

The electoral consequences of taxation in OECD countries

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ABSTRACT

Researchers, politicians, and pundits commonly expect that voters retrospectively punish and reward government parties for tax policies, but there is surprisingly little cross-country evidence that backs this claim. This study provides comprehensive evidence from 30 OECD countries, 1970–2020. It analyzes the electoral fates of government parties that increased or cut taxes on personal incomes and consumption. Our findings confirm the prevalence of electoral consequences, but these depend on the type and direction of tax change. Government parties lose votes when they increase personal income taxes while there is only marginal evidence suggesting electoral reward for income tax increases and electoral consequences after value-added tax changes. The findings also indicate the distributive effects of reforms to matter. The most pronounced consequences arise when governments raise income taxes on the poor. The moderating role of conditional factors such as government partisanship and fiscal pressure are explored, but no consensus emerges from the findings.

1. Introduction

Conventional wisdom holds that tax increases are unpopular with voters and thus highly risky for governments. Correspondingly, tax cuts supposedly bolster government popularity. Historical evidence appears to support these claims, such as George H. W. Bush’s promise of no new taxes (“read my lips!”) to attract voters in the 1988 presidential election. The subsequent loss to Bill Clinton in the 1992 election was blamed by many commentators on the breach of this tax pledge. The political downfall of Margaret Thatcher is closely linked to the introduction of the infamous poll tax (Gibson, 1994). But fear of electoral punishment is not restricted to the right. Recent surveys among left-wing politicians from Germany, Austria, and Spain show that they worry deeply about raising taxes even when such reforms only target a financially privileged minority (Elsässer et al., 2023; Fastenrath et al., 2022).

Empirical evidence on the electoral consequences of taxation is much less clear. Findings from country studies—which dominate the field—are inconclusive (e.g. Finseraas, 2012; Geys and Vermeir, 2008a, 2008b; Johnson et al., 2005; Stults and Winters, 2005). More comprehensive cross-country evidence is therefore necessary to reach more robust conclusions. However, this kind of evidence is surprisingly limited so far (Foucault et al., 2017; Tillman and Park, 2009). Another reason for taking a skeptical stance on the electoral consequences of taxation is given by evidence on changes to welfare policies. Although

the welfare state is popular and tends to be a salient issue, recent research suggests that there is no systematic electoral punishment (reward) for welfare retrenchment (expansion) (Ahrens and Bandau, 2023).

Focusing on the two main forms of taxation in terms of generated revenue—personal income taxes and consumption taxes—this article provides cross-country evidence on the electoral consequences of taxation in 30 OECD countries, 1970–2020. It presents the most comprehensive and fine-grained evidence to date, including a large number of countries and years. We pay particular attention to the proper measurement of tax changes and analyze the effects of both the implementation of tax reforms and tax rate changes. We explore under what conditions electoral consequences are most likely to occur and consider a broad range of political, economic, and tax-related factors that may amplify electoral punishment and reward.

Our findings suggest the prevalence of electoral consequences, but these consequences differ across tax types, reform direction, and the distributive effects of reforms. The main result is that increasing taxes on personal incomes is punished at the subsequent election while there is only partial support for the reward of tax decreases, pointing to the importance of voters’ negativity bias. Tax increases on relatively poor workers are punished more than increases on the rich, which we attribute to fairness concerns. We only find very limited support for the electoral consequences of value-added taxes. Depending on the

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specification, some results suggest that value-added tax decreases are rewarded. Finally, no clear pattern emerges concerning the moderating role of conditional factors such as government partisanship. The magnitude of electoral punishment and reward differs for left and right parties, but the statistical results are associated with insufficient certainty to draw strong inferences.

The remainder of this article is structured as follows. In the next section, we review the literature and discuss the limitations of previous research. We then outline our theoretical considerations on the electoral consequences of taxation. In this context, we present a list of conditions under which electoral consequences should become especially likely. Following the description of our quantitative approach, we provide evidence from statistical analyses. We finally discuss the main findings and limitations of our study.

2. State of the art

Research on the political costs of taxation goes back to the 1970s. Among the pioneers were public choice scholars such as William A. Niskanen (1975, 1979), who found that a rising tax burden hurts the incumbent in US presidential elections. Many researchers tested this ‘taxation matters’ hypothesis thereafter for the US, Canada, and several European countries. The supplementary material contains a detailed overview of these studies (see Table A1).

In contrast to Niskanen (1975, 1979), most research on the United States focused on gubernatorial and other state elections. While many studies find the tax burden and tax rates to have an electoral impact (Eismeier, 1983; Geys and Vermeir, 2008a; Kelleher and Wolak, 2007; Niemi et al., 1995), some authors come to the opposite conclusion (Peltzman, 1992; Turett, 1971). Other studies indicate that tax effects are contingent on additional factors. Reportedly, electoral consequences are especially likely when Republicans govern (Lowry et al., 1998; Sobel, 1998; Stults and Winters, 2005), when sales taxes are reformed (Kone and Winters, 1993; Stults and Winters, 2005), and when changes are implemented in election years (MacDonald and Sigelman, 1999; Nelson, 2000).

The research on the US is complemented by studies on other OECD countries. Research using Canadian data shows a negative relationship between taxation and incumbency voting on the federal level (Happy, 1992), whereas electoral costs vary across tax types on the state level (Landon and Ryan 1997). Germany’s federal government approval ratings are negatively related to tax burden increases (Cusack, 1999; Geys and Vermeir, 2008b). For Great Britain, Johnson et al. (2005) even conclude that an increase in the effective tax rate is associated with electoral defeat. In contrast, there seems to be no such negative link between changes in the personal tax burden and election outcomes in Norway (Finseraas, 2012).

Finally, several studies focus on the impact of taxation on local elections. Research on the electoral effects of British local taxes shows that incumbents are punished for raising tax rates (Gibson, 1988, 1994; Revelli, 2002). More recent studies from Belgium and Sweden do not detect this kind of punishment (Dassonneville et al., 2016; Mörk and Nordin, 2020). Many studies on local elections instead point to the impact of yardstick competition—that is, the impact of lower or higher tax rates on neighboring municipalities (e.g. Dubois and Paty, 2010; Revelli, 2002; Vermeir and Heyndels, 2006).

Evidence from cross-country studies is thus far limited to only two studies (Foucault et al., 2017; Tillman and Park, 2009). Tillman and Park (2009) analyze the electoral consequences of changes to marginal income tax rates in 19 OECD countries, 1990–2006. The authors find that changes to bottom marginal tax rates but not to top marginal tax rates have electoral effects. Reportedly, a “1 percentage point increase (decrease) in the basic income tax rate reduces (increases) the incumbent’s vote share by just over 0.5%” (Tillman and Park, 2009, p. 322). This effect is found to be conditional on government partisanship, with stronger electoral consequences for right-wing parties. Analyzing

effective tax rates, Tillmann and Park further find that the effective taxation at average earnings of families but not of singles relate to incumbents’ vote shares, although this effect is not conditional on partisanship.

