

Punished for austerity?

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Abstract

In both economics and political science, conventional wisdom states that austerity policies are unpopular among voters and that those governments which implements cutbacks in social spending will lose votes in the subsequent election. However, this claim has received little empirical support.

Contrary to earlier research, this paper finds that parties which implement fiscal adjustments are punished by the voters in the following election. The effects are larger when the adjustments are transparent and for the government party to which the prime minister belongs.

1 Introduction

Faced with international pressure and a public debt out of control, the Greek government implemented large budget cuts in response to the great recession. In the following election, the largest government party, PASOK, got their vote share reduced from 44 to 13 per cent. Greece is not alone. In the wake of the great recession, many countries will have to consolidate public finances. But is it possible to find political support for reducing debts and deficits, or does the Greek fate await any government that implements tax hikes or cuts in public spending?

The assumption that voters punish governments for fiscal austerity is a defining feature of many theories central to both economics and political science. It is one of the core assumptions of the *new politics* of the welfare state (Pierson 1998), it is supposed to give rise to *political business cycles* (Franzese 2002) and it is a cornerstone in the *public finance* literature (Alesina and Perotti 1995).

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Considering its real-world implications and theoretical importance, surprisingly few attempts have been made to test this assumption. Even more striking is the fact that most empirical evidence suggests that there are no electoral consequences for governments that pursue austerity measures (Alesina, Carloni and Lecce 2012; Giger and Nelson 2012), or that fiscal adjustments actually improve the chances of re-election (Brender 2003; Brender and Drazen 2008; Drazen and Eslava 2010). How is it possible that these studies contradict what appears so obvious at first sight?

In this paper I argue that previous research has underestimated the true effects because of two identification problems. First, it is possible that governments that are confident in being re-elected are more likely to consolidate public finances. Second, budget improvements often result from favourable economic conditions rather than discretionary fiscal consolidation. Both those problems make fiscal adjustments appear more popular than they actually are.

When the variables used in previous research are replaced by variables less susceptible to estimation bias, the results indicate that parties which implement fiscal adjustments are punished by the voters. The estimated effects are large. For every percent of GDP with which the budget balance is improved, the vote share for each government party is predicted to fall with one percentage point.

I also examine whether the electoral consequences differ depending on the transparency of fiscal consolidations and the degree of political accountability. I find that voters react more strongly to transparent adjustments and that parties to which the prime minister belongs are punished harder than other government parties.

2 Previous research

The research most closely related to this paper is the political economy literature concerned with budget deficits – henceforth called the public finance literature. This literature attempts to explain why countries sometimes accumulate unsustainable levels of debt. The conventional wisdom is that deficit reductions are costly for those who implement them. Governments that attempt to strengthen the budget balance – be it tax increases or spending cuts – are assumed to be punished by the voters in subsequent elections. This latter claim has, however, received very little empirical support.

I am familiar with seven studies that deal directly with the electoral consequences of fiscal consolidations. Three of them analyse the variation between countries to study fiscal policy at the national level. Alesina, Perotti and Tavares (1998) and Alesina, Carloni and Lecce (2012) examine cabinet changes in OECD countries while Brender and Drazen (2008) analyse a larger panel of countries. None of these studies find that governments are

more likely to be voted out of office following a fiscal consolidation. On the contrary, the results indicate that voters are fiscal conservatives and punish governments that weaken the budget balance. The other studies use regional variation within countries (Brender 2003; Drazen and Eslava 2010; Lowry, Alt and Ferree 1998; Peltzman 1992). All of them conclude that incumbents are punished for loose fiscal policies. More precisely, Peltzman (1992) concludes that voters dislike public spending no matter if it is paid by tax hikes or deficits, Lowry, Alt and Ferree (1998) find that voters dislike both positive and negative imbalances and Brender (2003) only finds an effect during the 1998 elections.

In other words, there has been little empirical support for the conventional wisdom that voters punish governments for fiscal adjustments. However, there are two reasons for why I think we should doubt the conclusions reached in previous research. In brief, I argue that all these studies have a design biased towards fiscally conservative voters. The arguments below are further supported by the first analysis in the results section.

First, the majority of the mentioned studies use cabinet changes instead of vote shares as the dependent variable (Alesina, Carloni and Lecce 2012; Alesina, Perotti and Tavares 1998; Brender 2003; Brender and Drazen 2008). That not only means losing a lot of variation, which is scarce in the context of large fiscal adjustments, but it also creates an unnecessary risk of reverse causality. Some governments anticipate that they have a high probability of remaining in office. Such knowledge can result from a large margin to their opponents in the public opinion polls or because the political landscape provides favourable possibilities of coalition building. If those governments are more likely to implement fiscal adjustments, that would bias the estimates so that fiscal consolidations appear more popular than they actually are. There are good reasons to believe this is true. If the government expects a close election, they might not take the risk of unpopular austerity measures. And if they anticipate a loss, they have less interest in strengthening the public finances and thereby increasing the room for manoeuvre for their political opponents (Alesina and Tabellini 1990; Persson and Svensson 1989; Pettersson-Lidbom 2001). A part of this problem remains when analysing changes in vote shares. However, it only concerns situations where the support for the government parties have increased since last election. Other factors that might influence the chances of re-election are only problematic when cabinet changes are used as the dependent variable. Moreover, to the extent that the problem remains, it would cause me to underestimate the true electoral costs for fiscal adjustments. Because this is the first study to find any punishment effects at all, I believe that is a minor problem.

