Essays in Social Policy

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Acknowledgements

I did not accidentally stumble into to the life of a PhD student. It has been a long-time dream of mine that started to develop throughout the course of my undergraduate studies. The idea of doing a PhD seemed indeed very intriguing: intellectual stimulation everywhere; 24/7 of (almost) unlimited personal freedom; and most importantly "the irrelevance of (market) relevance" or in other words: knowledge for the pure sake of knowledge.

But it has not been a fairy tale throughout. The underlying imposter syndrome that is constantly there; the monthly fight with an R package that is buggy in all possible terms; and the realization that, after all, academia can be a quite lonely place where most of the time we are groping in the dark and cannot find the signal among the noise.

Despite all these downsides, I cannot imagine another place to be now or ever in the future. This has not just to do with the personal freedoms that I mentioned above, but I truly believe that science is the only way to make sense of the world – especially in, or maybe because of, the polarizing times we are living in where knowledge, facts, and deep understanding of the *causes of things* became truly scarce resources.

I am very sure that the ups during the PhD would not have been so much fun, and the downs would not have been so short-lived without the immense support I have received from so many amazing people.

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to my brother Maximilian

Thesis abstract

This dissertation explores the political determinants and consequences of social policies in advanced economies. In its four papers, it aims to contribute to our understanding of how social policies influence the political and electoral behaviour of voters as well as the ways in which electoral outcomes determine social policies. *Paper 1* investigates the statistical sources of model uncertainty in the literature of welfare state determinants. It approaches this issue by quantifying the sensitivity of empirical estimates towards different model specification choices, including the control variable set, the fixed effect structure, the standard error type, the country sample, the period sample and the operationalisation of the dependent variable. By developing an Augmented Extreme Bounds Analysis (A-EBA) method combined with a grid-search deep learning approach, the paper reveals that the selection of the country and period sample and the operationalisation of welfare state generosity has a substantially larger impact on the coefficients and standard errors in generosity estimations than the control set. *Paper 2* quantifies the electoral returns to campaign promises on social benefit expansions. By applying a discontinuity-based research design on survey data in Germany, the results show that promises on pension benefit expansions swing voters to the pledge-making party. This alignment gain, however, is only transitory as the effect diminishes shortly after pledge fulfilment suggesting that even unbroken promises on very generous welfare state policy expansions do not yield long-term political support. *Paper 3* measures the mobilising effect of pension benefit expansions in the US. The paper matches voting records with a large administrative dataset on pension payments to retired public employees in Illinois. The results of a discontinuity-based research design show that pension benefits increase electoral participation in the short run among Democrat voters, particularly among those with lower levels of income. In contrast, Republican and/or unaffiliated voters remain unresponsive. Thus, the results suggest that policy feedbacks depend strongly on voters' ideological and material predisposition. *Paper 4* examines whether the provision of social benefits and assistance programmes can mitigate the political costs of unpopular policies, such as climate change policies. By applying an instrumental variable approach to a dataset covering a large set of advanced economies over time, the estimates show that the welfare state can be a viable tool to prevent electoral punishment the incumbent receives for passing unpopular policies. These findings highlight that the welfare state can generate the political and electoral support that is necessary for urgently needed measures in times of global warming.

Introduction

Social benefits became a defining element of modern market economies in the twenty-first century. There exist convincing economic rationales in support of the welfare state in various domains such as education (Behman 1997; Ashenfelter and Rouse 1998; Black and Lynch 1996; see Blundell et al. 1999; Sianesi and Van Reenen 2003; Harmon et al. 2003), health (Currie and Madrian 1999; Card et al. 2009; Sommers et al. 2012; Garthwaite et al. 2014; Korenman and Remler 2016; Goldin et al. 2021), and unemployment (Baily 1978; Gruber 1994; Chetty 2006; Chetty and Looney 2007; Kuka 2020). Yet, the provision of social benefits does not just have economic implications; it also comes with political ones. Since welfare state policies can have large material consequences for both individuals and societies, policies on social issues constitute an important subject in the arena of electoral politics.

Analysing the politics of the welfare state in advanced economies has indeed been a vibrant field of theoretical and empirical scholarship for decades. Broadly speaking, the literature can be classified in two main camps: one that investigates the political determinants of social policies and one that examines how social policies affect political and electoral outcomes. The four research articles contained in the present dissertation aim to contribute to both sides of the debate.

On the determinant side, scholars have proposed a wide range of institutional, economic, and political factors that shape social policy outcomes. However, since the identification of these numerous determinants is based on a specific set of countries, time period, generosity measures and further statistical assumptions, it is challenging to consolidate this knowledge and provide statements on how, when and under what conditions certain determinants (do not) affect social policy outcomes. Indeed, as the empirical results of this dissertation will show, the large number of possible empirical choices comes with a substantive amount of model uncertainty suggesting that the size, direction and statistical significance of common welfare state correlates varies widely with the model specifications they are estimated on.

Among all determinants, however, past social expenditure and social rights have the most consistent and robust effect on welfare state development, which supports the relevance of path dependency arguments (Pierson 1994; Pierson 1996; Mahoney 2000; Ebbinghaus 2005). One important mechanism that aims to explain the self-reproducibility and/or resilience of the welfare state concerns electoral politics (Pierson 1994; Pierson 1996; see Starke 2006). Since social policies may influence the political and electoral behaviour of voters, policymakers, especially vote- and office-seeking ones, may have strong incentives to adopt welfare state reforms for electoral purposes.

Yet, despite several studies supporting the effect of social policies on electoral behaviour of individuals (see below), evidence on the policy-behaviour nexus is lacking in several aspects. First, the durability of policy feedbacks has been largely ignored in the literature, even though it can improve our understanding about the mechanisms of policy feedbacks (Pierson 1993; see Béland 2010; Béland and Schlager 2019). Since interpretive effects, in contrast to resource ones, arguably fade with voters' awareness and memory, quantifying the longevity of policy feedbacks provides an opportunity to compare the relevance of both mechanisms. Second, the vast majority of studies in this field of research analyse the political effects of implemented policies. However, since electoral politics are largely programmatic, neglecting prospective forms of policymaking disregards an important characteristic of political dynamics prior to elections. Finally, third, the marginal benefits or costs of social policies are arguably a function of voters' economic and ideological predisposition as individuals with weak economic security and/or positive views of the welfare state gain more (economically and ideologically) from expansionary measures than their wealthier and/or more welfare state opposing peers. However, these two predetermined dimensions are often neglected in empirical studies on social-policy feedbacks. Therefore, this dissertation aims to address these shortcomings and, in this way, contribute to our understanding about the role of social policies in electoral politics.

The papers of this dissertations build on long-standing theoretical frameworks within political science – most notable policy feedbacks, welfare state expansion, and political economy of structural reforms – and utilise various quantitative methods from different social science disciplines – including sensitivity analysis, machine learning, and causal inference. Each of the four stand-alone research articles uses a distinct theoretical and empirical approach to shed light on a specific aspect of the politics of the welfare state. In the following, I review past scholarly work in this field of study, discuss relevant gaps in the literature, and provide an overview how the dissertation and the single paper contribute to the literature about the politics of the welfare state.

It is worth mentioning that this dissertation is neither critical nor supportive of the welfare state per se – neither economically nor politically. In contrast, the present work aims to outline the double-edged nature of the welfare state. On the one hand, this dissertation shows that social policies can be exploited for political and electoral objectives as they can have a substantive persuasive and mobilising effect on individual voters. On the other hand, however, this dissertation also shows that the welfare state can be used productively as it can help policymakers to mitigate the electoral damage of unpopular but long-term beneficial policies, such as climate change policies. Thus, in essence, the results of the dissertation suggest that social policies can be "political means to an efficient end" as well as "inefficient means to a political end".

I. Literature Review

Several demographic, technological and economic trends have challenged the welfare state in the last decades. One major issue concerns the "demographic time bomb". Longer life expectancy after retirement age combined with declining fertility rates have raised concerns the fiscal sustainability of pension systems in many advanced economies in times of population aging (Disney 1999; OECD Health 2004; Barr and Diamond 2006; United Nations 2022). Beyond changes in the structure of the population, technological shocks and environmental trends have also been pressuring the welfare state (Frankhauser et al. 2008; Acemoglu 2020; Barr 2020; Georgieff and Milanez 2021; Dauth et al. 2021; Gallego and Kurer 2022). Amongst other factors, the automatization of parts of the supply chain as well as the mitigation of climate change require social policymakers to promote costly cross-sectoral shifts of workers. However, policymakers' hands are often tied, especially when global firms can easily shift capital across borders, suggesting that policymakers need to strike a balance between economic and social considerations (Garrett and Mitchell 2001; Tanzi 2002).

The duty to "provide more social benefits with less resources/capacities to do so" is indeed economically challenging. However, it would be too short-sighted to assume that elected officials address these issues only from an economic perspective in a cost-benefit type-of-analysis. Political considerations have always played an important role in how social issues are addressed in the democratic process. Hereby, scholars have devoted substantive attention to the *politics of the welfare state* in the last decades and the analysis of its political determinants constitutes one of the largest streams in the social policy discipline.

During the Golden Age era, power resource theories used to be important frameworks to attribute changes in the size and scope of the welfare state to the power of the electoral platform. Their key argument is straightforward: since different parties represent different groups of voters, they act in the interest of their core constituency to safeguard their political power (Stephens 1979; Korpi 1983; Esping-Andersen 1985; Korpi 1989; Huber et al. 2001; Korpi and Palme 2003). Thus, in a class-based society, left-wing governments are expected to act in the interest of working-class people and adopt expansionary welfare state policies while the opposite is expected under conservative incumbents.

Indeed, researchers were able to provide widespread empirical evidence in support of power resource explanations during this time (Kittel and Obinger 2002, Potrafke 2017; Bandau and Ahrens 2020). However, since the Golden Age area, scholars have started to question the usefulness of ideology to explain social policy outcomes (Imbeau et al. 2001; Tavits and Letki 2009; Potrafke 2009; Garritzmann and Seng 2016; see Häusermann et al. 2013). The unpopularity of partisan-based arguments was to a large extent driven by the observation that left- and right-wing governments do not necessarily act in the way power resource theory predicts. Interestingly, the criticism against power resources arguments comes from two opposite directions.

While Pierson (1994) noted that even conservative governments – such as that led by Thatcher in the UK as well as by Reagan in the US – did not adopt major welfare state cuts, other scholars highlighted that several countries governed by left-wing parties in the late 1990s and early 2000s adopted far-reaching contractionary measures (see Klitgaard 2007). This does not mean, however, that government partisanship no longer plays any role in social policymaking (Allan and Scruggs 2004; Scruggs and Allan 2006; Gingrich 2011; Giger and Nelson 2011; Jensen and Mortensen 2014; Herzwartz and Theilen 2014; Herwartz and Theilen 2014; see Falkenbach et al. 2020). Rather, the criticism against power resource logics concerns the observation that left-wing (right-wing) governments do not adopt welfare state expansions (retrenchments) per se since other political aspects may outweigh or substitute partisan differences.

In this context, a central piece in the social policy scholarship is Pierson's work (1996) on the "New Politics of the Welfare State" (see Green-Pedersen 2002; Starke 2006). He associates welfare state trajectories with electoral considerations instead of ideological ones by noting that the politics of welfare state expansions are fundamentally different to the politics of welfare state retrenchment. While welfare state cuts create concentrated costs on certain groups and dispersed gains across all taxpayers (Green-Pedersen 2002), expansionary measures entail concentrated gains and dispersed costs. Thus, the asymmetric allocation of the costs and benefits make retrenchments politically inferior to expansions which, in turn, creates a downward resistance and path dependency of welfare states trajectories.

Pierson (1994) substantiates the resilience of the welfare state from two angles. First, augmenting the welfare state incentivises the establishment of interest groups on the recipient side which try to protect the current status quo against future cuts. Second, electoral motives incentivise policymakers to favour expansions over retrenchments as welfare programs are highly popular among beneficiaries. Therefore, policymakers refrain from cutting social programs and opt for expanding or establishing new ones to swing and mobilise voters in their favour. Scholars of social policy have paid substantive attention to this latter source of path dependency.

The study of the political and electoral consequences of policies forms a larger field of analysis within political science and are often analysed in the context of policy feedbacks (Schattschneider 1935; Pierson 1993; Skocpol 1995; Soss and Schram 2007; see Béland 2010; Béland and Schlager 2019). Hereby, policy feedback frameworks assume that policies affect but are also determined by political factors. Thus, in contrast to power resource arguments which assume a one-way causal link between, in their case, government partisanship and policy outcomes, policy feedbacks relax this assumption and model policy and political outcomes as a continuous process in which policies and politics shape, constrain and determine each other over time (Pierson 1993). Pierson (1993) argues that one important factor that furthers policy feedbacks is the behaviour of individual voters at the ballot box.

There are two main mechanisms through which policies may affect the political and electoral behaviour of individuals (Pierson 1993; see Béland and Schlager 2019). On the one hand, policies affect voters' resources – such as their financial situation, labour market skills, or health conditions, amongst others. Hereby, these resources can either create the necessary capacities to participate in the political process, or they can incentivise voters to engage politically to protect their new means. On the other hand, policies do not just entail material consequences, but they also provide information about the incumbent's goals, intent, and purpose for the future policymaking trajectory. In other words, policies provide new information about the relationship of the state towards the electorate which enables voters to update their beliefs about the congruence between their and the elite's future interests.

Empiricists have studied the effect of social policies on voting behaviour in a variety of ways. On the retrenchment side, the debate about the electoral consequences of welfare state cuts is largely unsettled. While several studies have provided evidence in support of Pierson's punishment hypothesis (Mulas-Granados 2004; Hübscher and Sattler 2017; Hübscher et al. 2020; Bojar et al. 2021), other empirical exercises, mostly focusing on austerity-based measures, identified no or only limited electoral costs for the incumbent (Alesina et al. 1998; Armingeon and Giger 2008; Giger and Nelson 2011; Schwander and Manow 2017; Arias and Stasavage 2019, Ahrens and Bandau 2022). Further empirical analyses have shown that the punishment effect is conditional and depends, among other contextual factors, on the partisanship of the incumbent (Schumacher et al. 2013; Armingeon et al. 2015; Horn 2020), the success of blame avoidance strategies (Weaver 1986, Pierson 1996, Wenzelburger 2011, Wenzelburger 2014, Wenzelburger et al.,2020), on media coverage (Barnes and Hicks 2018) as well as on the current state of the economy (Alesina et al. 2020).

On the expansion side, scholars have assessed policy feedbacks by examining the effect of expansionary measures on voting behaviour, with a substantive focus on conditional cash transfer programs in developing and emerging economies, for instance Manacorda et al. (2011) in Uruguay, Labonne (2013) in the Philippines, De La O (2013) and Cantú (2019) in Mexico, Hidalgo and Nichter (2015) in Brazil, Baez et al. (2012) and Gallego (2018) in Colombia, Khemani (2015) in the Philippines, and Pop-Eleches and Pop-Elches (2012) in Romania. In addition to vote choice and party alignment, others have assessed policy feedbacks by quantifying their effect on voter turnout (Markovich and White 2022; Clinton and Sances 2018; De La O 2013; Baez et al. 2012; Layton and Smith 2015; Baicker and Finkelstein 2018; Imai et al. 2020; Löffler 2022). In general, most of the empirical evidence in the policy feedback literature suggests that establishing or expanding welfare state programs increases electoral participation among beneficiaries and delivers electoral rewards to the incumbent.

Despite the broad empirical evidence supporting the presence of policy feedbacks, several questions remain unanswered. In the following section, I discuss these gaps in the literature and then summarise how each of the four research articles comprising this dissertation addresses them in greater detail (see Table 1).

II. Gaps in the literature

Several gaps in the literature on the *politics of the welfare state* are observable. First, studies on policy feedbacks often neglect the question of the durability of the effect of policies on voters' electoral behaviour. However, this is of great relevance since the durability of the effect can provide evidence about the mechanism through which policies affect political behaviour of the mass public. For instance, transitory policy feedbacks that fade with voters' awareness and memory may suggest that voters respond to the information and signals that policies provide to individuals (Pierson 1993). In contrast to this *interpretive mechanisms*, long-lasting policy effects may rather support the prevalence of the *resource mechanism* as policy-induced resources – such as time or money – remain even when voters' awareness and memory about the reform vanishes (Pierson 1993; Verba et al. 1995; Schlozman et al. 2001). Therefore, examining the longevity of policy effects can provide evidence about the resource-based or interpretive nature of policy feedback loops.

Second, beyond the durability, the literature has largely ignored how voters' economic and ideological predisposition impact and/or mediate the effect of social policies on political behaviour. When it comes to voters' economic position, one can expect that the political responsiveness of voters towards social benefits diminishes with economic security as such benefits do not necessarily satisfy a demand that remains otherwise unfulfilled (Wolfinger and Rosenstone 1980; Filer et al. 1993; Verba et al. 1995; Schlozman et al. 2001; Solt 2008; Leighley and Nagler 2013; Kasara and Suryanarayan 2015).

Likewise, heterogeneity in the policy-behaviour nexus can also emerge from ideological priors of individual voters (Campbell et al. 1980; Feldman and Zaller 1992; Alesina et al. 2004; Baldassarri and Gelman 2008; Gelman 2009; Page and Shapiro 2010; Ellis and Faricy 2011; Margalit 2013; Bisgaard 2019; Baldassarri and Park 2020). For instance, voters who take rather opposing stances on public welfare provision are unlikely to show the same behavioural changes compared to voters with more positive views of the welfare state – independent of their economic benefit they receive from an expansion. While both economic and ideological dimensions have been incorporated in models on public opinion formation, they are often neglected in the social-policy feedback literature.

Third, beyond these substantive gaps, most of the quantitative welfare state literature largely ignores two fundamental methodological problems of empirical social science: model uncertainty and endogeneity. With respect to the model uncertainty, numerous scholars have analysed the extent of the sensitivity of statistical estimates in various sub-disciplines or social science. Most of these studies draw a pessimistic conclusion by showing the estimates about the association between the determinants of economic growth, democracy, and conflict, amongst others, depend strongly on the control variable set (Leamer 1983; Levine and Renelt 1992; Levine and Zervos 1993; Sturm and de Haan 2005; Hegre and Sambanis 2006; Gassebner et al. 2013; Gassebner, et al. 2016; Miller et al. 2018). Surprisingly, such an analysis has not been conducted for the political determinants of generosity even though estimator sensitivity may be substantive given the large number of possible covariates, theoretical approaches, the numerous ways of measuring generosity[1] as well as various types of welfare state regimes.

Beyond model uncertainty, existing empirical exercises in the literature on the politics of the welfare state often do not adopt identification strategies that can derive causal estimates. Indeed, applying causal inference methods in this field of study is not trivial, particularly when estimating the effect of social policies on political and electoral behaviour. While field experiments may be technically suitable to establish causality, conducting randomized control trials is costly and can, most of the time, only be used for small-scale programs which may not be representative enough for real-world large-scale reforms. At the same time, adopting quasi-experimental methods can also be challenging because available datasets often lack the information that is required for accurate treatment assignment. For instance, when using survey data, information on respondents' exact location, date of birth, or income level is hardly available due to data privacy issues in most countries. The same is true for administrative databases containing information on voting behaviour. Most countries do not even provide voter files to the public or research community. But even when they do, matching these records with benefit recipient data is challenging because recipient records usually contain highly sensitive information (i.e. salary, date of birth, etc.) which may be outside the legal scope of data privacy rules.

[1] see dependent variable problem (Green-Pedersen 2004; Clasen and Siegel 2007)

III. Summary of contributions

The four research articles in this thesis aim to fill some of these gaps and, in this way, improve our understanding of the interaction between policymakers and the electorate in the context of welfare state reforms. To do so, *paper 1* contributes to the literature on the determinants of the welfare state by addressing the long-standing empirical issue of model uncertainty. The analysis uses a well-established method, but to date not used in the welfare state literature, to quantify model uncertainty that originates from various model specification choices. To do so, it augments a traditional sensitivity analysis approach – the Extreme Bounds Analysis – by further model specification choices[2] to identify the sources of model uncertainty. The analysis shows that the sample selection and dependent variable choice have a far larger effect on the coefficient and standard error of widely cited welfare state determinants than the control set.

By using a quasi-experimental approach, *paper 2* is one of the first empirical studies that provides causal evidence to the question whether campaign promises on social benefit expansions – one essential form of utilising prospective economic voting motives – are effective political tools to swing voters from competitor parties. In this way, the paper highlights that prospective pocketbook voting is a more relevant form of economic voting in advanced economies than currently assumed. Furthermore, *paper 3* uses administrative datasets covering individual voter file records and pension payment records to quantify the causal impact of pension expansion on political participation. The paper shows that

[2] Control variable set, fixed effect structure, the standard error type, the country sample, the period sample, and the operationalization of the dependent variable.

participatory effects are transitory and depends strongly on voters' ideological and economic predisposition. Finally, *paper 4* investigates whether the provision of social insurance and assistance can be a beneficial political tool for governments to overcome the political obstacles of unpopular policies such as climate change policies. By using an instrumental variable approach, the results show that providing sufficient social protection can successfully mitigate the electoral damage incumbent parties need to expect when adopting stricter environmental regulation.

The core strength of this dissertation can be found in its empirical contributions. Hereby, it tackles some of the most pressing empirical issues of social policy – namely model uncertainty and endogeneity – by utilizing existing quantitative methods from other fields of social science inquiry. To do so, the single papers use various data structures both from the macro- and micro-level perspectives by analysing country-year panels (paper 1 and 4), survey data (paper 2) and administrative records (paper 3). In addition, different methodological approaches – namely sensitivity analysis, machine learning, and causal inference – are utilized to address the research questions at hand. The combination of different methods and data structures aims to highlight that the existing statistical approaches within computational social science and econometrics provide very useful tools to answer long-standing questions within social policy. Thus, the dissertation aims to bring state-of-the-art econometric methods to the core of quantitative social policy and, in this way, contribute to the discipline as a whole.

IV. Thesis overview

Paper 1: Welfare State Determinants: An Augmented Extreme Bounds Analysis

What political and economic factors are associated with welfare state spending and generosity? We develop and apply an Augmented Extreme Bounds Analysis to test the robustness of 19 prominent factors in the welfare state literature across multiple model specification choices. The results from 396 million regressions with over 7.5 billion estimates show that statistical significance of all determinants is highly affected by numerous modelling choices beyond control variable inclusion. In addition, the large number of positive and negative significant coefficients for almost all determinants highlights that empirical results can found in support of contradicting theoretical arguments. To identify the most significant model specification choices algorithmically, we employ a deep learning approach: our results show that the choice of the control variable set is much less relevant for predicting the significance of welfare state determinants than choices such as sample selection, dependent variable choice, fixed effects, and error structure.

Paper 2: Are Campaign Promises Effective?

In democracies, political parties make campaign promises about future social benefit expansions to attract voters. However, we know relatively little whether such promises effectively translate into higher political support among benefiting recipients and to what extent prospective pocketbook motives determine alignment with electoral platforms. Using a regression-discontinuity design, we estimate the causal effects of a campaign promise made by the conservative party to expand pension benefits ahead of the German parliamentary election in 2013. The results show that the promise increased alignment with the pledge-making party by 12.2% among eligible beneficiaries, with particularly large effects among low-income individuals. The policy-induced alignment gain is, however, only transitory as it disappears once the pledge is fulfilled. In addition, the results show that promising additional social benefits can successfully swing voters who traditionally align with left-wing political parties, but they do not mobilize non-voters. Thus, campaign promises on benefit expansions are rather persuasive than mobilizing.

Paper 3: Do Pension Benefits Mobilize?

The elderly now represent one of the largest electoral groups and pensions constitute often the most expensive benefit program in advanced democracies. Yet, we still lack causal evidence on the effect of pension benefits on political behaviour. This article matches administrative pension payments data with voter file records of public-school teachers in Illinois to exploit the timing of a benefit-expanding reform in the late 1990s in a regression discontinuity design. We find that pension benefit expansions have a large, but temporary, mobilizing effect for democratic voters. The effect is mainly driven by individuals with lower salaries, suggesting that the prevalence of benefit-based policy feedbacks decreases with economic security. There are no mobilizing effects among republican or unaffiliated voters. Overall, the paper suggests that policy feedbacks are short-term, and their size strongly depends on voters' ideological and economic predisposition.

Paper 4: Are Climate Change Policies Politically Costly?

Are policies designed to avert climate change (Climate Change Policies, or CCPs) politically costly? Using data on governmental popular support and the OECD's

Environmental Stringency Index covering 30 countries between 2001 and 2015, our results show that CCPs are not necessarily politically costly: policy design matters. First, in contrast to non-market-based CCPs (such as emission limits), only market-based CCPs (such as emission taxes) entail political costs for the government. Second, the effects are only present when CCPs are adopted during periods of high oil prices, prior to elections, or in countries depending strongly on non-green (dirty) energy sources. Third, CCPs are only politically costly when inequality is high and/or social insurance/transfer does not sufficiently address the regressivity of CCPs. Our results are robust to numerous robustness checks including to address concerns related to endogeneity issues.

Table 1: Overview of papers

#	Paper title	Dependent variables	Independent variables	Mediating variables	Level of analysis	Structure of raw dataset	Region	Period	Method
1	Welfare State Determinants: An Augmented Extreme Bounds Analysis	**social spending, social rights**	multiple	-	macro	country-year panel	OECD+	1980-2020	EBA, ML/DL
2	Are Campaign Promises Effective?	party alignment	**eligibility for pension benefit**	income	micro	individual-level survey (panel)	Germany	1986-2019	RDD
3	Do Pension Benefits Mobilize?	turnout, voter registration	**eligibility for pension benefit**	income	micro	individual-level voter files	Illinois	1992-2020	RDD
4	Are Climate Change Policies Politically Costly?	popular support for incumbent	climate change policies	**social spending**	macro	country-year panel	OECD+	2001-2015	IV

Note: the table provides an overview about the title, variables, datasets, and methods being used for each paper of the DPhil project. Bold indicates which empirical role social policies play in the paper. EBA, ML, DL, IV, and RDD refers to Extreme Bounds Analysis, Machine Learning, Deep Learning, Instrumental Variables and Regression Discontinuity Design, respectively.

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Welfare State Determinants:
An Augmented Extreme Bounds Analysis

Michael Ganslmeier and Tim Vlandas

Abstract

What political and economic factors are associated with welfare state spending and generosity? We develop and apply an Augmented Extreme Bounds Analysis to test the robustness of 19 prominent factors in the welfare state literature across multiple model specification choices. The results from 396 million regressions with over 7.5 billion estimates show that the statistical significance of all determinants is highly affected by numerous modelling choices beyond control variable inclusion. In addition, the large number of positive *and* negative significant coefficients for almost all determinants highlight that empirical results can be found in support of contradicting theoretical arguments. To identify the most significant model specification choices algorithmically, we employ a deep learning approach: our results show that the choice of the control variable set is much less relevant for predicting the significance of welfare state determinants than choices such as sample selection, dependent variable choice, fixed effects, and error structure.

Introduction

What political and economic factors are associated with welfare state spending and generosity? Going back at least to the 1960s, identifying the factors associated with welfare state development has been the focus of several social science disciplines, including Economics, Sociology, and Political Sciences.[3] As a result, there now are numerous possible controls in the existing quantitative literature on welfare state development across countries and over time.[4] This large number of potentially relevant factors makes it challenging to define the most suitable set of control variables that should be included in the regression model because researchers must balance trade-offs between multicollinearity and omitted variable bias, as well as choosing between competing proxies, often with distinct time and geographical sample coverage.

Moreover, many further model specification choices have been intensely debated. One is the proper operationalization of generosity on the left-hand-side, also called the dependent variable problem (Siegel 2007; Allan and Scruggs 2004), which concerns the debate whether spending- or entitlement-based indicators are more suitable concepts to capture social policy outcomes. Another debate concerns the appropriate error structure and the inclusion/exclusion of lagged terms of the dependent variable. A third relates to the appropriate sample. For instance, seminal contributions about regime classifications have argued that the functioning of welfare states varied widely across countries and over

[3] Prominent examples include Briggs (1961), Cutright (1965), Offe (1972), Cameron (1978), Esping-Andersen (1990), Huber et al. (1993), Pontusson (1995), Garrett (1998), Rodrik (1998), Iversen and Wren (1998), Pierson (2001), Hall and Soskice (2001), and Allan and Scruggs (2004).
[4] Indeed, Google scholar returns 3 million results for 'welfare state development' and the literature on the welfare state has continued to grow over time, both in books and in journal articles (as shown by searches for 'welfare state' in https://books.google.com/ngrams and https://www.webofscience.com/wos/woscc/basic-search).

time. This would then require an investigation of how the effect of a given determinant changes depending on the chosen country-time observations.

This vast choice set of model specifications heightens the risk of model uncertainty. Finding solutions to this issue has been one of the main methodological issues in welfare state research as well as the field of Political Sciences more generally. A common approach concerns sensitivity analysis which enables scholars to test the robustness of empirical results with respect to several model specifications, such as sample choice, error structure, control variable set, and operationalization. Hereby, one of the earliest methods to investigate the robustness of empirical estimates is the Extreme Bounds Analysis (EBA) method proposed by Leamer (1978, 1983, 1985), which has been applied to various research questions in economics, sociology, and political science. Most of these EBA studies demonstrated the lack of robustness of variables that are considered fundamental factors in explaining key outcome variables in a relevant discipline, most notably economic growth in Economics (e.g. Levine and Renelt 1992; Levine and Zervos 1993), democratization in Political Science (Gassebner et al. 2013), and conflict in International Relations (Gassebner et al. 2016; Miller et al. 2016; Hegre and Sambanis 2006).

Despite its large impact on various empirical disciplines, the traditional version of EBA entails two shortcomings. *First,* traditional EBA approaches only focus on robustness towards the choice of control variables while largely ignoring other empirical assumptions that are made in the process of model building. *Second*, many models might be ‘wrong’ in the sense that they are incorrectly specified, in which case a null result should not be taken to imply non-robustness. One version of this criticism implicitly assumes that we know the 'true model' thanks to established theoretical frameworks, and hence could *ex ante* rule out the wrong specifications. But this criticism paradoxically assumes away the purpose of the empirical test, namely that the researcher cannot by design know what the 'true' determinants of a phenomenon are until these are robustly tested. A more convincing - empirically grounded - version of this criticism is that the EBA runs demonstrably wrong models on methodological grounds, for instance omitted variable bias or multicollinearity. While valid, this latter concerns calls for transparent and automated restrictions on the acceptable model universe rather than a rejection of EBA.

With the present paper, we seek to address both shortcomings by developing an Augmented Extreme Bounds Analysis (A-EBA). Our A-EBA tests the sensitivity of empirical results regarding several additional model assumptions and specification choices, including: (i) fixed effect structure, (ii) standard error type as well as (iii) country sample, (iv) period sample, and (v) operationalization of the dependent variable. In addition, our A-EBA analysis addresses the issue of misspecification in the model universe of the unrestricted EBA estimates by carrying out both (i) model restriction and (ii) model weighting.

We apply this method to the literature on the determinants of welfare state development, which has, to the best of our knowledge, never been subject to an EBA exercise. We first identify 19 key factors based on a literature review of long-standing debates of welfare state determinants. Using a country-year panel dataset covering 33 countries between

1980 and 2016, we then analyse how the robustness of these welfare state determinants is affected by different sources of model uncertainty: measurement of generosity; type of standard errors; fixed effect structure; regime and time periods sample selection; and the inclusion of different control variables. The extensive analysis of over 7.53 billion estimates (i.e. coefficient and standard error combinations) yield three key findings with important insights for broader discussions about methodology for many literatures that rely on Time-Series Cross-Section data, not just the literature on the welfare state.

First, our A-EBA results demonstrate that model uncertainty is substantially larger than previously shown, including by traditional EBA approaches. All key welfare state determinants of interest display great sensitivity to model specification, with none being significant in more than 70% of the cases. At the same time, even the most statistically fragile determinants indicate p-values below 0.1 in 23% of the time, or about 86 million significant coefficients. **Second,** despite the great sensitivity of all determinants, we find (at least) several subsets of possible model specifications for each independent variable that (wrongly) appear highly robust (with >95% of coefficients being statistically significant). In other words, for each independent variable, there exists single model specifications that can be used to show numerous seemingly robust results concerning welfare state determinants across several preselected model dimensions. **Third,** our results indicate that 17 out of 19 determinants have at least 5% of significance shares pointing in *both* directions (i.e. positive and negative). This finding highlights the substantial opportunities to find empirical evidence in favour of a particular welfare state theory. Our restricted and weighted A-EBA versions confirm these three findings even after we address concerns about model misspecification in the unrestricted model universe.

While these results highlight the large extent of model uncertainty in generosity estimations, these findings do not imply that none of the determinants have an effect or are non-robust. In contrast, this paper underlines that the model uncertainty originates from different sources. To identify the most relevant model specification choices, we adopt a deep learning approach using our A-EBA results to predict the significance class of an estimate based as a function of the model specification choices it is estimated on. For this, we use a grid search algorithm for hyperparameter tuning to identify the most suitable neural network structure that maximizes the test set accuracy. Based on these determinant-specific prediction models, we then calculate the feature importance of each input variable (in our case: model specification binary) to infer their predictive importance.

This analysis reveals that – among all model specification dimensions – the selection of the control variables plays, on average, the least important role in welfare state determinants. In contrast, the choice of the dependent variable, sample and fixed effect structure have substantially more predictive power. This finding highlights that robustness (or the lack thereof) does not primarily originate from the control set – which most model uncertainty methods[5] currently focus on – but instead from other empirical assumptions. With an accompanying R package that will be released in the future, we

[5] Traditional EBA, Bayesian Averaging Model (BMA), Weighted Average Least Squares (WALS)

hope to facilitate the work of empirical scholars by providing an off-the-shelf software identifying the key sources of model uncertainty in an algorithmic manner.[6]

The Determinants of Welfare State Development

What explains the variation in welfare state generosity across countries and over time? This question has been subject of numerous studies since the 1960s. There have been important contributions arguing that different factors have an impact on welfare state provision (see reviews by Quadagno 1987; Kittel and Obinger 2003; Busemeyer 2009). These different explanations often evolved into separate fields – including sociology and social policy (e.g. Esping-Andersen 1990; Clasen and Siegel 2007; Bonoli and Natali 2012; Emmenegger et al. 2007; Allan and Scruggs 2004), political economy and comparative politics (e.g. Garrett 1998; Swank and Steinmo 2002; Steinmo 1993; Rueda 2007; Pontusson 2006; Hassel 2014; Baccaro and Howell 2017; Thelen 2014; Pierson 1994; Hall and Soskice 2001; Iversen and Soskice 2006), and economics –which cumulatively improved our understanding of how welfare states have developed in different countries over time.

While conducting a fully inclusive and comprehensive literature review is beyond the scope of the present study, the determinants of welfare state development can be broadly classified in four larger camps. Table A1.1 provides an overview of the different strands in the welfare state literature. *First*, domestic economic factors have often been used to advance functionalist arguments and highlight the role of economic growth, urbanisation,

[6] See section A4 in the appendix for more details about the software.

(de-) industrialisation, and democratisation. Economic development and modernisation for instance are argued to create the fiscal capacity that are necessary to expand the welfare state while also creating new risks leading to demands for more protection.

Second, factors that capture international economic dynamics have aimed to account for the effects of globalisation in goods and services, financialization, or Europeanisation. On the one side, earlier treatments saw various forms of integration in the international economy as a way to increase the power of capital and to limit the ability of states to tax economic actors to expand the welfare state. On the other side, many prominent treatments contend that internationalisation heightens the risks of several key groups leading them to demand more protection.

Third, political factors, most notably partisanship, trade union strength, and the organisation and coordination of employers and firms emphasized the relevance of interests of political and economic agents. While trade unions and social democratic parties were seen as representing the interests of the working and lower middle classes in demanding greater decommodification and social services, more recent studies question whether this is still the case or whether these actors increasingly represent the interests of so-called insiders. In this latter reading, the focus shifts on employment protection legislation, while all political parties neglect the interests of labour market outsiders. Similarly, a debate about the position of employers has been ongoing for several decades, in particular whether they always oppose all forms of welfare state policies, or whether this depends on the type of capitalism in which they are located and/or the efficiency implications of these interventions.

Fourth, institutionalist approaches have investigated the effect of political institutions such as state structures and veto players, or economic institutions on welfare state generosity. Highly fragmented political systems appear less conducive to reforming the welfare state. At the same time, more inclusive (proportional representation) electoral systems may incite all parties to cater to a wider spectrum of social risks, thereby leading to more redistribution and larger welfare states.

Method and Data

The traditional Extreme Bounds Analysis approach

To estimate the association between an independent and dependent variable of interest, assumptions are needed. While imposing assumptions is necessary, it introduces a human component which can in principle affect the reliability of conventional tests designed to assess statistical significance. Human intervention must therefore be based on convincing rationales that the model choices are “reasonable”. In general, there are two ways to justify these choices. *First*, one can base decisions on established theoretical expectations and/or previous empirical exercises in a discipline. *Second*, one can show that the results remain unchanged when relaxing certain assumptions and/or altering model choices. In the latter case, a relationship that indicates stable results across different model specification is considered to be “robust”. While both strategies are important to motivate the validity of model choices, robustness checks have the added advantage that they require fewer *a priori* assumptions. In addition, since many model choices are potentially prone to theoretical disagreement among scholars, robustness checks are often the only attractive way to deal with model uncertainty.

However, robustness checks are not necessarily enough to satisfactory address the issue of model uncertainty because the potential model universe grows exponentially with the set of modelling and specification choices. One of the most prominent and earliest solution of quantifying model uncertainty is the Extreme Bounds Analysis (EBA) originally proposed by Leamer (1978, 1983, and 1985). In his EBA method, Leamer quantifies model uncertainty based on the inclusion and exclusion of selected control variables – which belongs to the most important choices empiricists make in the process of empirical modelling. Instead of running selected specifications based on/inspired by previous quantitative analyses, the EBA approach estimates a very large number of models which differ with respect to their covariates that are included in a single regression. In this way, Leamer measures how the set of controls drives the significance (and robustness) of an estimate of interest. In the original version of EBA, Leamer departs from a linear model in the form:

$$Y_j = \alpha_j + \widehat{\beta_{X_j}} X + \widehat{\gamma_{F_j}} F + \widehat{\delta_{D_j}} D + \widehat{\epsilon_j}$$

where Y_j is the dependent variable of model specification j; X the independent variable of interest (focus variable); D a set of doubtful control variables; F a vector of fixed covariates (free variables) and which are included in all regression models; and ϵ as the error term. After each regressor is assigned to one of these buckets (X, F, D), the researcher permutates over all possible combinations of D and estimates the coefficients and standard deviations of X. The model universe M_j with $j = \{1, \dots, m\}$ is then used to determine the extreme bounds $B_{lower} = \min(\widehat{\beta_{X_j}} - \tau\widehat{\sigma_{X_j}})$ and $B_{upper} = \max(\widehat{\beta_{X_j}} + \tau\widehat{\sigma_{X_j}})$, which are used to assign the robustness/fragility classification to a given X. With a pre-determined test statistic τ, X is robust if $(B_{lower} \times B_{upper}) > 0$, and fragile otherwise. Beyond the binary classification of X, the range $|B_{lower} - B_{upper}|$ is used as a general measurement for misspecification uncertainty (Leamer 1983), while a smaller (larger) range refers to smaller (larger) model uncertainty.

As such decision rules using B_{lower} and B_{upper} can be too restrictive as one single outlier in M_j can cause fragility (cf. McAleer et al. 1985), Sala-i-Martin (1997) conditions B_{lower} and B_{upper} by assigning weights to the estimates of X using $w_j = \frac{L_j}{\sum_{i=1}^{M} L_i}$ to compute $\bar{\beta} = \sum_{j=1}^{M} w_j \hat{\beta}_j$ and $\bar{\sigma}^2 = \sum_{j=1}^{M} w_j \hat{\sigma}_{X_j}{}^2$. In the exercise of Sala-i-Martin (1997), L_j is the likelihood ratio of McFadden (1973). If one assumes normally distributed coefficients such that $\beta \sim N(\bar{\beta}, \bar{\sigma}^2)$, one can then construct the cumulative density function $\phi_j(0|\hat{\beta}_j, \hat{\sigma}_{X_j})$ and weigh it by $\phi(0) = \sum_{j=1}^{M} w_j\, \phi_j(0|\hat{\beta}_j, \hat{\sigma}_{X_j})$ (see Sala-i-Martin 1997; Hlavac 2016). A variable is considered as robust if (at least) probability mass ψ lies on one side of the null point; otherwise, X is considered as fragile. Common levels for ψ are 0.9 and 0.95 (see Sala-i-Martin 1997; Gassebner et al. 2013; Sturm and de Haan 2005).

EBA approaches have been widely applied in the social sciences. The majority of results led to a very pessimistic view about the robustness of empirical results in their respective fields. For instance, in the economic growth literature, several scholars found a lack of robustness of many indicators which have previously been considered as fundamental factors to that explain changes in macroeconomic performance over time and across countries[7] (Levine and Renelt 1992; Levine and Zervos 1993, Sturm and de Haan 2005; Chakrabarti 2001). Likewise, in political science, more recent EBA applications in the literature on democratization (Gassebner et al. 2013), political coups (Gassebner et al. 2016; Miller et al. 2016), human rights protection (Hafner-Burton 2005) and civil wars (Hegre and Sambanis 2006) came to similar conclusions. Surprisingly, such an EBA exercise is missing in the welfare state development literature even though model uncertainty is most likely extensive in this sub-discipline of social science due to the numerous model specification choices.

Our Augmented EBA Approach

Our Augmented-EBA (A-EBA) approach differs in five central aspects from the traditional version of Leamer. *First,* instead of partitioning the selected regressors into free and doubtful variables, we start with an unrestricted version by considering all independent variables as doubtful. Beginning with this unrestricted approach has two advantages. One, identifying "must-have" controls ex-ante is very challenging for many research questions in the social sciences because the explanations have changed over time and what has been considered as important determinants in one period may not be true in another one. Two, in line with the criticism of McAleer et al. (1985), partitioning the regressors into free and doubtful variable buckets requires an important (subjective) judgement call which may create the very 'fishing problem' that EBA ultimately wants to solve. Thus, if an independent variable passes the unrestricted EBA (EBA without free

[7] Despite these critical results, there are also studies which reach a more positive outlook using the EBA method (Sala-i-Martin 1997; Bartley and Cohen 1998; Fowles and Merva 1996; Moser and Sturm 2011; Durham 2004).

variables), one can have higher confidence in the robustness of the significance of the relationship. Starting without free variables is also possible as we have access to the necessary computational capacities, which allows us to permutate over all combinations of D and report the restricted results in the classical EBA spirit afterwards.

Second, we split our panel dataset covering 33 countries between 1980 and 2016 into 21 sub-samples based on different time periods as well as the regime-classification provided by Ferrera (1996). This allows us to account for both sampling uncertainties and heterogeneous effects across country clusters and time periods. This sub-sampling strikes us as important given the fact that the effects of certain determinants can show large heterogeneity depending on the sample they are estimated on. In total, we sub-sample our overall dataset into seven country sub-samples, namely (1) Anglo-Saxon, (2) Bismarkian, (3) Scandinavian, (4) Southern-European, (5) South-Eastern European, (6) all minus South-Eastern European welfare state regimes and (7) all countries. Each of these country datasets is further split along the temporal axis: 1980-2000; 2000-2016; and 1980-2016. In total, we end up with (7 x 3 =) 21 country-year panel datasets which are separately fed into the EBA model.

Third, the inclusion and exclusion of unit and time fixed effects is of crucial importance for empirical analysis. Although fixed effects can address omitted variable bias and thus enhance the panel estimate of interest (especially when endogeneity is a real concern), they may also remove variation which might be beneficial for the analysis at hand (Woolridge 2010; Mummolo and Peterson 2018). Thus, the choice about the fixed effect structure depends on whether the researcher's aim is to explain *within*- or *cross-country* *heterogeneity* (Huber et al. 1993; Plümper and Troeger 2007). To allow for these differences in specification choices, we use EBA with and without country and/or year fixed effects and report all four resulting versions of fixed effect structures.

Fourth, the choice of appropriate standard error types is essential for statistical inference. In addition to the unadjusted standard error, we test whether the share of significance changes when we adjust for heteroscedasticity in the residual term by using robust ones. Furthermore, using the Huber-White standard errors (Huber 1967; White 1980) clustered at the country level is favoured by many applied scholars nowadays working with panel datasets as it takes into account that the error terms are likely to be correlated within units (Cameron and Trivedi 2005; Hoechle 2007; Cameron and Miller 2015; King and Roberts, 2015). However, despite their popularity, using clustered standard errors is only appropriate when the number of clusters is sufficiently large (Angrist and Pischke 2008; Abadie et al. 2017; Cunningham 2021), which may not always be the case when working with country-year panel datasets. Thus, we apply the EBA approach using all three types of standard errors to identify potential differences.

Fifth, to account for debates about the correct conceptualization and operationalisation of the dependent variable, we estimate our EBA models with three different measures of welfare state generosity. This is important because previous empirical studies have identified major differences between spending and entitlement measures (Wenzelburger et al. 2013). Specifically, we use three distinct dependent variables: total social expenditure as % GDP (Armingeon et al. 2015), social transfers as % GDP (Armingeon et al. 2015), and an index of welfare state entitlement (Scruggs et al. 2014). For each of these indicators, we run specification in levels as well as their first differences. While level-based measures are more appropriate to make direct size comparisons between welfare states, such measures are likely to suffer from non-stationarity and autocorrelation issues.

Formally, our A-EBA model estimates the following linear model for each regime-period sample, s,

$$Y_d = \alpha_0 + \beta_1 X + \beta_{k,p} C_{k,p} + \vartheta_i + \delta_t + \epsilon$$

with Y_d as the dependent variable where the subscript d reflects the fact that we re-run our EBA on different measures and operationalisations of welfare state generosity; ϑ_i and δ_t as country and year fixed effects, respectively; β_1 as the estimate of interest for the determinant of interest, X; β_k as the coefficient of covariate C_k with p indicating the set of control variables C that is included in the estimation. We re-run the analysis with/without ϑ_i and/or δ_t. We then extract β_1 (together with the accompanying standard errors) over all specifications of d, s and p to construct the extreme bounds and significance shares for each independent variable X.

Data

We collect a country-year panel dataset covering 33 countries between 1960 and 2016. Based on an in-depth literature review (see table A1.1), we select a wide range of variables that have been used to empirically test theoretical arguments from different strands of the literature. From this list of potential factors, we select 19 independent variables which we see as most relevant in the literature. Like other modelling choices, one can debate whether other aspects were more important to include and/or whether more combinations should be attempted. However, as in previous EBA application, selection is required as the number of regressions increases exponentially with the number of determinants and exceed our computational and technical resources available. For instance, using 30 control variables would results in 1,073,741,823 control sets which is more than 4,000 times as large as the number of control sets we are handling for the present analysis.

Most of our independent variables are taken from the Comparative Political Data Set compiled by Armingeon et al. (2015), namely: share of left-wing cabinet seats, share of left-wing parliament seats, share of centre cabinet seats, share of centre parliament seats, index of constitutional structures based on Huber et al. (1993), index of disproportionality proposed by Gallagher, EMU membership dummy, unemployment rate, share of elderly, trade openness, financial openness, debt as % GDP, union density, the Employment Protection Legislation (EPL) index, share of industrial employment, crisis dummy. In addition, we add GDP per capita data from the IMF World Economic Outlook database and the index of corporatism by Jahn (2016). Finally, to account for potential autoregressive effects, we construct a model-specific variable that records the first lag of the respective dependent variable. A description of all variables, including summary statistics and sources, is provided in Table A1.2.

Even though this selection of independent variables requires some (inevitably subjective) judgement calls and may be influenced by the understanding of the literature of the authors, many of these variables and data sources have been used in previous comparative studies. With $n = 19$ independent variables, we create $\sum_{k=1}^{n} C_k(n) = \sum_{k=1}^{n} \binom{n}{k} = \sum_{k=1}^{n} \frac{n!}{k!(n-k)!} = 2^n - 1 = 524{,}287$ control sets. Each of these sets is used in combination with (i) different country and time samples (21), (ii) country and time fixed effects structures (4), (iii) types of standard error (3), and (iv) different dependent variables (6). Thus, in total, with 524,287 control sets and 1,512 (21 x 4 x 3 x 6) model specifications, we end up with 792,721,944 regressions in the whole model space. These regressions include ~7.53bn estimates (coefficient and standard error combinations) with 396,360,216 estimates for each independent variable ($n = 19$). We were able to process this large computational task in a fraction of the time by distributing the estimation of the model universe across multiple computers on a High-Performance Computing Environment.

Results

Unrestricted Baseline Results

The key finding of the present paper shows that there exists large model uncertainty for the effect of all determinants under consideration. Our baseline findings are threefold. For each independent variable of interest, we retain 396,360,216 coefficients with their accompanying standard errors. In general, we do not find one single independent variable that reaches a significant coefficient (p-value <0.1) in more than 70% of the cases (Table 1). In contrast, only the first lag of the dependent variable, the crisis dummy (1 if real economic growth is negative, 0 otherwise), and trade openness reach significant coefficients in more than (at least) 48% of the cases. Beyond these three determinants, the lack of significant results for the other independent variables is surprising. While none of them appear to be particularly robust to changes in the underlying model specification, none of them are insignificant across all model assumptions either.

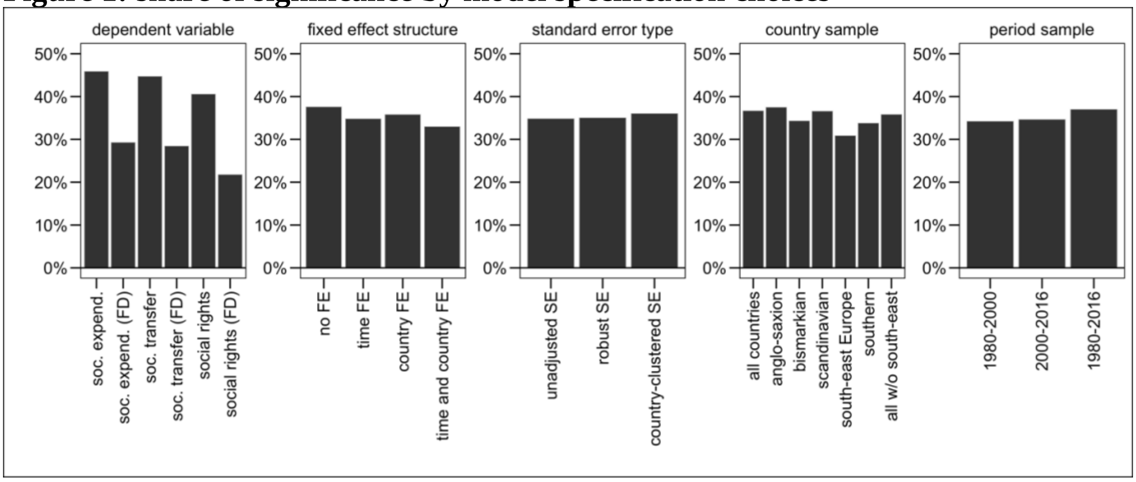
Table 1: Baseline EBA results – number and share of models by significance

Variable	% sign.	# of estimates	# sign. negative	% sign. negative	# of sign. positive	% sign. positive
first lag of dependent variable	67.6%	376185276	13085190	3.5%	241179970	64.1%
crisis dummy	54.5%	376479927	6903544	1.8%	198196159	52.6%
trade openness	48.9%	376479810	151820294	40.3%	32174414	8.5%
union density	38.4%	376457931	76982716	20.4%	67618676	18.0%
share of elderly	38.0%	376479852	42487020	11.3%	100559602	26.7%
unemployment rate	37.7%	376479987	61568934	16.4%	80495529	21.4%
EMU membership	36.8%	364895814	36495911	10.0%	97893703	26.8%
public debt %GDP	36.2%	376397949	68637866	18.2%	67805759	18.0%
employ. protect. legislation	34.0%	375942921	39577076	10.5%	88299725	23.5%
GDP per capita	32.7%	376479888	34967621	9.3%	88309936	23.5%
capital account openness	30.7%	354225825	52627320	14.9%	56220212	15.9%
share of industrial employment	30.0%	376476129	72731548	19.3%	40213494	10.7%
share of centrist parliamentarians	28.0%	376480014	29615059	7.9%	75839832	20.1%
share of left-wing parliament.	27.5%	376479930	41480174	11.0%	61997908	16.5%
share of left-wing party cabinet posts	26.7%	376480071	31498621	8.4%	69068045	18.3%
share of centrist party cabinet posts	26.0%	376479969	51431483	13.7%	46547791	12.4%
index of constitutional structure	26.0%	292977468	31341044	10.7%	44896773	15.3%
index of corporatism	25.6%	376480044	42681385	11.3%	53816240	14.3%
index of disproport.	23.1%	376479897	26423823	7.0%	60470671	16.1%

Note: the total number of estimates may differ slightly from the full model universe because certain models could not be estimated due to singularity, missing data, or convergence issues.

With respect to the signs of significant coefficients, we observe a similar scattered, at times contradictory, set of results. Among all variables under consideration, there is only one variable – namely the crisis dummy – for which a very large part of significant coefficients (~95%) has the same sign. The other determinants show substantial shares of significant coefficients pointing in both directions – often with similar likelihood, for instance partisanship indicators, public debt as % GDP, capital account openness, or union density. Even though variables with more significant coefficients tend to be concentrated in either positive or negative direction category (trade openness, share of elderly, employment protection legislation, and GDP per capita), we find nevertheless a large number of significant coefficients pointing in either direction even for these variables. Here, it is noteworthy that a share of 5%-significance in one direction refers to 19 million coefficients.

Figure 1: share of significance by model specification choices



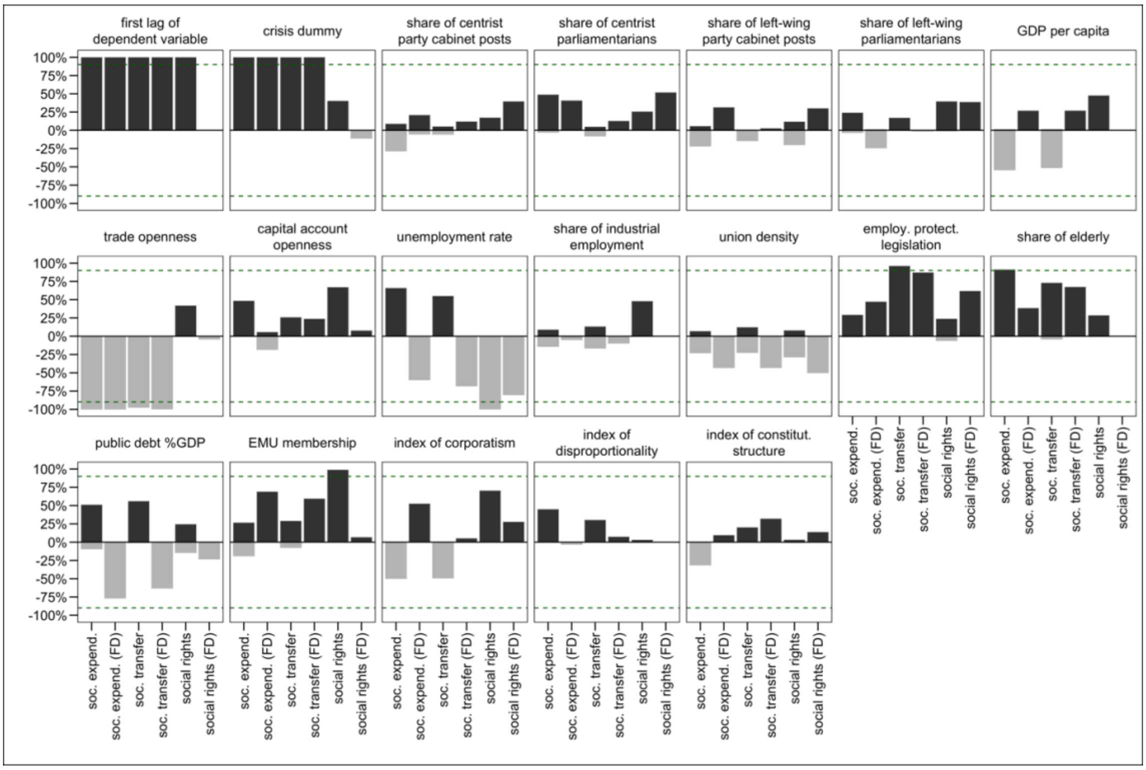
Note: the figure plots the share of (positive and negative) significant coefficients in the whole model universe for all independent variables under consideration across different model specification choices.

In addition to these differences in significance shares across independent variables, our results also indicate that model specifications can play a large role in whether a variable reaches significance or not. As Figure 1 shows, the choice of dependent variable carries substantial weight for the resulting significance shares. Most notably, using the first difference of a generosity measure decreases the shares by up to 50% (see, for instance, social rights indicator). This overall finding is in line with earlier research highlighting that the significance of the estimate can vary substantially depending on the generosity indicator that is used as dependent variable (Wenzelburger et al. 2013). In addition, we also note some variation in significance shares across country samples although these differences are smaller compared to the dependent variable choice. Interestingly, neither the fixed effect structure, nor the standard error type nor the period selection seems to make – on average – a major difference for the p-value distribution.

When we investigate the significance distribution for each independent variable separately, the importance of all empirical decisions becomes more visible. Here, for each plot, we only vary one model specification dimension while holding all others fixed. To be more specific, we depart from the baseline specification which uses (i) social expenditure %GDP as dependent variable, (ii) the full sample (all countries from 1980s onwards), (iii) with unadjusted standard errors, and (iv) both year and unit fixed effects included.

Based on this baseline specification, we change one model dimension at a time. For instance, when we discuss the impact of the dependent variable choice (Figure 2), the graph only shows regression results that uses the full sample, with unadjusted standard errors, and both fixed effects included. This form of discussing the results is necessary as it enables us to visualize the impact of one model assumption in a *ceteris paribus* manner.

Figure 2: share of significance by dependent variable choice

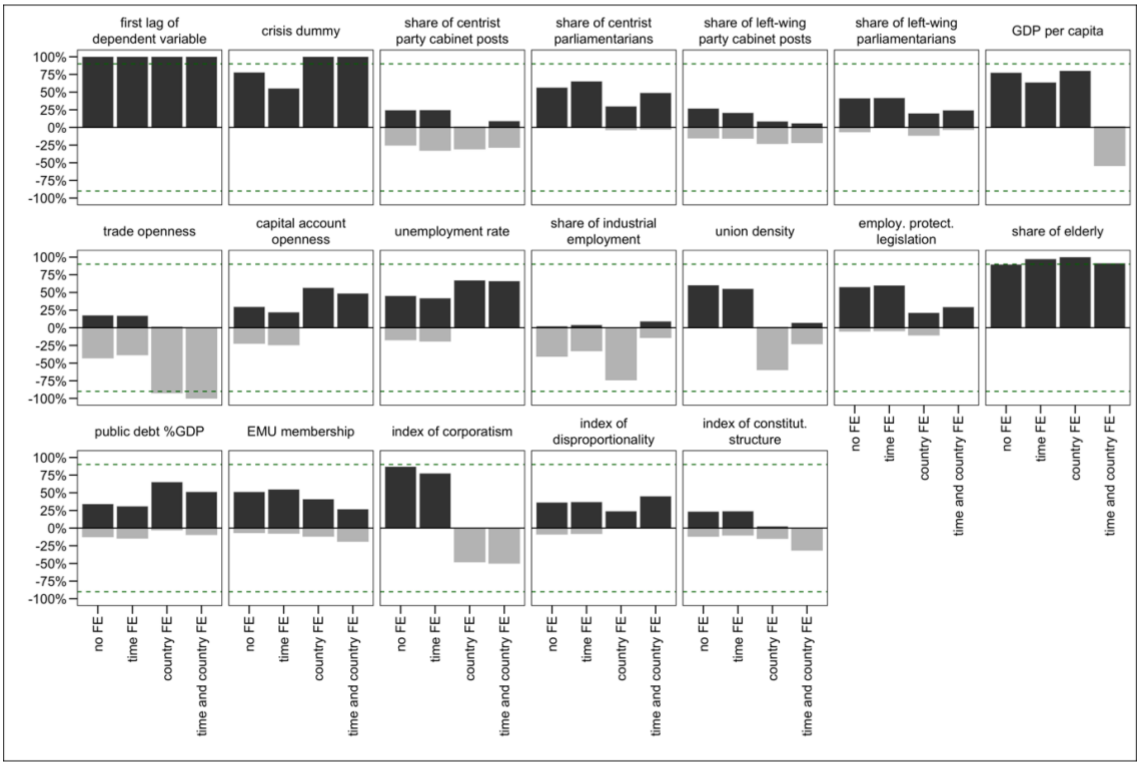


Note: the figure shows the share of positive (dark) and negative (light) significant coefficients for each independent variable by different dependent variable choices (levels and first differences (FD) versions). The model space contains all models with both fixed effects, unadjusted standard errors and all countries and time periods included (from 1980 onwards). 100% refer to ~262,143 estimates.

