fiscal contract involves bargaining and the development of democratic institutions, we can safely claim that the ink on this contract is relatively dry in Sweden’s century-old democratic system of government. In part a consequence of this general acceptance of the state and its right to collect taxes—the basis of which was laid several centuries ago (D’Arcy and Nistotskaya 2017), with a comparatively high level of tax morale in tow (Alm and Torgler 2006)—even direct taxes are relatively invisible to most citizens through a highly automated system of withholding. For individuals in employment, the local income tax—the one direct tax paid by essentially all Swedish income earners—is administered as a payroll tax, and the Swedish Tax Agency has streamlined the tax declaration process down to the point where taxpayers may confirm their pre-made year-end calculations by a single text message. In addition, local taxes in Sweden sort under the type of tax that Martin and Gabay (2018, 18) find to spur comparatively little protest, with diffuse costs devoted to funding general welfare services.³ Furthermore, factors present in a local Swedish context that ought to suppress results are not limited to the independent variable: High baseline rates of turnout in elections to

³ Martin and Gabay (2018) also remark that a diversified tax portfolio reduces the probability that any one tax will be subject to protest. While Swedish local taxes are high (15.5 % of GDP; [Statistics Sweden (2014, 17)]), both in real and comparative terms, another broad-based and regressive tax, the VAT, composes 9 % of GDP, and is thus at least roughly comparable to the burden stemming from local taxes, while point taxes on gasoline, tobacco, and alcohol are all substantial and arguably as or more visible than the local income tax.

Swedish municipal assemblies (ranging between 78 and 91 percent on average per election in the period under study) diminishes the room for any individual factor to positively impact turnout, and considering the fact that the Swedish tax burden for the period under study was often the world's highest, increases should plausibly have limited marginal impact on political behavior.

Relatedly, the likely most important reason for the high municipal turnout rates is that these elections are held concordantly with much more publicized national parliamentary elections, a level that holds little or no direct influence over local tax rates⁴ while decreasing the salience of any local issues come election day. In fact, Percival et al. (2007) use concurrence of first-order elections as a reason not to expect a tax-turnout link at the state level in U.S. elections at all. As noted in the introduction, I rather follow Andersen, Fiva, and Natvik (2014)'s (2014) approach and leverage inter-level turnout differences for identification purposes.

Swedish local government, taxation, and democracy

Although Swedish municipal organization stems from the mid-19th century, its modern iteration stems from a massive wave of agglomeration in the mid-1970s. Today, there are 290 municipalities that range from metropolitan (Stockholm, with 962,000 inhabitants) to very small rural communities (Bjurholm, with 2,500 inhabitants), each belonging to one of 21 counties, the higher local level of government. As Wollmann (2004, 647) notes, for a unitary country Sweden is "conspicuously decentralized." Indeed, municipalities wield a considerable level of legal and political autonomy and are bestowed with a wide-ranging set of tasks, including primary- and secondary education, child- and elderly care, and sanitation. The principal service organized at the county-level is healthcare.

Mirroring this extensive local provision of public goods and the fact that these are largely funded by a local income tax, Sweden stands out for its high level of local taxation, second only to Japan within the OECD (Statistics Sweden 2014, 75). Accordingly, local income tax accounts for a major part of total Swedish tax receipts, and insofar, a considerable share of the economy; in 2014, local taxation amounted to 15.5 % of GDP (Statistics Sweden 2014, 17). Due to its strict proportionality (above a given, nationally determined, threshold that for 2019 was set at 19,670 Swedish "kronor" [English,

⁴ For the period under study herein, the exception to this is a central-government mandated tax freeze in 1991-1993 and subsequent economic incentives to reduce tax increases in 1994 and 1997-1998; see figure A.1. in the supplemental material.

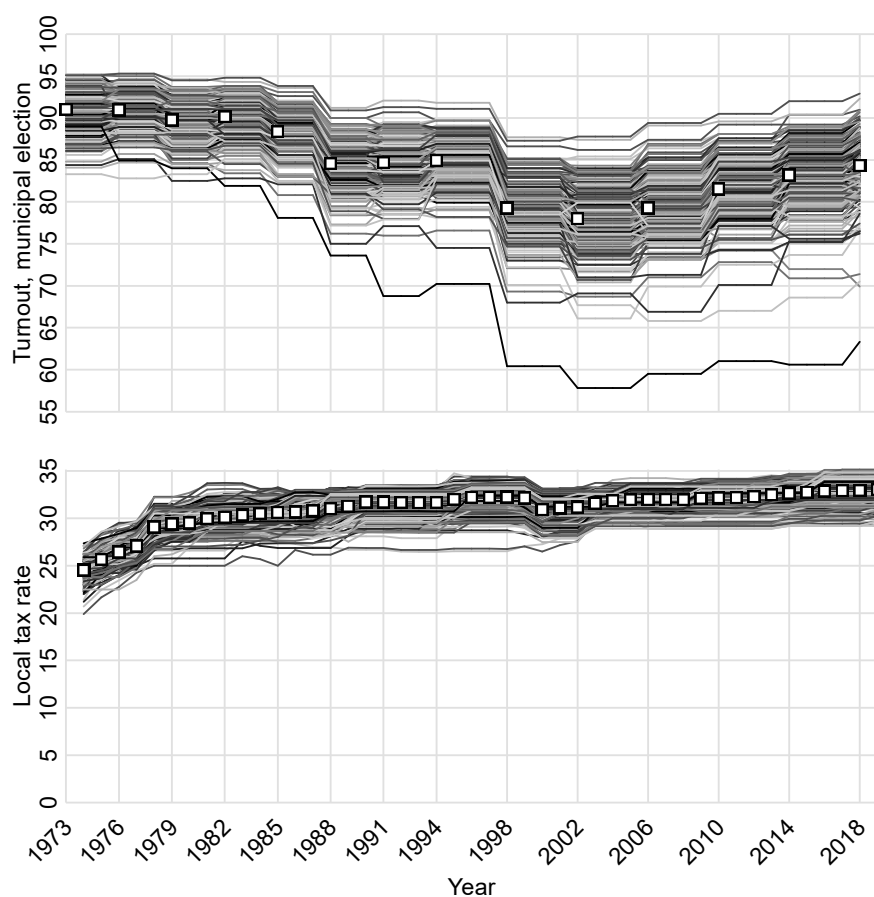
Crowns; ≈\$2,000] for the full year), the common unit of measuring and discussing local taxes—both in common parlance and research (Mörk and Nordin (2016); see also Andersen, Fiva, and Natvik (2014) for a similar approach in neighboring Norway)—comes in terms of a flat rate or “tax-krona” out of every hundred kronor earned, divided between the municipal- and county levels. As visible in figure 2, local tax rates have continuously grown since the mid-1970s; rapidly during the initial stage, and since the late 1970s in a more measured fashion.

The Swedish municipal electoral system is highly reminiscent of its national counterpart, with a popularly elected unicameral assembly, elected through proportional representation and a low electoral threshold (before 2018 natural; from 2018 2 % or 3 %, depending on the size of municipality) that appoints an executive board, a rough equivalent to a municipal government. Although local parties are relatively common, municipal political landscapes tend to be dominated by a set of national parties with representation in parliament, a group that has increased from five to eight since the 1970s.

Elections for all three tiers of government (parliament, county assembly, and municipal assembly) take place in September every four years (until 1994, every three years). Since 1974 eligibility for local (i.e., county and municipal) elections differs marginally from eligibility for parliamentary elections, as non-citizens over the age of eighteen who have been registered in Sweden during more than three years (and, following Sweden’s accession to the European Union in 1995, all EU-citizens) are allowed to vote in local but not national elections, while Swedish citizens living abroad are eligible to vote for national but not local elections. As figure 2 reveals, conversely to local tax rates, municipal turnout (measured as the share of the eligible population) has decreased during the observed period, from its mid-1970s pinnacle of above 90 % on average to a nadir of below 80 % in 2002, with a steady rebound during the early 21st century. Despite a fair amount of inter-municipal variation, both short- and long-term trends in taxation and turnout alike have been quite cohesive in the country during this time.⁵

⁵ Election year explains 71 % of the observed variation in municipal turnout level; year explains 66 % of local tax level.

FIGURE 2. TURNOUT AND TAX RATES IN SWEDISH MUNICIPALITIES, 1973-2018

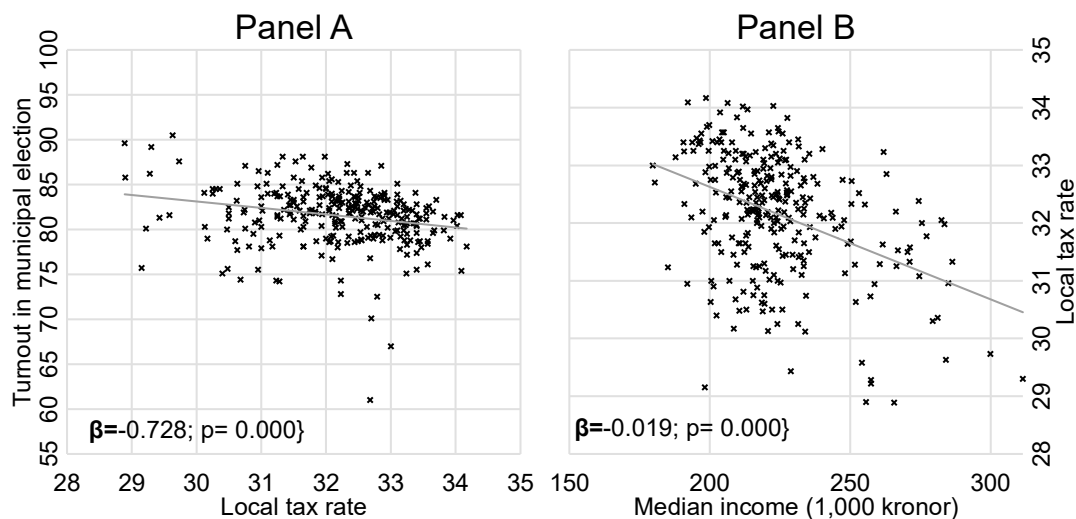


Note. Data from Statistics Sweden (2019). Squares represent unweighted national means.

