Voters' wrath? Policy change and government popularity

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Voters’ Wrath? Policy change and government popularity

Abstract

Recent research suggests that voters are bad at responding in a meaningful way to policy events when deciding for whom to vote. Voters rely on so-called ”blind retrospection,” punishing governments for events outside politicians’ control. However, another core aspect of the blind retrospection perspective has not been put to the test: are voters unable to respond to policy *decisions* that clearly are *under the politicians’ control*? We construct a unique large-N dataset on legislative changes in German old age pensions and unemployment protection to see if cutbacks and expansions lead to lower/higher support for the government. Our data are exceptionally fine-grained and allow us to track the policy-vote link for 416 months from 1977 to 2013 with a total of 329,167 respondents. We find substantial support for the notion that voters react in a meaningful way to policy changes, but also that they can be distracted by high-profile, extreme events.

Keywords: Blind retrospection; vote intention; policy reform; policy-vote link

1. Introduction

Do voters react to policy changes by rewarding or punishing the government in a meaningful way? This seemingly simple question has vexed political scientists for decades – and still is. On the one hand, at least since the seminal essay by Converse (1964) on the nature of belief systems in mass publics, an influential strand of research has argued that voters essentially are too uninformed to respond to what governments do. Vote choice can instead be driven by uncontrollable, but visible, events such as shark attacks, natural disasters, or sport results—all clearly outside the realm of government (Bartels and Achen, 2016, 116–135; Busby, Druckman, and Fredendall, 2016; Cole, Healy, and Werker, 2012; Healy, Malhotra, and Mo, 2010; Huber, Hill, and Lenz, 2012). This implies that voters fail to distinguish between events that governments can affect and events they cannot affect ("blind retrospection", Bartels and Achen 2016). On the other hand, however, empirical evidence also shows that governments tend to be punished for a bad economy (Duch and Stevenson, 2008; Lewis-Beck and Whitten, 2013), while the effects of extreme events such as shark attacks or sport results on voting have either been disputed (Fowler and Montagnes, 2015) or argued to be in line with a rational explanation that goes against the simple blind retrospection pessimism (see Ashworth, Buena de Mesquita, and Friedenberg, 2018).

In order to advance the debate, our paper sets out to test the policy-vote link in a most-likely design. That is, under circumstances where policy-makers are clearly responsible for the events, i.e. policy changes, to happen. Such a design adds to the existing literature on the policy-vote-link because the few studies that systematically investigate the policy-vote link either look at the effects of extreme events that are hard to generalize from or rely on indirect or unreliable measures of public policy such as public spending. Furthermore, existing work usually uses data aggregated across time (typically entire election periods) and place (typically countries or other geographical units).

Aggregation across entire election periods means that real effects risk being overlooked. The vote cast on Election Day is a function of a plethora of factors ranging from the state of the economy, length of incumbency, idiosyncratic events and, possibly, the mix between popular and unpopular policy decisions. Governments make hundreds of decisions across multiple policy domains. It is heroic to assume that we can isolate the effect of policy changes in any specific domain unless our time unit is a lot more decomposed than whole election cycles. Aggregation across geographical units bars us from investigating what voter segments respond to policies. Blind retrospection would expect—to the extent voters respond at all—that there will be no systematic tendency for the voters most affected by the policies to react the strongest.

A generalizable empirical proof of the policy-vote link is, in sum, very demanding. First, it requires systematic information about policy decisions on areas that we must expect the public to care about and, hence, where they would want to react to changes. Second, the number of policy decisions must be so high that it is possible to control for potential confounders and idiosyncrasies of individual policy events. Third, the test requires that it is possible to relate the policy decisions to fine-grained time series of opinion surveys in order to identify the effect of policy decisions on governments' popularity.

To move the debate forward, we integrate existing survey data from the German Politbarometer (Forschungsgruppe Wahlen, 2015) with a unique, new dataset on legislative policy decisions. The Politbarometer consists of monthly representative surveys all the way back to 1977, yielding a total of 416 survey-months and 329,167 respondents. For the policy dataset, we have coded all changes we could locate in German legislation regulating the generosity of old age pensions and unemployment protection, two highly salient areas for German voters. In total, we have coded 430 legislative changes from 1977 to 2013, allowing us to conduct a thorough test of the effect of policy decisions on vote intention. Accordingly, we can also explore whether voter reactions (measured as changing vote intention) are most intense among those likely to be affected by the policy and whether the policy-vote link is weaker when extreme events such as the fall of the Berlin wall or the 9/11 terrorist attacks are on the agenda.

The results clearly indicate that voters react to policy changes. On average, expanding generosity of old age pensions and unemployment protection leads to higher government popularity, while cutbacks have the opposite effect. We also find that reform of the highly popular old age pensions leads to stronger reactions than reforms of the more modestly popular unemployment protection. The effects are conditional on the immediate relevance of the programs for individual voters, as measured by their age (for pensions) and the unemployment rate (for unemployment protection). Voters that ought to care most about the changes are the ones that react the strongest. Adding nuance to these results, we show that voters indeed can be distracted if high-profile, extreme events top the agenda, thus leading to a weaker policy-vote link. Outside such periods, however, the policy-vote link is strong and substantial.

Our findings provide a caveat to the notion that voters are unable to play their part in democratic politics, at least in the limited sense that voters in fact respond to changes in policies as they should. Obviously, this does not mean that voters are not affected by events outside the realm of government responsibility, as we also show. However, it invites us to think about voters in a more elaborate, and perhaps positive, way than the concept of blind retrospection initially suggests.

2. Policy decisions and blind retrospection

Voters' limited knowledge about political facts is well-established (Carpini and Keeter, 1996; Galston, 2001), and the lack of sophistication arguably matters greatly for electoral outcomes. Voters find it hard to use relevant information when evaluating the performance of politicians. For instance, when reacting to the incumbent's economic record, voters only take the last 6–12 months into account, disregarding the much longer—and typically more important—period that went before in the electoral cycle (Bartels and Achen, 2016, 146–176; Healy and Lenz, 2014). Similarly, some scholars found random and uncontrollable events such as shark attacks, sport results, and natural disasters to affect vote choice (Bartels and Achen, 2016, 116–135; Busby, Druckman, and Fredendall, 2016; Cole, Healy, and Werker, 2012; Healy, Malhotra, and Mo, 2010).

Such extreme events are in all likelihood very important to voters, and the fact that voters respond is therefore comforting. In contrast, the fact that these policies are caused by highly visible but rare events also means that our ability to generalize from them is limited. Accordingly, later studies found that the effects of extreme events on voting were either not significant or systematic or that there were other, more rational explanations for the outcomes under review than shark attacks or sport results (see Ashworth, Buena de Mesquita, and Friedenberg, 2018; Fowler and Montagnes, 2015 or Fowler and Hall, 2018).

Still, the great majority of new policies are not spurred by this type of exogenous shocks but by factors endogenous to the political system. This is particularly the case on policy areas salient to many voters such as economic policy, health care, or social policy. It would indeed be troublesome if voters did not react to the government's policymaking on these specific policy areas.

We therefore argue that – even though public policies can be complex – voters should react in a meaningful and rational way to policy changes they are affected by if theories of democratic accountability hold true. Otherwise, the more pessimistic claim about blind retrospection would also hold for public policies that affect the citizens on a more regular base than extreme events such as natural disasters. In this scenario, the substantial representation of voters' preferences is in danger because voters are incapable of reacting to the past and visible actions of governments. Of course, we should not expect voters to respond to every single instance of policy change. The crucial question is whether voters respond to policy change they care about. So, for a sufficient policy-vote link in the democratic public, the following hypothesis must hold on a general base:

H1: Voters punish governments for actions that decrease their welfare and reward governments that increase their welfare.