Foucault et al. (2017) analyze the electoral consequences of tax revenue changes in 21 OECD countries, 1965–2011. Their analysis suggests that electoral consequences are especially likely under high clarity of responsibility and that left parties are rewarded and right parties punished for tax increases. The study also highlights the moderating role of economic circumstances, as right-wing governments are not punished by voters when they increase taxes during times of economic recession.

In sum, parts of the literature suggest that taxation is electorally consequential, but the evidence is far from consistent. It is unclear to what extent the findings can be generalized because they are derived from a limited number of countries and only two cross-country studies. Furthermore, especially the cross-country studies are held back by their conceptualization and measurement of tax changes. First, Foucault et al. (2017) and a large number of country studies use the rather blunt measure of tax revenue changes, which obfuscates who exactly is being taxed more or less. Tax revenues can also vary without political interference. Reverberating Esping-Andersen (1990), it is therefore difficult to imagine that individuals struggle over tax revenues *per se*. Second, Tillman and Park (2009) mainly rely on measures of bottom and top marginal income tax rates, which are difficult to compare across countries because they kick in at varying relative income levels. Overall, more comprehensive and robust evidence is required. Our empirical analysis will draw from the surveyed literature but move beyond it by measuring tax changes more appropriately and by including a larger set of countries and years in the analysis.

3. The electoral consequences of tax changes

The argument for the electoral consequences of taxation is based on the theory of retrospective economic voting, which holds that citizens vote either for or against the government based on economic performance indicators such as unemployment, inflation, and growth (see Duch and Stevenson, 2008; Lewis-Beck and Stegmaier, 2007, 2013). However, retrospective voting based on *policy performance* has found less attention, primarily regarding the electoral consequences of fiscal consolidations (Alesina et al., 1998, 2019; Tilley et al., 2018) and welfare state changes (e.g., Ahrens and Bandau, 2023; Horn, 2021). Based on the available evidence, Lewis-Beck and Stegmaier (2013) conclude that political factors such as policies play a limited role in retrospective voting decisions.

Applied to taxation, the retrospective voting argument holds that voters punish or reward government parties at the polls based on their retrospective assessment of tax policy performance. Their motivation is either material self-interest (“pocketbook voting”), sociotropic concerns about the material wellbeing of others (Ballard-Rosa et al., 2017), or efficiency-related concerns about the impact of taxation on economic performance (Emmenegger and Marx, 2019). The general expectation is that governments should be punished for tax increases and rewarded for tax decreases.¹

3.1. Reform characteristics

Drawing from previous research, the simple retrospective voting argument can be refined to arrive at more nuanced expectations of when tax changes should have electoral consequences. The first group of conditions concerns reform characteristics, in particular the scope,

¹ This expectation hinges on the ability of voters to notice tax changes and to correctly appreciate their distributive effects (cf. Ahrens and Bandau, 2023, p. 1635–1640).

distributive consequences, timing, and direction of tax changes.

First, electoral consequences should be most pronounced when more voters are directly affected by particular tax reforms (*scope of tax changes*), such as in the case of a universal raise of sales taxes (Kone and Winters, 1993; Landon and Ryan, 1997; Nelson, 2000).

Second, the findings by Tillman and Park (2009) indicate that the *distributive consequences of tax reforms* should be considered. Holding the scope of taxation constant, electoral consequences depend on whom tax reforms affect because voters are motivated by distributive justice concerns (Reeskens and van Oorschot, 2013). We expect that regressive tax reforms—which favor the rich or put additional strain on poorer households—should be perceived as unfair by the electorate and yield stronger negative electoral effects.² Pocketbook voting also matters regarding the distributive effect of reforms. A reform lowering taxes for lower and middle incomes while increasing taxes on the much smaller number of rich people should be electorally beneficial if voters are guided by economic self-interest.

Third, voters are boundedly rational and tend to have a short memory (Ahrens, 2023), which is why the *timing of tax reforms* becomes critical. Electoral effects should be stronger for reforms implemented shortly before an election (as confirmed by MacDonald and Sigelmann [1999] and Nelson [2000] for the US).³

Finally, insights from welfare state research suggest that the *direction of tax changes* matters, as voters exhibit negativity bias or loss aversion (Pierson, 1994, pp. 17–19; Weaver, 1986). The electoral costs of tax increases should be higher than the electoral gains of similarly sized tax cuts because voters react stronger to losing than to gaining resources. This asymmetry in voter reactions has been empirically confirmed by Kone and Winters (1993) for the US.

3.2. Political and economic context

A second group of conditions concerns political and economic factors, which were highlighted by Foucault et al. (2017). First and foremost, *government partisanship* should condition voters’ reactions because the voter bases of different party families have distinctive policy preferences (Hibbs, 1977). We expect that left-wing constituencies prefer higher levels of (progressive) income taxes and lower levels of (regressive) consumption taxes compared to right-wing constituencies (Ballard-Rosa et al., 2017; Stantcheva, 2021). This is due to both economic self-interest and connected fairness concerns: on average, left constituencies have lower incomes and use a greater share of their income for consumption than right-wing constituencies. Right constituencies may also consider taxation to be a more important issue for their voting decision because of their higher material stakes. Therefore, right parties should face a stronger electoral backlash for introducing progressive and tax-increasing reforms compared to left parties, and they should be rewarded more for regressive and tax-decreasing reforms.

It must be noted that the competing ‘Nixon goes to China’ logic popular in welfare state research (Ross, 2000) arrives at the opposite conclusion regarding partisanship. Left governments may be more vulnerable to tax increases because liberal and conservative parties have issue ownership regarding taxation and are thus both sheltered from critique and in a better position to lash out against their adversaries. However, available empirical evidence rather supports the traditional

² The negative electoral effects of regressive tax reforms should fail to materialize if voters rather adhere to a trickle-down narrative according to which all voters will eventually profit from tax cuts for the rich.

³ Classical models of the political business cycle assume that governments anticipate voters’ reactions and thus time fiscal reforms including taxation in an electorally favorable way (Nordhaus, 1975). Cross-country empirical evidence does, however, not support the hypothesis that governments systematically behave in this opportunistic way (Andrikopoulos et al., 2006; Klomp and De Haan, 2013).

partisan hypothesis (Foucault et al., 2017; Lowry et al., 1998; Sobel, 1998; Stults and Winters, 2005; Tillman and Park, 2009).