These countervailing trends in taxes and turnout, which provide a tentatively negative outlook for the tax-turnout hypothesis, are not limited to the temporal level. As evident in panel A in figure 3, which plots levels of municipal turnout in relation to local tax rates in 2010, the relationship is, again, decidedly negative. This result, however, by and large reflects the even more robust observation, displayed in panel B, that local tax rates are negatively related to income levels (Swedish Tax Agency 2015, 19). By extension, these trends thereby serve both as a clear indicator that the realm of Swedish municipalities contains meaningful and systematic differences in both rates of taxation and turnout,

and that statistical analysis into this relationship needs to be carefully designed in order to avoid attributing such underlying socioeconomic variation well-known to influence turnout, to tax rates.

FIGURE 3, TURNOUT, TAX RATES, AND WEALTH IN SWEDISH MUNICIPALITIES, 2010



Note. Data from Statistics Sweden (2019).

Data, modeling, and estimation strategy

As noted, due to the plethora of important confounders naturally held constant, drawing on data from a single country considerably facilitates isolating the tax-turnout relationship. Accurately modeling a test of the link is, however, still likely sensitive to non-trivial specification choices, applying specifically to the independent variable and generally to the statistical estimation and modeling strategy. This section deals with these matters in turn. I also describe how turnout in concurrent but formally independent elections is leveraged to model turnout roll-off, in order to further press the link on account of causal identification.

Independent variable

Thus far, this study has consistently referred to total local taxes, flowing to the municipal-, county-, and (until 1999) parish-level. This is not the most obvious level at which to measure taxation from

an accountability perspective, considering that the outcome of interest is turnout at the municipal level, and focus should thus, a priori, rather lie on taxation at this specific level. Nevertheless, a widespread connotation within Swedish public discourse that *all* local tax goes to the municipal level, or is at least intrinsically tied to it, considerably complicates this notion. As Martin and Gabay (2018) point out, traceability is a potentially important condition for taxes to spur political activity, and Swedish municipal taxes are in common parlance consistently bundled together with taxes to the county level. This conflation begins already with the Swedish term for local taxes, “kommunalskatt,” *municipal tax*, a remnant of the fact that the formal name for the county level until 1992 was “landstingskommun,” i.e. *county-municipality*. When it is parsed out, municipal tax is usually denoted with the very similar “skatt till kommun,” *tax to municipality*. This practice of bundling together local taxes can also be found in the official document taxpayers use to find out how much they are actually set to pay, their annual declaration form (see figure A.2. in the supplemental material for a real-life example of how this document presents Swedish taxpayers with their municipal- and county-level tax rates). Also here, municipal- and county taxes are presented as one, in the form of a total local tax rate (the county-level—but not municipal-level—tax is then separately presented).

In sum, restricting focus to municipal taxes is bound to skew actual tax rate fluctuations as experienced by voter-taxpayers, and, by extension, their true political consequences. As evident in a comparison of the trajectory of different tax rates in Boxholm (section A.3. in the supplemental material), the way in which taxes are calculated will furthermore result in potentially starkly different figures. Going forward, the empirical analysis below will employ a pragmatic strategy, primarily focusing on the totality of local taxes, while separately reporting results for the “correct,” municipal, level from an accountability standpoint in complementary robustness tests, available in the supplemental material.

Statistical estimation and modeling

The second main specification concern regards how to properly estimate the focal relationship statistically. Many comparative studies analyzing turnout rates employ pooled OLS regression (Cancela and Geys 2016). This is a feasible approach when the objective is to compare static system-level determinants between cases, such as electoral rules or voting laws. However, it also invites considerable and—for a study focusing on a time-variant determinant like tax rates—unnecessary risk of

omitted variable bias. Likely the most common remedy for this problem is by employing a unit fixed-effects (FE) approach. The dependent variable herein, municipal turnout rate, nevertheless displays strong persistence (the correlation with its one-term lag is $r=+0.88$ for the sampled period), thus indicating considerable serial correlation. In the following, I thereby focus directly on between-election-changes through a first-difference (FD) framework (whose one-term lag is a more moderate $r=+.18$), while reporting common alternative specifications in the supplemental material.⁶

In effect,

$$\Delta Y_{i,t} = \beta_0 + \beta_1 \Delta X_{i,t} + \beta_2 \Delta \lambda_{i,t} + \beta_3 \Delta \omega_{i,t} + \beta_5 \gamma_t + \Delta \epsilon_{i,t},$$

is the preferred regression equation used for the baseline model in the subsequent analyses of $\Delta Y_{i,t}$, change in municipal turnout for municipality $_i$ between elections $_{t-1}$ and $_t$. It is predicted by $\Delta X_{i,t}$, the between-election change in local tax rate, alongside $\Delta \lambda_{i,t}$, a set of socioeconomic controls, $\Delta \omega_{i,t}$, a set of electoral controls focusing on municipal political factors, as well as γ_t , election-fixed effects, to account for highly present nation-wide trends in turnout (see figure 2 above). As discussed in footnote 6, standard errors, $\Delta \epsilon_{i,t}$, are clustered at the municipal level.

As argued above, the single-country setting, which has seen only minor changes to its formal institutions during past decades, naturally controls for most primary rivaling predictors of turnout, as suggested by relevant literature. The ensuing models will nevertheless consider $\Delta \lambda_{i,t}$, a battery of controls capturing socioeconomic factors pertinent for turnout: Share Senior citizens (65 years and older), share Young adults (18-34 year-olds), share with Higher education, and In- and Outmigration from the municipality. This battery also considers demographic pressure, through share in Daycare & preschool (1-6 year-olds)- and Primary/secondary-school (7-18 year-olds)-age, as well as share

6 Explicitly comparing serial correlation in FD and FE estimations of the baseline regression model (as per equation 1 below) reveals that, although the FD model garners small and negative but significant serial correlation ($\hat{\rho}=-0.169$; $T=-8.11$), its estimate in an equivalent FE-model is—more problematically (Wooldridge 2013, 471)—larger and positive ($\hat{\rho}=0.503$; $T=17.9$). The remaining serial correlation in the preferred FD estimation is subsequently dealt with in several ways; in the main analysis by clustering standard errors on municipality, while analyses in the supplemental material report results from models including lagged level of the dependent variable.

Another common strategy for analyzing panels like the one herein is likely employing the GMM-framework, which also at heart employs first-differencing. However, models using such estimations fail to meet conventional measures of instrument validity, as captured by Hansen’s J-test, without being overburdened by a prohibitively large number of instruments (Roodman 2009); see table B.3.4-B.3.8. in the supplemental material.

Older seniors (80 years and older), three groups who are bound to use municipal goods, like child-care, schooling, and elderly care, and thereby affect municipal expenses.⁷

Furthermore, $\Delta\omega_{i,t}$ represents a number of municipality-specific political factors known to influence turnout. First, I include a variable measuring Bloc vote differential between the conventional left-right blocs, serving as a rough proxy for closeness of election and competitiveness. Second, I include Δ Eligible voters (measured in log-form) as the odds of a single vote being decisive diminish with a larger number of potential voters. Third, research (Geys 2006) has found that individuals may become both more and less prone to turn out and vote if they have a larger number of alternatives to vote for. In order to capture variation in the supply of parties between Swedish local elections, I include Δ Local party representation in the municipal assembly. Fourth, I consider whether the municipality arranges a Δ Concurrent referendum at the municipal level, alongside general elections, something that will plausibly further increase individuals' incentives to turn out (using data provided in correspondence with the Swedish Election Authority). Finally, I include Δ Eligible non-citizens, capturing the share of the municipal electorate that consists of foreign citizens eligible for local- but not parliamentary elections on the aforementioned grounds that limited voting eligibility reduces the benefit of turning out for this group.⁸

First-differenced data for turnout and tax rates are available between the 1979 and 2018 elections, while the introduction of socioeconomic controls eliminates the first three elections, leaving us with a sample with full data covering nine elections between 1988 and 2018.⁹

7 A reasonable reader is likely to find median income notably absent from this list. Its exclusion is motivated by a combination of concerns with colinearity and data availability. First, education and wealth are commonly considered to affect turnout for largely the same reasons (Wass and Blais 2017), involving their impact on individuals' ability and/or willingness to engage in politics. Municipal-level data for education is available for 1985-2018, while for median income 1991-2017. Predicting turnout change as a function of these two variables concurrently reveals that Δ Higher education outperforms Δ Median income, rendering the latter insignificant when controlling for the former (see table B.4.1. in the supplemental material). Therefore, to avoid losing observations from three election years (1988, 1991, and 2018), I exclude Δ Median income from the main model, but—especially considering the finding in figure 3 that income levels strongly predict local tax rates—report results including it in table B.2. and in fixed effects estimations (tables 3.2.-3.4.) in the supplemental material.

8 See section B.2. in the supplemental material for a longer discussion of the motivation for and specification of controls.

9 Among the control variables, only Share with higher education lacks data for 1979-1985. Additional checks using the full 1979-2018 period, without this factor, are run in table B.2. in the supplemental material.

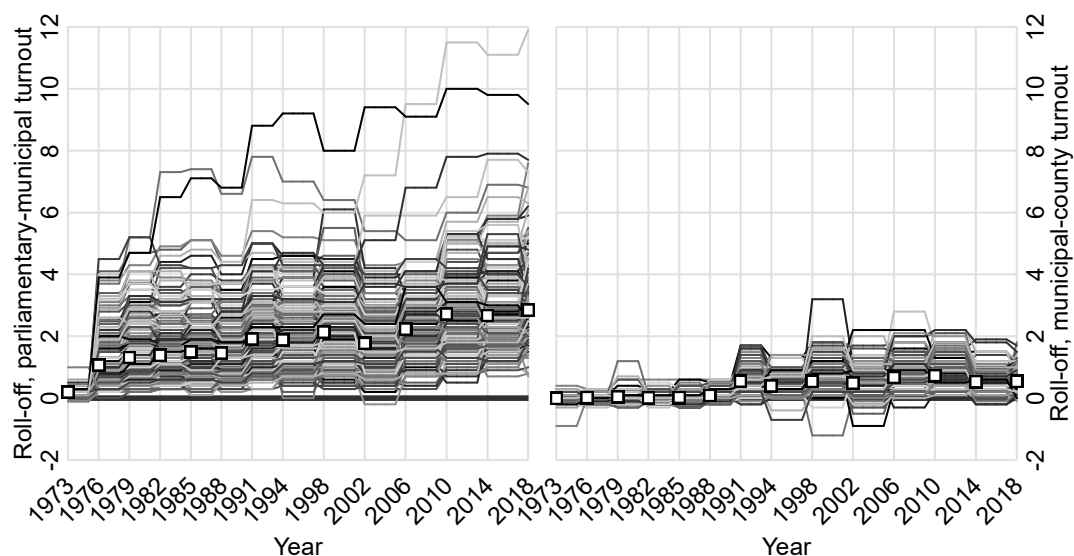
Roll-off

Finally, we take advantage of the fact that Swedish voters concurrently cast ballots in parliamentary, county-, and municipal elections, changing Y from capturing municipal voter turnout to a slightly different but closely related phenomenon of roll-off in turnout between concurrent elections of different orders (see also Andersen, Fiva, and Natvik (2014)). Specifically, I estimate both a first-to-second-order roll-off—difference between parliamentary- and municipal-level turnout—and a second-to-third-order roll-off—difference between municipal- and county-level turnout.¹⁰ The expectation is that local tax rate increases decrease the difference between parliamentary and municipal turnout but—grounded in the aforementioned heavy municipal connotation in discussion of Swedish local taxes—increase the difference between municipal and county turnout.