Overall, the unrestricted EBA results indicate five aspects which are worth highlighting. *First*, the impact of the dependent variable choice on the significance shares depends substantially on the independent variable under investigation (Figure 2). While using change-based measures as dependent variables results in usually smaller significance shares (as noted above), our findings show that this is particularly the case for the share of the elderly, capital account openness and the share of industrial employment. In addition, we note a tendency for expenditure-based indicators to reach higher shares of significance than entitlement measures do. We also find both positive and negative significant coefficients for all determinants under consideration (except the first lag of the dependent variable).

Second, in contrast to the small average impact of the fixed effect structure discussed above, the variation in significance shares becomes substantially larger when we zoom into certain independent variables. Two aspects are worth highlighting in Figure 3. On the one hand, the crisis binary and the share of the elderly – beyond the first dependent variable lag – are the only two variables that do not indicate sign switches and which reach relatively large significance shares. The sign switches are particularly severe in the case of corporatism, GDP per capita trade openness, union density and partisanship strength in the cabinet. On the other hand, we cannot confirm that the fixed effect assumptions generally change – on average – the significance level. In contrast, using both time and country fixed effects can sometimes even lead to larger significance shares for several independent variables under consideration (e.g. capital account openness, trade openness, unemployment rate, debt % GDP or disproportionality index).

Figure 3: share of significance by fixed effect structure choice

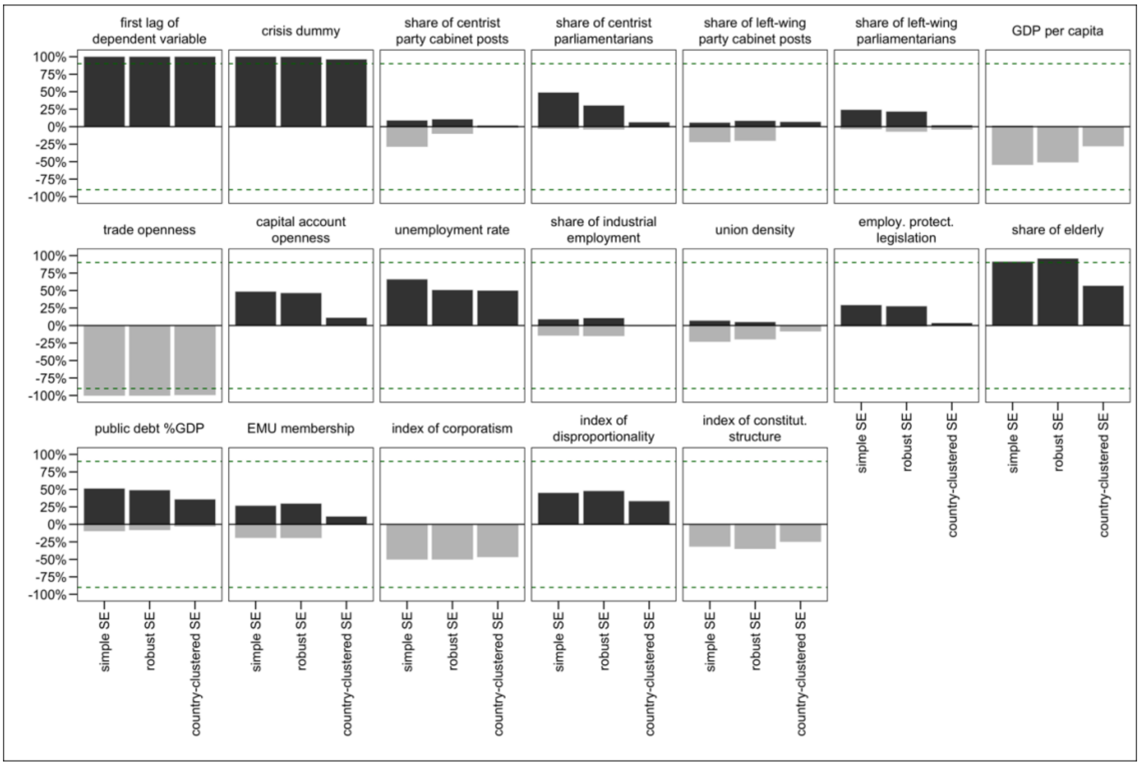


Note: the figure shows the share of positive (dark) and negative (light) significant coefficients for each independent variable by different fixed effect structures. The model space contains all models with social expenditure % GDP as dependent variable, unadjusted standard errors and all countries and time periods included (from 1980 onwards). 100% refer to ~262,143 estimates.

Third, we find that the choice of the standard error type can make a difference for certain determinants, where robust country-clustered standard errors tend to have lower levels of significance than unadjusted or robust ones (Figure 4). The differences are most notable for partisanship variables, GDP per capita, capital account openness and the share of elderly. For instance, in the case of share of elderly, the significance share can drop by over 50% (from 90% with unadjusted standard errors to 45% with country-clustered standard errors). These results show that even though we did not find substantial differences across standard error types when looking at the whole estimation universe

(Figure 1), focusing on models with selected specifications can make the standard error type an influential determinant of the significance share for certain independent variables.

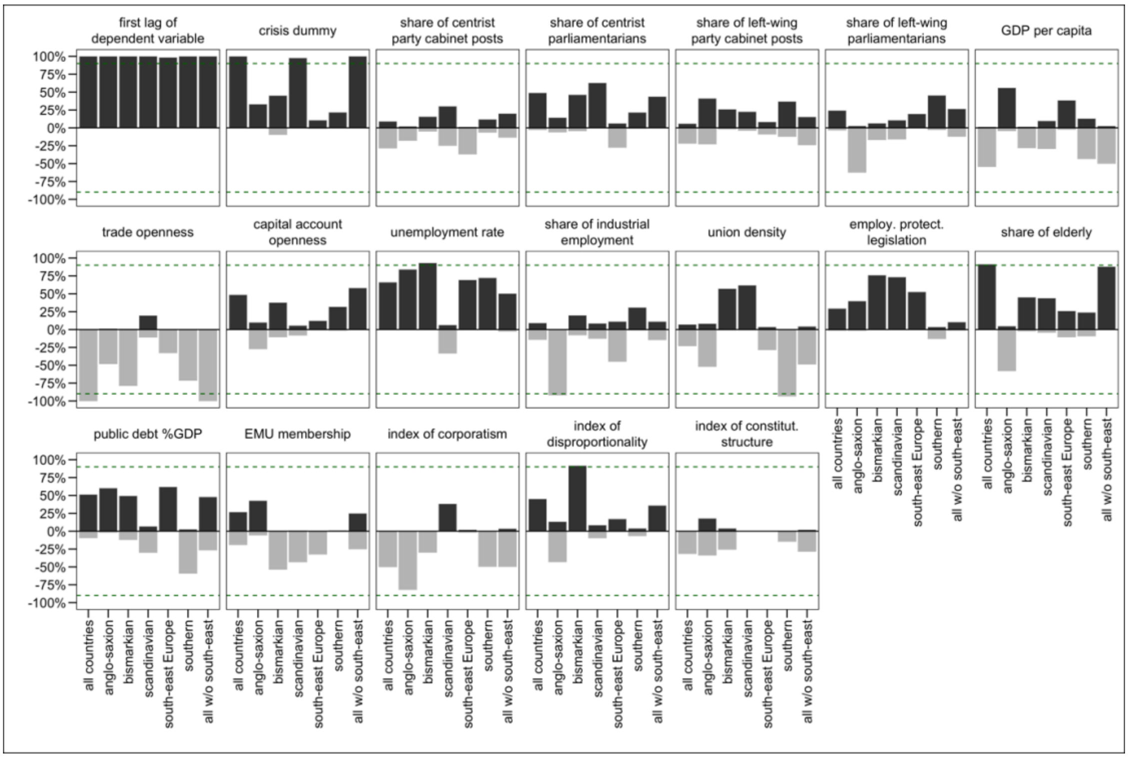
Figure 4: share of significance by standard error type choice



Note: the figure shows the share of positive (dark) and negative (light) significant coefficients for each independent variable by different standard error types. The model space contains all models with social expenditure % GDP as dependent variable, time and country fixed effects and all countries and time periods included (from 1980 onwards). 100% refer to ~262,143 estimates.

Fourth, when it comes to the sample selection by welfare state regimes, our results indicate great sensitivity across regimes samples (Figure 5). The vast majority of determinants under consideration show great fragility towards sample selection choices. The variation in significance share is particularly large for debt as % GDP, share of elderly, GDP per capita and union density.

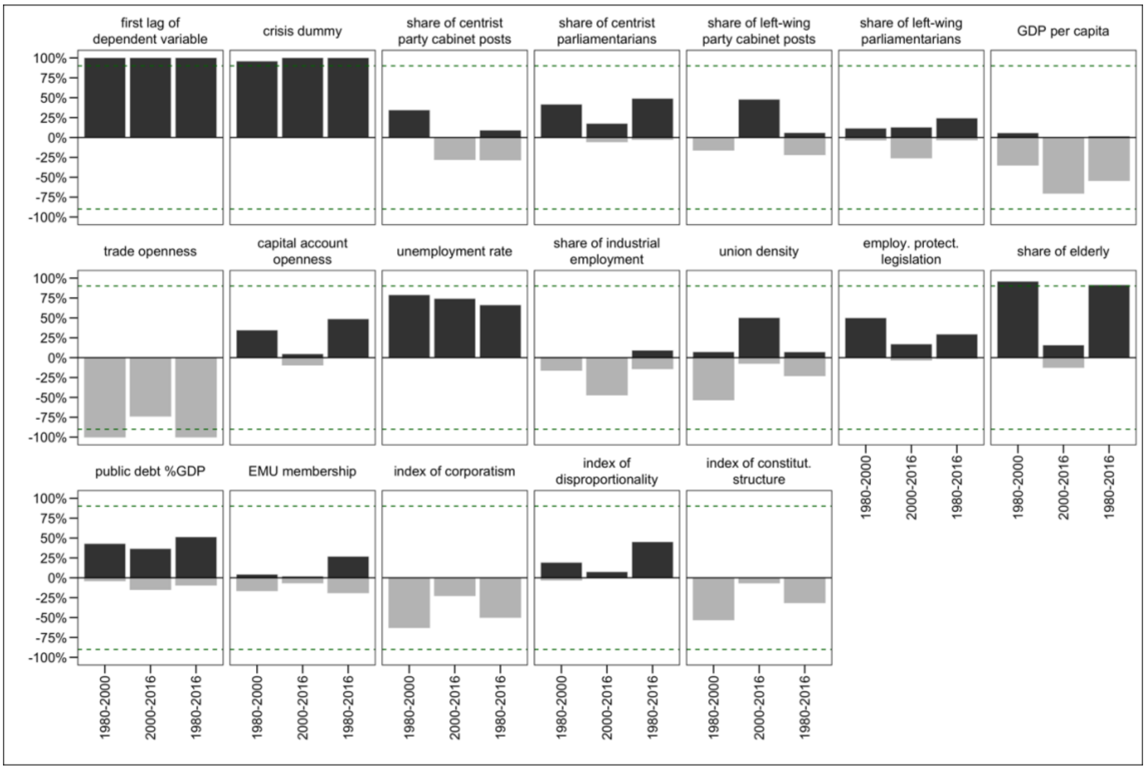
Figure 5: share of significance by country sample choice



Note: the figure shows the share of positive (dark) and negative (light) significant coefficients for each independent variable by different country samples. The model space contains all models with social expenditure % GDP as dependent variable, time and country fixed effects, unadjusted standard errors and all time periods included (from 1980 onwards). 100% refer to ~262,143 estimates.

Fifth, although the differences across time periods are less strong compared to the country samples, the sensitivity is nevertheless apparent for certain variables (Figure 6). For instance, the share of the elderly does not correlate robustly since 2000 but it mattered quite substantially between 1980 and 2000. This contrast is even stronger for union density where its association with generosity was positive after 2000, but negative before 2000. Similarly, the sign switches for several partisanship variables which provides evidence that the time period used can play an important role for the significance shares.

Figure 6: share of significance by period sample choice



Note: the figure shows the share of positive (dark) and negative (light) significant coefficients for each independent variable by different period samples. The model space contains all models with social expenditure % GDP as dependent variable, time and country fixed effects, unadjusted standard errors and all countries included. 100% refer to ~262,143 estimates.

To sum up, these baseline results highlight the importance that all model specification decisions under consideration play a key role for the robustness of welfare state determinants. Specifically, the choice of dependent variable as well as the country sample selection is likely to be – on average – the most influential decision that has to be made.

Restricted Results

One criticism of the unrestricted EBA results is that there might be a large share of mis-specified models in terms of empirical and/or statistical properties. If this is the case, EBA estimates can skew the results in both directions and bias the conclusion. Leamer (1985) himself was very much aware of this issue and he proposed a “model narrowing” step in which all unreasonable linear combinations are excluded. To address the issue of misspecification, the literature has resorted to two ways of dealing with this problem, namely applying (i) model universe restrictions or (ii) model weighting (through a goodness-of-fit measure).

The first solution entails to impose *a priori* restrictions on the model universe M to remove all estimates which fail to meet certain empirical and/or statistical properties. For the present analysis, we exclude all regression models that suffer from multi-collinearity by removing all models from M in which the independent variable of interest has a Pearson correlation coefficient (absolute size) that is larger than 0.5 with one (or more) covariates in a particular regression. In addition, we shrink the model universe further by restricting M to models which indicate a sufficient goodness of fit measure. Here, we only keep an estimate if its Akaike Information Criterion (AIC) belongs to the top 10% of the AIC-distribution over M for a given determinant. In the appendix, we also show EBA results when using the R-Squared instead of the AIC for restricting the EBA universe (cf. Granger and Uhlig 1990) (see Figure A3.3-A3.7). In general, restricting based on AIC or R-Squared returns similar results.

Overall, even though the significance shares change slightly for most independent variables, our main results on model uncertainty are still present after applying model narrowing and analysing only restricted results. In quantitative terms, restricting M by imposing restrictions decreases the significance share across all determinants – on average – by 2.2% (Table 2 for AIC-based restrictions; see Table A3.1 for R^2-based restrictions). The index of corporatism is the variable which experiences the largest drop in significance share (-6.6%), while the significance shares of trade openness show the largest increases (+5.8%) after the imposition of model restrictions. In other words, removing ill-specified models does not have a very large impact on significance in the model universe as a whole.

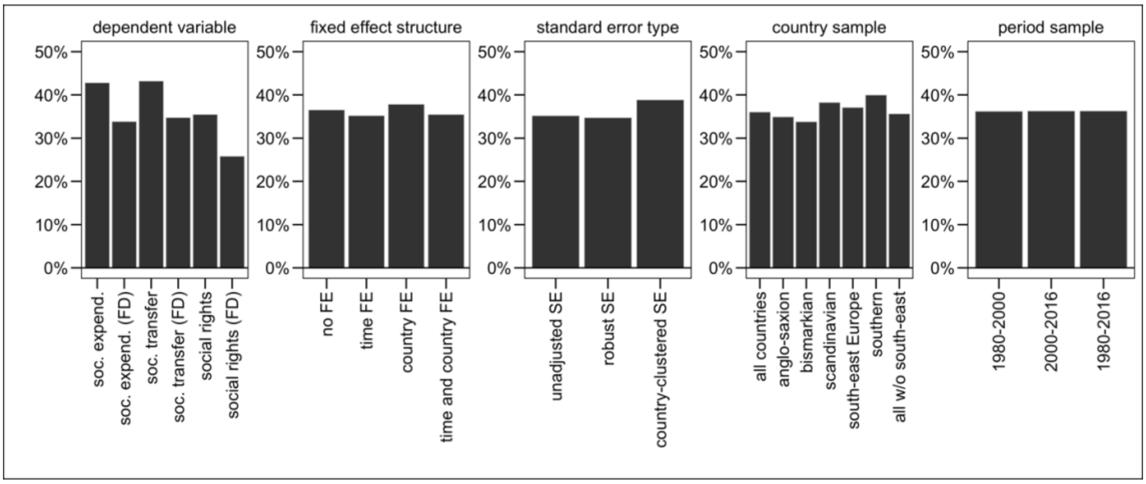
When we disentangle the restricted model universe along key model properties, our results are similar to our (unrestricted) baseline. As Figure 7 shows, the choice of dependent variable is still the specification choice that carries the largest bearing on the likelihood of observing a significant coefficient, followed by the selection of the regime sample (see Figure A3.2 for restricted results based on R^2). Again, neither the fixed effect structure, nor the standard error type nor the period selection are – on average – as important. Despite these low average changes, we find evidence that restricting M can indeed lead to clearer robustness and sensitivity patterns for certain independent variables under certain model conditions. In other words, restricting M does produce very low or very high significance shares for certain determinants once a given model specification is held fixed. For instance, as Figure A2.1 shows, trade openness has a very robust negative effect. The variation in significance shares by model specifications and independent variables are displayed in Figure A2.1-A2.5.

Table 2: AIC-restricted EBA results – number and share of models by significance

Variable	% sign.	# estimates	# sign. negative	% sign. negative	# sign. positive	% sign. positive
first lag of dependent variable	66.1%	37618266	3018693	8.0%	21859877	58.1%
crisis dummy	55.8%	37647747	590716	1.6%	20422203	54.2%
trade openness	54.7%	19004487	9365990	49.3%	1029328	5.4%
employ. protect. legislation	39.1%	9099486	707051	7.8%	2848674	31.3%
share of elderly	36.8%	22921944	2659769	11.6%	5765801	25.2%
union density	35.4%	13650045	3302095	24.2%	1526692	11.2%
EMU membership	34.3%	24024339	1916646	8.0%	6327803	26.3%
unemployment rate	32.9%	26730783	6005409	22.5%	2796463	10.5%
public debt %GDP	30.8%	19737243	4046288	20.5%	2031286	10.3%
GDP per capita	28.0%	17023521	1108371	6.5%	3657696	21.5%
share of centrist parliamentarians	27.8%	15240852	1235383	8.1%	2993968	19.6%
share of centrist party cabinet posts	25.7%	15240915	1377113	9.0%	2535690	16.6%
index of constitutional structure	24.6%	11514507	810829	7.0%	2025806	17.6%
capital account openness	24.5%	22019703	2918195	13.3%	2478089	11.3%
share of industrial employment	23.8%	18400914	2290017	12.4%	2081140	11.3%
share of left-wing party cabinet posts	22.9%	16946589	1087826	6.4%	2792250	16.5%
share of left-wing parliamentarians	22.9%	16946583	1024611	6.0%	2851553	16.8%
index of disproportionality	21.4%	21211920	2279550	10.7%	2254828	10.6%
index of corporatism	19.0%	10276533	520719	5.1%	1427281	13.9%

Note: the total number of estimates may differ slightly from the full model universe because certain models could not be estimated due to singularity, missing data, or convergence issues.

Figure 7: share of significance by model specification choices (AIC-restricted)



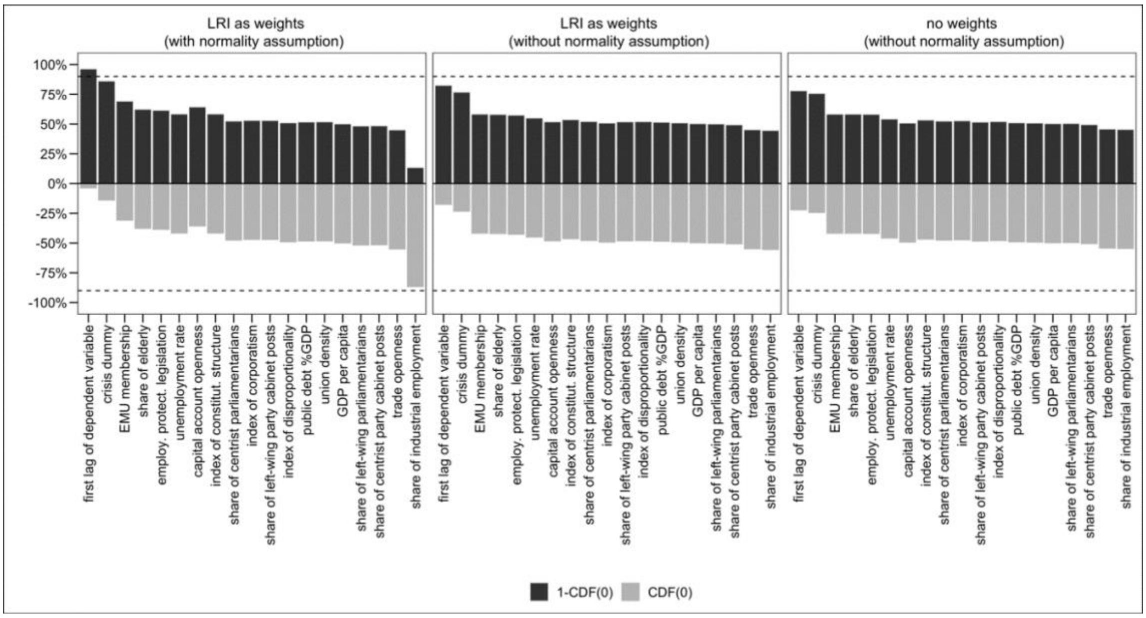
Note: the figure plots the share of (positive and negative) significant coefficients in the whole model universe for all independent variables under consideration across different model specification choices. The model space has been restricted by removing all models that suffer from multi-collinearity and weak goodness of fit measures based on the Akaike Information criterion.

Weighted Results

An alternative to restricting the model universe is to weigh the estimates with a goodness of fit measure, e.g. the likelihood ratio index (LRI), and then construct the cumulative density function (CDF) to determine the robustness/fragility of an explanatory variable of interest. This approach comes with the added advantage that it exploits the entire distribution of estimates without setting arbitrary exclusion criteria *a priori*.

When we use the models' LRI as weights across the entire model space, our results confirm our findings from the baseline and restricted EBA: independent of the type of CDF being applied, all independent variables have probability masses below 90% on either side of zero (Figure 8). In fact, most covariates have probability masses roughly equally balanced both on the positive and negative side. The only exception here is the crisis dummy, the first auto-regressive term, and the share of industrial employment which show probability masses close to or above 90%. Despite these rather robust findings for these three variables, we cannot conclude that the results using the CDF decision rule differs substantially from the original EBA bounds of Leamer. This finding is contrary to the results by Sala-i-Martin (1997) who found a set of robust predictors of economic growth after model weighting.

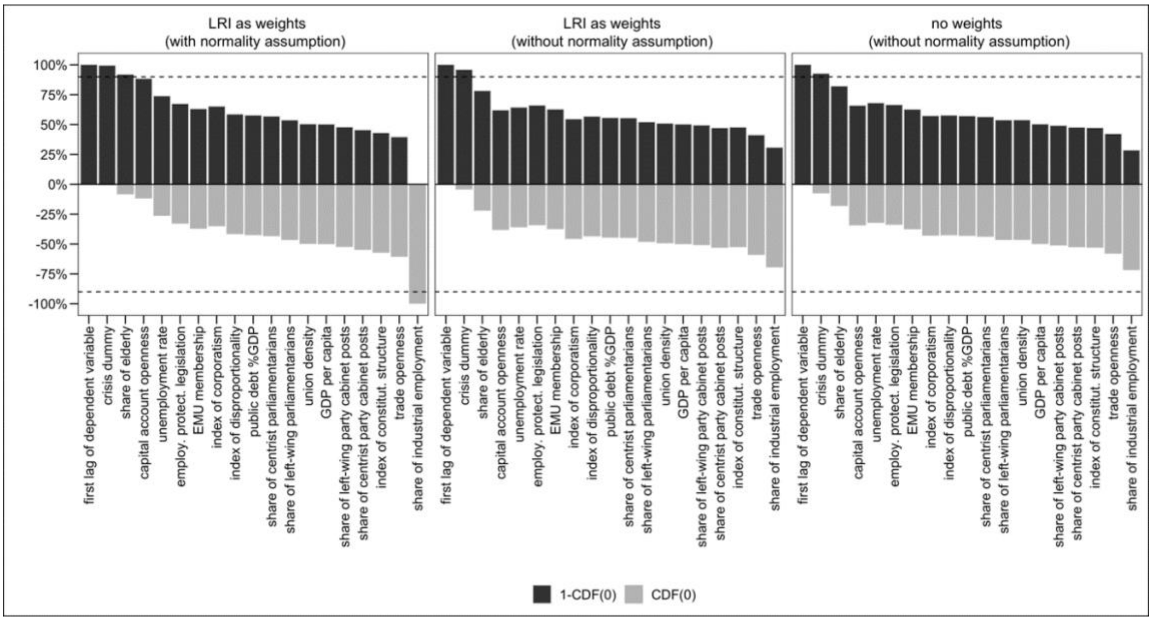
Figure 8: cumulative distribution function using the entire model space



Note: the figure plots the cumulative distribution function (CDF) using the entire model space across different weighting approaches and normality assumption. These three ways of constructing the CDF is equivalent to the results provided by Sala-i-Martin (1997).

Nevertheless, it might be that applying the LRI-weighting scheme to the entire model space might be inappropriate as LRI depends, amongst other factors, on the sample size. Therefore, in addition to considering the entire model space, we re-estimate the CDF after restricting the model space to regressions that are based on the full sample (all countries and years from 1980 onwards) and that uses social expenditure %GDP as dependent variable (baseline). Such a restricted version using one sample is equivalent to earlier EBA exercises (e.g. Sala-i-Martin 1997).

Figure 9: cumulative distribution function using a restricted model space



Note: the figure plots the cumulative distribution function (CDF) using a restricted model space (all estimations that are based on the full sample (all countries and year observations) and that uses social expenditure % GDP as dependent variable) across different weighting approaches and normality assumption. These three ways of constructing the CDF is equivalent to the results provided by Sala-i-Martin (1997).

However, the results are very similar to the CDF based on the entire model space (Figure 9). While it is true that the share of elderly and capital account openness indicate – in addition to the crisis dummy and share of industrial employment – a relatively robust association with social expenditure %GDP with the 1-CDF(0) being close to 90%, the majority of independent variables is considerably below this threshold. In summary, these results show that using goodness-of-fit measures as weights to determine the robust-fragile classification of an independent variable does not enable us to draw a more positive outlook about the stability of the empirical estimates.

Using deep learning to identify the sources of model uncertainty

In the result sections above, we have focused on one single model characteristic while holding all others model specification choices fixed. This enabled us to provide a more detailed discussion of how certain empirical assumptions affect the robustness of the coefficients. However, to provide a more holistic picture, we also plot the distribution of the significance share for all model specifications[8] (1,512 in total) for each independent variable (Figure 10; see Figure A2.6 and A3.8 for restricted results based on AIC- and R^2, respectively). Three aspects are noteworthy.

First, while many model specifications indicate very small significance shares close to zero across welfare state determinants, we find at least a few specifications with very high significance shares for most of the independent variables of interest. This means that *specification searching* is generally possible for all independent variables under consideration. *Second*, almost all variables have at least one model specification which reaches significant coefficients in more than 90% of the cases. In other words, for each factor, there exists a combination of empirical decisions that delivers highly robust results. *Third*, we find such "outlier" specifications for both directions for almost all variables (except a few partisanship indicators and industrial employment). Put differently, among 1,512 model specifications, there are very few specifications for a given determinant that can be used to (spuriously) claim robustness for either a positive or a negative effect.

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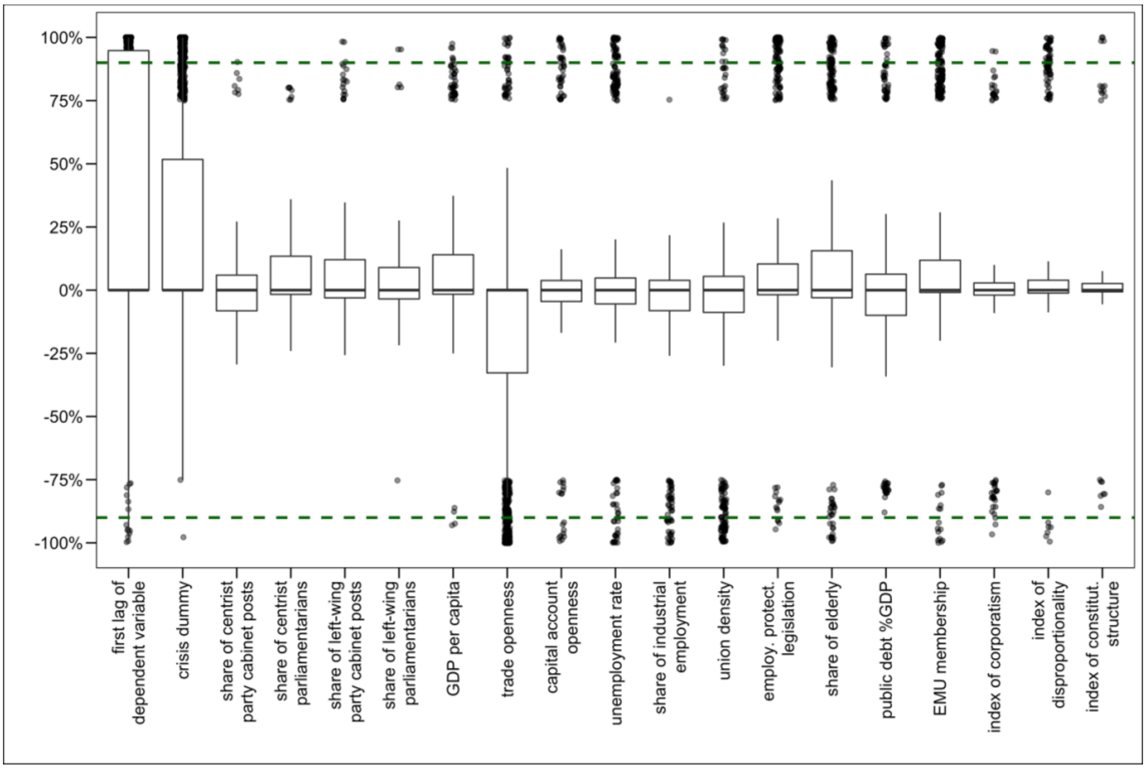
[8] defined as a combination of (i) dependent variable (6), (ii) fixed effect structure (4), (iii) standard error types (3) and (vi) sample selection (21). In sum, this results in 1,512 model specifications (6 x 4 x 3 x 21)

Figure 10: distribution of significance shares by model specification



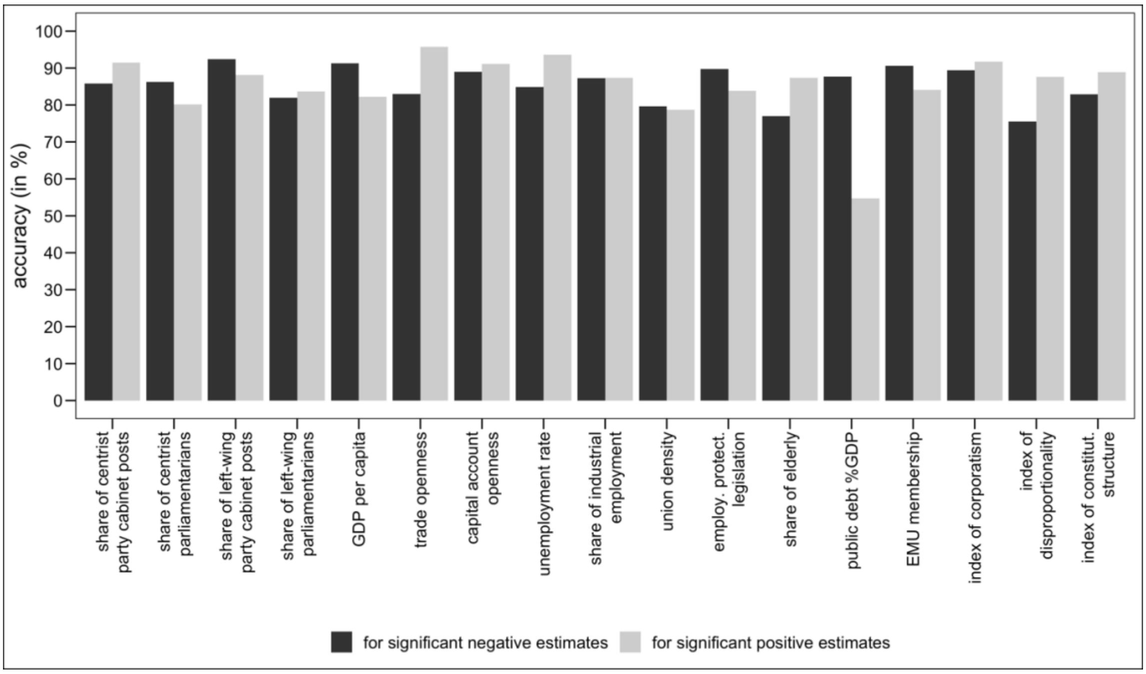
Note: the figure shows the boxplot (including outliers) of shares of positive and negative significant coefficients for each independent variable across different model specifications (1,512 in total). A model specification is defined as a selection of dependent variable, fixed effect structure, standard error type, country sample and period sample. The model space for one model specification refers to ~262,143 estimates.

Overall, our A-EBA shows that the presence of a large number of different model specifications increases coefficient sensitivity for almost all variables under consideration. In contrast to traditional EBA exercise and/or other methods quantifying model uncertainty, the A-EBA approach used in this paper suggests that – in addition to control variable selection – coefficient sensitivity can originate from several quite different sources. In what follows, we model the possible non-linear and/or interactive effects of multiple model specification on statistical significance. To do so, we adopt a deep learning approach that consists of two steps: (1) building a neural network to predict the significance classification based on the model specifications, and (2) assessing the influence of each dimension on the significance class prediction.

In the first step, we build a neural network that uses 42 model specification binaries[9] as input features[10] to predict the significance classification of an estimate of interest: ’positive significant’, ’negative significant’, and ’not significant’. The feature binaries on the right-hand-side are equal to 1 if a model specification is applied (e.g. whether a specific control variable is included), and 0 otherwise. The outcome variable is a binary coded 1 if an estimate is ‘significantly positive’ (or ’significantly negative’), and 0 otherwise. We formulate the prediction task as a binary classification problem to check whether the relevance of features differs with respect to the sign of the estimate. As the performance of neural networks may depend on certain hyperparameters[11] which are unknown *a priori*, we tune the neural network with a grid search algorithm that iterates over a set of hyperparameter combinations[12]. Due to computational limitations, we estimate the model on a random subset of regression models consisting of 250,000 regressions for each independent variable from the A-EBA exercise above. To prevent over-fitting, we split this full dataset in train, cross-validation, and test set which consist of 60%, 20%, and 20% of the full sample, respectively.

[9] Specifically: 19 control variables + 4 fixed effect structures + 6 dependent variables + 3 standard error types + 21 samples.
[10] Here we follow the machine learning terminology where independent variables are called ‘features’.
[11] These include the number of hidden layers, neurons, and dropout rate.
[12] These hyperparameters include dropout rate, $\nu \in \{0, 0.3, 0.5\}$ (to prevent overfitting) between the layers; number of hidden layers, $\xi \in \{4, 5, 6\}$; and number of neurons per hidden layer, $\phi \in \{32, 64, 96\}$.

Figure 11: performance of best models after hyperparameter tuning



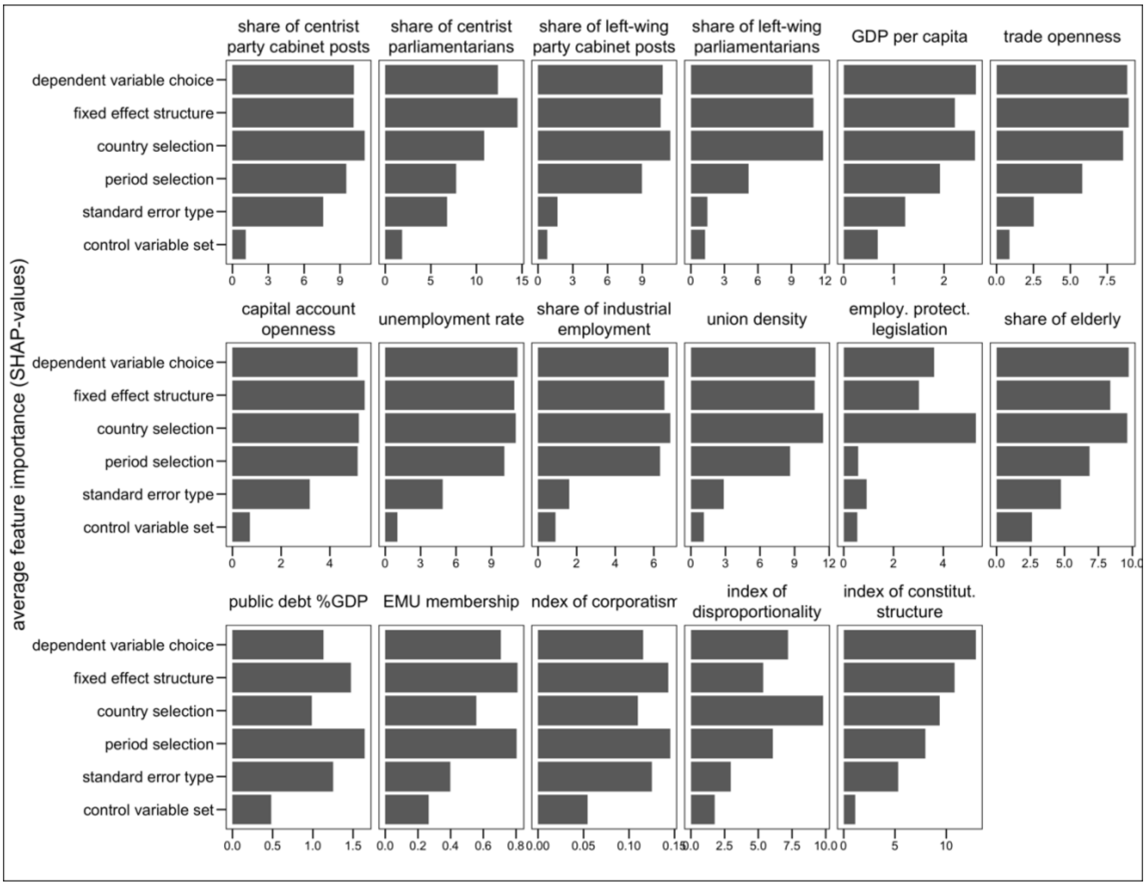
Note: the figure shows accuracy of the best-performing model for each determinant and coefficient direction after hyperparameter tuning. Model accuracy is defined as the share of correctly classified observations (significance classification of an estimate) out of all observations. The calculation of the accuracy is based on the test set.

As Figure 11 shows, our grid search approach is able to identify suitable model structures that can accurately predict the significance and direction of an estimate based on the model specification it is estimated on[13]. In a second step, using the best performing neural network model for each determinant-direction combination, we calculate the SHAP (**SH**apley **A**dditive ex**P**lanations) values proposed by Lundberg and Lee (2017) for each feature to determine its influence on the significance class prediction. This feature importance metrices is a common technique to represent NN-models in an easily

[13] For all determinants, the hyperparameter tuning enabled us to find a model structure that reaches at least 75% accuracy (share of correctly classified observations (significance classification of an estimate) out of all observations) for all determinant-significance classification with one exception: significant positive public debt %GDP could only be classified correctly in 50% of the cases.

interpretable way. Although alternative feature importance scores exist, SHAP values have the advantage of measuring the relevance of a variable at a global level.

Figure 12: feature importance for the prediction of significant negative estimates

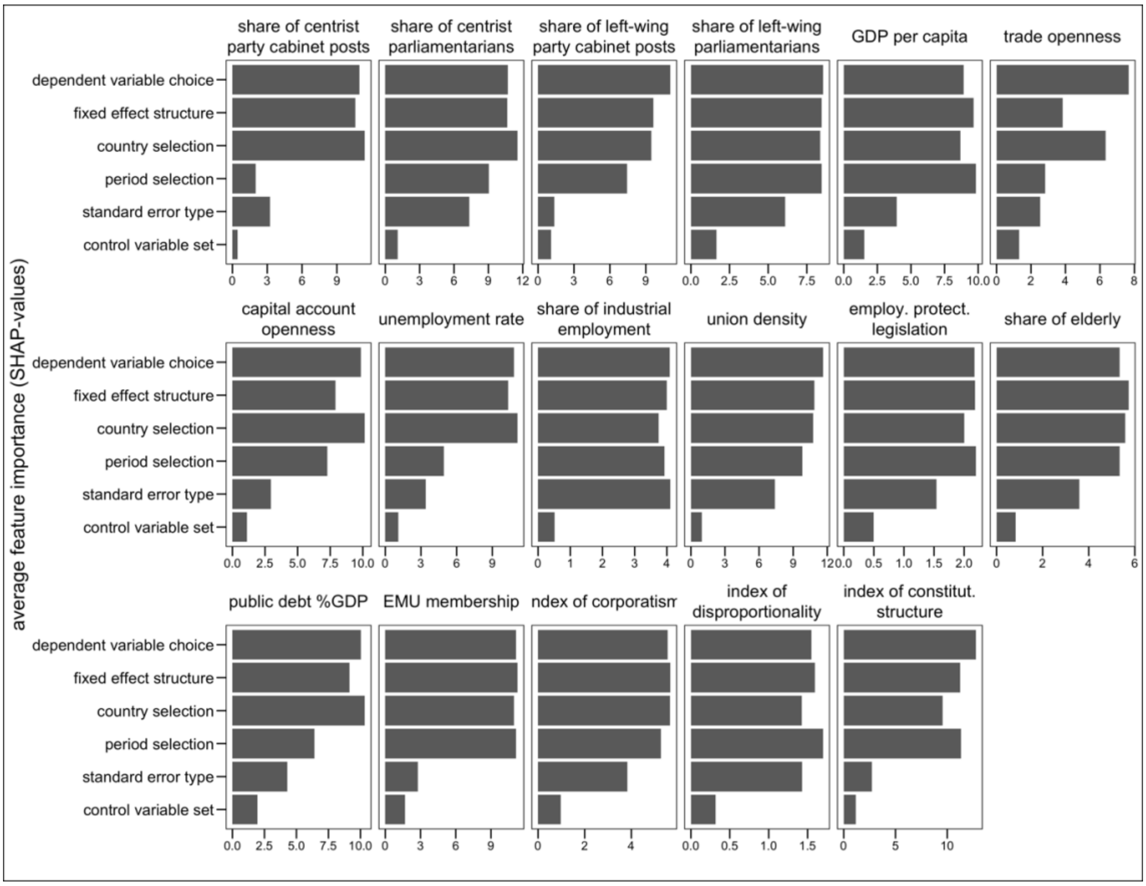


Note: the figure shows the average features importance (FI) (SHAP value) of six difference model specification choices across 17 independent variables (without the lagged dependent variable and crisis dummy). The scale of SHAP values is variable-specific which is why the scales of the individual plots differ. To calculate these SHAP values, we extracted a random set of 250,000 regression coefficients from the A-EBA exercise (see previous sections) and then adopted a deep learning approach to build a classifier that predicts whether an estimate is "negative significant" or "not significant". In total, the figure is based on 4.25 million estimates.

For readability and interpretability, we plot the averages for each model specification dimension in Figure 12 (prediction of significant negative estimates) and Figure 13 (prediction of significant positive estimates). The results show a relatively clear pattern: for all welfare state determinants under consideration, the control variable selection plays

– on average – the least important role for the prediction of the significance classification. By contrast, the sample selection (especially the country selection), the dependent variable choice, and the fixed-effect structure carry substantially more weight than the control set choice.

Figure 13: feature importance for the prediction of significant positive estimates



Note: the figure shows the average features importance (FI) (SHAP value) of six difference model specification choices across 17 independent variables (without the lagged dependent variable and crisis dummy). The scale of SHAP values is variable-specific which is why the scales of the individual plots differ. To calculate these SHAP values, we extracted a random set of 250,000 regression coefficients from the A-EBA exercise (see previous sections) and then adopted a deep learning approach to build a classifier that predicts whether an estimate is "positive significant" or "not significant". In total, the figure is based on 4.25 million estimates.

This highlights the fundamental sources of non-robustness: model uncertainty does not originate primarily from the selection of the control set, but rather from the choice of the sample, dependent variable, and/or fixed effect structure. Crucially, most of the model uncertainty approaches currently focus particularly on measuring estimate sensitivity related to control variable in-/exclusion: traditional EBA (Leamer 1978, 1983, 1985), Bayesian Model Averaging (see Fernandez et al. 2001), and specification curves (see Simonsohn et al. 2020). Yet, the deep learning analysis of our A-EBA results has shown that this is not where most model uncertainty originates from.

Conclusion

In conclusion, our Augmented EBA (A-EBA) has shown model uncertainty is substantial in the welfare state literature, which has wider implications for the threat of (intentionally or unintentionally) p-value hacking. We see three possible solutions to this methodological issue.

First, devoting greater attention to theory building is one of the most adequate first steps to improve the suitability of a modelling strategy for a research question at hand. Even though the data generating process is almost always unknown *a priori*, basing empirical decisions more explicitly upon theoretical reasoning can help to decrease the discrepancies between the true and the estimated model. However, this would require widely agreed upon criteria for identifying and considering all relevant theoretical frameworks, for instance using systematic literature reviews.

Second, since scientific papers are usually limited to a certain word or page limit, displaying robustness checks in conventional regression tables are unsuitable. Thus, using different visualization techniques – such as *specification curve analysis* (see Simonsohn et al. 2020) – or estimation approaches, such as *Bayesian Model Averaging* (see Fernandez et al. 2001), that quantify certain types of model uncertainty (e.g. choice of control variables) — can help to approximate the robustness of estimates with respect to certain model specifications.

Third, since these sensitivity approaches do not provide insights on how different model specification choices shape the statistical significance and direction of a coefficient of interest, the A-EBA as well as the deep learning approach presented in this paper aims to facilitate the work of applied researchers to identity the most relevant sources of model uncertainty in an easy-to-use way. In this way, our approach aims to shorten long-lasting robustness analyses and help scholars to focus on the assumptions that matter most.

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Appendix

A1: Literature review, variable selection, and summary statistics

Table A1.1. overview of previous welfare state literature

Main strands	**Key claim**	**Period of appearance**	**Key authors**	**Example of possible variables**
Functionalist	Economic developments drives welfare state development	1960s and 1970s	Cutright 1965; Jackman 1974; Wilensky and Lebeaux 1958; Wilensky 1974	Economic growth, industrialisation, urbanisation
Marxist	Welfare state serves legitimation and accumulation functions	1960s-1970s	Offe 1972; Gough 1979; Bowles and Gintis 1982; Piven and Cloward 1982; Miliband 1969, Domhoff 1972; Block 1977; Prezworski 1986	Capitalist development, elites in government, structural dependence on capitalists
Democratisation	Democracy leads to more welfare state spending	1960s and 1980s	Briggs 1961; Marshall and Bottomore 1992; Flora and Heidenheimer 1981; Haggard and Kaufmann 2020	Suffrage, democratic institutions, presence of civil rights
Power resources	Strength of labour movement in labour market and electoral arena leads to welfare state expansion	1970s – 1980s	Cameron 1978; Shalev 1983; Castles 1993; Korpi 1978; Korpi 1980; Stephens 1979; Esping-Andersen 1990; Korpi 1985; Stephen 1979; Hicks 1999	Left control of cabinet and parliament, union density and bargaining coverage, organisation of union movement

Main strands	**Key claim**	**Period of appearance**	**Key authors**	**Example of possible variables**
Regime approach	Historical class coalition dynamics are key policies get institutionalised	1990s-2000s	Esping-Andersen 1990; Ferragina and Seleib-Kaiser 2011; Van Kersbergen and Vis 2015; Arts and Gelissen 2010	Nature of rural-urban and religious cleavage
Statist	Explain cross-national variation with reference to the structure of the state	1980s	Skocpol 1980; Skocpol and Amenta 1986; Flora and Heidenheimer 1981	Mostly qualitative and typology
New institutionalist	Different state institutions shape policy outcomes by shaping interest, whether they manage to push their preferences and how they understand the meaning of their actions	1990s	Skocpol 1992; Immergut 1992; Steinmo 1993; Thelen 1999; Tsebelis 1995	Veto players, federalism, electoral system, policy legacies
Role of right	Right wing parties may have incentives to introduce social policies, under certain ideological and/or historical circumstances	1990s-2000s	Van Kersbergen 2003; Van Kersbergen and Manow 2009; Jensen 2014	Christian democratic control of government, right competing with hegemonic left, religion
New politics of welfare state	Welfare states are heavily constrained by their existing structure and level of generosity	1990s-2000s	Pierson 1994; Pierson 2001	Policy legacies, electoral system
Left irrelevance and insider-outsider	The Left is no longer more generous than right wing parties; higher EPL lowers incentive to provide insurance to insiders	2000s	Pontusson 1995; Boix 2000; Rueda 2007; Vlandas 2013	EPL, union density
Varieties of Capitalism	The type of capitalism shapes employer preferences, hence welfare state structure	2000s	Hall and Soskice 2001; Hancke et al. 2009, Estevez Abe 2005; Mares 2000; Swensson 1991	Employer coordination, stock market presence, EPL,

Main strands	Key claim	Period of appearance	Key authors	Example of possible variables
Globalisation and financialisation	Constraining market internationalisation & liberalisation limits expansion of welfare state and/or forces its retrenchment	1990s-2000s	Garrett 1998; Boix 2000; Rudra 2002; Rodrik 1998; Strange 1996; Swank and Betz 2003; Swank and Duane 2002	Trade and financial openness
Deindustrialisation and new service economy	Deindustrialisation and emergence of new service economy leads to new risks and destabilises traditional sources of welfare	1990s-2000s	Iversen and Wren 1998; Wren 2013	Share of services in economy, knowledge economy

Table A1.2: variables, sources, and summary statistics

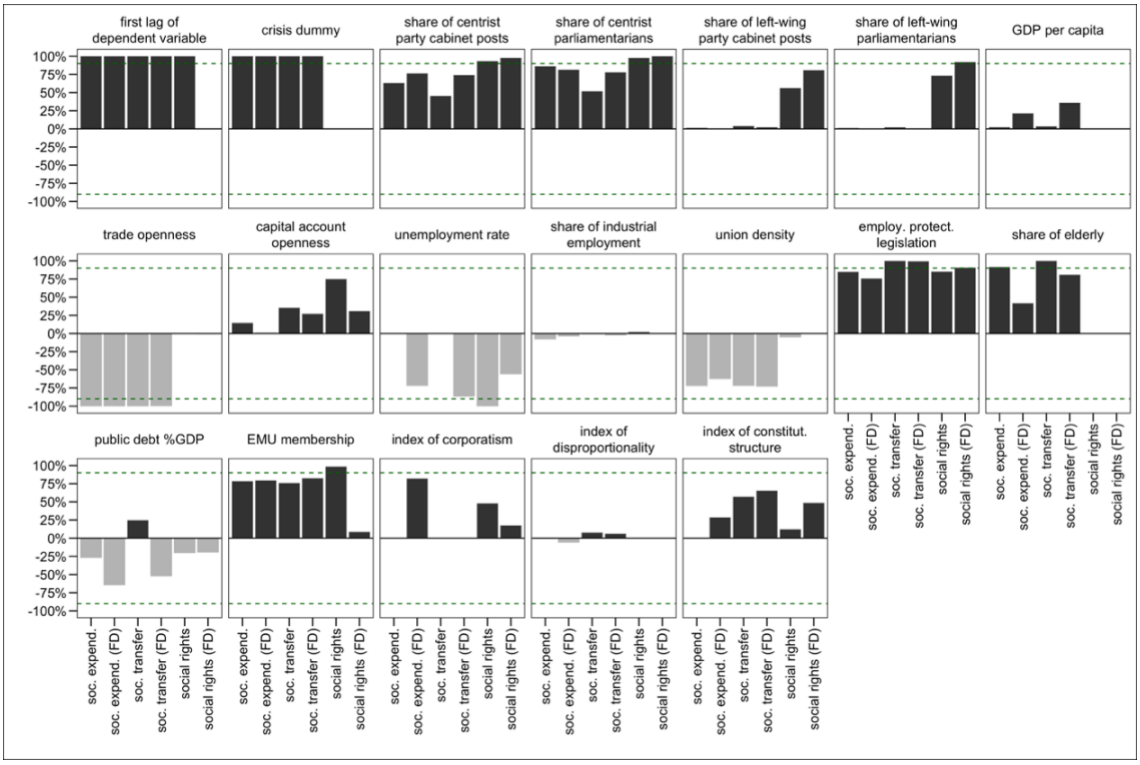
Variable	Description	Source	Mean	St. Dev.	Min	Max
social expenditure % GDP	total public and mandatory private social expenditure as a percentage of GDP	CPDS	20.29	4.89	5.70	34.65
social transfers % GDP	social security transfers as a percentage of GDP. Social assistance grants and welfare benefits paid by general government.	CPDS	12.45	3.85	3.48	23.40
social rights index	index of social rights constructed by Scruggs et al. (2014)	Scruggs	31.19	6.98	10.80	46.60
crisis dummy	binary equal to 1 if GDP growth <0%	CPDS	0.13	0.33	0.00	1.00
share of centrist party cabinet posts	cabinet posts of centre parties in percentage of total cabinet posts.	CPDS	22.01	30.20	0.00	100.00
share of centrist parliamentarians	relative power position of centre parties in government based on their seat share in parliament	CPDS	22.47	31.18	0.00	100.00
share of left-wing party cabinet posts	cabinet posts of social democratic and other left parties in percentage of total cabinet posts.	CPDS	32.63	36.52	0.00	100.00
share of left-wing parliamentarians	relative power position of social democratic and other left parties in government based on their seat share in parliament	CPDS	33.95	38.32	0.00	100.00
GDP per capita	GDP per capita	IMF	18865	17461	479	102913
trade openness	openness of the economy, measured as total trade (sum of import and export) as a percentage of GDP, in current prices.	CPDS	68.83	35.28	8.93	209.08
capital account openness	index for the degree of openness in capital account transactions.	CPDS	0.76	0.30	0.00	1.00
unemployment rate	unemployment rate, percentage of civilian labour force.	CPDS	6.53	4.40	0.00	27.50

share of industrial employment	share of civilian employment in industry of total employment	CPDS	0.31	0.08	0.05	0.67
union density	net union membership as a proportion wage and salary earners in employment	CPDS	39.34	20.16	6.53	99.07
employment protection legislation	employment protection strictness provided through legislation and as a result of enforcement processes (scale of 0-6; higher values indicate stricter employment protection).	CPDS	2.16	0.86	0.26	5.00
share of elderly	share of population aged >65	CPDS	13.61	3.01	5.73	25.06
public debt % GDP	share of public debt as percentage of GDP	CPDS	58.82	34.54	6.29	222.35
EMU membership	member of European Monetary Union	CPDS	0.15	0.36	0.00	1.00
index of corporatism	index of corporatism defined by Jahn (2016)	Jahn	0.01	0.68	-1.15	1.81
index of disproportionality	index of disproportionality according to the formula proposed by Gallagher (1991)	CPDS	6.07	4.77	0.42	24.61
index of constitutional structure	augmented index of constitutional structures based on Huber, Ragin and Stephens (1993)	CPDS	1.66	2.01	0.00	7.00

Note: the sources refer to the following references: (i) CPDS – Armingeon et al. 2015; (ii) Scruggs – Scruggs et al. (2014); (iii) IMF – IMF (2021); (iv) Jahn – Jahn (2016)

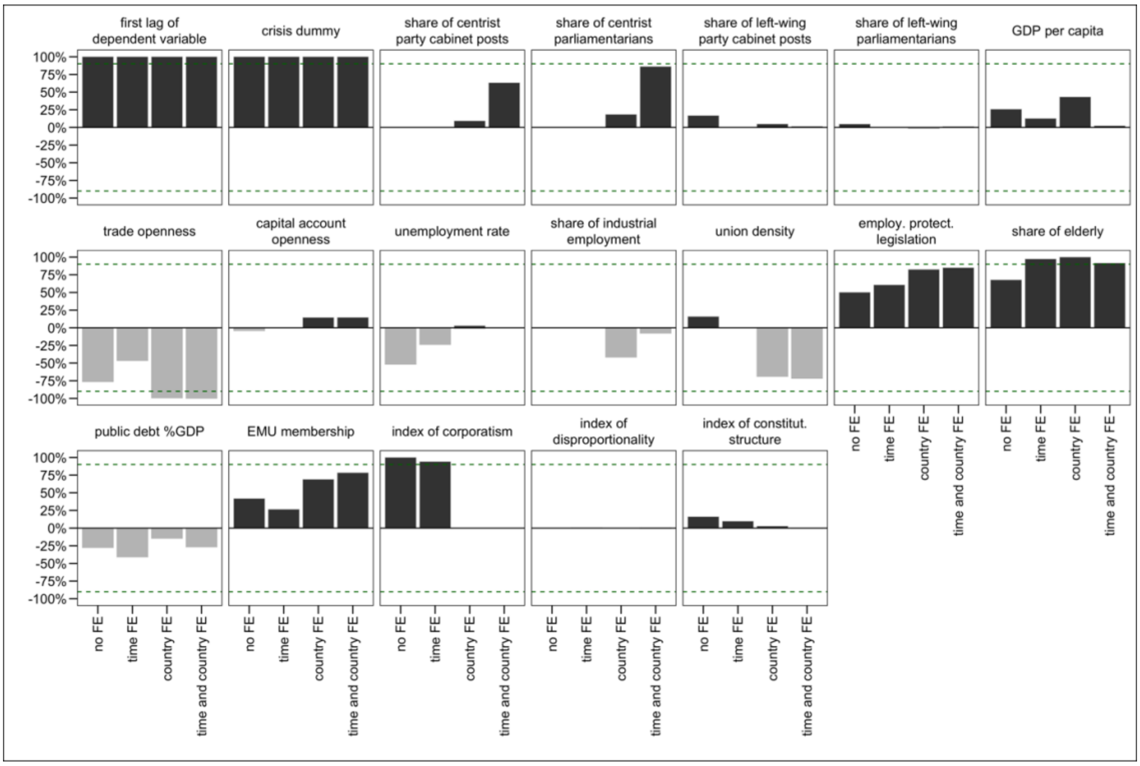
A2: Restricted EBA based on multi-collinearity and Akaike Information Criterion

Figure A2.1: share of significance by dependent variable choice (AIC-restricted)



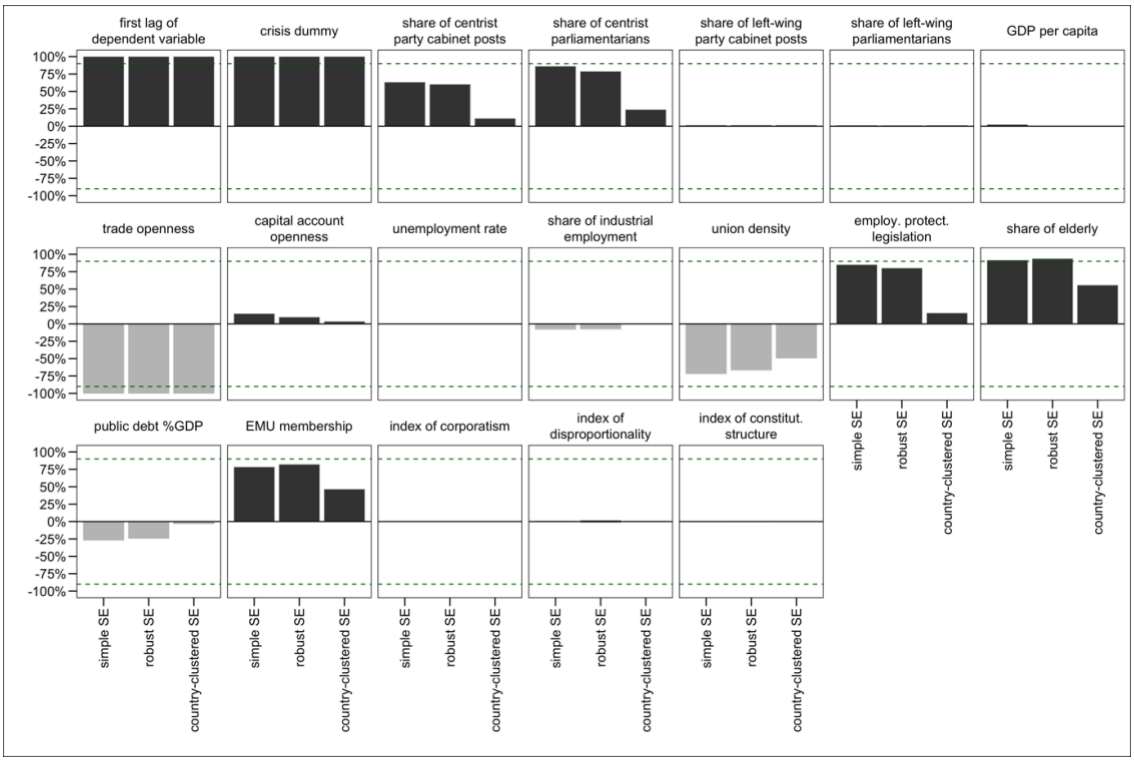
Note: the figure shows the share of positive (dark) and negative (light) significant coefficients for each independent variable by different dependent variable choices (levels and first differences (FD) versions). The model space contains all models with both fixed effects, unadjusted standard errors and all countries and time periods included (from 1980 onwards). The model space has been restricted by removing all models that suffer from multi-collinearity and weak goodness of fit measures based on the Akaike Information criterion. 100% refer to ~262,143 estimates.

