

# Austerity and Elections\*

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## Abstract

This paper revisits the conventional but unproven wisdom that voters penalize governments for adopting fiscal austerity in a sample of advanced economies. We consider the composition of the austerity package and the economic manifesto of the implementing government and find that austerity packages consisting mostly of tax hikes have a significant electoral cost, which is larger for government parties that campaigned on a free-market manifesto. Conversely, expenditure-based austerity is costlier for government parties that did not run on a small-government platform, but may be beneficial for those that did.

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## I. INTRODUCTION

Conventional wisdom holds that governments that reduce the budget deficit by raising taxes or cutting expenditures are punished at the polls. However, this belief is not supported by existing empirical evidence. In what is probably the most comprehensive empirical study on this topic, Brender and Drazen (2008) find a weak opposite effect: governments overseeing a reduction of the deficit are slightly rewarded.

This paper reassesses the conventional wisdom that austerity carries an electoral cost by making important progress in the identification of fiscal austerity and highlighting the importance of the “how”—whether austerity is done via tax hikes or expenditure cuts—and the “who”—whether the government that implements it campaigned on a free-market manifesto. As we show, these elements are key to reconcile the empirical evidence with the conventional wisdom.

To assess the “how”, we rely on a real-time narrative dataset of *ex-ante* tax- and expenditure-based austerity plans, in a sample of 16 OECD countries. In contrast to most of the existing literature that uses *ex-post* yearly data on the deficit-to-GDP ratio to measure austerity, this allows us to (i) analyze actual austerity policies rather than instances where the deficit decreases simply because the economy is in a boom, and (ii) identify the precise timing of each austerity plan to match it to the government that introduced it, which is crucial for the results.<sup>1</sup>

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<sup>1</sup> The budget deficit is a grossly imperfect measure of fiscal austerity, as it is endogenous to the state of the economy. The deficit-to-GDP ratio may go down not because a government has adopted austerity, but simply because the economy is in a boom. Conversely, the deficit may go up despite the introduction of austerity policies because the economy is deteriorating. For instance, in Denmark and Finland before the Global Financial Crisis (GFC) the deficit went down without the government adopting any austerity policy. On the other hand, in Ireland and Spain the deficit kept increasing after the GFC, despite the introduction of huge austerity packages by the governments in charge, which eventually ended up losing the election. In such a situation, a researcher relying on *ex-post* fiscal data may baselessly conclude that expansionary fiscal policy (and not austerity or weaker growth) is harmful for re-election. Moreover, austerity might be attributed to the wrong government because (i) *ex-post* fiscal data are available only at yearly frequency for most countries, while elections and changes in governments can take place at any time of the year, and (ii) fiscal adjustments are often decided early in the term of a government, as we document.

To examine the “who”, we rely on a new measure of economic manifesto. This captures the frequency of favorable mentions of free-market capitalism as an economic model during the electoral campaign preceding the election that brought the leader’s party into power. We assume that parties that favor free-market capitalism are also in favor of smaller governments and campaign for expenditure and tax cuts. The advantage of using economic manifesto to characterize political parties as opposed to classifying them according to their ideological leaning (such as left vs right) is twofold. First, it focuses specifically on economic ideology.<sup>2</sup> Second, it is time-varying, which allows for greater data variability and therefore for more precise estimates.

Combining these two datasets with novel data on political parties’ vote shares, we find strong evidence supporting the proposition that (some) austerity plans are politically costly. Our first key result is that how austerity is done matters: tax-based austerity carries large electoral costs. We find an austerity package worth 1% of GDP, carried out mostly through tax hikes, to be associated with a 7% reduction in the vote share of the leader’s party. This negative association between tax-based austerity and the electoral fortunes of the government introducing it has become somewhat stronger over time, with the tax-based austerity coefficient being about 15% larger in the second part of our sample (from 1994 onwards) relative to the first part. By contrast, we do not find expenditure-based consolidations to be associated with any change in the vote share on average across different governments, both in the first and in the second part of the sample.

These estimates are robust to a battery of robustness checks, aimed at addressing endogeneity by (i) focusing on the sample of consolidation plans that are announced in the first year of

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<sup>2</sup> The use of political leaning classification may be misleading as there may be instances in which, for example, right-leaning parties do not campaign on free-market platforms. Indeed, we also source a variable capturing general ideology and find that it is only weakly correlated to our measure of economic manifesto suggesting that the use of economic ideology indicator is not pleonastic.

government, when strategic considerations are less of a concern; (ii) controlling for the strength of the government; (iii) and for the choice of whether to introduce austerity; and (iv) using the augmented inverse probability weighting (AIPW) estimation proposed by Jordà and Taylor (2016). If anything, these exercises suggest that the true cost of tax-based austerity may be even larger.

Our second set of results highlights the importance of economic manifesto. We find that the reduction of the vote share associated with tax-based austerity is considerably larger in the case of government parties that campaigned on a free-market manifesto. We also find that expenditure-based consolidations are associated with a decrease of the vote share of the leader's party which did not campaign on a free-market platform and with an increase in the case that it did.

Economic manifesto matters even conditional on political ideology. We find that left-leaning government parties are punished at the polls if they implement expenditure-based austerity. But this electoral fallout is less severe, and can be entirely contained, the more the party campaigned on centrist economic policies. Similarly, right-leaning governments that campaigned on a free-market platform can make important electoral gains if they deliver expenditure cuts, whereas they miss out if they do not. These results suggest that what really matters for the electoral effects of austerity policies is economic ideology and whether the government that implements austerity does so in deviation from what the parties supporting it promised during the campaign trail. To the best of our knowledge, we are the first to study the heterogeneous effects of austerity depending on economic and political ideology and to underline the importance of consistency with the promised platform. In the working paper version of this paper (Alesina et al. 2024), we develop a stylized theoretical model supporting the importance of manifesto in explaining the electoral effects of austerity policies.

We close the paper by carrying out an extension to our baseline results and investigate whether the timing of austerity in relation to the business cycle matters. We find that the electoral fallout of

austerity policies—especially tax hikes—is more significant when these are implemented during bad economic times. This result is consistent with those of Alesina et al. (2023) who show that economic reforms are penalized during contractions but are often rewarded in expansions and suggest that voters may have difficulties distinguishing between underlying economic conditions and the growth effects of some government policies.

The rest of the paper is organized as follows. The next subsection provides a short review of the most recent literature on the topic. Section II discusses the dataset and the construction of our austerity variables, before presenting some stylized facts on the timing of austerity announcements in the electoral cycle and the characteristics of the governments responsible for them. We describe the empirical framework in Section III and present the main results in Section IV. Section V discusses a few extensions to the baseline analysis. Section VI concludes. Online Appendices A-B provides detailed information on the construction of the dataset and data sources, Online Appendix C presents the robustness checks of the empirical results, while Online Appendix D compares our results with those obtained in the previous literature for advanced economies.

### ***Related literature***

The results of the early literature on the effects of austerity, mostly based on *ex-post* yearly austerity measures, are generally inconclusive. Alesina et al. (1998) find that reductions in the primary deficit do not predict a future change in government in a sample of advanced economies, while Alesina et al. (2012) do not find any evidence that large improvements in the cyclically adjusted budget balance decrease the probability that a government is replaced by one with a different ideology. Alesina et al. (2019) in Chapter 10 review the literature on this point and provide new results which are again

inconclusive: it is hard to find a strong statistical correlation between austerity measures and the electoral fortunes of the incumbent.

While Alesina et al. (2019) also measure austerity using plans that are exogenous to cyclical economic conditions, our approach significantly improves upon that of these authors since (i) we identify the precise timing (month) of each austerity plan, and (ii) match it to the government that introduced it. As we show in Online Appendix D, these elements are key to properly identify the electoral costs of austerity. Close to our paper, Ardanaz et al. (2020) examine an action-based narrative data set of fiscal consolidation plans and find that tax-based fiscal adjustments are punished by voters, but expenditure-based ones are not. Further, they provide micro evidence showing that voters prefer expenditure cuts to tax increases as a fiscal consolidation tool. However, these authors consider a set of selected Latin American and Caribbean countries, which are very different from our sample of OECD advanced economies on both the economic and political domains, and do not consider the crucial issue of deviations from what promised during the campaign trail.

Other studies have also looked at the relation between fiscal performance and elections. Peltzman (1992), Brender (2003) and Drazen and Eslava (2003) study the effects of budget deficit on electoral results in state and local elections in, respectively, the United States, Israel and Colombia, and find that voters punish, rather than reward, budget deficits. Brender and Drazen (2008) examine 350 electoral campaigns across 74 countries and conclude that voters are (weakly) likely to punish rather than reward budget deficits accumulated during the leader's term in office. Relatedly, Arias and Stasavage (2019) study a sample of countries and a timeframe ranging from 1870 to 2011 and show that decreases in the expenditure level are not associated with government turnover. The main difference between our paper and these studies is that we use a real-time narrative database of *ex-ante* fiscal measures.