Figure 4 shows that turnout roll-off is moderate in Swedish municipalities but has consistently increased since the mid-1970s. The largest discrepancy is found between the parliamentary and the local levels, largely a result of the aforementioned 1974 reform allowing some foreign citizens to vote in local but not parliamentary elections, thus limiting the incentives for voting for this part of the electorate. However, this does not completely explain the observed roll-off variance, as demonstrated by the modest yet increasing disparity between turnout in municipal and county elections, for which eligibility is identical. During recent election cycles, this difference has on average been around a half percentage point, but frequently exceeds one point.

¹⁰ The notion of parliamentary elections being of the first order is supported by the observation that the municipal turnout rate is higher than for parliament only in one instance in our sample (Bjurholm 2002). Considering county elections as of the third order follow both from the smaller set of responsibilities (and a smaller share of local taxes) allotted to this tier, and the fact that municipal turnout is higher than county turnout in over two thirds of the observations in the sample, while the opposite is true only in 7.6 % of cases.

FIGURE 4, ROLL-OFF IN VOTER TURNOUT, 1973-2018



Note. Data from Statistics Sweden (2019). Y-axes display percentage-point differences. Squares represent unweighted national means.

Notably, this approach makes for a hard test that likely overcorrects and underestimates the true relationship between local taxation and municipal turnout, as it implies that local taxation has no effect on the propensity to vote in these other elections, an unrealistic assumption (especially for county-level turnout, since local taxes in part flow to this tier of government). An added benefit of this approach is that it precludes the need to include $\Delta\lambda_{i,t}$, the battery of socioeconomic controls (which accounts for factors that make people turn out to the polls in the first place), and therefore is estimable for the full 1979-2018 period for which we have data for local taxation and municipal turnout.

Findings

The empirical analysis begins with a general test of the tax-turnout hypothesis, is followed by an exploration into the specific mechanisms underpinning it, and concludes with an investigation into heterogeneity and scope conditions of the relationship.

Main results

In line with expectations, there is a positive general relationship between local tax hikes and increases in turnout for Swedish municipal elections. Both the analysis predicting Δ Municipal turnout (table, column 1) and Δ Roll-off, municipal-county turnout (column 3) garner significantly positive coefficients for Δ Local taxation, while the same is significantly negative in predicting Δ Roll-off, parliamentary-municipal turnout (column 2). Focusing on the preferred model of Δ Municipal turnout, we can observe a small but highly significant positive coefficient ($\beta=0.134$; $p<0.001$), indicating that a one-percentage point (“krona”) tax hike—an increase roughly equivalent to going from the first to third quartile in the full 1979-2018 sample (Q1: 0; Q3: 0.94)—is associated with a one-seventh of a percentage-point increase in turnout. Although this must be considered modest in size, it needs to be considered in relation to the previously mentioned highly potent national election-year effects on the development of municipal turnout (a test of joint significance of the election-year dummies garners a very large F-statistic of 678). Furthermore, the coefficient for Δ Local taxation is comparable to other commonly acknowledged drivers of turnout, like equivalent increase in the share of senior citizens, half of an equivalent increase in the share of the highly educated, or a ten-point tightening of the between-bloc differential.

TABLE 1, LOCAL TAXATION AND TURNOUT IN SWEDISH MUNICIPALITIES

	(1)	(2)	(3)
	Δ Turnout, municipal election	Δ Roll-off, parliamentary-municipal turnout	Δ Roll-off, municipal-county turnout
Δ Local tax rate	0.134 (0.038)	-0.024 (0.009)	0.034 (0.008)
Socioeconomic controls	Yes	No	No
Electoral controls	Yes	Yes	Yes
Observations	2585	3418	3392
Municipalities	290	290	289
Elections	9	12	12
Years	1988-2018	1979-2018	1979-2018

Note: β -coefficients reported. Standard errors, in parentheses, are clustered by municipality. Full results available in table B.4.2. in the supplemental material.

The principal conclusion drawn from these tests is that the central hypothesis stands; although municipal turnout fluctuations are, as noted above, largely influenced by national level trends, local taxation plays an important role at the margin. As such, these results suggest that taxation—although not necessarily the kind of revolutionary fodder it is sometimes referred to—does merit a place among the usual suspects of turnout-drivers.

A series of additional robustness checks are available in section B of the supplemental material. In brief, I make three types of adjustments to the preferred model, re-specifying the independent variable, accounting for different sets of covariates, and re-specifying the estimation strategy. Taken as a whole, the results of these analyses reaffirm the findings in table. They also provide further qualification: First, the observation that analyses of shorter and more recent samples display much larger coefficients for taxation—by up to four times larger than in table, despite the addition of a slew of additional controls, tentatively speaks for an increased importance of taxation during recent years. Second, comparatively weaker results using the FE estimator (B.3.2-B.3.4.) indicates that short-term changes in tax rates are more important than their levels offers an initial argument against the stakes based argument. Both these notions will be investigated further below.

Testing the mechanisms

Having found evidence in support of a positive tax-turnout link in Swedish municipalities, we proceed with an exploration into the more precise (stake-, reminder-, and conflict-based) mechanisms suggested above to underpin this relationship. The data at hand can only provide approximate evidence regarding either account. It is, for example, highly difficult to devise a clear-cut separation of the reminder- and conflict-based accounts, as the conflict-based account in part operates by raising political awareness and reminding citizens to vote. Along with a few theoretically informed assumptions, these tests nevertheless allow for weighing the evidence for or against each account, and furthermore serve as additional sanity checks of the general relationship.

Stakes

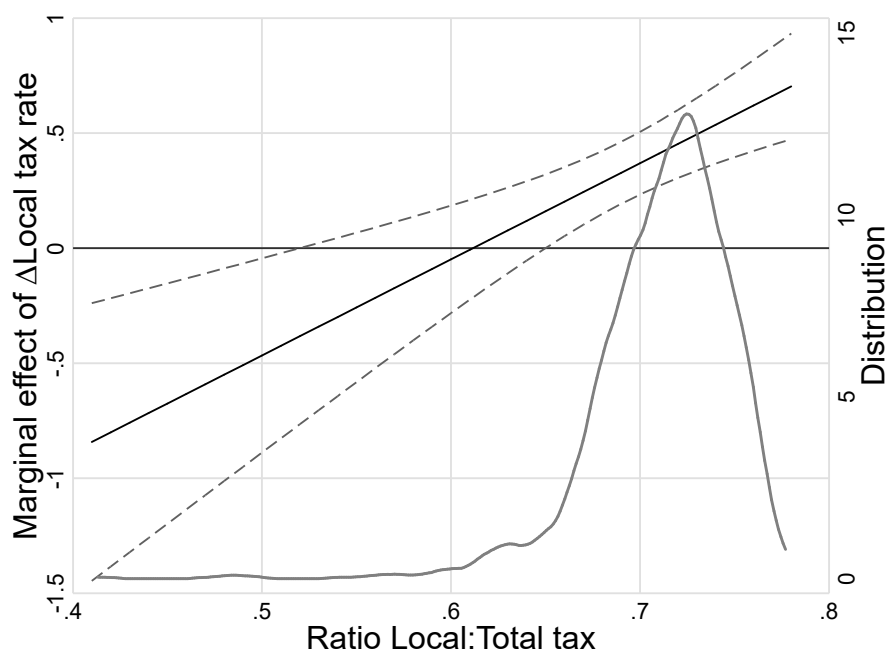
In order to further press the stake-based account, which to recapitulate assumes that higher taxes increase individuals' material incentives to monitor government by voting, we leverage variation in the weight of local taxes to total direct taxes paid by individuals by municipality. Richer municipalities

display lower ratios of local taxes to total direct taxes, since a higher share of their inhabitants earn enough to exceed the threshold for the central government income tax. If stakes are a viable mechanism through which local taxes bring people to the polls, it should follow that such taxes will matter more if they make up a larger share of peoples' total tax burden. Using data on the share of local taxes to total direct taxes paid by individuals in each municipality, available for four (2002-2014) election years, I therefore interact Δ Local tax rate with this variable.

Figure 5, displays the marginal effect of Δ Local tax rate, contingent on Ratio of local taxes to total direct taxes on individuals (using the baseline model in equation [1]). It provides support for the stakes-based notion, as a higher ratio of local to total tax strongly conditions the extent to which changes in local tax rates predict larger increases in turnout. In other words, where local taxes make up a larger part of inhabitants' actual tax payments, its potency to drive turnout is higher.

In light of the relatively weaker results for taxation in the FE-estimation—whose comparison with the FD-estimation above indirectly speak to the stakes-based argument—a picture emerges wherein voters display bounded rationality. On one hand, they are more prone to react to a tax type if it such a change is felt stronger in the pocketbook. On the other, such reactions are accentuated by visible triggers like rate changes, rather than long-term levels.

FIGURE 5, TEST OF STAKES



Note. p -value for interaction term: $p < 0.001$. Dashed lines display 95 % confidence intervals. Covariates held at mean or modal (Δ Concurrent referendum; Δ Representation of local parties) value, and year 2010. Data for 2002, 2006, 2010, and 2014 from Statistics Sweden (2019). Complementary analysis presented in the supplemental material (see figure B.4.1.) demonstrates that the apparent skewness in distribution of Ratio of local taxes to total direct taxes on individuals affects neither direction nor significance of the results presented here.

Reminder/conflict

The aggregated municipal level data used herein precludes from neatly disentangling the reminder- and conflict-based channels of transmission with certainty. Nevertheless, by estimating the tax-turn-out link contingent on the distance to the upcoming election a given rate change occurs, it is possible to conduct an approximate test, by which the two channels are at least partly separated. From the theoretical discussion above, we can make two common assumptions about the reminder- and conflict-channels: First, both may operate through cuts as well as hikes; second, both are likely more closely associated with rate changes in themselves than their magnitude.