Figure A2.2: share of significance by fixed effect structure choice (AIC-restricted)



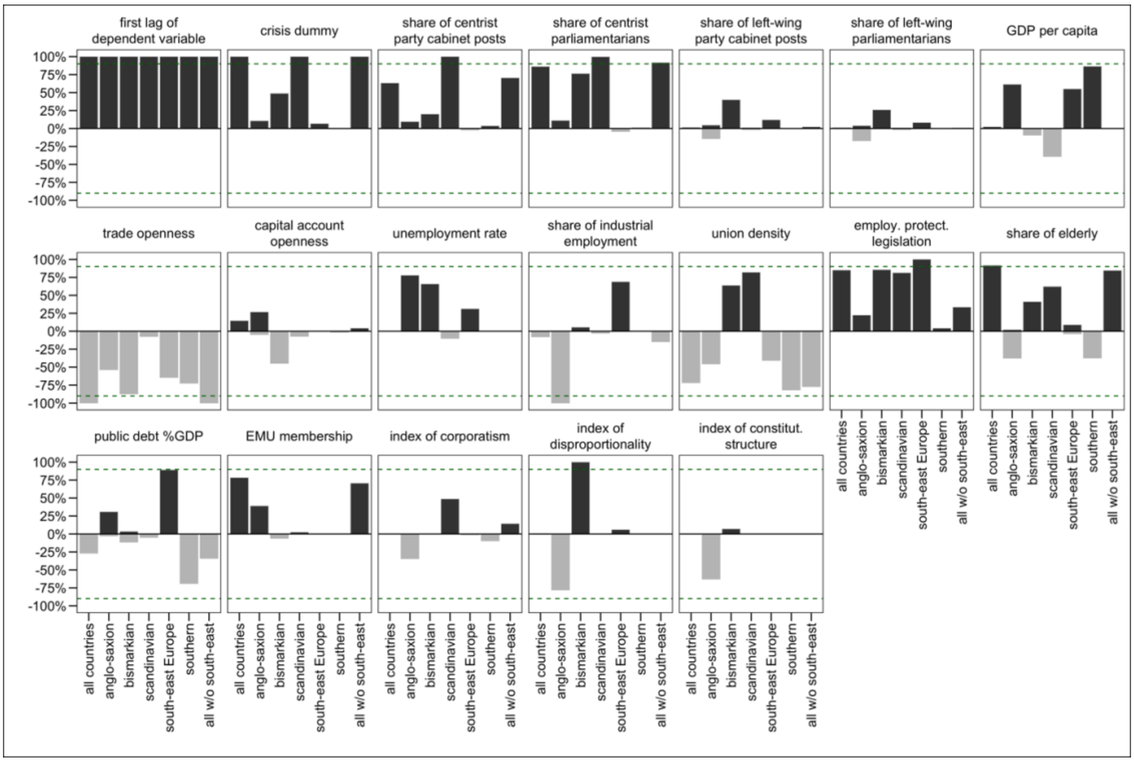
Note: the figure shows the share of positive (dark) and negative (light) significant coefficients for each independent variable by different fixed effect structures. The model space contains all models with social expenditure % GDP as dependent variable, unadjusted standard errors and all countries and time periods included (from 1980 onwards). The model space has been restricted by removing all models that suffer from multi-collinearity and weak goodness of fit measures based on based on the Akaike Information criterion.100% refer to ~262,143 estimates.

Figure A2.3: share of significance by standard error type choice (AIC-restricted)



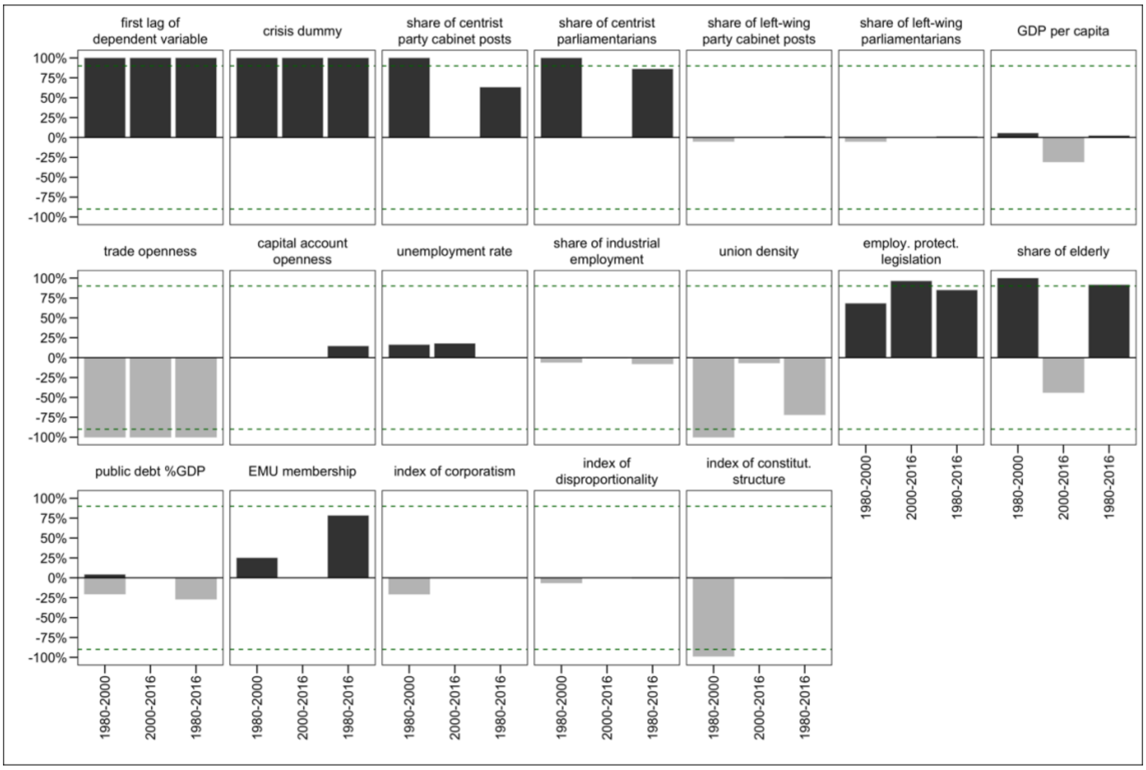
Note: the figure shows the share of positive (dark) and negative (light) significant coefficients for each independent variable by different standard error types. The model space contains all models with social expenditure % GDP as dependent variable, time and country fixed effects and all countries and time periods included (from 1980 onwards). The model space has been restricted by removing all models that suffer from multi-collinearity and weak goodness of fit measures based on based on the Akaike Information criterion. 100% refer to ~262,143 estimates.

Figure A2.4: share of significance by country sample choice (AIC-restricted)



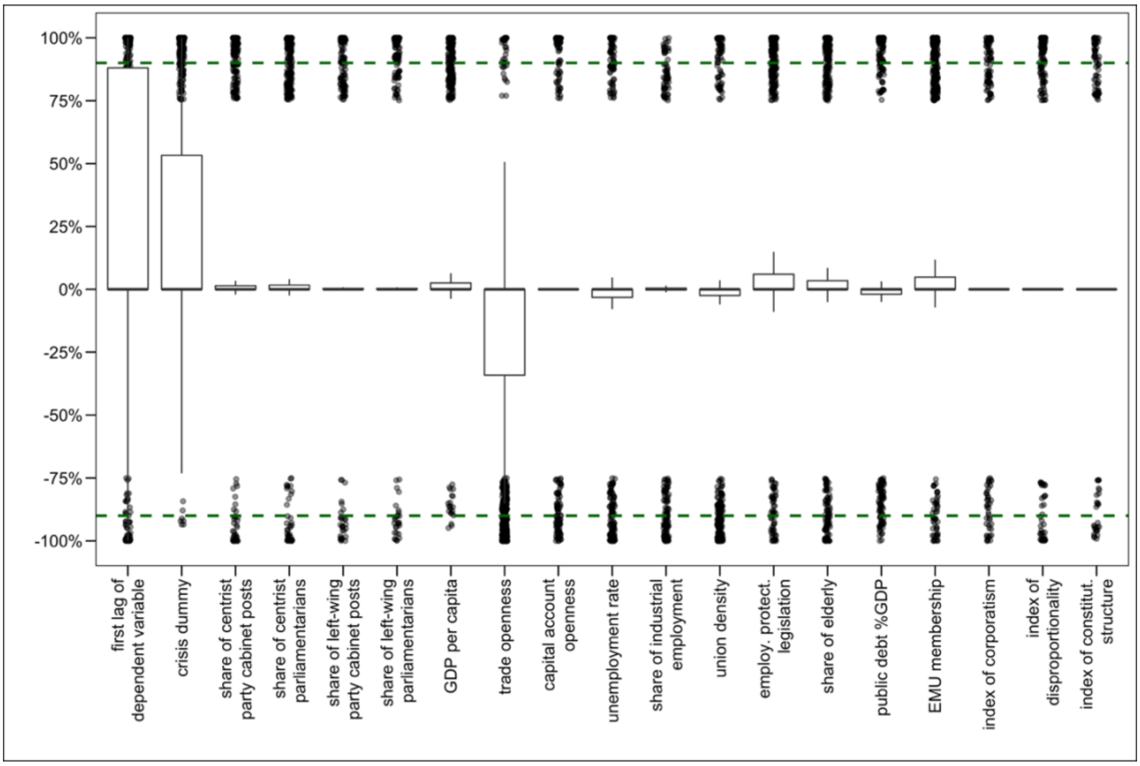
Note: the figure shows the share of positive (dark) and negative (light) significant coefficients for each independent variable by different country samples. The model space contains all models with social expenditure % GDP as dependent variable, time and country fixed effects, unadjusted standard errors and all time periods included (from 1980 onwards). The model space has been restricted by removing all models that suffer from multi-collinearity and weak goodness of fit measures based on based on the Akaike Information criterion. 100% refer to ~262,143 estimates.

Figure A2.5: share of significance by period sample choice (AIC-restricted)



Note: the figure shows the share of positive (dark) and negative (light) significant coefficients for each independent variable by different period samples. The model space contains all models with social expenditure % GDP as dependent variable, time and country fixed effects, unadjusted standard errors and all countries included. The model space has been restricted by removing all models that suffer from multi-collinearity and weak goodness of fit measures based on based on the Akaike Information criterion. 100% refer to ~262,143 estimates.

Figure A2.6: distribution of significance shares by model specification (AIC-restricted)



Note: the figure shows the boxplot (including outliers) of shares of positive and negative significant coefficients for each independent variable across different model specifications (1,512 in total). A model specification is defined as a selection of dependent variable, fixed effect structure, standard error type, country sample and period sample. The model space for one model specification refers to ~262,143 estimates. The model space has been restricted through the removal of models that suffer from multi-collinearity are weak goodness of fit based on the Akaike Information criterion (AIC) (only models with top 10% AIC-score remain in restricted model space). One model specification refers to ~262,143 estimates.

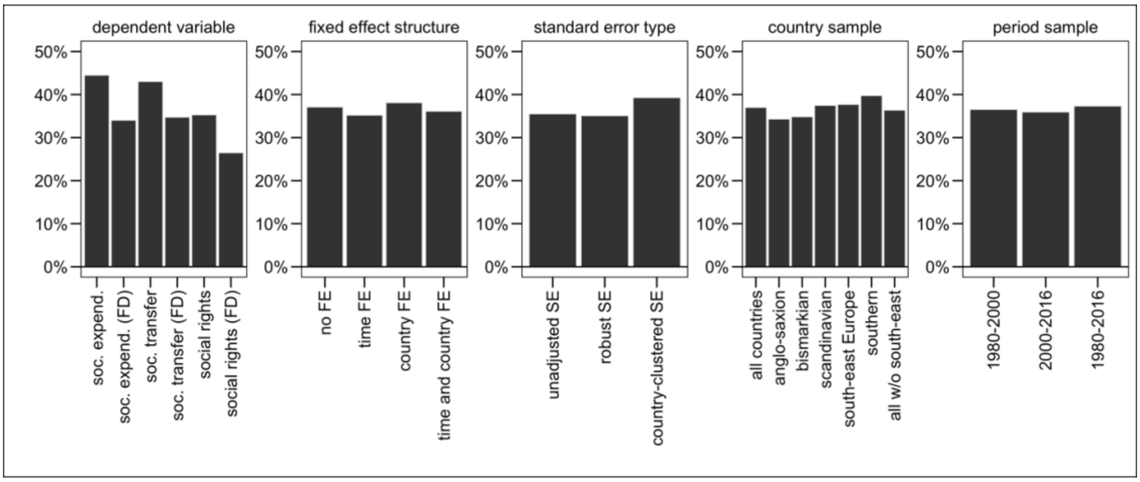
A3: Restricted EBA based on multi-collinearity and R-Squared

Table A3.1: number and share of models by significance (R2-restricted)

Variable	% sign.	# estimates	# sign. negative	% sign. negative	# sign. positive	% sign. positive
first lag of dependent variable	66.7%	37618269	3232006	8.6%	21846075	58.1%
crisis dummy	56.9%	37647708	560641	1.5%	20876340	55.5%
trade openness	54.8%	19004466	9479137	49.9%	932331	4.9%
employ. protect. legislation	38.2%	9099495	615435	6.8%	2862355	31.5%
share of elderly	36.9%	22921968	2818190	12.3%	5631151	24.6%
EMU membership	34.1%	24024339	1905143	7.9%	6275931	26.1%
unemployment rate	33.0%	26730768	6300996	23.6%	2516052	9.4%
union density	32.3%	13650090	3093676	22.7%	1316453	9.6%
public debt %GDP	31.9%	19737252	4441515	22.5%	1854201	9.4%
share of centrist parliamentarians	28.5%	15240945	1362511	8.9%	2983120	19.6%
GDP per capita	28.4%	17023503	1225761	7.2%	3605773	21.2%
share of centrist party cabinet posts	25.8%	15240987	1479358	9.7%	2457964	16.1%
capital account openness	25.4%	22019706	3264090	14.8%	2335047	10.6%
share of left-wing party cabinet posts	24.8%	16946613	1056728	6.2%	3153110	18.6%
share of left-wing parliamentarians	24.7%	16946619	1001616	5.9%	3176024	18.7%
index of constitut. structure	24.3%	11514510	740937	6.4%	2060551	17.9%
share of industrial employment	23.5%	18400920	2344943	12.7%	1981758	10.8%
index of disproportionality	21.0%	21211866	2283495	10.8%	2165027	10.2%
index of corporatism	19.1%	10276530	519750	5.1%	1440187	14.0%

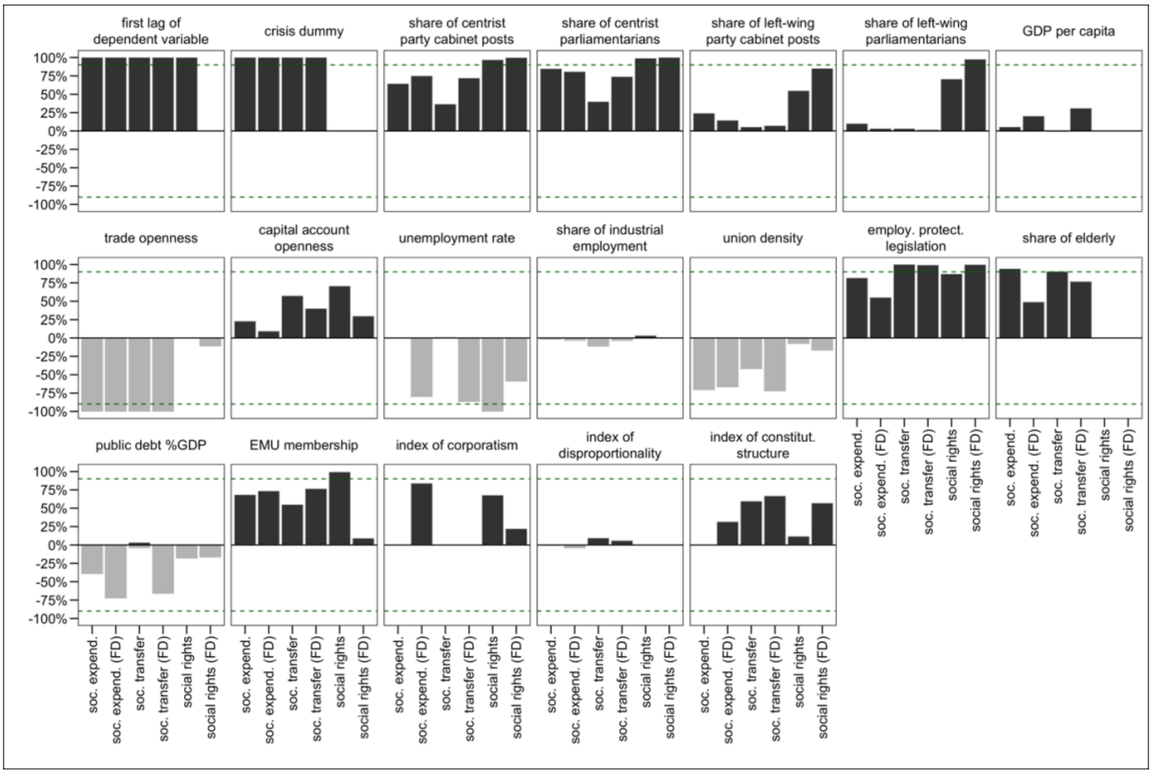
Note: the total number of estimates may differ slightly from the full model universe because certain models could not be estimated due to singularity, missing data, or convergence issues.

Figure A3.2: share of significance by model specification choices (R2-restricted)



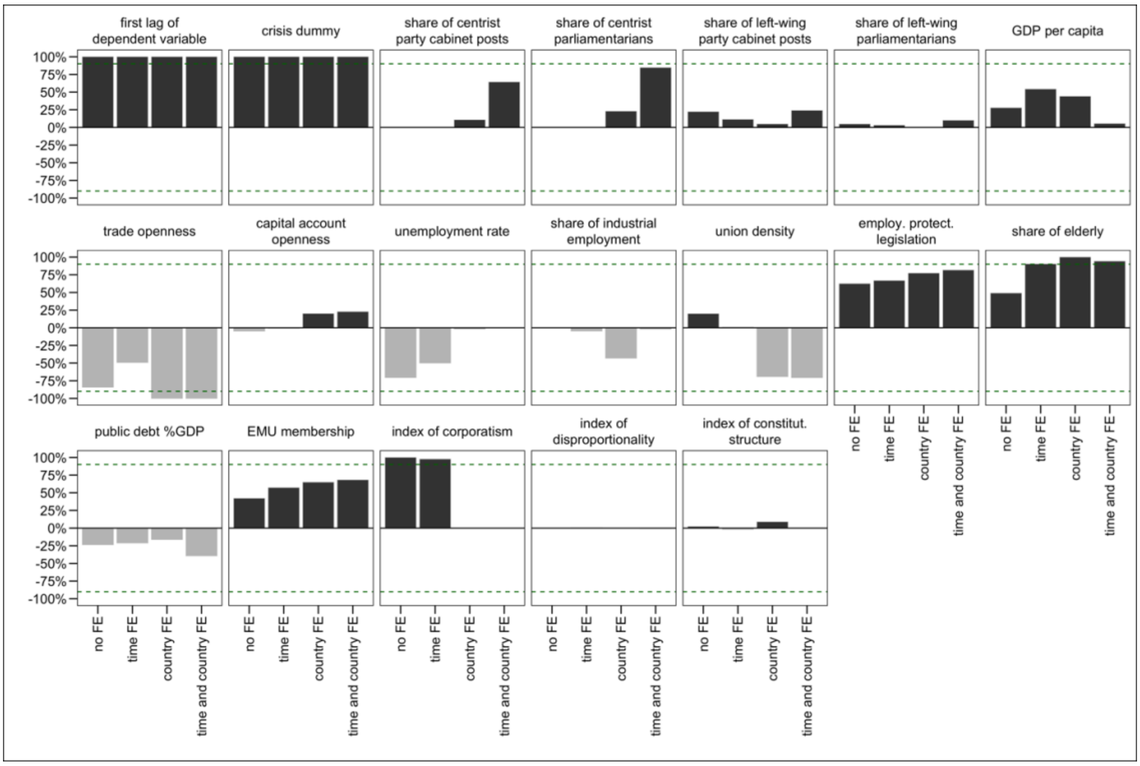
Note: the figure plots the share of (positive and negative) significant coefficients in the whole model universe for all independent variables under consideration across different model specification choices. The model space has been restricted by removing all models that suffer from multi-collinearity and weak goodness of fit measures based on R2.

Figure A3.3: share of significance by dependent variable choice (R2-restricted)



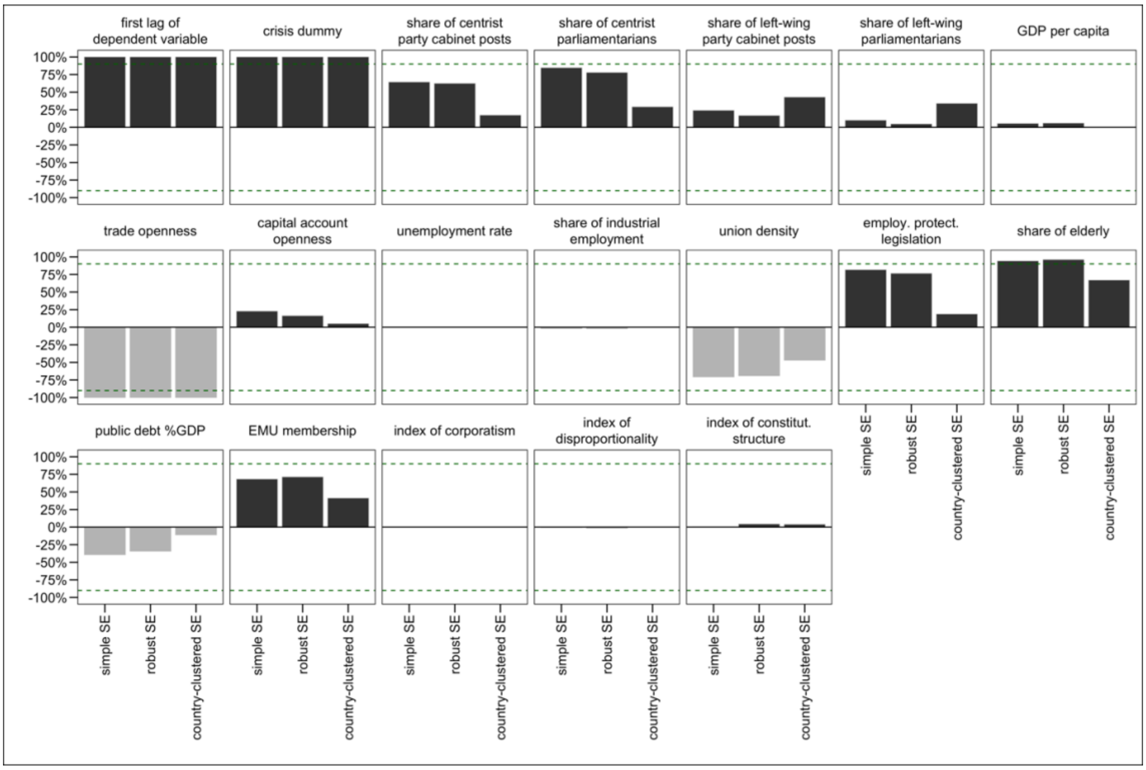
Note: the figure shows the share of positive (dark) and negative (light) significant coefficients for each independent variable by different dependent variable choices (levels and first differences (FD) versions). The model space contains all models with both fixed effects, unadjusted standard errors and all countries and time periods included (from 1980 onwards). The model space has been restricted by removing all models that suffer from multi-collinearity and weak goodness of fit measures based on R2. 100% refer to ~262,143 estimates.

Figure A3.4: share of significance by fixed effect structure choice (R2-restricted)



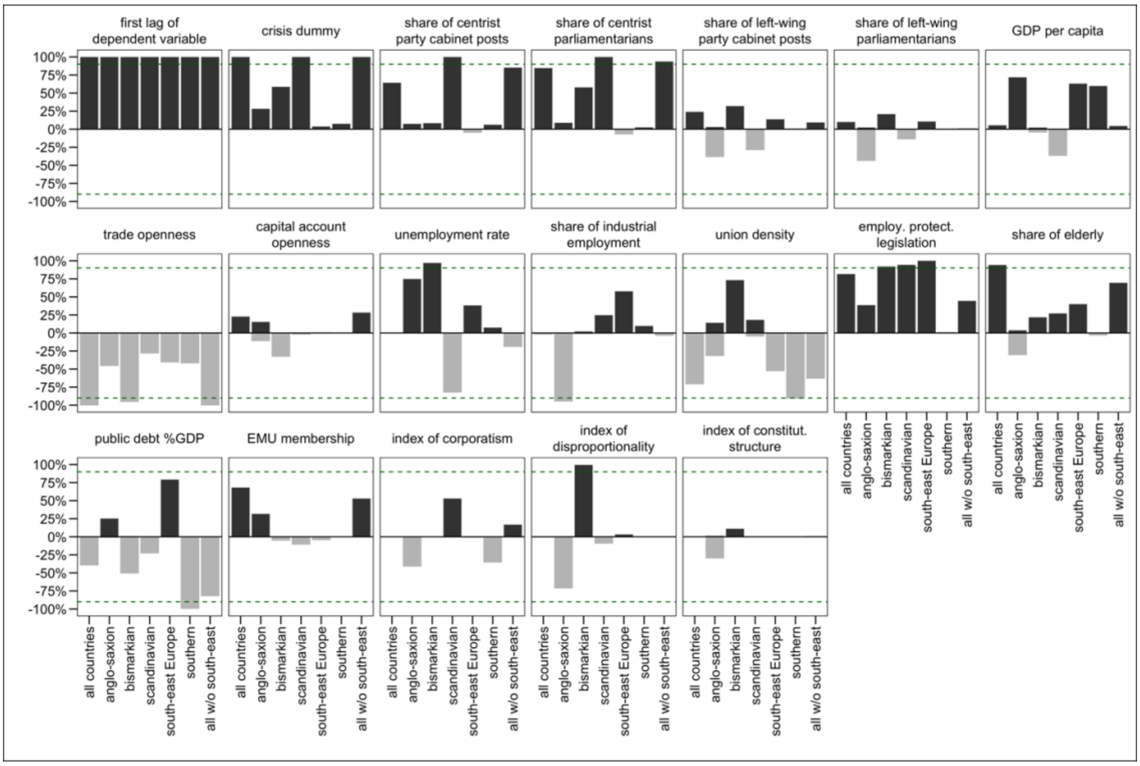
Note: the figure shows the share of positive (dark) and negative (light) significant coefficients for each independent variable by different fixed effect structures. The model space contains all models with social expenditure % GDP as dependent variable, unadjusted standard errors and all countries and time periods included (from 1980 onwards). The model space has been restricted by removing all models that suffer from multi-collinearity and weak goodness of fit measures based on R2.100% refer to ~262,143 estimates.

Figure A3.5: share of significance by standard error type choice (R2-restricted)



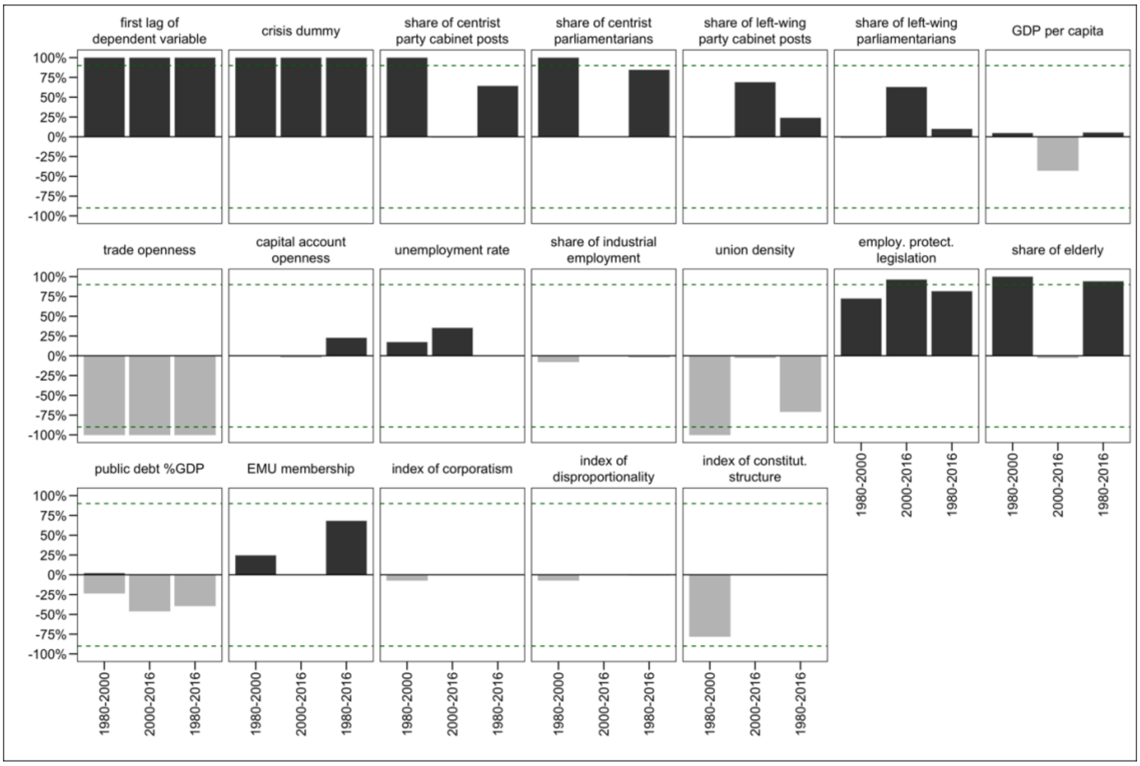
Note: the figure shows the share of positive (dark) and negative (light) significant coefficients for each independent variable by different standard error types. The model space contains all models with social expenditure % GDP as dependent variable, time and country fixed effects and all countries and time periods included (from 1980 onwards). The model space has been restricted by removing all models that suffer from multi-collinearity and weak goodness of fit measures based on based on R2. 100% refer to ~262,143 estimates.

Figure A3.6: share of significance by country sample choice (R2-restricted)



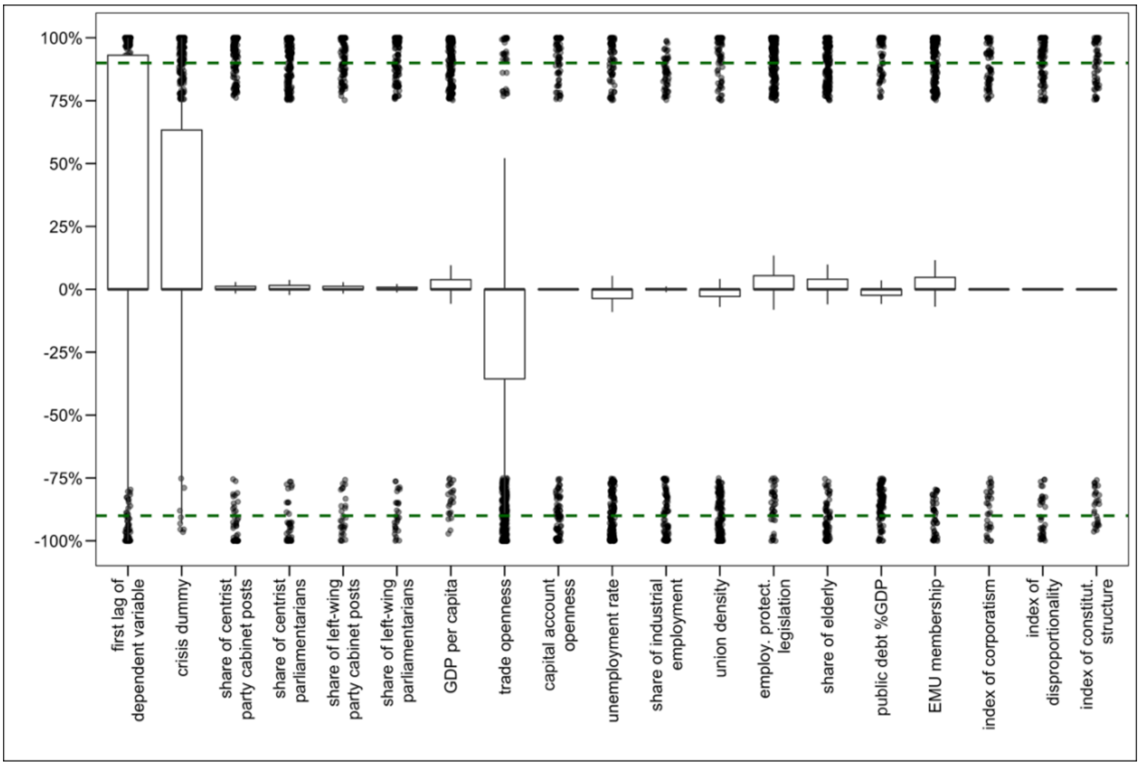
Note: the figure shows the share of positive (dark) and negative (light) significant coefficients for each independent variable by different country samples. The model space contains all models with social expenditure % GDP as dependent variable, time and country fixed effects, unadjusted standard errors and all time periods included (from 1980 onwards). The model space has been restricted by removing all models that suffer from multi-collinearity and weak goodness of fit measures based on based on R2. 100% refer to ~262,143 estimates.

Figure A3.7: share of significance by period sample choice (R2-restricted)



Note: the figure shows the share of positive (dark) and negative (light) significant coefficients for each independent variable by different period samples. The model space contains all models with social expenditure % GDP as dependent variable, time and country fixed effects, unadjusted standard errors and all countries included. The model space has been restricted by removing all models that suffer from multi-collinearity and weak goodness of fit measures based on based on R2. 100% refer to ~262,143 estimates.

Figure A3.8: distribution of significance shares by model specification (R2-restricted)



Note: the figure shows the boxplot (including outliers) of shares of positive and negative significant coefficients for each independent variable across different model specifications (1,512 in total). A model specification is defined as a selection of dependent variable, fixed effect structure, standard error type, country sample and period sample. The model space for one model specification refers to ~262,143 estimates. The model space has been restricted through the removal of models that suffer from multi-collinearity are weak goodness of fit based on R2 (only models with top 10% R2-score remain in restricted model space). One model specification refers to ~262,143 estimates.

A4: R Package

To facilitate the work of empirical scholars conducting empirical analysis, we develop an R library that provides an off-the-shelf solution to identify the sources of model uncertainty and estimate sensitivity. The package is based on the code we have used for the analysis of the present paper. The library will be available on CRAN and on our Github page. In general, beyond several visualization and helper functions, two functions will be at the core of the library. The first function creates an object that contains the A-EBA model universe along with relevant meta data. Here, we focus on the key model specification assumptions that have been discussed in this present paper, namely dependent variable choice, fixed effect structure, standard error type, and sample selection. We plan to expand the list of potential model specifications in the future and based on users' requests posted on Github. In addition, since the number of potential models increases exponentially with the number of model specifications and control variables, we allow the user to parallelize the estimation of the A-EBA model universe across multiple cores of the local computing environment.

The second function takes the result object of the A-EBA function as an input to conduct the (grid-search-based) deep learning approach to predict the significance classification based on the model specification. To execute this function, users are required to install Keras and Tensorflow beforehand. The function will enable users to define hyperparameters, e.g. type of loss function, number of layers, number of units, type of activation function, etc.. In addition to the grid search, the function will also implement the estimation of feature importance scores for each model specification.

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Are Campaign Promises Effective?

Michael Ganslmeier

Abstract

In democracies, political parties make campaign promises about future social benefit expansions to attract voters. However, we know relatively little whether such promises effectively translate into higher political support among benefiting recipients and to what extent prospective pocketbook motives determine alignment with electoral platforms. Using a regression-discontinuity design, we estimate the causal effects of a campaign promise made by the German conservative party to expand pension benefits ahead of the parliamentary election in 2013. The results show that the promise increased alignment with the pledge-making party by 12.2% among eligible beneficiaries, with particularly large effects among low-income individuals. The policy-induced alignment gain is, however, only transitory as it disappears once the pledge is fulfilled. In addition, the results show that promising additional social benefits can successfully swing voters who traditionally align with left-wing political parties, but they do not mobilize non-voters. Thus, campaign promises on benefit expansions are rather persuasive than mobilizing.

Introduction

Campaign promises are knowingly part and parcel of the electoral process. In democratic systems, parties use electoral pledges to signal their policy position to voters which enables them to minimize the distance between their personal policy preferences and the expected policy outcomes (Downs 1957). In this way, electoral pledges constitute an important instrument that translate the preferences of the electorate into legislative outcomes and, thereby, ensure that voters' interests are adequately represented in the aftermath of elections. Previous scholarly work has devoted substantive attention to the question whether elected officials fulfil their campaign promises after taking office. Although policymakers may face commitment issues and deviate from their pre-election promises, previous empirical results have shown that incumbent parties largely delivery on their electoral pledges (Thomson et al. 2017).

Surprisingly, even though we have a good understanding of pledge fulfilment after elections, we know relatively little whether voters care about electoral pledges in the first place. This is particularly questionable since previous studies have highlighted the role retrospective considerations for individual voting behaviour. Most of these studies have identified relatively large effects of pre-election policies on vote choice (Manacorda et al. 2011; Labonne 2013; De La O 2013, amongst others). In contrast, empirical evidence on the prevalence of prospective forms of economic voting – such as campaign promises – is scarce because it comes with inherent endogeneity-related issues that are challenging to address. On the one hand, since single policy positions are strongly correlated within party programs, it is difficult to isolate the effect of one electoral pledge from another one made by the same party. On the other hand, since the preferences of voters towards a specific policy proposal is (usually) unobserved, it is not clear a priori whether a certain campaign promise attracts or disunites voters from the pledge-making party.

The present paper aims to overcome these endogeneity-related challenges by estimating the causal alignment effects of a campaign promise, made by the conservative party, on a pension benefit expanding reform in Germany in the run-up to the federal election in 2013. Since the policy proposal of the reform, called the *Mütterrente*, benefited a certain group of voters conditional on a somewhat arbitrary eligibility criterion, we can infer voters' policy preferences based on eligibility status and adopt a regression-discontinuity design (RDD) to derive a causal effect of this single campaign promise on alignment among benefiting individuals[14]. In detail, the proposal of the *Mütterrente* entailed that it expands pension benefits only for individuals who have parented a child that was born prior to 01 January 1992 (Bach et al. 2014). Since this is the only eligibility criterion, the policy enables us to use children's date of birth in a discontinuity-based design to isolate the effect of the promise on the alignment of mothers from other potential confounding factors. Importantly, the criterion was very salient and visible during the campaign period which enabled voters to track and understand their eligibility status. We provide evidence that both identifying assumptions of discontinuity-based research designs hold (see Lee and Lemieux 2010; Eggers et al. 2015; Skovron and Titiunik 2015). The most crucial limitation usually confronted when implementing an RDD is the risk of people "sorting" themselves into the treatment group. The use of a child's date of birth – which was

[14] The policy reform increased the pension entitlements of 9.53 million parents (Keck et al. 2015), amounting to 21.5% of the voting population.

determined twenty years before the reform has even been proposed – eliminates this sorting risk.

In summary, this paper delivers three key findings about the political returns of campaign promises and the prevalence of prospective pocketbook motives. First, the electoral pledge of the *Mütterrente* increased alignment with the promise-making party by 12.2% among eligible recipients. This effect is both substantive and highly statistically significant. In addition to several robustness checks that use different statistical characteristics (bandwidth size, polynomial terms, structural form, non-parametric estimation strategy), three types of placebo tests (using placebo cut-off dates, placebo treatment groups and placebo survey periods) provide further validity of the research design.

Second, while the baseline estimate shows that promises are conducive to increasing party alignment among eligible recipients, the effect disappears briefly after reform implementation. In other words, the results suggest that even unbroken promises on very generous welfare state policy expansions does not yield long-term political gains for the pledge-making party. Thus, the transitory nature of benefit-based policy feedbacks highlights that policymakers cannot secure political support via one-off benefit expansiona, but they are rather required to adopt social benefit expansions on a continuous basis to keep voters' political support.

Third, although we do not find an effect on turnout, our results suggest that the promise of benefit expansions attracted voters which previously aligned with left-wing platforms.

Thus, policy proposals of benefit expansions may play a rather persuasive than mobilizing role, particularly in the context of advanced economies. Beyond voters' ideological predisposition, our analysis reveals further that poorer individuals are substantially more sensitive to the campaign promise compared to individuals with higher income. Echoing previous evidence on the income-gradient in the literature on political behaviour (Rosenstone and Hansen 1993; Campbell 2002; Schafer et al. 2022), this finding suggests that campaign promises of benefit expansions pay particularly large electoral dividends when targeted at voters with lower economic security.

The remainder of the paper is structured as follows. In section II, we review the literature on the political benefits of social expansions. In section III, we discuss how we use the institutional design features of the *Mütterrente* reform to estimate the causal effect of the promises using individual survey data. In section IV and V, we discuss the empirical strategy and the results, respectively. Section VI concludes.

Prospective Pocketbook Voting

The question whether individuals cast their vote based on economic considerations has been subject of political economy for decades (Tufte 1975; Campbell et al. 1980; Lewis-Beck 1986; Powell and Whitten 1993; Lewis-Beck and Stegmaier 2000; see review by Anderson 2007). Two main types of economic voting have been delineated: **sociotropic** and **egocentric** or **pocketbook** voting (see Kramer 1983). Sociotropic voting describes individuals who cast their ballot based on the state of the economy as a whole. By contrast, pocketbook voting argues that voters make their electoral decisions based on their individual expected utility, i.e., the social benefits they receive or expect to receive under a specific government. The survey-based literature (Fiorina 1981; Kinder and Kiewiet 1979; Kiewiet et al. 2011) tends to argue that sociotrophic motives are more important than pocketbook ones in determining vote choices (see Healy et al. 2017).

Beyond this macro-/micro differentiation, the literature also distinguishes between retrospective and prospective forms of economic voting (Barro 1973; Ferejohn 1986; Reed 1998; Downs 1957; Lindbeck and Weibull 1987) – see Lewis-Beck (1990), Persson and Tabellini (2002) and Born et al. (2018) for reviews. Retrospective voting assumes that voters make their decisions based on past policies, while prospective voting assumes that voters are forward-looking and vote for the policy bundle that maximize their (expected) future payoffs. Hence, studies built on the retrospective assumption investigate the effects of past policies on voting behaviour, while the prospective-based approach aims to understand the impact of policy promises. In essence, this temporal dimension measures whether voters use either (i) past policies or (ii) promises on future policies as signals for the future performance of candidates and parties.

Based on these two main dimensions of economic voting, voters' electoral decision can be stated as a weighted combination of "macro-centred-ness" (sociotropic vs. egotropic) and "past-centred-ness" (prospective vs. retrospective). We model this voting decision $\boldsymbol{V}$ of voter $\boldsymbol{i} \boldsymbol{\epsilon} \boldsymbol{I}$ as function of $\boldsymbol{i}$'s utility that she receives from sociotropic macro-level output $\boldsymbol{Y_T}$ (i.e. economic performance) and egotropic micro-level benefit $\boldsymbol{B_T}$ (i.e. individual social benefits) in time $\boldsymbol{T}$**.** We depart from a model with two time periods, $\boldsymbol{T} =$

$\{\mathbf{0},\mathbf{1}\}$, which refer to the pre-election period, $\boldsymbol{t}$, and post-election period, $\boldsymbol{t}+\mathbf{1}$, respectively. The utility function of $\boldsymbol{i}$ can then be stated as

$$\boldsymbol{U}(\boldsymbol{Y},\boldsymbol{B}|\boldsymbol{\alpha},\boldsymbol{\beta},\boldsymbol{K}) = \boldsymbol{\beta}\boldsymbol{\alpha}\boldsymbol{Y}_{\boldsymbol{t}} + (\mathbf{1}-\boldsymbol{\beta})\boldsymbol{\alpha}\frac{\boldsymbol{B}_{\boldsymbol{t}}}{\boldsymbol{K}} + \boldsymbol{\beta}(\mathbf{1}-\boldsymbol{\alpha})\boldsymbol{Y}_{\boldsymbol{t}+\mathbf{1}} + (\mathbf{1}-\boldsymbol{\beta})(\mathbf{1}-\boldsymbol{\alpha})\frac{\boldsymbol{B}_{\boldsymbol{t}+\mathbf{1}}}{\boldsymbol{K}}$$

with $\mathbf{0} \leq \boldsymbol{\alpha} \leq \mathbf{1}$ representing the degree for “macro-centred-ness” (weight on macro-level output); $\mathbf{0} \leq \boldsymbol{\beta} \leq \mathbf{1}$ representing the degree of “past-centred-ness” (weight on past utility level gained from macro-level output and micro-level benefit); and $\boldsymbol{K}$ as a time-invariant constant for economic endowments of $\boldsymbol{i}$ (i.e. wealth, income). We discount $\boldsymbol{B}$ by $\boldsymbol{K}$ to adjust the individual benefits a voter receives by her economic endowments she already possesses (i.e. wealth) assuming that the marginal utility gain of $\boldsymbol{B}$ is lower for richer agents.

This model is based on several assumptions: (i) $\boldsymbol{\alpha}$ and $\boldsymbol{\beta}$ are exogenous and time-invariant; (ii) the budget is balanced within $\boldsymbol{T}$ (thus, intertemporal budget allocation does not exist); and (iii) $\mathbf{Cov}(\boldsymbol{K},\boldsymbol{B}_{\boldsymbol{T}}) = \mathbf{0}$ and thus, the entitlement to social benefits does not change with economic endowments. Given that $\boldsymbol{\alpha},\boldsymbol{\beta}$ and $\boldsymbol{K}$ are pre-determined and electoral platforms propose a bundle of $\boldsymbol{c} = \binom{\boldsymbol{Y}}{\boldsymbol{B}}$, the vote choice $\boldsymbol{V}_{\boldsymbol{i}}(\boldsymbol{c}^{*})$ is then specified at $\boldsymbol{c}^{*}$ at which $\mathbf{max}\,\boldsymbol{U} = \boldsymbol{U}(\boldsymbol{c}^{*}|\boldsymbol{\alpha},\boldsymbol{\beta},\boldsymbol{K})$. Since $\boldsymbol{\alpha}$ and $\boldsymbol{\beta}$ are exogenous, we can use these two parameters to map the four types of economic voters. Table 1 displays their characteristics with respect to both dimensions of economic voting. While each of these four types may be present within the voting population and each rational drives $\boldsymbol{V}_{\boldsymbol{i}}$ as long as $\boldsymbol{\alpha} > \mathbf{0}$ and $\boldsymbol{\beta} > \mathbf{0}$ for a given voter $\boldsymbol{i}$, the present paper focuses, in contrast to previous studies investigating the other three types of economic voting (see literature review below), on the prospective pocketbook voter type (upper-left quadrant in Table 1), with low $\boldsymbol{\alpha}$ and $\boldsymbol{\beta}$. The theoretical framework above allows us to derive three broader implications about their behaviour.

Table 1: Parameters of different types of economic voters

Degree of "past-centred-ness" (β)	Degree of "macro-centred-ness" (α): pocketbook	Degree of "macro-centred-ness" (α): sociotropic
prospective	$\boldsymbol{\alpha \leq 0.5}$ ($\boldsymbol{low\ \alpha}$) $\boldsymbol{\beta \leq 0.5}$ ($\boldsymbol{low\ \beta}$)	$\boldsymbol{\alpha > 0.5}$ ($\boldsymbol{high\ \alpha}$) $\boldsymbol{\beta \leq 0.5}$ ($\boldsymbol{low\ \beta}$)
retrospective	$\boldsymbol{\alpha \leq 0.5}$ ($\boldsymbol{low\ \alpha}$) $\boldsymbol{\beta > 0.5}$ ($\boldsymbol{high\ \beta}$)	$\boldsymbol{\alpha > 0.5}$ ($\boldsymbol{high\ \alpha}$) $\boldsymbol{\beta > 0.5}$ ($\boldsymbol{high\ \beta}$)

First, prospective pocketbook voters with low $\boldsymbol{\alpha}$ are less sensitive to the macroeconomic environment $\boldsymbol{Y_t}$ under which an election takes place. At the same time, they are also less responsive to the expected macroeconomic output $\boldsymbol{Y_{t+1}}$. This is because in the extreme case of $\boldsymbol{\alpha = 0}$, $\boldsymbol{Y_t}$ and $\boldsymbol{Y_{t+1}}$ cancel out of (1). Thus, we expect individuals with pocketbook motives to show less behavioural changes neither to past nor future/expected aggregated outputs.

Second, prospective pocketbook voters with low $\boldsymbol{\beta}$ are less sensitive to past individual benefits, $\boldsymbol{B_t}$, because in the extreme case of $\boldsymbol{\beta} = \mathbf{0}$, then $\boldsymbol{B_t}$ cancels out. This implies that these voters have lower levels of gratitude/loyalty due to past policies. We can see this if we augment the two-period framework to a repeated-game model[15] with $\boldsymbol{T} = \{\mathbf{0}, \mathbf{1}, \mathbf{2}, \ldots\}$. If we assume that the party $\boldsymbol{c}$ won the election in $\boldsymbol{t}$ and adopted $\boldsymbol{B_{t+1}}$, then the expected utility of $\boldsymbol{i}$ in $\boldsymbol{t} = \mathbf{2}$ does not depend on $\boldsymbol{B_{t+1}}$ for prospective pocketbook voters. This means that the pre-election proposal of $\boldsymbol{B_{t+1}}$ can only affect the behaviour of prospective pocketbook voters for the upcoming election ($\boldsymbol{t}$), while it cannot bind them for subsequent elections ($\boldsymbol{t} > \mathbf{0}$), even after pledge-fulfilment. Thus, one can expect that the promise effect of single benefit proposals disappears after an election. Thus, we expect that, for instance, behavioural changes induced by changes in social benefits are only transitory and they revert to their status-quo (behaviour in $\boldsymbol{t} = \mathbf{0}$) after $\boldsymbol{B_{t+1}}$ has been implemented.

Third, echoing evidence on the income-gradient in the political behaviour literature (Rosenstone and Hansen 1993; Campbell 2002; Schafer et al. 2022), the impact of individual benefit $\boldsymbol{B}$ is stronger for voter with lower $\boldsymbol{K}$. In other words, the behaviour of pocketbook voters is driven by the voters' marginal benefit they receive from $\boldsymbol{B}$ rather than its absolute one which implies that individuals with less economic endowments (low $\boldsymbol{K}$) are more responsive to the size of proposal $\boldsymbol{B}$ than agents with better economic positions. Again, we assume that $\mathbf{Cov}(\boldsymbol{K}, \boldsymbol{B_T}) = \mathbf{0}$. In this paper, we test the second and third theoretical implications of our model empirically. Before doing so, we provide an overview about the existing empirical literature on economic voting in the following.

[15] without credibility or commitment problem

Literature Review

Most empirical exercises depart from a retrospective framework by quantifying the impact of pre-election policies – such as benefit programs – on political dynamics such as vote choice, turnout, and party alignment (lower-right quadrant in Table 1) using a retrospective egotropic framework. A large literature has focused on the effect of conditional cash transfer programs on political behaviour of affected individuals in developing and emerging market economies. Most studies find relatively large and statistically significant effects. For example, Manacorda et al. (2011) employ a discontinuity-based approach to show how an anti-poverty cash transfer program in Uruguay increases the likelihood of favouring the current government – compared to the previous one – by 11-13% among beneficiaries. Labonne (2013) finds an even larger effect by conducting a randomized roll-out of a conditional cash transfer program in Philippian villages: his estimates indicate that recipient villages report 26% higher support for the incumbency compared to untreated units. The high effectiveness of such electoral strategies has also been documented in other developing and emerging economies: De La O (2013) and Cantú (2019) in Mexico, Hidalgo and Nichter (2015) in Brazil, Baez et al. (2012) and Gallego (2018) in Colombia, Khemani (2015) in the Philippines, and Pop-Eleches and Pop-Elches (2012) in Romania.

In contrast to retrospective voting, the evidence of a prospective relationship to voting decisions is more limited. One key methodological challenge concerns endogeneity which makes it difficult to quantify the electoral returns to campaign promises (Ferland and Dassonneville 2021). Several issues are especially difficult to address. First, in contrast to actual implemented programs, campaign promises are often highly ambiguous which make them difficult to precisely measure which is, however, a requisite for acute treatment assignment (*measurement error*). Second, the political alignment of individual voters is often based on their position in a multi-dimensional space of policy issues. Thus, to single-out the effect of a single policy proposal requires substantive conditioning along all other issue dimensions which are not always observable (*omitted variable bias*). Third, competing parties often support similar policy proposals. However, if multiple parties make the same promise to certain voters, eligible individuals receive double treatment which puts a downward bias on empirical estimates of the treatment effect.

The two studies that have come closest to drawing a causal relationship between campaign promises and political alignment are from the Philippines and Sweden. Cruz et al. (2019) conducted a large-scale field experiment in the Philippines and show that randomly selected voters who were informed about current campaign promises are more likely to favour the party that is closer to their own preferences than un-informed voters who did not receive the informational treatment. However, the treatment of the study consists of numerous electoral pledges of candidates (and not just the ones that directly benefits a given voter), Therefore, this study measures the effect of providing information about the intended policy package a platform offers to their electorate rather than testing how prevalent prospective pocketbook considerations at the individual-level are.

Elinder et al. (2015) investigated the effect of family policy promises on support for the social democratic incumbent in Sweden during the 1990s. The authors find that benefiting/ families are significantly more responsive to the promise compared to ineligible groups. This effect disappears after the policy is implemented. However, the eligibility criteria for benefit receipt, associated with age of children in 5-year brackets (ages 0-4 and 6-11), is likely to create an unbalanced treatment and control group which may bias the findings. Beyond its identification strategy and double treatment (due to multiple reform proposals made by multiple parties), the paper does not identify whether these family benefits are persuasive or mobilizing and it does not investigate the effect size varies along the income distribution. The paper aims to address these methodological limitations.

The Mütterrente

In the post-war period, parenting was primarily considered to be a female duty. This cultural norm corresponded to a lower take-up of paid employment among women, which ultimately, amongst others, resulted in limited contributions to the pension system. In the mid-1980s, in a bid to promote the social security of women and enhance their independence from their marital partner, the cabinet under Chancellor Kohl in West Germany adopted a new policy, entitled the "Baby-Jahr" reform. This policy provided an additional benefit to parenting individuals and was executed as a pension entitlement equivalent to one year of typical pension contributions per child (Rentenbescheid 2020). The reform was a first step towards greater recognition of parenting activities in the pension system.

With the introduction of the *Sechstes Sozialgesetzbuch* on 01 January 1992 (Rentenbescheid 2020), the cabinet under Chancellor Kohl increased the level of parenting-based allowance from one to three years per child, which was a sizable expansion of the initial “Baby-Jahr” reform. However, the new legislation only expanded pension benefits for parents of children born after 01 January 1992; parents with children born on 31 December 1991 and before – a *seemingly arbitrary* cut-off date – did not benefit from the new policy. This created a sharp inequity: parents below this threshold received only one year of pension contributions per child, while parents above this threshold received three years of pension contributions.

Even though the distributive consequences of this policy were well known as early as its proposal in 1992, it took until the end of 2011 that the issue became part of the political agenda. During a time of both rising demand for policies promoting gender equality and on the back of the 2008 Financial Crisis, the conservative party made the issue a lightning rod within public and political discourses. At the forefront of the debate, the female-only working group of the conservative party – called *Frauenunion* – were leading the call to correct the previous inequity. They promoted a new reform – called *Mütterrente* – that sought to close (or at least minimize) the resultant imbalances of the *Sechstes Sozialgesetzbuch*. Their first proposal was published on 14 November 2011 at the annual convention of the CDU Germany in Leipzig which entailed additional pension entitlements associated with each child born prior to 01 January 1992 (Roßmann 2012). The reform proposal was welcomed by the broader conservative party as it does not just enable them to underline their focus on traditionalism related to family values but it also provided benefits to mid-age women in their 40s and 50s which have constituted an electoral group that has supported the conservative party for decades.

In the following two years, the proposal gained more and more traction in the public debate, and it soon became one of the most salient promises of the conservative party during the campaign period before the federal election in 2013 (Bäcker 2018). Crucially, the *Mütterrente* was solely proposed by the conservative party, while other platforms criticized the reform during the election campaign of 2013 (Focus Online 2013). Following a successful election, another cabinet under Chancellor Merkel was formed, and with it the *Mütterrente* reform passed parliament on 23 May 2014 and came into force on 01 July 2014 (Bundestag 2014). Table 2 provides an overview of key events leading up to and immediately after the reform.

The macro- and microeconomic consequences of the reform were substantial. At an aggregated level, the *Mütterrente* constitutes with over 9.53 million beneficiaries the largest and most expensive German pension reform in recent decades (Keck et al. 2015). But also on an individual level, the *Mütterrente* led to a substantive increase in pension benefits of eligible parents. For instance, parenting mothers[16] with one child born before 1992 gained an additional 90,05 Euro per month before taxes, as of July 2022[17]. In comparison, women (with and without children) receive, on average, 1,001 Euro per month after health and nursing insurance contributions, as of July 2021 (Blumenroth 2022). Table 2 provides an overview of the key events of the reform process.

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[16] who lives in West-Germany
[17] See website of Allianz (2022)

Table 2: Timeline of Mütterrente-Reform

Period	Event	Regulation: pension entitlements for parents per child
Jan. 1986	"*Baby-Jahr*"-Reform	**- for every child born:** parenting individuals receive 1 years of pension contributions
Jan. 1992	Introduction of *Sechstes Sozialgesetzbuch*	
Nov. 2011	At the annual convention of the CDU: Frauen-Union proposes new pension reform called *Mütterrente*	**- for every child born after 1992:** parenting individuals receive 3 years of pension contributions
Jan.-Sept. 2013	*Mütterrente* is a key campaign promise of conservative party (CDU/CSU) during campaign period	**- for every child born before 1992:** parenting individuals receive 1 years of pension contributions
Sept. 2013	CDU/CSU under Chancellor Merkel win the election and enter coalition with social democratic party (SPD)	
May 2014	*Mütterrente* reform passes parliament	**- for every child born after 1992:** parenting individuals receive 3 years of pension contributions
Jul. 2014	*Mütterrente* reform comes into force	**- for every child born before 1992:** parenting individuals receive 1 years of pension contributions + additional payment (*Mütterrente*)
until Jul. 2015	All affected parents are informed how the *Mütterrente* affects their pension entitlements via "*Rentenbescheid*" (annual information documents)	

Empirical Strategy

Treatment Assignment and Data

The *Mütterrente* is an appealing case to measure the effect of campaign promises of pension entitlements on political behaviour. On the one hand, the reform was very salient and visible to voters during the campaign/promise period. This is important as the effect of individual preferences on voting behaviour depends strongly on the visibility and informational structure of social policies (Gingrich 2014). On the other hand, the reform's design of using a child's date of birth – which is the only eligibility rule to be *automatically* enrolled into the program – creates a balanced treatment and control group for use in a regression-discontinuity design (RDD). Leveraging eligibility-based discontinuities is commonly used to derive estimates of causal effects and has been a popular form of identification in labour economics where age-based eligibility criteria are frequently employed (Card et al. 2007; Card et al. 2009; Lalive 2007; Mastrobuoni 2009; Le Barbanchon et al. 2019). However, due to issues around data privacy, employing the demographic characteristics of an individual's child(ren) has been a rather rare form of identification strategy, with two noteworthy exceptions. Carneiro et al. (2015) use children's birth dates as a discontinuity to estimate the causal effect of maternal leave benefits on long-run child outcomes. Olafsson and Steingrimsdottir (2020) use children's birth dates as a discontinuity to estimate how paternity leave influences marital stability.

In a similar vein, we use the Socio-Economic Panel (2020) (SOEP-Core, v36) data by the German Institute for Economic Research (DIW Berlin) which records the month and year of a child's birth under the female household member. With over ~30,000 respondents from ~15,000 households, it is one of the largest public opinion survey datasets in

Germany. In contrast to most other public opinion surveys in Germany, the SOEP tracks single individuals over time since 1984 on an annual basis.

We filter the full survey dataset with responses in three ways. First, for the baseline analysis, we use responses from the survey waves of 2012, 2013, and 2014 (01 January 2012 – 31 December 2014). Since voters need to be aware of a policy promise to be influenced by it, we use as starting date the date when the reform proposal became subject of the public debate, primarily after the annual meeting of the conservative party at the end of 2011. For the end date, we include all responses until the of the year when the reform was implemented on 01 July 2014 (Bundestag 2014). In robustness checks and placebo tests, we shift the start and end date to show how the promise effect changes over time. Furthermore, to address attenuation bias, we restrict the baseline analysis to individuals who have responded in at least two out of three survey waves. In this way, we aim to ensure that the sample remains balanced over time and does not change considerably due to non-responses/dropouts.

Second, we restrict the sample to female respondents only. This is due to two reasons. On the one hand, despite the technical eligibility of men, the *Mütterrente* is a *female-targeted* reform (as the name suggests), and it was widely recognized as such across the broader public at the time of its proposal and adoption. Among the 9.53 million individuals eligible for the *Mütterrente* (Keck et al. 2015), only 1.4 percent are male (Stiftung Warentest 2021). On the other hand, the survey dataset does not allow for the identification of fathers because only female respondents are linked to the records of their children. This data limitation would be problematic were men more likely to benefit from the policy, yet it is most likely inconsequential to this analysis due to women being the primary group of interest.

