

Essay II

Abstract: Voter opposition to fiscal consolidation is often attributed to intergenerational exploitation, short-sightedness or lack of information. While these mechanisms are likely at play, the effect of voters' moral considerations are largely absent from the public finance literature. This study addresses the effects of blame and feelings of personal responsibility on support for budget consolidations. We argue that voters will feel less responsibility for fiscal problems originating from a crisis in the banking sector than if those problems result from a continuous accumulation of deficits, and will therefore be less supportive of austerity measures to repay the resulting debt. Our results, which make use of both cross-country data and a survey experiment, are consistent with this. Since financial crises and many other costly events are practically random, this can have the profoundly counter-intuitive consequence that governments are punished harder for things that are outside of their control.

This essay is co-authored with Rafael Ahlskog.*

*The cross-national analysis was designed and conducted by Pär Nyman with valuable feedback from Rafael Ahlskog. Both authors have contributed equally to all other parts of the research process, involving planning, reading the previous literature and formulating the hypotheses, designing the experiment and writing the manuscript. Apart from the classification of free-text responses, where inter-coder reliability required that the authors conducted the classification independently of each other, most of the work has been carried out in tandem. Both authors take full responsibility for the entire content of the paper.

Not my problem

Fairness and fiscal responsibility in the age of austerity

Many people are quite reluctant to engage in economic discussions. Questions of economic policy may appear difficult and too technical for most ordinary citizens to grasp. Nonetheless, issues related to *debt* are often a subject of much discussion. Suddenly, everyone has an opinion. ‘The Greek people have lived beyond their means,’ explains the man in the street, ‘surely they must pay what they owe’. These, however, are not economic, but *moral*, statements. Graeber (2011) asks himself whether paying one’s debts is not what morality is all about; ‘fulfilling one’s obligations to others, just as one would expect them to fulfill their obligations to you’. Moral arguments also dominate on the other side of the fence. ‘Why should we pay for what the banks did?’ they ask. In fact, survey data from Greece has shown that those who believe that everyone is responsible for the crisis are much more positive towards the austerity programmes than those who deny such a collective responsibility (Rüdig and Karyotis 2014).

Questions of morality and feelings of responsibility are virtually absent from the public finance literature. The budget balance represents an inter-temporal allocation of public consumption. As long as voters are rational, their preferences for fiscal consolidation should not be affected by the reasons why the country has accumulated a debt. In this literature, there are essentially four explanations for why fiscally responsible governments could be at an electoral disadvantage. First, voters might want to exploit future generations (Cukierman and Meltzer 1989). Second, voters might be short-sighted, so that they value their current well-being much higher than their well-being in the future (Buchanan and Wagner 1977). Third, voters might suffer from a fiscal illusion, which means that they observe that they have less money in their wallets, but not that the government has improved its budget balance (Rogoff and Sibert 1988). Fourth, it is possible that influential interest groups demand different kinds of government spending without internalizing the financing costs for these reforms (Velasco 2000).

However, there is now an abundance of studies showing how norms of fairness can help predict the economic behaviour of individuals (Akerlof 1980, 1982; Bowles and Hwang 2008; Kahneman et al. 1986). Closest to our case are probably studies which show that tax evasion increases when people perceive the fiscal system as being unfair. As soon as people do not feel that they get anything back from the government, or believe others evade their taxes too, their propensity to evade taxes increases (Andreoni et al. 1998; Bordignon 1993). For similar reasons, we should expect citizens to be more supportive of fiscal consolidation if they feel that they have benefited from the spending which caused the deficits, and less supportive if they feel that the debt is someone else's responsibility.

Consider the following thought experiments. What would have happened to the repayment of the enormous debts incurred by the Anglosaxon countries during World War II, had the same debt originated from a crisis in the banking sector rather than a heroic war effort? The fiscal discipline of these countries has traditionally been attributed to favourable constitutional provisions (Persson and Tabellini 2003), but would we really expect the cause of the debt to play no role whatsoever? Would we have seen a seemingly endless succession of debt crisis after debt crisis in Latin American countries had these countries not been characterized by a high degree of rent-seeking and the lasting scars of a colonial history? It seems likely that in such a context, the sense of personal responsibility on behalf of voters for poorly balanced state budgets may, perhaps rightfully, be very low.

This study tests the hypothesis that the support for fiscal consolidation to a large degree depends on whether the voters perceive the fiscal adjustment as fair from a perspective of responsibility. Voters can be expected to be more supportive of austerity measures if they feel partly responsible for the public debt than if they feel that the fiscal problems were caused by someone else – even if the cause of the debt does not affect the economic incentives for fiscal consolidation.

This essay makes an important contribution to the part of the literature on retrospective voting that focuses on how governments are punished for events that were out of their control. While such behaviour is commonly attributed to the problem of distinguishing between signal and noise, the results in this essay highlight an interesting paradox. Because voters refuse to bear the burden of events out of their control, they will punish any government that imposes the cost on the citizens, even if the long-term cost would be larger if the government chose to do nothing.

The essay will proceed methodologically in three steps. First, a cross-country analysis of electoral consequences of fiscal adjustments tests whether the electorate punishes an incumbent government for auster-

ity measures harder when there have been large bailouts. Since such a cross-country design cannot directly show causality, the second part presents results from a simple between-subjects survey experiment, testing how the framing of a debt crisis affects resistance to austerity. Third and last, a close reading of free-text responses in the experiment tests whether effects can in fact be explained by a diminished sense of responsibility for the debt.

Theory

The use of social sanctions to reward those who do good and punish wrong-doers is fundamental to all social interaction. One might even say that the attribution of blame is at the very core of human sociality. A large body of research now illustrates not only our proclivity for so-called altruistic punishment (that is, punishing a defector even at a net cost to oneself), but also that this behavioural trait is likely necessary for prosociality to arise in the first place (Fehr and Gächter 2002).

The process for how people arrive at causal explanations for events and assign blame to those responsible is what psychologists call *attribution*. If a person was not responsible for a negative event, he or she should not have to endure any negative consequences for what happened. In political psychology, this logic has been shown to influence a multitude of attitudes, including opinions on welfare, abortion, gay rights, terrorism and war (Sahar 2014).

However, when great losses are incurred by the public by accident, the attribution process often leads to unfair and irrational behaviour. As the consequences of negative events get worse, people find it increasingly difficult to accept that they are just ‘accidents’ and that no one can be held accountable for the events (Walster 1966). In such situations, people therefore assign undue levels of responsibility to the government or other human actors (Kumagai et al. 2006). The most obvious examples are natural disasters, but an externally caused financial crisis also has the same lack of accountable domestic political actors. Even if governments can affect the probability of banking crises through financial regulation, it is usually not the incumbent government which implemented the current legislation.

The problem arises when the public has to pay for these events. While the fiscal footprints of natural disasters are relatively small, the costs for a crisis in the financial sector can be substantial. For example, during the Great Recession, both Iceland and Ireland spent more than 40 per cent of GDP on their banking rescues. The Bush administration spent a little less than one per cent of GDP on Hurricane Katrina, which nonetheless makes it the costliest disaster in US history.

From the perspective of a social planner, it is only natural that the government pays for such costs and then distributes the burden on the citizens by implementing tax hikes or spending cuts. But the idea that people should pay higher taxes to pay for something they did not do contradicts the moral logic of many citizens. They will therefore find fiscal adjustments to be unjust when the need for fiscal consolidation is caused by a banking crisis, or other factors out of their control, and thus punish the government in the subsequent election.

In the literature on retrospective voting, it is now well established that voters regularly punish incumbents for events that were out of the government's control. Both Leigh (2009) and Wolfers (2007) find that voters are unable to distinguish economic developments caused by the world economy from factors that could at least potentially be attributed to the government. Indeed, voters punish governments for shark attacks and extreme weather (Achen and Bartels 2004) and even losses by the local football team (Healy et al. 2010). That kind of irrational voting is usually ascribed to the fact that information is costly and cognitive resources are scarce, which makes it too time consuming for the voters to distinguishing signal from noise or government competence from stochastic factors.

The causal mechanism suggested in this essay is something different, because it explains why governments can actually be punished harder for random accidents than for events under their control. Voters feel less responsibility for such events, either because they did not cause the event or because they never gained anything from it. When they are forced to pay for the accident, it triggers a moral reaction that causes some of the citizens to not vote for the incumbent.

Study 1: cross-country evidence

The first part of this study uses a cross-national observational design to analyse how bank bailouts have affected the electoral consequences of fiscal adjustments. This connects to the hypothesis proposed above in a very direct way: voters will feel less responsible for debt caused by banks and thus punish governments harder for fiscal consolidations that are preceded by large bailouts. The analytical framework is identical to the one employed in the first essay.

Methods and data

The units of analysis consist of political parties measured over an election period. The dependent variable is the change in vote share compared to the previous election ($\Delta v_{p,e}$), measured in percentage points. This variable is regressed on the size of the fiscal adjustments that were implemented

during the party's time in office ($\Delta sbb_{p,e}$), the size of bank bailouts during the five years before the election ($bail_{p,e}$) and an interaction between these two variables. The model also includes a bank crisis dummy which takes the value 1 if there has been a banking crisis in the country during the last five years.²⁶

To isolate the effect from possible confounders, the change in the unemployment rate since the last election as well as the average GDP growth and inflation during the election period are included as control variables. They have all been shown to affect voters' evaluation of the incumbent (Campbell 2005; Lewis-Beck and Stegmaier 2000). All specifications also include the net migration and the party's vote share in the previous election. The model is illustrated in the equation below, where $x_{p,e}$ denotes the vector of control variables.²⁷

$$\begin{aligned} \Delta v_{p,e} = & a + b_1 \times \Delta sbb_{p,e} + b_2 \times \Delta sbb_{p,e} \times bail_{p,e} \\ & + b_3 \times bail_{p,e} + \delta \times x_{p,e} + e_{p,e}. \end{aligned} \quad (2.1)$$

The size of fiscal adjustments is operationalized as the accumulated change in the cyclically adjusted net lending over the period during which the party was in the governing coalition. If correctly estimated, this measure will equal the (more or less) discretionary decisions taken by the government. Data on the budget balance, economic growth and unemployment are gathered from OECD Economic Outlook (OECD 2015). Election results and cabinet composition are supplied by the ParlGov database (Döring and Manow 2015), while the bailout data are collected from the banking crisis database by Laeven and Valencia (2012). Data on inflation and net migration come from the World Development Indicators.

The sample consists of elections between 1974 and 2013 in 27 OECD countries, which totals 78 election periods. All analyses are restricted to parties which spent at least half the election period in a governing coalition. Because the mechanisms surrounding fiscal expansion are unlikely to mirror those of fiscal consolidation (Lowry et al. 1998), episodes with a positive change in the structural budget balance are excluded. To control for national differences in the cost of ruling, or incumbency advantage,

²⁶The definition of a banking crisis is that the relevant country-years are included in the banking crisis database by Laeven and Valencia (2012). Because the same source is used for data on bailouts, the bailouts are a subset of the banking crises.

²⁷Because the size of the fiscal consolidation is defined as the change during which the party was in the governing coalition, and coalitions often change between elections, the equation presented here is a simplification. To be precise, the measure of the budget balance ($\Delta sbb_{p,e}$) is only identical to $sbb_e - sbb_{e-1}$ when a party spent the whole election period as incumbent.