In contrast, the two channels depart with the difference between politics and policy, and the fact that individuals become aware of rate changes in part as a political decision and in part through the policy shift itself. Thereby, in order to parse out conflict we make two further assumptions unique to this channel: First, political discussion around a new tax rate will, by design, involve some measure of conflict. Second, a considerable portion of such a debate will precede the implementation of the new rate. Thus, the conflict channel is intrinsically tied to policy shifts in the near-future. In Boxholm, such conflict was on display in 2010, when an eventual 2011 tax cut of 0.35 points was proposed by the ruling Social Democrats in the lead-up to the 2010 election, while the center-right alliance campaigned on a more substantial cut of 0.84 points (that is, neutralizing the larger hike of 2010; Boxholm – Socialdemokraterna vill sänka skatten (2010)). Along this line of reasoning, a conflict-based hypothesis posits that local tax rate changes in years directly following election years ($t+1$) will become salient fodder in those election campaigns and thereby have a positive impact on turnout in election year t . Note that, *ceteris paribus*, politicians will be prone to advertise envisioned tax cuts but speak quietly of hikes, leading to the additional expectation that future cuts will be a more likely turnout driver than future hikes.

The reminder-based channel also relies on two distinct assumptions: First, a change in tax rate is directly observable to taxpayers through their pocketbooks, regardless of whether its decision stimulated conflict. Second, voters are myopic and react stronger to policy shifts closer in time, a notion supported by the literature on retrospective voting (see, for example, Huber, Hill, and Lenz (2012; Healy and Lenz 2014)); insofar, the reminder-based channel can be captured through recency bias: If rate changes draw people to the polls by directly reminding them of the relevance of local government, this effect will reasonably be comparatively larger when the actual reminder occurs close to election day. In Sweden, information of a rate change on election-day in September of year t has recently been conveyed to voters through two distinct signals: In part, most taxpayers (including wage-earners and senior citizens) have directly observed a change for year t in their pockets by January of the same year when they received a bank statement of their salary or pension, for the first time subtracted by the new tax rate. In part, the most recent, and arguably most explicit, piece of information of a rate change available to voters on election day regards changes in year $t-1$, as declaration forms with the rate for this year are sent home to citizens the following spring (as well as the amount of money they actually paid in $t-1$; see figure A.2. in the supplemental material for a real-life example).

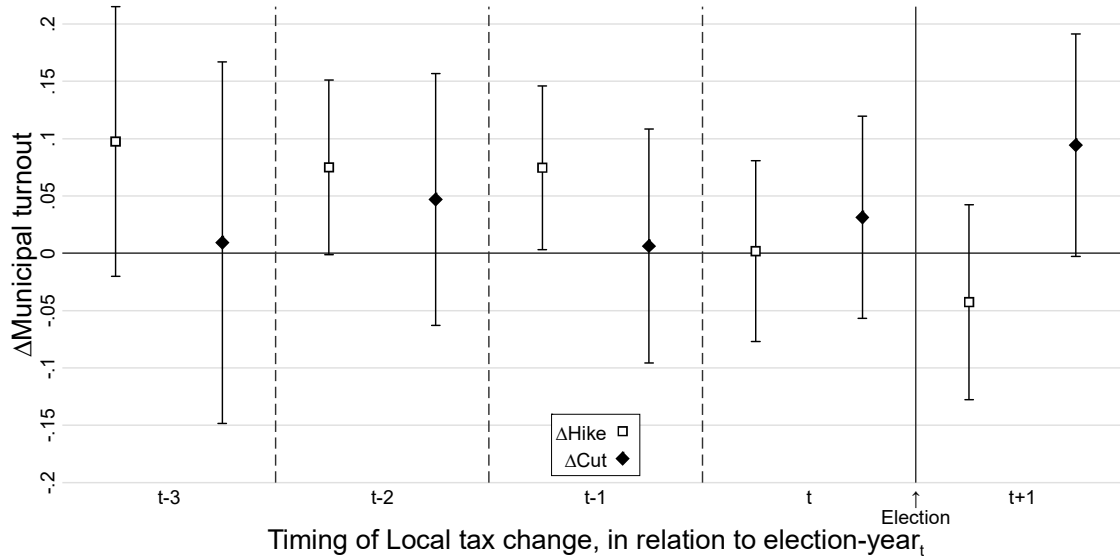
Thus, a reminder-based hypothesis would suggest that tax rate changes the two years directly preceding elections (t and $t-1$) will have a greater impact on turnout than prior years. Recalling the Boxholm episode, this implies that, of the two tax hikes that took place in the municipality prior to the 2010 elections, the latter should be more important for driving up turnout, not only because it was bigger, but because it occurred closer to the upcoming election.

Figure 6 again displays β -coefficients for local tax rates on Δ Municipal turnout, here recalculated as (differenced) dummy-variables of both hikes and cuts to the yearly local tax rate, disaggregated by distance to election t . In brief, the reminder-channel receives tepid support, as hikes, which prior to election years are at or near significance, are *least* strongly associated to municipal turnout on election years. This result furthermore complements the previous conclusion derived from the discrepancy between the FD- versus FE-estimations that citizens are more stimulated by short term tax changes than long-term levels, demonstrating that they are not short-sighted to a fault.

On the other hand, while coefficients for cuts in all pre-election years are near-zero, it is near significant ($p=0.057$) the year after election. This finding indicates that the conflict-channel carries at least some credence in explaining the main results.¹¹ It also outlines a potential for tax *cuts* to contribute to the domain of political efficacy, accountability, and representation, a hitherto little explored link in the greater taxation-democracy literature.

11 The coefficient for post-election hikes are insignificantly negative. Again, coefficients for post-election hikes are likely biased downward, due to the aforementioned disincentives for politicians to boast of such increases on the stump. A formal test of joint significance for the two post-election changes reinforces the salience of the conflict-channel ($F= 3.08$; $p=0.047$), while the corresponding variables during the “high-reminder-”period (year t & $t-1$) are farther from joint significance ($F=1.24$; $p=0.294$) than for the “low-reminder-”period (year $t-2$ & $t-3$ [after 1994]; $F=1.85$; $p=0.120$).

FIGURE 7, TAX HIKES AND CUTS, AND Δ TURNOUT, BY DISTANCE TO ELECTION



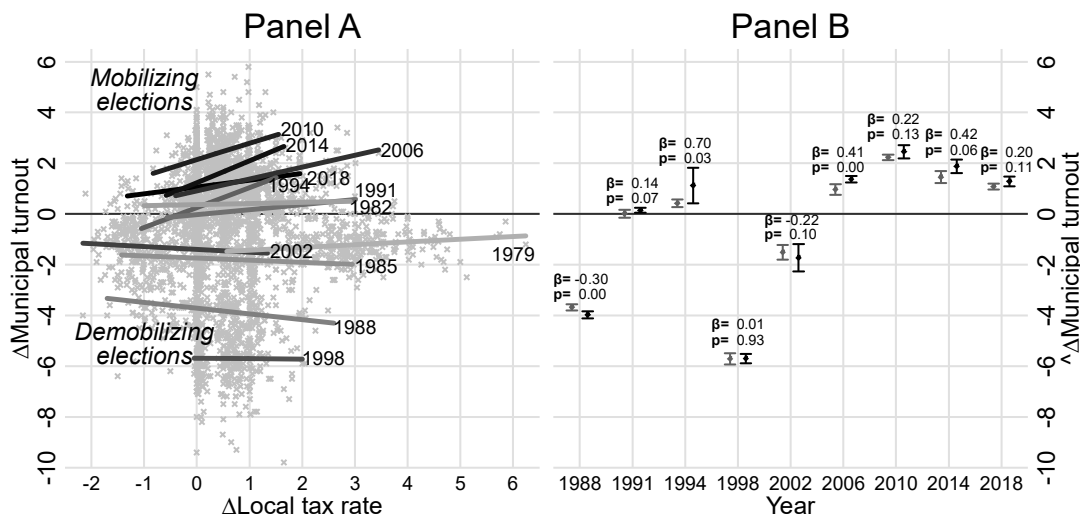
Note. Results from model with substantive controls (equivalent to table, column 3. Capped lines display 95 % confidence intervals.

Contextual heterogeneity

We conclude the empirical analysis with a deeper exploration into the limits of the general relationship found above. In addition to an observed substantial national-level influence on municipal turnout fluctuation, we can also observe a large amount of temporal heterogeneity in the strength—and direction—of the tax-turnout relationship when disaggregated by election. Panel A in figure 7 plots the bivariate relationship between Δ Municipal turnout and Δ Local taxation for each election in between 1979 and 2018. Although the tax-turnout link is significantly positive in seven elections (1979, 1991, 1994, 2006, 2010, 2014, and 2018), it is null in four (1982, 1985, 1998, and 2002), and significantly negative in one (1988). This heterogeneity reflects the findings from the robustness checks (see table B.2. in the supplemental material) that the tax-turnout link is stronger using more recent samples. Recency does not, however, seem to be the only relevant factor, as the heterogeneity is visibly related to nation-level turnout dynamics, with the general level of mobilization for the given elections strongly conditioning the tax-turnout link. In other words, in all elections where turnout increases nationally, local taxation seems to provide an additional boost at the municipal level; in elections where nation-level turnout decreases, local taxation is irrelevant at most. This is true for all

elections apart from the earliest observation, 1979, which displays a positive sign (following the largest increases by a magnitude in local taxation on record), despite turnout slightly down from record levels in 1976. Further supporting the notion of a national mobilization effect conditioning the municipal tax effect, panel B predicts Δ Municipal turnout by individual election, contingent on a one-point increase and status quo in Δ Local taxation,¹² including the full set of control variables for the 1988-2018 sample used in the baseline analysis in table. Indeed, predicted turnout is higher for all elections where average turnout increased (significant in three elections and near-significant in three) and negative (significant for 1988) for the three elections when turnout decreased.

FIGURE 8. Δ TAX & Δ TURNOUT, BY ELECTION



Note. Panel A: Gray 'x's denote each municipal-election observation for the full (1979-2018) sample. Panel B: Black lines indicate predicted change in turnout contingent on a 1 percentage point increase in local tax rate; gray line for no change in tax rate. Covariates held at mean or modal (Δ Concurrent referendum; Δ Representation of local parties) value. Capped lines display 95% confidence intervals.