Regarding the political and economic reform context, Foucault et al. (2017) point to two further relevant factors. First, the *clarity of responsibility* influences whether voters can identify who is responsible for a tax reform, which depends on the number of governing parties and additional veto players (Hobolt et al., 2013). Responsibility is clear when a government is dominated by a single party, when government parties are ideologically cohesive, and when there is no cohabitation. Maximum clarity can be achieved under single-party governments. Second, voters’ willingness to punish government parties for tax hikes may also depend on economic conditions. There is a tradeoff between taxation and the budget where, for example, a tax cut slashes into available revenue. Based on the observation that voters not only like low taxes but are also fiscally conservative (Bansak et al., 2021; Peltzman, 1992), the willingness to punish the government for tax hikes should be less pronounced during times of *fiscal crisis*.

Table 1 presents a summary of the conditional expectations. It lists the conditions under which electoral consequences are especially likely and thus serves as a guide for our empirical analysis.

4. Data and method

4.1. Measuring electoral changes

We evaluate whether tax policies have electoral consequences by compiling a dataset that tracks the development of tax policies and vote shares of government parties from 30 OECD countries between 1970 and 2020, although some empirical measures are available for less countries and years. The selection of countries and years is driven by data availability and the wish to keep a relatively consistent sample between indicators with different spatial availability.

One observation in our dataset relates to the incumbency of a specific government party. We include one observation per government party and legislative period. The dependent variable is the percentage point change in votes between the election that brought a party into power and the subsequent election. We consider a party to be in government between the start date of the first cabinet it participated in and either the end date of the last cabinet it participated in or the day of the subsequent election, whatever date is earlier. We exclude parties in caretaker governments and parties that governed less than one year. The data cover 637 government parties formed by 185 unique parties in 298 governments. We use the ParlGov database for all information regarding parties, elections, and cabinets (Döring and Manow, 2023).

Table 1
Summary of conditions that favor electoral consequences.

	Condition	Stronger consequences	Explanation
<i>Reform characteristics</i>	Scope of change	Tax reform affects many voters	Self-interest, fairness, efficiency
	Distributive effects	Regressive tax reform	Reform is perceived as more unfair
	Timing of change	Reform close to election	Voters have a short memory
	Direction of change	Tax increase	Negativity bias of voters
<i>Political and economic context</i>	Government partisanship	Right government	Constituents more strongly affected by tax policy
		Left government	Right parties more credible on taxes ('Nixon goes to China')
	Clarity of responsibility	High clarity of responsibility	Voters can attribute tax change
	Fiscal crisis	Low fiscal pressure	Voters see no fiscal necessity

Our data setup with party-incumbencies as the unit of observation stands in contrast with previous cross-country research, which relied on country-year or country-election period setups. We prefer our data setup because it allows for direct evaluations of differences in electoral consequences across varying party families, but we use a country-election setup in robustness tests.

4.2. Measuring tax changes

We focus on two tax types in our measurement of tax changes, namely personal income taxes (PIT) including social security contributions and value-added taxes (VAT). Voters should care most deeply about these taxes because they are directly subjected to them (as opposed to, for example, corporate income taxes). Furthermore, PIT and VAT bring in a large proportion of overall tax revenues (OECD, 2023). The outstanding scope of these tax types and their direct relevance regarding pocketbook voting and fairness concerns implies that electoral consequences should be most pronounced.

We use several indicators to measure tax changes. We first use the IMF's Tax Policy Reform Dataset (Amaglobeli et al., 2018), which contains information on the adoption of tax reforms in 16 OECD countries.⁴ The data record each major tax reform⁵ with separate entries in the dataset for the tax type (PIT or VAT), dimension of change (rate or base change), and direction of change (increase or decrease). For example, a reform package that decreases the PIT rate but broadens the tax base is recorded in two separate entries in the dataset. The data record that a reform was rolled out at a specific point in time. While the magnitude of change remains unquantified, the data allow us to pinpoint exactly when tax reforms were implemented.

We use several reform indicators for both PIT and VAT. Net change indicators record how many tax-increasing reforms were implemented relative to tax decreasing reforms during government incumbencies. For disaggregated analyses of tax increases and decreases, we use separate variables recording the number of tax in-/decreasing reforms. And for analyses on the effects of reforms shortly before an election, we use tax reform measures that record reforms in the 12 months before a government left office.

Our second type of tax change indicator measures the development of tax rates between the years a government party entered and left government within an electoral period (available for for 30 countries).⁶ Short-term changes are measured with tax rate changes in the last government year. First, we use a measure of the development of standard VAT rates. Our various data sources are listed in the supplementary material. Second, we use the development of effective tax rates on labor income from dependent employment, including social security contributions. The data stem from Scruggs (2022) and are based on Taxing Wages data (OECD, 2023). We rely on wage tax rates averaged over a large number of household and earning profiles: singles at 50%, 100%, 150%, and 200% of mean earnings; single parents with two children at 50% and 100% of mean earnings; and couples with two children at 100%–0%, 50%–50%, 100%–50%, and 150%–150% of mean earnings. We also use these individual effective tax rates for disaggregated analyses to assess the implications of tax changes for particular household and earnings profiles.

We use this selection of indicators because we want to assess the

⁴ Australia, Austria, Canada, Czech Republic, Denmark, France, Germany, Greece, Ireland, Italy, Japan, Luxembourg, Poland, Portugal, Spain, and United Kingdom.

⁵ We exclude data on minor tax reform announcements. For example, tax rate changes below 1% are defined as minor.

⁶ Australia, Austria, Belgium, Canada, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Iceland, Ireland, Italy, Japan, Latvia, Lithuania, Netherlands, New Zealand, Norway, Poland, Portugal, Slovakia, Slovenia, Spain, Sweden, Switzerland, and United Kingdom.

validity of our results across different measurement approaches that all have advantages and disadvantages. Table 2 offers a summary of the various up- and downsides of the indicators.

Fig. 1 presents descriptive statistics of the tax change measures. It shows to what extent the government parties in our compiled dataset implemented tax changes. It becomes evident that governments tended to cut the PIT while increasing the VAT. Furthermore, VAT rates are more stable: about 50–70% of governments did not change the VAT, depending on the indicator.

4.3. Method

We estimate the following linear regression model with ordinary least squares (OLS):

$$\Delta vote_{pce} = \alpha + \beta \Delta tax_{pce} + \delta cntrl_{pce} + \gamma_c + \vartheta_e + \epsilon_{pce}$$

where $\Delta vote_{pce}$ is the vote change of government party p from country c in election period e (i.e. between an election in year t and the subsequent election in year $t+1$), Δtax_{pce} denotes a tax change indicator, $cntrl_{pce}$ a vector of control variables, γ_c country fixed effects, ϑ_e time fixed effects (government end in 1970–1975, 1976–1980, etc.), and ϵ_{pce} the error term. The tax rate change measures quantify rate changes between the years a party entered and left government (within an electoral period), and the reform measures quantify the number of tax reforms during parties' incumbencies.

In further analyses, we assess whether tax increases and decreases have different electoral implications. We split the tax change indicators Δtax_{pce} into the two components $\Delta_{post} tax_{pce}$ and $\Delta_{neg} tax_{pce}$, whereas the former only records tax increases and takes the value zero otherwise and the latter only records tax decreases and is zero otherwise.⁷ We then enter the two components together in the regression model above to estimate separate slope coefficients for them.

