The electoral consequences of welfare state changes
a sober look at theory and evidence[1]

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Abstract

The expectation that welfare retrenchment and expansion have electoral consequences for governing parties is widespread in welfare state research. Previous research either argues that welfare state change has electoral consequences across the board or that this is at least the case under certain conditions, such as a left party in government. In this study, we synthesize existing theoretical approaches into a stylized theoretical model and discuss why the assumptions underlying the electoral consequences argument may be questionable. We then conduct an empirical analysis of the electoral fates of government parties in 20 European countries. A wide range of statistical specifications provide practically no evidence for electoral consequences of welfare state changes even under favorable conditions. The importance of welfare changes for electoral outcomes may therefore be overstated.

Keywords: economic voting; partisan theory; political behavior; retrospective voting; welfare expansion; welfare retrenchment

INTRODUCTION

Welfare state scholars often expect the welfare state to be popular with voters. Paul Pierson (1994, 1996) famously argued that welfare programs create their own supporters once they are implemented. One implication of this argument is that changes to the welfare state should have electoral consequences for incumbent parties that oversaw these changes. Retrenchment of welfare programs should lead to a decline in the vote share, whereas parties that expand the welfare state should reap electoral rewards.

Even though the expectation that welfare changes have electoral consequences is widespread, it is not backed up by comprehensive empirical evidence. Quantitative studies cannot confirm that there is a general relationship between welfare changes and votes (Armingeon and Giger 2008; Giger and Nelson 2013; Schumacher et al. 2013). They rather indicate that electoral consequences only arise conditionally. They are especially likely for pro-welfare parties such as social democrats (Giger and Nelson 2011; Horn 2021; Schumacher et al. 2013) as well as for parties that cannot engage in blame shifting (Wenzelburger 2014; Wenzelburger et al.

2020). However, the empirical evidence is mixed, and there is no consensus on the significance of the various conditional factors.

Researchers also argued that voters react to legislative decisions rather than the implementation of welfare changes, for which comprehensive data only became available recently (Jensen and Wenzelburger 2021a). The scarce evidence indicates that the adoption of welfare reforms impacts government popularity (Lee et al. 2020). However, it remains unclear whether this translates into actual electoral consequences.

The objective of this article is to put theories on the electoral consequences of welfare state changes under scrutiny. We discuss the different theoretical approaches based on a unified theoretical model that allows us to lay open their pivotal assumptions, and we explain why these assumptions may be unreasonable from a theoretical perspective. We then conduct an empirical assessment of hypotheses on general and conditional electoral consequences using data on the electoral fates of government parties from 20 countries between 1970 and 2019.

We draw from previous research but move beyond it in several ways. We assess whether welfare changes have electoral consequences across a range of empirical measures. Our analysis complements standard outcome measures of welfare state change (spending, welfare generosity) with new data on legislative changes (Jensen and Wenzelburger 2021a; Lee et al. 2020). The data are up-to-date, reaching up to 2019 for the outcome measures and 2014 for the reform measures. Furthermore, we conduct a wide range of statistical tests for electoral consequences under favorable conditions.

The analyses result in practically no evidence speaking for either general or conditional electoral consequences. We find little evidence which suggests that government parties systematically win or lose votes even under favorable conditions, such as pro-welfare parties in government or clear political responsibility for the reforms. These results pertain to all measures of welfare change, and they replicate across a wide range of specifications.

This study contributes to the literature by offering a critical examination of the assumptions underlying the supposed electoral consequences of welfare change, which are often taken for granted. It suggests that welfare policies matter less for electoral outcomes than commonly assumed. We believe that this message has important implications for welfare state research and beyond. The assumption that individuals observe changes in policy outputs and evaluate them against their mostly fixed preferences underpins most adjacent work on electoral dilemmas and electoral punishment, such as the insider-outsider literature and research on the electoral backlash against austerity. All these approaches assume that parties face electoral consequences for policies that adversely affect the material interests of their voters. This is not a wrong assumption *per se*, but our study suggests that it may rely on shaky theoretical foundations. Voters neither generally support expansion nor generally reject retrenchment. Parties can have considerable leeway in swaying public opinion. Furthermore, voters may be unaware of welfare state change or be guided by alternative factors in their vote decision.

THE ELECTORAL CONSEQUENCES OF WELFARE STATE CHANGE

Paul Pierson's (1994, 1996) seminal work on the 'new politics of the welfare state' argues that the welfare state creates its own supporters, which is why welfare cuts are highly unpopular among large parts of the electorate. Voters thus turn away from governing parties that implement welfare cuts, which suffer electoral losses as a result. Correspondingly, governing parties may reap electoral gains when they expand popular welfare programs (Bonoli 2012). However, the negative electoral effects of retrenchment are expected to be higher than the gains resulting

from welfare expansion because of a 'negativity bias' exhibited by individuals (Pierson 1996, 144–47; Weaver 1986).

FIGURE 1: Theoretical model of electoral consequences of welfare state change
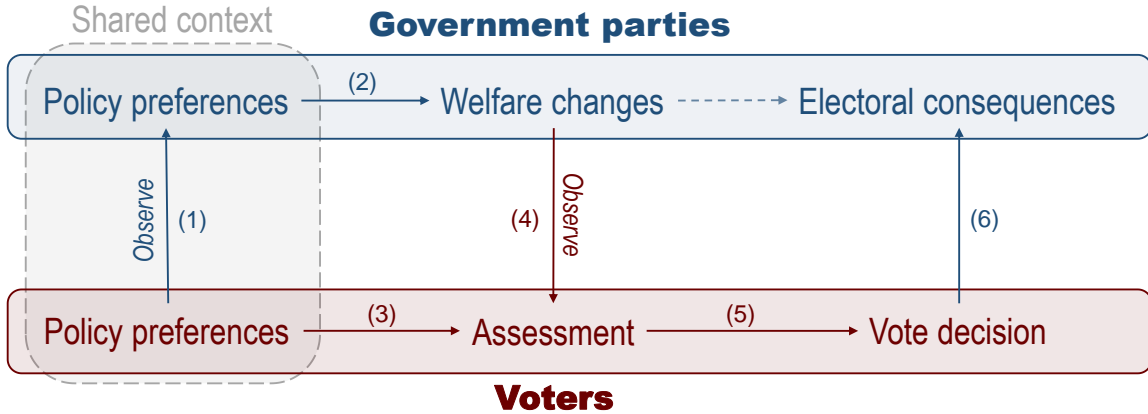

Figure 1 offers a stylized representation of the causal mechanism, which serves as a backdrop for our discussion of the arguments in favor of and against electoral consequences of welfare changes. Government parties and voters both have policy preferences. These include attitudes on the ideal level of welfare entitlements and spending, but they also cover fiscal and economic concerns, which are tightly linked with public provision of welfare. The policy preferences of government parties depend on party ideology, but they are also shaped by voters' policy preferences (arrow 1) due to vote-seeking behavior (Adams et al. 2004; Romeijn 2020). Government parties implement and oversee welfare changes based on their policy preferences (2). Voters then compare these welfare changes with their own preferences to assess the policy-preference congruence (3 and 4). Based on this assessment, voters punish or reward government parties in their vote decision (5). Lastly, the aggregated votes decide over the electoral consequences of welfare changes (6). Electoral consequences ultimately arise due to preference (dis-)congruence between parties and voters according to the model.

According to the 'new politics' argument, vote-seeking policymakers should favor welfare expansion and avoid retrenchment due to the popularity of welfare. However, economic and fiscal constraints, ideological predispositions, and a biased perception of voter preferences (Broockman and Skovron 2018; Dekker and Ester 1989) can nevertheless lead policymakers to diverge from their voters' preferences. In this case, government parties should be punished at the polls.

It must be noted that the theory is ambiguous about what exactly voters punish and reward. Pierson assumes self-interested individuals, where voters care about their own material well-being. Extending this theory, one can also expect that voters follow sociotropic considerations (Mau 2004), implying that even voters not personally affected by welfare changes could opt for punishing or rewarding the government. Furthermore, it is unclear at what stage welfare change has electoral consequences. Voters may react (a) to the actual receipt and loss of welfare income and services, (b) to changes in entitlement generosity, and/or (c) to the adoption of welfare reforms in the legislature, which can considerably predate the rolling out of the reforms. We contend that all these expectations are reasonable.

Empirical research offers only weak support for the theoretical expectations. Individual case studies show that welfare changes can have electoral consequences (e.g., Arndt 2013;

Schwander and Manow 2017). However, quantitative research consistently fails to confirm the prevalence of general electoral consequences (Armingeon and Giger 2008; Giger 2011, 135–41; Schumacher et al. 2013).

THEORETICAL REFINEMENTS AND FUNDAMENTAL CRITIQUES

Inspired by the lack of confirming evidence, researchers refined the theoretical argument on the electoral consequences of welfare change. Each contribution tweaks one of the building blocks of Figure 1 to arrive at a more nuanced view on the conditions under which electoral consequences should arise. We review these theoretical refinements in this section. Our aim is to identify conditions under which electoral consequences are especially likely. We also draw from a wider political science literature and discuss why welfare changes may ultimately have no electoral consequences even under such favorable conditions.

Voter and party preferences

Researchers firstly refined the specification of voters' policy preferences. *Partisan theory* highlights that the social constituencies of political parties have distinctive policy preferences (Häusermann et al. 2013; Hibbs 1977). Voters of left parties, and to a lesser extent of Christian democrats (van Kersbergen 1995), hold especially strong pro-welfare preferences and assign more weight to their social policy preferences than other voters. Therefore, pro-welfare parties should face stronger electoral consequences than their competitors. Specifically, it is left parties that should be punished for retrenchment and rewarded for welfare expansion (Schumacher et al. 2013).[2]

Several studies present confirming evidence, but the empirical results remain mixed. Arndt (2013) shows that social democratic parties suffered painful vote losses after engaging in welfare cutbacks in the United Kingdom, Germany, Denmark, and Sweden. Schumacher et al. (2013) conclude that only parties with a positive welfare image (radical left parties, social democrats, and Christian democrats) are punished for welfare cuts. Horn (2021) finds that only left parties face long-term electoral consequences. In contrast, Giger and Nelson (2011) find no electoral consequences for party families situated on the left but positive electoral effects for religious and liberal parties engaging in welfare retrenchment.

Other refinements of the specification of policy preferences also help to explain why welfare changes may have little or no electoral consequences. Research suggests that voters neither generally favor welfare expansion nor generally reject retrenchment. Voters are aware of potential tradeoffs, most importantly regarding the budgetary implications of welfare changes (Giger and Nelson 2013). As Busemeyer and Garritzmann (2017) show, the popularity of welfare expansion drops significantly when voters are faced with a budget constraint. Therefore, voters should not blindly punish welfare retrenchment by governments operating under adverse economic conditions (Duch and Stevenson 2010; Giger and Nelson 2013). Likewise, they should not blindly reward welfare expansion. Rather, electoral consequences of welfare change should be especially likely when the changes run counter to current budgetary and economic conditions, such as welfare retrenchment under a budget surplus.

Moreover, the theory behind Figure 1 neglects that parties are able to influence the policy preferences of their voters, which prevents incongruences in the preferences of voters and

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[2] Following the competing ‚Nixon goes to China' logic, left parties are supposed to be *less* vulnerable when they implement welfare cuts because of their higher credibility as defenders of the welfare state (Green-Pedersen 2001).

parties (Bullock 2011; Slothuus and Bisgaard 2021). This may happen through successful framing efforts in public discourse. Research shows that parties prevent electoral punishment after welfare retrenchment by strategic framing, such as by proclaiming the undeservingness of recipients (Esmark and Schoop 2017; Slothuus 2007), highlighting popular reform elements (Nelson 2016), and pointing to the inevitability of the reforms (Green-Pedersen 2001). But even without such efforts, committed voters align their policy preferences with observed welfare changes to protect their partisan identity or because they use party positions as informational shortcuts. Strikingly, Slothuus and Bisgaard (2021) show that supporters of the Danish People's Party and the Danish Liberals rapidly adapted their welfare preferences after their parties unexpectedly announced cutbacks to unemployment benefits and an early retirement program. The implication is that the potential for electoral consequences is more limited than assumed.

Voters' assessment of welfare changes

We now move on to theoretical refinements of voters' assessment of welfare changes (arrows 3 and 4 in Figure 1). In contrast to the stylized model presented in Figure 1, voters neither systematically nor always correctly observe welfare change, which limits their ability to assess whether policies are in line with their preferences or not. Research not only shows that voters' knowledge about welfare programs is often limited or inaccurate (Geiger 2018; Jensen and Zohlnhöfer 2020) but also that voters tend to misperceive the distributive effects of welfare reforms (Wilson et al. 2012). These misperceptions are aggravated by the fact that media attention is restricted to certain kinds of welfare reforms (Jensen and Wenzelburger 2021b).

There is an extensive literature on the different strategies of *blame avoidance* and *credit claiming* that parties use to exploit voters' limited ability to observe and assess welfare changes (Bonoli 2012). Welfare scholars argue that parties strategically alter reform saliency via the timing of reforms and the choice of specific policy instruments (Jensen and Wenzelburger 2021a; Pierson 1994, 13–26; Vis 2016; Wenzelburger 2014; Wenzelburger et al. 2020). The timing argument states that voters have a short memory, which is why welfare reforms are quickly forgotten.[3] Parties may exploit this by strategically timing their reforms to maximize electoral outcomes, for example by avoiding cutbacks and favoring expansion as an election approaches (Jensen and Wenzelburger 2021a; Wenzelburger 2014; Wenzelburger et al. 2020). The policy instrument argument expects that voters only observe changes to the most visible welfare program characteristics such as replacement rates, while they have limited knowledge about more technical characteristics such as benefit indexation. Again, parties may exploit this by strategically targeting program characteristics in their reforms, for example by using more visible policy instruments such as benefit replacement rates when expanding the welfare state and by retrenching through less visible instruments such as benefit indexation (Jensen and Wenzelburger 2021a, ch. 6).

Voters' ability to correctly observe and attribute welfare changes also depends on the political context. Electoral consequences are especially likely under high clarity of responsibility, i.e. when voters are able to distinguish that a specific government party is responsible for a welfare change rather than other political entities (Giger 2011, 48–51). Low clarity of responsibility, in contrast, makes electoral consequences less likely, especially because parties can exploit the situation for strategical blame avoidance. Apart from institutional factors such as bicameralism or federalism, clarity of responsibility depends on government composition

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[3] For example, the literature on fiscal austerity indicates that reform related drops in government popularity are often temporary and thus do not translate into electoral punishment if timed in the right way (Hübscher and Sattler 2017; Jacques and Haffert 2021).

(Hobolt et al. 2013). Responsibility is clear when a government is dominated by a single party, when government parties are ideologically cohesive, and when there is no cohabitation. Maximum clarity can be achieved under single-party governments.

The theoretical discussion suggests that electoral consequences are especially likely when it can be reasonably assumed that voters are aware of welfare changes and can attribute the changes to government parties. This is the case when welfare changes concern visible program characteristics, when they are implemented shortly before an election, and when clarity of responsibility is high. However, the theoretical discussion can also be used to justify why welfare change should have little or no electoral consequences altogether. Possibly, voters are insufficiently able or willing to observe and attribute welfare changes, for example because they do not care or pay little attention.

The link between voters' assessment and vote decision

A last point that questions the prevalence of electoral consequences altogether is that the issue welfare may matter less for vote decisions than commonly assumed (arrow 5). Firstly, other issues can be electorally more relevant than the welfare state. Especially in times of strong sociocultural conflicts, "social policy does not have the outstanding relevance for voters as assumed by the social policy literature" (Giger 2011, 415). Secondly, factors other than government policies and performance can be more important altogether, such as candidate evaluations (Quinlan and McAllister 2022). For example, candidate evaluations topped policy considerations in the 2009, 2013, and 2017 German federal elections (Hansen and Olsen 2020).

Summary and expectations

Table 1 presents a summary of the theoretical arguments drawn from the literature. It lists the different conditions that make electoral consequences especially likely. If there is a tendency for electoral consequences, this is where we should observe them. We will use the table as a guide for our empirical analysis.