Second, most of these studies use a measure of the budget balance that has not been adjusted for macroeconomic conditions (Brender 2003; Brender and Drazen 2008; Drazen and Eslava 2010; Lowry, Alt and Ferree 1998). Because most budget improvements result from economic upturns, rather

than discretionary fiscal actions, this causes an obvious identification problem. Unless economic conditions are properly controlled for, the use of unadjusted budget balances might lead us to estimate the electoral consequences of favourable economic development instead of fiscal consolidations. And as the literature on economic voting has shown, governments are rewarded for both low unemployment and fast growth (Lewis-Beck and Stegmaier 2000). Of these four studies, it is only Brender and Drazen (2008) that uses a satisfactory set of macroeconomic control variables.¹

Another strand of research related to this paper is the welfare state research on 'new politics' and the retrenchment of the welfare state. One of the core assumptions in this literature is that cutbacks in social programs – much more than tax hikes – are so unpopular among voters that governments which implement them will try to avoid public debate and responsibility for the reforms (Pierson 1996). However, similar to the situation in the public finance literature, the conventional wisdom that voters punish governments for retrenchment has received little empirical support (Giger and Nelson 2012).

While spending cuts constitute a larger part than tax hikes in most fiscal consolidations (Devries et al. 2011), retrenchment differs from fiscal adjustments in three important ways. First, it has been a continuous process – going on for decades in many countries, and is therefore commonly referred to as a state of 'permanent austerity' (Pierson 1998). Second, considering that retrenchment in many countries has been accompanied by large tax reductions, these cutbacks are not necessarily implemented to strengthen public finances. Because retrenchment tends to be accompanied by tax reductions, most studies in this field actually estimate the joint effect of cutbacks and tax reductions and not only the austerity effect. Third, when measuring retrenchment, theoretical relevance and politically contested reforms appear to have precedence in favor of empirical concerns. Most studies therefore analyse reforms of a much smaller scale than what is done in the public finance literature. For example, the largest fiscal adjustments in this paper amounts to almost 15 per cent of GDP. Reducing the replacement rate in the unemployment insurance with five percentage points – which Armingeon and Giger (2008) calls a 'major retrenchment' – would in most countries not even cut public spending by 0.1 per cent of GDP.

3 Theory and heterogenous effects

It is easy to doubt the findings of previous research when they contradict conventional wisdom. However, it is not obvious why voters should oppose fiscal consolidation. In fact, if voters are either discounting the future at the

¹I am the first to acknowledge the difficulties involved in adjusting the budget balance for the business cycle. However, that is not an excuse for not trying.

same rate as financial markets, or if they are able to compensate government policy through changes in private saving, they should be indifferent to the inter-temporal reallocation of government spending. It is also difficult to imagine why governments would implement supposedly unpopular policies if they did not believe them to be beneficial for the citizens.

To argue that we should expect voters to punish governments for fiscal consolidations, we must make additional assumptions about how voters behave. For example, in the public finance literature it has traditionally been assumed that voters are short-sighted (Buchanan and Wagner 1977), that they exploit future generations (Alesina and Perotti 1995; Bowen, Davis and Kopf 1960; Cukierman and Meltzer 1989) or that they lack information about the nation's financial position (Rogoff and Sibert 1988). If those assumptions are true, voters might prefer government spending today in favor of future government spending and therefore disapprove with fiscal adjustments implemented by the government.

The 'new-politics' approach emphasizes the asymmetry between gains and losses and thereby provides another reason for why voters might oppose reallocations of public consumption. Saving today to consume tomorrow is not only about the inter-temporal distribution of consumption. It is also a matter of imposing losses today to create room for gains tomorrow. There are several reasons to believe that voters react different to gains and losses (Pierson 1996). First, following the seminal works of Kahneman and Tversky (1979), an extensive research has established that people in general are loss averse and value a loss of something they own higher than gaining something of the same 'objective' value. A similar negativity bias is found in how voters react to positive and negative economic conditions (Bloom and Price 1975; Lewis-Beck and Paldam 2000). Second, in the case of fiscal adjustments, the losses are much more concrete than the future gains. It is clear who losses what, while the future gains and their beneficiaries are diffuse and uncertain. According to dominating theories of collective action, the clearly defined and concentrated interests will gain a stronger political support than the diffuse and scattered (Olson 1965; Wilson 1973).

Transparency and accountability

It is difficult for voters to punish incumbents if they cannot observe the size of fiscal adjustments or if they do not know which party that was responsible. I use the terms *transparency* and *accountability* to denote the degree to which voters have information about fiscal policy and whether it is obvious which party that should be held accountable for it.

Transparency is an important concept in both the public finance and the retrenchment literature. Researchers in the former tradition have argued that fiscal transparency reduces budget deficits, because governments cannot hide them from the voters (Alt and Lassen 2006a). In the latter literature, it

has long been argued that retrenchment bears no costs for incumbents that manage to avoid blame and public debate. Transparency can be operationalized in many different ways. In this paper I will examine four different kinds of transparency and how it affects the voter reactions to fiscal adjustments.