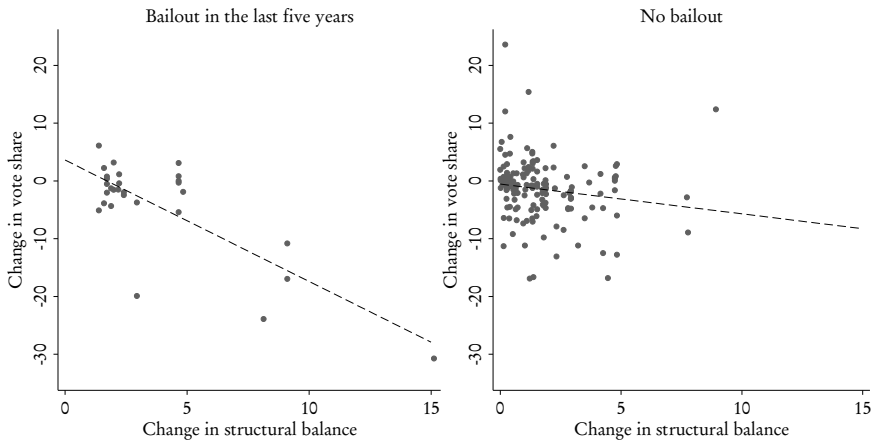


Figure 2.1: Bailouts and the electoral consequences of fiscal consolidation

every model includes country fixed effects.²⁸ Time trends in the data are controlled for through a restricted cubic spline of the election date.²⁹

Results

The pattern of bailouts and the electoral consequences of fiscal consolidations is illustrated in Figure 2.1. Both graphs show the size of the fiscal consolidation on the horizontal axis and the change in the vote share for the responsible party on the vertical axis. The left graph consists of countries where there was a bailout during the five years preceding the election, while the right graph is made up of countries where no bailout took place. Judging from the negative slope in the left graph, it appears that large consolidations are costly if they are implemented simultaneously with bailouts to the banks. The right graph only indicates a weak relationship between the size of fiscal adjustments and electoral outcomes.

How bailouts condition the electoral consequences of fiscal adjustments is tested more formally through a regression framework. The analysis begins by studying the electoral effects of a banking crisis without any variables which measure the actions taken by the government. The regression results are presented in Table 2.1.

²⁸Differentiating the variables is sometimes done instead of using fixed effects. However, even if most of our variables measure changes between elections, this will not eliminate cross-national differences. For example, Bengtsson et al. (2014, p. 126) find that the cost of ruling (the change in vote share for incumbent parties) is larger in southern Europe than in Germany and the Nordic countries. In our model, such differences are captured by the fixed effects.

²⁹The spline uses five knots distributed at the 5th, 27.5th, 50th, 72.5, and 95th percentiles, as recommended by Harrell (2001, p.21) and implemented as Stata's default option.

Table 2.1: Electoral consequences of fiscal consolidations

	(1)	(2)	(3)	(4)
Banking crisis	-2.77** (1.24)	0.04 (1.32)	-3.30** (1.52)	-4.57** (2.24)
Bailout		-0.31*** (0.07)	0.52*** (0.17)	0.58** (0.22)
Δ Unemployment rate	-0.92** (0.43)	-1.18*** (0.40)	-0.97*** (0.36)	-1.32*** (0.46)
Average inflation	0.26 (0.20)	0.25** (0.12)	0.08 (0.10)	0.15* (0.09)
Average growth	-0.06 (0.58)	-0.52 (0.51)	-0.65 (0.46)	-1.24* (0.63)
Previous vote share	-0.11*** (0.04)	-0.11*** (0.04)	-0.09** (0.04)	-0.07* (0.04)
Δ Net migration	-0.14 (0.68)	-0.51 (0.61)	-0.58 (0.57)	-0.66 (0.61)
Δ Str. budget balance (Δ SBB)			-0.13 (0.26)	-0.77 (0.82)
Δ SBB \times Bailout			-0.09*** (0.02)	-0.08** (0.04)
Δ SBB \times Banking crisis				0.45 (0.67)
Δ SBB \times Δ Unemployment rate				0.18 (0.11)
Δ SBB \times Average inflation				0.01 (0.14)
Δ SBB \times Average growth				0.32 (0.22)
Δ SBB \times Previous vote share				-0.01 (0.02)
Δ SBB \times Δ Net migration				-4.92*** (0.76)
Constant	4.04 (8.40)	7.97 (7.62)	9.16 (8.04)	13.36* (7.71)
Country dummies	Yes	Yes	Yes	Yes
Cubic time spline	Yes	Yes	Yes	Yes
Observations	171	171	171	171
Adjusted R^2	0.32	0.37	0.42	0.41

Dependent variable: The party's change in vote share compared to the last election (per cent of votes cast). Clustered standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The first column shows a model without the bailout and consolidation variables. As shown in the first row, incumbent parties are expected to lose 2.8 percentage points in the election following a banking crisis, even when we control for macroeconomic development. This is a troubling result from a normative standpoint, because the incumbent government has little influence over the sudden occurrence of a financial crisis. The traditional interpretation of such effects is that voters want to punish incompetent

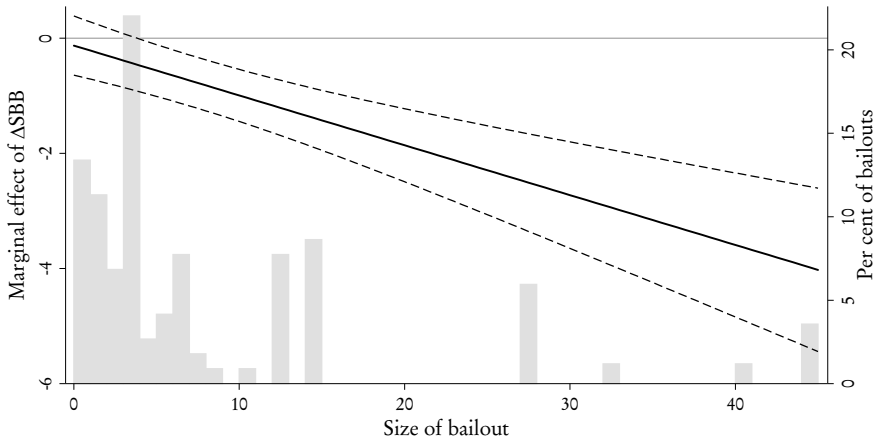


Figure 2.2: Marginal effects on electoral support (left Y-axis) of structural budget balance, with histogram over bailout size (right Y-axis)

governments, but that they measure competence with a lot of random noise, which results in a situation where governments are punished for bad luck. In the second column, when the size of bailouts is added to the model, the earlier effect of a banking crisis disappears. It would seem that voters do not punish governments for fiscal crises, unless they provide the banks with large bailouts.

The main results are presented in the third column, where the model is augmented by an interaction term between the size of bailouts and the size of the implemented fiscal adjustments. While the estimated effect of fiscal consolidation in the absence of bailouts is close to zero (-0.13), the interaction has a large negative effect which is statistically significant at the 99 per cent level. This means that voters punish the incumbent parties for fiscal consolidations, but only when the latter have been combined with large bailouts to the banks. In other words, governments face electoral losses when they force the voters to pay for something that the voters were hardly responsible for. The bailout now has a positive effect, showing that voters do not dislike bailouts per se. A possible interpretation is that voters reward governments for successful rescue operations of the banks, as long as the bailouts do not cause any immediate negative consequences. This interaction effect is illustrated in Figure 2.2, where the estimated marginal effect of a change in the structural balance is plotted over the size of the bailout. As shown by the upper confidence interval, the electoral effect of consolidations is statistically significant for bailouts above 4 per cent of GDP. The histogram shows the relative frequency of bailouts of different sizes.

If there are other variables which are correlated with the size and occurrence of bailouts, and also condition the electoral consequences of fiscal consolidations, the estimated interaction effect suffers from omitted variable bias. The fourth column therefore includes interaction terms between the structural balance and each of the control variables. However, the results remain virtually unchanged, except for larger standard errors.

Judging by Figure 2.1, it appears that there have been seven cases where the consolidation was large enough to give the observation a strong leverage. However, according to the *dfbeta* statistics for the interaction effect, none of these observations are particularly influential. The reason for this is that they have relatively small residuals and that the removal of multiple observations is needed to fundamentally change the results. The only strongly negative *dfbeta* statistic is found for the Dutch party VVD during the 2012 election, which is the upper left observation in Figure 2.1. However, removing the observation only reduces the interaction coefficient to -0.07 . It remains statistically significant at the 99 per cent level.

To sum up, the cross-national analysis supports the hypothesis that voters are more strongly opposed to fiscal consolidation when it is combined with a sizeable bailout of the banks. However, there are many reasons why the results should be interpreted with great care. First, there has only been a small number of large fiscal consolidations which can be assigned to one election period, which means that the results are driven by relatively few observations. While the estimates are surprisingly robust to different specifications, as well as the exclusion of one or two influential observations, it is inescapable that the results are dependent on a few cases. Second, there can be other variables which condition the electoral consequences of consolidations. If they are correlated with the occurrence of bailouts, the interaction effect will be affected by omitted variable bias. While these regression models have included controls for the most common suspects, including their interaction terms, it would be impossible to control for everything. Third, even if the conditioning effect of bailouts is causal, we cannot tell if the causal mechanism is the one proposed in this essay. How can we be sure that bailouts cause people to feel less responsible for the public debt? To address these limitations, the analysis will now proceed with an experimental approach.

Study 2: survey experiment

The cross-country results highlight that there are patterns in electoral data consistent with the hypothesis that governments are punished harder for austerity measures when they have bailed out banks. To add a possible

causal interpretation of the effects, the cross-national analysis is here combined with a simple between-subjects survey experiment on how the framing of the crisis affects support for austerity measures.

Methods and data

An online survey was distributed to a total of 1007 Americans on Amazon MTurk. The respondents were given three blocks of questions. The first block consisted of a standard set of demographic survey items. These were age (from 1 = 19 or younger, via five year intervals, to 12 = 70 or older), gender, highest level of education (from 1 = no high school degree to 6 = professional school degree), annual pre-tax household income (from 1 = up to \$10,000 to 7 = \$150,000 or more) and the number of children (from 1 = none to 5 = four or more).

In the second block, the respondents were randomized into three groups which received different versions of the same short vignette. Everyone was asked about their opinion on a proposed fiscal consolidation, but the explanation was varied for why there was a large debt. The first group was told that the debt originated from a crisis in the banking sector and subsequent bailouts. The second group was not given any explicit explanation for the fiscal problems, and thus acted as a control. Finally, the third group was told that the debt had accumulated over several decades. The exact wording is given below. The respondents answered on a scale from 1 = strongly disagree to 5 = strongly agree.

- The national debt has recently risen to unsustainable levels. The main reason for this debt is that the crisis in the banking sector led to rising unemployment and required large bailouts to the banks. To begin repaying the national debt, some economists argue that the government should reduce spending and increase taxes. What is your opinion about this proposal?
- The national debt has recently risen to unsustainable levels. To begin repaying the national debt, some economists argue that the government should reduce spending and increase taxes. What is your opinion about this proposal?
- The national debt has recently risen to unsustainable levels. The main reason for this debt is that the government has been running budget deficits for several decades. To begin repaying the national debt, some economists argue that the government should reduce spending and increase taxes. What is your opinion about this proposal?

The third block consisted of questions about party identification (Democrat, Independent or Republican), where they placed themselves on a left-right scale (1 = left, 10 = right) and how much confidence they have in the federal government (1 = very little, 10 = very much). These questions were asked after the treatment to avoid priming the respondents. It is therefore possible that the treatment affected how they answered.

Table 2.2: Balance of the treatment and control groups

	Banks	None	Accumulated	p_1	p_2
<i>Before treatment</i>					
Age	4.09	4.27	4.11	0.30	0.92
Male	0.61	0.53	0.61	0.06	0.96
Income	3.69	3.58	3.68	0.29	0.88
Kids	1.58	1.68	1.60	0.20	0.78
Education	3.56	3.50	3.58	0.39	0.75
<i>After treatment</i>					
Democrat	0.45	0.42	0.45	0.46	0.91
Republican	0.17	0.16	0.18	0.59	0.93
Right-wing	4.64	4.57	4.51	0.73	0.48
Conf. in gov.	2.38	2.52	2.41	0.06	0.60

Note: p_1 and p_2 shows the results from a t-test of whether the 'population mean' in the bank treatment group is equal to the population mean for the group without an explanation (p_1) and the group with the accumulated deficit explanation (p_2).