Third, we remove all respondents with children born before 1972 and after 2012. This results in a dataset spanning a 40-year period: 20 years before and after the cut-off date. The number of mothers with children born prior to 1972 is very small per month and year which would make the variance prior to 1992 relatively large. After 2012, the change in party alignment of mothers may have been strongly affected by the introduction of the *Kinderförderungsgesetz* in 2008 which provides parents with the legal right to a place at a day-care centre for 1-3-year-old children from 01 August 2013 onwards (Bundesministerium für Familie, Senioren, Frauen und Jugend 2018). The adoption and realization of the *Kinderförderungsgesetz* law was a key cornerstone in family policymaking during the Merkel era and may have led to significant changes in political support for the conservative party. To mitigate the confounding risk of the 2008 reform, we exclude all mothers who did not give birth to their first child prior to 2013.

Empirical Analysis

The **running variable** of the RDD is based on the respondent's first child's date of birth. Since the SOEP data provides information about the month and year of the child's birth, we use this information and transform it into a numeric variable which measures the number of months prior/after the cut-off date (year-month). Thus, if the first child is born in January 1992, the running variable is equal to 0, while it equates to 11 when the first child is born in December 1992. In contrast, the running variable is equal to -12 when the first child is born in January 1991.

We use the date of birth of the first child for treatment assignment because it ensures the cleanest form of identification by comparing eligible with non-eligible mothers. While it is technically possible to utilize the date of birth of subsequent children, this would disturb treatment assignment as the control group then includes mothers which are eligible for the benefit expansion when their first (or second, etc.) child was born prior to January 1992. And even if we were to do it by restricting the treatment group to mothers with multiple children only, we would introduce imbalances between treatment and control group by construction. Thus, to avoid these empirical pitfalls, we primarily focus on the differences between eligible and non-eligible individuals only. However, in heterogeneity analysis, we test how the effect varies by family size.

As the main **dependent variable**, we construct a measure of party alignment that is based on the multi-answer question “Which party do you feel closest to?”. We use this survey question to create a binary variable that takes value 1 if a respondent aligns with the conservative party (CDU/CSU), and 0 otherwise. While alignment measures do not equate vote choice, we use this common proxy for political preferences as it enables us to test how the effect size has evolved over time. Furthermore, we include **control variables** which may drive political alignment of individuals, namely (i) age, (ii) binary for being married, (iii) the number of children, (iv) binary for having no school degree, (iv) years of full/part-time employment experience (to capture the size of future pension entitlements), (v) net household income, and (vi) satisfaction with household income (Weisberg 1987; Hellwig 2008; Lago and Lago 2021). Even though further relevant control variables may technically be available, we do not include them due to weak data coverage.[18] Summary statistics are provided in Table 3.

Table 3: Variables and summary statistics

Statistic	N	Min	Max	Mean	SD
Alignment with conservative party	3,254	0	1	0.373	0.484
Age	3,254	22	79	48.250	9.925
Binary for being married	3,254	0	1	0.757	0.429
Number of children	3,254	1	3	1.880	0.700
Binary for no degree	3,254	0	1	0.008	0.087
(log) net household income (monthly)	3,254	5.832	10.519	8.143	0.540
Years of work experience	3,254	0.000	49	19.725	10.616
Satisfaction with household income	3,254	0	10	7.047	2.141
Alignment with social democrats (SPD)	3,254	0	1	0.318	0.466
Alignment with Green party (Die Grünen)	3,254	0	1	0.240	0.427
Alignment with Liberal party (FDP)	3,254	0	1	0.017	0.128
Alignment with The Left (Die Linke)	3,254	0	1	0.048	0.214
Alignment with far-right party (AFD)	3,254	0	1	0.002	0.043
Net personal income (monthly)	2,350	0.000	11,025	1,510	1,089
Voted in federal election in 2013	4,756	0	1	0.791	0.407

The baseline specification takes the following form:

$$\boldsymbol{Y_i = \alpha + \lambda D_i + \beta_1 (X_i - c) + \beta_2 D_i (X_i - c) + \delta_K K_i + \epsilon_i}$$

with Y as the binary for alignment with the conservative party (dependent variable) for survey response i; D as the treatment variable; X as the recoded first child's date-of-birth variable (running variable) represented as the number of month before/after the cut-off date; c as the cut-off date value (which is equal to 0 for children born in January 1992);

[18] For example, occupation, union membership, religiosity, self-placement on left-right scale, degree of political interest, and satisfaction with social security system

K as a vector of control variables with their respective coefficients (δ_K); and ϵ as the error term. The local average treatment effect (LATE) – the parameter of interest – is λ.

For bandwidth selection, we use the method proposed by Imbens and Kalyanaraman (2012) to choose the optimal window size around the discontinuity in a data-driven way. Beyond this baseline specification, we conduct several robustness tests. Specifically, we (i) vary the set of control variables included, (ii) test different bandwidth sizes from 5 to 20 years on each side of the cut-off, (iii) use logit as functional (instead of a linear) form, (iv) equalize the slope on both sides of the cut-off, and (v) adopt a non-parametric estimation approach (implemented by Stigler and Quast (2016)).

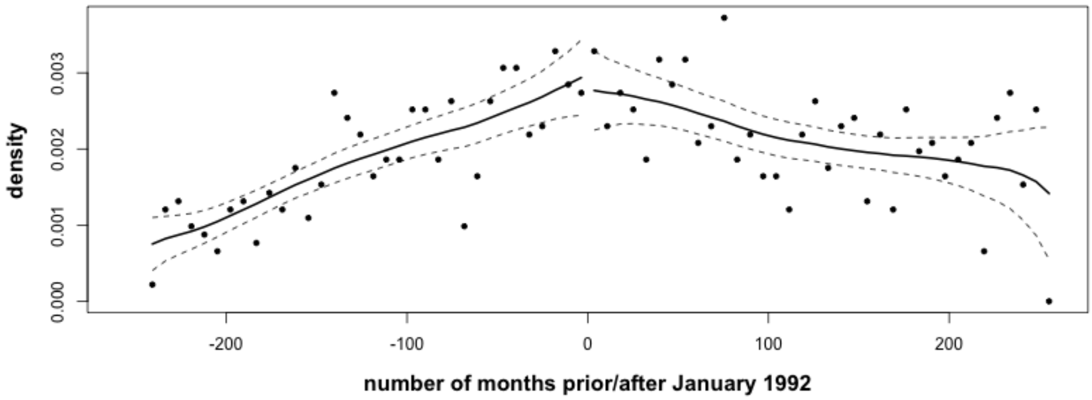
An essential empirical choice concerns the functional form of the model when pursuing an RDD approach. Hereby, one key decision that needs to be made relates to the number of polynomial terms of the forcing variable that are included as regressors on the right-hand-side. While regressions with higher order terms create a better fit to the data, they come with the threat of overfitting. To decide on this variance-bias trade-off, it is common to select the structural form based on their respective information criteria. Thus, we calculate the Bayesian Information Criteria (BIC) for both linear and quadratic polynomial models. Since the linear version results in a lower BIC than the quadratic version, we choose the simpler model with a linear fit as our baseline approach. However, we show that our findings are robust to the inclusion of the quadratic term of the forcing variable. We opted for the BIC instead of the AIC because the BIC is argued to be superior to AIC for explanatory exercises while the reverse is true for predictive ones (Sober 2002, Shmueli 2010).

Identifying Assumptions

Regression discontinuity designs require two assumptions to hold: (i) absence of sorting around the cut-off, and (ii) continuity/balance around the cut-off (Lee and Lemieux 2010; Eggers et al. 2015; Skovron and Titiunik 2015). While neither of these assumptions can be proved with certainty, scholars using discontinuity-based research designs commonly provide empirical checks to test whether these two identifying assumptions are likely to hold.

With respect to the **absence-of-sorting assumption**, we conduct the McCrary density test (2008) to visually and empirically check whether the density changes significantly on either side of the cut-off date. As Figure 1 shows, this does not appear to be the case. The high p-value (0.63) of the density test provides further evidence that the absence-of-sorting assumption is likely to hold. It is worth noting that in the case of the *Mütterrente*, bunching/sorting/manipulation is difficult – if not impossible – as the cut-off criteria is determined twenty years prior to when the reform is being implemented. Parents of course did not know that the date of their child's birth would have significant material consequence more than two decades into the future. Before the introduction of the *Sechstes Sozialgesetzbuch*, any incentives from the benefit system to reproduce would have actually favoured a later birth, after 31 December 1991, as this already qualified parents for additional pension entitlements. Thus, there is a strong case against any potential sorting into the treatment group.

Figure 1: Density plot along the running variable (McCrary plot)

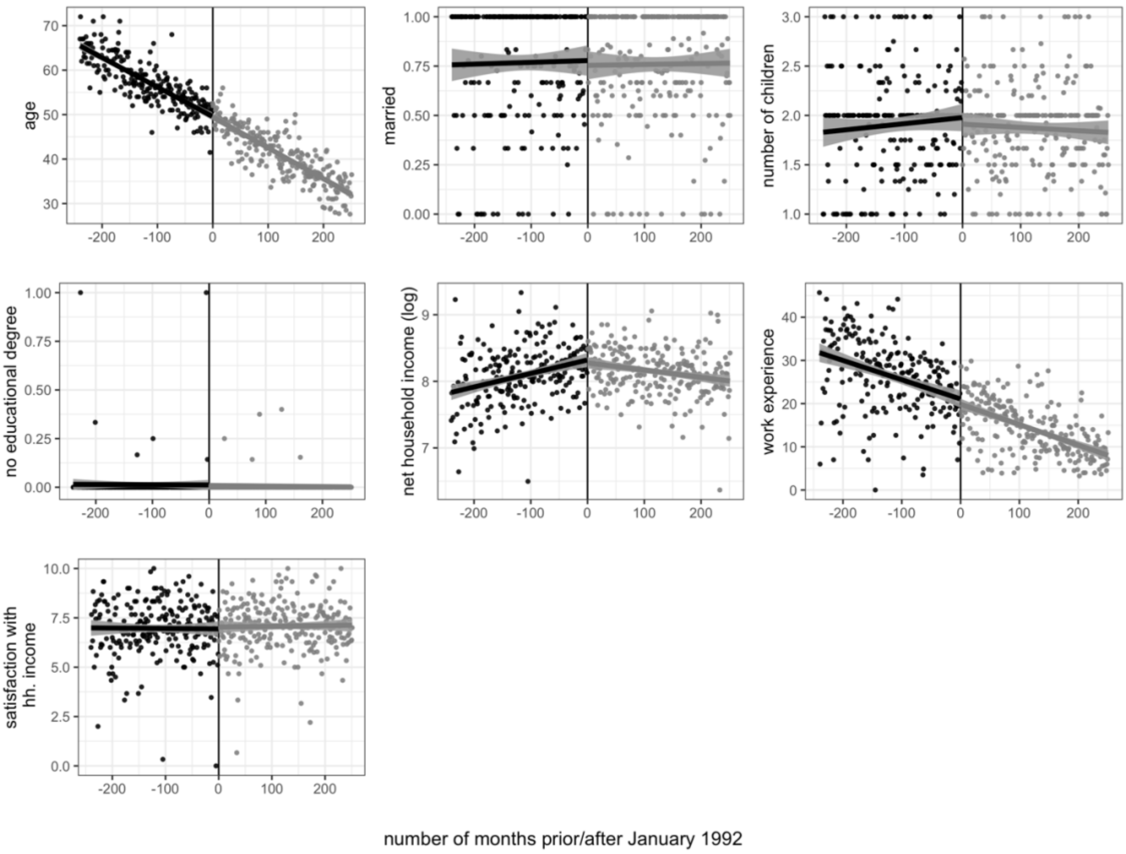


Note: The figure plots the density along the running variable (number of months since January 1992) of the baseline RDD model ($\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$). The p-value of the McCrary density test is 0.6324.

With respect to the **continuity assumption**, we conduct both visual inspections and hypothesis-based tests. As Figure 2 shows, none of the control variables included in the baseline regression indicates a discontinuity around the cut-off.[19] The lack of visual discontinuity in observables is also supported by empirical estimates. As Table 4 shows, the treatment variable is insignificant for all control variables considered. Hereby, it is worth mentioning that the treatment effect is identified at respondents who were approximately 50 years old in 2013 (and correspondingly 29 in 1992) when their first child was born. These numbers are in line with the average age of mothers in Germany giving birth to their first child (Statistisches Bundesamt 2022). In addition, since the effect is identified at individuals who were far below statutory retirement age, the treatment estimates the effect of future/expected pension entitlement after retirement, and not actual pension benefit payments.

[19] self-placement on a right-left scale, current life satisfaction, satisfaction with personal income

Figure 2: Plots of continuity of observables around cut-off date



Note: The panels show monthly averages of observables (y-axis) by the number of months of the date of birth of the first child before/after January 1992. The vertical line at x=0 refers to January 1992 as the cut-off date of the RDD estimation.

Even though both the visual and the empirical test support the validity of the continuity assumption, they can only investigate discontinuities around the threshold on observable variables but not unobservables. While the timing of the reform makes it unlikely to create imbalances between treatment and control group as noted above, one might argue that the first benefit expansion, the introduction of the *Sechstes Sozialgesetzbuch* in 1992, has created incentives to give birth after 31 December 1991. Thus, the first reform may have created differences in outcomes due to differences in unobservable characteristics between treatment and control group that originate from the first reform. To ensure that changes in political alignment are due to the *Mütterrente* and not pre-existing differences (i.e. due to the earlier reform), we analyse previous survey waves after applying the same sample restrictions discussed above and plot the outcome for both groups over time.

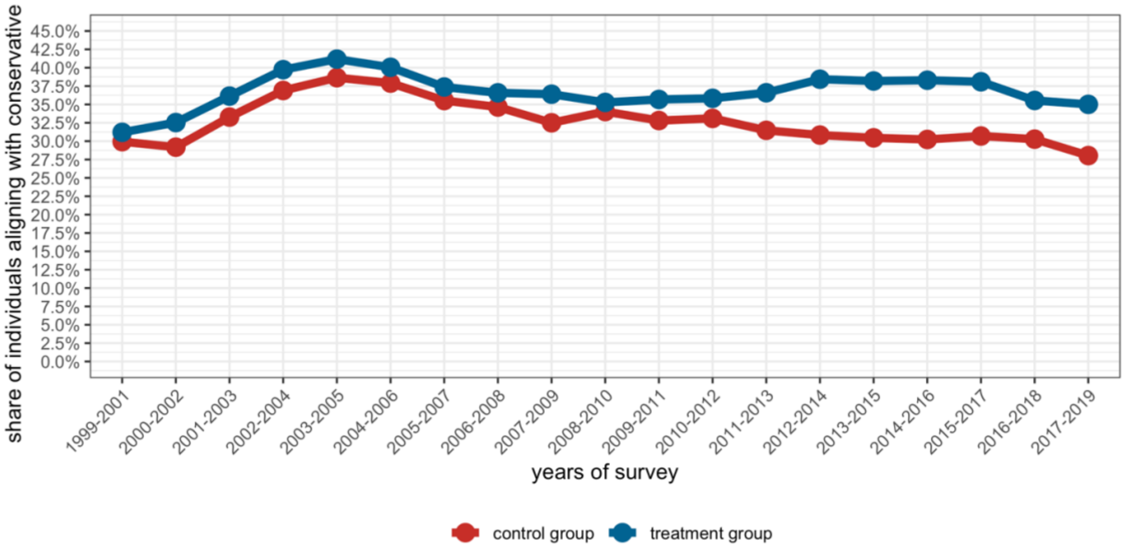
Table 4: Tests for continuity in observables

	Dependent variable						
	age	married	no. of children	no degree	household income (log)	years of work experience	satisf. with household income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
D	**-0.519**	**-0.012**	**-0.069**	**-0.007**	**0.008**	**-0.765**	**0.196**
	(0.298)	**(0.028)**	**(0.046)**	**(0.006)**	**(0.029)**	**(0.523)**	**(0.130)**
(X-c)	-0.051***	-0.001***	-0.004***	-0.00002	0.003***	-0.012**	-0.006***
	(0.002)	(0.0002)	(0.0004)	(0.0001)	(0.0002)	(0.005)	(0.001)
D*(X-c)	-0.008*	0.001***	0.001	-0.00002	-0.003***	0.005	0.011***
	(0.003)	(0.0003)	(0.001)	(0.0001)	(0.0003)	(0.006)	(0.001)
Constant	39.603***	-1.999***	1.709***	0.178***	6.363***	-15.027***	-12.617***
	(1.437)	(0.145)	(0.247)	(0.034)	(0.099)	(2.834)	(0.668)
N	2770	2770	2770	2770	2770	2770	2770
R^2	0.709	0.208	0.205	0.039	0.455	0.450	0.319

Note: The outcome variable is one of the respective observables indicated at the top of each column. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1 (X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable. All estimations include the following set of control variables excluding the variable that is used as dependent variable: age, binary for being married, number of children, no educational degree, household income, years of work experience (full- and part-time), and satisfaction with household income. *p<0.05; **p<0.01; ***p<0.001.

In this case, we define the treatment (control) group as mothers who gave birth to their first child up to five years before (after) the cut-off date. As Figure 3 shows, the differences in outcomes between treatment and control group have been very small and relatively constant over time before the policy proposal about the *Mütterrente* at the end of 2011 has been made. Afterwards, the differences increased strongly due to increases in the alignment level of benefiting individuals. While this descriptive evidence does not allow for a causal interpretation of the reform effect at this stage, it nevertheless aims to support the conclusion that the treatment and control groups had similar levels of alignment with the conservative party before the policy has been proposed. Thus, it seems rather unlikely that the first reform has created such strong differences in outcomes that threatens the continuity assumption on unobservables of our discontinuity-based research design.

Figure 3: Alignment with the conservative party between eligible and non-eligible mothers over time



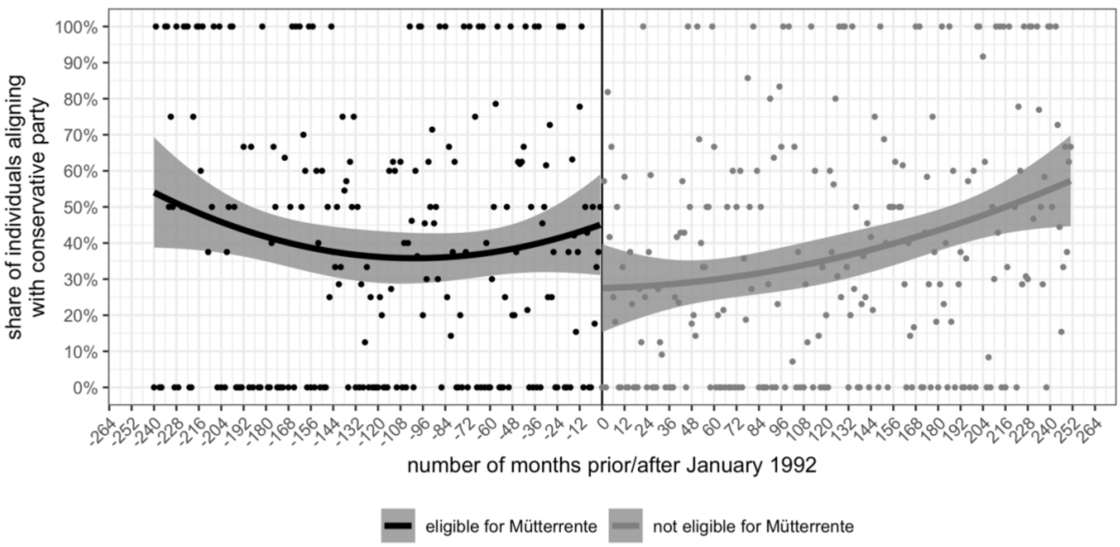
Note: The figure plots the 3-year-wave averages of alignment with the conservative party over time for both treatment (blue) and control group (red). The treatment group consists of mothers who gave birth to their first child between 01 January 1987 and 31 December 1991 (eligible for Mütterrente); the control group consists of mothers who gave birth to their first child between 01 January 1992 and 31 December 1996 (not eligible for Mütterrente).

Results

Baseline

The main finding is that individuals who were to benefit from the pension benefit expansion are substantially and statistically significantly more likely to align with the conservative party because of the policy promise made in the run-up to the federal election in 2013. The descriptive result in Figure 4 shows a clear jump in alignment levels around the discontinuity. Eligible individuals close to the cut-off indicate a higher likelihood of political support for the conservative party than their non-eligible peers with children born after 31 December 1991. In table 5, the empirical results from the baseline model show that the promise of the *Mütterrente* increased alignment with the conservative party by 12.2%. This point estimate is robust to the in- and exclusion of various control variables (Table 5). It is important to note that a negative coefficient in table 4 refers to a positive campaign effect. This is because the treatment group is below, and the control group is above the cut-off value.

Figure 4: Alignment with the conservative party by first child's birthyear



Note: The figure plots the average alignment level with the conservative party of mothers by the date of birth (year-month) of the first child. Black dots consist of mothers who gave birth to their first child before January 1992 (eligible for Mütterrente); grey dots consist of mothers who gave birth to their first child after January 1992 (not eligible for Mütterrente). Shaded areas indicate the 95%-confidence intervals of the respective quadratic fit.

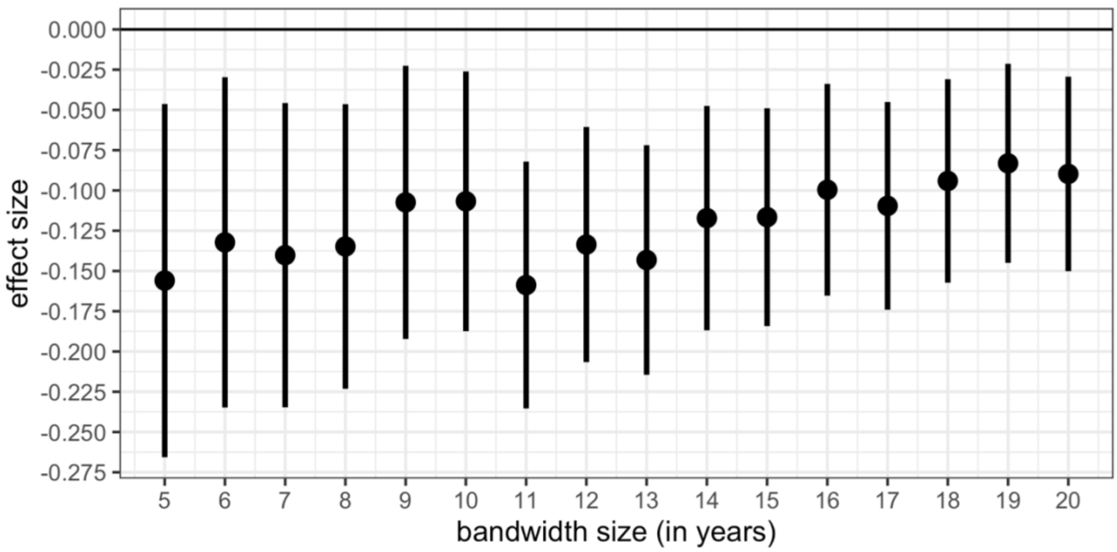
Table 5: The effect of eligibility on party alignment with different control variable sets

	Dependent variable: binary for alignment with conservative party							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D	**-0.104****	**-0.108****	**-0.106****	**-0.109****	**-0.110****	**-0.116****	**-0.117*****	**-0.122*****
	(0.036)	**(0.036)**	**(0.035)**	**(0.035)**	**(0.035)**	**(0.035)**	**(0.036)**	**(0.036)**
(X-c)	-0.0001	-0.0005	-0.0004	-0.001	-0.001	-0.001*	-0.001*	-0.001*
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
D*(X-c)	0.001*	0.001*	0.001*	0.001*	0.001*	0.001**	0.001**	0.001*
	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)	(0.0004)
age		-0.006**	-0.005**	-0.007***	-0.008***	-0.010***	-0.009***	-0.009***
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
married			0.155***	0.163***	0.162***	0.108***	0.106***	0.106***
			(0.022)	(0.022)	(0.022)	(0.025)	(0.025)	(0.025)
# of children				-0.041**	-0.040**	-0.048***	-0.052***	-0.048**
				(0.014)	(0.014)	(0.014)	(0.015)	(0.015)
no degree					-0.119	-0.066	-0.076	-0.077
					(0.106)	(0.107)	(0.107)	(0.107)
household income						0.096***	0.098***	0.058*
						(0.020)	(0.020)	(0.024)
work experience							-0.001	-0.001
							(0.001)	(0.001)
household income satisfaction								0.017**
								(0.005)
Constant	0.366***	0.667***	0.524***	0.692***	0.700***	0.061	0.041	0.257
	(0.025)	(0.104)	(0.105)	(0.120)	(0.120)	(0.181)	(0.182)	(0.194)
N	2500	2500	2500	2500	2500	2472	2472	2451
R^2	0.006	0.010	0.029	0.032	0.033	0.042	0.042	0.045

Note: The outcome variable is party alignment with the conservative party. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X-c) + \beta_2 D(X-c) + \delta_K K + \epsilon}$. Columns differ with respect to the inclusion of the control variable set. *p<0.05; **p<0.01; ***p<0.001.

We conduct several robustness checks to test for the sensitivity of the LATE estimate. In an RDD, model uncertainty can mainly arise from three sources. First, the estimate is biased if observations too far away from the cut-off point are erroneously included in the regression. Thus, in addition to the baseline bandwidth size determined via the Imbens-Kalyanaraman method (2012), we alternate the bandwidth parameter using values ranging from 5 to 20 years. As Figure 5 shows, independent of the bandwidth choice, the point estimates are very similar, and all coefficients remain negative and statistically highly significant across specifications (see also Table A1.1).

Figure 5: The effect of eligibility on party alignment with conservative party with different bandwidth size parameters



Note: The outcome variable is party alignment with the conservative party. Estimates are based on $\boldsymbol{Y} = \boldsymbol{\alpha} + \boldsymbol{\lambda D} + \boldsymbol{\beta_1}(\boldsymbol{X} - \boldsymbol{c}) + \boldsymbol{\beta_2 D}(\boldsymbol{X} - \boldsymbol{c}) + \boldsymbol{\delta_K K} + \boldsymbol{\epsilon}$. All estimations include the following set of control variables: age, binary for being married, number of children, no educational degree, household income, years of work experience (full- and part-time), and satisfaction with household income. Vertical lines represent the 95% confidence interval around the coefficient of the treatment effect.

Second, misspecifications in the statistical model itself can lead to bias in the estimates. To test for the robustness towards different model specifications, we alter the baseline model by (i) adding higher order polynomials of the running variables as additional regressors, (ii) imposing equal-slope restrictions on each side of the cut-off, and (iii) using a logit (instead of a linear) structural form. Again, the treatment effect is still statistically significant and robust across specifications (see Table A1.2 and A1.3)

Third, we re-estimate the baseline specification by using a non-parametric estimation method which is equivalent to a local linear regression approach.[20] The use of non-parametric methods comes with the advantage of placing greater weight on data closer to the cut-off without imposing bandwidth restrictions. Again, the non-parametric approach leads to coefficients that are very similar to the LATE estimate from the baseline model (see Figure A1.4).

Table 6a: The effect of eligibility on party alignment with cut-off dates before January 1992 (placebo test)

Dependent variable: binary for alignment with conservative party							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
D	**-0.002**	**-0.018**	**0.005**	**0.023**	**0.014**	**-0.012**	**-0.037**
	(0.037)	**(0.036)**	**(0.034)**	**(0.035)**	**(0.033)**	**(0.032)**	**(0.031)**
(X-c)	-0.002***	-0.002***	-0.002***	-0.002***	-0.002***	-0.001***	-0.001***
	(0.0005)	(0.0004)	(0.0004)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
D*(X-c)	0.002***	0.002***	0.002***	0.002***	0.002***	0.001***	0.001***
	(0.0005)	(0.0004)	(0.0004)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Constant	0.218	0.203	0.178	0.158	0.156	0.158	0.152
	(0.172)	(0.171)	(0.171)	(0.171)	(0.170)	(0.169)	(0.169)
Cut-off date	Jan 1984	Jan 1985	Jan 1986	Jan 1987	Jan 1988	Jan 1989	Jan 1990
N	2362	2480	2566	2598	2599	2673	2716
R^2	0.039	0.039	0.039	0.039	0.039	0.039	0.040

Note: The outcome variable is party alignment with the conservative party. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1 (X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the selection of alternative placebo cut-off dates. All estimations include the following set of control variables: age, binary for being married, number of children, no educational degree, household income, years of work experience (full- and part-time), and satisfaction with household income. *p<0.05; **p<0.01; ***p<0.001.

[20] In the analysis, we use the implementation by Stigler and Quast (2016).

To provide further evidence about the credibility of the identification strategy, we also conduct two kinds of placebo tests. First, it is common in RDD application to test for the absence of discontinuities – apart from the true one – at alternative (placebo) locations along the forcing variable. Thus, we re-estimate the baseline specification with cut-offs at January 1984, 1985, 1986, 1987, 1988, 1989, 1990 as well as 1994, 1995, 1996, 1997, 1998, 1999 and 2000. As Table 6a and 6b show, not one of the coefficients of interest is statistically significant at any of these placebo cut-off values.

Table 6b: The effect of eligibility on party alignment with cut-off dates after January 1992 (placebo test)

	Dependent variable: binary for alignment with conservative party						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
D	**-0.048**	**0.003**	**-0.003**	**-0.020**	**0.043**	**0.0004**	**0.046**
	(0.031)	**(0.031)**	**(0.031)**	**(0.032)**	**(0.033)**	**(0.035)**	**(0.037)**
(X-c)	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
D*(X-c)	0.002***	0.002***	0.002***	0.002***	0.002***	0.002***	0.002***
	(0.0002)	(0.0002)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0004)
Constant	0.111	0.065	0.075	0.090	0.043	0.080	0.059
	(0.167)	(0.166)	(0.166)	(0.164)	(0.163)	(0.162)	(0.161)
Cut-off date	Jan 1994	Jan 1995	Jan 1996	Jan 1997	Jan 1998	Jan 1999	Jan 2000
N	2796	2759	2755	2731	2731	2719	2641
R^2	0.044	0.043	0.043	0.043	0.044	0.043	0.043

Note: The outcome variable is party alignment with the conservative party. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the selection of alternative placebo cut-off dates. All estimations include the following set of control variables: age, binary for being married, number of children, no educational degree, household income, years of work experience (full- and part-time), and satisfaction with household income. *p<0.05; **p<0.01; ***p<0.001.

Second, for campaign promises to be effective in positively influencing support, alignment must be observed to occur *towards* the promise-making party; thus, competitor parties should either remain unaffected, or their popular support should decline among eligible individuals. To conduct this placebo test, we change the dependent variable of the baseline specification by using the alignment binaries of the competitor parties. Table 7 provides evidence that none of the competitor parties gained support among eligible individuals. The Green Party were the only party to suffer a decline in support among eligible individuals, indicating that the conservative party attracted voters from this left-wing political competitor. Indeed, this finding is in line with the descriptive evidence showing that women (under 60) form a large constituency of the green party (Egeler 2013; Hin and Schneider 2014).

Table 7: The effect of eligibility on party alignment for competitor parties (placebo test)

	Dependent variable: binary for alignment with different parties				
	Social Democrats (SPD)	Green Party (Die Grünen)	Liberal Party (FDP)	The Left (Die Linke)	Far-Right Party (AFD)
	(1)	(2)	(3)	(4)	(5)
D	**-0.006**	**0.123*****	**0.016**	**-0.016**	**0.0001**
	(0.035)	**(0.032)**	**(0.010)**	**(0.016)**	**(0.003)**
(X-c)	0.0002	0.001***	-0.0001	-0.001***	0.00004
	(0.0003)	(0.0003)	(0.0001)	(0.0001)	(0.00003)
D*(X-c)	-0.0003	-0.001	0.0001	0.0004*	-0.0001*
	(0.0004)	(0.0004)	(0.0001)	(0.0002)	(0.00004)
Constant	1.039***	-0.981***	-0.069	0.448***	0.030
	(0.193)	(0.176)	(0.053)	(0.087)	(0.019)
N	2770	2770	2770	2770	2770
R^2	0.025	0.055	0.006	0.044	0.005

Note: The outcome variable is party alignment with different competitor parties that did not make the Mütterrente promise. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the choice of the dependent variable. All estimations include the following set of control variables: age, binary for being married, number of children, no educational degree, household income, years of work experience (full- and part-time), and satisfaction with household income. *p<0.05; **p<0.01; ***p<0.001.

Durability

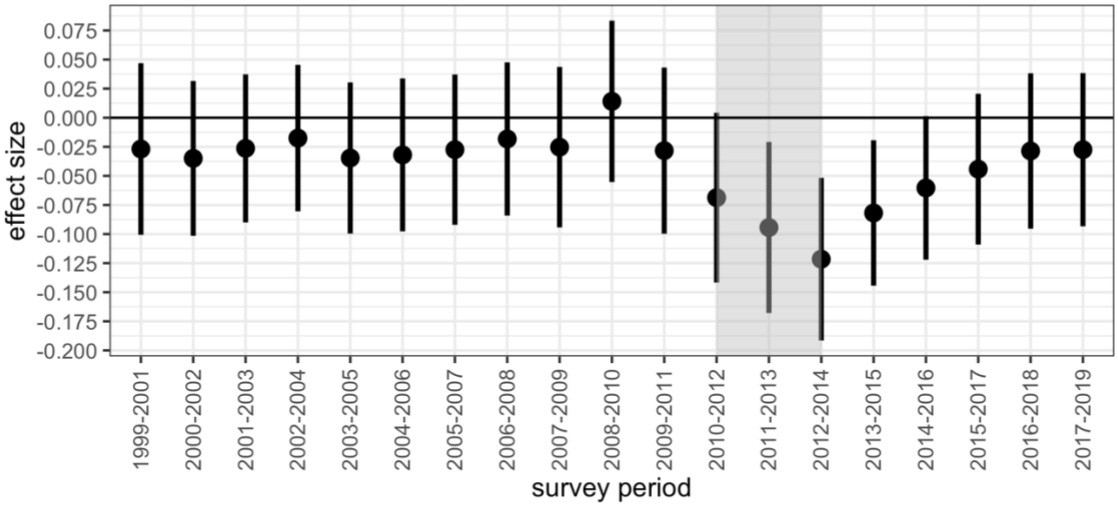
A pertinent question in the field of political science is how long the effect of a (campaign promise of a) policy lasts? From a prospective pocketbook lens, once the reform is implemented and thus the pledge fulfilment completes the policy-vote exchange, the initial economic incentive no longer remains. Voters that had been motivated by the policy may in fact revert to their pre-held political dispositions.

In the empirical literature, on retrospective economic voting though, some scholars who have tested the binding potential of targeted transfer programs have shown that the effects last for a while after a benefit program has ended. In this context, the analysis by Bechtel and Hainmüller (2011) found that disaster benefits in Germany significantly increased the vote share among recipients for the party adopting them (the social democrats); this effect lasted for two subsequent elections before it died out. Similarly, Manacorda et al. (2011) show that cash transfers to the poor in Uruguay have a lasting impact on political support for the government among affected individuals – even after the program has ended. Yet, results by Zimmermann (2021) highlight that the expiration of an anti-poverty program in India led to a substantial decline in governmental support among eligible individuals. These short-term effect findings are in line with the results of both Zucco (2013) and Lü (2014) who have shown that conditional cash transfers in Brazil and social policies in China, respectively, cannot secure long-term political support.

To test the durability of the promise/policy effect in the case of expanding pension entitlements, we apply the same sample restriction steps as in the baseline and shift the survey time forward to check whether conservative alignment between treatment and control group differ significantly after reform implementation. To keep the degree of sampling uncertainty comparable over time, we set the bandwidth size equal to the baseline specification – which was derived via the bandwidth selection method by Imbens and Kalyanaraman (2012). The survey period spans all responses between 01 January in k and 31 December in $k+2$ with start year $k = \{2011, \ldots, 2017\}$. Furthermore, using the same methodological approach, we estimate the treatment effect also on earlier survey waves – with $k = \{1999, \ldots, 2010\}$ – to test whether the reform does not result in a statistically significant estimate of the effect before the policy was first debated at the annual meeting of the conservative party in November 2011.

Two findings stand out. First, and in line with the expectations and the visual evidence provided in Figure 6, we do not find any significant effects in any of the periods before the *Mütterrente* became part of public discourse. This pre-promise placebo tests provides further evidence for the credibility of the identification strategy. Second, the post-promise estimates show that the policy (proposal) was most effective in attracting political support among benefiting voters between 2012 and 2014, which refers to the campaign and immediate post-election period. Afterwards, the difference in alignment declines gradually and it converges back to the pre-promise level after the electoral pledge was fulfilled in 2015.

Figure 6: The effect of eligibility on party alignment with conservative party with different survey periods



Note: The outcome variable is party alignment with the conservative party. Estimates are based on $\boldsymbol{Y} = \boldsymbol{\alpha} + \boldsymbol{\lambda D} + \boldsymbol{\beta_1}(\boldsymbol{X} - \boldsymbol{c}) + \boldsymbol{\beta_2 D}(\boldsymbol{X} - \boldsymbol{c}) + \boldsymbol{\delta_K K} + \boldsymbol{\epsilon}$. The x-axis indicates the survey years being used for the respective estimate. All estimations include the following set of control variables: age, binary for being married, number of children, no educational degree, household income, years of work experience (full- and part-time), and satisfaction with household income. Vertical lines represent the 95% confidence interval around the coefficient of the treatment effect. The grey area represents (approximately) the promise period.

The latter finding highlights an important dynamic within the pledge-alignment process. In the case of the pension expansion of the *Mütterrente*, the promise-induced alignment gains among benefiting voters diminished after pledge fulfilment, implying that benefit expansions are not able to secure popular support in the medium- or long-run.

Heterogeneity

To investigate the underlying mechanism of the promise-induced alignment shifts, we test whether the baseline effect size varies with economic security which has been identified as a central correlate of pro-welfare state preferences (Rehm 2009; Margalit 2013; Barnes 2015). Previous empirical evidence has shown that vote buying seems to be more rewarding – in political terms – when targeted at poorer individuals (Brusco et al.

2004). Thus, we expect that voters at the lower end of the income distribution show greater sensitivity towards the reform promise since the marginal utility of additional pension benefits is higher for individuals with lower levels of income (and lower contemporary pension contributions) compared to their richer counterparts.

Thus, to test whether economic insecurity is a moderating factor of promise effects, we use different correlates of individuals' economic position to divide the full sample into different sub-samples. First, we compare the LATE estimate between the baseline (without household income as control) and low-income individuals. Hereby, we define low-income individuals as respondents who earn less than 850 Euro per month. Employees with ≤850 Euro per month are on so-called "Mini-/Midi-Job" contracts which provide no or only limited compulsory social insurance contributions towards, for example, health or unemployment insurance. While these types of jobs contribute to the pension systems, they provide only narrow social insurance at retirement age, especially due to their low contribution rates. The results show that low-income individuals are generally more sensitive to the reform promise than what the baseline estimate would imply. To be more specific, the promise effect among individuals with an income equal to/below 850 Euro per month reaches 29.8% (Table 8, column (1) vs. (2)). This estimate is statistically significantly larger in absolute terms (p-value: 0.025) than the corresponding treatment coefficient of 11.8% among the whole sample. Thus, the fact that individuals on low-income contracts indicate larger promise-induced alignment shifts is in line with our theoretical prior.

Table 8: The heterogenous effects of eligibility on party alignment among different groups of individuals

	Dependent variable: binary for alignment with conservative party							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D	**-0.118*****	**-0.298*****	**-0.117****	**-0.034**	**-0.147*****	**-0.125*****	**-0.210****	**-0.097***
	(0.036)	**(0.072)**	**(0.036)**	**(0.067)**	**(0.042)**	**(0.036)**	**(0.065)**	**(0.042)**
(X-c)	-0.001	0.002*	-0.001	-0.001*	-0.0004	-0.001*	0.001	-0.001**
	(0.0003)	(0.001)	(0.0003)	(0.001)	(0.0004)	(0.0003)	(0.001)	(0.0004)
D*(X-c)	0.001	-0.0005	0.001*	0.002*	0.0005	0.001*	0.0002	0.001*
	(0.0004)	(0.001)	(0.0004)	(0.001)	(0.0005)	(0.0004)	(0.001)	(0.0005)
Constant	0.633***	0.655*	0.176	0.528	0.063	0.040	0.614	0.257
	(0.121)	(0.264)	(0.193)	(0.356)	(0.231)	(0.188)	(0.347)	(0.242)
Group	baseline	small income	baseline	single child	multiple children	baseline	not married	married
Sub-sampling rule	-	net income <=850 Euro	-	1 child	2 or more children	-	not married	married
Scale of sub-sampling variable	-	continuous	-	1-3	1-3	-	binary	binary
p-value of H0	-	0.0254	-	0.2763	0.5905	-	0.2543	0.6164
N	2770	645	2770	839	1931	2770	660	2110
R^2	0.042	0.066	0.041	0.067	0.041	0.038	0.064	0.023

Note: The outcome variable is party alignment with the conservative party. Estimates are based on $\boldsymbol{Y} = \boldsymbol{\alpha} + \boldsymbol{\lambda D} + \boldsymbol{\beta_1}(\boldsymbol{X} - \boldsymbol{c}) + \boldsymbol{\beta_2 D}(\boldsymbol{X} - \boldsymbol{c}) + \boldsymbol{\delta_K K} + \boldsymbol{\epsilon}$. Columns differ with respect to different (sub-)samples being used for the estimation. All estimations include the following baseline set of control variables to avoid double measurement: in column (1) and (2), household income is removed; in column (3)-(5), number of children is removed; in column (6)-(8), the binary for being married is removed. For each sub-sample estimate, we conduct a t-test and compare is with the corresponding baseline estimate. The null hypothesis refers to equality between the baseline and sub-sampling estimate. *p<0.05; **p<0.01; ***p<0.001.

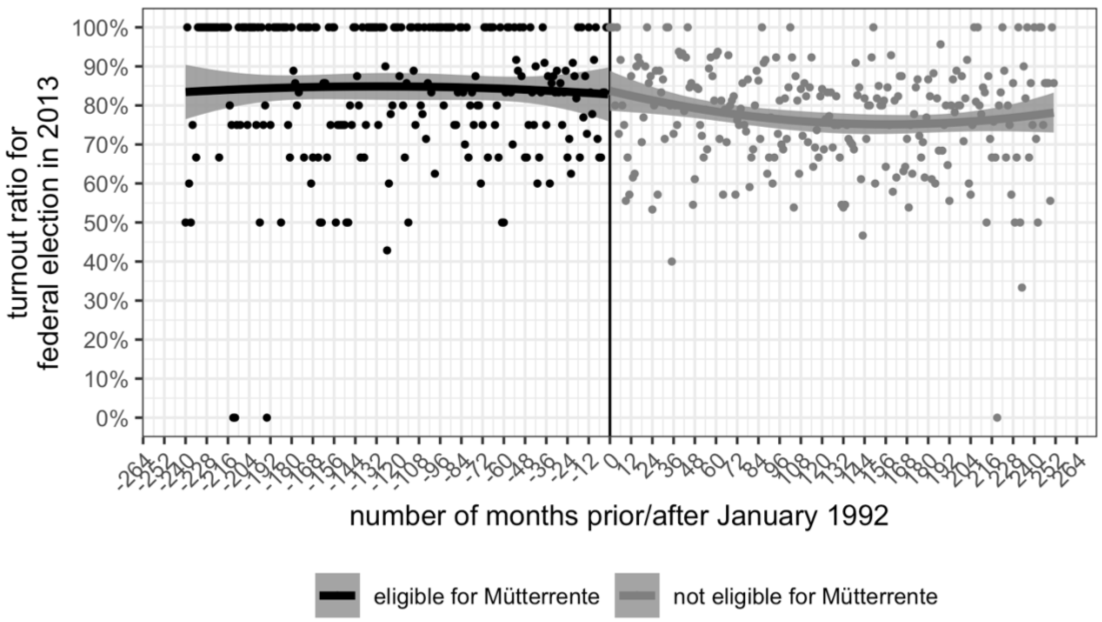
Second, the family/household structure may have a large effect on individuals' economic position – both in terms of wealth and income. One aspect concerns the number of children. Hereby, mothers with more children may benefit more from parenting-based pension entitlements because they have, on average, less years of employment-based pension contributions due to upbringing time. To investigate the number of children as mediating factor, we split the baseline dataset into two sample: mothers with one child and mothers with two or more children. The estimates show that the promise effect is statistically significant, and larger among respondents with two or more children compared to the baseline coefficient (Table 8, column (3) vs. (4) and (5)). In contrast, the estimate among mothers with one child is smaller and statistically not significant. While this may suggest that the size of the promise effect indeed increases with the number of children, none of these coefficients is statistically different from the baseline coefficient (p-values > 0.05). Thus, we cannot provide a final answer whether family size amplifies the effectiveness of the electoral pledge.

Third, marital status may also play a mediating role as it affects various aspects related to cost-sharing, production, and labour division within households (Häusermann et al. 2016). When we split the sample into married and non-married mothers, we find that the effect is substantially larger for non-married mothers than the baseline estimate (Table 8, column (6) vs. (7) and (8)). In contrast, the coefficient for married mothers is smaller than the baseline LATE. While these point estimates may suggest that the average treatment effect in the baseline sample is driven by non-married mothers, the accompanying t-test between the baseline estimate and the sub-sample coefficients does not allow us to reject the null hypothesis. Thus, we cannot conclude with some level of confidence that the effect size varies between married and un-married individuals.

Turnout

Beyond alignment, a central research question concerns whether social benefits or the promise of them, incentivize individuals to participate in the next election. Indeed, previous studies in the retrospective economic voting literature – mostly on targeted transfer programs – show relatively large effects of benefit increases on voter turnout (Carreras and İrepoğlu 2013; De La O 2013; Nichter 2008). These empirical studies focus mostly on emerging and developing countries and depart from a retrospective frame by investigating how adopted policies before elections affect electoral participation.

Figure 7: Average turnout ratios in federal election in 2013 by first child's date of birth



Note: The figure plots the average turnout ratios for the federal election in 2013 of mothers by the date of birth (year-month) of the first child. Black dots consist of mothers who gave birth to their first child before January 1992 (eligible for *Mütterrente*); grey dots consist of mothers who gave birth to their first child after January 1992 (not eligible for *Mütterrente*). Shaded areas indicate the 95%-confidence intervals of the respective quadratic fit.

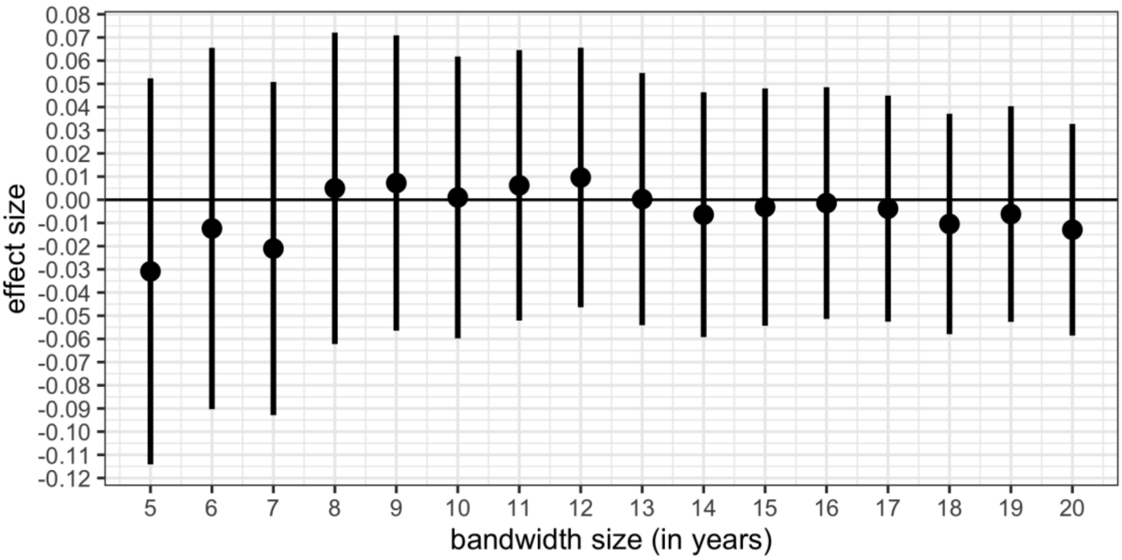
Table 9: The effect of eligibility on turnout with different control variable sets

	Dependent variable: binary for turnout in federal election in 2013							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D	**-0.015**	**-0.022**	**-0.016**	**-0.017**	**-0.021**	**-0.005**	**-0.004**	**-0.003**
	(0.028)	**(0.027)**	**(0.027)**	**(0.027)**	**(0.027)**	**(0.027)**	**(0.027)**	**(0.027)**
(X-c)	-0.0001	0.001***	0.002***	0.002***	0.001***	0.001***	0.001***	0.001***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
D*(X-c)	-0.0005	-0.0003	-0.0004	-0.0004	-0.0004	-0.00002	-0.00003	-0.0001
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
age		0.023***	0.022***	0.023***	0.022***	0.019***	0.017***	0.017***
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
married			0.119***	0.115***	0.112***	0.033*	0.037*	0.037*
			(0.013)	(0.014)	(0.014)	(0.016)	(0.016)	(0.016)
# of children				0.009	0.011	-0.008	0.001	0.003
				(0.009)	(0.009)	(0.009)	(0.010)	(0.010)
no degree					-0.269***	-0.229***	-0.185**	-0.182**
					(0.066)	(0.066)	(0.069)	(0.069)
household income						0.149***	0.143***	0.130***
						(0.014)	(0.015)	(0.017)
work experience							0.003**	0.003**
							(0.001)	(0.001)
Household income satisfaction								0.005
								(0.003)
Constant	0.836***	-0.272***	-0.342***	-0.380***	-0.343***	-1.295***	-1.217***	-1.149***
	(0.022)	(0.067)	(0.067)	(0.077)	(0.077)	(0.121)	(0.124)	(0.130)
N	3585	3585	3581	3581	3576	3457	3440	3429
R^2	0.008	0.087	0.106	0.106	0.109	0.135	0.133	0.133

Note: The outcome variable is turnout during the federal election in 2013. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X-c) + \beta_2 D(X-c) + \delta_K K + \epsilon}$. Columns differ with respect to the inclusion of the control variable set. *p<0.05; **p<0.01; ***p<0.001.

However, there is little evidence on whether electoral pledges have a similar positive impact on turnout among *prospective* beneficiaries. Thus, we test whether turnout in 2013 was higher among positively affected individuals after the promise was made but before it was fulfilled. To do so, we construct a binary variable from the survey wave in 2014 which is equal to 1 if an individual reported that she participated in the federal election in 2013, and 0 otherwise. Descriptively, the difference between eligible and non-eligible voters seems to be negligible (Figure 7). Likewise, when modifying the baseline specification with the turnout binary as dependent variable, the null hypothesis cannot be rejected – independent of the control variable set (Table 9) or the bandwidth size (Figure 8, see also Table A1.5). Thus, the results suggest that the campaign promise did not influence the probability of voting *per se* but rather the alignment with a party itself.

Figure 8: The effect of Mütterrente eligibility on turnout with different bandwidth size parameters



Note: The outcome variable is turnout in the federal election in 2013. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. All estimations include the following set of control variables: age, binary for being married, number of children, no educational degree, household income, years of work experience (full- and part-time), and satisfaction with household income. Vertical lines represent the 95% confidence interval around the coefficient of the treatment effect.

Conclusion

This paper has shown that campaign promises are viable strategies for electoral competitors to increase popular support among the electorate, especially for short-run gains in a particular election. The prevalence of prospective pocketbook motives can indeed be substantial. In the case of the German *Mütterrente* reform, alignment of eligible individuals with the pledge-making conservative party increased by 12.2% among benefiting individuals. These estimates are robust to numerous placebo, validity, and sensitivity tests.

The paper has also shown that alignment with the promise-making party among benefiting individuals declines gradually after the pledge is fulfilled. This finding suggests that expansion pledges can indeed successfully attract pocketbook-motivated voters prior to elections, but single benefit expansions do not become manifested in long-run political support. This conclusion implies that additional promises of further benefit expansions are required to secure the electoral support of prospective egotropic voters over time.

In addition, while the campaign promises do not seem to impact voter turnout, our empirical analysis has shown that electoral pledges on benefit expansions are particularly successful to attract support from voters who are traditionally aligned with left-wing platforms. This finding suggests that promises on benefit expansions are rather persuasive than mobilizing, at least in the context of an advanced democracy with a developed welfare state and a multiparty proportional representation system.

Finally, this paper has also identified heterogeneity in the size of the promise effect. Most notably, low-income individuals are up to three times more likely to align with the promise-making party due to the pledge than what the average treatment effect across the whole eligible group would imply. This result echoes previous evidence on the income-gradient in the literature on political behaviour (Rosenstone and Hansen 1993; Campbell 2002; Schafer et al. 2022) and it shows that the political gains of expansion benefits depend rather on their marginal return to individuals than their nominal dollar value.

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Appendix

Table A1.1: The effect of eligibility on party alignment with conservative party with different bandwidth size parameters

	Dependent variable: binary for alignment with conservative party							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D	**-0.156****	**-0.132***	**-0.140****	**-0.135****	**-0.107***	**-0.107****	**-0.159*****	**-0.134*****
	(0.056)	**(0.052)**	**(0.048)**	**(0.045)**	**(0.043)**	**(0.041)**	**(0.039)**	**(0.037)**
(X-c)	-0.001	-0.0004	-0.0001	-0.001	-0.001*	-0.0004	0.0004	-0.0001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.0005)	(0.0004)	(0.0004)
D*(X-c)	0.002	-0.0004	-0.0004	0.0003	0.001	-0.0004	-0.0004	-0.0001
	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.0005)
Constant	-0.463	-0.064	0.073	0.037	0.143	0.321	0.405	0.286
	(0.296)	(0.276)	(0.254)	(0.242)	(0.230)	(0.221)	(0.210)	(0.201)
Bandwidth	5 years	6 years	7 years	8 years	9 years	10 years	11 years	12 years
Observations	1215	1371	1592	1811	1960	2109	2302	2522
R2	0.083	0.073	0.054	0.054	0.054	0.048	0.042	0.043

Note: The outcome variable is party alignment with the conservative party. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1 (X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. All estimations include the following set of control variables: age, binary for being married, number of children, no educational degree, household income, years of work experience (full- and part-time), and satisfaction with household income. Columns differ with respect to the bandwidth size. *p<0.05; **p<0.01; ***p<0.001.

Table A1.1 (cont'): The effect of eligibility on party alignment with conservative party with different bandwidth size parameters

	Dependent variable: binary for alignment with conservative party							
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
D	**-0.143*****	**-0.117*****	**-0.117*****	**-0.100****	**-0.110*****	**-0.094****	**-0.083****	**-0.090****
	(0.036)	**(0.036)**	**(0.035)**	**(0.034)**	**(0.033)**	**(0.032)**	**(0.032)**	**(0.031)**
(X-c)	-0.0002	-0.001*	-0.001*	-0.001*	-0.001*	-0.001**	-0.001***	-0.001***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0002)	(0.0002)
D*(X-c)	0.0004	0.001*	0.001*	0.001*	0.001**	0.001**	0.001***	0.001***
	(0.0004)	(0.0004)	(0.0004)	(0.0003)	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Constant	0.297	0.288	0.238	0.123	0.193	0.182	0.237	0.185
	(0.197)	(0.193)	(0.190)	(0.183)	(0.180)	(0.177)	(0.174)	(0.169)
Bandwidth	13 years	14 years	15 years	16 years	17 years	18 years	19 years	20 years
N	2654	2793	2940	3085	3195	3320	3439	3577
R^2	0.044	0.044	0.038	0.035	0.036	0.037	0.042	0.043

Note: The outcome variable is party alignment with the conservative party. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1 (X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. All estimations include the following set of control variables: age, binary for being married, number of children, no educational degree, household income, years of work experience (full- and part-time), and satisfaction with household income. Columns differ with respect to the bandwidth size. *p<0.05; **p<0.01; ***p<0.001.

Table A1.2: The effect of eligibility on party alignment with a quadratic fit (second order polynomial of running variable) and different control variable sets

Dependent variable: binary for alignment with conservative party								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D	**-0.134***	**-0.131***	**-0.132***	**-0.133***	**-0.135***	**-0.142****	**-0.142****	**-0.143****
	(0.054)	**(0.053)**	**(0.053)**	**(0.053)**	**(0.053)**	**(0.053)**	**(0.053)**	**(0.053)**
(X-c)	0.002	0.002	0.001	0.001	0.001	0.001	0.001	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
(X^2-c)	0.00001	0.00001	0.00001	0.00001	0.00001	0.00001	0.00001	0.00001*
	(0.00001)	(0.00001)	(0.00001)	(0.00001)	(0.00001)	(0.00001)	(0.00001)	(0.00001)
D*(X-c)	-0.002	-0.002	-0.002	-0.002	-0.002	-0.002	-0.002	-0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
D*(X^2-c)	-0.00001	-0.00001	-0.00001	-0.00001	-0.00001	-0.00001	-0.00001	-0.00001
	(0.00001)	(0.00001)	(0.00001)	(0.00001)	(0.00001)	(0.00001)	(0.00001)	(0.00001)
		-0.006**	-0.005**	-0.007***	-0.008***	-0.010***	-0.009***	-0.009***
		(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
			0.154***	0.161***	0.161***	0.106***	0.104***	0.104***
			(0.022)	(0.022)	(0.022)	(0.025)	(0.025)	(0.025)
# children				-0.042**	-0.041**	-0.049***	-0.052***	-0.049**
				(0.014)	(0.014)	(0.014)	(0.015)	(0.015)
no degree					-0.123	-0.069	-0.077	-0.077
					(0.106)	(0.107)	(0.107)	(0.107)
household income						0.098***	0.100***	0.058*
						(0.020)	(0.020)	(0.024)
work experience							-0.001	-0.001
							(0.001)	(0.001)
household income satisfaction								0.018***
								(0.005)
Constant	0.419***	0.719***	0.573***	0.748***	0.757***	0.106	0.090	0.326
	(0.037)	(0.107)	(0.108)	(0.123)	(0.123)	(0.182)	(0.183)	(0.196)
N	2500	2500	2500	2500	2500	2472	2472	2451
R^2	0.008	0.011	0.031	0.034	0.035	0.043	0.044	0.047

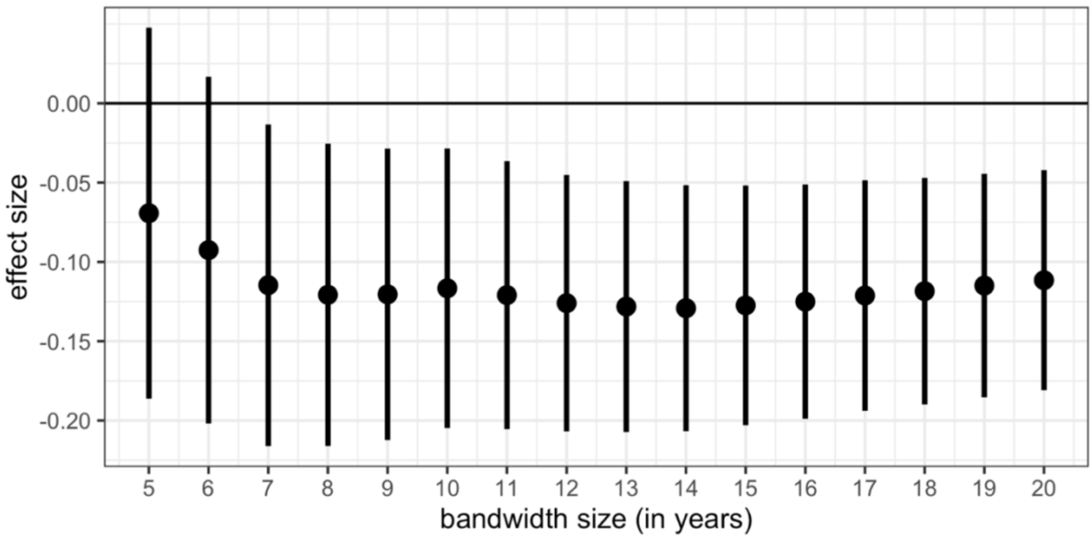
Note: The outcome variable is party alignment with the conservative party. Estimates are based the baseline specification with additional polynomial terms of the running variable. Columns differ with respect to the inclusion of the control variable set. *p<0.05; **p<0.01; ***p<0.001.