We weight all statistical analyses by the inverse of the number of

Table 2
Advantages and disadvantages of tax change measures.

Indicator	Advantages	Disadvantages
Number of PIT and VAT tax reforms	Considers changes to tax bases Considers changes to reduced VAT rates Data record the exact timing of reforms	Imperfect measure of the magnitude of reforms Voters may have trouble observing more technical reforms
Changes in the effective taxation of wages	Wages are the most significant income source for most of the employed Quantifies the magnitude of tax changes Considers changes to tax bases Allows disaggregated analysis of tax changes for different earnings profiles	Voters may have trouble observing changes to effective tax rates Disregards taxation of alternative income sources (e.g., self-employment or financial income) Only measured once per year
Changes in VAT rates	Voters may primarily observe tax rates (rather than bases) Quantifies the magnitude of tax changes	Does not consider changes to tax bases Does not consider changes to reduced VAT rates Only measured once per year

⁷ For tax rate changes, the separate increase and decrease indicators are constructed based on the overall change Δtax_{pce} . For example, the tax rate increase indicator only records values above zero if the overall tax rate change is positive (and vice versa). For the reform indicators, we can use a more fine-grained measure. The tax increase and decrease indicators are given by the number of tax-increasing and tax-decreasing reforms, respectively, implying that both can deviate from zero concurrently.

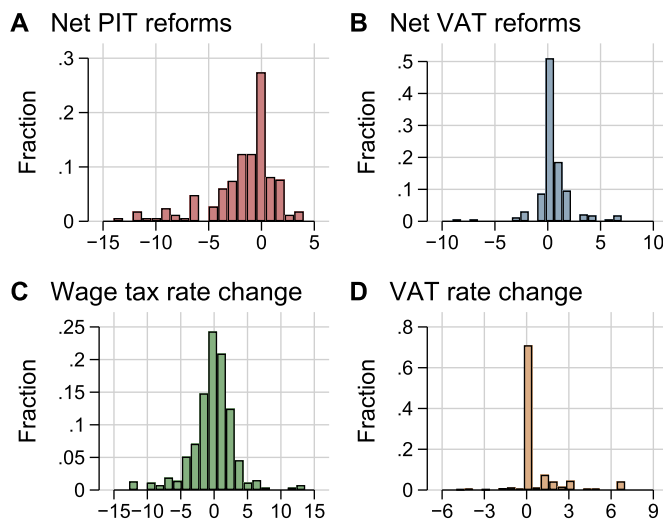


Fig. 1. Distributions of the tax change indicators.

coalition parties in a government⁸ to tackle a shortcoming of using party-incumbencies as the unit of analysis. As there can be multiple observations per government, coalition governments—and thereby countries with proportional voting systems—have an inflated influence on unweighted results. The weighting approach avoids this.

The regressions use standard errors clustered by governments that are consistent in the presence of heteroskedasticity and autocorrelation within coalition governments. We estimate the clustered standard errors using wild cluster bootstrap,⁹ which returns valid standard errors even when there are few clusters, few observations within clusters, and/or an unequal amount of observations between clusters.

Lastly, we winsorize all variables (including the controls) at the 0.5th and 99.5th percentiles to limit the impact of extreme variable values on the results.¹⁰ We show the results of regressions using the unrestricted values in the supplementary material.

4.4. Analysis of conditional electoral consequences

We use additional dummy variables to differentiate between government parties that are more and less likely to experience electoral consequences. First, a *left party* dummy identifies social democrats, left socialists, and ecologists parties; and a *right party* dummy identifies liberals, conservatives, and Christian democrats. Second, a dummy identifies governments with *high clarity of responsibility*. We construct this dummy based on the continuous ‘government clarity’ indicator proposed by Hobolt et al. (2013), using the sample median as a threshold to differentiate lower- and higher-clarity governments. Lastly, a *high fiscal pressure* dummy identifies governments with a budget deficit of more than 3% of GDP, which is a threshold inspired by the Maastricht criteria.¹¹

We use these dummies in interaction models based on the main regression specification formulated above. We include additional interactions between the dummies and the respective tax change indicator

(s) as well as the dummies in uninteracted form. We then calculate marginal effects for targeted subsets of the data, such as parties operating under high clarity of responsibility.

4.5. Controls

We control for a number of variables drawn from previous research on electoral consequences and economic voting: parties’ vote share in the initial election in t , the effective number of parties using the measure by Golosov (2010), the government duration in days, both average levels and changes in unemployment and GDP growth (where changes relate to the years that a government began and ended), the budget balance and debt levels in the first government year, as well as time fixed effects (government end in five-year intervals: 1970–1975, 1976–1980, and so on).¹²

5. Main results

We begin the inquiry with descriptive statistics. Detailed information on the univariate distributions of all variables is available in Table A2 in the supplementary material. Here we focus on the bivariate relationship between tax and vote changes. Fig. 2 depicts scatterplots of vote changes and the four tax change measures, with separate linear fits for tax hikes and cuts. It becomes evident that there is a negative relationship between (wage) income tax and vote changes, which holds for both reforms and effective taxation changes. The relationship is much more pronounced for tax hikes, implying electoral punishment for raising personal income taxes. However, these results are not replicated by the two VAT change measures, which display mostly flat relationships between tax and vote changes. The scatterplots therefore offer partial support for the theoretical expectations.

5.1. Tax reforms

We move on to evidence from multivariate regressions including control variables and uncertainty estimates, which allows for more robust inferences. Fig. 3 presents the results of 24 regression models that

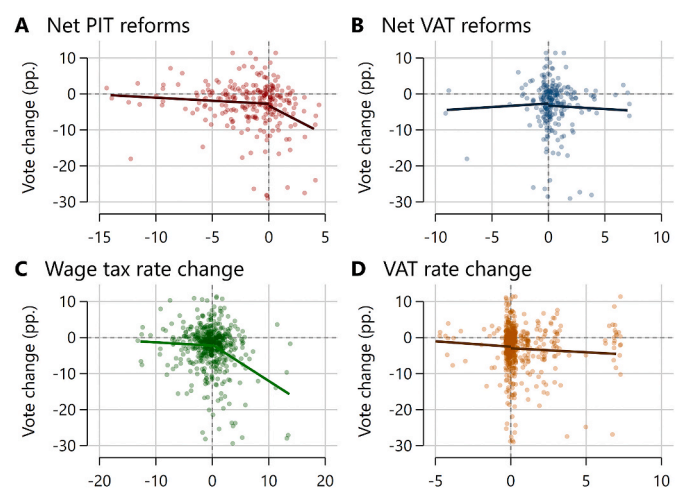


Fig. 2. Scatterplots of tax and vote changes

Note: The data include a minor amount of random dispersion because of the discrete measurement of tax reforms.