A recent literature has looked at the effect of fiscal austerity on populism. For example, Fetzer (2019) documents a significant association between the exposure of an individual or area to the UK government's austerity-induced welfare reforms begun in 2010 and the subsequent rise in support for the UK Independence Party. Relatedly, Gabriel et al. (2023) and Hübscher et al. (2022) use our same austerity dataset and find that austerity packages lead to a significant increase in polarization and a decrease in turnout. Our paper differs from these contributions by (i) directly linking austerity to the electoral fortunes of the government implementing it, (ii) distinguishing between tax hikes and expenditures cuts, and (iii) highlighting that what matters for the electoral outcomes of austerity policies is whether the government deviated from what promised in its manifesto.

Finally, our results are connected to the theoretical literature focusing on the strategic determinants of fiscal policy. The literature hinges on relaxations of some assumptions of Tiebout's proposition that voting by migrating is a sufficient commitment device to align fiscal policy incentives by giving incumbents some market power, which typically comes from migration frictions (Bradford et al. 1969, Courant, Gramlich, and Rubinfeld, 1979, Epple and Zelentiz, 1981), and asymmetric information on politician's competence: Rogoff (1990) points out that in equilibrium politicians run larger deficits before elections in order to signal their ability in the production of investment goods, while Besley and Case (1995) argue that in equilibrium they benchmark their deficits to neighboring regions used as a control group for inference about politicians' ability. More related to our findings, the literature starting with Barro (1973) and Ferejohn (1986) has analyzed moral hazard in policy makers. Banks and Sundaram (1991) view elections as a punishment device against politicians who deviate from policies because they signal incompetence. Schwabe (2009) shows that if politicians have private information both on competence and on effort, reputation is not a strong enough incentive to have them provide effort in office. de Mesquita and Friedenber (2011) show that lack

of knowledge of the incumbent's ability makes voters better off with incumbents that implement what they promise regardless of future states of the economy, rather than adapting to such states.

## II. DATASET

Our sample covers 16 advanced economies (Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Ireland, Italy, Japan, Portugal, Spain, Sweden, the United Kingdom and the United States) and spans the 1978-2014 period. Both the country and time dimensions of the sample are dictated by the availability of the austerity data, as we explain next.

### A. Austerity Data

We use the action-based dataset of Alesina et al. (2019), who build upon Devries et al. (2011), and identify fiscal consolidation plans in 16 OECD countries from 1978 to 2014 relying on a narrative approach à la Romer and Romer (2010). In particular, the dataset covers over 3500 different fiscal measures adopted in the context of more than 250 austerity plans. Some of these measures are announced for future implementation (over the 5 subsequent years), while others are implemented within the same announcement year. The data record the budgetary impact (as estimated in the documents that accompany their adoption) of these fiscal measures relative to a baseline of no policy change.

This dataset has three important advantages for our analysis. First, it allows us to determine the exact month of the announcement of each austerity plan decided by the government. This is possible because Alesina et al. (2019) link each plan to the official policy records used to identify it. We retrieve the precise month of each announcement by reading the policy records and, when these



do not contain the announcement date, by retrieving it from other sources including news media and commentaries. Once the month of announcement is identified, we assign the austerity plan to the government that announced (implemented) it. This would be impossible using standard methods to identify austerity measures from *ex-post* yearly changes in the budget, since elections and changes in governments take place at any time of the year.

The second advantage of the Alesina et al. (2019) dataset is that it shows that austerity measures are typically introduced by means of multi-year plans which should affect voters' expectations about the future. Since the dataset lists both measures for immediate and future implementation, we can account for policies that are yet to be implemented but that still influence voters' choices at the election. The third advantage is that it only covers discretionary measures—that is, changes in the budget that are exogenous to cyclical economic conditions. Focusing on discretionary actions is important to isolate the electoral effects of government actions from the electoral effects of countercyclical policies. Moreover, the exogeneity of our austerity measure to output allows us to test whether austerity policies have effects on electoral variables through their effects on output. We provide more details on the dataset of Alesina et al. (2019) in Online Appendix A.

## **B. Construction of the Austerity Variables**

To construct our fiscal variables, we first assign each austerity plan recorded in Alesina et al. (2019) to the government that announced it. For each plan, we then calculate the overall budgetary impact

expected in real time over a five-year period—that is, considering both measures for immediate and future implementation—as a fraction of GDP.<sup>3</sup>

Following Alesina et al. (2019), we distinguish between tax- and expenditure-based plans. We classify each austerity plan as either tax- or expenditure-based, depending on whether 55% or more of the overall consolidation effort is achieved via tax hikes or expenditure cuts, respectively. By doing so, we exclude from the analysis a few plans that are close of 50/50% because the classification of these plans would be arbitrary. However, our main results are robust if 50% or 60% thresholds were used instead of 55% to define tax- and expenditure-based plans.<sup>4</sup> Finally, to obtain the key regressors that we use in the analysis, we sum up all consolidation plans announced during the government term, still distinguishing between tax- and expenditure-based plans.<sup>5</sup>

To sum up, our two main austerity variables measure the budgetary impact expected over a five-year period of all new tax- and expenditure-based consolidation plans decided during the government's term. Therefore, we also consider measures that are announced and not yet implemented when voters head to the poll, which are likely to influence their preferences as much as the plans that are already implemented. The general election of 2011 in Ireland illustrates this point well. Following the international bailout in November/2010, the government had to adopt a draconian package of austerity measures to be implemented in 2011 and 2012. This contributed to a political crisis that culminated with the dissolution of the parliament as soon as the budget was approved. At

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<sup>3</sup> As in Alesina et al. (2019), we use GNP and not GDP for Ireland.

<sup>4</sup> For a more detailed discussion on this point, see Alesina et al. (2019). For alternative results using 50% or 60% thresholds see Online Appendix C.

<sup>5</sup> Aggregating all plans in the same government term implies that tax- and expenditure-based plans are not mutually exclusive in one term. This requires the additional assumption that fiscal plans are not correlated across different time periods, i.e., that a tax(expenditure)-based plan at period  $t$  predicts an expenditure(tax)-based plan at  $t+k$ , with  $t$  and  $t+k$  being in the same government term. Although the correlation between tax- and expenditure-based plans implemented in different years of the same government term is indeed low, in a robustness check we also consider alternative variables constructed dropping this assumption, therefore considering tax- and expenditure-based plans that are mutually exclusive within one term. Our results are robust to this alternative specification (see Online Appendix C).

the elections of February/2011, which were heavily centered on the recently adopted—but not yet implemented—fiscal austerity measures, the incumbent’s party suffered a historic defeat. Not accounting for the yet-to-be-implemented austerity announcement would clearly miss an important policy that influenced voters’ choices. The flipside of our approach is that we assume that the execution of austerity measures announced by a previous government does not impact the electoral outcome. Another approach would be to consider only measures that are implemented in the term, consistent with the rationale that the current government must be held responsible even for policies announced by previous governments that it did not decide to overturn. Our results are also robust to this approach (see Online Appendix C).

Basic descriptive statistics on the austerity variables as well as the other variables used in the analysis are provided in Table 1.

### **C. Electoral data**

In this section, we summarize the construction of our electoral variables. We provide more details, together with descriptive statistics, in Online Appendix B.

We start by using the electoral dataset compiled by Alesina et al. (2023) to retrieve the dates of each general election, as well as the start and end dates of each government’s term in office. For parliamentary systems—that is, all countries in our sample except France and the United States—we rely on Wikipedia and Döring and Manow (2019) to reconstruct the party or coalition of parties supporting the government. Then, since in parliamentary systems there can be multiple governments within the same legislature, we construct a “party term” variable that captures the period featuring the same governing party (or coalition of parties) within the same legislature. Governments within the same legislature that are not supported by the same parties are treated as belonging to different

party terms. In the case of presidential systems, given that the president stays on for a fixed amount of time, we simply retrieve the party to which she belongs and record the party term as the term of the president. This party term variable serves as the basis for our analysis, in that we investigate the effect of austerity policies announced during the party term on the electoral performance of the party(ies) supporting the government.

Next, we rely on the dataset of Döring and Manow (2019) to collect information on the vote share obtained by the party of the chief executive at the two elections defining the start and the end of the legislature and compute the percent change.<sup>6</sup> We also construct a variable measuring the vote share change of the overall coalition of governing parties, as well as similar other variables to be used in the robustness checks—the vote share change at the previous election, the percent change in parliamentary seats share, and the election turnout.

