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The Electoral Advantage of the Left in Times of Fiscal Adjustment

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Abstract

Despite widely held views on fiscal adjustment as a political minefield for incumbents, the empirical literature on the issue has been surprisingly inconclusive. A crucial variable that has been often overlooked in the debate is partisan politics. Building on the micro-logic of Albert Hirschman's "exit, voice and loyalty" framework, this article offers a novel theoretical perspective on the conditioning impact of partisanship in the electoral arena. Due to their more limited exit options at their disposal, left-wing voters are less likely to inflict electoral punishment on their parties, offering the latter an electoral advantage over their right-wing rivals. Relying on the largest cross-national dataset to date on the evolution of close to 100 parties' popularity rating in 21 democracies, time-series-cross-section results confirm this electoral advantage.

Keywords: fiscal adjustment, partisan politics, exit voice and loyalty, party competition, elections, popularity

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Introduction

As advanced industrial economies are emerging from the Great Recession and the ensuing sovereign debt crisis that afflicted a great number of countries in the Eurozone and beyond, the political economy of fiscal adjustment is back in the limelight of the scholarly community and popular discourse alike. Earlier episodes of fiscal adjustment in OECD countries, mostly occurring in the 1980s and early 1990s, had triggered a large body of research that sought to resolve a number of relevant issues surrounding the political logic of austerity. One particular theme that widely resonates through these accounts is that fiscal adjustment inherently carries political risks for the main actors involved in the game. In particular, governing parties that decide to tighten the purse under fiscal stress are unlikely to receive standing ovation by the electorate when the electoral verdict is due.

Pessimistic accounts on the political consequences of fiscal adjustment chime in well with a wide array of different literatures that implicitly share a common view on voters. Political budget cycle theory (See de Haan and Klomp, 2013 for a more recent and Shi and Svenson, 2003 for an earlier review article of this voluminous literature) posits that voters favor expansionary policies prompting governments to relax the purse before elections and consolidate afterwards, if necessary. Economic voting (See Duch, 2007;

Hellwig, 2010; Lewis-Back and Paldam, 2000 and Lewis-Beck and Stegmaier, 2000 for excellent reviews), in turn holds that voters' main leverage over their elected governments is the ability to hold them accountable for the economic climate they create. If fiscal tightening dampens economic activity via its standard Keynesian channels[1], economic voting promises little solace for governments that engage in balancing the books.

Theoretically, there are further reasons to expect fiscal adjustment to carry such electoral risks. One powerful reason has been provided by welfare state research with its more narrow and fine-grained focus on the single largest item of government finances: social expenditure. Since Paul Pierson's seminal contribution (1994), welfare state scholars' main theoretical premise has been an electorate that is closely wedded to the post-war welfare settlement. Both because of voters' ideological and value-based attachment to major welfare programs (Boeri et al, 2001; Larsen, 2008; Taylor-Gooby, 2004; von Oorschot, 2000) and because of the concentrated losses on large segments of welfare recipients that large fiscal adjustment efforts inevitably entail (Pierson, 1996), retrenching the welfare state has been seen as an electoral minefield where only clever blame avoidance strategies can potentially prevail (Weaver, 1986; Vis and Van Kersbergen, 2013).

Such scholarly consensus on the dire electoral repercussions of fiscal adjustment masks a more complex empirical reality, however. As my review of the relevant empirical literature will show below, governments have survived and occasionally benefited from adjustment policies. This article's main contribution is a novel perspective on why this has been the case. In particular, I will apply Albert Hirschman's loyalty, exit and voice framework (1970) to

[1] See Monastiriotis (2014) for a recent review on the debate on the Keynesian vs. non-Keynesian effects of austerity.

argue that the partisan color of governments is crucial to understand the electoral consequences of austerity. Relying on the largest cross-national dataset to my knowledge on political parties' popularity ratings, I will then empirically show that while right-wing governments are in fact losing support in times of fiscal adjustment, no such empirical regularity is found for their left-wing rivals.

The rest of this article proceeds as follows. Section II offers a brief review of the literature on the political economy of fiscal adjustment and presents the theoretical framework of this article. Section III discusses the data and variables that I use for this study. Section IV outlines my empirical strategy and presents the main findings. Section V runs a number of robustness checks to probe the validity of the results. Section VI concludes.

The (not so) Unpopular Nature of Fiscal Adjustment

Literature Review

As a wave of fiscal adjustment efforts swept through advanced industrial economies in the 1980s and 1990s, scholarly interest in their political consequences gathered steam. The underlying premise at the time was the inherently unpopular nature of fiscal adjustment (Eslava, 2011). As the number of successful fiscal adjustment episodes – defined by the duration and sustainability of debt reduction efforts (Alesina and Perotti, 1995) – increased, a more systematic empirical literature emerged that asked two different, but conceptually related questions. First, what factors are responsible for the political commitment that sustained debt reductions efforts require? Second, do governments get systematically punished for tight fiscal policies in general and fiscal adjustments in particular?

The first question prompted empirical studies to probe the determinants of the duration and persistence of fiscal adjustment efforts. Initiated by Alesina and Perotti's study (1995), a series of other contributions (Alesina and Perotti, 1997, Illera and Mulas-Granados, 2008, Perotti, 1999, Tavares, 2004) followed up to investigate the reasons for the rich empirical arsenal of sustained adjustment efforts. If adjustment is as politically costly as received wisdom suggests, governments should turn their back from such policy at the first opportunity afforded by markets and/or institutional creditors. Beyond macroeconomic variables such as the size of the debt level (Mulas-Granados, 2006:3), political variables such as government fragmentation (Illera and Mulas Granados, 2008) and the electoral timetable (Alesina, 2006), the main line of argument largely converged around the notion of credibility. Since the main goal of fiscal adjustment is to signal commitment to financial markets, investors and consumers, duration can be understood as a commitment device: "the success of a fiscal adjustment depends crucially on its credibility, i.e., how permanent the initial change in the deficit is perceived to be" (Tavares, 2004 p. 249). Accordingly, Tavares (2004) has shown that when left-wing governments cut expenditure and right-wing governments increase taxes – which presumably run contrary to their political preferences – adjustments tend to last longer. Moreover, regardless of partisanship, Alesina et al (1998), Alesina and Ardagna (2009), Alesina et al (2011) and von Hagen et al (2002) argue that for adjustments to be credible and hence durable, they have to concentrate on the expenditures that are "politically most sensitive such as transfers subsidies and wage expenditures" (von Hagen et al, 2002 p. 513). Indeed, according to these empirical studies, those adjustments that concentrate on these budgetary items have an increased probability to succeed and permanently reduce the public debt burden.

The notion of credibility ties back to the highly contentious "expansionary austerity thesis" initiated by Giavazzi and Pagano (1990). As the argument goes, credible adjustments lower the expected future tax burden and hence result in lower credit risk premia, higher current investment and consumption by the private sector and ultimately an economic boom. Although such credibility effects have been documented in a number of case studies such as Ireland and Denmark (Giavazzi and Pagano, 1990; Alesina et al, 1998) and through an explicit analysis of the evolution of risk premia during adjustment periods (Ardagna, 2009), the "expansionary austerity thesis" fell out of fashion after a series of rebukes by Devries et al (2011), Perotti (2011) and perhaps most prominently by Krugman (2010). The main thrust of these critiques revolved around the (mis)-specification of austerity periods – I will expand on this debate in the next section – and the failure to acknowledge the idiosyncratic nature (an export-boom facilitated by economic openness, exchange rate behavior, the response by monetary authorities etc.) of some of the success stories.

If the alleged expansionary effects of fiscal adjustment are in fact not borne out by historical data, the high frequency of sustained adjustment efforts is all the more striking. If they do not deliver the immediate economic benefits, the direct burden of austerity on society is politically compounded by the detrimental effects via economic voting discussed earlier. These expectations have led scholars to turn towards the second important question on the political economy of austerity: do governments get systematically punished by their electorates when they adjust?

Empirical studies on the electoral consequences of fiscal adjustment, however have failed to confirm the widely expected punishment effect. The landmark study of Alesina at al (1998) finds no negative effects of adjustment periods on various measures of government termination. Blochliger et al. (2012) find in their sample of OECD countries that more than half of the governments that implemented adjustment were re-elected and some have continued their adjustment efforts afterwards. Alesina et al (2011) present similar findings and refute the very plausible objection on the grounds of reverse causality: perhaps, strong governments that have little to fear systematically initiate more adjustment episodes. By taking into account "government strength" in their empirical models, the authors show that this is not the case.[2]

In addition to the exclusive focus on adjustment periods by the studies cited above other scholars have looked at the aggregate electoral response to overall changes in the fiscal stance. The findings that emerge from this related strand of literature largely conform to these previous results. Brender and Drazen (2008) argue and demonstrate that the electorate at large is "fiscally conservative" as it systematically punishes high deficits. In her review on the political economy of fiscal deficits, Eslava (2011) discusses a number of country-level findings (Brender (2003) for Israel, Peltzman (1992) for the US, Drazen and Eslava (2010) for Columbia) that point in a similar direction. The counterintuitive result that emerges from these studies is that contrary to the main premises of political budget cycle theory, voters do not reward expansionary policies at the polls.

One possible way to reconcile these empirical findings with the opposite theoretical expectations is zooming in program-specific policy initiatives and

[2]For a contrary view, Mulas-Granados (2006:7) presents statistical evidence for electoral punishment in the wake of adjustment periods.

their electoral consequences. Voters may have a conservative attitude towards public borrowing overall but are still willing to defend public programs through organized interests or at the polls. The historical relationship between popular protest and austerity documented by Ponticelli and Vogth (2011) suggests that it indeed may be the case: while they may recognize the overall need for balancing the books, voters are ready to participate in collective action to defend their program-specific interests. This is confirmed by a recent examination of Greek popular protest in the country's recent struggle with austerity that finds that an astonishing 29% of respondents participated in strikes or demonstrations in the year of 2010 alone (Rudig and Karyotis, 2014).