First, the least transparent cutbacks are probably those that do not require any discretionary actions at all. It is well established that governments can obfuscate retrenchment by allowing inflation and wage increases to erode the value of benefits and transfers (Green-Pedersen 2002; Lindbom 2007; Pierson 1994). Because most government expenditures are not tied to wages – or only partially so – that argument applies to all forms of public spending. Consequently, the faster the economy grows, the larger are the possibilities to obfuscate retrenchment and the smaller should we therefore expect the voter reactions on fiscal adjustments to be.

Second, transparency is also a matter of scale. Armingeon and Giger (2008) argue that voters do not even notice small and incremental cutbacks. And even if they did, voters' knowledge and opinion about a particular issue mostly matters when the issue is on the political agenda (Krosnick and Kinder 1990). According to this argument, fiscal adjustments only affect the vote decision if they are large enough to be a part of public debate. In terms of model specifications, this hypothesis corresponds to a non-linear relationship such that the electoral costs of additional budget improvements is increasing with the size of fiscal adjustments.

Third, the budget process is often said to be transparent when budgets are easily available to the public and presents consolidated information in a 'bottom line' measure (Poterba and Hagen 1999), so that the voters can assess the nation's financial position and the economic and social implications of government activities (Craig and Kopits 1998). While it is commonly assumed that fiscal transparency enhances budget discipline, by making fiscal misconduct more visible, it is also possible that it makes fiscal consolidations more transparent and therefore more costly for the government that implements them. To examine this hypothesis, I will condition the voter reactions to fiscal adjustments on the commonly used index of fiscal transparency that was created by Alt and Lassen (2006b). The index is based on criteria such whether reports on the fiscal outlook are released prior to elections and whether the budget documentation contains projections of future expenditures.

Fourth, parties can choose to publicly announce their plans of fiscal consolidation. While it arguably increases the transparency of reform, it can also give policy makers a chance to motivate their decisions and – if done before they are elected – provide them with a stronger political mandate. In this paper I will use the Manifesto Project's database of election manifestos to examine whether the electoral consequences of fiscal adjustments depend on the direction for fiscal policy that the party announced during their election campaign.

It is not always evident which party that should be held accountable for fiscal policy. The ruling party can differ between the regional and national level, the president might belong to another party than the parliament majority, governments can be formed as a multiparty coalition and minority governments must negotiate with other parties in parliament. When the responsibility for fiscal policy is shared between several parties, we might expect the electoral consequences to be smaller for any single party.

There are several studies that support this hypothesis. Lowry, Alt and Ferree (1998) finds that accountability is stronger when the governor and the state legislative majority belong to the same party. Alesina, Carloni and Lecce (2012) presents some suggestive evidence for that minority and coalition governments are more likely than majority and single-party governments to be re-elected after a fiscal consolidation. And the literature on economic voting has shown that small parties in coalition governments can gain votes from the dominant parties when the latter are punished for poor economic conditions (Brug, Eijk and Franklin 2007).

In this paper I examine whether the electoral consequences are larger for i) parties to which the prime minister belongs, ii) majority governments, iii) single-party governments and iv) parties that were in government also during the last election period.

Partisan differences

Partisan differences are often neglected, both within the political economy literature and the 'new politics' perspective of the welfare state research. It is still possible to find two opposing ideas about whether left-wing or right-wing parties are most likely to be punished for fiscal austerity. On the one hand, fiscal adjustments tend to affect government spending more than taxes (Devries et al. 2011) and left-wing parties could thus be held accountable to a larger degree because voters expect them to defend the welfare state. If these parties would implement cutbacks, it could be interpreted as a broken promise and cause dramatic voter reactions. In support of this hypothesis, Schumacher, Vis and Kersbergen (2013) finds that only parties with a 'positive welfare image' are punished for welfare state retrenchment.

On the other hand, maybe the credibility of left-wing parties in these matters is so strong that it makes them immune to criticism about being anti-welfare. Kitschelt (2001, p. 275) argues that just like it was the anti-communist Nixon who went to China, social democratic or labour parties have more credibility in social policy and are therefore less likely to be punished by the voters. Tavares (2004) uses the same metaphor to make a similar point: Implementing policies in disagreement with the party's ideological profile is a way of signalling to the voters the necessity of a fiscal consolidation. Therefore, he argues, governments must implement reforms that collide with their ideological position. It is only when right-

wing governments raise taxes and left-wing governments cut expenditures that the voters give them credibility. However, he finds that adjustments implemented by left-wing governments have a larger share of tax-hikes than those of right-wing governments.

Previous empirical research does not offer much more guidance than that, because it is typically concerned with slightly different topics. There does not appear to be any partisan differences in the size of debt or deficits. Neither do Lowry, Alt and Ferree (1998) find any differences in voter reactions between democratic and republican incumbents. However, they find that voters punish republicans for increasing the size of government and democrats for unexpected cutbacks, which is difficult to reconcile with the findings in Tavares (2004). And while it has been shown that left-wing parties are less likely to implement cutbacks (Allan and Scruggs 2004; Korpi and Palme 2003), which in turn has been interpreted as if their electoral incentives differ (Giger and Nelson 2011), it could very well reflect purely ideological differences.