#### **D. Ideology and manifesto data**

We complement the dataset with political variables. First, we source a variable measuring the economic platform of the leader's party (manifesto) in the election that brought it to power from the dataset of Volkens et al. (2021). Unfortunately, this dataset does not contain direct information on promises made on taxes and expenditures, so we rely on a variable measuring the frequency of favorable mentions of free markets and free-market capitalism as an economic model. The variable may capture favorable references to the *laissez-faire* economy, the superiority of individual enterprise

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<sup>6</sup> Our approach using the vote share change improves upon earlier analyses relying on dummy variables to measure political effects of government actions. Brender and Drazen (2008) use a 0/1 dummy taking value 1 when the chief executive is re-elected and 0 otherwise. The same approach is also followed in Peltzman (1992), Brender (2003), Drazen and Eslava (2010), and Jones, Meloni and Tommasi (2012) to answer similar research questions. Using the change in the vote (or seats) share allows to more precisely quantify the electoral effects of austerity. Using a continuous rather than a dichotomous variable has also the advantage to estimate the specification using simply OLS—rather than Probit or Logit—and to include party fixed effects in the regression without sacrificing any observation.

over state and control systems, private property rights, personal enterprise and initiative, and need for unhampered individual enterprises. We normalize it to range between 0 and 10, with 0 indicating no favorable mentions of free markets. Second, from Döring and Manow (2019), we source a variable measuring the political leaning of the leader's party (political ideology) and normalize it to range between 0 and 10. This variable, which does not vary over time, takes value 0 for the most left-leaning party and higher values for more right-leaning parties.

As one may expect, the two variables are correlated. The correlation between the political ideology variable and the average value of the free-market manifesto variable at the party-level is 42%. However, this positive correlation is almost entirely driven by differences between left-of-center and right-of-center parties. Indeed, when considering only right-of-center parties, the correlation is just 15%, meaning that more right-leaning parties do not always campaign on more free-market platforms. Further, there is large variation in the extent that right-of-center parties' campaign on free-market platforms. The manifesto variable takes value below 1 in 40% of the cases of right-of-center governments, indicating little or no favorable mention at all of free markets or free-market capitalism in the incumbent party's electoral manifesto, while for about 15% of the cases the manifesto variable takes value larger than 5, indicating a fairly strong promise to adopt free-market policies (see Figure 1). We even observe considerable variation in the electoral manifesto within the same parties over time. As an example, the manifesto variable ranges between 0 and 7 for the LDP party in Japan and between 0 and 5 for the CDU party in Germany.

By contrast, left-of-center parties are much more homogenous in their economic manifestos. In an overwhelming majority of cases (almost 80%) there is little or no favorable mention at all of free markets or free-market capitalism in the incumbent party's electoral manifesto. Moreover, within the same party, we observe relatively similar manifestos over time.

## **E. Other data**

The database is complemented by other macroeconomic data that we collect to run some robustness checks and extensions. From the IMF World Economic Outlook (WEO) Spring 2019 Edition, we source real per capita GDP growth, the unemployment rate, the budget balance, the inflation rate and the 10-year real government bond yield. We source the election year and post-election year GDP growth expected at the start as well as at the end of the government term from past vintages of the IMF WEO Fall Edition.

From the OECD Statistical Database, we source data on national disposable income (in constant prices and national currency). We linearly interpolate these variables to have them at the monthly frequency and calculate (i) their value in the month of the election, (ii) their averages over the entire duration of the government term, and (iii) their change both during the last 12 months of the government and over the entire term. Further, we source indicators of economic liberalization in the areas of capital account transactions, trade tariff barriers, domestic finance and labor market from Alesina et al. (2023) and calculate their change between the first and last year of the government term.

## **F. Who Adopts What Type of Austerity and When**

This section presents a few key stylized facts on the type and timing of austerity plans. These stylized facts suggest that: (i) governments tend to avoid announcing austerity close to elections and concentrate austerity plans at the beginning of their mandate, when they have stronger political support; (ii) weak governments—perhaps recognizing their vulnerability—tend to implement less austerity than strong ones; (iii) in the first year of government, the type of austerity depends on the government's manifesto: governments that campaigned on a free-market platform implement more

expenditure-based austerity, while those that did not run on a free-market manifesto implement more tax-based austerity. In the rest of this section, we discuss these stylized facts more in detail. The uninterested reader can move directly to Section III.

We begin by exploring whether there exists a systematic pattern in the timing of austerity announcements. In Figure 2, we plot the size of the average austerity announcement in each of the first four years of the government term—for the overall consolidation effort (Panel A) and for tax- and expenditure-based announcements (Panel B).<sup>7</sup> Austerity is larger in the first than any other year of the term. This is mostly driven by tax-based consolidations, which are almost four times as large in the first as in the fourth year of the term. In contrast, expenditure-based consolidations do not follow a systematic pattern over the government term, on average.

We next explore whether the choice to carry out austerity depends on the strength of the government's mandate. For this purpose, we consider the change in the vote share obtained by the incumbent party in the election that brought it to power and distinguish among governments based on whether the change in the vote share is above or below the median. The descriptive statistics reported in Table 2 suggest that strong governments (vote share change above median) carry out austerity plans more frequently and adopt larger packages than weak ones, on average. This holds both for tax- and expenditure-based austerity and both across the entire government term and the first year in office. In the first year, when our measure of strength better captures the popularity of the government just elected, strong governments carry out austerity packages that are about 2½ times larger than those adopted by weak ones (this difference is statistically significant at the 95%

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<sup>7</sup> If the term starts in the last six months of the year, we consider the start year of the term to also include the following year. Similarly, if the term ends in the first six months of the year, we consider the last year to also include the previous one. To ensure a fair comparison, we scale each announcement in the first and last year of the term by the total number of months in those two years.

confidence interval). We further differentiate based on ideology and find that the strength of the mandate matters for both left- and right-leaning governments. However, the difference is more pronounced among the former and especially in the case of expenditure-based austerity.

Finally, in Table 3, we consider how the choice of austerity depends on the political platform adopted by the incumbent party when it ran for election. Tax-based austerity is carried out more frequently by governments that did not run on a free-market platform, while the opposite holds true for expenditure-based austerity. Zooming in on the first year of government, when platforms should be more relevant, we notice that governments that campaigned on a free-market one disproportionately opt for expenditure-based austerity, while those that did not tend to favor tax-based austerity.

Overall, these findings suggest that strategic selection might be at play: some governments recognize that austerity may be unpopular with their electoral base, and they try to announce it when they are relatively stronger.<sup>8</sup> We explicitly address this issue in Online Appendix C, where we examine the electoral effects austerity in the first year of the government term and distinguish between governments with strong and weak mandates.

### III. ECONOMETRIC SPECIFICATIONS

We regress the percent change of the vote share of the parties supporting the government onto our two austerity variables, which respectively measure the tax- and expenditure-based consolidation plans announced during the term. We include party fixed effects to limit the potential endogeneity arising from strategic selection in the announcement and composition of austerity plans depending

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<sup>8</sup> These findings are also in line with Hübscher (2016) and Hübscher and Sattler (2017). The former finds that strategic considerations play an important role in the timing and design of fiscal consolidations, while the latter conclude that electorally vulnerable governments strategically avoid consolidations towards the end of the legislative term in order to minimize electoral punishment.



on the government's political ideology. We also include GDP growth at the time of the election, as many other papers have shown this variable to be an important predictor of electoral results (for instance, see Brender and Drazen, 2008). The baseline equation is as follows:

$$\Delta Vote_{i,e} = \alpha + \gamma_{ij} + \beta Tb_{i,e} + \delta Eb_{i,e} + \vartheta Z_{i,e} + \varepsilon_{i,e} \quad (1)$$

where the subscripts  $i$  and  $e$  refer respectively to country  $i$  and election  $e$ , while the subscript  $ij$  refers to the leader's party  $j$  in country  $i$ ;  $\Delta Vote_{i,e}$  is our dependent variable, which measures the percent change of the vote share between current and previous elections of either the coalition of parties supporting the government or the party of the incumbent leader;  $\alpha$  is a constant term;  $\gamma_{ij}$  are party fixed effects;  $Tb_{i,e}$  and  $Eb_{i,e}$  respectively denote the tax- and expenditure-based austerity plans announced during the government term, in % of GDP;  $Z_{i,e}$  is a matrix of control variables capturing macroeconomic developments such as GDP growth and political factors; and finally  $\varepsilon_{i,e}$  is an error term assumed to be uncorrelated with the regressors.  $\beta$  and  $\delta$  are the main coefficients to be estimated. For the estimation, we rely on OLS. Standard errors are clustered at the party-level (robust standard errors).<sup>9</sup>

We then augment Equation (1) to investigate whether the effect of austerity depends on the manifesto of the parties implementing it. To do so, we interact our austerity measures with the economic manifesto variable and add the interaction terms to Equation (1), according to the following specification:

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<sup>9</sup> The statistical significance of the results does not change when we cluster standard errors at the country or election year level (Table C.1 in Online Appendix C).

$$\Delta Vote_{i,e} = \alpha + \gamma_{ij} + \beta Tb_{i,e} + \omega Tb_{i,e} * X_{i,e} + \delta Eb_{i,e} + \sigma Eb_{i,e} * X_{i,e} + \theta X_{i,e} + \varphi Z_{i,e} + \varepsilon_{i,e} \quad (2)$$

where  $X_{i,e}$  is the economic manifesto variable, taking larger values the more pro free-market was the electoral manifesto that brought the incumbent party to power. The rest is as in Equation (1).

