

Pär Nyman

Austerity Politics

Is the Electorate Responsible?

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List of papers

This thesis is based on the following essays.

- I Nyman, Pär (2016) 'Punished for austerity? The electoral consequences of fiscal adjustments'. Working Paper, Department of Government, Uppsala University.
- II Nyman, Pär and Rafael Ahlskog (2016) 'Not my problem: fairness and fiscal responsibility in the age of austerity'. Working Paper, Department of Government, Uppsala University.
- III Nyman, Pär (2016) 'Politics or perceptions? The fiscal consequences of uninformed policy makers'. Working Paper, Department of Government, Uppsala University.
- IV Nyman, Pär (2016) 'Economic growth is deflating the welfare state'. Working Paper, Department of Government, Uppsala University.

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When I began this journey, I had no ideas for the subject of my thesis. I had just left my job at the Swedish Fiscal Policy Council and I promised myself that I would write my dissertation about something far more interesting than fiscal policy. With the final product in my hands, it is obvious that I have failed. Apart from broken promises, my time as a PhD Candidate has been an exciting and joyful experience. For this, I am grateful to a large number of people. Regrettably, there are so many people I would like to thank, I cannot acknowledge them all by name.

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Introduction

The tension between the market logic and democracy is fundamental to the history of democratic capitalism. As markets have expanded into new aspects of social life – and especially when the market outcomes have fundamentally contradicted other principles – they have consistently been met with popular resistance. To protect society from what has been perceived as the destructive potential of free markets, solidarity organizations and legislation have been used to subordinate markets to societal control. The best example is probably how unfair prices (Kahneman et al. 1986), typically represented by price hikes following a sudden shortage or wage cuts in a recession, have been met with minimum wages and other forms of price control. This dialectical process between market expansion and society's counter-movement is what Polanyi has called a *double movement* (Polanyi 1944).¹

Many economists have been puzzled by this resistance to market mechanisms. Rodrik (1996) raises the question of why people protest against the structural adjustment programmes, and Bryan Caplan's conclusion that people have an anti-market bias in their beliefs and attitudes (Caplan 2001, 2004) echoes the frustration found among many 19th century economists (Bastiat 1965; Newcomb 1893). When it comes to pro-market reforms, many social scientists have noted this popular resistance but reached the conclusion that the goal can justify the means (Stokes 1996). Harberger (1993) is therefore full of admiration for a handful of 'heroes' who have implemented market-friendly reforms despite public resistance.

Because the struggle against the market logic can lead to inefficiencies – and ultimately threaten the profits and property of capital owners – there has always been a pro-market reaction against this counter-movement. To secure the market forces and the property of capital owners, government repression against labour unions, as well as institutional constraints on populist governments, have restricted the power that this counter-

¹It would not make sense to take a normative stance in the general conflict between the market logic and the principles that cause this counter-movement. These principles constitute a mix of religious beliefs, psychological biases, vested interests, traditions, preferences for equality, human rights, norms about social justice, etc. Besides, the relative merits of the market logic, and the resistance against it, differ between contexts. Few, if anyone, would like within-family relations to follow the same logic as transactions on eBay. For the same reasons, it is not my intention to apply any sort of guilt by association when actors of different times are placed on the same side in this conflict.

movement exercises over the economy. In its most extreme forms, this reaction has required the elimination of democracy. For example, most of Harberger's heroes sided with the military and implemented their reforms following a coup d'état.

Today, the tension between economic prescriptions and popular will is most obvious in the treatment of public debt and deficits. Sovereign default is an old and recurring phenomenon, but public debt sustainability is far more challenging today than ever before, with debt in most countries being on unsustainable trajectories (Bertelsmann 2013). Dramatic austerity measures will be required to achieve fiscal sustainability, but it is widely assumed that such measures will be met by popular resistance. To improve fiscal discipline, governments are therefore advised to protect fiscal policy from too much popular influence by hiding cutbacks behind de-indexation, committing to expenditure ceilings several years in advance, and creating fiscal institutions with economic experts that evaluate the government's policies. The most far-reaching proposals involve the delegation of actual policy to such independent bodies, similar to the way in which monetary policy has been delegated to independent central banks.

In order to find an effective balance between popular influence and fiscal discipline, it is important that we understand citizens' opposition to fiscal consolidation, as well as what other factors have contributed to the fiscal problems. The first three essays in this thesis therefore analyse the reasons why governments accumulate large levels of debt. One essay argues that misperceptions of the business cycle would have caused fiscal problems even if policy-making was conducted by independent experts. The other two provide new knowledge about when and why voters choose to punish governments that implement fiscal adjustments. Over the last decades, many countries have reformed their budget process and other institutions, in order to improve fiscal discipline. In the fourth essay I argue that those institutions have a bias toward a reduced public sector.

I will use this introduction to situate the field of public finance in the broader context outlined in the introductory paragraphs. Because most readers will not be familiar with the public finance literature, this approach should provide a more stimulating reading experience than if the introduction was to have been limited to public debt and deficits. Besides, taking a wider view in terms of time, contexts and disciplines is a good way of broadening one's horizon and identifying perspectives that are missing from the more narrowly focused literature.

The next section provides several examples of how perceived threats from the market have been met with a Polanyian counter-movement, and how the power of this movement has subsequently been restricted to secure the functioning of the market. I begin the section with the

most common forms of resistance in non-democracies before I discuss how this dialectic process has developed with the emergence of organized labour and universal suffrage. The historical examples eventually lead up to the contemporary problem of fiscal sustainability. The historical review is followed by a theoretical discussion of how we can understand this popular resistance. I then turn to democratic theory and the costs of putting constraints on the political process. This introduction ends with a brief summary of the contributions made in the four essays, together with a concluding discussion.

A story about the counter-movement

Resistance against the market logic has existed for thousands of years. In ancient Mesopotamia, where most transactions were based on credit, it was common that peasants borrowed from merchants for their own consumption. If the peasants could not pay, their possessions were soon appropriated. This began with grain and livestock, but if that was not enough to cover the debts, the debtors were forced to sell their servants, their family and – ultimately – themselves as slaves or debt-peons (Graeber 2011). These consequences must have been perceived as wildly unfair by the peasant population. To maintain social tranquility, both Sumerian and Babylonian kings began to periodically write off all consumer debt and return debt-peons, slaves and land to their original owner. In less than two centuries, Babylonian kings decided to carry out no less than fifteen such debt cancellations (Hudson 1993). The motivation given by Hammurabi himself for the 1761 BC restoration was that ‘the strong might not oppress the weak’. Naturally, this tradition was bad for the business of creditors as well as the functioning of the credit market. To protect their trade agreements from such intervention, creditors developed legal devices that made their transfers immune from debt remission edicts (Dassow 2011, p. 208).²

Anyone who has taken a basic course in economics has probably learned of the disastrous effects of rent controls in the housing market. The Swedish economist Assar Lindbeck went so far as to conclude that ‘next to bombing, rent control seems in many cases to be the most efficient technique so far known for destroying cities’ (Lindbeck 1971, p. 39). Yet,

²The Sumerian word *amargi* is the first known written reference to freedom or liberty (Kramer 1970, p. 79) and its cuneiform glyph has become a common symbol for libertarian organizations around the world. *Amargi* literally means ‘return to the mother’ and was used to denote these ancient debt reliefs, where persons and property were returned to their original owners. Thus, many hardcore libertarians today bear tattoos of the symbol of one of the world’s first state interventions.

different forms of rent control have been implemented in most rich countries, typically following a shock in either demand or supply (Willis 1950). In 1755, Lisbon was hit by one of the deadliest earthquakes in history. Most buildings in the city were destroyed and landowners responded to this shift in supply by increasing rents. This is an extreme example of when the market logic differs from the moral – most people must certainly have perceived this behaviour as the landlords taking advantage of human misery. Consequently, King Joseph prevented this ‘grave damage of third persons in the present calamity’ by freezing all rents (Willis 1950, p. 67).

The emergence of the modern market economy is best symbolized by 18th and 19th century Britain. After the Glorious Revolution in 1688, Britain was ruled by a parliament which was dominated by the wealthy. They successfully expanded markets by enclosures, colonial expansion and the repression of organized labour, all paid for by dramatically increased taxes and government debt (Greif 2008; North and Weingast 1989). As workers attempted to raise wages and implement price controls on food and clothing, they were always met with resistance. Factory owners made sure that the innovation of new machinery was aimed at reducing the bargaining power of skilled workers (Bruland 2004). Neither was collective bargaining an alternative, because combining to raise wages had long been illegal according to common law and in 1799 it was also made a statutory crime (Harris 2004). The industrial revolution drastically reduced demand for skilled craftsmen. When they could not participate in peaceful negotiations, many workers engaged in machine-wrecking and other forms of ‘collective bargaining by riot’ (Hobsbawm 1952). This was not taken lightly by the rulers and no less than 12 000 troops were deployed to break up the rioters.

The Polanyian counter-movement has been stronger among, but has not been limited to, peasants and the working class. Eventually the British parliament would also take actions to protect society from the worst consequences of the growing market economy. A mining accident at Huskar Colliery in Silkstone, near Barnsley, took the life of 26 children. The youngest victim was a girl of only 8 years old. Following this tragedy, Queen Victoria ordered a royal commission of inquiry which was led by Lord Anthony Ashley. In 1842, four years after the accident, the parliament passed the *Mines and Collieries Act*, which prohibited women and boys under ten years old from working underground.³ To the frustration

³This legislation was also motivated by gender norms and Victorian prudery. One of the pro-regulation arguments made by the commissioner Lord Ashley was that ‘girls and women [were] wearing trousers and working bare breasted in the presence of boys and men’. Female miners would later campaign for the right to work underground and in the year 1900 these parts of the acts were repelled.

of the more ideological liberals, more laws would soon follow and the amount of social legislation enacted by the parliament grew quickly. In 1884, the liberal political theorist Herbert Spencer – one of the most famous philosophers at this time – published his book *The man versus the state*. Spencer was obviously frustrated with the current state of affairs and in this 200 pages outcry, which referred to bans against child labour as well as most other legislation, he concluded that ‘interferences with the law of supply and demand, which a generation ago were admitted to be habitually mischievous, are now being daily made by Acts of Parliament in new fields’ (Spencer 1884, p. 85).

There are hundreds of similar examples. The Mesopotamian civilizations might have been the first, but debt cancellation came to be the standard demand in peasant revolts for millennia to come (Graeber 2011). Periodic debt cancellations were common in many cultures and the old testament gives support to such *Jubilees* every fiftieth year.⁴ The earthquake in Lisbon has few parallels in terms of real estate destruction, but the societal response has often been similar in the aftermath of a sudden shock to either market rents or wages. For example, rent control has been implemented following the 16th and 17th century plague in Paris, the 1917 fire in Salonika, the Chinese immigration to Hong Kong in the 1920s and – most importantly – during the two world wars, when most modern systems of rent control were created (Willis 1950). Machine breaking was mainly a British phenomenon, but similar demands have been raised by organized labour in every modern society. And social legislation would soon follow in other countries as they caught up to Britain’s fast industrialization.

The early days of democracy

None of the societies described thus far have been democracies. Most organized resistance by peasants and labourers were met with contempt from intellectuals and others who participated in the public debate.⁵ In the United States, where male suffrage was already a fact, leading intellectuals regularly expressed how unreasonable it was to give poor people the same power as well-respected entrepreneurs.⁶ The unreliable populace was also used as an argument for why the government should not be too involved in the economy. As we will soon observe, the debate about the government’s

⁴This is also a good example of how the market logic contradicted the religious. If land belongs to God, and all men are his servants, how then can it be the permanent property of any man? See for example the Book of Leviticus (Leviticus 25:23).

⁵See Mcneil (2001) for a review of how one of the largest riots was depicted in British newspapers.

⁶This attitude was perhaps best captured by the contemporary historian Francis Parkman in his article *The failure of universal suffrage* (Parkman 1878).

role in the economy – and how we can design institutions that protect the economy from populist influence – would grow when democracy eventually prevailed.

How much democracy the economy can handle was a frequent topic for discussion in the early days of democracy. One of the first debates on democracy and fiscal performance took place in the United States between the progressives and the more conservative administration of William Howard Taft, when the framework for the modern federal budget was created (Rubin 1994). The progressives favoured an activist and expanding government, while the Taft conservatives wanted to shrink the government and were opposed to most government interventions. The progressives thought highly of the public and wanted to educate it to demand good government. The conservatives, on the other hand, lacked confidence in both the public and its representatives. In his book *Popular Government*, Taft argues that ‘the man from whom the people really secure the best service is the man who acts on his own judgment as to what is best for his country and for the people, even though this be contrary to the temporary popular notion or passion’ (Taft 1913, p. 60–61). This argument bears a strong resemblance to more contemporary views of why economic policy must be protected from the short-sightedness of voters and opportunistic politicians.

Polanyi’s double movement should not be understood as two *balancing* forces. On the contrary, during the interwar period, the tension between the masses’ attempts to take control over the market and the repression from those who feared this rising power became insuperable. The growing strength of labour unions and radical parties, the threat from the Russian revolution and the introduction of universal suffrage in many countries led to unprecedented power in the hands of the masses. But in too many places, this success soon came to an end as fascism came to power. It was not until after the the second world war, when there was eventually consensual support for democracy in most Western countries, that the golden decades of democratic capitalism could begin.

The post-war counter-movement

Throughout the postwar period, the most important demand from workers came to be the resistance against wage cuts and the protection from unemployment. The Great Depression had demonstrated that self-adjusting mechanisms are unreliable on the labour market, where workers do not accept wage cuts to clear the markets, and there was now a case for economists who argued that the government should take an active role in stabilizing demand over the business-cycle. Best known is of course John Maynard Keynes, but at the same time – if not earlier – similar ideas were

developed by Gunnar Myrdal in Sweden and Michał Kalecki in Poland⁷. Most capital owners had opposed programmes for full employment during the 1930s, except for in Germany where there was no risk of the workers becoming too powerful (Kalecki 1943), but a post-war consensus was eventually reached. The threat of unemployment was reduced by an active stabilization policy and successively expanded unemployment insurance, while the economy benefited from class peace between capital and labour.

Democratic capitalism worked exceptionally well for almost three decades. Most rich countries experienced rapid GDP growth, a steady expansion of the welfare state and high levels of voter turnout, as well as other forms of political participation. The fast economic growth and the reduced degree of conflict in both policy-making and on the labour market probably contributed to the fact that mainstream economics in particular, but also other social science, adopted a relatively naive idea about how policy-making works. During the golden era of democratic capitalism, public finance was dominated by the social planner model. Not too far removed from Plato's philosopher king, policy makers were replaced by an anonymous agent concerned with nothing but maximizing the interest of others. Researchers were occupied with defining an optimal policy and governments were assumed to follow it. There were of course exceptions. Marxists emphasized the state's class character and argued that it had always worked as an instrument for the ruling classes (Jessop 1982), but marxists were at this time mainly found within the humanities and their influence was small. And from the emerging public choice school came a growing body of research which had developed alongside the dominant approach, with a much more cynical idea of how policy-making works, but their influence on the main writings in public finance was yet limited (Winer and Hettich 2008, p. 443).

During the first three postwar decades, it was widely believed that a trade-off existed between inflation and unemployment, in which governments could choose which position on this 'Phillips curve' they preferred. In the trade-off between unemployment and inflation, economists and conservative politicians have always favoured lower inflation compared to the electorate in general (Samuelson 1977; Senate 1952). Political scientists analysed the trade-off between inflation and unemployment as a conflict between socioeconomic groups (Boix 2000; Hibbs 1977) and as late as the late 1960s, this policy choice could still be described as a class conflict even in the most prestigious economic journals (Johnson 1968, p. 986). Confident in their own expertise, and sceptical about the government's, many economists argued throughout the postwar period that central banks should exercise a fair degree of independence from the democratic pro-

⁷Foster and Catchings' 1927 book *Business Without a Buyer* elaborated on similar ideas.

cess (Senate 1952; Southard 1969), or, alternatively, that monetary policy should follow a pre-determined rule (Friedman 1962), otherwise inflation would be too high. However, because the goals of monetary policy were still contested, policy-making remained under democratic control.⁸

All of this would change with the stagflation of the 1970s. Real wages could no longer keep up with the productivity growth and the wage share began to drop. The magnitude and timing differed between countries, but everywhere it was accompanied by the unusual combination of both high inflation and increased unemployment. The failures were blamed on different forms of opportunistic behaviour and Samuel Huntington concluded that it was an 'excess of democracy' which had caused the difficulties in governing the economy (Crozier et al. 1975). Parallel to the economic problems, economists developed a theoretical explanation of the stagflation. The message was that in the long run, unemployment could not be held below its natural rate, and that popular attempts to reduce unemployment with expansive policies would only cause inflation to accelerate.⁹ This theoretical development, together with the stagflation of the 1970s and the relative success of early adopters like Germany, Switzerland and the United States, has influenced most rich countries to create independent central banks (Lohmann 2006).¹⁰ At least until 2008, independent central banks were claimed to have solved both the problem of inflation and major output fluctuations (Alesina and Stella 2010).

Just as inflation was put under control a new problem emerged. Government debt kept rising in many countries, even after the macroeconomic situation improved, and the deficits were larger than what could be justified by tax smoothing arguments (Barro 1979). The German sociologist Wolfgang Streeck analyses this sequence of happenings (wage resistance, inflation, public debt) as a result of a Polanyian counter-movement, where

⁸Even in countries with de jure independent central banks, policy was de facto influenced by governments.

⁹More precisely, if unemployment is kept under its natural rate, inflation will *accelerate* because wage-setting is based on expected inflation (Friedman 1968; Phelps 1967, 1968) and these expectations are affected by both observed inflation and expected policy. Kydland and Prescott (1977) added that governments have a problem with time consistency and credibility. After the government has announced its future policies and wages have been negotiated, the government has an incentive to change their policies in an expansionary direction. Because that temptation is anticipated by rational agents, the expansion turns into inflation. Barro and Gordon (1983a,b) argued that the problem would be smaller if monetary policy was rule-based and Rogoff (1985) showed that governments could overcome this time consistency problem by delegating policy to an independent authority.

¹⁰Central bank independence means that governments are prohibited from giving instructions to central bankers and typically includes a target for monetary policy, the irremovability of central bankers and sometimes even rules against re-election.

workers demand non-market allocation principles when the economy is stagnating (Streeck 2011, 2013). He describes a difficult dilemma, where governments must choose between protecting the economy and being responsive to voters (Streeck 2014). Economists share a similar idea of a deficit bias inherent in the democratic process. For different reasons (see next section), there are electoral incentives for governments to run larger deficits than optimal. A wide range of institutions have been proposed and implemented to reduce this deficit bias. Medium term expenditure ceilings force governments to commit to a maximum spending level a few years in advance; targets for the budget balance make it evident for voters when a government is spending more than it should, and de-indexation of public expenditures decreases the political cost of fiscal consolidation. Inspired by the delegation of monetary policy, economists have also suggested that some decisions regarding fiscal policy should be delegated to independent experts. Most proposals suggest that decisions about the budget balance can be delegated to such institutions (Bertelsmann 2013; Calmfors and Wren-Lewis 2011; Debrun et al. 2009; Wyplosz 2005), but Blinder (1997) goes further and argues that specific tax rates should also be placed under expert rule. The euro area has gone furthest in this process. Since the implementation of the ‘six-pack’ in 2011, the Commission has been given the mandate to hand out fines to member states that do not follow the Council recommendations with regard to the budget balance and other sources of macroeconomic instability, including the unemployment rate, the unit labour costs and other highly political factors. Moreover, the delegation of monetary policy has also given independent authorities the last say on demand management, because monetary policy can be used to counter the effects of the government’s fiscal policy.

Common to all these institutions is the fact that they partly protect the policy makers from pressure from the electorate. In its simplest form, the idea is that by correcting for the deficit bias, policy will be closer to what is optimal for the majority of voters. While cross-national studies have shown that conservative fiscal frameworks increase fiscal discipline (Hallerberg et al. 2007), these institutions have not stopped the public debt from growing. The Great Recession caused public debt to reach unprecedented levels and as shown in Figure A, the aggregate net debt in the OECD now amounts to approximately 70 per cent of GDP. If anything, we can expect the reforms of the budget process and fiscal frameworks to continue. How far can this process go on? Are some reforms more important than others? I will soon discuss the possible drawbacks of institutions which might widen the gap between public opinion and implemented policy, but first I will examine the theoretical explanations for the popular resistance described above.

