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And Yet It Grows: Crisis, Ideology, and Interventionist Policy Ratchets

Christian Bjørnskov and Martin Rode

And Yet It Grows

Crisis, Ideology, and Interventionist Policy Ratchets

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Abstract: Previous studies of policy responses to economic crises argue that crises may lead to more interventionist policy but also cause deregulation. The empirical evidence in previous studies is equally mixed. The present paper argues that whether or not governments implement more or less interventions depend on their core political ideology. We thus expect ideologically heterogeneous policy reactions to crises yet also note that crisis responses theoretically may cause ‘policy ratchets’ where temporary crisis policies become permanent. Employing a panel of 68 countries with Western political institutions observed between 1975 and 2010, and exploring the evolution of indicators of government size and regulatory policy, we find that crises in general cause more interventionist policies when countries have centrist or left-wing governments. We also find clear evidence of policy ratchets in all policy areas. The ideological crisis policies mainly relate to government consumption and market regulations.

JEL Codes: D72; H70; H11

Keywords: Economic crisis; regulation; government consumption; government ideology

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1. Introduction

Discussions on the optimal level of government intervention in the economy have become very intense in recent years, especially following the 2008 financial and economic crisis. The hardships produced by this temporal economic downturn have led to some very heated debates among the economics profession on how much government should intervene in the economy to offset the associated negative effects for income and unemployment. Some prominent voices, such as Krugman (2012), or Piketty (2014), have recently argued for much more active government, postulating that current public sector size, or regulatory activity, are insufficient to overcome the long-term effects of what has also become known as the *Great Recession*.

Interestingly, large increases in government spending were precisely the initial reaction to the 2008 crisis followed by practically all affected high-income countries, according to recent findings by Alesina et al. (2015). In an attempt to offset the expansionary increase in government spending though, many OECD governments shortly afterwards designed deficit reduction policies that had exactly the opposite objective, namely to diminish the weight of the public sector in economic activity once again. As this concrete example of a recent depression episode highlights the question whether crisis driven increases in government size and regulation become permanent, whether they are purely temporary, or whether perhaps the overall effect is even negative, as suggested by the many recent “austerity” claims in popular debate, is a highly relevant and very contemporary question in economic policy.

Previous theoretical and empirical studies argue that economic crises may lead to more interventionist policies and bigger governments, but also cause deregulation and reductions of public sector size. For instance, Buchanan and Wagner (1977) argue that the acceptance of Keynesian economic policy measures tends to relax political constraints on deficit finance, allowing government to spend more than otherwise possible during an economic crisis. Regarding the long-term effects of such an event, Higgs (1987) believes it to be unlikely that government size will be rolled back to pre-crisis levels, once the economic slump is over, giving every crisis a permanent component via lasting expansion of the public sector. On the other hand, several authors sustain that an economic crisis should actually be beneficial to the conduction of pro-market reforms, including the reduction of public sector size and economic regulation (e.g. Drazen and Grilli 1993; Kruger 1993; Rodrick 1996). In this view, an economic crisis is a unique moment in time where deteriorating economic conditions facilitate major policy changes to permit a more liberalized economy with a smaller public sector.

Interestingly, both viewpoints interpret major economic crises as a type of critical juncture, where political reactions will, at least in part, be determined by historically path dependent processes, but where the decisions taken at the moment of the critical juncture will also affect future events in a path dependent manner (cf. Capoccia and Keleman 2007; Soifer 2012). So because of small differences in initial conditions and the decisions taken to combat the slump, the same crisis can send countries down radically different paths with respect to government intervention in the economy. According to Acemoglu et al. (2009), this is why all empirical studies dealing with critical juncture events need to control for constant, potentially historical, factors.

Maybe for this same reason, the empirical evidence from cross-country studies on crisis and government size is equally mixed than the theoretical literature, with some authors finding that crises in general cause governments to tighten the interventionist grip on the economy and others finding that crises are used as moments in which deregulation and less interventionist policy can be implemented: While Baier et al. (2012) conclude that crisis episodes are associated with lower overall economic freedom, de Haan et al. (2009) only encounter this effect to be temporary and find that in the long-run public sector size is actually reduced after a banking crisis. Likewise, Pitlik and Wirth (2003) find that substantial crisis episodes facilitate overall economic regulation, and Pitlik (2008) adds the important notion that this relationship is mediated by political institutions, with democratic governments being more prone to liberalize the economy after a crisis. Recently, Young and Bologna (2016) undertake an exhaustive study of how several different types of crisis affect the size and scope of government, concluding that reactions are for the most part very idiosyncratic and cannot be generalized across different countries. According to their findings, whether a crisis results in increases or decreases of government are therefore conditional on the particulars of time and country.

Given the previous findings, our paper introduces an important innovation that has not been empirically tested in the existing literature. First, we argue that while economic crises do represent politically critical junctures and history will thus partly determine the political reaction to a crisis, it is also a moment where more substantial change is possible, and whether or not governments implement more or less interventionist policies will depend on their core political ideology. To say it in the words of Young and Bologna (2016), we explicitly test for one of the particulars of time and country, namely the political ideology of those who decide upon the size and scope of a government stabilization program. Theoretically, the extent of stabilization policies undertaken should heavily depend on the political ideology of present authorities, due to the fact that views of government intervention and the market economy

often differ greatly across the ideological polity spectrum. A long tradition in political science on ‘issue ownership’ documents that specific types of parties own, or clearly represent, specific issues that are of particular relevance to certain groups in society (Petrocik 1996), and that voters align their political choices along ‘mental models’ where many different positions on political, social, and economic issues are aggregated into a political ideology (Denzau and North 1994). For example, economically it is often assumed that left-wing parties represent poorer socioeconomic segments, consisting mainly of ordinary labour, or perhaps low-skilled labour, while right-wing parties tend to represent capital owners, high-skilled labour, and entrepreneurs. Recent empirical studies by Potrafke (2010), Pickering and Rockey (2011), Bjørnskov and Potrafke (2012), and Faccini and Melki (2014) all confirm that government ideology is a potentially important determinant of economic policy, particularly regarding increases in the size of government and its regulatory role. The association between government ideology and the level of government size is further confirmed by a different study by Pickering and Rockey (2013), who find the interaction of ideology and mean income to be a key determinant of US state government size. We thus expect ideologically heterogeneous policy reactions to crises, with more left-wing governments creating bigger and more permanent stabilization programs, and more right-wing governments creating smaller and purely temporary programs.¹

Second, we argue that Tullock’s (1975) transitional gains trap, as well as democratic political dynamics, may prevent crisis policies from being rolled back when the crisis is over. Tullock (1975) argued that any policy by government benefitting a particular industry with monopoly power will inevitably create a dynamic, where the short-run transitional losses from renewed deregulation exceed the welfare cost of reduced output and higher prices from regulation. In addition, the industry in question will use intense lobbying activity to prevent the program from being eliminated, despite the fact that exceptional benefits only accrue to the first generation of license holders. According to evidence in Thomas (2009), rent-seeking groups may thus be able to maintain such regulations for extended periods of time, if substantial technological change is absent in a particular market. Given that this dynamic is extended to a sufficient amount of groups, for example via Keynesian anti-crisis programs, democratic economic policy may very well become dominated by intense lobbying activity that has the principal goal to maintain or expand these programs. In the words of Olson (1982), these lobbying activities lead to ‘institutional sclerosis’ in which

¹ Potentially, ideology could have an effect, either because it informs the government about what would be the appropriate response, or because the crisis provides an opening for the government to more aggressively push its ideology.

special interests and poor policies reinforce each other.² To the extent that interventionist crisis policies create transitional special interests and political credibility problems, we therefore expect to observe policy ratchets, i.e. that temporary crisis policies become permanent by increasing government share of GDP and overall economic regulation.

Employing a panel of 68 countries with Western political institutions observed between 1975 and 2010, we find that crises in general cause more interventionist policies when countries have centrist or left-wing governments. We track the development in indicators of government size and regulatory interventions by using two subcomponents of the Economic Freedom of the World index developed by Gwartney et al. (2015). The affected ideological policies are mainly related to government consumption, taxation and market regulation. We also find clear evidence of policy ratchets in all policy areas, indicating that governments of all ideological convictions tend to perpetuate the policies chosen during the crisis. Finally, we discuss evidence suggesting that our main findings are robust and can be interpreted as reasonably causal, and conclude that economic crises can have permanent effects through these policy ratchets.

The rest of the paper is structured as follows. Section 2 describes our data and the estimation strategy. Section 3 reports the results in three subsections covering main results, ratchets and specific policy domains. We discuss the findings and conclude in section 4.