When considering election outcomes in the wake of welfare retrenchment efforts, however, the results are considerably less conclusive. In a landmark study on this question, Giger (2010) shows only very limited impact of social policy retrenchment on election outcomes. Wenzelburger (2014) contends that the lack of electoral punishment can be partly explained by taking into account blame avoidance strategies that governments use to diffuse the issue in the campaign. It remains debatable, however whether blame avoidance strategies can be so systematically and effectively employed that they shelter governments from electoral punishment across space and time. Other studies further this debate in a promising direction from the standpoint of this article by highlighting that the partisan identity of parties that initiate adjustments matters. While parties with a positive welfare image tend to be punished after welfare retrenchment, traditionally more hostile parties towards the welfare state are not (Schumacher et al, 2013). Moreover, according to Giger and Nelson's (2011) findings, some parties – liberal and religious parties in particular – are even rewarded at the polls after social policy retrenchment. Quite opposite to what the blame avoidance literature leads one to expect, these parties are pro-actively claiming credit for their ability to implement successful retrenchment.

While the notion that the traditional supporters of public services and social programs have more to fear from austerity sounds intuitive, it sits uneasily with a relatively recent turn in welfare state scholarship. Cukierman and Tomassi (1998) formally model a policy-making context where credibility advantage on a given issue domains translates into a policy-position contrary to where the party traditionally stood. Such Nixon-goes-to-China policy dynamics are brought closer to our domain of interest by Levy (1999), Ross (2000) and Kitschelt (2001) who show that in contemporary welfare reforms, it was often progressive left-wing governments who found it electorally more palatable to cut popular programs.

This article contends that such Nixon-in-China patterns of policy-making are key to bridge the gap between the inconsistencies and often contradictory empirical findings of different literatures highlighted by this brief review above. In particular, for a more fine-grained theorization of the micro-logic that guides electoral choice, I turn to Albert Hirschman's Exit, Voice and Loyalty (EVL) framework to set up the empirical analysis that follows.

Theory: Exit, Voice and Loyalty

Albert Hirschman's classic work (1970) on the behavior of the individuals facing a decline in product quality has been applied for various political contexts by generations of political scientists. In its simplest form, a consumer dissatisfied with a product has three choices: 1) Exit, ie. abandon the product for one that is perceived as superior; 2) Signal her dissatisfaction to the producer by voicing discontent; 3) Remain loyal and hope for change. In political settings, as Dowding et al's review (2000) on the relevant literature argues, these options are not always straightforward to interpret. In repressive regimes, for instance, voice can be interpreted as collective action in demonstrations, or trying to change the regime from within by participating in its formal institutions (e.g. via party membership in the formerly communist one-party states). Exit, in turn, in its milder form could mean withdrawal from public life and in the extreme, emigration ("voting with one's feet").

In the electoral arena, the choices are equally, if not more tricky to pin down. In a recent formulation of the EVL framework, Weber (2011) argues that protest voting in second-order election is a form of voice; the voter is not ready to abandon her preferred party, but signals discontent by choosing a different party in an electoral context that is perceived to be of secondary importance[3]. In contemporary parliamentary democracies, more direct forms of voice, however, are extremely hard to express. With the "cartelization" of parties (Katz and Mair, 2009) and dwindling memberships (Biezen and Poguntke, 2014), an individual's access to decision-making in political parties is extremely limited. Participation in mass protests, in turn, is hindered by collective action problems (Olson, 1965; 1982) and the low expected likelihood of collective voice to bring about policy change.

Electoral exit is comparatively straightforward. Voting abstention is one obvious form of exit. It can result from alienation whereby a voter feels that parties have too little to offer to make it worthwhile for her to vote, or indifference whereby her preferred policy is equidistant from the competing parties' electoral platforms (Brody and Page, 1972). Alternatively, a more

[3] See also Hix and Marsh (2007) for an application of protest voting in the context of the European Parliamentary elections.

radical form of exit from one's preferred party is vote-switching: a voter chooses a different party from the one she has traditionally (or most recently) voted for.

Finally, the concept of loyalty is crucial to understand electoral stability over time. Dowding et al (2000) conceptualizes loyalty as a value-based attachment to and identification with a group. In electoral terms, the importance of party-identification was put forward as early as Campell et al's path breaking work (1960) which has later become known as the Michigan-model[4]. Party identification serves as an important conceptual chain separating the preferences of an individual from her ultimate vote choice. In other words, for a party's policy shift to change an individual's party choice, it has to be large enough for the individual to be willing to pay the psychological "exit tax" (Dowding et al, 2000; Weber, 2011) that a changing party identification involves.

Fiscal adjustment, the subject of this article, is a convenient example of a policy shift on a single issue space ranging from economic orthodoxy (right) to economic activism (left). Importantly, parties are perceived to be located on certain points on the issue-space before the policy shift. Parties that tend to constitute governments in OECD countries (see the empirical section for details) range from center-left to center-right on this policy space. Center-left parties include social-democratic and labour parties, whereas center-right parties belong to christian-democratic, liberal and conservative party families. Camia and Caravani (2012) have recently demonstrated that the latter three parties in Western party systems occupy very similar positions on the economic dimension, clearly separating them from their social democratic

[4] See also Clarke (1998) for a review on the conceptualization and the evolution of partisan identification over time.

counterparts. For the purposes of this article, it is therefore fruitful and convenient to consider them collectively as center-right.

Beyond these mainstream choices, a number of party systems offer voters a choice to vote for more extreme alternatives. Extreme-left and green parties tend to occupy a position left of the mainstream governing alternatives whereas radical right parties often mix economic nationalism, welfare chauvinism and neo-liberal ideas in their rhetoric (Kriesi et al, 2006; Rennwald and Evans, 2014) making their left-right position on the economic domain harder to identify. What unites these parties, however, is a frequent display of economic populism (Zaslove, 2008) and extreme positions on a number of non-economic issues, such as euroscepticism (Halikiopoulo et al, 2012), and the so-called "new right" issues such as immigration (Kriesi et al, 2006). Moreover, most of these parties tend to be perennial opposition parties with little governing experience.

To anticipate the electoral fate of different party types upon fiscal adjustment, it is crucial to keep in mind their relative position on the one-dimensional policy space. Figure 1 and Figure 2 offer such a stylized depiction of a one-dimensional policy space lining up parties with preferences for less (left) to more (right) fiscal adjustment. For the initial discussion, it is helpful to simplify the analysis to two mainstream parties (left and right labelled "L" and "R" respectively) and one party with extreme-left fiscal preferences, labelled "EL". Since most parties belonging to the extreme-left and green party families all satisfy this criterion and many of the radical right wing parties tend to be vocal critics of fiscal adjustment programs, this stylized illustration is a plausible reference point for the analysis to follow. I also assume that the electorates' fiscal preferences are single-peaked and normally distributed: the electorate's density distribution peaks at the middle of the policy-space. In other words, I assume that most voters are centrist and only a relatively small subset of the electorate holds extreme preferences for extremely tight and loose fiscal policy[5].

Figure 1: Electoral consequences of fiscal adjustment by the center-left

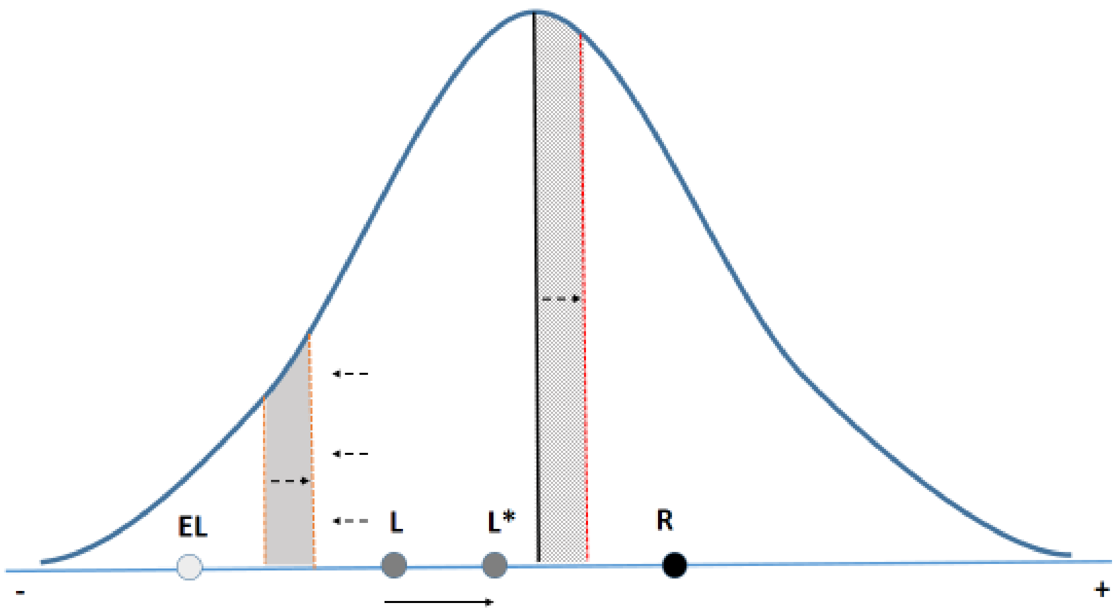
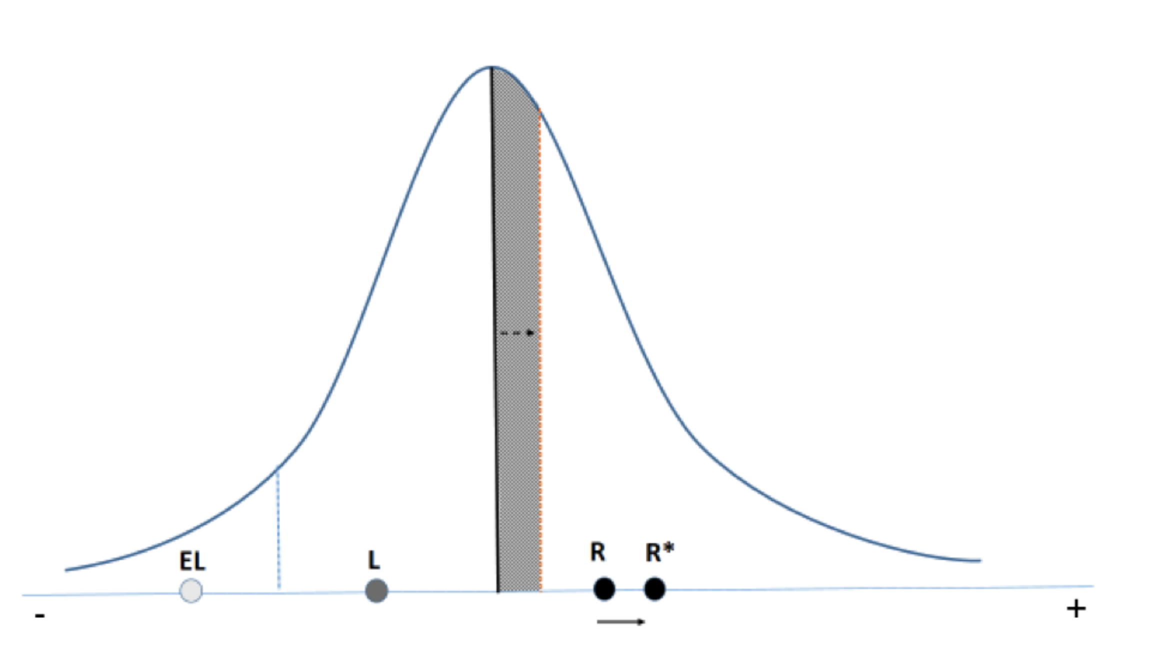