When it comes to the timing of the voters' reaction, we draw upon findings from economic voting, which demonstrated a recency bias in vote choice (Nordhaus 1975). The recency bias, which means that the reward-punishment logics kick in shortly after a policy change, originates in the voters' cognitive and time constraints. Voters therefore rely on information that is easier to recall and gather and use the most recent economic events as a proxy for evaluating the incumbent government (e.g. Healy and Lenz 2014; MacKuen et al. 1992; Nordhaus 1975). Consequently, we expect:

H2: Voters react quickly to policy changes; the more time passes the weaker the reaction.

Since a lot of prominent work has studied natural disasters and terrorist attacks and how policy responses to these events led voters to either reward or punish the incumbent (Bechtel and Hainmuller, 2011; Gasper and Reeves, 2011; Healy and Malhotra, 2009), we also must incorporate the effects of extreme events into our argument that voters react at least to a sufficient degree rational to public policy decisions. In contrast to the simple blind retrospection argument, Ashworth, Buena de Mesquita, and Friedenberg, (2018) have recently argued that extreme events do not cause irrational reactions from the voters per se, but mute the voters' reactions to everyday politics as they rely on different and new information to judge the incumbents. In other words, the policy-vote link is weakened if an extreme event happens as voters judge the incumbents at least partly based on the perception of the extreme event and not on their political decisions made before. Hence, we hypothesize:

H3: The policy-vote link is weakened in presence of extreme events.

3. The issue

For a thorough test of the policy-vote link, we first need to select a policy area where we can expect most citizens to care about legislative developments. Social policy is an excellent policy area in this sense because it is typically a very important issue to many voters, including the German ones we study (Aardal and Wijnen, 2005). At the same time, it has a real impact on the living conditions of citizens and inequalities in society (Brady, 2009; Scruggs and Allan, 2006).

Social policy is an extremely broad collection of subfields, whose boundaries are not clearly defined. At the core, however, one finds programs that compensate citizens unable to earn an income on the labor market, above all else the old and the jobless. For the test of the policy-vote link, we therefore focus on legislation regulating old age pensions and unemployment protection programs. Although both tend to be important for many voters, the structure of public opinion is different across them. For one thing, recipients of old age pensions are normally regarded as much more deserving of society's help than the jobless (van Oorschot, 2006). The baseline reaction to policy changes should therefore also be different. For pensions, we might expect a strongly negative reaction among all segments of the public as a response to cutbacks, whereas cuts in unemployment protection should lead to more muted reactions. Conversely, voters across-the-board should generally welcome expansions in old age pension generosity, while expansions in unemployment protection should leave the public comparably unimpressed. Hence,

H4: Voters held incumbents more accountable for changes in the pension system than changes in the unemployment insurance.

The structure of public opinion is also different on the two programs because they are relevant to different people at different points in time. Old age pensions are by design aimed at the elderly. To the extent that people are particularly responsive to policy changes that may affect themselves directly, age should be an important conditional factor when it comes to pensions. The risk of unemployment fluctuates with the economy. In periods with low unemployment, the risk of losing ones job is correspondingly low, but it increases if the unemployment rate grows. To the extent that voters take their risk exposure into account when responding to changes in unemployment protection, we would expect the unemployment rate to condition the policy-vote link.

H5a: Older voters have the strongest reactions to policy changes on pensions.

H5b: The change of the unemployment rate affects the strength and direction of the policy-vote link for the unemployment protection.

4. The German case

We rely on German data to examine the policy-vote link. Germany is a well-established parliamentary democracy with a proportional representation electoral system akin to those found in many European, Asian, and Latin American countries. Governments virtually always have a majority in the lower house, the *Bundestag*, meaning that attribution of responsibility is comparably straightforward. However, at the same time, Germany's federal system includes a moderately strong upper house representing the regional governments, the *Bundesrat*. Taken together, the dual features of majority governments and a federal system entail that the ability of voters to place responsibility for policy decisions should be neither extremely good nor extremely bad. Indeed, on the so-called clarity of responsibility index, which is calculated by Hobolt, Tilley, and Banducci (2013) for 27 European countries and ranges from 0 to 1, Germany scores 0.50, which is very close to the mean of 0.58. In short, Germany is a typical example of a Western parliamentary democracy.

In Germany, both old age pensions and unemployment protection are mainly insurance-based with wage earners and their employers paying social security contributions to acquire rights to pensions and unemployment benefits (Hinrichs, 2010). Crucially, the government is setting the rules for all the key elements of the programs, including the eligibility period, retirement age, and benefit levels (we account for all elements in the next section). To all intents and purposes, German old age pensions and unemployment protection are public, and consequently citizens are directly affected by legislative changes. For this reason, too, Germany is a good case for studying the policy-vote link.

5. Data and methods

To examine the policy-vote link, we need data on policy decisions and voters' reaction. To get at the latter, we employ the pooled Politbarometer surveys 1977–2013 [ZA-NR 2391] (Forschungsgruppe Wahlen, 2015). The Politbarometer is collected for the public broadcaster ZDF since 1977 on a monthly basis and contains various items on party preferences, issue salience, and socio-demographic information. The Politbarometer are representative surveys with typical sample sizes of 1,000 to 1,200 respondents. The data are well suited for our purpose because we can inspect the voters' reaction on the micro level directly after one or several policy events happened. We include respondents from Eastern Germany after 1989. We merged our pooled and harmonized Politbarometer 1977–2013 with our policy reform data, yielding a total of 416 survey months were we have data for both our dependent variable and the policy changes (N = 329,167).

Our dependent variable measures whether respondents shifted from a government/opposition party based on the vote recall question for the last federal election and the vote intention for the next election. In this way, we can measure the electoral effects directly at the individual level.[1] Our dependent variable has four categories: "stayed with government parties," "defected from government to opposition," "stayed with opposition," "shifted from opposition to government." Accordingly, we create an exhaustive measure that captures all possible voter reactions: punishing the government after reforms, rewarding the government after reforms, or re-electing a government or opposition party. To focus on the theoretically most interesting results in the figures below, we only report the percentage of voters who switched from the government to the opposition, the "defectors," and voters who switched from the opposition to the government, the "rewarders." In this way, we capture the reactions of voters after policy events; e.g., whether the probability of defection systematically increases after cutbacks had been implemented. The full set of regression tables and the results for the categories "stayed with government parties" and "stayed with opposition" are found in the online appendix. The online appendix (part B) also contains various sensitivity tests with different specifications of the dependent variable given the limitations of the vote recall item. An important robustness check is, for instance, whether we measure support for the Chancellor's party or the junior coalition partner after reforms – given that many studies have shown that such "coalition-directed voting" is mainly concerning the Chancellor's party (Fortunato and Stevenson 2013). However, all these regressions yield similar conclusions when it comes to the policy vote link and the logic of rewarding and punishing.

A major hurdle when investigating the policy-vote link is the lack of a large-N dataset on legislative policy decisions. For the purpose of our study, we therefore collected information about all German old age pension and unemployment protection legislation from 1977 to 2013. We rely on Marshall's (1992 [1950], 8) classic concept of the social rights of citizens to define the type of legislation we are interested in. A social right is a legal entitlement to benefits or services improving the material living conditions of the recipient. It is their social rights that concern citizens when it comes to old age pensions and unemployment protection: "how much and under what condition can I get my benefits?" We do not code changes in the administrative organization of pensions and unemployment protection as such changes have no direct effect on the social rights of citizens.