TABLE 1: Summary of conditions that favor electoral consequences

Condition	Favors consequences	Explanation
Welfare change direction	Retrenchment	Negativity bias of voters
Partisanship	Left government party	Constituents put strong emphasis on social policy
Political-economic context	Retrenchment under low fiscal pressure	Voters see no policy-tradeoff
Timing of change	Reform close to election	Voters have a short memory
Policy instrument	Use of visible instruments	Voters only observe visible changes
Clarity of responsibility	High clarity of responsibility	Voters can attribute welfare change

We start the empirical analysis by evaluating whether there are electoral consequences across all parties and governments. However, both the theoretical discussion and previous empirical evidence suggests that we are unlikely to find such general effects. Therefore, we move on to refined analyses of conditional electoral consequences, successively going through all favorable conditions outlined in Table 1. But against the backdrop of the more fundamental critique, we contend that a predominance of null results may also be the outcome of our evaluation.

DATA AND METHOD

We compiled a dataset that tracks the electoral fates of government parties from 20 highly developed countries between 1970 and 2019.[4] Each observation relates to the incumbency of a government party. For example, the German CDU and SPD governed in a coalition between 2005 and 2009. Our dataset contains one observation relating to the SPD and another observation relating to the CDU (both concerning their 2005-2009 incumbency and their election results in 2005 and 2009). Observations of parties are in many cases repeated because, for example, the SPD entered multiple governments during our observation period.

We include one observation per government party and election period. We consider a party to be in government between the start date of the first cabinet it participated in and either the end date of the last cabinet it participated in or the day of the subsequent election, whatever date is earlier. We exclude parties in caretaker governments, parties that governed less than one year, as well as special issue parties. The data cover 484 government parties after listwise deletion, formed by 117 unique parties in 236 governments.[5] All data relating to parties, elections, and cabinets are sourced from the ParlGov database (Döring and Manow 2022).

Variables

The dependent variable measures parties' change in votes between the election that brought them into power (*t*) and the subsequent election (*t+1*). Vote change is expressed in percent and not in percentage points. For example, a party that received 10% of votes in *t* and 5% of votes in *t+1* has a vote change of -50%. This measurement approach allows us to meaningfully compare electoral outcomes between parties with different levels of overall success. It correctly reflects that, for example, a five-percentage point loss marks a substantially different loss for a party with an initial vote share of 10% (namely -50%) compared to a party with an initial share of 40% (namely -12.5%).

We use three types of indicators to measure welfare state change during parties' incumbency. The first indicator is the development of social spending as a proportion of GDP between the years a party entered and left government (sourced from the Comparative Political Data Set by Armingeon et al. [2021]). For further analyses on whether changes implemented shortly before an election matter, we instead use social spending changes in the last government year.[6] Social spending is a measure of the overall size of the welfare state. It indicates to what extent citizens currently benefit from welfare policies.

Secondly, we use the development of unemployment benefit and pension entitlement generosity between the years a party entered and left government (sourced from the Comparative Welfare Entitlements Dataset by Scruggs [2022]). Again, we also use alternative versions of the indicators that solely measure changes implemented in the last government year. The generosity indicators measure to what extent citizens can *expect to benefit* from the welfare state. Our main analyses rely on two summary generosity indicators that consider the following program characteristics to quantify the overall strength of social entitlements: average replacement rates, benefit duration (i.e. retirement age in the case of pensions), qualification period, waiting period (only unemployment benefits), the proportion of employee contributions (only

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[4] Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, and United Kingdom.

[5] We exclude one observation with an abnormally large vote change of +157% from the dataset (the Norwegian Center Party and their 1989-1993 incumbency) because this is an outlier with potentially strong effects on OLS regression results (see Figure A1 in the online appendix).

[6] For example, if a party governed between 2000 and 2005, we use the change between 2004 and 2005.

pensions), and coverage rates. For further analyses of whether visible changes have electoral consequences, we use alternative summary generosity indicators that only record changes in the most visible subdimensions of the generosity indicators, namely replacement rates and benefit duration (Jensen and Wenzelburger 2021a).[7]

Thirdly, we use data on the adoption of legislative reforms to unemployment benefits and pensions during the incumbency of government parties (sourced from the Welfare State Reform Dataset by Jensen and Wenzelburger [2021a]). Unfortunately, the reform data is only available for five countries.[8] The data records each individual legislative reform of the two welfare programs, with separate entries for each dimension of change (e.g., replacement rate, duration period, etc.) and direction of change (cutback, retrenchment, introduction/abolishment). For example, a welfare reform package that decreases replacement rates but increases benefit duration of unemployment benefits is recorded in two separate entries in the dataset. The data only record that a reform happened at a specific point in time (e.g., pension replacement rates were lowered), but there is no information on the magnitude of change.

Following Jensen and Wenzelburger (2021a), we use net change indicators for pensions and unemployment benefits. They quantify the number of welfare-expanding reforms relative to the number of retrenching reforms under a government.[9] For example, the value two indicates that a government party introduced two more expanding reforms than retrenching reforms during their incumbency. For the analyses of recent welfare changes, we use alternative reform measures that only record reforms adopted in the 12 months before the election; and for the analyses of visible welfare reforms, we use alternative measures that only consider reforms to the most visible program dimensions according to Jensen and Wenzelburger (2021a, ch. 6): benefit levels, age brackets, benefit duration, employability, and means-tests.

The combination of indicators ensures that welfare change is measured comprehensively, which takes note of the 'dependent variable problem' discussed in welfare state research (Clasen and Siegel 2007; Green-Pedersen 2004; Wenzelburger et al. 2013). Each of the indicators measures a different dimension of welfare change. This is an advantage because the theory is not clear on what type of welfare changes voters react to. Our broad measurement approach ensures that we cover all possibilities: voters may react to actual benefit receipt, which is best measured by social spending; to the level of current welfare entitlements, which is best measured be the generosity indicators; or to legislative decisions, which is best measured by the reform indicators. The inclusion of the reform indicators also captures that the actual rolling out of reforms often lags behind legislative decisions, which is a potential disadvantage of solely using outcome measures if voters primarily react to legislative decisions.

We use further dummy variables for our assessment of welfare changes under conditions that should favor electoral consequences. Firstly, a *pro-welfare party* dummy identifies parties with a positive welfare image, which includes social democratic, left socialist, and ecologist parties. Secondly, a dummy identifies governments with *high clarity of responsibility*. We construct this dummy based on the continuous 'government clarity' indicator proposed by Hobolt et al. (2013), using the sample median as a threshold to differentiate lower- and higher-clarity governments. Thirdly, a *low fiscal pressure* dummy identifies governments with a balanced or surplus budget in the first year of incumbency.

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[7] Replacement rates and benefit duration are weighted equally in these alternative summary indicators.
[8] Denmark, Finland, France, Germany, and United Kingdom.
[9] Welfare program introductions are considered as welfare expansion and program abolishment as welfare retrenchment.

We use a standard set of controls inspired by previous studies on electoral consequences: parties' vote share in the election that brought them into power are included to control for regression to the mean effects, i.e. parties with higher vote shares are likely to lose votes in the subsequent election. We use the effective number of parties measured by the indicator of Golosov (2010) because parties are expected to lose more votes when there are more alternatives. We control for the government duration in days to capture the "cost of governing", i.e. a commonly observed decrease in government popularity over time. Furthermore, we choose a set of control variables based on the economic voting literature, which claims that voters retrospectively punish bad economic conditions and reward good economic conditions. Controlling for economic conditions is important because they should be a common cause of welfare changes and vote results. For example, low growth or a high budget deficit may lead governments to retrench welfare while such adverse conditions may also be punished at the polls due to economic voting. We use the central government budget balance, the unemployment rate, and GDP growth as controls. Budget balance is measured in the first year of incumbency to avoid post-treatment bias since changes to the welfare state also affect the budget. Regarding unemployment and GDP growth, we use both their average levels during governments' incumbencies as well as changes between the years a party entered and left government. The motivation is that it is unclear whether, for example, voters react to persistently high levels of unemployment or to a surge in unemployment during an election period. Lastly, we use time fixed effects to capture unobserved heterogeneity over time.

Method

We estimate the following linear regression model with ordinary least squares (OLS) to assess whether welfare changes have electoral consequences:

$$\Delta\text{vote}_{pe} = \alpha + \beta\Delta\text{welfare}_{pe} + \delta\text{cntrl}_{pe} + \gamma_{t+1} + \epsilon_{pe}$$

where $\Delta vote_{pe}$ is the vote change of government party p in election period e (i.e. between an election in year t and the subsequent election in year $t+1$), $\Delta welfare_{pe}$ denotes a welfare state change indicator, $cntrl_{pe}$ a vector of control variables, γ_{t+1} time fixed effects (government end in 1970-1979, 1980-1989, etc.), and ϵ_{pe} the error term.

We also assess whether welfare expansion and retrenchment have different electoral implications. We split the welfare change indicators $\Delta welfare_{pe}$ into the two components $\Delta_{pos}welfare_{pe}$ and $\Delta_{neg}welfare_{pe,}$ whereas the former only records welfare expanding changes and takes the value zero otherwise and the latter only records welfare retrenching changes and is zero otherwise.[10] We then enter the two components in the regression model above to estimate separate coefficients for them.

We rescale all welfare change measures so that their standard deviations equal one. All regression coefficients therefore indicate the expected vote change when the respective welfare change measure increases by one standard deviation. This simplifies the interpretation of the results because the welfare change indicators are on different measurement scales, which are also difficult to evaluate (for example, it is unclear what exactly a one-unit increase in pension generosity entails). Further, we use robust standard errors with multi-way clustering by both

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[10] For changes in social spending and entitlement generosity, the expansion and retrenchment indicators are constructed based on the overall change $\Delta welfare_{pe}$. For example, the welfare expansion indicator only records values above zero if the overall change is positive. For the reform indicators, we can use a more fine-grained measure. The expansion and retrenchment indicators are given by the number of welfare-expanding and -retrenching reforms, respectively, where both can deviate from zero concurrently.

governments and parties. This is motivated by the results of specification tests showing that there is intra-cluster correlation within governments (i.e. repeated observations within government coalitions) and parties (i.e. repeated observations of the same party across different governments). These standard errors are also heteroskedasticity-consistent.

Discussion

We believe that our empirical approach refines previous research in several ways. Firstly, we measure welfare change comprehensively by assessing the effects of several dimensions of change: social spending, welfare generosity, and legislative reforms, all with up-to-date data. Previous studies, in contrast, mainly relied on the development of program replacement rates to measure welfare state change (see Armingeon and Giger 2008; Giger and Nelson 2011; Schumacher et al. 2013). Replacement rates are a crucial feature of welfare programs, but they do not capture other important dimensions of welfare change, such as benefit duration. In addition, the effect of legislative decisions is understudied in quantitative research (Jensen and Wenzelburger 2021a). Lee et al. (2020) study effects on government support, but it remains unclear to what extent their findings extend to actual voting behavior.

Secondly, most previous research focused on retrenchment, but the theory also predicts that welfare expansion is electorally relevant. We show below that welfare expansion was actually the more common direction of welfare change in the covered period, which implies that focusing solely on retrenchment is lopsided.

Thirdly, our statistical specification is refined. Its unit of analysis (government parties) is appropriate because the inferences relate to electoral consequences for particular parties. Previous research analyzed combined vote shares of all incumbent parties, which jeopardizes analyses of electoral consequences for particular party families due to the possibility of ecological fallacy. Further, our specification of the dependent variable in terms of percent changes rather than percentage point changes improves the comparability of electoral consequences for small and large parties.

FIGURE 2: Distributions of the welfare state change indicators
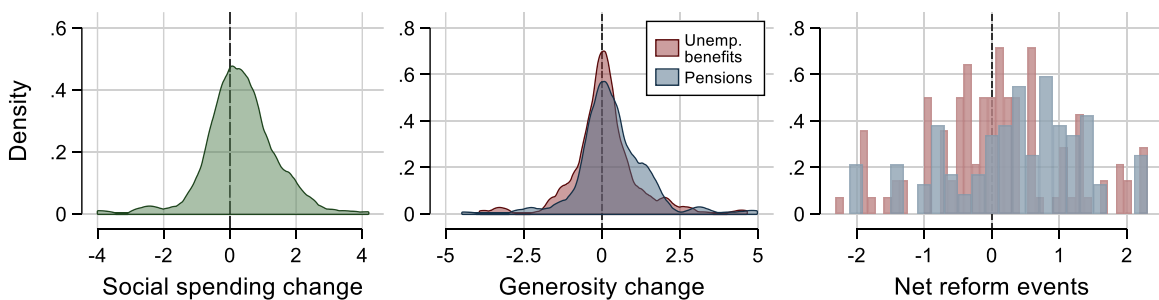
RESULTS

This section presents the results of the empirical analysis. We begin with descriptive statistics on welfare state change (a full set of descriptive statistics is available in Table A1 in the online supplementary material). Figure 2 shows the distributions of welfare changes according to five indicators. What is notable is that welfare expansion is at least equally as common as welfare retrenchment. It is therefore a fruitful endeavor to include welfare state expansion in the analysis.

FIGURE 3: The effect of social spending changes on vote change
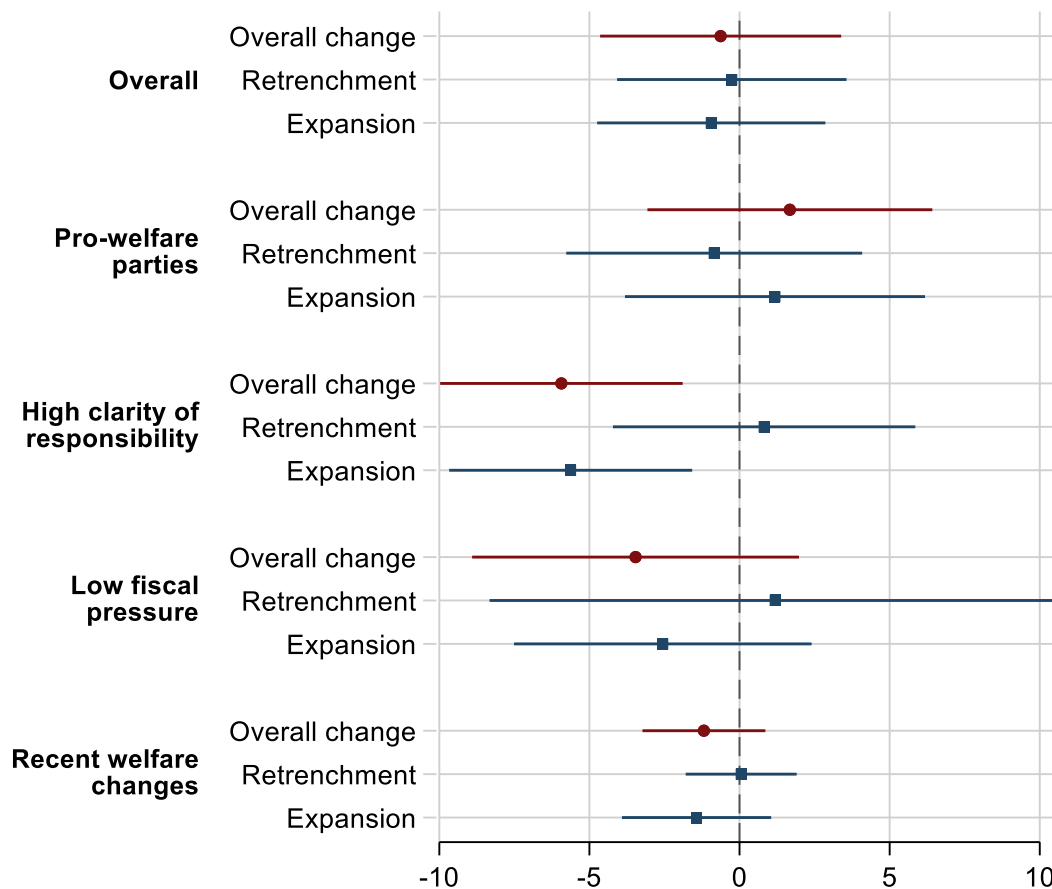
Note: The figure shows the results from 10 regression models that include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties. The full regression results are available in the online supplementary material.

The further statistical analysis proceeds in three steps. We start with changes in social spending, which is the most general and coarse of our three indicators. We then turn to measures of welfare generosity and finally to legislative changes. In each case, we use the full set of observations available after listwise deletion. Note that this amount varies somewhat between the independent variables because of different data availability.[11]

The effects of social spending changes

Figure 3 depicts the empirical results regarding the electoral consequences of social spending changes. It plots selected slope coefficients with 95% confidence obtained from 10 regression models. The complete results are available in the online supplementary material. As for all results reported hereafter, the underlying regressions include the full set of control variables.