Figure 1 captures the electoral consequences of fiscal adjustment undertaken by the Left. Implementing a policy contrary to its (and its constituents') fiscal preferences amounts to a relatively large right-ward shift on the policy space, indicated by a move towards L*. This has potentially important consequences for left-wing voters. On the one hand, the Left is vulnerable to losing support towards the left-end of its electoral coalition. Following the EVL framework laid out above, lacking effective voice strategies[6] to implement policy change these voters could exit via abstention or via voting for a more extreme left-wing alternative.

[5] By analyzing public attitudes on the preferred pace of debt consolidation, Stix (2013) and Hayo and Neumeir, (2013) report evidence consistent with this assumption from Austria and Germany, respectively.

[6] Protest voting at second-order elections to voice discontent is not an available option when analyzing vote choice at parliamentary elections.

Figure 2: Electoral consequences of fiscal adjustment by the center-right



However, a number of considerations suggest that mass exit will be limited. Most importantly, a large part of voters remain loyal despite the austerity shock because they find the "exit tax" discussed above prohibitively expensive. Those sufficiently alienated by the adjustment policy to desert the party will find voting for the extreme left alternative difficult. First, they may perceive these extremes to hold views on non-economic issues contrary to their own (the position taken on the post-material left-libertarian/right-authoritarian axis (Benoit and Laver, 2006; Inglehart, 1977; Kriesi et al, 2006) being a prominent example). Second, extreme parties' lack of governing experience and mainstream parties' regular efforts to portray them as lacking competence to manage the economy in challenging times may make voting for them unappealing. Third, in majoritarian electoral systems where strategic calculations are important, they may be reluctant to waste their votes on small and isolated parties without governing potential. Therefore, a number of forces indicated by the dashed arrows pointing leftwards will limit the electoral space that the left loses to its more extreme rivals. To the extent that electoral exit does occur, it will mostly manifest itself in electoral abstention, which results in a considerably more limited dent in the left's electoral strength than desertion to other parties would do.

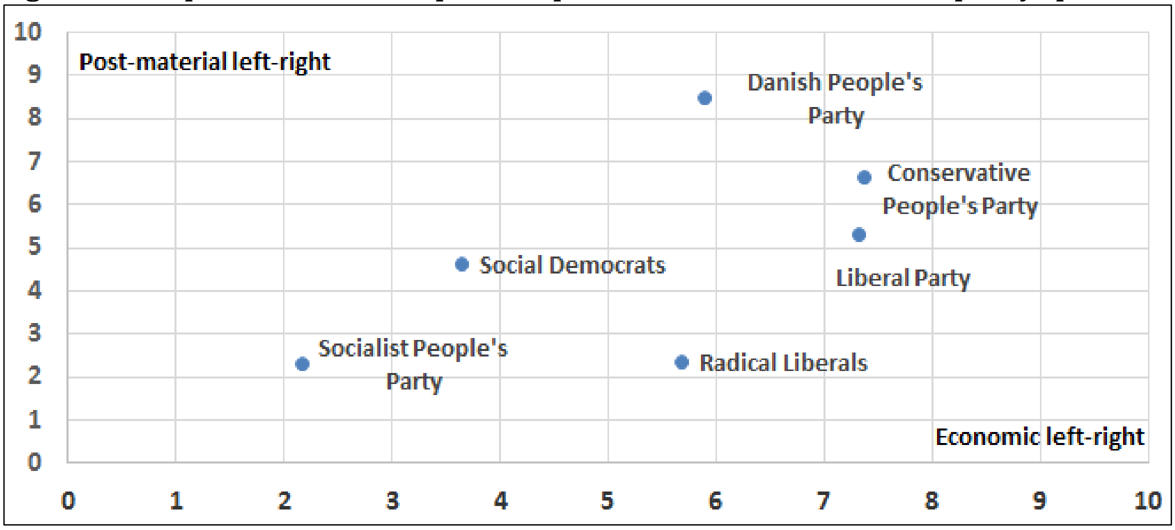
Moreover, electoral losses among its anti-austerity constituencies can be counterbalanced by new voters entering the left-wing coalition. Voters around the center of the issue-space, in particular, may find the left-wing party more appealing once they demonstrate the political resolve to implement economies policies that they perceive as "sound and responsible". Some of these centrist voters may have been sympathetic to the left for non-economic reasons but may have found their economic position too left-wing. Once the left converges to the middle of the issue space by the fiscal adjustment, some of these centrist voters may therefore switch sides. Although such switching will also be limited by loyalty considerations on the right, the other sources of electoral stability discussed above will play no major role: by demonstrating its willingness to tighten the purse when necessary, the left can thus counterbalance its losses on the left by gains at the center. To the extent that the electorate's density distribution is fundamentally centrist, such electoral gains could even result in net gains for the left-wing electoral coalition despite the losses they suffer on the margins.

Turning to figure 2 depicting a fiscal adjustment undertaken by the right, the expected electoral consequences are fundamentally different. Since the right is perceived to have a greater attachment to economic orthodoxy than the left, the fiscal adjustment results in a smaller shift to the right on the economic issue-space. Crucially, however, this shift takes place *away* from the center of the space – indicated by the dashed arrow pointing right-wards – leaving an important segment of the centrist bloc up for grabs for the left. Again, although electoral exit will be limited by loyalty considerations, the reasoning provided before still holds: disaffected centrist voters willing to pay the "exit tax" now have a convenient alternative to turn to: the mainstream-left with a less pro-austerity agenda. Moreover, unlike the left capitalizing on its ability to sway voters at the center when implementing adjustment, the right is unable to mitigate its losses by searching for new voters at the right tail of the voter distribution. On the economic domain, mainstream right parties are unlikely to capture new voters, since it had few rivals with a more orthodox economic platform to begin with. In other words, fiscal adjustment undertaken by the right forces the party in the electoral corner of the economic issue-space leading to vote losses at the center.

The right's electoral difficulties are further compounded by the nature of many multi-party systems in the democratic world. Whereas the left shares the left space of the economic domain with rivals widely perceived to be inexperienced and extreme (see discussion above), the right side of the issue-space is often divided up by multiple parties belonging to Christian democratic, liberal and conservative party families. Whenever at least one of these is in opposition during fiscal adjustment implemented by the right, disaffected voters have further alternatives to turn to at low exit costs.

These abstract and general considerations are illustrated below by one of these multi-party systems in Europe: that of Denmark. Taking into account the fact that party politics is not uni-dimensional, I placed parties based on Chapel Hill expert surveys (Bakker et al, 2012; Hooghe et al, 2010; Steenbergen and Marks, 2007)) on two salient dimensions: the economic domain under discussion (x-axis) and the clash between the left-libertarian-right-authoritarian value systems (y-axis) consisting of such post-material issues as immigration, multiculturalism, gay rights, the environment etc. Party positions are numerical averages across 4 waves of surveys conducted in Denmark on the respective dimensions. On both dimensions, lower scores correspond to more left-wing positions.

Figure 3: The position of Danish political parties on a two-dimensional policy space



Source: Chapel Hill Surveys (average across the waves)

The positions of the seven major Danish political parties illustrate reasonably well the preceding discussion. The Danish center-left represented by the Social Democrats (SD) has only one competitor on the economic-left: the Socialist People's Party (SF). To the extent that vote losses to this left-wing competitor are contained by the considerations above, the SD, when in government, can implement austerity relatively safely by moving to the center of the issue space in the process. This policy, however, seems considerably riskier for the three center-right parties in Denmark: the Radical Liberals (RV), the Liberals (V) and the Conservative People's Party (KF). By moving to the right, not only do they leave open the center of the policy-space to the center-left, they may also lose votes to their center-right rivals as well as to the Danish radical right - Danish People's Party (DF) - due to their proximity either on the economic or the cultural dimension (or both). This tentative illustration on a single country thus suggests that fiscal adjustment undertaken by the right, contrary to much of the literature and a lot of popular commentary, is electorally riskier than that undertaken by the left. The formal hypothesis that I will test in the empirical sections below can thus be formulated as follows:

H: The center-left in government is electorally immune to austerity; the center-right, by contrast is electorally vulnerable to a significant amount of vote loss.