Fiscal policy is often analysed as an inter-temporal distribution of consumption. Because the lines of conflict between left and right are different from those between generations, the fiscal balance is not often politicized in the literature. However, I argue that the distributive aspects of fiscal balances are more evident when government default is perceived as an alternative. The worst losers of large deficits are then the holders of government bonds that lose their investments when the government defaults or deflates its debt instead of repaying the debtors. For example, following the second world war, many countries had debts in the range of 100–200 per cent of GDP. Had governments chosen to implement fiscal adjustments instead of letting inflation take care of the debt, it would have had radically different redistributive effects in favor of the often wealthy rentiers.² It is likely that voters get disappointed with a left-wing government that they perceive takes the side of the bond owners. For that reason I would expect left-wing parties to be punished harder than right-wing parties for fiscal consolidations, at least when the risk of default is substantial. To measure this risk, I use the nation's long-term interest rates on government bonds compared to the interest rate on government bonds in Germany.

4 Method

The empirical strategy of this paper is to regress each party's change in vote share between two elections on the size of fiscal adjustments implemented

²I am not arguing in favor of defaulting on public debt. The fact that capital owners lose more than other citizens does not alone imply that a policy is either just or sound. It should also be noted that countries differed in their post-war policies on public debt. For example, Britain were more keen on repaying their debtors than was Germany or France (Piketty and Goldhammer 2014).

while the party was in government. I follow the common practise in the political economy literature of defining fiscal adjustments as the accumulated change in the structural budget balance, which is a measure of what the budget balance would be during normal economic circumstances (when output is equal to its long-run trend and the inflation is stable). If correctly estimated, an increase in this measure corresponds to either a tax raise or a reduction of expenditures (if not both).

Each observation corresponds to a party (p) over one election period (e) and, unless otherwise noted, the sample consists of parties that spent at least half the election period in government. The main sample includes elections from 27 countries between 1974 and 2013³. The basic structure of most regressions follow the equation below, where the party's vote change ($v_{p,e}$) is regressed on the change in the structural budget balance while the party was in cabinet ($sbb_{p,e}$), a vector of control variables also specific for when the party was in cabinet ($\phi_{p,e}$), another vector of variables that are constant within elections (χ_e) and a third vector⁴ of party-specific variables (ψ_p). To identify conditional effects, I sometimes interact the change in the structural budget balance with different measures of transparency, accountability and partisanship.

$$v_{p,e} = a + sbb_{p,e} + \phi_{p,e} + \chi_e + \psi_p + e_{p,e} \quad (1)$$

Because there can be multiple cabinets during an election period, all variables that differ between cabinets are first calculated at the cabinet level (c) and then aggregated to election periods. For example, the change in the structural budget balance for which a party is accountable ($sbb_{p,e}$) is calculated using the equation below, where $sbb_{last,c}$ and $sbb_{first,c}$ denotes the structural budget balance during the cabinet's first and last year in office and cab_c is a dummy variable equal to 1 if the party was a part of the cabinet. Under the assumption that cabinets influence the economy and fiscal policy for the first year after they enter office, and that they have no influence the year after they leave office, the difference between the cabinet's first and last year can be interpreted as the development for which the cabinet is accountable.

$$sbb_{p,e} = \sum_{c=1}^n (sbb_{last,c} - sbb_{first,c}) \times cab_c \quad (2)$$

Interaction variables where the conditioning variable differs between cabinets are calculated in a similar way. To illustrate, the following equation shows

³The included countries are Australia, Austria, Belgium, Canada Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Iceland, Ireland, Italy, Japan, Luxembourg, Netherlands, New Zealand, Norway, Poland, Portugal, Slovenia, Spain, Sweden, Switzerland and United Kingdom

⁴The term 'vector' might be misleading, because in many specifications it only includes one variable.

how the interaction effect of majority governments is calculated. The only difference to the previous equation is that each cabinet’s fiscal policy is multiplied with a dummy variable (maj_c) that is equal to 1 if the cabinet is a majority government.

$$sbb_{p,e} = \sum_{c=1}^n (sbb_{last,c} - sbb_{first,c}) \times cab_c \times maj_c \quad (3)$$

The literature on economic voting has shown that incumbents are punished for slow growth (Campbell 2005) and high unemployment (Lewis-Beck and Stegmaier 2000). Because both GDP growth and unemployment might affect the probability of fiscal adjustments, I include the average GDP growth, the change in GDP growth and the change in unemployment rate as economic control variables. Because my data sources for inflation are missing many observations, regressions including inflation are only provided in the supplementary material. In addition to these economic variables, every regression includes a dummy variable for whether the party is considered left-wing or right-wing. Note that fiscal consolidations hurt macroeconomic performance in the short run and that by controlling for growth and unemployment I might bias the estimates toward more positive electoral consequences.⁵

There are reasons to believe that the electoral consequences of fiscal consolidations are not the additive inverse of fiscal expansions (Lowry, Alt and Ferree 1998). I therefore exclude episodes with a positive change in the structural budget balance.⁶

Data on vote shares, coalition composition and the ideological position of parties is provided by the ParlGov database. All economic data comes from OECD Economic Outlook 94 and the index of fiscal transparency is collected from Alt and Lassen (2006b). I have created a joint measure of announced austerity by taking the difference of two (standardized) variables in the Manifesto Project Database. The first variable (per414) measures how much is written in the manifesto about reduction of budget deficits and the need for retrenchment, among other things. The second variable (per409) measures the extent to which the manifesto calls for demand-oriented policy through increasing social expenditures. A complete variable list is included at the last page of this paper.