The three regression coefficients on the top side of Figure 3 (labeled "overall") indicate whether social spending changes have electoral consequences across the whole analysis sample. The top coefficient shows the effect of social spending changes in any direction, while the two

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[11] Most importantly, the welfare reform variables are only available up to 2014, whereas social spending and the generosity variables are available up to 2019 and 2018, respectively. Furthermore, the welfare reform variables are only available for five countries.

coefficients right below present results from a disaggregated analysis of the effects of welfare retrenchment and expansion. To confirm the theoretical expectations, we should see a positive coefficient for overall welfare change, which would imply that expansion is rewarded and retrenchment punished; and we should see a positive coefficient for expansion as well as a negative coefficient for retrenchment.

FIGURE 4: The effect of generosity changes on vote change
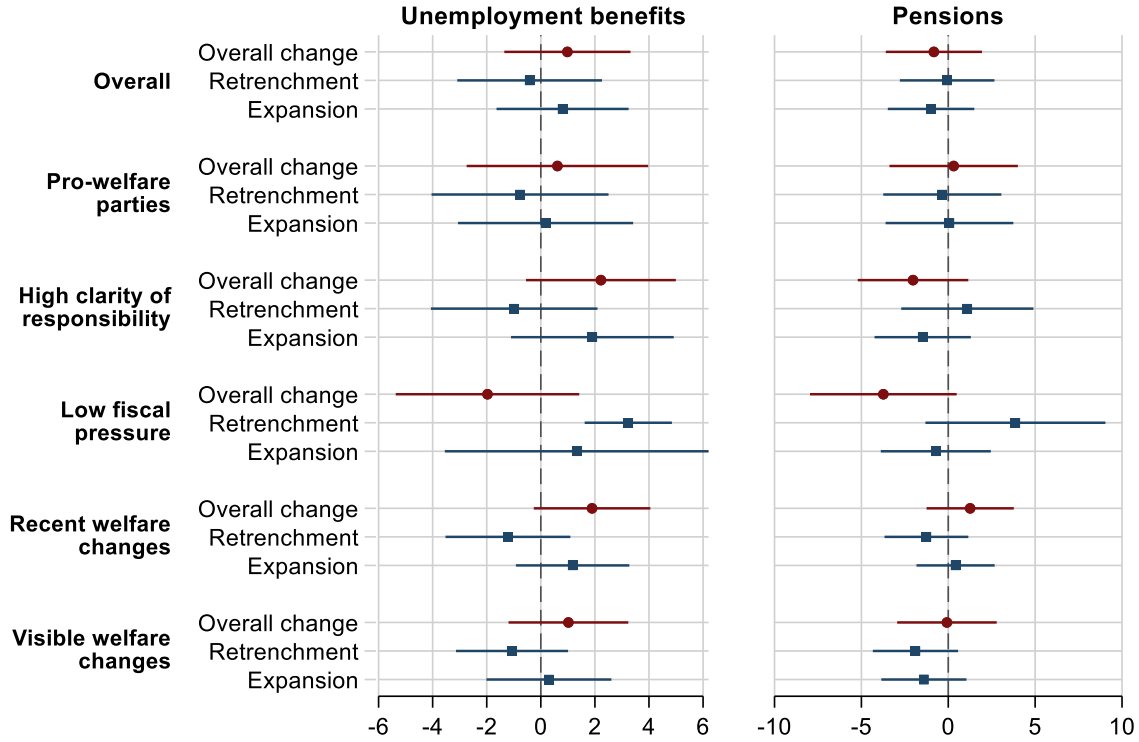
Note: The figure shows the results from 24 regression models that include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties. The full regression results are available in the online supplementary material.

Thereafter, Figure 3 presents results of analyses on whether social spending changes have electoral consequences under favorable conditions, i.e. when the government party is pro-welfare, there is high clarity of responsibility, under low economic pressure, and when the spending changes happen shortly before an election. To assess the former three conditional expectations, we estimate regression models with interaction terms between social spending and additional dummies (e.g., a pro-welfare dummy). We only report the relevant results that relate to the favorable conditions in Figure 3 (for example, we show the slope coefficients for pro-welfare parties but not for other parties). Again, the full results from the interaction models are available in the online supplementary material. For the analysis on recent social spending changes, we use the alternative change indicator that relates to changes in the last government year.

The results depicted in Figure 3 do not confirm that social spending has electoral consequences, neither overall nor under favorable conditions. Most estimated coefficients are small and insignificant, and the only coefficients with considerable effect size and/or significant hypothesis tests have results that point in the direction opposite of what the theoretical expectations are.

Notably, the results suggest that social spending expansion has negative electoral effects under high clarity of responsibility.

The effect of generosity changes

We move on to the electoral consequences of welfare generosity changes. Figure 4 presents analyses on whether changes to unemployment benefit and pension generosity have electoral consequences across the whole sample as well as under favorable conditions. The empirical approach mirrors the approach of the social spending analysis above, except that we present an additional test of the effects of visible generosity changes, using our alternative generosity measure only relating to replacement rates and benefit duration.

The results depicted in Figure 4 do not confirm the prevalence of electoral consequences in line with theoretical expectations, neither across the whole sample nor under favorable conditions. The coefficients are mostly small and insignificant. There are some exceptions that hint at the prevalence of electoral consequences. Most importantly, welfare expansion has a positive and retrenchment a negative coefficient when unemployment benefits were changed under high clarity of responsibility and when the changes were recent. However, the coefficients remain small and the hypothesis tests are insignificant, implying that null effects cannot be ruled out. Furthermore, other results also directly contradict theoretical expectations, such as the positive coefficients of unemployment benefit and pension retrenchment under low fiscal pressure. Overall, the results do not confirm the theoretical expectations.

The effect of welfare reforms

We now turn to whether legislative reforms have electoral implications. The empirical approach is largely the same as above, but remember that the reform data is only available for five countries. The empirical analyses are thus based on a restricted analysis sample with much less observations than before. Further, we do not present estimates for welfare reforms under low economic pressure because there is insufficient variation in the data.

Again, the results depicted in Figure 5 do not confirm the prevalence of electoral consequences, neither across the whole sample nor under favorable conditions. There are some coefficients that are in line with theoretical expectations, although insignificantly. Notably, retrenchment of unemployment benefits under high clarity of responsibility has a negative and substantially important coefficient. However, other results also contradict the expectations, such as the positive coefficients of pension retrenchment under several conditions. Overall, the theoretical expectations are not confirmed.

Robustness tests

We run extensive sensitivity tests to check whether our main result that welfare changes tend to have null effects is robust. We re-estimate all models reported above using alternative specifications. The results are available in the online supplementary material.

Firstly, we test whether we can tease out electoral consequences if we combine several favorable conditions that were analyzed separately above. We restrict the data to pro-welfare parties and, in another analysis, to governments with high clarity of responsibility. We then re-estimate all conditional tests from above. This allows us to gauge whether, for example, pro-welfare parties under high clarity of responsibility are punished when they retrench the welfare state. Secondly, we repeat our analyses on conditional effects for pro-welfare parties using a different conceptualization of what constitutes a pro-welfare party. Following Schumacher et al. (2013), we additionally include Christian Democrats in the pro-welfare camp because they

also tend to be proponents of the welfare state. Thirdly, we exclude the younger democracies Greece, Spain, and Portugal from the estimation sample. This is usually done in similar studies because these countries had a late transition to democracy, implying that there is no tradition of party competition over social policies (e.g., Armingeon and Giger 2008; Horn 2021). Fourthly, we omit all control variables from the regression models to show that the results are not a byproduct of our model specification. And lastly, we use a different specification of the dependent variable and measure vote chance in terms of percentage *point* changes rather than percent changes, which follows previous quantitative research.

FIGURE 5: The effect of welfare reforms on vote change
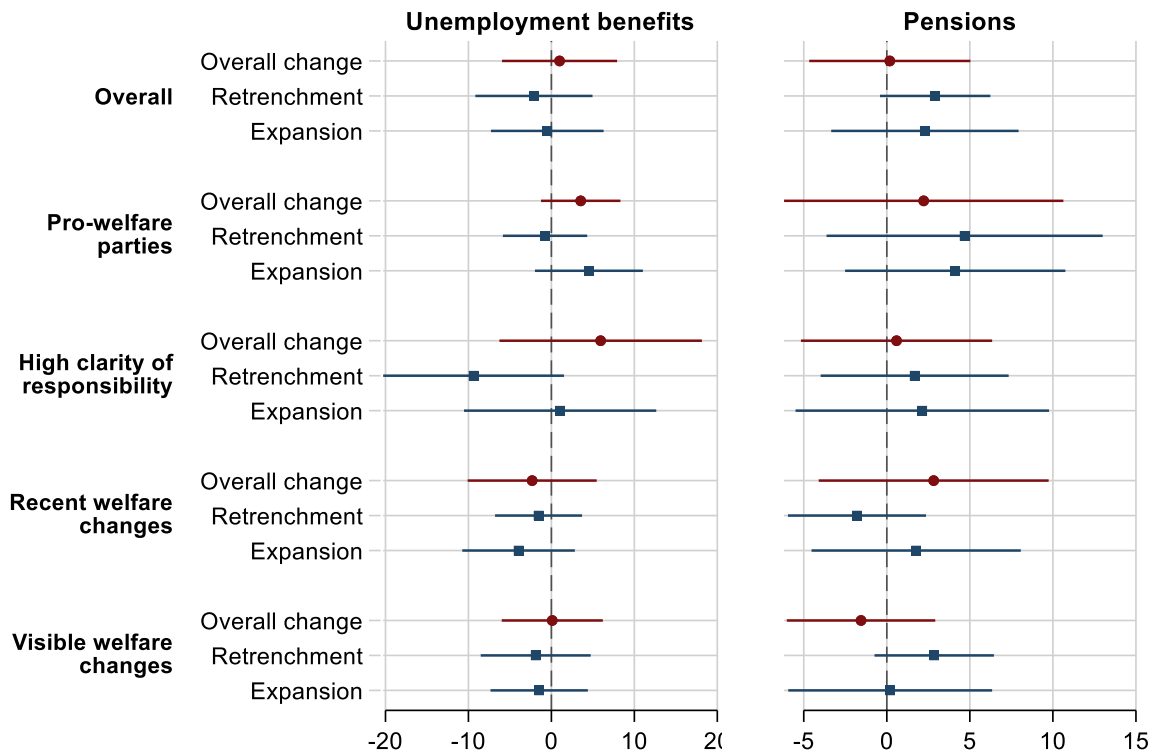
Note: The figure shows the results from 20 regression models that include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties. The full regression results are available in the online supplementary material.

The results from the robustness tests broadly line up with the results from our main specifications. It is impossible to give a detailed account of all results because we run hundreds of regressions, but the overall picture is that the coefficients remain small and insignificant in all but a few cases. Again, there are individual results that line up with theoretical expectations but, at the same time, just as many results directly contradict them. This is to be expected because we run so many different regression specifications that the results should fluctuate into positive and negative regions by mere chance in some cases. This also pertains to the main regression specifications, which yielded some significant effects with no clear directional tendency as well. Overall, the results do not reliably speak for the prevalence of electoral consequences following welfare change.

DISCUSSION AND CONCLUSION

This study evaluated whether government parties gain and lose at the polls after overseeing changes to the welfare state. The empirical evidence does not confirm that this is the case. It cannot be rejected that there is a null relationship between welfare change and electoral results. This finding also holds under conditions where electoral consequences should be most likely.

Our findings diverge from previous research, which also found that there are no general electoral consequences of welfare state change but partly presented evidence for conditional effects (Armingeon and Giger 2008; Giger 2011; Giger and Nelson 2011; Schumacher et al. 2013). This divergence can be attributed to several differences in the empirical approach between our and previous studies. We believe that our evidence is credible because we improved upon previous approaches in several regards, including a more comprehensive measurement of welfare state change, more recent data, and an improved statistical specification. Further, our findings also diverge from Lee et al. (2020), who found that legislative welfare reforms affect government popularity, whereas we find no effects on actual vote change. A possible explanation for this discrepancy is that the effects on popularity dissipate before the election (Arndt et al. 2021; Jacques and Haffert 2021; Wenzelburger et al. 2020).

We want to highlight that our study does not imply that welfare changes never have electoral implications. Several country studies suggest that welfare retrenchment can have crucial electoral costs (e.g. Arndt 2013; Schwander and Manow 2017). There is also evidence that governing parties paid a price for austerity measures implemented in the wake of the Great Recession (Bremer et al. 2020; Hobolt and Tilley 2016), although the evidence on austerity remains mixed overall (Jacques and Haffert 2021, 191). We expect that governments which would implement more extreme reforms than what we observe in the data, such as halving pension payments, should be severely punished at the polls. It is also a different question whether welfare changes have long-term rather than immediate consequences, as Horn's (2021) analysis suggests. While welfare change may be mostly inconsequential in the short-term, for example because of stable partisan identification, prolonged discontent with welfare changes may lead to dealignment. Lastly, a more technical point is that, while we find no credible evidence for electoral consequences, statistically insignificant results do not necessarily imply the complete absence of effects. So, what should one make of our results?

Our findings suggest that there seems to be no general tendency for electoral consequences even under favorable conditions. Parties thus have more leeway to implement supposedly unpopular reforms than assumed by the literature. On the flipside, parties also have less potential to attract votes by implementing welfare expanding reforms than assumed. This does not mean that they can do what they want to the welfare state. But, under *politics as usual*, changes to the welfare state do not seem to matter much for electoral outcomes.

A fundamental critique to be raised against our analysis is that we should rarely observe electoral punishment for retrenchment in observational data. Parties anticipate backlash and avoid the implementation of reforms when blame avoidance techniques are not available, the argument goes (Hübscher et al. 2021; Wenzelburger 2014). We acknowledge this critique. However, our empirical approach carefully checks for electoral consequences under conditions that make blame avoidance techniques unlikely, such as when reforms were implemented shortly before an election or when visible program characteristics were changed. Our results also hold under these conditions. Furthermore, even when one assumes that there would be electoral consequences of welfare change that we do not observe due to parties' strategical reform choices, it remains unexplained why we do not observe electoral gains following welfare expansion. Expansion is supposedly popular and parties are vote-seekers. If there were electoral

consequences, parties should be able to exploit this for electoral gain. Our empirical analysis suggests that this is not the case. We believe that a reasonable explanation is that welfare change is electorally less relevant than assumed.

The results may come as a surprise for some welfare scholars, but they are in line with previous research that questions the assumptions underpinning electoral consequence theory. Firstly, even the voters of pro-welfare parties do not unanimously oppose welfare cuts and support welfare expansion, implying that retrenchment is not generally punished and expansion not generally rewarded (Bansak et al. 2021; Busemeyer and Garritzmann 2017; Giger 2012). Secondly, there is less potential for electoral punishment due to preference mismatch between parties and voters than assumed because parties are influenced by their voters (Adams et al. 2004; Bernardi et al. 2021; Engler and Zohlnhöfer 2019; Romeijn 2020) while parties are also able to shift the preferences of their voters (Bullock 2011; Slothuus and Bisgaard 2021). Thirdly, voters can be unwilling or unable to observe welfare changes (Jensen and Zohlnhöfer 2020), which limits the possibility of electoral consequences. Lastly, vote choice is strongly determined by factors other than social policy, such as party identification, candidate evaluations, or other policy issues.

Our results question the mechanistic worldview that underpins many theoretical approaches in welfare state and political economy research. People do not necessarily pay close attention to welfare changes to compare them to their (mostly fixed) preferences, which in turn informs their vote choice. This mechanism is pivotal, for example, for the insider-outsider dilemma of social democratic parties (Lindvall and Rueda 2014; Rueda 2005). Studies on the electoral backlash against austerity rely on a similar mechanism (Hübscher et al. 2021; Jacques and Haffert 2021). While this research offers important insights, our findings emphasize that the potential limitations regarding its required assumptions should be taken more seriously. For example, the finding that voters disapprove of spending cuts, as shown by survey experiments (Hübscher et al. 2021), will not have the proposed electoral implications when voters do not observe actual spending cuts or when party elites are able to shift voter preferences in the political process. The insider-outsider and austerity literatures are only two examples, but the underlying mechanistic worldview exceeds them.

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ONLINE APPENDIX

The electoral consequences of welfare state changes:
a sober look at theory and evidence

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Contents

Information about the data

Figure A1: Excluded outlier values

Note: The value to the right of the dotted line is an outlier and we exclude it from the analyses.