We also estimate an alternative specification in which we allow for discretely different effects for free-market and not-free-market parties, by splitting them in two groups depending on whether the free-market manifesto variable is below or above the sample mean. The equation that we estimate is as follows:

$$\Delta Vote_{i,e} = \alpha + \gamma_{ij} + \beta Tb_{i,e}^{fm} + \omega Tb_{i,e}^{nfm} + \delta Eb_{i,e}^{fm} + \sigma Eb_{i,e}^{nfm} + \theta X_{i,e} + \varphi Z_{i,e} + \varepsilon_{i,e} \quad (3)$$

where the superscripts  $fm$  and  $nfm$  denote free-market and not-free-market parties respectively and the rest is as in Equation (1).

## IV. RESULTS

### A. Baseline Results

We begin by estimating a regression which includes only our tax- and expenditure-based consolidation variables. We then build upon this parsimonious specification by adding, first, the party fixed effects and, second, GDP growth at the time of the election. Other controls are added later in the robustness check exercises. Table 4 presents the estimated effects of tax- and expenditure-based

consolidations on the percent change of the vote share of both the party of the incumbent leader (Columns 1-3) and the overall coalition of governing parties (Columns 4-6).

Our main finding is that tax-based consolidations are associated with a considerable electoral fallout, while expenditure-based consolidations are not, on average. An austerity package worth 1% of GDP, carried out mostly through tax hikes, is associated with a 7% reduction of the vote share of the leader's party (Column 1). This reduction is (i) sizeable, as it corresponds to about 1/3 standard deviation of the change in the vote share (see basic descriptive statistics in Table 1), (ii) statistically significant at the 99% confidence level, (iii) robust to the inclusion of leader's party fixed effects (Column 2), and (iv) robust to controlling for GDP growth, which, as expected, has itself a significant positive effect on the vote share (Column 3). On the other hand, expenditure-based consolidations are not associated with a statistically significant change of the vote share for the party of the incumbent leader on average. Note that the coefficient of tax-based austerity is slightly diminished when we control for growth. This is consistent with the fact that tax-based austerity has more recessionary effects than expenditure-based austerity (Alesina, et al., 2015) and suggests that the response of GDP might be one channel for the effects of tax-based austerity on the incumbent's vote share, although certainly not the only one.

When we consider the entire coalition of governing parties (Columns 4-6), we estimate slightly lower (in absolute value) coefficients for tax-based consolidations, which suggests that voters hold the party of the incumbent leader as more accountable for austerity policies. To validate this intuition, in Table 5 we report results obtained estimating Equation (1) on the restricted sample of coalition governments, using as the dependent variable the percent change in the vote share of (i) the overall coalition, (ii) the party of the incumbent leader, and (iii) the rest of the governing parties. Estimates for the overall coalition and the main party are in line with those of Table 4, while those

for the rest of the coalition are not significantly different from zero. These results are consistent with prior findings in the literature on clarity of responsibility (Powell and Whitten, 1993). To close this analysis on coalition governments, we also consider the importance of the party affiliation of the minister of finance. We find that the negative association between tax-based austerity and the electoral outcomes of the party of the incumbent leader is considerably stronger in cases in which the minister of finance belongs to the same party than when he/she is affiliated to a different one (Column 2 of Table 6). In the remainder of the paper, we focus exclusively on the party of the incumbent leader and build upon Column 3 of Table 4 as our baseline specification.

We assess the sensitivity of our baseline results to a battery of different specifications, which we discuss in detail in Online Appendix C (Tables C1-C8). Our results (i) are not driven by a single political party or country; (ii) are robust to using alternative variables to measure electoral performance and fiscal austerity; (iii) are robust to the inclusion of several political, structural and macroeconomic control variables, including the level and the change of the 10-year real yield as well as of the fiscal balance and the debt, which account for the fact that austerity might be announced in response to market pressures; and (iv) are broadly similar across different country samples. Further, we show that the statistical significance is not affected by the way we compute the standard errors.

We also try to address potential sources of endogeneity. Some politicians might decide not to implement austerity because they believe that this might cost them reelection. If this were to be the case, the electoral effects of austerity policies would be estimated with a bias. To address this and other potential sources of endogeneity, we (i) focus on the sample of consolidation plans that are announced in the first year of government, when strategic considerations are less of a concern; (ii) control for the strength of the government (measured through the electoral performance of the leader's party in the election that brought it into power); (iii) control for the choice of whether to introduce

austerity; and (iv) use the augmented inverse probability weighting (AIPW) estimation proposed by Jordà and Taylor (2016). If anything, the results from these alternative specifications tend to suggest that our OLS baseline estimates are likely to underestimate the “true” negative electoral effects of tax-based austerity (Online Appendix C, Tables C9-C13).<sup>10</sup>

Overall, our results depart from many existing studies, which have not found evidence that austerity is politically costly. As we show in Online Appendix D, both the use of *ex-ante* austerity data and the matching of austerity policies to the actual government announcing them are key to correctly identify the political effects of austerity.

Before proceeding to the importance of electoral manifesto, we carry out a few additional exercises. First, we consider possible time variation in our coefficients. Covering the 1978-2014 period, our time sample is large, and the way in which voters receive austerity policies may have changed over time. Of particular interest are the austerity packages decided to reduce the increase in public debts after the Global Financial Crisis (GFC), with the conventional wisdom holding this post-GFC austerity as being especially costly in electoral terms. Unfortunately, we are not able to convincingly analyze the impact of such packages, as these were decided in 2011 or later and we only observe two party terms which start in 2011 or later and end in 2014 or earlier (with 2014 being the last year for which the narrative austerity data is available). We instead divide the sample in two parts with roughly the same number of observations and analyze whether the estimated coefficients are different for party terms that started in the 1994-2014 period versus those that started in the earlier 1978-1994 period. We estimate a somewhat larger negative coefficient for tax-based consolidations

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<sup>10</sup> Another potential source of endogeneity that would bias our results due to attrition within the panel is given by the event that incumbent parties do not run at the next elections because they worry that their voters would punish them in response to austerity. However, this potential issue is very marginal in practice since this event only occurred one time in our sample (the Amato I cabinet in Italy in 1992-1993).

in the later period (-6.2% versus -5.4%), while that for expenditure-based consolidations is again statistically indistinguishable from zero in both cases (Colum 3 of Table 6). These findings cautiously suggest that the cost of tax-based austerity may have increased over time. A more thorough analysis of the post-GFC period is however warranted.

Second, we note that some austerity episodes might be imposed on governments by external forces. For instance, sometimes governments implement austerity as part of macroeconomic restructuring programs agreed with international institutions. These episodes are less endogenous to electoral considerations: therefore, focusing on them may be a good way to further address endogeneity issues. On the other hand, voters might hold the executive as less responsible for austerity measures when these are mandated by other institutions.<sup>11</sup> We focus on programs adopted in EU countries in response to an Excessive Deficit Procedure (EDP) initiated on them by the European Commission.<sup>12</sup> Column 4 of Table 6 reports coefficients derived from an alternative specification to our baseline in which we let tax-based and expenditure-based consolidation packages to have different effects based on whether they were implemented in response to an EDP, estimated on the restricted sample of EU countries. The coefficient for tax-based consolidations implemented in response to an EDP is smaller than that estimated for other tax-based consolidations. Both expenditure-based coefficients are not statistically different from zero, in line with our baseline results. These results provide suggestive evidence that voters might hold the government as less

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<sup>11</sup> IMF programs offer perhaps the best examples of externally imposed austerity measures. Our sample does not contain any such program, however.

<sup>12</sup> The EDP is an injunction made by EU institutions to a country's government to commit to a path of cyclically adjusted fiscal deficit reductions over a few years in order to bring fiscal deficit below the threshold of 3% of GDP. While this path can be thought of as imposed by EU institutions, albeit it is often subject of negotiations between EU and the country in question, the composition of the negotiated adjustment is typically left to the discretion of country's governments. Thus, voters may still judge the government as responsible for the type of adjustment.

accountable for carrying out austerity when this is done in response to pressures from external bodies. However, these results should be interpreted with caution due to the limited sample size of tax- and expenditure-based austerity implemented as part of the EDP in our sample (respectively 8 and 11).