For the dataset, we first identified the universe of welfare reforms in Germany collecting as many secondary sources as possible and supplementing these with searches in the legislative database of the *Bundestag* as well as in regular reports by Ministries or other bodies (in the case of Germany, for instance, the Ministry of Social affairs publishes a report of changes in social policies ('Sozialbericht') on a regular basis). In a second step, we then analyzed these reforms and coded what kind of policy instrument was changed relating to the 13 aspects of citizens' social rights listed in Table 1, with numbers 1–6 reflecting rules of access, numbers 7–10 reflecting rules of generosity, and numbers 11–13 reflecting rules of recipient conduct. For each of the 13 dimensions, we identified whether the legislation implied a reduction or an expansion in citizens' social rights. For instance, in July 1996, the German parliament passed a law stipulating that if an unemployed person declines a job offer, benefits will be cut by 25 percent. This is coded as a reduction in the social rights relating to employability (no. 11). In December 1984, it was decided that unemployed persons older than 49 years in the future could receive benefits for 18 months rather than the previous 12 months. This is coded as an expansion of the social rights relating to duration (no. 7). All coding was done by a team of trained, native-speaking research assistants. A native-speaking senior researcher subsequently controlled all coding decisions. In the event the senior researcher did not agree with the original coding, the relevant research assistant and the senior researcher discussed the coding decision in detail to reach agreement; however, there were very few such instances. Accordingly, our independent variables are the number of old age pension and unemployment protection changes (cutbacks or expansions) in the month before the respective Politbarometer was collected (see Table A1 in the online appendix for descriptive statistics and BLINDED FOR REVIEW for more details).

Table 1. Policies coded

Dimension	No.	Policy instrument	Description
Rules of access	1	Qualification period	How long does it take for a person to become eligible?
	2	Contribution period	How long must a person contribute to a scheme before becoming eligible?
	3	Contribution level	How much must a person contribute?
	4	Waiting period	How long does it take after a social risk occurs before a person is eligible?
	5	Age brackets	How old must a person be to be eligible?
	6	Means test	Is there a means test?
Rules of benefits	7	Duration period	How long can a recipient receive benefits?
	8	Nominal value	What is the nominal value of the benefits?
	9	Indexation rule	Is the nominal benefit automatically regulated and with what factor?
	10	Assessment base	Has the base for calculating benefits changed?
Rules of conduct	11	Employability	Is the recipient required to or offered the opportunity to voluntarily participate in activities meant to increase the likelihood of getting a job?
	12	Health documentation	Is the recipient required to document that she is unable to work?
	13	Residence	Does it matter where and under which circumstances the recipient lives?

Table 2. Distribution of months with/without cutbacks and expansions

Panel A: Old age pension						
		Expansions				
		0	**1**	**2**	**3+**	**Total**
Cutbacks	**0**	399	6	3	5	413
	1	16	3	3	1	23
	2	3	0	0	1	4
	3+	2	0	0	2	4
	Total	420	9	6	6	444
Panel B: Unemployment protection						
		Expansions				
		0	**1**	**2**	**3+**	**Total**
Cutbacks	**0**	384	9	1	7	401
	1	18	5	1	0	24
	2	3	4	0	4	11
	3+	3	3	0	0	6
	Total	408	21	2	11	442

Source: Own coding of policy changes in Germany in 1977–2013. See main text for detailed description.

Table 2 displays the distribution of cutbacks and expansions for old age pensions (Panel A) and unemployment protection (Panel B), respectively. As expected, most months see no reform activity. Still, over the 37 years covered by our data, 45 and 58 months witnessed legislative changes of old age pensions and unemployment protection, respectively. In most instances, a cutback or expansion occurs as a single event, but sometimes multiple changes happen in the same month—and sometimes both cutbacks and expansions take place simultaneously. Below, when we estimate the effect of cutbacks, we set the value of expansions in the same month at zero, and vice versa for expansions. However, as an alternative to account for the possibility that both cutbacks and expansions can happen in the same month, we have also constructed a measure of the "net balance" of reforms by subtracting expansions in a given month from the cutbacks. This sensitivity test yields substantially similar results to those reported below and appears in the online appendix.

To account for extreme events that could distract the voters, we create an "extreme-event dummy." The dummy takes the value "1" if a voter reported that an uncontrollable and high-profile event (e.g., the fall of the Berlin Wall in 1989/1990 or the Fukushima Daiichi nuclear disaster in 2011) was among the two most important issues facing German society today (see Table A2 in the online appendix for a full list of the events). Later, we interact this extreme-event dummy with our policy change variables to see whether voters react differently when one of these extreme events are salient.

We control for a variety of socio-economic background variables: social class, age group, education, occupational status, sex, union membership, and religion. We would have preferred to explore the role of the respondents' ideological orientation as well, but Politbarometer unfortunately does not contain a consistently phrased question that allows us to do so.[2] At the macro level, we include controls for change in the unemployment rate in the month before the survey was collected and the change in the GDP in the quarter before. We use quarterly data because there is no monthly data for GDP growth that covers the whole period. To control for the composition of the government, we include dummies for the four government coalitions of Germany during the period (CDU/CSU/FDP, CDU/CSU/SPD, SPD/FDP, and SPD/Greens). We have tested whether the effects reported below vary by government and whether the main results change if we allow the control variables to vary by government constellation (see Figure B3 in the online appendix). We further inspected whether a government's reform activity is predicted by changes in its popularity to rule out endogeneity problems. This is not the case (see Tables B3-B5 in the online appendix). All robustness checks and sensitivity tests appear in the online appendix, part B. Finally, we control for the cost of ruling. The cost of ruling variable counts the months passed after the last federal election for every sitting government in a given survey and goes from 0 months to 48 months, the legal maximum length of an election term in Germany. An alternative measurement capturing each government's time in office yielded similar results. The exact construction of all variables is explained in the online appendix. We use multinomial logit models since our dependent variable is categorical, and we use robust standard errors since not every variable fulfils the assumption of homoscedasticity.

6. Empirical Analysis

6.1. Exploring the policy-vote link

To get a first and purely descriptive impression of the correlational structure of the data, Figure 1 displays the vote share of governments in months after policy change has occurred and compares it with the months with no preceding events (marked as the dashed line at 42.5 percent of the vote share). The vote share in months after cutbacks is marked as the light grey bars, and the vote share in months after expansions is marked as the dark grey bars.

The results are telling. Looking first at old age pensions in the left-hand side, it transpires that governments' vote share on average is 39.3 percent in the month after old age pension cutbacks, or 3.2 percentage points below the average of months without a preceding reform event (the dashed line). A t-test indicates that the difference is statistically significant ($p < 0.05$). Turning to the vote share in months after expansions, we see an average vote share of 43.6 percent, i.e., 1.1 percentage points above the baseline or 4.3 percentage points above months that follow cutbacks. Both of these differences are statistically significant ($p < 0.05$). The same basic patterns appear when looking at unemployment protection, although the variation is a little more muted. Governments' vote share in the month after cutbacks is normally 40.2 percent, whereas it is 44.1 percent in expansion months. Again, the differences are statistically significant ($p < 0.05$). This yields some first support for Hypothesis 1 that voters react in a meaningful way to policy changes. It is further noteworthy that the apparent decline in vote share following cuts is substantially bigger than the increase after expansions. This is the first of several instances in our results where voters' tendency to punish governments harder for cutbacks than for expansions is visible, essentially corroborating the theory of negativity bias where people care more about what is done against them than what is done for them (Tversky and Kahneman, 1991).