Results

Table 2.2 shows descriptive statistics by treatment condition. The last two columns in the table show the p-value from two-sided t-tests of whether the mean in the bank group differs from the mean in the other groups. Out of the first set of questions, only one out of ten tests shows a significant effect at the 90 per cent level. That is also what we would expect by chance if the randomization was successful. We can also see that the sample is predominantly male, on average in their early thirties, and that Republicans are under-represented compared to the general American population.

The share that supported the proposed fiscal consolidation (the share that answered 4 or 5 out of everyone who answered either 1–2 or 4–5) is shown in Figure 2.3. There are two things that stand out. First, fiscal consolidation receives quite strong support. After excluding the 30 per cent who neither agreed nor disagreed, there is a majority which agrees with the proposal. However, online surveys of this kind are not representative of the American population. As noted above, this survey has an over-representation of young, male Democrats. Second, and more importantly, there is a much larger resistance to fiscal consolidation in the group which was told that debt was caused by a large bailout. Here, only 50 per cent of the respondents support the proposal, compared to 58 per cent in the other two groups.

By analyzing the data in a regression framework, it is possible to increase the precision of the estimates by including a set of control variables. Because respondents answered the first block of questions before they were given the treatment, we can include these questions without any risk

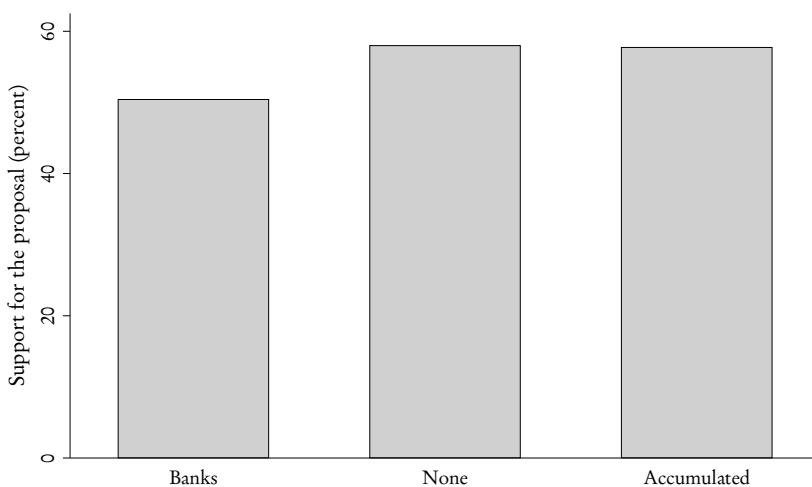


Figure 2.3: The share of respondents that agree with the proposal

of reverse causality. This is not true for the last block of questions. To maximize variation, the five-point scale is used as the dependent variable in all regressions.

The regression results are presented in Table 2.3. In the first column the opinion of the proposed austerity measure is regressed on the received treatment. Compared to those who were not given any explanation for the deficit, the ones who were told about the bank crisis were on average 0.2 points more opposed to the proposal. This effect is quite large, given the fact that the question concerns a real event about which most people already have an opinion. When asked about the reason for their opinion, several respondents who did not answer ‘in line’ with the treatment rightly opposed themselves to how the debt was motivated in the experiment. If the *actual* cause could have been manipulated, the effects would probably have been of an entirely different magnitude. To reduce the standard errors, a set of covariates are included in the model presented in the second column. The third column includes questions asked after the outcome question and which therefore can be endogenous to the treatment. The results in both the second and the third column echo the results found in the first column.

Study 3: free-text analysis

So far, we have seen that governments are punished harder if austerity is combined with bank bailouts, and that respondents in a survey experiment are more opposed to austerity measures when told that they are needed because of a crisis in the financial sector. This still begs the question

Table 2.3: Main results

	(1)	(2)	(3)
Banks	-0.19** (0.09)	-0.20** (0.09)	-0.18* (0.09)
Accumulated	-0.04 (0.09)	-0.05 (0.09)	-0.04 (0.09)
Age		-0.02 (0.02)	-0.02 (0.02)
Male		0.10 (0.08)	0.15* (0.08)
Education		0.10** (0.04)	0.08* (0.04)
Income		0.01 (0.03)	0.01 (0.03)
Children		0.02 (0.04)	0.06 (0.04)
Democrat			0.10 (0.09)
Republican			-0.08 (0.12)
Right wing			-0.02 (0.02)
Gov. confidence			0.17*** (0.04)
Constant	3.12*** (0.07)	2.76*** (0.19)	2.36*** (0.22)
Observations	1007	1007	1007

Dependent variable: support for fiscal consolidation (1=strongly disagree, 5=strongly agree). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

whether the causal mechanism is, as hypothesized, a diminished sense of responsibility. To lay bare the mechanism in greater detail, the respondents in study 2 were also asked to motivate their opinion in a free-text response. These responses are subject to separate analysis in this third study.

Methods and data

The number of respondents who reject fiscal consolidation (i.e. those who chose 1 or 2 on the main outcome) because they do not believe the debt to be their (the respondent's, the American people or the tax payer's) responsibility were counted, by treatment condition. Respondents who mention such motivations were coded as 1, and those who do not were coded as 0. Respondents who did not provide a free-text response are treated as missing observations.

The benefit of using a free-text response over pre-made categories

is that it avoids priming the respondents with arguments they may not have thought of themselves. As such, it is more likely to capture the real mechanism, and not a post-hoc justification on behalf of the respondent. As opposed to Rüdiger and Karyotis (2014), who measured the sense of responsibility and support for austerity measures in a cross-section of Greeks, this design is also more likely to capture actual causal effects since the treatment is exogenous.

Among the answers, statements like ‘the sins of the past should not be borne by the present’, ‘I should not have to pay for all the waste and stupidity that goes on in government’, and even that ‘national debt should not be the fault of the citizens’ can be found. Many respondents also blame the wasteful government for the debt and suggest they ‘they’ should pay for it with lower wages and shorter vacation or with cuts in government programmes. Some blame the banks and suggest that the banks should repay their debt instead. Such responses, which include proposals for how to improve the budget balance, were coded as 0, since they show an acceptance for the fact that it is in the public interest to find a solution to the debt problem.³⁰

To ensure the reliability of the classification procedure, the classification was done by both authors independently of one another, with treatment condition blinded. A cross-comparison showed a very high (247 out of 252 cases, i.e. 98 per cent) inter-coder reliability. The non-matching cases were re-evaluated and agreed upon together by the authors.

Results

A total of 252 free-text responses were given by respondents answering either 1 or 2 on the main outcome item (that is, essentially rejecting the proposed austerity package). Out of these, 9.9 per cent cited reasons relating to lack of responsibility. This number, however, varied widely between treatment conditions.

Figure 2.4 shows the share of respondents who motivated their opinion with a denial of responsibility for the public debt. Among those who were told that the debt was caused by bank bailouts, 15 per cent justified their resistance by arguing that the American people should not be punished for things they are not responsible for. The corresponding number was 4 per cent among the respondents who were not given any reason for the fiscal problems, and 8 per cent among those who were told that the government had accumulated the debt over decades. The difference between the first two groups is statistically significant at the 95 per cent level ($p = .02$).

³⁰When the answer included a general statement about how the public lacks responsibility for what the government does, it was always coded in that way, even if it was followed by suggestions for how to decrease the debt.

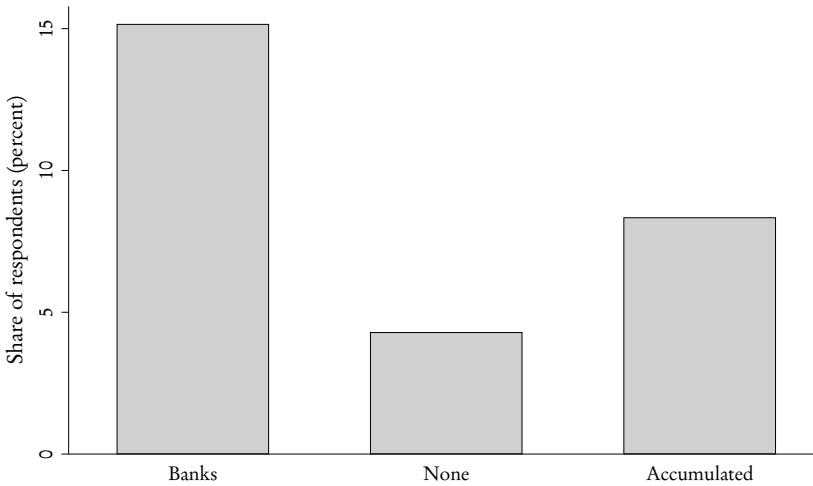


Figure 2.4: Respondents that deny responsibility for the public debt

While a proper mediation analysis using this data is not possible, these results show clearly that, even without being prompted, respondents in the banking condition were much more likely to voluntarily motivate their resistance with a lack of responsibility. This adds further strength to our proposed mechanism.

Discussion

While behavioural insights have enriched much of the modern economic literature, behavioural models are virtually absent from the public finance literature on debts and deficits. The most common explanations for why governments are not rewarded for fiscal discipline are that voters are short-sighted or that they seek to exploit future generations, that voters lack information about the public finances, and that voters or other actors demand costly reforms without internalizing the financing costs.

These theories offer intuitive explanations for why we should expect the budget balance to be slightly weaker than optimal. However, none of these theories can explain the overwhelming protests against austerity which we have recently witnessed, or the elimination of the political party which was responsible for most of the fiscal consolidation in Greece. The information about public finances is better than ever, the problems are acute and few costly reforms are implemented. How is it possible that voters are so desperate to exploit future generations now, when the costs for doing so are obvious? In times when the cost for fiscal indiscipline is smaller, fiscal policy appears to be surrounded by far less conflict.

In this essay, it has been demonstrated that a particular type of morality affects the popular support for fiscal adjustments. More precisely, it is argued that voters are more likely to refuse fiscal consolidation when they do not feel responsible for the public debt. The argument is supported by three different methodological approaches. First, using cross-national data, it is found that governments that implement fiscal consolidations following large bailouts to the banks are punished harder by the voters than governments that consolidate in another context. Second, when survey respondents are asked about their attitude towards fiscal consolidation, the number of people who oppose the proposal increases when they are told that the debt was caused by a crisis in the banking sector. Finally, the respondents who got the message about the bank crisis were almost four times more likely to justify their opposition to fiscal consolidation by saying that the public debt is not their responsibility.

As previously mentioned, the different methodological approaches have different strengths and weaknesses. On the one hand, the cross-country approach has low causal validity and merely highlights the empirical contingency of getting punished for austerity on having had a banking crisis and having bailed out the banks. It cannot say that this relationship is truly causal or what the mechanism could be if it was. A survey experiment, on the other hand, has quite low external validity and effects found may very well be unreplicable in the real world. Additionally, it is well-known that survey experiments often over-estimate effect sizes compared to natural or field-experiments (Barabas and Jerit 2010). The three parts combined, however, overcome at least some part of their individual weaknesses.

The findings in this essay have important implications for the normative justification of economic voting. The theory of retrospective voting asserts that voters evaluate incumbent performance at the ballot box. As Fiorina (1981) argued, this theory is appealing because it provides an argument for how the democratic process can be effective even if voters lack the information and engagement to make informed choices. Maybe they do not know anything about what decisions the government has made, but as long as they observe their own well-being and vote accordingly, incompetent incumbents that destroy the economy will be voted out of office and every government will have an incentive to maximize their efforts.