Next, we investigate whether austerity may have any bearing on the probability that the government falls before the natural end of its mandate. In parliamentary systems, governments can be voted out of office through a vote of non-confidence by the parliament, and indeed the average length of the party term is well below the statutory term of the legislature, indicating that governments tend to fall earlier than the natural end of their term. Here, we analyze whether one reason for governments to fall early is the adoption of austerity policies. We construct an early-fall dummy variable, taking value 1 when the government falls before the natural end of the legislature and 0 otherwise, and estimate a logistic regression model using our tax- and expenditure-based consolidation variables plus GDP growth in the last 12 months of the government term.<sup>13</sup> Leader's party fixed effects cannot be included since some of them would perfectly predict the dependent variable for some parties, thus reducing the sample. The specification is as follows:

$$EarlyFall_{i,e} = \alpha + \beta Tb_{i,e} + \delta Eb_{i,e} + \varphi Z_{i,e} + \varepsilon_{i,e} \quad (4)$$

where  $EarlyFall_{i,e}$  is the dummy capturing instances in which the government falls before the end of the legislature and the rest is as in Equation (1). Column 4 of Table 6 reports the results—coefficients are normalized so to express marginal probabilities.

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<sup>13</sup> To construct the early-fall dummy variable, we do not consider as an early fall instances in which the government ends within the last six months of the legislature, as those cases might be the result of strategic considerations rather than of the genuine fall of support of the government.

Broadly in line with our results on the vote share, we find that tax-based consolidations are associated with a higher probability that the government falls before the natural end of the legislature, whereas expenditure-based consolidations are not, on average. An austerity package worth 1% of GDP, to be achieved mainly through tax hikes, is associated with an 8% increase in the probability of an early government fall. This coefficient is statistically significant at the 90% confidence level. The main take-away from this analysis is that tax-based consolidations not only decrease the vote share of the leader's party but also reduce its spell in government. An early government fall happens usually because one of the parties pulls out from the coalition: hence these results also provide suggestive evidence as to why only the party of the incumbent leader sees a reduction of the vote share after tax-based austerity. The junior parties in the coalition may react to austerity policies by pulling out from the government when they see that austerity is badly received by the public, thus leaving the leader's party as mainly responsible and limiting their own electoral fallout.

## **B. The Importance of Economic Manifesto**

Partisan models of political economy hold that voters weigh key macroeconomic variables differently according to their preferences. Crucially, these preferences also matter for the desired size of governments. Political platforms put forward in the 2019 general election in the United Kingdom offer a valid example, with the right-leaning Conservative Party pledging no tax hikes in its manifesto and the left-leaning Labour Party vowing big expenditure and tax increases. In this section, we investigate whether the electoral effects of different austerity policies depend on the economic ideology of the parties implementing them.



To do so, we estimate Equation (2), where we interact our tax- and expenditure-based austerity variables with the free-market manifesto variable. This variable ranges between 0 and 10, with higher values indicating more frequent favorable mentions of free markets in the incumbent's party manifesto during the campaign preceding the election that brought her to power. We also estimate an alternative specification where we split parties in two groups depending on whether the manifesto variable takes value below or above the sample mean, as in Equation (3). Table 8 presents the results. Column 1 shows our baseline results not accounting for economic ideology, while Columns 2 and 3 report the newly estimated coefficients, accounting for it. The results confirm our hypothesis that the more pro-market a government is the more it is penalized from tax hikes, and the less it is penalized (and it may even be rewarded) from expenditure cuts. The coefficient of the tax-based consolidation variable is approximately -4.4%, while the interaction term is about -2.6% and statistically significant. This suggests that tax-based austerity is always detrimental but has stronger negative effects for parties who previously campaigned in favor of free-market capitalism. In contrast, the coefficient of the expenditure-based consolidation variable is about -2.5%, while that of the interaction term is about 1.6% (both statistically significant). This suggests that expenditure cuts are costly for not-pro-market parties, but not for more centrist parties, while they can be even beneficial for more pro-market ones.

The patterns just discussed are broadly confirmed when we allow for discretely different effects of tax-based consolidations for not-pro-market and pro-market parties. A tax-based austerity package worth 1% of GDP announced by a pro-market government is associated with a reduction of the vote share of the incumbent's party of almost 22%, while the coefficient estimated for the average not-pro-market government is about one fourth of it. An expenditure-based austerity package worth 1% of GDP is associated with a 1.5% reduction of the vote share of the incumbent party on average in the case of not-pro-market governments (although not statistically significantly), and to 7.6%

increase on average in the case of pro-market governments (statistically significant at the 95% confidence level).

Next, we examine whether the electoral cost of a tax-based consolidation carried out by a pro-market government is significantly larger than the cost of an expenditure-based consolidation carried out by a not-pro-market government, as suggested by the point estimates (almost -22% and -1.5% respectively). An F-test confirms that the difference of these electoral effects is statistically significant at the 99% confidence level. This means that, on average, tax hikes have larger electoral costs than expenditure cuts, even after accounting for the economic ideology of the government, potentially reflecting the fact that voters generally dislike paying higher taxes.

Our empirical evidence suggests that when governments deviate from what promised during the campaign trail they may be punished at the polls: we find tax(expenditure)-based consolidations to be associated with larger electoral losses the more (less) the governing parties had campaigned on a free-market agenda. But why would politicians promise something during the campaign trail only to then deviate when they are in government, at the risk of being punished at the polls and losing power? In the working paper version of this paper (Alesina et al., 2024), we develop a stylized theoretical model featuring polarized voter mobilizers, infinitely repeated elections and asymmetric information, and we show that these three ingredients are enough to explain the empirical evidence.

In Table 9 we replicate the analysis done using the free-market manifesto variable with a time-invariant variable measuring political ideology (also bounded between 0 and 10 and with higher values denoting a more right-wing political ideology). Broadly, the results are similar to the specification using free-market manifesto, with the interaction between tax-based austerity and political ideology being negative and significant and that between expenditure-based austerity and

political ideology being positive and significant. However, the coefficients we found using our preferred free-market manifesto variable are larger.

## V. EXTENSIONS

In this section we explore the role played by: (i) the response of the economy to austerity; and (ii) the economic conditions at the time of announcement as a potential channel driving the electoral effects of austerity.

### A. The Response of the Economy to Austerity

One potential reason for the different electoral consequences of tax- and expenditure-based consolidations could be their different effect on the economy. Indeed, using the same narrative austerity data that we use, a large literature finds tax-based consolidations to induce large and relatively persistent output losses, while expenditure-based consolidations are usually found to have only mild, if any, contractionary effects (see Alesina et al., 2015, among others). To the extent that voters care about macroeconomic performance and income more in general, this differential response might explain why tax-based austerity is more politically costly on average.

Thus, we augment the baseline model (Equation 1) with indicators of macroeconomic performance during the government term to investigate the effect of austerity controlling for the response of the economy, essentially “shutting down” this potential transmission channel. We consider the average growth rate of GDP and disposable income over the term, as well as the mean yearly change of the unemployment rate, the inflation rate, and the budget balance over the term.

Table 10 presents the results. For ease of reference, the baseline results, as in Column 3 of Table 4, are reported in Column 1.

The coefficients estimated for the control variables have the expected signs—positive for GDP and disposable income growth as well as the fiscal balance and negative for unemployment and inflation—and they are all statistically significant at standard confidence levels except that for inflation. Looking at tax- and expenditure-based consolidations, their estimated coefficients are not substantially changed. The inclusion of the fiscal balance over the term (Column 5) and of all the macroeconomic variables together (Column 7) only slightly reduces the coefficient of tax-based consolidations.

Because austerity may impact both current and expected future GDP growth, we complement the analysis above by investigating the role of growth expectations, using data on GDP forecast contained in the Fall Edition of the IMF WEO. As a start, we control for electoral-year expected growth, as data on realized GDP growth is released only with a lag. We then control for (i) the change in electoral GDP forecast over the term (difference between the forecast for election year GDP growth made in the election year itself and in the first year of the government), (ii) GDP growth expected for the year following the election (post-electoral-year GDP growth), and (iii) the change in post-electoral-year GDP growth over the term. The substantially reduced sample notwithstanding, the results are qualitatively similar and not statistically different from our baseline (Table 10). We further find the level as well as the change over the term of expected growth to be weakly positively associated with an increase in the vote share.

All in all, these results suggest that the response of the economy explains only partially the electoral effects of tax-based consolidations and political economy considerations may be more important, as suggested by the analysis above.

## B. Macroeconomic Conditions at the Time of Announcements

In a second extension, we investigate whether the electoral effects of austerity policies are affected by the macroeconomic conditions prevailing at the time of the announcement. If voters are credit-constrained, they might punish a tax hike, which reduces disposable income, particularly strongly when income is lower to start with, as in a recession. Drawing from previous literature studying whether austerity tends to have different effects on output growth depending on the state of the business cycle (Auerbach and Gorodnichenko, 2012; Ramey and Zubairy, 2018; Alesina et al., 2019), we test the hypothesis that austerity is better received when it is carried out in good economic times. Since, all else equal, higher taxes and lower government expenditures might decrease disposable incomes, it is reasonable to expect austerity to be more accepted when growth is high, and incomes are rising. Moreover, as discussed in Alesina et al. (2023), voters may not be able to distinguish between underlying economic conditions and the effect of policies on economic conditions, therefore penalizing (rewarding) any policy choice during recessions (booms).