Figure 1. Vote share of governments in the month after cutbacks or expansions

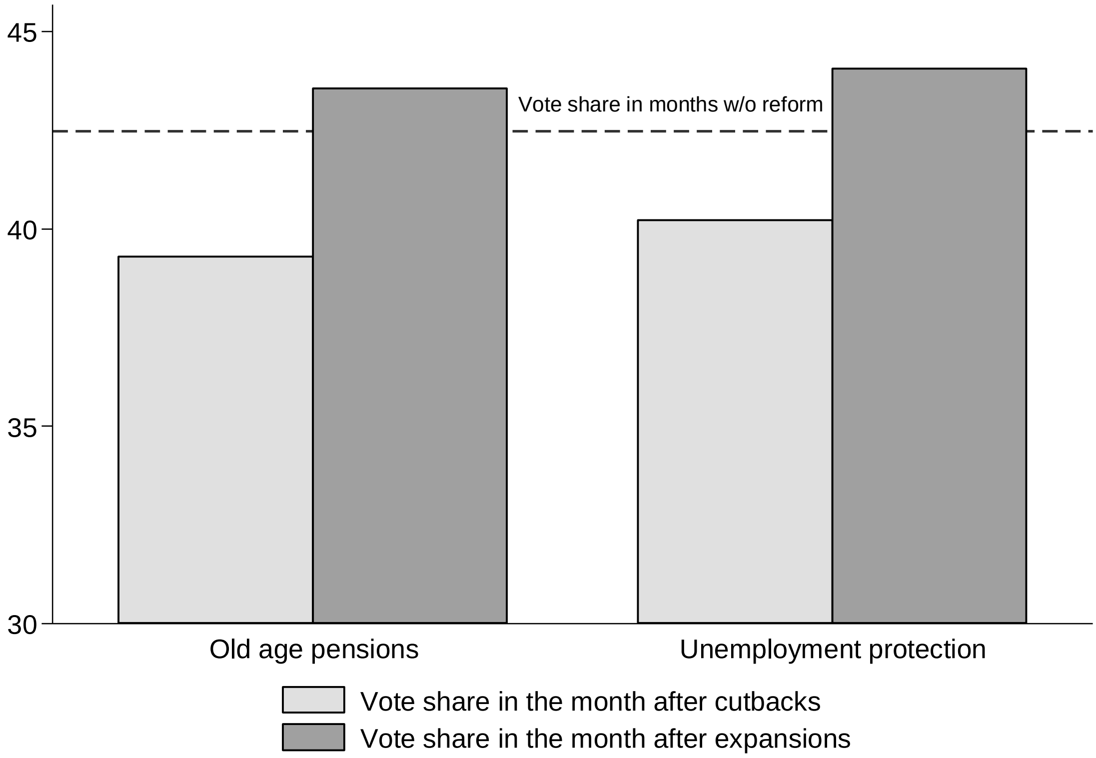


Figure 1 indicates that policy changes seem to affect vote choice. Yet the results in Figure 1 are descriptive, and we therefore turn to the multinomial regressions to see if the findings stand up to scrutiny. Table 3a and 3b yields the regression coefficients for various model specifications testing the hypotheses on the policy-vote link stated above. The coefficients in Table 3a and 3b largely confirm our hypotheses as voters first defect from the incumbents after cutbacks but do reward governments for expansions in all baseline specifications (H1). Second, we observe stronger effects of policy changes in the absence of extreme events (the constitutive term for expansions and cutbacks in the model containing the interaction *reforms*special event*) which confirms H3. Third we observe stronger and more significant policy-vote links for pensions than for unemployment protection (H4) when comparing the effect sizes for pensions in Table 3a with those for unemployment insurance in Table 3b. The dummy specification of the dependent variable in the very right-hand column shows that voters have a stronger preference for the opposition after a cut and a higher for the government after an expansion.

Table 3a. Effects of pensions reforms on defecting from or rewarding government

Specification	Baseline without controls	Baseline with macro variables as controls	Baseline with all controls	With interaction reforms*age cohort	With interaction reforms*special event	Baseline with all controls and dummy speci-fication of dep-endent variable
			Probability of defection			
Number of expansions	-0.214*** (0.013)	-0.253*** (0.014)	-0.258*** (0.015)	-0.210*** (0.041)	-0.267*** (0.016)	-0.125*** (0.008)
Expansions*30-44				0.013 (0.021)		
Expansions*45-59				0.007 (0.022)		
Expansions*60+				-0.221*** (0.029)		
Single event*expansion					0.137** (0.047)	
Number of cutbacks	0.201*** (0.008)	0.218*** (0.008)	0.222*** (0.009)	0.230*** (0.024)	0.228*** (0.009)	0.108*** (0.006)
Cuts*30-44				-0.037 (0.029)		
Cuts*45-59				-0.019 (0.029)		
Cuts*60+				0.035 (0.029)		
Single Event*cut					-0.192** (0.056)	
			Probability of rewarding			
Number of expansions	0.083** (0.025)	0.022 (0.026)	0.028 (0.029)	-0.103 (0.091)	0.019 (0.032)	–
Expansions*30-44				0.120 (0.102)		
Expansions*45-59				0.132 (0.106)		
Expansions*60+				0.206! (0.108)		
Single event*expansion					0.063 (0.073)	
Number of cutbacks	0.003 (0.021)	-0.001 (0.022)	0.006 (0.024)	-0.008 (0.068)	0.015 (0.025)	–
Cuts*30-44				0.022 (0.078)		
Cuts*45-59				0.017 (0.080)		
Cuts*60+				0.008 (0.083)		
Single Event*cut					-0.132 (0.102)	
N	382,648	382,648	329,167	329,167	329,167	

Note: The entries are logit coefficients from multinomial logistic regression of government support with staying with the government as reference category. Category staying with opposition left out for reasons of space. Source: Politbarometer 1977-2013 and The Welfare State Reform Dataset; *** p<.001, ** p<.01, p<.05, ! p<.10.

Table 3b. Effects of labour market reforms on defecting from or rewarding government

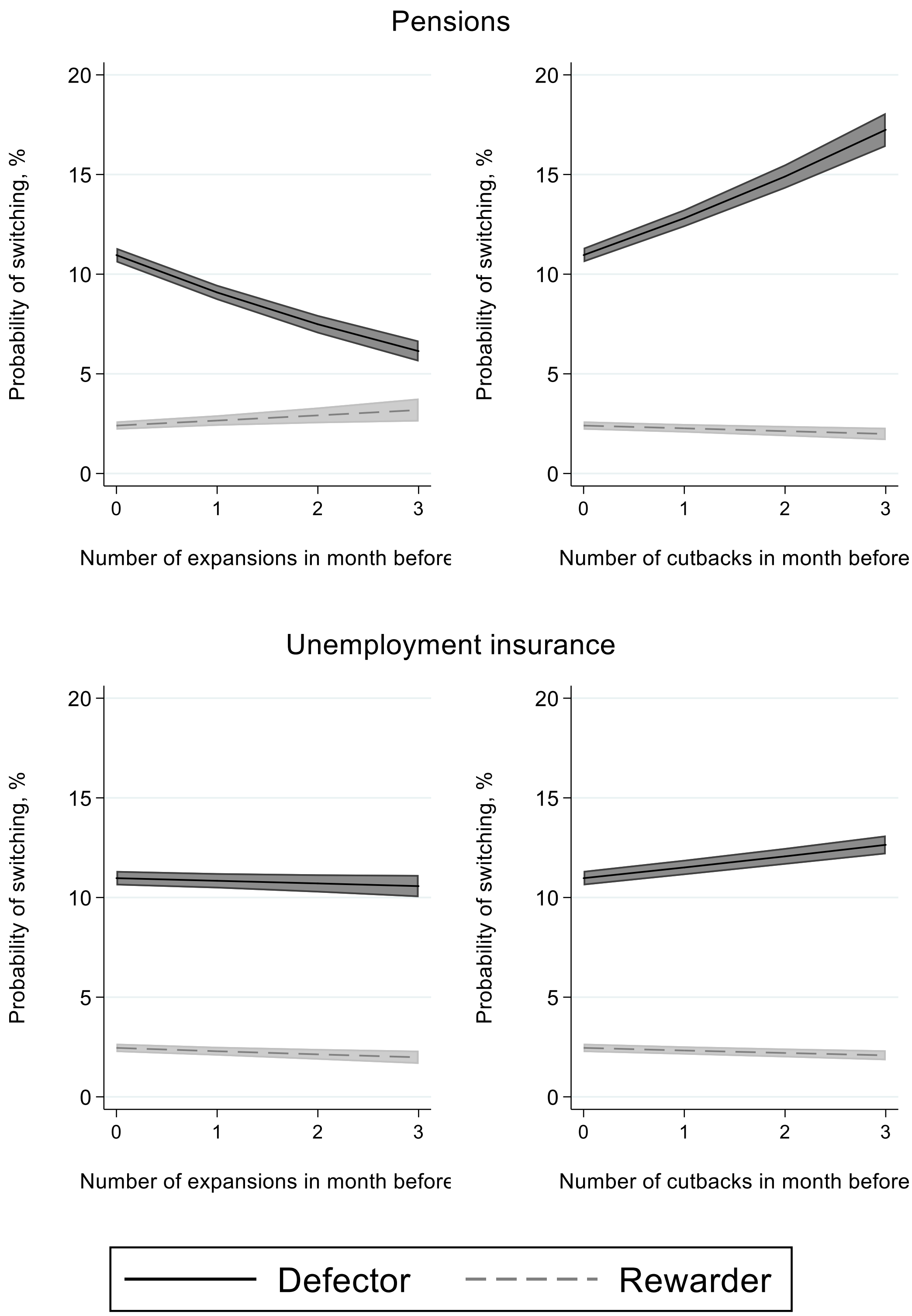
Specification	Baseline without controls	Baseline with macro variables as controls	Baseline with all controls	With interaction reforms*Δ unemployment rate month before	With interaction reforms*special event	Baseline with all controls and dummy specification of dependent variable
			Probability of defection			
Number of expansions	-0.014! (0.007)	-0.032*** (0.008)	-0.028** (0.009	0.026* (0.013)	-0.025** (0.009)	-0.022*** (0.006)
Expansions*Δ unemployment rate month				-0.104*** (0.016)		
Single event*expansion					-0.139* (0.064)	
Number of cutbacks	0.054*** (0.004)	0.062*** (0.004)	0.062*** (0.004)	0.030** (0.011)	0.062*** (0.004)	0.028*** (0.003)
Cuts*Δ unemployment rate month				0.038** (0.013)		
Single Event*cut					0.016 (0.047)	
			Probability of rewarding			
Number of expansions	-0.029 (0.021)	-0.096*** (0.024)	-0.087** (0.026)	-0.017 (0.033)	-0.101*** (0.027)	–
Expansions*Δ unemployment rate month				-0.172** (0.053)		
Single event*expansion					0.233* (0.108)	
Number of cutbacks	-0.033* (0.014)	-0.042** (0.015)	-0.040* (0.016)	-0.035 (0.028)	-0.039* (0.016)	–
Cuts*Δ unemployment rate month				-0.012 (0.032)		
Single Event*cut					-0.095 (0.244)	
N	382,648	382,648	329,167		329,167	377,595