⁸ No. coalition partners

⁹ We use 10,000 bootstrap repetitions and Rademacher weights.

¹⁰ Winsorizing ensures that a variable cannot exceed a certain threshold by recoding values above/below the threshold to the threshold. VAT changes are winsorized at the 0.5th and 97th percentiles because of severe top-side outliers between the 97th and 99.5th percentiles.

¹¹ We recode the continuous variables clarity of responsibility and budget balance into dummies because this makes the analysis of conditional effects consistent with the other conditions, which are measured in binary variables as well. This allows us to concisely present a large number of statistical results.

¹² We use five-year bins for the time fixed effects to get a more consistent country coverage in the bins, as the selection of countries with governments that end in a given years varies considerably over the years.

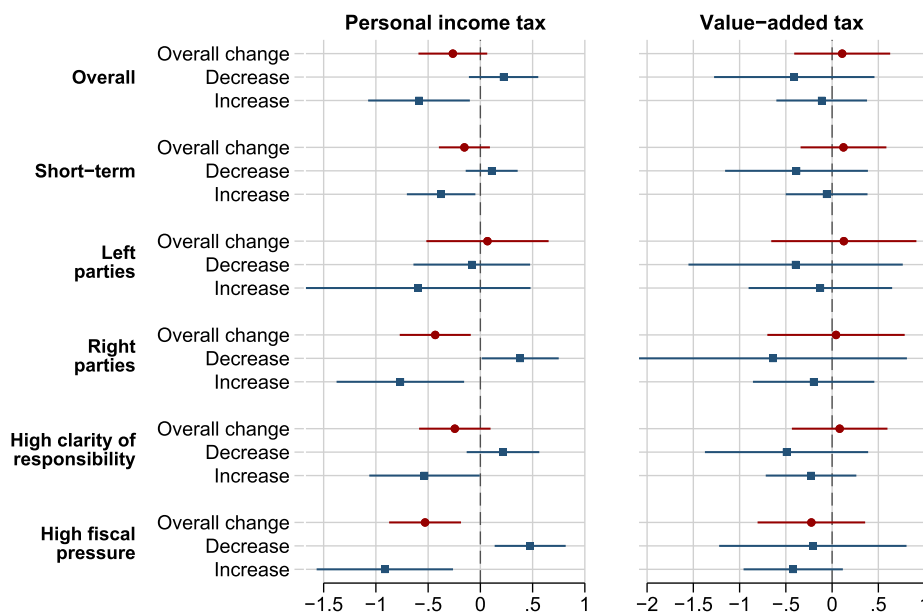


Fig. 3. Effects of tax reforms on vote changes

Note: The analyses include 16 countries (PIT) and 15 countries (VAT), 1970–2014. The horizontal bars are 95% confidence intervals obtained from standard errors clustered by governments (estimated with wild cluster bootstrap). Full regression tables of the 24 underlying regressions are available in the supplementary material (Tables A3–A6).

assess the effects of tax reforms. The models on the left-hand side relate to PIT/SSC reforms and the models on the right-hand side to VAT reforms. For both tax types, we begin with an assessment of tax changes across the whole sample (“overall”), first assessing the effects of tax changes in any direction and then the separate effects of tax increases and decreases. We move on to an analysis of short-term tax changes—that is, tax reforms implemented in the 12 months before a government ended. Finally, we present results from sub-group analyses, zooming in on left parties, right parties, governments operating under high clarity of responsibility, and governments under high fiscal pressure. The displayed coefficients for these subgroups are marginal effects estimated from interaction models with binary condition identifiers.

Fig. 3 provides the following results regarding PIT reforms. Overall, tax reforms are negatively related to vote changes, implying that tax hikes are punished and tax cuts rewarded. The disaggregated analysis of tax decreases and increases confirms this overall finding, although the positive coefficient for tax cuts only reaches significance on a 10% level.

The effect sizes for PIT reforms are substantially important. Parties are estimated to lose 0.6 percentage points in votes per tax increasing and win 0.3 percentage points per tax decreasing reform (coefficients from the “overall” results). Since the mean government implements about two tax increasing and 3.5 tax decreasing reforms—with the PIT reforms variable having a standard deviation of about three—these effect estimates have considerable size.

The subsequent analyses of short-term changes and subgroup effects show that it is difficult to pinpoint where exactly electoral consequences arise. Although the estimates are sometimes weaker (for example, short-term tax changes) and sometimes stronger (for example, tax reforms by right government parties), the effect estimates remain statistically indistinguishable from the overall results across all governments and tax changes. It is noteworthy that tax cuts by left governments are estimated to have no effect at all, which contrasts with the strong estimate of electoral reward after tax cuts by right governments; and that tax hikes are estimated to be punished even under high fiscal pressure. However, even these effects are statistically indistinguishable from the main results once the uncertainty estimates are considered.

The results regarding the VAT indicate null effects of VAT reforms. All estimates are insignificant, and most coefficients are near zero. The

results therefore do not suggest that there are electoral consequences after VAT reforms, even under most likely circumstances such as high clarity of responsibility.

5.2. Tax rate changes

Fig. 4 repeats the approach with the tax rate measures. Its left side presents the results regarding changes to average effective wage tax rates and the right-hand side regarding changes to VAT rates. The results on wage taxes suggest that—across the whole sample—wage tax changes are negatively related to vote shares. Tax hikes significantly reduce the vote share of government parties in the next election. The coefficient for tax cuts is positive but does not reach significance.

The effect size of tax increases (“overall” results) is substantially important. Government parties are estimated to lose 0.7 percentage points in votes after increasing the effective taxation of wages by one percentage point. Wage tax increases have a standard deviation of about 3 in our dataset, thus indicating a considerable effect size.

As before, it is difficult to pinpoint where exactly electoral consequences arise. The coefficients of short-term wage tax rate changes and of the subgroup analyses vary, but—taking the uncertainty estimates into account—they cannot be distinguished robustly. Still, it is noteworthy that tax increases are estimated to have clear negative effects on left parties in government, while tax cuts are estimated to be wholly inconsequential for those parties. Furthermore, tax hikes are punished even under adverse fiscal conditions.

The results regarding VAT changes hint at electoral reward for VAT decreases, but the results are insignificant in every specification. At least in our sample, left parties and parties operating under high clarity of responsibility experience an increasing vote share after reducing VAT rates. Short-term decreases are estimated to be relevant as well. The results point toward a strong relationship, with a one-percentage point VAT rate decrease being associated with between about 1 and 3 percentage points more votes for the incumbents at the subsequent election. But the insignificance of the results prevents us from confirming our hypothesis with sufficient certainty. It must also be noted that VAT increases are in some cases also positively related to vote gains, which counters theoretical expectations.