Table A1: Descriptive statistics

	N	mean	median	sd	min	max
Vote change (%)	483	-8.43	-8.43	25.4	-94.2	100
Vote change (pp.)	484	-2.09	-2.09	5.20	-30.7	20.7
Social spending change	398	0.34	0.34	1.00	-3.98	4.18
Social spending retrenchment	398	0.44	0.44	1.00	0	7.86
Social spending expansion	398	0.79	0.79	1.00	0	5.93
Short-term social spending change	419	0.21	0.21	1.00	-5.90	4.71
Short-term social spending retrenchment	419	0.45	0.45	1.00	0	10.9
Short-term social spending expansion	419	0.66	0.66	1.00	0	6.78
UB generosity change	464	0.083	0.083	1.00	-3.90	4.63
UB generosity retrenchment	464	0.50	0.50	1.00	0	6.76
UB generosity expansion	464	0.56	0.56	1.00	0	6.90
Visible UB generosity change	469	0.058	0.058	1.00	-3.27	3.78
Visible UB generosity retrenchment	469	0.51	0.51	1.00	0	6.01
Visible UB generosity expansion	469	0.46	0.46	1.00	0	5.25
Short-term UB generosity change	471	-0.0087	-0.0087	1.00	-5.56	4.77
Short-term UB generosity retrenchment	471	0.53	0.53	1.00	0	9.06
Short-term UB generosity expansion	471	0.49	0.49	1.00	0	7.36
Pension generosity change	455	0.34	0.34	1.00	-4.46	4.96
Pension generosity retrenchment	455	0.43	0.43	1.00	0	9.75
Pension generosity expansion	455	0.70	0.70	1.00	0	6.51
Visible pension generosity change	459	0.52	0.52	1.00	-2.38	5.87
Visible pension generosity retrenchment	459	0.40	0.40	1.00	0	7.03
Visible pension generosity expansion	459	0.77	0.77	1.00	0	6.98
Short-term pension generosity change	464	0.24	0.24	1.00	-3.77	5.24
Short-term pension generosity retrenchment	464	0.46	0.46	1.00	0	8.02
Short-term pension generosity expansion	464	0.61	0.61	1.00	0	6.91
UB reform	118	0.082	0.082	1.00	-2.25	2.25
UB retrenchment reform	118	1.15	1.15	1.00	0	4.38
UB expansion reform	118	1.02	1.02	1.00	0	3.52
Visible UB reform	118	0.17	0.17	1.00	-1.87	2.97
Visible UB retrenchment reform	118	1.21	1.21	1.00	0	4.87
Visible UB expansion reform	118	1.02	1.02	1.00	0	4.21
Short-term UB reform	118	0.20	0.20	1.00	-4.27	3.13
Short-term UB retrenchment reform	118	0.63	0.63	1.00	0	6.82
Short-term UB expansion reform	118	0.81	0.81	1.00	0	4.11
Pension reform	118	0.35	0.35	1.00	-2.00	2.20
Pension retrenchment reform	118	0.92	0.92	1.00	0	4.53
Pension expansion reform	118	1.41	1.41	1.00	0	5.06
Visible pension reform	118	0.65	0.65	1.00	-2.35	2.94
Visible pension retrenchment reform	118	0.82	0.82	1.00	0	6.15
Visible pension expansion reform	118	1.40	1.40	1.00	0	4.83
Short-term pension reform	118	0.44	0.44	1.00	-3.13	3.13
Short-term pension retrenchment reform	118	0.46	0.46	1.00	0	6.33
Short-term pension expansion reform	118	0.78	0.78	1.00	0	6.16
Pro-welfare party	484	0.33	0.33	0.47	0	1
Pro-welfare party (including CDs)	484	0.48	0.48	0.50	0	1
High clarity of responsibility	484	0.50	0.50	0.50	0	1
Low fiscal pressure	484	0.25	0.25	0.43	0	1
Government duration in days	484	1,203	1,203	339	382	1,848
Initial vote share	484	22.5	22.5	14.8	1.10	51.6
Effective number of parties	484	3.87	3.87	1.76	1.38	9.21
Average unemployment	484	6.99	6.99	3.49	0.80	25.9
Unemployment change	484	0.19	0.19	2.44	-8.20	10.4
Average GDP growth	484	2.29	2.29	1.58	-6.64	8.26
GDP growth change	484	-0.055	-0.055	3.00	-9.50	10.1
Initial household balance	484	-2.34	-2.34	4.78	-16.2	15.1

4

Full regression tables of the main models

Table A2: Effect of social spending changes

	(1)	(2)
Social spending change	-0.63	
	(2.05)	
Social spending retrenchment		-0.26
		(1.95)
Social spending expansion		-0.94
		(1.94)
Government duration in days	-0.010**	-0.0097**
	(0.0040)	(0.0041)
Initial vote share	0.0081	0.0074
	(0.12)	(0.12)
Effective number of parties	1.65	1.63
	(1.00)	(1.01)
Average unemployment	-0.28	-0.25
	(0.46)	(0.46)
Unemployment change	-0.51	-0.51
	(0.70)	(0.69)
Average GDP growth	0.99	1.02
	(1.22)	(1.18)
GDP growth change	-0.20	-0.22
	(0.62)	(0.62)
Initial household balance	0.095	0.10
	(0.36)	(0.36)
Constant	-3.63	-3.71
	(10.2)	(10.2)
Observations	397	397
Adjusted R-squared	0.050	0.048
Period FE	Yes	Yes

Note: Robust standard errors clustered by governments and parties in parentheses. * p<.1, ** p<.05, *** p<.001 (two-tailed tests).

Table A3: Effect of social spending changes – interaction models

	(1)	(2)	(3)	(4)	(5)	(6)
Social spending change	-2.14		3.46		0.023	
	(2.16)		(2.37)		(2.22)	
Social spending retrenchment		0.41		0.13		-0.084
		(1.88)		(2.15)		(2.04)
Social spending expansion		-2.07		4.27*		-0.060
		(2.03)		(2.19)		(2.40)
Social spending change * Pro-welfare party	3.82*					
	(2.06)					
Social spending change * High clarity of responsibility			-9.39***			
			(2.43)			
Social spending change * Low fiscal pressure					-3.49	
					(2.90)	
Social spending retrenchment * Pro-welfare party		-1.26				
		(2.20)				
Social spending retrenchment * High clarity of responsibility				0.69		
				(3.08)		
Social spending retrenchment * Low fiscal pressure						1.27
						(5.20)
Social spending expansion * Pro-welfare party		3.25				
		(2.41)				
Social spending expansion * High clarity of responsibility				-9.89***		
				(2.61)		
Social spending expansion * Low fiscal pressure						-2.50
						(3.15)
Pro-welfare party	-9.40***	-10.1***				
	(2.78)	(3.69)				
High clarity of responsibility			5.66	10.4*		
			(4.06)	(5.82)		
Low fiscal pressure					1.59	1.72
					(4.11)	(5.66)
Government duration in days	-0.0091**	-0.0088**	-0.0091**	-0.01**	-0.01**	-0.01**
	(0.0038)	(0.0039)	(0.0039)	(0.0041)	(0.0040)	(0.0042)
Initial vote share	0.057	0.059	-0.0065	0.00093	0.0081	0.0078
	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)
Effective number of parties	1.77*	1.76*	1.94*	2.21**	1.70*	1.69
	(0.98)	(0.99)	(0.98)	(1.00)	(1.00)	(1.02)
Average unemployment	-0.096	-0.082	-0.061	-0.067	-0.25	-0.25
	(0.48)	(0.48)	(0.45)	(0.44)	(0.46)	(0.46)
Unemployment change	-0.65	-0.65	-0.24	-0.12	-0.52	-0.51
	(0.69)	(0.68)	(0.63)	(0.61)	(0.66)	(0.67)
Average GDP growth	0.74	0.74	1.45	1.40	1.01	1.02
	(1.19)	(1.16)	(1.08)	(1.09)	(1.21)	(1.23)
GDP growth change	-0.29	-0.30	-0.28	-0.34	-0.28	-0.28
	(0.62)	(0.62)	(0.60)	(0.60)	(0.63)	(0.63)
Initial household balance	0.16	0.17	0.10	0.10	0.12	0.12
	(0.36)	(0.36)	(0.34)	(0.35)	(0.45)	(0.46)
Constant	-4.06	-3.83	-10.6	-13.7	-4.26	-4.32
	(9.93)	(9.95)	(9.74)	(10.1)	(10.6)	(10.8)
Observations	397	397	397	397	397	397
Adjusted R-squared	0.071	0.066	0.079	0.080	0.048	0.043
Period FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: Robust standard errors clustered by governments and parties in parentheses. * p<.1, ** p<.05, *** p<.001 (two-tailed tests).

Table A4: Effect of short-term social spending changes

	(1)	(2)
Short-term social spending change	-1.19	
	(1.04)	
Short-term social spending retrenchment		0.055
		(0.94)
Short-term social spending expansion		-1.43
		(1.27)
Government duration in days	-0.0098**	-0.0096**
	(0.0041)	(0.0042)
Initial vote share	-0.015	-0.018
	(0.13)	(0.13)
Effective number of parties	1.46	1.45
	(1.01)	(1.02)
Average unemployment	-0.27	-0.24
	(0.48)	(0.48)
Unemployment change	-0.84	-0.82
	(0.54)	(0.55)
Average GDP growth	0.76	0.71
	(1.20)	(1.20)
GDP growth change	-0.28	-0.32
	(0.63)	(0.63)
Initial household balance	0.047	0.096
	(0.33)	(0.35)
Constant	-2.31	-1.72
	(10.0)	(10.3)
Observations	418	418
Adjusted R-squared	0.048	0.047
Period FE	Yes	Yes

Note: Robust standard errors clustered by governments and parties in parentheses. * $p<.1$, ** $p<.05$, *** $p<.001$ (two-tailed tests).

Table A5: Effect of unemployment benefit generosity changes

	(1)	(2)
UB generosity change	0.98	
	(1.19)	
UB generosity retrenchment		-0.41
		(1.37)
UB generosity expansion		0.80
		(1.25)
Government duration in days	-0.0081**	-0.0082**
	(0.0039)	(0.0039)
Initial vote share	-0.036	-0.036
	(0.11)	(0.11)
Effective number of parties	1.33	1.32
	(0.96)	(0.96)
Average unemployment	-0.32	-0.33
	(0.46)	(0.45)
Unemployment change	-0.70	-0.70
	(0.54)	(0.54)
Average GDP growth	0.42	0.41
	(1.10)	(1.11)
GDP growth change	-0.26	-0.26
	(0.50)	(0.50)
Initial household balance	0.026	0.021
	(0.30)	(0.30)
Constant	-1.83	-1.76
	(9.36)	(9.30)
Observations	463	463
Adjusted R-squared	0.063	0.060
Period FE	Yes	Yes

Note: Robust standard errors clustered by governments and parties in parentheses. * p<.1, ** p<.05, *** p<.001 (two-tailed tests).

Table A6: Effect of unemployment benefit generosity changes – interaction models

	(1)	(2)	(3)	(4)	(5)	(6)
UB generosity change	1.92		0.19		1.97	
	(1.50)		(1.63)		(1.33)	
UB generosity retrenchment		-0.14		0.51		-2.25*
		(1.53)		(2.09)		(1.34)
UB generosity expansion		2.49*		0.56		0.41
		(1.46)		(1.47)		(1.39)
UB generosity change * Pro-welfare party	-1.30					
	(2.17)					
UB generosity change * High clarity of responsibility			2.04			
			(2.02)			
UB generosity change * Low fiscal pressure					-3.95*	
					(2.05)	
UB generosity retrenchment * Pro-welfare party		-0.63				
		(2.01)				
UB generosity retrenchment * High clarity of responsibility				-1.49		
				(2.63)		
UB generosity retrenchment * Low fiscal pressure						5.48***
						(1.53)
UB generosity expansion * Pro-welfare party		-2.31				
		(2.03)				
UB generosity expansion * High clarity of responsibility				1.34		
				(2.03)		
UB generosity expansion * Low fiscal pressure						0.91
						(2.72)
Pro-welfare party	-7.78***	-6.33**				
	(2.43)	(2.78)				
High clarity of responsibility			4.15	4.40		
			(3.78)	(4.57)		
Low fiscal pressure					0.54	-3.07
					(3.61)	(4.47)
Government duration in days	-0.0072*	-0.0076**	-0.0084**	-0.0088**	-0.0080**	-0.0081**
	(0.0036)	(0.0037)	(0.0040)	(0.0041)	(0.0039)	(0.0039)
Initial vote share	0.031	0.032	-0.062	-0.060	-0.032	-0.031
	(0.11)	(0.12)	(0.12)	(0.12)	(0.11)	(0.11)
Effective number of parties	1.49	1.47	1.96**	1.97**	1.30	1.30
	(0.95)	(0.96)	(0.97)	(0.99)	(0.97)	(0.96)
Average unemployment	-0.21	-0.25	-0.33	-0.36	-0.33	-0.23
	(0.47)	(0.47)	(0.46)	(0.45)	(0.46)	(0.46)
Unemployment change	-0.75	-0.75	-0.86	-0.85	-0.76	-0.74
	(0.54)	(0.54)	(0.57)	(0.56)	(0.54)	(0.53)
Average GDP growth	0.41	0.40	0.29	0.25	0.31	0.40
	(1.03)	(1.05)	(1.09)	(1.11)	(1.12)	(1.11)
GDP growth change	-0.36	-0.38	-0.29	-0.30	-0.34	-0.37
	(0.51)	(0.51)	(0.49)	(0.50)	(0.50)	(0.49)
Initial household balance	0.063	0.025	0.020	0.0031	0.016	0.061
	(0.30)	(0.30)	(0.30)	(0.30)	(0.39)	(0.41)
Constant	-2.96	-3.33	-4.88	-4.84	-1.77	-1.22
	(9.02)	(9.05)	(8.81)	(8.90)	(9.29)	(9.16)
Observations	463	463	463	463	463	463
Adjusted R-squared	0.081	0.078	0.064	0.060	0.063	0.064
Period FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: Robust standard errors clustered by governments and parties in parentheses. * $p<.1$, ** $p<.05$, *** $p<.001$ (two-tailed tests).

Table A7: Effect of visible and short-term unemployment benefit generosity changes

	(1)	(2)	(3)	(4)
Visible UB generosity change			1.02	
			(1.13)	
Visible UB generosity retrenchment				-1.06
				(1.06)
Visible UB generosity expansion				0.30
				(1.18)
Short-term UB generosity change	1.90*			
	(1.10)			
Short-term UB generosity retrenchment		-1.22		
		(1.18)		
Short-term UB generosity expansion		1.18		
		(1.07)		
Government duration in days	-0.0067*	-0.0067*	-0.0083**	-0.0080*
	(0.0039)	(0.0039)	(0.0040)	(0.0041)
Initial vote share	-0.052	-0.052	-0.028	-0.031
	(0.11)	(0.11)	(0.11)	(0.11)
Effective number of parties	1.28	1.28	1.40	1.40
	(0.97)	(0.96)	(0.96)	(0.96)
Average unemployment	-0.32	-0.32	-0.32	-0.31
	(0.46)	(0.46)	(0.46)	(0.46)
Unemployment change	-0.52	-0.52	-0.74	-0.76
	(0.56)	(0.56)	(0.52)	(0.52)
Average GDP growth	0.57	0.56	0.46	0.47
	(1.11)	(1.11)	(1.10)	(1.10)
GDP growth change	-0.27	-0.27	-0.23	-0.24
	(0.47)	(0.47)	(0.48)	(0.48)
Initial household balance	0.0096	0.011	0.047	0.047
	(0.30)	(0.30)	(0.30)	(0.30)
Constant	-3.14	-3.10	-1.97	-1.98
	(9.34)	(9.30)	(9.37)	(9.37)
Observations	470	470	468	468
Adjusted R-squared	0.065	0.063	0.062	0.061
Period FE	Yes	Yes	Yes	Yes

Note: Robust standard errors clustered by governments and parties in parentheses. * $p<.1$, ** $p<.05$, *** $p<.001$ (two-tailed tests).

Table A8: Effect of pension generosity changes

	(1)	(2)
Pension generosity change	-0.83	
	(1.41)	
Pension generosity retrenchment		-0.064
		(1.39)
Pension generosity expansion		-0.99
		(1.27)
Government duration in days	-0.0068*	-0.0066
	(0.0040)	(0.0040)
Initial vote share	-0.056	-0.058
	(0.11)	(0.11)
Effective number of parties	1.41	1.38
	(0.98)	(0.99)
Average unemployment	-0.37	-0.35
	(0.49)	(0.49)
Unemployment change	-0.65	-0.70
	(0.56)	(0.58)
Average GDP growth	0.40	0.35
	(1.16)	(1.13)
GDP growth change	-0.22	-0.24
	(0.51)	(0.50)
Initial household balance	0.091	0.10
	(0.33)	(0.32)
Constant	-2.30	-1.95
	(9.70)	(9.69)
Observations	454	454
Adjusted R-squared	0.062	0.060
Period FE	Yes	Yes

Note: Robust standard errors clustered by governments and parties in parentheses. * $p<.1$, ** $p<.05$, *** $p<.001$ (two-tailed tests).