Note: The entries are logit coefficients from multinomial logistic regression of government support with staying with the government as reference category. Category staying with opposition left out for reasons of space. Source: Politbarometer 1977-2013 and The Welfare State Reform Dataset; *** p<.001, ** p<.01, p<.05, ! p<.10.

Given the challenge of interpreting simultaneous interaction effects in multinomial logistic regressions as necessary for testing Hypotheses 5a and 5b and to compare logits across different models, we provide a more detailed interpretation of the results based on visualized predicted probabilities and marginal effects in what follows.[3] This includes a thorough analysis of the voters' reaction time to policy-change, which could not be presented here for reasons of space.

Figure 2 displays the results for old age pensions and unemployment protection, respectively (with the full regression tables for Model 1 and 2 in the online appendix). The horizontal axis in the figure goes from the natural minimum of policy changes (zero) to three changes in the month before.

Figure 2. Probability of switching after pension and unemployment insurance reforms



Source: Predicted probabilities derived from Models 1–2 in online appendix.

The upper left-hand side of Figure 2 shows how German governments, on average, have been rewarded for expanding old age pensions. The likelihood of voters to defect from the government decreases with the number of expansions. The decreasing probability is statistically significant after a single expansion (with $p < 0.05$). Moreover, some voters even switch from the opposition to the government, although the change in probability is only statistically significant after four expansions ($p < 0.05$). Since very few reforms contain four expansions, it is safest to conclude that rewarding is at best a marginal phenomenon, at least when we study these average effects. We find the reverse pattern for cutbacks in the upper right-hand side of Figure 2 with the increased probability of defection statistically significant after one cutback ($p < 0.05$). Similarly, the share of rewarders decreases after cutbacks, although the decline is only significant after five or more cutbacks ($p < 0.05$). Overall, the effect of policy changes confirms our first hypothesis that voters react in meaningful way to policy changes and this mainly in terms of affecting defection. We further run tests with a disaggregation of the Chancellor's Party and the junior partner. These show that the Chancellor's Party is held accountable for policy changes to a stronger degree than the junior partner (cf. Table B9 and B10 in the appendix).

Moving to unemployment protection in the lower panel in Figure 2 reveals a similar pattern for cutbacks as the one we found for pensions, but a different one for expansions. In contrast to pension expansions, there is no significant decrease of defections if governments make the unemployment insurance more generous. Moreover, the share of rewarders is falling slightly across the observed number of expansions, although the difference in probability is only significant after four expansions ($p < 0.05$). This hints that some voters are dissatisfied with expansions of unemployment protection; something we return to later. It would seem that credit claiming is easier when it comes to old age pensions compared to unemployment protection.

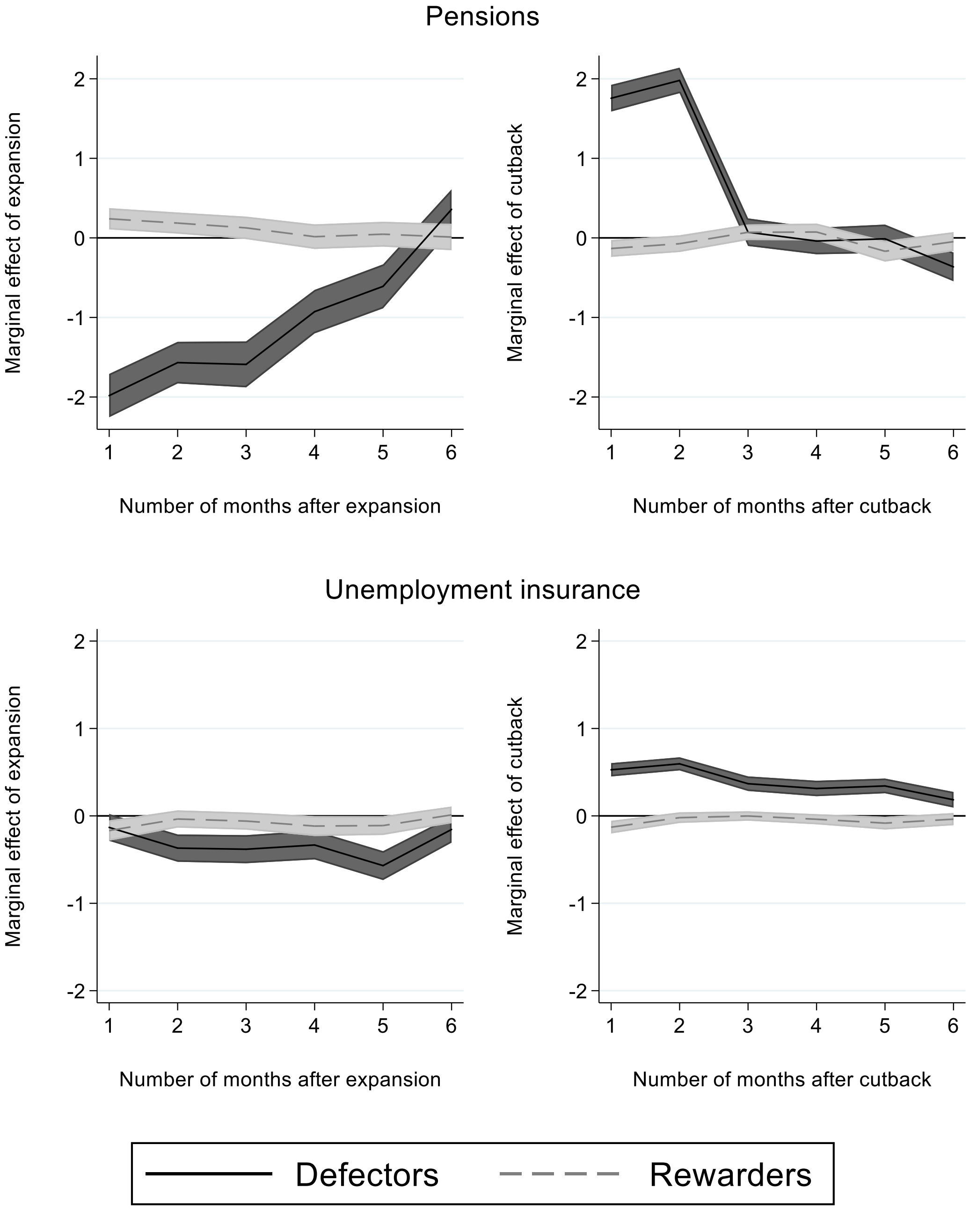
In the lower right-hand side of Figure 2, we see that cutbacks increase the likelihood of defection; the change in probability is significant after two cutbacks. The probability of rewarding also declines a bit, but the drop is only significant after five cutbacks. When we compare pensions (upper half) and unemployment insurance (lower half), it is evident that the government faces a stronger backlash when cutting pensions than unemployment protection. This supports our hypothesis that voters held incumbents more accountable for changes in the pension system than changes in the unemployment insurance (H4). For instance, the probability of defection increases with around 1.8 percentage points with a single pension cutback, but only by 0.5 percentage points with a cut in unemployment protection (the difference between these two marginal effects are statistically significant with $p < 0.05$).[4]