Data and Measurement: Operationalizing Electoral Strength, Fiscal Adjustment and Partisanship

Given the political micro-logic of fiscal adjustment laid out above, it would be theoretically appealing to rely on micro-level data to (dis)-confirm whether individuals with different fiscal preferences cast their vote according to our expectations. However, electoral survey data have a number of limitations. Most importantly, there are very few longitudinal surveys that are cross-nationally comparable, making it all but impossible to cover the vast empirical arsenal of adjustment episodes across space and time. The widely used alternatives, cross-sectional surveys typically obtained from electoral study programs or cross-national research projects (International Social Survey Project, European Social Survey, Eurobarometer etc.), do not typically allow the researcher to trace the evolution of fiscal preferences of individuals beyond the actual point in time when the survey is taken. A partial exception is offered by the four modules of the Comparative Study of Electoral Systems (CSES) project that include questions on previous vote choice of individuals. However, it is methodologically dubious to rely on self-reported vote choice; as argued above, vote-switching – our central point of theoretical interest – requires a quiet demanding cognitive process (the "exit tax"), possibly introducing a lot of false reporting and systematic measurement error in the surveys. Finally, an empirical strategy linking fiscal adjustment to electoral behavior is severely limited when the survey dates are temporally far removed from fiscal adjustment episodes.

For these reasons, I opted to conduct the empirical analysis on the aggregate level. If our theoretical expectations are to be confirmed empirically, the aggregated impact of individual-level vote switching and turnout decisions should show up in the changing electoral strength of political parties. Contrary to much of the economic voting literature that uses election outcomes (Hellwig and Samuels, 2007; Hodgson and Maloney, 2012; Powell and Whitten, 1993) as the dependent variable, I focused on the popularity aspect of the so-called Vote-Popularity Function (Lewis-Beck and Paldam, 2000). In line with studies that use vote intentions as opposed to actual vote shares as the dependent variable (Bellucci and Lewis-Beck, 2011; Stegmaier and Lewis-Beck, 2011), this choice allowed me to trace the evolution of electoral strength in a continuous fashion at equal intervals, increasing the number of observations in the analysis. This is especially important when one is interested in the electoral impact of a political event which is temporally distributed in a non-random fashion vis a vis the electoral cycle. There are serious grounds to suspect, that in our case, this is a valid concern, as empirically verified by the political budget cycle literature (Alt and Lassen, 2006; Haan and Klomp, 2010; Shi and Svenson, 2003) on the one hand and related findings in the fiscal adjustment debate on the other (Alesina, 2006). To the extent that incumbents systematically undertake adjustment in the early phases of the electoral cycle, it is highly dubious to what extent electoral outcomes several years down the road pick up that effect.

The vote intention panel structures that my choice fell upon were thus constructed as follows. I collected polling data from publicly available sources and consulted polling agencies – see Table II in the Appendix for details - for additional material when necessary. Overall, I was able to build a vote-intention dataset[7] consisting of 96 political parties nested in 21 parliamentary democracies. I included parties only with sizeable support (2+ % of respondents) and constant parliamentary representation throughout the study period (1970-2013). The length of the series thus depended on data availability as well as on parties' formation and entry to (or occasionally exit from in case of break-up, merger or dissolution) the respective party systems. The sample of countries provides sufficient variation in different important dimensions of party systems, such as the electoral formula, number of electorally relevant parties and the stability/volatility of the party system itself. In addition to consolidated parliamentary democracies, it also includes five relatively new democracies among the formerly communist countries of the European Union. Further details of the sample characteristics are in Table I in the Appendix.

Although the vote intention indicator is typically measured on a monthly (in a few cases quarterly) basis, using annual units in the empirical analysis is preferable as fiscal adjustment is measured on an annual basis (see the discussion below). I thus took a numerical average of all the monthly/quarterly measures for a given year and used that as the dependent variable for the analysis. Moreover, to ward off concerns about selection bias identified by the literature (Alesina et al, 2011) emanating from the fact that strong/popular governments may be more likely to undertake adjustment, I used the first difference of the vote intention variable. In other words, the empirical models seek to predict the *changes* in the electoral strength of parties, rather than

[7] The only exception is France where the popularity measure indicates the percentage of respondents expressing a positive view on the political party.

electoral strength itself. Estimating changes are less likely to be tainted by selection bias as governments are unable to time fiscal adjustment according to the vote loss/gains these adjustments may entail.

Operationalizing adjustment episodes, the main explanatory variable of the study, also poses a number of difficulties. Following Alesina et al's (1998) landmark paper on the political economy of fiscal adjustment, a lively debate has ensued on the merits of the authors' coding scheme. They define years of fiscal adjustment when the cyclically adjusted primary balance improves by at least 1.5% of (potential) GDP (ibid, p.201). While admitting the somewhat arbitrary nature of this threshold, subsequent contributions to the debate defend this choice by highlighting the fact that it is high enough to exclude episodes that are not strictly speaking adjustment years (Alesina et al, 2011):
"With the definition of a "large fiscal adjustment", and given that the deficit is cyclically adjusted, one tries to capture years in which fiscal policy was decisively contractionary with, most likely, active discretionary fiscal policies which were not business as usual or the result of the cycle" (p.5)

The most prominent criticism of this measurement strategy can be traced to the seminal contribution of Devries et al (2011) who argue that cyclical adjustment is a highly imperfect measure that fails to take into account such windfall revenues as property taxes during a housing boom. Therefore, the authors propose to shift the focus from fiscal outcomes to the underlying policy change (budgetary acts) to define adjustment episodes. Despite the merits of their argument, I am uneasy about using their dataset for the purposes of this article. First, the adjustment years that the authors focus on pay little attention to implementation; it is not all that clear whether adjustment budgets voted upon in parliament actually resulted in the intended fiscal changes. Second, their dataset is limited both temporally and geographically, significantly reducing our empirical leverage (by the exclusion of Eastern European countries for instance). Third, to the extent that those windfall revenues that the authors identify are positively correlated with the business cycle (Barrios and Rizza, 2010 p. 11), cyclical adjustment at least partly corrects for the temporary nature of those revenue items. For these reasons, this article follows the outcome-based approach and uses the 1.5% benchmark as the starting point. In our robustness checks, acknowledging the arbitrary nature of the threshold, we modify the threshold in two directions to see if the results are robust to a broader/stricter definition of adjustment years.

Regarding the conditioning impact of partisanship to measure the impact of austerity on popularity, we follow a two-pronged approach to identify the partisan standing of political parties on the economic left-right domain. The conventional approach, as typically used in welfare-state studies follows from the logic of the power-resource theory (Korpi, 1983; 2006, Huber and Stephens, 2001), according to which party families reflect long-standing ideologies and allegience to different socioeconomic groups that largely determine the macroeconomic policies that different parties pursue when in government (Hibbs, 1977; Cusack, 2001). In this vein, our first variable identifies (center)-left parties that belong to the Social-Democratic party family and (center)-right parties that belong to Christian-Democratic, Conservative and Liberal party families[8]. Parties belonging to agrarian, green, extreme-right, extreme-left and other (e.g. regional or ethnic parties) were coded separately.

Recognizing the heterogeneity of parties' platform and their constituencies' composition within the same party family, I used an alternative variable of a continuous form to place parties on the left-right domain. To do so, I relied on

[8] To classify parties, I relied on the party family variable in the Comparative Manifesto Project, cross-validated by parties' membership in international political organisations, such as parliamentary groups in the European Parliament and Internationals.

the most extensive cross-national expert series projects that place parties on a left-right scale in a number of dimensions. For EU member states, I relied on Chapel Hill surveys (Bakker et al, 2012) and used the average left-right score across all waves of the survey series as my partisan variable. For non-EU democracies (Australia, Canada, Iceland and Norway in my sample) where Chapell Hill scores are not available I relied on Benoit and Laver's (2006) expert survey and used the tax-spending variable as the best proxy for the partisan variable of interest. In both surveys, parties are assigned scores between 0 and 10 with higher scores indicating a more right-wing position. We prefer using these expert surveys to the most commonly used alternative, the Comparative Manifesto Dataset because our theoretical considerations rely on the cumulative perception of parties' economic ideology in voters' minds over many years or even decades. We expect a sufficiently large number of country-experts to provide a more accurate picture of this perception than a numerical measure of policy emphasis on a single written document at any given election[9].

Finally, it is important to control for a number of variables which may correlate with our key independent variables of interest possibly biasing the estimates. First, in the footsteps of the economic voting literature, I control for GDP *growth, unemployment* rate (both changes and levels) and *inflation* (both changes and levels) to filter out the electoral effect of adjustment which occurs through its immediate economic impact. While GDP growth is theoretically speaking a change variable itself, it is *a priori* less clear whether voters prioritize changes or levels of unemployment and inflation when attributing responsibility. For these two variables, therefore, I follow a "let the data speak" approach and include both levels and changes in the models.

[9] The reliability of CMP estimates has been subject to a long-standing debate and various criticisms (see Budge and Bara, 2001 for a critical review).

Furthermore, I use a set of dummy variables to model the evolution of popularity over the electoral cycle (*election year, post-election year, pre-election year* with the remaining "mid-cycle years" as the reference category) to allow for the possibility that different partisan governments time their adjustment policies in a different manner. Finally, I introduce an additive index capturing institutional and political *constraints* (bicameralism, federalism and coalition government) that governing parties are under to capture varying levels of clarity of responsibility for policy outcomes (Powell and Whitten, 1993; Whitten and Palmer, 1999).

Estimation, model-set up and results

Equipped with all the necessary variables to investigate the electoral impact of fiscal adjustment, I set up a cross-section time-series model of the following general form.

$$\Delta VI_{pt} = \beta_0 + \beta_i (A^*P^*G) + \beta_j (C^*P^*G) + \alpha_{p+} \mu_t + \varepsilon_{pt}$$

Where ΔVI_{pt} is the annual change in the vote intention share of party p at time t, β_0 is a regression intercept, β_i is a vector of coefficients that estimate the interaction effect between the fiscal adjustment dummy (A), the partisanship variable(s) (P) and the government status dummy (G)[10] as well as its constituent terms, β_j is a vector of coefficients estimating the interaction effect between the partisanship variable(s) (P), the government status dummy (G)

[10] For the government status dummy, parties were coded 1 when they were in power at the *beginning* of the calendar year. For years of government change, any coding scheme is admittedly problematic; my coding decision is motivated by the consideration that annual budgets are prepared in the year before hence the parties at the power during the preparation face are likely to be held at least partly responsible.

and a set of control variables (C) as well as its constituent terms, $\alpha_p + \mu_t$ are p-1 and t-1 party, and time-dummies, respectively and ε is the error term.