The normative appeal of retrospective voting hinges on the assumption that citizens can attribute responsibility to the right decision makers. There are many examples of when voters are mistaken (Anderson 2007; Healy and Malhotra 2013), but they can usually be explained as unfortunate outcomes from rational processes. The findings in this essay are

special because they point to a fascinating paradox: If voters refuse fiscal consolidation because the roots of the problem were out of the country's control, will governments in fact be held accountable to a higher degree for actions they were not responsible for? From a normative standpoint, it is difficult to argue why governments should be punished harder when they are not to blame for the debt, yet this seems to be the implication of the findings.

A significant amount of research remains to be done in order to illuminate the complex ways in which voters evaluate policy proposals. The results from this study can serve to illuminate one of the possible ways in which moral considerations about blame and responsibility, rather than short-sightedness or other failures of individual rationality, could play an important role. Other openings for further research concern how this mechanism might be involved in the electoral effects of for example natural disasters, large-scale migration or the costly mistakes of past governments. Given the pertinent tendency of humans to assign blame, it seems likely that similar effects could be found elsewhere.

Essay III

Abstract: Cross-national variations in fiscal performance have traditionally been seen as resulting from differences in electoral systems and types of government. However, such politico-institutional explanations appear to be sensitive to the time-period analysed. This paper provides a new explanation of why some countries have managed to consolidate public finances, while others have accumulated unsustainable levels of debt. Using real-time data for a panel of 31 OECD countries over the 1997–2012 period, the paper shows that governments have responded to biased economic forecasts with more expansionary fiscal policies than they would have if projections had been unbiased. The estimated effects are large. On average, biased projections have weakened annual budget balances by approximately one per cent of GDP.

Politics or Perceptions

The fiscal consequences of uninformed policy makers

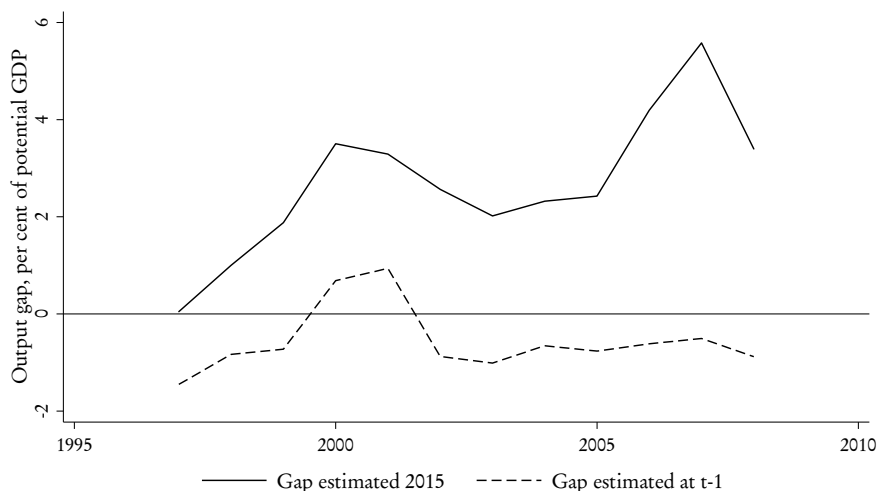
The global economic crisis that began in 2008 has once again put public finances at the heart of academic and public debate. In many countries, public debt has soared to unsustainable levels and forced governments to implement austerity measures, with severe economic and social consequences. The worst affected countries are those where public finances had developed structural weaknesses well before the crisis. It is generally assumed that such fiscal problems originate from political indiscipline inherent in democratic systems, like voter myopia, fragmented governments or a strategic use of debt (Wyplosz 2013). Using this as a starting point, explanations of cross-national differences have focused first and foremost on those institutions and conditions that affect the motivations of policy makers and cause governments to internalise the costs of budget deficits. Constitutional provisions, the degree of political fragmentation and the quality of budget institutions are the institutions and conditions most frequently cited (Alesina and Perotti 1995, 1999).

I propose an explanation of budget deficits that does not build upon this notion of political failure. Instead, I examine how the fiscal performances of advanced democracies have been affected by biased economic projections. The underlying idea is that discretionary economic policy is based on *perceptions* of the economy – rather than on *actual* economic conditions – and that we cannot fully understand the development of public finances if we only take the latter into account. If the perceptions that economic advisers and policy makers have when they pass a budget later turn out to be false, retrospective policy analyses that only consider actual economic outcomes might not provide an accurate view of the workings of politics.

As an illustration of how such perceptions can differ from ex-post estimations, Figure 3.1 shows the business cycles of Portugal, Ireland, Italy, Greece and Spain – measured as an average of the output gap estimations that the OECD has made for these countries.³¹ The most recent assess-

³¹The output gap is the difference between actual and potential output, usually expressed as a percentage of potential GDP. The output gap is negative during 'bad times' and positive during 'good times'.

Figure 3.1: Output gap estimates 1997–2008, PIIGS



Both lines show the average output gap for Portugal, Ireland, Italy, Greece and Spain. The solid line is made from output gap estimations published in November 2015, while the dashed line shows the forecast made one year before the year of concern. Data comes from OECD Economic Outlook.

ments are represented by a solid line, and the real-time projections, i.e., the projections made at the time that budgets were passed, are represented by the dotted line. As shown by the solid line, today these economies are judged as having performed well above their potential throughout the period. In retrospect, it appears to have been a golden decade and a great opportunity to consolidate public finances. It is therefore easy to condemn the lax fiscal policies of these countries and invoke the standard politico-institutional explanations of their high debt levels.

However, as the real-time projections show, these countries have repeatedly been told that they are in the midst of economic downturns, with output below long-run potential. Consequently, both the sustainability of public finances and the need for fiscal stimuli have been systematically overstated. Temporary increase in revenues were mistaken as permanent, which might have justified tax cuts and increased spending. Let us take Ireland as an illustrative example.

”When I have it, I spend it and when I don’t, I don’t”. These unfortunate words are from Charlie McCreevy, the Irish Minister of Finance between 1997 and 2004, and have frequently been used to illustrate the country’s irresponsible fiscal policies during Ireland’s boom years. And indeed, during years of rapid growth and output well above potential, Ireland ran large structural budget deficits hidden behind temporarily high tax revenues. Its lax fiscal policy during this period has recently been criti-

cised by the IMF and the European Commission (European Commission 2011; IMF 2009).

Ireland, however, is also known for the difficulties involved in gauging a country's business cycle, with economic advisors repeatedly projecting much lower output gaps than those that materialise ex-post. As a result, fiscal policy has been based on economic projections very different from the realities that have subsequently emerged. It is therefore possible that Irish budgets were balanced, given how policy makers perceived economic conditions.

This hypothesis is supported by a quick review of policy advice given throughout this period by organizations mandated to monitor Irish fiscal policy. While both the EU and the IMF tended to be slightly more fiscally conservative than the Irish government, recommending modest fiscal tightening from time to time, their advice was moderate compared to the harsh criticisms delivered years later. In general, they supported and sometimes even praised the government's policies. In their final evaluations before the fiscal crisis, the IMF recognised Ireland's fiscal position as sound (IMF 2006), and the EU Council concluded that "the medium-term budgetary position is sound and the budgetary strategy provides a good example of fiscal policies conducted in compliance with the Stability and Growth Pact" (Council of the European Union 2007).³² Recent estimations made by the IMF indicate that Ireland at this time ran a structural primary deficit of close to 12 per cent of potential GDP (IMF 2015).

It appears that the structural fiscal weaknesses Ireland developed can at least partly be explained by how both policy makers and independent experts perceived the economic situation. But is the Irish case unique, or are economic perceptions an important explanation of budget deficits in other countries as well?

To answer this question, we must couple data on actual economic conditions with information about how the economy was perceived when budgets were passed. While increasingly common in the area of economic forecasting and the analysis of monetary policy (Croushore 2011), similar approaches are virtually absent in the political-economy literature on fiscal policy.³³ Therefore, this essay makes a unique contribution to the analysis

³²See O'Leary (2010) for a more in-depth comparison of the ex-post positions of the IMF, the European Commission and the OECD with the advice that was continuously given by these institutions during the 2001–2007 period.

³³Three notable exceptions are Jonung and Larch (2006), Frankel (2011) and Easterly (2012). However, they are all based on the notion that governments knowingly manipulate the official forecasts to disguise budget deficits. This essay uses forecasts made by independent experts, which are much less susceptible to political manipulation, with the purpose to analyse how sincere perceptions of the economy has influenced policy.

of budget deficits and debt accumulation.

The analysis is conducted on 31 OECD countries over the period 1997–2012.³⁴ The empirical results confirm that over the last 15 years, economic perceptions have been important to the conduct of fiscal policy and account for large portions of the differences between countries with respect to debt accumulation. Governments have reacted strongly to perceptions of economic conditions, and systematic projection errors have caused fiscal policy to be overly expansionary. In the average OECD country, this projection bias is estimated to have weakened the annual budget balance by more than 1 per cent of GDP. For some countries, such as Ireland, Portugal and the United Kingdom, the effect has been twice as large. Moreover, the traditional politico-institutional explanations for budget deficits find little support during this period and appear to be highly sensitive to the sample used.

The politics of debt control

The modern politico-institutional literature on debt control began where earlier attempts to explain budget deficits had proven insufficient. Previous work, most notably the tax-smoothing hypothesis of Barro (1979), assumed that deficits arise because benevolent budget planners seek to maintain *stable* taxes to meet a given path of government expenditure rather than balance the budget at every opportunity. Budget deficits, instead of temporary tax increases, would then constitute the optimal response to wars, economic downturns and unfavourable demographic conditions. However, as debt levels rose, it became apparent that negative fiscal positions could not be easily explained by the factors suggested by Barro and others. This insight inspired a lively literature that turned attention toward politics, with politico-institutional factors now sought to explain differences in fiscal performance.

This literature has shown that there are many reasons why governments might pursue overly expansionary fiscal policies, even if governments realise that such policies are not intertemporally efficient. For example, voters might not fully internalise the future costs of deficits because they are short-sighted (Buchanan and Wagner 1977), are willing to exploit future generations (Bowen et al. 1960; Cukierman and Meltzer 1989) or

³⁴The start of the sample is limited by the first real-time projections available and the end of the sample is chosen so there has been at least three years of possible revisions. The included countries are Australia, Austria, Belgium, Canada, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Iceland, Ireland, Israel, Italy, Japan, Korea, The Netherlands, New Zealand, Norway, Poland, Portugal, Slovakia, Slovenia, Spain, Sweden, Switzerland, United Kingdom and the United States.

are uninformed about the country's fiscal position (Rogoff and Sibert 1988). However, various explanations depicting fiscal policy as a common-pool problem have been the most influential (Wyplosz, 2013, see Weingast et al., 1981, for an early contribution). The key idea is that a large number of small interest groups, government parties or policy makers, all of whom demand costly reforms without internalising their full costs, decide on budgets where the fiscal balance is worse than each of the involved actors would have preferred. One of the most appealing aspects of this theory lies in its many empirically testable implications. Consequently, a large portion of the literature has attempted to explain cross-country variations in fiscal performance in terms of politico-institutional differences that aggravate or alleviate these common-pool problems. Studies of political fragmentation and fiscal institutions have been most important in this regard.