6.2. How quickly do voters react?

In our main models, we employ a one-month lag, assuming that a reform in a given month leads to a change in a government's vote share in the month after. The results so far clearly indicate that many voters indeed react in the first month. Yet other voters may continue to defect or reward the government in the months that follow. To capture the effect of policy changes on voters' reactions over time, we present the marginal effect of a policy change on defecting or rewarding over a period of six months for both old age pensions and unemployment insurance. Figure 3 presents the marginal effects of expansions and cutbacks on vote share when gradually increasing the lag from one to six months (reported on the horizontal axis).

Figure 3 reveals that most voters react immediately to changes in pensions, which supports Hypothesis 2. Accordingly, the biggest marginal effects are located in the first and second month after the policy changes passed. Afterwards, the effects become insignificant except for defection after an expansion, which diminishes but remains significant until the sixth month, A similar pattern exists for unemployment protection, as evident from Figure 3. Rewarding is only significant in the first month after the policy change for both expansions and cutbacks. The effect of a cutback on defection stays significant, but reduces substantially with time, and it does so from a low starting point compared with the marginal effects for old age pensions. Generally, Figure 3 suggests that voters respond to policy changes within the first month or two and that very few react after half a year. [5] This supports our second hypothesis that voters react immediately to policy changes.

Figure 3. Marginal effects of policy change on vote with different lags



Source: Marginal effects derived from Models reported in Table A9 and A10 in online appendix.

6.3. Variation in the policy-vote link by age and unemployment rate

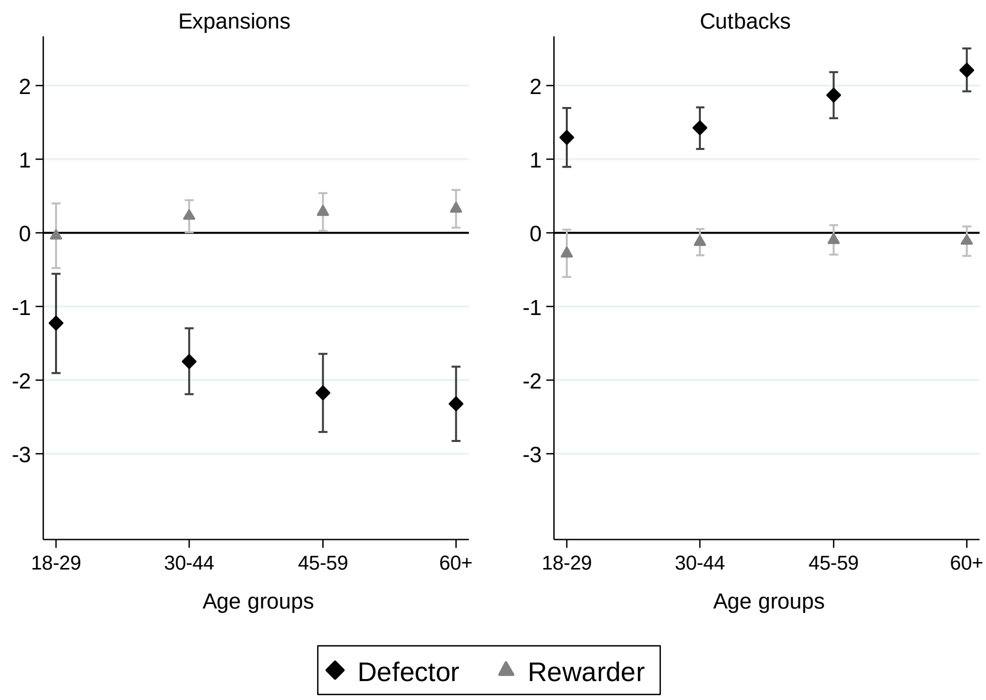
We have already shown that voters in general react to policy changes by rewarding governments for expansions and punishing them for cutbacks. In addition, we have seen that the effects are strongest for old age pensions (reflecting its popularity with voters) and for cutbacks (reflecting voters' negativity bias). Now, we want to test our two hypotheses that voters react stronger when they are exposed to the risks that the programs are meant to protect them against. To reiterate, we expect that:

H5a: Older voters have the strongest reactions to policy changes on pensions

H5b: The change of the unemployment rate affects the strength and direction of the policy-vote link for the unemployment protection

To test H5a and H5b, we interact old age pension changes with the age group variable from the Politbarometer and interact unemployment protection changes with the change in the unemployment rate in the month before. To facilitate interpretation, we show the marginal effects of a single policy change on the probability of becoming a defector or rewarder (the full regression tables of Models 3 and 4 are reported in the online appendix).

Figure 4. Marginal effect of policy changes on pensions on switching across age groups



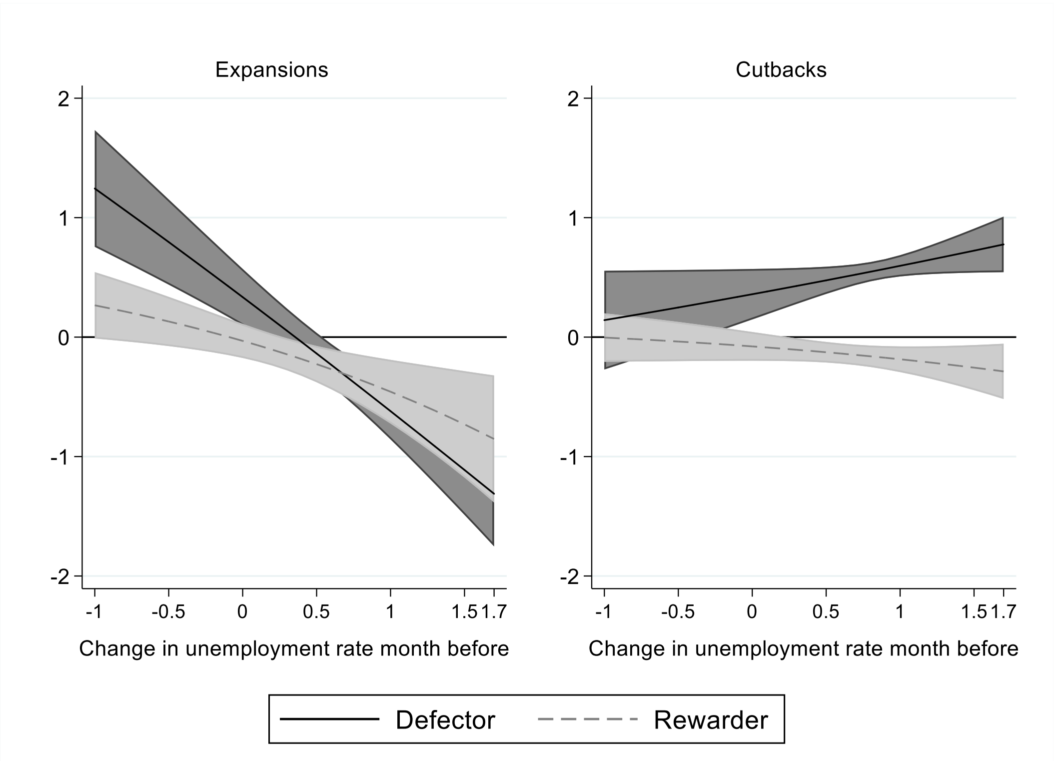
Source: Model 3 in online appendix.

The left-hand side of Figure 4 demonstrates how the effects of a single pension expansion on government support is conditioned by age group. With an expansion of pensions, the risk of defection decreases with rising age. The risk of becoming a defector after an expansion is reduced by 2.3 percentage points among voters who are 60 years and older compared to young voters where the risk only decreases by 1.2 percentage points. This is mirrored for the rewarders. The marginal effects of becoming a rewarder is also significantly different from zero in all age groups except the group 18–29. This underpins our Hypothesis H5a that older voters have the strongest reaction to changes on pensions. This also suggests that governments can secure the loyalty of older voters through expansions and attract some voters who used to vote for the opposition.

When looking at the effect of cutbacks on the right-hand side of Figure 4, we see a similar picture in favour of H5a. The effect of a cutback on the risk of defection increases with age. Voters aged 60 and older punish harder than voters in the 18–29 and 30–44 age groups. The risk that older voters defect as a consequence of a cutback is more than two percentage points, whereas it is around 1.3 percentage points for the younger voters under 45 years. Interestingly, no age group has a significant negative marginal effect of rewarding. In short, governments can win votes from the elderly by expanding generosity, while cutbacks appeal to no one. Once again, this is an indication of voters' negativity bias.

Turning to unemployment protection, Figure 5 presents the marginal effect of changes on unemployment protection across the observed range of monthly unemployment rate changes. Doing so reveals a much clearer pattern compared to our baseline model presented above. The probability of defection after an expansion is highest if the unemployment rate has dropped in the previous month. Conversely, an increase in the unemployment rate of 0.5 percent and more is associated with a reduced probability of defection. Assuming that the unemployment rates capture at least part of the voters' risk exposure, this makes sense: expansions make people stick with the government when risks are high, whereas expansions are regarded as unnecessary when risks are low. This supports Hypothesis 5b that public support behind unemployment protection is conditioned by the unemployment rate. The conditionality of public support is further evident from the fact that the probability of rewarding the government declines when unemployment increases. In other words, while one group of voters become more inclined to remain with the government, another group of voters become more likely to stay away from it. Expanding unemployment protection clearly creates heterogeneous reactions in the electorate depending the economic situation.

Figure 5. Marginal effect of changes in unemployment protection on government support over employment



Source: Model 4 in online appendix.

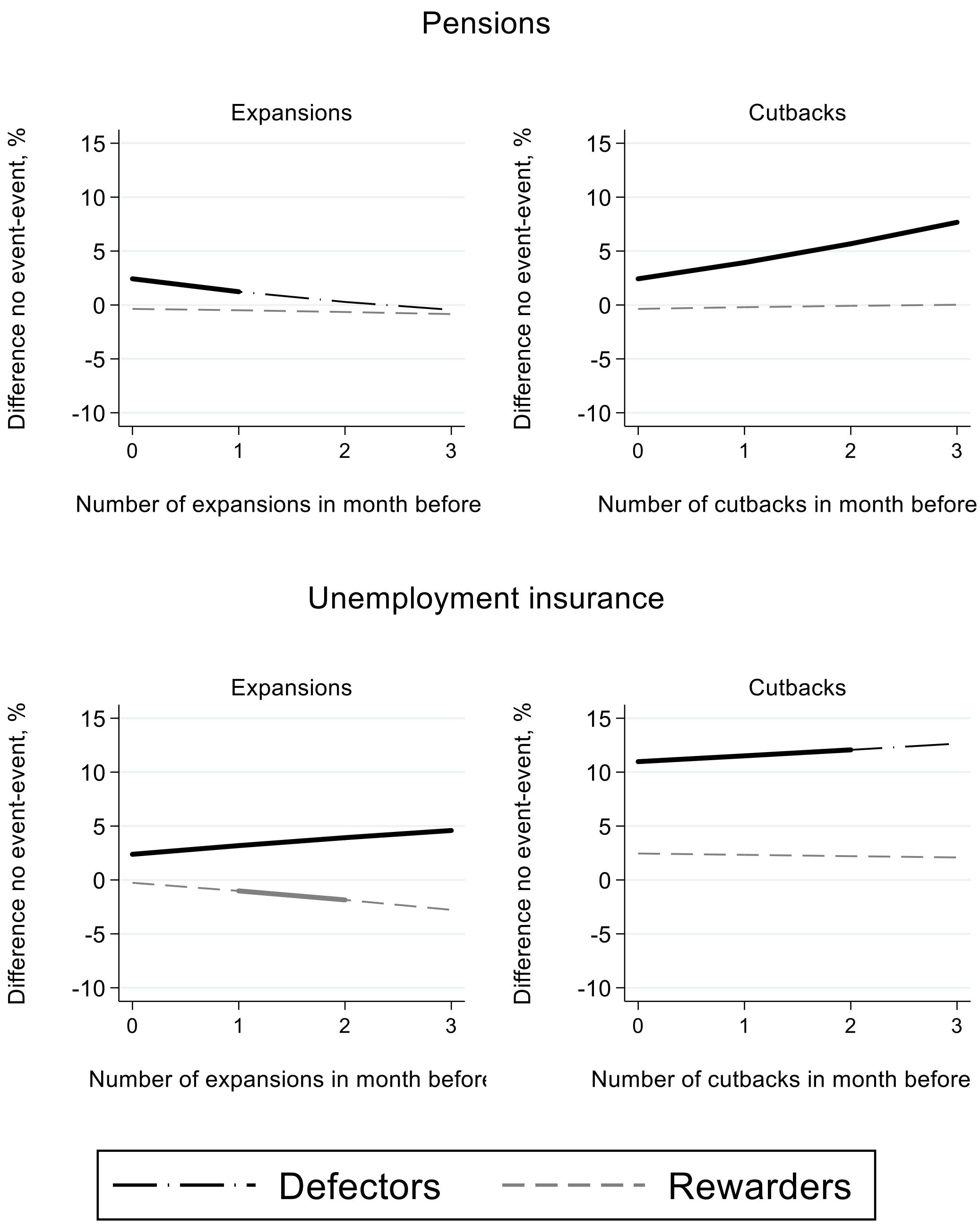
When it comes to cutbacks, voters react much more uniformly as shown in the right-hand side of Figure 5. There are no significant differences between rewarders and defectors as long as the unemployment rate does not increase. As the unemployment rate starts to grow, the effect of cutbacks on both defection and rewarding becomes significant with probability of defection increasing and rewarding decreasing.[6]

In sum, Figure 5 suggests relatively systematic but conditional voter reactions in line with Hypothesis 5b if we take the risk of becoming unemployed into account. When the unemployment rate is decreasing, voters punish for expansions and endorse cutbacks. This makes sense if we assume that voters feel less need for unemployment protection (and, by extention, cares more about not wasting tax money). Similarly, cutbacks are punished when voters are confronted with rising unemployment. Voters seem to consider their own risk environment when deciding whether to vote for the government or not.