Table A9: Effect of pension generosity changes – interaction models

	(1)	(2)	(3)	(4)	(5)	(6)
Pension generosity change	-1.66		1.12		0.15	
	(1.80)		(1.69)		(1.74)	
Pension generosity retrenchment		0.55		-0.93		-2.00
		(2.22)		(1.37)		(1.39)
Pension generosity expansion		-1.42		0.41		-1.28
		(1.46)		(2.12)		(1.53)
Pension generosity change * Pro-welfare party	1.97					
	(2.36)					
Pension generosity change * High clarity of responsibility			-3.15			
			(2.09)			
Pension generosity change * Low fiscal pressure					-3.89	
					(2.79)	
Pension generosity retrenchment * Pro-welfare party		-0.90				
		(2.64)				
Pension generosity retrenchment * High clarity of responsibility				2.03		
				(2.23)		
Pension generosity retrenchment * Low fiscal pressure						5.87**
						(2.96)
Pension generosity expansion * Pro-welfare party		1.49				
		(2.19)				
Pension generosity expansion * High clarity of responsibility				-1.88		
				(2.53)		
Pension generosity expansion * Low fiscal pressure						0.56
						(2.21)
Pro-welfare party	-9.44***	-9.41***				
	(2.70)	(3.21)				
High clarity of responsibility			4.54	3.82		
			(3.75)	(4.54)		
Low fiscal pressure					1.78	-2.11
					(3.82)	(4.03)
Government duration in days	-0.0059	-0.0058	-0.0066	-0.0065	-0.0066*	-0.0059
	(0.0037)	(0.0037)	(0.0040)	(0.0040)	(0.0039)	(0.0038)
Initial vote share	0.014	0.013	-0.079	-0.081	-0.061	-0.072
	(0.11)	(0.12)	(0.12)	(0.12)	(0.11)	(0.11)
Effective number of parties	1.62	1.61	1.90*	1.85*	1.36	1.32
	(0.98)	(0.99)	(0.99)	(1.00)	(0.99)	(1.00)
Average unemployment	-0.27	-0.27	-0.34	-0.32	-0.34	-0.27
	(0.49)	(0.50)	(0.49)	(0.49)	(0.49)	(0.50)
Unemployment change	-0.71	-0.73	-0.81	-0.82	-0.82	-0.85
	(0.55)	(0.58)	(0.57)	(0.61)	(0.59)	(0.62)
Average GDP growth	0.35	0.32	0.42	0.40	0.27	0.39
	(1.06)	(1.04)	(1.16)	(1.14)	(1.17)	(1.12)
GDP growth change	-0.33	-0.34	-0.27	-0.27	-0.19	-0.24
	(0.52)	(0.51)	(0.51)	(0.51)	(0.50)	(0.50)
Initial household balance	0.12	0.12	0.062	0.068	0.12	0.097
	(0.32)	(0.32)	(0.32)	(0.33)	(0.42)	(0.42)
Constant	-2.99	-2.83	-6.54	-5.95	-2.37	-1.87
	(9.32)	(9.36)	(9.12)	(9.56)	(9.74)	(9.67)
Observations	454	454	454	454	454	454
Adjusted R-squared	0.085	0.081	0.064	0.060	0.062	0.064
Period FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: Robust standard errors clustered by governments and parties in parentheses. * p<.1, ** p<.05, *** p<.001 (two-tailed tests).

Table A10: Effect of visible and short-term pension generosity changes

	(1)	(2)	(3)	(4)
Visible UB generosity change			-0.078	
			(1.46)	
Visible UB generosity retrenchment				-1.89
				(1.25)
Visible UB generosity expansion				-1.40
				(1.25)
Short-term UB generosity change	1.26			
	(1.28)			
Short-term UB generosity retrenchment		-1.26		
		(1.23)		
Short-term UB generosity expansion		0.42		
		(1.15)		
Government duration in days	-0.0072*	-0.0071*	-0.0070*	-0.0063
	(0.0040)	(0.0040)	(0.0040)	(0.0040)
Initial vote share	-0.047	-0.053	-0.052	-0.069
	(0.11)	(0.11)	(0.11)	(0.11)
Effective number of parties	1.37	1.31	1.40	1.25
	(0.97)	(0.98)	(0.98)	(0.98)
Average unemployment	-0.32	-0.33	-0.30	-0.20
	(0.48)	(0.47)	(0.45)	(0.44)
Unemployment change	-0.74	-0.78	-0.66	-0.78
	(0.56)	(0.57)	(0.57)	(0.56)
Average GDP growth	0.59	0.54	0.48	0.32
	(1.12)	(1.13)	(1.12)	(1.07)
GDP growth change	-0.21	-0.24	-0.26	-0.31
	(0.50)	(0.50)	(0.49)	(0.49)
Initial household balance	0.062	0.063	0.086	0.12
	(0.31)	(0.31)	(0.32)	(0.31)
Constant	-3.16	-2.10	-2.82	-1.23
	(9.63)	(9.76)	(9.55)	(9.34)
Observations	463	463	458	458
Adjusted R-squared	0.062	0.061	0.061	0.064
Period FE	Yes	Yes	Yes	Yes

Note: Robust standard errors clustered by governments and parties in parentheses. * $p<.1$, ** $p<.05$, *** $p<.001$ (two-tailed tests).

Table A11: Effect of unemployment benefit reforms

	(1)	(2)
UB reform	0.98	
	(3.54)	
UB retrenchment reform		-2.11
		(3.60)
UB expansion reform		-0.50
		(3.46)
Government duration in days	-0.016*	-0.014
	(0.0093)	(0.0086)
Initial vote share	-0.28	-0.29
	(0.21)	(0.22)
Effective number of parties	-0.54	-0.24
	(3.64)	(3.47)
Average unemployment	0.47	0.80
	(1.33)	(1.67)
Unemployment change	2.04	1.91
	(1.44)	(1.52)
Average GDP growth	4.56	3.96
	(4.88)	(5.17)
GDP growth change	-0.51	-0.59
	(0.76)	(0.80)
Initial household balance	-0.60	-0.57
	(0.93)	(1.00)
Constant	7.95	6.60
	(20.5)	(19.5)
Observations	118	118
Adjusted R-squared	0.053	0.047
Period FE	Yes	Yes

Note: Robust standard errors clustered by governments and parties in parentheses. * p<.1, ** p<.05, *** p<.001 (two-tailed tests).

Table A12: Effect of unemployment benefit reforms – interaction models

	(1)	(2)	(3)	(4)
UB reform	0.18		0.44	
	(5.10)		(3.49)	
UB retrenchment reform		-2.42		2.48
		(5.47)		(3.02)
UB expansion reform		-1.67		1.62
		(3.88)		(2.91)
UB reform * Pro-welfare party	3.35			
	(4.13)			
UB reform * High clarity of responsibility			5.50	
			(7.38)	
UB retrenchment reform * Pro-welfare party		1.65		
		(5.02)		
UB retrenchment reform * High clarity of responsibility				-11.9*
				(6.45)
UB expansion reform * Pro-welfare party		6.19		
		(3.72)		
UB expansion reform * High clarity of responsibility				-0.57
				(6.45)
Pro-welfare party	-9.16	-16.7		
	(5.47)	(10.3)		
High clarity of responsibility			16.0**	29.8*
			(7.71)	(14.7)
Government duration in days	-0.017*	-0.014*	-0.019*	-0.020*
	(0.0093)	(0.0074)	(0.011)	(0.011)
Initial vote share	-0.14	-0.14	-0.26	-0.36
	(0.21)	(0.23)	(0.23)	(0.27)
Effective number of parties	-0.45	-0.38	2.78	2.38
	(3.43)	(3.19)	(2.73)	(2.57)
Average unemployment	0.99	0.79	0.094	0.051
	(1.38)	(1.62)	(1.60)	(1.72)
Unemployment change	1.68	2.05	1.70	2.37*
	(1.39)	(1.64)	(1.45)	(1.29)
Average GDP growth	4.18	4.59	5.65	5.48
	(4.69)	(5.19)	(4.59)	(4.53)
GDP growth change	-0.65	-0.49	-0.69	-0.74
	(0.77)	(0.79)	(0.71)	(0.73)
Initial household balance	-0.49	-0.53	-0.91	-0.96
	(1.00)	(1.07)	(0.98)	(0.92)
Constant	6.35	6.95	-9.76	-8.91
	(19.4)	(17.9)	(16.8)	(17.5)
Observations	118	118	118	118
Adjusted R-squared	0.058	0.049	0.094	0.114
Period FE	Yes	Yes	Yes	Yes

Note: Robust standard errors clustered by governments and parties in parentheses. * p<.1, ** p<.05, *** p<.001 (two-tailed tests).

Table A13: Effect of visible and short-term unemployment benefit reforms

	(1)	(2)	(3)	(4)
Visible UB reform			0.11	
			(3.11)	
Visible UB retrenchment reform				-1.90
				(3.38)
Visible UB expansion reform				-1.47
				(2.99)
Short-term UB reform	1.70			
	(2.90)			
Short-term UB retrenchment reform		-3.42		
		(3.04)		
Short-term UB expansion reform		-0.69		
		(2.67)		
Government duration in days	-0.018*	-0.017*	-0.015	-0.013
	(0.0098)	(0.0092)	(0.0092)	(0.0088)
Initial vote share	-0.27	-0.30	-0.28	-0.30
	(0.20)	(0.21)	(0.20)	(0.22)
Effective number of parties	-1.08	-0.71	-0.31	0.088
	(3.69)	(3.60)	(3.61)	(3.44)
Average unemployment	0.56	0.56	0.48	0.76
	(1.33)	(1.29)	(1.30)	(1.48)
Unemployment change	2.20	2.16	1.96	1.80
	(1.47)	(1.43)	(1.41)	(1.46)
Average GDP growth	4.84	3.96	4.33	3.75
	(4.80)	(4.84)	(4.77)	(4.87)
GDP growth change	-0.59	-0.73	-0.53	-0.60
	(0.70)	(0.73)	(0.74)	(0.76)
Initial household balance	-0.48	-0.43	-0.52	-0.43
	(0.89)	(0.91)	(0.92)	(1.01)
Constant	10.6	13.0	6.86	5.65
	(20.9)	(21.9)	(20.1)	(19.6)
Observations	118	118	118	118
Adjusted R-squared	0.057	0.063	0.052	0.048
Period FE	Yes	Yes	Yes	Yes

Note: Robust standard errors clustered by governments and parties in parentheses. * $p<.1$, ** $p<.05$, *** $p<.001$ (two-tailed tests).

Table A14: Effect of pension reforms

	(1)	(2)
Pension reform	0.18	
	(2.48)	
Pension retrenchment reform		2.91*
		(1.70)
Pension expansion reform		2.29
		(2.88)
Government duration in days	-0.015	-0.021**
	(0.0100)	(0.0094)
Initial vote share	-0.28	-0.23
	(0.20)	(0.22)
Effective number of parties	-0.27	-0.31
	(3.52)	(3.50)
Average unemployment	0.50	0.43
	(1.34)	(1.25)
Unemployment change	1.98	1.64
	(1.31)	(1.26)
Average GDP growth	4.31	4.11
	(4.49)	(4.41)
GDP growth change	-0.53	-0.69
	(0.73)	(0.66)
Initial household balance	-0.54	-0.51
	(0.96)	(0.93)
Constant	6.36	8.21
	(23.6)	(23.9)
Observations	118	118
Adjusted R-squared	0.052	0.059
Period FE	Yes	Yes

Note: Robust standard errors clustered by governments and parties in parentheses. * p<.1, ** p<.05, *** p<.001 (two-tailed tests).

Table A15: Effect of pension reforms – interaction models

	(1)	(2)	(3)	(4)
Pension reform	0.18		-5.56	
	(2.66)		(4.88)	
Pension retrenchment reform		1.65		6.41
		(1.56)		(4.62)
Pension expansion reform		1.52		-3.92
		(4.77)		(4.91)
Pension reform *	2.05			
Pro-welfare party	(4.19)			
Pension reform *			6.15	
High clarity of responsibility			(6.19)	
Pension retrenchment reform *		3.04		
Pro-welfare party		(4.11)		
Pension retrenchment reform *				-4.73
High clarity of responsibility				(5.10)
Pension expansion reform *		2.60		
Pro-welfare party		(5.48)		
Pension expansion reform *				6.06
High clarity of responsibility				(7.47)
Pro-welfare party	-9.43	-14.6		
	(6.59)	(12.0)		
High clarity of responsibility			12.0	8.78
			(8.66)	(15.9)
Government duration in days	-0.016	-0.020**	-0.015	-0.018
	(0.0099)	(0.0081)	(0.012)	(0.012)
Initial vote share	-0.17	-0.12	-0.29	-0.25
	(0.21)	(0.22)	(0.22)	(0.25)
Effective number of parties	0.038	-0.15	2.79	2.55
	(3.33)	(3.18)	(2.53)	(2.65)
Average unemployment	1.09	1.01	0.30	0.21
	(1.37)	(1.23)	(1.39)	(1.41)
Unemployment change	1.57	1.40	1.59	1.40
	(1.34)	(1.20)	(1.33)	(1.14)
Average GDP growth	3.56	3.34	4.89	4.71
	(4.51)	(4.36)	(4.13)	(4.16)
GDP growth change	-0.76	-0.85	-0.72	-0.80
	(0.73)	(0.66)	(0.69)	(0.67)
Initial household balance	-0.51	-0.42	-0.48	-0.43
	(1.01)	(1.03)	(1.03)	(0.92)
Constant	4.50	7.28	-10.8	-7.61
	(22.4)	(22.3)	(18.8)	(19.4)
Observations	118	118	118	118
Adjusted R-squared	0.053	0.056	0.084	0.073
Period FE	Yes	Yes	Yes	Yes

Note: Robust standard errors clustered by governments and parties in parentheses. * p<.1, ** p<.05, *** p<.001 (two-tailed tests).

Table A16: Effect of visible and short-term pension reforms

	(1)	(2)	(3)	(4)
Visible Pension reform			-1.55	
			(2.28)	
Visible Pension retrenchment reform				2.85
				(1.83)
Visible Pension expansion reform				0.20
				(3.13)
Short-term Pension reform	1.37			
	(2.72)			
Short-term Pension retrenchment reform		-1.15		
		(2.01)		
Short-term Pension expansion reform		1.35		
		(3.02)		
Government duration in days	-0.016	-0.016	-0.015	-0.018**
	(0.0098)	(0.0098)	(0.0090)	(0.0087)
Initial vote share	-0.28	-0.29	-0.28	-0.25
	(0.21)	(0.22)	(0.21)	(0.22)
Effective number of parties	-0.41	-0.42	-0.10	-0.45
	(3.51)	(3.58)	(3.55)	(3.48)
Average unemployment	0.65	0.64	0.25	0.19
	(1.37)	(1.34)	(1.40)	(1.39)
Unemployment change	2.05	2.07	1.83	1.81
	(1.43)	(1.40)	(1.32)	(1.30)
Average GDP growth	4.16	4.18	4.27	4.23
	(4.54)	(4.46)	(4.41)	(4.35)
GDP growth change	-0.53	-0.53	-0.45	-0.52
	(0.71)	(0.70)	(0.69)	(0.65)
Initial household balance	-0.52	-0.52	-0.39	-0.50
	(0.88)	(0.88)	(0.90)	(0.93)
Constant	5.90	6.13	8.05	10.0
	(21.9)	(23.8)	(21.1)	(20.8)
Observations	118	118	118	118
Adjusted R-squared	0.054	0.045	0.054	0.051
Period FE	Yes	Yes	Yes	Yes

Note: Robust standard errors clustered by governments and parties in parentheses. * $p<.1$, ** $p<.05$, *** $p<.001$ (two-tailed tests).