Note that this national mobilization bias is unlikely to be a statistical artifact, since the room for any local factor to increase turnout shrinks in years of increased aggregate levels of turnout when the baseline level of turnout is consistently much closer to the theoretical maximum than the minimum.¹³

12 This is roughly equivalent to a 'Boxholm-sized' increase of 0.84 points, or going from the first (0) to the third (0.94) quartile in the sample.

13 This applies for increases, but not decreases, in tax rates; as evident in figures 2 and 7, local tax increases are much more common than decreases for the present sample.

If anything, the relative proximity to the ceiling of full turnout in Sweden should render municipal-level factors less effective in times of general mobilization. Instead, the most reasonable explanation for this variation is likely found by heeding Aldrich (1993)'s ((1993)) advice and keep in mind that, despite turnout being critical for democracy in the aggregate, it tends to be a marginal decision for a given individual; in a climate of political mobilization, a larger share of people at the margin of turning out are attentive enough to register a local tax increase, link this to the political sphere, and, as a result, show up come election day. The national mobilization factor thereby stands out as an important scope condition for the tax-turnout link in the present context, and likely for other factors in lower-order elections more generally as well.

Conclusion

Although the list of factors suggested to spur electoral turnout is already long, it remains essentially void of taxation. This is puzzling given that the factor has simultaneously held a place as one of the most prominent explanations for the rise of democracy in the first place. As such, the aim of this study has been to highlight a noticeable gap between two separate literatures—respectively focusing on understanding differences in turnout and linking taxation with outcomes relating to democracy—that should reasonably be closely intertwined, yet thus far have been kept conspicuously far apart.

Using the universe of Swedish municipalities to test such a tax-turnout link arguably presents a hard case for this hypothesis, considering the country's stable fiscal contract and same-day voting for local and national elections. Furthermore, this electoral system of concurrent elections and relative institutional homogeneity facilitates isolating the focal relationship. The empirical analysis demonstrates that hikes in the local tax rate are tied to small but noticeable increases in turnout at the municipal level. In the baseline model, accounting for a series of temporal, socioeconomic, and electoral confounders, a one-percentage point increase in local taxation can be tied to a one-seventh percentage point increase in turnout. Although the magnitude of this link is modest, it should be placed into context of a relative invisibility of direct taxes in Sweden, as well as a limited scope for turnout rates to grow in a country displaying turnout rates exceeding 80 %, sometimes even 90 %, and—likely most importantly—the primacy of parliamentary elections taking place concurrent with municipal elections.

The title to this study's title implies that a recent and modest tax increase in a small Swedish municipality could at heart be compared to the events leading up to the American Revolutionary War. Although hyperbolic, available data supports this statement. Going forward, these findings call for further study into the tax-turnout relationship, both at the national level, in other contexts than the present one, and—perhaps most importantly—using micro data in order to further disentangle the individual-level mechanisms behind it. Not only will such work improve our knowledge of what determines electoral turnout, but will in all likelihood provide new insights into both how democracy arose in the first place and how it can thrive in the future.

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Boxholm Tea Party: Taxation and Voter Turnout in a Mature Democracy Supplemental information

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A Background information

A.1 Annual local tax rate changes, 1975-2018

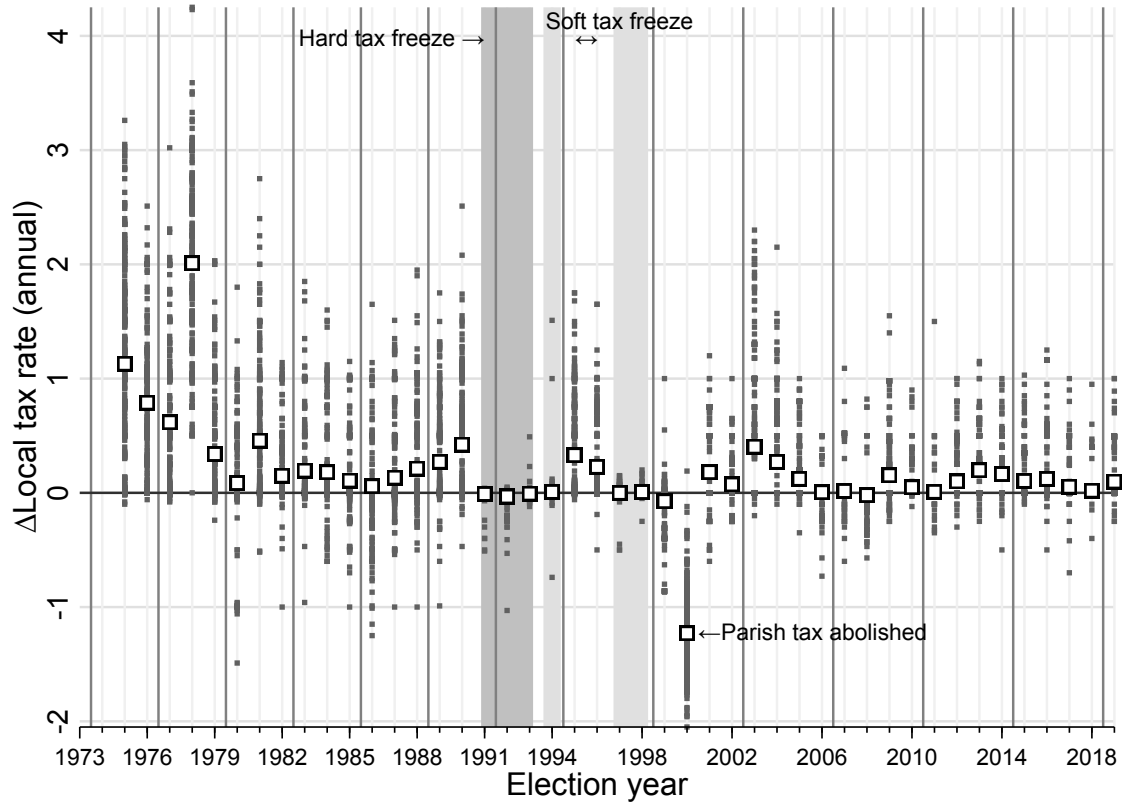


Figure A.1: Annual local tax rate changes, 1975-2018

Note. Hard tax freeze mandated by central government and applied for municipalities and counties 1991-1993; increases these individual years are either retractions from previous tax cuts within the freeze period or stem from increases in the Parish tax rate. Soft tax freeze consisted of economic incentives by central government not to increase taxes at the municipal and county level 1997-1998 (Swedish Agency for Public Management, 2011). Dark vertical grid lines indicate election. Data from Statistics Sweden (2019).

A.2 Example of Swedish income declaration form

Preliminär skatteuträkning till Inkomstdeklaration 1 för inkomstår 2018

Person-organisationsnummer: [REDACTED]

Adress: [REDACTED] Hemortskommun 1 november 2017: GÖTEBORG Län/Kom: 1480 Kommunalt skatt %: 32,60

Avdragskonto för skattebetalning: [REDACTED] Kyrkoavgift: Svenska kyrkan %: [REDACTED] Avgift: annat trosamfund %: [REDACTED] Begravningsavgift %: 0,242

Skatteuträkningen stämmer inte om du ändrar eller lägger till något belopp i deklarationen.

Sammanställning förvärvsinkomst	Sammanställning kapitalinkomst
Inkomst av tjänst + [REDACTED]	Räntekomster, utdelningar m.m. + [REDACTED]
Fastställd förvärvsinkomst = [REDACTED]	Avdrag för ränteutgifter m.m. - [REDACTED]
Grundavdrag - [REDACTED]	Underskott av kapital = [REDACTED]
Beskattningsbar förvärvsinkomst = [REDACTED]	

Preliminär skatteuträkning

Kommunalt inkomstskatt, 32,60 % (varav landstingsskatt 11,48 %)	+	[REDACTED]
Begravningsavgift, 0,242 %	-	[REDACTED]
Skattereduktion för arbetsinkomst (jobbskatteavdrag)	-	[REDACTED]
Skattereduktion för fackföreningsavgift	-	[REDACTED]
Skattereduktion för underskott av kapital	-	[REDACTED]
Summa	=	[REDACTED]
Avdragen skatt enligt kontrolluppgifter	-	[REDACTED]
Beräknat belopp som du ska få tillbaka (exklusive ränta)	=	[REDACTED]

Här finns information till din skatteuträkning

Din allmänna pensionsavgift är [REDACTED] kr. Du har fått skattereduktion med hela beloppet.

Underlag för skattereduktion för arbetsinkomst (jobbskatteavdrag) är [REDACTED] kr.

Läs mer på skatteverket.se/jobbskatteavdrag.

SKV

H

Vid inbetalning: Bankgiro 5050-1055. Du kan också beställa ett inbetalningskort via vår servicebatalon 020-507 000, drakebal 6704.

Figure A.2: Income declaration form

Note. Real example of an income declaration form for an inhabitant of Gothenburg municipality for 2018. Document informs recipient that their total local tax rate for the past year (2019) was 32.60 % of earned income, and that 11.48 % went to the county level (this information is underlined in yellow). Notably, the component of the tax rate set by the municipality (which amounts to 21.12 %) is not explicitly parsed out or stated. Since the recipient is not a registered member of a religious organization, they are informed that they paid a mandatory annual burial fee of 0.242 % of earned income. Form arrived by mail in spring 2019; confirmation deadline for recipient May 2nd.

A.3 Trajectory of tax rates in Boxholm municipality, 1974-2019

Figure A.3 tracks the trajectory of tax rates in one of Sweden's 290 municipality, Boxholm, in Östergötland county, disaggregated in nine different ways between 1974 and 2019. Its primary purpose is to demonstrate that, in Swedish municipalities, different tax rates do not always develop in tandem or even always in the same direction, illustrating the importance and difficulty of properly selecting the tax variable.

The raw measure of municipal tax rate varies substantially during the covered period, increasing from below 15 % to above 22 % of earned income. However, the bulk of this increase can be attributed to tax exchanges, whereby responsibility for major policy areas—in particular, an exchange of elderly care during the 1990s—shifted from the county to municipal levels. Occasionally, smaller such exchanges have gone in the other direction. These reforms muddle the line between actual tax raises and merely an increased set of responsibilities for a given level of government.¹

In reality, after an initially expansive period during the late 1970s, taxpayers of Boxholm have experienced more modest increases to their total local rates—from 30.2 % in 1978 to 32.5 % in 2018. This limited variation underlines the relative impact of the twin 2008/2010 hikes chronicled in the introduction. Complicating matters further, a 2000 municipal tax cut coincided with the national-level transition from a mandatory Parish tax to a voluntary Church fee, which most people have kept paying, primarily through automatic (opt-out) membership in the Swedish Church.² Even with this fee included, the highest local rate, from 2017, is at 33.9, resulting in a 12 % increase over forty years.