The strategy that we follow is to allow for the effects of the fiscal plans adopted during the government term to depend on whether each of these plans was adopted in a boom or in a downturn. In particular, following Auerbach and Gorodnichenko (2012), we first transform standardized output growth,  $Z$ , into a variable  $G(Z)$  ranging between 0 and 1 and increasing in  $Z$ .  $G$  is defined to be the following logistic function:  $G(Z_{i,t}) = \frac{\exp(-\gamma Z_{i,t})}{1 + \exp(-\gamma Z_{i,t})}$ , where the subscripts  $i$  and  $t$  refer to country  $i$  and year  $t$ ;  $Z_{i,t}$  is the real GDP growth rate, normalized to have zero mean and unit variance within country; and  $\gamma$  is a smoothing parameter taking value 1.5. We then interact  $G(Z_{i,t})$  and  $1 - G(Z_{i,t})$  with the yearly austerity variables and take the sum of the resulting products over each year of the government term. We label the variables obtained in this way with the superscript  $L$  and  $H$ , to denote

austerity announced during periods of low and high economic growth. We then estimate the following specification:

$$\Delta Vote_{i,e} = \alpha + \gamma_{ij} + \mu Tb_{i,e}^L + \rho Tb_{i,e}^H + \theta Eb_{i,e}^L + \varphi Eb_{i,e}^H + \tau Z_{i,e}^H + \omega Z_{i,e}^L + \varepsilon_{i,e} \quad (5)$$

Table 11 reports the results. Column 1 shows the baseline results (Equation 1), while Column 2 lists the coefficients obtained estimating Equation (5). Column 3 reports coefficients obtained replacing the  $G(Z_{i,t})$  logistic function with a simple dummy taking value 1 if growth in the year preceding the austerity announcement was below zero. The results offer two main insights. First, austerity policies, and particularly tax increases, have significantly more negative effects on the vote share when carried out during bad economic times. Second, the government may increase the vote share by announcing expenditure-based austerity during good economic times.<sup>14</sup>

## VI. CONCLUSION

The electoral effects of fiscal policy choices are difficult to measure. We make progress by using new data that allow us to precisely match the announcement of an austerity plan with the government introducing it, and to select only measures that are discretionary and not motivated by the state of the economy, which can be critical for voters' decisions. This approach allows us to obtain much more clear-cut evidence on the electoral consequences of fiscal austerity relative to previous literature.

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<sup>14</sup> The coefficient that we estimate for expenditure cuts during good economic times is positive but not statistically significant. However, it should be borne in mind that what we estimate is the average effect, not accounting for ideology and manifesto, which also play a role in shaping the electoral effects of expenditure cuts, as discussed above.

Our main result strongly suggests that tax- and expenditure-based austerity might have very different electoral consequences. A tax-based austerity package worth 1% of GDP is associated with a 7% decline of the vote share of the incumbent party, while we estimate an insignificant coefficient for the average expenditure-based package. This result holds even after controlling for the more negative effects on growth of tax- relative to expenditure-based austerity. Endogeneity is a concern, as governments may strategically refrain from carrying out austerity when they perceive that it might cost them votes. We perform several exercises that seem to confirm this possibility. However, these exercises suggest that the true electoral cost of tax-based austerity might be even larger than what we estimate.

Our second set of results uncovers an important role of ideology and economic manifesto in shaping the electoral consequences of austerity policies. The negative effect of tax-based consolidations is much stronger for government parties that campaigned on a free-market manifesto, while expenditure-based consolidations are costly for parties that did not campaign on a free-market manifesto but are beneficial for those that did so. Similarly, we find evidence that the effect of tax (spending)-based consolidations is stronger for more right (left)-wing governments.

We also find that the electoral costs of fiscal austerity are larger when tax hikes or expenditure cuts are introduced during periods of weak economic activity. In contrast, fiscal austerity is not typically costly, and can be rewarded by voters, if implemented during times of strong economic conditions.

Overall, our results suggest that fiscal consolidation can be costly but not always. Political economic considerations related to the “how” austerity is carried out, by “whom” and “when” need to be internalized in order to assess the electoral consequences of fiscal austerity.

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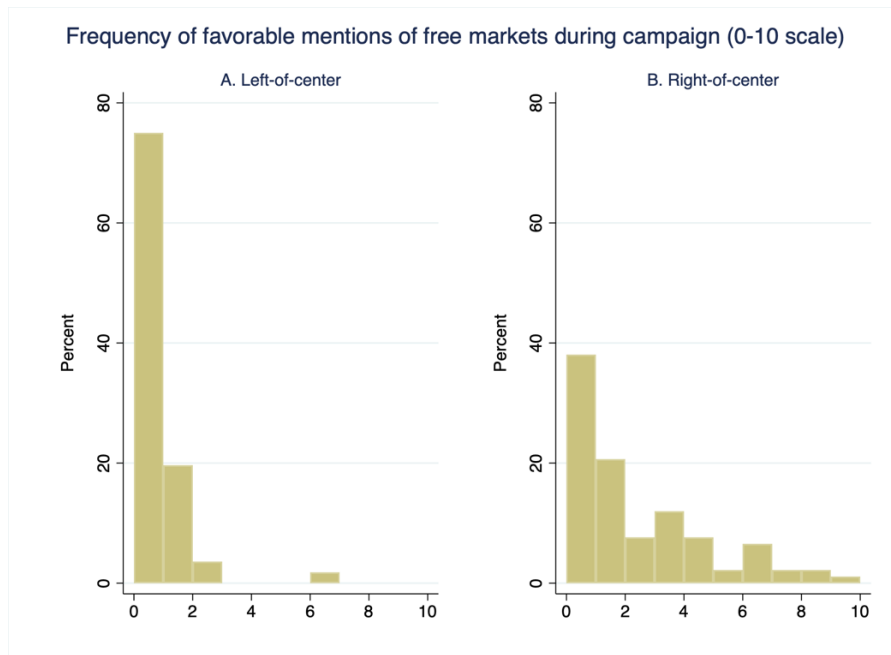
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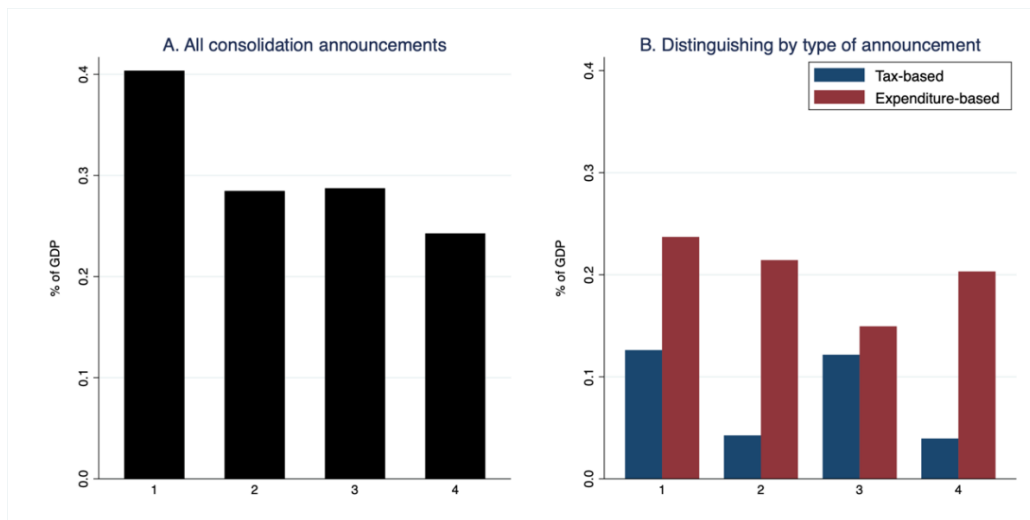
## FIGURES

**Figure 1. Frequency of favorable mentions of free markets during electoral campaign**



*Notes:* the figure shows the distribution of the free-market manifesto variable for left-of-center and right-of-center government parties. The variable captures the frequency of favorable mentions of free markets during the electoral campaign preceding the election that brought the party into power on a 0-10 scale.

**Figure 2. The electoral cycle of austerity**



*Notes:* the figure plots the value of the full sample average consolidation announcement observed over the government term (measured in percent of GDP). Panel A reports values for all consolidation announcements, while Panel B distinguishes between tax- (red bars) and expenditure-based announcements (green bars). The y-axis denotes the value of the announcement, while the x-axis denotes the timing relative to the election year.