⁵If voters can distinguish the economic performance caused by fiscal policy from other parts of the economy, there is no bias. Moreover, there are those who argue that consolidations can be expansionary also in the short term. If they are right – which I doubt – the effects would instead be biased upwards.

⁶Results for the full sample are included in the supplementary information. Like Lowry, Alt and Ferree (1998), I find that governments are also punished for creating structural budget deficits. However, it is difficult to separate discretionary budget decisions from downward revisions of potential output (upward revisions are less frequent), which are often associated with real-estate bubbles and economic turbulence, so the results must be interpreted with great care.

Table 1: The electoral consequences of fiscal adjustments

| | (1) | (2) | (3) | (4) |
|-----------------------------------|-------------------|--------------------|--------------------|--------------------|
| | Re-election | Vote change | Vote change | Vote change |
| <i>Change while in government</i> | | | | |
| Structural budget balance | 0.06 (0.06) | | | -1.02*** (0.25) |
| Non-adjusted budget balance | | -0.21 (0.20) | -0.69*** (0.22) | |
| Unemployment rate | -0.06 (0.07) | | -0.94** (0.40) | -0.73* (0.38) |
| GDP growth | 0.02 (0.04) | | 0.07 (0.19) | 0.02 (0.17) |
| Average GDP growth | -0.20** (0.08) | | 0.18 (0.51) | -0.29 (0.49) |
| Right-wing party | 0.34 (0.22) | 1.16 (0.85) | 1.06 (0.79) | 1.01 (0.75) |
| Constant | 0.64** (0.28) | -2.96*** (0.66) | -2.51* (1.31) | -0.79 (1.31) |
| Observations | 182 | 182 | 182 | 182 |
| Adjusted R^2 | | 0.006 | 0.143 | 0.182 |

Clustered standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5 Results

This section is organised around five tables, in which the regression results are presented (I will reduce this number and narrow the focus of the paper at a later stage). Table 1 displays the main results and shows why the choice of variables is so important. The conditional effects of transparency and accountability are presented in Tables 2 and 3. Table 4 presents suggestive evidence for how the results depend on partisanship and the risk for default. In contrast to the first four tables, Table 5 shows the results from regressions where non-cabinet parties are included. Such an extension of the sample makes it possible to examine which parties that gain votes when the vote shares of the government parties are reduced.

Table 1 shows how the estimated electoral effects of fiscal adjustments depend on the choice of variables that separates this study from previous research.⁷ The first column shows the results from a probit regression where the dependent variable is the binary outcome of re-election. The coefficient of the structural budget balance is positive but close to zero, meaning that the probability of re-election is at least not smaller for governments that implement fiscal adjustments than it is for other governments. This finding is in line with previous studies that use cabinet changes as the dependent variable.

The second and third column show the estimated effects when a measure

⁷I do not attempt to replicate the models used in the cited papers. It is therefore possible that the conclusions would be different if the regressions were run on their samples.

of the budget balance is used that has not been adjusted for the business cycle. In the second column I do not include any macroeconomic control variables and neither do I find any electoral effects of fiscal adjustments. To the model presented in the third column I have added the change in unemployment and growth during the party's time in government and the average growth rate during the election period. These controls work as suppressor variables, suppressing the positive relationship between voter support and budget improvements caused by favourable economic conditions, which allows me to finally identify a statistically significant effect of the budget balance. The fourth column shows the model I prefer, where the dependent variable is the party's change in vote share and where a cyclically adjusted budget balance is used to measure fiscal adjustments. In this model, an improvement in the structural budget balance with one per cent of GDP is estimated to reduce each government party's vote share with one percentage point. This is a very large effect that is also statistically significant at the 99 percent level.

The results in Table 1 support my criticism of previous research. Arguably, both the choice of dependent variable and the adjustment for macroeconomic conditions is critical. As soon as cabinet changes are used as dependent variable, or if macroeconomic conditions are not taken into account, the estimated effects of fiscal adjustments disappear. One could argue that staying in cabinet is the ultimate goal for a party – and that votes are only valuable if they affect the probability of incumbency – but then they should not interpret the effects in terms of voter behaviour. And as argued in Section 2, I believe that the differences in results are caused by an identification problem that is aggravated when cabinet changes are used as the dependent variable.

Table 2 extends the preferred model in Table 1 by including interactions between the size of fiscal adjustments and different indicators of fiscal transparency. The first column shows that during rapid GDP growth, the electoral costs of fiscal adjustments are smaller. I have two explanations for this phenomenon. First, public spending as share of GDP decreases automatically when GDP grows, because most expenditures are not fully indexed to the growth of output and wages. Such obfuscated cutbacks are probably the least transparent of all. Second, voters might not pay as much attention to a tax hike if they experience a simultaneous increase of their gross wage, because their net income would then remain unchanged. Armingeon and Giger (2008) argue that it is better for governments to implement incremental adjustments over a long time instead of doing it all at once, because voters do not react to small cutbacks. This argument is supported by the results in the second column, which shows that the effects are non-linear. The marginal electoral effect of an increase in the budget balance is increasing with the total size of the consolidation.