¹Recall that, as mentioned in section 4.1, the conflation between municipal- and county tax rates is already pervasive within public discourse, including media reports. For example, a press release issued by the tax-sceptic Association of Taxpayers (Skattebetalarna, 2017), reprinted in several local newspapers, neatly illustrates the blurred lines routinely presented to citizens when it—correctly—notes that citizens in 162 municipalities had seen their tax rates rise during the most recent term period, but fails to mention that the actual number of *municipalities* that carried out such hikes was only a little more than half of those, at 87. The remaining increases took place at the county level, thus irrespective of any decisions at the municipal level.

²Like its predeceasing Parish tax, the Church fee remains administrated by the Swedish Tax Authority and is visible on the tax form in a similar format as before. Membership rates in the Swedish Church declined from 83 % in 2000 to 58 % in 2019 (Swedish Church, 2019). These numbers do not include other denominations, which also receive membership fees through the tax receipt, but—importantly—must receive consent from the individual to make the payment.

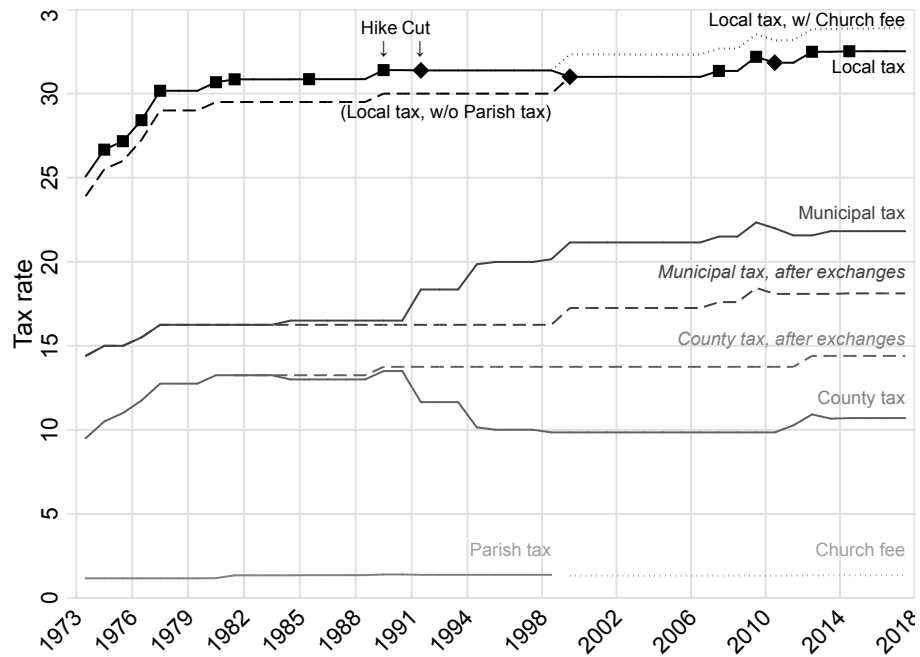


Figure A.3: Taxes in Boxholm municipality, 1974-2019

Note. Data from Statistics Sweden (2019); Swedish Agency for Public Management (2012) and Statistics Sweden (2012) (Tax exchanges 1991-2012); Mörk and Nordin (2016) (Tax exchanges 1982-1990); Various sources (Tax exchanges 2013-2019 [see code-book])

A.4 Discussion of control variables

Although the left-right dimension has heavily dominated Swedish politics for the period under study, and division along this cleavage has been used to capture political competitiveness in previous research on Swedish municipalities (Svaleryd and Vlachos, 2009), rainbow coalitions are nevertheless highly common. Therefore, using a shorter panel covering the 2010 and 2014 elections, for which reasonably reliable coalition data is available, an alternative variable to Δ Bloc vote differential, capturing Δ Vote share, ruling coalition is used; see table B.2 below.

Both Δ Bloc vote differential and Δ Local party representation are measured going into a given election campaign, that is, as the lagged difference from election $t-2$ to election $t-1$. In a context where the campaign is heavily covered in the media, and opinion polls are publicly available, thus giving voters a realistic picture of how the upcoming assembly might differ from the current one, it would make sense to measure both closeness and supply after the given election. However, for the generally small and local setting covered herein (the median municipal population in the sample barely exceeds 15,000 inhabitants), with pre-election polls usually only available for the very largest municipalities, voters' heuristics for such factors are likely best conveyed by using the results from the last round.

Both Δ Eligible voters and Δ Eligible non-citizens straddle the line between the socioeconomic and the political. In particular, the turnout literature suggests that ethnic heterogeneity and minority population are associated with low turnout (Cancela and Geys, 2016), mechanisms that work independently of eligibility concerns. Although there are other, likely more valid ways to capture this dynamic, Δ Eligible non-citizens is critical for estimating roll-off in turnout between the national- and local-level, and considering the risk of multicollinearity between this measure and, for example, measures of share foreign-born, I contend that the former is to prefer as a catch-all operationalization.

Finally, modeling parliamentary-municipal roll-off requires an additional control: Δ Non-eligible citizens, a group that consists of expatriate Swedes, who are still eligible to vote in parliamentary, but not local elections.

A.5 Summary statistics

Table A.1: Summary statistics

	Obs	Mean	Std.Dev.	Min	Max
Turnout					
Turnout, municipal election	3444	83.97	4.83	57.8	94.8
Δ Turnout, municipal election	3431	-0.55	2.44	-9.8	5.8
Turnout, parliamentary election	3444	85.96	4.35	67.2	95.7
Δ Turnout, parliamentary election	3431	-0.40	2.48	-11.0	5.9
Turnout, county election	3420	83.59	5.02	56.9	94.8
Δ Turnout, county election	3405	-0.59	2.47	-10.3	6.3
Roll-off, parliamentary-municipal turnout	3444	1.99	1.21	-0.2	11.9
Δ Roll-off, parliamentary-municipal turnout	3431	0.15	0.46	-2.1	2.3
Roll-off, municipal-county turnout	3420	0.39	0.39	-1.2	3.2
Δ Roll-off, municipal-county turnout	3405	0.04	0.31	-2.1	2.9
Taxation					
Local tax rate	3431	31.48	1.54	25.0	35.1
Δ Local tax rate	3418	0.53	1.04	-2.2	6.2
Municipal tax rate	3431	19.16	2.88	9.7	33.6
Δ Municipal tax rate	3418	0.56	0.91	-1.7	5.0
Municipal tax rate, after exchanges	3431	16.76	1.47	9.7	33.6
Δ Municipal tax rate, after exchanges	3418	0.21	0.46	-1.7	5.0
County tax rate	3427	11.60	1.69	0.0	14.5
County tax rate, after exchanges	3153	14.14	1.37	0.0	16.9
Parish tax rate	1982	1.27	0.23	0.5	1.9
Church fee rate	1449	1.29	0.16	0.8	1.9
Local tax rate, without Parish tax	3431	30.74	1.83	24.1	35.2
Δ Local tax rate, without Parish tax	3418	0.62	0.91	-1.7	6.0
Local tax rate, with Church fee	3431	32.02	1.90	25.0	36.5
Δ Local tax rate, with Church fee	3418	0.64	0.93	-1.7	6.2
Ratio local taxes:total direct taxes on individuals	1159	0.72	0.04	0.4	0.8
Δ Hike _{t+1}	3418	-0.05	0.68	-1.0	1.0
Δ Cut _{t+1}	3418	-0.00	0.46	-1.0	1.0
Δ Hike _t	3418	-0.07	0.58	-1.0	1.0
Δ Cut _t	3418	0.00	0.38	-1.0	1.0
Δ Hike _{t-1}	3418	-0.06	0.64	-1.0	1.0
Δ Cut _{t-1}	3418	-0.00	0.43	-1.0	1.0
Δ Hike _{t-2}	3132	-0.05	0.69	-1.0	1.0
Δ Cut _{t-2}	3132	-0.00	0.61	-1.0	1.0
Δ Hike _{t-3}	3128	0.03	0.49	-1.0	1.0
Δ Cut _{t-3}	3128	0.00	0.27	-1.0	1.0
Controls					
Higher education (%)	2881	9.46	5.96	2.2	45.0
Δ Higher education (%)	2591	1.16	0.90	-0.5	10.0
Day-care age (%)	3444	7.06	1.15	4.0	12.8
Δ Day-care age (%)	3431	-0.12	0.75	-3.2	2.7
Primary/secondary school age (%)	3444	15.49	2.13	8.2	28.1
Δ Primary/secondary school age (%)	3431	-0.29	0.94	-3.9	2.4
18-34-year olds (%)	3444	20.27	3.08	12.7	31.9
Δ 18-34-year olds (%)	3431	-0.38	0.93	-4.7	3.0
Senior citizens (%)	3444	19.47	4.50	4.5	34.5
Δ Senior citizens (%)	3431	0.62	0.82	-2.4	4.2
80-plus (%)	3444	4.99	1.61	0.5	10.4
Δ 80-plus (%)	3431	0.26	0.29	-1.2	1.4
In-migration, (%)	3438	4.83	1.64	1.6	14.8
Δ In-migration, (%)	3425	0.12	1.09	-6.7	7.3
Out-migration, (%)	3438	4.60	1.43	1.5	14.1
Δ Out-migration, (%)	3425	0.11	0.81	-7.1	7.8
Eligible voters	3444	24053.14	46561.53	1944.0	746313.0
Δ Eligible voters	3431	0.01	0.04	-1.5	0.2
Local party representation _{t-1}	3431	0.41	0.49	0.0	1.0
Δ Local party representation _{t-1}	3418	0.02	0.42	-1.0	1.0
Concurrent referendum	3431	0.02	0.13	0.0	1.0
Δ Concurrent referendum	3418	0.00	0.18	-1.0	1.0
Bloc vote differential _{t-1}	3431	17.75	13.54	0.0	76.3
Δ Bloc vote differential _{t-1}	3418	-0.34	8.16	-31.9	40.5
Eligible Non-Citizens (%)	3444	3.62	2.85	0.2	28.8
Δ Eligible Non-Citizens (%)	3431	0.28	0.62	-2.5	5.1
Non-eligible Citizens (%)	3444	0.71	0.89	0.0	8.4
Δ Non-eligible Citizens (%)	3431	0.13	0.31	-6.4	3.8
Median income	2023	180.85	45.90	96.9	343.5
Δ Median income	1733	20.46	5.82	2.5	40.4
Municipal fiscal result	1736	503.08	1509.58	-13703.9	9412.0
Δ Municipal fiscal result	1445	561.17	1623.46	-14472.8	14433.5
Unemployment, 18-64 (%)	1449	6.51	2.41	1.3	18.0
Δ Unemployment, 18-64 (%)	1158	0.16	1.75	-7.7	6.4
Δ Tax power, pc	1447	19179.51	5812.48	3039.0	50012.0
Δ Vote share, ruling coalition	578	-0.80	9.11	-49.8	55.5
Δ Vote share, mayoral party	867	0.28	10.12	-38.5	38.0
Left	869	0.38	0.48	0.0	1.0
Cross-ideological	869	0.25	0.43	0.0	1.0
Right	869	0.37	0.48	0.0	1.0
Social Democrats	1159	0.52	0.50	0.0	1.0
Moderates	1159	0.25	0.43	0.0	1.0
Center Party	1159	0.18	0.38	0.0	1.0
Christian Democrats	1159	0.02	0.13	0.0	1.0
Liberal Party	1159	0.02	0.12	0.0	1.0
Left Party	1159	0.01	0.10	0.0	1.0
Green Party	1159	0.00	0.04	0.0	1.0
Other Party	1159	0.01	0.11	0.0	1.0

B Robustness checks

B.1 Alternate specifications of taxation

I recalibrate the independent side by exchanging Δ Local tax rate for other feasible—but, as I argue in the article, comparatively less appropriate—measures.