## TABLES

**Table 1. Basic descriptive statistics**

	<b>Obs.</b>	<b>Mean</b>	<b>S.d.</b>	<b>Min.</b>	<b>Max.</b>
<b><math>\Delta</math>Vote party</b>	158	-6.85	19.48	-80.54	67.56
<b><math>\Delta</math>vote coalition</b>	150	-7.65	17.37	-80.54	67.56
<b>Overall austerity</b>	158	1.10	1.83	0.00	13.85
<b>Tax-based austerity</b>	158	0.29	0.76	-0.26	6.39
<b>Expenditure-based austerity</b>	158	0.72	1.44	0.00	8.50
<b>Tax-based austerity – first year</b>	158	0.13	0.44	-0.28	2.74
<b>Expenditure-based austerity – first year</b>	158	0.24	0.79	-0.35	5.63
<b>Growth electoral year</b>	157	1.92	2.00	-4.70	9.59
<b>Free-market manifesto</b>	149	1.72	2.17	0.00	10.00

*Notes:* the table reports basic descriptive statistics of the main dependent and explanatory variables used in the analysis.  $\Delta$ vote party (coalition) is the % change of the share of the party (coalition) of the incumbent leader. Austerity variables are expressed in % of GDP.

**Table 2. Government mandate and austerity plans**

	<b>Overall</b>		<b>Tax</b>		<b>Expenditure</b>	
	<b>Strong</b>	<b>Weak</b>	<b>Strong</b>	<b>Weak</b>	<b>Strong</b>	<b>Weak</b>
<b>% terms w. Austerity</b>	55.84	50.65	32.47	28.57	38.96	35.06
<b>Total</b>	1.24	0.99	0.30	0.30	0.85	0.61
<b>First year</b>	0.59**	0.24**	0.19*	0.06*	0.39**	0.10**
<b>First year – left-of-center</b>	0.99**	0.21**	0.22**	0.03**	0.78**	0.05**
<b>First year – right-of-center</b>	0.46	0.23	0.18	0.08	0.21	0.12

*Notes:* the table reports values of the average austerity announcement (in % of GDP) made by governments with a ‘weak’ and a ‘strong’ mandate, distinguishing by the type of consolidation announced (whether tax- or expenditure-based), the ideology of the party of the incumbent leader (whether left- or right-leaning), and the time of the announcement (whether in the first year or across the all sample). Governments with a ‘strong’ (‘weak’) mandate are defined as those in which the change in the vote share of the main party at the election that brought the government into power is above (equal to or below) the sample median. \*, \*\*, \*\*\* indicate that the mean between government with a ‘strong’ and ‘weak’ mandate are statistically different from each other at the 90%, 95% and 99% confidence level, respectively.

**Table 3. Government manifesto and austerity plans**

	<b>Overall</b>		<b>Tax</b>		<b>Expenditure</b>	
	<b>FM</b>	<b>Not FM</b>	<b>FM</b>	<b>Not FM</b>	<b>FM</b>	<b>Not FM</b>
<b>% terms w. Austerity</b>	54.35	50.00	23.91	32.35	47.83	30.39
<b>Total</b>	0.90	1.15	0.16*	0.36*	0.68	0.72
<b>First year</b>	0.38	0.40	0.07	0.15	0.31	0.18
<b>First year – left-of-center</b>	0.79	0.47	0.20	0.10	0.59	0.30
<b>First year – right-of-center</b>	0.33	0.32	0.05*	0.21*	0.27**	0.06**

*Notes:* the table reports values of the average austerity announcement (in % of GDP) made by governments that campaigned on a free-market (FM) and not-free-market (Not FM) manifesto, distinguishing by the type of consolidation announced (whether tax- or expenditure-based), the ideology of the party of the incumbent leader (whether left- or right-leaning), and the time of the announcement (whether in the first year or across the all sample). Governments that campaigned on a free-market manifesto are those defined as having a value larger than 1 in the manifesto variable. \*, \*\*, \*\*\* indicate that the mean between government with a ‘strong’ and ‘weak’ mandate are statistically different from each other at the 90%, 95% and 99% confidence level, respectively.

**Table 4. Austerity and electoral outcomes – main results**

	(1) Party	(2) Party	(3) Party	(4) Coalition	(5) Coalition	(6) Coalition
Tax	-7.0*** (1.6)	-8.2*** (1.4)	-7.3*** (1.3)	-6.3*** (1.3)	-6.7*** (1.3)	-5.9*** (1.2)
Expenditure	-0.0 (1.4)	0.7 (1.6)	0.2 (1.5)	-0.9 (0.8)	-0.7 (0.8)	-1.3 (0.8)
Growth			2.8*** (0.9)			2.6*** (0.9)
Obs.	158	158	157	149	149	148
R <sup>2</sup>	0.07	0.12	0.20	0.10	0.14	0.24
Party FE	NO	YES	YES	NO	YES	YES

*Notes:* “tax” and “expenditure” report the % change of the vote share of either the governing party (Columns 1-3) or the coalition of governing parties (Columns 4-6) associated with a tax- and expenditure-based consolidation announcement worth 1% of GDP, respectively, estimated from Equation (1). “Growth” reports the change of the vote share associated with a 1 p.p. increase in the growth rate in the year of the election. \*, \*\*, \*\*\* indicate statistical significance at respectively the 90%, 95% and 99% confidence level, based on robust standard errors (in parenthesis). Specifications in Columns 1 and 4 do not include incumbent party fixed effects, while all the others do.

**Table 5. Austerity and electoral outcomes – coalition governments**

	(1) Overall coalition	(2) Main party	(3) Rest of coalition
Tax	-6.4*** (1.8)	-8.7*** (2.2)	-0.4 (4.6)
Expenditure	-0.1 (0.7)	2.0 (1.9)	-3.0 (1.9)
Growth	1.8* (0.9)	2.4** (1.1)	0.7 (1.6)
Observations	76	76	76
R-squared	0.40	0.31	0.06
Party FE	YES	YES	YES

*Notes:* “tax” and “expenditure” report the % change of the vote share of either the entire government coalition (Column 1), the party of the incumbent leader (Column 2) or the other parties in the coalition (Column 3) associated with a tax- and expenditure-based consolidation announcement worth 1% of GDP, respectively, estimated from Equation (1). “Growth” reports the change of the vote share associated with a 1 p.p. increase in the growth rate in the year of the election. \*, \*\*, \*\*\* indicate statistical significance at respectively the 90%, 95% and 99% confidence level, based on robust standard errors (in parenthesis). All specifications include incumbent party fixed effects.

**Table 6. Austerity and electoral outcomes – additional results**

	(1)	(2)	(3)	(4)	(4)
	Baseline analysis	Finance minister	Time sample	EDP consolidation	Government falls
Tax	-7.3*** (1.3)				0.08* (0.05)
Tax – finance minister same party		-8.2*** (2.7)			
Tax – finance minister different party		-2.5 (2.3)			
Tax – pre-1994 sample			-5.3** (2.5)		
Tax – post-1994 sample			-7.5*** (2.4)		
Tax – EDP				-1.8 (6.9)	
Tax – not EDP				-5.4** (2.5)	
Expenditure	0.2 (1.5)				-0.02 (0.03)
Expenditure – finance minister same party		-0.4 (2.2)			
Expenditure – finance minister different party		3.2 (2.5)			
Expenditure – pre-1994 sample			1.7 (2.0)		
Expenditure – post-1994 sample			-1.2 (2.0)		
Expenditure – EDP				-5.0 (5.9)	
Expenditure – not EDP				0.1 (1.4)	
Growth	2.8*** (0.9)	2.5* (1.2)	2.6*** (0.9)	2.4 (1.6)	-0.04** (0.02)
Observations	157	76	157	82	195
R-squared	0.20	0.36	0.22	0.23	
Party FE	YES	YES	YES	YES	NO
Pseudo R-squared					0.04

*Notes:* Column 1 reports baseline results. Column 2 reports estimates from a different specification in which the austerity variables are interacted with dummies to indicate cases in which the finance minister was (not) affiliated to the same party of the government leader. Column 3 reports estimates from a different specification in which the austerity variables are interacted with dummies to indicate cases in which the first year of the party term was before/after 1994. Column 4 reports estimates from a different specification in which the austerity variables are interacted with dummies to indicate cases in which the consolidation was adopted in response to an Excessive Deficit Procedure. Column 5 reports estimates on the association between austerity announcements and the percent probability of an early fall of government, estimated from Equation (4). \*, \*\*, \*\*\* indicate statistical significance at respectively the 90%, 95% and 99% confidence level, based on robust standard errors (in parenthesis).