Political fragmentation

Political fragmentation is the most common politico-institutional explanation of budget deficits. In their seminal work, Roubini and Sachs (1989) created an index of political cohesion, finding that it is more difficult for fragmented governments to achieve consensus on deficit reduction and that debt ratios therefore increase more in countries where coalition governments are common. Their findings spurred a vast volume of subsequent research and are still influential. Edin and Ohlsson (1991) reexamined the results and, by decomposing the index into dummy variables for coalition and minority governments, showed that minority governments had poorer budget discipline than majority governments and that this was the effect captured by Roubini and Sachs. This approach was later broadened by Kontopoulos and Perotti (1999), who compared the effects of the number of parties in government with those of the number of spending ministers and concluded that it was the latter that mattered for fiscal performance. Subsequent research has confirmed similar findings in US states (Besley and Case 2003) and in Eastern Europe (Fabrizio and Mody 2006).

A closely related strand of research has examined constitutional differences, typically focusing on larger sets of countries. The most influential studies in this line of research are the works of Persson and Tabellini (2003). In a cross-sectional analysis of 60 democracies, they show that over the 1960–1998 period, countries with majoritarian electoral systems have had smaller budget deficits than countries with proportional representation. In later work, they conclude that this relationship results from the fact that proportional representation entails more fragmented party systems in which coalition governments are more frequent (Persson et al. 2007). Later studies have focused on the degree of proportionality in

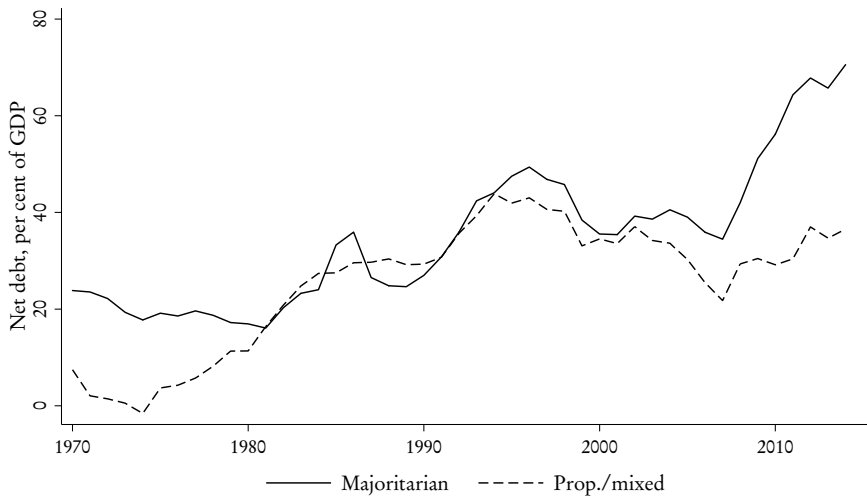
the electoral system, typically measured as a district magnitude. Fabrizio and Mody (2006) examines fiscal policy in a panel of Eastern European countries, none of which have majoritarian systems, and finds that large district magnitude (many elected candidates per district) is associated with larger budget deficits.

While the theoretical arguments provide appealing explanations of why politically fragmented governments have poorer fiscal discipline, they do not necessarily imply that countries in which fragmentation is prevalent always run larger budget deficits. As debt accumulates, it is reasonable to expect counteracting forces to stabilise or even reduce debt levels. Governments will attempt to repay debt, through regular payments or devaluation. Institutions might be created to bolster fiscal prudence, which has been theorised to occur more in countries where coalition governments are common (Carlin and Soskice 2009; Soskice 2007). And if nothing else, financial markets will eventually lose confidence in public finances and force debt reductions when governments cannot pay the costs of debt servicing. In other words, even if the effects of political fragmentation are real, they might not always show up as budget deficits in aggregate data. Besides, there are also reasons to believe that fiscal consolidations are easier to implement when political fragmentation is high. An important insight in the literature on economic voting is that voters are more likely to hold incumbents accountable when there is a clarity of responsibility (Powell and Whitten 1993).

This could explain why some later studies have failed to confirm the results of the literature on political fragmentation. Indeed, replication studies commonly find that results are sensitive to the time period analysed (Beck et al. 2000; Hahm et al. 1996). This is well-illustrated by Figure 3.2, which shows how average debt levels have developed in countries with majoritarian electoral systems and in countries with mixed or proportional systems. The figure shows that debt levels rose faster in countries with mixed and proportional systems during the 1970s and 1980s, which broadly corresponds to the period analysed in most of the studies referred to above. Since the mid 1990s, these countries have consolidated public finances and performed better than countries with majoritarian systems.³⁵

³⁵The reason for the large debt and its decreasing trend in countries with majoritarian systems can be traced to the Second World War. When the war ended, most of these countries had accumulated extreme levels of public debt. In the UK, for example, debt was more than twice the size of GDP. Despite decades of fast growth it took most of these countries until around 1980 to return debt to 'normal' levels. For differing reasons, most countries with mixed or proportional electoral systems ended the war with public debt under control and hence did not have the same need as countries with majoritarian systems for prudent fiscal policy during the decades that followed. After 1995, a difference emerges once again between countries with majoritarian and

Figure 3.2: Net debt and electoral system



The graph shows the average net debt in countries with majoritarian (solid line) and mixed/proportional (dashed line) systems. The series is created from different editions of OECD Economic Outlook.

Fiscal institutions

After it had been widely acknowledged that mechanisms inherent in the political system were at the root of many countries' fiscal problems, budget institutions were suggested as a way to overcome these problems without compromising the core values of democracy. With such institutions further actualised through European economic-political integration, a rapidly growing area of research has emerged. As it is an open question which types of fiscal institutions that most strongly affect fiscal performance, several different indices have recently been proposed. Unfortunately, most indices are only cross-sectional (Alt and Lassen 2006a; Darvas and Kostyleva 2011) or limited to European countries (Fabrizio and Mody 2006; Hallerberg et al. 2009).

The difference indices measure different aspects of the budget process. The index presented in Hallerberg et al. (2007, 2009) consists of two components. The first component measures the decision-making power of the finance minister and is denoted *delegation*. The second component is designated *contracts* and is a measure of the degree to which political parties

proportional systems. This divergence could potentially be attributed to the fiscal and institutional demands of the European economic-political integration, which mainly affects countries with proportional electoral systems. It is worth noting that the dataset used by Persson and Tabellini (2003) shows the same thing. Debt is consistently higher in countries with majoritarian systems, but because the differences decrease over time, such countries appear to perform better when the budget balance is examined.

or other agents have agreed to commit themselves to certain fiscal targets. Both of these components are closely aligned with the idea of fiscal policy as a common-pool problem. Through delegation of power to a finance minister, who is believed to internalise the costs of deficits to a greater extent than spending ministers would, or binding budget agreements negotiated by all parties in parliament, which should internalise the costs of debt more than the government parties alone would, the common-pool problem should be mitigated.

Alt and Lassen (2006a,b) constructs an index of fiscal transparency, based on the degree to which the budget documentation includes medium- and long-term projections for public finances, whether the government uses accrual accounting and whether the documents are subject to auditing and independent review. Fiscal transparency is assumed to provide voters with more accurate information about public finances and thereby enable them to assess the fiscal performance of the incumbent. We could therefore expect fiscal transparency to mitigate the problem of fiscal illusion (Rogoff and Sibert 1988) and reduce the electoral incentives for incumbents to accumulate debt. The different indices are strongly correlated, but I will use Alt and Lassen's index in the empirical part's of this essay because it has information about a larger number of countries.

The idea of fiscal policy as a common-pool problem – aggravated by political fragmentation and mitigated by fiscal institutions – has provided an appealing rationale for why governments run large deficits and why some countries perform more poorly than others. However, as noted, there are reasons to believe that such explanations cannot account for cross-national variations in fiscal performance over recent decades. The following section therefore suggests an alternative explanation of why deficits have arisen.

Perceptions matter for policy making

When policy makers establish economic policies for the following year, they base their actions on their own and their advisers' *perceptions* of the economy. How the economy will eventually develop is unknown to them. However, research on macroeconomic policy making has traditionally only examined economic outcomes as we observe them today, implicitly assuming that these ex-post assessments correspond to the information available to policy makers at the time policies were implemented (Orphanides 2001). Thus, while it is widely acknowledged that macroeconomic circumstances can only explain a small part of the variations in fiscal performance (Alesina and Perotti 1995; Fabrizio and Mody 2006) and that governments tend to run budget deficits even when deficits are not

motivated by economic circumstances (Calmfors and Wren-Lewis 2011), the analysis behind these propositions has typically not considered economic conditions as they were perceived by policy makers. Instead, policy has been analysed according to how economists today – often decades later – judge the situation. As will be shown, the differences between how economic conditions were initially perceived and how they eventually developed are large enough to have important implications for the results.

Fiscal policy is increasingly described by means of a policy reaction function, where the government attempts to set the budget balance³⁶ (b_t) as a response to the business cycle (y_t), typically measured as the output gap (Cimadomo 2012). When no distinction is made between economic realities and economic perceptions, β is thought to capture both automatic responses – like the sensitivity of tax revenues to changes in employment – and discretionary actions taken by the government.

$$b_t = \alpha + \beta \times y_t + \epsilon_t \quad (3.1)$$

However, fiscal actions are not based on actual economic conditions but on how they are perceived by governments. There are three primary reasons why these perceptions differ from economic realities. First, there is a time lag between the time a policy is agreed upon and the time it is implemented. For example, a budget bill is typically passed during the autumn prior to the year in which it is implemented. During this time, economic circumstances can change significantly. Second, some economic variables are difficult to measure and therefore subject to large and frequent revision. This is especially true of economic growth. Third, what matters most for policy making is not the actual values of growth or unemployment, but how they compare with their estimated long-run potential. Neither potential output nor equilibrium unemployment is directly observable, and even the latest revised estimates are characterised by significant uncertainty. Nevertheless, ex-post estimates are much more accurate than those made ex-ante. Moreover, such estimations improve over time as a result of new insights and methodological innovations. Combined, these factors ensure that perceptions of the economy that were held when budgets were passed typically differ substantially from actual economic outcomes. Hence, β in Equation 3.1 does a poor job of capturing the intentions of governments, and we therefore wish to

³⁶There are many ways to define and measure budget outcomes. *Budget balance* is used here as a generic term referring to the primary balance, net lending or the change in government debt. The budget balance is a stochastic outcome and never under direct political control, but it is a good indicator of policy makers' intentions and is often interpreted as directly set by governments. The preferred choice of budget measure, the annual change in the net debt to GDP ratio, is discussed in the Supporting Information.

Table 3.1: Categorising the political response

Fiscal regime	ϕ
Pro-cyclical	$\phi < 0$
Neutral	$\phi = 0$
Counter-cyclical	$\phi > 0$

separate perceptions from realities. In Equation 3.2, the earlier model is augmented with economic perceptions, designated by an asterisk (y_t^*). The fiscal effects of economic realities and perceptions are thus captured separately by ψ and ϕ .

$$b_t = \alpha + \psi \times y_t + \phi \times y_t^* + \epsilon_t \quad (3.2)$$

Assuming that policy makers react to *perceptions* of the economy, while the non-discretionary components of budgets respond to economic *realities* rather than perceptions, it is possible to differentiate the intentions of policy makers from the direct budgetary effects of the business cycle. Similar strategies to distinguish between discretionary responses and automatic stabilisers have previously been employed by Bernoth et al. (2008) and von Kalkreuth and Wolff (2007). In Table 3.1, these intentions are categorised as either pro-cyclical, neutral or counter-cyclical.³⁷ A policy maker with counter-cyclical intentions attempts to stabilise the business cycle through discretionary actions by weakening the budget balance during downturns and strengthening it during upturns. Counter-cyclical intentions are here represented by a positive value of ϕ .

The 'neutral' policy maker does not take action to interfere with the business cycle but allows automatic stabilisers to work freely. Without discretionary measures, the budget balance will still be positively affected by the business cycle, but this effect will be captured by ψ . Put differently, with neutral policy makers the *structural* budget balance³⁸ is not correlated with the business cycle, and ϕ will be close to zero.