First, the naïve measure of Δ Municipal tax rate garner results, in line with expectation, similar to those presented above, albeit weaker in magnitude (in particular when estimating first-order roll-off).

This trend is accentuated when estimating “pure” municipal rate changes with tax exchanges subtracted from the Δ Municipal tax rate measure, garnering highly similar results as for the unadjusted municipal rate, although further slightly weakened.

Next, I deal with the abolishment in 2000 of religious taxation, with Δ Local tax rate recalculated to either completely include or exclude payments to religious entities (before 2000 as the mandatory parish tax, and starting in 2000 as a voluntary [opt-out] church fee). Results are essentially identical to the original estimation (results in table B.1 below).

Table B.1: Municipal taxation and turnout in Swedish municipalities

	(1)	(2)	(3)
	Δ Turnout, municipal election	Δ Roll-off, parliamentary-municipal turnout	Δ Roll-off, municipal-county turnout
Δ Municipal tax rate	0.129 (0.044)	-0.003 (0.011)	0.017 (0.012)
Δ Municipal tax rate, after exchanges	0.085 (0.060)	-0.006 (0.014)	0.010 (0.012)
Δ Local tax rate, with Church fee	0.123 (0.038)	-0.023 (0.009)	0.034 (0.008)
Δ Local tax rate, without Parish tax	0.122 (0.039)	-0.021 (0.009)	0.033 (0.008)
Socioeconomic controls	Yes	No	No
Electoral controls	Yes	Yes	Yes
Observations	2585	3418	3392
Municipalities	290	290	289
Elections	9	12	12
Years	1988-2018	1979-2018	1979-2018

Note. Standard errors, clustered by municipality, in parentheses.

B.2 Alternate controls

I add a series of additional controls to the baseline model (equivalent to column 1, table 1), separately and together: Δ Median income, Δ Tax power (a more comprehensive measure of taxable income for inhabitants in each municipality [lagged one year as this iteration correlates more closely with the tax rate measure]), Municipal fiscal result [lagged two years, as this iteration correlates more closely with the tax rate measure]), measured as both level and between-term change, Δ Unemployment, Δ Ideology and Δ Vote share of the ruling coalition, as well as Δ Mayoral party and its Δ Vote share.

Although the addition of these variables invariably results in a loss of observations of varying magnitude, the main observation of a positive Δ Local tax- Δ Municipal turnout link remains unshaken.

Finally, the table displays results with Δ Higher education dropped from the socioeconomic battery of controls, thereby using the full 1979-2018 period for a model nearly identical to the preferred baseline model, still garnering a highly significant tax-turnout link (results in table B.2 below).

Table B.2: Local taxation and turnout in Swedish municipalities: Main results, Additional controls.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	Δ Median income	Δ Tax-power	Fiscal result	Δ Fiscal result	Δ Unemployment	Δ Vote share, Ruling coalition	Δ Ruling coalition	Δ Mayoral party vote share	Δ Mayoral party	All	Without Higher education
Δ Local tax rate	0.265 (0.058)	0.311 (0.057)	0.244 (0.052)	0.306 (0.057)	0.409 (0.060)	0.364 (0.127)	0.371 (0.124)	0.495 (0.079)	0.514 (0.081)	0.394 (0.134)	0.090 (0.024)
Δ Median income	-0.005 (0.008)									0.001 (0.014)	
Δ Tax power, pc		-0.000 (0.000)								-0.000 (0.000)	
Municipal fiscal result			0.000 (0.000)							0.000 (0.000)	
Δ Municipal fiscal result				-0.000 (0.000)						-0.000 (0.000)	
Δ Unemployment, 18-64 (%)					0.024 (0.026)					-0.007 (0.041)	
Δ Vote share, ruling coalition						0.000 (0.006)				-0.000 (0.007)	
Δ Left							-0.060 (0.111)			-0.001 (0.161)	
Δ Cross-ideological							0.001 (0.107)			0.035 (0.114)	
Δ Vote share, mayoral party								-0.004 (0.004)		0.005 (0.008)	
Δ Social Democrats									0.500 (0.530)	0.005 (0.609)	
Δ Moderates									0.614 (0.530)	0.172 (0.596)	
Δ Center Party									0.619 (0.553)	0.099 (0.617)	
Δ Christian Democrats									0.611 (0.587)	0.306 (0.651)	
Δ Liberal Party									0.750 (0.499)	0.164 (0.630)	
Δ Left Party									0.501 (0.733)	-0.132 (1.059)	
Δ Green Party									1.200 (0.562)	1.027 (0.636)	
Constant	0.318 (0.109)	-1.161 (0.221)	-5.474 (0.149)	-1.190 (0.209)	1.117 (0.183)	2.526 (0.288)	2.509 (0.285)	0.860 (0.213)	0.842 (0.216)	2.607 (0.451)	-1.426 (0.081)
Socioeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Electoral controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1727	1447	1732	1445	1159	578	578	867	869	577	3418
Municipalities	290	290	290	290	290	289	289	290	290	289	290
Years	1994-2014	2002-2018	1998-2018	2002-2018	2006-2018	2010-2014	2010-2014	2006-2014	2006-2014	2010-2014	1979-2018

Note. Standard errors, clustered by municipality, in parentheses. Δ Right ruling coalition (cols 5 & 9) and Δ Other party (cols 7 & 9) excluded reference categories. Data for Δ Median income (reference code HE0110K1), Tax power (per capita; reference code OE0101A0), vote shares (reference code ME0104B2) from Statistics Sweden (2019), Δ Municipal fiscal result (measured before extra-ordinary costs), from KOLADA (2019) (reference code N03107), Δ Unemployment rate (18-64 year-olds), from KOLADA (2019) (reference code N00919). For information on data for ruling coalition and mayoral parties, see codebook.

B.3 Alternate estimation techniques

I eschew the preferred estimation technique in equation (1) (i.e., first-differencing with clustered standard errors) for several other feasible specifications.

To the original FD-estimation, I replace the clustered SEs for a lagged dependent variable to account for serial correlation (table B.3.1). Results are near-identical to those presented above.

Using the Fixed effects estimator garners more substantive alterations to the results, which in turn are comparatively more dependent on how serial correlation is dealt with. Both using clustered SEs (table B.3.2) and LDVs (table B.3.3) garner results for turnout roll-off in line with the baseline results. However, in estimating the baseline model of Municipal turnout, the coefficient for Local tax rate is null. This is likely related to the finding in figure 3 suggesting that levels of tax rates are heavily driven by income levels. Indeed adding Median income to the substantive set of confounders in the baseline model, the results of Local tax are reminiscent of the FD-result, positive and-significant at the 90 % level (95 % in LDV model). When directly including an AR(1) disturbance (table B.3.4), FE results are much closer to equivalent to the original results, and consistently significant.

Finally, GMM-estimation, of both the system and difference variety, fail to reach an acceptable combination of instrument validity (measured through Hansen's J-statistic) and moderate number of instruments, and thus produce consistently highly unstable estimates (tables B.3.5, B.3.6, B.3.7, and B.3.8, present these results for reference, and prioritize either [arbitrarily, and likely liberally, defined] acceptable J-scores [$p > 0.2$] or a moderate number of instruments [$\sim < N/2$]).

Table B.3.1: Local taxation and turnout in Swedish municipalities: First-difference, with LDV

	(1)	(2)	(3)
	Δ Turnout, municipal election	Δ Roll-off, parliamentary-municipal turnout	Δ Roll-off, municipal-county turnout
Δ Local tax rate	0.138 (0.039)	-0.024 (0.009)	0.034 (0.008)
Socioeconomic controls	Yes	No	No
Electoral controls	Yes	Yes	Yes
Observations	2585	3418	3392
Municipalities	290	290	290
Elections	9	12	12
Years	1988-2018	1979-2018	1979-2018

Note: Standard errors, clustered by municipality, in parentheses.

Table B.3.2: Local taxation and turnout in Swedish municipalities: Fixed effects

	(1)	(2)	(3)	(4)
	Δ Turnout, municipal election	Turnout, municipal election (w/ Median income)	Roll-off, parliamentary-municipal turnout	Roll-off, municipal-county turnout
Local tax rate	0.025 (0.053)	0.104 (0.059)	-0.023 (0.011)	0.017 (0.008)
Socioeconomic controls	Yes	Yes	No	No
Electoral controls	Yes	Yes	Yes	Yes
Observations	2875	2017	3708	3681
Municipalities	290	290	290	289
Years	1985-2018	1991-2014	1976-2018	1976-2018

Note: Standard errors, clustered by municipality, in parentheses.

Table B.3.3: Local taxation and turnout in Swedish municipalities: Fixed effects+LDV

	(1)	(2)	(3)	(4)
	Δ Turnout, municipal election	Δ Turnout, municipal election (w/ Median income)	Roll-off, parliamentary-municipal turnout	Roll-off, municipal-county turnout
Local tax rate	0.016 (0.032)	0.111 (0.050)	-0.019 (0.007)	0.012 (0.006)
Socioeconomic controls	Yes	Yes	No	No
Electoral controls	Yes	Yes	Yes	Yes
Observations	2875	2017	3708	3679
Municipalities	290	290	290	289
Years	1985-2018	1991-2014	1976-2018	1976-2018

Note: Standard errors in parentheses.