Because the data structure (temporally sequential observations clustered in parties that are in turn clustered in countries) is likely to violate the i.i.d. (independent and identically distributed) assumption of the errors, pooled OLS regressions are likely to introduce bias in the estimates and/or in their standard errors (Beck and Katz, 1995; Kittel and Winner, 2005). Serial correlation, a common concern in time-series cross-section designs is a relatively minor problem for us because of the first-difference specification of the dependent variable. There is no theoretical reason to expect changes in popularity in $year_t$ to be systematically correlated with the change in $year_{t+1}$; in fact the correlation coefficient between the dependent variable and its first lag is only 0.11 in the sample. Panel heteroskedasticity (error variances differing across the units) and cross-sectional dependence, on the other hand, are more serious concerns. It is important to recall that in our data structure, party-years, rather than country-years are the units of analysis. Parties of different sizes are likely to exhibit different variation in electoral strength around their mean and we are unable to explicitly model all the sources of this variation. As a result, the variance of the errors is likely to systematically differ between parties, violating the homoscedastic error assumption. Moreover, since vote intention is measured in % of total respondents with a party preference, electoral support for parties in the same country are likely to correlate negatively with each other. If one party's support drops for reasons unaccounted for in the models, it automatically translates into higher support for the other parties even if the absolute electoral strength of the latter party(ies) remains constant. For this reason, the party-specific errors within the same country are likely to violate the cross-sectional independence assumption. A series of statistical tests – results available in the regression output tables below – confirmed these expectations: the variance-covariance matrix of the errors violates a number of the i.i.d. assumptions. I thus proceeded to follow the "Beck and Katz standard" (Beck and Katz, 1995; Beck, 2001) and ran OLS regressions with panel-corrected standard errors and (party-specific) fixed-effects[11]. The inclusion of the latter takes care of the possible correlation between the main independent variables of interest and party-specific error terms, causing omitted variable bias in the estimates.

The first sets of empirical models – results shown in Table 1 below – are presented as follows. First (column 1), I set up a baseline economic voting model where the change of vote intentions is regressed on the standard economic variables (gdp growth, levels and changes in unemployment and inflation) and their interaction with a government status dummy. Second, (column 2) adds the fiscal adjustment dummy as a fourth regressor, again interacted with the government status dummy to investigate unconditional effects of fiscal adjustment on governing party popularity. The third model (column 3) adds the additional controls (electoral timetable and the index on government constraints) in interaction with government status. From the regression output, I suppress the party-specific fixed effects to keep the output table at a manageable length.

Table I: Regression output table for the baseline specifications

	Δvoteint	Δvoteint	Δvoteint
Growth	-0.034	-0.029	-0.032
	(0.70)	(0.60)	(0.70)
Government	-0.987	-0.929	-1.563
	(2.05)**	(1.96)*	(2.61)***
government*growth	0.116	0.109	0.073
	(1.08)	(1.02)	(0.72)
unemployment	0.042	0.039	0.038
	(1.32)	(1.22)	(1.19)

[11] Lacking a theoretical reason for common shocks to affect popularity changes across the board, I did not include year fixed-effects in the models; introducing year dummies, however, do not change the substantive nature of the results.

government*unemployment	-0.138	-0.129	-0.105
	(3.04)***	(2.82)***	(2.34)**
Δunemployment	0.061	0.065	0.059
	(0.52)	(0.55)	(0.51)
government* Δunemployment	-0.745	-0.754	-0.827
	(3.09)***	(3.17)***	(3.57)***
inflation	0.014	0.019	0.030
	(0.42)	(0.56)	(0.88)
government*inflation	-0.003	-0.002	-0.023
	(0.04)	(0.03)	(0.39)
Δinflation	-0.014	-0.008	-0.009
	(0.29)	(0.16)	(0.19)
government*Δinflation	-0.212	-0.217	-0.210
	(2.28)**	(2.34)**	(2.33)**
adjustment		0.099	0.136
		(0.37)	(0.51)
government*adjustment		-0.759	-0.696
		(1.41)	(1.27)
election			-0.441
			(1.80)*
government*election			1.713
			(3.67)***
pre-election			-0.218
			(0.98)
government*pre-election			0.883
			(1.96)*
post-election			-0.144
			(0.60)
government*post-election			0.319
			(0.72)
constraints			0.282
			(1.30)
government*constraints			-0.082
			(0.34)
R^2	0.10	0.10	0.11
P-value for Modified Wald test χ^2 for panel heteroskedasticity	<0.000	<0.000	<0.000
P-value for Pesaran CD-statistic for cross-sectional independence	0.11	0.12	0.08
N	2,442	2,379	2,293

* *p*<0.1; ** *p*<0.05; *** *p*<0.01

Table 2 builds on this last, fully specified model and adds the partisan variables. The first two partisan models (columns 4 and 5) introduce two dummy variables for center-left and center-right governments, respectively. The final model (column 6) captures partisanship on a 0-10 scale from the expert surveys discussed earlier and uses this continuous variable in interaction with the other explanatory variables (government status and adjustment dummy) to probe the conditioning effect of partisanship on fiscal adjustment's electoral impact. From Table 2, due its extended length as a result of the three-way interactions, I also suppress the control variables (electoral timetable dummies and constraints) and their interactions in addition to the party-specific fixed effects. Moreover, to further ease interpretation of these interactive models, I follow Brambor et al (2006) and present marginal effects plots for the fiscal adjustment dummy for different combinations of government status and partisanship. This is especially crucial to interpret the three-way interactions from Table 2.

Table II: Regression output table for the fully-specified 3-way interaction models for alternative (center-left dummy, center-right dummy, left-right score) measures of partisanship

	Δvoteint	Δvoteint	Δvoteint
adjustment	-0.038	-0.099	0.162
	(0.16)	(0.32)	(0.42)
government	-2.147	-1.460	0.471
	(3.15)***	(1.55)	(0.27)
adjustment*government	-1.094	0.403	1.180
	(2.24)**	(0.58)	(0.89)
centerleft	2.750		
	(1.01)		
adjustment*centerleft	0.406		
	(0.55)		
government*centerleft	1.022		
	(0.65)		
adjustment*government*centerleft	1.585		
	(1.36)		
growth	-0.058	0.072	0.161
	(1.25)	(1.18)	(1.42)
government*growth	0.103	-0.070	-0.188
	(1.01)	(0.46)	(0.65)
centerleft*growth	0.191		
	(1.15)		
government*centerleft*growth	-0.116		
	(0.44)		
unemployment	0.069	-0.019	-0.017
	(1.94)*	(0.28)	(0.13)
government*unemployment	-0.074	-0.109	-0.348
	(1.28)	(1.46)	(2.25)**
centerleft*unemployment	-0.036		
	(0.31)		
government*centerleft*unemployment	-0.170		
	(1.33)		
Δunemployment	-0.053	0.236	0.294
	(0.47)	(1.42)	(1.07)
government* Δunemployment	-0.663	-1.237	-1.343
	(2.62)***	(3.51)***	(1.84)*
centerleft* Δunemployment	0.527		

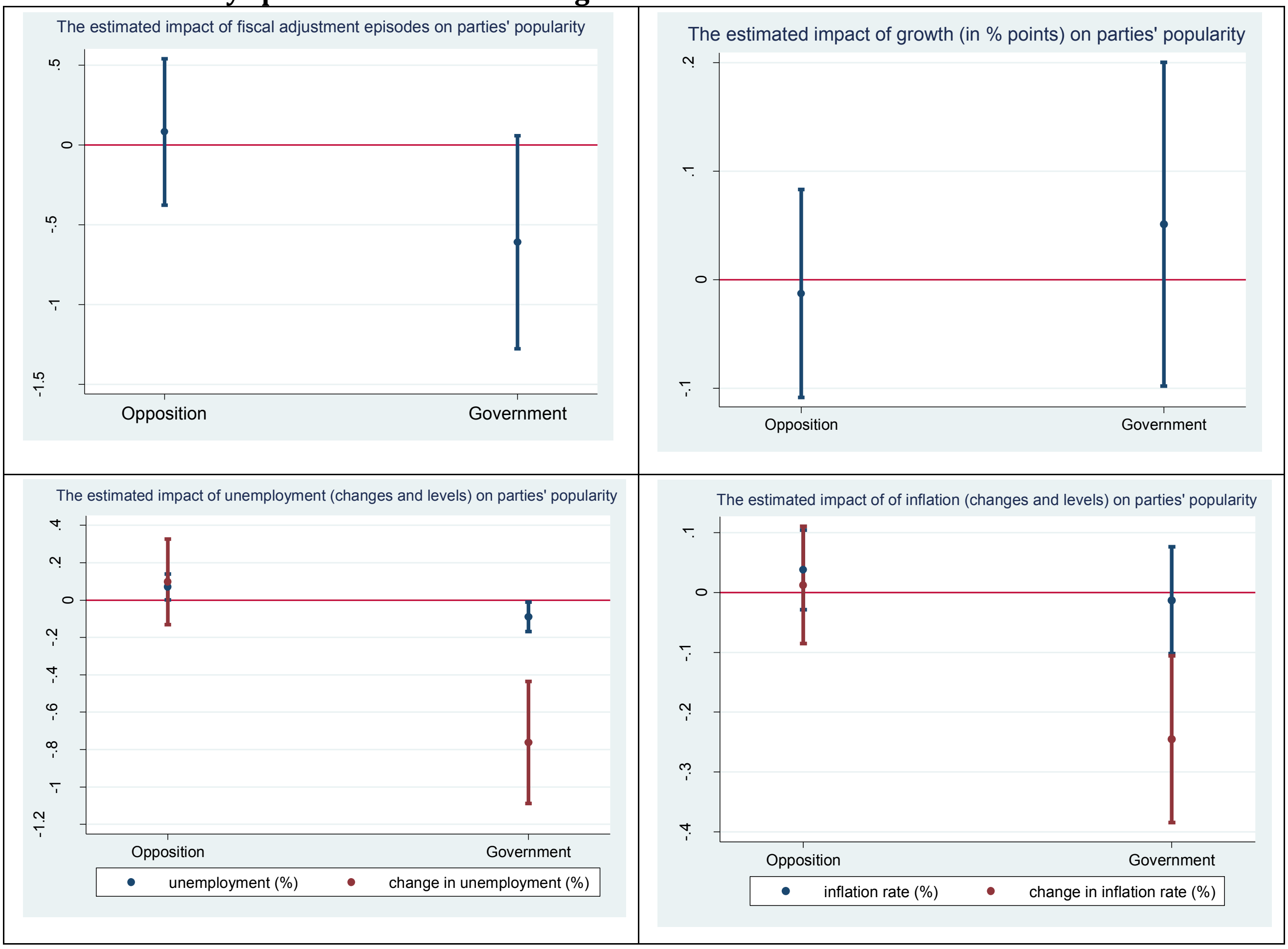
	(1.46)		
government*centerleft* Δunemployment	-0.692		
	(1.14)		
inflation	0.038	-0.009	-0.011
	(1.08)	(0.21)	(0.18)
government*inflation	-0.052	0.013	-0.123
	(0.83)	(0.15)	(0.91)
centerleft*inflation	-0.033		
	(0.29)		
government*centerleft*inflation	0.084		
	(0.62)		
Δinflation	-0.019	0.049	0.140
	(0.34)	(0.79)	(1.41)
government* Δinflation	-0.269	-0.097	-0.167
	(2.75)***	(0.71)	(0.65)
centerleft*Δinflation	0.082		
	(0.50)		
government*centerleft*Δinflation	0.067		
	(0.29)		
centerright		-2.273	
		(1.33)	
adjustment*centerright		0.214	
		(0.46)	
government*centerright		-0.374	
		(0.29)	
adjustment*government*centerright		-1.814	
		(2.08)**	
centerright*growth		-0.201	
		(2.06)**	
government*centerright*growth		0.251	
		(1.37)	
centerright*unemployment		0.131	
		(1.42)	
government*centerright*unemployment		-0.033	
		(0.34)	
centerright* Δunemployment		-0.300	
		(1.17)	
government*centerright* Δunemployment		0.620	
		(1.35)	
centerright*inflation		0.064	
		(1.01)	
government*centerright*inflation		-0.051	
		(0.52)	
centerright* Δinflation		-0.115	
		(1.14)	
government*centerright* Δinflation		-0.260	
		(1.51)	
leftright			-2.111
			(0.29)
adjustment*leftright			-0.029
			(0.36)
government*leftright			-0.397
			(1.25)
adjustment*government*leftright			-0.331
			(1.42)
growth*leftright			-0.038
			(1.82)*
government*growth*leftright			0.050