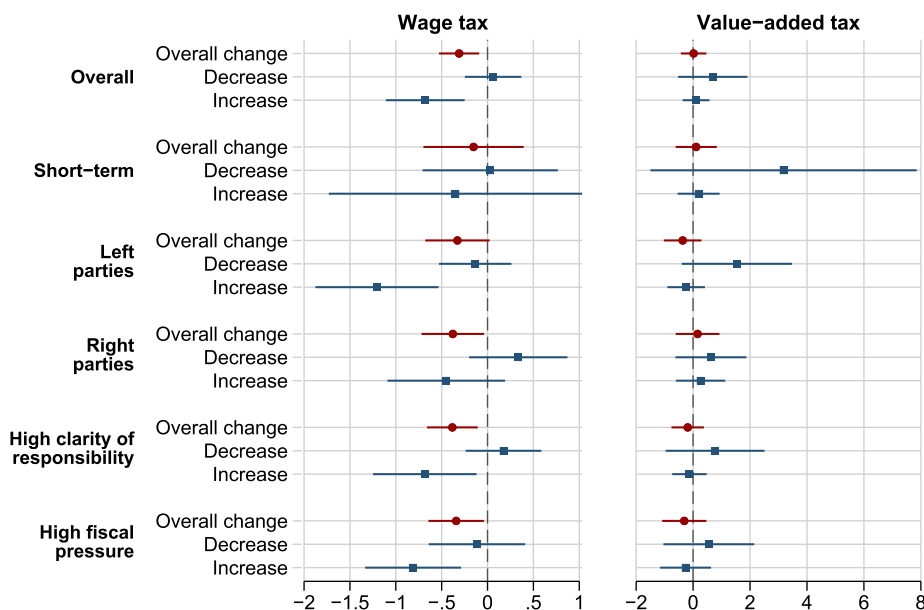


Fig. 4. Effects of wage and value-added tax rate changes on vote changes
 Note: The analyses include 28 countries (wage tax) and 29 countries (VAT), 1970–2020. The horizontal bars are 95% confidence intervals obtained from standard errors clustered by governments (estimated with wild cluster bootstrap). Full regression tables of the 24 underlying regressions are available in the supplementary material (Tables A7–A10).

5.3. Robustness tests and additional analyses

We conduct several sensitivity tests to gauge whether our main results from Figs. 3–4 are robust to alternative specifications. The full results are available in the supplementary material. First, our specification of the unit of observation (government parties) allows for targeted subgroup analyses, but the effect estimates depend to a greater extent on parties with a higher initial vote shares because—on average—they experience larger vote changes. We therefore analyze all government parties together in additional regressions by collapsing the dataset by election period, resulting in country-election periods as the unit of analysis. The results are consistent with the main results and even show a sizable positive effect of VAT decreases (see Figs. A1–A2).

Next, we measure the dependent variable as percent changes rather than percentage point changes. The motivation is that the main specification using percentage point changes may mask the electoral consequences for smaller parties. For example, a three-percentage point loss in votes would be substantial for a junior coalition partner with an initial vote share of 6%, but such changes can be overshadowed by the expectably larger percentage point changes of larger parties. Using percent changes puts smaller and larger parties on equal footing. The results are largely consistent with our main results (see Figs. A3–A4), but the effect estimates of tax changes are not significant in every model where they were before (e.g., increases to wage taxes).

Because putting parties of different sizes on equal footing produces results less consistent with theoretical expectations, we suspect that electoral consequences are primarily experienced by larger parties. We therefore conduct exploratory analyses of electoral consequences conditional on parties’ initial vote share, derived from models with interactions between tax changes and initial vote share (see Figs. A16–A23). The results offer partial support for our suspicion. The wage tax increase variable (which turned insignificant in the percent change specification) has a much stronger effect as the initial vote share increases and a null effect for small parties.

Returning to the robustness tests, we estimate several alternative specifications to show that the main results are not driven by a cherry-picked model specification. All results are almost identical to our main results, and the significance levels of the coefficients leave our

inferences unchanged (see Figs. A5–A14). In particular, we first omit all control variables except the fixed effects. Second, we re-run the main models using the original variables before winsorizing outlier values. Third, we use an alternative version of the five-year time dummies that relies on the start year of the government instead of the end year. Fourth and fifth, we use standard errors clustered by (a) parties and (b) countries instead of clustering by governments because there is also autocorrelation within parties and countries. We consider clustering by governments to be most appropriate for the main models because both vote and policy changes are clearly interrelated between coalition partners but want to ensure that the results are not affected by alternative clustering approaches.

6. The effects of wage tax changes across the income distribution

We now test whether electoral consequences are especially likely when wage tax changes affect particular earnings and household profiles along the income distribution. To recapitulate, we expect that tax changes have different effects across the income distribution for two reasons. First, fairness considerations could lead voters to punish reforms that benefit the rich rather than the poor and reward reforms that reward the poor rather than the rich. Second, the effects of pocketbook voting should be less pronounced for reforms of higher incomes since only a small number of voters are affected by those reforms.

We disaggregate the average wage tax measure into six of its sub-components (effective taxes on singles with 50%, 100%, 150%, and 200% of mean earnings, as well as on couples with 50%–50% and 150%–150% of mean earnings). We use these indicators in separate regression models to gauge their effects on electoral results.

The results depicted in Fig. 5 suggest that negative electoral consequences following tax increases are especially likely when poorer people are affected, as the coefficient are consistently stronger and only reach significance for 50% earnings singles and 50%–50% couples. Furthermore, the results suggest that tax decreases have stronger positive effects when they concern richer individuals, as the results on 100% and 150% singles and 150%–150% couples show.

Despite the results depicted in Fig. 5, it remains difficult to

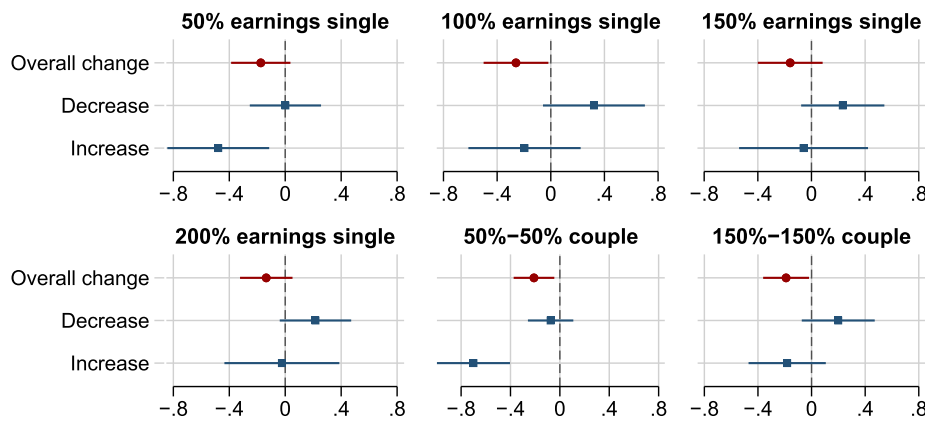


Fig. 5. Effects of effective wage taxation by earnings and household profile
 Note: The analyses include 28 countries, 1970–2018. The horizontal bars are 95% confidence intervals obtained from standard errors clustered by governments (estimated with wild cluster bootstrap). Full regression tables of the 12 underlying regressions are available in the supplementary material (Tables A11–A12).

disentangle what types of tax-increasing reforms are electorally consequential because the confidence intervals of the estimates are partly overlapping. Furthermore, the wage tax change measures have considerable correlation (upwards of $r = 0.45$). In analyses depicted in Fig. 6, we therefore opt for another approach where we enter the wage tax change measures for poorer and richer singles (Model 1) and poorer and richer couples (Model 2) in the same regression models to control for contemporaneous changes. Fig. 6 also displays the results of targeted null hypothesis tests of coefficient equality, which allows us to assess the significance of effect differences between, for example, tax increases on poorer and richer individuals.

Fig. 6 shows that tax increases are associated with electoral punishment when they concern poorer rather than richer individuals. This is indicated by significant effect size differences regarding tax increases on the poor and rich. There is even some evidence suggesting that tax increases on the rich are rewarded electorally. Furthermore, the evidence suggests that tax decreases are rewarded electorally when they concern richer rather than poorer individuals, with positive and significant coefficients for decreases on richer individuals that also deviate significantly from the effect of tax decreases on the poor. Overall, the results indicate that tax changes are not electorally consequential per se—it matters who is being taxed. They also suggest that the average wage tax measure used as the main indicator masks significant heterogeneity in the effects of wage tax changes at particular points in the wage distribution.

To gauge the robustness of the results, we run a shortened selection of the sensitivity tests from above (see Figs. A24–A26 in the supplementary material). We first repeat the regressions using the collapsed

version of our data where governments rather than government parties are the unit of analysis, thereby pooling coalition governments together. Second and third, we alter the clustering level of the standard errors, clustering by parties and countries rather than governments. All these tests support the finding that tax increases on the poor are punished while tax increases on the rich are not, but they offer no or less support for tax decreases on the rich being rewarded (more than tax decreases on the poor).

Overall, the results suggest that tax increases on the poor are punished to a greater extent than tax increases on the rich, which we attribute to fairness concerns. To be sure, the findings are also consistent with the theory that the effect differences result from a combination of pocketbook voting and the scope of the tax changes. Under typical income distributions, more voters have 50% of average earnings rather than 200% of average earnings, implying that tax changes concerning 50% of earnings have stronger effects because they affect more people. However, we present additional evidence in Fig. A27 in the supplementary material that supports the fairness-based argument. It compares the effects of wage tax changes for poorer workers (50% and 50%–50% of earnings) with effects of changes for average (rather than rich) earners (100% and 150%–50% of earnings). This comparison avoids the tax scope disparity. The results show that only the effects regarding poorer workers are significant and negative, while the effects for average earners are insignificant, with point estimates near zero. Although the effect estimates do not differ significantly from one another in this analysis, this gives us confidence that fairness considerations—rather than pocketbook voting—drive the different results.

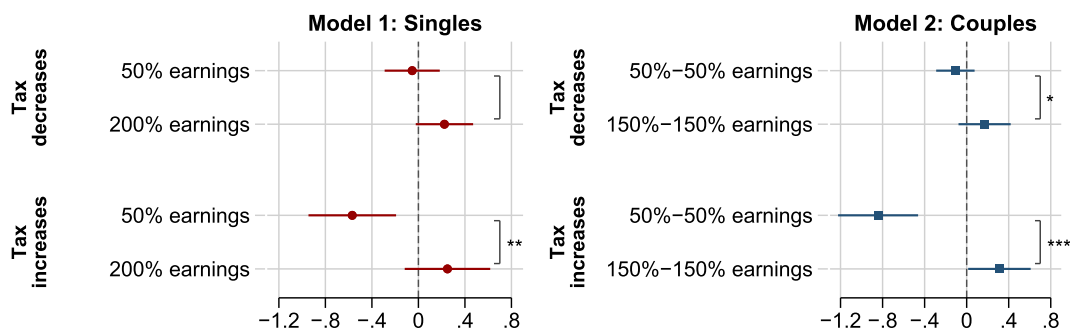


Fig. 6. Relative effect of wage taxation changes on the poor and rich
 Note: The analyses include 28 countries, 1970–2018. The horizontal bars are 95% confidence intervals obtained from standard errors clustered by governments (estimated with wild cluster bootstrap). The significance test results displayed within the coefficient plots are results from tests for coefficient equality, also obtained from wild cluster bootstrap (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.001$, two-tailed tests). A full regression table of the two underlying regressions is available in the supplementary material (Table A13).

7. Placebo tests

As a last step of our analysis, we explore the electoral consequences of changes to (a) tax revenues and (b) corporate income taxes. There are two motivations for this. First, tax revenue changes were used prominently in previous research, and the results will therefore serve as a helpful reference point against which our analysis can be compared. Second and more importantly, we use both tax revenue and corporate taxes to conduct placebo tests. That is, we use tax change indicators that—according to our theory—should have no or negligible relationships with electoral outcomes. To recapitulate, we expect null results for both tax revenue and corporate taxes because changes to them are difficult to observe and have unclear implications for people’s material well-being. If there are indeed null results, this would boost our confidence in the main results based on personal income and value-added tax changes. It would suggest that the material relevance and the visibility of tax changes matter for their electoral implications, and it would show that the main results are not spuriously created by peculiarities of our methodological setup.

The results displayed in Fig. 7 suggest that tax revenue and corporate taxes are not related to electoral results. All coefficients are insignificant, and the large majority has effect estimates at or near zero. There are some notable exceptions, such as the positive coefficients for tax revenue decreases under left parties or close to elections. Another striking result emerges from the analysis of corporate tax changes under right parties, which should arguably experience the most pronounced consequences for corporate tax changes due to their business-adjacent constituencies. Though our results point in this direction, the coefficients remain insignificant and statistically indistinguishable from the overall results. Fig. A15 in the supplementary material underscores these results based on a robustness test of the collapsed data that analyzes the electoral fates of all government parties together, which returns only insignificant results once again. Overall, the placebo tests therefore offer support for our theoretical argument, empirical analysis, and methodological choices.

8. Conclusion

This study evaluated whether governments gain or lose at the polls after overseeing tax changes. Based on a theory of retrospective economic voting, we argued that voters should punish governments electorally for tax increases and reward them for tax decreases, with several conditions potentially weakening and strengthening these consequences.

The empirical results support the expectation that tax policies have electoral consequences. The first key finding is that increasing taxes on personal income—especially via taxing labor income—is punished at the subsequent election. Some estimates also indicate that decreasing personal income taxes and value-added taxes is rewarded at the subsequent election, but these results are unstable and depend on the exact statistical specification. The results therefore suggest that voters display *negativity bias*, as they have stronger reactions to tax hikes compared to tax cuts. Further analyses indicate that the distributive effects of tax changes matter. Tax increases are not electorally consequential *per se*—governments are mainly punished for increasing personal income taxes falling on lower-income households, which we interpret as the result of low perceived fairness of such reforms.

It is puzzling why these effects are not mirrored in the analysis of VAT changes because the VAT affects all voters and has regressive distributive effects. In our view, there are two explanations. First, the limited variance of VAT changes in our data reduces the empirical robustness of the related results because they are driven by few cases. Second, VAT increases are presumably less contentious and salient in election campaigns than increases in PIT. For left parties, VAT increases promise revenues needed to finance state intervention, while right parties prefer them over progressive reforms of income taxation to balance the budget (Kemmerling and Truchlewski, 2021, pp. 85–86).

It is notable that the empirical reform patterns coincide with the political incentives of parties. Governments tended to decrease the PIT and increase the VAT, as Fig. 1 shows. Policymakers may therefore be aware of the different electoral costs of tax changes.

Additional analyses do not enable us to pinpoint further conditions that exacerbate electoral consequences. The effect estimates vary for the

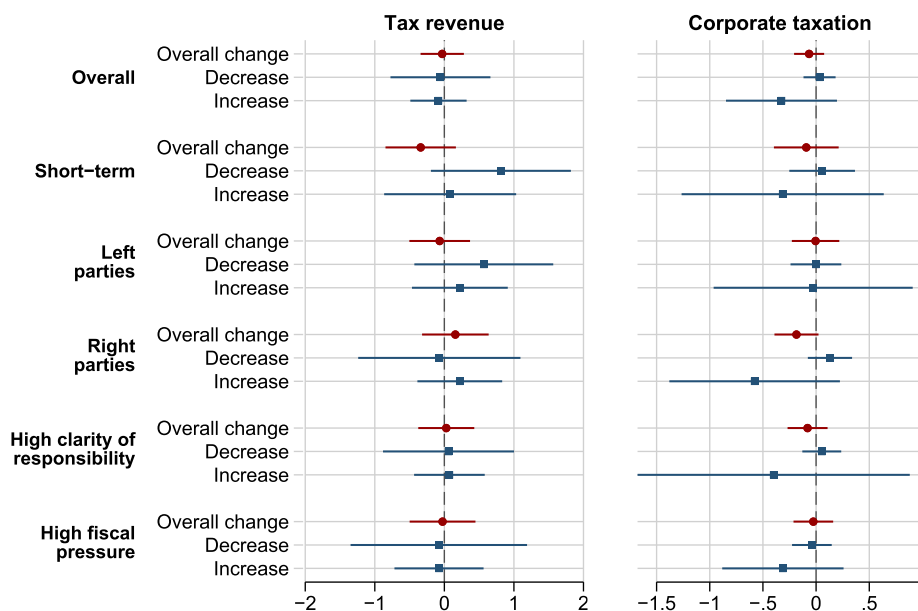


Fig. 7. Effects of tax revenue and corporate tax changes on vote changes

Note: The analyses include 30 countries (tax revenue) and 29 countries (corporate taxation), 1970–2020. The horizontal bars are 95% confidence intervals obtained from standard errors clustered by governments (estimated with wild cluster bootstrap). Full regression tables of the 24 underlying regressions are available in the supplementary material (Tables A14–A17). Tax revenues are measured as total tax revenues across all government levels relative to GDP (OECD, 2023). Corporate taxes are measured as statutory rates combined across all government levels (Tax Foundation, 2022).

subgroups of left parties, right parties, parties operating under high clarity of responsibility, and parties facing budgetary pressure. However, the estimates remain statistically indistinguishable. The same holds for tax changes shortly before an election. The results therefore offer no support for the success of typical blame avoidance and credit claiming strategies via strategical reform timing, diffusion of political responsibility, and using fiscal pressure to justify tax hikes.

The findings regarding tax changes close to a coming election are notable. Observed tax changes should display stronger consequences in the short term not only because voters tend to have a short memory but also as a result of endogeneity. Politicians are known to strategically time reforms (Wenzelburger et al., 2020), implying that they should implement reforms they expect to be popular shortly before an election and avoid unpopular ones. That is, the tax changes we observe shortly before an election may depend on the (expected) electoral payoffs. We do not observe this to be the case, suggesting that strategic timing may have limited success.

Our results stand in contrast to previous research. Unlike both Tillman and Park (2009) and Foucault et al. (2017), we do not find that partisanship matters for the severity of electoral consequences. Furthermore, our placebo tests assessing tax revenue changes result in null findings across the board. This indicator was used by many previous studies, with many of them presenting positive findings (e.g., Foucault et al., 2017). We attribute these contrasting results to our methodological setup, which differs from previous research regarding its tax change measures and unit of analysis. We argue that our setup is appropriate for the analysis of electoral consequences. Especially the effective wage tax indicators measure the actual tax burden of particular voters, which is not the case for broad measures such as tax revenue or marginal tax rates. And using government parties as the unit of analysis enables a targeted analysis of electoral consequences conditional on partisanship.

Our findings are particularly striking when contrasted with findings from welfare state research. Recent evidence suggests that—unlike tax changes—welfare changes have no systematic electoral consequences for governing parties (Ahrens and Bandau, 2023). The empirical evidence thus indicates that tax changes are electorally more consequential than welfare changes. This is in line with a majority of research on the electoral effects of fiscal austerity, which shows that tax-based austerity packages are particularly unpopular with voters (Alesina et al., 1998; Alesina et al., 2013; Alesina et al., 2019, pp. 175–193; Ardanaz et al., 2020; Bremer and Bürgisser, 2023; Giuliani, 2022).

Survey experiments on voters' policy preferences indicate that such a distinction between tax and welfare state changes might be too simplistic. Conjoint experiments on austerity packages demonstrate that the specific design of reform packages matters to voters (Bansak et al., 2021; Bremer and Bürgisser, 2023; Hübscher et al., 2023). Reportedly, certain austerity measures—such as cuts to health care and pensions and tax increases for low-income households—substantially reduce public support, whereas tax increases for top-income earners and cuts to the public sector are more popular with voters. Taking the popularity and distributional consequences of specific tax and welfare reforms into account, policymakers might thus be able to compensate for unpopular measures such as tax increases and design reform packages in an electorally favorable way.

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CRediT authorship contribution statement

Leo Ahrens: Writing – review & editing, Writing – original draft, Visualization, Methodology, Data curation, Conceptualization. **Frank Bandau:** Writing – review & editing, Writing – original draft, Investigation, Conceptualization.

Declaration of competing interest

None

Data availability

A replication package including all data and code are available at the following OSF repository: <https://doi.org/10.17605/OSF.IO/E3RFB>.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.electstud.2024.102774>.

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