Table B.3.4: Local taxation and turnout in Swedish municipalities: Fixed effects, AR(1)

	(1)	(2)	(3)	(4)
	Δ Turnout, municipal election	Turnout, municipal election (w/ Median income)	Roll-off, parliamentary-municipal turnout	Roll-off, municipal-county turnout
Local tax rate	0.123 (0.045)	0.233 (0.061)	-0.024 (0.010)	0.024 (0.008)
Socioeconomic controls	Yes	Yes	No	No
Electoral controls	Yes	Yes	Yes	Yes
Observations	2585	1727	3418	3392
Municipalities	290	290	290	289
Years	1988-2018	1994-2014	1979-2018	1979-2018

Note: Standard errors in parentheses. Estimated with first-order autoregressive disturbance terms (`xtregar` command in Stata)

Table B.3.5: Local taxation and turnout in Swedish municipalities: System-GMM (Prioritizing Instrument count)

	(1) Turnout, municipal election	(2) Roll-off, parliamentary-municipal turnout	(3) Roll-off, municipal-county turnout
Local tax rate	-0.007 (0.068)	-0.020 (0.015)	0.008 (0.008)
AR2 (p)	0.98	0.22	0.12
Hansen J statistic (p)	0.00	0.00	0.00
No. Instruments	115	133	126
Lags of instruments	1-1	1-1	1-1
Socioeconomic controls	Yes	No	No
Electoral controls	Yes	Yes	Yes
Observations	2875	3708	3390
Municipalities	290	290	289
Years	1985-2018	1976-2018	1979-2018

Note. Twostep Windmeijer-corrected standard errors in parentheses. Local tax rate, Bloc vote differential_{t-1}, Local party representation_{t-1}, Concurrent referendum, and LDVs treated as predetermined and instrumented GMM-style. Remaining covariates instrumented IV-style. Models prioritize keeping the instrument count below or near half of number of municipalities. Model (3) includes two lags of the dependent variable to ascertain insignificant AR(2) autocorrelation.

Table B.3.6: Local taxation and turnout in Swedish municipalities: System-GMM (Prioritizing Hansen's J-test)

	(1) Turnout, municipal election	(2) Roll-off, parliamentary-municipal turnout	(3) Roll-off, municipal-county turnout
Local tax rate	-0.038 (0.040)	-0.017 (0.015)	0.007 (0.007)
AR2 (p)	0.90	0.19	0.17
Hansen J statistic (p)	0.22	0.43	0.35
No. Instruments	285	277	270
Lags of instruments	1-5	1-4	1-4
Socioeconomic controls	Yes	No	No
Electoral controls	Yes	Yes	Yes
Observations	2875	3708	3390
Municipalities	290	290	289
Years	1985-2018	1976-2018	1979-2018

Note. Twostep Windmeijer-corrected standard errors in parentheses. Local tax rate, Bloc vote differential_{t-1}, Local party representation_{t-1}, Concurrent referendum, and LDVs treated as predetermined and instrumented GMM-style. Remaining covariates instrumented IV-style. Models prioritize keeping p-value for Hansen's J-test above 0.2. Model (3) includes two lags of the dependent variable to ascertain insignificant AR(2) autocorrelation.

Table B.3.7: Local taxation and turnout in Swedish municipalities: Difference-GMM (Prioritizing Instrument count)

	(1) Turnout, municipal election	(2) Roll-off, parliamentary-municipal turnout	(3) Roll-off, municipal-county turnout
Local tax rate	-0.057 (0.084)	-0.019 (0.018)	-0.015 (0.012)
AR2 (p)	0.49	0.51	0.43
Hansen J statistic (p)	0.00	0.00	0.00
No. Instruments	109	126	124
Lags of instruments	1-3	1-2	1-2
Socioeconomic controls	Yes	No	No
Electoral controls	Yes	Yes	Yes
Observations	2585	3418	3390
Municipalities	290	290	289
Years	1988-2018	1979-2018	1979-2018

Note. Twostep Windmeijer-corrected standard errors in parentheses. Local tax rate, Bloc vote differential_{t-1}, Local party representation_{t-1}, Concurrent referendum, and LDVs treated as predetermined and instrumented GMM-style. Remaining covariates instrumented IV-style. Models prioritize keeping the instrument count below or near half of number of municipalities.

Table B.3.8: Local taxation and turnout in Swedish municipalities: Difference-GMM (Prioritizing Hansen's J-test)

	(1) Turnout, municipal election	(2) Roll-off, parliamentary-municipal turnout	(3) Roll-off, municipal-county turnout
Local tax rate	-0.022 (0.066)	-0.034 (0.016)	-0.022 (0.011)
AR2 (p)	0.61	0.20	0.48
Hansen J statistic (p)	0.33	0.30	0.24
No. Instruments	267	255	253
Lags of instruments	1-6	1-5	1-5
Socioeconomic controls	Yes	No	No
Electoral controls	Yes	Yes	Yes
Observations	2585	3418	3390
Municipalities	290	290	289
Years	1988-2018	1979-2018	1979-2018

Note. Twostep Windmeijer-corrected standard errors in parentheses. Local tax rate, Bloc vote differential_{t-1}, Local party representation_{t-1}, Concurrent referendum, and LDVs treated as predetermined and instrumented GMM-style. Remaining covariates instrumented IV-style. Models prioritize keeping p-value for Hansen's J-test above or near 0.2.

B.4 Assorted

Table B.4.1: Median income, Higher education, and turnout in Swedish municipalities

	(1)	(2)	(3)
Δ Median income	0.007 (0.008)		-0.007 (0.008)
Δ Higher education (%)		0.263 (0.032)	0.310 (0.041)
Constant	0.255 (0.108)	-3.884 (0.070)	0.315 (0.110)
Socioeconomic controls	Yes	Yes	Yes
Electoral controls	Yes	Yes	Yes
Observations	1727	2585	1727
Municipalities	290	290	290
Years	1994-2014	1988-2018	1994-2014

Note: Dependent variable: Δ Turnout, municipal election. Standard errors, clustered by municipality, in parentheses.

Table B.4.2: Taxation and turnout in Swedish municipalities: Main results—full table

	(1)	(2)	(3)
	Δ Turnout, municipal election	Δ Roll-off, parliamentary-municipal turnout	Δ Roll-off, municipal-county turnout
Δ Local tax rate	0.134 (0.038)	-0.024 (0.009)	0.034 (0.008)
Δ Higher education (%)	0.265 (0.032)		
Δ Local party representation _{t-1}	-0.117 (0.051)	0.014 (0.014)	-0.030 (0.012)
Δ Concurrent referendum	0.195 (0.135)	0.189 (0.048)	0.090 (0.032)
Δ Bloc vote differential _{t-1}	-0.015 (0.003)	0.001 (0.001)	-0.000 (0.001)
Δ Eligible Non-Citizens (%)	-0.496 (0.047)	0.430 (0.017)	
Δ Eligible voters	-1.992 (0.680)	0.353 (0.115)	-0.024 (0.116)
Δ Day-care age (%)	0.124 (0.064)		
Δ Primary/secondary school age (%)	0.026 (0.055)		
Δ 18-34-year olds (%)	0.011 (0.047)		
Δ Senior citizens (%)	0.151 (0.046)		
Δ 80-plus (%)	-0.250 (0.097)		
Δ Out-migration, (%)	0.098 (0.033)		
Δ In-migration, (%)	-0.043 (0.028)		
Δ Non-eligible Citizens (%)		-0.307 (0.086)	
Constant	-3.938 (0.071)	0.308 (0.029)	-0.076 (0.024)
Socioeconomic controls	Yes	No	No
Electoral controls	Yes	Yes	Yes
Observations	2585	3418	3392
Municipalities	290	290	289
Years	1988-2018	1979-2018	1979-2018

Note: Standard errors, clustered by municipality, in parentheses.

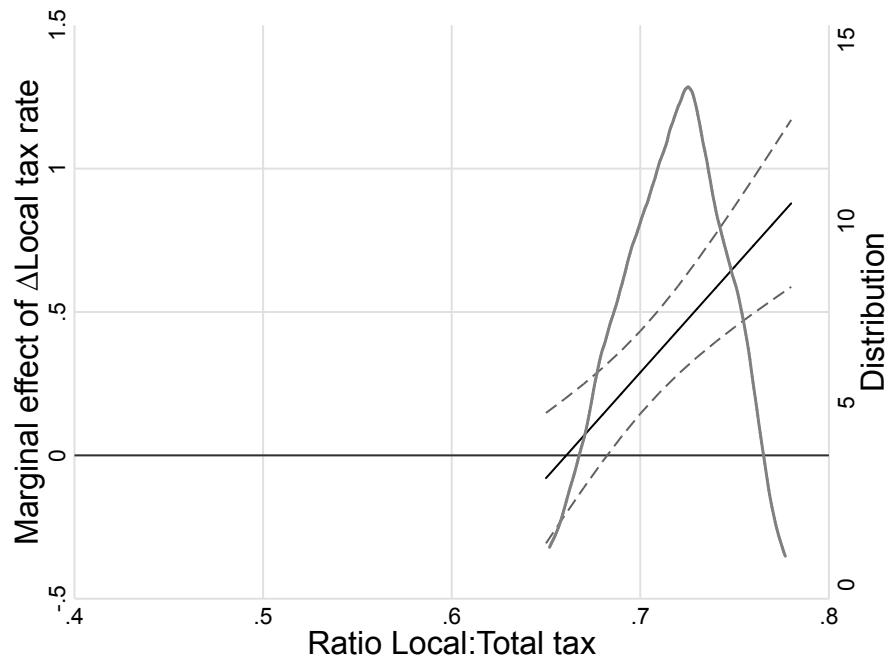


Figure B.4.1: Test of Stakes, 2002-2014, excluding 5 % with lowest on Local:total tax ratio

Note. Results from model identical to the one used to produce figure 5, but with observations with less than 0.6539 on Local:total tax ratio excluded (57 cases; 5 % of observations). Data for 2002, 2006, 2010, and 2014 from Statistics Sweden (2019) (Matrix no. OE0101D1 [Local tax]; HE0110S9 [Ratio local taxes:total direct taxes on individuals]). Dashed lines display 95 % confidence intervals.

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