A pro-cyclical policy maker attempts to counter the budget weakening effects of an economic downturn by strengthening the budget through tax increases or reduced spending. If $\phi + \psi = 0$, the actual budget balance will be unrelated to the business cycle. Because it would make little sense to take larger discretionary actions than are required to counteract the

³⁷In this essay, the budget balance is operationalised as the annual change in debt. Consequently, all the parameters have the opposite signs.

³⁸The structural budget balance is a generic term for any cyclically adjusted measure of the budget balance. It can be interpreted as what the budget balance would be during a normal economic situation, that is, when actual output equals potential output.

automatic stabilisers, we would not expect $|\phi|$ to be larger than ψ . Strict budget balancing is seldom advocated today but was the primary goal of fiscal policy during the Great Depression (Dalton 1934). Pro-cyclical fiscal policy is also commonly found in developing countries, which can be explained by political distortions and borrowing constraints (Alesina et al. 2008; Gavin and Perotti 1997).

As shown above, the effects of perceptions on public finances depend upon the fiscal regime. If policy makers have counter-cyclical intentions, they will react to expected downturns by increasing expenditures or reducing taxes. But if the objective of balancing the budget outweighs stabilisation motives, the effect will be the opposite. Naturally, the same logic applies to economic upturns. When good times are expected, the counter-cyclical policy maker will take the opportunity to strengthen the budget, whereas the pro-cyclical policy maker will find this unnecessary.

Research design

How can we know how governments perceive economic conditions? There are no policy maker surveys available that are suitable for this purpose. And even if there were, we would not know whether the responses reflect the policy makers' actual beliefs or whether the policy makers adjust their answers to warrant whatever policies they pursue.

As neither economic realities nor perceptions are perfectly observable, I use the approach commonly employed in the emergent literature on 'real-time' data and operationalise them using projections made by economic forecasters. Therefore, while the most recent assessments are our best estimates of actual economic outcomes, projections made when budget bills were passed are assumed to reflect the economic perceptions that policy makers and economic advisers had at that time. The economic situation, both actual and perceived, is primarily measured as the output gap, which is an estimation of how economic output relates to its potential level. A negative output gap implies an economic downturn and that the economy is performing below potential. Correspondingly, upturns are indicated by a positive output gap. The output gap plays a central role in the analysis and conduct of both monetary and fiscal policy (Koske and Pain 2008).

The notation applied henceforth uses subscripts to denote the years that projections cover and superscripts to indicate when projections were made. For example, a budget bill for year t is typically approved during the previous year ($t - 1$) and is therefore based on information that was available through the one-year-ahead forecast made that year (y_t^{t-1}). As time passes, more data are collected, and estimation methods are

improved, resulting in continuous revisions of the actual output gap. The most recent revision is here denoted as y_t^{rev} and can be viewed as our best estimate of the output gap. As previously mentioned, it is reasonable to assume that discretionary fiscal policy reacts to *perceptions* of the economy, while the non-discretionary components of the budget respond to economic *realities* rather than perceptions (von Kalckreuth and Wolff 2007). It is therefore possible to differentiate the intentions of policy makers from the direct budget effects of the business cycle by including the forecast made at $t - 1$ and the revised estimate in the same equation. An advantage of this approach, compared with the common method of relating *cyclically adjusted* budget balances to the output gap, is that the traditional method might falsely interpret semi-automatic stabilisers and incorrect budget elasticities as discretionary actions. The intended discretionary response to the business cycle is here captured by ϕ , while ψ captures the budgetary effects of automatic stabilisers and other cyclical factors, including semi-automatic stabilisers such as active labour market policies. Because automatic stabilisers are known to differ across countries, ψ is estimated separately for each country, as indicated by the subscript i .

$$b_{i,t} = \alpha + \psi_i \times y_{i,t}^{rev} + \phi \times y_{i,t}^{t-1} + \epsilon_{i,t} \quad (3.3)$$

A constant problem when estimating the cyclicity of discretionary policy concerns possible endogeneity of the output gap. The purpose of counter-cyclical fiscal policy is to stabilise the business cycle, and any exogenous variation in the budget balance will likely be positively correlated with the output gap. Consequently, estimations of cyclicity that only use revised data will be positively biased towards more pro-cyclical policies. This problem is partly solved by the use of real-time data. Presumably, any effect that fiscal policy has on the output gap will be captured by the revised series rather than by forecasts that were made during the previous year. Compared with other methods, estimations of cyclicity that use real-time data therefore tend to find a greater degree of counter-cyclicity (Golinelli and Momigliano 2008).

In the political-economic literature on budget deficits, the conventional method of addressing possible serial correlation is to include a first-order lag of the dependent variable. This is also the approach I use, even though this autoregressive term turns out to be insignificant in most regressions. If we let $\mathbf{x}_{i,t}$ denote a vector of control variables, including year and country dummies, the preferred regression equation can be written:

$$b_{i,t} = \alpha + \phi \times y_{i,t}^{t-1} + \psi_i \times y_{i,t}^{rev} + \lambda \times b_{i,t-1} + \delta \times \mathbf{x}_{i,t} + \epsilon_{i,t} \quad (3.4)$$

It is well known that adding fixed effects to a model with a lagged dependent variable causes biased parameter estimates (Nickell 1981). With an average of more than 10 observations per country, this bias is relatively small but far from negligible (Katz and Beck 2009). Alternative estimators are therefore examined in the robustness section.

Real-time projections and other data

The benefits of using so-called real-time data in the analysis of fiscal policy should now be apparent, but what is the best source of such forecasts, and do they differ systematically from revised estimations? If so, what are the reasons for this bias?

Following the seminal work of Orphanides (2001), the number of studies using real-time data to analyse monetary and fiscal policy has steadily risen (see Cimadomo (2011) for a recent review of the 'real-time literature' concerned with fiscal policy). It is commonly argued that the gap estimations that best capture policy makers' real-time perceptions of the economy are those provided by the OECD (Cimadomo 2012). Because these estimations are published in December, they should take into account most of the discretionary measures approved by governments during the year, while they are nevertheless close in time to the budget bills usually passed at the end of the year. They are also well suited to the purpose of this essay, as they are less likely to suffer from political bias than the projections published by national authorities. After all, politically manipulated projections do not reflect the actual beliefs of policy makers. And compared with data from the IMF and the European Commission, older vintages are available from the OECD.

However, there are several reasons why projections of the output gap might be biased, even if made by independent experts. First, if the downside risks of forecasts exceed the upside 'risks', reflecting, for example, the possibility of an unlikely but potentially disastrous financial crisis, modal forecasts (describing the most likely scenario) will be biased (Wallis 1999). Second, partly to avoid generating expectations regarding particular economic reforms, many forecasters assume unchanged policies in their projections. If discretionary fiscal actions are expansive on average, which they should be if public expenditures are not fully indexed to prices and wages, economic growth – and therefore also the output gap – will be higher than forecasted. Third, forecasters might have an agenda that goes beyond making the most accurate projections; indeed, it is often claimed that official forecasts of national agencies are manipulated to make governments look good. However, as shown by Gilles Saint-Paul (2011), independent forecasters may also provide projections that influence policies in directions that accord with the forecasters' own preferences.

Table 3.2: Output gap projections made by OECD

Time for projection	1997–2012	
	Obs	Mean
Projection made at $t-1$	378	-1.45
Projection made at t	378	-1.29
Projection made at 2014	378	0.33

Finally, in many countries, potential growth and employment have been decreasing for decades. Such trends are just as difficult to predict as they are to observe and they can easily be misinterpreted as temporary downturns. By retrospectively estimating real-time output gaps using different time series filters, Orphanides and van Norden (2002) show how such structural changes give rise to negatively biased output gaps for the United States.

Table 3.2 summarises the output gap projections that the OECD has published in the December editions of *Economic Outlook*. The revised output gap projections for 1997–2012 show a mean value of 0.33, indicating that output has, on average, been slightly above its potential during this period. The projections for the same years published in real-time tell a different story, with the output gap averaging -1.45 ($t - 1$) or -1.29 (t) per cent of potential GDP. That is a difference of two percentage points!³⁹

While revisions of the output gap are typically caused by revisions of actual rather than potential output (Koske and Pain 2008), this does not necessarily mean that pessimistic assessments of actual output are behind this bias. On the contrary, it is commonly assumed that forecasts are optimistic rather than pessimistic. It is not possible to deconstruct the output gap revisions into revisions of actual and potential GDP. While GDP revisions during the past 15 years have been much larger than required to explain this bias – in this essay’s sample, the real-time estimations of GDP are on average 4 percentage points lower than their revised counterparts – such revisions have more to do with changes in classification rules and other definitions than with actual misjudgements of the level of economic activity. Examining revisions of equilibrium unemployment (NAIRU) and forecasts of accumulated growth, the overall impression is that both optimism about potential output and pessimism about actual output contribute to the output gap bias.⁴⁰

³⁹Projection bias is also confirmed by statistical tests. Similarly to the conclusions drawn by Koske and Pain (2008), a Wald test separately rejects the joint hypothesis that the real-time estimates (made at $t-1$) are efficient and unbiased predictors of revised estimations (made in 2014) for most countries in the sample.

⁴⁰In this sample, both the NAIRU and five-year GDP growth ($t + 1$ compared to $t - 4$) have on average been revised upwards by 0.8 percentage points.

The main sample used in this essay is determined by the availability of real-time projections of output gaps. The OECD has published such estimates biannually since 1996, covering between 20 and 31 of its member countries. With some exceptions caused by a lack of other data, these parameters determine the time-period and countries analysed. Following Barro (1979), Roubini and Sachs (1989) and others, my preferred choice of dependent variable is the annual change in the net debt to GDP ratio.⁴¹ To control for unexpected debt servicing costs and debt deflation, I include the annual change in inflation and net interest payments as explanatory variables. Extensive research on so-called electoral business cycles has found that incumbent parties manipulate fiscal policy to increase their chances of re-election (Franzese 2002). To control for such effects, I include one dummy for election years and one dummy for years that succeed election years. To allow for partisan effects, I also include a dummy variable for whether the chief executive belongs to a left-wing party. Because it is possible that the ideological orientation affects the degree of Keynesian policy (Allan and Scruggs 2004), I also include an interaction between the left-wing dummy and the real-time projection. Remaining variables are the index of fiscal transparency described earlier, data on coalition and minority governments from the Database on Political Institutions (DPI), cross-sectional data on majoritarian and presidential systems from Persson and Tabellini (2003) as well as GDP growth and the unemployment rate from the OECD. For a discussion about the choice of variables and a description of data sources, the reader is referred to the supplemental information available online.

Results

This section presents the results for the different regression models, most of which are variants of Equation 3.4. I begin the section by investigating whether the traditional politico-institutional explanations of fiscal policy can account for the development of debt and deficits during the last two decades. Each model is first estimated on a sample similar to the one

⁴¹There are several reasons to use the change in debt instead of a more direct measure of the budget balance. First, it properly treats debt depreciation due to inflation as debt repayment. Second, the change in debt is easier to compare between countries. While the optimal structural budget balance depends on GDP growth and debt levels, among other things, many economists have argued that a stable debt constitutes the best response to a debt shock (Schmitt-Grohé and Uribe 2004; Wren-Lewis 2010). Third, the debt level is less susceptible to political manipulation through creative accounting (von Hagen and Wolff 2006). My robustness tests also show that the results hold when the change in debt is replaced by the primary balance, although the size of the effect is reduced.

used when the explanation first appeared in the literature and then on the sample used in this essay. I then proceed to the main results, where I analyse how biased real-time perceptions have affected fiscal policy. Before concluding, the robustness of the results are examined through a large set of alternative model specification.