			(1.04)
unemployment*leftright			0.014
			(0.65)
government*unemployment*leftright			0.039
			(1.41)
Δunemployment*leftright			-0.043
			(0.87)
government* Δunemployment*leftright			0.085
			(0.69)
inflation*leftright			0.008
			(0.68)
government*inflation*leftright			0.019
			(0.83)
Δinflation*leftright			-0.030
			(1.44)
government* Δinflation*leftright			-0.015
			(0.34)
R^2	0.13	0.12	0.12
P-value for Modified Wald test χ^2 for panel heteroskedasticity	<0.000	<0.000	<0.000
P-value for Pesaran CD-statistic for cross-sectional independence	0.11	0.11	0.13
N	2,141	2,141	2,141

* p<0.1; ** p<0.05; *** p<0.01

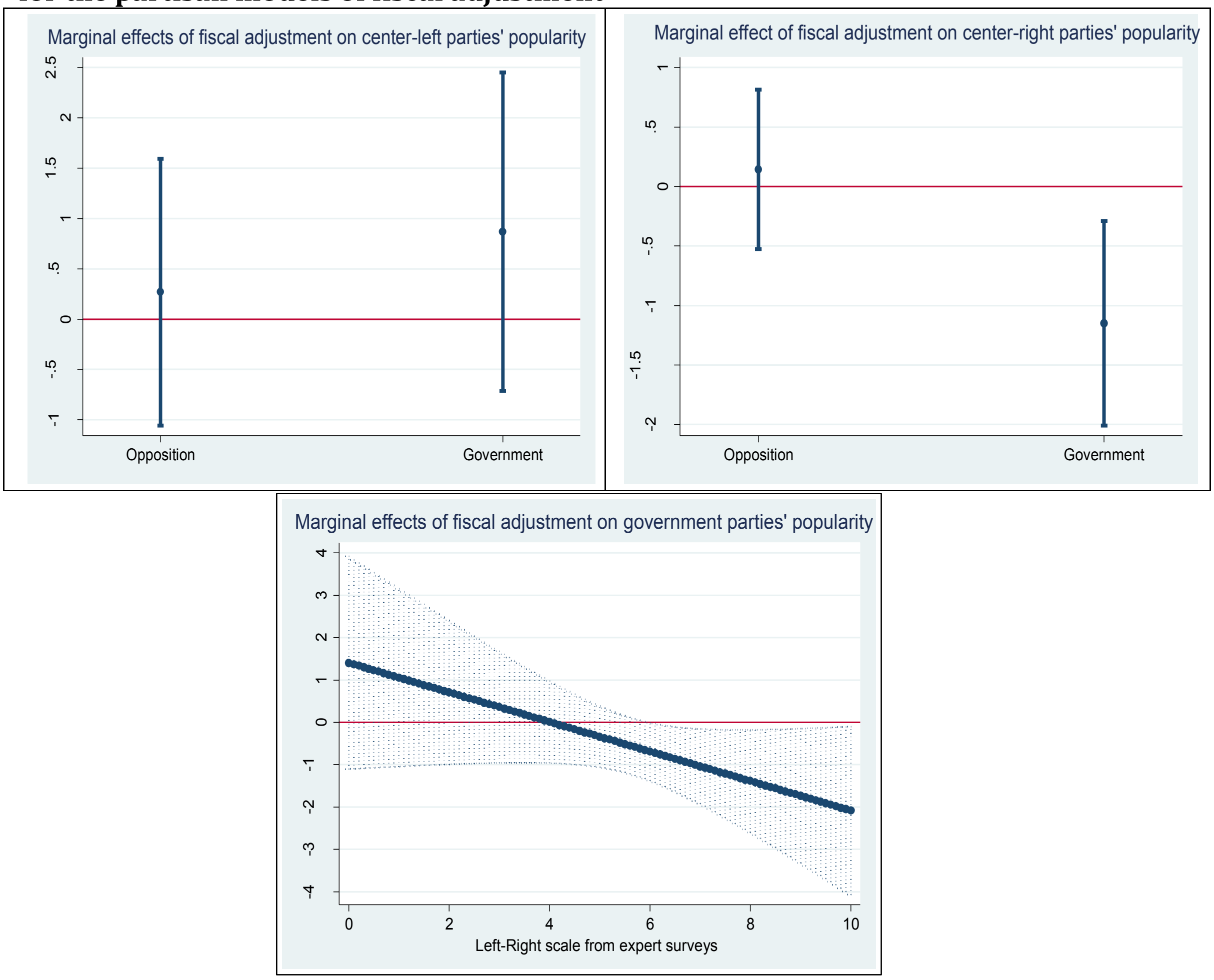
The calculated marginal effects from Table 1 reveal a relatively simple story. While the state of the economy clearly carries electoral risks for incumbents – government parties are punished for high levels of unemployment, rising unemployment and rising inflation – there is no comparable evidence found for fiscal consolidation episodes: although the estimated impact of an adjustment year on popularity change is negative (-0.59%), it doesn't reach statistical significance at the 5% level. Somewhat surprisingly, the impact of growth rates when controlling for the unemployment and inflation variables also stops short of statistical significance. The estimated marginal effects and 95% confidence intervals are shown below on Figure 4.

Figure 4: Marginal effects plots with point estimates and 95% confidence intervals from the fully specified economic voting model



When turning to the main results from the partisanship models, calculating marginal effects becomes somewhat more complicated. Now the marginal effect of an adjustment year is both a function of government status and partisanship. Figure 5 shows the calculated effects as a function of these variables, shown separately for the center-left dummy, the center-right dummy and the continuous partisanship measure obtained from the expert surveys. All three plots reveal the same pattern: for center-right parties presiding over austerity, the average annual impact on their popularity is around -1%, conveniently reaching statistical significance at conventional levels. For the center-left, on the other hand, no such punishment effect is found and the sign of the estimated marginal effect is positive (albeit non-significant). This pattern remains the same when partisanship is measured on the continuous scale: for parties that are scored 6 or higher by the country experts (0 being extreme left, 10 being extreme right), there is a clearly discernible punishment effect when these parties are in power during fiscal adjustment. For parties scored below that point, again, no statistically significant effect is found and the point estimate turns positive at 3 or below on the 10-point left-right scale. These results provide corroborative evidence for the main argument of this article.

Figure 5: Marginal effects plots with point estimates and 95 % confidence intervals for the partisan models of fiscal adjustment

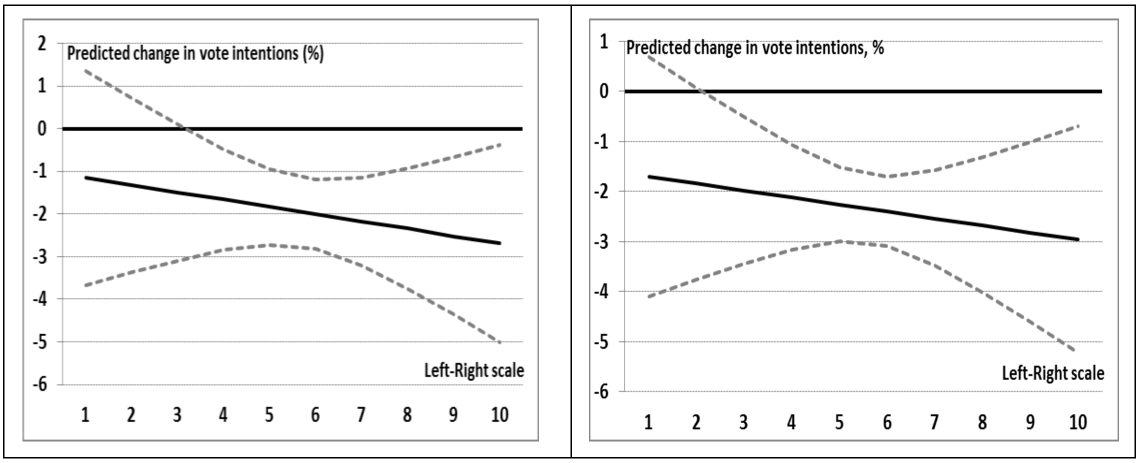


The next step in the analysis is making sense of the substantive impact of fiscal adjustment under different partisan governments. Two important considerations complicate a quick assessment of this substantive impact. First, the interactive specifications, as shown above, imply different marginal effects of fiscal adjustment at different levels (values) of partisanship. Second, the economic control variables (growth, unemployment and inflation) are likely to respond to fiscal adjustment hence a "ceteris paribus" assessment of the substantive impact of the latter makes little theoretical sense. The following hypothetical comparison thus assumes a scenario where different partisan governments engage in fiscal adjustment at the (sample) average level of macroeconomic conditions (inflation, unemployment and growth) in an electorally tranquil period (ie. middle of an electoral cycle) with average levels of government constraints (1.5). However, in response to this fiscal adjustment, growth is likely to drop in the short-run via its standard Keynesian channels (Monastiriotis, 2014) with a concomitant rise in unemployment (Okun's Law) and falling inflation as lower aggregate demand and higher spare capacity in the economy ease price pressures.