Table A1.3: The effect of eligibility on party alignment with conservative party with slope equality, additional polynomial terms as additional regressors and logit form

	Dependent variable: binary for alignment with conservative party					
	(1)	(2)	(3)	(4)	(5)	(6)
D	**-0.122***** **(0.036)**	**-0.143**** **(0.053)**	**-0.122***** **(0.036)**	**-0.120***** **(0.036)**	**-0.584***** **(0.167)**	**-0. 678**** **(0.246)**
Polynom. order	1	2	1	2	1	2
slope	separate	separate	same	same	separate	separate
Model Type	linear	linear	linear	linear	logit	logit
N	2770	2770	2770	2770	2770	2770
R^2	0.045	0.047	0.043	0.046	0.036	0.039

The outcome variable is party alignment with the conservative party. Estimates are based on the baseline specification with different modifications: column (1) is the baseline estimate from Table X; column (2), (4) and (6) include additional polynomial terms of the running variable; column (3) and (4) impose slope equality; column (5) and (6) use the logit as structural form, respectively. All estimations include the following set of control variables: age, binary for being married, number of children, no educational degree, household income, years of work experience (full- and part-time), and satisfaction with household income. Columns differ with respect to the number of polynomial terms being included. *p<0.05; **p<0.01; ***p<0.001.

Figure A1.4: The effect of eligibility on party alignment with conservative party with different bandwidth size parameters using non-parametric regression



Note: The outcome variable is party alignment with the conservative party. Vertical lines represent the 95% confidence interval around the coefficient of the treatment effect. All estimations include the following set of control variables: age, binary for being married, number of children, no educational degree, household income, years of work experience (full- and part-time), and satisfaction with household income.

Table A1.5: The effect of eligibility on turnout with different bandwidth size parameters

	Dependent variable: turnout in federal election in 2013							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D	**-0.031**	**-0.012**	**-0.021**	**0.005**	**0.007**	**0.001**	**0.006**	**0.010**
	(0.042)	**(0.040)**	**(0.037)**	**(0.034)**	**(0.032)**	**(0.031)**	**(0.030)**	**(0.029)**
(X-c)	0.002	0.001	0.002**	0.001	0.001*	0.001*	0.001*	0.001**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.0004)	(0.0004)	(0.0003)	(0.0003)
D*(X-c)	-0.001	-0.0003	-0.001	-0.00002	-0.00000	0.0001	-0.00005	-0.0001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.0005)	(0.0004)	(0.0004)
Constant	-0.997***	-1.103***	-1.203***	-1.177***	-1.146***	-1.085***	-1.094***	-1.144***
	(0.207)	(0.194)	(0.177)	(0.167)	(0.159)	(0.152)	(0.144)	(0.138)
Bandwidth	5 years	6 years	7 years	8 years	9 years	10 years	11 years	12 years
N	1248	1483	1769	2020	2280	2529	2790	3049
R^2	0.150	0.146	0.151	0.146	0.140	0.132	0.131	0.134

Note: The outcome variable is turnout during the federal election in 2013. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1 (X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. All estimations include the following set of control variables: age, binary for being married, number of children, no educational degree, household income, years of work experience (full- and part-time), and satisfaction with household income. Columns differ with respect to the bandwidth size. *p<0.05; **p<0.01; ***p<0.001.

Table A1.5 (cont'): The effect of eligibility on turnout with different bandwidth size parameters

	Dependent variable: turnout in federal election in 2013							
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
D	**0.0003**	**-0.006**	**-0.003**	**-0.001**	**-0.004**	**-0.010**	**-0.006**	**-0.013**
	(0.028)	**(0.027)**	**(0.026)**	**(0.025)**	**(0.025)**	**(0.024)**	**(0.024)**	**(0.023)**
(X-c)	0.001***	0.001***	0.001***	0.001***	0.001***	0.001***	0.001***	0.001***
	(0.0003)	(0.0003)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
D*(X-c)	-0.0001	-0.0001	-0.0001	-0.00002	0.0001	0.0001	0.0002	0.0002
	(0.0003)	(0.0003)	(0.0003)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Constant	-1.119***	-1.148***	-1.165***	-1.133***	-1.173***	-1.216***	-1.241***	-1.258***
	(0.134)	(0.129)	(0.125)	(0.121)	(0.118)	(0.115)	(0.112)	(0.110)
Bandwidth	13 years	14 years	15 years	16 years	17 years	18 years	19 years	20 years
N	3272	3500	3721	3974	4185	4416	4582	4685
R^2	0.131	0.134	0.134	0.134	0.136	0.139	0.143	0.144

Note: The outcome variable is turnout during the federal election in 2013. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1 (X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. All estimations include the following set of control variables: age, binary for being married, number of children, no educational degree, household income, years of work experience (full- and part-time), and satisfaction with household income. Columns differ with respect to the bandwidth size. *p<0.05; **p<0.01; ***p<0.001

Do Pension Benefits Mobilize?

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Abstract

The elderly now represent one of the largest electoral groups and pensions constitute often the most expensive benefit program in advanced democracies. Yet, we still lack causal evidence on the effect of pension benefits on political behaviour. This article matches administrative pension payments data with voter file records of public-school teachers in Illinois to exploit the timing of a benefit-expanding reform in the late 1990s in a regression discontinuity design. We find that pension benefit expansions have a large, but temporary, mobilizing effect for democratic voters. The effect is mainly driven by individuals with lower salaries, suggesting that the prevalence of benefit-based policy feedbacks decreases with economic security. There are no mobilizing effects among republican or unaffiliated voters. Overall, the paper suggests that policy feedbacks are short-term, and their size strongly depends on voters' ideological and economic predisposition.

Introduction

Nearly 10% in the world is above 65 and this share is predicted to rise to 16.5% by 2050 (United Nations 2022). Population ageing comes with important implications. From a political perspective, the rising share and their high rates of political participation make elderly voters an essential and powerful constituency in advanced democracies (Blais 2004; Tepe and Vanhuysse 2009; Bhatti and Hansen 2012; Tawfik et al. 2012; Vlandas et al. 2021). From on economic perspective, since the elderly depend on pension benefits, old-age transfer programs are the most expensive items on public balance sheets in many developed welfare states (OECD 2017). In other words, while reforming old-age benefit systems are imperative to ensure fiscal sustainability in the long-run (Barr and Peter Diamond 2008), changing benefit levels of a growing group of voters can entail large electoral implication for sitting governments.

To understand how aging shapes political and policy outcomes in democratic countries, the literature on the politics of pension benefits and reforms is one of the most extensively researched domains within the welfare state literature (Pierson 1994; Weaver 1998; Mulligan and Sala-i-Martin 1999; Pierson 2001; Myles and Pierson 2001; Bonoli 2003; Lynch 2006; Bonoli and Palier 2007; Weaver 2010; Ebbinghaus 2010; Ebbinghaus 2011; Ebbinghaus and Gronwald 2011). However, in contrast to other types of social policies, we know relatively little about the impact of pension benefits on the political behaviour of affected voters. While in principle, changing the benefits for such a large and powerful group of voters within the electorate may affect their voting behaviour, we currently have no causally identified study exploring this question.

With the present paper, we aim to contribute to the literature on the policy-voting nexus by measuring the effect of pension benefit expansions on political participation. To do so, we utilize a natural experiment in the US state Illinois in the late 1990s to estimate the effect of pension benefits on electoral participation. Hereby, we create a unique dataset matching Illinois' pension payment information with voter registration data of public-school teachers which provides information on individuals' income level before retirement, their party affiliation, as well as their voting history since 1992. Our natural experiment then leverages a policy design feature of one of the largest public pension benefit expansions in the history of Illinois: in the late 1990s, a major pension reform changed the system of the annual multiplier of public-school teachers which led to a substantive expansion of their retirement benefits. The benefit expansion, however, only affected teachers who were in employment after the reform became effective on 01 July 1998, while teachers who have retired before this date were not eligible for the benefit increase. Since we have the exact date of retirement and the benefit level, we utilise the timing of the reform and teachers' retirement date in a discontinuity-based research design to estimate the causal effect of the pension benefit expansion on political participation. In addition, we measure the longevity of the mobilizing effect of the pension expansion and test whether the effect varies with individuals' economic security and ideological predisposition.

We develop a simple theoretical framework specifying how the effect of greater pension generosity on political mobilization depends on both the material and ideological position of voters. Our empirical analyses yield four key findings about the impact of pension benefits on electoral participation. First, we find a positive causal effect on turnout among democratic voters in the immediate aftermath of the reform. The effect is sizeable: turnout in the first post-reform election (four months after reform implementation) was 18.8% larger for the treatment group than the control group. Thus, this finding provides causal evidence that expanding pension entitlements does substantially increase the political participation of benefiting democratic retirees. This evidence highlights that resource-based explanations of civic engagement are still viable ways to understand political participation in addition to anger-based approaches which are popular in the literature on voting behaviour in US (Valentino et a 2011; Weber 2013; Cramer 2016; MacWilliams 2016; Tolchin 2018; Valentino et al. 2018).

Second, our results show that the mobilizing effect of benefit expansions among democratic voters is mainly driven by voters with lower salaries. This corroborates an economic security mechanism where those who are more economically deprived are most strongly affected in their political participation by greater welfare state benefits. The size of policy feedbacks channelled via the mass public therefore depends more on the marginal benefit individual voters receive from a program than on their absolute economic value. Our finding echoes an income-contingent effect that has been identified in the literature on public opinion and political behaviour (Rosenstone and Hansen 1993; Campbell 2002; Schafer et al. 2022).

Third, even among democratic voters the significance of the turnout effect disappears within two to four years after reform implementation. Thus, the participatory implications of pension benefit expansions are temporary and roughly equal to one electoral cycle in the US. A long-term positive feedback process between welfare state expansion and greater political participation appears unlikely, in line with previous empirical studies emphasising the transitory effects of social policies on political behaviour (e.g. Zucco 2013; Lü 2014; Zimmermann 2021).

Fourth, in contrast to democratic voters, we do not find a significant mobilizing effect among republican or unaffiliated voters – independent of the level of economic security of an individual pensioneer. Thus, prior ideological predisposition crucially shapes the political gains that parties derive from welfare state expansion. This finding is in line with previous findings highlighting that the political behaviour in response to and public opinion of policy outcomes is itself a function of partisanship.

We present a vast range of placebo tests and sensitivity checks to demonstrate the robustness of our findings. We conduct placebo tests using (i) alternative dates as placebo cut-off values, (ii) non-teacher public employees as placebo treatment group, and (iii) pre-treatment elections as placebo periods. Our baseline estimate is also robust to (i) the inclusion of high-order polynomials of the forcing variable and (ii) different choices of the bandwidth size. In general, these placebo tests and sensitivity checks support the validity of our methodological approach.

Overall, this paper contributes to our understanding of benefit-based policy feedbacks. Our analysis shows that redistributive measures prior to elections can have large participatory consequences. However, since benefit expansions have only transitory mobilizing effects and depend both on individuals' pre-existing ideological and economic stance, welfare state expansions may not be effective to mobilize voters in the longer run.

The remainder of the paper is structured as follows. In section II, we review the literature on benefit-induced mobilization and develop a framework to illustrate how the mobilization effect of pension expansions depends on individuals' economic and ideological position. In section III, we provide details on the reform affecting teachers' pension benefits which we use as a natural experiment to address crucial endogeneity concerns. Section IV presents the dataset and the empirical strategy. In section V, we discuss our results about the mobilizing effects of pension benefits and examine how the magnitude varies over time and across individuals. Section VI concludes.

The effect of benefits on political participation

A large and valuable literature on policy feedbacks has analysed how institutions and policies shape political participation[21]. The investigation of the electoral implications of social policies has been a popular empirical focus in recent scholarship, with most of them finding rather large mobilizing effects. For instance, using a Supreme Court's decision in the US as natural experiment in a spatial discontinuity design, Clinton and Sances (2018) show that Medicaid expansion increased turnout in expanding states. De La O (2013) exploits a randomized component of a conditional cash transfer program in Mexico and in this way measures a substantive causal effect of receiving social benefits on electoral

[21] Political participation has been declining in many advanced democracies (Franklin 1999; Blais and Rubenson 2013; Dassonneville and Hooghe 2017; Kostelka and Blais 2021). Several possible explanations have been posited in political science, including the declining political interest among younger individuals (Blais 2000; Blais and Rubenson 2013), falling trust in institutions (Chanley et al. 2000; Cox 2003; Grönlund and Setälä 2007), changes in electoral competition (Franklin 2004; Engstrom 2012; Simonovits 2012), the move of left-wing parties to the centre (Azzolini and Evans 2020), and rising levels of inequality (Goodin and Dryzek 1980; Solt 2008; Anderson and Barmendi 2008), amongst others.

participation. Markovich and White (2022) match voting records with salary data of New York City municipality employees. By adopting a difference-in-difference design, the authors quantify a substantive participatory effect of a minimum wage increase among benefiting individuals. Further empirical evidence about the mobilizing effect of social policy and/or social benefit expansions came to similar conclusions (Baez et al. 2012; Layton and Smith 2015; Baicker and Finkelstein 2018; Imai et al. 2020; Löffler 2022).

Despite the important potential political consequences of pension systems, we still lack evidence on the causal effect of pension benefit expansions on political participation. This gap is problematic for two reasons. On the one hand, state pensions represent the largest welfare state expenditure in many advanced economies (OECD 2017). Indeed, OECD countries spend on average nearly 8% of GDP on old-age and survivor pension cash benefits and this share is expected to increase further as their population continue to age (OECD 2020). In comparison, OECD countries have spent, on average, 8.8% of GDP on healthcare in 2019 (OECD 2021), implying that providing cash benefits to pensioneers is almost as extensive for the welfare state as providing public health insurance to the whole population.

On the other hand, in aging societies, the rising share of elderly voters constitutes a powerful group of the electorate (‘grey power’) which can play a decisive role in winning and/or swinging elections (Goerres 2007, 2008, 2009; Busemeyer et al. 2008; Tepe and Vanhuysse 2009; Vlandas 2018). For instance, in the US, the share of the elderly (65+) increased from 9.8% in 1970 to 17.1% in 2022 (United Nations 2022). At the same time, the elderly are also politically more active. While the average participation rates in the

US presidential election in 2020 reached 67%, individuals over 65 years voted with a probability of 75% (US Census 2021). In contrast, while the elderly show higher engagement with politics, the opposite is true for contemporary younger cohorts which indicate lower rates of political participation than their parents and/or grandparents did at the same age (Dalton 2017). In other words, cohort effects may further elevate the political leverage of the elderly.

Thus, understanding the consequences of pension benefits on electoral participation can be a rewarding exercise particularly in times of aging societies. To model the causal effect of social benefits, long-standing scholarship about the origins and structure of public opinion and political behaviour depart from individuals' perception of a policy, which depends crucially on two dimensions, namely voters' (i) economic position and (ii) their ideological stance.

First, political scientists have provided ample theoretical and empirical evidence highlighting the role of economic endowments on both public opinion and political participation. Hereby, the Civic Voluntarism Model (Verba et al. 1995; Schlozman et al. 2001) postulates that, beyond three other components[22], (material) resources determine individual voting probability as citizens require economic endowments – such as time and money – to participate politically. Indeed, empirical analysis on the income-turnout gap – indicating that rich individuals show higher rates of electoral participation than poor voters – provides robust evidence about the mobilizing effect of material resources (Wolfinger and Rosenstone 1980; Filer et al. 1993; Verba et al. 1995; Solt 2008; Leighley

[22] Psychological engagement, recruitment, and issue engagement

and Nagler 2013; Kasara and Suryanarayan 2015). Yet, while the *level* of income has a positive impact on electoral participation, several studies have shown that *changes* in income appear to have diminishing marginal returns on turnout (Rosenstone and Hansen 1993; Campbell 2002; Schafer et al. 2022). In other words, while income shocks have a large impact on the political participation of the poor, the effect declines or even disappears when moving up the income distribution. When dealing with benefit-based policies that affect voters' economic endowments, it follows that there might be diminishing marginal returns to expanding social policies. Thus, the mobilizing effect of benefit expansions should be higher among individuals with lower income than for other voters with higher economic security.

Second, the perception of policies depends on voters' pre-determined ideological stance towards a legislative change and/or policy reform (Campbell et al. 1980; Shapiro 1992; Gelman 2009). In the last few decades, the effect of ideology on policy preferences has been well-documented and robust across several designs (Feldman and Zaller 1992; Alesina and Glaeser 2004; Baldassarri and Gelman 2008; Ellis and Faricy 2011; Enns and McAvoy 2012; Margalit 2013; Bolsen et al. 2014; Leeper and Slothuus 2014; Pearson-Merkowitz et al. 2016; Bisgaard 2019; Baldassarri and Park 2020). These dynamics are especially prevalent in the US, which is characterised by high levels of political polarization on policy issues along partisan lines. Indeed, in the context of social policies, previous analyses using individual-level surveys have shown that republican voters are less supportive to the public provision of social insurance and benefits compared to their democratic counterparts (Gramlich 2016; Doherty et al. 2016; Parker et al. 2019). Thus, beyond economic self-interest, it is important to consider voters' ideological predisposition when theorising the potential mobilizing effect of policy changes such as benefit expansions.

To illustrate how the mobilizing effect of benefiting voters varies along both an economic and an ideological axis, Table 1 distinguishes between these two dimensions to identify four types of voters. We expect the largest mobilizing effect among voters with low income *and* positive views of social benefits (group 4, lower-right quadrant) because the marginal benefit of expansions is large – in relative terms – for these individuals, while, at the same time, social benefits are in line with their policy preferences. In the extreme opposite case, we expect the least sizeable mobilization of benefit expansions for high-income voters with negative views on public welfare provision (group 1, upper-left quadrant). Whether benefit expansions actually de-mobilizes these voters is questionable, because, after all, even income rich individuals will nevertheless improve their economic position when receiving additional social benefits. However, their economic position means their relative net gains are minimal while they may be incurring ideological costs given their opposition to benefit expansion.

Moreover, individuals with either high income and positive views (group 2, upper-right quadrant) or low income and negative views (group 3, lower-left quadrant) are cross-pressured (Lipset 1959): while expansions may provide ideological (economic) value for these voters, their economic (ideological) position may nullify them. Thus, we only expect a clear mobilizing effect among individuals with low income *and* positive views about the welfare state, while other voters may be largely unresponsive to benefit expansions in (Table 1). Whether sociotropic ideological bias trumps more egotropic economic self-interest remains an open question within political science and so we do not have expectation about which dimensions will have greater empirical importance.

Table 1: Voter types by economic and ideological position towards social benefit expansions

		Ideological view of social benefits	
		negative	positive
Economic security	high	Group 1: weak/no de-mobilizing effect	Group 2: weak/no mobilizing effect
	low	Group 3: weak/no mobilizing effect	Group 4: mobilizing effect

Pension reforms in the US

The US is considered as one of the most liberal welfare state regimes and liberal economy among advanced economies (e.g. Esping-Andersen 1990, Hall and Soskice 2001, Hacker and Pierson 2005). Although the electorate in the US is, on average, less supportive of public welfare provision than in Europe (Alesina and Glaeser 2001, Pontusson 2005), legislation on the pension system is an intensely debated policy issue in the United States (Lynch 2006). Like other countries, the low birth rates and increasing life expectancy has put pension systems under immense fiscal pressure. With the Great Recession and the large associated losses in asset value this brought about, many states have adopted reforms in the aftermath of the financial crisis to close or at least dampen the funding gaps of public pension systems (Aubry and Crawford 2017). However, despite these legislative efforts, the average funding ratio (share of assets as percentage of liabilities) of state and local government pension systems of 53.8% (FED 2021) raises substantive concerns about the financial sustainability of retirement systems in all parts of the country (Sarfati and Ghellab 2012).

Among all US states, the public pension system of Illinois is the second most underfunded public retirement system[23] after New Jersey[24] (FED 2021). Pension experts have blamed the lack of reforms and past benefit expansions for the high unsustainability of the current public pension system. The issue is broadly known as Illinois' pension crisis. One of the state's largest benefit expansion reforms was the introduction of the 2.2 formula for members of the Chicago Teachers' Pension Fund (CTPF) and Teacher Retirement System (TRS) in 1998, under *Public Act 90-582* (Illinois General Assembly 1998). This reform is the focus of the present paper.

Public school teachers have been a powerful and sizeable group within Illinois' public sector for a long time[25]. Their interests are represented by different trade unions with the "Illinois Education Association" (IEA) with more than 135,000 members being the most influential labour organisation in the North-East state (Illinois Education Association 2022). Like other trade unions, the IEA campaigns for higher wages, improvement of

[23] The funding ratio of the public pension system in Illinois was 33.7% in 2019 (FED 2021).
[24] Amongst other reasons, commentators have argued that the low funding ratio of the public pension system is one of the reasons why Illinois has the lowest credit rating among all states (Moody's Investors Services 2021).
[25] At the time of the reform under consideration, 215,000 public school teachers were part of the public sector in the state of Illinois (Whelpley 1998).

working conditions, and increases in pension entitlements for retired public-school teachers, which has been one of their main policy objectives, especially before the 2000s (Illinois Education Association 2018).

In general, policymakers can adjust three different elevating screws of the benefit formula that increase or decrease the amount an individual receives after retirement: the number of years in service, the annual multiplier, and the maximum average salary the benefit calculation is based on (The Civic Federation 2015). Among these determinants, the annual multiplier plays a particularly important role in the overall size of the pension plan as it affects not just high-income individuals but the entire workforce. In essence, the annual multiplier is the percentage an employee collects for each year of employment. At retirement, the sum of these percentage points is then used to calculate the amount of pension benefits. For instance, a retirement system with a constant annual multiplier of 2% grants an employee 60% of her last salary earned if she has been employed/made pension contributions for 30 years.

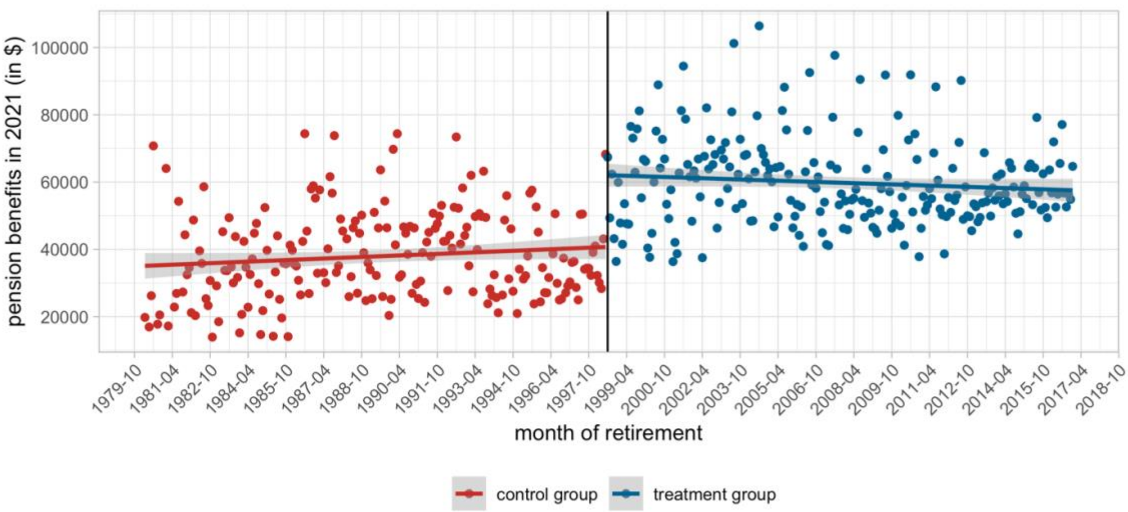
The IEA has promoted the increase of the annual multiplier of public-school teachers for two decades, arguing that Illinois' teachers should be entitled to receive a similar level of pension benefits as teachers in other states do (Illinois Education Association 2018). Before the reform, the formula for public-school teachers has been based on a stepwise annual multiplier that granted 1.67% for the first 10 years of service, "1.9% for each of the next 10 years, 2.1% for each of the following 10 years and 2.3% for each year above 30" (Chicago Teachers' Pension Fund 2021, p. 136). For example, a teacher with 31 years of service prior to 1998 has achieved a total multiplier of 59% (=(10x1.67%) +

(10x1.69%) + (10x1.9%) + (1x2.3%)) at the age of retirement. The IEA's plan entailed the replacement of this stepwise system with a flat-rate one that grants 2.2% for each year of employment, which grants, for example, 68.2% for 31 years of service. While the adoption of the "2.2 formula" was part of the IEA's agenda for a long time, it was not before the 1998 that teachers' unions and republican governor Jim Edgar reached a compromise on the adoption of the 2.2 formula under the *Public Act 90-582*. The law was signed on 27 May 1998 and became effective on 01 July 1998.

While the policy itself had large micro- and macro-level consequences for the public retirement system in Illinois, it was designed in a way that has created three different groups of teachers. First, teachers who have been hired on or after the implementation date: they became subject to the new flat rate of 2.2% for their entire career. Second, teachers who have retired before the adoption date: they remained subject to the old stepwise system and did not benefit from the pension expansion at all. And third, teachers who have been hired before the implementation date but continued to work after June 1998: they were compensated depending on when a given year of service has been earned. In detail, years of service earned prior to July 1998 are subject to the old system and years of service earned after June 1998 are subject to the new 2.2% flat rate.

However, in contrast to teachers who have retired before the reform, the third group of teachers (hired before July 1998 but retired afterwards) were granted an additional opportunity: they could buy an upgrade to transfer their years of service prior to the reform to the flat rate system. The "upgrade cost is determined by multiplying 1 percent times the number of years taught before July 1, 1998, by [the] highest salary rate during the four school years before [a teacher applies] to make the upgrade contribution" (Teachers' Retirement System 2017). However, this upgrade option was not available to all individuals but only to teachers who had either been employed on 01 July 1998 or have contributed – for at least one year – after July 1998. Thus, this upgrade opportunity enabled these individuals to stock up their pension benefits, but it was not available to teachers who have retired prior to the reform.

Figure 1: Median pension benefit level by month of retirement (treatment assignment plot)



Note: The figure shows the median annual benefit level of retirees in 2021 (y-axis) by the month of retirement (x-axis). The vertical line indicates 01 July 1998 as the cut-off date. The grey area indicates the 95% confidence interval.

The reform resulted in a large differential in pension benefits between employees who retired shortly before and shortly after the cut-off date. To illustrate the benefit gap between the old and the new multiplier system visually, Figure 1 displays the median benefit level of retired teachers in 2021 by the month of retirement for all CTPF (Chicago) and TRS (downstate/suburban) members who have retired with a minimum of 20 and a maximum of 34 years of service. The present paper exploits this quasi-random assignment of pension benefit expansion to measure its effect on political participation.

Data and method

Voter registration and pension payment data in Illinois

We utilize this reform from 1998 as a natural experiment to measure the effect of pension benefits on political participation. We use two administrative datasets, namely pension payment data in 2021 (Illinois Answers Project 2022), and the voter registration file as of September 2022 (Illinois State Board of Elections 2022).

The full pension dataset consists of 78,566 retired teachers, and it provides their full names accompanied by information on the benefit level, the date of retirement, the number of years of service, the occupation/name of the pension system, and the average salary of the last years in employment before retirement. Within the sample, 14.4% (12,333) have retired prior to 01 July 1998, while 85.6% (73,438) retired afterwards. From the full sample of retired teachers, we drop observations in three ways. First, since teachers in Illinois can only retire without discounted benefits if they have at least 20 years of service (Johnson and Southgate 2014), we remove all individuals who do not achieve this minimum service threshold. Second, the state of Illinois adopted several pension reforms in 1999 which affected teachers who have taught for more than 34 years (Teachers' Retirement System of the State of Illinois 2016). Thus, to ensure that we only estimate the effect of the adoption of the benefit expansion via the 2.2 formula in 1998, we remove all individuals from the sample who have worked for more than 34 years.

Third, since the pension payment data does not provide a unique identifier for all individuals, we remove all records that share the same first and last name. In summary, our sample includes 41,983 retired teachers as of 2021.

As a second data source, we acquired Illinois' voter file which consists of 8,772,691 voters as of October 2022 including voters' turnout history. The voter file provides information on voters' full name, their addresses[26], and the date of registration along with other demographic and political variables including age, gender, and party affiliation, amongst others. While the data also includes individuals who are inactive or who have been removed from the registry (e.g. due to death), we drop these observations as we have no information about the date of removal for these voters from the electoral. In summary, our sample includes 8,044,873 individuals registered as active voters[27].

We match the pension payment data with the voter files based on the first, middle (if available) and last name[28]. The matching can lead to three possible outcomes: (i) unique match, (ii) no match, and (iii) multiple voter matches. For the analysis, we can only use retirees who can be uniquely matched to voter records or who cannot be matched at all. We treat the ladder group as non-registered voters whose turnout binaries are equal to zero which follows the work by Nyhan et al. (2017) and Markovich and White (2022). In total, we receive approximately 49.2%, 29.3% and 21.5% for unique match, non-match,

[26] The street number is redacted due to data privacy regulation.
[27] as of October 2022
[28] To reach the highest possible matching quality based on the information that is available in both the voter and pension registry, we extract and clean[28] the single name components – first, middle and last name – in both datasets. Afterwards, for each pensioneer in the pension data, we search for a voter record for which the last name matches exactly in both datasets, and the retiree's first name is included in the first name of the electoral registry. If this results in multiple matches, we subset the remaining voter records further by matching the initial of the middle name – if it is available in both datasets.

and multiple matches, respectively. Overall, this leaves us with a sample of 63% registered and 37% non-registered individuals for Illinois[29], which is comparable to the matching/non-matching ratio of Markovich and White (2022).

In total, the matched dataset provides a sample of over 33 thousand voters. If we compare the full sample of retired teachers (before cleaning and filtering) with the final dataset we are using for the subsequent analysis, we can observe that the composition has only changed to a minor extent (see Table A1.1.1). Hereby, the most notable difference refers to the gender ratio: while the full dataset consisted of 27.9% male teachers, this ratio drops to 23.7% in the final dataset. In other words, our data analysis is based on a dataset that is slightly more female than the underlying population of all retired public-school teachers in Illinois. In contrast, other characteristics such as year of retirement, years of service and salary/benefit level are broadly similar (although the differences are statistically significant).

In addition, we also compare retired public-school teachers to the characteristics of the whole retired workforce of the public sector in Illinois. Overall, the descriptive statistics in Table A1.1.2 suggest that teachers have a higher female share, more years of service, and a higher salary/pension benefit level than the average public employee in the state of Illinois. Thus, even though teachers form one of the largest occupational groups in the US, it is important to note that our analysis is rather local in the sense that the estimates

[29] In 2020, Illinois recorded a ratio of registered individuals as a percentage of the eligible voting population of 74.4% (US Census 2021).

are based on a sample consisting of individuals that is, on average, more female and more economically secure than the retired public workforce in the state.

After matching and selecting individual pensioneers, we then construct several variables. First, we define election-specific turnout binaries which are equal to 1 if a voter participated in an election, and 0 otherwise, conditional on the registration date being smaller than the election date. For retired teachers who could not be matched with a voting record, the turnout variables are zero as we treat them as non-registered, non-voting individuals. Second, in addition to turnout binaries, we also construct registration binaries which are equal to 1 if an individual is registered to vote by the end of a given year, and 0 otherwise. Again, we treat unmatched retirees as unregistered voters and thus, their registration binaries are equal to 0. Third, we construct a gender binary which is equal to one if a voter identifies as male, and zero otherwise. For unmatched/unregistered retired individuals, we predict their gender based on their first name using the U.S. Social Security Administration baby name and U.S. Census datasets[30].

Fourth, we construct binaries for party affiliation based on the turnout history data. Since Illinois is a state with an open primary system which does not require an official affiliation with a specific party, we derive party ideology from individuals' turnout history in primary elections. To be more specific, an individual is affiliated with the Democratic party if the majority of her primary election votes have been cast at Democratic primary elections. We use the same assignment rule for republican and other (non-democratic and non-republican) voters. In addition, a retired teacher who has never participated in a

[30] This was done with the gender package in R using all data sources from the US.

primary election, who is not registered at all or who has an equal amount of democratic and republican primary participations is recorded as "unaffiliated". Our sample consists of 37.7% democratic, 27.3% republican, 31.8% unaffiliated, and 3.2% other voters.

In total, we end up with a sample that consists of 33,832 retired teachers as of 2021. Table 2 provides an overview about all variables that are used in the subsequent analysis along their respective summary statistics.

Table 2: Variables and summary statistics

Statistic	N	Min	Max	Mean	St. Dev.
Binary for male	32,924	0	1	0.237	0.425
Years of service	33,832	20	34	29.067	4.704
Year of retirement	33,832	1974	2021	2006	8.084
Year of voter registration	24,581	1907	2022	2001	17
Average salary during last years in employment (log)[31]	33,088	1.841	12.838	11.179	0.633
Annual pension benefits	33,832	7,716	351,676	69,432.140	28,251.100
Binary for Chicago teacher	33,832	0	1	0.151	0.358
Binary for affiliation with democratic party	33,832	0	1	0.377	0.485
Binary for affiliation with other parties	33,832	0	1	0.032	0.177
Binary for affiliation with republican party	33,832	0	1	0.273	0.445
Binary for affiliation with no party	33,832	0	1	0.318	0.466
Binary for turnout in 1992	21,232	0	1	0.538	0.499
Binary for turnout in 1994	23,175	0	1	0.513	0.500
Binary for turnout in 1996	25,589	0	1	0.624	0.484
Binary for turnout in 1998	25,421	0	1	0.593	0.491
Binary for turnout in 2000	27,489	0	1	0.686	0.464

[31] The final average salary of individuals in our sample "is calculated over teachers' 4 consecutive highest-compensated years of service during their final 10 service years." (Johnson and Southgate 2014, p.1).

Binary for turnout in 2002	27,049	0	1	0.635	0.481
Binary for turnout in 2004	28,919	0	1	0.706	0.456
Binary for turnout in 2006	28,441	0	1	0.644	0.479
Binary for turnout in 2008	30,021	0	1	0.716	0.451
Binary for turnout in 2010	29,557	0	1	0.670	0.470
Binary for turnout in 2012	30,640	0	1	0.715	0.452
Binary for turnout in 2014	30,455	0	1	0.673	0.469
Binary for turnout in 2016	31,592	0	1	0.717	0.450
Binary for turnout in 2018	31,979	0	1	0.695	0.460
Binary for turnout in 2020	33,065	0	1	0.721	0.449

Empirical approach and identification strategy

Our empirical analysis adopts a regression discontinuity design using the date of retirement. In other words, the running variable is the number of days an individual retired before (negative) or after (positive) the cut-off date 01 July 1998. As baseline **dependent variables**, we use election-specific turnout binaries which are equal to 1 if an individual has casted her vote in a specific election, and 0 otherwise – conditional on the person's registration date is on or before the election date. The baseline specification to predict turnout of individual $\boldsymbol{i}$ with partisanship $\boldsymbol{p}$ ∈ **P={democrat, republican, unaffiliated}** in election year $\boldsymbol{j}$ ∈ **J = {1992, 1994, …, 2018, 2020}** takes the following form:

$$\boldsymbol{Y_{i,P=p,J=j} = \alpha + \lambda D_i + \beta_1(X_i - c) + \beta_2 D(X_i - c) + \delta_K K_i + \epsilon_i}$$

with Y as the binary for turnout for individual i; D as the treatment variable; p as an indicator for party affiliation; X as the recoded date of retirement; c as the cut-off value – which is 0 on 01 July 1998; K as a vector of control variables with their respective coefficients (δ_K); and ϵ as the error term. The baseline set of control variables includes gender, years of service, log of average annual salary from last years of employment, and a binary for being a teacher in Chicago. The parameter of interest – the local average treatment effect (LATE) – is λ. The standard errors are robust towards heteroscedasticity with the degrees of freedom correction (MacKinnon and White 1985).

Furthermore, we use the method proposed by Calonico et al. (2014, 2018, 2019, 2020) to select the optimal bandwidth size prior and after the cut-off date algorithmically based on the mean squared error. Calonico et al. (2018) use higher-order distributional approximations to demonstrate that robust bias-corrected inference performs better than other available alternatives. The bandwidth selection procedure sets the bandwidth parameter based on a variance-bias optimum. In addition to these baseline estimates, we conduct two robustness checks, including (i) varying the bandwidth size and (ii) the inclusion of higher-order polynomial terms of the running variable.

We adopt a sharp RDD design because the reform design does not allow us to use a fuzzy one. One key pre-requisite of fuzzy RDD is that the treatment probability increases with proximity to the cut-off value. However, this is not true for the problem at hand because the date of retirement does not increase pensioneers' benefit they can reap from buying the upgrade. In contrast, the gain of the upgrade depends solely on the number of years worked prior to July 1998, not on the date of retirement itself. In other words, the upgrade opportunity is conditional on the date of retirement, but the benefit level itself does not increase with it. In fact, in our case, it is even more likely that individuals with later date of retirement would benefit less from the upgrade because later retirement means that these retirees have, on average, earned less years of service prior to July 1998. Thus, the likelihood of treatment, if anything, decreases with the forcing variable, which is why we do not adopt a fuzzy RDD approach. Therefore, we use a sharp instead of a fuzzy RDD approach.

Identifying assumptions

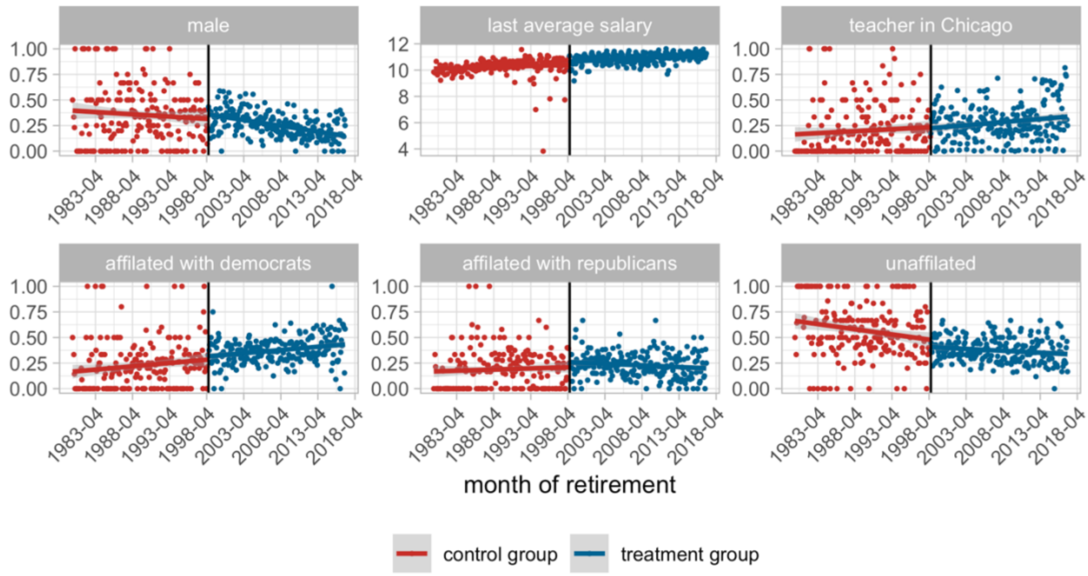
Before discussing the results, we test the identifying assumptions of the research design. Discontinuity-based strategies require two key assumptions to be true, namely (i) balance (continuity-in-observables), and (ii) absence of sorting. While none of these assumptions can be verified or falsified with certainty, several tests are standard when conducting RDD exercises. With respect to the balance assumption, we employ two tests. First, we inspect visually whether one of the control variables – apart from years of service which may be directly affected by the reform – breaks discontinuously around the cut-off. To do this, we plot the average of the control variables against the running variable. As Figure 2 shows, none of the control variables shows a clear discontinuity. Second, to back up these visual findings with a statistical model, we estimate the baseline by using one of the observables as a dependent variable at a time. Table 3 indicates that none of the treatment variables is significant which supports the validity of our empirical approach.

Table 3: Effect of pension benefit expansion on observables (test for continuity-in-observable assumption)

	Dependent variable:						
	Male	Salary	Chicago teacher	Democratic voter	Republican voter	Unaffiliated voter	Other voter
D	0.026	-0.003	0.056	0.020	-0.007	0.006	-0.009
	(0.029)	(0.048)	(0.029)	(0.030)	(0.026)	(0.029)	(0.009)
N	7098	9181	4850	9644	14515	16502	14620
BW_{left}	1790	2000	4850	2028	2603	3149	2698
BW_{right}	1790	2000	1434	2028	2603	3149	2698

Note: The outcome variable is one of the respective observables indicated at the top of each column. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable being used. All estimations include years of service binary for male, salary, years of service, and binaries for party affiliation as covariates exclusive the dependent variable being used. Robust standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

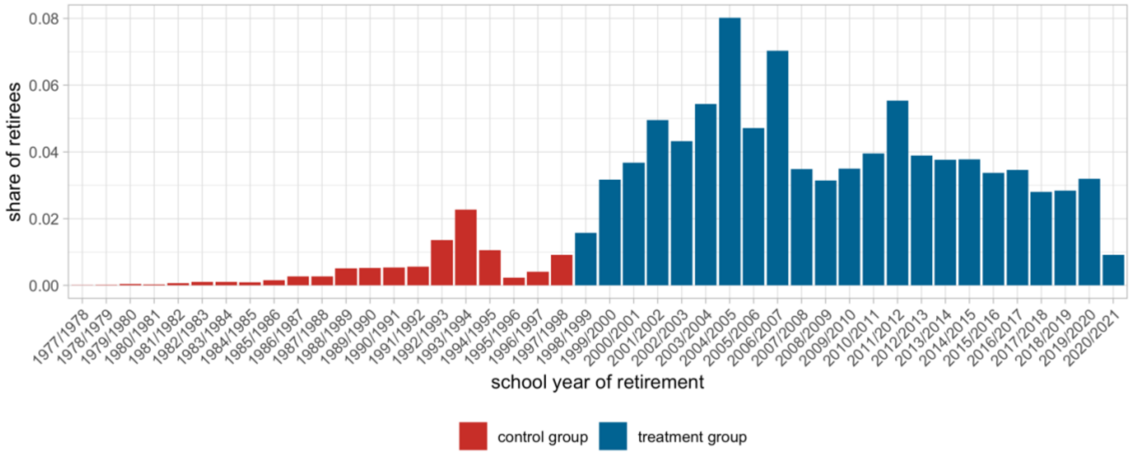
Figure 2: Mean level of covariates by month of retirement



Note: The panels show the average levels of selected observables in 2021 (y-axis) by the month of retirement (x-axis). The vertical line indicates 01 July 1998 as the cut-off date. The grey area indicates the 95% confidence interval.

With respect to the sorting assumption, it is common to plot the density along the running variable and conduct the McCrary (2008) bunching test. Figure 4 shows the number of new retired teachers increases from 1998 onwards. While the plot suggests a discontinuous break in the density and may indicate bunching/manipulation around the cut-off, this empirical phenomenon is, in our case, rather expected than surprising. Since the reform expanded pension payments, the marginal benefit of employment-based income decreases as the benefit expansion lowered the differential between teachers' salary and their expected pension benefits. In other words, with the introduction of the 2.2 formula, teachers are indirectly incentivized to leave the labour market earlier and move into retirement. Thus, density-based bunching tests are arguably not suitable to test strategic sorting and manipulation but they rather present indirect effects of the pension expansion itself.

Figure 4: Density plot along the running variable



Note: The figure plots the density along the school year of retirement. A school year is defined from 01 July until 30 June of a given year. The p-value of the McCrary density test based on the baseline specification (see above) is <0.05.

While we cannot provide statistical evidence supporting the absence of sorting around the cut-off, the timing of the reform process makes it nevertheless difficult for individual teachers to sort into treatment. Since the legislation was adopted only a little more than one month before the cut-off date, it is questionable whether teachers are willing to delay their planned retirement age on such short-term notice and work for another school year just to receive the opportunity to buy an upgrade. Since teacher contracts are usually aligned with the school year (in 2022/23: from 22 August 2022 to 9 June 2023), individuals are not able to gain access to the upgrade by working just a few months longer. In addition, the teacher pension systems need up to three months to process a retirement application before the first payment is transferred (Chicago Teachers' Pension Fund 2022). Thus, individuals who have planned to retire by June 1998 (after the school year 1997/98) should have applied for retirement by March/April 1998. However, at this point, the new 2.2 formula has not yet been adopted.

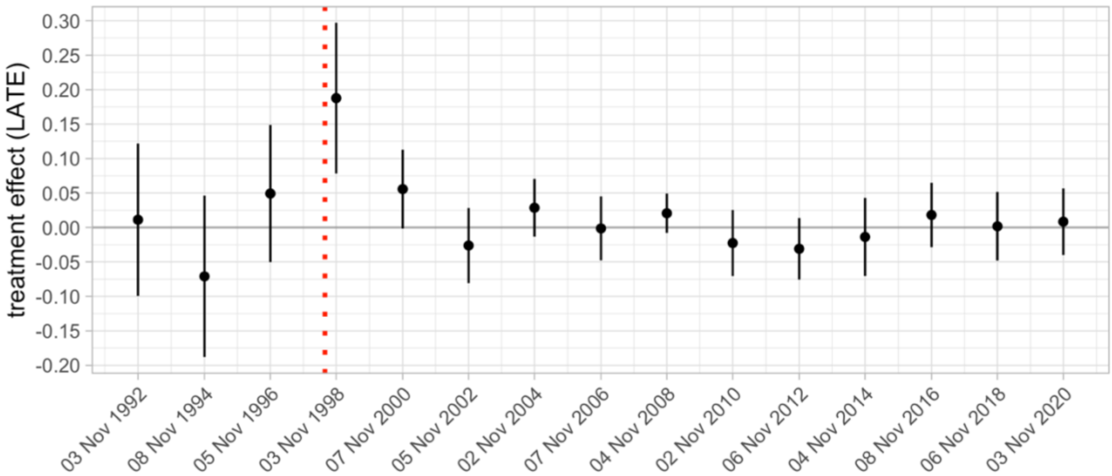
Results

Democratic voters

Based on our theoretical framework, we start our analysis with the sample of democratic voters who we assume to be more positive towards benefit expansions. As the estimates in Figure 5 show, the reform had a large positive, statistically significant impact on the electoral participation of democratic voters in the immediate aftermath of the legislative process. In detail, after the policy was adopted on 27 May 1998 and took effect on 01 July 1998, our estimates suggest that turnout was, on average, 18.8% higher among benefiting voters compared to their peers in the control group (see corresponding estimates in the tables in A2.1). This point estimate is large in economic terms and statistically highly significant. In addition, because the data-driven bandwidth selection of the coefficient for the November-1998 election resulted in a relatively small window (+/- 945 days), the estimate has a supposably high degree of internal validity.

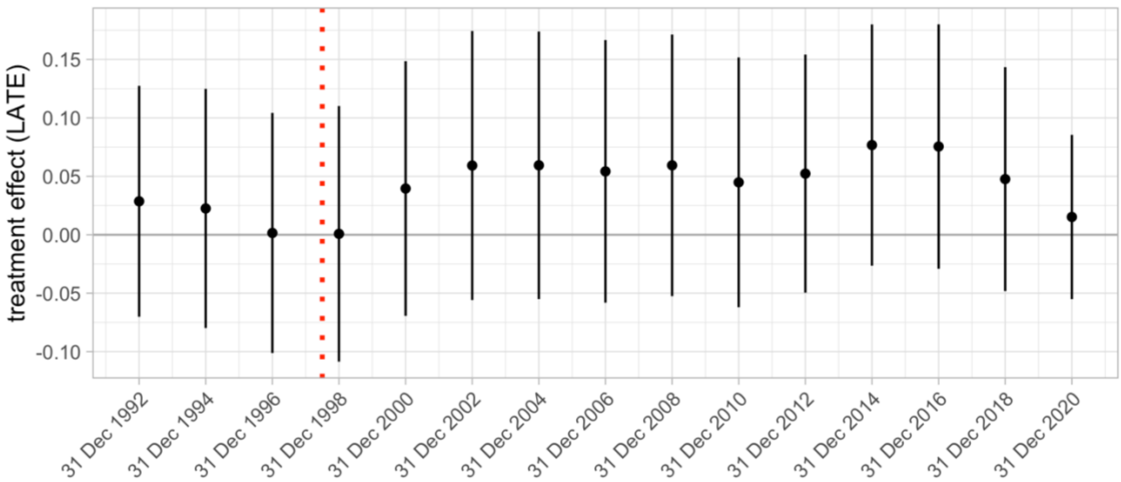
Beyond the first post-reform election, we observe that the effect size drops quickly within the following years. Even though the coefficients for these subsequent elections are more precisely estimated, none of them reaches conventional significance levels. The only exception here is the reform effect on turnout during the election in 2000, in which case the coefficient is statistically significant at the 10% level. These findings reveal an important pattern about the effect of benefit expansions on electoral participation among democratic voters: while such policies have a large mobilization effect in the short run, they are not able to bring electoral participation to a higher level in the medium to long run future. In addition, we do not find an effect on voter registration rates (Figure 6).

Figure 5: Effect of pension expansion on turnout across elections among democratic voters (baseline)



Note: The figure plots the local average treatment effect ($\boldsymbol{\lambda}$) (y-axis) for each election under consideration (x-axis). The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors. The dotted line (red) represents the implementation date of the pension expansion (01 July 1998).

Figure 6: Effect of pension benefit expansion on voting registration among democratic voters across election years



Note: The figure plots the local average treatment effect ($\boldsymbol{\lambda}$) (y-axis) for each election year under consideration (x-axis). The outcome variables are registration binaries for each full election year. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors. The dotted line (red) represents the implementation date of the pension expansion (01 July 1998).

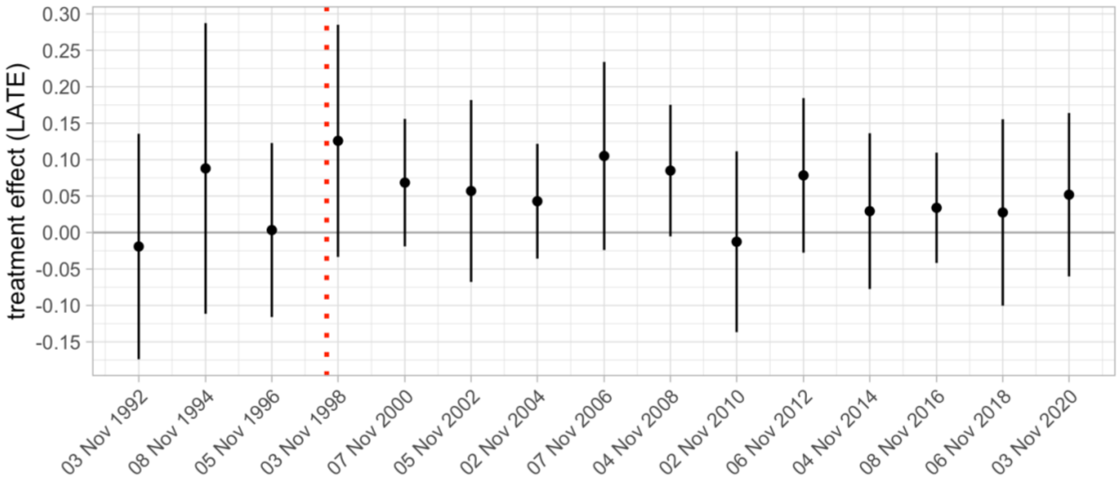
This general finding on turnout is robust to three placebo tests. **First**, as Figure 5 shows, we do not find a significant difference in turnout ratios between the treatment and control group for elections that have taken place prior to the reform. This placebo test strikes us as central as it backs the validity of our empirical approach. **Second**, it is common to test whether the results are robust to alternative cut-off values. The reasoning behind this is that the true cut-off is the only score value that causes a sudden change in the probability of receiving the treatment, so it should also be the only score value that causes a discontinuity in the outcome. When we shift the cut-off date to -10, -5, 5, 10 and 15 years before/after the factual cut-off date, the point estimates for the turnout in the election on 05 November 1998 are insignificant throughout specification (Table 4). Finally, **third**, since the pension records from Illinois do not just provide information on teachers but also on other professions which have not been affected by the policy, we can use them as placebo treatment group. We use non-teacher public employees as a placebo treatment group. Again, the results support the validity of our findings. As Figure 7 shows, none of the coefficients are statistically significant for any election under consideration.

Table 4: Effect of pension benefit expansion on turnout using placebo cut-off dates

	Dependent variable: turnout during election on 05 November 1998				
D	0.106	-0.006	0.015	-0.008	0.048
	(0.076)	(0.041)	(0.026)	(0.040)	(0.042)
N	135	821	4028	3110	1818
BW_{left}	1022	2231	1675	1257	871
BW_{right}	1022	2231	1675	1257	871
cutoff	-3650	-1825	1825	3650	5475

Note: The outcome variable is turnout for the election on 05 November 1998. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the selection of the placebo cut-off dates. Cut-off values refer to the number of days before (negative) or after (positive) 01 July 1998. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Robust standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

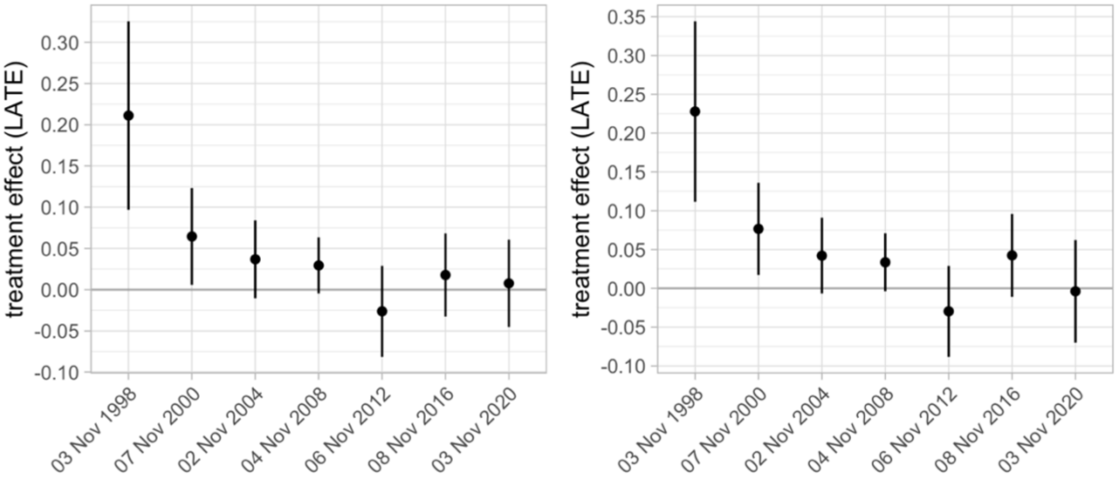
Figure 7: Effect of pension benefit expansion on turnout among democratic voters using non-teacher public employees as placebo treatment group



Note: The outcome variables are turnout binaries for different elections among non-teacher public employees. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1 (X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. All estimates include a binary for being male, salary, and years of service as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors. The dotted line (red) represents the implementation date of the pension expansion (01 July 1998).

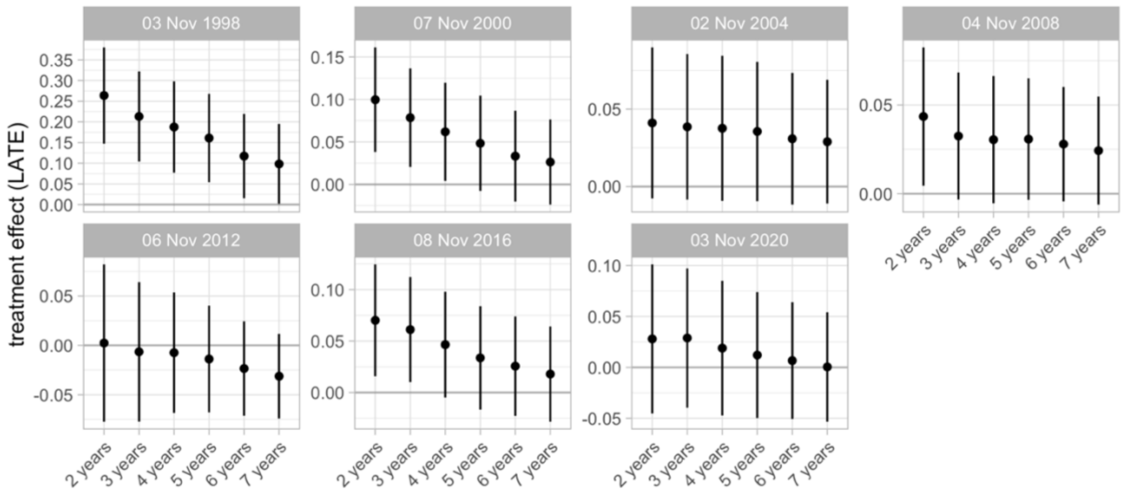
Beyond these placebo tests, we also test the robustness to two key statistical assumptions we have made in the baseline specification. On the one hand, it is common to test the robustness towards the structural form of the RDD model. Beyond the linearity assumption used in the baseline specification, we check how the results differ when we include further polynomial terms of the running variable on the right-hand side. As Figure 8 shows, changing the structural form of the estimation does not alter the conclusion we draw from the baseline results discussed above.

Figure 8: Effect of pension benefit expansion on turnout among democratic voters across elections with additional polynomial terms



Note: The figure plots the local average treatment effect ($\boldsymbol{\lambda}$) (y-axis) for each election under consideration (x-axis). The outcome variables are turnout binaries for different elections. The left (right) panel indicates the treatment effects across different elections with a second (and third) order polynomial term of the running variable as additional covariate. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors.

Figure 9: Effect of pension benefit expansion on turnout among democratic voters across elections with different bandwidth choices



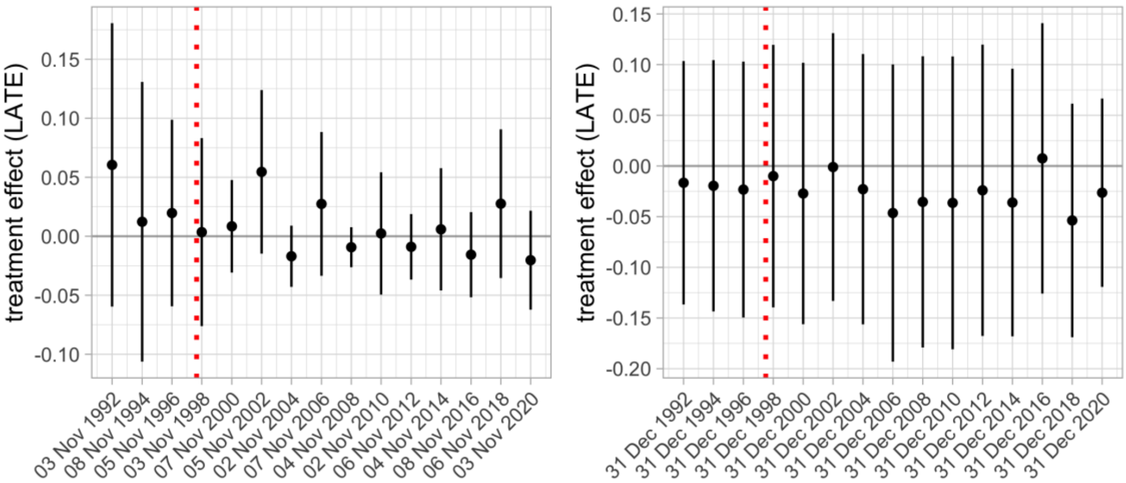
Note: The figure plots the local average treatment effect ($\boldsymbol{\lambda}$) (y-axis) for each election (panel) across different bandwidth choices (x-axis). The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1 (X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors.

On the other hand, one crucial assumption in discontinuity-based approaches concerns the selection of the bandwidth. While there is no unique way of deriving this central model parameter, scholars often resort to data-driven methods to select a bandwidth that finds an optimal variance-bias trade-off. For the baseline analysis, we have followed the approach by Calonico et al. (2022). However, to test whether alternative bandwidth choices lead to similar findings, we augment the window size around the cut-off date by several years. As Figure 9 shows, the coefficients for the first post-reform elections indicate that turnout among democratic voters is strongest in the immediate aftermath of the policy – independent of the bandwidth size – although we note that the size of the coefficient decreases with higher window sizes. In addition, the coefficients for the other elections are largely insignificant and substantially smaller – like the baseline has already suggested – compared to the 1998 election. If anything, the turnout differences between the treatment and control group may be significantly positive for the second post-reform election (presidential election in 2000) and the presidential election in 2016, especially when choosing a small bandwidth size.

Republican and unaffiliated voters

When we compare the turnout responsiveness between democratic and republican voters, we find strong differences. Figure 10 displays the estimates of the treatment effect on turnout ratios among republican voters for all elections since 1992 (see corresponding estimates in tables in A2.2.1, A2.2.2 and A2.2.3). As the results show, none of the estimates are statistically different from zero which suggests that the policy did not have a mobilizing or de-mobilizing effect on republican voters. We do also not observe a difference in registration rates. Overall, the insignificance of the treatment effect for turnout among republican voters is robust to the same validity and sensitivity tests we have performed on the democratic sample. The results of the placebo tests and robustness checks displayed in A2.2.4, A2.2.5, A2.2.6, and A2.2.7 support this finding.

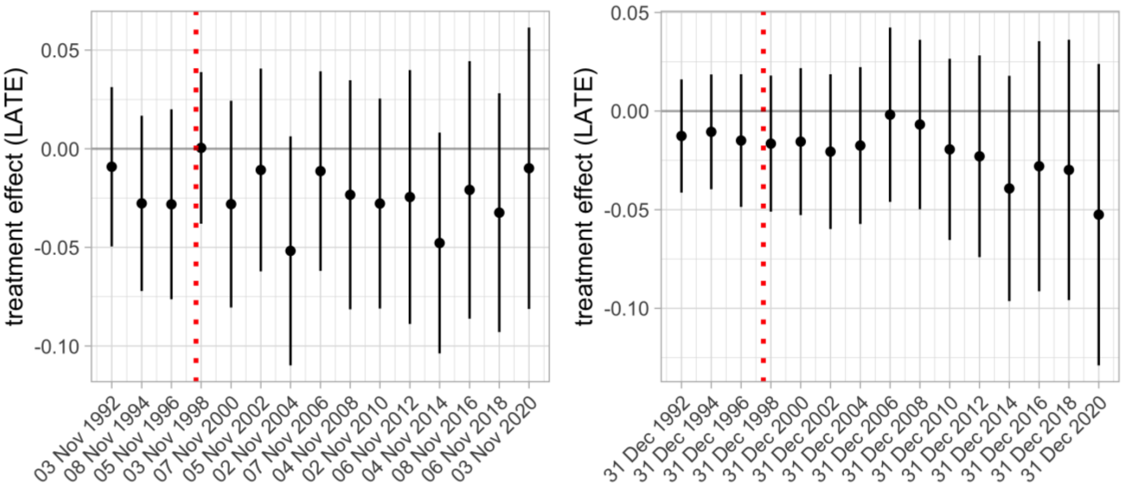
Figure 10: Effect of pension benefit expansion on turnout and voter registration among republican voters across elections



Note: The figure plots the local average treatment effect ($\boldsymbol{\lambda}$) (y-axis) for each election under consideration (x-axis). The outcome variables are turnout binaries for different elections (left panel) and registration binaries for each full election year (right panel). Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1 (X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors. The dotted line (red) represents the implementation date of the pension expansion (01 July 1998).

When we look at the effect size among unaffiliated and unregistered voters, we come to a similar conclusion: the policy did have neither a short- nor long-term effect on electoral participation among these individuals. This is true for both turnout and voter registration rates (Figure 11, see corresponding estimates in tables in A2.3.1, A2.3.2 and A2.3.3). Again, we perform the same placebo and statistical robustness checks to test the validity and statistical sensitivity of this null finding. The estimates displayed in A2.3.4, A2.3.5, A2.3.6, and A2.3.7 support this conclusion.

Figure 11: Effect of pension benefit expansion on turnout and voter registration among unaffiliated voters across elections



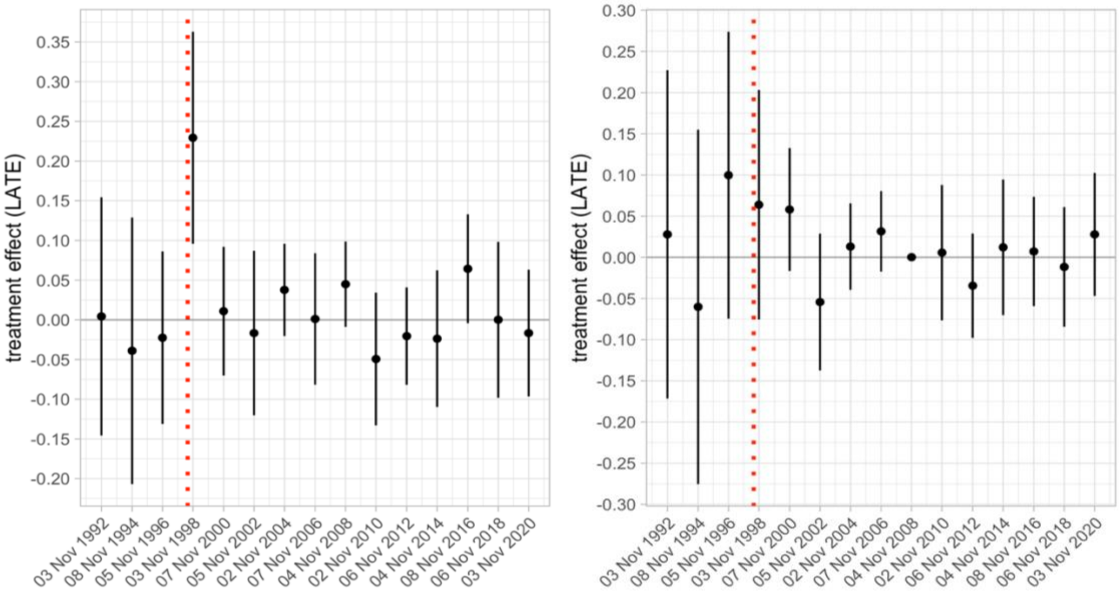
Note: The figure plots the local average treatment effect ($\boldsymbol{\lambda}$) (y-axis) for each election under consideration (x-axis). The outcome variables are turnout binaries for different elections (left panel) and registration binaries for each full election year (right panel). Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors. The dotted line (red) represents the implementation date of the pension expansion (01 July 1998).

Heterogeneity

In addition to these direct effects, one might ask whether the effect of benefit expansion on electoral participation varies by individuals' benefit/income levels. The question is particularly important as heterogeneity along individuals' salary levels can reveal whether the responsiveness depends on the absolute or relative value of the benefit expansion as previous results on the income-gradient in the public opinion/behaviour literature suggest (Rosenstone and Hansen 1993; Campbell 2002; Schafer et al. 2022).

Starting with the democratic sample, the results of the heterogeneity analysis shows that the short-term responsiveness towards the benefit increase is mainly driven by individuals at lower income levels (Figure 12). To be more specific, while we do not find a significant effect for individuals with salaries above the median, the corresponding estimate for lower income peers is positive, sizeable, and statistically highly significant. This finding suggests that the effect size decreases with income levels, highlighting the relative relevance of pension benefits on turnout. Sensitivity tests for both sub-samples support this conclusion (Figure A3.1.1-A3.1.6).

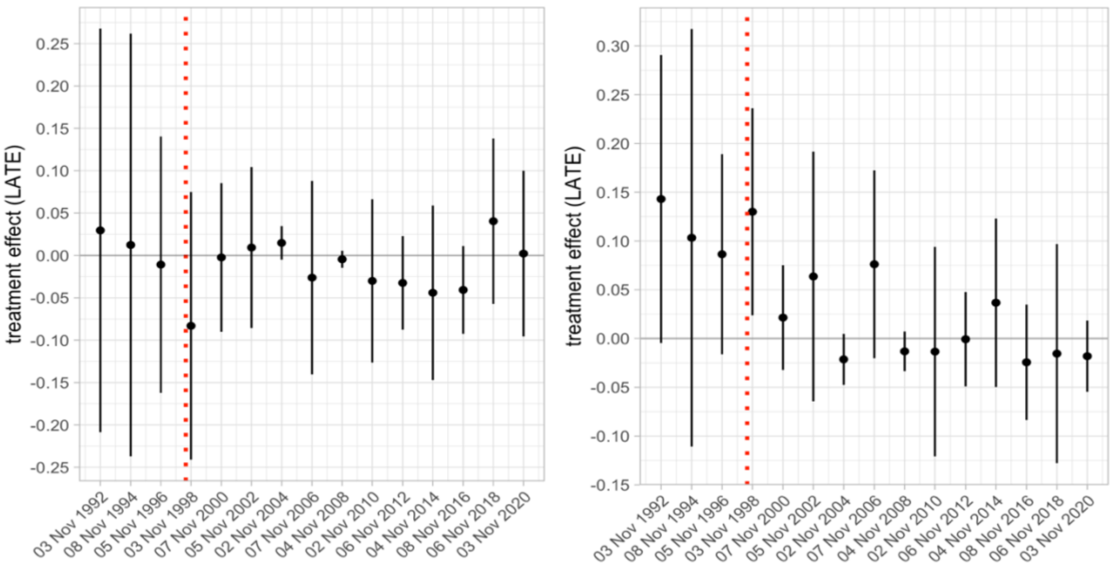
Figure 12: Effect of pension benefit expansion on turnout across elections among democratic voters below (left) and above median salary (right)



Note: The figure plots the local average treatment effect ($\boldsymbol{\lambda}$) (y-axis) for each election under consideration (x-axis). The outcome variables are turnout binaries for different elections. The estimates in the left (right) panel are based on individuals earning equal to/less (more) than the median of the average salary during the last years of employment. Estimates are based on $\boldsymbol{Y} = \boldsymbol{\alpha} + \boldsymbol{\lambda D} + \boldsymbol{\beta_1}(\boldsymbol{X} - \boldsymbol{c}) + \boldsymbol{\beta_2 D}(\boldsymbol{X} - \boldsymbol{c}) + \boldsymbol{\delta_K K} + \boldsymbol{\epsilon}$. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors. The dotted line (red) represents the implementation date of the pension expansion (01 July 1998).

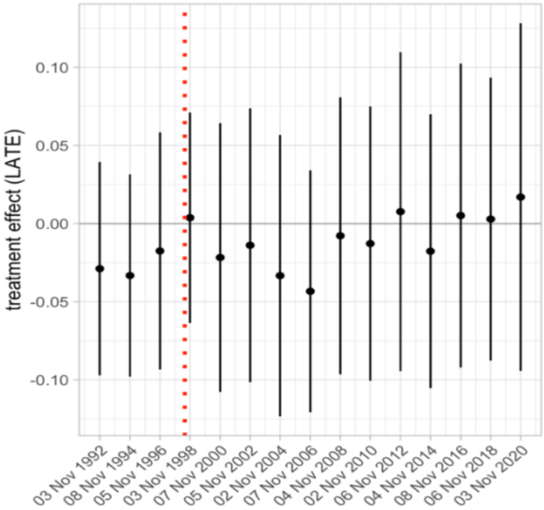
In contrast to democratic voters, we find some evidence that republican voters with salaries above the median have higher electoral participation in the immediate aftermath of the reform (Figure 13). However, this finding is not robust to neither the structural form nor the bandwidth size (Figure A3.2.1-A3.2.6). Thus, the result on the mediating effect of income remains inconclusive among republican voters. With respect to unaffiliated/unregistered voters, we find no difference across individuals' relative economic position (Figure 14). To be more specific, none of the estimates reaches conventional significance levels for any election or sub-sample under consideration. This finding is robust to several sensitivity tests (Figure A3.3.1-A3.3.6).

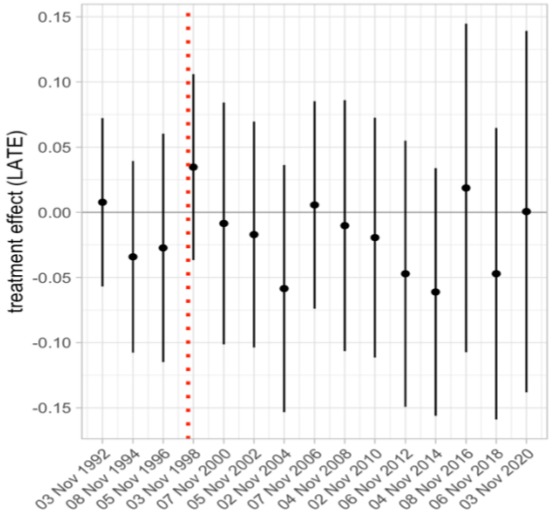
Figure 13: Effect of pension benefit expansion on turnout across elections among republican voters below (left) and above median salary (right)



Note: The figure plots the local average treatment effect ($\boldsymbol{\lambda}$) (y-axis) for each election under consideration (x-axis). The outcome variables are turnout binaries for different elections. The estimates in the left (right) panel are based on individuals earning equal to/less (more) than the median of the average salary during the last years of employment. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1 (X - c) + \beta_2 D (X - c) + \delta_K K + \epsilon}$. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors. The dotted line (red) represents the implementation date of the pension expansion (01 July 1998).

Figure 14: Effect of pension benefit expansion on turnout across elections among unaffiliated voters below (left) and above the median salary (right)





Note: The figure plots the local average treatment effect ($\boldsymbol{\lambda}$) (y-axis) for each election under consideration (x-axis). The outcome variables are turnout binaries for different elections. The estimates in the left (right) panel are based on individuals earning equal to/less (more) than the median of the average salary during the last years of employment. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1 (X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors.

Conclusion

The objective of this paper was to provide answers whether the expansion of pension benefits has mobilizing effects on benefiting retired voters. Understanding the political consequences of old-age social benefits is of great relevance, especially in times of demographic change when policymakers depend more and more on the electoral support of the augmenting group of elderly voters to win elections (Goerres 2007, 2008, 2009; Busemeyer et al. 2008; Tepe and Vanhuysse 2009; Vlandas 2018). In sum, our analysis has shown that pension benefit expansions have a large positive impact on electoral participation among democratic voters. This effect, however, is transitionary as it diminishes shortly after reform adoption. Thus, while an increase in generosity has indeed some mobilizing effects on left-wing voters, such expansions do not generate a consistent rise in electoral participation among benefiting individuals in the medium- to long-term in subsequent elections. Interestingly, we do not find an effect on turnout among republican or unaffiliated voters, neither in the short nor in the long run. Overall, our finding suggests that policy feedbacks related to welfare state expansions are conditional on the ideological position of the individual. Finally, the analysis has also shown that the positive effect among democrats is mainly driven by higher responsiveness among individuals with lower income. This ladder result highlights that integrating resource-based explanations into policy feedback frameworks is important to accurately model the mobilizing effects of social benefits.

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Appendix

A1 Descriptive Analysis

A1.1 Sample Characteristics

Table A1.1.1: Differences in observable characteristics between full and cleaned sample of retirees

Variable	mean in full sample	mean in our sample	p-value of difference
male	0.28	0.24	0.00
year of retirement	2006.97	2006.30	0.00
years of service	28.41	29.07	0.00
salary	77165.33	80426.24	0.00
pension benefit	67579.12	69432.14	0.00

Table A1.1.2: Differences in observable characteristics between teacher and non-teacher retirees

Variable	mean in full sample	mean in our sample	p-value of difference
male	0.44	0.28	0.00
year of retirement	2009.85	2006.97	0.00
years of service	22.29	28.41	0.00
salary	53979.39	77165.33	0.00
pension benefit	37504.70	67579.12	0.00

A2 Baseline Analysis

A2.1 Democratic Voters

Table A2.1.1: Effect of pension benefit expansion on turnout among democratic voters with robust standard errors

	Dependent variable: turnout in				
	1992	1994	1996	1998	2000
D	0.011	-0.071	0.049	0.188 ***	0.056
	(0.056)	(0.060)	(0.051)	(0.056)	(0.029)
N	1772	1952	1021	629	1030
BW_{left}	2185	2066	1403	945	1127
BW_{right}	2185	2066	1403	945	1127

Note: The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable being used. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Robust standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

Table A2.1.1: Effect of pension benefit expansion on turnout among democratic voters with robust standard errors

	Dependent variable: turnout in				
	2002	2004	2006	2008	2010
D	-0.026	0.029	-0.001	0.021	-0.023
	(0.028)	(0.021)	(0.024)	(0.015)	(0.024)
N	3738	2300	3152	3192	3281
BW_{left}	2747	1812	2419	2200	2459
BW_{right}	2747	1812	2419	2200	2459

Note: The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable being used. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Robust standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

Table A2.1.1: Effect of pension benefit expansion on turnout among democratic voters with robust standard errors

	Dependent variable: turnout in				
	2012	2014	2016	2018	2020
D	-0.031	-0.014	0.018	0.002	0.008
	(0.023)	(0.029)	(0.024)	(0.025)	(0.025)
N	2054	2678	2794	5205	4393
BW_{left}	1555	1902	1960	3232	2549
BW_{right}	1555	1902	1960	3232	2549

Note: The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable being used. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Robust standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

Table A2.1.2: Effect of pension benefit expansion on turnout among democratic voters with bias-corrected standard errors

	Dependent variable: turnout in				
	1992	1994	1996	1998	2000
D	0.011	-0.071	0.049	0.188 ***	0.056 *
	(0.048)	(0.050)	(0.045)	(0.052)	(0.027)
N	1772	1952	1021	629	1030
BW_{left}	2185	2066	1403	945	1127
BW_{right}	2185	2066	1403	945	1127

Note: The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1 (X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable being used. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Bias-corrected standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

Table A2.1.2: Effect of pension benefit expansion on turnout among democratic voters with bias-corrected standard errors

	Dependent variable: turnout in				
	2002	2004	2006	2008	2010
D	-0.026	0.029	-0.001	0.021	-0.023
	(0.024)	(0.017)	(0.021)	(0.013)	(0.021)
N	(0.028)	(0.021)	(0.024)	(0.015)	(0.024)
BW_{left}	3738	2300	3152	3192	3281
BW_{right}	2747	1812	2419	2200	2459

Note: The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1 (X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable being used. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Bias-corrected standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

Table A2.1.2: Effect of pension benefit expansion on turnout among democratic voters with bias-corrected standard errors

	Dependent variable: turnout in				
	2012	2014	2016	2018	2020
D	-0.031	-0.014	0.018	0.002	0.008
	(0.019)	(0.025)	(0.020)	(0.022)	(0.021)
N	2054	2678	2794	5205	4393
BW_{left}	1555	1902	1960	3232	2549
BW_{right}	1555	1902	1960	3232	2549

Note: The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1 (X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable being used. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Bias-corrected standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

Table A2.1.3: Effect of pension benefit expansion on turnout among democratic voters with conventional standard errors

	Dependent variable: turnout in				
	1992	1994	1996	1998	2000
D	0.003	-0.059	0.038	0.166 **	0.044
	(0.048)	(0.050)	(0.045)	(0.052)	(0.027)
N	1772	1952	1021	629	1030
BW_{left}	2185	2066	1403	945	1127
BW_{right}	2185	2066	1403	945	1127

Note: The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable being used. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Conventional standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

1Table A2.1.3: Effect of pension benefit expansion on turnout among democratic voters with conventional standard errors

	Dependent variable: turnout in				
	2002	2004	2006	2008	2010
D	-0.027	0.022	0.001	0.015	-0.018
	(0.024)	(0.017)	(0.021)	(0.013)	(0.021)
N	3738	2300	3152	3192	3281
BW_{left}	2747	1812	2419	2200	2459
BW_{right}	2747	1812	2419	2200	2459

Note: The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable being used. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Conventional standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

Table A2.1.3: Effect of pension benefit expansion on turnout among democratic voters with conventional standard errors

	Dependent variable: turnout in				
	2012	2014	2016	2018	2020
D	-0.023	-0.019	0.014	0.007	0.006
	(0.019)	(0.025)	(0.020)	(0.022)	(0.021)
N	2054	2678	2794	5205	4393
BW_{left}	1555	1902	1960	3232	2549
BW_{right}	1555	1902	1960	3232	2549

Note: The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable being used. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Conventional standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

A2.2 Republican Voters

Table A2.2.1: Effect of pension benefit expansion on turnout among republican voters with robust standard errors

	Dependent variable: turnout in				
	1992	1994	1996	1998	2000
D	0.060	0.012	0.020	0.003	0.008
	(0.061)	(0.060)	(0.040)	(0.041)	(0.020)
N	1654	2141	1938	1992	3557
BW_{left}	1912	2188	1854	2018	2901
BW_{right}	1912	2188	1854	2018	2901

Note: The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable being used. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Robust standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

Table A2.2.1: Effect of pension benefit expansion on turnout among republican voters with robust standard errors

	Dependent variable: turnout in				
	2002	2004	2006	2008	2010
D	0.055	-0.017	0.027	-0.009	0.002
	(0.035)	(0.013)	(0.031)	(0.009)	(0.026)
N	1609	1683	2307	641	1824
BW_{left}	1733	1527	2102	885	1779
BW_{right}	1733	1527	2102	885	1779

Note: The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable being used. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Robust standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

Table A2.2.1: Effect of pension benefit expansion on turnout among republican voters with robust standard errors

	Dependent variable: turnout in				
	2012	2014	2016	2018	2020
D	-0.009	0.006	-0.016	0.028	-0.020
	(0.014)	(0.026)	(0.018)	(0.032)	(0.021)
N	3790	3032	1868	3103	1132
BW_{left}	2615	2406	1602	2415	1396
BW_{right}	2615	2406	1602	2415	1396

Note: The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable being used. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Robust standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

Table A2.2.2: Effect of pension benefit expansion on turnout among republican voters with bias-corrected standard errors

	Dependent variable: turnout in				
	1992	1994	1996	1998	2000
D	0.060	0.012	0.020	0.003	0.008
	(0.053)	(0.052)	(0.034)	(0.034)	(0.017)
N	1654	2141	1938	1992	3557
BW_{left}	1912	2188	1854	2018	2901
BW_{right}	1912	2188	1854	2018	2901

Note: The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1 (X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable being used. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Bias-corrected standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

Table A2.2.2: Effect of pension benefit expansion on turnout among republican voters with bias-corrected standard errors

	Dependent variable: turnout in				
	2002	2004	2006	2008	2010
D	0.055	-0.017	0.027	-0.009	0.002
	(0.031)	(0.011)	(0.027)	(0.008)	(0.022)
N	1609	1683	2307	641	1824
BW_{left}	1733	1527	2102	885	1779
BW_{right}	1733	1527	2102	885	1779

Note: The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1 (X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable being used. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Bias-corrected standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

Table A2.2.2: Effect of pension benefit expansion on turnout among republican voters with bias-corrected standard errors

	Dependent variable: turnout in				
	2012	2014	2016	2018	2020
D	-0.009	0.006	-0.016	0.028	-0.020
	(0.012)	(0.023)	(0.016)	(0.028)	(0.018)
N	3790	3032	1868	3103	1132
BW_{left}	2615	2406	1602	2415	1396
BW_{right}	2615	2406	1602	2415	1396

Note: The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1 (X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable being used. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Bias-corrected standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

Table A2.2.3: Effect of pension benefit expansion on turnout among republican voters with conventional standard errors

	Dependent variable: turnout in				
	1992	1994	1996	1998	2000
D	0.050	0.006	0.020	0.003	0.008
	(0.053)	(0.052)	(0.034)	(0.034)	(0.017)
N	1654	2141	1938	1992	3557
BW_{left}	1912	2188	1854	2018	2901
BW_{right}	1912	2188	1854	2018	2901

Note: The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable being used. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Conventional standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

Table A2.2.3: Effect of pension benefit expansion on turnout among republican voters with conventional standard errors

	Dependent variable: turnout in				
	2002	2004	2006	2008	2010
D	0.044	-0.014	0.021	-0.007	0.003
	(0.031)	(0.011)	(0.027)	(0.008)	(0.022)
N	1609	1683	2307	641	1824
BW_{left}	1733	1527	2102	885	1779
BW_{right}	1733	1527	2102	885	1779

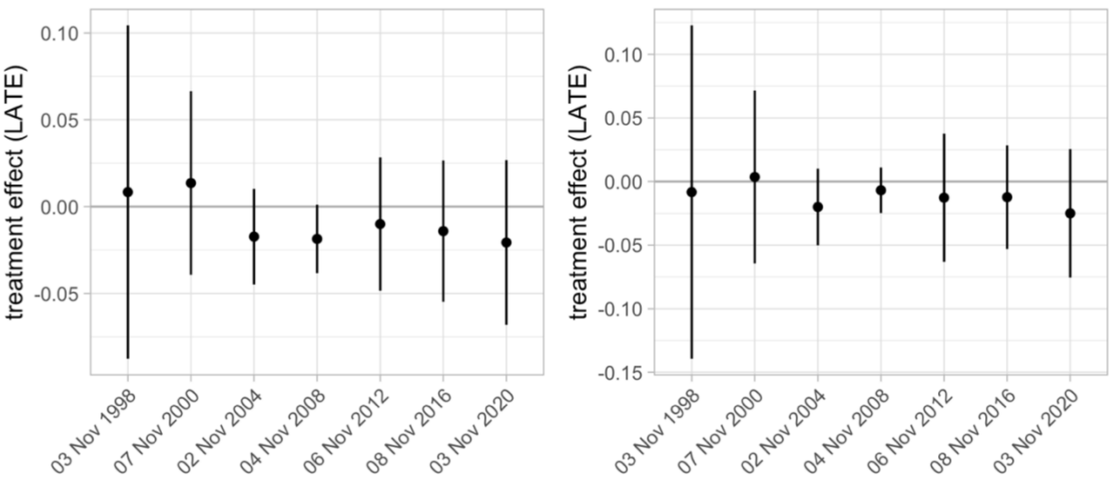
Note: The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable being used. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Conventional standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

Table A2.2.3: Effect of pension benefit expansion on turnout among republican voters with conventional standard errors

	Dependent variable: turnout in				
	2012	2014	2016	2018	2020
D	-0.007	0.008	-0.013	0.029	-0.019
	(0.012)	(0.023)	(0.016)	(0.028)	(0.018)
N	3790	3032	1868	3103	1132
BW_{left}	2615	2406	1602	2415	1396
BW_{right}	2615	2406	1602	2415	1396

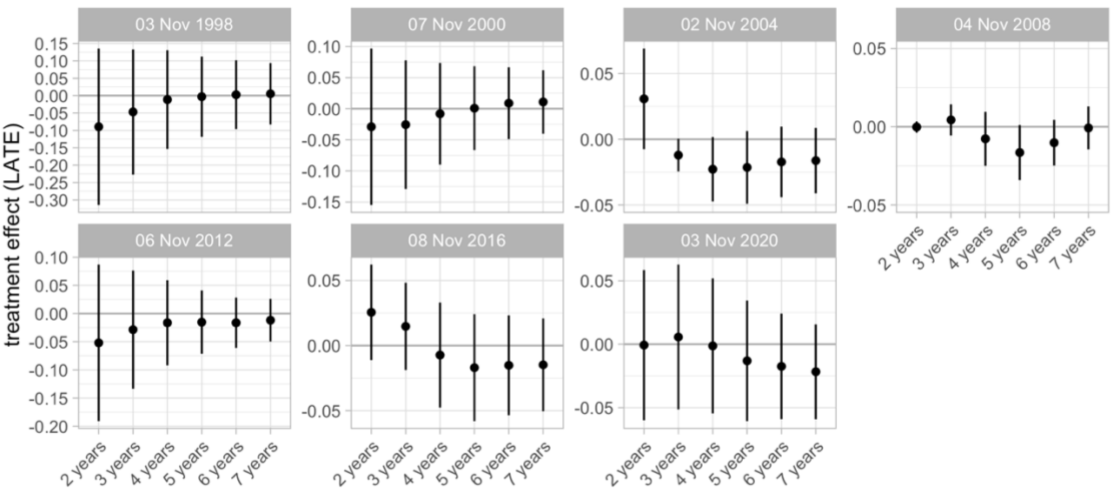
Note: The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable being used. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Conventional standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

Figure A2.2.4: Effect of pension benefit expansion on turnout among republican voters across elections with additional polynomial terms



Note: The figure plots the local average treatment effect (λ) (y-axis) for each election under consideration (x-axis). The outcome variables are turnout binaries for different elections. The left (right) panel indicates the treatment effects across different elections with a second (and third) order polynomial term of the running variable as additional covariate. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors.

Figure A2.2.5: Effect of pension benefit expansion on turnout among republican voters across elections with different bandwidth choices



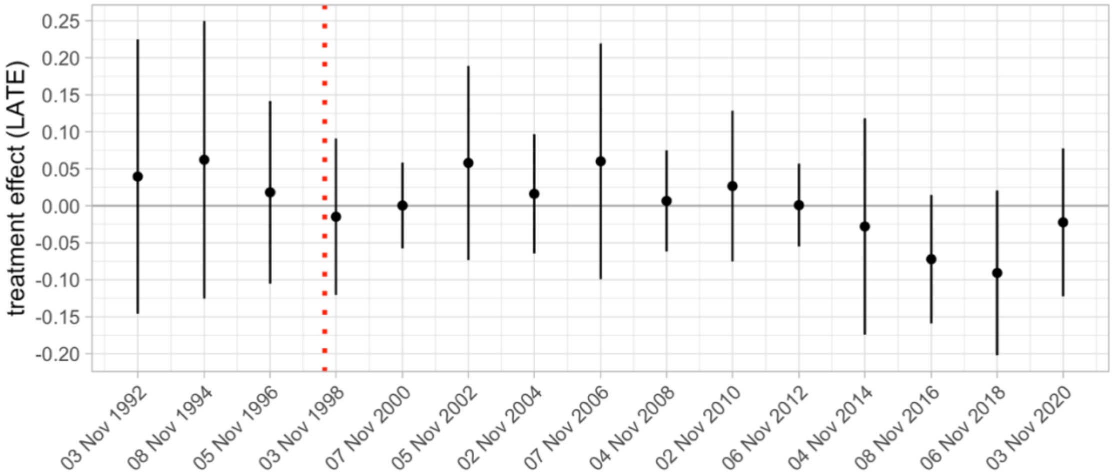
Note: The figure plots the local average treatment effect (λ) (y-axis) for each election (panel) across different bandwidth choices (x-axis). The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors.

Figure A2.2.6: Effect of pension benefit expansion on turnout among republican voters using placebo cut-off dates

	Dependent variable: turnout during election on 05 November 1998				
D	0.024	-0.019	0.007	-0.042	-0.009
	(0.055)	(0.040)	(0.029)	(0.072)	(0.060)
N	282	821	3082	1278	1266
BW_{left}	1297	2055	1613	877	994
BW_{right}	1297	2055	1613	877	994
cutoff	-3650	-1825	1825	3650	5475

Note: The outcome variable is turnout for the election on 05 November 1998. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the selection of the placebo cut-off dates. Cut-off values refer to the number of days before (negative) or after (positive) 01 July 1998. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Robust standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

Figure A2.2.7: Effect of pension benefit expansion on turnout among republican voters using non-teacher public employees as placebo treatment group



Note: The outcome variables are turnout binaries for different elections among non-teacher public employees. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. All estimates include a binary for being male, salary, and years of service as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors. The dotted line (red) represents the implementation date of the pension expansion (01 July 1998).

2.3 Unaffiliated Voters

Table A2.3.1: Effect of pension benefit expansion on turnout among unaffiliated voters with robust standard errors

	Dependent variable: turnout in				
	1992	1994	1996	1998	2000
D	-0.009	-0.028	-0.028	0.000	-0.028
	(0.021)	(0.023)	(0.025)	(0.020)	(0.027)
N	2554	3076	3123	3113	3164
BW_{left}	2058	2379	2383	2471	2387
BW_{right}	2058	2379	2383	2471	2387

Note: The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1 (X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable being used. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Robust standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

Table A2.3.1: Effect of pension benefit expansion on turnout among unaffiliated voters with robust standard errors

	Dependent variable: turnout in				
	2002	2004	2006	2008	2010
D	-0.011	-0.052	-0.011	-0.023	-0.028
	(0.026)	(0.030)	(0.026)	(0.030)	(0.027)
N	3109	2654	2643	3063	3216
BW_{left}	2271	1923	2029	2187	2316
BW_{right}	2271	1923	2029	2187	2316

Note: The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1 (X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable being used. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Robust standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

Table A2.3.1: Effect of pension benefit expansion on turnout among unaffiliated voters with robust standard errors

	Dependent variable: turnout in				
	2012	2014	2016	2018	2020
D	-0.024	-0.048	-0.021	-0.032	-0.010
	(0.033)	(0.029)	(0.033)	(0.031)	(0.036)
N	2547	3291	2831	3417	3596
BW_{left}	1840	2381	2092	2307	2352
BW_{right}	1840	2381	2092	2307	2352

Note: The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1 (X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable being used. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Robust standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

Table A2.3.2: Effect of pension benefit expansion on turnout among unaffiliated voters with bias-corrected standard errors

	Dependent variable: turnout in				
	1992	1994	1996	1998	2000
D	-0.009	-0.028	-0.028	0.000	-0.028
	(0.018)	(0.020)	(0.022)	(0.017)	(0.023)
N	2554	3076	3123	3113	3164
BW_{left}	2058	2379	2383	2471	2387
BW_{right}	2058	2379	2383	2471	2387

Note: The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1 (X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable being used. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Bias-corrected standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

Table A2.3.2: Effect of pension benefit expansion on turnout among unaffiliated voters with bias-corrected standard errors

	Dependent variable: turnout in				
	2002	2004	2006	2008	2010
D	-0.011	-0.052	-0.011	-0.023	-0.028
	(0.023)	(0.027)	(0.022)	(0.027)	(0.024)
N	3109	2654	2643	3063	3216
BW_{left}	2271	1923	2029	2187	2316
BW_{right}	2271	1923	2029	2187	2316

Note: The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1 (X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable being used. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Bias-corrected standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

Table A2.3.2: Effect of pension benefit expansion on turnout among unaffiliated voters with bias-corrected standard errors

	Dependent variable: turnout in				
	2012	2014	2016	2018	2020
D	-0.024	-0.048	-0.021	-0.032	-0.010
	(0.029)	(0.025)	(0.029)	(0.027)	(0.031)
N	2547	3291	2831	3417	3596
BW_{left}	1840	2381	2092	2307	2352
BW_{right}	1840	2381	2092	2307	2352

Note: The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1 (X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable being used. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Bias-corrected standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

Table A2.3.3: Effect of pension benefit expansion on turnout among unaffiliated voters with conventional standard errors

	Dependent variable: turnout in				
	1992	1994	1996	1998	2000
D	-0.006	-0.024	-0.024	0.003	-0.024
	(0.018)	(0.020)	(0.022)	(0.017)	(0.023)
N	2554	3076	3123	3113	3164
BW_{left}	2058	2379	2383	2471	2387
BW_{right}	2058	2379	2383	2471	2387

Note: The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable being used. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Conventional standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

Table A2.3.3: Effect of pension benefit expansion on turnout among unaffiliated voters with conventional standard errors

	Dependent variable: turnout in				
	2002	2004	2006	2008	2010
D	-0.007	-0.042	-0.006	-0.015	-0.021
	(0.023)	(0.027)	(0.022)	(0.027)	(0.024)
N	3109	2654	2643	3063	3216
BW_{left}	2271	1923	2029	2187	2316
BW_{right}	2271	1923	2029	2187	2316

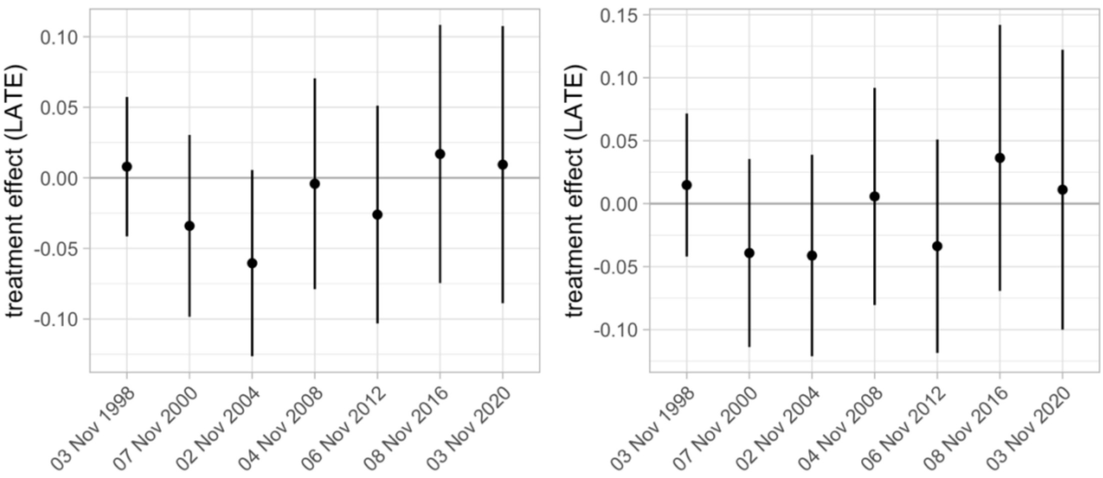
Note: The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable being used. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Conventional standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

Table A2.3.3: Effect of pension benefit expansion on turnout among unaffiliated voters with conventional standard errors

	Dependent variable: turnout in				
	2012	2014	2016	2018	2020
D	-0.017	-0.041	-0.013	-0.026	-0.008
	(0.029)	(0.025)	(0.029)	(0.027)	(0.031)
N	2547	3291	2831	3417	3596
BW_{left}	1840	2381	2092	2307	2352
BW_{right}	1840	2381	2092	2307	2352

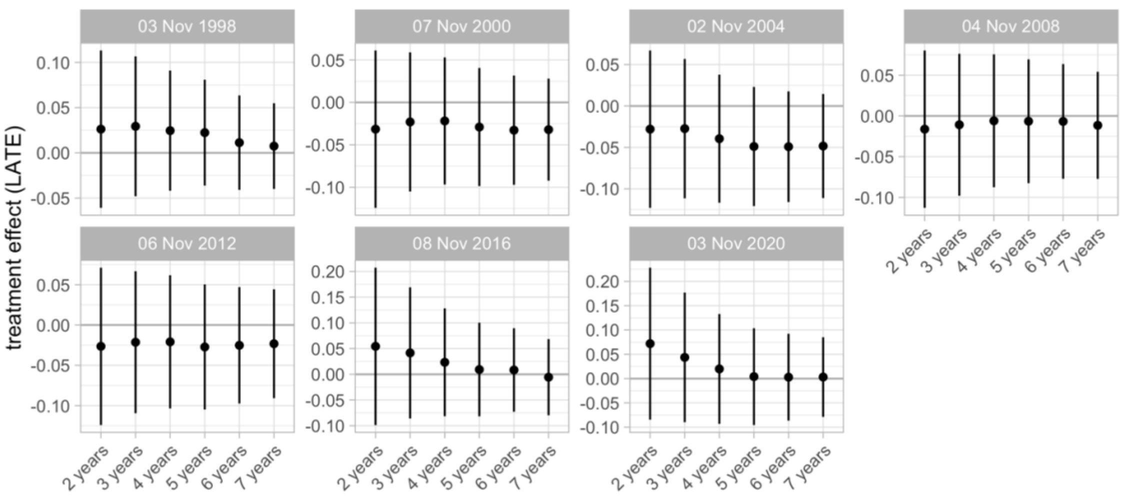
Note: The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the dependent variable being used. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Conventional standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

Figure A2.3.4: Effect of pension benefit expansion on turnout among unaffiliated voters across elections with additional polynomial terms



Note: The figure plots the local average treatment effect (λ) (y-axis) for each election under consideration (x-axis). The outcome variables are turnout binaries for different elections. The left (right) panel indicates the treatment effects across different elections with a second (and third) order polynomial term of the running variable as additional covariate. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors.

Figure A2.3.5: Effect of pension benefit expansion on turnout among unaffiliated voters across elections with different bandwidth choices



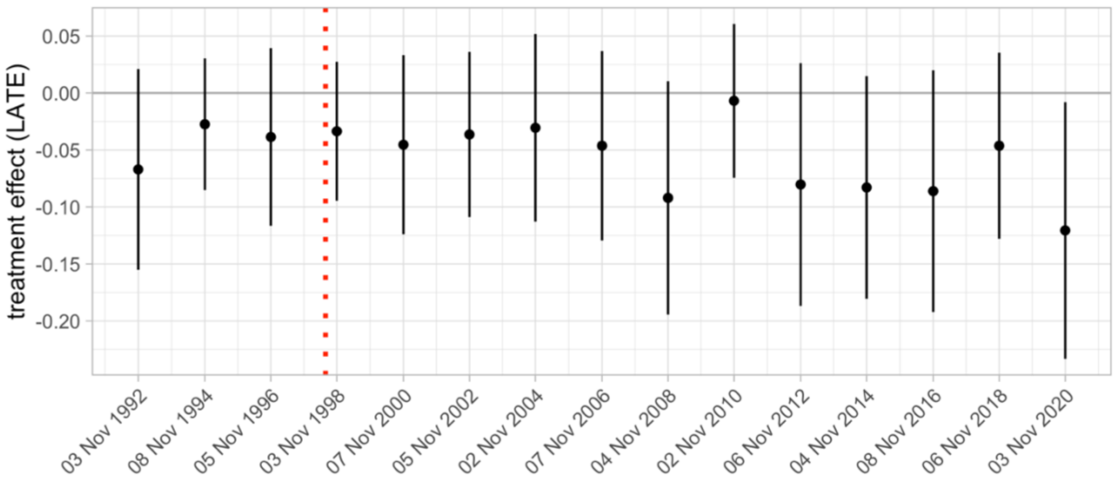
Note: The figure plots the local average treatment effect (λ) (y-axis) for each election (panel) across different bandwidth choices (x-axis). The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y} = \boldsymbol{\alpha} + \boldsymbol{\lambda D} + \boldsymbol{\beta_1}(\boldsymbol{X} - \boldsymbol{c}) + \boldsymbol{\beta_2 D}(\boldsymbol{X} - \boldsymbol{c}) + \boldsymbol{\delta_K K} + \boldsymbol{\epsilon}$. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors.

Table A2.3.6: Effect of pension benefit expansion on turnout among unaffiliated voters using placebo cut-off dates

	Turnout during election on 05 November 1998				
D	-0.003	-0.006	0.021	-0.038	0.002
	(0.018)	(0.020)	(0.023)	(0.024)	(0.023)
N	477	1845	3380	1488	1589
BW_{left}	1317	2686	1482	1063	1150
BW_{right}	1317	2686	1482	1063	1150
cutoff	-3650	-1825	1825	3650	5475

Note: The outcome variable is turnout for the election on 05 November 1998. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. Columns differ with respect to the selection of the placebo cut-off dates. Cut-off values refer to the number of days before (negative) or after (positive) 01 July 1998. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Robust standard errors in parenthesis. *p<0.05; **p<0.01; ***p<0.001.

Figure A2.3.7: Effect of pension benefit expansion on turnout among unaffiliated voters using non-teacher public employees as placebo treatment group

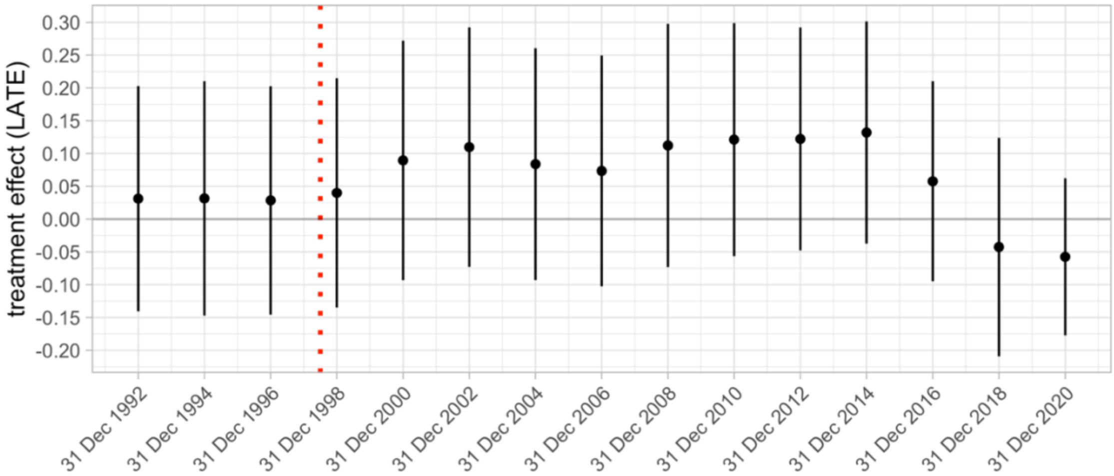


Note: The outcome variables are turnout binaries for different elections among non-teacher public employees. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. All estimates include a binary for being male, salary, and years of service as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors. The dotted line (red) represents the implementation date of the pension expansion (01 July 1998).

3. Heterogeneity Analysis

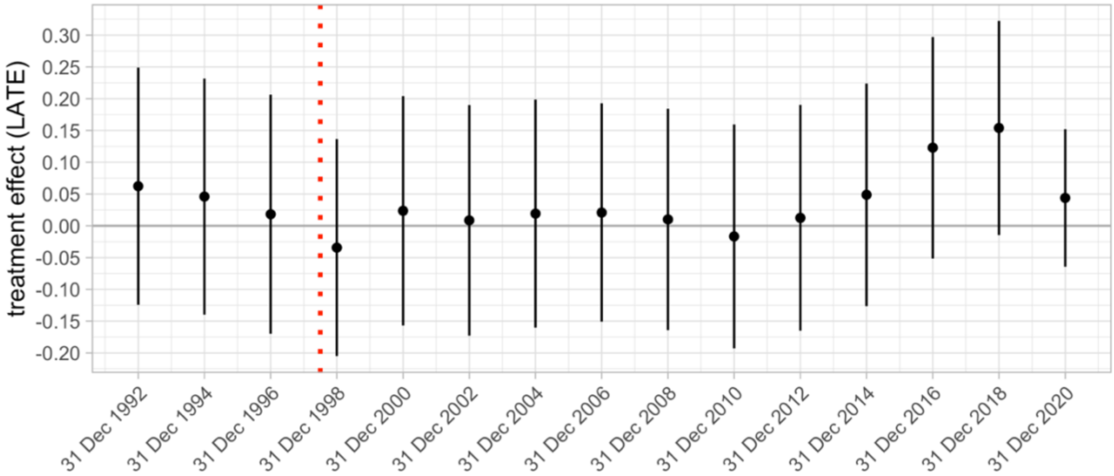
3.1 Democratic Voters

Figure A3.1.1: Effect of pension benefit expansion on voting registration among democratic voters with above-median salaries across election years



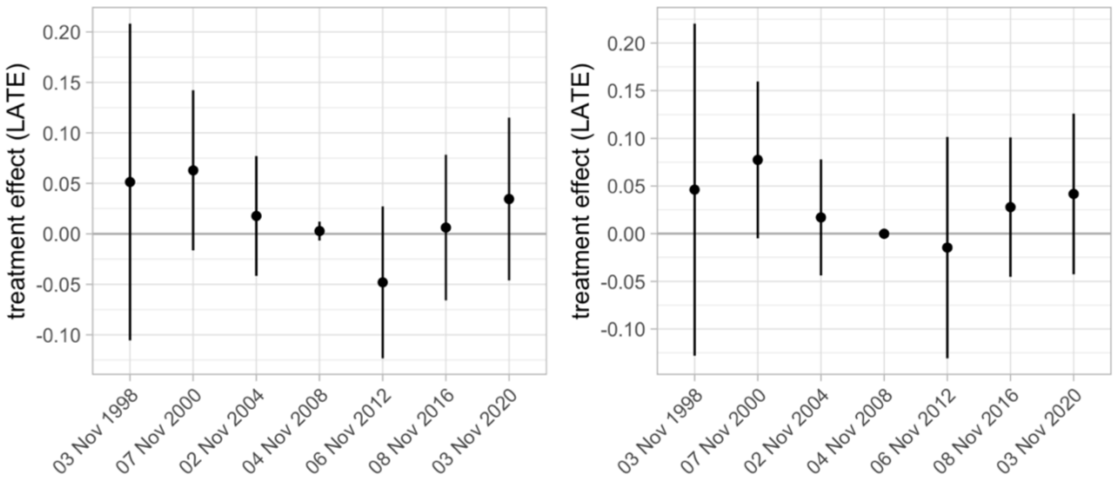
Note: The figure plots the local average treatment effect (λ) (y-axis) for each election year under consideration (x-axis). The outcome variables are registration binaries for each full election year. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors. The dotted line (red) represents the implementation date of the pension expansion (01 July 1998).

Figure A3.1.2: Effect of pension benefit expansion on voting registration among democratic voters with below-median salaries across election years



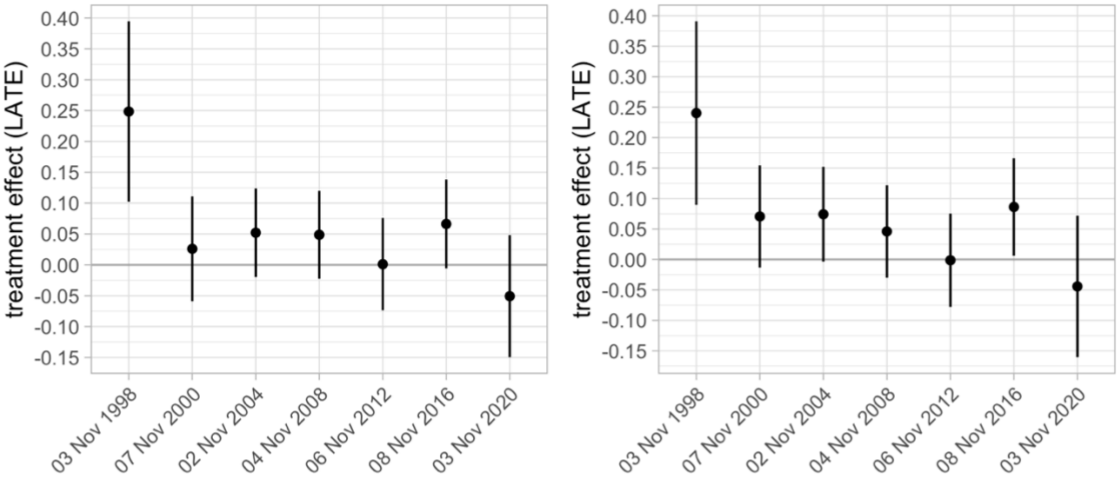
Note: The figure plots the local average treatment effect (λ) (y-axis) for each election year under consideration (x-axis). The outcome variables are registration binaries for each full election year. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors. The dotted line (red) represents the implementation date of the pension expansion (01 July 1998).

Figure A3.1.3: Effect of pension benefit expansion on turnout among democratic voters with above-median salaries with additional polynomial terms



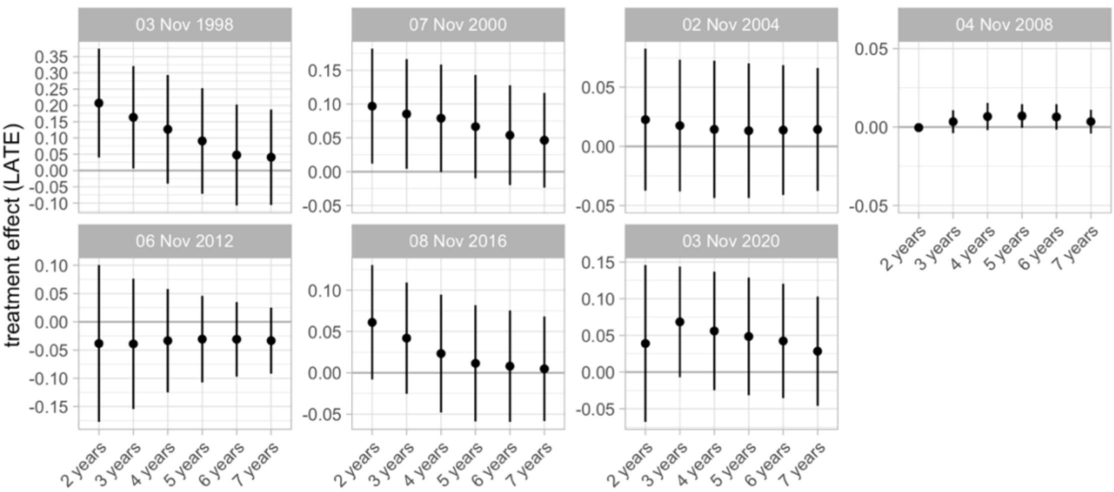
Note: The figure plots the local average treatment effect (λ) (y-axis) for each election under consideration (x-axis). The outcome variables are turnout binaries for different elections. The left (right) panel indicates the treatment effects across different elections with a second (and third) order polynomial term of the running variable as additional covariate. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors.

Figure A3.1.4: Effect of pension benefit expansion on turnout among democratic voters with below-median salaries with additional polynomial terms



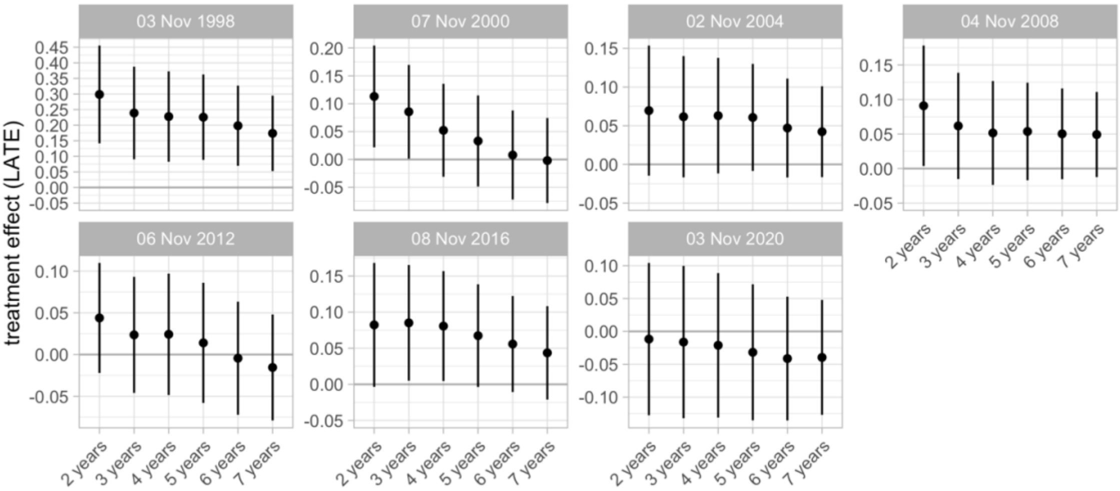
Note: The figure plots the local average treatment effect (λ) (y-axis) for each election under consideration (x-axis). The outcome variables are turnout binaries for different elections. The left (right) panel indicates the treatment effects across different elections with a second (and third) order polynomial term of the running variable as additional covariate. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors.

Figure A3.1.5: Effect of pension benefit expansion on turnout among democratic voters with above-median salaries with different bandwidth choices



Note: The figure plots the local average treatment effect (λ) (y-axis) for each election (panel) across different bandwidth choices (x-axis). The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1 (X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors.

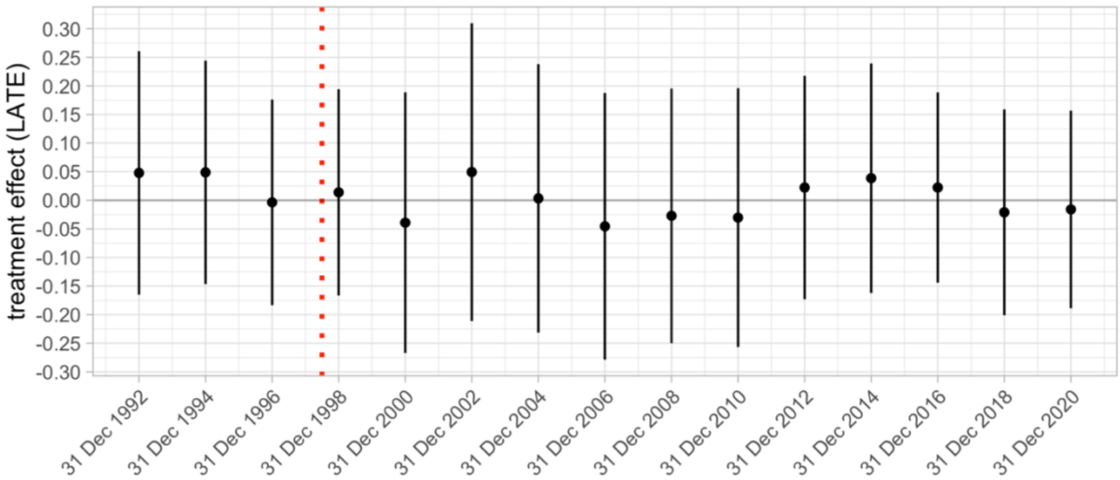
Figure A3.1.6: Effect of pension benefit expansion on turnout among democratic voters with below-median salaries with different bandwidth choices



Note: The figure plots the local average treatment effect (λ) (y-axis) for each election (panel) across different bandwidth choices (x-axis). The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y} = \boldsymbol{\alpha} + \boldsymbol{\lambda D} + \boldsymbol{\beta_1}(\boldsymbol{X} - \boldsymbol{c}) + \boldsymbol{\beta_2 D}(\boldsymbol{X} - \boldsymbol{c}) + \boldsymbol{\delta_K K} + \boldsymbol{\epsilon}$. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors.

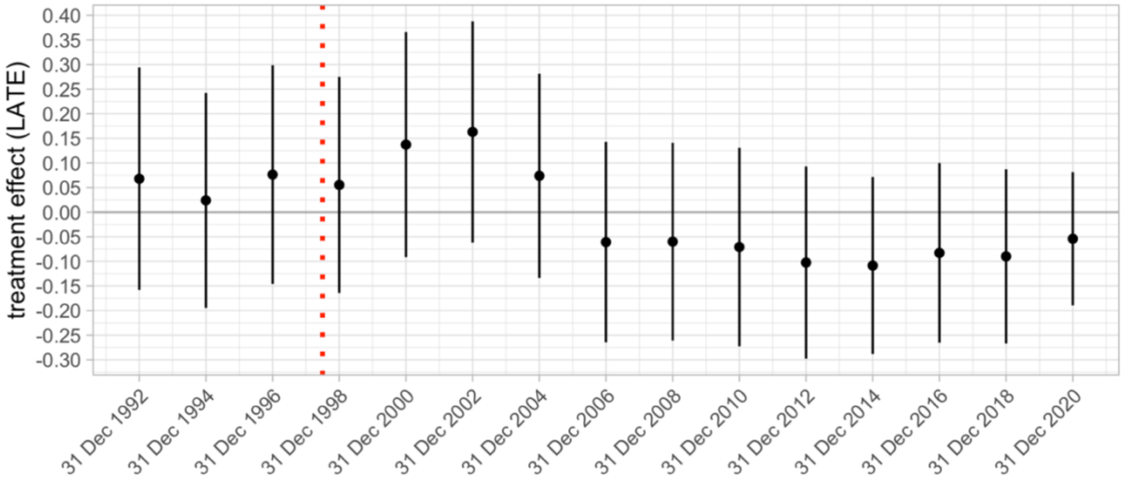
3.2 Republican Voters

Figure A3.2.1: Effect of pension benefit expansion on voting registration among republican voters with above-median salaries across election years



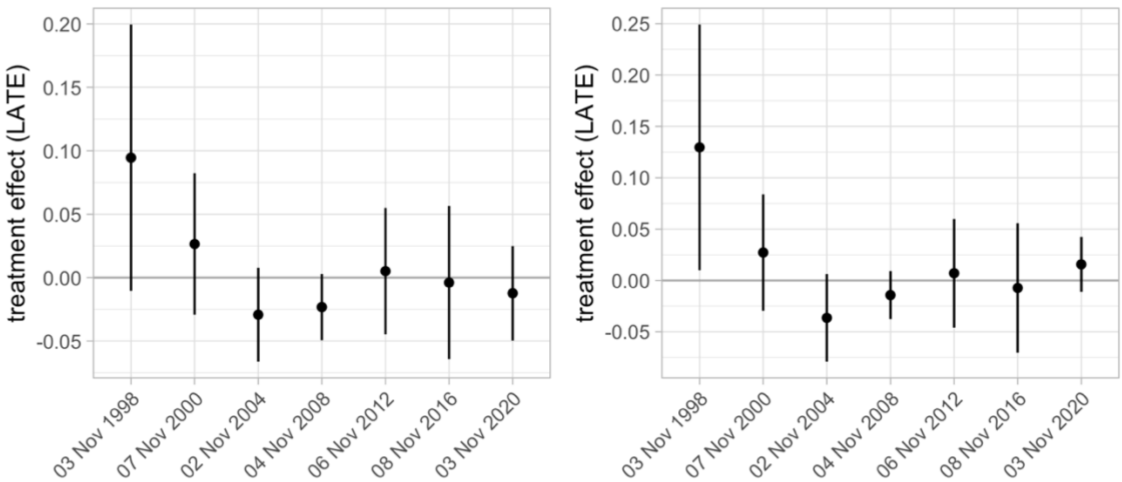
Note: The figure plots the local average treatment effect (λ) (y-axis) for each election year under consideration (x-axis). The outcome variables are registration binaries for each full election year. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors. The dotted line (red) represents the implementation date of the pension expansion (01 July 1998).

Figure A3.2.2: Effect of pension benefit expansion on voting registration among republican voters with below-median salaries across election years



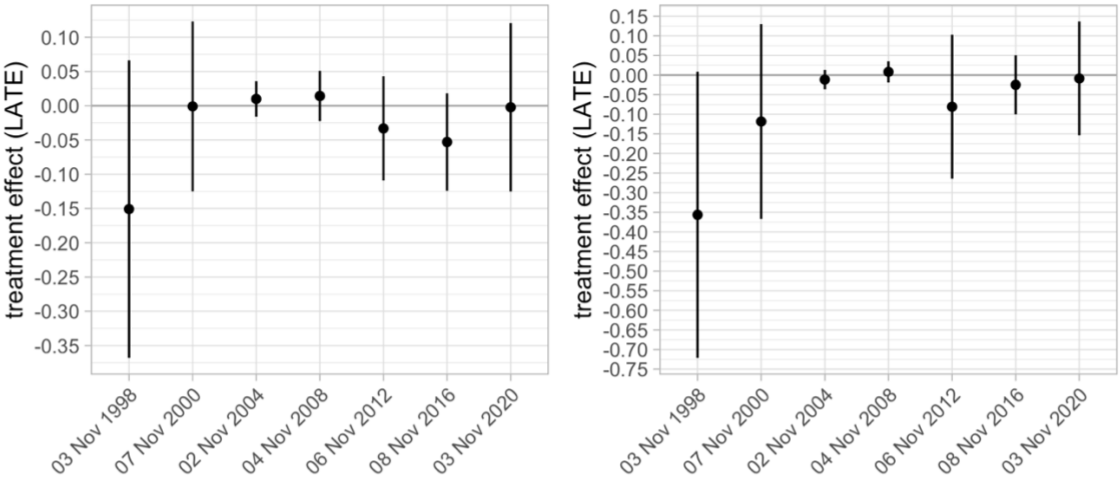
Note: The figure plots the local average treatment effect (λ) (y-axis) for each election year under consideration (x-axis). The outcome variables are registration binaries for each full election year. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors. The dotted line (red) represents the implementation date of the pension expansion (01 July 1998).