Table 3.3 summarises the regression results for the politico-institutional models. The first two models consist of the variables that were used by Roubini and Sachs (1989) and Edin and Ohlsson (1991). Political fragmentation is coded as two dummy variables, one for coalition governments and one for minority governments, as Edin and Ohlsson suggested. The control variables are the lagged dependent variable, a dummy for Japan and the change in the unemployment rate, the debt servicing cost and the GDP growth. However, the data sources are different from theirs and the model is therefore not an exact replication. The model in the first column is estimated on a sample which is restricted to the observations used by Edin and Ohlsson (1991) and Roubini and Sachs (1989)⁴². Both coalition and minority governments are associated with larger deficits, but it is only minority governments that have an effect that is large enough to be statistically significant. This is also what Edin and Ohlsson (1991) found. However, when the sample is substituted with data for 1997–2012, the effect of both coalition and minority governments changes sign.

The third and fourth columns show the budgetary effects of constitutional differences. As far as possible, I use the same variable definitions as Persson and Tabellini (2004), which means that the dependent variable in these two regressions is the central government's budget deficit, as per cent of GDP. The independent variables are the form of government, the electoral system, an index of civil and political rights from Freedom house, the natural log of the population size, the amount of trade as share of GDP and a dummy for whether the country is a federation or not.⁴³ The third column uses the same cross-sectional dataset as Persson and Tabellini, which they created by averaging yearly outcomes over the 1990–1998 period, with one exception. To make the different models comparable, I only include countries which are members of the OECD. The results show that both majoritarian and presidential systems are associated with better fiscal discipline, but it is only the effect of the electoral system that is statistically significant. Countries with majoritarian electoral systems are estimated to have a 3 percentage points smaller deficit, which is almost identical to the estimate in Persson and Tabellini (2004). The fourth column applies the

⁴²This restriction is based on their data on political fragmentation. I am unable to remove observations for which they had missing data on other variables.

⁴³Because I only include OECD countries, I have to exclude variables with little variation within the OECD, like the dummy variables for continents and colonial origin.

Table 3.3: Traditional explanations and newer samples

	(1)	(2)	(3)	(4)	(5)	(6)
Lagged net debt	0.58*** (0.08)	0.17* (0.10)			0.31** (0.12)	0.18 (0.11)
Δ Unemployment rate	0.56* (0.26)	1.83*** (0.20)				
Unemployment rate					0.26*** (0.08)	0.25 (0.28)
Δ Debt service	0.01*** (0.00)	0.00 (0.00)				
Δ GDP growth _t	-0.03 (0.08)	-0.10 (0.08)				
GDP growth _t					-0.34** (0.14)	-0.96*** (0.10)
Japan	1.38** (0.55)	5.40*** (0.94)				
Coalition government _t	0.38 (0.53)	-0.95 (0.73)				
Minority government _t	1.71* (0.95)	-2.36** (0.91)				
Presidential			-1.07 (2.25)	2.00 (2.58)		
Majoritarian			-3.47** (1.58)	0.71 (1.53)		
Freedom house			3.13 (4.24)	10.20** (3.66)		
Log of population			0.24 (0.46)	0.26 (0.63)		
Openness			-0.03* (0.01)	0.03 (0.03)		
Federal			-0.20 (2.21)	1.55 (1.69)		
Fiscal transparency					-0.35*** (0.12)	0.08 (0.25)
Election _t					-0.03 (0.38)	0.45 (0.87)
Election _{t-1}					-0.23 (0.65)	1.96** (0.80)
Constant	-0.19 (0.34)	0.96* (0.54)	2.21 (4.71)	-14.51* (7.21)	3.00*** (0.73)	-0.17 (1.35)
Sample	RS	Mine	PT	Mine	AL	Mine
Year dummies	No	No	-	-	No	No
Country dummies	No	No	-	-	No	No
Observations	153	366	23	21	258	299
Adjusted R ²	0.480	0.216	0.182	0.222	0.175	0.226

Country-clustered robust standard errors in parentheses. The coefficients for revised output gaps vary between countries and are not reported in the table. The revised output gaps in this table are from 2011 because they go further back in time. The samples are restricted to country-years which are included in Roubini and Sachs (1989) (column 1), Persson and Tabellini (2003) (column 3), Alt and Lassen (2006a) (column 5) and my later analysis (columns 2, 4 and 6). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

same model to the time period analysed in this essay. When the data is substituted with averages over the 1997–2012 period, both the constitutional variables changes sign and are no where near statistical significance.

The final two columns show the budgetary effects of elections and fiscal transparency. The fifth column uses the same time period as Alt and Lassen (2006a), while the model shown in the sixth column is estimated on the 1997–2012 period used in the remainder of this essay. Once again, we find the expected effect on the old sample: debt has accumulated slower in countries with transparent budgets. But when the sample is substituted with more recent data, the effect disappears. As it seems, neither the traditional measures of political fragmentation or fiscal transparency can account for the cross-national differences in fiscal performance over the last two decades. The estimated effect of elections is only significant in the latter sample and for the years following elections, which could reflect that promises made during the election campaign are often implemented in the budget for the succeeding year.

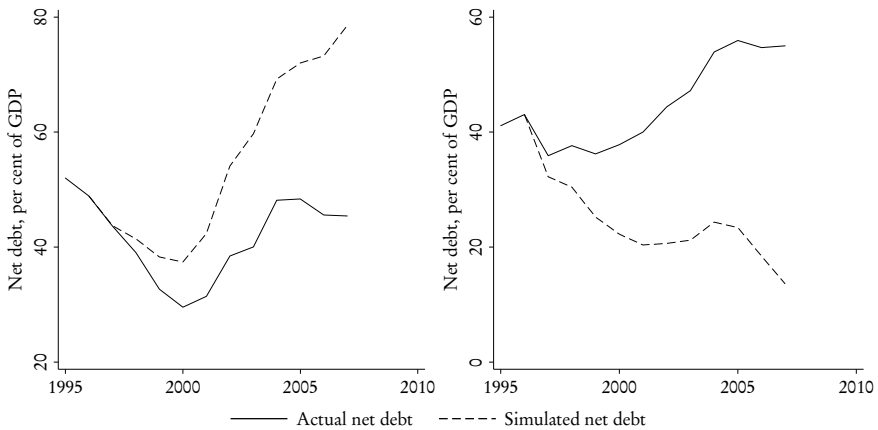
We now proceed to the main results. Table 3.4 presents regression results for the models with economic perceptions. Column 1 and 2 broadly correspond to the equations with the same numbers (with GDP growth and the unemployment rate added as controls). The first column represents a model with revised estimates of GDP growth and the output gap. Because GDP growth and the unemployment rate has been centered around their means, the constant can be interpreted as the predicted debt change during normal economic circumstances, i.e., when output is at potential and both growth and unemployment are at their average levels. Note that because of space restrictions, the country-specific coefficients for the output gap are not shown in the table. The second column shows what happens when real-time perceptions are added to the model. Two key observations are worth noting. First, the real-time projections of the output gap have a large and significant effect on changes in debt. When the gap is expected to be negative, policy makers respond with fiscal stimulus, and the debt grows (or decreases more slowly), and vice versa. The estimated effect is large, indicating a fiscal response of 0.92 per cent of GDP for each 1 per cent expected deviation of output from potential GDP. Second, the value of the constant compared to its value in column 1 shows how the change in debt during 'normal' economic circumstances is affected by economic perceptions. Because policy makers respond to perceived downturns with tax cuts or increased expenditures, and output gap projections are negatively biased, debt is higher than it would have been if economic projections were unbiased. The estimated average effect on the annual change in debt is as large as 1.5 per cent of GDP, as illustrated by the different constants in columns 1 and 2.

Table 3.4: The role of perceptions

	(1)	(2)	(3)	(4)	(5)
Gap_t^{t-1}		-0.92*** (0.23)	-0.94** (0.35)	-1.10** (0.42)	
Gap_t^t					-1.01** (0.48)
Left-wing \times Gap_t^{t-1}				0.27 (0.38)	
Left-wing \times Gap_t^t					0.54 (0.52)
GDP growth _t	-0.91*** (0.20)	-0.84*** (0.23)	-0.52 (0.63)	-0.55 (0.60)	-0.39 (0.50)
Unemployment rate	0.25 (0.19)	0.08 (0.15)	0.29 (0.28)	0.33 (0.28)	0.54 (0.42)
Election _t			-0.10 (0.96)	-0.07 (0.97)	0.13 (1.01)
Election _{t-1}			0.29 (0.69)	0.31 (0.69)	0.50 (0.63)
Δ Inflation _t			0.59* (0.31)	0.59* (0.32)	0.42 (0.29)
Δ Int.payments _t			-2.14 (1.64)	-2.31 (1.76)	-2.01 (2.26)
Δ Debt _{t-1}			-0.12 (0.07)	-0.12* (0.07)	-0.10 (0.07)
Left-wing			-0.32 (0.78)	0.00 (.)	0.00 (.)
Coalition			-0.27 (1.81)	-0.42 (1.76)	-1.11 (1.96)
Minority			-0.14 (1.06)	-0.14 (1.07)	-0.21 (1.12)
Constant	0.21 (0.59)	-1.27 (0.76)	-2.69 (2.56)	-2.58 (2.44)	-2.12 (1.71)
Year dummies	No	No	Yes	Yes	Yes
Country dummies	No	No	Yes	Yes	Yes
Observations	378	378	332	332	329
Adjusted R ²	0.271	0.318	0.553	0.554	0.545

Country-clustered robust standard errors in parentheses. The coefficients for revised output gaps vary between countries and are not reported in the table. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The third column adds a first-order lag of the dependent variable, time and country fixed effects, election dummies, three dummies for left-wing executives, coalition governments and minority governments as well as controls for unexpected inflation and interest payments. The estimate of ϕ remains virtually unchanged at -0.94 , which is a surprisingly robust result and hopefully an indication that omitted variables are not a major concern. The estimated effects of both coalition and minority governments have signs opposite those that have traditionally been proposed, but none of



The US with Irish forecast errors (left) and Portugal with unbiased forecasts (right).

Figure 3.3: Debt projections

them are statistically significant. The fourth column builds on the model in column 3 with the addition of an interaction term between the realtime projection and the executive's ideological orientation. The coefficient for the interaction effect is small and not significant, indicating that left-wing and right-wing governments have reacted to the perceived economic situation in a similar way. This does not necessarily mean that they have had similar ambitions for Keynesian activism, because the response to realtime projections is affected by both counter-cyclical ambitions and reactions to the estimated structural balance. The estimated effect of perceptions has increased a little bit compared to column 3, but that only reflects the change in interpretation which follows from the addition of the interaction variable.

Finally, the last column addresses a possible endogeneity problem. Another explanation of these results could be that fiscal policy has been more expansive than what OECD anticipated, which in these cases would have raised output above the projected levels and caused a spurious correlation between the budget balance and the output gap revisions similar to the one described above. To rule out the possibility that it was unexpected fiscal policy that caused the revisions of the output gap, I replace the realtime projections made a year in advance with the corresponding estimations that were made at the end of the fiscal year. These estimations should include information about virtually all fiscal policy that was implemented during the year and should therefore be less susceptible for the kind of reverse causation outlined above. Fortunately, using a later projection only marginally affects the size of ϕ (it changes from -1.10 to -1.01).

So, how significant are these findings for fiscal performance? Figure

3.3 presents two simulations of the trajectories of debt in the United States and Portugal if perceptions had been different. The American situation during this period is typical of OECD countries. The second half of the 1990s was characterised by fiscal consolidation that came to a halt with the bursting of the IT-bubble. Real-time projections of the output gap were, on average, about one per cent of GDP lower than the most recent estimations indicate. Portugal and Ireland are two of the countries where real-time projections of the output gap have shown the largest bias relative to revised assessments. Unlike in Ireland, however, debt in Portugal was soaring at an alarming pace well before the crisis struck. As such, Portugal is a more illustrative case.