6.4. The effect of extreme events on the policy-vote link

Our last step is to explore if voters react differently when extreme events are salient. To reiterate, we expected that the policy-vote link is weakened in presence of extreme events (H3). We investigate this expected relationship by interacting our extreme-event dummy with the policy change variables. The results are presented as the difference in the probability of being a defector or a rewarder when an extreme event is salient relative to periods where no extreme event is salient (i.e., not among the two most important issues the government should deal with according to the respective voter). In Figure 6, we indicate statistically significant differences by thick and solid lines and insignificant differences by thin and dashed lines. A significant effect indicates that voters reacted differently when an extreme event was salient.

Figure 6. Different voter reactions in months with/without extreme events for pensions and unemployment insurance



Source: Models 5 and 6 in online appendix. Note: The curves show the probability of defection/rewarding if no single event occurred minus the probability of defection/rewarding if a single event occurred in percent. Thick and solid lines indicate significant differences between the two conditions (at $p < 0.05$), and dashed lines indicate no significant differences.

We begin with the effect of extreme events on voters' reactions towards old age pensions reforms. Here, in the upper left-hand side of Figure 6, we observe that the likelihood of defection is slightly higher without an extreme event in a situation with a single expansion. The difference disappears with more expansions. However, many reforms only contain a single expansion, so this is not an irrelevant finding. This impression is reinforced when we look at cutbacks in the upper right-hand side of Figure 6. While an extreme event does not change the probability of becoming a rewarder, it reduces the probability of becoming a defector substantially. This suggests that governments can cushion the negative effects of pension cutbacks if voters are preoccupied with a natural disaster, international crisis, or the like. Hence, for pensions, we can confirm our third hypothesis that the policy-vote link is weakened if extreme events occupy the voters' attention.

The lower part of Figure 6 repeats the analysis for unemployment benefits. When looking at the lower left-hand side, it becomes clear that the probability of defection is significantly higher if no extreme event attracted the voters' attention. Over the range of expansions, the probability of defection is three to seven percentage points higher in the context of extreme events. Likewise, the probability of rewarding for expansions is lower, but the effect is only significant across a smaller part of the observed range of expansions. The lower right-hand side of Figure 6 shows the probabilities of defection and rewarding following cutbacks. There is a significantly reduced effect for defection under extreme events, but only for a low number of cuts. We might speculate that as the number of cutbacks increases, the attention of voters is drawn back to the issue in any case. Summing up, we find support for Hypothesis 3 that the policy-vote link is weakened in presence of extreme events as voters apparently base their evaluation of the incumbents partly on the extreme events. In other words, when extreme events are salient, voters tend to react less strongly to policy changes on pensions or the unemployment insurance than when they are not salient. This implies that voters most of the time defy the pessimistic view of blind retrospection and react in a meaningful way to policy changes if not occupied by natural disasters or the like. Still, it may be that politicians can strategically engineer extreme events to occur simultaneously with the introduction of unpopular reforms, in which case the fact that there are so few matters less. We cannot tell if such engineer occurs, but we see from the results that it clearly does not crowd-out the main effect of voters reacting to policy changes.

7. Conclusion

For representative democracy to work, politicians need to be held accountable for their decisions. Without such accountability, politics is likely to fall prey to special interests and corruption. This is why the voters' ability to react has received such wide scrutiny by political scientists for the past decades. Our findings add to this debate by showing that voters on average actually do appear to notice policy changes, at least on areas that we know most voters care about. We did not find support for the notion of blind retrospection that voters are unable to react meaningful to policy decision as they lack information or react to extreme events. This is good news for anybody concerned with the state of modern democracy.

Generally, cutbacks created stronger effects than expansions, which fits well with the notions of negativity bias and loss aversion in decision-making processes (Tversky and Kahneman, 1991). Although negativity bias may be suboptimal from a normative standpoint—as politicians refrain from necessary but unpopular actions—the fact that citizens appear to respond to reforms according to their innate dispositions is, in a narrow sense, comforting. Our findings suggest that voters make some meaningful decisions when confronted with visible changes of public policies such as cutbacks in the generosity of unemployment protection, which in our view go beyond blind retrospection.

Our conclusions are based on data from a single country and pertaining to a single policy issue, namely social policy. Still, we believe that our findings have relevance for a broader set of countries and issues. Germany is a typical example of a Western parliamentary democracy, as we argued above, and there is no reason to suspect that this country's political system should be particularly likely to host the effects we have documented. The social policy issue is one of the most salient and visible issues in Western democracies and therefore a most likely case for finding rational reactions from voters against the notion of blind retrospection. In this respect, we do expect that similar patterns will occur for other, highly visible issues such as immigration policy, health care or international conflict, on which most voters have clearly distinguishable preferences and so do parties and governments. On the other hand, there might be policy areas where governments have more leeway in legislating without facing strong reactions from voters as the issues are too complex or voters find it harder to gauge their gains and losses of a given policy change. Such areas could be tax policies or business regulation, and, more generally, policies where voters need to discount long-term gains and losses of a policy change. Here we possibly find weaker policy-vote links and more blind retrospection. Accordingly, future research ought to disentangle policy areas with strong and rational policy-vote links from those where policy-makers have more leeway in shielding themselves from the voters' wrath.

Clearly, this conclusion does not mean that there are no limits to voters' abilities. First of all, time matters: Our results indicate that voters seem to forget about policy changes after a couple of months and do not punish or reward governments for cutbacks or expansions they adopted six months before. In contrast, they do react immediately after policy events by adjusting their approval of government. This clearly opens up a window of opportunity for governments to behave strategically and time cutbacks and expansion in a way that doesn't harm the electoral prospects. Moreover, several studies have documented that citizens are prone to react to events that are outside the control of the government, and we have added to this insight by showing that the policy-vote link tend to be weaker when extreme events are highly salient. It remains one of the discipline's key tasks to explore the balance between this type of noise, on the one hand, and relevant policy signals in citizens' vote choice on the other hand.

[1] We matched the Politbarometer surveys with the actual results for the parties in government at the Federal Elections since 1976. The correlation between the actual results and the last survey before respectively the first survey after a Federal Election was 0.92 and 0.95. The slight difference is in our view attributed to the 2005 election and the notorious underestimation of the CDU under Kohl, but captured through our dummies for government composition.

[2] Running the analysis on a reduced sample with valid left-right self-placements did not yield any differences between models containing the ideological orientation and models without (cf. Models M6 and M7 in Tables B1 and B2).

[3] The complete regression tables as well as various robustness checks can be found in the appendix.

[4] The results for pensions and unemployment insurance remain similar if we model changes and in pensions and unemployment benefits simultaneously in one model (See Tables B1 and B2 and Figure B1). We also ran sensitivity tests with a binary dependent variable only and perceived government performance instead of vote intention as dependent variable. We further separated the data by former government respectively opposition support and then conducted binary logit models for supporting the government or opposition. All three specifications arrive at similar conclusions as our base models presented above (Models M2, M3, and M5 in Table B1 and B2).

[5] Note that these results does not inform us about how lasting the effect of a vote change is for the individual voter. The results only inform us about for how long the electorate in the aggregate is affected by reforms.

[6] We ran further models with an individual level dummy for being unemployed (no/yes) as moderator. The results match our findings as unemployed persons punish harder for cutbacks than employed persons, while we find mixed support for rewarding the government for expansions as above (analysis available on request).

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