The third column presents the results of a model which includes Alt

Table 2: Effects conditional on transparency

| | (1) | (2) | (3) | (4) |
|-----------------------------------|--------------------|-------------------|--------------------|--------------------|
| <i>Change while in government</i> | | | | |
| Structural budget balance | -1.11*** (0.22) | -0.19 (0.48) | -0.42* (0.22) | -0.92*** (0.32) |
| Unemployment rate | -0.51 (0.42) | -0.61 (0.41) | 0.23 (0.30) | -0.76* (0.44) |
| GDP growth | 0.06 (0.16) | 0.03 (0.16) | -0.29* (0.15) | 0.08 (0.17) |
| <i>Interaction with SBB</i> | | | | |
| SBB × Average growth | 0.16*** (0.05) | | | |
| SBB × SBB | | -0.10** (0.04) | | |
| SBB × Fiscal transparency | | | -0.38* (0.22) | |
| SBB × Austerity manifesto | | | | 0.40** (0.19) |
| Fiscal transparency | | | 0.01 (0.42) | |
| Austerity manifesto | | | | -0.78* (0.40) |
| Average GDP growth | -0.52 (0.47) | -0.29 (0.49) | 0.74** (0.33) | -0.54 (0.53) |
| Right-wing party | 0.73 (0.71) | 0.76 (0.72) | 0.49 (0.57) | 0.72 (0.69) |
| Constant | -0.45 (1.28) | -1.43 (1.41) | -2.91*** (0.97) | -0.05 (1.45) |
| Observations | 182 | 182 | 147 | 162 |
| Adjusted R^2 | 0.210 | 0.200 | 0.106 | 0.127 |

Clustered standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

and Lassen's index of fiscal transparency. The interaction effect is negative, meaning that the electoral consequences are larger in countries where public finances are transparent. While it makes perfect theoretical sense, the finding complicates some established facts in previous research, because fiscal transparency is commonly assumed to always benefit the sustainability of public finances. The last model includes an interaction between the size of the fiscal adjustment and a measure of to which degree the party announced contractionary fiscal policy in the election manifesto preceding the election period. In other words, it is a measure of whether the party announced the fiscal consolidation beforehand. The positive coefficient of the interaction term indicates that voters are less likely to punish a government for adjustments that had already been announced. Because the consolidation was announced during the campaign, it is possible that the electoral cost was imposed in the previous election. However, replacing the dependent variable with the vote change over two elections does not change the results. In other words, it might be a good strategy for parties to adjust voters' expectations

on the party so that they conform to the party's intentions.

As apparent from these four indicators, transparency can mean quite different things. It is also possible to come up with other explanations for these results. We should therefore be careful when we interpret the effect of any single indicator. However, the broader picture clearly indicates that voters react more strongly to fiscal adjustments if they are transparent. When austerity is difficult to observe – because wage increases hollow out the benefits or the cutbacks are too small to cause debate – the electoral consequences are absent.

Table 3 presents the results from four regressions where the electoral consequences are conditioned on the degree of political accountability. The first column makes it clear that the party to which the prime minister belongs is punished harder for fiscal adjustments than other government parties. According to this estimation they are punished three times as hard, but this difference is very sensitive to the exact specification of the model.⁸ The effects of political accountability are less clear cut in the other three models. While they all have the expected sign – with larger effects for majority governments, single-party governments and parties that were responsible for fiscal policy also before last election – none of the coefficients is even close to being statistically significant.

Table 4 examines whether the effects differ between left-wing and right-wing parties, by including an interaction term between the size of the fiscal adjustment and a dummy variable that is coded 1 for right-wing parties and 0 for left-wing parties. According to the estimate in the first column, which uses the same sample as previous tables, voters are less inclined to punish right-wing parties. The estimated difference is large, with right-wing parties experiencing less than half of the effect for left-wing parties, but the estimate is imprecise and only significant at the 90 per cent level.

In the second and third columns, the sample has been divided based on the countries' interest rate. The second column includes cases where the interest rate on government bonds was more than three percentage points higher than in Germany, indicating that the financial markets treat the risk of default as relatively large. The rest of the observations where I have data on interest rates are included in the third column. Unfortunately, the small number of cases with high interest rates makes the estimates very imprecise, but the tendencies are interesting. The difference between left-wing and right-wing parties in column 1 is obviously driven by episodes of large risk for default. However, the differences between columns are not statistically significant and the evidence should at best be interpreted as suggestive.⁹

In previous regressions, the sample consisted of parties that were in office

⁸See the supplementary information for alternative specifications.

⁹Note that while the change in the coefficient for the structural budget balance might appear large, it here refers to the effects for left-wing parties and not the average effect.