Figure A3.2.3: Effect of pension benefit expansion on turnout among republican voters with above-median salaries with additional polynomial terms



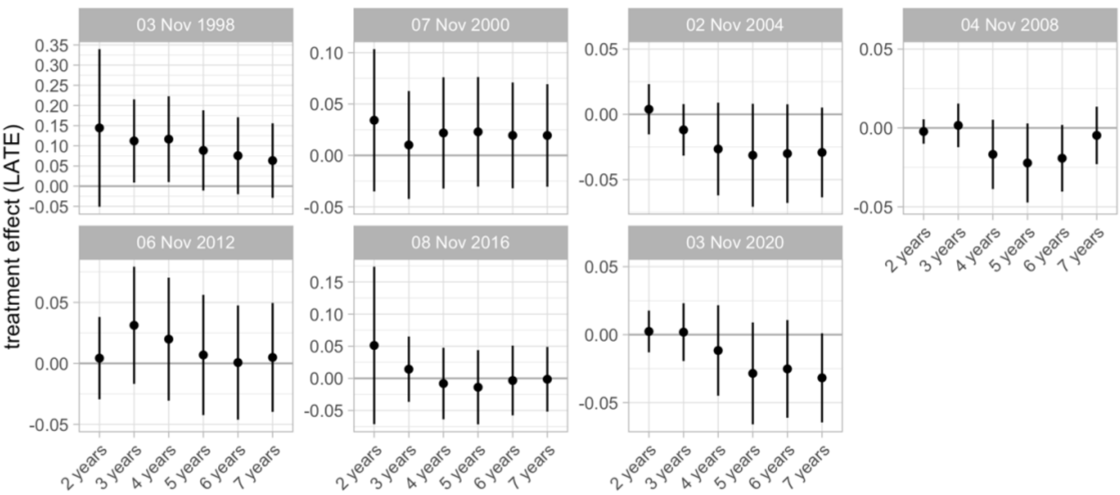
Note: The figure plots the local average treatment effect (λ) (y-axis) for each election under consideration (x-axis). The outcome variables are turnout binaries for different elections. The left (right) panel indicates the treatment effects across different elections with a second (and third) order polynomial term of the running variable as additional covariate. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors.

Figure A3.2.4: Effect of pension benefit expansion on turnout among republican voters with below-median salaries with additional polynomial terms



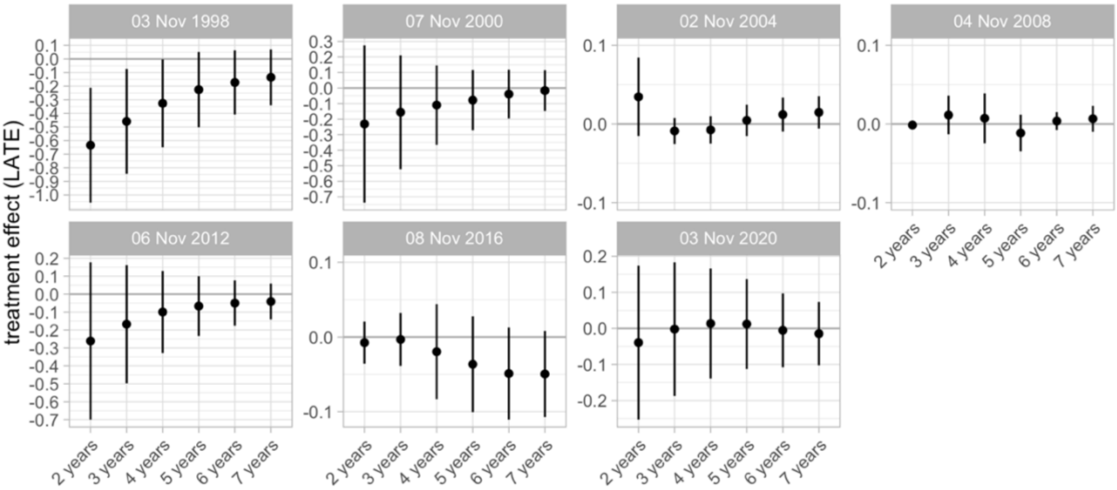
Note: The figure plots the local average treatment effect (λ) (y-axis) for each election under consideration (x-axis). The outcome variables are turnout binaries for different elections. The left (right) panel indicates the treatment effects across different elections with a second (and third) order polynomial term of the running variable as additional covariate. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors.

Figure A3.2.5: Effect of pension benefit expansion on turnout among republican voters with above-median salaries with different bandwidth choices



Note: The figure plots the local average treatment effect (λ) (y-axis) for each election (panel) across different bandwidth choices (x-axis). The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors.

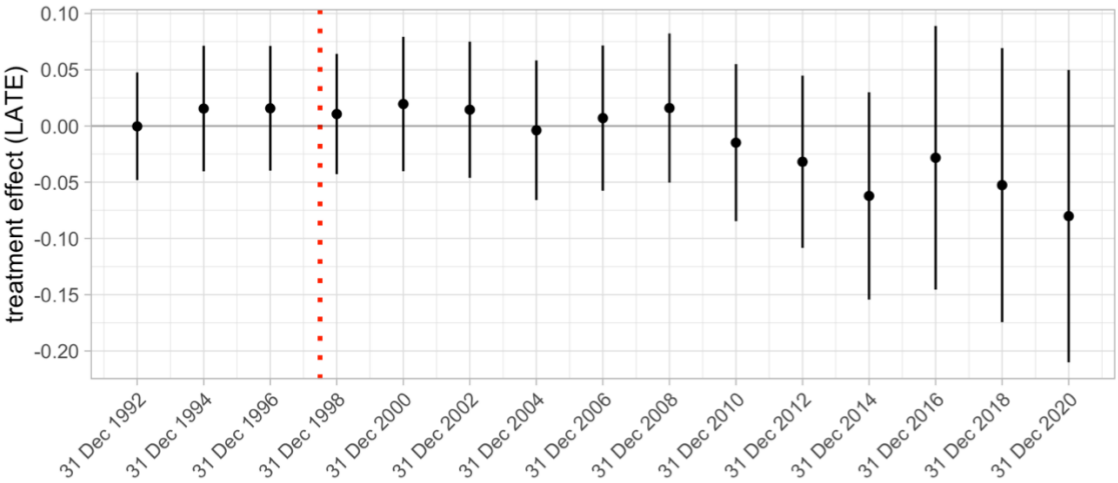
Figure A3.2.6: Effect of pension benefit expansion on turnout among republican voters with below-median salaries with different bandwidth choices



Note: The figure plots the local average treatment effect (λ) (y-axis) for each election (panel) across different bandwidth choices (x-axis). The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors.

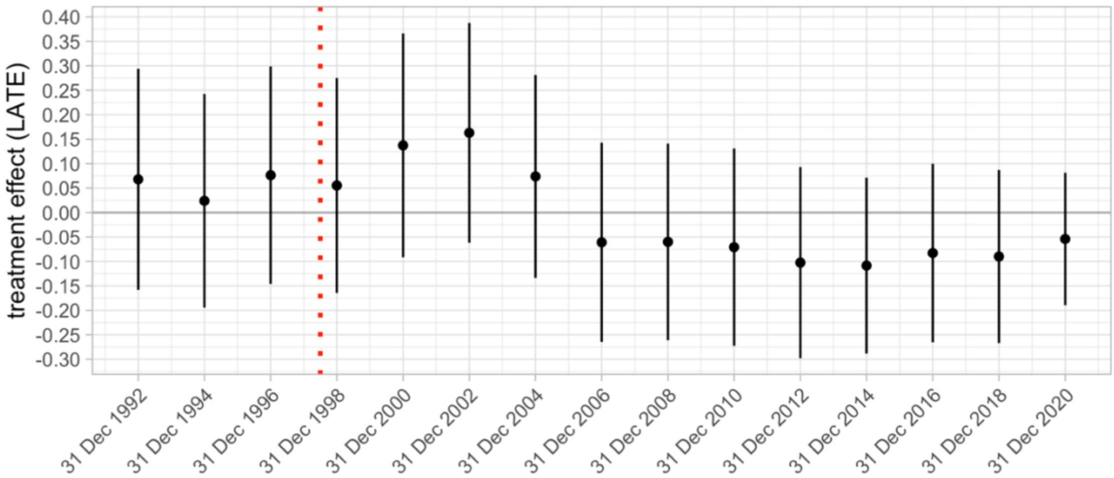
3.3 Unaffiliated Voters

Figure A3.3.1: Effect of pension benefit expansion on voting registration among unaffiliated voters with above-median salaries across election years



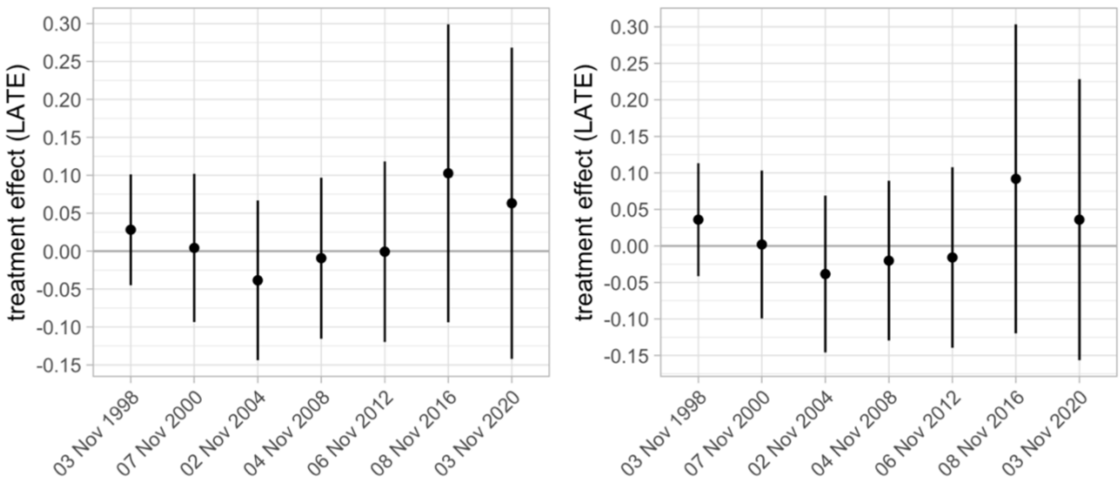
Note: The figure plots the local average treatment effect (λ) (y-axis) for each election year under consideration (x-axis). The outcome variables are registration binaries for each full election year. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors. The dotted line (red) represents the implementation date of the pension expansion (01 July 1998).

Figure A3.3.2: Effect of pension benefit expansion on voting registration among unaffiliated voters with below-median salaries across election years



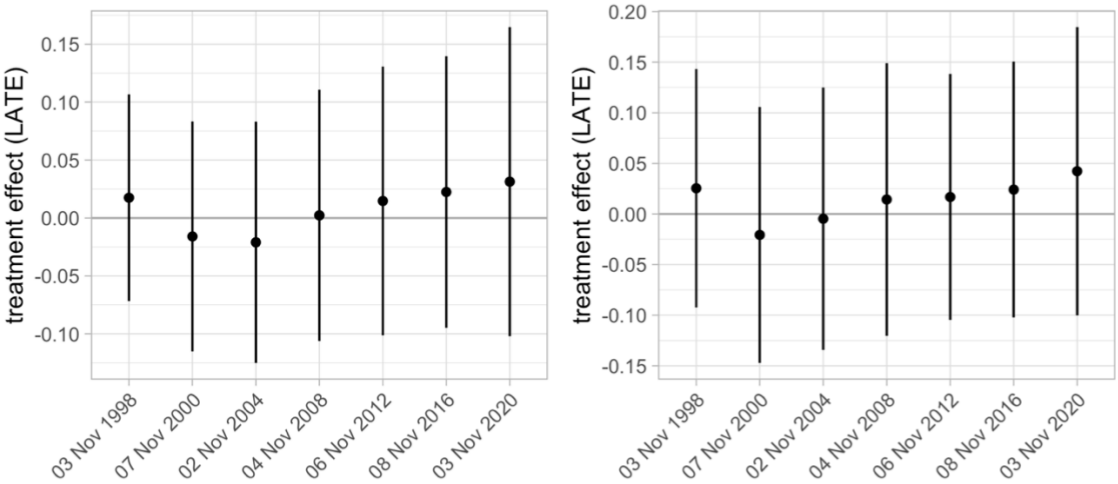
Note: The figure plots the local average treatment effect (λ) (y-axis) for each election year under consideration (x-axis). The outcome variables are registration binaries for each full election year. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1(X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors. The dotted line (red) represents the implementation date of the pension expansion (01 July 1998).

Figure A3.3.3: Effect of pension benefit expansion on turnout among unaffiliated voters with above-median salaries with additional polynomial terms



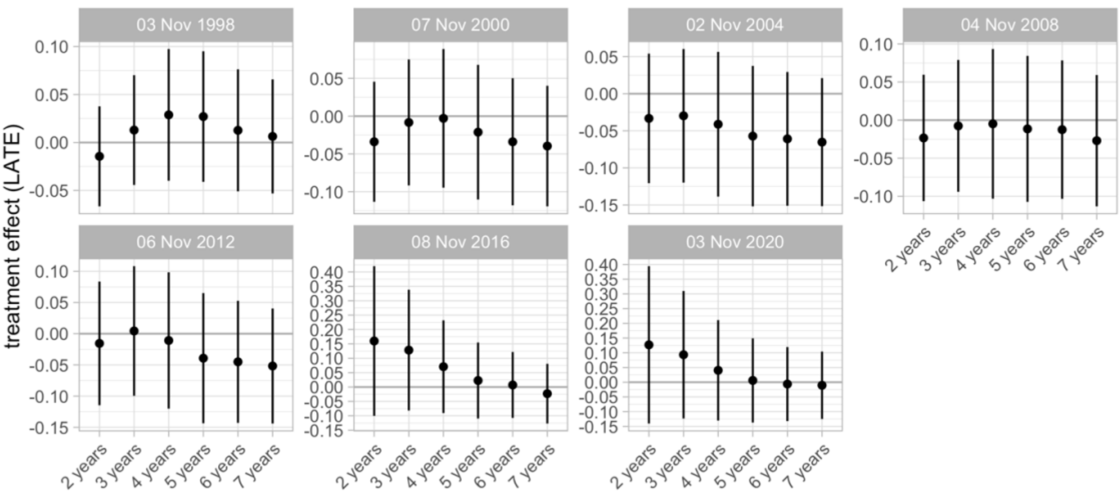
Note: The figure plots the local average treatment effect (λ) (y-axis) for each election under consideration (x-axis). The outcome variables are turnout binaries for different elections. The left (right) panel indicates the treatment effects across different elections with a second (and third) order polynomial term of the running variable as additional covariate. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors.

Figure A3.3.4: Effect of pension benefit expansion on turnout among unaffiliated voters with below-median salaries with additional polynomial terms



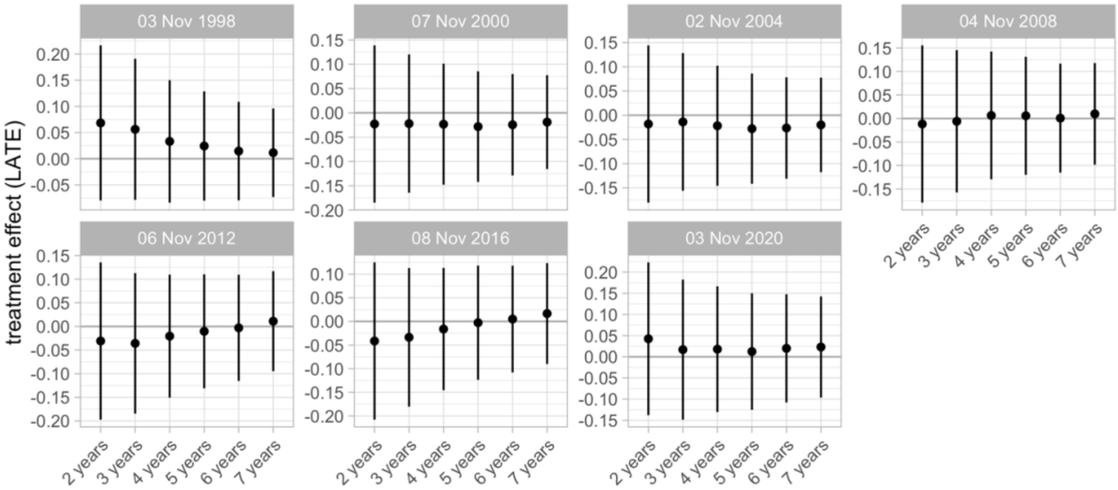
Note: The figure plots the local average treatment effect (λ) (y-axis) for each election under consideration (x-axis). The outcome variables are turnout binaries for different elections. The left (right) panel indicates the treatment effects across different elections with a second (and third) order polynomial term of the running variable as additional covariate. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors.

Figure A3.3.5: Effect of pension benefit expansion on turnout among unaffiliated voters with above-median salaries with different bandwidth choices



Note: The figure plots the local average treatment effect (λ) (y-axis) for each election (panel) across different bandwidth choices (x-axis). The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1 (X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors.

Figure A3.3.6: Effect of pension benefit expansion on turnout among unaffiliated voters with below-median salaries with different bandwidth choices



Note: The figure plots the local average treatment effect (λ) (y-axis) for each election (panel) across different bandwidth choices (x-axis). The outcome variables are turnout binaries for different elections. Estimates are based on $\boldsymbol{Y = \alpha + \lambda D + \beta_1 (X - c) + \beta_2 D(X - c) + \delta_K K + \epsilon}$. All estimates include a binary for being male, salary, years of service, and a binary for Chicago teachers as covariates. Vertical bars indicate 95% confidence intervals. Confidence intervals are based on robust standard errors.

Are Climate Change Policies Politically Costly?

Davide Furceri, Michael Ganslmeier and Jonathan D. Ostry

Abstract

Are policies designed to avert climate change (Climate Change Policies, or CCPs) politically costly? Using data on governmental popular support and the OECD's Environmental Stringency Index covering 30 countries between 2001 and 2015, our results show that CCPs are not necessarily politically costly: policy design matters. First, in contrast to non-market-based CCPs (such as emission limits), only market-based CCPs (such as emission taxes) entail political costs for the government. Second, the effects are only present when CCPs are adopted during periods of high oil prices, prior to elections, or in countries depending strongly on non-green (dirty) energy sources. Third, CCPs are only politically costly when inequality is high and/or social insurance/transfer does not sufficiently address the regressivity of CCPs. Our results are robust to numerous sensitivity checks and concerns related to endogeneity issues.

Introduction

There are few issues that have sparked more attention in recent years than how to avoid the environmental and human catastrophe that climate change is inflicting on our planet. Yet, despite the rising demand for greater governmental efforts to reduce carbon emission, the hesitancy of politicians to adopt the necessary measures is remarkable. This is disturbing from two perspectives.

From an *economic* perspective, the empirical evidence on the economics of climate change shows that the long-term costs of unmitigated climate change will outweigh by a wide margin the short-term adjustment costs from mitigation (Stern 2008). Although economists expect that poor countries will carry larger costs (Diffenbaugh and Burke 2019; Dell et al. 2012), industrial countries are also estimated to suffer losses of ½-2 percent of GDP if global temperature rises by 2-4 degrees Celsius by 2100 (Hsiang et al. 2017). Kahn et al. (2019) project an even greater economic damage showing that unmitigated climate change will reduce global real GDP per capita by more than 7 percent by the end of the century. Importantly, since the economic costs are increasing in time because of greater accumulation of emissions in the atmosphere (Burke et al. 2015), hesitancy comes at a very high price as later interventions are required to be at a larger scale.

From a *political* perspective, the politics of climate change has also become more favourable towards pro-environmental policy outcomes. For instance, the number of green parties has increased in many advanced democracies in the last decades (Dolezal 2010). Since green parties have been challenging many non-green parties on climate-related topics, the pressure of mainstream electoral platforms to adopt green issues in their manifestos should facilitate the adoption of CCPs (Meguid 2005; Adams et al. 2006; Spoon et al. 2014; Green-Pedersen and Mortensen 2010; Ezrow et al. 2010). Moreover, with more frequent natural disasters, people are becoming more risk averse about climate change, and large-scale global protests have granted green issues a dominant position on political agendas around the world (Liu et al. 2011; Steves and Teytelboym 2013; Hsiang and Burke 2014; Herrnstadt and Muehlegger 2014; Bird et al. 2014; Welsch and Biermann 2014; PEW Research Center 2019; EMDAT 2020).

However, despite these push factors, governments in democratic countries remain hesitant even when effective mitigation instruments are broadly available (Weitzman 1974; McKibbin and Wilcoxen 2002; Aldy et al. 2003; Li and Lin 2013; Carl and Fedor 2016; Akerlof et al. 2019). To understand the absence of welfare-increasing reforms, the debate about political costs has long dominated the political-economy literature (Alesina and Drazen 1989; Drazen and Grilli 1993; Persson and Tabellini 2002; Tsebelis 2002; Alesina et al. 2006; Alesina et al. 2020; Alesina et al. 2021). While previous contributions focused mostly on economic and structural reforms, the tie-in with climate policies has been investigated less.[32] With the present paper, we aim to fill this gap by analyzing whether the adoption of climate-related policies entails political costs for the incumbent government.

[32] For instance, Klenert et al. (2018) study how optimal environmental taxation can be designed to prevent regressive consequences, Klenert et al. (2017) show how the public support of carbon pricing policies can be enhanced via revenue recycling, and Sovacool et al. (2015) discuss the role distributional considerations and competing interests play for the success or failure of climate adaption projects.

To do so, we estimate the average effect of CCPs on popular support for the government implementing them (governmental support for short)—where CCPs are proxied by the OECD's Environmental Policy Stringency (EPS) indicators (Botta and Kozluk 2014) and governmental support is proxied by the "Index of Popular Support" produced by the International Country Risk Guide (ICRG) (2020) from public opinion polls for the period between 2001 and 2015.[33] In addition, we adopt an Instrumental Variable (IV) approach that exploits cross-sectional variation in a country's likelihood to implement CCPs—such as measures of country vulnerability to climate change event as the length of coastline—and time-varying variation at the global level—such as the global number of floods per annum. Overall, our results show that endogeneity in the OLS estimator indeed is likely to result in underestimation of the true causal political cost of climate change policies.

Our paper delivers three key findings. *First*, the results show that CCPs reduce, on average, popular support for the government, but this mainly reflects the impact of market-based policy measures, such as emission taxes, rather than non-market-based measures, such as emission limits. As many economists see Pigouvian taxation as the first-best corrective tool for carbon emissions, opting for second-best nonmarket-based measures can be an efficient alternative when market-based measures are not politically viable (Jenkins 2014; Goulder and Parry 2008; Goulder et al. 2019; Stiglitz 2019; IMF 2019).

[33] As a robustness check we include alternative measures to examine the political effects of CCPs, such as the annual averages of the polls of the incumbent party, the vote share of all government parties, and the vote share of the largest government party.

Second, we show that political costs are larger when they are adopted in times of high fuel prices and/or prior to elections. These two findings indicate that political costs depend on the visibility of the reform and on the existing price level of affected products (e.g. fuel). In addition, the empirical analysis reveals that CCPs create a larger political backlash when adopted in countries whose industries depend strongly on non-green (dirty) energy sources.

Third, our findings indicate that the political costs are only salient when inequality is increasing and when social benefits—in the form of direct transfers to households, unemployment benefits to workers that loss their job or active labour market policies to help job reallocation—do not counterbalance the short-term cost in terms of output and employment (Känzig 2021). Remarkably, in the counterfactual to these situations, the political cost of CCPs is not statistically different from zero. The latter finding highlights that climate-related policymaking is ultimately a social question, implying that sufficient social insurance mechanisms are imperative to enable the adoption of far-reaching but necessary action against climate change.

The comparative perspective taken in this paper allows us to investigate political obstacles that may apply in the broader context of environmental policymaking. Due to the large regional coverage, the analysis sheds light on how unpopular CCPs can be made political feasible in countries which are responsible for more than 43% of annual global carbon emissions. In addition, the time coverage of our data enables us to track how the political consequences of CCPs have ebbed and flowed over time. Finally, while previous literature has shown that (certain types of) environmental policies are unpopular among (certain groups of) voters, there is surprisingly little evidence on how the adoption of CCPs affects the popularity of the government. This paper aims to contribute to the policy-feedback literature by investigating how environmental legislation translates into actual political costs for the incumbent, and how policy design can be tweaked to make CCPs more acceptable among the electorate.

The paper is structured as follows. In the next section, we review the literature on CCPs, paying attention to the allocation of costs and benefits from stricter environmental regulation, the challenges of international agreements, and strategies on the mitigation of political costs. In section III and IV, we present the data, outline our empirical approach and discuss the results of the direct effects of CCPs. Section V presents the results on how political costs of CCPs vary across periods, countries, and institutions. Section VI concludes with implications for policymakers in advanced economies.

The political economy of climate change policies

There are parallels between the literature on the political economy of structural reforms and the political economy of environmental policies. Both types of reform are subject to resistance and a lack of political will even when they improve welfare in the long run. In seminal contributions by Alesina and Drazen (1989), Drazen and Grilli (1993), Persson and Tabellini (2000) and Tsebellis (2002), the reform process is modelled as a function of veto players who are required to distribute the benefits and costs in a way that satisfies all of them.[34]

This approach applies also to environmental policies. On the one hand, economic adjustments from CCPs can be costly in the short run, while benefits materialize only over time. Additionally, the benefits of CCPs are not directly observable because they materialize in the absence of environmental damage. On the other hand, CCPs put a large burden of adjustment on a few stakeholders, while benefits are distributed widely (Stokes 2016). Indeed, the regressive nature of certain CCPs has spurred concerns related to inequality in many countries around the world (Goulder et al. 2019; Stiglitz 2019; Rojas-Vallejos and Lastuka 2020; Känzig 2021). Thus, the concentration of costs is expected to create strong interest groups against climate-protecting policies, while the dispersion of benefits diffuses support for pro-environmental positions.

Beyond these institutional considerations at the country level, environmental protection has always been hostage to the political agenda at the international level. Despite ambitious international agreements over the last decade, enforceable solutions are hampered by a dearth of sanctioning mechanisms to prevent free-riding behaviour. For instance, governments can be incentivized to undercut other countries' environmental standards to attract foreign direct investment from multinational corporations, which would ultimately induce regulatory race-to-the-bottom dynamics (Koch and Basse Mama 2019). In addition, climate change may also elevate international economic inequalities

[34] Alesina et al. (2020) find that domestic (product and labor market) and external (trade and capital) structural reforms tend to reduce the vote share of the incumbent's party, especially when implemented close to elections.

further as the costs of global warming vary widely across countries. As the Center for Global Development (2015) estimates, 79 percent of global carbon emissions from 1980 to 2011 have been generated by developed countries. However, these countries have not borne most of the cost of climate change as of today (Eckstein et al. 2019).

To address the distributional consequences of reforms (Ostry et al. 2019; Ostry et al. 2021; Markkanen and Anger-Kraavi 2019), it is essential to understand how they affect different groups of society. CCPs can affect individuals in two ways. First, they often entail mark-ups on certain products — either directly through taxation or indirectly through rises in production costs (Rao 2013). Although price increases for oil and gas prices matter to all economic agents, they tend to have larger effects on poorer households, for whom energy consumption constitutes a larger share of household income, and because of the hit to employment for less skilled workers (Känzig 2021). This regressivity is a common argument against CCPs and a source of political opposition (Metcalf 2009; Habla and Roeder 2017; Goulder et al. 2019).

Second, CCPs have negative side effects for certain industries (Steves and Teytelboym 2013; Fankhaeser et al. 2008). Although new employment opportunities created by CCPs can mitigate job displacement to some degree (IMF 2019, IMF 2020), the adjustment costs are nevertheless salient in the short run. This is especially the case for the elderly, and for employees with specialized industry-specific capital or low educational levels (OECD 2012; Guivarch et al. 2011). Since it is well-known that income losses and unemployment have a strong negative impact on individuals' satisfaction with the government, this paper tests whether complementary measures — such as direct transfers to households, unemployment benefits, and active labour market policies—can mitigate the political backlash against CCPs.

Data

To estimate the political costs of CCPs, we use a country-year panel dataset covering 30 developed and emerging economies between 2001 and 2015. The large number of countries and years — our sample covers countries that are responsible for nearly half of global emissions — enables us to estimate the effects of CCPs on governmental support. The empirical analysis uses data from different sources. For the main **dependent variable,** we use a measure of governmental popular support constructed by the International Country Risk Guide (ICRG 2020). The measure assesses the government's ability to carry out its declared program(s), and its ability to stay in office. The popular support measure is based on public opinion polls and scaled between 0 (very high political risk) and 4 (very low political risk). In other words, a value of 1 implies that the current government is at high risk of losing office, while a value of 4 implies that the incumbent is able to carry out its declared program with ease and has a very high likelihood of re-election. In our sample, popular support was highest in the USA in 2002 and lowest in France in 2014.

Compared to indicators that are based on parties' vote shares, the popular support index provides an annual time series per country over a relatively long period of time. While poll data are also available, they usually do not provide a similar level of country coverage. Previous studies have used the dataset by ICRG to examine political stability and popular support both in economics and political science (Lambsdorff 2003; Torgler and Schneider 2007; Chan et al. 2019; Yabré et al. 2021; Cotoc et al. 2021). Nevertheless, to ensure that this main dependent variable is a valid proxy for popular support, we also use alternative indicators for popular support, such as the vote share of all government parties, the vote share of the largest government party, and the annual averages of the polls of the incumbent party.

As main **independent variable,** we use the Environmental Policy Stringency (EPS) indicators constructed by the OECD (Botta and Kozluk 2014). These data are the most comprehensive source for environmental policy measures across countries (28 OECD and 6 BRICS countries) and time (1990–2015). All policy indicators are scaled from 0 (not stringent at all) to 6 (very stringent). In addition to its large geographical and temporal coverage, the dataset provides a breakdown by instrument type. The dataset includes both market-based and non-market-based measures, such as indices of taxation of emissions, trading schemes and feed-in tariffs (market-based), as well as indices of emission limits and R&D subsidies (non-market-based). The availability of these sub-indices strikes us as central as it allows us to test whether some instruments are politically more costly than others. Previous research has shown that market-based policies such as emission taxes are the most effective instruments to reduce carbon emission (Jenkins 2014; Goulder and Parry 2008; Goulder et al. 2019; Stiglitz 2019; IMF 2019). However, since they may not always be politically feasible due to their distributional consequences (Känzig 2021), opting for non-market-based measures can be a suitable second-best solution. Table A1.1 presents an overview and summary statistics for all variables.

The political costs of CCP

Empirical analysis

The baseline analysis examines whether stricter CCPs have an effect on popular support for the government and, if so, whether the negative effects differ across instruments. To test this hypothesis, we regress the measure for popular support of the incumbent on the change in the overall EPS indicator. Here, we use the change of the policy index to test whether alteration of the current status-quo lowers popular support for the government (Samuelson and Zeckhauser 1988; Fernandez and Rodrik 1991; Pierson 1994). Our OLS specification (baseline) reads as follows:

$$y_{i,t} = \beta \Delta EPS_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \epsilon_{i,t}$$

where y is popular support for the incumbent and ΔEPS is the change in the EPS indicator. Beyond including country (α_i) and year fixed effects (γ_t) to control for unobserved factors and limit omitted variable bias, the specification includes various economic, political, and demographic indicators within X. Specifically, we control for the fiscal deficit as percent of GDP (OECD), the share of the elderly (65+) in percent of population (OECD), real GDP growth rate (World Bank), unemployment rate (World Bank), consumer price index (OECD), lagged government's vote share (Alesina et al. 2020), the average tax wedge (World Bank), and five economic reform indicators by Alesina et al. (2020).

The inclusion of these variables is based on different streams of the political economy literature related to economic voting, political capabilities, and structural reforms. For instance, by controlling for the tax wedge, we take account of the empirical finding that voters do not favour taxes[35] (Alesina et al. 2021). Similarly, controlling for macroeconomic factors such as GDP growth, inflation, unemployment rate, and fiscal space, we account for a large body of empirical work on economic voting which has shown that the state of the economy has a significant impact on the government's chances for re-election. Additionally, since older individuals may have different preferences towards climate change policies (Torgler and Garcia-Valiñas 2007; De Vries and Giger 2014), we include the share of the elderly as an additional control. Finally, previous empirical evidence has shown that reforms in other domains influence the popularity of the government. To account for policymaking in other domains, we add five structural reform indicators covering reform dynamics in policy areas related to finance, labour, and product markets.

Overall, the baseline results show that increasing environmental policy stringency has significantly negative and sizeable effects on the popular support for the government (Table 1). A major change in CCPs—equivalent to an increase in the EPS indicator from the 25th to the 75th percentile of the EPS distribution—is found to reduce popular support by about 10 percent. The statistical significance of the results is robust to alterative sets of controls and the magnitude of the coefficients does not change with model specification. In line with previous findings of the literature (see Alesina et al. 2020 and

[35] The results remain unchanged if we use the change in tax revenue as control variable instead of the levels.

references therein), we also find that better economic conditions, higher fiscal deficits, and lower inflation are associated with higher political support. We find that the level of popular support is typically larger in countries with an older share of the population as age tends to be positively associated with voting for the incumbent at the individual level (De Vries and Giger 2014).

Table 1: The effect of EPS changes on popular support of the government

Variables	(1)	(2)	(3)	(4)	(5)	(6)
change in EPS index	**-0.231***** **(0.079)**	**-0.246***** **(0.079)**	**-0.247***** **(0.080)**	**-0.266***** **(0.086)**	**-0.305***** **(0.091)**	**-0.297***** **(0.092)**
public deficit %GDP	0.039*** (0.014)	0.028* (0.015)	0.027* (0.015)	0.025 (0.017)	0.016 (0.014)	0.015 (0.015)
share of elderly	0.163*** (0.051)	0.177*** (0.055)	0.194*** (0.063)	0.172** (0.069)	0.125 (0.089)	0.095 (0.090)
GDP growth	0.063** (0.029)	0.047 (0.030)	0.051 (0.031)	0.048 (0.033)	0.035 (0.026)	0.036 (0.028)
unemployment rate		-0.035** (0.017)	-0.033* (0.018)	-0.028 (0.022)	-0.050** (0.020)	-0.057** (0.025)
CPI			0.299 (0.309)	0.233 (0.314)	0.094 (1.016)	0.342 (1.172)
gov. parties' vote share				0.010* (0.005)	-0.003 (0.006)	-0.003 (0.007)
average tax wedge					-0.071 (0.044)	-0.059 (0.049)
financial reform index						0.806 (1.929)
product reform index						-0.167 (0.743)
current account reform index						-1.437 (1.300)
capital account reform index						1.485 (1.507)
labour reform index						0.854 (1.732)
constant	0.659 (0.624)	0.724 (0.673)	0.281 (0.924)	0.628 (1.010)	3.050 (1.910)	1.868 (2.846)
Observations	370	370	370	326	260	260
R-squared	0.455	0.466	0.468	0.464	0.548	0.554
Number of countries	30	30	30	26	23	23
Year Fixed Effects	YES	YES	YES	YES	YES	YES
Country Fixed Effects	YES	YES	YES	YES	YES	YES

Note: The outcome variable is popular support of the government. Estimates are based on $y_{i,t} = \beta \Delta EPS_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \epsilon_{i,t}$. Columns differ with respect to control variable set. All estimations include country and year fixed effects. The main independent variable of interest is change in the EPS index (ranging from 1 (low stringency) to 6 (high stringency)). A coefficient of -0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p<0.1; **p<0.05; ***p<0.01.

To test the robustness of these findings, we conduct three additional checks. First, we test whether our estimates are robust to alternative dependent variables. In particular, we re-estimate the baseline with three different indicators, namely the annual averages of the polls of the incumbent party (Table A2.1), the vote share of all government parties (Table A2.2), and the vote share of the largest government party (Table A2.3). As the results show, the effect remains negative and significant for all dependent variables under consideration. Again, the magnitude of the impact is sizeable. For instance, increasing the EPS indicator by one-unit results in a 11.36% loss in vote share of all government parties (Table A2.2).

Second, the baseline estimates may suffer from model uncertainty and misspecification. Here, one major source of model uncertainty originates from the covariate selection. While our baseline analysis shows that the inclusion or exclusion of a broad range of political, economic, and demographic control variables does not change the estimate of interest, we conduct two additional model averaging exercises using weighted-average least squares (WALS) (Magnus et al. 2010) and Bayesian model averaging (BMA) (Fernandez et al. 2001). As the results show (Table A2.4), the change in the EPS index is one of the most robust variables among all covariates under consideration and the point estimates are very similar to the baseline results.

Third, it is possible that the political costs of CCPs have been declining over time as more frequent natural disasters, rising risk aversion about the consequences of climate change, and large-scale global protests have made the median voter greener over time. To test this argument, we re-estimate the baseline model by dropping observations after year k with

$k = \{2006, 2008, 2010, 2012, 2014\}$. We use this sub-sampling approach instead of a rolling-window because restricting the dataset to only a few years would lead to extensive sampling uncertainty, and thus large standard errors. The results show that the political costs of CCPs have indeed decreased over time. While the coefficient based on observations with $k = 2006$ is -0.377, the estimate drops to -0.225 when including all observations up to 2014. In other words, even though both estimates are highly significant, the results indicate that the political costs of CCPs have declined over time (Table A2.5). This result is consistent with the evidence that the support for green issues has been increasing in the recent decade (Spoon et al. 2014).

Endogeneity

Beyond measurement error of the dependent variable and covariate selection, the OLS estimates do not enable us to draw a causal interpretation due to two main forms of endogeneity. The first is omitted variable bias (OVB). While our model accounts for unobserved cross-country and -period heterogeneity and controls for numerous determinants of government support, we cannot exclude the possibility of OVB. A second issue is reverse causality, where the bias can go in either direction. On the one hand, governments may require political capital — proxied by popular support — to implement unpopular reforms. This selection bias implies a positive effect of the dependent variable on our policy variable and biases the OLS estimate towards zero. On the other hand, a government might implement CCPs because its unpopularity implies it has little to lose from implementing such reforms. This would imply a negative effect of popular support on CCPs and thus the possibility that the OLS estimate could overestimate the true effect. In addition, despite the robustness of our results to alternative indicators of popular support, we cannot exclude measurement error. This is especially true when using policy indices. If the measurement error is uncorrelated with the error term, we lose precision. If there is correlation, attenuation bias leads to distorted coefficient estimates.

To address these issue, we adopt an IV approach. Following Nunn and Quian (2014), the approach consists of interacting a time-varying global term with a constant country-specific term. The global term we consider approximates the “policy pressure” that climate change induces at the supra-national level. To be more specific, we use indicators on the occurrence and consequences of extreme weather events each year such as the number of flood events, the number of people affected by earthquakes, the number of major hurricanes in the North Atlantic, and the number of wildfires around the globe per annum. This choice of instrument is consistent with previous evidence that preferences toward CCPs changes after major natural disasters (Bird et al. 2014; Welsch and Biermann 2014; Latré et al. 2017). The country term we consider captures the vulnerability towards climate change which makes the adoption of CCPs more likely. For this purpose, geographical characteristics seem suitable measures since they can reasonably be assumed to be randomly distributed across countries and thus should not drive government support. We consider country-specific measures such as the length of the coastline, the minimum distance of a country’s centroid to the coast, the share of urban population in coastal area, and agricultural land (in km^2) per capita.

The theoretical rationale for our pressure-vulnerability instrument is based on the “war of attrition” model proposed by Alesina and Drazen (1989). According to this model, reforms to correct unstainable long-term trends (such as persistent increases in debt or in emissions in our case) are often delayed when they have distributional implications. Reforms occur only when a given group concedes and is "forced" to bear the adjustment. In this model, a crisis (natural disaster) may induce reform "*because the relative cost of fighting the war tilts in favor of concession*" (Alesina et al. 2006, p.5). While both the global and country term can be assumed to be exogenous to popular support for the government, recent research has shown that one term being exogenous is enough for Bartik-like instruments to be valid (Goldsmith-Pinkham et al. 2020; Borusyak et al. 2022).

Overall, we have constructed four instruments consisting of interactions between a time-varying global term and a constant country term.[36] Our IV estimation reads as follows:

$$y_{i,t} = \beta \Delta \widehat{EPS}_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \epsilon_{i,t}$$

with

$$\Delta \widehat{EPS}_{i,t} = \vartheta S_{i,t} + \varphi X_{i,t} + \alpha_i + \gamma_t + \upsilon_{i,t}.$$

where S is the instrument. The analysis also controls for country and time fixed effects and can therefore be seen as a *difference-in-difference* approach. For all four instruments, the first stage estimates suggest that the instrument is "strong" and statistically significant. The Kleibergen–Paap rk Wald F statistic — which is equivalent to the F-effective statistic

[36] The instruments are the interaction between: (i) the global number of flood events and the length of a country's coastline; (ii) the global number of people affected by earthquakes and the (log) share of urban extent in coastal zone affected by sea level elevation; (iii) the frequency of major hurricanes in the North Atlantic and distance from the centroid of a country to the nearest coast; and (iv) the global number of wildfires and a country's agricultural land per capita.

for non-homoskedastic error in case of one endogenous variable and one instrument (Andrews et al. 2019) — is higher than the associated Stock-Yogo critical value.

Our IV results support the findings obtained with OLS (Table 2): the IV estimates indicate a significant negative effect of EPS changes for all four instruments with similar-sized coefficients. However, the magnitude of the IV coefficient estimates is (more than) three times larger than the OLS estimate, which suggests that OLS estimates are biased towards zero. This is an informative outcome given that the direction of bias is ambiguous *ex-ante*.

To test the validity of our instruments, we run several checks. First, we test whether the instruments have a direct effect on popular support by including them stepwise as additional controls in the baseline model. If the coefficients turn out to be significant, one can argue that the instruments are part of the error term and thus do not satisfy the exclusion restriction. As the results show (Table A2.6), this is not the case, since all four instruments turn out to be insignificant. Second, instead of regressing popular support on the instruments, we also directly test the association of the baseline residuals and the instrument. Again, the relationship is indistinguishable from zero (Table A2.7), which supports the validity of our instruments.

Table 2: The effect of EPS changes on popular support of the government using instrumental variable regressions

Variables	(1)	(2)	(3)	(4)
change in EPS index	**-0.994*** (0.386)**	**-1.047** (0.533)**	**-0.635* (0.341)**	**-0.879** (0.377)**
Observations	370	370	370	361
Instrument: product of				
- global term (varying)	# of floods	# of people affected by earthquake	# of major hurricanes	# of wild fires
- country term (constant)	coast length	share of urban extent in coastal zone	distance from centroid to nearest coast	agricultural land per capita
1st-stage coef.	0.003*** (0.001)	0.016*** (0.004)	-7.552*** (1.165)	9.580*** (2.542)
1st-stage F-Stat	30.79	14.98	42.05	14.21
Year Fixed Effects	YES	YES	YES	YES
Country Fixed Effects	YES	YES	YES	YES

Note: The outcome variable is popular support of the government. Estimates are based on $y_{i,t} = \beta\Delta\widehat{EPS}_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \epsilon_{i,t}$ with $\Delta\widehat{EPS}_{i,t} = \vartheta S_{i,t} + \varphi X_{i,t} + \alpha_i + \gamma_t + \upsilon_{i,t}$. Columns differ with respect to the instrumental variable being used. All estimations include country and year fixed effects. The main independent variable of interest is the change in the EPS index (ranging from 1 (low stringency) to 6 (high stringency)). Control variables have been partialled out. A coefficient of -0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Third, it might well be that our global term is associated with other global factors that affect political support. For instance, climate change pressure might be associated with global trends in political conflict or aversion towards globalization. These factors could affect popular support via country-specific factors spuriously related to countries' vulnerability to climate change. To test for this concern, we augment the baseline IV specification by including the interaction between length of the coastline with global factors such as: the overall globalization index from the KOF dataset (see Gygli et al. 2019), which is an aggregate of economic, social and political dimensions of globalization; the global average of the weighted conflict index from the ICRG dataset; and the global average of the riot index from the ICRG dataset (Table A2.8).

Fourth, it is possible that our country specific term is associated with country characteristics (such as country size) that may affect political support through factors spuriously related to global pressure for climate change (such as globalization). For example, globalization may be more relevant for smaller countries and the popular support for globalization policies for smaller economies may be different than for larger economies. To address this concern, we further control for the interaction between the number of flood events and country size and the interaction of the globalization index and country size. The results are similar to, and not statistically different from, the baseline IV results (Table A2.8).

Instrument Types

Beyond the average effect of CCPs on popular support for the government, we test whether the political costs vary across instrument types. This is important as the literature portrays taxes on emissions as the most effective tool to reduce global greenhouse gases (Jenkins 2014; Goulder and Parry 2008; Goulder et al. 2019; Stiglitz 2019; IMF 2019). At the same time, carbon taxes play only a limited role in national environmental legislation (Beiser-McGrath and Bernauer 2019; Carattini et al. 2018). Thus, we test whether governments rationally hesitate to adopt market-based measures to avoid political costs. To do so, we re-estimate the baseline specification for each sub-component of the EPS indicator to test whether increasing environmental policy stringency via market-based instruments (i.e., taxes on emission) has a larger negative effect on popular support than using non-market-based instruments (i.e., emission limits).

The results suggest important differences between market-based and nonmarket-based instruments (Table 3). Market-based measures, and especially taxes on emissions, are broadly consistent with the baseline results pointing to negative consequences on governmental support. In contrast, non-market-based measures are typically not statistically significant. One interpretation is that price mark-ups are more visible to consumers than supply limits. The fact that households are constantly confronted by fuel prices—for instance through household energy bills or at the gasoline station—makes it easy for them to trace the price mark-up back to tax rises. In contrast, non-market measures either do not spark nearly the same level of public attention, or limits translate into price changes only with a lag. Thus, using non-market-based measures — which overall are still efficient ways to reduce carbon emissions (IMF 2019) — seem to stand a reasonable chance of escaping political blame (Weaver 1986; Pierson 1994) and thus of overcoming the political cost of CCPs. An exception is the EPS on diesel. While diesel tax changes do not have a significant effect on popular support, nonmarket-based measures on diesel do (Table A2.9).

Table 3: The effect of EPS changes on popular support of the government, market-based vs. non market-based EPS

Variables	(1)	(2)	(3)	(4)
change in market-based EPS index	-0.153**			
	(0.074)			
change in tax-based EPS index		-0.199**		
		(0.092)		
change in non-market-based EPS index			-0.066	
			(0.048)	
change in limit-based EPS index				-0.004
				(0.065)
public deficit %GDP	0.038***	0.039***	0.039***	0.039***
	(0.013)	(0.013)	(0.014)	(0.013)
share of elderly	0.166***	0.161***	0.149***	0.149***
	(0.053)	(0.051)	(0.050)	(0.051)
GDP growth	0.064**	0.058**	0.061**	0.061**
	(0.028)	(0.027)	(0.027)	(0.027)
Constant	0.621	0.684	0.835	0.819
	(0.647)	(0.628)	(0.600)	(0.611)
Observations	370	373	378	378
R-squared	0.452	0.445	0.444	0.441
Number of countries	30	30	30	30
Year Fixed Effects	YES	YES	YES	YES
Country Fixed Effects	YES	YES	YES	YES

Note: The outcome variable is popular support of the government. Estimates are based on $y_{i,t} = \beta \Delta EPS_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \epsilon_{i,t}$. Columns differ with respect to the choice of the sub-index of EPS (each ranging from 1 (low stringency) to 6 (high stringency)). A coefficient of -0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. All estimations include country and year fixed effects. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Heterogeneity of political costs of CCP

Empirical Analysis

Beyond the direct effects, we test five factors that might mitigate the political costs of CCPs. First, global energy prices (FRED 2020a; FRED 2020b) might play an essential role in how citizens perceive stricter climate change legislation. This is because environmental policy measures create economic costs through price or quantity changes which consumers have to absorb. Second, the political consequences of CCPs might be larger when their adoption is more visible to voters. Thus, we test whether adopting CCPs is politically more costly towards the end of the legislative term when governmental decisions are more in the public eye during campaign periods. Third, because CCPs impact especially emission-intensive sectors (Fankhaeser et al. 2008), we expect those countries with a large input share of non-green (dirty) to experience more political headwinds. Here, we proxy the dependence of non-green input factors by the value-added share of dirty-energy mining industries (OECD 2018). Fourth, the political costs of CCPs may also depend on the initial status-quo of environmental protection compared to other countries. To test for these potential non-linearities, we use the value of the EPS index for each country to examine whether governments with strict environmental standards in place experience larger political costs when adopting CCPs. Fifth, we look at whether CCPs are politically more costly at times when income inequality is rising and in the absence of social insurance mechanisms. To proxy the shape of the income distribution, we use (market- and net-based) Gini coefficient as well as different income shares (OECD 2017; WID 2020). To investigate the mediating role of social benefits, we use indicators capturing social benefits to households (% GDP) as well as social expenditure on active labor market policies and unemployment benefits (% GDP) based on data from Adema et al. (2011).

To test these hypotheses, we depart from the following specification:

$$y_{i,t} = F(z_{i,t}) * \left[\beta^L \Delta EPS_{i,t} + \theta^L X_{i,t}\right] + (1 - F(z_{i,t})) * \left[\beta^H \Delta EPS_{i,t} + \theta^H X_{i,t}\right] + \alpha_i + \gamma_t + \epsilon_{i,t}$$

with

$$F(z_{i,t}) = \frac{e^{-1.5 * z_{i,t}}}{1 + e^{-1.5 * z_{i,t}}}$$

where z is the z-score (normalized to have zero mean and unit variance) of the following variables (*M*): (i) oil and gas price; (ii) value added share of dirty-energy mining industries (mining and oil); (iii) income shares and inequality measures; and (iv) social expenditure in percent of GDP. $z_{i,t} = \left(\frac{\bar{M}_{it} - \bar{M}_i}{\sigma_{Mi}}\right)$.[37] $F(z_{it})$ is the smooth transition function of the variable *z*. The coefficients β^L and β^H capture the impact of changes in EPS in cases of low M ($F(z_{it}) \approx 1$ when *z* goes to minus infinity) and high M ($(1 - F(z_{it}) \approx 1$ when z goes to plus infinity), respectively.[38]

The use of the smooth transition function is equivalent to the smooth transition autoregressive (STAR) model developed by Granger and Teravirta (1993) to assess non-linear effects above/below a given threshold or regime. The main advantage of this approach relative to estimating SVARs for each regime is that it uses a larger number of observations to compute the effects, improving the stability and precision of the estimates.

[37] Thus, we look at within-country variation over time. Since the importance of the dirty energy mining industry and the initial levels of EPS is influenced by CCPs, we address this endogeneity issue by looking at *cross-country* variation—defining $z_{i,t} = \left(\frac{M_{i_{first}} - \bar{M}}{\sigma_M}\right)$. In other words, we use the first value that is available in our dataset.

[38] $F(z_{it})$=0.5 is the cut-off between the weak and strong regimes. The approach is similar to considering a dummy variable that takes value 1 when the variable is about the country-specific mean—that is, $F(z_{it})$>=0.5, and zero otherwise. The difference is that instead of considering two discrete values (0 and 1), the smooth transition approaches allow the regimes to continuously vary between 0 and 1.

In addition, this estimation strategy can handle the potential correlation of the standard errors within countries more easily by clustering at the country level.

Period characteristics

Starting with the results for fuel prices, we find that the effect of CCPs on governmental support depends on gasoline and oil market conditions, consistent with both sources of energy being the main sources of household energy consumption in OECD countries (Eurostat 2020). In times of high oil and gasoline prices, the political damage from CCPs is statistically significantly negative—with coefficients 1.5 to 2 times as large as the direct effect in our baseline (Table 4). In contrast, the effect is not statistically different from zero when EPS rises in times of low global fuel prices. This implies that timing plays a substantial role from a political standpoint: governments can mitigate the political costs to a large extend by passing new legislation when fuel market conditions are favourable. This said, our estimation strategy only allows us to draw conclusions about fuel prices in the year of CCP implementation. Thus, it could be that governments are penalized in subsequent years should fuel prices rise again.

Table 4: The effect of EPS changes on popular support of the government mediated by fuel prices, electoral timing, dirty energy dependence, and initial level of EPS

Variables	(1)	(2)	(3)	(4)	(5)
change in EPS index x low oil price	-0.029 (0.272)				
change in EPS index x high oil price	-0.320** (0.129)				
change in EPS index x low gas price		0.222 (0.301)			
change in EPS index x high gas price		-0.493*** (0.170)			
change in EPS index x low # of years until election			-0.322** (0.153)		
change in EPS index x high # of years until election			-0.204 (0.136)		
change in EPS index x low share of dirty energy				-0.200 (0.119)	
change in EPS index x high share of dirty energy				-0.264** (0.119)	
change in EPS index x low initial EPS level					-0.145 (0.113)
change in EPS index x high initial EPS level					-0.321*** (0.076)
public deficit %GDP	0.039*** (0.014)	0.040*** (0.013)	0.037*** (0.014)	0.040*** (0.009)	0.039*** (0.009)
share of elderly	0.165*** (0.052)	0.165*** (0.050)	0.111 (0.095)	0.163*** (0.054)	0.160*** (0.051)
GDP growth	0.063** (0.028)	0.061** (0.028)	0.055** (0.026)	0.063*** (0.019)	0.065*** (0.019)
Constant	0.634 (0.633)	0.628 (0.614)	1.343 (1.170)	-0.681 (0.896)	0.000 (0.000)
Observations	370	370	329	370	366
R-squared	0.456	0.460	0.466	0.337	0.338
Number of countries	30	30	30	30	29
Year Fixed Effects	YES	YES	YES	YES	YES
Country Fixed Effects	YES	YES	YES	YES	YES
p-value of coefficient equality	0.428	0.105	0.604	0.751	0.234

Note: The outcome variable is popular support of the government. Estimates are based on $y_{i,t} = \beta \Delta\widehat{EPS}_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \epsilon_{i,t}$ with $\Delta\widehat{EPS}_{i,t} = \vartheta S_{i,t} + \varphi X_{i,t} + \alpha_i + \gamma_t + \upsilon_{i,t}$. Columns differ with respect to the choice of the mediating variable. A coefficient of -0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. All estimations include country and year fixed effects. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Overall, one can argue that adopting new measures when fuel prices are low gives an offsetting effect from more stringent environmental regulation on domestic budget constraints. In addition, there is a well-established literature on an inverted U-shaped relation between incomes and support for CCPs—a kind of environmental Kuznets curve (Dinda 2004): environmental damage increases initially with rising levels of income per capita, but then diminishes. There is also micro evidence that support for CCPs rises with incomes, and so a greater willingness-to-pay for green policies (Carlsson and Johansson-Stenman 2000; Franzen 2003; Kotchen et al. 2013; see Torgler et al. 2007). So, it is plausible that relaxing budget constraints through real income gains in times of low fuel prices could increase support for CCPs among voters.

Beyond contemporaneous fuel prices, our estimates indicate that political costs are only present when they are adopted towards the end of the legislative term (Table 4). In other words, governments can mitigate some of the political backlash when they adopt CCPs directly after elections. This finding is in line with the economic voting literature on voters' recency bias indicating that individuals base their voting decision more strongly on current policy outcomes prior to elections, while disregarding earlier legislative changes at the beginning of the term (see Alesina et al. 2021). In addition to electoral timing, we also test whether the partisanship of the government alters the political costs of CCPs. Indeed, the results show that left-wing and centre-governments are punished more strongly for increasing environmental policy stringency, while the coefficient is not significant for right-wing incumbents (Table A2.10). These ideological differences are in line with the related literature on welfare state retrenchment showing that the governments are punished to different extents along partisan lines (Horn 2021): as workers in non-green (dirty) energy-dependent sectors are traditionally tied to left-wing parties, the political costs are particularly large when they adopt stricter environmental measures.

Country characteristics

With respect to industrial composition, our results show that in countries with a large share of dirty-energy sectors such as Norway and Indonesia, more stringent CCPs garner larger political costs (Table 4). In contrast, the effect is not significant for countries where dirty-energy sectors have a very low weight in national input shares such as in Slovenia and Ireland. As employees in dirty-energy sectors are adversely affected by more stringent EPS, governments in countries with large employment shares in such industries are expected to experience stronger resistance against CCPs among the electorate. While CCPs would also create new employment opportunities, transition costs could be substantial, and matching skills in declining sectors to new job opportunities may be difficult.

In addition to the industrial composition, we also find that political costs are higher in countries which started from stricter levels of environmental legislation in the early 2000s (Table 4). This finding points towards potential non-linear effects from a cross-sectional perspective: voters from environmental-friendly countries penalize the government for stricter CCPs when they feel that their economy is losing out against others which do not undertake necessary action against global warming. This finding highlights that international coordination of climate-related policymaking facilitates the adoption of necessary measures also at the national level. Finally, we test whether left-wing parties –

which have historically a strong commitment to industrial workers (Kono 2020) – face greater political costs when adopting stricter CCPs. Indeed, our results provide evidence in favor of this hypothesis (Table A2.10).

Inequality and Social Insurance

Table 5: The effect of EPS changes on popular support of the government mediated by inequality (Gini coefficients)

Variables	(1)	(2)
change in EPS index x lower market-based GINI coefficient	-0.066	
	(0.279)	
change in EPS index x higher market-based GINI coefficient	-0.380**	
	(0.170)	
change in EPS index x lower net-based GINI coefficient		-0.006
		(0.264)
change in EPS index x higher net-based GINI coefficient		-0.402**
		(0.162)
public deficit %GDP	0.038***	0.039***
	(0.014)	(0.014)
share of elderly	0.099	0.103
	(0.094)	(0.093)
GDP growth	0.057**	0.057**
	(0.026)	(0.026)
Constant	1.487	1.445
	(1.149)	(1.139)
Observations	326	326
R-squared	0.470	0.470
Number of countries	30	30
Year Fixed Effects	YES	YES
Country Fixed Effects	YES	YES
p-value of coefficient equality	0.442	0.301

Note: The outcome variable is popular support of the government. Estimates are based on $y_{i,t} = \beta \Delta \widehat{EPS}_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \epsilon_{i,t}$ with $\Delta \widehat{EPS}_{i,t} = \vartheta S_{i,t} + \varphi X_{i,t} + \alpha_i + \gamma_t + \upsilon_{i,t}$. Columns differ with respect to the choice of the mediating variable. A coefficient of -0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. ALMP and UB refers to active labor market programs and unemployment benefits, respectively. All estimations include country and year fixed effects. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table 6: The effect of EPS changes on popular support of the government mediated by inequality (income shares)

Variables	(1)	(2)	(3)	(4)	(5)	(6)
change in EPS index x low top 1% income share	0.023 (0.192)					
change in EPS index x higher top 1% income share	-0.300* (0.156)					
change in EPS index x lower top 10% income share		0.033 (0.182)				
change in EPS index x higher top 10% income share		-0.320** (0.155)				
change in EPS index x lower top 20% income share			0.022 (0.151)			
change in EPS index x higher top 20% income share			-0.320** (0.146)			
change in EPS index x lower bottom 1% income share				0.010 (0.264)		
change in EPS index x higher bottom 1% income share				-0.254 (0.314)		
change in EPS index x lower bottom 10% income share					-0.304* (0.156)	
change in EPS index x higher bottom 10% income share					0.021 (0.210)	
change in EPS index x higher bottom 20% income share						-0.352** (0.150)
change in EPS index x higher bottom 20% income share						0.082 (0.198)
Constant	0.278 (0.907)	0.252 (0.912)	0.227 (0.918)	0.442 (0.928)	0.269 (0.912)	0.208 (0.907)
Observations	301	301	301	259	301	301
R-squared	0.513	0.513	0.513	0.524	0.513	0.514
Number of countries	24	24	24	21	24	24
Year Fixed Effects	YES	YES	YES	YES	YES	YES
Country Fixed Effects	YES	YES	YES	YES	YES	YES
p-value of coefficient equality	0.297	0.235	0.176	0.633	0.331	0.164

Note: The outcome variable is popular support of the government. Estimates are based on $y_{i,t} = \beta \Delta \widehat{EPS}_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \epsilon_{i,t}$ with $\Delta \widehat{EPS}_{i,t} = \vartheta S_{i,t} + \varphi X_{i,t} + \alpha_i + \gamma_t + \upsilon_{i,t}$. Columns differ with respect to the choice of the mediating variable. A coefficient of -0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. In addition to the baseline control variables (not shown due to space constraints), all estimations include country and year fixed effects. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Our results show that CCPs decrease the popular support for the government when income inequality is increasing, while stricter environmental legislation has no effect when inequality is declining (Table 5 and 6). This finding is robust to various measures of income inequality, including Gini coefficients and income shares. The only exception concerns the bottom 1%-income share, for which we do not find significant effects. However, overall, the results are in line with our hypothesis: since stricter EPS impacts prices of basic goods which by their nature are not easily substitutable and have a low-price elasticity of demand (Eitches and Crain 2016), a price jump for such products affects particularly households at the lower end of the income distribution (Rao 2013)[39].