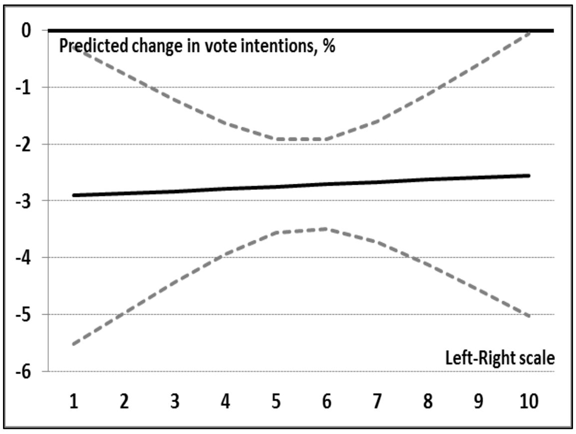
Since the vast empirical literature on fiscal multipliers and output gaps lies beyond the purpose of this article, this hypothetical scenario will be repeated for different assumptions on the response of growth, unemployment and inflation to fiscal adjustment relying on the empirical distribution of the relevant variables. In particular, I will compare three scenarios: 1) no response of macro-variables to fiscal adjustment, 2) moderate response whereby growth and inflation falls to the 25th percentile and unemployment rises to the 75th percentile in the empirical distribution of the variables in the sample and 3) strong response where growth and inflation fall to the 10th percentile whereas unemployment rises to the 90th percentile. While these thresholds are admittedly arbitrary, they merely serve as an illustration on how different parties' popularity evolves during years of fiscal adjustment and its possible recessionary consequences. The simulated response based on the model estimates are shown below on Figure 6:

Figure 6: Simulated changes of government parties' popularity in fiscal adjustment years under different macro-scenarios

Scenario 1 Scenario 2



Scenario 3



The first simulation (left panel), assuming a "ceteris paribus" scenario, confirm the previous findings. For left-wing governments, the expected loss of popularity is moderate, around 1% of respondents and for the left end of the spectrum, statistically indistinguishable from 0. As one moves to the right on the left-right scale, the predicted loss increases in years of fiscal adjustment, reaching 2% around a left-right score of 6 and approaching 3% towards the right end of the scale. In the second simulated scenario (middle panel), growth slows to 1.3% from the sample average of 2.5%, unemployment rises by 0.58% to a level of 8.5%, while inflation drops by 1.3% to an average annual rate of price increase of 2.1%. The qualitative pattern still holds: though all government parties' popularity ratings take a hit, the impact of fiscal adjustment compounded by its assumed effect on the economy impacts on right-wing parties more than on left-wing ones. Under the final scenario (right-panel), when fiscal adjustment takes a heavy toll on the economy (now the economy shrinks by -0.49%, unemployment increases by 1.37% to a level of 11.35% and inflation is reduced by 3.13% to a 1.2% annual rate), the previous patterns disappear and even slightly reverse. Compounded by its heavy recessionary impact, fiscal adjustment now entails heavy electoral losses for all political parties, ranging between 2.5 and 3% of respondents. To sum up, therefore, while the partial impact of fiscal adjustment on popularity has been shown to affect center-right parties only, all parties lose popularity in adjustment years when/if these periods are marked by recession, high and rapidly rising unemployment as well as falling inflation.

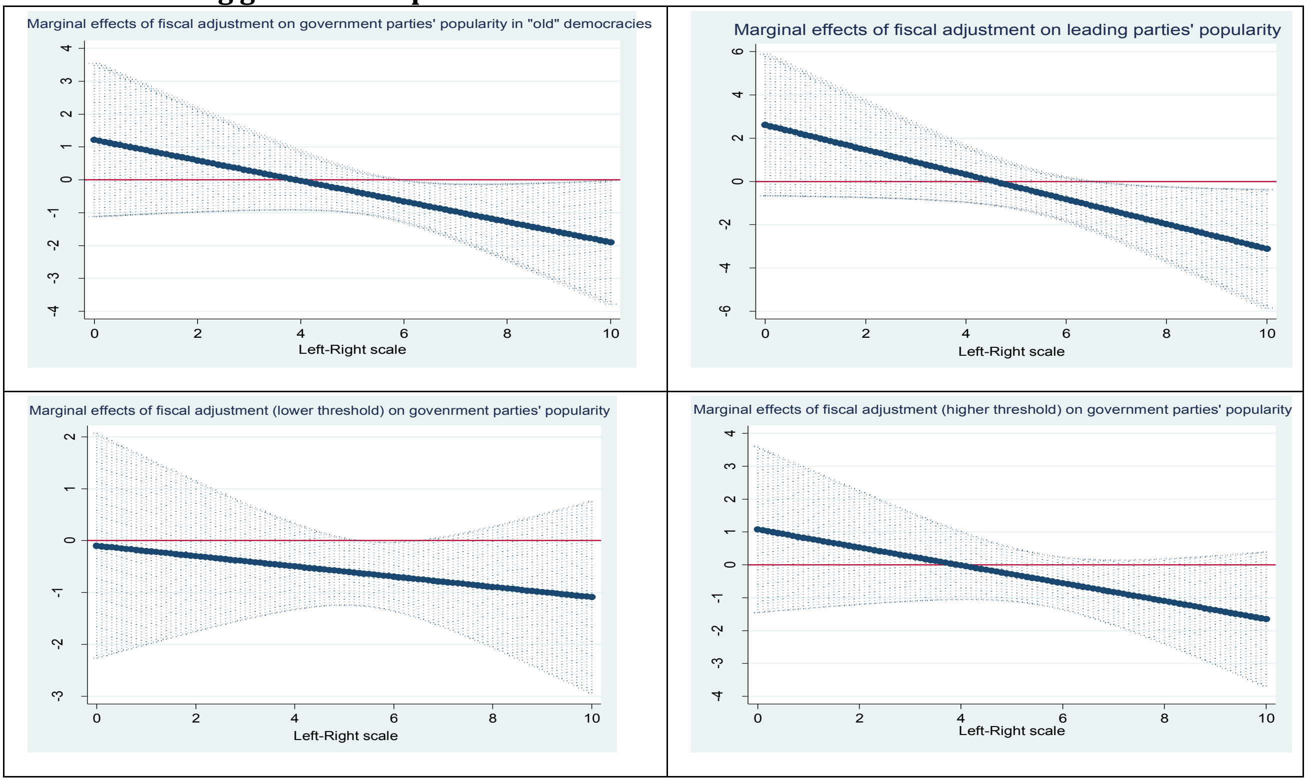
Robustness checks

There are two broad lines of possible objections that I seek to address in this penultimate section. First, the heterogenous country sample of the empirical analysis raises the possibility that "center-left" and "center-right" may mean very different kinds of parties in different party systems. Same goes for the continuous measure of partisanship; country experts from different political systems may have very different scoring criteria when placing parties on the left-right scale. This is a particular concern for post-communist democracies where the left-right cleavage tends to be more dominant in issues relating to the role of nationhood, minorities, religion, traditional values and more generally speaking, in issues rooted in different conceptualization of public morality (Kitchelt, 1992; Vachudova, 2008; Rovny, 2014). The second possible objection relates to measurement problems. Admittedly, the 1.5% of potential

GDP threshold is arbitrary when identifying years of fiscal adjustment. It is therefore important to analyze the stability of the results when different thresholds are used. Also, the analysis focused on government status, implicitly assuming that all government parties share equal responsibility for economic outcomes when facing the electoral verdict. This assumption, however, sits uneasily with the clarity of responsibility thesis (Powell and Whitten, 1993; Whitten and Palmer, 1999) in economic voting that posits that voters, on the aggregate, are very much capable of distinguishing between clear and blurred responsibility contexts. Although my additive control variable of government constraints partially addressed this issue, collapsing all government parties in the same dummy variable (government status) is still a potential concern.

To address these objections, I implemented three rounds of robustness checks in the analysis. First, I restricted the sample to "old" democracies to make sure that the results are not (partly) driven by the idiosyncratic politics of post-communism. Secondly, I modified the threshold of the fiscal adjustment dummy to 1.25% and 1.75% of GDP to address "type 1" (excluding fiscal adjustments when they should be included) and "type 2" (including fiscal adjustments when they should be excluded) errors. Thirdly, I replaced the government status dummy with a leading party dummy so that for coalition governments, only the largest party that delegates the prime minister is coded as the government party. This new coding scheme estimates the impact of fiscal adjustment only on one party for each country-year. The marginal effects plots for these robustness checks are shown on Figure 7 below, taking the left-right continuous measure as the partisan variable.