2. Data

In this paper, we implicitly focus on the growth of government and changes in its regulatory activity. Potentially, each can act as substitutes for the other, as highlighted by Posner (1971): If one accepts his description of regulation as a form of taxation, any empirical investigation will automatically need to take both aspects into account, because by focusing only on the budgetary actions of government, we would be leaving out an important dimension of government activity that is directly related to its overall size (i.e. Holcombe 2005). Similarly, Young and Bologna (2016) focus on the *size and scope* of government in their recent work, even though their measurement of specific variables is somewhat different.

² A major difference between these theories is that Tullock's transitional gains trap supposes that the crisis policy itself creates the potential rent-seeking lobby, while Olson's argument rests on the fact that these lobbies already exist and are simply able to further divert the flow of resources in the event of an economic crisis. While these theories have been highly influential in public choice and political economy, a third option that must be considered is that the structure of post-crisis politics implies that the sector or policy is so far from political salience that no one has any interest of changing the crisis policy in any direction.

We use two underlying elements of the Economic Freedom of the World (EFW) index by Gwartney et al. (2015) to measure government size and economic regulation in our paper. This index is published annually by the Canadian Fraser Institute, reflecting the degree to which the economic institutions and policies of a country correspond to free market principles. The index is divided into five major areas: *1 Size of government: Expenditure, taxes, and enterprises*, *2 Legal structure and security of property rights*, *3 Access to sound money*, *4 Freedom to trade internationally*, and *5 Regulation of credit, labor, and business*. For all areas in the original index, zero represents the least free and ten the most free.

For reasons of better interpretation, we reverse the index such that *higher* index values imply *larger* government consumption and *more* intervention. For our purpose, area 1 is employed to measure variations in the size of government, and area 5 to measure variations in governments' regulatory activity. Both are rather broadly encompassing measures of each concept, which assures that we do not overlook any possible relationship by focusing on just a single indicator. In addition, we split area 1 into its four individual components, so as to highlight the exact variations of the underlying variables and make sure that our investigation does not miss out on some important details. These are: *1a Government consumption*, *1b Transfers and subsidies*, *1c Government enterprises and investment*, and *1d Top marginal tax rate*.

The focus in this paper will be on explaining the changes in government size and regulatory activity, not in comparative levels. Holcombe (2005) highlights that explanations for the size of government and its growth are naturally interrelated and differences are subtle, but they are nonetheless important for the following reason: While government size could hypothetically be the outcome of some collective choice mechanism with a preference driven optimality, related theories ultimately fail to explain why government would keep growing, unless preferences change to a substantial degree. Ultimately, this makes collective choice theories incompatible with explanations that highlight budget maximization and policy ratchets as primary reasons for government growth, where there is really no pre-defined maximum size that government can have (e.g. Peltzman 1980; Miller and Moe 1983; Holcombe 1993). As this paper explicitly tests for political-ideological budget maximization motives and crisis as a critical juncture that affects the relaxation of constraints on taxation, spending and debt accumulation, we will focus on the explanation of government growth in this paper, not on comparative levels.

To relate an economic crisis to variations in the overall size of government and its regulatory activity, we further need a good measure to capture an economic crisis.³ In this paper we opt for defining it as the count of years with negative annual growth that a country passes through at five year intervals between 1975 and 2010. In addition, we create two variables that distinguish between a *hard crisis* and a *soft crisis* operationalized as follows: Hard crises are defined so that the cumulative GDP losses for the corresponding economy have to be larger than four percent of GDP, while all other crises are defined as a soft crisis. Both are also operationalized such that they capture the amount of years that a country spends with either negative annual growth or with large GDP losses. Effectively, this also separates short and longer crises, as we observe no crises with large GDP losses that last only for one year.

As argued above, whether or not governments implement more or less interventionist policies during an economic crisis will probably depend on their core political ideology, with more left-wing governments arguably tending to create bigger increases in government spending or regulation during a crisis. Several measures of government ideology are available for developed countries, but this is not the case for most of the developing world. Nonetheless, we are able to use an alternative to the Database of Political Institutions (DPI) by Beck et al. (2010) from Bjørnskov and Potrafke (2011), which has two particular features. First, the database includes information on many of the missing data points for developing and middle-income countries in the DPI, and second, the ideology measure is based on a more fine-grained operationalization of ideological differences between political parties.

In detail, the ideology variable is measured with modern social democrat parties as an anchor around which other parties are placed. All parties in parliament are distributed on a scale from -1 to 1, with -1 representing completely left-wing (communist or unreformed socialist) parties, -.5 representing modern socialist parties, 0 representing modern social democratic parties such as the SPD in Germany, .5 represents conservative parties, and 1 is set to represent parties with an ideological base in classical liberalism. As such, all parties in our dataset are explicitly rated based on their position on *economic* policy, so that their ideological approach to non-economic social issues or immigration can differ substantially from their economic ideology.⁴ This is an important difference to other ideology measures that focus

³ Even though we don't test for this hypothesis, political crises might also produce similar results. The recent coup attempt in Turkey is a good example of a political crisis that is being used for a deep transformation of the state, which could perfectly affect its size and also its regulatory role.

⁴ It bears mention that some parties such as Front National in France, the Austrian Freedom Party, or the Danish People's Party are often considered very right-wing in public media and occasionally by political scientists due to their particular position on

heavily on social policy positions to classify parties, often judging economic policy positions to be of secondary importance. Due to the fact that the economic ideology of parties in government are of vital interest for the policies adopted during an economic crisis though, we feel that our measure is far more adequate for the investigations conducted in this paper. This categorization is employed to place all governments on a -1 to 1 scale, where the seat share in parliament of each party is used as weights when calculating the ideology of coalition governments.

Most importantly, we introduce an interaction term between government ideology and our crisis variables, which will be used to test any possible heterogeneous crisis reactions by governments of differing political ideology. In accordance with evidence from previous research, we expect the reactions of left-wing administrations for the size and scope of government to be stronger and more durable across time, compared to the reactions of right-wing governments.

Furthermore, we introduce a number of additional controls, which have been shown to be significantly related to government size across countries by previous research. These are all summarized in Table 1. Additional control variables are as follows: Following the famous Wagnerian law, stating that the size of government grows as a share of GDP with rising average per capita income (i.e. Wagner 1893), GDP per capita is a primary control to explain cross country variations in government size. Studies by Pitlik and Wirth (2003), Pitlik (2008), and Young and Bologna (2016) also utilize per capita income as a control variable in their models, finding that initial GDP is a fundamental determinant of changes in government size and scope. Therefore, the logarithm of initial GDP per capita in purchasing power parity terms is also included in our basic model. This data is taken from the Penn World Tables, mark 7.1 (Heston et al., 2012), which naturally also provides data for our coding the crisis variables.

Table 1 about here

Similarly to the reasoning underlying the Wagnerian law; it has more recently been argued by a number of scholars that there is a direct connection between openness to trade and the size of government. Most notably, Rodrik (1998) examines the direct relationship between openness and immigration policy and focus on traditional social values. Our exclusive focus on economic policy in the present paper means that we do not code them as right-wing. Instead, we code the strongly interventionist Front National as socialist (-.5) and the Austrian Freedom Party and the Danish People's Party as social democrat (0). This classification is supported by the fact that all these parties have recently received much electoral support from formerly socialist and social democratic electorates.

government size, finding that more open economies also have larger public sectors. This author argues that comparatively larger governments create a type of security mechanism against external trade shocks for countries that are heavily dependent on international exchange for the generation of economic welfare. Recently, Bennaroch and Pandey (2012) re-examine Rodrik's findings with different data, failing to encounter such a relationship. Still, so as to account for the existence of such a possible mechanism, we introduce a measure of openness to our basic model, which is the total volume of all traded goods over total GDP. In a related manner, recent papers have further found smaller countries to present larger public sectors, arguing that country size, government size, and trade openness are all interconnected (i.e. Alesina and Wacziarg 1998). The underlying logic is that by default smaller countries trade more, but also create comparatively larger governments as a mentioned security mechanism. Therefore, we also include the log of total population into our model, so as to control for the size of countries. Data for these two variables is also from the Penn World Tables.