To test whether the provision of social insurance can counteract potential concerns related to economic hardship among adversely affected groups, we examine the role of social benefits to households as well as social expenditure on ALMPs and unemployment benefits (Table 7). In line with our hypothesis, our results show that increasing social benefits to households on active labor market policies and unemployment benefits reduces (and in some cases eliminates) the political costs of CCPs.

[39] This is in line with a range of empirical studies showing that preferences towards environmental policies varies across income groups (Carlsson and Johansson-Stenman 2000; Franzen 2003; Kotchen et al. 2013; see Torgler et al. 2007).

Table 7: The effect of EPS changes on popular support of the government mediated by social benefits to households as well as social expenditure on ALMP and unemployment benefits

Variables	(1)	(2)	(3)	(4)
change in EPS index x	-0.463***			
lower in-cash social benefits	(0.164)			
change in EPS index x	0.006			
higher in-cash social benefits	(0.155)			
change in EPS index x		-0.512**		
lower in-kind social benefits		(0.247)		
change in EPS index x		-0.053		
higher in-kind social benefits		(0.146)		
change in EPS index x			-0.483***	
lower social expend. on ALMP			(0.158)	
change in EPS index x			0.084	
higher social expend. on ALMP			(0.153)	
change in EPS index x				-0.544***
lower social expenditure on UB				(0.170)
change in EPS index x				0.155
higher social expenditure on UB				(0.218)
public deficit %GDP	0.041***	0.039***	0.033**	0.034***
	(0.014)	(0.012)	(0.014)	(0.013)
share of elderly	0.165***	0.144***	0.156**	0.171**
	(0.051)	(0.048)	(0.062)	(0.070)
GDP growth	0.065**	0.061**	0.047**	0.050*
	(0.029)	(0.028)	(0.023)	(0.027)
Constant	0.650	0.939	0.832	0.666
	(0.612)	(0.576)	(0.743)	(0.827)
Observations	370	340	334	321
R-squared	0.459	0.470	0.462	0.498
Number of countries	30	28	27	26
Year Fixed Effects	YES	YES	YES	YES
Country Fixed Effects	YES	YES	YES	YES
p-value of coefficient equality	0.0947	0.187	0.0405	0.0544

Note: The outcome variable is popular support of the government. Estimates are based on $y_{i,t} = \beta \Delta\widehat{EPS}_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \epsilon_{i,t}$ with $\Delta\widehat{EPS}_{i,t} = \vartheta S_{i,t} + \varphi X_{i,t} + \alpha_i + \gamma_t + \upsilon_{i,t}$. Columns differ with respect to the choice of the mediating variable. A coefficient of -0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. ALMP and UB refers to active labor market programs and unemployment benefits, respectively. All estimations include country and year fixed effects. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Endogeneity

We extend the IV estimation to include the mediating factors described previously. The setup is similar to the regime-dependent IV approach of Ramey and Zubairy (2018):

$$y_{i,t} = \beta \Delta \widehat{EPS^D}_{\iota,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \epsilon_{i,t}$$

with

$$\Delta \widehat{EPS^D}_{\iota,t} = \vartheta \left[F^D(Z) * S_{i,t} \right] + \varphi X_{i,t} + \alpha_i + \gamma_t + \upsilon_{i,t}$$

while $D \in \{H, L\}$.

The IV results in the extended model support our previous OLS estimates. Adopting CCPs in times of high oil and gas prices (Table A2.11), as well as closer to elections, raises political costs (Table A2.12). Likewise, a higher dirty-energy input share raises political costs, which are three times larger compared to low-dirty-energy-input-share economies (Table A2.12). We also find causal evidence that the political costs of CCPs are higher in countries which had already higher levels of environmental protection in the early 2000s (Table A2.12). With respect to inequality, the IV estimates show that CCPs are larger when inequality is higher (Table A2.13, A2.14, and A2.15). Finally, the findings on the mediating effect of social benefits to households as well as social expenditure on ALMPs and unemployment benefits are also in line with the baseline estimates (Table A2.16 and A2.17).

Conclusion

While our results confirm that CCPs on average may undercut the popularity of governments on average, we show that there are effective mitigating strategies that limit or even remove the detrimental political consequences.

First, the type of CCP matters. While market-based measures (i.e. emission taxes) lower government popularity substantially, the political cost is not significant for non-market-based instruments (e.g., emission limits). Since economists consider market-based instruments to be most efficient, it is important to internalize that such measures might also be relatively costly in political terms. Yet, as non-market-based measures remain viable instruments to reduce carbon emissions (IMF 2019), such second-best options are efficient alternatives to tackle global warming (Stiglitz 2019).

Second, the consequences of tightening environmental regulation seem to be more visible when the changes adjust energy prices – especially energy and fuel prices for households. Thus, adopting CCPs when world energy prices are low can provide an effective avenue for overcoming political-economy challenges. We have also shown that adopting stricter environmental legislation prior to elections entails greater political costs. This finding suggests that visibility and salience of reforms – which is higher in campaign periods – condition the political costs of legislative changes.

Third, inequality concerns play a key role for the feasibility of CCPs since the economic burden from CCPs are concentrated among groups with weaker initial conditions and less resilience (Känzig 2021). Our results show that when CCPs are adopted in times of increasing inequality, political costs are magnified. However, redistributive instruments targeted at individuals experiencing higher economic insecurity are viable strategies to overcome the political fallout from CCPs and support governments to take necessary action without risking losing office.

Climate change will be on the global policy agenda for years to come. As with all policies that generate winners and losers, CCPs require political support to be viable. Rational governments will continue to hesitate and delay because the political damage is palpable. Overcoming this bad equilibrium of inaction is urgent. Our hope is that the evidence and strategies identified in this paper may provide some guidance on ways forward.

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Appendix

Table A1.1: Description and summary statistics of variables

Variables	Source	N	mean	max	min
index of popular support for the government parties	ICRG	465	2.223	3.792	0.500
annual average poll of incumbent party	Jennings	279	35.40	57.20	7.113
vote share of government parties	ICRG	712	44.80	97.58	0
vote share of largest government party	ICRG	656	35.27	94	0
index of environmental policy index (EPS)	OECD	730	1.634	4.133	0.292
market-based EPS index	OECD	730	1.093	3.983	0
non-market-based EPS index	OECD	755	2.138	5.500	0.500
tax-based EPS index	OECD	733	1.370	4	0
limit-based EPS index	OECD	755	2.307	6	0
taxation on CO2 EPS index	OECD	755	0.140	6	0
taxation on Diesel EPS index	OECD	733	3.701	6	0
taxation on Nox EPS index	OECD	755	0.723	6	0
taxation on SOx EPS index	OECD	755	0.874	6	0
emission limits on Diesel EPS index	OECD	755	3.160	6	0
emission limits on NOx EPS index	OECD	755	2.428	6	0
emission limits on PM EPS index	OECD	755	1.421	6	0
emission limits on SOx EPS index	OECD	755	2.220	6	0
aggregated feed-in-tariffs EPS index	OECD	755	1.258	6	0
feed-in-tariffs for solar EPS index	OECD	755	1.224	6	0
feed-in-tariffs for wind EPS index	OECD	755	1.293	6	0
aggregated trading scheme EPS index	OECD	752	0.588	5.200	0
trading scheme for CO2 EPS index	OECD	752	0.916	6	0
trading scheme for Energy Efficiency Certificate EPS index	OECD	755	0.159	6	0
trading scheme for Reneable Energy Certificate EPS index	OECD	755	0.473	6	0
R&D subsidies for renewable energy EPS index	OECD	755	1.968	6	0
aggregated R&D subsidies EPS index	OECD	755	1.968	6	0
general government deficit (as % of GDP)	OECD	610	-2.686	18.63	-32.06
share of the elderly (65+) (as % of total population)	OECD	806	13.51	26.65	3.458

growth rate of GDP at market prices (LCU)	WB	798	2.533	25.16	-13.13
unemployment, total (% of total labor force)	WB	775	8.379	33.47	1.777
consumer price index	OECD	747	0.877	2.616	0.0698
vote share of government parties	Alesina	643	-2.532	27.23	-29.86
average tax wedge of couple with 2 children	OECD	563	31.68	50.56	8.019
domestic financial liberalization indicator	Alesina	754	0.837	1	0.111
product market liberalization indicator	Alesina	754	0.543	1	0
current account reform indicator	Alesina	754	0.894	1	0.250
capital account reform indicator	Alesina	754	0.856	1	0.250
labor market liberalization indicator	Alesina	722	0.628	1	0.298
global price for brent crude	FRED	806	47.79	112.0	12.72
global price of natural gas	FRED	775	3.950	8.895	1.451
value-added weighted mean of input share of "mining and extraction of energy producing products" and "Coke and refined petroleum products" for other industries	OECD	465	0.0272	0.265	0.0004
share of pre-tax national income of top 1%	WID	519	0.113	0.296	0.0445
share of pre-tax national income of top 10%	WID	519	0.346	0.651	0.229
share of pre-tax national income of top 20%	WID	519	0.493	0.792	0.377
share of pre-tax national income of bottom 1%	WID	519	$-4.9*10^{-5}$	0.00180	-0.0057
share of pre-tax national income of bottom 10%	WID	519	0.0104	0.0265	-0.0195
share of pre-tax national income of bottom 20%	WID	519	0.0438	0.0784	0.0125
market-based Gini coefficient	OECD	705	46.70	69.40	28.23
net Gini coefficient	OECD	705	31.61	59.40	17.55
total public social expenditure as percentage of GDP	OECD	683	20.03	34.18	2.584
public social expenditure for ALPM as % of GDP	OECD	684	0.616	2.714	0.00400
public social expenditure for unemployment as % of GDP	OECD	655	1.056	4.643	0
years left in current term	ICRG	705	1.780	5	0
social benefits to households (in-cash)	OECD	627	12.74	23.12	0.0314
social benefits to households (in-kind)	OECD	585	11.08	19.39	4.240

index of globalization (total) (global average)	ICRG	806	53.80	61.75	43.63
index of total conflict (global average)	ICRG	775	808.9	2,891	179.4
index of riot	ICRG	775	0.678	3.839	0.0323
(log) population	WEO	796	3.251	7.152	0.682
number of people affected by earthquakes (global)	WB	496	$6.103*10^6$	$4.672*10^7$	786,413
percentage of urban extent in coastal zone (km2) affected by sea level elevation	CIESIN	496	0.00731	0.082	0
number of floods (global)	EMDAT	496	165.3	226	128
length of coastline	WRI	496	28,511	265,523	0
frequency of major hurricanes in the North Atlantic	Hurrican Database	496	3.188	7	0
distance from centroid of country to nearest coast (km)	PSU	496	250.2	1,713	2.944
number of wildfires (global)	EMDAT	496	11.81	30	4
agricultural land (km2) per capita	NM	480	12.56	208.2	0.365

Table A2.1: The effect of EPS changes on poll of incumbent party

Variables	(1)	(2)	(3)	(4)	(5)	(6)
change in EPS index	**-2.638***	**-3.173****	**-3.06*****	**-2.662****	**-2.480****	**-2.500****
	(1.368)	**(1.435)**	**(1.160)**	**(1.161)**	**(0.997)**	**(1.228)**
public deficit %GDP	0.349**	0.154	0.146	0.152	0.179	0.204
	(0.172)	(0.169)	(0.116)	(0.125)	(0.122)	(0.145)
share of elderly	0.327	0.448	0.702	0.752	0.835*	1.275
	(0.715)	(0.750)	(0.546)	(0.529)	(0.472)	(1.067)
GDP growth	0.684**	0.369	0.506	0.422	0.513*	0.441
	(0.289)	(0.328)	(0.326)	(0.301)	(0.270)	(0.298)
unemployment rate		-0.830**	-0.787**	-0.802**	-0.629*	-0.701**
		(0.369)	(0.346)	(0.348)	(0.381)	(0.357)
CPI			43.52**	47.49**	53.41***	61.50***
			(20.384)	(20.763)	(18.582)	(21.029)
gov. parties' vote share				0.021	-0.034	-0.042
				(0.160)	(0.170)	(0.161)
average tax wedge					-0.078	-0.441
					(0.453)	(0.453)
financial reform index						43.666
						(43.618)
product reform index						-6.838
						(7.630)
current account reform index						59.327
						(102.600)
capital account reform index						1.459
						(12.370)
labour reform index						27.261
						(34.318)
constant	33.061***	37.900***	15.026	12.259	8.282	-106.580
	(10.960)	(11.883)	(15.680)	(15.940)	(23.572)	(94.927)
Observations	239	239	238	233	226	223
R-squared	0.683	0.703	0.724	0.721	0.716	0.734
Number of countries	23	23	23	21	21	21
Year Fixed Effects	YES	YES	YES	YES	YES	YES
Country Fixed Effects	YES	YES	YES	YES	YES	YES

Note: The outcome variable is annual average poll of the incumbent party. Estimates are based on $y_{i,t} = \beta \Delta EPS_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \epsilon_{i,t}$. Columns differ with respect to control variable set. All estimations include country and year fixed effects. The main independent variable of interest is change in the EPS index (ranging from 1 (low stringency) to 6 (high stringency)). A coefficient of -0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A2.2: The effect of EPS changes on vote share of the government

Variables	(1)	(2)	(3)	(4)	(5)
change in EPS index	**-11.361**** **(4.457)**	**-11.351**** **(4.739)**	**-10.994**** **(4.989)**	**-11.514*** **(5.660)**	**-12.648**** **(5.927)**
(lag) gov. parties' vote share	-0.284 (0.176)	-0.284 (0.178)	-0.227 (0.219)	-0.061 (0.195)	-0.276 (0.214)
public deficit %GDP	0.136 (0.284)	0.140 (0.355)	0.051 (0.381)	0.059 (0.401)	0.204 (0.528)
share of elderly	0.378 (1.953)	0.378 (1.964)	0.066 (2.259)	0.766 (2.350)	-0.071 (1.978)
GDP growth	-0.324 (0.293)	-0.324 (0.292)	-0.244 (0.281)	-0.612* (0.335)	-0.953* (0.461)
unemployment rate		0.010 (0.332)	-0.034 (0.346)	0.130 (0.301)	-0.292 (0.356)
CPI			-15.707 (19.805)	-24.907 (27.860)	-47.533 (37.104)
average tax wedge				0.160 (0.626)	0.605 (0.811)
financial reform index					-25.116 (25.936)
product reform index					7.024 (15.660)
current account reform index					41.159* (23.036)
capital account reform index					17.835 (25.089)
labour reform index					13.569 (26.893)
constant	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	38.002 (37.062)
Observations	143	143	142	131	123
R-squared	0.261	0.261	0.255	0.239	0.304
Number of countries	30	30	30	26	24
Year Fixed Effects	YES	YES	YES	YES	YES
Country Fixed Effects	YES	YES	YES	YES	YES

Note: The outcome variable is vote share of the government parties. Estimates are based on $y_{i,t} = \beta \Delta EPS_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \epsilon_{i,t}$. Columns differ with respect to control variable set. All estimations include country and year fixed effects. The main independent variable of interest is change in the EPS index (ranging from 1 (low stringency) to 6 (high stringency)). A coefficient of -0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. Standard deviations based on Driscoll-Kraay standard errors with two lags in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A2.3: The effect of EPS changes on vote share of the largest government party

Variables	(1)	(2)	(3)	(4)	(5)
change in EPS index	**-4.985******	**-5.268******	**-5.850******	**-5.387****	**-7.163****
	(1.573)	**(1.803)**	**(1.894)**	**(1.894)**	**(2.813)**
(lag) largest gov. parties' vote share	-0.217*	-0.228*	-0.274**	-0.307**	-0.348***
	(0.120)	(0.119)	(0.111)	(0.109)	(0.104)
public deficit %GDP	0.071	-0.017	0.047	-0.010	-0.043
	(0.357)	(0.412)	(0.325)	(0.308)	(0.334)
share of elderly	-0.755	-0.746	-0.500	-0.627	-0.778
	(0.879)	(0.912)	(0.870)	(0.954)	(1.293)
GDP growth	-0.577***	-0.596***	-0.681***	-0.641**	-0.207
	(0.180)	(0.182)	(0.236)	(0.284)	(0.585)
unemployment rate		-0.239	-0.168	-0.206	-0.414**
		(0.225)	(0.188)	(0.226)	(0.152)
CPI			18.228	23.117	58.271
			(12.536)	(18.801)	(37.039)
average tax wedge				0.262	0.509
				(0.268)	(0.337)
financial reform index					-50.398***
					(12.311)
product reform index					6.987*
					(3.933)
current account reform index					24.883**
					(10.819)
capital account reform index					-7.121
					(10.641)
labour reform index					6.610
					(26.830)
constant	0.000	0.000	34.076*	0.000	0.000
	(0.000)	(0.000)	(18.460)	(0.000)	(0.000)
Observations	131	131	130	122	115
R-squared	0.234	0.242	0.300	0.324	0.423
Number of countries	28	28	28	25	23
Year Fixed Effects	YES	YES	YES	YES	YES
Country Fixed Effects	YES	YES	YES	YES	YES

Note: The outcome variable is vote share of the largest government party. Estimates are based on $y_{i,t} = \beta \Delta EPS_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \epsilon_{i,t}$. Columns differ with respect to control variable set. All estimations include country and year fixed effects. The main independent variable of interest is change in the EPS index (ranging from 1 (low stringency) to 6 (high stringency)). A coefficient of -0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. Standard deviations based on Driscoll-Kraay standard errors with two lags in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A2.4: The effect of EPS changes on poll of incumbent party using WALS and BMA

Variables	(1)			(2)		
	Coef.	Std. Err.	t-value	Coef.	Std. Err.	t-value
change in EPS index	**-0.262****	**(0.102)**	**-2.56**	**-0.262****	**(0.101)**	**-2.60**
public deficit %GDP	0.006	(0.013)	0.50	0.000	(0.003)	0.10
share of elderly	0.165*	(0.092)	1.79	0.146	(0.130)	1.13
GDP growth	0.024	(0.019)	1.28	0.002	(0.009)	0.23
unemployment rate	-0.061***	(0.019)	-3.19	-0.084***	(0.017)	-5.04
CPI	0.302	(1.181)	0.26	-0.011	(0.268)	-0.04
gov. parties' vote share	-0.004	(0.005)	-0.67	-0.000	(0.002)	-0.13
average tax wedge	-0.029	(0.034)	-0.84	-0.004	(0.017)	-0.26
financial reform index	0.617	(1.266)	0.49	0.045	(0.386)	0.12
product reform index	-0.259	(0.479)	-0.54	-0.046	(0.216)	-0.22
current account reform index	-4.506*	(2.547)	-1.77	-3.358	(3.496)	-0.96
capital account reform index	0.325	(1.133)	0.29	0.060	(0.369)	0.16
labour reform index	-0.670	(1.648)	-0.41	-0.022	(0.403)	-0.05
share of green parties in parliament	-0.005	(0.021)	-0.22	0.000	(0.005)	0.02
partisanship of chief executive of gov.	-0.015	(0.035)	-0.42	-0.001	(0.011)	-0.09
margin of majority	-0.076	(0.628)	-0.12	0.003	(0.159)	0.02
Herfindahl index of opposition parties	0.009	(0.294)	0.03	-0.008	(0.080)	-0.11
index of democratic accountability	-0.282*	(0.151)	-1.87	-0.235	(0.232)	-1.01
index of legislative strength	0.270***	(0.075)	3.58	0.387***	(0.076)	5.08
couple's net household income (levels)	0.000	(0.000)	0.32	0.000	(0.000)	0.07
couple's net household income (change)	-0.000	(0.000)	-0.34	-0.000	(0.000)	-0.18
constant	6.150	(3.812)	1.61	4.588	(4.399)	1.04
Number of countries	232			232		
Model	WALS			BMA		
Year Fixed Effects	YES			YES		
Country Fixed Effects	YES			YES		

Note: The outcome variable is popular support of the government. The estimates are the results of weighted-average least squares (WALS) and Bayesian model averaging (BMA) regressions. The control variable set includes fiscal deficit as percentage of GDP, the share of the elderly (65+) as percentage of population, real GDP growth rate, unemployment rate, consumer price index, lagged government's vote share, average tax wedge, five economic reform indicators, share of green parties in parliament, partisanship of chief executive of government, margin of majority, Herfindahl index of opposition parties, index of democratic accountability, index of legislative strength, and net household income for couple with two children (levels and changes). ***,**,* denote significance at 1, 5 and 10 percent, respectively.

Table A2.5: The effect of EPS changes on popular support of the government over time

Variables	(1)	(2)	(3)	(4)	(5)
change in EPS index	**-0.377****	**-0.342****	**-0.232****	**-0.256*****	**-0.225*****
	(0.180)	**(0.141)**	**(0.107)**	**(0.088)**	**(0.081)**
public deficit %GDP	0.012	-0.008	0.035**	0.039***	0.038***
	(0.035)	(0.025)	(0.015)	(0.014)	(0.014)
share of elderly	-0.011	0.040	0.056	0.109	0.166***
	(0.252)	(0.147)	(0.119)	(0.093)	(0.062)
GDP growth	0.034	0.093**	0.077***	0.055**	0.056**
	(0.050)	(0.040)	(0.030)	(0.026)	(0.026)
constant	3.112	2.306	2.145	1.367	0.636
	(3.123)	(1.790)	(1.436)	(1.149)	(0.749)
Observations	156	212	271	331	357
R-squared	0.563	0.492	0.480	0.468	0.456
Number of countries	27	28	30	30	30
Year Fixed Effects	YES	YES	YES	YES	YES
Country Fixed Effects	YES	YES	YES	YES	YES
All observations included until	2006	2008	2010	2012	2014

Note: The outcome variable is popular support of the government. Estimates are based on $y_{i,t} = \beta \Delta EPS_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \epsilon_{i,t}$. Columns differ with respect to the observations being included (temporal sub-sampling). All estimations include country and year fixed effects. The main independent variable of interest is change in the EPS index (ranging from 1 (low stringency) to 6 (high stringency)). A coefficient of -0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A2.6: Validity of instrument (1): adding instrumental variable as control variable to baseline estimation

Variables	(1)	(2)	(3)	(4)
change in EPS index	-0.207***	-0.226***	-0.215***	-0.240***
	(0.080)	(0.079)	(0.079)	(0.081)
public deficit %GDP	0.040***	0.040***	0.039***	0.040***
	(0.014)	(0.014)	(0.014)	(0.014)
share of elderly	0.159***	0.163***	0.164***	0.150***
	(0.050)	(0.051)	(0.053)	(0.048)
GDP growth	0.061**	0.063**	0.063**	0.064**
	(0.028)	(0.029)	(0.029)	(0.029)
constant	0.988	0.660	0.569	0.962*
	(0.603)	(0.627)	(0.636)	(0.566)
Instrument	**-0.000**	**-0.000**	**-0.000**	**-0.000**
	(0.000)	**(0.000)**	**(0.000)**	**(0.000)**
Observations	370	370	370	361
R-squared	0.459	0.456	0.456	0.458
Instrument: product of				
- global term (varying)	# of floods	# of people affected by earthquake	# of major hurricanes	# of wild fires
- country term (constant)	coast length	share of urban extent in coastal zone	distance from centroid to nearest coast	agricultural land per capita
Number of countries	30	30	30	29
Year Fixed Effects	YES	YES	YES	YES
Country Fixed Effects	YES	YES	YES	YES

Note: The outcome variable is popular support of the government. Estimates are based on $y_{i,t} = \beta \Delta EPS_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \epsilon_{i,t}$. Columns differ with respect to the instrumental variable being included as additional control variable. All estimations include country and year fixed effects. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A2.7: Validity of instrument (2): correlation between baseline residuals and instrumental variables

Variables	(1)	(2)	(3)	(4)
public deficit %GDP	0.001 (0.014)	0.000 (0.014)	-0.000 (0.014)	0.000 (0.014)
share of elderly	-0.005 (0.050)	-0.000 (0.051)	0.000 (0.053)	-0.013 (0.048)
GDP growth	-0.002 (0.028)	0.000 (0.029)	0.000 (0.029)	0.001 (0.029)
constant	0.329 (0.603)	0.002 (0.627)	-0.090 (0.636)	0.303 (0.566)
Instrument	**-0.000** **(0.000)**	**-0.000** **(0.000)**	**-0.000** **(0.000)**	**-0.000** **(0.000)**
Observations	370	370	370	361
R-squared	0.00718	0.00141	0.00259	0.00239
Instrument: product of				
- global term (varying)	# of floods	# of people affected by earthquake	# of major hurricanes	# of wild fires
- country term (constant)	coast length	share of urban extent in coastal zone	distance from centroid to nearest coast	agricultural land per capita
Number of countries	30	30	30	29
Year Fixed Effects	YES	YES	YES	YES
Country Fixed Effects	YES	YES	YES	YES

Note: The outcome variable is the residuals from the baseline estimation with popular support of the government as dependent variable: $y_{i,t} = \beta \Delta EPS_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \epsilon_{i,t}$. Columns differ with respect to the instrumental variable being included as additional control variable. All estimations include country and year fixed effects. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A2.8: The effect of EPS changes on popular support of the government using instrumental variable regressions with additional control variables

Variables	(1)	(2)	(3)	(4)	(5)
change in EPS index	**-0.976** (0.405)**	**-1.555*** (0.433)**	**-1.388*** (0.396)**	**-0.963*** (0.320)**	**-0.966*** (0.368)**
Observations	370	357	357	370	370
Additional controls:					
- (# of floods) interacted with	globaliz. index	conflict index	riot index		
- (country's coast length) interacted with				(log) population	
- additional control variable					(log) population * globaliz. index
1st-stage F-Stat	26.36	23.46	18.38	29.45	28.17
Year Fixed Effects	YES	YES	YES	YES	YES
Country Fixed Effects	YES	YES	YES	YES	YES

Note: The outcome variable is popular support of the government. Estimates are based on $y_{i,t} = \beta \Delta \widehat{EPS}_{\iota,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \epsilon_{i,t}$ with $\Delta \widehat{EPS}_{\iota,t} = \vartheta S_{i,t} + \varphi X_{i,t} + \alpha_i + \gamma_t + \upsilon_{i,t}$. Columns differ with respect to the inclusion of different control variables. All estimations include country and year fixed effects. For all columns, the baseline instrument, (# of floods) x (coast length), is used. Control variables have been partialled out. The main independent variable of interest is the change in the EPS index (ranging from 1 (low stringency) to 6 (high stringency)). A coefficient of -0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A2.9-1: The effect of market-based EPS changes on popular support of the government

Variables	(1)	(2)	(3)	(4)
change in CO_2-tax-based EPS index	-0.071**			
	(0.035)			
change in Diesel-tax-based EPS index		0.019		
		(0.066)		
change in NO_x-tax-based EPS index			-0.090**	
			(0.042)	
change in SO_x-tax-based EPS index				-0.196***
				(0.052)
public deficit %GDP	0.039***	0.039***	0.039***	0.039***
	(0.013)	(0.014)	(0.013)	(0.013)
share of elderly	0.151***	0.159***	0.150***	0.153***
	(0.051)	(0.052)	(0.050)	(0.051)
GDP growth	0.062**	0.060**	0.059**	0.061**
	(0.027)	(0.027)	(0.027)	(0.027)
constant	0.785	0.695	0.839	0.799
	(0.619)	(0.628)	(0.611)	(0.626)
Observations	378	373	378	378
R-squared	0.443	0.439	0.447	0.450
Number of countries	30	30	30	30
Year Fixed Effects	YES	YES	YES	YES
Country Fixed Effects	YES	YES	YES	YES

Note: The outcome variable is popular support of the government. Estimates are based on $y_{i,t} = \beta \Delta EPS_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \epsilon_{i,t}$. Columns differ with respect to the choice of the sub-index of EPS (each ranging from 1 (low stringency) to 6 (high stringency)). A coefficient of -0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. All estimations include country and year fixed effects. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A2.9-2: The effect of market-based EPS changes on popular support of the government

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
change in FIT-based EPS index	-0.028						
	(0.022)						
change in solar-FIT-based EPS index		-0.022					
		(0.018)					
change in wind-FIT-based EPS index			-0.024				
			(0.020)				
change in trading-scheme-based EPS index				-0.084			
				(0.060)			
change in CO_2-trading-scheme-based EPS index					-0.039		
					(0.026)		
change in EEF-trading-scheme-based EPS index						-0.13**	
						(0.058)	
change in REC-trading-scheme-based EPS index							0.021
							(0.038)
public deficit %GDP	0.039***	0.039***	0.039***	0.038***	0.038***	0.039***	0.039***
	(0.013)	(0.014)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)
share of elderly	0.151***	0.147***	0.154***	0.159***	0.159***	0.148***	0.149***
	(0.051)	(0.052)	(0.051)	(0.050)	(0.051)	(0.050)	(0.051)
GDP growth	0.061**	0.062**	0.061**	0.066**	0.065**	0.062**	0.061**
	(0.027)	(0.027)	(0.027)	(0.028)	(0.028)	(0.027)	(0.027)
constant	0.807	0.848	0.765	0.704	0.684	0.845	0.812
	(0.619)	(0.630)	(0.620)	(0.608)	(0.613)	(0.604)	(0.619)
Observations	378	378	378	374	374	378	378
R-squared	0.444	0.443	0.444	0.455	0.456	0.446	0.442
Number of countries	30	30	30	30	30	30	30
Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES
Country Fixed Effects	YES	YES	YES	YES	YES	YES	YES

Note: The outcome variable is popular support of the government. Estimates are based on $y_{i,t} = \beta \Delta EPS_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \epsilon_{i,t}$. Columns differ with respect to the choice of the sub-index of EPS (each ranging from 1 (low stringency) to 6 (high stringency)). A coefficient of -0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. FIT, EEF, and REC refers to feed-in-tariffs, Energy Efficiency Certificate, and Renewable Energy Certificate, respectively. All estimations include country and year fixed effects. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A2.9-3: The effect of non-market-based EPS changes on popular support of the government

Variable	(1)	(2)	(3)	(4)
change in non-market-based EPS index	-0.066 (0.048)			
change in limit-based EPS index		-0.004 (0.065)		
change in R&D-based EPS index (RE)			-0.050* (0.027)	
change in R&D-based EPS index (SUB)				-0.050* (0.027)
public deficit %GDP	0.039*** (0.014)	0.039*** (0.013)	0.039*** (0.014)	0.039*** (0.014)
share of elderly	0.149*** (0.050)	0.149*** (0.051)	0.148*** (0.050)	0.148*** (0.050)
GDP growth	0.061** (0.027)	0.061** (0.027)	0.062** (0.027)	0.062** (0.027)
Constant	0.835 (0.600)	0.819 (0.611)	0.836 (0.604)	0.836 (0.604)
Observations	378	378	378	378
R-squared	0.444	0.441	0.445	0.445
Number of countries	30	30	30	30
Year Fixed Effects	YES	YES	YES	YES
Country Fixed Effects	YES	YES	YES	YES

Note: The outcome variable is popular support of the government. Estimates are based on $y_{i,t} = \beta \Delta EPS_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \epsilon_{i,t}$. Columns differ with respect to the choice of the sub-index of EPS (each ranging from 1 (low stringency) to 6 (high stringency)). A coefficient of -0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. RE and SUB refers to renewable energy and sub-index, respectively. All estimations include country and year fixed effects. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A2.9-4: The effect of non-market-based EPS changes on popular support of the government

Variable	(1)	(2)	(3)	(4)
change in Diesel-limit-based EPS index	-0.206**			
	(0.103)			
change in NO_x-limit-based EPS index		0.014		
		(0.041)		
change in PM-limit-based EPS index			-0.043	
			(0.041)	
change in SO_x-limit-based EPS index				0.020
				(0.037)
public deficit %GDP	0.039***	0.039***	0.040***	0.039***
	(0.013)	(0.013)	(0.013)	(0.013)
share of elderly	0.145***	0.148***	0.146***	0.150***
	(0.051)	(0.052)	(0.050)	(0.051)
GDP growth	0.057**	0.061**	0.061**	0.062**
	(0.025)	(0.027)	(0.027)	(0.027)
Constant	0.891	0.834	0.856	0.810
	(0.618)	(0.627)	(0.604)	(0.618)
Observations	378	378	378	378
R-squared	0.450	0.442	0.443	0.442
Number of countries	30	30	30	30
Year Fixed Effects	YES	YES	YES	YES
Country Fixed Effects	YES	YES	YES	YES

Note: The outcome variable is popular support of the government. Estimates are based on $y_{i,t} = \beta \Delta EPS_{i,t} + \theta X_{i,t} + \alpha_i + \gamma_t + \epsilon_{i,t}$. Columns differ with respect to the choice of the sub-index of EPS (each ranging from 1 (low stringency) to 6 (high stringency)). A coefficient of -0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. All estimations include country and year fixed effects. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A2.10: The effect of EPS changes on popular support of the government mediated by ideology of incumbent party

Variables	(1)	(2)	(3)	(4)	(5)	(6)
change in EPS index x left-wing incumbent	-0.42**	-0.437**	-0.440**	-0.47**	-0.418*	-0.428*
	(0.185)	(0.179)	(0.180)	(0.207)	(0.218)	(0.224)
change in EPS index x center incumbent	-0.190	-0.231**	-0.239**	-0.243**	-0.304**	-0.305**
	(0.119)	(0.115)	(0.117)	(0.119)	(0.125)	(0.118)
change in EPS index x right-wing incumbent	0.175	0.169	0.168	0.140	0.038	0.037
	(0.190)	(0.170)	(0.169)	(0.177)	(0.127)	(0.141)
public deficit %GDP	0.036**	0.021	0.019	0.017	0.016	0.016
	(0.016)	(0.016)	(0.016)	(0.017)	(0.014)	(0.015)
share of elderly	0.071	0.063	0.081	0.079	0.110	0.076
	(0.097)	(0.093)	(0.094)	(0.096)	(0.098)	(0.102)
GDP growth	0.075**	0.056*	0.061*	0.057	0.038	0.041
	(0.031)	(0.033)	(0.035)	(0.037)	(0.029)	(0.030)
unemployment rate		-0.050***	-0.048***	-0.046**	-0.048**	-0.054**
		(0.016)	(0.018)	(0.019)	(0.020)	(0.025)
CPI			0.501	0.416	0.064	0.355
			(0.309)	(0.337)	(1.176)	(1.467)
gov. parties’ vote share				0.007	-0.004	-0.004
				(0.006)	(0.007)	(0.008)
average tax wedge					-0.087*	-0.079
					(0.050)	(0.055)
financial reform index						1.004
						(2.104)
product reform index						-0.062
						(0.763)
current account reform index						-1.353
						(1.699)
capital account reform index						1.134
						(1.665)
labour reform index						0.984
						(2.367)
constant	1.856	2.270**	1.655	1.777	3.582	2.344
	(1.184)	(1.124)	(1.210)	(1.230)	(2.187)	(3.450)
Observations	305	305	305	294	245	245
R-squared	0.466	0.484	0.488	0.487	0.549	0.554
Number of countries	26	26	26	24	22	22
Year Fixed Effects	YES	YES	YES	YES	YES	YES
Country Fixed Effects	YES	YES	YES	YES	YES	YES

Note: The outcome variable is popular support of the government. Columns differ with respect to control variable set. All estimations include country and year fixed effects. The interaction terms between incumbent ideology binaries with change in the EPS index (ranging from 1 (low stringency) to 6 (high stringency)). A coefficient of -0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A2.11: The effect of EPS changes on popular support of the government mediated by fuel prices using instrumental variable regressions

Variables	(1)	(2)	(3)	(4)
change in EPS index x low oil price	2.065			
	(8.226)			
change in EPS index x high oil price		-1.241**		
		(0.608)		
change in EPS index x low gas price			-1.808	
			(4.388)	
change in EPS index x high gas price				-1.184*
				(0.630)
Country FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
1^{st}-stage F-Stat	0.02	12.161	2.52	12.82
Observations	370	370	370	370

Note: The outcome variable is popular support of the government. Columns differ with respect to the interaction term being instrumented. For all columns, the instrument, (# of people affected by earthquake) x (share of urban extent in coastal zone), is used. In addition to the baseline control set, all estimations include country and year fixed effects. Control variables have been partialled out. A coefficient of -0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p<0.1; **p<0.05; ***p<0.01.

able A2.12: The effect of EPS changes on popular support of the government mediated by Structural factors using instrumental variable regressions

Variables	(1)	(2)	(3)	(4)	(5)	(6)
change in EPS index x low # of years until election	-1.521** (0.605)					
change in EPS index x high # of years until election		0.202 (0.615)				
change in EPS index x low share of dirty energy			-1.374* (0.773)			
change in EPS index x high share of dirty energy				-3.675** (1.769)		
change in EPS index x low initial EPS level					-3.530 (3.976)	
change in EPS index x high initial EPS level						-1.343*** (0.513)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
1^{st}-stage F-Stat	14.44	42.18	18.10	9.85	1.631	25.166
Observations	370	370	370	370	366	366

Note: The outcome variable is popular support of the government. Columns differ with respect to the interaction term being instrumented. For column III-VI, (# of people affected by earthquake) x (share of urban extent in coastal zone) is used as instrument. Due to higher strength (size of F-Stats), for column I and II, (# of floods) x (coast length) is used. In addition to the baseline control set, all estimations include country and year fixed effects. Control variables have been partialled out. A coefficient of -0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A2.13: The effect of EPS changes on popular support of the government mediated by inequality using instrumental variable regressions

Variables	(1)	(2)	(3)	(4)
change in EPS index x	1.454			
lower market-based GINI	(1.876)			
change in EPS index x		-4.569***		
higher market-based GINI		(1.757)		
change in EPS index x			4.748	
lower net-based GINI			(3.315)	
change in EPS index x				-3.343***
higher net-based GINI				(0.852)
Country FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
1^{st}-stage F-Stat	9.77	6.93	8.50	29.20
Observations	326	326	326	326

Note: The outcome variable is popular support of the government. Columns differ with respect to the interaction term being instrumented. For all columns, the instrument, (# of floods) x (coast length), is used. In addition to the baseline control set, all estimations include country and year fixed effects. Control variables have been partialled out. A coefficient of -0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A2.14: The effect of EPS changes on popular support of the government mediated by income shares using instrumental variable regressions

Variables	(1)	(2)	(3)	(4)	(5)	(6)
change in EPS index x lower top 1% income share	13.098 (16.554)					
change in EPS index x higher top 1% income share		-3.50*** (0.561)				
change in EPS index x lower top 10% income share			11.496 (14.939)			
change in EPS index x higher top 10% income share				-4.482*** (0.862)		
change in EPS index x lower top 20% income share					7.378 (7.012)	
change in EPS index x higher top 20% income share						-5.500*** (1.546)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
1st-stage F-Stat	0.52	73.36	0.43	30.61	0.71	17.65
Observations	301	301	301	301	301	301

Note: The outcome variable is popular support of the government. Columns differ with respect to the interaction term being instrumented. For all columns, the instrument, (# of floods) x (coast length), is used. In addition to the baseline control set, all estimations include country and year fixed effects. Control variables have been partialled out. A coefficient of -0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A2.15: The effect of EPS changes on popular support of the government mediated by income shares using instrumental variable regressions

Variables	(1)	(2)	(3)	(4)	(5)	(6)
change in EPS index x lower bottom 1% income share	-2.05** (0.837)					
change in EPS index x higher bottom 1% income share		-0.458 (0.577)				
change in EPS index x lower bottom 10% income share			-2.643** (1.197)			
change in EPS index x higher bottom 10% income share				0.707 (2.046)		
change in EPS index x higher bottom 20% income share					-4.291*** (1.112)	
change in EPS index x higher bottom 20% income share						4.840 (3.334)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
1^{st}-stage F-Stat	8.42	29.84	25.02	1.14	24.82	1.02
Observations	259	259	301	301	301	301

Note: The outcome variable is popular support of the government. Columns differ with respect to the interaction term being instrumented. For all columns, the instrument, (# of floods) x (coast length), is used. In addition to the baseline control set, all estimations include country and year fixed effects. Control variables have been partialled out. A coefficient of -0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A2.16: The effect of EPS changes on popular support of the government mediated social expenditure using instrumental variable regressions

Variables	(1)	(2)	(3)	(4)
change in EPS index x	-0.94**			
lower in-cash social benefits to households	(0.415)			
change in EPS index x		-0.630		
higher in-cash social benefits to households		(1.063)		
change in EPS index x			-1.051**	
lower in-kind social benefits to households			(0.430)	
change in EPS index x				-0.632
higher in-kind social benefits to households				(1.759)
Country FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
1^{st}-stage F-Stat	66.98	2.93	109.99	1.80
Observations	370	370	340	340

Note: The outcome variable is popular support of the government. Columns differ with respect to the interaction term being instrumented. For all columns, the instrument, (# of people affected by earthquake) x (share of urban extent in coastal zone) is used. In addition to the baseline control set, all estimations include country and year fixed effects. Control variables have been partialled out. A coefficient of -0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Table A2.17: The effect of EPS changes on popular support of the government mediated social expenditure using instrumental variable regressions

Variables	(1)	(2)	(3)	(4)
change in EPS index x	-1.863*			
lower social expenditure on ALMP	(1.037)			
change in EPS index x		10.620		
higher social expenditure on ALMP		(21.066)		
change in EPS index x			-1.860**	
lower social expenditure on UB			(0.834)	
change in EPS index x				-15.330
higher social expenditure on UB				(12.759)
Country FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
1st-stage F-Stat	76.26	0.38	36.28	1.18
Observations	334	334	321	321

Note: The outcome variable is popular support of the government. Columns differ with respect to the interaction term being instrumented. For all columns, the instrument, (# of floods) x (coast length), is used. In addition to the baseline control set, all estimations include country and year fixed effects. Control variables have been partialled out. A coefficient of -0.2 is equivalent to a 10 percent decline in popular support from an increase in EPS from the 1st to the 3rd quartile of the EPS distribution. Standard deviations based on robust standard errors clustered at the country level in parentheses. *p<0.1; **p<0.05; ***p<0.01.

Conclusion

In many advanced economies, governments spend a large share of their annual budget on social security, especially in the form of social benefits. While the economic rationales for providing social security have been widely documented[40], the provision of these social programs has important political implications which are yet to be extensively examined. The aim of this dissertation was to contribute to our understanding how the promise and implementation of social policies affect the political and electoral behaviour of individual voters. The dissertation consists of four quantitative papers. These research articles use different theoretical frameworks and apply various empirical methods – including sensitivity analysis, machine learning, and causal inference – to distinct data sources and structures (time-series cross-country panels, individual-level public opinion surveys, large administrative data). In this section of the dissertation, I summarize the dissertations' main findings and design limitations, and provide an outlook about potential avenue of further research.

[40] see for instance Behman (1997); Ashenfelter and Rouse (1998); Black and Lynch (1996); Blundell et al. (1999); Sianesi and Van Reenen (2003); Harmon et al. (2003), Currie and Madrian (1999); Card et al. (2009); Sommers et al. (2012); Garthwaite et al. (2014); Korenman and Remler (2016;) Goldin et al. (2021), Baily (1978); Gruber (1994); Chetty (2006); Chetty and Looney (2007); Kuka (2020)

I. Main findings and policy implications

This dissertation makes six broader contributions to the academic literature on the politics of the welfare states, with several policy implications.

1. *Social benefit expansions can persuade voters*.

Numerous studies in the economic voting literature have shown that sociotropic and egotropic considerations impact the political and electoral behaviour of individual voters (Tufte 1975; Campbell et al. 1980; Lewis-Beck 1986; Powell and Whitten 1993; Lewis-Beck and Stegmaier 2000; see review by Anderson 2007). Both retrospective and prospective forms of evaluation have been extensively theorized (Barro 1973; Ferejohn 1986; Reed 1998; Downs 1957; Lindbeck and Weibull 1987; for reviews, see Lewis-Beck 1990; Persson and Tabellini 2002, and Born et al. 2018). Yet, most of the empirical studies focus on retrospective voting by estimating how the state of the economy and/or adopted policies affect voters' behaviour at the ballot box (Manacorda et al. 2011; Labonne 2013; De La O 2013, Cantú 2019, Hidalgo and Nichter 2015, Baez et al. 2012, Gallego 2018, Khemani 2015, Pop-Eleches and Pop-Elches 2012). In contrast, empirical studies on prospective egotropic studies are very limited.

To fill this gap, the dissertation contributes to the literature on economic voting by investigating whether prospective pocketbook motives play a role at the ballot box. To do so, the dissertation provides credible evidence to this question by estimating the effect of campaign promises on the adoption of pension benefit expansions. The empirical estimates suggest a large re-alignment effect among affected individuals in favour of the pledge-making party. Overall, the dissertation underlines that voters are, at least to some extent, forward-looking and driven by pocketbook motives as their political support depends on their expected individual utility they gain from a certain party in power and the policies they promise to implement. In other words, beyond evaluating current and past economic and policy outcomes, this finding provides evidence that voters are forward-looking agents that listen attentively to programmatic signals parties send about their future policy trajectory.

Beyond its academic contribution, the findings suggest that political parties can utilize on prospective pocketbook motives of voters and, in this way, exploit the welfare state for electoral purposes. Put differently, since the promise of benefit expansions can motivate voters to side with a particular electoral platform, political parties have an inherent incentive to promise expansionary social policies that maximize their vote share, instead of economic returns for society. In addition, exploiting benefit expansions for electoral purposes may also cause an inefficient distribution of monetary resources across social policy domains. For instance, since certain groups within the electorate may play a larger role for electoral outcomes than others, parties are incentivised to direct social benefits to more powerful electoral groups which may cause an overspending on these individuals at the cost of others.

2. Social policies can dampen inequality of political representation.

Political scientists have been theorizing the role of income on both public opinion and political participation for a long time – see, for instance, the Civic Voluntarism Model (Verba et al. 1995; Schlozman et al. 2001). Indeed, numerous empirical studies have identified an income-turnout gap by indicating that rich individuals show higher rates of electoral participation than poor voters (Wolfinger and Rosenstone 1980; Filer et al. 1993; Verba et al. 1995; Solt 2008; Leighley and Nagler 2013; Kasara and Suryanarayan 2015).

Yet, despite a positive relationship between income and electoral participation, several studies have identified strong non-linearities. In fact, evidence on the income-gradient has shown that changes in income brackets have diminishing marginal returns on turnout which suggests that the effect of additional financial means declines, or and may potentially disappear, when moving up the income distribution (Rosenstone and Hansen 1993; Campbell 2002; Schafer et al. 2022). Based on these findings, one can expect that the effect of social benefits on electoral participation changes with pre-existing economic endowments of individual voters.

Indeed, the dissertation has identified diminishing marginal participatory returns to benefit expansions. This corroborates an economic security mechanism where those who are more economically deprived are most strongly affected in their political participation by greater welfare state benefits. In other words, the evidence suggests that the size of policy feedbacks depends rather on individuals' relative gain they receive from a programme than their absolute dollar value. Therefore, the dissertation has shown that economic security has a strong conditioning effect on the presence (or absence) of policy feedbacks.

Beyond its academic contribution, this result underlines the positive political side-effects of expansionary programs. Especially in times of a widening gap of electoral participation across social classes (Evans 2000; Evans and Tiley 2017), the dissertation highlights that the public provision of social security can compensate unequal political representation along the income distribution and promote political participation of voters with lower levels of income. Guaranteeing political representation is not just important to legitimize democratic institutions as a whole, but it also ensures that policymakers are hold accountable towards the interest of these deprived groups. Therefore, the welfare state seems to be an efficient instrument to counter the trend of rising inequality of political representation in democratic countries.

3. Low-income individuals are more sensitive to benefit-expanding campaign promises.

Previous studies have shown that clientelist tactics pre-dominantly target voters with lower economic security (Jensen et al. 2014; Çarkoğlu and Aytaç 2015). Since the marginal utility of additional income decreases with pre-existing economic endowments, poorer voters are expected to show higher sensitivity to vote-buying offers than their richer peers. While the scholarship on clientelism applies first and foremost to emerging and developing economies (Hicken 2011), the electoral returns to benefit-expanding campaign promises in advanced economies are likely to originate in low-income voters in a similar vein.

To compare voters' sensitivity towards electoral pledges along the income distribution, the dissertation has shown that benefit-expanding campaign promises are more persuasive among individuals with lower levels of income compared to their richer counterparts. In other words, promising social benefits induces larger shifts in political alignment among the poor than the rich, suggesting that prospective pocketbook motives are more influential among deprived groups of voters. Thus, the dissertation has shown that the economic position shapes which type of economic voting an individual voter adopts.

If campaign promises on benefit expansions are more effective at the lower tail of the income distribution, candidates and parties can make such electoral pledges to gain the political support of deprived individuals. Therefore, the higher responsiveness of low-income voters towards transfer promises incentivizes politicians to play on these voters' egotropic motives to swing them in their favour. To discourage electoral platforms from exploiting the welfare state for electoral purposes, providing a more generous social net and/or strengthening voters' general economic position may be one possible solution.

4. One-off social benefit expansions have only transitory effects on the political behaviour of voters.

A pertinent question in the field of political science is how long the effect of a (campaign promise of a) policy lasts. From a pocketbook lens, once the reform is implemented and thus the pledge fulfilment completes the policy-vote exchange, the initial economic incentive no longer remains. Thus, voters that had been motivated by the expansion promise may in fact revert to their pre-held political position of alignment. Empirical studies testing the binding potential of targeted transfer programs come to relatively mixed and contrary findings (Bechtel and Hainmüller 2011; Manacorda et al. 2011; Zimmermann 2021; Zucco 2013; Lü 2014).

Two papers of this dissertation aim to contribute to this literature, using the case of pension benefits. Both articles have shown that retirement entitlements can only mobilize and/or persuade benefiting individuals in the short- to medium-run as the effects disappear within one electoral cycle. This finding suggests that even very generous welfare state expansions do not yield long-term political gains for the pledge-making party. Therefore, policy feedbacks seem to fade with voters' awareness. While this result shows that policy feedbacks are useful frameworks to explain short-term fluctuations in political and electoral behaviour, they may not be suitable to understand long-term trends in electoral outcomes.

This finding comes with two broader implications. On the one hand, it entails that policymakers cannot secure permanent political support via one-off benefit expansions, but they are rather required to deliver such benefits to individual voters on a continuous basis. In other words, the limited durability of the feedback loop suggests that elected officials remain accountable as they must keep delivering benefits to their voters to secure their political support. On the other hand, however, this transitory nature may also entail an ongoing over-expansion of benefits for certain groups of voters whose political support is pivotal to win elections. In other words, since single benefit expansions can only generate political support for the next election, office-seeking policymakers are incentivized to further an economically inefficient swelling of the welfare state that pays electoral dividends, however.

5. *Social policies can help policymakers to overcome the political resistance of unpopular policies*.

Unpopular policies are often not adopted because policymakers fear a loss in electoral support. The political resistance towards a policy is especially high when its costs are concentrated on certain groups of individuals (cost concentration) and its benefits are spread across society (benefit dispersion). According to the literature on structural reforms (Alesina and Drazen 1989; Drazen and Grilli 1993; Persson and Tabellini 2000; Tsebellis 2002), cost concentration and/or benefit dispersion is a significant obstacle to policies even when they are beneficial in the long run.

A policy domain with particularly high political resistance concerns climate change policies (Stokes 2016). Since they are rather regressive in nature by putting a large burden on certain (mostly deprived) groups of voters, cost concentration creates strong incentives to form interest groups against such policies, while the dispersion of benefits diffuses support for them (Goulder et al. 2019; Stiglitz 2019; Rojas-Vallejos and Lastuka 2020; Känzig 2021). Therefore, policymakers may rightly hesitate to adopt measures against carbon emission due to their expected political costs.

The dissertation has shown, however, that the political damage of climate change policies can be mitigated when sufficient social insurance and assistance (i.e. unemployment benefits) and/or adequate labour market policies (i.e. active labour market policies) are in place to protect negatively affected individuals. Thus, these results highlight that the welfare state can help to dampen the short-term distributional consequences of unpopular but long-term beneficial policies (Ostry et al. 2019; Ostry et al. 2021; Markkanen and Anger-Kraavi 2019). Therefore, policymakers are well-advised to incorporate the social dimension into their policy strategies against climate change and, in this way, generate the political support that is necessary for the urgently needed measures against global warming.

6. *Model uncertainty in comparative social policy emerges from sample selection and measurement error rather than the choice of the control variables.*

One of the most pressing empirical issues of scientific inquiry concerns the fact that quantitative estimates depend strongly on the statistical choices an analyst makes. To measure the extent of model uncertainty for a given statistical relationship, one of the earliest methods quantifying the sensitivity of empirical estimates towards statistical assumptions is the Extreme Bounds Analysis (EBA) method proposed by Leamer (1983, 1985), which has been applied to various research questions in economics, sociology, and political science.

Most EBA studies led to a very pessimistic view about the robustness of empirical results in their respective fields. Several scholars found a lack of robustness of many indicators which have previously been considered fundamental to explain changes in economic growth (Levine and Renelt 1992; Levine and Zervos 1993, Sturm and de Haan 2005), democratization (Gassebner et al. 2013), political coups (Gassebner et al. 2016; Miller et al. 2016), and civil wars (Hegre and Sambanis 2006).

Such a sensitivity analysis has been missing in the literature welfare state development even though model uncertainty is expected to be particularly large because numerous potential determinants, regime types, and generosity measures, have created a large potential model space in the last decades. The dissertation has aimed to fill this gap by conducting a sensitivity analysis about welfare state determinants. However, instead of applying the traditional EBA method, the paper has advanced Leamer's approach by testing the sensitivity to numerous model specification choices, namely the control variable set, the fixed effect structure, the standard error type, the country sample, the period sample, and the operationalization of the dependent variable.

As the results have shown, empirical estimates in the welfare state determinants literature indicate high levels of model uncertainty which primarily originates from sample selection and the operationalisation of social policies. In contrast, the choice of the control set plays a comparatively minor role with respect to the statistical significance, direction, and size of the coefficient. These findings highlight that the explanatory power of welfare state determinants vary widely across countries and time periods. The high sensitivity towards sample selection suggests that external validity of empirical estimates in social policy may be particularly limited and scholars are well advised to be careful when applying empirical results out-of-sample.

In addition to the social policy discipline, the development of the Augmented Extreme Bounds Analysis (A-EBA) approach can also be a valuable method for applied scholars in other social sciences. Since model uncertainty and the identification of its sources is a common task of empirical analysis, the method aims to provide an off-the-shelf toolkit that supports empiricists to track down the key statistical assumptions in an easy and time-efficient way. In this way, the method can hopefully facilitate the process of empirical analysis by helping researchers to engage more rigorously – both theoretically and empirically – with the key statistical assumptions that have the largest impact on the resulting coefficients and standard errors.

II. Limitations

This dissertation comes with three main limitations. *First*, quantitative social science research usually faces a dilemma between internal and external validity. Within the dissertation, the extent of this trade-off differs across the four research articles. For instance, while the identification strategies used in paper 2 and 3 benefit from their exploitation of quasi-random treatment assignment, the empirical results may not be representative beyond German (paper 2) and US (paper 3) borders. Indeed, both papers analyse the effect of pension benefit expansions in countries (Germany: 53%; USA: 51%) which have net pension replacement rates that are substantially below the OECD average (62%) (OECD 2021). Therefore, the estimates about the persuasive and mobilizing effect of pension benefit expansions are rather applicable to less generous countries and the effects may not be present in environments in which retirement benefits are already at high levels.

Second, paper 2 and 3 investigate individual reforms which had a relatively large impact both on individual voters and the national pension systems, respectively. Thus, the empirical findings apply first and foremost to large and salient reforms that can attract substantive public attention and entail sizeable material consequences. In other words, the identified effects may not be present for social policies that are smaller with respect to their monetary size and/or are less taken account of by voters. Indeed, one can expect that large-scale benefit expansions induce larger changes in political and electoral behaviour than smaller and less-salient ones do. However, the papers were not able to estimate how ‘much’ money and salience towards a policy is needed to put the policy feedbacks into operation.

Third, the dissertation uses data from a certain point in time which may not adequately represent contemporary political behaviour. This may be particularly troublesome for paper 4 which uses data from the beginning of the current century and therefore disregards the fact that voters' public opinion on certain issues – such as the long-term damage of climate change – has shifted. Thus, the mitigation effect of social security on the political costs of climate change policies may differ for a greener median voter. However, adding the environmental policy preference to the theoretical framework on the political costs of structural reforms is difficult as these policies preferences are arguably endogenous to economic endowments and thus, eligibility for social benefits.

III. Avenues for further research

This dissertation is far from providing a comprehensive picture about the political determinants and political consequences of social policies in advanced democracies. While the aim of the four papers was to contribute to our understanding of the political aspects of the welfare state, they also raise further questions that can be avenue for further research. The following angles may be particularly interesting.

First, as the findings on the robustness (or the lack thereof) in generosity estimations in *paper 1* have shown, empirical social policy may benefit from further investigation of model uncertainty in other streams of the literature. For instance, beyond welfare state determinants, it may be fruitful to test the robustness of empirical estimates of the effect of social policies on economic, political, and sociological outcomes. Beyond the research question itself, the study of model uncertainty ought to be expanded towards (quasi-)experimental approaches. In the past, most of such papers using sensitivity analysis approaches test the robustness of statistical assumptions in simple ordinary least square regression setups. However, with the immense popularity gain of causal inference approaches in the social sciences in the last two decades, it may be a productive exercise to test whether (quasi-)experimental approaches are more prone towards model uncertainty and whether concerns about robustness and p-value hacking are less prevalent when using randomization methods.

In addition, *paper 2* has identified a ‘persuasion effect’ of prospective pension expansions in the case of Germany. Beyond examining the external validity in other country contexts, we still lack evidence on whether campaign promises on other social policy domains are similarly effective. In addition to retirement benefits, it may be especially important to understand whether promises on social investments – such as education – deliver similar electoral dividends for pledge-making parties. The comparison between the size of policy feedbacks between pension and education expansions can be particularly fruitful in times of population aging when the elderly becomes a powerful group of voters and may crowd out the interests of younger cohorts.

Furthermore, this dissertation – in particularly *paper 2* and *3* – have examined welfare state expansions and how they persuade and mobilize individual voters. However, many modern welfare state systems have seen benefit cuts in the last decades. While the austerity and anger-based literature (mostly in the US) have tried to estimate whether voters electorally punish the incumbent for retrenching the welfare state, most of these studies do not address endogeneity-related issues using observational data. Thus, we are hardly able to derive a causal interpretation of voters' political responsiveness to welfare state downsizing. To quantify the political and electoral consequences of retrenchments, further analyses adopting causal inference approaches are required to provide credible evidence about the electoral punishment of welfare state cuts, especially in the context of austerity.

Finally, the articles in this thesis have largely ignored shifts in public opinion and how this may elevate (or dampen) the size of policy feedbacks. This seems to be important due to two underlying trends of policy preferences within the electorate in the last years. On the one hand, the rise of the vote share of far-right parties suggests that a non-insignificant part of the electorate has estranged itself from mainstream platforms in many advanced economies (Golder 2016). Thus, it can be fruitful to test whether the expansion of social benefits in favour of these individuals can satisfy their policy demand and support the re-alignment of these estranged voters with non-extremist electoral platforms. On the other hand, awareness about the devastating consequences of unmitigated global warming has increased substantially in the last years. While the analysis in *paper 4* has shown that social security can encourage voters to accept the short-term burden of the costs of climate change policies without punishing the incumbent, one can assume that greener policy preferences have similar effects on individual acceptance of climate-related policies. Thus, further analysis is needed to investigate whether general shifts in public opinion towards climate change can substitute social compensatory payments which are otherwise necessary to overcome the political resistance of unpopular but long-term beneficial policies.

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07th of January 2023

"Welfare State Determinants: An Augmented Extreme Bounds Analysis"

With this letter, the two authors of the paper ***"Welfare State Determinants: An Augmented Extreme Bounds Analysis"*** confirm that Michael Ganslmeier has done more than 70% of the work for the paper. In addition, the authors consent that Michael Ganslmeier can use this paper as part of his DPhil project.

Tim Vlandas

Michael Ganslmeier

07th of January 2023

"Do Pension Benefits Mobilize?"

With this letter, the three authors of the paper ***"Do Pension Benefits Mobilize?"*** confirm that Michael Ganslmeier has done more than 70% of the work for the paper. In addition, the authors consent that Michael Ganslmeier can use this paper as part of his DPhil project.

Margaryta Klymak

Margaryta Klymak

Vlandas TRG.

Tim Vlandas

Michael Ganslmeier

13th of October 2020

"Are Climate Change Policies Politically Costly?"

With this letter, the three authors of the paper ***"Are Climate Change Policies Politically Costly?"*** **(2020)** confirm that Michael Ganslmeier has done more than 70% of the work for the paper. In addition, the authors consent that Michael Ganslmeier can use this paper as part of his DPhil project.

Davide Furceri

Jonathan Ostry

Michael Ganslmeier