Figure 7: Robustness checks. Marginal effects plots with point estimates and 95% confidence intervals for old democracies, alternative fiscal adjustment thresholds and leading government parties



The upper-left panel in Figure 7 shows the results after taking the post-communist "new" democracies out of the sample. The overall pattern remains the same: positive and non-significant point estimates for center-left governments and statistically significant electoral punishment for center-right parties. The only slight difference is the somewhat smaller substantive impact of fiscal adjustment on center-right governing parties' popularity ratings compared to the larger sample. Moving on to the upper-right panel where the government status dummy is replaced with the leading party dummy, the same pattern holds with an even steeper slope. Confirming the clarity of responsibility thesis, when restricting the analysis to the largest governing party, the conditioning impact of partisanship increases: the marginal impact of fiscal adjustment for parties on the right end of the Left-Right scale surpasses 3% of respondents. Finally, varying the threshold for the identification of fiscal adjustment episodes, our previous results become somewhat weaker: while the overall pattern (negative slope for the point estimates as a function of partisanship) remains, under the alternative thresholds, almost all point estimates are now statistically indistinguishable from 0.

One possible interpretation runs as follows. For the lower threshold (bottom-left panel), the fiscal adjustment variable now includes a large number of years where fiscal tightening might have resulted from circumstances other than deliberate policy design (Devries et al, 2011), weakening the reliability of the adjustment measure. For the higher threshold (bottom-right panel), we may be encountering the opposite problem: though putting the threshold high ensures that only real adjustment years fall under our measure, the low number of positive observations (i.e. adjustment years) increases the standard errors of the estimates, making statistical inference difficult. This interpretation is consistent with the fact that the slope of the marginal effects plot is very similar to the one under the baseline specification with considerably wider confidence intervals, however.

Conclusion

The negative relationship between fiscal adjustment and incumbents' popularity is arguably as close as it gets to a conventional wisdom in political science. Either via its Keynesian effects on the macro-economy or via the concentrated losses it entails for important voting blocs with only uncertain, disperse and temporally distant payoffs in the future, fiscal adjustment is widely presumed to result in electoral losses for government parties executing them. The problem with many conventional wisdoms of course is that they are insufficiently subjected to proper empirical scrutiny and critical theorization. This one is no exception.

This article sought to bridge this gap by building on the Hirschmanian framework of exit, voice and loyalty (EVL). I posited that the degree to which the electorate punishes incumbent governments crucially depends on the available partisan alternatives. If these alternatives are perceived to lie further to the right on the policy dimension of economic orthodoxy, disaffected voters have little reasons to switch their votes in their favour. If alternatives are seen as too extreme on other salient political dimensions or lacking governing experience and competence to shield the electorate from the burdens of adjustment, they are equally unlikely to sway a decisive share of disaffected voters. Conversely, when there are available partisan alternatives as more credible defenders of existing government programs, the electoral threat is considerably greater. Combining these considerations led to a simple hypothesis that this article set out to test: the center-left is in an electorally superior position to implement fiscal adjustment compared to the center-right.

Building on a novel and largest dataset to date on close to 100 parties' popularity ratings from 21 democracies, this hypothesis has been largely confirmed by the data. The partial effect of fiscal adjustment on vote intention shares on ruling parties is negative and significant only when the parties in question belong to center-right party families or they are scored right-of-center by country experts. Unless the immediate effects of fiscal adjustment on growth, unemployment and inflation are extreme, governing center-right parties consistently lose more support in years of fiscal adjustment than their center-left counterparts. This pattern has been shown to hold under an alternative country sample restricted to old democracies, a more restrictive coding of governing parties and alternative measurement thresholds for identifying adjustment years.

A concluding thought that arises from these results is their implication for partisan politics. If the center-left can consistently get away with fiscal adjustment without ever being punished for it, it should be only a matter of time before the very notion of the political left hollows out, ending partisan politics as we knew it in line with the "*new politics of the welfare state*" thesis (Pierson, 1996). Alternatively, it is only a matter of time before new policy entrepreneurs fill the policy space left wide open by the center-left. Thus far, the only limited electoral success of these more extreme contenders has been testimony to the relative resilience of the center, limiting a mass exodus of formerly center-left voters to the far-left as my theory predicted. How long can the center hold in the post-crisis context of austerity politics may yet turn out to be one of the main electoral stories of our times.

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Appendix

Table A-1: Political parties in the sample

Country	Parties	Timeframe	Party family
Australia	Australian Labour Party	1994-2003	Center-left
	Liberal+National coalition	1994-2003	Center-right
	Green Party	1994-2003	Green
Austria	Austrian People's Party	2001-2013	Center-right
	Social Democratic Party	2001-2013	Center-left
	Freedom Party	2001-2013	Extreme-right
	Greens	2001-2013	Green
Canada	Liberal Party	1980-2013 (2002-2003 missing)	Center-right
	Conservative Party/Progressive Conservative Party	1980-2013 (2003;2003 missing)	Center-right
	New Democratic Party	1980-2013 (2003;2003 missing)	Center-left
	Bloc Quebecois	1991-2013 (2002-2006 missing)	Other
Czech Republic	Civic Democratic Party	1996-2013	Center-right
	Social Democratic Party	1996-2013	Center-left
	Communist Party	1996-2013	Extreme-left
Denmark	Social Democratic Party	1971-2013	Center-left
	Conservative People's Party	1971-2013	Center-right
	Socialist People's Party	1971-2013	Center-left
	Danish People's Party	1996-2013	Extreme-right
	Liberal Party	1971-2013	Center-right
	Radical Liberal Party	1971-2013	Center-right
Finland	Social Democratic Party	1995-2013	Center-left
	Left Alliance	1995-2013	Extreme-left
	National Coalition Party	1995-2013	Center-right
	Centre Party	1995-2013	Center-right
	Green League	1995-2013	Green
	Swedish People's Party	1995-2013	Other
	Christian Democratic Party	1995-2013	Center-right
	True Finns	1997-2013	Extreme-right

France	Socialist Party	1976-2013	Center-left
	Rally for the Republic/Union for a Popular Movement	1979-2013	Center-right
	National Front	1985-2013	Extreme-right
	Communist Party	1976-2013	Extreme-left
	Greens	1993-2013	Green
Germany	Christian Democratic Union/Christian Social Union	1978-2013	Center-right
	Social Democratic Party	1978-2013	Center-left
	Greens	1981-2013	Green
	Free Democratic Party	1978-2013	Center-right
	Party of Democratic Socialism/ Left Party	1992-2013	Extreme-left
Hungary	Alliance of Young Democrats	1995-2013	Center-right
	Socialist Party	1995-2013	Center-left
Iceland	Progressive Party	1995-2013	Center-right
	Independence Party	1995-2013	Center-right
	Social Democratic Alliance	2000-2013	Center-left
	Left-Green Movement	1999-2013	Green
Ireland	Fianna Fail	1986-2013	Center-right
	Fine Gael	1986-2013	Center-right
	Labour Party	1986-2013	Center-left
Italy	Democratic Party	2002-2013	Center-left
	Forza Italia	2002-2013	Center-right
	Nothern League	2002-2013	Extreme-right
Netherlands	Labour Party	1971-2013	Center-left
	People's Party for Freedom and Democracy	1971-2013	Center-right
	Christian Democratic Appeal	1977-2013	Center-right
	Green League	1990-2013 (2001-2002 missing)	Green
	D66	1971-2013	Center-right
	Party of Freedom	2005-2013	Extreme-right
	Socialist Party	2003-2013	Extreme-left
Norway	Labour Party	1975-2013	Center-left
	Progress Party	1975-2013	Extreme-right
	Conservative Party	1975-2013	Center-right
	Christian Democratic Party	1975-2013	Center-right

	Center Party	1975-2013	Other
	Socialist Left Party	1975-2013	Extreme-left
	Liberal Party	1981-2013	Center-right
Poland	Democratic Left Alliance	1996-2013	Center-left
	Civic Platform	2002-2013	Center-right
	Law and Justice	2002-2013	Center-right
	Peasant Party	1996-2013	Other
Portugal	Socialist Party	1987-2013	Center-left
	Social Democratic Party	1987-2013	Center-right
	People's Party	1987-2013	Center-right
	United Democratic Coalition	1987-2013	Extreme-left
Slovakia	Movement for Democratic Slovakia	1999-2013	Other
	Democratic and Christian Union	1999-2013	Center-right
	Party of Hungarian Coalition	1999-2013	Other
	National Party	1999-2013	Extreme-right
	Direction	2001-2013	Center-left
	Christian Democratic Movement	2002-2013	Center-right
Slovenia	Liberal Democratic Party	2000-2013	Center-right
	Democratic Party	2000-2013	Center-right
	People's Party	2000-2013	Center-right
	Social Democratic Party	2000-2013	Center-left
	National Party	2000-2010	Extreme-right
	Desus	2000-2013	Other
Spain	People's Party	1987-2013	Center-right
	Socialist Workers' Party	1985-2013	Center-left
Sweden	Social Democratic Party	1971-2013	Center-left
	Left Party	1971-2013	Extreme-left
	Green Party	1982-2013	Green
	Moderate Party	1971-2013	Center-right
	Liberal Party	1971-2013	Center-right
	Christian Democratic Party	1986-2013	Center-right
	Center Party	1971-2013	Center-right
UK	Conservative Party	1979-2013	Center-right
	Labour Party	1979-2013	Center-left
	Liberal Democratic Party	1979-2013	Center-right

Table A-2 : Sources of polling data

Country	Polling Firms
Austria	**OGM, Gallup**
Australia	**RoyMorgan**
Canada	**Environics, Nanoresearch, Ipsos Reid**
Czech Republic	**CVVM**
Denmark	**Gallup**
Finland	**Taloustutkimus**
France	**TNS-Sofres**
Germany	**Forschungsgruppe**
Hungary	**Ipsos**
Iceland	**Capacent**
Ireland	**Ipsos, RedC**
Italy	**Ipsos**
Netherlands	**NIPO, Peil, Ipsos**
Norway	**TNS-Gallup**
Poland	**CBOS, TNS**
Portugal	**Euroexpansao, Eurosondagem, Marktest**
Slovakia	**FocusResearch**
Slovenia	**Ninamedia**
Spain	**CIS**
Sweden	**Demoskop**
UK	**Ipsos-Mori**

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