Table 3: Effects conditional on accountability

| | (1) | (2) | (3) | (4) |
|-----------------------------------|-------------------|------------------|-------------------|--------------------|
| <i>Change while in government</i> | | | | |
| Structural budget balance | -0.44 (0.32) | -0.70 (0.45) | -1.38** (0.53) | -0.95*** (0.27) |
| Unemployment rate | -0.66* (0.39) | -0.72* (0.38) | -0.60 (0.40) | -0.42 (0.27) |
| GDP growth | 0.02 (0.17) | 0.02 (0.17) | 0.08 (0.17) | -0.06 (0.13) |
| <i>Interaction with SBB</i> | | | | |
| SBB × PM's party | -0.87** (0.41) | | | |
| SBB × Majority government | | -0.41 (0.51) | | |
| SBB × Government parties | | | 0.20 (0.19) | |
| SBB × Incumbent last period | | | | -0.05 (0.55) |
| Prime minister's party | 0.00 (1.32) | | | |
| Majority government | | 0.16 (1.55) | | |
| Government parties | | | 0.59 (0.37) | |
| Incumbent last period | | | | 1.93 (1.43) |
| Average GDP growth | -0.24 (0.51) | -0.28 (0.49) | -0.18 (0.48) | 0.27 (0.34) |
| Right-wing party | 1.06 (0.76) | 1.08 (0.76) | 1.26* (0.73) | 1.34* (0.77) |
| Constant | -1.15 (1.38) | -0.91 (1.84) | -3.18* (1.73) | -3.40** (1.50) |
| Observations | 182 | 179 | 182 | 171 |
| Adjusted R^2 | 0.203 | 0.179 | 0.219 | 0.218 |

The effects conditional on prime ministry, majority status and the number of government parties are not technically interaction terms. To improve precision, these factors of accountability are multiplied with changes in the budget balance at the cabinet level before they are added up to form election periods. Conceptually, the effects can still be interpreted as interaction effects. Clustered standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Partisan effects

| | (1) | (2) | (3) |
|-----------------------------------|--------------------|-------------------|------------------|
| <i>Change while in government</i> | | | |
| Structural budget balance | -1.25*** (0.25) | -1.76** (0.74) | -0.42 (0.36) |
| SBB × Right-wing | 0.72* (0.42) | 1.56 (1.97) | -0.23 (0.41) |
| Unemployment rate | -0.70* (0.37) | -0.94 (0.79) | -0.37 (0.36) |
| GDP growth | 0.03 (0.17) | 0.54 (0.63) | -0.06 (0.17) |
| Average GDP growth | -0.32 (0.48) | -0.90 (1.30) | 0.19 (0.43) |
| Right-wing party | -0.29 (1.01) | -2.28 (3.37) | 1.43 (1.07) |
| Constant | -0.26 (1.33) | 0.22 (3.64) | -2.61* (1.35) |
| Observations | 182 | 24 | 136 |
| Adjusted R^2 | 0.189 | 0.525 | 0.017 |
| Risk for default | – | Large | Small |

Clustered standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

for at least half the period since last election. Such a restricted sample corresponds well to the counter-factual question about how these parties would have performed in the election if they had implemented different policies. In Table 5, the sample has been augmented with parties outside the cabinet and even some parties outside the parliament. Instead of only comparing cabinets with different fiscal policy, we now also compare them with parties outside government. After all, episodes of large fiscal adjustments might differ from normal circumstances in more than one way. Maybe they coincide with a general mistrust in the established parties, so that also parties outside the government lose support? Our previous models could not identify such tendencies. When the sample is extended, I also add a set of variables that measure the total change over the election period regardless of whether the party was in government.

The new sample modified the point of comparison, but the results are very similar to before. As the sum of the two SBB-coefficients in the first column shows, government parties are still expected to lose about one percentage point of the vote share for each percentage point of adjustment. It might be interesting to note that the average party outside government is not expected to gain very much from the fiscal adjustment, but this is simply a result of the number of cabinet parties being much smaller than the number of parties outside the cabinet.

Using this design, it is also possible to examine the characteristics of parties that gain votes after a fiscal consolidation. During the last years, Europe has experienced a rapid growth of both radical left parties and

Table 5: Including non-cabinet and non-parliament parties

| | (1) | (2) | (3) |
|-----------------------------------|--------------------|--------------------|--------------------|
| <i>Change while in government</i> | | | |
| Structural budget balance | -1.53*** (0.28) | -1.69*** (0.39) | -1.53*** (0.32) |
| Unemployment rate | -0.55 (0.36) | -0.55 (0.37) | -0.57 (0.38) |
| GDP growth | -0.18 (0.17) | -0.19 (0.17) | -0.18 (0.18) |
| <i>Change during period</i> | | | |
| Structural budget balance | 0.31** (0.15) | 1.02*** (0.24) | 0.24 (0.26) |
| Unemployment rate | 0.00 (0.10) | 0.01 (0.11) | 0.01 (0.12) |
| GDP growth | 0.03 (0.08) | 0.03 (0.08) | 0.03 (0.08) |
| <i>Interaction with SBB</i> | | | |
| SBB × Parliament experience | | -0.82*** (0.26) | |
| SBB × Government experience | | 0.24 (0.39) | |
| SBB × Right-wing scale | | | 0.03 (0.05) |
| SBB × Distance from centre | | | -0.04 (0.11) |
| Parliament experience | | -5.96*** (0.84) | |
| Government experience | | 0.29 (0.68) | |
| Right-wing scale | | | -0.07 (0.17) |
| Distance from centre | | | 0.05 (0.19) |
| Average GDP growth | -0.07 (0.12) | -0.05 (0.13) | -0.07 (0.12) |
| Right-wing party | 0.90** (0.44) | 0.70 (0.47) | 0.92 (0.78) |
| Constant | -0.35 (0.45) | 5.49*** (0.90) | -0.13 (0.81) |
| Observations | 561 | 561 | 561 |
| Adjusted R^2 | 0.159 | 0.169 | 0.154 |

Clustered standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

right-wing extremist parties. To investigate if there is a direct connection to fiscal consolidations, I added a few variables that might characterize these parties. Column 2 interacts the size of the consolidation with two measures of how established the party is – namely, whether the party has ever been represented in the parliament and whether it has ever held a seat in the government. As shown by the large and negative interaction effect, it is parties without previous parliament experience that attracts the most voters following a fiscal adjustment. However, this coefficient is difficult to interpret, because the parties included in this sample are not in any way representative for parties outside the parliaments. On the contrary, most parties in this sample are included because they have entered or have been close to enter the parliament. Still, the results indicate that it is new and peripheral parties that stand to gain from the cabinet’s losses. The third column examines whether parties on the left, right or just far from the centre gain more from consolidation. However, both the estimated effects are close to zero and statistically non-significant.

Before concluding, I should mention that I have experimented with many alternative specifications and carried through a large number of robustness tests. The results from some of these attempts are briefly summarized below. These, and other alternative specifications, are all provided in the supplementary information. First, one could expect larger parties to be punished harder, in percentage points, than small parties. However, if an interaction term is included with the size of the party, it turns out to be statistically insignificant. Replacing the dependent variable with relative vote changes results in worse fits in all models. Second, as argued by Lindbom (2014), if voters expected the government parties to implement fiscal adjustments, and therefore ‘punished’ them in the preceding election, I would have underestimated the true consequences. Fortunately, the estimated effects are unchanged – or slightly smaller – when I replace the dependent variable with the change in vote share over two elections. Third, the inter-election correlation of the dependent variable is negative, meaning that parties that gain votes in one election tend to reduce their vote share in the election after that. If parties that do well are more likely to implement fiscal adjustments, that could possible bias my results. However, controlling for the vote change during previous election does not alter any of my conclusions. Last, none of the conclusions drawn in this paper are sensitive to the removal of any single party or election.

6 Conclusions

In many theories central to both economics and political science, a core assumption is that voters punish governments which implement fiscal adjustments. This is also a commonly held view among policy makers and

political commentators. To the best of my knowledge, this is the first paper to provide empirical evidence in support of this claim.

The estimated effects are large. According to my estimates, each government party loses one percentage point of its vote share for every per cent of GDP with which the budget balance is improved.

The electoral consequences are smaller during periods of rapid economic growth and in countries where the degree of fiscal transparency is low. In addition, I do not find any effect at all of incremental fiscal adjustments. My interpretation of these results is that voters only react to transparent fiscal consolidations.

The largest identification problem, in this as well as in previous studies, might be the risk for reverse causation. If popular governments – or less popular, for that matter – are more likely to implement fiscal consolidations, that would bias the results. I believe that the best way to handle this problem would be to analyse the dynamics and timing of political support and fiscal adjustments. It is therefore promising that comprehensive cross-country datasets on vote-intention polls are under development (see for example Jennings and Wlezien [2013]).

There are many examples of fiscal adjustments that have resulted in tremendous vote losses for the incumbent parties. There are also many examples of governments being re-elected after implementing large fiscal consolidations. Such differences indicate that more research is needed both on the circumstances that shape voter behaviour and how government coalitions are formed during times of economic and political turbulence.

After losing 70 per cent of their voters, PASOK is still a part of the coalition government in Greece.

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Table 6: Variables

| Variable | Definition |
|----------------------------------|--|
| <i>From the ParlGov database</i> | |
| Vote change | Change in vote share since last election (percentage points). |
| Re-election | Government party in the first cabinet following the election (1 or 0). |
| Right-wing scale | Right-wing index (10 = extreme right, 0 = extreme left). |
| Right-wing party | Measures if the right-wing index is above 5 (1 or 0). |
| Distance from centre | The absolute value of [right-wing scale minus 5]. |
| Prime minister's party | Measures if the prime minister of the cabinet belongs to the party (1 or 0 for cabinets). |
| Majority government | Measures if the government parties hold a majority of the seats in parliament (1 or 0 for cabinets). |
| Government parties | The number of parties in the government (1 to 7). |
| Incumbent last period | Measures if the party was in government for more than half of last election period (1 or 0). |
| Parliament experience | Measures if the party has ever been in parliament before the election. (1 or 0). |
| Government experience | Measures if the party has ever been in government before the election. (1 or 0). |
| <i>Variables from OECD</i> | |
| Structural budget balance | Accumulated change in the cyclically adjusted net lending, general government (per cent of potential GDP). |
| Non-adjusted budget balance | Accumulated change in net lending, general government (per cent of GDP). |
| Unemployment rate | Accumulated change in the unemployment rate (percent of the labor force). |
| GDP growth | Accumulated change in the real GDP growth (percentage points). |
| Average GDP growth | Average GDP growth (per cent). |
| Interest rate | Average long-term interest rate on government bonds minus the same rate for Germany (percentage points). |
| <i>Other sources</i> | |
| Fiscal transparency | The standard score of the index of fiscal transparency developed by Alt and Lassen (2006b). |
| Austerity manifesto | The standard score of subtracting the standard score of per409 (Keynesian Demand Management) from the standard score of per414 (Economic Orthodoxy). Source: Manifesto Project Database. |

Exact calculations and summary statistics are provided in the supplementary information.