The left graph shows the American net debt together with a projection of how it would have developed had forecast errors of the output gap been the same as in Ireland.⁴⁴ As shown, the predicted fiscal position would have been distinctly different. The fiscal consolidation of the Clinton years would have been entirely reversed, and at the advent of the fiscal crisis, the US debt would already have been much larger than at its peak level during the 1990s. This would, in turn, have limited the room for fiscal manoeuvre during the current economic downturn. The right graph shows debt levels in Portugal together with predictions of what they would have been had perceptions been in line with actual outcomes. If the estimations are correct, the debt would have been smaller in 2007 than it was a decade earlier instead of increasing by 20 per cent of GDP. Needless to say, the perceptions that faced policy makers in these two countries were critical to their different fiscal performances.

Robustness

This section addresses four minor concerns. First, what if the results are sensitive to the sample used? Fortunately, the baseline results appear to be relatively robust to the removal of observations. Excluding the countries one by one shows that the effect remains statistically significant at the 95 per cent level, with one exception. If Greece is removed from the sample, the estimate of ϕ drops to -0.76 and the effect is only statistically significant at the 90 per cent level. The results are not sensitive to the removal of any single observation.

Second, would a different measure of the business cycle yield different results? The OECD estimations of the output gap are less susceptible to political manipulation than official government assessments. Despite

⁴⁴The projection simply adjusts the actual debt development with the difference in forecast errors compared with Ireland, multiplied by the estimate of ϕ from column 4 in Table 3.4. In other words, it assumes that the policy response to the estimated output gap is both linear and unaffected by the debt level.

Table 3.5: Different robustness tests

	(1)	(2)	(3)	(4)
Gap _t ^{t-1}	-0.94** (0.35)		0.12** (0.05)	-0.93*** (0.30)
Gap _t ^{t-1} (HP-filter)		-1.68*** (0.57)		
GDP growth _t	-0.52 (0.63)	-0.31 (0.59)	-0.10 (0.09)	-0.52 (0.55)
Election _t	-0.10 (0.96)	0.18 (0.93)	-0.21 (0.19)	-0.05 (0.84)
Election _{t-1}	0.29 (0.69)	0.45 (0.60)	0.03 (0.17)	0.30 (0.60)
ΔInflation _t	0.59* (0.31)	0.48 (0.30)	0.08 (0.08)	0.60** (0.27)
ΔInt.payments _t	-2.14 (1.64)	-2.27 (1.81)	0.25 (0.22)	-2.06 (1.47)
ΔDebt _{t-1}	-0.12 (0.07)	-0.10 (0.07)		-0.12* (0.07)
Unemployment rate	0.29 (0.28)	0.45 (0.32)	-0.07 (0.09)	0.31 (0.26)
Left-wing	-0.32 (0.78)	-0.49 (0.80)	0.22 (0.27)	-0.37 (0.67)
Coalition	-0.27 (1.81)	-0.45 (1.91)	-0.32 (0.54)	-0.47 (1.48)
Minority	-0.14 (1.06)	-0.27 (1.00)	-0.49* (0.27)	-0.18 (0.91)
Primary balance _{t-1}			0.69*** (0.03)	
Constant	-2.69 (2.56)	-1.36 (2.47)	1.69*** (0.54)	2.56 (3.46)
Observations	332	332	332	301

Country-clustered robust standard errors in parentheses. The coefficients for revised output gaps vary between countries and are not reported in the table. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

this, however, there remains a risk that the OECD projections could be influenced by national governments. If so, governments might manipulate assessments in such a way that fiscal policies appear to be better adapted to economic conditions. The estimates of ϕ could then reflect reverse causality – changes in projections motivated by changes in fiscal policy instead of the policy response discussed in this essay. To address such concerns, I have experimented with replacing the output gap estimations made by the OECD with a simple Hodrick-Prescott filter⁴⁵, which was

⁴⁵The HP-filter is probably the most commonly used smoothing method in the macroeconomic and real business cycle literature. It is a simple algorithm that deconstructs a single time-series into one trend component and one cyclical component.

applied to annual real-time projections of GDP published by the OECD. The first column in Table 3.5 is identical to the third column in Table 3.4 and serves as a point of reference to which the results in the other columns can be compared. As shown in the second column, the coefficient for the output gap generated with a HP-filter is much larger and statistically significant at the 99 per cent level. The reason for the size difference is that the HP-filter generated much less volatile output gaps.

Third, I have argued extensively for the change in net debt as my dependent variable, but does this choice affect my results? In the third column I have replaced the dependent variable with the underlying primary balance, which is a measure of the budget balance – excluding one-time expenditures and the net interest payments on debt – that has been adjusted for the business cycle. The effect remains statistically significant, but is now smaller. A permanent decrease in the real-time gap projection with 1 per cent of potential GDP is estimated to worsen the annual budget balance with 0.4 ($0.12/(1-0.69)$) per cent of GDP.

Last, and as earlier noted, including fixed effects in a model with a lagged dependent variable makes the OLS estimator biased. The fourth column of Table 3.5 shows the model estimated with the consistent GMM estimator of Arellano and Bond (1991).⁴⁶ The regression coefficient for the real-time output gap remains significant and is similar in size to previous estimates.

Conclusions

When policy makers believe that the country is in an economic downturn, they pursue more expansionary policies to stimulate demand and return output to potential. Negative estimations of the output gap will also cause the structural budget balance calculations to indicate that the budget balance will improve as soon as the economy returns to normal. Similarly, governments respond to economic upturns by raising taxes or reducing expenditures. So far, so good.

Unfortunately, the perceptions on which governments have based their decisions have systematically differed from actual economic outcomes. Policy makers have repeatedly been told that their countries are in the midst of economic downturns, assessments that have often turned out to be false. Consequently, fiscal policy has been much more expansive than it would have been had perceptions been unbiased. This essay does not determine whether fiscal policy would have been well balanced if perceptions were unbiased; it only provides an estimation of how different

⁴⁶All available instruments are used. A Sargan test does not reject the null hypothesis that the overidentifying restrictions are valid.

such policies would have been. According to the estimations, the average effects of biased perceptions on annual budget balances have been in the range of 1–2 per cent of GDP.

One could object that policy makers should have learned by now that projections are biased and that they should have adjusted fiscal policy accordingly. Such an objection could question both the results found in this essay as well as my interpretation of them. What if governments anticipated the projection errors and still accumulated so large debts? However, I think that would be to demand too much from the politicians. I would expect the experts to react faster than the policy makers, and there are many reasons to doubt that the former have adapted to previous forecast errors. First, it does not appear as if the bias has decreased over time. On the contrary, the projections during the 1990's were on average revised upwards with 1.1 percentage points, compared to 1.9 percentage points for the period 2000–2012. Second, when the EU, the OECD and the IMF have given policy advice to member countries, they have – as far as I know – not acknowledged the risk that their calculations are biased. As argued in the introduction, they did not criticize fiscal policy until they revised their economic projections. Third, similar projection errors probably brought the Federal Reserve to excessive activism both at the brink of the Great Depression (Orphanides 2003) and during the Great Inflation of the 1970s (Orphanides 2002).

There are reasons to believe that this finding is not limited to the period studied in this essay but that the same mechanism has been in effect during earlier periods of debt accumulation. Let us first establish that two conditions must be fulfilled for these effects to occur. The first condition is that governments must react to perceived economic downturns with expansionary fiscal policy. With the monetaristic 1980s as a possible exception, this has arguably been the case in most of the postwar period. In addition, fiscal policy need not be Keynesian. Negatively biased assessments of the output gap will still affect fiscal policy through overestimates of the structural budget balance.

The second condition is that perceptions of the business cycle must be negatively biased. This condition is more difficult to examine, because comparative data is not available before the 1990s, but there are three arguments for why I believe this to be the case. First, the years between 1970 and the 1990s was a period of rising equilibrium unemployment and a substantial decline in potential growth. Unless this development was correctly interpreted as a structural change it would have caused assessments of the business cycle to be overly negative. This is also what Orphanides and van Norden (2002) find when they use a large set of detrending methods to retrospectively estimate real-time projections of

the output gap for the United States between 1966 and 1997. Their real-time projections were consistently revised upwards, just like the data used in this essay. Second, although the regular estimation of output gaps is a more recent development, the OECD has presented business cycle analyses in its Economic Outlook since 1967. A thorough examination of the December editions published before 1996 shows that only once has the OECD identified an economic upturn (the last years of the 1980s). With few exceptions, the world economy is described as being in the midst of – or recovering from – an economic downturn. In contrast, the OECD today identifies three distinct upturns during the same time period. Third, it is easy to gather anecdotes of how the perceptions of the economy has influenced policy during previous crises. For example, Roubini and Sachs (1989) describe how the fiscal consolidation was delayed following the shocks of 1973 and 1979 because analysts wrongly expected the growth slowdown and the rise in unemployment to reverse.

To summarise, there are good reasons to believe that both of the necessary conditions were fulfilled during the decades preceding the time-period examined in this essay. If true, biased perceptions contributed to the growth of public debt during the 1970s and 1980s.

In one sense, this is a positive message. Perhaps the proposed tension between democratic representation and fiscal performance is exaggerated and politics is working better than some authors have claimed (Alesina 1988; Persson and Tabellini 2003)? If not for biased perceptions, public finances would have been in much better shape. It could also be that things have changed. Past experience with debt crises and fiscal austerity might have made voters more aware of budget constraints and altered the economic-political discourse.⁴⁷ Earlier research has also made essential contributions by pointing to the problem and proposing a plethora of solutions. During recent decades, in response to this research, we have witnessed pervasive reforms of budgetary rules and other fiscal institutions aimed at alleviating the common-pool problems of politics and consolidating public finances.

On the other hand, the results also point to limitations on what can be achieved by further advances in this direction. Because the policies advocated by the EU and the IMF were only marginally different from those carried out by governments in Ireland and similarly situated countries, it appears that not even a complete delegation of fiscal policy to independent experts would guarantee a sustainable fiscal policy. This is an argument in favour of general fiscal prudence, especially when debt levels are above average. It also highlights the problems that arise when fiscal policy is too

⁴⁷In fact, it is difficult to find evidence that governments have ever been punished for fiscal consolidation (Alesina et al. 2012).

dependent on forward-looking budgetary rules and unreliable estimates of structural balances, which is the case with the European Stability and Growth Pact.

Compared with what has been accomplished with respect to monetary policy, which is primarily governed by independent central banks, there is much to be done with real-time data in the analysis of fiscal policy. This is symptomatic of research within the political economy field. The notion of political failure has become the self-evident answer to all issues regarding fiscal performance, effectively impeding the search for complementary explanations. The findings of this essay highlight several issues to address.

First, the failure of the traditional politico-institutional explanations to account for fiscal policies pursued in recent years requires more attention. While the common-pool theory and its variants are perfectly compatible with the idea that fiscal policy is strongly affected by economic perceptions, it is probably not a coincidence that it is during periods of (relative) fiscal discipline that measures of political fragmentation lose their explanatory power. Can it be that political fragmentation undermines fiscal discipline during more favourable conditions, but helps governments to escape electoral punishment during times of fiscal consolidation? The dynamics surrounding fiscal discipline, on the one hand, and debt accumulation and fiscal consolidation, on the other, should be further researched.

Second, the relationship between economic perceptions and official government forecasts is an important area of future research. Several recent studies have shown that domestic forecasts are biased relative to actual outcomes and that this bias negatively affects fiscal policy. Such studies conclude that forecasts are susceptible to manipulation and therefore should be made by independent agencies. However, similar bias can be found in forecasts made by the OECD. Distinguishing between political manipulation and biased perceptions, where the latter are shared by independent experts, would advance this field further. This includes an examination of how governments treat competing forecasts from different sources or that have different policy implications.