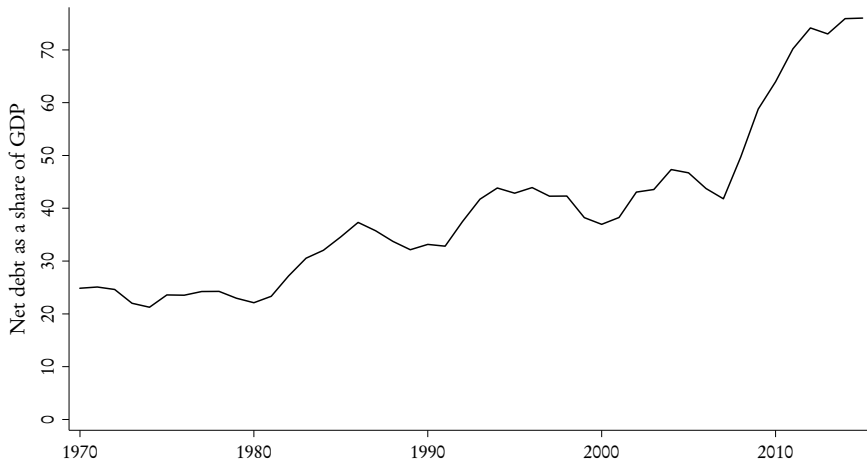


Figure A: Aggregate public net debt in the OECD

Understanding popular resistance

It is an old insight that unless perfect markets are disturbed by government invention, they will generate Pareto optimal outcomes – that is, no one can be better off except at someone else's expense. Why then, have people throughout history counteracted the market mechanisms by legislated price levels, production stops and the cancellation of previous agreements? What caused the Mesopotamian peasants to favour a system that must have made it difficult to borrow money at reasonable interest levels, the people of Lisbon to favour price control before market prices, and the British workers to destroy machinery instead of moving to an employer that paid better? Was it the same reasons that were behind the wage-inflation-spiral during the 1970s and which now cause people in Greece and other debt-burdened countries to reject austerity measures?

It is obvious that people hold a non-market notion of social justice (Streeck 2011). However, it will always be contested to which degree these actions reflect a legitimate class struggle between the oppressed and their oppressors, the insiders' protection from competition from others that are less privileged, or a sympathetic but counter-productive struggle against what most people perceive as inhumane and inappropriate. Functionally speaking, we can imagine four kinds of purposes for these anti-market actions. The first two are distributional, the third is caused by bounded rationality and the last corresponds to different kinds of social preferences. Below I briefly comment on the different options and the weight they are given in the public finance literature.

The first option is that the populace has engaged in a legitimate class struggle against their oppressors. In most cases, this corresponds to a

distributional conflict between buyers and sellers on a market, such as a labour market dispute between workers and employers. Class conflict is irrational on a perfect market, because market distortions make both buyers and sellers worse off. But as soon as a market is not perfect, for example because buyers have a monopolistic position or because the quantity of goods or services is limited, there can be a surplus to fight over.

This perspective is surprisingly absent from the fiscal sustainability literature, where budget deficits are typically analysed as an inter-temporal distribution of government consumption. That works well as long as governments are restricted to repaying their debt, but the possibility of default is fundamental to fiscal sustainability. Governments do default from time to time – the interest rate on government bonds is a sign of this – and when they do it has major distributional consequences. Defaulting on sovereign debt is definitely not a free lunch, but it is arguably more costly for the owners of government bonds. When the bonds are owned by other governments, governments in debtor countries are more negative towards fiscal consolidation than the creditor governments (Maatsch 2014). Moreover, expansive fiscal policy can also increase inflation, which has immediate redistributive consequences that differ between the wealthy and those in debt.

The second option corresponds to a distributional conflict between different groups of buyers or sellers, which arises because there are groups which do not internalize the consequences for actors outside their own group. This is the bearing assumption for many theories of political economy. We can easily apply the insider–outsider theory of Lindbeck and Snower (1988) to many pre-industrial labour markets, where guild systems, small villages and low labour mobility must have made it difficult for outsiders to underbid the insiders. As a result, insiders could receive wages higher than market wage without losing their job. Olson (1982) stated that a group will distort market outcomes when it is neither too small to be without power or too large to internalize the preference satisfaction of the majority. Calmfors and Driffill (1988) applied this insight to the labour market and argued that wages are higher than the market clearing level when unions are strong enough to influence wages but not united enough to internalize the societal costs of unemployment. Other examples include rent control on the housing market and other cases where the insiders prefer a queuing system instead of a market-clearing price.

There are also versions of this idea within the public finance literature. First, voters can exploit future generations – which of course cannot vote – by supporting large budget deficits and sending the bill to the future

(Bowen et al. 1960). Second, fiscal policy is often analysed as a common-pool where different interests demand reforms without internalizing their full costs (Velasco 2000; Wyplosz 2013).

The third option is that this resistance is caused by bounded rationality. In other words, the actors do not achieve what they attempt because of restrictions on the available information, their cognitive capacity or some other resource required for perfectly rational decisions. First, information is costly, and it can therefore be rational for individuals not to gather all available information about how markets work or the nation's public finances. Voters' lack of information about the public finances is a common explanation for why governments might be incentivized to run larger deficits than what the voters would have liked (Rogoff and Sibert 1988). Another common example of bounded rationality is temporal discounting – the tendency to want things now rather than later. Temporal discounting is often used to explain why we lack the discipline to quit smoking or start a savings account, even if we want to live a healthier life as well as save for our retirement. If voters value consumption during the current year much higher than future consumption, this would also provide governments with electoral incentives for fiscal profligacy.

The fourth option is that actors have social preferences, such that they do not solely attempt to maximize their material interests, but also try to follow their moral sentiments. Such preferences include altruism, reciprocity and the desire to uphold ethical norms (Bowles and Hwang 2008). In this introduction, I have presented a large number of examples of when the market has been regulated because the market outcomes were in conflict with societal norms. In the economic literature, examples can be found in the analysis of wages (Akerlof 1980, 1982), tax evasion (Andreoni et al. 1998; Bordignon 1993) and price setting on consumer goods (Kahneman et al. 1986). However, social preferences are virtually absent from the public finance literature concerned with fiscal sustainability.

We could imagine a fifth option for market interventions that are motivated by market failures or make markets work better by securing property rights and reducing transaction costs. The reason I did not include it here is because it does not constitute a threat to the market logic and it is unrelated to popular resistance. New economic institutionalism – quite similar to what political scientists call rational choice institutionalism – deals primarily with institutions that fill this purpose.

The legitimacy and efficiency of economic-political institutions

There is a consensus among democracy theorists that some constraints on the majority rule, such as clear rules for the decision-making procedure, strengthen the democratic process. For the same reasons, it is obvious that

certain kinds of budget rules indeed strengthen the deliberative element of policy-making (Molander 2001). This is especially true for rules that make trade-offs visible and reduce the information advantage that governments exert over the voters. However, other fiscal institutions are difficult to defend on grounds other than that they improve fiscal discipline. These include institutions that are i) irreversible, ii) increase the information asymmetry between the incumbent and the public, iii) induce a political bias, or iv) institutionalize contested policies. Examples of such institutions are the fiscal rules of the European Union, where revisions require a consensus among all member states; the non-indexation of expenditures, which makes it easy for governments to obfuscate cutbacks; and expenditure ceilings, which introduce an asymmetry in the treatment of spending hikes and tax cuts as well as prevent a Keynesian reaction to economic downturns.

At the same time, fiscal indiscipline has time after time led to debt crises where agreements with creditors or even military coups have resulted in democratic deficits of an entirely different magnitude. So what kind of constraints should be placed on the popular influence on fiscal policy? If a consensus exists among economists on the optimality of these constraints, can they still be questioned for their democratic legitimacy? While the essays in this thesis are focused on questions related to the output side – e.g., is the democratic process to blame for fiscal imbalances? – we cannot assess the normative aspects of fiscal institutions unless we also consider the costs for imposing constraints on the majority rule. Most of the arguments in this section are equally true for other aspects of policy-making, like the delegation of monetary policy to independent central banks.

One of the main arguments in favour of democracy is that it produces good outcomes because elections provide voters the ability to throw out incumbents when they expect the challenger to be more competent, thereby incentivizing politicians to maximize public welfare (Healy and Malhotra 2013). Using such sanctioning and selection models, it is easy to theorize when the representative democracy yields the best outcomes and when it is better to delegate decisions to an independent authority. Among economists, Alesina and Tabellini (2007, 2008) and Maskin and Tirole (2004) have written the most influential texts about the normative aspects of policy delegation. However, they are exclusively concerned with policy outcomes, which makes it a trivial conclusion that ‘if society could write optimal performance contracts with its policymakers, /.../ bureaucratic delegation under an optimal contract would always dominate /.../ [and] there would be no need for elections or politicians’ (Alesina and Tabellini 2008).

While economists have analysed the normative aspects of policy delegation with a strong focus on the policy outcomes, most political scientists analyse constraints on democracy as a trade-off between two partly conflicting goals, e.g. *legitimacy* and *effectiveness* (Lipset 1959, 1960), *input legitimacy* and *output legitimacy* (Scharpf 1999, 2013) or *responsiveness* and *responsibility* (Mair 2009, 2013).¹¹ Even if it were possible to delegate policy-making to a just and competent philosopher king, such a political process would lack democratic legitimacy and undermine the political system. I will briefly comment on three of the most common arguments for why output is an insufficient criteria – input legitimacy, deliberation and accountability.

Scharpf (1999, 2013) describes democratic legitimacy as a two-dimensional phenomenon. The *input* dimension concerns the congruence between policy choices and the salient preferences of the electorate while the *output* dimension is concerned with the degree to which outcomes attributable to the government satisfy the electorate's preferences. Legitimacy in both these dimensions, Scharpf argues, is important for the support of the government and, ultimately, the democratic legitimacy of a political regime. However, the two dimensions can be in conflict with each other, and in order to 'maintain output legitimacy through functionally effective policies, governments may need to employ instruments that undermine their input legitimacy'.

Scharpf's argument points to an important reason why the successful delegation of monetary policy cannot easily be transferred to fiscal policy. Despite its stabilizing role and distributive effects, monetary policy is considered to be less salient than fiscal policy. Therefore, the delegation of monetary policy to independent central banks is less of a dilemma than the corresponding delegation of fiscal policy, simply because it has a smaller effect on the input legitimacy of democracy.

Another way of theorizing input legitimacy comes from Mair (2009, 2013). He describes the regulation of macroeconomic policy-making as a conflict between responsibility – a difficult concept related to efficiency as well the commitment to agreements and accepted procedures – and responsiveness – the degree to which government action follows the preferences of the electorate. Because of the increased importance of fiscal frameworks, obligations to international organizations and commitments to prior agreements, among other things, Mair argues that it has become increasingly difficult for governments to bridge the tension between responsibility and

¹¹Economists, on the other hand, have accused political scientists of having a one-sided focus on the political *process* and ignoring its outcomes (Calmfors 2010). I acknowledge that most scholars, myself included, probably perceive other disciplines as more one-sided – and their own discipline as more balanced – than what is actually true.

responsiveness. Mair also claims that parties in government have become more responsible than responsive, or as Streeck (2011) puts it, ‘citizens increasingly perceive their governments not as their agents, but as those of other states or of international organizations, such as the IMF or the European Union’. The destiny of the Greek government under George Papandreou is an illustrative example. After close co-operation with the creditors, with little responsiveness to the Greek voters, his party, PASOK, went from holding 43.9 per cent of the votes in the 2009 elections to 4.7 percent in 2015.

Mair connects the lacking responsiveness of governing parties with the growth of populist parties. The more *responsible* the governing parties become – the more room there will be for opposition parties that promise to end fiscal consolidation, renegotiate terms with creditors and restore the country’s sovereignty. In the more extreme examples, such as Golden Dawn in Greece, such parties can even advocate the abolishment of democracy.

The second virtue of a democratic process is that it promotes deliberation and preference formation in the electorate. Follesdal and Hix (2006) go as far as to conclude that preference formation through deliberation and party contestation is what separates democratic rule from regulated benevolent authoritarianism. Claus Offe argues that non-participation in politics negatively affects preference formation (Offe 2011, 2013), making the opinions of those who do not participate ‘more ill-considered and unreflective’. The same argument could be used for whenever policy is removed from public debate. Deliberation is not only a matter of developing one’s own preferences, but also about fostering empathy with others and creating acceptance for their opinions.

The third argument in favour of the democratic processes concerns accountability, which can be defined as the possibility to replace the office-holders with other candidates (Gustavsson 2009). This minimalist definition is often expanded with the existence of a political opposition, making it possible for voters to identify alternatives and determine whether those in office are doing a good job. Needless to say, when policy is delegated to an independent authority, it is difficult for citizens to hold anyone accountable for implemented policy. This could entail increased discontent and disengagement with formal politics (Hay 2007). It could also create more space for populist opposition parties that do not feel obliged to honour the fiscal framework, while the traditional parties might be perceived as superfluous if they all subscribe to the same policies. In addition, if governments can blame the implemented policies on external constraints, they have even stronger incentives for advocating populist policies – not even in the long run can voters hold them accountable for the economic outcomes.

To sum up, there is a risk that more constraints on policy-making can weaken the input legitimacy, preference formation and accountability of our democratic system. This, in turn, can lead to reduced trust towards established parties, benefit populist parties, and reduce political participation – further decreasing policy responsiveness (Erikson 2015; Lijphart 1997). Admittedly, it is difficult to analyse these questions with a convincing identification strategy, and the theoretical support for these arguments is stronger than the empirical. But following a precautionary principle, less constraints on policy-making would always be preferred as long as they do not affect policy outcomes. Because constraints are likely to affect policy outcomes in a good way, this is a genuine trade-off. Trade-offs are seldom zero-sum games. By increasing our knowledge about the deficit bias, we can also learn more about what measures are needed to correct it. Applying ineffective or even counter-productive constraints will hurt legitimacy more than it improves fiscal discipline.

Contributions

This thesis consists of four essays which all contribute to the larger question of fiscal sustainability. In this section I will briefly summarize these contributions, focusing on the questions raised in this introduction. Each of the subsections broadly corresponds to one of the essays.

The electoral incentives for fiscal indiscipline

Conventional wisdom states that austerity policies are unpopular among voters and that those governments which implement cutbacks in social spending will lose votes in the subsequent election. But is it so? Can we be sure that voters punish governments, even if the latter only do what they think is right?

Most readers probably find it surprising that the assumption of a fiscally irresponsible electorate has received little empirical support. In fact, all attempts to test this claim have concluded that there is no electoral disadvantage for governments which implement fiscal consolidations and – in some cases – that voters even reward governments for fiscal discipline (Alesina et al. 1998, 2012; Brender 2003; Brender and Drazen 2008; Drazen and Eslava 2010; Lowry et al. 1998; Peltzman 1992). On the other hand, there are methodological problems with these studies, which call for a reassessment of the data. In my first essay, I claim that previous research has underestimated the true consequences for governments which implement fiscal consolidations. Contrary to the results of previous research, I find that voters indeed punish governments that implement large fiscal adjustments. This finding underlines the dilemma which arises when the

will of the people contradicts the consensus among economists or goes against previous commitments and international agreements. Should fiscal consolidations be implemented, even if the electorate wants otherwise? If so, how can we design institutions that reduce the electoral disadvantage for governments which implement unpopular policies?

While I find support for the assumption that voters punish governments for fiscal consolidations, the evidence is still not in line with the concept of a deficit bias. A deficit bias implies that budget deficits are larger than they would have been if policy had been decided by a benevolent social planner. The most common explanation of the deficit bias is that the incumbent government would be at an electoral disadvantage if it were to implement more conservative fiscal policy. There are several mechanisms which could explain the electoral incentives for a deficit bias. First, if voters care more about their own well-being than that of future generations, they can exploit the latter by voting for parties which run large deficits today and send the bill to future generations. Second, according to Olson (1982), concentrated minor interests are over-represented compared to diffuse majority interests. When public spending and tax subsidies are directed to concentrated interests, these interests will be more influential than the interests of those who eventually must pay for the reforms, which can cause the deficit to be higher than any of these interests would prefer (Velasco 2000). Third, voters might lack information about the public finances. If voters notice when they receive tax cuts and better services, but not when the government improves the budget balance, vote-seeking incumbents will not prioritize the latter. Last, voters might have time-inconsistent preferences, which causes them to react more positively to consumption today than consumption in the future.

All these mechanisms provide compelling arguments for why fiscally disciplined incumbents might be at an electoral disadvantage. As there is nothing in these mechanisms which suggests an asymmetrical relationship between fiscal policy and election results, we should expect governments to be rewarded for fiscal expansions and punished for consolidations. However, this is not what the data shows. According to the evidence presented in the first essay, voters punish governments which implement large fiscal adjustments, but they appear to be indifferent to small consolidations and fiscal expansions. Similarly, Ponticelli and Voth (2011) find that fiscal consolidations increase the probability of social unrest, but they do not find a similar decrease in instability following budgetary expansions. In other words, there are electoral incentives for fiscal profligacy, but only because voters oppose large consolidations.

These results suggest that there is an asymmetry in the popular resistance, such that people are more inclined to take actions to prevent any

worsening of their current position, than they are to demand improvements of their standard of living. Indeed, as illustrated by the anecdotes in this introduction, history is packed other examples of how popular resistance has not arisen before economic or social conditions have deteriorated. As long as the economy has performed well, opportunistic and market distorting behaviour has been relatively uncommon. When new machines were introduced during times of rising prosperity, they were not faced with much resistance (Hobsbawm 1952, p. 63). And during the golden decades after World War II, inflation was low, public finances were in good shape and there were only a small number of labour market disputes. But when the oil crises hit, labour unions took a stand against wage cuts and the number of strikes skyrocketed. At the same time, inflation had begun to accelerate and it did not take long before governments started to accumulate large levels of debt.

While there are many theories which can explain high inflation, large deficits and why wages are higher than market wages, most of them cannot account for why we do not observe the same phenomena during more stable times. For example, intergenerational exploitation could explain the lack of fiscal discipline observed in many countries. However, if voters seek to exploit future generations, why were debt levels not rising during the 1950s and 1960s? And why are there no electoral rewards for governments which implement fiscal expansions? To understand such contextual differences, I believe we must consider the fourth category of market resistance: social preferences.

Public debt and morals

We have now established that, on average, governments which implement fiscal adjustments lose votes in the subsequent elections, compared to governments which do not improve the budget balance. In other words, there appear to be electoral incentives for fiscal irresponsibility. But as argued above, this finding is difficult to reconcile with the concept of a general deficit bias, because people's acceptance of fiscal consolidation differs so much between contexts and situations.

During the second world war, half of the American population bought war bonds issued by the government. The bonds had a rate of return below the market value, but people bought them because it was the right thing to do. When others risked their lives at the front, those at home felt obliged to contribute in other ways. During the winter of 1944, the most common New Year's Resolutions were to improve one's character, go to the church more often and buy more government bonds (Gallup 1972).

In other situations, people have been much less willing to pay for government expenditure. As shown in the first essay, governments which

implement large fiscal consolidations are often punished in the subsequent elections. How is it possible that the 1944 Americans were ready to personally lend money to the government – which was even seen as a civic virtue – while in other situations, voters have found it unacceptable that the government raises taxes or cuts spending to reduce the country's debt?

From the earthquake in Lisbon to the resistance against wage cuts, I have presented many examples of how people resist market outcomes that they perceive as unfair. And if you ask people for their opinion on fiscal consolidation, you can be almost sure that they will answer you with a moral argument. However, fairness and other kinds of morality are virtually absent from the public finance literature. In the essay 'Not my problem – fairness and fiscal responsibility in the age of austerity', Rafael Ahlskog and I propose a new explanation for why voters may oppose fiscal consolidations. Our claim is that citizens are more willing to accept fiscal consolidations when they feel a responsibility for the public debt.

Responsibility is closely linked to the attribution of blame (Sahar 2014). When citizens feel partly responsible for the debt, or perceive that it was accumulated for a greater good, they are more likely to accept that they must pay a part of the cost when the debt is to be repaid. We apply this assumption to the consequences of banking crises and argue that voters are more likely to be opposed to fiscal consolidations if the fiscal problems are caused by large bailouts to the banks.

We use three different kinds of evidence to test our claim. First, an analysis of cross-national data shows that governments are punished hard for fiscal consolidations if there have been any large and recent bank bailouts. We find no electoral consequences of fiscal policy in periods without bailouts. Second, we conduct a survey experiment where we provide respondents with different information about the reasons for the public debt. One third of the respondents were informed that the debt was caused by a bailout to the banks. The other respondents were either not given any reason for the fiscal problems, or were informed that the debt was accumulated because the government ran a budget deficit over several decades. The respondents were then asked about their opinion on implementing a mix of tax hikes and spending cuts to start repaying the debt. The results support our hypothesis. When the banks are blamed for the fiscal problems, the respondents were more negative towards fiscal consolidation than when the fiscal problems were blamed on budget deficits which were accumulated over decades. Third, we categorize the free-text answers which the respondents used to motivate their opinion on fiscal consolidation. Among those who were told that the debt problems were caused by a crisis in the banking sector, it was much more common to argue that the debt was not the responsibility of the respondent or the (American) people.