Another important factor that has been argued to affect the size and scope of government in reform processes is a country's political regime (Pitlik 2008). Here, it is generally accepted knowledge among political economists that under democratic regimes the public sector grows, as a response to voter's redistributive demands, while non-socialist authoritarian regimes are able to keep public sectors small (e.g. Tavarés and Wacziarg 2001). This hypothesis is also indirectly supported by Aidt and Jensen (2009), who find that the expansion of the voting franchise significantly raised government taxation and spending in the 19th and early 20th century. We therefore introduce two different dummy variables to our model that takes the value of one, if the country in question presents a political regime that is a civilian autocracy, or a military dictatorship. Both variables are from a dataset by Cheibub et al. (2010), which codes democratic and autocratic regimes worldwide since 1946.⁵

Relatedly, Persson and Tabellini (1998) find strong support that the size of government is smaller under presidential regimes, when compared to parliamentary regimes, which they ascribe to the different structures of legislative bargaining.⁶ Consequently, a dummy variable that takes the value of one if a

⁵ The dataset in Cheibub et al. (2010) ends in 2008. However, we have in connection to other work expanded the data to include more recent years until 2015. With respect to the category of civilian autocracy, it closely corresponds to what is otherwise sometimes called electoral autocracy, competitive authoritarianism and illiberal democracy (LeDuc et al. 2010; Levitsky and Way 2002; Zakaria 1997).

⁶ Relatedly, Presidential regimes might at times have a divided government, meaning that the legislative and executive branch don't represent the same political ideology.

country has a democratic presidential regime is further introduced to our basic model. Again, this variable is taken from the dataset by Cheibub et al. (2010). Finally, Persson and Tabellini (1998) and Kontopoulos and Perotti (1999) also find that countries with proportional representation electoral systems and frequent coalition governments have, on average, comparatively larger governments. The underlying reason that is usually mentioned to explain these findings is that political bargaining processes in a coalition have a tendency to increase the size of new spending programs. Therefore, we introduce a common measure of government concentration to our basic model, the Herfindahl-Hirschmann index of the legislature, which is derived from our data on political parties.

Regarding the data structure and our estimation procedure, it should be noted that we observe aggregated five-year changes in government size and regulatory activity as a function of the variables mentioned above. In particular, our dataset contains 68 countries with Western political institutions, aggregated at five-year periods between 1975 and 2010, giving us up to 471 individual observations. Our sample is restricted by requiring that all countries are either democratic or as a minimum have a democratic constitution similar to that of Western democracies, such that the *de jure* political institutions resemble those of Western Europe and North America. This restriction is theoretically necessary as our background for understanding and interpreting the relation between crises and policy changes rests on the existence of, at least in principal, democratically structured political processes.

All estimations were conducted using simple OLS fixed effects regression for panel data, introducing a one-period time lag of five years between the dependent and independent variables. The initial levels of government size and regulation are also introduced as primary control variables to the model, but due to the estimation procedure with country (and time) fixed effects, the dependent variable will only capture changes in both variables of interest. The use of country fixed effects also means that we can effectively control for the possible presence of path dependent reactions to the extraordinary event of an economic crisis, as highlighted by Acemoglu et al. (2009). Marginal effects of interacted variables are calculated by the Delta method (Brambor et al. 2006). Formally, in our baseline specification we model changes in the size and scope of government in country i at time t , as follows:

$$\begin{aligned} \text{GovSize}_{it} = & \beta_0 + \beta_1 \text{GovSize}_{it-1} + \beta_2 \text{Crisis}_{it-1} + \beta_3 \text{Ideology}_{it-1} + \beta_4 (\text{Crisis}_{it-1} \times \text{Ideology}_{it-1}) \\ & + \beta_5 \text{Controls}_{it-1} + \text{cfe}_i + \text{tfe}_t + \varepsilon_i \end{aligned}$$

While we cannot rule out that the occurrence and severity of crises is affected by changes in government size or regulatory activity, we can still make causal claims. The reason is that even while the crisis variables may be endogenous to changes in interventionist policies, the interaction term between government ideology and crises is unlikely to suffer from endogeneity bias. As Nizalova and Murtazashvili (2016) show, when one of the interaction variables is plausibly endogenous, an interaction term and thus the heterogeneity of an effect can be interpreted causally. This is exactly the situation in our case because the ideology of the government is not affected by crises.⁷

3. Results

3.1. Main results

Table 2 presents the results of our baseline model, where we look at the current effects of a crisis, presenting also the full set of individual control variables. Area 1 is employed to capture changes in the size of government, and area 5 to capture changes in overall economic regulatory activity. Columns 1 and 2 show estimates for the entire country sample, while columns 3 and 4 exclude military dictatorships, and equations 5 and 6 exclude all political autocracies – civilian or military – from the model.

Before describing the principal association of interest between government size, economic crisis, and ideology, a few words apply regarding the other control variables and their relation to the dependent variables. First, we find that initial levels of government size and economic regulation enter all columns in Table 2 with a positive and statistically highly significant coefficient at the 1% level. Contrary to common intuition, this result indicates that those countries with already bigger governments and more regulation also have a larger tendency to increase both elements in the following period. Second, we find little or no indication that GDP per capita levels, openness, or coalition governments bear any relation to changes in government size and economic regulation, although this result may arguably be influenced by the fact that we are not looking at comparative levels but medium-run changes in our model. Third, we find quite some indications that larger countries, those with a presidential regime, and military dictatorships all share a tendency to increase economic regulation, all else being equal. Interestingly, all three of these categories are

⁷ The data show that the probability that the incumbent government is reflected is probably affected by crises. Yet, the particular ideological position of the incumbent government or the party or coalition winning an election following a crisis is *not* affected by whether or not a country experiences a crisis. As we deal with in the appendix, the only substantial complication is when the political institutions allow the incumbent government to call an early election.

usually associated with smaller public sectors, as reviewed in the previous section. In line with Posner (1971), these results would indicate that a smaller government is perhaps compensated for by comparatively higher increases in economic regulation. Fourth, the latter finding is further confirmed by the results for civilian autocracies, which also present a tendency to increase economic regulation, but seem to present a parallel inclination to reduce government size in the economy. This would also point in the direction that especially for politically autocratic regimes a substitution effect between taxation and regulation might actually exist to some degree.

Coming to our principle variables of interest, Table 2 confirms ordinary expectations and previous research by showing that governments with a more conservative (i.e. economically classic liberal) ideology reduce public sector size in the economy outside of crises, but that government ideology presents absolutely no relation with changes in economic regulation.⁸ In turn, coefficients on the current crisis variable show that more years spent in an economic downturn are associated with larger following increases in both the size of the public sector and overall regulation. Still, these coefficients should not be interpreted without considering the interaction effects of both variables, which is also our primary focal point in this paper. The lower part of Table 2 present the marginal effect of years spent in an economic crisis, dependent on different levels of government ideology. Here it can be seen in column 1 that all governments seem to present a tendency to increase government size during a crisis, regardless of their political ideology. Still, coefficients also show that more socialist governments also generate larger increases in the public sector, as a reaction to economic crisis. Similarly, left-wing governments also present comparatively larger increases in regulation as a crisis reaction in column 2, while results for right-wing governments are statistically insignificant in this case. These findings are confirmed when excluding military and civilian dictatorships in columns 4 to 6, demonstrating that they are not driven by a specific type of political regime that may only have the appearance of democracy. In fact, a comparison of the coefficients indicates that the effect seems to be stronger when excluding these regimes, as can be observed in the corresponding models.

So far, a major finding from Table 2 is that governments react to a current economic crisis in rather heterogeneous ways, which is also in line with common expectations from political economy:

⁸ It must be stressed that because the specification includes an interaction term between ideology and crisis, the point estimate of government ideology *per se* represents the effect of ideology when there is no crisis. Any effect of ideology within a crisis is the sum of this point estimate and the interaction.

Governments of all political ideology increase the size of government as an immediate crisis reaction, but this increase is far more pronounced for governments on the left side of the political spectrum than for those on the right. Similarly, to the extent that the average crisis estimates are unbiased, only socialist governments increase economic regulation to a significant degree, while governments with a conservative ideology in general seem to refrain from doing so.

Table 2 about here

Given the fact that we find heterogeneous reactions to a current economic crisis raises the question whether potential ratchet effects are also primarily driven by political ideology, and if left-wing administrations create more lasting increases in the size and scope of government than right-wing administrations? Table 3 evaluates this particular question by simply introducing a one-period lagged crisis effect to our basic model to capture policy ratchets. All models presented here further employ the full set of control variables from the previous table, but due to the fact that findings are practically identical, these are simply omitted in Table 3.

Generally, the estimates in Table 3 confirm the earlier results that governments with a more conservative / classical liberal ideology reduce public sector size in the economy, while more years spent in a current economic downturn are associated with larger following increases in the size of the public sector and overall regulation. In turn, there is absolutely no overall effect of the lagged crisis variable whatsoever. The lower part of Table 3 again presents the marginal effect of years spent in a current economic crisis, depending on different levels of government ideology. Here, the earlier finding that, as a reaction to a current economic crisis, more left-wing governments also generate larger increases in the size and scope of government is confirmed. In addition, marginal effects for lagged economic crises are further presented in the lower part of the table where it can be seen that we find absolutely no significant evidence at all for heterogeneous policy reversals: Lagged economic crises present no association with increases in the size of government or economic regulation, regardless of the political ideology of the government in power.

The results of Table 3 thus show no evidence of any policy reversals, regardless of the ideology of the next government in power. Still, this also means that the previous increases made primarily by socialist governments are, at least on average, not rolled back again after the crisis is over. In the long run, this would automatically lead to a permanent increase in the size and scope of the public sector, given that there is sufficient alternation in government so that it accommodates administrations of all political

ideologies over time. In the end, this indicates that Higgs (1987) was correct and that policy ratchets as a result of economic crisis are a general phenomenon in Western democracies. Interestingly, these findings are somewhat hard to reconcile with the currently popular austerity claims in Europe, because they show that the size and scope of government is, in fact, not rolled back as a reaction to economic crisis (not even by right-wing governments), at least not over the period under observation here. At most, government size and economic regulation are maintained at equal levels by some administrations, although this can only be denoted *austerity*, if one holds the view that both elements should actually increase over time.

Table 3 about here

3.2. Specific areas

The previous two tables show that one main association of current economic crisis reactions by government seems to be in the area of government size. As area 1 of the EFW index is by itself a compound indicator of government size, we believe it is interesting to delve deeper into which underlying factors are actually responsible for this overall increase. Therefore, Table 4 repeats estimations from the previous tables, but splits area 1 into its four principal components. These measure the separate concepts of government consumption (1a), transfers and subsidies (1b), government enterprises and investment (1c), and top marginal tax rate (1d). As in Table 3, we include the full baseline but only report the specific results.

The lower part of Table 4 indicates that governments with a more left-wing ideology greatly tend to increase government consumption and transfers and subsidies as a reaction to a current economic crisis, while no such thing can be observed for right-wing governments. Hence, we observe strong, politically heterogeneous responses to government consumption and transfers and subsidies. In turn, both sides of the political spectrum seem to somewhat increase government enterprises and investment, which would point to a non-ideological response in this particular area. Again, we find almost no evidence for heterogeneous ratchet effects. Lagged economic crises present no association with variations in government consumption, transfers and subsidies, and government enterprises and investment, disregarding political ideology. Only in the case of top marginal tax rates, there is some evidence that left-wing governments have a stronger tendency to increase these in the years following, which somewhat confirms common intuition.

Table 4 about here

Finally, Table 5 repeats the main estimation from the previous two tables, but separates economic crises according to their total, immediate impact on the economy. Due to the fact that we find the presence of ratchet effects, it is important to know whether these are a basic consequence of any economic crisis that is confronted by an incumbent left-wing government, or whether only crises of some magnitude will drive permanent increases in government size and scope. Here, hard crises are distinguished from light crises by requiring that the former generated cumulative losses in overall income that are larger than four percent of initial GDP per capita.

The lower part of Table 5 shows that practically all of our previous findings are driven by hard economic crises. For example, it can be observed in equation 1 that all governments increase government size during a hard economic crisis, regardless of their political ideology. Still, coefficients again show that more socialist governments generate larger increases in the public sector. Separating for the underlying sub-indicators in columns 2 to 5, we also confirm that more left-leaning governments drive the findings in area 1 by increasing government consumption and transfers and subsidies, as a reaction to a hard economic crisis. Similarly, left-wing governments also present comparatively larger increases in regulation as a crisis reaction in column 6, but results for right-wing governments also show that they share a significant tendency to increase economic regulation during a hard economic crisis. Generally speaking, it seems that a more severe economic crisis therefore creates stronger incentives for policymakers of all political ideologies to relax constraints on government spending and augment the regulatory activities of government, but these tendencies remain substantially stronger for all socialist and social democratic types of governments.

Table 5 about here

Of course, some might argue that the findings presented above are actually an artefact, produced by the compound nature of the EFW index, which has an overall tendency to increase over the time period under observation. To some degree, we already show by disaggregating area 1 (government size) that this is probably not the case. Still, in order to further enhance the certainty that our findings actually present crisis reactions by governments and not something else, we repeat all estimations with EFW area 3 as dependent variable, which measures the access to sound money. Theoretically, there is no reason to believe that governments of different political ideology should produce different reactions in monetary policy when

combatting a crisis, due to the fact that most Western style political systems nowadays have central banks that are very independent of the political process. As a consequence, we should find absolutely no results when employing area 3 as the predicted variable in our estimations, and in fact we do not.⁹ This also means that our findings probably do represent crisis reactions by governments and not overall increases in the EFW index areas over time.

3.3 Endogeneity and placebo tests

A problem that is a little more difficult to solve is the question of endogeneity, since it is perfectly possible that countries with a comparatively larger government (or excessive and inadequate regulation) might also present a larger economic crisis risk. In fact, this seems to be highly probable, according to some recent contributions (e.g. Taylor 2008; Bjørnskov 2016). Still, as we argue above, what is important in this context is not necessarily our crisis variable, but rather the interaction term with government ideology: Government ideology can be argued to be reasonably exogenous, because most democratic governments do not have the possibility, or the rational foresight, to call elections just before the onset of a crisis, when it might best suit them to do so. The governments that do have this possibility are a minority, and they do not significantly influence our results (see Table A4 of the appendix). In addition, there is also no reason to believe that either right- or left-wing governments are more likely to create an economic crisis, due to the fact that crisis-conductive policies are usually of a much more structural and long-term nature (Taylor 2008). As it is, our dataset shows a very similar distribution of government ideology with and without the occurrence of an economic crisis. If we interpret government ideology as exogenous to crisis, one can claim that our interaction term will also produce unbiased coefficients, as long as the direction and extent of omitted variable bias produced by the crisis dummy does not directly depend on government ideology (cf. Dreher et al. 2016). As we argue, this is unlikely to be the case and, in addition, all country-specific structural factors that could jointly produce crisis and governments of a certain ideology are effectively controlled for by the country and time fixed effects of our basic estimation model. Therefore, coefficients on the crisis-ideology interaction variable can reasonably be interpreted as exogenous.

Finally, we nevertheless conduct a number of placebo tests, where all findings further point towards the interpretation of causality forwarded above. Results are all shown in the appendix. First, we conduct a test where the crisis indicator is placed in the incorrect time period, adding one additional lag to

⁹ These results are outlined in detail in the appendix.

the economic crisis. If causality really ran the other way and government size and scope were the main drivers of crisis risk, we should observe the estimates getting larger and more significant. Instead, this test produces broadly insignificant results, suggesting that even the average estimates can be interpreted causally. Second, we randomly assign the values of our crisis variable to that of a neighbouring country. Due to the fact that many economic crises have a strong international component – for about half of the crisis events, one or more neighbouring countries also experienced a crisis – this test should have very little effect on our empirical results, if causality is such that government intervention is the major cause of economic crisis. It also represents a test of whether the observed policy changes are actual responses to country-specific crises instead of policy spill-overs from more regional events. However, we find largely insignificant results on the interaction term in 100 permutations, increasing the probability that what we identify above are really national policy responses to an economic crisis, and not something else.

Overall, we therefore find what we tentatively interpret as causal evidence of ideologically heterogeneous policy responses to economic crises. These findings pertain particularly to government consumption, transfers and subsidies, and market regulation, and also apply to a set of alternative crisis indicators (shown in the appendix). For two of these policy areas – consumption and regulations – the estimates remain robust to a set of tests, including a jackknife exercise reported in the appendix. We conversely find no evidence that these policy responses are reversed after the crisis is over, such that crisis policies tend to become permanent. We therefore proceed to concluding and discussing the potential importance of the findings.

4. Conclusions

The hardships produced by the 2008 financial and economic crisis have led to some very heated public debate on how much government should intervene in the economy. Here, especially the role of the crisis and the ideology of governments in power have been heavily discussed. Previous theoretical and empirical studies argue that economic crises may lead to more interventionist policies and bigger governments, but also cause deregulation and reductions of public sector size. The empirical evidence from previous cross country studies has been equally mixed with some authors finding that crises in general cause governments to tighten the interventionist grip on the economy, and others finding that crises are used as critical junctures in which deregulation and less interventionist policy can be implemented despite special interest resistance. In this paper, we argue that whether or not governments implement more or less interventionist

economic policy may well depend on their core political ideology. So far, this hypothesis has not been tested in the empirical literature on the topic.

Employing a panel of 68 countries with Western political institutions observed between 1975 and 2010, and using the development of a set of indicators of government size and regulation, we find that crises in general cause more interventionist policies when countries have socialist and social democrat administrations: As an immediate reaction to economic crisis, more left-wing governments generate larger increases in the size of government, which appear mainly to be driven by surges in government final consumption and government transfers and subsidies. To be sure, governments of all types of political ideology may increase the size of government as an immediate crisis reaction, but this increase is far more pronounced for governments on the left side of the political spectrum than for those on the right. Likewise, socialist governments also increase economic regulation to a significant degree, while conservative or other right-wing governments seem in general to refrain from doing so. Finally, we encounter that more severe economic crises create stronger incentives for policymakers of all political ideologies to relax constraints on government spending and augment the regulatory activities of government, but again these tendencies remain much stronger for all left-leaning governments.

Interestingly, we find absolutely no evidence at all for heterogeneous policy reversals, meaning that all increases in the size and scope of government present a permanent component, regardless of the political ideology of the incumbent government. These results are hard to reconcile with popular austerity claims, due to the fact that they imply that the size and scope of government is not rolled back as a long-term reaction to economic crisis. It rather seems as if economic crises act as events that permanently relax the ordinary political constraints on government, which especially left-wing administrations are prone to momentarily use to maximize government budgets and introduce tighter regulations on markets. Conversely, right-wing administrations tend not to reduce the size of government on their next ascent to power, thereby effectively creating policy ratchets.

To some degree, these findings point to a fundamental flaw in many economic policy recommendations, where government is often modelled as an omniscient benevolent dictator that will eliminate market failures and thereby benefit overall economic outcome (i.e. Buchanan and Wagner 1977; Holcombe 2012). In such a world, any government would choose an optimal size, given the economic situation of the country, which should be adapted to current performance and be entirely independent of the political ideology of those in power. Needless to say, that with such an underlying model of government, it would be impossible to explain the findings presented in this paper.

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Appendix

This appendix contains a set of additional robustness tests for “And yet it grows: crisis, ideology and interventionist policy ratchets.” We briefly refer to these tests in the paper and elaborate on them in the following.

We begin by providing two placebo tests. In the first placebo test, we provide an intuitive test for reverse causality. If the crisis is not the initiating event, but the policy response either causes the crisis or is simultaneously determined with the crisis (an option for which we see no theoretical background), we would expect to be able to see signs of this when lagging the crisis variable. In other words, the first placebo test consists of placing all crises half a decade too late. To some extent, this is also the way we identify the potential policy ratchets, yet we here include only the lagged crisis variable. The intuition behind this test is simply that if policy changes are either simultaneous or cause crises, the association between crises and the policy response should be at least as strong as in the tests in the paper, or arguably stronger. Table A1 suggests otherwise, as the lagged crisis variables only attains significance in one case – government consumption – and the ideological heterogeneity is substantially weaker and imprecisely measured.

Table A1 about here

In the second placebo test, our worry is instead that the policy responses merely reflect a regional crisis, and therefore cannot be ascribed to a particular government. We test for this by assigning the crisis

indicator of *randomly* chosen geographically neighbouring country. This is a particularly difficult test to pass because neighbouring countries in about half of all cases in the dataset both have economic crises at the same time. This placebo test is therefore biased towards providing a false negative. Yet, the results of the placebo test show that this political worry is probably unfounded, as the interaction terms even change signs across the test.

Table A2 about here

We are also interested in the degree to which our findings generalize to other types of crises. We therefore employ the IMF Financial crisis episodes database (Laeven and Valencia, 2012), from which we draw data on three types of crisis: banking, currency and debt crises. We report our main results in Table A3. All regressions include the full sample although we note that all interaction effects would be substantially more precisely estimated had we excluded all non-democracies. We find that our results tend to generalize to banking and currency crises, but not to the much more predictable and slowly evolving debt crises.

Table A3 about here

Next, we deal with a particular problem associated with ascribing the policy response to a particular exogenously given ideology. For us to be able to interpret the interaction effect causally, at least one of the interacting variables must be approximately exogenous (Nizalova and Murtazashvili, 2016). We argue that although deep economic crises may raise the risk that the incumbent government is ousted from power, the ideology of the potentially new government is not associated with either the crisis or the policy response. However, in a relatively limited number of countries, it is within the power of the government or parliament to call an early election. Denmark provides the starkest example of this particular institutional detail, as the prime minister can call an election at any time in the four years following the last election.

The problem we address is therefore that when an early election is possible, government ideology may not be properly exogenous to the policy response. The reason is that if the incumbent government can call or provoke an early election, it is able to test the electoral support for a proposed policy response. The policy response may thus decide the election and thus the ideology of the government that implements any crisis response. A similar logic applies to the situation in which the parliament forces an early election as a

response to a policy proposal or a political crisis associated with the economic crisis. These considerations only apply when early elections are constitutionally or de facto possible. Based on information in CIA (2016) and the PARLINE (2016) database, we therefore code a variable that captures whether or not it is possible to call an early election. We introduce this variable in triple interactions with our main interaction, government ideology and the crisis indicator, which allows us to assess whether our main findings are driven by the potentially endogenous influence from early elections. We present the main findings in Table A4.

Although it is difficult to interpret triple interactions, we observe a consistent pattern across all regulation regressions, and the government size regression in column 5. We find that while the interaction between government ideology and the crisis indicator is significant and slightly larger than in the corresponding Table 2 in the paper, the triple interaction *early * crisis * ideology*, which captures the *additional* effect in countries that allow for early elections, is always of the opposite sign and almost as large. This indicates that the results in the paper are almost entirely driven by the large majority of democracies with fixed term limits whereas we cannot make any conclusive statements about the comparative effects in smaller number of countries that allow early elections.

Table A4 about here

Our final separate test consists of only including countries that have a perfect record of democracy for all years since 1970 in which it has been de facto independent. We thus, for example, count Estonia as fully democratic while Uruguay is not included due to its military dictatorship in the 1970s. This final test ensures that our results are not driven by new democracies but generalize to established political institutions.

Although the estimates in Table A5 are necessarily less precise due to the somewhat smaller sample size, a comparison with the results in Table 4 reveal that the overall pattern is unchanged: even with a substantially smaller sample of 48 relatively similar countries with a long democratic tradition, we find that crisis responses are ideologically heterogeneous in overall government size, government consumption, transfers and subsidies, and most clearly in regulatory activity.

Table A5 about here

As noted in the paper, we there is little theoretical reason to believe that ideologically different governments also implement different reactions in monetary policy when experiencing a crisis. In particular since the 1980s, Western democracies have moved towards having politically independent central banks. We should therefore not observe any clear results when employing area 3 – sound money – as the predicted variable in our estimations. However, the possibility remains that right-wing governments employ alternative interventionist policies such as monetary interventions instead of fiscal interventions. We test this option in Table A6 in which we employ area 3 in the same specification as all other tests. As is evident, we find no significantly different responses across types of political ideology, and even fail to find clear responses when we restrict the sample to including only stable regimes. As such, we can reject that the ideological difference is simply due to the choice of policy instrument.

Table A6 about here

Finally, we have performed a set of country jackknife tests, in which we repeat the main estimates excluding a single of the 68 countries in the sample. We illustrate the results of these tests in Figures A1-A3, where we plot the estimated effect of crises evaluated at the 25th and 75th percentiles of government ideology. These correspond to the reactions of a social democrat and a clearly conservative government.

We plot the results for the two policy types for which we find clear evidence: area 1a, i.e. government consumption, area 1b, transfers and subsidies, and area 5, market regulations. For all three types, the figures illustrate how relatively stable the estimates are across the jackknife tests. Overall, the figures first reveal that the estimates for transfers and subsidies are substantially less stable than for the two other policy areas. Only three of the estimates as evaluated with a social democrat government (the 25th percentile) are significant at $p < .01$, and in one case (excluding Romania, country no. 56), the estimate fails significance at $p < .05$.

Our estimates turn out to be much more stable for government consumption and market regulations. However, in both cases, the exclusion of Nicaragua (country no. 49) reduces the ideological difference visibly. In the case of government consumption, the exclusion of Bolivia (country no. 9) conversely increases the effect with left-wing government. Yet, in both cases the size of effects of crises with right-wing governments is less stable than with left-wing governments. The effects evaluated with a social democrat government, or governments to the left of that, remain significant at $p < .01$ in all tests and substantially different across types of government ideology. We must therefore conclude that the evidence

for crisis effects and their ideological heterogeneity appears very robust for government consumption and regulatory activity, but not for transfers and subsidies.

Figure A1 about here

Figure A2 about here

Figure A3 about here

5. References

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Table 1. Descriptive statistics

Variable	Mean	Std. deviation	Observations
Area 1	4.226	1.636	484
Area 5	3.498	1.278	463
Area 1 a	20.309	6.896	485
Area 1 b	12.169	8.516	466
Area 1 c	27.468	14.639	455
Area 1 d	5.477	2.836	455
Crisis years	1.216	1.161	485
Light crisis	.351	.478	485
Hard crisis	.328	.469	485
Log GDP per capita	9.427	.839	485
Openness	.814	.553	485
Log population size	8.968	1.637	485
Presidential democracy	.279	.449	485
Civilian autocracy	.097	.296	485
Military dictatorship	.058	.234	485
Coalition government	.437	.497	485
Government ideology	.199	.408	473

Table 2. Current effects

<i>Sample Dependent</i>	All Area 1 1	All Area 5 2	No military Area 1 3	No military Area 5 4	No autocracies Area 1 5	No autocracies Area 5 6
Initial level	.385*** (.041)	.475*** (.045)	.384*** (.043)	.470*** (.046)	.381*** (.043)	.469*** (.046)
Log GDP per capita	-.326 (.235)	.281 (.196)	-.164 (.273)	.313 (.211)	.004 (.337)	.522** (.259)
Openness	-.395 (.267)	-.134 (.194)	-.565** (.277)	-.123 (.198)	-.483 (.308)	.026 (.214)
Log population size	-.229 (.443)	.752** (.349)	-.198 (.465)	.687* (.357)	.188 (.509)	1.115*** (.389)
Presidential democracy	-.308 (.349)	.584** (.265)	-.489 (.445)	.807** (.357)	-	-
Civilian autocracy	-.806*** (.302)	.452** (.222)	-.890*** (.337)	.555** (.253)	-	-
Military dictatorship	-.487 (.361)	.468* (.272)	-	-	-	-
Coalition government	-.056 (.124)	-.038 (.091)	-.009 (.125)	-.055 (.092)	-.015 (.128)	.025 (.092)
Government ideology	-.343** (.168)	-.136 (.121)	-.346** (.173)	-.134 (.125)	-.342* (.178)	-.029 (.128)
Number of crises years	.152*** (.039)	.123*** (.029)	.129*** (.042)	.124*** (.032)	.168*** (.044)	.153*** (.032)
Crisis * ideology	-.078 (.079)	-.143** (.058)	-.065 (.083)	-.169*** (.062)	-.156* (.087)	-.211*** (.063)
Period FE	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	471	435	443	422	407	392
Countries	68	68	68	68	66	66
Within R squared	.449	.681	.442	.674	.461	.666
Between R squared	.357	.299	.557	.294	.498	.125
F statistic	17.46	41.42	16.71	41.01	18.59	41.41
<i>Crisis effect at:</i>						
10 th percentile	.183*** (.057)	.180*** (.043)	.156** (.060)	.192*** (.046)	.231*** (.063)	.237*** (.047)
25 th percentile	.152*** (.039)	.123*** (.029)	.129*** (.042)	.124*** (.032)	.168*** (.044)	.153*** (.032)
Median	.131*** (.040)	.084*** (.029)	.112*** (.042)	.078** (.031)	.126*** (.043)	.096*** (.031)
75 th percentile	.113** (.048)	.052 (.035)	.097* (.051)	.039 (.036)	.091* (.052)	.047 (.036)
90 th percentile	.107** (.052)	.041 (.037)	.092 (.055)	.027 (.039)	.079 (.056)	.031 (.039)

Note: Standard errors in parentheses. *** (**) [*] denote significance at $p < .01$ ($p < .05$) [$p < .10$]. Marginal effects of interactions are calculated by the Delta method (Brambor et al., 2006).

Table 3. Including ratchet effects

<i>Sample</i>	All	All	No military	No military	No autocracies	No autocracies
<i>Dependent</i>	Area 1	Area 5	Area 1	Area 5	Area 1	Area 5
	1	2	3	4	5	6
Full baseline included						
Government ideology	-.433** (.198)	-.106 (.145)	-.435** (.203)	-.115 (.149)	-.552*** (.212)	.013 (.156)
Number of crises years	.151*** (.040)	.122*** (.030)	.132*** (.043)	.124*** (.032)	.179*** (.044)	.156*** (.033)
Crisis * ideology	-.080 (.079)	-.142** (.058)	-.069 (.084)	-.167*** (.062)	-.164* (.087)	-.207*** (.063)
Lagged crises	-.055 (.044)	.015 (.034)	-.035 (.045)	.014 (.034)	-.046 (.047)	.046 (.036)
Lagged crisis * ideology	.084 (.083)	-.028 (.064)	.078 (.086)	-.018 (.066)	.171* (.092)	-.047 (.069)
Period FE	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	471	435	443	422	407	392
Countries	68	68	68	68	66	66
Within R squared	.452	.681	.444	.674	.467	.668
Between R squared	.331	.295	.525	.289	.519	.110
F statistic	15.81	37.10	14.97	36.51	16.69	36.61
<i>Current crisis effect at:</i>						
10 th percentile	.183*** (.057)	.179*** (.043)	.159*** (.051)	.191*** (.047)	.244*** (.064)	.239*** (.048)
25 th percentile	.151*** (.040)	.122*** (.030)	.132*** (.043)	.124*** (.032)	.179*** (.044)	.156*** (.033)
Median	.129*** (.040)	.084*** (.029)	.113*** (.043)	.079** (.031)	.134*** (.044)	.099*** (.031)
75 th percentile	.111** (.049)	.052 (.035)	.097* (.051)	.041 (.037)	.096* (.053)	.052 (.037)
90 th percentile	.105** (.053)	.041 (.038)	.092 (.055)	.028 (.039)	.084 (.057)	.037 (.039)
<i>Lagged crisis effect at</i>						
10 th percentile	-.089 (.064)	.026 (.051)	-.067 (.066)	.022 (.052)	-.114 (.070)	.065 (.055)
25 th percentile	-.055 (.044)	.015 (.034)	-.035 (.045)	.014 (.034)	-.046 (.047)	.046 (.036)
Median	-.032 (.042)	.007 (.030)	-.014 (.043)	.009 (.031)	.001 (.043)	.034 (.031)
75 th percentile	-.013 (.048)	.001 (.034)	.004 (.050)	.005 (.036)	.039 (.051)	.023 (.035)
90 th percentile	-.007 (.052)	-.001 (.037)	.009 (.054)	.004 (.038)	.053 (.055)	.019 (.038)

Note: Standard errors in parentheses. *** (**) [*] denote significance at $p < .01$ ($p < .05$) [$p < .10$]. Marginal effects of interactions are calculated by the Delta method (Brambor et al., 2006).

Table 4. Including ratchet effects, specific sub-indices

<i>Sample Dependent</i>	All <i>Area 1a</i> 1	All <i>Area 1b</i> 2	All <i>Area 1c</i> 3	All <i>Area 1d</i> 4
	Full baseline included			
Government ideology	.556 (.761)	.437 (.685)	6.902*** (2.242)	-.749* (.383)
Number of crises years	-.569*** (.158)	-.336** (.138)	-.773* (.461)	.028 (.081)
Crisis * ideology	.507 (.309)	.254 (.281)	-.370 (.924)	.078 (.169)
Lagged crises	.279 (.171)	.118 (.149)	.279 (.496)	-.147* (.083)
Lagged crisis * ideology	.025 (.322)	-.263 (.288)	-.760 (.947)	.159 (.161)
Period FE	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes
Observations	484	440	429	399
Countries	68	67	67	68
Within R squared	.375	.288	.478	.593
Between R squared	.501	.614	.137	.128
F statistic	11.85	7.14	15.64	22.68
<i>Current crisis effect at:</i>				
10 th percentile	-.772*** (.226)	-.438** (.203)	-.625 (.656)	-.003 (.125)
25 th percentile	-.569*** (.158)	-.336** (.138)	-.773* (.461)	.028 (.081)
Median	-.432*** (.157)	-.267* (.137)	-.873* (.468)	.049 (.074)
75 th percentile	-.316* (.188)	-.209 (.165)	-.958* (.568)	.067 (.088)
90 th percentile	-.277 (.203)	-.190 (.179)	-.986 (.613)	.073 (.096)
<i>Lagged crisis effect at</i>				
10 th percentile	.270 (.250)	.223 (.222)	.583 (.718)	-.211* (.126)
25 th percentile	.279 (.171)	.118 (.149)	.279 (.496)	-.147* (.083)
Median	.287* (.159)	.046 (.139)	.072 (.475)	-.104 (.073)
75 th percentile	.292 (.185)	-.014 (.162)	-.101 (.558)	-.067 (.084)
90 th percentile	.294 (.198)	-.034 (.175)	-.159 (.600)	-.055 (.091)

Note: Standard errors in parentheses. *** (**) [*] denote significance at $p < .01$ ($p < .05$) [$p < .10$]. Marginal effects of interactions are calculated by the Delta method (Brambor et al., 2006).

Table 5. Separating short and long crises

<i>Sample</i> <i>Dependent</i>	All Area 1 1	All <i>Area 1a</i> 2	All <i>Area 1b</i> 3	All <i>Area 1c</i> 4	All <i>Area 1d</i> 5	All Area 5 6
Full baseline included						
Government ideology	-.475** (.199)	.759 (.759)	-.033 (.714)	6.564*** (2.247)	-.779** (.375)	-.249* (.141)
Light crisis	-.109 (.116)	-.049 (.444)	-.283 (.414)	-.027 (1.329)	-.414* (.227)	.028 (.083)
Light * ideology	.426* (.255)	-.479 (.981)	.263 (.936)	-3.517 (2.915)	.878* (.502)	-.003 (.183)
Hard crisis	.410*** (.119)	-1.589*** (.452)	-1.198*** (.425)	-2.476* (1.355)	.085 (.235)	.371*** (.088)
Hard * ideology	-.267 (.250)	2.058** (.955)	1.156 (.902)	-.051 (2.819)	.172 (.487)	-.213 (.183)
Period FE	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	471	482	440	429	399	435
Countries	68	68	67	67	68	68
Within R squared	.453	.389	.293	.481	.596	.685
Between R squared	.295	.469	.627	.129	.131	.284
F statistic	15.87	12.56	7.31	15.83	22.95	37.72
<i>Light crisis effect at:</i>						
10 th percentile	-.279 (.189)	.142 (.727)	-.388 (.699)	1.379 (2.161)	-.765** (.381)	.029 (.137)
25 th percentile	-.109 (.116)	-.049 (.444)	-.283 (.414)	-.027 (1.329)	-.414* (.227)	.028 (.083)
Median	.006 (.099)	-.179 (.383)	-.211 (.340)	-.979 (1.158)	-.176 (.184)	.028 (.071)
75 th percentile	.104 (.119)	-.289 (.464)	-.151 (.411)	-1.785 (1.398)	.025 (.219)	.027 (.086)
90 th percentile	.136 (.131)	-.325 (.509)	-.131 (.454)	-2.049 (1.533)	.091 (.241)	.027 (.094)
<i>Hard crisis effect at</i>						
10 th percentile	.517*** (.183)	-2.413*** (.697)	-1.661** (.668)	-2.455 (2.052)	.016 (.368)	.456*** (.135)
25 th percentile	.410*** (.119)	-1.589*** (.452)	-1.198*** (.425)	-2.476* (1.355)	.085 (.235)	.371*** (.088)
Median	.338*** (.111)	-1.032** (.423)	-.885** (.386)	-2.489* (1.293)	.132 (.209)	.313*** (.082)
75 th percentile	.276** (.134)	-.561 (.512)	-.621 (.464)	-2.501 (1.564)	.171 (.247)	.265*** (.098)
90 th percentile	.256* (.146)	-.406 (.557)	-.534 (.505)	-2.505 (1.697)	.184 (.269)	.285** (.106)

Note: Standard errors in parentheses. *** (**) [*] denote significance at $p < .01$ ($p < .05$) [$p < .10$]. Marginal effects of interactions are calculated by the Delta method (Brambor et al., 2006). Hard crises are distinguished from light crises by having cumulative GDP losses larger than four percent of GDP.

Table A1. Lagging crisis

<i>Sample</i> <i>Dependent</i>	All Area 1 1	All <i>Area 1a</i> 2	All <i>Area 1b</i> 3	All <i>Area 1c</i> 4	All <i>Area 1d</i> 5	All Area 5 6
			Full baseline included			
Government ideology	.533*** (.164)	-1.199* (.634)	-.763 (.558)	-6.344*** (1.861)	.636* (.326)	.260** (.126)
Lagged crises	.067 (.045)	-.341** (.172)	-.139 (.150)	-.339 (.496)	.152 (.082)	-.012 (.034)
Lagged crisis *	-.059 (.084)	-.116 (.325)	.205 (.288)	.649 (.946)	-.153 (.159)	.057 (.065)
Period FE	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	471	484	440	429	399	435
Countries	68	68	67	67	68	68
Within R squared	.432	.353	.276	.472	.592	.665
Between R squared	.312	.565	.602	.161	.132	.328
F statistic	16.27	12.08	7.52	17.10	25.28	38.46

Note: Standard errors in parentheses. *** (**) [*] denote significance at $p < .01$ ($p < .05$) [$p < .10$].

Table A2. Randomly assigned neighbour crises

<i>Sample Dependent</i>	All Area 1		All Area 5	
	Min. interaction	Max. interaction	Min. interaction	Max. interaction
Crises	-.012 (.039)	-.179*** (.054)	-.060* (.031)	-.017 (.029)
Crisis * ideology	-.123 (.079)	.250** (.112)	-.096 (.063)	.132* (.067)
<i>No. significant</i>				
Crises		20		44
Interaction		0		1
<i>No. correct sign</i>				
Crises		96		100
Interaction		57		55

Note: Standard errors in parentheses. *** (**) [*] denote significance at $p < .01$ ($p < .05$) [$p < .10$].

Table A3. Alternative crisis indicators

<i>Sample Dependent</i>	All Area 1 1	All Area 5 2	All Area 1 3	All Area 5 4	All Area 1 5	All Area 5 6
Full baseline included						
Government ideology	.565*** (.131)	.404*** (.098)	.477*** (.138)	.272*** (.099)	.466*** (.132)	.342*** (.097)
Banking crisis	-.166 (.133)	-.098 (.096)				
Banking * ideology	-.534* (.298)	-.399* (.217)				
Currency crisis			-.291** (.134)	-.500*** (.100)		
Currency * ideology			.051 (.265)	.426** (.194)		
Debt crisis					-.269 (.185)	-.308** (.144)
Debt * ideology					.033 (.319)	-.131 (.250)
Period FE	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	471	435	471	435	471	435
Countries	68	68	68	68	68	68
Within R squared	.442	.672	.436	.687	.432	.669
Between R squared	.508	.281	.295	.287	.340	.319
F statistic	16.94	39.77	16.53	42.51	16.25	39.13
<i>Crisis effect at</i>						
10 th percentile	.047 (.217)	.062 (.159)	-.311 (.196)	-.671*** (.146)	-.282 (.222)	-.256 (.176)
25 th percentile	-.166 (.133)	-.098 (.096)	-.291** (.134)	-.500*** (.100)	-.269 (.185)	-.308** (.144)
Median	-.311*** (.117)	-.206** (.082)	-.277** (.129)	-.385*** (.096)	-.259 (.207)	-.344** (.159)
75 th percentile	-.433*** (.143)	-.298*** (.101)	-.266* (.155)	-.287** (.113)	-.253 (.248)	-.374** (.191)
90 th percentile	-.473*** (.157)	-.328*** (.111)	-.261 (.167)	-.255** (.122)	-.250 (.265)	-.383* (.204)

Note: Standard errors in parentheses. *** (**) [*] denote significance at $p < .01$ ($p < .05$) [$p < .10$]. Marginal effects of interactions are calculated by the Delta method (Brambor et al., 2006). Hard crises are distinguished from light crises by having cumulative GDP losses larger than four percent of GDP.

Table A4. Accounting for early elections

<i>Sample</i>	All	All	No military	No military	No autocracies	No autocracies
<i>Dependent</i>	Area 1	Area 5	Area 1	Area 5	Area 1	Area 5
	1	2	3	4	5	6
Full baseline included						
Government ideology	.342* (.205)	.209 (.145)	.306 (.214)	.166 (.151)	.289 (.224)	.010 (.159)
Number of crises years	-.113** (.048)	-.085** (.037)	-.069 (.052)	-.076* (.039)	-.104* (.054)	-.118*** (.039)
Early elections	.439 (.499)	1.132*** (.372)	.698 (.569)	1.775*** (.447)	1.534* (.886)	2.309*** (.593)
Crisis * ideology	.102 (.092)	.147** (.066)	.108 (.097)	.192*** (.070)	.216** (.103)	.247*** (.074)
Early * ideology	-.041 (.378)	-.242 (.259)	.009 (.381)	-.187 (.261)	.046 (.379)	-.058 (.259)
Early * crisis	-.097 (.093)	-.033 (.065)	-.136 (.095)	-.043 (.067)	-.108 (.095)	-.013 (.066)
Early * crisis * ideology	-.057 (.207)	-.113 (.143)	-.078 (.208)	-.159 (.144)	-.195 (.208)	-.202 (.142)
Period FE	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	471	435	443	422	407	392
Countries	68	68	68	68	66	66
Within R squared	.453	.694	.449	.695	.473	.688
Between R squared	.338	.364	.532	.315	.116	.174
F statistic	14.35	35.52	13.77	36.06	15.23	35.70

Note: Standard errors in parentheses. *** (**) [*] denote significance at $p < .01$ ($p < .05$) [$p < .10$].

Table A5. Only stable democracies

<i>Sample</i>	All	All	All	All	All	All
<i>Dependent</i>	Area 1	Area 1a	Area 1b	Area 1c	Area 1d	Area 5
	1	2	3	4	5	6
			Full baseline included			
Government	.261	-.290	-.892	-7.696***	.499	.243*
ideology	(.216)	(.783)	(.967)	(2.748)	(.482)	(.144)
Lagged crises	-.237***	.750***	.489**	1.433***	-.023	-.122***
	(.053)	(.172)	(.210)	(.597)	(.109)	(.036)
Lagged crisis *	.191*	-.661*	-.651	.127	.022	.145**
ideology	(.110)	(.343)	(.437)	(1.265)	(.228)	(.073)
Period FE	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	318	329	302	276	273	311
Countries	48	48	48	47	48	48
Within R squared	.504	.514	.331	.534	.619	.710
Between R squared	.380	.536	.121	.062	.034	.134
F statistic	14.22	13.80	5.78	11.97	17.66	33.38

Note: Standard errors in parentheses. *** (**) [*] denote significance at $p < .01$ ($p < .05$) [$p < .10$].

Table A6. Determinants of monetary policy responses

<i>Sample Dependent</i>	All Area 3 1	No military Area 3 2	No autocracies Area 3 3	Only stable Area 3 4
Initial level	.394*** (.045)	.398*** (.046)	.410*** (.049)	.364*** (.056)
Log GDP per capita	.653 (.403)	.492 (.455)	.436 (.581)	2.141*** (.597)
Openness	-.009** (.004)	-.009** (.004)	-.009* (.005)	-.017*** (.005)
Log population size	-.778 (.747)	-.639 (.774)	-.683 (.891)	-1.403 (.909)
Presidential democracy	-.109 (.603)	.889 (.754)	-	-.913 (1.551)
Civilian autocracy	.472 (.509)	.978* (.559)	-	.804 (.742)
Military dictatorship	-.261 (.619)	-	-	-2.025*** (.773)
Coalition government	.455** (.211)	.526** (.212)	.435* (.224)	.501** (.254)
Government ideology	.147 (.287)	.173 (.292)	.282 (.317)	.192 (.337)
Number of crises years	-.314*** (.067)	-.292*** (.070)	-.268*** (.077)	-.057 (.082)
Crisis * ideology	.193 (.135)	.112 (.141)	.086 (.154)	.004 (.169)
Period FE	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes
Observations	486	457	417	329
Countries	68	68	66	48
Within R squared	.531	.525	.517	.534
Between R squared	.342	.357	.384	.129
F statistic	25.13	24.17	23.99	16.77
<i>Crisis effect at:</i>				
10 th percentile	-.391*** (.086)	-.337*** (.101)	-.303*** (.111)	-.058 (.121)
25 th percentile	-.314*** (.067)	-.292*** (.070)	-.268*** (.077)	-.057 (.082)
Median	-.262*** (.068)	-.261*** (.071)	-.245*** (.076)	-.056 (.081)
75 th percentile	-.218** (.082)	-.236*** (.086)	-.225** (.091)	-.055 (.098)
90 th percentile	-.203** (.089)	-.227** (.093)	-.218** (.099)	-.055 (.106)

Note: Standard errors in parentheses. *** (**) [*] denote significance at $p < .01$ ($p < .05$) [$p < .10$]. Marginal effects of interactions are calculated by the Delta method (Brambor et al., 2006).

Figure A1. Jackknife estimates, area 1a

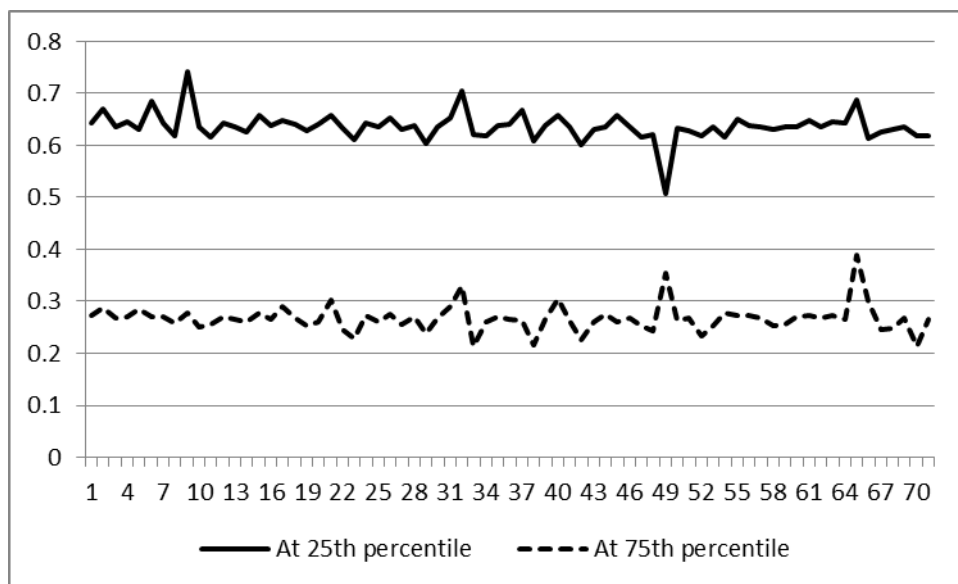


Figure A2. Jackknife estimates, area 1b

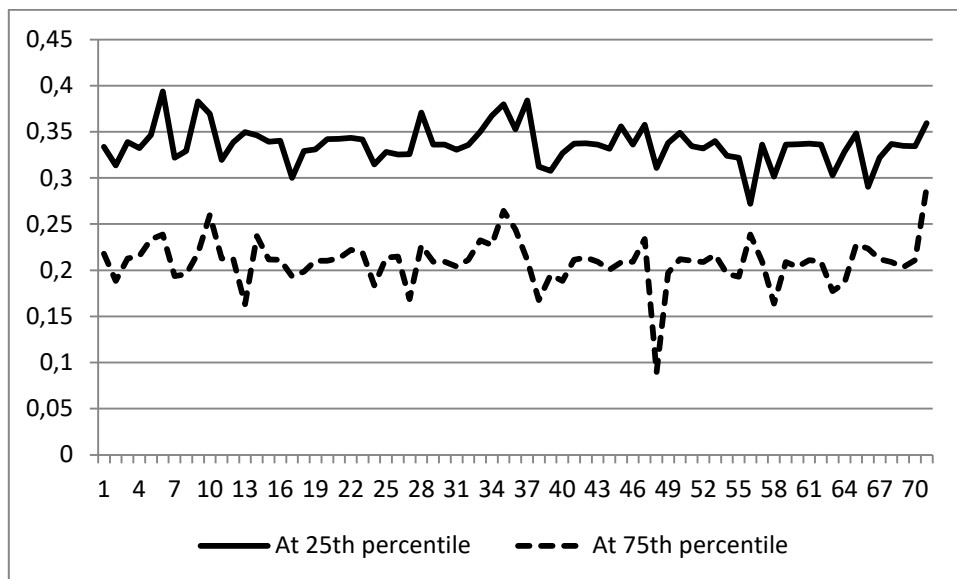


Figure A3. Jackknife estimates, area 5