Robustness test 1: Samples restricted to pro-welfare parties

Figure A2: The effect of social spending changes
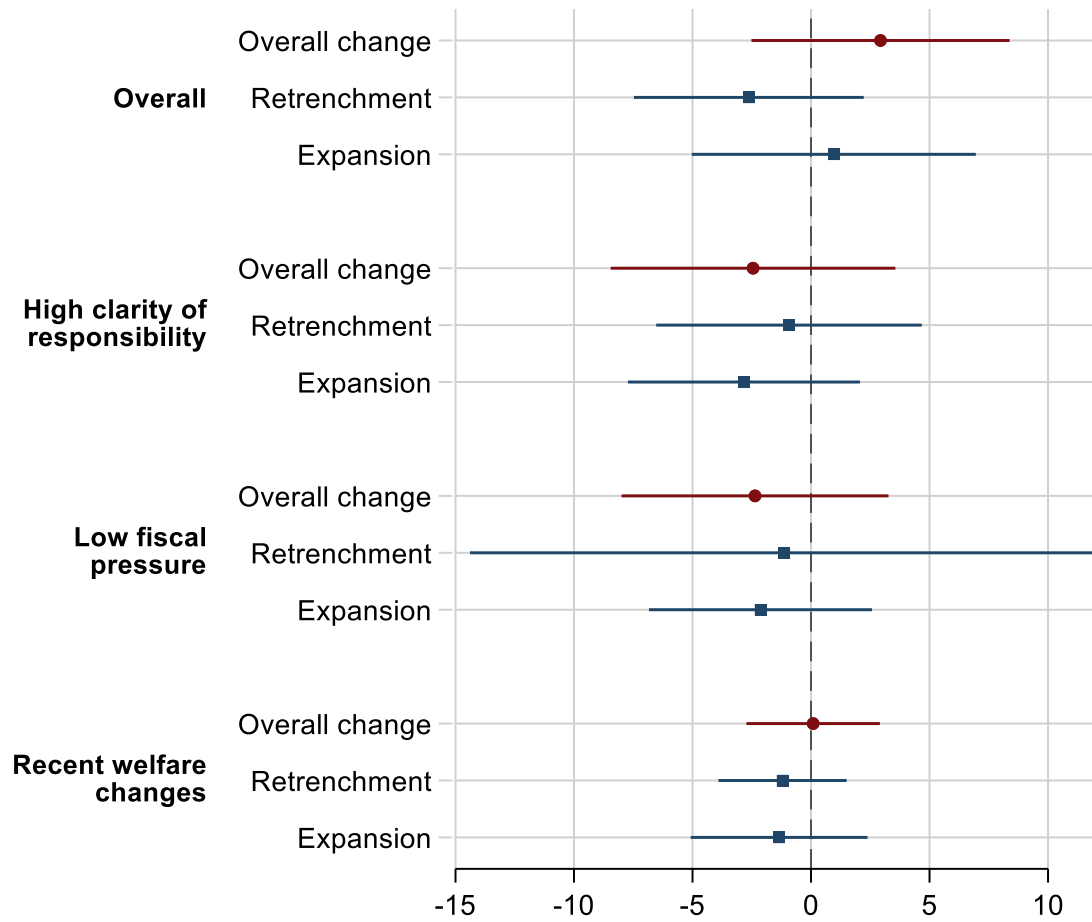
Note: All regressions include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties.
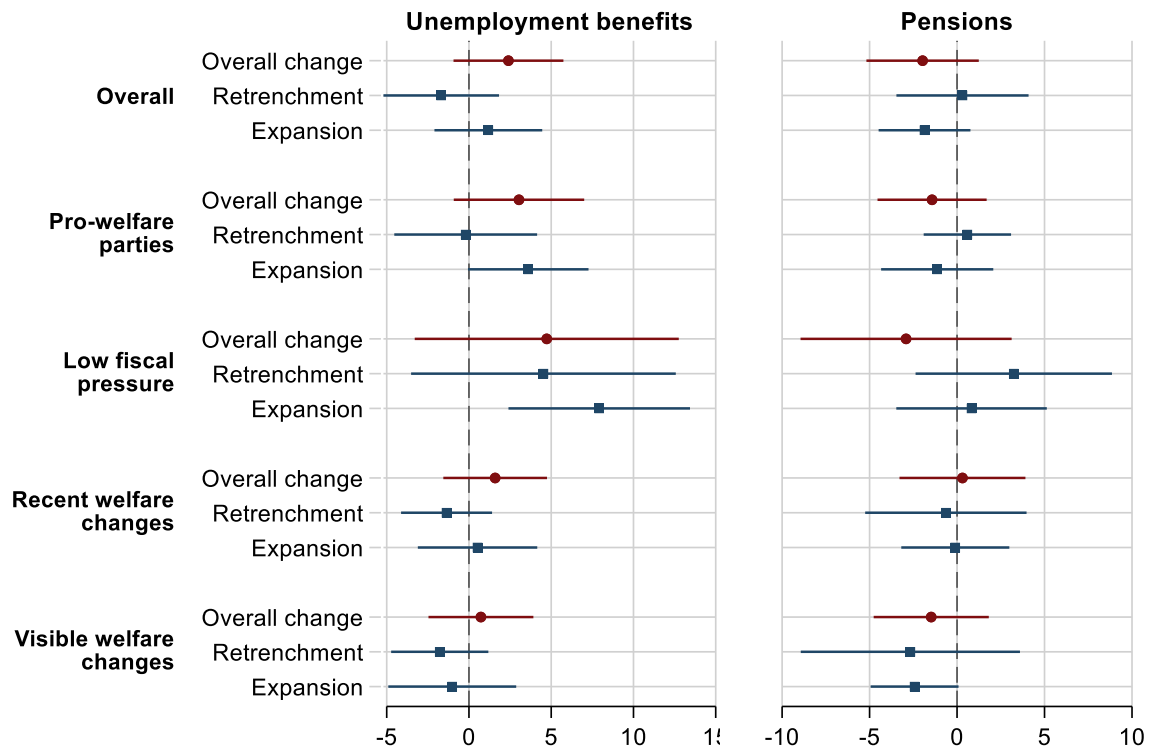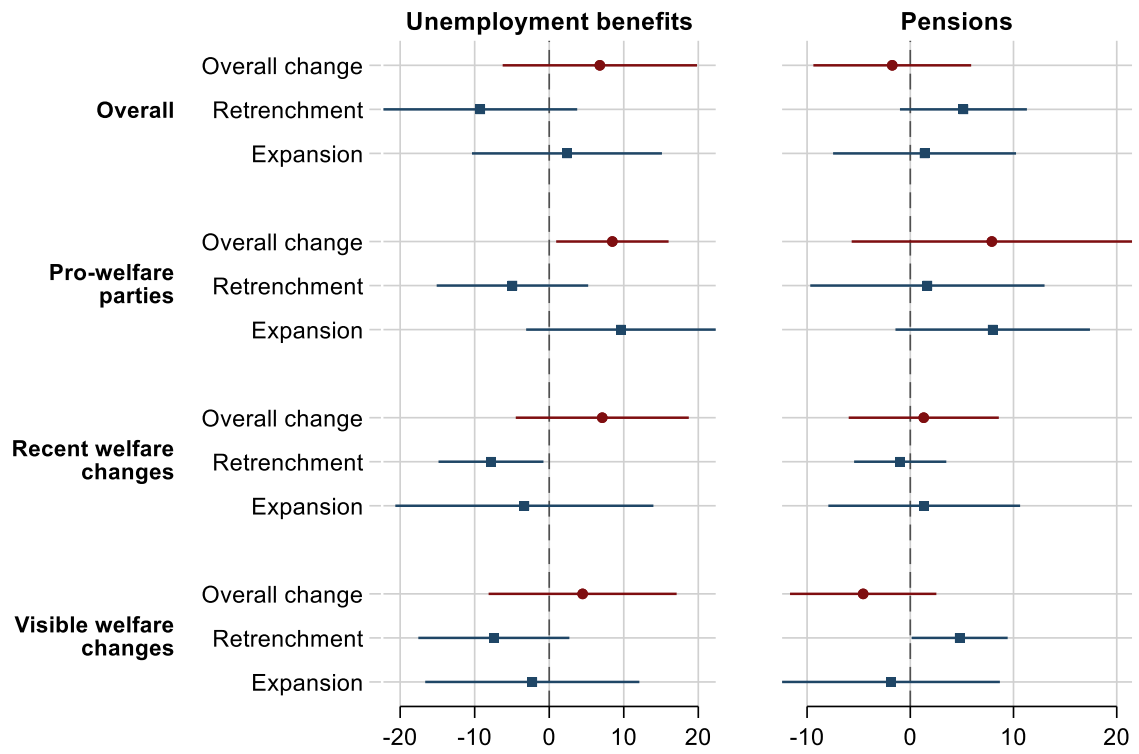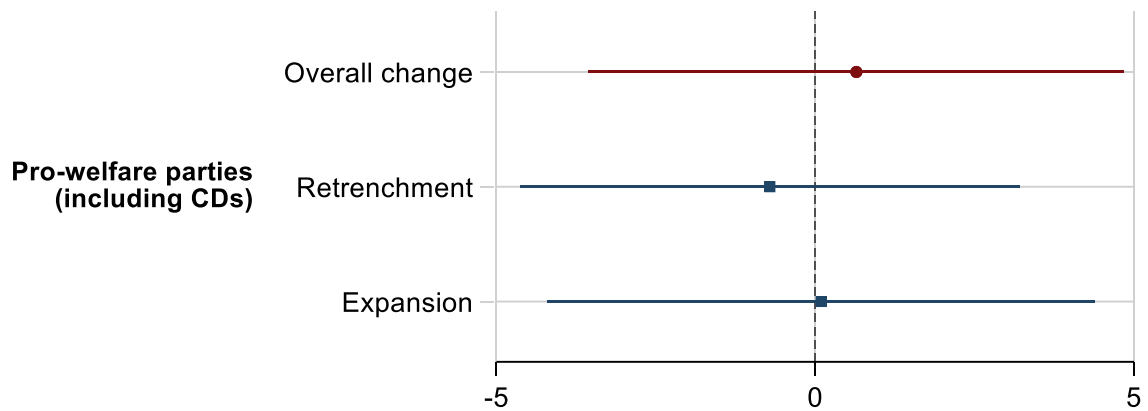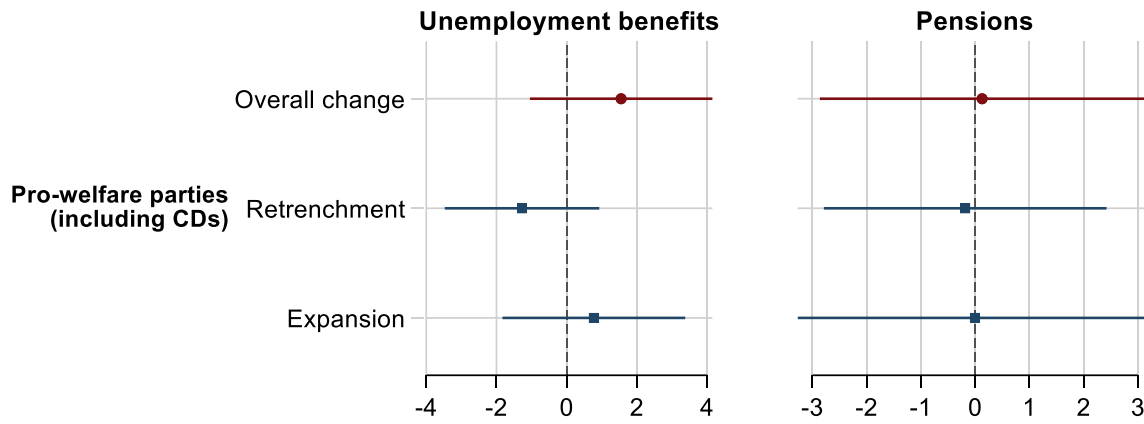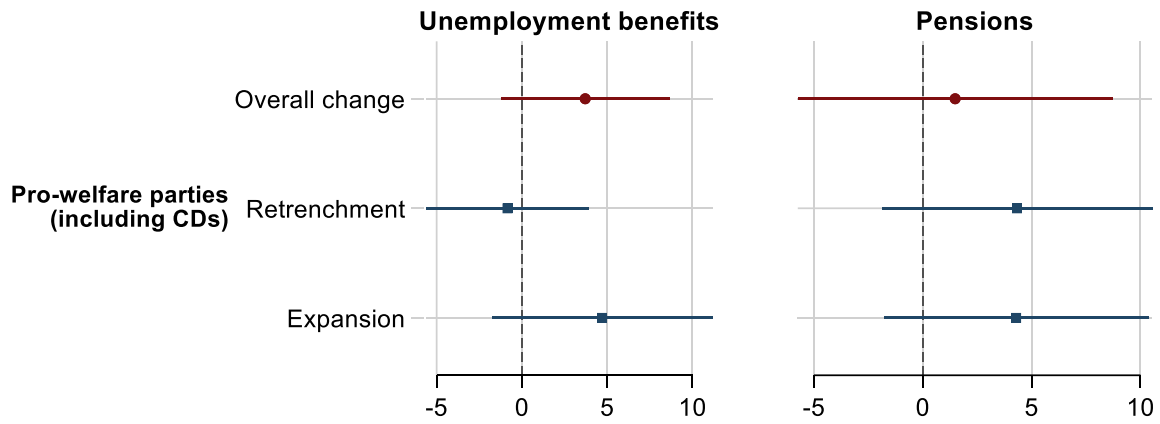
Figure A3: The effect of welfare generosity changes
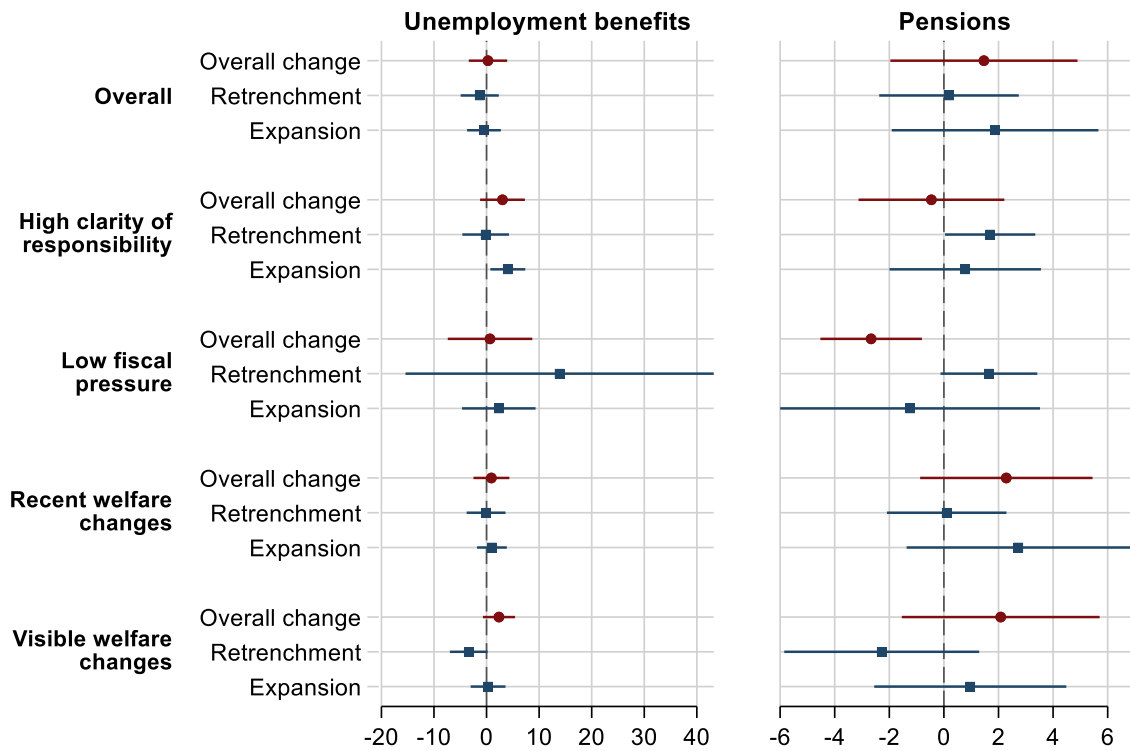
Note: All regressions include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties.