**Table 7. Austerity and electoral outcomes – accounting for electoral manifesto**

	(1) No manifesto	(2) Interaction	(3) Dummy
Tax	-7.3*** (1.3)	-4.4*** (1.4)	
Tax * free-market manifesto		-2.6* (1.4)	
Tax * (1 – dummy free-market manifesto)			-5.5*** (1.1)
Tax * dummy free-market manifesto			-19.9*** (7.3)
Expenditure	0.2 (1.5)	-2.5** (1.2)	
Expenditure * free-market manifesto		1.6*** (0.3)	
Expenditure * (1 – dummy free-market manifesto)			-0.5 (1.2)
Expenditure * dummy free-market manifesto			1.1 (3.2)
Free-market manifesto		-0.3 (0.7)	0.9 (0.8)
Growth	2.8*** (0.9)	2.4*** (0.8)	2.5*** (0.9)
Observations	157	148	148
R-squared	0.20	0.33	0.23
Tax * dummy free = Exp * (1 – dummy free)			0.01
Party FE	YES	YES	YES

*Notes:* the table reports estimates on the association between austerity announcements and the electoral fortunes of the main party of the government introducing them, depending on its electoral manifesto. Column 1 reports baseline coefficients, not controlling for the electoral manifesto, and estimated from Equation (1). Columns 2 and 3 report estimates when accounting for electoral manifesto, obtained from Equations (2) and (3) respectively. Electoral manifesto is measured through a continuous variable (“free-market manifesto”), ranging from 0 to 10 and taking more positive values the more frequent are favorable mentions to free markets in the manifesto of the party of the incumbent leader in the election that brought it to power. The “dummy free-market manifesto” variable takes value equal to 1 for observations in which the free-market manifesto variable takes value above the mean. \*, \*\*, \*\*\* indicate statistical significance at respectively the 90%, 95% and 99% confidence level, based on robust standard errors (in parenthesis). The row “F-test Tax \* dummy free = Exp \* (1 – dummy free)” reports the p-value from the following F-test: Tax \* dummy free-market manifesto = Expenditure \* (1 – dummy free-market manifesto). All specifications include incumbent party fixed effects.



**Table 8. Austerity and electoral outcomes – accounting for political ideology**

	(1) No ideology	(2) Interaction	(3) Dummies
Tax	-7.3*** (1.3)	0.4 (3.2)	
Tax * right-wing ideology		-1.6** (0.6)	
Tax * dummy left-of-center			-4.4 (3.3)
Tax * dummy right-of-center			-9.0*** (1.5)
Expenditure	0.2 (1.5)	-5.2** (2.2)	
Expenditure * right-wing ideology		1.2* (0.6)	
Expenditure * dummy left-of-center			-3.2*** (1.1)
Expenditure * dummy right-of-center			1.9 (2.0)
Growth	2.8*** (0.9)	2.7*** (0.9)	2.7*** (0.9)
Observations	157	156	156
R-squared	0.20	0.26	0.24
Party FE	YES	YES	YES

*Notes:* the table reports estimates on the association between austerity announcements and electoral outcomes depending on the political leaning of the governing party. Column 1 reports coefficients as estimated from Equation (1). Column 2 reports coefficients estimated from an alternative specification of Equation (2), where the free-market manifesto variable is replaced with a right-wing ideology variable, spanning from 0 to 10 and (higher values more right-wing ideology). Column 3 reports coefficient estimated from Equation (3), where parties are divided in two groups based on the ideology rather than free-market manifesto variable. Left-of-center (right-of-center) parties are defined as those for which the ideology variable takes values below (above) 5, on a 0-10 scale. \*, \*\*, \*\*\* indicate statistical significance at respectively the 90%, 95% and 99% confidence level, based on robust standard errors (in parenthesis). All specifications include incumbent party fixed effects.

**Table 9. Austerity and electoral outcomes – economy response as driver**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Growth EY (baseline)	Growth over term	U. change over term	CPI change over term	Budget change over term	Disposable income growth	All
Tax	-8.5*** (1.9)	-8.3*** (1.9)	-6.5*** (1.7)	-8.8*** (1.9)	-5.7*** (1.7)	-8.2*** (1.9)	-5.7*** (1.5)
Expenditure	1.0 (1.7)	1.2 (1.7)	1.7 (1.7)	0.8 (1.6)	0.5 (1.5)	1.3 (1.8)	1.0 (1.3)
Growth		2.2 (1.6)					5.6* (2.8)
Unemployment			-6.7*** (2.3)				-4.2* (2.4)
Inflation				-1.7 (1.0)			-4.0** (1.8)
Fiscal budget					4.2*** (1.4)		2.6 (1.7)
Disposable income						2.6 (1.8)	-1.6 (2.7)
Observations	144	144	144	144	144	144	144
R-squared	0.11	0.13	0.18	0.12	0.17	0.13	0.25
Party FE	YES	YES	YES	YES	YES	YES	YES

*Notes:* the table reports the association between austerity announcements and the electoral fortunes of the main governing party of the government introducing them, controlling for macroeconomic developments over the government term. All coefficients are obtained estimating Equation (1). Column 1 reports estimates when not controlling for the response of the economy. Columns 2 to 5 report estimates obtained when controlling for, in turn, (i) average GDP growth over government term, (ii) the average unemployment rate over the term, (iii) the average inflation rate over the term, and (iv) the average value of the government budget balance over the term. Column 6 reports estimates when all controls are included at the same time. All coefficients report the % change in the vote share of the party of the incumbent leader associated with the respective variable considered. In the case of “tax” and “expenditure” these are a tax- and expenditure-based consolidation announcement worth 1% of GDP. \*, \*\*, \*\*\* indicate statistical significance at respectively the 90%, 95% and 99% confidence level, based on robust standard errors (in parenthesis). All specifications include incumbent party fixed effects.

**Table 10. Austerity and electoral outcomes – changes in expected growth as driver**

	(1)	(2)	(3)	(4)	(5)	(6)
	Growth EY (baseline)	Growth EY (restricted)	EY expected growth	Change EY expected growth	Post-EY expected growth	Change post- EY expected growth
Tax	-7.3*** (1.3)	-8.4*** (3.0)	-8.9*** (2.7)	-9.4** (4.4)	-8.4** (3.3)	-10.3** (4.4)
Expenditure	0.2 (1.5)	-1.2 (2.4)	-0.2 (2.2)	1.4 (4.0)	-0.3 (2.2)	2.4 (3.9)
Growth electoral year	2.8*** (0.9)	2.7* (1.4)				
Electoral year expected growth			0.9 (1.1)			
Change in electoral year expected growth				1.4 (1.3)		
Post-electoral year expected growth					3.4 (2.1)	
Change post-electoral year expected growth						6.0** (2.8)
Observations	157	81	81	68	81	67
R-squared	0.20	0.27	0.22	0.26	0.24	0.32
Party FE	YES	YES	YES	YES	YES	YES

*Notes:* the table reports associations between austerity announcements and electoral outcomes controlling for (changes in) expected GDP forecast. All coefficients are obtained estimating Equation (1). Column 1 reports estimates when not controlling for (changes in) expected GDP forecast estimated on the full sample (baseline specification). Column 2 reports estimates when not controlling for (changes in) expected GDP forecast estimated on the restricted sample for which forecast are available. Columns 3-6 report estimates obtained, respectively, controlling for electoral year GDP forecast, the change of electoral year GDP forecast over the government term, the post-electoral year GDP forecast and the change of post-electoral year GDP forecast over the government term. All coefficients report the % change in the vote share of the party of the incumbent leader associated with the respective variable considered. In the case of “tax” and “expenditure” these are a tax- and expenditure-based consolidation announcement worth 1% of GDP. \*, \*\*, \*\*\* indicate statistical significance at respectively the 90%, 95% and 99% confidence level, based on robust standard errors (in parenthesis). All specifications include incumbent party fixed effects.

**Table 11. Austerity and electoral outcomes – role of macroeconomic conditions**

	(1) Baseline	(2) Logistic function	(3) Recession dummy
Tax	-7.3*** (1.3)		
Tax – high state		-8.1 (5.9)	
Tax – low state		-6.5*** (1.6)	
Tax – expansion			-5.9** (2.4)
Tax – recession			-9.1*** (1.7)
Expenditure	0.2 (1.5)		
Expenditure – high state		4.1 (6.5)	
Expenditure – low state		-3.1 (4.1)	
Expenditure – expansion			1.2 (2.4)
Expenditure – recession			-1.0 (1.4)
Growth	2.8*** (0.9)		
Growth – high state		3.0 (1.8)	
Growth – low state		1.8 (1.8)	
Growth - expansion			2.6* (1.4)
Growth - recession			3.1 (2.5)
Observations	157	157	156
R-squared	0.20	0.22	0.21
Party FE	YES	YES	YES

*Notes:* the table reports results on the role of macroeconomic conditions at the time of austerity announcements. Column 1 reports baseline coefficients (as in Column 1 of Table 3). Column 2 reports estimates when accounting for the economic cycle, obtained from Equation (5). The state of the cycle is measured through a continuous 0-1 smooth transition function variable,  $G(Z_{i,t})$ , taking higher values the higher is the probability that economy is in a recession. Rows denoted by “[...]–high state” (“[...]–low state”) report the effect of austerity announcements made when the probability of being in an expansion is 100% (0%), and are obtained interacting the austerity variables with  $1 - G(Z_{i,t})$  ( $G(Z_{i,t})$ ). Column 3 reports results from a similar specification in which the effect of austerity announcements are allowed to differ depending on whether the announcement is done during a period of positive or negative growth. \*, \*\*, \*\*\* indicate statistical significance at respectively the 90%, 95% and 99% confidence level, based on robust standard errors (in parenthesis). All specifications include incumbent party fixed effects.