But what can we learn from these results, other than that governments should think twice before implementing fiscal consolidations shortly after a large bank bailout? For the problems discussed in this introduction, the most important question is probably how people's sense of responsibility is affected by their influence over fiscal policy. When things go wrong and a need for fiscal consolidation arises, do people feel more responsible if fiscal policy has been debated in the election campaigns? It is common for governments to use the central bank as a scapegoat for labour market outcomes (Alesina and Stella 2010). Will the electorate support fiscal consolidation, if the government blames the problem on international organizations like the IMF and the EU? Until more research is conducted on this topic, we can only speculate. But as soon as people's actions are affected by their moral sentiments, it becomes much more difficult to design institutions which yield optimal incentives for the desired outcomes (Bowles and Hwang 2008).

Politics or perceptions?

The first two essays show that it can be costly for governments to implement large fiscal adjustments, but why do governments put themselves in these difficult situations? None of the essays find that there are electoral incentives for governments to accumulate debt through expansionary policy. Arguably, the problems would be small if governments consolidated public finances during times of favourable economic conditions.

In the third essay, titled 'Politics or perceptions', I argue that many governments have also accumulated debt during favourable times, simply because the policy makers and their economic advisers perceived the situation as less favourable than it actually was. If governments believe that the economy is in a temporary downturn and that the economic situation will improve in a few years time, their fiscal policy will be more expansive than if they had believed that they were at the peak of the business cycle. There are two reasons for this connection between perceptions of the economy and implemented policy. First, if the government believes in counter-cyclical fiscal policy, it will implement expansionary policy to bring output back to its potential level. Second, if they believe that the economic situation will improve, they will also assume that the budget balance will improve automatically when the economy improves, and therefore underestimate the need for discretionary action.

The decade preceding the Great Recession was characterized by fast growth and low unemployment. In retrospect, governments should have used this opportunity to consolidate public finances. However, these years were not perceived as an economic upturn at the time when the budgets were passed. On the contrary, independent experts consistently assessed

that output in most OECD countries was slightly below its long-run trend. For this reason, policy advice given by international organizations like the IMF and the European Commission was also very lauditive about many countries' half-hearted attempts at consolidating finances, compared to the harsh criticism delivered to them after the financial crises struck.

Orphanides (2001, 2002, 2003) and Orphanides and Williams (2004) find similar results with regard to the conduct of monetary policy during the Great Inflation. They argue that a persistent downward shift in productivity led to misperceptions about the economy's cyclical position, which in turn resulted in overly expansionary policy and accelerating inflation. This view of the Great Inflation as an 'honest mistake' stands in sharp contrast to those who argue that policy-making suffered from an 'inflation bias' (Barro and Gordon 1983b).

Do my results imply that there is no deficit bias in fiscal policy? I previously defined a deficit bias as a setting where deficits are larger than they would have been had policy been decided by a benevolent social planner. However, it is difficult to test this claim, because we do not know what fiscal policy would have looked like had it been conducted by independent experts. And compared to monetary policy and inflation, it is more difficult to evaluate the conduct of fiscal policy.¹² But at the least, we can conclude two things. To begin with, honest misperceptions about the economic situation can account for a large part of the lack of fiscal discipline during the 1996–2012 period. Moreover, the results imply that fiscal problems are not in themselves sufficient proof that something is wrong with policy-making. When debt grows to unsustainable levels, it can also be the result of honest but unfortunate mistakes.

De-indexation and fiscal outcomes

The sustainability of public finances is more challenging today than ever before, with debt in most countries being on unsustainable trajectories (Bertelsmann 2013). As I show in my first essay, it is unlikely that governments can revert this trend without facing electoral losses. How should we understand these challenges that governments face? What can be done to reduce the electoral consequences of fiscal adjustments?

The 'new-politics' approach to the welfare state research is based on the claim that welfare state retrenchment is different from welfare state expansion because it requires incumbents to implement unpopular policies (Pierson 1996). In contrast to the credit-claiming efforts of welfare state expansion, the capacity to pursue austerity politics is determined by the

¹²The largest difference between fiscal and monetary policy is probably that the stock of debt is more important in fiscal policy than the price level is for monetary policy.

potential to lower the visibility of the reforms (Pierson 1996; Weaver 1986).

The least visible cutbacks are probably those which do not require discretionary actions. When expenditures are formulated on a nominal basis, governments can allow inflation and wage increases to erode the value of benefits and transfers (Green-Pedersen 2002; Lindbom 2007; Pierson 1994). In line with this kind of obfuscation, the results in my first essay show that the electoral consequences are smaller if fiscal consolidation is implemented during times of rapid growth.

Further de-indexation of public expenditure has recently been proposed as a ‘less painful’ strategy to improve public finances (Marcel 2014). By making it easier for governments to obfuscate cutbacks, de-indexation is probably good for fiscal discipline, but it comes at a cost.

The first problem with de-indexation is that it might hurt the input legitimacy of fiscal policy, as it decreases the transparency of the budget process. During the last decade, both the OECD and the IMF have emphasized the importance of fiscal transparency to improve the sustainability of public finances. Transparent budget processes are thought to improve fiscal rectitude because they increase the political risks of unsustainable policies and make it difficult to hide fiscal profligacy from markets and voters (Craig and Kopits 1998). As long as it both improves fiscal discipline and increases voters’ information about implemented policy, it is easy to argue in favour of increased fiscal transparency. However, some aspects of the budget process would most likely reduce fiscal discipline if they were made more transparent. This is particularly true for the indexation of public expenditures, which would make it difficult for governments to hide cutbacks from voters. This could be the reason why neither the OECD’s ‘Best Practices’ (OECD 2002) or the IMF’s ‘Fiscal Transparency Code’ (IMF 2014) require that governments disclose information about how the real value of existing programmes are deflated.

The second problem is that de-indexation can distort policy outcomes. In the fourth essay, titled ‘Economic growth is deflating the welfare state’, I argue that economic growth has eroded the value of both public transfers and services in many countries. As a consequence, countries with rapid growth have reduced the size of their public sectors. In fact, differences in economic growth can account for large parts of the cross-national variation in welfare state development over the last two decades. In other words, while de-indexation can improve fiscal discipline, it might affect the size of the public sector in a direction that has not been chosen by voters.

This situation creates a dilemma, because consolidating public finances is one of the most challenging tasks that governments face. It is therefore important that we can develop institutions which facilitate budget im-

provements without distorting effects in politically contested areas. One such possibility would be to combine de-indexation with medium-term targets for the tax ratio. If governments would like to use the fiscal space on tax cuts, they would have to change their tax targets. A second possibility is to increase the share of temporary spending with so called sunset clauses.¹³ Such explicitly temporary expenditures are probably best suited for counter-cyclical policy during economic downturns.

Conclusions

There is an ongoing tendency toward the depoliticization of fiscal policy, with an increased reliance on independent fiscal institutions and constitutional balanced-budget requirements (Fernández-Albertos 2015). This trend is strongest in the European union, where fiscal decisions are now monitored by supranational authorities. The depoliticizing tendency is not without a reason. Indeed, the fiscal problems of OECD countries are worse than ever before, and the ongoing debt crisis has demonstrated the consequences for countries which lose control over their public finances. Moreover, the euro has increased the interdependence of European economies and increased the costs for imbalances between countries.

At the same time, many democratic theorists worry about an increasing citizen distress in established democracies. Political participation as well as trust in the democratic institutions has fallen (Pharr and Putnam 2000). This, in turn, has worsened inequality in political responsiveness (Erikson 2015) and increased the over-representation of people with high incomes and conservative attitudes (Hacker and Pierson 2010). Colin Crouch argues that we are moving towards a post-democratic society, where the impotence of egalitarian politics and the trivialization of democracy is transforming democratic institutions to a formal shell (Crouch 2000, 2004). According to Crouch, the treatment of the eurozone crisis is a good example of how politics works in a post-democracy (Crouch 2013). There are no quick fixes to this dilemma, but by carefully analysing the causes behind our fiscal and democratic deficits we can hopefully strike a better balance in the trade-off described above.

Taken together, the results in the first three essays have made me sceptical to the fruitfulness of *deficit bias* as an analytical concept. A bias usually denotes a distortion that is always present. Indeed, all the common explanations for fiscal indiscipline provide incentives for the government to consistently let the budget balance deviate from optimal policy. Why, then, was fiscal discipline so strong during the three decades after the

¹³Sunset clauses provide a date for when a law, a tax reduction or spending item will cease to exist.

second world war? Arguably, the case for exploiting uninformed voters and future generations should have been much larger when independent central banks could not counter-act expansionary policies and the debt was too small for financial markets to worry. Instead, as I find in the first essay, the electoral incentives for a deficit bias are asymmetrical, so that voters oppose large fiscal consolidations but are more or less indifferent to fiscal expansions or small fiscal consolidations. This result is difficult to reconcile with the theoretical arguments usually suggested for why fiscally disciplined governments are at an electoral disadvantage.

The second essay investigates one reason why voters behave differently in different contexts. Ahlskog and I find that voters are more positive to fiscal consolidation if they perceive they have a responsibility for the public debt. We also find that large bank bailouts are one way of reducing that feeling of responsibility. This kind of moral argument is virtually absent from the public finance literature, but I believe that we must understand how moral sentiments affect attitudes if we want to explain the variation in citizens' degree of fiscal responsibility.

In the third essay I argue that fiscal imbalances would arise even if fiscal policy was governed by independent experts. The reason for this is that we have a tendency to perceive slowdowns caused by structural changes as temporary downturns, while economic upturns are perceived to be the normal state of the economy. Because such misperceptions are more likely when the economic situation changes, this is also a potential explanation of why debt in most countries did not start to grow until the 1970s.

To me, it is obvious that fiscal policy has been too expansive *on average*, but that this tendency is better understood as a set of specific problems than as a permanent bias in the political system. These include resistance to large fiscal consolidations and an unwillingness to pay for things one is not responsible for, as well as systematic misperceptions about the business cycle. But what does that tell us about how fiscal policy should be conducted? I will end this introduction by briefly commenting on some of the dilemmas we face when designing fiscal frameworks and other institutions.

It is increasingly common to base fiscal policy on indicators of what the budget balance would be during normal economic circumstances. For example, the European Stability and Growth Pact requires countries to define their medium-term budget targets in structural terms. Such practice has strong theoretical appeal, but these indicators are difficult to calculate. And as shown in the third essay, biased perceptions about the economic situation will cause the structural budget balance to look better than it actually is. I therefore find it unlikely that fiscal frameworks that are

dependent on forward-looking budgetary rules and estimates of structural balances will improve fiscal discipline.

While a deficit bias in the traditional sense could easily be solved by forcing the incumbent to follow a forward-looking rule for the budget balance, it would not eliminate misperceptions about the economic situation and other kinds of honest mistakes. According to my results, even independent experts would periodically accumulate more debt than optimal. This is an argument in favour of general fiscal prudence and some sort of correction mechanism for previous mistakes. The latter can be achieved through backward-looking indicators of the budget balance or even targets for the debt level.

Most important is perhaps that we humbly acknowledge that governments will continue to accumulate large levels of debt, even if we manage to improve fiscal discipline. For example, both Iceland and Ireland spent more than 40 per cent of GDP on their recent banking rescues. A major war would cost much more than that. Fiscal shocks of that size can cause an immediate need for fiscal consolidation. And as shown in the first essay, governments might be at an electoral advantage if they choose to delay such adjustments. So how can we increase our capacity to handle a sudden increase in debt? As highlighted by the results in three of my essays, we here face two dilemmas.

First, a common tactic to reduce the electoral consequences for cut-backs is to allow growth and inflation to erode the real value of expenditures. Indeed, in my second essay I find that the electoral costs for fiscal adjustments are smaller during periods of fast growth. However, such obfuscation measures come at a cost. As I argue in my fourth essay, a budget process where expenditures are set in nominal terms may increase fiscal discipline, but it will also distort the size of the public sector from what it would have been had the electorate made an informed decision.

Second, Ahlskog and I find that responsibility for the public debt is fundamental for citizens' willingness to support fiscal consolidation. But how is this sense of responsibility affected if fiscal policy is based on rules or decided by independent experts? Is it possible that the gap between the citizens' policy preferences and the views of economic experts will widen, if fiscal policy is brought further away from immediate popular influence? Are there measures we can take to improve fiscal discipline with the support of the people? Lipset (1959) argued that democratic legitimacy determines a country's capacity to recover from recessions and lost wars. Improving fiscal policy while maintaining its legitimacy is one of the largest challenges we face today.

Essay I

Abstract: In both economics and political science, conventional wisdom states that austerity policies are unpopular among voters, and that those governments which implement tax hikes and cutbacks in public spending will lose votes in the subsequent election. However, this claim has received little empirical support. This paper finds that parties which implement fiscal consolidations are punished by the voters in the following election, a result that goes against previous research, but one which is in line with conventional wisdom. The estimated effects are larger when the adjustments are visible and when there is a unified control of policymaking. There do not appear to be any electoral consequences for implementing fiscal expansions.

Punished for austerity?

The electoral consequences of fiscal adjustments

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

The assumption that voters punish governments for fiscal austerity is a defining feature of many theories central to both economics and political science. It is one of the core assumptions of the *new politics* of the welfare state (Pierson 1998); it is supposed to give rise to *political business cycles* (Franzese 2002); and it is a cornerstone of the *public finance* literature (Alesina and Perotti 1995). However, this assumption has received little empirical support. Most evidence suggests that there are no electoral consequences for governments that pursue austerity measures (Alesina et al. 2012; Giger and Nelson 2012), or that fiscal adjustments actually improve the chances of re-election (Brender 2003; Brender and Drazen 2008; Drazen and Eslava 2010). How is it possible that these studies contradict what at first sight appears so obvious?

In this essay I will argue that previous research has underestimated these electoral consequences as a result of two identification problems. First, it is possible that governments that are confident of being re-elected are more likely to consolidate public finances. This problem of reverse causation is larger when re-election is used as the dependent variable, instead of the change in vote shares. Second, budget improvements often result from favourable economic conditions rather than discretionary fiscal adjustments. It is therefore important to use a cyclically adjusted measure of the budget balance or control for macroeconomic conditions. Unless both of these problems are addressed, fiscal adjustments will appear more popular than they actually are.

After replacing the variables used in previous research with variables less susceptible to estimation bias, I find that parties which implement fiscal consolidations are punished by the voters. The estimated effects are large. For every percent of GDP by which the budget balance is improved, the vote share for each government party is predicted to fall by one percentage point. I also examine whether the effects differ depending on the visibility of fiscal consolidations and the degree of clarity of responsibility. The results confirm that the electoral consequences are larger for the prime minister's party and when the adjustments cannot be hidden behind economic growth. I also find suggestive evidence that left-wing governments are punished harder when defaulting was an alternative to consolidation.

On the other hand, I do not find that voters reward governments which implement fiscal expansions. My interpretation of this asymmetry between consolidation and expansion is that the concept of a general *deficit bias* can be misleading. Under normal circumstances, there are no electoral reasons for why fiscal policy should be different from what it would be with a social planner.¹⁴

Previous research

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

I am familiar with seven studies that deal directly with the electoral consequences of fiscal consolidations. Three of them analyse the variation between countries to study fiscal policy at the national level. Alesina et al. (1998) and Alesina et al. (2012) examine cabinet changes in OECD countries, while Brender and Drazen (2008) analyse a larger panel of countries. None of these studies find that governments are more likely to be voted out of office following a fiscal consolidation. On the contrary, the results indicate that voters are fiscal conservatives and punish governments that weaken the budget balance. The other studies use regional variation

¹⁴There are explanations of a deficit bias which do not build upon an electoral disadvantage for fiscally disciplined governments, like the strategic debt approach by (Alesina and Tabellini 1990; Persson and Svensson 1989).

within countries (Brender 2003; Drazen and Eslava 2010; Lowry et al. 1998; Peltzman 1992). All of these conclude that incumbents are punished for loose fiscal policies. More precisely, Peltzman (1992) concludes that voters dislike public spending regardless of whether it is paid by tax hikes or deficits, Lowry et al. (1998) find that voters dislike both positive and negative imbalances and Brender (2003) only finds any effect in one out of the three elections analysed.

In other words, little empirical evidence has been provided for the conventional wisdom that voters punish governments for fiscal adjustments. However, there are two reasons why I think we should doubt the conclusions reached in previous research.

First, the majority of the studies mentioned use cabinet changes instead of vote shares as the dependent variable (Alesina et al. 1998, 2012; Brender 2003; Brender and Drazen 2008). Not only does this mean that a lot of variation is lost, which is scarce in the context of large fiscal adjustments, it also exacerbates the risk of reverse causality. If governments which anticipate a high probability of remaining in office are more likely to implement fiscal adjustments, this would bias the estimates so that fiscal consolidations appear more popular than they actually are. There are good reasons to believe this is true. If the government expects a close election, they might not take the risk of unpopular austerity measures. If the governing parties are certain of defeat, they may not worry too much about the election result, but might still postpone fiscal consolidation in order to restrict their political opponents' room for manoeuvre (Alesina and Tabellini 1990; Persson and Svensson 1989; Pettersson-Lidbom 2001).

There are many reasons why a government party might expect to be re-elected. One reason could be that support for the party has grown in the opinion polls since the last election, in which case the problem of reverse causality remains regardless of how the dependent variable is measured. Other reasons include how close the last election was, the outlook for coalition formation, and different party characteristics which affect the probability of being included in the government. For example, populist and ideologically extreme parties are probably less likely to be re-elected than fiscally responsible and centrist parties. For all these reasons, the risk of reverse causality disappears when the change in vote share is used as the dependent variable. Moreover, to the extent that the problem remains, it causes a bias toward an underestimation of the true electoral costs of fiscal adjustments. Because this is the first study to find any punishment effects at all, I believe that is a minor problem.

Second, most of these studies use a measure of the budget balance that has not been adjusted for macroeconomic conditions (Brender 2003; Brender and Drazen 2008; Drazen and Eslava 2010; Lowry et al. 1998).

As most budget improvements result from economic upturns rather than discretionary fiscal actions, and voters reward incumbents for both low unemployment and fast growth (Lewis-Beck and Stegmaier 2000), the estimated electoral consequences will be biased unless the economic situation is properly taken into account. In other words, the use of unadjusted budget balances might lead us to estimate the electoral consequences of favourable economic development instead of fiscal consolidations. This problem is best solved by using a cyclically adjusted measure of the budget balance. If such measures are not available, researchers should at least control for the unemployment rate or the GDP gap.¹⁵

Another strand of research related to this essay is the welfare state research on ‘new politics’ and the retrenchment of the welfare state. One of the core assumptions in this literature is that cutbacks in social programmes – much more than tax hikes – are so unpopular among voters that governments which implement them will try to avoid public debate and responsibility for such reforms (Pierson 1996). However, similar to the situation in the public finance literature, the conventional wisdom that voters punish governments for retrenchment has received little empirical support (Armingeon and Giger 2008; Giger and Nelson 2011, 2012; Schumacher et al. 2013).

While spending cuts constitute a larger part than tax hikes in most fiscal consolidations (Devries et al. 2011), retrenchment differs from fiscal adjustments in three important ways. First, it has been a continuous process – going on for decades in many countries – and is therefore commonly referred to as a state of ‘permanent austerity’ (Pierson 1998). Second, considering that retrenchment in many countries has been accompanied by large tax reductions, these cutbacks are not necessarily implemented to strengthen public finances. Therefore, most studies in this field actually estimate the joint effect of cutbacks and tax reductions, not only the effect of austerity. Third, when measuring retrenchment, theoretical relevance and politically contested reforms appear to have precedence over empirical concerns. Most studies therefore analyse reforms of a much smaller scale than what is done in the public finance literature. For example, the largest fiscal adjustments in this essay amount to almost 15 per cent of GDP. Reducing the replacement rate in the unemployment insurance by five percentage points – which Armingeon and Giger (2008) call a ‘major retrenchment’ – would in most countries not even cut public spending by 0.1 per cent of GDP.¹⁶

¹⁵Of these four studies, only Brender and Drazen (2008) use a satisfactory set of macroeconomic control variables. I acknowledge the difficulties involved in adjusting the budget balance for the business cycle. However, that is not an excuse for not trying.

¹⁶Because of these three differences, this study should not be interpreted as a test of new

Why would voters punish responsible governments?

It is easy to doubt the findings of previous research when it contradicts conventional wisdom. However, as the provocative title of this section suggests, it is not obvious why voters would oppose fiscal consolidation. In fact, if voters are either discounting the future at the same rate as the financial markets, or if they are able to compensate for government policy through changes in private saving, they should be indifferent to the inter-temporal reallocation of taxes and public spending. It is also difficult to imagine why governments would implement supposedly unpopular policies if they did not believe them to be beneficial for the citizens. To argue that we should expect voters to punish governments for fiscal consolidations, we must make additional assumptions about how voters behave. Broadly speaking, we can understand opposition to fiscal consolidations as the result of either a deficit bias or a bias against negative events.

In the public finance literature, it has traditionally been argued that politics is characterized by a deficit bias. The behaviour of voters plays a key role in most of the explanations for this bias. Either because voters are short-sighted (Buchanan and Wagner 1977), exploit future generations (Alesina and Perotti 1995; Bowen et al. 1960; Cukierman and Meltzer 1989) or lack information about the nation's financial position (Rogoff and Sibert 1988). If at least one of those assumptions is true, voters might prefer government spending today in favour of future government spending and therefore disapprove of fiscal adjustments implemented by the government. For the same reason, we would expect voters to reward governments for fiscal expansion.

Voters might also oppose fiscal consolidation because they react asymmetrically to contractions and expansions. This is a cornerstone of the 'new-politics' approach, which emphasizes how retrenchment of the welfare state is something fundamentally different from the expansion of the same. Pierson (1996) states two reasons for this asymmetry. First, the costs of retrenchment are typically concentrated, immediate and well-defined. According to dominating theories of collective action, clearly defined and concentrated interests will gain stronger political support than those which are diffuse and scattered (Lohmann 1998; Olson 1965; Wilson 1973). For example, a school closure will typically face resistance from both pupils and teachers, but when a decision is taken to fund the establishment of a new school, there will be no one to celebrate the decision. Pierson's second argument is that voters exhibit a negativity bias, in that they react more strongly to losses and negative risks than to gains and other positive

politics. However, the results in this essay, as well as the clarification of these differences, should interest anyone who analyses retrenchment of the welfare state.

events. Following the seminal works of Kahneman and Tversky (1979, 1984), this loss aversion has been confirmed in a countless number of experiments. Beginning with Bloom and Price (1975), this has also been a common finding in the literature on retrospecting voting (Lewis-Beck and Paldam 2000). To Pierson's arguments, we can add that the economic news coverage appears to be more responsive to negative events (Soroka 2006).¹⁷

Accountability and conditional effects

The ability of the electorate to punish the parties responsible for fiscal adjustments requires that voters both observe government policy and know which parties are responsible for it. I use the term *visibility* to denote the degree to which voters have information about the government's fiscal policy, while *clarity of responsibility* refers to whether voters know which party should be held accountable for it. These concepts bear close resemblance to König and Wenzelburgers (2014) division of political strategies into those which affect *the perception of policies* and those which affect *the attribution of responsibility*. However, the term strategies would be misleading in my case, because most variables which affect visibility and responsibility are primarily governed by other things than strategic choices by the incumbent parties. For example, it is probably difficult to escape blame for the government party to which the prime minister belongs, but having one of its members appointed prime minister has more to do with election results than with strategies of blame avoidance. I end this section with a discussion of whether voters hold left- and right-wing parties accountable to the same degree.

Visibility

Different concepts of visibility and transparency are important in both the public finance and the retrenchment literature. Researchers in the former tradition have argued that fiscal transparency reduces budget deficits, because governments cannot hide them from the voters (Alt and Lassen 2006b). In the latter literature, it has long been argued that retrenchment bears no costs for incumbents that manage to avoid blame and public

¹⁷ A fourth reason for why we might estimate asymmetric effects has to do with how consolidations are measured. The size of a fiscal adjustment is usually defined as the change in the cyclically adjusted budget balance. However, these adjustments are difficult to make, and what appears as large fiscal expansions in the data often result from downward revisions of potential output. Similar upward revisions are much less common, which makes the size of fiscal consolidations more reliable than the size of fiscal expansions.

debate. Visibility can be operationalized in many different ways. In this essay I will examine three kinds of visibility and how it affects the voter reactions to fiscal adjustments.

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

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

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

Clarity of responsibility

It is not always evident which party should be held accountable for fiscal policy. When no party has a majority of their own, government policy

will be the result of negotiations – either between parties in a coalition government or between the government and other parties in parliament. Power can also be divided between the national and the local level or between the executive and the legislative majority. When the responsibility for fiscal policy is shared between several parties, I would expect the electoral consequences to be smaller for any single party.

Much of the literature on retrospective voting builds on the concept of *clarity of responsibility*, which was first developed by Powell and Whitten (1993). Their argument is that ‘the greater the perceived unified control of policymaking by the incumbent government, the more likely is the citizen to assign responsibility for economic and political outcomes to the incumbents’. I will sometimes refer to it as just *responsibility*, because separation of powers is factual and not only a matter of clarity.¹⁸

After numerous empirical studies (Bengtsson 2004; Duch and Stevenson 2008, 2010; Powell and Whitten 1993; Rudolph 2003), a consensus has emerged that economic voting is conditioned by the clarity of responsibility.¹⁹ Hobolt et al. (2013) find that electoral accountability is more affected by variables related to the government, which vary over time, than by constitutional variables. Based on their results, and because most of the variation in my dataset is found over time, I will focus my study on variables that determine how much influence each party has compared to other parties.

The literature on economic voting is primarily concerned with unemployment, growth and inflation, but there are also indications that responsibility conditions the effects of fiscal consolidations. Lowry et al. (1998) find that accountability is stronger when the governor and the state legislative majority belong to the same party. Alesina et al. (2012) present some suggestive evidence that minority and coalition governments are more likely to be re-elected after a fiscal consolidation than majority and single-party governments. And the literature on economic voting has shown that small parties in coalition governments can gain votes from

¹⁸Whether weak and disperse governments are held accountable to a lesser extent because voters do not know which parties to blame, or because voters realize that parties should not be punished when their influence over policy was limited compared to the influence of other parties or exogenous shocks, does not affect how I design my models. While the first approach is dominant in this field, there are also exceptions (Duch and Stevenson 2005).

¹⁹This consensus is problematic. There is an unfortunate habit in this literature of not formally testing the hypothesis – e.g., through an interaction term – but instead running split sample regressions and drawing conclusions from why some coefficients are significant and others are not. See Gelman and Stern (2006) for the problems with this approach. In fact, the difference in accountability between clear and non-clear responsibility does not appear to be statistically significant in many of these studies (Bengtsson 2004; Powell and Whitten 1993).

the dominant parties when the latter are punished for poor economic conditions (van der Brug et al. 2007).

In this essay I include three dummy variables which measure the degree to which a government party can be held responsible for implemented policy. These are assigned the value one if the prime minister belongs to the party, if the government holds a majority of the seats in parliament and if the government is a coalition government. They are otherwise coded as zero. The clarity of responsibility is assumed to be high when a party has the position of the prime minister and constitutes a majority government on its own.

Partisan differences

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

On the other hand, the credibility of left-wing parties in these matters may be so strong that it makes them immune to criticism about being anti-welfare. Both Kitschelt (2001, p. 275) and Ross (2000) argue that, just as it was the anti-communist Nixon who went to China, social democratic or labour parties have more credibility in social policy and are therefore less likely to be punished by the voters. Tavares (2004) uses the same metaphor to make a similar point: implementing policies in disagreement with the party’s ideological profile is a way of signalling to the voters the necessity of a fiscal consolidation. Therefore, he argues, governments must implement reforms that collide with their ideological position. It is only when right-wing governments raise taxes and left-wing governments cut expenditures that the voters give them credibility.

Previous empirical research does not offer much more guidance than that, because it is typically concerned with slightly different topics. Lowry et al. (1998) do not find any differences in voter reactions to deficits between democratic and republican incumbents. However, they find that voters punish republicans for increasing the size of government and

democrats for unexpected cutbacks, which is difficult to reconcile with the findings in Tavares (2004). Pettersson-Lidbom (2001) finds that right-wing governments accumulate debt when they face a high probability of defeat. His interpretation is a strategic use of debt in the sense of Persson and Svensson (1989), but the same pattern would emerge if parties are more vote-seeking when facing a close election and voters are more responsive to the fiscal policies of right-wing parties. And while it has been shown that left-wing parties are less likely to implement cutbacks (Allan and Scruggs 2004; Korpi and Palme 2003), which in turn has been interpreted as showing that their electoral incentives differ (Giger and Nelson 2011), it could very well reflect purely ideological differences.

Fiscal policy is often analysed as an inter-temporal distribution of consumption. Because the lines of conflict between left and right are different from those between generations, the fiscal balance is not often politicized in the literature. However, I would argue that the distributive aspects of fiscal balances are more evident when a government default is perceived as an alternative. The worst losers of large deficits are then the holders of government bonds that lose their investments when the government defaults or deflates its debt instead of repaying the debtors. For example, following the second world war, many countries had debts in the range of 100–200 per cent of GDP. Had governments chosen to implement fiscal adjustments instead of letting inflation take care of the debt, it would have had radically different redistributive effects in favour of the often wealthy rentiers.²⁰ It is likely that voters become disappointed when they perceive a left-wing government as taking the side of the creditors. For that reason, I would expect left-wing parties to be punished harder than right-wing parties for fiscal consolidations, at least when the risk of default is substantial. To measure this risk, I use the nation's long-term interest rates on government bonds compared to the interest rate on government bonds in Germany.

Method

The empirical strategy of this essay is to regress each party's change in vote share between two elections on the size of fiscal adjustments implemented while the party was in government. The size of the fiscal adjustment is defined as the accumulated change in the cyclically adjusted net lend-

²⁰I am not arguing in favour of defaulting on public debt. The fact that capital owners lose more than other citizens does not alone imply that a policy is either just or sound. It should also be noted that countries differed in their post-war policies on public debt. For example, Britain was more keen on repaying its debtors than Germany or France (Piketty 2014).

ing, which is a measure of the budget balance that has been adjusted for macroeconomic conditions. I will refer to it as the structural balance. If correctly estimated, changes in the structural balance will reflect discretionary actions, such that an increase in the structural budget balance will correspond to either a tax raise or a reduction of expenditure (if not both).

Each observation corresponds to a party (p) over one election period (e) and the sample consists of parties that spent at least half the election period in government. The main sample includes elections from 27 countries between 1974 and 2014²¹. The basic structure of most regressions follows the equation below, where the party's change in vote share ($\Delta v_{p,e}$) is regressed on the change in the structural budget balance while that party was in cabinet ($\Delta sbb_{p,e}$) and a vector of control variables ($\mathbf{x}_{p,e}$). In the second table, I use interaction variables to analyse whether the electoral consequences are conditioned by different measures of visibility, responsibility and partisanship. In all these models I also include interaction terms between the change in the structural balance and the control variables.

$$\Delta v_{p,e} = a + b \times \Delta sbb_{p,e} + \delta \times \mathbf{x}_{p,e} + e_{p,e} \quad (1.1)$$

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

$$\Delta sbb_{p,e} = \sum_{c=1}^n (sbb_{last,c} - sbb_{first,c}) \times cab_{p,c} \quad (1.2)$$

The literature on economic voting has shown that incumbents are punished for slow growth (Campbell 2005), rising prices and high unemploy-

²¹The countries included are Australia, Austria, Belgium, Canada, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Iceland, Ireland, Italy, Japan, Luxembourg, Netherlands, New Zealand, Norway, Poland, Portugal, Slovenia, Spain, Sweden, Switzerland and United Kingdom

ment (Lewis-Beck and Stegmaier 2000). Because all these variables might affect the probability of fiscal adjustments, I include the average GDP growth, the average inflation and the change in the unemployment rate as control variables. They are all measured over the whole election period. I also include the change in the annual net migration, measured as a share of the population, and the size of the party at the previous election. To control for time trends in the data, the election date has been transformed to a cubic time spline which is included in all models.²² Every model also includes country fixed effects. Because the main variables measure changes over time, these country dummies control for the cross-national variation in electoral performance (compared to the previous election) for government parties. For example, because country characteristics could affect both the volatility of fiscal policy and the cost of ruling,²³ the estimations would be biased unless those country characteristics are controlled for.

As argued earlier, there are reasons to believe that the electoral consequences of fiscal consolidations are not the additive inverse of fiscal expansions. More precisely, voters might punish governments for fiscal consolidation to a higher degree than they reward them for fiscal expansions. I will test this asymmetry through an interaction effect, before eventually restricting the sample to episodes of fiscal contraction. Data on vote shares, coalition composition and the ideological position of parties is provided by the *Parlgov* database. All economic data comes from OECD Economic Outlook 96 and the index of fiscal transparency is collected from Alt and Lassen (2006a). A complete variable list is included on the last pages of this thesis.

Results

I present my regression results in three tables. Table 1.1 contains the main results and illustrates why previous research has not found that voters punish incumbents for fiscal consolidations. This is also where I test the asymmetric effects between expansions and consolidations. The conditional effects of visibility, clarity of responsibility and party ideology are presented in Table 1.2. Table 1.3 presents the results from a set of robustness checks.

²²A cubic spline is a set of piecewise third-order polynomials which pass through a set of knots. I use 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. The second derivative of each polynomial is set to zero at the endpoints.

²³Incumbent parties have indeed fared better in Germany and the Nordic countries than governments in southern Europe, where fiscal policy has tended to be less conservative (Bengtsson et al. 2014, p. 126).

Table 1.1: The electoral consequences of fiscal adjustments

	(1) Re-election	(2) Vote change	(3) Vote change	(4) Vote change
Δ Str. budget balance (Δ SBB)	0.01 (0.01)		-0.33** (0.14)	-1.11*** (0.23)
Δ Non-adjusted budget balance		0.03 (0.10)		
Δ Unemployment rate	-0.02 (0.02)		-0.59** (0.23)	-0.52** (0.22)
Average inflation	0.00 (0.01)		-0.09 (0.09)	-0.10 (0.07)
Average growth	0.00 (0.03)		-0.04 (0.39)	-0.09 (0.37)
Previous vote share	0.01*** (0.00)	-0.17*** (0.03)	-0.16*** (0.03)	-0.16*** (0.03)
Δ Net migration	-0.03 (0.12)	-0.32 (0.79)	-1.29* (0.69)	-0.41 (0.67)
Expansion				-2.93*** (0.82)
Δ SBB $\times$ Expansion				0.93*** (0.33)
Constant	0.43 (0.62)	12.41* (6.58)	10.82 (7.42)	11.02 (7.63)
Country dummies	Yes	Yes	Yes	Yes
Cubic time spline	Yes	Yes	Yes	Yes
Observations	341	341	341	341
Adjusted R^2	0.05	0.17	0.24	0.27

Election-clustered standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1.1 shows how the estimated electoral effects of fiscal adjustments depend on the choice of dependent variable and whether the macroeconomic conditions are properly controlled for. The first column presents the results from a linear regression where the dependent variable is the binary outcome of re-election.²⁴ The coefficient of the structural budget balance is positive but very close to zero, indicating that an incumbent party's probability of re-election is unaffected by fiscal adjustments. This finding is in line with previous studies that use cabinet changes as the dependent variable.

The second column shows the estimated effects when a measure of the budget balance is used that has not been adjusted for the business cycle. Like all models, it includes a time spline, which might capture some common economic shocks, but all macroeconomic control variables are omitted. The estimated effect of fiscal adjustments is still positive and close to zero. This is probably because the coefficient here captures the

²⁴To facilitate comparisons between the models, I present the results from a linear regression even if the dependent variable in this case is binary. The interpretation remains the same if I use logit or probit instead.

joint effect of budget improvements that result from economic upturns (positive electoral consequences) and discretionary fiscal actions (negative electoral consequences).

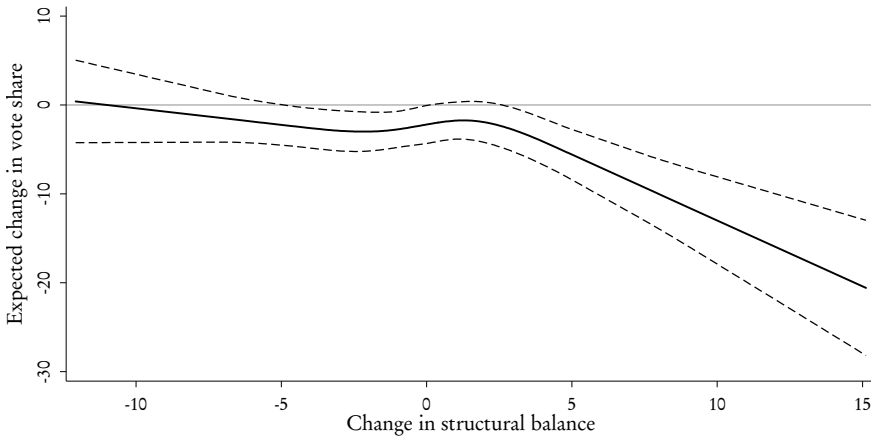
In the model presented in the third column, I have replaced the nominal budget balance with a cyclically adjusted balance and added the change in the unemployment rate, the average inflation and the average growth as control variables. Compared to the model in the first column, it is instead the dependent variable which differs. The estimated electoral effect of fiscal policy is now statistically significant. For every per cent of GDP by which the government raises (lowers) taxes or reduces (increases) spending, an incumbent party is expected to decrease (increase) its vote share by 0.33 percentage points.

So far, the results in Table 1.1 support my criticism of previous research. Arguably, both the choice of dependent variable and the adjustment for macroeconomic conditions are critical. As soon as cabinet changes are used as the dependent variable, or if macroeconomic conditions are not taken into account, the estimated effects of fiscal adjustments disappear. One could argue that staying in cabinet is the ultimate goal for a party – and that votes are only valuable if they affect the probability of incumbency – but then they should not interpret the effects in terms of voter behaviour. Besides, this argument is invalid if the reason behind the difference in results is that the estimation bias caused by reverse causation is larger when re-election is used as the dependent variable.

As argued earlier, there are reasons to believe that voters are more willing to punish governments for fiscal consolidations than they are to reward fiscal expansions. To differentiate between these effects, the model in the fourth column includes an interaction term between the change in the structural budget balance and a dummy variable for fiscal expansions (1 if ΔSBB is negative, 0 if ΔSBB is positive). The top row coefficient can now be interpreted as the electoral consequences of fiscal *consolidations* – and it increases substantially. For every per cent of GDP by which the budget balance is improved, the party is expected to decrease its vote share by one percentage point. The interaction term has a positive effect of a similar size, which implies that the punishing effect of consolidation has no rewarding counterpart for fiscal expansions. The negative effect of the expansion dummy implies that, compared to a modest fiscal expansion, the consolidation must amount to at least a few percent of GDP for any electoral punishment to take place.

This asymmetry is illustrated in Figure 1.1, which shows the estimated vote share conditioned by the change in the structural budget balance. The regression model is identical to the model represented in the last column of Table 1.1, with the exception that a cubic spline is used here

Figure 1.1: The asymmetric effect of fiscal policy



Predicted change in vote share when the change in the structural budget balance is modeled using a restricted cubic spline and the values of all other variables are set to their mean. The shaded area represents a 95 percent confidence interval for the estimate.

to allow for a more flexible functional form than permitted by the linear interaction. The graph confirms the previous conclusions. First, there do not seem to be any major electoral consequences of fiscal expansions. On average, the vote share for incumbent parties increases as long as they do not implement fiscal consolidations, but large fiscal expansions do not yield any further advantages. Second, neither can we identify an electoral effect of small consolidations, because the slope does not become negative until the budget balance is increased by at least 2–3 percentage points. This could reflect the fact that the budget balance is usually improved in the absence of discretionary actions, because most expenditures are not tied to the increase in wages or GDP, but it could also be a pure coincidence.

We will now proceed to the conditional effects. For this analysis, the sample has been restricted to observations where there was a positive change in the structural balance. The alternative would have been to include interactions for any variable where we might expect asymmetric effects. Because this applies to more or less all the variables, it would only have resulted in a more complex parametrization. Table 1.2 presents the results from four regressions where the electoral consequences are conditioned by the degree of visibility, clarity of responsibility and the party's ideological position. All models expand upon column 3 in Table 1.1 by including an interaction term between the size of the fiscal adjustment and a set of conditioning variables. All models also include interactions between the change in the structural balance and the different control variables, but they are excluded from the table because of space restrictions.

Table 1.2: Conditional effects

	(1)	(2)	(3)	(4)
Δ Str. budget balance (Δ SBB)	-0.35 (0.74)	0.25 (1.77)	1.74* (0.94)	-0.81 (1.85)
Δ Unemployment rate	-2.04*** (0.74)	-0.60 (0.40)	-1.51*** (0.52)	-1.92*** (0.62)
Average inflation	0.36** (0.14)	-0.02 (0.12)	1.72*** (0.58)	1.48 (0.90)
Previous vote share	-0.10** (0.05)	-0.29** (0.12)	-0.10** (0.04)	-0.31*** (0.11)
Δ Net migration	-1.99** (0.76)	-17.93*** (2.36)	0.85 (0.74)	-20.18*** (3.16)
Average growth	-1.59** (0.71)	-0.27 (0.48)	-0.56 (0.48)	-1.22** (0.53)
Fiscal transparency	-0.82 (0.74)			0.96 (1.05)
Prime minister's party		7.53** (2.86)		8.06*** (2.89)
Majority government		0.25 (2.01)		-0.72 (1.99)
Coalition		0.83 (2.92)		0.87 (3.02)
Right-wing party			3.21* (1.69)	1.73 (1.63)
Int. rate dif.			-0.79 (0.48)	-0.73 (0.50)
Right-wing party $\times$ Int. rate dif.			-0.58* (0.33)	-0.14 (0.50)
Δ SBB $\times$ Average growth	0.43*** (0.14)			0.45** (0.20)
Δ SBB $\times$ Δ SBB	-0.10 (0.11)			0.20 (0.17)
Δ SBB $\times$ Fiscal transparency	-0.45** (0.21)			-0.27 (0.23)
Δ SBB $\times$ Prime minister's party		-1.81** (0.79)		-2.30*** (0.83)
Δ SBB $\times$ Majority government		-1.17* (0.66)		-0.47 (0.54)
Δ SBB $\times$ Coalition		0.07 (1.11)		0.60 (1.13)
Δ SBB $\times$ Right-wing party			-1.75* (0.90)	-1.02 (1.13)
Δ SBB $\times$ Int. rate dif.			-0.36** (0.18)	-0.69* (0.38)
Δ SBB $\times$ Right-wing party $\times$ Int. rate dif.			0.49** (0.22)	0.42 (0.39)
Constant	18.39** (7.31)	11.01 (7.35)	2.30 (7.81)	7.83 (10.53)
Observations	148	168	165	143
Adjusted R^2	0.22	0.45	0.41	0.35

All models include country fixed effects, a cubic time spline and interactions between SBB and the control variables (unemployment, inflation, previous vote share and net migration). Election-clustered standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The first column includes three variables which are assumed to affect the visibility of fiscal consolidations. These are the average GDP growth during the election period, the square of the size of the consolidation and an index of fiscal transparency. As shown by its positive coefficient, rapid growth reduces the electoral costs of fiscal adjustments. I have two explanations for this phenomenon. First, public spending as share of GDP decreases automatically when GDP grows, because most expenditures are not fully indexed to the growth of output and wages. Such decrementalistic cutbacks are probably the least visible of all. Second, voters might not pay as much attention to a tax hike if they experience a simultaneous increase of their gross wage, because their net income would then remain unchanged.²⁵ Armington and Giger (2008) argue that it is better for governments to implement incremental adjustments over a long time instead of doing it all at once, because voters do not react to small cutbacks. If they are right, and the marginal electoral effect of an improvement in the budget balance increases with the total size of the consolidation, the squared balance would have a negative effect. While the coefficient has the right sign, it is not statistically significant. Alt and Lassen (2006b) argue that fiscal transparency increases the visibility of government actions, because it allows voters and other actors to separate policy decisions from stochastic factors. The negative coefficient for the interaction with their index of fiscal transparency provides support for their argument. When fiscal transparency is high, the predicted electoral loss following a consolidation is larger. To sum up, the results presented in the first column provide support for the fact that the electoral consequences of fiscal consolidations are larger when the latter are visible to the voters.

The model presented in the second column includes variables which measure the degree to which the party is responsible for the implemented policy. It is assumed that the prime minister's party has more influence over policy than other government parties, that a majority government has more influence than minority governments – which are dependent on the support of parties outside parliament – and that parties in coalition governments have less influence than single-party governments because they must negotiate with the other government parties. The interaction terms for both the prime minister's party and belonging to a majority government has large effects in the expected direction, although the latter is only significant at the 90 per cent level. It should also be noted that the model includes an interaction term between the change in the structural balance and the previous vote share, so the effects should not be driven by

²⁵ A third – although less likely – alternative, is that voters are more responsive to negative actions of the incumbent when they are in a bad mood or already critical to the economic performance (Bower 1981)

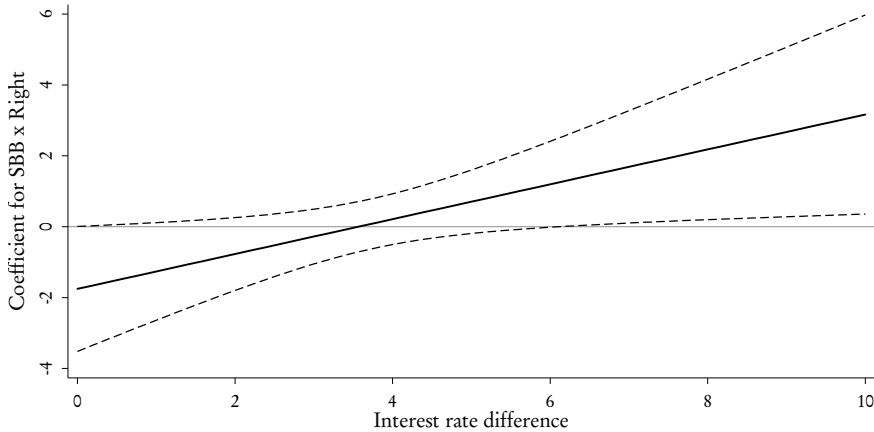
large parties being punished harder in absolute terms. In other words, the results indicate that voters hold influential parties responsible to a higher degree than government parties with little influence over policy.

The third column shows how the electoral consequences are conditioned by the interest rate difference compared to Germany and party ideology, with left-wing parties serving as the category of reference. The hypothesis is that left-wing parties are punished harder for fiscal consolidations when default is perceived as an alternative. The probability of default is here measured as the interest rate difference. The interaction effect between the change in the budget balance and the interest rate (-0.36) shows that the electoral cost that left-wing parties face after fiscal consolidation increases with the interest rate. Because the three-way interaction has a positive coefficient of similar size (0.49), no such effect is found for right-wing parties. This partisan difference in how the interest rate conditions electoral consequences is statistically significant at the 95 per cent level. This result supports the hypothesis that left-wing parties are punished harder for implementing fiscal adjustments when there is a large probability of default on the government debt. However, the table does not tell us whether the difference between left-wing and right-wing parties is significant at high interest rates. We can only see that the difference between left- and right-wing parties borders on significant when the interest rate difference is zero (-1.75).

A graphical solution to this problem is presented in Figure 1.2. The line shows how the ideological position of a party conditions the electoral consequences of fiscal consolidations. A positive value means that left-wing parties lose more votes than right-wing parties when they implement a fiscal adjustment. For interest rate differences above 4 percentage points, the estimated consequences are larger when left-wing parties implement fiscal consolidations compared to when right-wing parties do it. This difference is statistically significant for interest rate differences above 6 percentage points.

This three-way interaction has a strong theoretical appeal. Fiscal adjustments are ideologically neutral when it is a matter of consumption today or consumption in the future. But when default is a realistic alternative, fiscal consolidations have distributional consequences in favour of the creditors, and it is probably more costly for left-wing governments to be perceived as serving the interests of big banks or the IMF. However, the results should be interpreted as suggestive. The effect is barely significant at the 95 per cent level, it is estimated on relatively few observations and – as shown in the last column of Table 1.2 – it is no longer statistically significant when we include all regressors in the same model.

Figure 1.2: Ideology, the risk of default and electoral consequences



This graph shows how the ideological position of a party conditions the electoral consequences of fiscal consolidations. A positive value means that left-wing parties lose more votes than right-wing parties when they implement a fiscal adjustment. The height of the line is given by the coefficient for $SBB \times Right\text{-wing party}$ (-1.75) plus the coefficient for $SBB \times Right\text{-wing party} \times Int. \text{ rate dif}$ (0.58) multiplied by the interest rate. The shaded area represents a 95 percent confidence interval.

On the one hand, it is not surprising that the standard errors are inflated when so many interaction variables are included in the same model. This is the nature of multicollinearity and country level data. On the other hand, it means that we cannot separate the effects of the different variables from one another with sufficient precision. Therefore, we cannot be sure whether the conditioning effects of fiscal transparency, majority governments and party ideology were indeed spurious, or if it only is the lack of precision which is the reason for why they are no longer statistically significant.

To examine whether the main results are robust to alternative specifications, I have estimated different variations of the main model. The results from these regressions are presented in Table 1.3. The model in the first column is identical to that of the third column of Table 1.1, with the difference that fiscal expansions have been excluded from the sample, and it serves as a point of reference for the other columns. All the other models address specific problems that might affect my results.

First, as argued by Lindbom (2014), if the fiscal consolidations were expected by the voters, they might have already punished the responsible parties in the preceding election, in which case the estimations in this essay would underestimate the true effects. To investigate this possibility, the second column shows a model where the dependent variable is substituted

Table 1.3: Robustness tests

	(1)	(2)	(3)	(4)	(5)
Δ Str. budget balance (Δ SBB)	-0.94*** (0.27)	-0.72** (0.32)	-0.90*** (0.27)		-0.54** (0.26)
Δ Underlying primary balance				-1.15*** (0.28)	
Δ Unemployment rate	-0.80** (0.40)	-0.99*** (0.30)	-0.82** (0.37)	-1.00*** (0.37)	-0.81** (0.40)
Average inflation	0.09 (0.12)	0.17 (0.20)	0.11 (0.12)	0.28** (0.13)	0.08 (0.11)
Average growth	-0.22 (0.53)	-1.24** (0.50)	-0.37 (0.52)	-0.45 (0.50)	-0.50 (0.50)
Previous vote share	-0.10** (0.04)	0.06 (0.05)	-0.07** (0.04)	-0.10*** (0.04)	-0.10** (0.04)
Δ Net migration	-0.19 (0.60)	-2.57*** (0.87)	-0.58 (0.65)	-0.29 (0.54)	-0.46 (0.56)
Vote change prev. election			-0.16* (0.10)		
Constant	4.80 (8.38)	3.27 (10.69)	4.48 (7.79)	0.90 (9.18)	8.18 (7.81)
Observations	171	170	170	171	169
Adjusted R^2	0.36	0.17	0.38	0.39	0.38

Election-clustered standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

with the change in vote shares compared to two elections ago. If there is prospective punishment, we would expect the coefficient to increase, but instead the estimated effect is now marginally smaller than before.

Second, the inter-election correlation of the dependent variable is negative, meaning that parties that gain votes in one election tend to reduce their vote share in the election after that. If parties that do well are more likely to implement fiscal adjustments, that could possibly bias my results. However, as shown in the third column, controlling for the lagged change in vote share does not alter any of my conclusions.

Third, in this essay I have measured the size of fiscal consolidations as the change in the cyclically adjusted net lending. Another alternative would be to use the underlying primary balance, which is a measure of the budget balance where the interest payments on government liabilities have been excluded. As shown in the fourth column, changing variables does not affect my results.

Fourth, while most observations consist of minor fiscal adjustments, the large adjustments exert a major influence on the estimated effects. Calculating $dfbeta$ statistics reveals that 15 observations can be characterized as influential, using the common threshold of $|dfbeta| > 2/\sqrt{n}$. There is nothing suspicious about any of these influential observations and they

certainly belong in the sample. However, it is still interesting to see how sensitive the results are to the exclusion of one or two observations.

The most influential observations are the 2012 election for the Greek party PASOK and the 2013 result for the Social Democratic party in Iceland. In both these cases, the ruling party faced huge vote losses after having implemented large fiscal adjustments. If both observations are excluded from the sample, the coefficient drops to -0.54 , but it is still significant at the 95 per cent level. This model is presented in the last column of Table 1.3. A substantial negative dfbeta coefficient was found for the Liberal Party of Canada, which only lost 3 percentage points in the 1997 elections, even though the party had implemented an extensive fiscal consolidation and promised to continue the same policy if they remained in office. The successful consolidation in Canada has partly been attributed to its strong public support. For two decades, it was politically extremely difficult to run deficits in Canada (Guillemette 2010).

Conclusions

In many theories central to both economics and political science, a core assumption is that voters punish governments which implement fiscal adjustments. This is also a commonly held view among policy makers and political commentators. To the best of my knowledge, this is the first study to provide empirical evidence in support of this claim.

The estimated effects are significant. According to my estimates, each government party loses one percentage point of its vote share for every per cent of GDP by which the budget balance is improved. I have also found that political parties can avoid electoral consequences when cutbacks can be hidden behind economic growth and when the party's responsibility for policy implemented is unclear. Finally, I have found suggestive evidence for the hypothesis that left-wing parties are punished harder when fiscal discipline can be framed as a conflict between debtors and creditors instead of a purely inter-temporal distribution.

The largest identification problem, in this as well as in previous studies, is most likely the risk for reverse causation. If popular governments – or less popular, for that matter – are more likely to implement fiscal consolidations, that would bias the results. I believe that the best way to handle this problem would be to analyse the dynamics and timing of political support and fiscal adjustments. It is therefore promising that comprehensive cross-country datasets on vote-intention polls are under development (see, in particular, Jennings and Wlezien [2016]).

The asymmetrical responses to fiscal expansions and consolidations have important implications. First, they provide an argument against a

deficit bias caused by fiscal illusion (Rogoff and Sibert 1988), short-sighted voters (Buchanan and Wagner 1977) and the exploitation of future generations (Bowen et al. 1960). Possible explanations for this asymmetry can be found in the new-politics approach (Pierson 1996, 2001), which is built on the fundamental differences between the expansion and retrenchment of the welfare state. Second, constitutional settings that increase accountability are usually thought to be beneficial for fiscal discipline (Persson and Tabellini 2003). However, if the main challenge lies in the electoral incentives for governments to implement consolidations, we could expect fiscal discipline to benefit from institutions that *reduce* accountability, at least during periods of austerity.

My results also indicate that more research is needed both on the circumstances that shape voter behaviour, and on how government coalitions are formed during times of economic and political turbulence. There are many examples of fiscal adjustments which have been followed by tremendous vote losses for the incumbent parties. But there are also many examples of governments being re-elected after implementing large fiscal consolidations. Moreover, even when parties experience large reductions in voter support, they may sometimes manage to remain in government. One of the most dramatic examples of this is PASOK who, after losing 70 per cent of their voters, managed to spend another election period as part of the coalition government in Greece.

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.

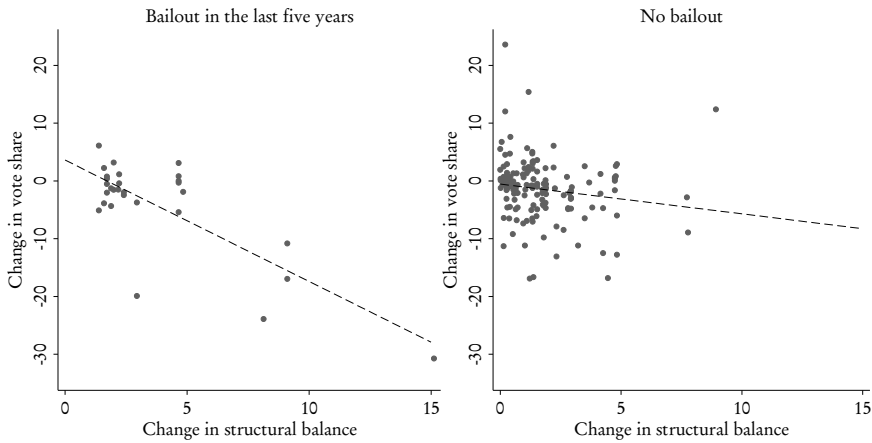


Figure 2.1: Bailouts and the electoral consequences of fiscal consolidation

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

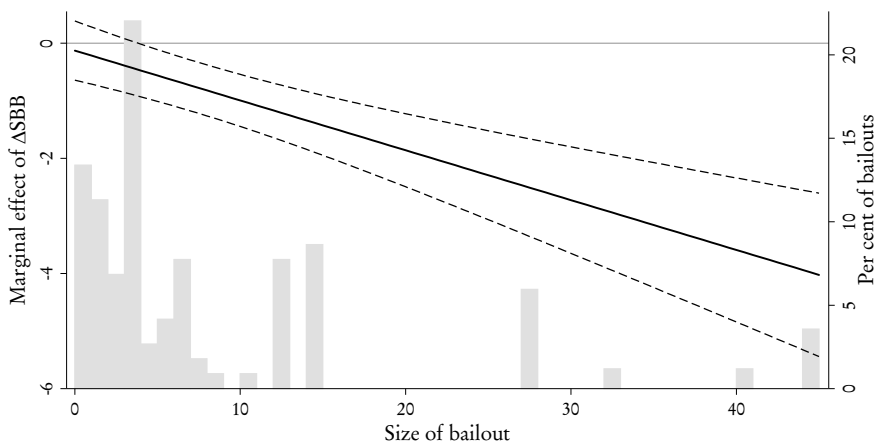


Figure 2.2: Marginal effects on electoral support (left Y-axis) of structural budget balance, with histogram over bailout size (right Y-axis)

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

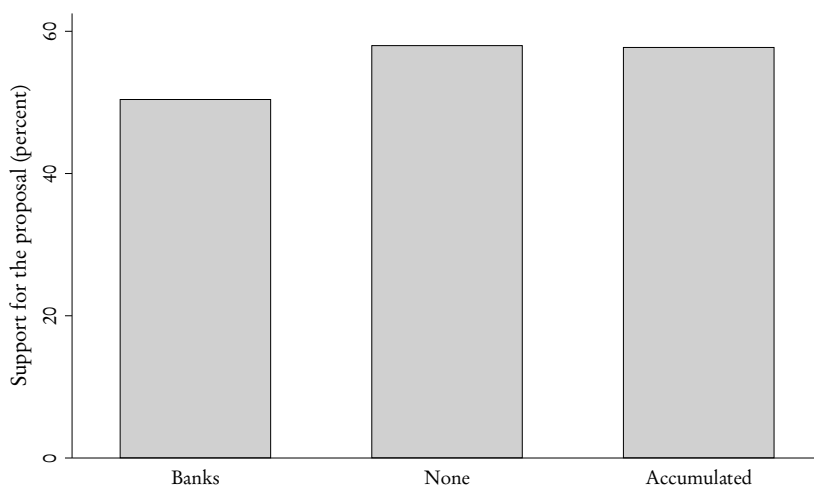


Figure 2.3: The share of respondents that agree with the proposal

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.

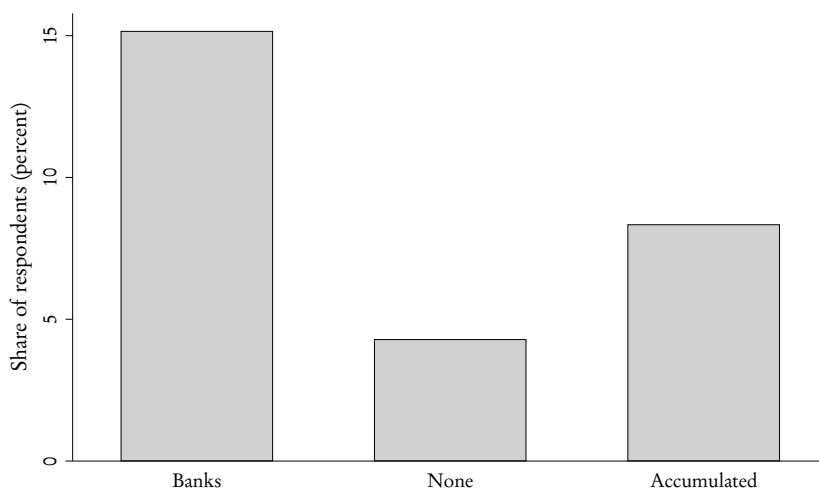


Figure 2.4: Respondents that deny responsibility for the public debt

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

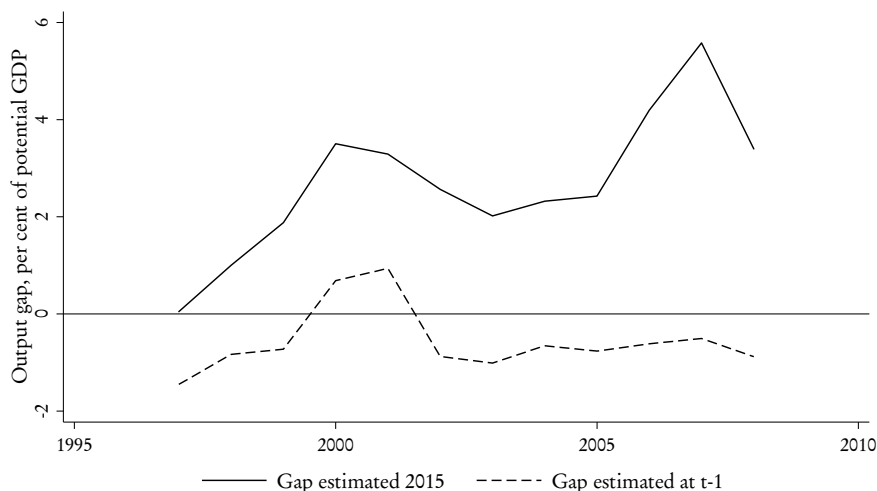
The global economic crisis that began in 2008 has once again put public finances at the heart of academic and public debate. In many countries, public debt has soared to unsustainable levels and forced governments to implement austerity measures, with severe economic and social consequences. The worst 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

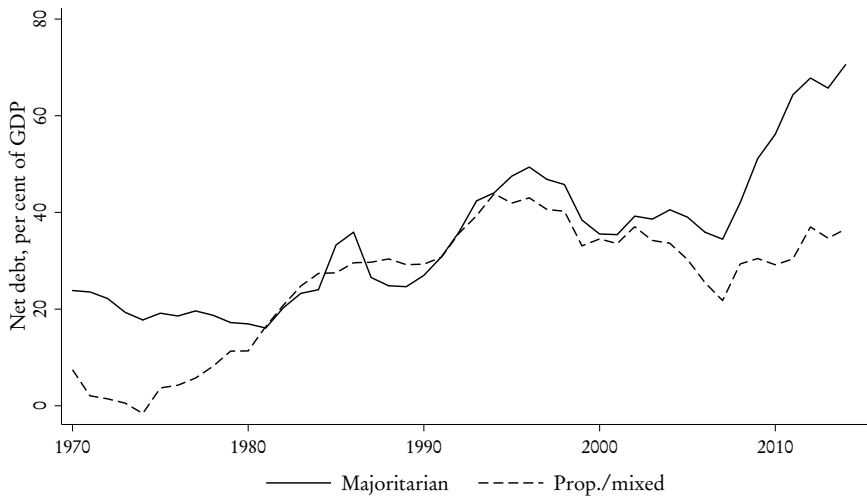
the electoral system, typically measured as a district magnitude. Fabrizio and Mody (2006) examines fiscal policy in a panel of Eastern European countries, none of which have majoritarian systems, and finds that large district magnitude (many elected candidates per district) is associated with larger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

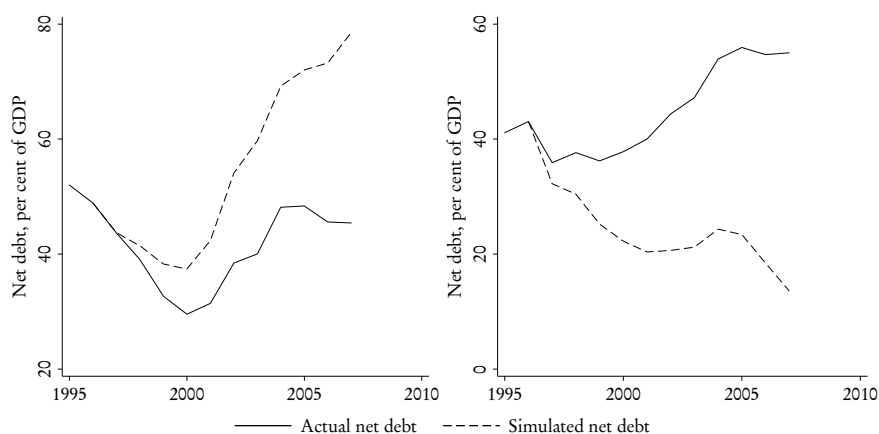
We now proceed to the main results. Table 3.4 presents regression results for the models with economic perceptions. Column 1 and 2 broadly correspond to the equations with the same numbers (with GDP growth and the unemployment rate added as controls). The first column represents a model with revised 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)
$\text{Left-wing} \times \text{Gap}_t^{t-1}$				0.27 (0.38)	
$\text{Left-wing} \times \text{Gap}_t^t$					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)
$\Delta\text{Inflation}_t$			0.59* (0.31)	0.59* (0.32)	0.42 (0.29)
$\Delta\text{Int.payments}_t$			-2.14 (1.64)	-2.31 (1.76)	-2.01 (2.26)
ΔDebt_{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)
$\Delta\text{Inflation}_t$	0.59* (0.31)	0.48 (0.30)	0.08 (0.08)	0.60** (0.27)
$\Delta\text{Int.payments}_t$	-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.

Essay IV

Abstract: Differences in economic growth is one of the primary explanations for why welfare state retrenchment has occurred in some countries and not others. Because public spending is only partially indexed to earnings, the welfare state shrinks as the economy prospers. Using growth data from countries with synchronized business cycles as an instrument, I demonstrate that the relationship can be interpreted as a causal effect of growth on both tax ratios and social spending.

Economic growth is deflating the welfare state

Most theories on institutional change assume that changes fill a purpose. The power resources approach analyse welfare outcomes as the result of distributive conflict between socioeconomic classes, typically mediated through class-based parties (Allan and Scruggs 2004; Korpi and Palme 2003). Scholars within the Varieties of Capitalism approach claim that institutions evolve to complement other institutions in a country (Hall and Soskice 2001). Yet others have argued that welfare state development is governed by demographic pressure and inter-generational conflict (Wilensky 1975), or pre-determined by globalization and its economic constraints (Cameron 1978; Schwartz 1994).

Comparative studies of welfare spending typically analyse the welfare state as a distributive conflict between those who benefit from public spending and those who do not (Huber and Stephens 2001; Meltzer and Richard 1981; Moene and Wallerstein 2003). When formalized, the size of the public sector is usually assumed to follow the preferences of the median voter. Identifying the goals and constraints of policy makers or interest groups is often a fruitful approach for understanding reforms, but searching for a purpose when there is none will always prove futile.

In this essay I argue that much of the recent development of taxation and social spending does not reflect voter preferences or the power of organized interests. On the contrary, the different trajectories that welfare states have taken during the last two decades can to a large extent be explained by differences in economic growth. Slow-growing economies have expanded their public sector, while countries with rapid growth have reduced both taxes and social spending.⁴⁸ This relationship is illustrated by Figure 4.1, which shows the average annual growth and the accumulated change in tax ratio between 1995 and 2015. We can see a clear pattern where the fastest growing economies have reduced the size of their public sector by around 5 per cent of GDP, while the slow-growing economies have experienced a similar expansion.

⁴⁸I will later argue for the assumed causal direction and for why the tax ratio is a good measure of welfare effort.

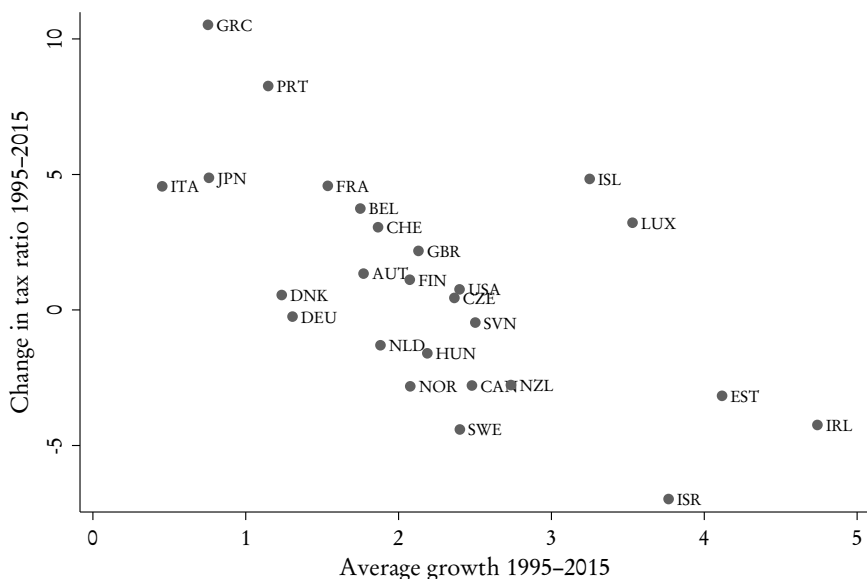


Figure 4.1: Growth and welfare state development 1995–2015

This relation between economic growth and welfare state retrenchment should not be confused with economic determinism – there is no reason why rich countries cannot sustain high taxes and generous welfare systems. Instead, I argue that the relationship reflects an unintended consequence of how the budget process works. When the economy grows, public spending automatically decreases as a share of GDP, because most expenditures are not tied to wages or GDP growth. Unless this erosion of existing programmes is neutralized by government decisions to increase spending, there will be a reduction in the resources available for social insurance and welfare services.

There are several reasons to believe that this deflating effect is not neutralized by discretionary actions. First, when a government faces fiscal space for reforms, it might come naturally to spend it on a mix of tax cuts and spending increases, even if the budget space resulted from a lack of expenditure indexation. It is usually the discretionary actions of a budget that are debated, and by both cutting taxes and increasing expenditure it appears that both sides are being at least partly met. Second, it is well-established that governments can obfuscate cutbacks by letting inflation and wage increases erode the value of transfers instead of taking discretionary actions (Green-Pedersen 2002; Lindbom 2007; Pierson 1994). In addition to obfuscating the cutbacks, rapid growth might also function as a sweetener that makes cutbacks more acceptable. If a government wants to retrench the welfare state without public resistance, we would expect

them to do it during times of economic growth. Third, many countries have introduced medium-term ceilings for aggregate expenditure. When those ceilings are set low enough to be binding, they force governments to implement tax cuts instead of increased spending whenever economic growth has provided fiscal space.

The assumed causal direction in this essay is that economic growth affects the tax ratio and social spending, but the correlation illustrated in Figure 4.1 is of course equally compatible with an effect that goes from tax cuts to economic growth. To avoid the risk of reverse causation, I rely on an instrumental variable approach to identify the causal effect of growth. The GDP growth for each country is instrumented on a weighted average of the growth in countries with synchronized business cycles. While this strategy does not guarantee unbiased identification, I consider it an improvement to common approaches in comparative research, and I expect any remaining bias to be small compared to the estimated effects.

Traditional OLS regressions and my IV approach both show the same thing: during periods of fast growth, both taxes and public social spending has been reduced as a share of GDP. A permanent increase in the growth rate by one percentage point is expected to reduce the size of the public sector by about 0.5 per cent of GDP.

This essay contributes to several different literatures. First, by providing a new explanation of how welfare states have developed over the last decades, it contributes to the comparative welfare state research. Second, the essay provides a new argument in the debate about whether the size of the public sector reflects the preferences in the electorate, or if there is a bias in any direction (Downs 1960). Because this argument directly relates to institutions created to improve fiscal discipline, the essay also contributes to the public finance literature concerned with fiscal sustainability. Finally, the essay provides a reason why cross-national studies that find a correlation between tax cuts and growth cannot interpret it as a causal effect of taxes.

Indexation and the budget process

The mechanism proposed in this essay rests on two necessary conditions. The first is that public spending automatically decreases as a share of GDP when the economy grows. Because the tax ratio is less affected by economic growth, a growing economy creates a fiscal space which the government can use for tax cuts and spending hikes. Governments can of course create fiscal space by abolishing old programmes or decide on new taxes, but budgeting is still incremental in the sense that major reallocations are rare (Schick 1983, 2009). The second condition is that

governments use a large part of this fiscal space on tax cuts, even if the space was created by a reduction in spending as a share of GDP. Together, these two conditions cause the public sector to shrink as the economy grows. This section deals with one condition at a time, beginning with the reasons why the spending share decreases with economic growth.

Budgets are typically formulated on a nominal basis with discretionary decisions required to adjust expenditures to inflation or increased programme utilization (Marcel 2014). These decisions are made in relation to a baseline estimation, which can be equal to the previous year's expenditure in nominal terms or adjusted for factors like inflation, GDP or population growth. How these baselines are calculated is an informal process in most countries and usually not codified by budget rules (Schick 2009). During times of austerity, the baselines are often manipulated to facilitate fiscal adjustments (Bozeman and Straussman 2015). The calculation of baselines can also vary wildly between different levels of government or between regions in a country (Crain and Crain 1998). The end result is usually that *nominal* expenditures for a programme rise compared to the previous year, but not enough to offset the projected increases in prices and programme utilization (Schick 1986, 2007).

Unlike other expenditures, the indexation of social insurance entitlements usually follows relatively well-defined rules. Most entitlements are tied to prices or wages with either automatic indexation or institutionalized discretionary actions. Unemployment benefits usually have one component that depends on previous earnings and one component that is set in nominal terms or tied to prices or the minimum wage. Family benefits and social allowances are linked to inflation in about half of the OECD countries and not indexed at all in the other half. Price inflation is the most common anchor for the indexation of pensions, but weighted averages of prices and wages are also common, often in combination with stronger indexation for lower pensions. It should also be stated that, in practice, indexation rules have often been over-ruled in times of extreme growth and inflation (Whitehouse 2009).⁴⁹

Because taxes are to a large extent proportional to income, they tend to follow GDP much more closely than public spending. However, the wage increases associated with GDP growth can move people into a higher tax bracket if the thresholds are not tied to wages. This tendency has primarily been debated during times of high inflation and is usually referred to as

⁴⁹There are also other reasons why the spending share will fall over time, like annual reductions in the budget baselines to account for assumed productivity growth and time-limited programmes that will cease to exist unless discretionary actions are taken. However, because they are unrelated to GDP growth they are irrelevant for the case made in this essay.

‘bracket creeping’. If the tax system is very progressive, nominal wage increases can have an *increasing* effect on the tax ratio, compared to the *decreasing* effect on public spending as share of GDP.

I will now present three arguments for why we should expect automatic decreases in the spending share of GDP – which provide the government with a fiscal space for reforms – to cause tax cuts. The first reason is that the budget process makes it natural for governments to cut taxes. Most rich countries today use a top-down budgeting approach, where aggregate spending is not determined as the sum of spending requests from the ministries, but centrally decided as a choice between tax cuts and increased spending (or tax hikes and spending cuts). Moreover, inter-governmental discussions and public debate tend to be focused on the discretionary allocation of these resources. Some interest groups will demand lower taxes while other claimants lobby for reforms that increase expenditure. The government cannot reconcile these conflicting interests, but deciding to use the fiscal space on a mix of tax cuts and spending hikes will at least foster the appearance that both interests are being partly met (Schick 1986).

The second reason is that GDP growth provides governments with an opportunity to obfuscate welfare state retrenchment because they can cut back on spending without taking discretionary actions. It is widely accepted that governments use different forms of *blame avoidance* to avoid negative feedback from voters. Pierson (2001) argues that *obfuscation* is the most important of these strategies, and the specific tactic to freeze expenditures within a growing economy is something he refers to as *decrementalism* (Pierson 1994, p. 20). In support of his argument, it has been shown that non-indexed programmes have been more affected by cutbacks than indexed programmes (Green-Pedersen 2002; Lindbom 2007). For the same reasons we should also expect larger spending cuts during periods of fast growth.

Third, many countries have fiscal rules that can prevent governments from meeting unexpected GDP growth with increased spending. During the last decades, many countries have implemented a variety of fiscal rules to improve budget discipline, and different kinds of spending targets can now be found in most rich countries (see Ayuso i Casals [2012] for a survey of the EU member states). They function as a pre-determined ceiling for the government’s expenditure, often set a few years in advance, to ensure that governments cannot let temporary revenues cause a permanent increase in spending, and that soaring costs in one area of expenditure must be compensated for with cuts in other areas. Spending targets are usually set in nominal terms, which strengthens their macroeconomic stabilization effect. The point of spending targets is that they should be set

so low that unexpected increases in income cannot be used for increased expenditures. Thus, when the economy grows faster than expected and the spending target is binding, governments must choose between tax cuts and improvements in the budget balance.

Data and design

This section is divided into three parts. In the first part, I provide an extensive argumentation for why I operationalize welfare effort with tax revenues and public social spending, both measured as shares of GDP. How welfare effort should be measured is a heated subject in the welfare state research, but I believe that most participants in this debate have overlooked the advantages of using tax ratios. In the second part, I describe the instrumental variable approach I use to manage the problem of reverse causality. By using the growth rate in countries with synchronized business cycles, I get an instrument that is strongly correlated with the endogenous growth variable, and which should only be marginally influenced by the country's tax policy. The third part presents the model specifications and the remaining data.

Measuring welfare effort

Esping-Andersen (1990) famously stated that no one 'struggled for spending per se' in his argument for why social spending is an insufficient outcome if we want to understand the expansion of the welfare state. Since then, entire volumes have been written about this dependent variable problem (Clasen and Siegel 2007). Because most authors have been critical about the use of expenditure-based measures – and many of their arguments apply to tax ratios as well – I will briefly comment on their criticism and how it applies to my study. I have divided this discussion into four arguments. In short, I argue that i) measures based on spending (or revenue) data are suited for the causal mechanisms described in the previous section, that ii) previous research has exaggerated the 'denominator problem' of GDP ratios, that iii) while social expenditure is more directly related to money spent on welfare services, other forms of public spending also have a decommodifying effect, and that iv) there are methodological advantages of using the tax ratio as the dependent variable.

My first argument is that aggregate spending (or revenues) is a logical choice for dependent variable given the mechanisms I have described. With his quote, Esping Andersen did not claim that social spending is a bad measure of welfare effort per se. His argument was that we must begin with demands that were promoted by the actors which were important for the welfare state development if we want to test the causal theories that

involve these actors. This argument is sometimes restated to suggest that the choice of dependent variable must follow from the causal argument (Green-Pedersen 2007; Jensen 2011) and that expenditure-based measures can be perfectly justified from a different theoretical perspective (Green-Pedersen 2004). The mechanisms described in this essay are not limited to specific actors or welfare programmes, nor can they be identified as reforms or discretionary actions. On the contrary, they focus on retrenchment that happens because actions are *not* taken to counteract the eroding effect of growth. They should therefore be analysed on the aggregate level and not on single rules or changes in nominal amounts.

My second argument is that the denominator problem of spending shares (or tax ratios) is overstated. Many have argued that the spending share of GDP is problematic as a measure of welfare effort, because it is impossible to separate the amount of money spent by the government and the size of GDP and, similarly, that the ratio can easily divert from nominal or real spending when the economy grows (Clayton and Pontusson 1998; Kühner 2007; Olaskoaga et al. 2013; Scruggs 2006, 2007). There are several problems with this argument. First, it is not the denominator which makes it difficult to separate welfare effort from economic circumstances, but the fact that we are comparing different contexts. The reason for using a denominator is usually that it facilitates comparisons, not because it makes them misleading, and the problem is therefore not isolated to measures that use a denominator. For example, life expectancy is five years longer in Japan than in the United States. Does equal welfare effort still imply equal retirement age? Second, wages tend to follow GDP closely. Thus, constant replacement rates in social insurance schemes as well as maintaining the number of public employees, require that those parts of public spending grow at the same pace as GDP. Using consumer prices instead of GDP as the deflator would therefore lead to misleading conclusions. For example, real social expenditure grew by about 30 per cent under both Reagan and Thatcher, but I doubt that anyone would describe their policies as an expansion of the welfare state.⁵⁰ Third, even if productivity gains in the public sector mean that GDP growth will exaggerate the cost development for public spending somewhat, I argue that it is still a good comparison for welfare effort. An effort must always be understood in relation to the resources available. Just as it requires a larger effort to put food on the

⁵⁰Ireland during the period 1980–2000 has become the favourite illustration of how GDP growth distorts spending ratios (Olaskoaga et al. 2013; Scruggs 2006, 2007). However, it was not the GDP growth that caused this anomaly – where spending ratios fell at the same time as replacement rates in social insurance increased – but a combination of an unprecedented reduction in the wage share, falling dependency ratios and reduced means-tested transfers (Timonen [2003] and my own calculations).

table if you are poor, new medical discoveries do not mean we can close hospitals and claim that our welfare effort is the same. It therefore makes sense to measure welfare effort as *the share* of our resources that we spend on welfare programmes.

My third argument is that virtually all public funding strengthens the decommodifying effect of the welfare state. Esping-Andersen stated that social rights should be measured as ‘the degree to which they permit people to make their living standards independent of pure market forces’ (Esping-Andersen 1990). Hence, social rights have usually been operationalized as the comprehensiveness and generosity of social insurance. The higher the replacement rates in the insurances, the less your consumption will fall if you become sick or unemployed. Measuring welfare effort with the tax ratio is sometimes criticized because government revenues can be used to pay for things usually *not* considered as social rights or welfare services, like road networks, public transportation and recreational facilities. While they might not constitute the core of the welfare state, they might still contribute to its decommodifying effect. When these goods are paid for by tax revenues instead of road tolls, entry fees and bus prices, the consumption of these goods is made independent of one’s market income, much like social insurance guarantees a certain living standard for those without a wage. However, if tax revenues are used to pay for non-excludable goods like prisons or the military, there would be no such decommodifying effect. And if the public resources are primarily spent on privileged groups, heavy taxation could – at least in theory – increase people’s dependency on their market income. But while many have claimed that wealthy citizens extract at least as much as the poor in terms of many welfare services (Le Grand 1982), tax financing still means that ‘services are redistributive in an egalitarian direction, albeit less so than are cash transfers’ (Esping-Andersen and Myles 2009). Besides, as shown in Figure 4.2, countries with high shares of social spending also have high taxes. This strong *spatial* correlation suggests that it might be more important to look at the measures’ cyclical properties – and, in particular, how it is affected by automatic stabilizers – which makes room for my last argument.

Fourth, it is my opinion that the most serious problem with spending data is that government expenditure will always be affected by changes in the dependency of welfare services.⁵¹ We cannot talk about a stable welfare effort if budgets and policies do not adjust to face increasing risks or other forms of changing social circumstances. Economic downturns are the most obvious example, but the demographic structure, changes in sickness

⁵¹This does not mean that measures based on rules and rights are free of this problem (Hacker 2004).

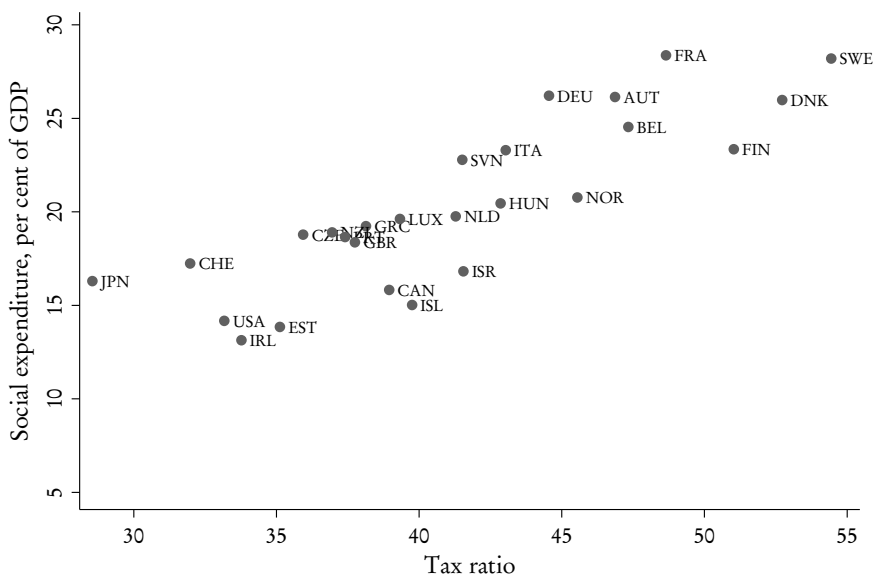


Figure 4.2: Taxes and social spending year 2000

leave and flows of refugees also give rise to cyclical patterns in expenditure that are unrelated to the rights of citizens. However, the latter should not cause any problems for the empirical analysis in this essay, because they can only affect the results to the extent that these factors are correlated with growth. And, given my instrumental variable approach, only to the extent that they are correlated with economic growth in countries with synchronized business cycles. I will therefore focus my discussion on expenditures related to the business cycle. For social spending, it is obvious that fast growth is correlated with lower needs for unemployment benefits and social assistance. But while automatic stabilizers would bias the effect of growth on expenditure ‘upwards’ (away from zero), the tax ratio is instead biased ‘downwards’ (towards zero) and the bias is much smaller. During economic upturns, less people are unemployed, which means that the costs for unemployment benefits and other transfers would decrease during times of economic growth, even if the generosity of the welfare systems remained unchanged. On the other hand, the tax ratio – in absence of discretionary actions – tends to increase during times of rapid growth. Both profits and sales usually increase faster than GDP, and thus also the revenues from corporate and sales taxes. This is the primary reason for why I trust the results for the tax ratio more than those for social spending.

I will conclude with a few additional arguments for why taxation can be a better measure of the long-term commitment to welfare spending

than expenditure-based measures. First, the tax ratio is less susceptible to all kinds of dependency-driven effects – not only the automatic stabilizers discussed above – because deviations in expenditure do not require tax changes, provided that the deviations are temporary. Annually balancing the budget through tax changes would cause the distorting effects of taxes to be larger than necessary (Barro 1979). Second, while *unexpected* growth might lower the spending share of GDP because budgets were based on expected growth (Iversen and Cusack 2000; Roubini and Sachs 1989), noticeable changes in the tax ratio would not happen without discretionary actions. Therefore, they are also less likely to be reverted as soon as the government realizes that the forecast was wrong. Third, I acknowledge that technical differences like taxation of transfers will artificially inflate the tax ratio in some countries compared to others (Adema and Ladaïque 2009). However, such differences should be stable over time and can therefore be handled by including country fixed effects in the models.

The instrument

The main methodological challenge in this essay is the obvious risk of reverse causation. A negative correlation between growth and the size of the public sector could also result from an effect of taxes or social spending on growth. To minimize this risk, I rely on an instrumental variable approach to create a plausibly exogenous variation in the growth rate. I exploit the fact that some countries have relatively synchronized business cycles, usually because they have a similar business structure or because they are major trade partners. By using a weighted average of the growth in a set of countries with similar business cycles, I get an instrument that is strongly correlated with a country's growth. While the instrument cannot eliminate the risk of reverse causation, I expect it to remove most of the bias that this problem might cause. In this section I describe the method I use, discuss various threats to the instrument's validity and compare my instrument with other approaches in the literature.

I use recursive all-subsets regression to identify countries with synchronized business cycles.⁵² For each combination of country i and year t , I regress the nation's annual growth between 1960 (or the oldest observation available) and $t - 1$ on the growth in the world's N largest economies for which I have GDP data since 1960.⁵³ All possible subsets of the N largest economies are considered before I choose the set of countries which

⁵²Recursive means that I estimate a separate model for each year where only observations before that year are included. The all-subsets regression is performed using the leaps-and-bounds algorithm developed by Furnival and Wilson (1974) and the best subset is defined as the subset of variables which minimizes Mallows's C .

⁵³ N is set to the number of observations minus five, but is capped at a maximum of 20 countries.

minimizes the information criteria Mallows's C . I then use the regression coefficients from this regression and predict the growth rate in year t based on the growth rate this year in the regressor countries. I also record the average change in the tax ratio and social spending for these countries. These variables are later used to control for policy diffusion.

Expressed in less technical terms, the model which provides the best fit for the 1960–1989 period is extrapolated to make a prediction for 1990. I then identify which subset of countries best fits the 1960–1990 data, make a prediction for 1991, and so on. When I have analysed the 1960–2014 period to make a prediction for 2015, I repeat the same procedure for every other country. These predictions then constitute the instrument I will use in the regression analyses.

Fortunately, the instrument turns out to be strongly correlated with the endogenous regressor (Pearson's r is close to 0.7), but there are two problems with its exogeneity. First, the exclusion restriction requires that the instrument only affects the dependent variable through the endogenous regressor. If this restriction is violated, the estimated effect will suffer from bias. There are reasons to suspect minor violations of this restriction. Just as national economies are interdependent, policy can also spread from one country to another (Simmons and Elkins 2004). If the fast growth in neighbouring countries is caused by tax cuts or reduced social spending, it might be their policy decisions which affect policy in the investigated country, and not the rapid growth. To control for such policy diffusion, I use the average change in the tax ratio and social spending, calculated across the countries which were used to instrument the growth rate. In other words, the unbiasedness of this estimator rests on the assumption that – controlling for policy changes – growth in neighbouring countries only affects changes in the tax ratio through its effect on the growth rate.

The second problem is that we cannot rule out a reverse effect of tax policy on the instrument, because the instrument is not entirely exogenous to the endogenous regressor. A share of the covariation between the regressor and its instrument will be caused by economic growth in the investigated country affecting economic activity in the countries used for the instrument. If policy affects growth, and the growth rate in turn affects growth in the countries used to create the instrument, the estimated effects of growth on policy would suffer from bias caused by reverse causation. Fortunately, this reverse effect will be small compared to how much the other countries affect growth in the investigated country, because the aggregated size of the former economies tends to be much larger. On average, the total GDP of the countries used to create the instrument is six times larger than that of the investigated country. Besides, a large part of the correlation in growth rates originates from common shocks

that are exogenous to the policy process in both the investigated country and the countries used for the instrument. If most of the covariation between the endogenous growth variable and its instrument consists of common shocks and the influence from other countries, it is safe to assume that the bias caused by a causal effect of the endogenous regressor on the instrument will be small compared to the size of the effect. I investigate this further in the robustness section.

The most common way of instrumenting growth is to use lagged values of growth (Barro 2000) or growth predictors (Acemoglu et al. 2008). However, the exclusion restriction would be violated as soon as tax changes are anticipated and growth reacts to expectations of future taxes. Apparently, this would also be a weaker instrument than the one I use. Others have used the quality of institutions (Dollar and Kraay 2002) or weather variations (Miguel et al. 2004), but institutions cannot explain short-term variation in growth, and weather is only relevant for agricultural economies.

An approach similar to mine has been used by Acemoglu et al. (2008) and Brueckner et al. (2015), but we differ in how the weights are assigned. They use trade shares as weights, but bilateral trade is only one reason behind business cycle synchronicity. Other reasons include similarities in industrial structure and factor intensity, the degree of industrialization, currency unions, financial integration, similarities in export and import baskets and the openness of the economy (Baxter and Kouparitsas 2005; De Haan et al. 2008). By restricting countries with synchronized business cycles to countries with much bilateral trade, the instrument becomes much weaker.⁵⁴ This restriction could be motivated if bilateral trade were less susceptible to exogeneity problems than business cycle synchronicity caused by other factors, but I find that unlikely. On the contrary, I would argue that both the problems described above are of larger concern for countries that trade a lot than, for example, countries where business cycles are synchronized because their import and export baskets are similar. Trade partners are likely candidates for policy diffusion and their business cycles will be correlated because of trade, not because of common exogenous shocks.

To sum up, I recognize that the exogeneity of my instrument is not perfect, but compared with OLS it should drastically reduce any bias caused by reverse causation. The strength of the instrument also means that minor violations of the exclusion restriction will only marginally affect the estimated effects (Murray 2006). I will now proceed to the model specifications.

⁵⁴When the variables have been recalculated to growth instead of levels, the correlation between the instrument and the regressor is 0.16 in Acemoglu et al. (2008).

Empirical specification

The OLS regressions follow Equation 4.1, where the tax ratio ($T_{i,t}$) is regressed on its lagged value ($T_{i,t-1}$), the growth rate (g_i^t) and a vector of control variables ($\chi_{i,t}$). These are country dummies, the annual inflation, the output gap, the cyclically adjusted budget balance, the unemployment rate, the size of the working age population, a dummy for election years, a dummy for right-wing governments and a linear time trend. The choice of a time trend over year dummies is discussed in the results section. When the tax ratio is substituted by different measures of social spending, I only substitute the dependent variable and its lagged value.

$$T_{i,t} = a_1 + \beta_1 \times T_{i,t-1} + \beta_2 \times g_{i,t} + \delta_1 \times \chi_{i,t} + e_{i,t} \quad (4.1)$$

In the IV approach, the growth rate is replaced by the predicted growth rate ($\hat{g}_{i,t}$), which is estimated in the first stage equation. To control for policy diffusion effects, I also include the average change in the tax ratio in the countries which were used to create the predicted growth rate ($\Delta T_{i,t}^*$). The equation for the predicted growth rate includes the same variables as the second-stage equation, in addition to an instrument based on growth rates in countries with synchronized business cycles ($g_{i,t}^*$). The two equations are estimated using 2SLS.

$$\begin{aligned} T_{i,t} &= a_3 + \beta_6 \times T_{i,t-1} + \beta_7 \times \Delta T_{i,t}^* + \beta_8 \times \hat{g}_{i,t} + \delta_3 \times \chi_{i,t} + u_{i,t} \\ g_{i,t} &= a_2 + \beta_3 \times T_{i,t-1} + \beta_4 \times \Delta T_{i,t}^* + \beta_5 \times g_{i,t}^* + \delta_2 \times \chi_{i,t} + e_{i,t} \end{aligned} \quad (4.2)$$

In this essay, the tax ratio is defined as the sum of taxes on production and imports (indirect taxes), current taxes on incomes and wealth (direct taxes), social security contributions, and other current receipts, expressed as a percentage of GDP. The measure of public social spending is a gross measure; i.e., it has not been adjusted for cross-national differences in the taxation of transfers. However, because such adjustments are quite stable over time, it should not affect the results in this essay.

Most of the data come from the OECD. The tax ratio, growth, unemployment rate and inflation are gathered from OECD Economic Outlook 98 (December 2015). The share of youths and elderly people is collected from the publication Country Statistical Profiles, data on public social expenditure are from the Social Expenditure Database (SOCX) and both election years and the ideological position of the government come from the Database of Political Institutions. To extend the growth series back to 1960, I add data from Economic Outlook 87 (May 2010). I also use this version of Economic Outlook together with data from the World Bank to

replace missing values in the inflation variable. Imputation and linking procedures are described in the supplementary information.

Results

The regression results are presented in three tables. Table 4.1 presents different specifications with the tax ratio as the dependent variable. One of these specifications is then used when the tax ratio is substituted with different kinds of public social expenditure. These results are presented in Table 4.2. The section ends with an examination of how robust the results are to alternative specifications. Table 4.3 presents the results from the robustness checks.

The first column of Table 4.1 shows the results from when the tax ratio is regressed on the GDP growth and the main set of control variables. An increase in GDP growth by one percentage point is estimated to immediately reduce the tax ratio by 0.08 percentage points. To put the size of this coefficient into perspective, for a country with an average sized public sector (40 per cent of GDP), where 50 per cent of public spending is tied to economic growth, each additional percentage point of growth will reduce public expenditure by 0.2 per cent of GDP. If this fiscal space is equally divided between taxes and expenditure, the tax ratio would fall by 0.1 per cent of GDP, which can be compared to the coefficient of 0.08. We can also calculate the long-term change in the tax ratio, which would follow from a permanent change in the growth speed. In this case, a permanent increase in the growth speed by one percentage point would reduce the tax ratio by 0.3 per cent of GDP ($-0.08/(1 - 0.73)$).

In this specification, it is assumed that the effect of all independent variables decay geometrically at the same rate. This assumption would be false if governments compensate their previous mistakes as soon as they realized that the tax ratio had fallen unintentionally. To allow for a temporary effect of growth, the model presented in the second column includes two lags of the GDP growth. However, both the lags have coefficients close to zero, indicating that changes in the tax ratio caused by growth are just as persistent as other factors that have affected the tax ratio. The growth lags are therefore excluded from subsequent models.

If changes in the tax ratio affect the GDP growth, the OLS estimates in the first column are biased. To address this problem of reverse causation, columns 3–4 present the results from models where the growth is instrumented on the growth in other countries. As shown by the difference between column 1 and column 3, the IV approach actually leads to larger effects. The immediate effect of a change in growth by one percentage point is now a reduction in the tax ratio by 0.13 per cent of GDP. The first-stage F statistic is 14.24, which confirms the strength of the instru-

Table 4.1: The effect of growth on the tax ratio

	(1)	(2)	(3)	(4)	(5)
Tax ratio (t-1)	0.73*** (0.05)	0.73*** (0.05)	0.74*** (0.05)	0.79*** (0.06)	0.77*** (0.05)
Growth	-0.08*** (0.03)	-0.08*** (0.03)	-0.13* (0.07)	-0.37** (0.16)	-0.12*** (0.04)
Year	-0.03** (0.01)	-0.03* (0.01)	-0.03** (0.01)		
Inflation	-0.04 (0.03)	-0.04 (0.03)	-0.02 (0.07)	-0.14 (0.10)	-0.06* (0.03)
Output gap	0.02 (0.03)	0.01 (0.04)	0.07 (0.07)	0.20* (0.12)	0.04 (0.04)
Adj. net lending	0.07** (0.03)	0.06** (0.03)	0.08*** (0.02)	0.09*** (0.03)	0.08** (0.03)
Unemployment	0.07** (0.03)	0.07** (0.03)	0.10** (0.04)	0.08 (0.06)	0.01 (0.03)
Working age population	-0.18* (0.09)	-0.18* (0.09)	-0.25*** (0.09)	-0.13 (0.13)	-0.13* (0.07)
Election	-0.08 (0.07)	-0.07 (0.07)	-0.06 (0.07)	-0.02 (0.09)	-0.09 (0.07)
Right-wing	-0.13 (0.19)	-0.14 (0.19)	-0.19 (0.18)	-0.20 (0.15)	-0.15 (0.17)
Growth (t-1)		0.01 (0.03)			
Growth (t-2)		0.01 (0.02)			
Δ Tax, instr. countries			0.30*** (0.08)	-0.14 (0.15)	
Constant	81.44*** (28.27)	77.39** (30.74)	88.17*** (25.38)	19.34** (9.68)	21.51*** (5.55)
Estimator	OLS	OLS	2SLS	2SLS	OLS
Country dummies	Yes	Yes	Yes	Yes	Yes
Year dummies	No	No	No	Yes	Yes
Observations	379	377	352	352	379
First stage F statistic	-	-	14.24	3.80	-

Dependent variable: tax revenues as a percentage of GDP. Country-clustered standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

ment (Sovey and Green 2011). However, instrumenting growth on the growth in other countries reduces precision, and the coefficient is only significant at the 90 per cent level. The large standard errors mean that the difference between the OLS and IV estimates can be coincidental, but if the OLS estimates are actually biased towards zero, one explanation could be that tax cuts are associated with decreased spending and that the fiscal multipliers for government expenditure are larger than those for taxes (IMF 2012). Another possibility is that ‘imported’ growth is more difficult to predict and that the policy response differs between predicted and unpredicted growth.

The model shown in the fourth column includes year dummies. This is usually standard procedure, but the dummy variables cause problems when they are included in a model where the growth is instrumented on the growth in other countries. The reason for this is that the instrument becomes much weaker when the year dummies effectively control for world growth, which in turn leads to inflated standard errors and a smaller F statistic. The size of the estimated coefficient (-0.37) is much larger than in previous models and also larger than what I would expect from theory. While I find it reassuring that year dummies do not reduce the effect, I interpret this estimate as a result of bad precision.

To improve the precision, the fifth column presents the results from when year dummies are added to the model presented in the first column. The estimated coefficient is now larger than when only a linear time trend was included, confirming that the omitted variable bias caused by excluding the time dummies at least do not make the effects appear larger than they actually are.

Taken together, these results support the argument that rapid economic growth reduces the size of the public sector. However, it is not possible to obtain precise estimates while both including year dummies and using the IV approach, which makes it difficult to know exactly how large this effect is. Because I regard reverse causation as my main identification problem, I will use the IV model without year dummies as I proceed to analyse the change in social spending.

There are methodological arguments for why I believe that the tax ratio is better than spending data for measuring welfare effort. These are discussed in detail in the next section. However, it is still interesting to see which kinds of expenditure are most affected by growth, and how the effect differs compared to when the tax ratio is used as the dependent variable. All regressions on spending data follow the same specification as the model presented in the third column in Table 4.1.

The results for these regressions are presented in Table 4.2. The first column shows how growth affects total public social expenditure, as a share of GDP. As expected, the estimated effect is much larger than for taxes. The second and third column have the effect decomposed into cash benefits and benefits in kind, which indicates that the aggregate effect is driven by cash benefits. Column 4–6 show how the effects differ between expenditure related to unemployment (unemployment benefits and active labour market policy), health (spending related to health and incapacity) and other sources (family related, pensions, housing benefits, etc.). The three areas of expenditure show similar effects, but spending related to unemployment amounts to a much smaller share of GDP than the other areas of expenditure and is therefore – relatively speaking – more strongly

Table 4.2: The effect of growth on social expenditure

	(1)	(2)	(3)	(4)	(5)	(6)
Growth	-0.37*** (0.07)	-0.30*** (0.05)	-0.05** (0.02)	-0.16*** (0.05)	-0.09** (0.04)	-0.16*** (0.05)
Lagged dependent	0.89*** (0.04)	0.94*** (0.04)	0.92*** (0.04)	0.96*** (0.08)	0.84*** (0.05)	0.87*** (0.05)
Δ DV, instr. countries	-0.04 (0.09)	-0.13 (0.12)	0.31*** (0.11)	-0.40* (0.22)	0.05 (0.11)	-0.02 (0.09)
Inflation	-0.15*** (0.03)	-0.11*** (0.03)	0.01 (0.04)	-0.04* (0.03)	-0.06** (0.02)	-0.08*** (0.03)
Output gap	0.03 (0.07)	0.07 (0.06)	-0.02 (0.02)	0.05 (0.04)	0.00 (0.04)	0.00 (0.04)
Adj. net lending	-0.05 (0.03)	-0.03 (0.02)	-0.02 (0.01)	-0.01 (0.01)	-0.02 (0.01)	-0.04*** (0.01)
Unemployment	-0.08* (0.05)	-0.01 (0.03)	-0.07*** (0.02)	0.01 (0.02)	-0.06** (0.03)	-0.05* (0.03)
Working age population	-0.05 (0.08)	-0.01 (0.05)	-0.09 (0.06)	0.05 (0.04)	-0.01 (0.04)	0.05 (0.06)
Election	0.17*** (0.06)	0.10* (0.06)	0.04 (0.04)	0.06 (0.04)	0.03 (0.03)	0.09** (0.04)
Right-wing	0.00 (0.07)	-0.05 (0.05)	0.11 (0.08)	-0.00 (0.03)	0.07 (0.06)	-0.02 (0.05)
Year	-0.03*** (0.01)	-0.03*** (0.01)	0.00 (0.01)	-0.02*** (0.01)	-0.01 (0.01)	-0.01 (0.01)
Constant	66.79*** (19.43)	54.04*** (13.86)	3.80 (25.00)	39.75*** (11.03)	13.95 (13.19)	22.08* (12.92)
Dep. variable	Total	Cash	In kind	Unempl.	Health	Other
Estimator	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Country dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year dummies	No	No	No	No	No	No
Observations	350	350	350	302	303	292
First stage F statistic	9.44	10.17	11.31	4.42	5.72	6.94

Dependent variable: Public social expenditure as a percentage of GDP. From left to right: total expenditure, cash benefits, benefits in kind, expenditure related to unemployment, expenditure related to health and expenditure related to other areas. Country-clustered standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

affected by growth. The large effects on cash benefits and expenditure related to unemployment, as well as the large effects on social spending compared to taxes, probably reflect the fact that automatic stabilizers reduce spending when the economy grows. It is also worth noting that I only find policy diffusion on benefits in kind. This would be expected if the variation in cash benefits primarily consisted of variation in the number of beneficiaries, and not policy decisions. The smaller sample used for the last three regressions resulted in a weaker instrument, as illustrated by the small F statistics. The results should therefore be interpreted carefully.

Robustness

In this section I address two specific concerns. The first concern is whether the endogenous growth variable affects the instrument. If tax policy (or social spending) affects growth, and the growth rate in turn affects growth in the countries used to create the instrument, the estimated effects of growth on tax policy would suffer from bias caused by reverse causation. Because most economies are much smaller than the sum of the economies used to create the instrument, this effect is probably small compared to the effect that the instrument exercises over the endogenous variable. On average, the total GDP of the countries used to create the instrument is six times larger than that of the country for the endogenous variable. Besides, a large part of the correlation between the countries consists of common shocks which are exogenous to implemented policy.

However, for the largest countries in the sample, the size of their economy often equals the size of the economies used to create the instrument. In these cases, the bias caused by reverse causation (tax policy affects growth which in turn affects the growth in the countries used for the instrument) could be substantial. To analyse whether this bias has had any major impact on the results, I create a variable which is defined as a country's GDP over the GDP in the countries used to create the instrument. I then allow for an interaction-effect between this measure of relative GDP and the growth rate. If there is a bias caused by reverse causation, and this bias is larger for large economies, the interaction effect will have the same sign as the bias. The effect of the growth variable can then be interpreted as the estimated effect when the bias is small (if the bias is linear to relative GDP, it can be interpreted as the effect in the absence of bias).

The results from these regressions are presented in the first two columns of Table 4.3. The first column uses the tax ratio as the dependent variable, while the second column uses public social expenditure as a share of GDP. Fortunately, the coefficient for the interaction term is close to zero. In fact, it is slightly positive, indicating that the bias from reverse causation might make the effects appear smaller than they actually are. Similarly, if I, instead of including the interaction-term, excluded the largest economies – measured as GDP relative to GDP in the countries used for the instrument – the estimated effect would become larger. The more observations I removed, the larger the effect would become. In other words, both these methods show that the estimated effects are larger for countries where the endogenous growth variable can be expected to exert little influence over its instrument. Similar to when we went from OLS regressions to the IV approach in Table 4.1, there is nothing to suggest that reverse causation induces a *negative* bias in the estimates of how growth

Table 4.3: Robustness checks

	(1)	(2)	(3)	(4)
Growth	-0.14*	-0.32***	-0.12***	-0.22***
	(0.08)	(0.10)	(0.03)	(0.05)
Tax ratio (t-1)	0.74***		0.73***	
	(0.04)		(0.05)	
Social spending (t-1)		0.88***		0.86***
		(0.05)		(0.03)
Δ DV, instr. countries	0.29***	0.16	0.27***	0.33***
	(0.08)	(0.20)	(0.07)	(0.08)
Growth $\times$ Rel. GDP	0.09	0.18		
	(0.36)	(0.15)		
Relative GDP	-0.12	-0.48		
	(0.79)	(0.35)		
Year	-0.03**	-0.02**	-0.03**	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Inflation	-0.02	-0.13***	0.01	-0.07***
	(0.07)	(0.04)	(0.05)	(0.02)
Output gap	0.07	0.00	0.04	-0.03
	(0.06)	(0.07)	(0.04)	(0.02)
Adj. net lending	0.08***	-0.06*	0.09***	-0.06***
	(0.02)	(0.03)	(0.02)	(0.02)
Unemployment	0.10**	-0.08**	0.12***	-0.06**
	(0.04)	(0.04)	(0.03)	(0.02)
Working age population	-0.25***	-0.05	-0.28***	-0.14**
	(0.09)	(0.07)	(0.08)	(0.05)
Election	-0.06	0.16**	-0.02	0.14***
	(0.08)	(0.06)	(0.07)	(0.05)
Right-wing	-0.19	-0.00	-0.19	0.01
	(0.18)	(0.06)	(0.19)	(0.07)
Constant	87.75***	53.06***	94.94***	38.71*
	(24.78)	(19.98)	(29.69)	(20.53)
Dep. variable	TR	SS	TR	SS
Estimator	2SLS	2SLS	2SLS	2SLS
Country dummies	Yes	Yes	Yes	Yes
Year dummies	No	No	No	No
Observations	352	352	341	351

Dependent variable: Tax revenues (column 1 and 3) and public social expenditure (column 2 and 4) as per cent of GDP. Country-clustered standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

affects the tax ratio.

The second concern is the Nickell (1981) bias which is introduced when a model combines fixed effects with a lag of the dependent variable. Most regressions in this essay average 17–18 observations per country, which means that OLS should perform relatively well (Katz and Beck 2009). I have therefore used OLS (and 2SLS) in my main specifications. To examine whether the results are robust to alternative specifications, the last two columns of Table 4.3 present the results from the popular Arellano-Bond estimator. The estimated coefficients are similar to before.

Additionally, when the lags of all the other variables are included as instruments, the precision of the growth effect increases and the effects in both column 3 and 4 are now statistically significant at the 99 per cent level.

We have now looked at a wide range of regression models, and it appears that the results are quite robust to different specifications. In short, economic growth reduces the size of the public sector, regardless of whether it is measured as tax revenues or public social spending.

Conclusion

The need for improved public finances is larger now than ever before. Different kinds of deindexation have been proposed as a ‘less painful way of generating fiscal space’ (Marcel 2014). This argument is probably correct. Obfuscation has strong support in the ‘new politics’ literature (Pierson 1996; Weaver 1986) and in the first essay in this thesis I find that voters only punish governments for fiscal consolidations when the growth rate is not fast enough to hide the spending cuts.

However, institutionalizing the obfuscation of spending cuts can be questioned from a democratic point of view. Not only does it hurt the input legitimacy, but this essay provides evidence that it also affects political outcomes in a direction unrelated to the actions and wishes of the people. Here I provide a new argument to an old discussion about how the size of the public sector relates to the preferences in the electorate (Downs 1960).

Institutions that strengthen fiscal discipline through constraints on the policy process are typically defended on the basis that they move policy closer to the electorate’s actual preferences. However, that argument is only true when fiscal discipline is achieved through actions on both the spending and the revenue side. Institutions that ‘bias’ the size of the public sector can also be criticised for their distorting effects on policy outcomes. To solve this problem, one should consider whether the effects outlined in this essay can be counter-acted by medium term targets for the tax ratio or other kinds of fiscal institutions. Another possibility is to increase fiscal transparency with regard to how expenditures are deflated through wage increases and inflation. This would be preferable in terms of input legitimacy, but making cutbacks more transparent could also reduce the electoral incentives for governments to consolidate public finances. This, in turn, could entail worse outcomes compared to medium term targets for the tax ratio.

In addition to these dilemmas between input and output legitimacy, this essay also contributes to two other areas of political science. First,

many of the explanations for the *expansion* of the welfare state have proven to be less relevant if we want to understand how expanded welfare states develop. By showing that differences in economic growth to a large extent can account for the cross-national trends in how the size of the public sector has developed, this essay also makes an important contribution to the welfare state research. To expand on this insight, future research should attempt to calculate how responsive expenditures are to growth and inflation, and how this responsiveness differs between countries.

Second, the causes and consequences of economic growth lie at the heart of many debates in political science. In some specific contexts, it has been possible to create valid instruments for economic growth. For example, Miguel et al. (2004) present a natural experiment where rainfall is used as an instrument for economic growth in African countries. However, it is difficult to find similar instruments for advanced economies. In this essay I have suggested a ‘middle-ground’ between the naive OLS regressions and natural experiments. For any relatively open economy, we can construct a strong instrument by regressing the economic growth on the growth rate in other countries. By controlling for the change in the tax ratio in the countries used to create the instrument, and ensuring that the results are not driven by large economies that are likely to influence the growth rate in other countries, the instrument allows for relatively strong causal claims. There are many fields of research which could benefit from the use of this or similar instruments.

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Data appendix

Table A.1: Variables in Essay I and first part of Essay II

	Obs	Mean	St. dev.	Min	Max
Average growth ¹	341	2.22	1.82	-6.71	8.54
Average inflation ²	341	3.12	3.57	-0.68	36.95
Coalition ³	341	0.83	0.37	0.00	1.00
Fiscal transparency ⁴	289	0.00	0.98	-1.77	3.50
Int. rate dif. ⁵	331	2.71	2.29	-1.96	14.36
Majority government ⁶	337	0.76	0.41	0.00	1.00
Δ Net migration ⁷	341	0.01	0.31	-2.60	2.82
Δ Non-adjusted budget balance ⁸	341	-0.06	3.60	-14.55	9.58
Previous vote share ⁹	341	21.97	14.24	0.00	51.60
Δ Underlying primary balance ¹⁰	341	-0.02	2.64	-11.51	16.07
Prime minister's party ¹¹	341	0.47	0.50	0.00	1.00
Re-elected ¹²	341	0.57	0.47	0.00	1.00
Right-wing party ¹³	341	0.62	0.49	0.00	1.00
Δ Str. budget balance ¹⁴	341	-0.03	2.85	-12.08	15.12
Δ Unemployment rate ¹⁵	341	0.25	2.77	-6.96	14.82
Δ Vote share ¹⁶	341	-2.18	6.05	-30.74	23.60

¹ Average GDP growth (per cent). Source: OECD.

² The arithmetic mean of the annual inflation during the election period. Inflation data from World Development Indicators.

³ Measures if there are more parties in the cabinet than one (1 or 0 before aggregated to election periods). Source: Parlgov.

⁴ The standard score of the index of fiscal transparency developed by Alt and Lassen (2006a).

⁵ Average long-term interest rate on government bonds minus the same rate for Germany (percentage points). Source: OECD.

⁶ Measures if the government parties hold a majority of the seats in parliament (1 or 0 before aggregated to election periods). Source: Parlgov.

⁷ The difference between the number of immigrants and the number of emigrants, as a share of the total population. Both migration and population data from World Development Indicators.

⁸ Accumulated change in net lending, general government (per cent of GDP). Source: OECD.

⁹ The election result during the previous election. Source: Parlgov.

¹⁰ Government net lending excluding automatic stabilizers, interest payments and one off events. Source: OECD.

¹¹ Measures if the prime minister of the cabinet belongs to the party (1 or 0 before aggregated to election periods). Source: Parlgov.

¹² Government party in the first cabinet following the election (1 or 0). Source: Parlgov.

¹³ Measures if the right-wing index is above 5 (1 or 0). Source: Parlgov.

¹⁴ Accumulated change in the cyclically adjusted net lending, general government (per cent of potential GDP). Source: OECD.

¹⁵ Accumulated change in the unemployment rate (percent of the labor force). Source: OECD.

¹⁶ Change in vote share since last election (percentage points). Source: Parlgov.

Table A.2: Variables in the survey experiment in Essay II

	Obs	Mean	St. dev.	Min	Max
Accumulated ¹	1007	0.35	0.48	0	1
Age ²	1007	4.15	2.24	1	12
Banks ³	1007	0.35	0.48	0	1
Children ⁴	1007	1.62	1.03	1	5
Democrat ⁵	1007	0.44	0.50	0	1
Education ⁶	1007	3.55	0.90	1	6
Gov. confidence ⁷	1007	2.43	0.99	1	5
Income ⁸	1007	3.65	1.42	1	7
Male ⁹	1007	0.58	0.49	0	1
Republican ¹⁰	1007	0.17	0.38	0	1
Responsibility ¹¹	253	0.10	0.30	0	1
Right wing ¹²	1007	4.57	2.45	1	10
Support for fiscal consolidation ¹³	1007	3.04	1.16	1	5

¹ One of the treatment conditions.

² Age of the respondent, ranging from 1 = 19 or younger, via five year intervals, to 12 = 70 or older.

³ One of the treatment conditions.

⁴ The number of children (from 0 = none to 5 = four or more).

⁵ 1 if the respondent identifies himself/herself as Democrat, rather than Independent or Republican.

⁶ The highest level of education obtained (from 1 = no high school degree to 6 = professional school degree).

⁷ How much confidence the respondent has in the federal government (1 = very little, 10 = very much).

⁸ Annual pre-tax household income (from 1 = up to \$10,000 to 7 = \$150,000 or more).

⁹ Male (1) or female (0).

¹⁰ 1 if the respondent identifies himself/herself as Republican, rather than Independent or Democrat.

¹¹ Takes the value 1 if the respondent is opposed to fiscal consolidation and motivates his or her opposition with an argument about lack of responsibility for the debt. The variable takes the value 0 for other motivations and no value is assigned if the respondent supported the proposal.

¹² Where the respondent placed himself/herself on a left-right scale (1 = left, 10 = right).

¹³ The respondents opinion about the proposed fiscal consolidation (1 = strongly disagree to 5 = strongly agree).

Table A.3: Variables in Essay III

	Obs	Mean	St. dev.	Min	Max
Coalition ¹	377	0.69	0.46	0.00	1.00
Debt ²	378	28.16	47.67	-166.87	129.22
Δ Debt ³	378	0.20	6.89	-34.30	28.55
Election ⁴	378	0.00	0.47	-0.32	0.68
Gap ⁵	378	0.35	2.80	-13.33	9.21
Gap _t ⁶	375	-1.29	2.51	-15.00	5.50
Gap _{t-1} ⁷	378	-1.41	2.45	-18.23	5.53
GDP growth ⁸	378	-0.03	2.74	-11.00	9.13
Gap _{t-1} ⁹ (HP-filter) ⁹	378	-0.26	1.43	-7.08	3.75
Δ Inflation _t ¹⁰	330	0.01	1.55	-8.53	7.61
Δ Int.payments _t ¹¹	345	-0.08	0.38	-2.39	3.21
Left-wing ¹²	348	0.52	0.50	0.00	1.00
Minority ¹³	377	0.20	0.40	0.00	1.00
Primary balance ¹⁴	378	-0.27	3.06	-11.30	6.41
Unemployment rate ¹⁵	378	7.06	3.24	1.64	24.79

¹ A dummy which takes the value 1 if the government is a coalition government. Source: Database of political institutions.

² General government net debt, as a share of GDP. Source: OECD.

³ Annual change in general government net debt, as a share of GDP. Source: OECD.

⁴ A dummy which takes the value 1 if there was a legislative or presidential election. The variable was then centered at zero. Source: Database of political institutions.

⁵ Revised output gap. Source: OECD.

⁶ Output gap estimated at the end of the year. Source: OECD.

⁷ Output gap estimated at the end of the previous year. Source: OECD.

⁸ Annual GDP growth. Source: OECD.

⁹ A real-time output gap estimated using a Hodrick-Prescott filter. Source: OECD and my own calculations.

¹⁰ Change in inflation of consumer prices. Source: World Development Indicators.

¹¹ Change in the annual interest payments. Source: OECD.

¹² Party orientation of the executive party (1=left, 0=center or right). Source: Database of political institutions.

¹³ A dummy which takes the value 1 if the government is a minority government. Source: Database of political institutions.

¹⁴ The net lending excluding interest payments. Source: OECD.

¹⁵ The number of unemployed as a share of the labour force. Source: OECD.

Table A.4: Variables in Essay IV

	Obs	Mean	St. dev.	Min	Max
Δ Tax, instr. countries ¹	374	0.09	0.53	-2.05	1.39
Adj. net lending ²	379	-2.11	3.17	-17.74	5.28
Election ³	379	0.26	0.39	0.00	1.00
Growth ⁴	379	1.75	2.59	-14.26	9.49
Inflation ⁵	379	2.53	2.34	-0.92	23.47
Output gap ⁶	379	-0.68	3.15	-15.81	9.19
Predicted growth ⁷	352	1.79	2.62	-20.46	8.89
Right-wing ⁸	379	0.44	0.46	0.00	1.00
Soc. expenditure: total ⁹	358	24.32	4.13	14.49	35.52
Soc. expenditure: cash ¹⁰	358	14.50	3.31	5.06	21.91
Soc. expenditure: in kind ¹¹	358	9.04	2.29	0.00	16.15
Soc. expenditure: unemployment ¹²	311	2.38	1.42	0.24	6.63
Soc. expenditure: health ¹³	312	9.08	1.34	5.56	12.47
Soc. expenditure: other ¹⁴	302	12.67	2.91	6.26	18.57
Tax ratio ¹⁵	379	44.79	4.75	36.18	56.87
Unemployment ¹⁶	379	7.63	3.47	1.99	27.47
Working age population ¹⁷	379	66.57	1.57	62.02	70.68

¹ Average change in tax ratio in the countries used to create the growth instrument. Source: OECD and my own calculations.

² Cyclically adjusted net lending, as a share of GDP. Source: OECD.

³ A dummy which takes the value 1 if there was a legislative or presidential election. Source: Database of political institutions.

⁴ Annual GDP growth. Source: OECD.

⁵ Change in inflation of consumer prices. Source: OECD.

⁶ Revised output gap. Source: OECD.

⁷ A growth prediction based on the growth rate in countries with synchronized business cycles. Source: OECD and my own calculations.

⁸ Party orientation of the executive party (1=right, 0=center or left). Source: Database of political institutions.

⁹ Public spending for social purposes, as a share of GDP. Source: OECD.

¹⁰ Public spending for social purposes: cash benefits, as a share of GDP. Source: OECD.

¹¹ Public spending for social purposes: benefits in kind, as a share of GDP. Source: OECD.

¹² Public spending for social purposes: unemployment benefits and active labour market policy, as a share of GDP. Source: OECD.

¹³ Public spending for social purposes: expenditures related to health and incapacity, as a share of GDP. Source: OECD.

¹⁴ Public spending for social purposes: other expenditures, as a share of GDP. Source: OECD.

¹⁵ The sum of taxes on production and imports (indirect taxes), current taxes on incomes and wealth (direct taxes), social security contributions, and other current receipts, expressed as a percentage of GDP. Source: OECD.

¹⁶ The number of unemployed as a share of the labour force. Source: OECD.

¹⁷ The share of the population which is older than 14 and younger than 65. Source: OECD.