Figure A4: The effect of welfare reforms
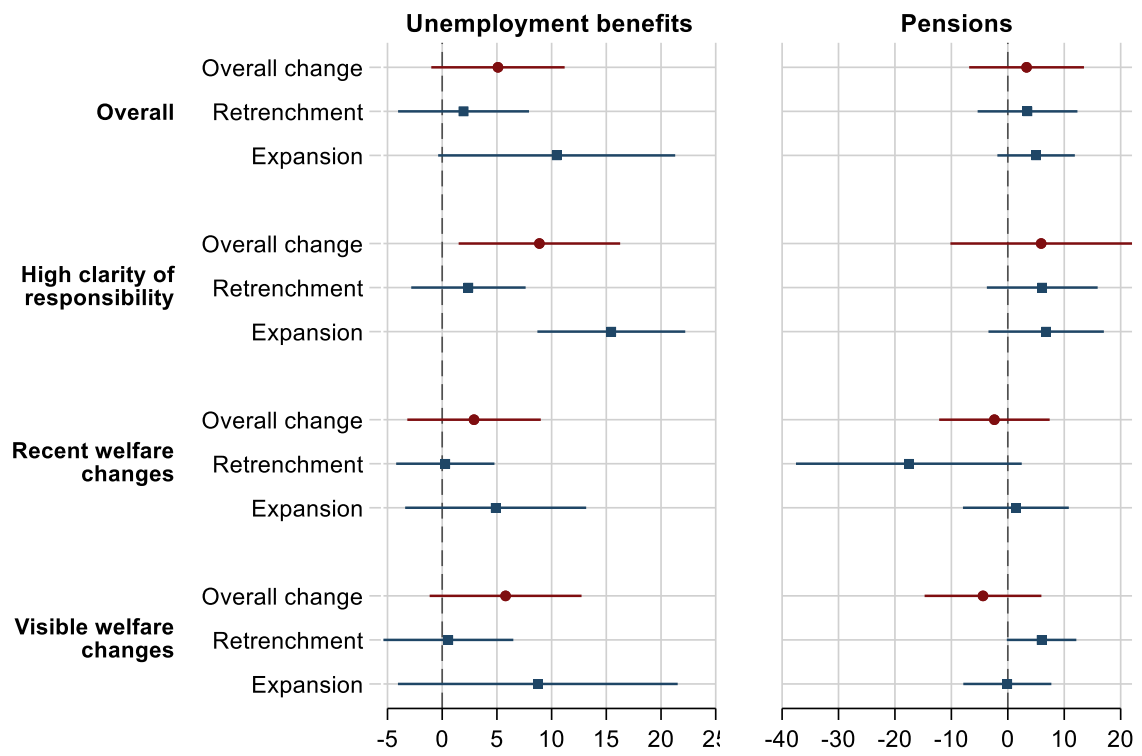
Note: All regressions include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties.

Robustness test 2: Samples restricted to high clarity of responsibility governments

Figure A5: The effect of social spending changes
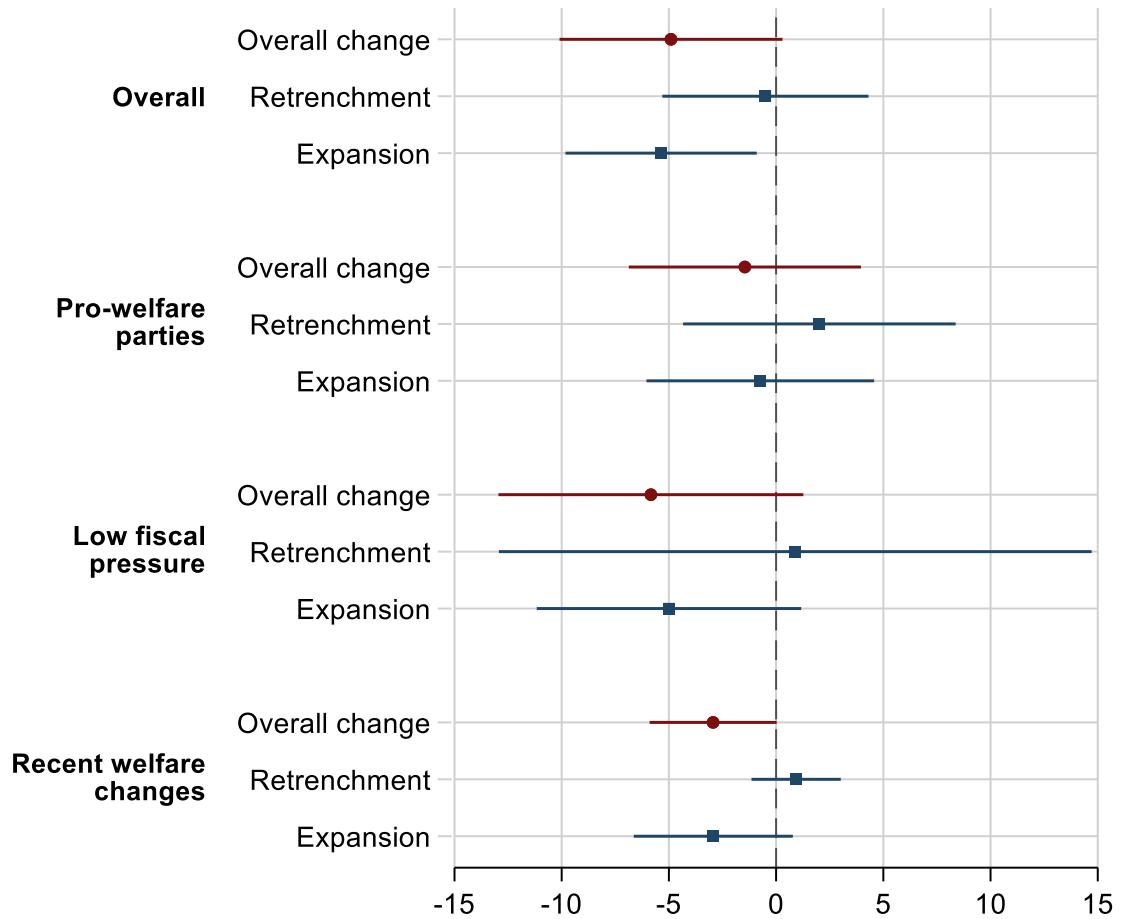
Note: All regressions include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties.

Figure A6: The effect of welfare generosity changes

Note: All regressions include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties.
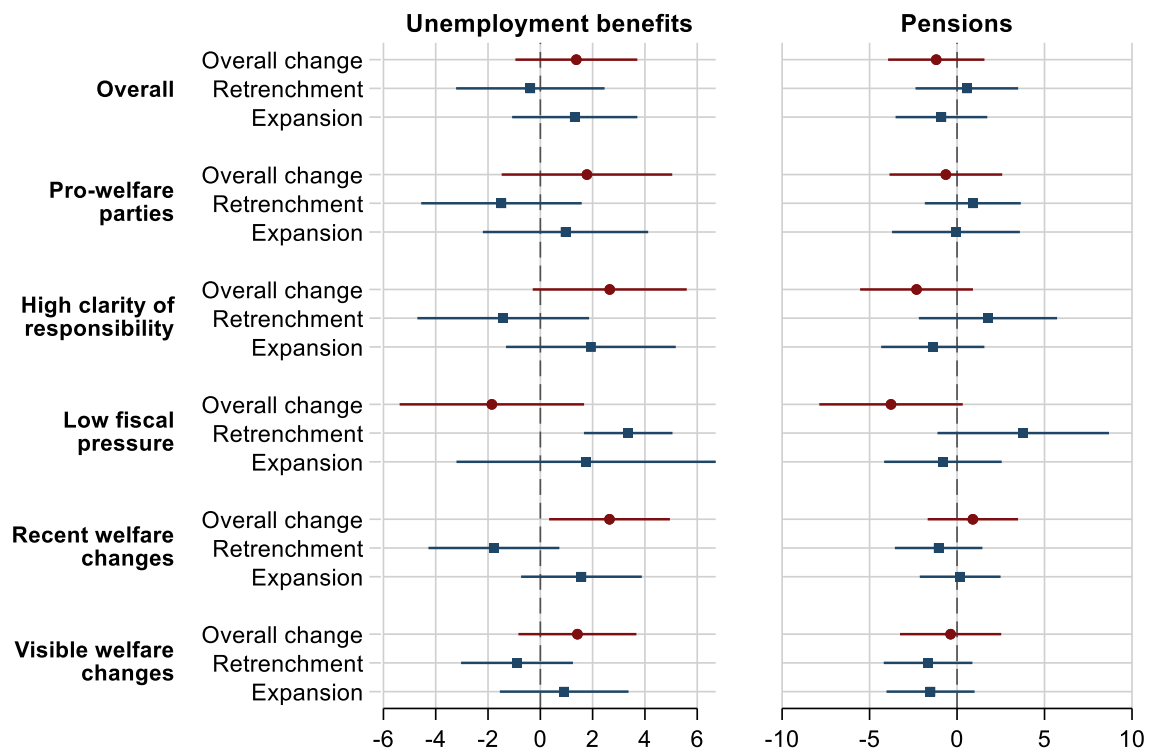
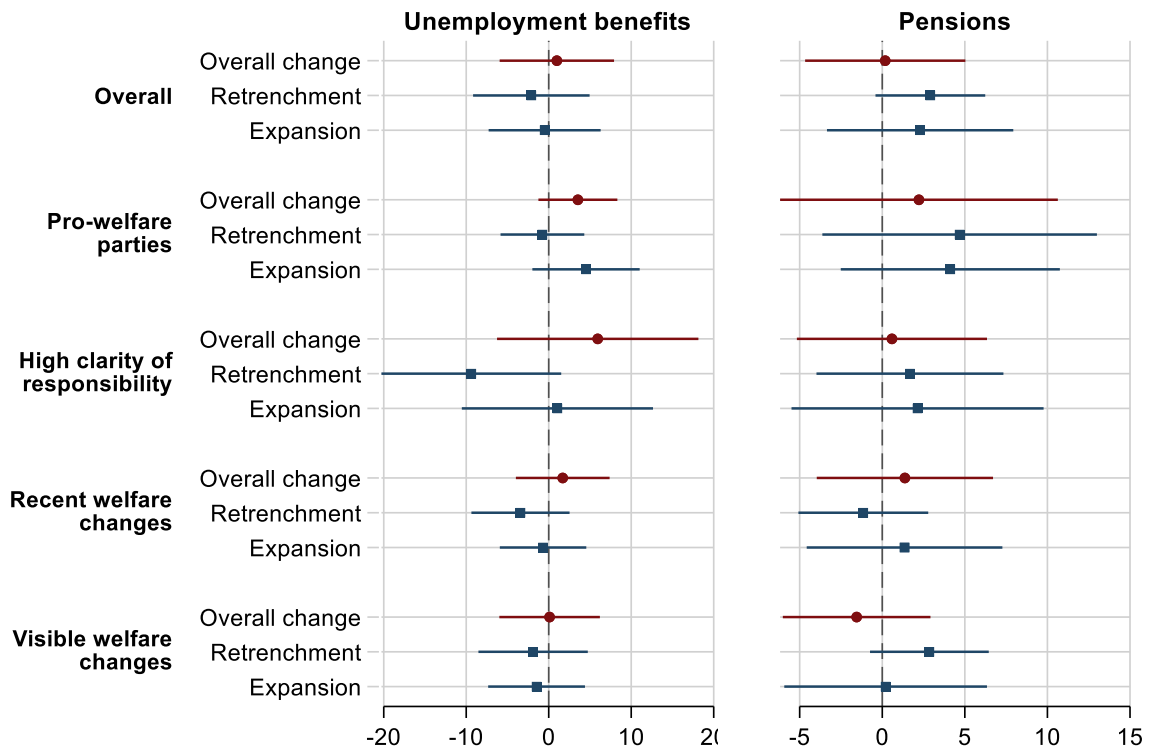**Figure A7: The effect of welfare reforms**

Note: All regressions include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties.

Robustness test 3: Different conceptualization of pro-welfare parties

Figure A8: The effect of social spending changes

Note: All regressions include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties.

Figure A9: The effect of welfare generosity changes

Note: All regressions include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties.
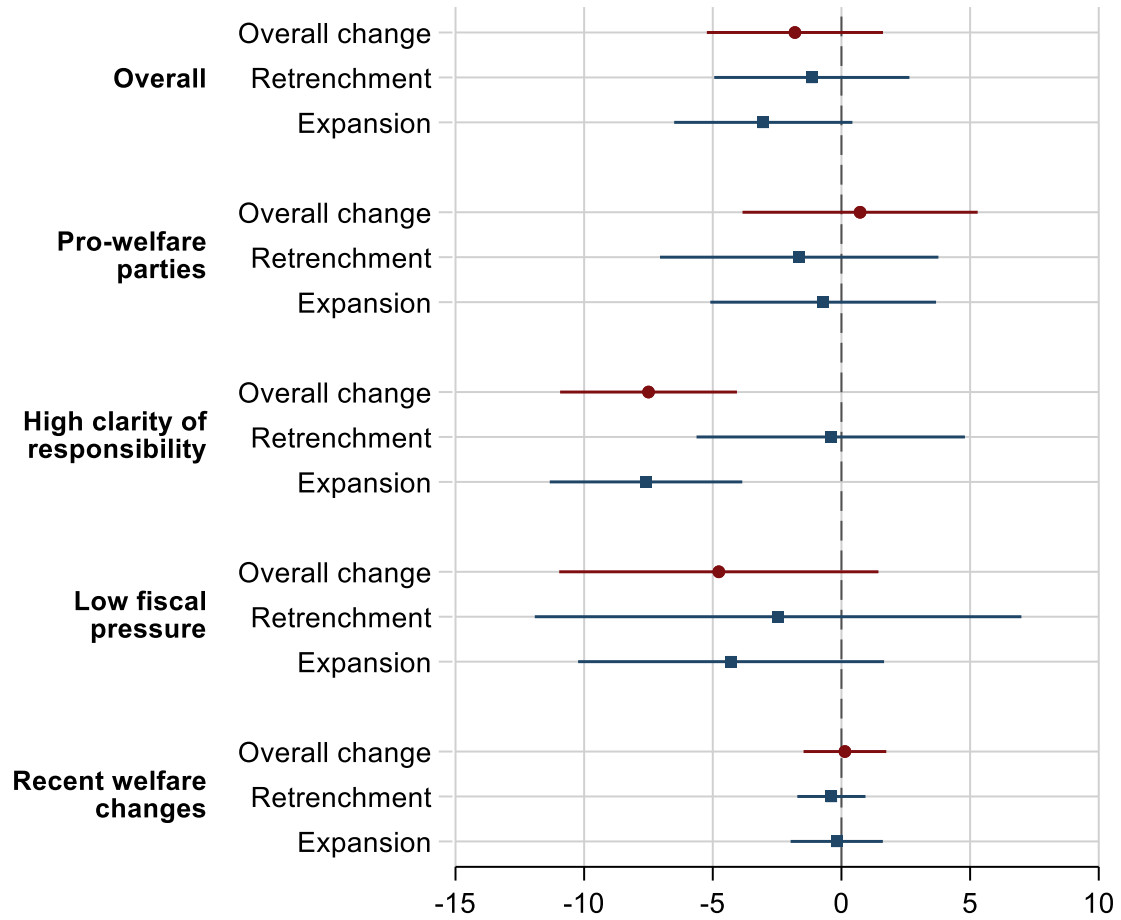
Figure A10: The effect of welfare reforms

Note: All regressions include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties.
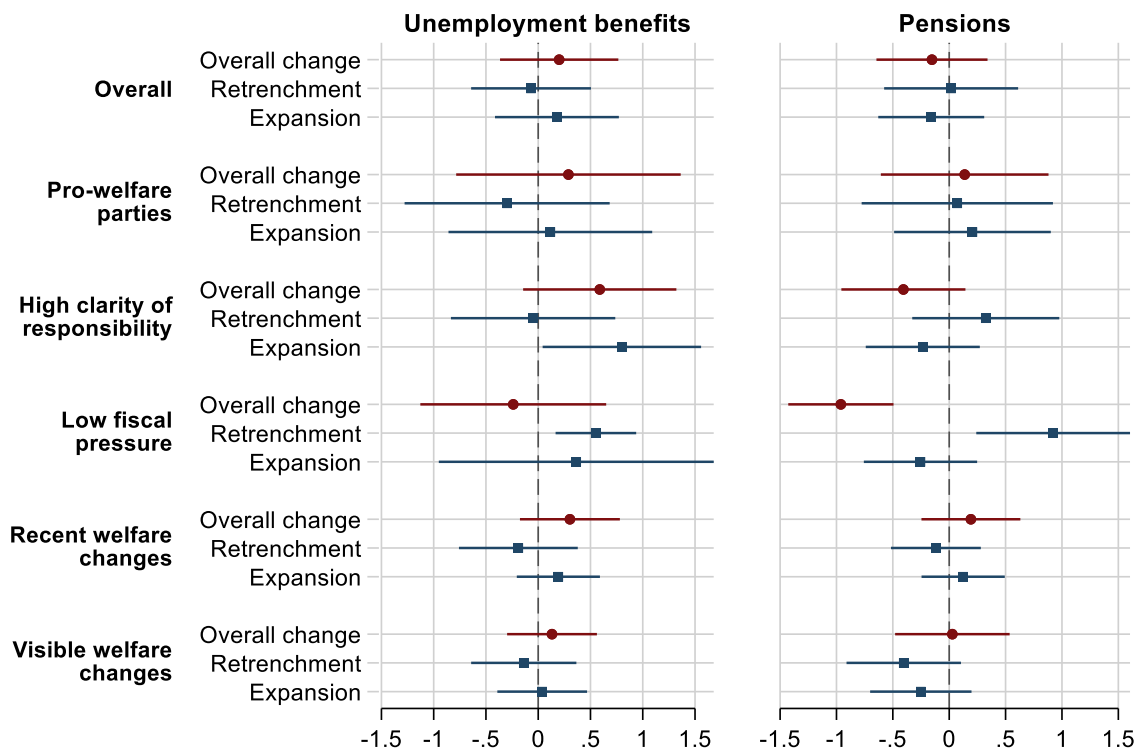
26

Robustness test 4: Young democracies excluded

Figure A11: The effect of social spending changes
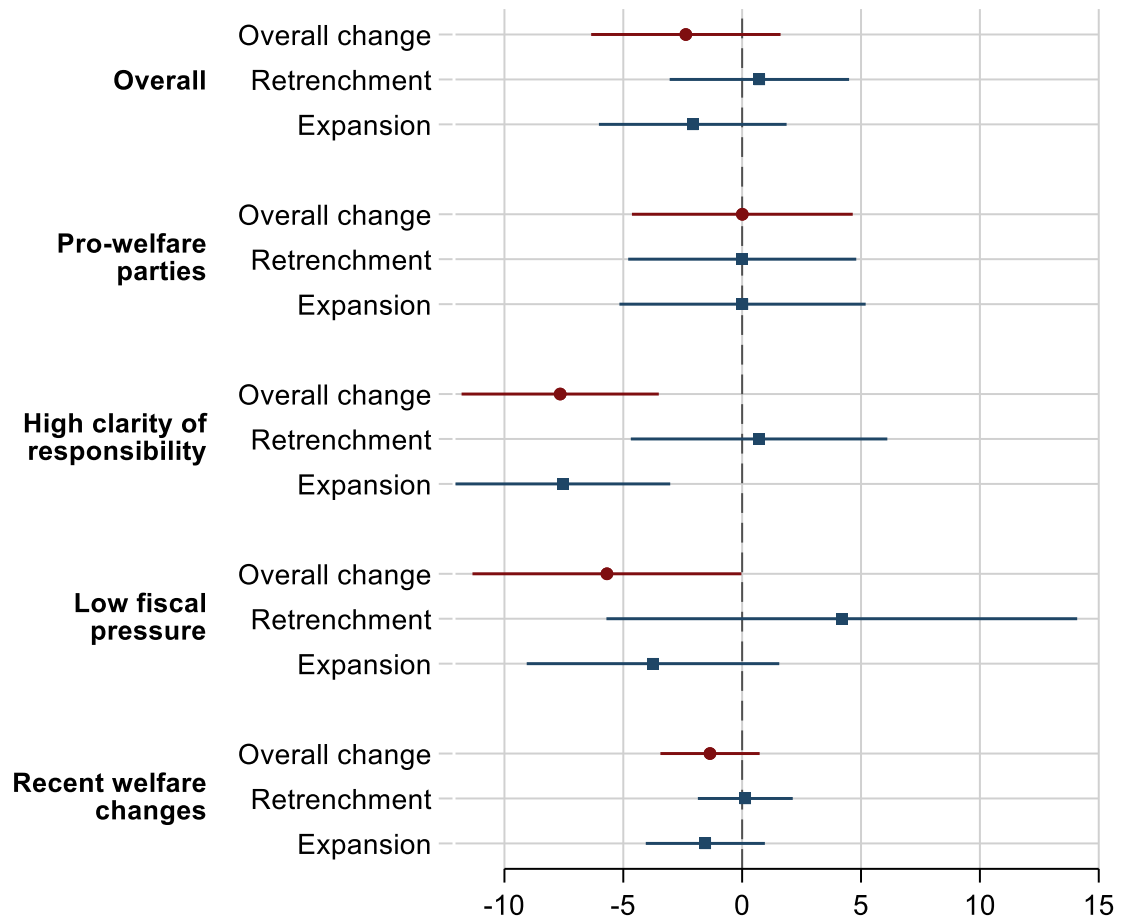
Note: All regressions include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties.

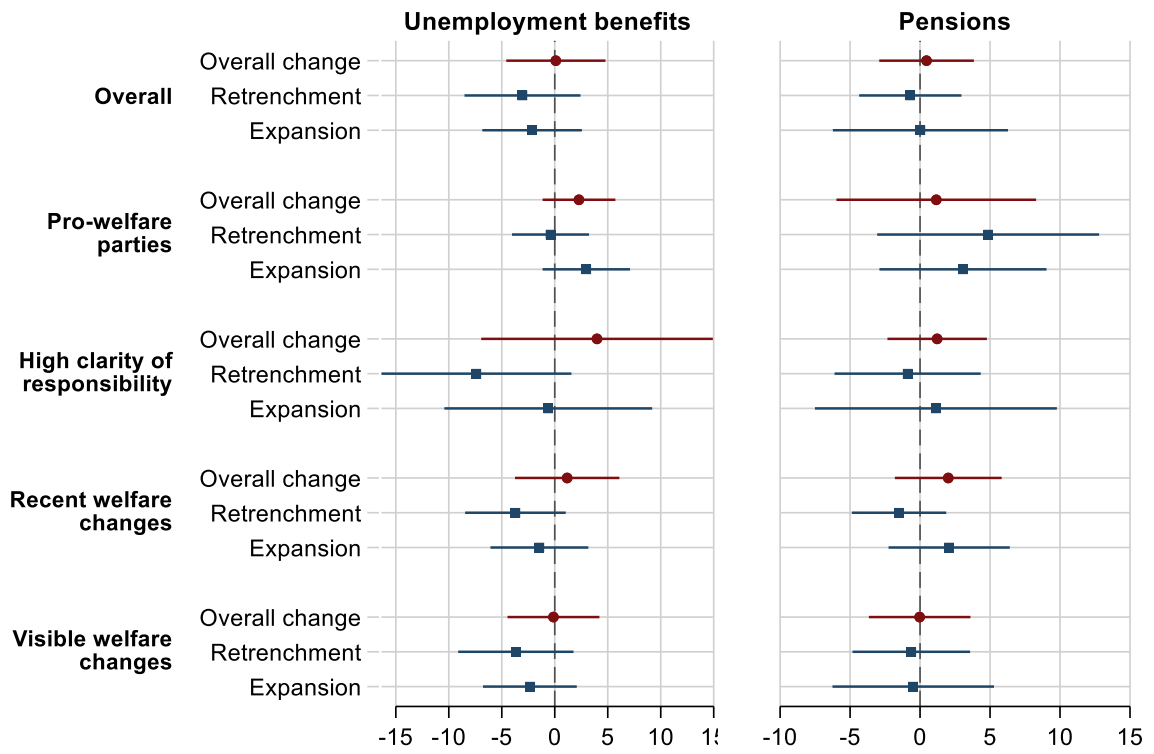**Figure A12: The effect of welfare generosity changes**

Note: All regressions include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties.

Figure A13: The effect of welfare reforms

Note: All regressions include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties.

Robustness test 5: No control variables

Figure A14: The effect of social spending changes

Note: All regressions include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties.
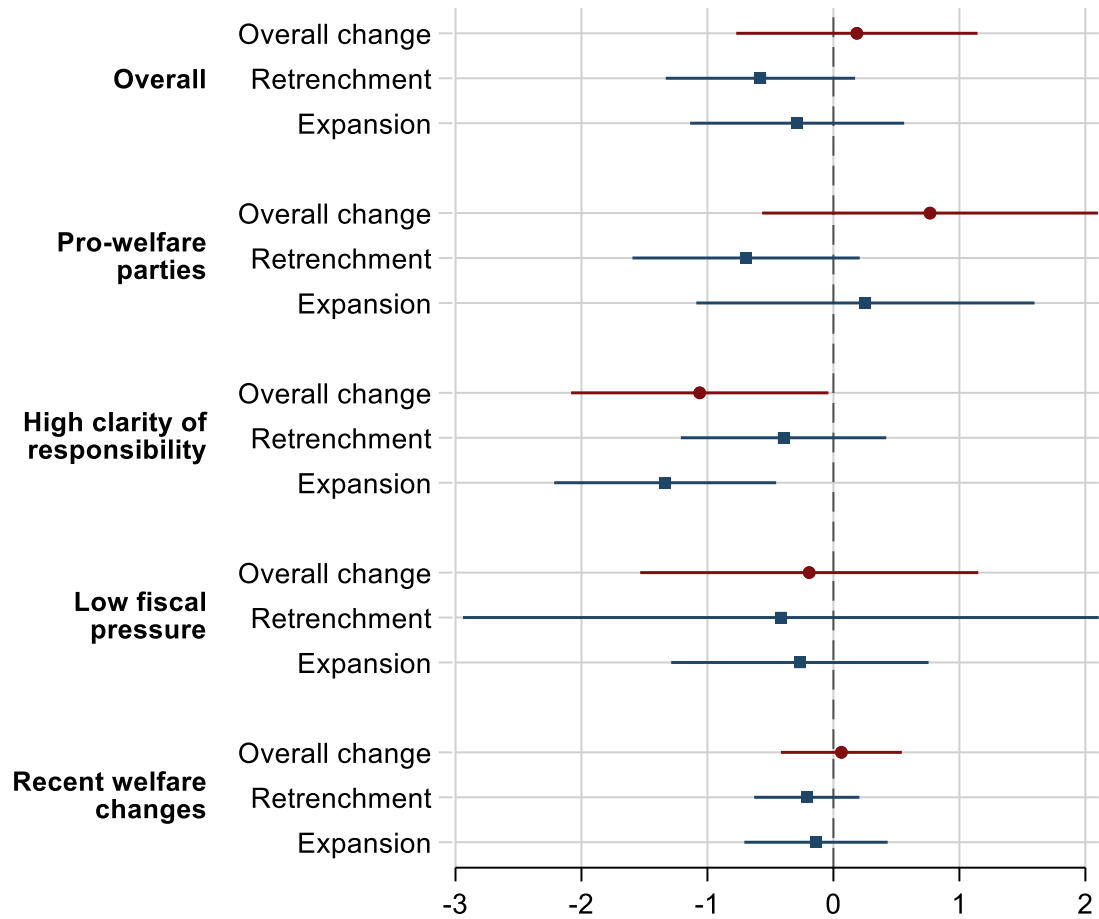
Figure A15: The effect of welfare generosity changes

Note: All regressions include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties.
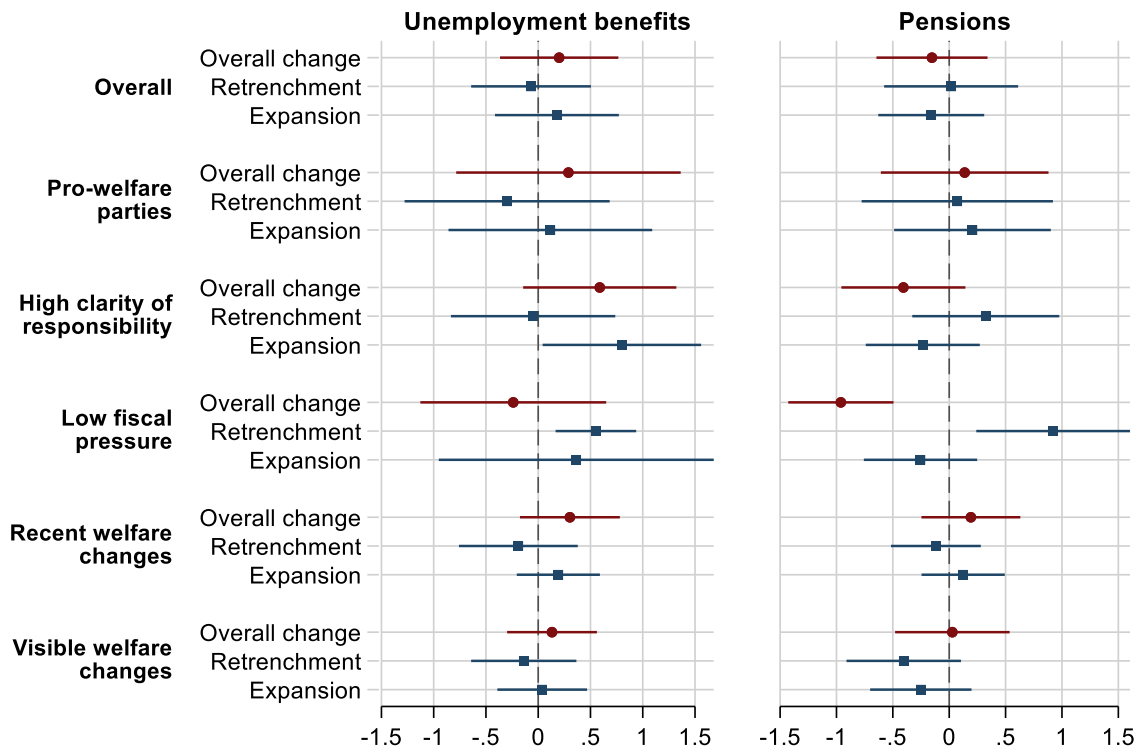
Figure A16: The effect of welfare reforms

Note: All regressions include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties.

Robustness test 6: Dependent variable that measures percentage point changes

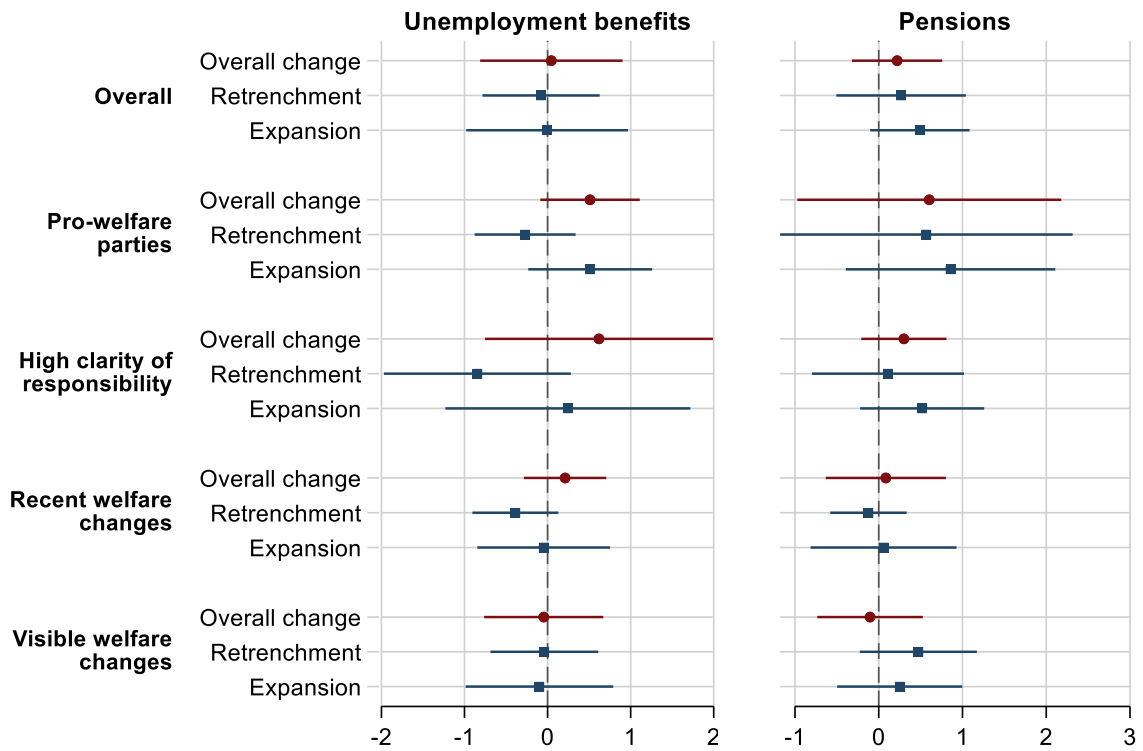**Figure A17: The effect of social spending changes**

Note: All regressions include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties.

Figure A18: The effect of welfare generosity changes

Note: All regressions include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties.

Figure A19: The effect of welfare reforms

Note: All regressions include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties.