

Inequality and the demand for redistribution

Six essays on the causes and consequences of policy preferences

Dissertation

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I hereby confirm that I have written this dissertation independently and without unauthorized assistance. I have indicated all sources. The dissertation has not been accepted or rejected in any previous doctoral examination.

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Abstract

This cumulative dissertation analyzes public opinion on policies that redistribute income: progressive taxes and targeted social transfers. It primarily explains why individuals and countries support more or less redistribution. The fundamental expectation is that people are driven by two motives: to expand and maintain their disposable income (“self-interest”) and to ensure a fair distribution of income (“fairness”). Those with less (more) income, those who expect to lose (gain) income, and those who find the current income distribution unfair (fair) should demand more (less) redistribution. While these theories are drawn from previous research, I revise common theoretical approaches to arrive at a deeper understanding of public opinion.

Firstly, people’s ability to pursue their economic self-interest is inhibited by cognitive limits. Those with less income (expectations) only increase their support for redistribution when they know and think about expectable financial benefits, which is often not the case. In accordance with this theory, the dissertation presents quantitative evidence suggesting that those who gain income over time reduce their demand for redistribution but those who lose income do not increase it. Moreover, those who experience more economic risk of substantial income drops are not found to increase their demand for redistribution. In contrast, the implications of employment are fully in line with rationalist expectations: those who become unemployed demand more redistribution while those who re-gain employment reduce their demand again.

Secondly, this dissertation advances the understanding of how unfairness perceptions and resulting redistribution demand arise. It uses equity theory to argue that people consider the income distribution to be unfair when there is inequality between workers with similar skills and efforts, which I coin unfair inequality. Quantitative analyses show that redistribution demand is not only higher among those who believe that unfair inequality is higher; there is also more support of redistribution in countries where objectively realized inequality between workers with similar skills and efforts (i.e., unfair inequality) is higher. This shows that unfairness beliefs are not idiosyncratic traits but are grounded in reality.

The dissertation further shows that self-interest and unfairness perceptions are not equally strong drivers of preferences across countries. People only turn their aversion to inequality (be it due to self-interest or a desire for fairness) into demand for public redistribution when redistribution can be implemented fairly and effectively. This is the case when the government wields an effective bureaucracy and is free of corruption and nepotism, i.e., when it has high-quality institutions. Quantitative evidence offers strong support for these expectations. Income and unfairness perceptions are only strong predictors of differences in public support of redistribution when the quality of government of a country is sufficiently high.

In a last step, the dissertation also analyzes the consequences of public support of redistribution. It argues that public opinion influences voting behavior (via issue-voting) and policymaking (via office- and policy-seeking politicians). Zooming in on a popular expectation subsumed under these arguments, the dissertation analyzes whether parties that governed are retrospectively punished and rewarded for implementing changes to the welfare state. It is typically assumed that parties will be punished (rewarded) for implementing welfare policies that oppose (are in line) with the policy preferences of their voters. Quantitative evidence suggests that this is not the case, not even under favorable circumstances such as a left government party. That notwithstanding, previous research suggests that prospective issue-voting does matter, where voters chose parties on the ballot paper that promise to act in accordance with their redistribution preferences.

Zusammenfassung

Diese kumulative Dissertation analysiert die öffentliche Meinung zu umverteilenden Policies, also zu progressiven Steuern und Sozialtransfers. Sie beschäftigt sich vorrangig damit, warum Individuen und Länder mehr oder weniger Umverteilung befürworten. Die grundlegende Erwartung ist, dass Präferenzen durch zwei Motivationen erklärt werden können: das verfügbare Einkommen zu erhöhen und zu bewahren („Eigeninteresse“) und eine faire Verteilung von Einkommen herzustellen („Fairness“). Wer mehr Einkommen hat oder erwartet und wer die Einkommensverteilung als unfairer einschätzt fordert demnach mehr Umverteilung. Diese Dissertation nimmt eine Reihe von Verfeinerungen dieser in der Literatur üblichen Theorien vor.

Erstens ist das Verfolgen von ökonomischem Eigeninteresse durch kognitive Beschränkungen limitiert. Ökonomisch schlechter Gestellte erhöhen ihre Nachfrage nach Umverteilung nur, wenn sie den zu erwartenden Nutzen kennen und in ihre Entscheidung einfließen lassen, was oft nicht der Fall ist. Im Einklang mit dieser Theorie zeigt diese Dissertation, dass die Unterstützung von Umverteilung mit steigendem Einkommen sinkt, aber nicht mit sinkendem Einkommen steigt. Darüber hinaus hat ökonomisches Risiko keinen feststellbaren Zusammenhang mit Policy-Präferenzen. Im Einklang mit rationalistischen Erwartungen erhöhen aber Arbeitslose ihre Unterstützung von Umverteilung und senken sie wieder bei Rückeintritt in den Arbeitsmarkt.

Zweitens untersucht die Dissertation die Entstehung von Unfairnesswahrnehmungen. Sie argumentiert, dass Individuen die Einkommensverteilung als unfair einschätzen, wenn Ungleichheit zwischen Arbeiter*innen mit ähnlichen Fähigkeiten und ähnlichem Einsatz („unfaire Ungleichheit“) besteht. Quantitative Analysen zeigen dementsprechend eine höhere Unterstützung von Umverteilung bei Personen auf, die eine höhere unfaire Ungleichheit wahrnehmen; darüber hinaus weisen Länder, in denen die empirisch realisierte unfaire Ungleichheit höher ist, eine höhere Unterstützung von umverteilenden Policies auf. Das zeigt, dass Unfairnesswahrnehmungen nicht eigentümliche Wahrnehmungen, sondern in der Realität verwurzelt sind.

Die Dissertation zeigt weiterhin, dass sich die Relevanz vom Eigeninteresse und dem Wunsch nach einer fairen Einkommensverteilung zwischen verschiedenen Ländern unterscheidet. Individuen überführen ihre Ungleichheitsaversion nur in eine Nachfrage nach Umverteilung, wenn Umverteilung effektiv und fair umgesetzt werden kann. Das ist bei einer effektiven Bürokratie und einer Abwesenheit von Korruption und Nepotismus der Fall, also wenn eine hohe Regierungsqualität besteht. Quantitative Evidenz bestätigt diese Theorie. Einkommen und wahrgenommene Unfairness werden stärkere Prädiktoren von Unterschieden in der Unterstützung von Umverteilung, wenn die Regierungsqualität steigt.

Im letzten Schritt untersucht die Dissertation Konsequenzen der öffentlichen Meinung zu Umverteilung. Sie argumentiert, dass sich die öffentliche Meinung auf das individuelle Wahlverhalten und die Formulierung von Policies durch Regierungen auswirkt. Im Spezifischen analysiert die Dissertation, ob Regierungsparteien für Änderungen des Wohlfahrtsstaates retrospektiv an der Wahlurne bestraft und belohnt werden. Nach der typischen Erwartung verlieren (gewinnen) Parteien Stimmen für Reformen, die vermeintlich im Gegensatz zu (Einklang mit) Wählerinteressen stehen. Quantitative Analysen zeigen, dass dies nicht mal unter vergünstigenden Bedingungen (wie einer linken Regierungspartei) der Fall ist. Nichtsdestotrotz zeigt bisherige Forschung die Relevanz von prospektivem Wahlverhalten auf: Wähler entscheiden sich für Parteien, die Policies im Einklang mit individuellen Präferenzen zu Umverteilung versprechen.

List of submitted studies

- Paper A Ahrens, Leo (2022). Unfair inequality and the demand for redistribution: why not all inequality is equal. *Socio-Economic Review* 20(2): 463-487.
DOI: [10.1093/ser/mwaa051](https://doi.org/10.1093/ser/mwaa051)
- Paper B Ahrens, Leo (2022). The (a)symmetric effects of income and unemployment on popular demand for redistribution. *West European Politics* 45(7): 1407-1432.
DOI: [10.1080/01402382.2021.1963139](https://doi.org/10.1080/01402382.2021.1963139)
- Paper C Ahrens, Leo (2022). Labor market risks and welfare preferences: a bounded rationality approach. Revise and resubmit at *Socio-Economic Review*.
- Paper D Ahrens, Leo (2019). Theorizing the impact of fairness perceptions on the demand for redistribution. *Political Research Exchange* 1(1): 1-17.
DOI: [10.1080/2474736X.2019.1617639](https://doi.org/10.1080/2474736X.2019.1617639)
- Paper E Ahrens, Leo (2022). The quality of government conditions political disagreement over redistributive policies. Under review.
- Paper F Ahrens, Leo & Bandau, Frank (2022). The electoral consequences of welfare state changes: a sober look at theory and evidence. Forthcoming in *Journal of European Public Policy*.
DOI: [10.1080/13501763.2022.2096669](https://doi.org/10.1080/13501763.2022.2096669)

Framework paper

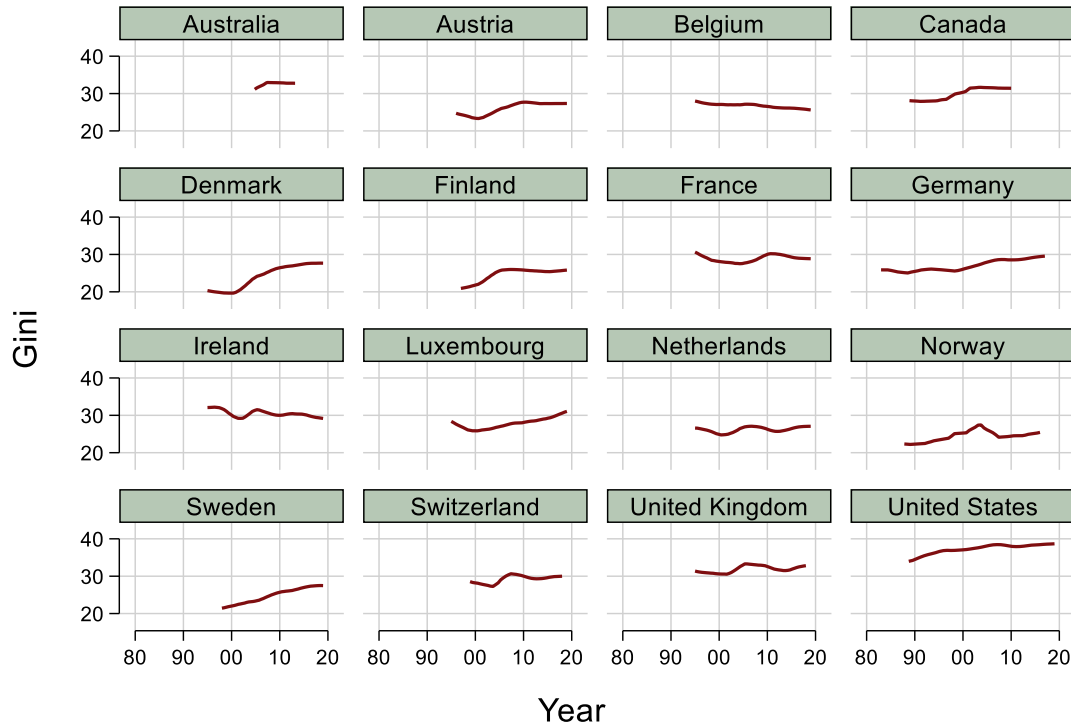
INTRODUCTION

Following the golden years of post-war capitalism, inequality has been on the rise in highly developed countries since the 1980s. The distributions of income (Chancel et al. 2022; Piketty and Saez 2014), wealth (Piketty 2014), and economic insecurity (Hacker et al. 2014) have become more unequal in many countries. Figure 1.1 exemplifies this with recent data on the development of income inequality in highly developed countries (see page 2). Inequality increased in most countries under study, in some countries such as the USA sharply. On average, the Gini coefficient increased by about 0.1 points every year. However, the trend is not encompassing – countries such as Ireland also experienced decreasing inequality.

The increase of inequality within highly developed countries is one of the defining phenomena of our time. Researchers warn that inequality is increasing to levels lastly seen in the robber baron phase of capitalist development (Piketty 2014, 2020). This is not only the case in liberal-leaning countries; traditionally highly egalitarian countries, for example those in Scandinavia, have also embarked on a path of rapidly increasing inequality. Furthermore, post-communist countries seem to be stuck in a state of high inequality after initial hopes of equitable growth have long been fading.

The increase of inequality in highly developed countries is puzzling since these countries tend to be democracies. Democratic majorities have the power to counteract increases in market income inequality via the tax-transfer-system (Guillaud et al. 2019). In fact, a majority of citizens have a material incentive to support inequality reduction (Meltzer and Richard 1981; Romer 1975). And even if people are rather guided by normative goals, rising inequality should trigger a democratic response insofar as inequality is perceived as unfair or otherwise illegitimate. Corresponding arguments can be made about the inequalities of wealth and economic insecurity. Contrary to these expectations, Figure 1.1 shows that many democracies under study have chosen not to curb inequality. Why is this?

This dissertation analyzes how public opinion on redistribution emerges and how it impacts the politics and policies of highly developed democratic countries. It is mostly concerned with the link between realized resource distributions and public opinion on redistribution, focusing on the role of income inequality. Is there a link between objective inequality and public demand for redistribution, and do national institutions amplify or dampen this relationship? Furthermore, the dissertation examines the consequences of what people want. Does public opinion on redistribution influence election outcomes, party politics, and policymaking? By providing answers to these questions, the dissertation sheds light on the puzzling lack of a democratic response to inequality.

FIGURE 1.1: Inequality of net equivalized household income, 1980-2020

Source: The Gini estimates are based on Luxembourg Income Study (LIS) and Eurostat microdata. The data are retrieved from the World Income Inequality Database (WIID).

Note: The time series are smoothed with local polynomials.

This framework paper is an introduction to the cumulative dissertation. It follows the common thread that weaves through all of the six studies it contains. It outlines a unified theoretical framework and shows how the papers fit into it. Moreover, the framework paper summarizes the dissertation's core themes and identifies its theoretical and empirical contributions to the understanding of the political economy of redistribution.

The dissertation in short

I first summarize the whole argument before revisiting the puzzling lack of a democratic response to inequality. I argue that individuals have two motives for supporting more or less redistribution: they follow their material self-interest (Meltzer and Richard 1981; Romer 1975), and they are driven by normative concerns about the fairness of resource distributions (Alesina and La Ferrara 2005; Fong 2001). In short, I expect that individuals with less material resources and those who perceive realized inequality as unfairer demand more redistribution. Regarding the consequences of public opinion, I expect that people's support of redistribution structures voting behavior. People are more inclined to vote for left parties as their demand for redistribution increases. Assuming that parties implement their policy goals, this drives a positive relationship between public opinion on redistribution and actual redistribution.

While these expectations largely reverberate previous research, I introduce several theoretical refinements to arrive at a more nuanced understanding of public opinion and its consequences. Moreover, all of the dissertation's papers also provide novel empirical evidence that furthers the credibility of some of these theories (while questioning the validity of others). This evidence is

consistently derived from quantitative data analyses. I analyze both individual-level data from multi-country surveys as well as country-level data.

The dissertation can be summarized along the following themes. Firstly, *self-interest matters, but human rationality is limited*. Two papers of the dissertation (B and C) theorize that humans are principally guided by material self-interest in their demand for redistribution, but they have incomplete information and rely on heuristics in their preference formation. The implication is that material self-interest plays a role, but only inconsistently. The relationship between objective financial circumstances and redistribution preferences primarily persists for individuals who face a situation where their material self-interest is abundantly clear to them.

Both Papers B and C contribute novel empirical evidence derived from quantitative analyses with credible causal effect identification strategies. The papers support the expectation that changes in income and (un)employment over time influence redistribution preferences, but mirroring the limits to rationality, only income increases have an impact while income decreases do not. Furthermore, labor market risks are estimated to be largely inconsequential, which is explained by workers' lacking information on their risk exposure and the low saliency of future-related concerns in preference formation. This latter finding goes against the grain of contemporary political economy scholarship.

Secondly, *the objective unfairness of the income distribution matters*. Two papers (A and D) develop and apply the theoretical argument that people demand more redistribution when there is income inequality between people with the same labor-related merits, such as education and occupation. The reason is that such inequality is considered to be unfair, and people seek to reduce unfair inequality with redistribution. Paper D supports this expectation with quantitative evidence. Paper A further argues that the extent to which people consider resource distributions to be unfair is grounded in reality, i.e., unfairness perceptions depend on *actual* inequalities between people with the same labor-related merits. Quantitative evidence then shows that the average redistribution demand in countries can be meaningfully explained by an empirical quantification of inequality between people with similar labor-related merits.

Thirdly, *established political economy theories on preference formation are primarily applicable to highly developed countries*. Paper E argues that high-quality public institutions are a scope condition for the expectation that material self-interest and unfairness structures public opinion on redistribution. The reason is that redistribution can only be implemented fairly and effectively if public institutions are of sufficient quality. Since people anticipate the effects of redistribution, financial circumstances and unfairness perceptions become stronger determinants of redistribution preferences under higher-quality governments. The empirical analysis of Paper E supports this view. Household income and perceived pay unfairness strongly structure public opinion in high-quality settings, whereas the preferences of those with different income and unfairness perceptions do not diverge in lower-quality settings. Therefore, traditional political economy theories on policy preferences and connected political phenomena such as voting behavior apply less in countries with lower-quality institutions.

Lastly, *individuals do not seem to retrospectively punish (reward) government parties that implement redistributive policies which go against (are consistent with) their policy preferences*. The viewpoint that people hold mostly fixed policy preferences based on their material standing and that they retrospectively punish and reward government parties at elections for having implemented policies that are more or less consistent with these preferences is endemic in political economy research (e.g., Emmenegger and Häusermann 2012; Hübscher et al. 2021a; Hübscher et al. 2022; Rueda 2005, 2006, 2014). Paper F analyzes this expectation regarding the welfare state (i.e., one of the levers used to implement redistribution). It asks whether government parties experience electoral consequences after they implement and oversee welfare changes

during their incumbency. The results show that this is not even the case under favorable conditions, such as a left party in government or high clarity of responsibility. There are several reasons for this. Most importantly, voters are aware of the trade-offs of welfare and do not simply vote according to their narrow interests; policy preferences are partly endogenous to the political process and influenced by, e.g., the strategic framing of parties; and voters do not always observe (and remember) actual welfare changes.

It must be noted that Paper F does not imply that policy preferences are inconsequential for downstream political phenomena in general. While it questions the prevalence of *retrospective* voting based on the past policy performance of government parties, I continue to expect that policy preferences are important determinants of *prospective* voting behavior. That is, people vote for parties whose platforms are more in line with their stance on redistribution. Furthermore, my analysis of retrospective voting only pertains to redistribution via the welfare state and not via tax policies.

Understanding the lack of a democratic response to inequality

Much has been written about the puzzling lack of a democratic response to inequality. Several contributions dispute that material self-interest is a leading motivation for supporting redistribution, implying that a democratic majority does not necessarily want to “soak the rich” if this does not conform to their social justice beliefs (Luebker 2007; Mau 2003, 2004). Politicians are not incentivized to implement the policy preferences of the relatively poor because the less well-off participate less in elections (Schäfer and Schwander 2019). Politicians also supposedly care less about the poor because members of their profession are primarily recruited from the middle and upper classes (Elsässer et al. 2021; Elsässer and Schäfer 2022). The owners of capital have stronger instrumental and structural power over governments (Emmenegger et al. 2015; Hacker and Pierson 2010; Woll 2016). Iversen and Soskice (2006) argue that majoritarian electoral institutions induce a political coalition of the middle and upper classes that disregards the pro-redistribution stance of the lower classes (see also Iversen and Soskice 2009). Lastly, researchers highlight that there has in fact been additional redistribution in response to inequality (Georgiadis and Manning 2012; Kenworthy and Pontusson 2005), although disposable incomes have become more unequal nonetheless (see Figure 1.1).

This dissertation sheds new light on the absence of “Robin Hood policies”. As I will show below, all of its core themes explain why—in many countries—democratic majorities do not curtail increasing market income inequality with redistribution. Note that the dissertation does not offer a complete theory that aims to replace the aforementioned arguments; it rather provides a deeper understanding to complement previous research.

Firstly, inequality increases the incentives for the masses to “soak the rich”, but people are unable or unwilling to pursue this because their decision-making relies on heuristics and informational shortcuts. Importantly, people do not perfectly observe inequality and may not be aware of the increase in inequality in their country, as this development cannot be directly observed. Likewise, few are able to connect rising inequality to their policy preferences. For example, it takes considerable sophistication to understand that rising inequality increases the material benefits of redistribution for median earners (Bartels 2005). Therefore, inequality does not necessarily lead to a democratic response even when people are guided by material self-interest or normative concerns about inequality.

Secondly, while rising inequality may well trigger unfairness perceptions, which in turn should drive redistribution demand and then actual redistribution, rising inequality is not necessarily seen as unfair. The reason is that, according to this dissertation’s equity theory framework, people assess outcome unfairness by comparing people with similar labor merits such as

education and occupation and by checking whether their returns to these merits are unequal. The implication is that people will not see inequalities as unfair when they solely develop *between* people with different merits (e.g., across different occupations); primarily inequalities *within* groups of people with similar merits should trigger unfairness perceptions. Unfairness perceptions do not necessarily arise when people drift apart but only ever compare to others who concurrently drifted apart (Mijs 2021). In effect, there will be no normatively motivated democratic response to inequality.

Research confirms that inequalities have primarily arisen between rather than within groups of people with various educational and occupational backgrounds. Labor markets have dualized, i.e., there has been an increasing rift between skilled workers, who are in demand, and unskilled workers, whose standing in the labor market is deteriorating (Emmenegger and Häusermann 2012; Hope and Martelli 2019; Kurer 2020; Kurer and Palier 2019). Likewise, occupations with strong union representation flourish while occupations not covered by collective bargaining agreements, primarily in the service sector, are assigned to the fringes of the labor market (Emmenegger and Häusermann 2012). In addition, incomes flow more and more to managers and owners of capital rather than to labor (Chancel et al. 2022; Hope and Martelli 2019). These developments imply that increasing inequality is not necessarily perceived as unfair, which hinders a democratic response.

Thirdly, voters may not be able to induce inequality-reducing redistribution because they are unable to punish governments retrospectively for redistributive policies that run against their interests (but note that this finding only pertains to welfare and not to taxation). The implication is that even if voters would wish for more redistribution, they may find it difficult to vote parties into government that further this goal. Voters seem to primarily use prospective voting, electing parties into government that promise to further their redistributive goals. However, punishing and rewarding governments based on their policy performance constitutes another crucial mechanism that ensures good governance. The use of this mechanism may be limited, which helps to explain the lack of redistribution in response to rising inequality.

Lastly, rising inequality may not be cushioned by redistribution in countries with lower government quality. In these countries, public institutions have limited capacity to redistribute fairly and effectively. For this reason, redistribution is less politicized. In effect, rising inequality may be left unchecked.

The relevance of studying redistribution preferences

The previous section demonstrated that the dissertation helps to understand the democratic non-response to inequality. Before turning to the details, it is worthwhile to consider further reasons for why learning about redistribution preferences is important. Firstly, I expect that redistribution preferences influence various political phenomena such as voting behavior, party preferences, and policymaking. Learning about the causes of policy preferences therefore implies learning about the causes of these political phenomena as well.

Next, learning about redistribution preferences backs research on democratic representation. A growing literature in political science analyzes whether representation by parties and governments is unequal across society (see Elkjær and Klitgaard 2021 for a recent review). The typical worry is that people higher up in the social hierarchy hold sway over how parties think and act (e.g., Elsässer et al. 2021; Schakel et al. 2020), which has negative implications for downstream phenomena such as electoral participation (Schäfer and Schwander 2019). To put it differently, redistribution preferences may have an unequal effect on politics and policies along the social hierarchy. It is therefore fruitful to analyze the determinants of policy preferences to unearth the roots of unequal representation.

TABLE 1.1: Submitted studies and their short titles

Paper A	Unfair inequality and the demand for redistribution: why not all inequality is equal
Paper B	The (a)symmetric effects of income and unemployment on popular demand for redistribution
Paper C	Labor market risks and welfare preferences: a bounded rationality approach
Paper D	Theorizing the impact of fairness perceptions on the demand for redistribution
Paper E	The quality of government conditions political disagreement over redistributive policies
Paper F	The electoral consequences of welfare state changes: a sober look at theory and evidence

Note: See the list of submitted studies for full bibliographical citations.

Finally, learning about redistribution preferences uncovers truths about the nature of humanity. For example, the assumption that humans are driven by material self-interest dominates theorization in political economy research. This dissertation critically assesses such assumptions, which improves the foundations of future theory building. Moreover, learning about humans is interesting in itself. Humanity is constantly struggling about the distribution of resources – the politics of redistribution drive at the heart of this quarrel.

Outlook

The remainder of this framework paper is structured as follows. The next section outlines the overarching theoretical framework and situates the individual papers within it. The five subsequent sections each focus on one node of the overarching theoretical model. They discuss the dissertation’s individual papers, summarize their theories, and outline their empirical evidence and contributions. Finally, the last section concludes.

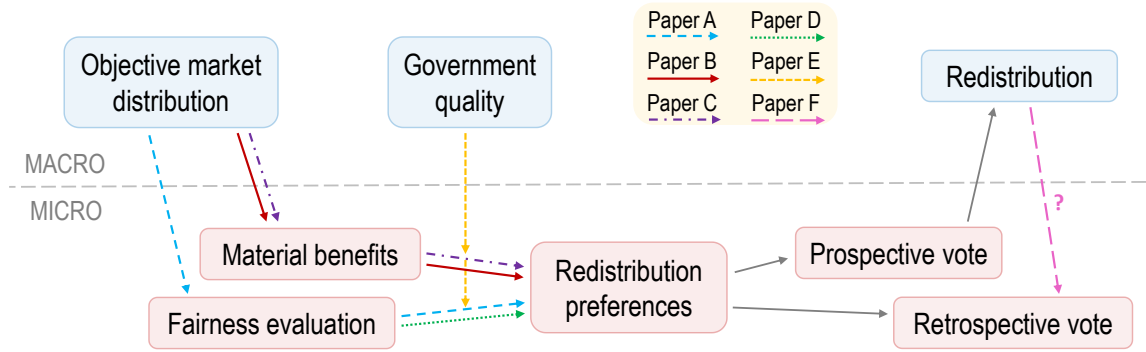
THEORETICAL FRAMEWORK

This section introduces the theoretical framework of the dissertation. It serves as an overview of the general theoretical argument and situates the individual papers within it. Table 1.1 lists the shorthand titles for all papers again.

Definition of concepts

I first define important concepts. I follow Aalberg (2003: ch. 1) and theorize that people hold three types of beliefs about inequality. Firstly, *ideals* are basic guiding principles on what state of society is desirable. For example, individuals are more or less egalitarian, i.e., they desire different degrees of inequality. Other important ideals are equity, which holds that economic resource distributions should be governed by individual merit, and need, which requires that people’s needs should be satisfied (Aalberg 2003: ch. 2; Deutsch 1975, 1985). Secondly, *perceptions* are specific beliefs about the state of observable reality. For example, individuals have perceptions about the level of realized inequality and its normative desirability. Lastly, people hold *preferences*, which are specific beliefs about how deeply held ideals can be obtained. For example, individuals can prefer a more or less progressive design of income taxes.

The central concept of this dissertation is *redistribution preference*, which refers to individuals’ specific beliefs about the desired level of redistribution via taxation and social spending. I

FIGURE 1.2: Theoretical model of the dissertation

also use the terms *redistribution demand* and *support* interchangeably. Another significant concept is *unfairness perceptions*, by which I mean beliefs about the fairness of realized resource distributions or the processes that created them. Lastly, inequality-related ideals such as egalitarianism are important concepts, but I take them for granted in this dissertation. That is, I do not theorize why people hold certain ideals such as egalitarianism and how these may change.

Theoretical argument

Figure 1.2 presents the overarching theoretical framework in the form of a graphical micro-macro model. The dissertation is primarily concerned with the determinants of redistribution preferences in Papers A-E, i.e., the left-hand side of Figure 1.2. But the theoretical framework also includes the political consequences of redistribution preferences, i.e., the right-hand side of Figure 1.2.

There are two motivations for broadening the framework to include the consequences of policy preferences. Firstly, Paper F assesses whether voters retrospectively punish and reward government parties at the polls for implementing policies that are more or less congruent with their redistribution preferences, which relates to the right-hand side of Figure 1.2. Secondly, exploring the consequences of public opinion underscores the relevance of studying redistribution preferences. Figure 1.2 therefore includes theoretical expectations that are not analyzed in the individual papers (such as the link between redistribution preferences and prospective voting).

I now turn to the determinants of redistribution preferences. My central assumption is that people form their preferences in accordance with instrumental motivations. People desire a certain level of redistribution because they want to achieve two goals: (a) to receive and maintain disposable income (i.e., “material self-interest”) and (b) to achieve a fair distribution of disposable income. The extent to which these two motivations guide individuals differs, not least because they may clash or reinforce each other. And it is important to note that this list of motivations is not exhaustive. There are other determinants of redistribution preferences, and it is not my goal to explain preferences fully.

I start with the role of material self-interest. The baseline expectation is that the support of redistribution decreases as individuals’ relative positions in the income and risk distributions improve. This is because those with a weaker (stronger) relative position consider that they stand to gain (lose) from redistribution. Papers B and C use this basic rational choice framework and refine it by theorizing on the limits to human rationality. Their starting point is (a) that individuals have imperfect knowledge about their place in the income and risk distributions and (b) that this information is not consistently used in preference formation. Paper B argues that

changes in income and (un)employment over time therefore do not necessarily have symmetrical effects; and Paper C uses the same bounded rationality framework to argue that labor market risks could be mostly inconsequential for policy preferences because workers are not necessarily informed about their risk exposure and only use this information when it becomes salient.

Moving back to the theoretical framework depicted in Figure 1.2, people also assess the fairness of the income distribution and not only their expected material benefit. They consider whether the distribution is congruent with deeply held distributive justice ideals, most importantly equity (are outcomes consistent with individual merit?), equality (are outcomes equal enough?), and need (do the needy receive sufficient resources?) (Aalberg 2003; Deutsch 1975, 1985). If this is not the case, they consider the observed distribution to be unfair and demand redistribution as a result. The underlying assumption is that people instrumentally seek to equalize unfair inequality via redistribution.

Papers A and D utilize this fairness-based theory. Paper D applies equity theory from social psychology (Adams 1965) to explain under what circumstances people consider the income distribution to be unfair (which then results in redistribution demand). The argument goes that people compare themselves and others to individuals with similar labor market characteristics and consider the distribution to be unfair when similar individuals receive different pay. Accordingly, Paper A argues that countries where inequalities are higher between those with similar labor-related merits should demand more redistribution.

I further expect that the extent to which self-interest concerns and fairness perceptions influence redistribution preferences depends on the institutional context, in particular the quality of government. Paper E argues that, even when people support an equalization of the income distribution, they do not necessarily turn to the state and demand public redistribution. The reason is that not all governments have the capacity to redistribute effectively and fairly. The implication is that self-interest and fairness perceptions become more influential in higher-quality settings.

I now turn to the consequences of redistribution preferences, i.e., the right-hand side of Figure 1.2. I expect that preferences structure voting behavior and policymaking. Regarding voting behavior, I again assume that people have instrumental motivations: they seek to vote a party into government that will further their redistributive goals. That is, voters care about the issue redistribution and adjust their voting behavior accordingly. However, I contend that other motivations (such as candidate orientations) also drive voting behavior. For the purpose of this dissertation, I assume that such motivations are independent from redistributive issue-voting.

There are two types of economic voting, namely prospective and retrospective voting (Duch and Stevenson 2008; Lewis-Beck and Stegmaier 2013). The prospective voting argument holds that people with higher demand for redistribution should vote for left-leaning parties with higher probability. The reason is that left-leaning parties have a more pro-redistribution policy agenda. In contrast, retrospective voting entails that voters reward or punish incumbent parties after their term, depending on whether they implemented redistributive policies in accordance with their policy preferences. However, this latter expectation may not transpire since there is only inconsistent evidence on voters paying heed to policies in their retrospective vote (Lewis-Beck and Stegmaier 2013).

Paper F offers a theoretical and empirical evaluation of the retrospective voting argument specifically applied to the welfare state. Do parties experience electoral consequences after implementing changes to welfare? The paper identifies the theoretical assumptions of retrospective voting theory and discusses their validity. Its comprehensive empirical analysis fails to find evidence for electoral consequences of welfare change. However, further evidence from the

literature suggests that taxation may have electoral consequences. Due to the mixed evidence, a question mark is added to the arrow pointing to retrospective voting in Figure 1.2.

Lastly, redistribution preferences also structure actual redistribution. There are two reasons. Firstly, citizens' redistribution preferences determine what parties end up in government. A stronger support of redistribution will raise the probability of a pro-welfare party forming a government, which subsequently implements more redistribution. Secondly, politicians are vote- and office-seekers; they respond to public opinion to ensure their re-election (Stimson et al. 1995).

Placing the framework in the literature

It is worthwhile to compare the core pillars of my theoretical model to other approaches in the political economy literature. Firstly, I use a simple unidimensional policy space that ranges from a low to a high preferred level of redistribution. This resembles dominant public choice models (Meltzer and Richard 1981; Romer 1975) and bears similarities to the power resources approach (Korpi 1985, 2006). However, this unidimensional policy space differs from more sophisticated approaches such as the constrained partisanship model (Beramendi et al. 2015) that use a multi-dimensional specification, for example focusing on disagreement between policies targeting consumption and investment. But it must be noted that the economic left-right conflict centered around distributional issues is generally pivotal in political economy models. My theoretical approach can therefore be characterized as a focused account of classical (re-)distributive conflict, and it is not my intention to characterize other policy cleavages as inconsequential.

Secondly, my model differs from most political economy frameworks because it not only considers materialist rational choice expectations (of course there are exceptions, such as the work of Alberto Alesina and his associates; see Alesina and Angeletos 2005; Alesina and La Ferrara 2005). That is, I theorize that people's policy preferences depend on motivations other than optimizing disposable income and economic security, namely a wish for economic fairness. In my view, broadening the theoretical scope in this way leads to a much deeper understanding of public opinion.

Thirdly, I characterize public opinion and electoral politics as central determinants of policymaking. This is akin to the constrained partisanship model (Beramendi et al. 2015) and public choice models (Meltzer and Richard 1981; Romer 1975). However, the approach differs from many approaches in political economy, which often characterize organized interests and their struggles as determinants of public policies with little regard for the electoral arena (Hall and Soskice 2001; Martin and Swank 2004; Thelen 2012, 2014). Following Beramendi et al. (2015), I advocate for the crucial role of electoral politics in determining distributional outcomes, while it is not my intention to sideline the importance of other approaches, for example of those focusing on producer group coalitions.

Lastly, my model focuses on political demand and neglects political supply. In contrast, other theoretical approaches stress the importance of issues being politicized by political entrepreneurs to give them electoral importance (e.g., Beramendi et al. 2015). I contend that this is a realistic depiction of the political process, but I neglect supply to keep my model simple. This requires the assumption that political supply—such as a left pro-redistribution party on the ballot—is present in the analyzed countries.

Outlook

Following this summary and discussion, the remainder of the framework paper discusses the theoretical arguments and their relation to the literature in more detail. Each of the subsequent sections deals specifically with one of the nodes in Figure 1.2. The sections review the state of

the art in the literature, outline shortcomings and weaknesses, and introduce the arguments and findings of the dissertation’s papers against the background of previous research.

THE ROLE OF MATERIAL SELF-INTEREST (PAPERS B AND C)

I begin with the expectation that material self-interest influences redistribution preferences. The guiding question is whether and to what extent the income and risk distributions structure public opinion via material self-interest calculation.

Theoretical background

The central reference points on redistribution of the political economy literature are the public choice models by Romer (1975) and Meltzer and Richard (1981). In the Meltzer-Richard formulation, governments redistribute via lump-sum transfers that are financed by a linear income tax. They assume that voters follow their material self-interest and seek to maximize their disposable income. The model implies that individuals’ support of redistribution depends negatively on the distance between their income to the mean income.

Two expectations can be derived from the Meltzer-Richard model. On the micro-level, the demand for redistribution should decrease with income because the expected material utility of redistribution decreases with income.¹ On the macro-level, there should be a positive relationship between inequality and the average level of support for redistribution. The reason is that income distributions are always right-skewed, implying that mean incomes exceed median incomes. As inequality rises, the mean increases further relative to the median. In effect, average redistribution support rises as a majority of income recipients has a stronger material incentive to “soak the rich”.

Further rationalist accounts apply the perspective of the Meltzer-Richard model to the future. The expectation is that people not only seek to maximize current but also future income (Alesina and La Ferrara 2005; Benabou and Ok 2001; Moene and Wallerstein 2001, 2003). Redistribution preferences depend on income expectations rather than current income in this alternative formulation. Those who expect to increase (decrease) their income should already demand less (more) redistribution in the present.

The future-oriented approach holds that individuals use redistributive policies (such as unemployment assistance) as a form of social insurance against shocks to market income. The perspective explains why redistribution is popular among those who currently have limited benefit from redistribution, such as the middle class (Moene and Wallerstein 2001, 2003). The future is always uncertain, and even people who are high-earners today may benefit from redistribution tomorrow, for example after losing employment.

Given that labor is the most significant income source for most, a large literature expects that individuals demand redistribution to insure against labor market risk. Labor market risk is conceptualized as the individual probability of becoming unemployed or underemployed. The baseline expectation is that those with a higher probability or greater cost of becoming unemployed should demand more redistribution (Alt and Iversen 2017; Iversen and Soskice 2001; Marx 2014; Rehm 2009, 2011).

¹ This expectation is exacerbated when moving away from the simplistic model because, in reality, governments do not redistribute via flat-rate taxes and lump-sum transfers; they specifically target poorer individuals with their transfers and impose progressive taxes on income (Guillaud et al. 2019).

Empirical evidence so far

Rationalist accounts of redistribution preferences are generally supported by empirical research. Regarding the micro-level expectations of the Meltzer-Richard model, studies comparing people with more and less income consistently find that redistribution support decreases with income (e.g., Beramendi and Rehm 2016; Corneo and Grüner 2002; Franko et al. 2013; Rueda and Stegmueller 2019). This finding is mirrored in studies that analyze longitudinal changes in income over time. Those who become richer (poorer) over time also tend to decrease (increase) their support for redistribution, albeit to a weaker extent and with less consistency than suggested by cross-sectional research (Gidron and Mijs 2019; Margalit 2013; O’Grady 2019; Owens and Pedulla 2014).

The future-oriented perspective also finds empirical support. The literature identifies a diverse set of labor market risks and generally shows that objective risk exposure is associated with more demand for redistribution and social policies. This holds for the risk factors of occupational unemployment (Alt and Iversen 2017; Cusack et al. 2006; Rehm 2009, 2011; Vlandas 2020), automation and digitization (Busemeyer and Sahm 2022; Dermont and Weisstanner 2020; Thewissen and Rueda 2019), globalization exposure (Walter 2010, 2017), and skill specificity (Cusack et al. 2006; Iversen and Soskice 2001; Rehm 2009). However, studies on these risk factors that employ credible experimental designs are inconsistent with the theoretical expectations (Gallego et al. 2022; Zhang 2019), which casts doubts on the findings of cross-sectional research. Nevertheless, further research shows that atypical employment forms such as temporary contracts are associated with stronger demand for redistribution (Burgoon and Dekker 2010; Marx 2014; Pahontu 2022). The unemployed also increase their redistribution demand beyond what their drop in market income can explain, which supports the view that future-related concerns are at play (Cusack et al. 2006; Margalit 2013; Naumann et al. 2016; Rehm 2011).

Lastly, the macro-level implication of the Meltzer-Richard model—a positive association between inequality and average redistribution demand—finds limited support. Some studies do find such an association (Dallinger 2008; Finseraas 2009; Jæger 2013; Johnston and Newman 2015), but other studies report null findings (Dallinger 2010; Kenworthy and McCall 2008; Luebker 2007). Furthermore, mixed-level research shows that the (inconsistent) effect of macro inequality cannot be explained by material self-interest on the micro level. The difference in redistribution support among the rich and poor *decreases* in more unequal societies even though the poor should have a stronger incentive to “soak the rich” (Dimick et al. 2017; Finseraas 2009; Schmidt-Catran 2016), which runs contrary to Meltzer and Richard (1981). This suggests that the effect of inequality may rather stem from a normative rejection of inequality, a topic that I shall return to later.

Shortcomings of previous research

Papers B and C of this dissertation both depart from a critique of previous research employing rational choice theories. Firstly, previous theories impose partly unrealistic rationality assumptions. To truly optimize their disposable income and economic security, people require knowledge of their positions in the income and risk distributions, and they must make use of this knowledge in preference formation. I argue that, especially regarding economic risk, these assumptions are not always met, which limits utility optimization and makes the relationship between people’s relative economic standing and policy preferences less consistent than assumed.

Secondly, previous empirical research on the effects of financial circumstances (i.e., income, unemployment, and labor market risks) predominantly relied on cross-sectional data, where causal effects are possibly not identified due to omitted variable bias. This shortcoming is most

pronounced in research on labor market risks. It is entirely possible that self-interest concerns play a more marginal role in preference formation than expected, especially because normative predispositions acquired during socialization are likely to bias cross-sectional estimates (O’Grady 2019; Wehl 2019).

Theoretical argument

Papers B and C generally follow previous research and assume that people pursue the instrumental motivation of optimizing their current and future disposable income through their demand for redistribution. However, I characterize humans as only boundedly rational. They principally follow their material self-interest in the formation of their policy preferences. However, they have insufficient information to arrive at utility-optimizing preferences (i.e., information relating to objective distributions of income and risk as well as to the nuts and bolts of the tax-transfer-system) (Engelhardt and Wagener 2018; Fernández-Albertos and Kuo 2018; Jensen and Zohlnhöfer 2020). Individuals also do not always make use of the information they possess because they only utilize a limited amount of currently salient information (Jones 1999; Simon 1985). Therefore, the expectations of rational choice theory are especially likely to transpire when information about objective distributions is readily available and when individuals are primed to consider their material self-interest during preference formation.

Paper B uses the bounded rationality framework to explore whether income and unemployment may have asymmetric effects where, e.g., becoming unemployed has a differently sized effect compared to gaining employment. There are several reasons for why this may be the case. Firstly, changing financial circumstances equip workers with new information that may persist even after a reversal of said changes, for example information about the likelihood of unemployment. In effect, the reversal of a change in circumstances (e.g., unemployment) may be associated with a differently sized change in policy preferences compared to the initial change. Secondly, information about the deterioration and improvement of financial circumstances may be associated with a different degree of saliency, implying that information use is asymmetric. For example, people may exhibit a negativity bias and reduce their redistribution support after gaining income but not the other way around.

Paper C uses the bounded rationality framework to critically assess whether labor market risks really are influential in preference formation. Due to the limited rationality of workers, the assumption that they are well informed about their objective risk exposure is overly strong. I rather expect that workers only possess information on the most pronounced risks. Furthermore, they do not necessarily use information on their risk exposure in preference formation. Rather, they only consider their future financial circumstances when it is abundantly clear that future disposable income is at stake. The implication is that only the most pronounced and unambiguous risks should lead to an increase in redistribution support. Primarily unemployment—the primary realized risk—and perhaps other significant risks such as temporary employment should be influential in preference formation. In contrast, most risks identified by the literature such as occupational unemployment and skill specificity should be less influential.

Empirical evidence

A crucial element of Papers B and C is to quantify the causal effects of financial circumstances based on more credible assumptions compared to previous research, which predominantly relied on cross-sectional data prone to omitted variable bias. Both papers do so by analyzing changes in financial circumstances (i.e., income, unemployment, risk) and policy preferences that occur within units (i.e., individual workers or occupations) over time, while previous research compared the policy preferences of those with better and worse financial circumstances. The

advantage of this approach is that stable differences in financial circumstances and policy preferences, e.g., between different workers, cannot bias the estimates. While this within-unit design requires additional assumptions about the data (Imai and Kim 2021; Kim and Imai 2019; Plümper and Troeger 2019), it effectively sweeps away one of the most pronounced issues associated with cross-sectional data (Brüderl and Ludwig 2015). Paper B even uses a difference-in-differences (DID) design, which has especially credible identification assumptions (Lechner 2010).

Paper B uses individual-level panel data from Great Britain supplied by the British Election Study (BES). The results suggest that income and unemployment really have a causal effect on redistribution support in line with rationalist expectations. Unemployment is found to have a strong and symmetrical effect, where those who become unemployed increase their redistribution demand and those who become re-employed decrease their demand again. Income is also found to affect policy preferences, but only income increases over time are associated with a reduction in support for redistribution while income decreases have no effect. This supports bounded rationality theory, which predicted possible effect asymmetry.

Paper C conducts two empirical investigations, the first using individual-level panel data from the Swiss Household Panel (SHP) and the second using repeated cross-sections from the first nine waves of the European Social Survey (ESS). The results suggest that only unemployment—the primary realized labor market risk—has an effect that is consistent with the expectations of future-oriented rational choice theory. On the individual-level, only becoming unemployed increases support for social security spending, whereas all other risks that currently employed workers are exposed to (e.g., occupational unemployment, skill specificity, and even fixed-term employment) leave preferences unaffected. The results also show that many objective risk factors discussed in the literature are not even associated with workers' subjective risk experience. This supports bounded rationality theory and the expectation that only the most pronounced risks should affect redistribution demand.

The ESS data subsequently corroborate the finding that occupation-level risks such as occupational unemployment—which are given most attention in the literature—leave preferences unaffected. The empirical analysis evaluates whether over-time changes in the risk exposure of certain occupations are associated with over-time changes in the redistribution and welfare demand of workers within this occupation. The results show that the national unemployment rate is associated with welfare demand (but not redistribution demand), whereas the occupation-level risks leave preferences unchanged. Overall, the bounded rationality framework finds extensive support.

Main contributions

The main contribution of Papers B and C is their empirical evidence. They rely on quantitative analyses with defensible causal effect identification assumptions, whereas the cross-sectional approach of most previous research is always disputable due to possible omitted variable bias. What can be learned from the papers is that self-interest really matters in the formation of redistribution preferences, but less so than previously assumed. While income and unemployment are found to causally affect demand for redistribution and welfare, the objective risk exposure of employed workers leaves preferences unchanged. This finding contrasts with a large literature built on cross-sectional estimates.

Both papers also introduce a novel bounded rationality framework that helps to understand the limits of human rationality. This is a comparably minor contribution as the imperfect rationality of humans is undisputed. Nevertheless, Papers B and C pinpoint where exactly utility optimization fails, which guides future theorization.

THE ROLE OF INCOME UNFAIRNESS (PAPERS A AND D)

I move on to the link between the distribution of income and redistribution preferences via unfairness perceptions. The argument is that those who view the income distribution as unfair demand more redistribution. This section will also argue that unfairness perceptions are not idiosyncratic traits but rather are grounded in empirical reality.

Theoretical background

Assuming that people seek to reduce normatively undesirable inequality, their support of redistribution should increase with their growing aversion to inequality (Fong 2001; Gee et al. 2017). How much inequality people reject depends on their distributive fairness beliefs (Aalberg 2003: ch. 2). Firstly, those who support more egalitarian ideals support more redistribution by virtue of their ideological stance (Dawes et al. 2007). Secondly, people may also perceive realized inequality as unfair, i.e., as inconsistent with their distributive fairness ideals (Alesina and La Ferrara 2005; Fong 2001; Gee et al. 2017; Piff et al. 2020). For example, people demand redistribution when they feel that incomes are disproportional to individual merits. This dissertation focuses on such unfairness perceptions, placing particular attention on perceived rejection of the fairness ideal equity.

A variety of unfairness perceptions are important in the formation of policy preferences. Unfairness perceptions can first be distinguished into egotropic unfairness perceptions, which relate to the self, and sociotropic unfairness perceptions, which relate to society in general. Furthermore, unfairness perceptions concern either outcomes (i.e., the realized distribution) or processes (i.e., procedures that created the realized distribution) (Aalberg 2003: ch. 2). All forms of unfairness perceptions should spur demand for redistribution, but it is important to note that this dissertation focuses solely on outcome unfairness rather than process unfairness.

Reverberating Meltzer and Richard (1981), theories focusing on distributive fairness expect that more inequality may lead to increased demand for redistribution, but they offer a different explanation compared to rationalist theory: inequality should increase demand for redistribution insofar as people consider inequality in their country to be unfair (Luebker 2007, 2014). However, the impact of inequality should vary across countries due to differences in the objective makeup of inequality and people's normative orientations.

Empirical evidence so far

Empirical research strongly supports the notion that redistribution preferences increase with the perception that realized incomes are unfair. People who think that the realized income distribution does not reflect differences in individual merit (Alesina and Giuliano 2010; Alesina and La Ferrara 2005; Fong 2001; Isaksson and Lindskog 2009; Linos and West 2003) and those who think that ethical and actual wages diverge demand more redistribution (Kuhn 2010). Experimental research shows that the relationship is causal. People are more willing to equalize income distributions in laboratory experiments when factors outside the control of individuals determine incomes rather than skills and efforts (Cappelen et al. 2010; Gee et al. 2017; Piff et al. 2020). Furthermore, Becker (2020) shows that US citizens demand more redistribution when they are accurately informed about presumably unfair income gaps between groups with differences determined at birth (i.e., outside their control), such as sex and race.

Research also shows that unfairness perceptions relating to processes (rather than outcomes) affect redistribution demand. Those who think that there is inequality of opportunity tend to demand more redistribution (Jaime-Castillo and Marqués-Perales 2014; Page and Goldstein 2016), which is confirmed by experimental research (Grimalda et al. 2016; Shariff et al. 2016).

Further research suggests that the relationship between macro inequality and redistribution demand is driven by normative orientations rather than self-interest, which contradicts rational choice theory (Meltzer and Richard 1981). Luebker (2007) shows that people’s normative evaluation of inequality in their country is a strong predictor of average redistribution demand. Several studies show that the rich have stronger redistribution demand in more unequal countries (Dimick et al. 2017, 2018; Finseraas 2009; Schmidt-Catran 2016), which results in higher overall support of redistribution in more unequal societies. However, the relationship between inequality and policy preferences remains inconsistent, with several studies finding null effects (Dallinger 2010; Kenworthy and McCall 2008; Luebker 2007).

Shortcomings of previous research

Both Papers A and D are motivated by insufficient knowledge on the objective foundations of unfairness perceptions. It remains unclear to what extent unfairness perceptions (and resulting policy preferences) are grounded in reality. Previous research mostly treats unfairness perceptions as idiosyncratic traits or relies on simple conceptions of what makes distributions unfair (such as varying the income source between luck and work in lab experiments). However, the objective foundations of preferences and resulting political phenomena are of central interest in political economy, partly because the discipline is occupied with the distribution of economic resources and partly because theories deriving their expectations from objective facts produce more robust predictions. Therefore, the evaluation of the objective foundations of unfairness in both theory and empirics contributes to the understanding of public opinion.

This issue also raises the question of the connection between macro inequality and redistribution demand. While several studies find a relationship they attribute to distributive justice beliefs, just as many find null relationships. This makes sense as it is questionable to assume that the relationship between inequality and unfairness perceptions is the same across societies. However, having the ability to predict under what circumstances inequality spurs redistribution demand would be beneficial.

Theoretical argument

Relying on the argument that unfairness perceptions increase support of redistribution, Papers A and D develop a framework that helps to draw a link between objective inequality and policy preferences. Focusing on distributive (rather than processual) fairness, Paper D develops the theoretical argument in detail using equity theory from social psychology (Adams 1965; Stouffer et al. 1949). It argues that people desire input-output-proportionality in income outcomes. Realized distributions are perceived as fair as long as received incomes correspond to what people give in return. Since labor is the most significant income source, the most important merits relate to labor-related characteristics such as education, occupation, and working hours. These merits have no inherent monetary value, which is why people evaluate fairness by comparing themselves and others to similar individuals. Income distributions are considered to be fair as long as people with similar merits receive similar incomes, whereas different returns to relevant merits result in perceived unfairness (and ultimately in demand for redistribution). For example, inequality between two workers is perceived to be unfair when they have similar jobs and education.

Assuming that people ground their unfairness perceptions in empirical reality, it can be concluded that objective inequality increases redistribution demand insofar as there is inequality between people with similar labor-related merits. That is, people increase their demand for redistribution when they observe that people with comparable occupational and educational traits have differing incomes. To be sure, other forms of unfairness—such as inequality of

opportunity—may also spur redistribution demand, but I limit the scope of my argument to outcome unfairness according to equity theory.

Paper A applies the theory to the macro relationship between inequality and redistribution demand. The argument is that primarily unfair inequality and not overall inequality increases support of redistribution. Following equity theory, unfair inequality is defined as inequality between individuals with similar labor-related merits (broadly defined as all job-related characteristics). Therefore, countries where incomes diverge to a greater extent between people with similar occupational and educational traits should, on average, demand more redistribution due to fairness-related concerns. This argument reverberates the supposed macro relationship between inequality and policy preferences postulated by Meltzer and Richard (1981) but expects that fairness concerns—rather than material self-interest—drive the relationship. Furthermore, my argument implies that macro inequality and redistribution demand are not necessarily related; the level of *unfair* inequality is important. This explains the inconsistent relationship between inequality and redistribution demand found in previous research.

Empirical evidence

Paper D evaluates the predictions of equity theory. It conducts multilevel regression analyses of comparative micro-level survey data from 39 countries supplied by the International Social Survey Programme (ISSP Research Group 2017). The analyses evaluate whether respondents' redistribution preferences covary with their egotropic and sociotropic unfairness perceptions. The results confirm theoretical expectations. Those who find their income disproportional to their skills and efforts demand more redistribution. What is especially noteworthy is that this also pertains to respondents who consider their own income to be too large and not only those who find their income too small. Furthermore, redistribution demand is also higher among those who think that inequality is not determined by labor-related merits but rather by circumstances which should be irrelevant, such as an advantageous family background.

Paper A conducts a quantitative analysis of the macro-level expectations. It assesses whether unfairness of the objective income distribution relates to countries' demand for redistribution. It first quantifies unfair inequality of labor incomes in 16 European countries for a total of 48 country-years using data from the Luxembourg Income Study (LIS). Consistent with equity theory, unfair inequality is defined as inequality between individuals with similar labor-related merits, such as education and occupation. The results show that overall and unfair labor income inequality are positively correlated, but far from perfectly. Overall inequality and unfair inequality are therefore related but distinct concepts.

Paper A moves on to match the unfair inequality measure to micro-level survey data from the first eight waves of the European Social Survey (ESS). Multilevel models controlling for individual characteristics show that countries with more unfair inequality support more redistribution, on average. The unfair inequality measure can explain both within- and between-country variation in redistribution preferences, and it is a better predictor of preferences than traditional measures of overall inequality. The results confirm the theoretical expectations. They draw a link between unfairness in the objective income distribution and the average level of redistribution support in a country. The results imply that unfair inequality—rather than overall inequality—drives policy preferences.

Main contributions

Papers A and D contribute to the literature by unveiling the objective foundations of the relationship between unfairness perceptions and redistribution preferences. While previous research

shows that unfairness perceptions matter, the papers suggest that unfairness perceptions are not idiosyncratic traits but rather grounded in reality.

Paper A also improves the understanding of the relevance of macro inequality for redistribution demand. It draws out that overall inequality and redistribution preferences do not have a consistent relationship because income fairness differs across countries. It is primarily unfair inequality—rather than overall inequality—that spurs redistribution demand. This is important because the consequences of inequality for policy preferences and politics are a pivotal topic in political economy scholarship, where Meltzer and Richard (1981) remains a central reference point (e.g., Alt and Iversen 2017; Finseraas 2009; Kevins et al. 2018; Luebker 2007; Lupu and Pontusson 2011; Moene and Wallerstein 2001; Schmidt-Catran 2016). Even though recent contributions tend to depart from the simplistic view of the Meltzer-Richard model, they continue to characterize overall inequality as an important determinant and often continue to rely on rationalist theories. This dissertation questions this practice because unfair inequality cannot be consistently derived from overall inequality.

GOVERNMENT QUALITY AS A SCOPE CONDITION (PAPER E)

I now move on to the moderating role of the quality of government. As before, I expect that redistribution preferences depend on objective resource distributions due to a mix of material self-interest and fairness concerns. This section introduces a scope condition of this expectation: the determinants of redistribution demand are more influential when the country an individual lives in has higher-quality government institutions.

Shortcomings of previous research

Previous research on the demand for redistribution and distributive politics tends to focus on rich countries in Northern America and Europe (which also applies to most empirical research of this dissertation). Even though this dissertation and related research support the theoretical model outlined in Figure 1.2, it remains unclear to what extent the results are applicable to lesser developed countries.

The empirical scope of the theoretical model is an intriguing question because most of the world's inhabitants live outside highly developed countries. As these countries catch up economically, it is an important question whether they will develop egalitarian tax-transfer-systems akin to, e.g., Western and Northern Europe. Research on redistribution preferences is informative for this question because, as outlined in Figure 1.2, the structure of public opinion influences whether a country implements redistributive policies, such as progressive income taxes.

Theoretical argument

Paper E argues that the theories behind Papers A-D, which state that financial circumstances and unfairness perceptions explain individual demand for redistribution, primarily apply to higher-developed countries. Both self-interest concerns and unfairness perceptions drive a desire to equalize the distribution of income. However, this desire will only translate into support for public redistribution if a country's government has sufficient quality to implement redistributive policies effectively and fairly.

Assuming that people have the instrumental motivation to influence the resource distribution in accordance with their distributive desires, there should be larger heterogeneity in redistribution support in higher-quality settings among those with different financial circumstances and unfairness perceptions. The reason is that, in higher-quality settings, people know that redistribution will actually be implemented, which leads to the determinants of redistribution

preferences having a stronger effect. In lower-quality settings, in contrast, disagreement will be muted; people abstain from asking for public redistribution even when they are driven to support income equalization because they know that the state will be unable to implement satisfactory tax and welfare policies.

Empirical evidence

Paper E analyzes comparative survey data from 39 institutionally diverse countries supplied by the International Social Survey Program (ISSP Research Group 2017). It evaluates whether the determinants of redistribution preferences (i.e., financial circumstances and unfairness perceptions) have heterogeneous effects across countries with different quality of government. The paper analyzes support for redistribution in general and support for public unemployment assistance in particular, and it assesses the effects of household income and perceptions of socio-tropic pay unfairness.

The results show that the effects of income and unfairness perceptions become more pronounced as the quality of government increases. While these determinants are mostly inconsequential in lower-quality settings, they drive significant preference heterogeneity in higher-quality settings. The analysis also shows that, as a result, countries with higher-quality institutions tend to disagree more about redistribution. Overall, the analysis supports the theoretical expectations.

Main contributions

The main contribution of Paper E is to show that government quality is a scope condition of common political economy theories on redistribution preferences (and therefore of theories on distributive politics and policymaking). The paper shows that—in contrast to typical expectations—the classical determinants of redistribution preferences are mostly inconsequential in lower-quality settings. This argument has found little consideration in previous research. While scholars tend to limit their analyses to highly developed countries based on the implicit expectation that their theories might not apply in vastly different settings, Paper E provides theoretical understanding of why this is the case for redistributive politics.

The findings have important implications for political phenomena that are downstream from policy preferences. Most importantly, the economic left-right divide structured around redistribution should be less relevant in weaker-quality settings. Since politics and policymaking depend on public opinion, as Figure 1.2 suggests, these countries should place less importance on disagreements about redistribution in voting behavior, party politics, and policymaking (see Kasara and Suryanarayan [2015, 2020] for empirical evidence on this expectation).

REDISTRIBUTION PREFERENCES AND VOTING BEHAVIOR (PAPER F)

While the previous sections were concerned with the determinants of redistribution preferences, I now move on to their political and economic consequences, beginning with voting behavior. My motivation is that one of the dissertation's papers deals with the implications of policy preferences for voting behavior. Moreover, showing that redistribution preferences are influential for election outcomes underscores the relevance of the dissertation's main research topic.

Theoretical background

I expect that redistribution preferences influence voting behavior. Assuming that voters have instrumental motivations and are driven by the issue redistribution in their voting behavior, it can be expected that they vote for parties that further their (re-)distributive policy goals. The

typical expectation is that stronger demand for redistribution leads to voting for left parties because these parties run the most egalitarian programs.

Instrumental and issue-oriented voting behavior can be either prospective or retrospective (Duch and Stevenson 2008; Rueda and Stegmueller 2019: ch. 8). The prospective voting argument holds that people form expectations about the redistributive policies that will be implemented by parties on the ballot paper, and that they vote for the party with the highest congruence between their preferences and expected policy outputs (Downs 1957). In more technical terms, the probability of voting for a specific party increases as the distance between a voter's policy preferences and the party's expected policy performance decreases (Duch and Stevenson 2008).

Another type of voting behavior is retrospective voting. The expectation is that people assess the past policy performance of governing parties and then decide whether to keep them in government or whether to vote them out (Duch and Stevenson 2008). Applied to the topic of this dissertation, this means that voters should form their decision based on whether they are content with the redistributive policy performance of government parties (Tilley et al. 2018). Retrospective voting is forward-looking in the sense that people seek to further their redistributive goals in the future, but expectations about the future are solely based on the past policy performance of parties that governed.

Empirical evidence so far

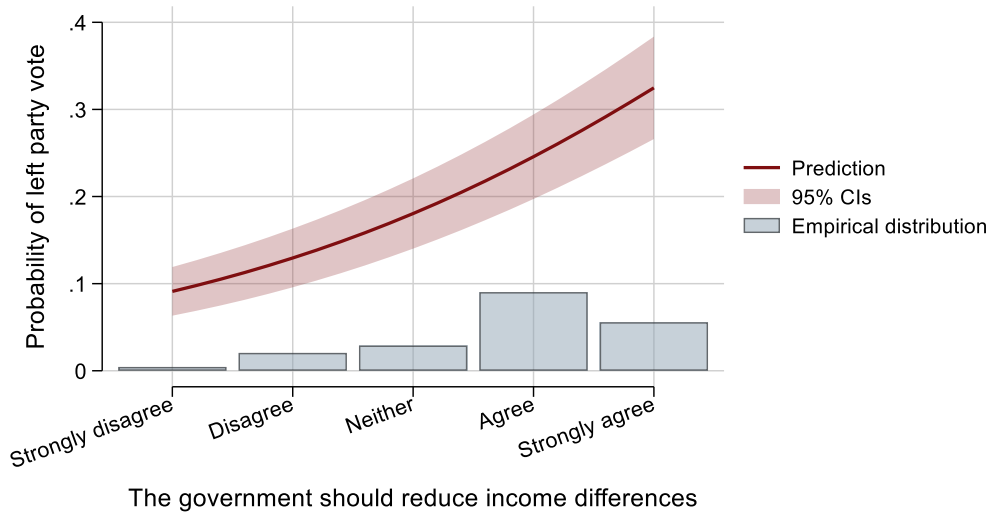
Studies that expect prospective voting mainly trace voting behavior back to objective circumstances and ideological stances while merely assuming that redistribution preferences and issue-oriented voting are responsible for the relationships. Empirical research covers objective circumstances such as income (Emmenegger et al. 2015), class (Evans and Tilley 2012; Langsæther et al. 2019; Langsæther et al. 2021), and labor market conditions (Emmenegger et al. 2015; Marx 2014; Marx and Picot 2013), generally showing that economically disadvantaged voters turn to left parties. Further research shows that egalitarian distributive justice perceptions predict left party voting (Attewell 2021).

Several studies conduct a more direct test of the argument and assess whether redistribution and welfare preferences influence voting behavior. Comparative multi-country studies and studies focused on the USA generally find that the probability of voting for a left party is much higher among those who favor redistribution (Lewis-Beck and Nadeau 2011; Quinlan and Okolikj 2020; Rueda and Stegmueller 2018a, 2018b, 2019). Furthermore, Gingrich (2014) shows that people are more likely to vote for parties that are ideologically closer to themselves regarding the issue welfare (especially in countries where the welfare state is visible).

Figure 1.3 underscores the importance of redistribution preferences with regression evidence based on data from the European Social Survey (ESS). It shows that those with stronger support of redistribution are much more likely to vote for parties with a left ideology, controlling for a wide range of socio-demographic background characteristics. The evidence therefore supports the expectation that redistribution preferences structure voting behavior, although evidence with more credible effect identification is still lacking.

Further research also hints at the possibility of retrospective voting. While retrospective voting in general finds much empirical support (Duch and Stevenson 2008; Lewis-Beck and Stegmaier 2013), political factors such as policies seem to play a limited role (Lewis-Beck and Stegmaier 2013: 376–78). The particular expectation that those who are more content with the redistributive policy performance of governing parties are more likely to keep them in office finds only mixed support.

FIGURE 1.3: Redistribution preferences and left party voting in Europe, 2018-19



Note: The figure depicts predicted probabilities of left party vote choices obtained from a weighted logit regression based on data from 24 countries of the ninth wave of the ESS. Left parties include mainstream and radical left parties following the classification in Armingeon et al. (2021). The controls are: gender, age, education, household income, employment status, and class (eight-category Oesch scheme plus an additional category for those with missing values). The 95% confidence intervals are obtained from robust standard errors clustered by countries. The results are weighted using the post-stratification and country size weights of the ESS.

Research analyzing the electoral consequences of welfare state changes partly confirms the prevalence of retrospective voting, but the evidence is inconsistent. Studies generally find no electoral consequences across all parties and governments (Armingeon and Giger 2008; Giger and Nelson 2013; Horn 2021; Schumacher et al. 2013). These studies rather suggest that electoral consequences can arise under specific circumstances, above all a left government party (Horn 2021; Schumacher et al. 2013). However, this finding is contradicted in other research (Giger and Nelson 2013). Furthermore, Tilley et al. (2018) show that UK citizens punish the incumbent when their economic situation worsened as a result of reduced public benefits.

Research on the electoral consequences of tax policies is sparse but tends to find retrospective voting. Tillman and Park (2009) find in a cross-country study that especially right-leaning parties are punished for increasing income taxes. Foucault et al. (2017) find that left parties and parties operating under high clarity of responsibility are punished for increasing tax revenues. Several country studies, mostly focusing on the US, have similar results (Kone and Winters 1993; Niemi and Jennings 1991). However, studies from other countries find null effects (Finseraas 2012), and the evidence on austerity is rather weak, suggesting that fiscal rebalancing via taxation may have no electoral consequences (Alesina et al. 1998; Alesina et al. 2013; Alesina et al. 2019; Jacques and Haffert 2021). Overall, the evidence for electoral consequences is mixed.

Shortcomings of previous research

The expectation that tax and welfare policies have electoral consequences (i.e., the retrospective voting argument) is endemic in political economy and welfare state research, but empirical evidence on this expectation is mixed (see the review above). Approaches such as the insider-outsider theory (Rueda 2005, 2006, 2014), the constrained partisanship model (Beramendi et al. 2015), and the literature on the consequences of austerity (Hübscher et al. 2021b; Jacques and Haffert 2021) crucially rely on the assumption that government parties will be punished (rewarded) if they implement policies that oppose (are in line with) the preferences of their voters,

whereas voter preferences are most often assumed to depend primarily on material self-interest. The implicit or explicit importance that previous research places on the expectation of retrospective voting is at odds with the mixed evidence.

Theoretical argument

Paper F takes a “sober look” at the theory and evidence underpinning retrospective voting theory, focusing specifically on the electoral consequences of changes to the welfare state. Its theoretical discussion integrates previous theories into a unified theoretical framework using a stylized model. The model expects that government parties implement welfare changes based on their policy preferences, which voters compare against their own policy preferences to arrive at an assessment of welfare state changes. If this assessment is positive (negative), voters will reward (punish) the incumbents. The model implies that electoral consequences ultimately arise because voters and parties have different policy preferences regarding welfare and connected issues such as fiscal spending. The typical assumption is that voters support the welfare state even under adverse fiscal and economic conditions, which implies that voters should punish welfare retrenchment and reward welfare expansion (Pierson 1994, 1996, 1998). As aforementioned, however, this simple theory does not find empirical support.

Paper F goes on to discuss refinements to this simple theory. The literature firstly argues that voters have distinctive policy preferences. Especially the voters of left parties place great emphasis on the issue welfare, which is why primarily left parties should experience electoral consequences (Horn 2021; Schumacher et al. 2013). Furthermore, the literature theorized on voters’ limited ability to observe and assess welfare changes (Hobolt et al. 2013; Jensen and Wenzelburger 2021a; Wenzelburger et al. 2020). Electoral consequences should primarily arise when voters are able to observe and remember welfare changes, such as under high clarity of responsibility or when welfare changes are implemented right before an election.

Lastly, the paper also draws from a wider political science literature to argue that there may be no electoral consequences even under favorable conditions. Firstly, welfare is not as popular as commonly assumed, and voters do consider the tradeoffs of welfare in their vote choice, such as cost (Busemeyer and Garritzmann 2017; Giger 2012). Therefore, even the voters of left parties may not generally punish (reward) welfare retrenchment (expansion). Secondly, parties also have a sway in determining the preferences of their voters, for example via strategic framing (Cavaillé and Neundorf 2022; Slothuus and Bisgaard 2021). This limits the potential for mismatched preferences between parties and voters. Thirdly, the rationality of voters may be too limited to conduct systematic retrospective voting, for example due to their short memory. And lastly, considerations other than the issue welfare may be paramount in determining the vote, such as candidate orientations.

Empirical evidence

Paper F conducts the most comprehensive empirical evaluation to date of the expectation that changes to the welfare state have electoral consequences. It builds a dataset of 484 government parties from 20 European countries between 1970 and 2019. The dependent variable is vote change between two subsequent elections and the independent variables are different measures of welfare state changes between these two elections. For example, one observation in the dataset relates to the 2005-2009 incumbency of the German SPD and the development of their vote shares and the welfare state between 2005 and 2009. The analysis relies on a range of welfare state change measures: (1) changes in social expenditure as a proportion of GDP, (2) changes in entitlement generosity of unemployment benefits and pensions, and (3) legislative reforms to unemployment benefits and pensions.

The results suggest that welfare changes do not have electoral consequences. The vote shares of parties on average remain stable following welfare changes measured by any of the three change indicators. This result also holds under a range of favorable circumstances that are drawn from the theoretical discussion: left government party ideology, favorable fiscal circumstances, high clarity of responsibility, welfare changes implemented shortly before an election, and changes to salient program characteristics, such as program replacement rates. The results do not imply that governments can do what they want to the welfare state, but—under business as usual—welfare changes do not seem to have electoral consequences.

Main contributions

Paper F shows that the mechanistic worldview that underpins influential political economy theories is potentially flawed from both a theoretical and empirical standpoint. Many theoretical approaches crucially rely on the assumption that the redistributive policy performance of governments is of central electoral importance. Paper F questions this assumption, at least regarding redistribution via the welfare state. Besides its novel empirical evidence, the paper advances the theoretical understanding of why there is such a weak relationship between redistributive policy performance and election results. Among others, voters may have insufficient ability to observe and assess past policy performance (see the theoretical discussion above).

REDISTRIBUTION PREFERENCES AND POLICIES

This section outlines the implications of redistribution preferences for policymaking. It is important to note that none of the dissertation’s papers conducts a direct assessment of this relationship. The purpose of this section is to demonstrate that redistribution preferences are politically influential, which underscores the relevance of the research conducted in the other papers.

Theory

Redistribution preferences influence actual policies because, firstly, the structure of redistribution support among the electorate influences what parties end up governing the country. Parties that seek to implement a pro-redistribution agenda are more likely to be elected into government when the public favors more redistribution, implying a positive link between redistribution support and actual redistribution. Secondly, public opinion influences partisan policymakers because politicians seek re-election and may have an inherent wish to represent the public (Adams et al. 2004; Hager and Hilbig 2020; Romeijn 2020; Stimson et al. 1995). These theoretical arguments foot on the following assumptions: (a) people base their voting behavior on the issue redistribution; (b) politicians honor their policy proposals once they are in office; (c) politicians are vote- or office-seekers, or they may desire high-quality representation; and (d) politicians are able to monitor public opinion on redistribution.

There is ample disagreement about redistribution across parties (Volkens et al. 2020), but it remains an open question to what extent partisanship really matters for redistributive policy outputs and outcomes. The most important debate in political economy concerns the structural constraints imposed by globalization. The globalization of production, trade, and finance makes capital internationally mobile while the tax and welfare state remain bound to the nation (Rodrik 2012). The implication is that governments are limited in their ability to impose progressive taxes (Genschel and Schwarz 2011), and the resulting budget constraint at least complicates the maintenance and expansion of a generous welfare state (Genschel 2002, 2004). However, the

extent to which globalization trumps national demands for economic well-being and security remains contested (Basinger and Hallerberg 2004; Plümpner et al. 2009; Rodrik 1998).

Another reason for a possibly limited significance of partisanship for redistributive policies is that the economic left-right conflict may be displaced by new conflict dimensions such as universalism vs. particularism (Bornschiefer et al. 2021; Lux et al. 2022; Mau et al. 2020). Redistribution may not guide voting behavior as much as it did in the past, for example because other issues such as abortion rights or same-sex marriage are given more weight. Therefore, parties may have less of an incentive to implement their redistributive policy goals once in office.

Empirical evidence

I first discuss partisan effects on taxation before turning to welfare. Studies do not generally support the simple view that left parties implement more progressive taxes. Rather, partisanship structures the level of overall taxation as well as the tax mix. A stylized finding is that more left-leaning governments raise more tax revenue and rely on indirect and thus regressive taxes to do so (Haffert 2021; Kemmerling and Truchlewski 2021), especially in corporatist countries (Beramendi and Rueda 2007). Further evidence suggests that the left imposes higher taxes on labor to increase tax revenue (Cusack and Beramendi 2006; Haffert 2021; Kemmerling and Truchlewski 2021), but other studies also suggest that they rather prefer taxing capital (Angelopoulos et al. 2012; Osterloh and Debus 2012).

The finding that left parties rely on taxing labor and consumption can be explained by the constraints of globalized capitalism. Most importantly, research suggests that tax competition indeed reduces the ability of governments to impose taxes on capital (Basinger and Hallerberg 2004; Devereux et al. 2008; Egger et al. 2019; Plümpner et al. 2009; Swank 2006, 2016), although the doomsday scenarios discussed in the 1990s did not transpire. Left parties must therefore accept more regressive forms of taxation to continue funding a strong and redistributive welfare state (Genschel 2002, 2004).

Turning to welfare, there is a large literature in political science that analyzes whether left- and right-leaning governments implement different welfare policies. Quantitative evidence suggests that partisanship does make a difference (i.e., left parties implement stronger welfare), but the difference has been waning over time (see Bandau and Ahrens 2020 for a systematic overview). However, recent studies relying on improved methodology find that partisanship still makes a difference (Garritzmann and Seng 2020; Schmitt 2016). Furthermore, historical evidence suggests that left parties were crucial drivers of redistributive welfare policies and institutions (Esping-Andersen 1990; Iversen and Soskice 2006; Manow 2008).

Research on inequality suggests that income and wealth differences in highly developed countries are formed by partisanship. Inequality is generally lower (higher) after the incumbency of left (right) parties (Brady and Leicht 2008; Huber et al. 2019; Huber and Stephens 2014; Neal 2013; Pontusson et al. 2002). This is especially the case after long periods of time as this allows partisans to entrench redistributive institutions, which then become “immovable objects” (Pierson 1994, 1996, 1998).

Further research on government responsiveness and representation finds that there is a congruence between the economic preferences of voters and policymaking (Elkjær 2020; Elkjær and Iversen 2020; Elsässer et al. 2021; Jensen and Vestergaard 2022; Schakel et al. 2020). This supports the view that redistribution preferences are influential. However, the research program also shows that policymaking better reflects the preferences of the rich and educated. Put differently, the relationship between redistribution preferences and actual redistribution is stronger for better-situated citizens. Overall, while the literature shows that preferences matter, it also points towards a problematic lack of democratic equality.

In sum, the evidence does suggest that parties make a difference, but in more subtle ways than expected by simple theoretical approaches. It is clear that the varying redistribution preferences of people make their way into the policy agendas of parties (Volkens et al. 2020). Once in government, however, parties are constrained by the realities of global capitalism. As governments find ways to limit the constraints of globalization, preferences may drive more tractable variation in redistributive outputs again (Ahrens et al. 2022b; Ahrens et al. 2022a; Ahrens and Bothner 2020).

CONCLUSION

This section offers a brief summary before closing with my final thoughts about the politics of redistribution. This dissertation has four central messages for political economy research and beyond. Firstly, self-interest matters, but human rationality is limited. People in more advantageous financial circumstances demand less redistribution, which is in line with the dominant approach in political economy. However, the relationship is less consistent than assumed, and concerns regarding the present carry more weight than future-related concerns, which I ascribe to bounded rationality. Future theories should therefore critically assess whether the rationality assumptions underpinning their expectations are reasonable, which is also relevant for theories that ascribe a central role to policy preferences in their causal chain.

Secondly, unfairness perceptions and their consequences for redistribution demand are grounded in reality. The dissertation demonstrated that unfairness perceptions stem from inequality between workers with the same labor-related merits. This underscores the relevance of unfairness-based arguments for political economy research, which takes a special interest in the material foundations of political phenomena. Therefore, research on the objective unfairness of distributions offers a promising avenue for future research, for example by analyzing the political implications of inequality of opportunity.

Thirdly, dominant political economy approaches to redistributive politics and policymaking are primarily applicable to highly developed countries, where redistribution can be implemented effectively and fairly. In contrast, preferences and resulting political phenomena should be less divided along the quintessential economic left-right divide centered around redistribution in less developed countries. This is relevant for future theory-building, especially because the analytical lens increasingly moves away from the usual suspects of rich OECD countries.

Fourthly, individuals do not seem to retrospectively punish (reward) government parties for implementing welfare policies which go against (are consistent with) their policy preferences. Various political science theories either implicitly or explicitly assume that voters hold mostly exogenous policy preferences and will punish incumbent parties when their policy performance diverges from these preferences. This assumption is not supported empirically in regard to the welfare state (while the electoral consequences of taxation warrant further inquiry). In any case, future theories should consider that retrospective economic voting may be limited, for example in theories on policy dilemmas for governing parties.

I conclude the dissertation with more general thoughts about the politics of redistribution. I already outlined that my framework sheds light on the lack of a public response to inequalities in income, wealth, and economic insecurity. Here, I discuss how it may be possible to curtail inequality democratically. This discussion focuses on left parties as they are more likely to advocate and implement redistribution. This requires that I engage with arguments on political supply. I have merely assumed so far that political actors such as parties politicize the issue redistribution. Relaxing this simplifying assumption implies that people's latent support of redistribution is no longer sufficient to ensure the implementation of redistributive policies; the

issue must be politicized by skillful political entrepreneurs, which is not guaranteed (Beramendi et al. 2015).

First of all, left parties need to present future-oriented policy demands. This argument is based on the finding that prospective issue voting trumps retrospective issue voting regarding redistributive policies. Parties apparently attract votes via *promising to enact certain policies* rather than by a *history of good policy performance*. Such policy promises are optimally woven into simple yet powerful narratives comprising gripping frames. This has proven to be an effective method for mobilizing and expanding voter bases around certain issues (Emmenegger and Marx 2019).

Furthermore, this dissertation suggests that redistribution demand is best stimulated by depicting both existing and increasing inequalities as unfair because—as we have seen—unfairness perceptions spur demand for a redistributive response (Scheve and Stasavage 2016). Effective framing is required for this endeavor. Whether inequality is unfair is not set in stone but depends on interpretation.

A more fine-grained point is that people could be made aware of their material self-interest. Self-interest stimulates redistribution demand, but if people remain unaware of possible material benefits, they cannot act accordingly. For them to do so, they require basic knowledge about the makeup of inequality and the benefits they would gain from redistribution. However, this information should be communicated in a non-technical way that people can understand. Left parties may use simple narratives to nudge people to follow their material self-interest. For example, they may frame proposed reforms in terms of “taxing CEOs” to “fund public pensions” or “deteriorating infrastructure”, illustrating that not even the middle classes will be hit by a reform package but rather benefit from it.

What concrete policies could left-leaning parties promise and what narratives could they offer? Following left-leaning economists of the hour (Chancel et al. 2022; Piketty 2014, 2020; Saez and Zucman 2019), I argue that left parties should focus on the taxation of wealth and high incomes. They should advocate for progressive taxation while pursuing both strategies laid out above, namely triggering unfairness perceptions and inducing self-interest guided behavior. Taxing high incomes and wealth can be depicted as a reduction of unfair inequality because top-end inequality has ballooned in many countries. Top incomes and large stashes of wealth can be framed as disproportional to individual merits and inconsistent with ideals such as equality of opportunity. Furthermore, left parties can clarify that tax reforms would only hit an exclusive minority. An especially promising option is to raise taxes on inheritances. The tax burden falls on people who did not earn the assets and the tax can be levied only on the rich. Besides inheritances, exorbitant wealth and incomes could also be framed as illegitimate while gently reminding people of their material self-interest and persuading them to back increases in top marginal income or wealth taxes rates.

Another important point is that the left should make more use of crises, following Churchill’s mantra of never putting a good crisis to waste. As stressed by historical institutionalism, crises open critical junctures that, for a short time, create opportunities for fundamental change (Acemoglu and Robinson 2012; Capoccia 2010). The left could have exploited several political shocks in recent history. The financial crisis, the Euro crisis, and Covid-19 were all unexpected shocks that increased inequality while ballooning public debt. Left parties could have actively demanded redistribution while depicting crisis-induced inequalities as unfair and inducing people to follow their self-interest (Scheve and Stasavage 2016). For example, inequalities caused by the financial crisis could have been depicted as unfairly created by the rich and these wealthy people held to carry the financial burden via progressive taxation. While evidence shows that exactly such unfairness frames were indeed effective in raising tax progressivity in the aftermath

of the financial crisis (Limberg 2019, 2020), the left failed to produce fundamental changes. For example, the German social democrats were content with technocratically managing the crisis without fundamentally politicizing it. There was a distinct possibility of the left mobilizing democratic majorities for far-reaching reforms, such as reinstating wealth taxes. The critical junctures are closed now, but—judging by recent history—the next crisis is right around the corner.

The left must expect counter-narratives from their political adversaries if they are to demand more redistribution. Right-leaning parties typically claim redistribution to be unfair and warn about its negative effects on economic prosperity (Emmenegger and Marx 2019; Hilmar and Sachweh 2022). They characterize the left as irresponsible, envious spendthrifts that endanger equitable growth. Left parties have internalized this reaction and tend to shy away prematurely from politicizing fundamental redistributive demands (Fastenrath et al. 2022). Especially social democrats embarked on a strategy of imitating their political adversaries to project economic competence, an approach that is best exemplified by Germany’s chancellor Olaf Scholz. If left parties wish to pursue fundamental redistributive reforms, they must have a strategy to meet their adversaries head-on in the political arena.

To round off the discussion, I devote my closing remarks to a brief discussion on how the framework of this dissertation can also be used to advance the goals of liberal and conservative parties, which often wish to preserve existing inequalities or even aim to implement regressive reforms. To achieve this goal, they may obscure the actual level of inequality and the distributive effects of reforms. An effective strategy is to falsely imply that progressive tax reforms would hit a majority of tax payers rather than a rich minority or to suggest that regressive reforms would benefit the poor (Emmenegger and Marx 2019). Furthermore, liberal and conservative parties may depict inequality as fair and redistribution as unfair, for example by emphasizing the already large tax contributions of the rich. Another strategy championed by the US Republicans is to characterize the state as incompetent because, as discussed, citizens need to consider their government effective if their aversion to inequality is to be translated into a demand for redistribution. These discursive strategies have been successful in the past, and they will continue to be successful in the future.

Of course, parties are in no way required to rely on such political strategies. This notwithstanding, the closing discussion demonstrates that the arguments of this dissertation can be exploited to help understand the politics of redistribution, in whatever way they may transpire in the future.

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Unfair inequality and the demand for redistribution (Paper A)

Abstract

Political economy research commonly expects a positive relationship between income inequality and the demand for redistribution, which is increasingly attributed to inequality aversion grounded in norms and values. However, people are not averse to a proportion of inequality that fairly results from differences in individual merit. Therefore, this study argues that the effect of inequality crucially depends on the extent to which income fairness is realized. It is primarily unfair inequality, rather than overall inequality, that affects individual redistribution support. The argument is substantiated with an empirical quantification of unfair inequality that measures whether individuals have unequal returns to their labor-related merits. Multilevel models using repeated cross-sections show that this quantification of unfair inequality can explain both within- and between-country variance in redistribution preferences and that it is a better predictor than overall inequality. The results suggest that public opinion cannot be inferred directly from the overall level of inequality.

INTRODUCTION

What is the relationship between income inequality and popular demand for redistribution? The ubiquitous model of Meltzer and Richard (1981) and related rational choice approaches expect a positive relationship because inequality increases the material incentive for the masses to “soak the rich”. However, empirical results only offer inconsistent support for this expectation (Dallinger 2008; Jæger 2013; Johnston and Newman 2015; Kenworthy and McCall 2008; Luebker 2007), which has led to widespread dissatisfaction with the view that individual rationality is sufficient to explain the political implications of inequality (Dimick et al. 2017). At the same time, aggregate inequality does seem to play a role, with a number of studies finding positive correlations with redistribution preferences (Dallinger 2008; Jæger 2013; Johnston and Newman 2015). In an answer to this puzzle, comparative political economy has rediscovered other-regarding preferences in its theorization. Several contributions argue that inequality does affect the demand for redistribution but only insofar as inequality triggers inequality aversion, which depends on the normative stance of citizens and the specific makeup of inequality (Cavallé and Trump 2015; Dimick et al. 2017; Luebker 2007; Lupu and Pontusson 2011; Shayo 2009).

This study contributes to this literature by developing a theory about the circumstances under which inequality affects redistribution preferences and by testing its implications with a novel empirical approach. The argument draws from another literature that focuses on subjective beliefs about inequality. It is a robust finding that individuals actively support a

considerable proportion of inequality that can be explained by differences in individual merit (Cappelen et al. 2010; Kuhn 2011; Lewin-Epstein et al. 2003; Mijs, 2018b). The underlying normative ideal has been referred to as *meritocracy*, *equity*, or *economic fairness* (e.g., Aalberg 2003: ch. 2; Deutsch 1975; Konow 1996). It demands that incomes should be fair, i.e., proportional to individual merit.¹ What is relevant for research on redistribution preferences is that individuals support inequality reduction via redistribution especially when they perceive inequality to be unfair while inequality perceived as fair is less consequential (e.g., Ahrens 2019; Alesina and La Ferrara 2005; García-Sánchez et al. 2020).

In line with recent political economy research, I argue that objective inequality affects redistribution support due to normative concerns about inequality. However, it is paramount to take the implications of income fairness research seriously. It is unreasonable to believe that fairness perceptions are separate from objective reality. Some national income distributions should be fairer than others according to an objective standard. Furthermore, individuals should primarily seek to reduce *unfair inequality* via redistribution (i.e., the proportion of inequality that is not warranted by labor merit). *Fair inequality*, on the other hand, should be less consequential because people are less inclined to distort a legitimate income distribution. This is an important point for research on objective inequality because commonly used inequality measures convey no information about the fairness of distributions, and there is no reason to believe that distributive fairness is the same across different populations.

The theoretical expectations are tested empirically with a novel research design. It relies on an empirical quantification of unfair inequality of labor income, applying the measurement approach of Almås et al. (2011). The quantification is guided by a fundamental income fairness principle, namely that people with comparable occupations, skills, and effort should receive similar labor income, i.e., non-discrimination. It results in an *unfairness Gini*, which is a variant of the Gini index that solely evaluates whether people receive unequal rewards for their labor-related merits. In contrast to the conventional Gini index commonly used in research, it explicitly considers distributive fairness. The unfairness Gini should therefore capture a form of inequality that spurs redistribution preferences to a better degree than the conventional Gini index.

Unfair inequality as well as different versions of the conventional Gini index measuring overall inequality are estimated for 48 country-years using data from the Luxembourg Income Study (LIS 2020). In a next step, the effect of these inequality variables on individual demand for redistribution is assessed with multilevel models using repeated cross-sections from the European Social Survey (ESS 2002-2014). The theoretical expectations are supported. The results show that unfair inequality has a positive relationship with redistribution preferences and that it can explain both within- and between-variance of national redistribution preferences. Furthermore, unfair inequality is a much better predictor of redistribution preferences than overall inequality, which supports the argument that unfair rather than overall inequality affects redistribution support.

This study makes several contributions to the literature. In line with recent political economy scholarship, it firstly underscores that other-regarding preferences drive the relationship between aggregate inequality and redistribution preferences. Secondly, this study reconciles findings from research on objective and subjective income inequality. It takes a novel approach by showing that objectively realized income unfairness is associated with more redistribution support, whereas previous observational work used subjective unfairness evaluations as explanatory variables. Likewise, this study shows that objectively realized inequality matters, which has

¹ Please note that fairness ideals are usually more extensive than this short summary may imply. Most importantly, they typically demand equality of opportunity in addition to simple outcome fairness (i.e., proportional merits and rewards). However, this study focuses on outcome fairness.

increasingly been questioned by studies focusing on subjective beliefs (see the review by Janmaat 2013). Lastly, this study also has an implication for research exploring the socio-political consequences of objective inequality, namely that *not all inequality is equal*. The quantification of unfair inequality and its robust relationship to redistribution preferences suggest that the proportion of inequality that is normatively rejected is not given by a fixed proportion of overall inequality. If possible, it is therefore advisable to consider differences in distributive fairness when exploring the effects of inequality on political preferences and other dependent variables, at least when fairness concerns are influential.

INEQUALITY AND THE DEMAND FOR REDISTRIBUTION

What is the relationship between objective inequality and the demand for redistribution? Meltzer and Richard's (1981) rational choice approach posits that individual redistribution support negatively depends on the own income relative to the mean income. Under typical lognormal income distributions, this translates into a positive macro association between income inequality and redistribution preferences. In line with the model, a myriad of studies show that relatively rich people support less redistribution than relatively poor people, thus underscoring the importance of rational motivations (e.g., Rehm 2009; Schmidt-Catran 2016). The model's more fine-grained predictions, however, receive little support. Only some studies find a positive association between aggregate inequality and redistribution preferences (Dallinger 2008; Jæger 2013; Johnston and Newman 2015) while others report null findings (Dallinger 2010; Kenworthy and McCall 2008; Luebker 2007; Roller 1998). Furthermore, the discrepancy in redistribution support between the poor and rich should increase with inequality, but empirical estimates support the opposite pattern, i.e., *less* variance between the rich and poor (Dimick et al. 2017; Finseraas 2009; Schmidt-Catran 2016).

In an answer to the shortcomings of rationalist predictions, political economists have rediscovered the role of norms and values. Several contributions continue to theorize a positive impact of objective inequality on redistribution support but expect that inequality aversion drives the relationship. In Dimick et al.'s (2017) model, inequality increases redistribution support because people care about the utility of their peers. This other-regarding motivation is allegedly stronger in richer individuals because they assign more utility to social welfare. An empirical analysis confirms this conditional relationship with US data. Luebker (2007) finds a positive cross-country effect of inequality, but only once the differing normative stances prevalent in countries are controlled for. Lupu and Pontusson (2011) argue that rather the *structure* of inequality matters. Specifically, middle-income voters will increasingly emphasize with the poor and support redistributive policies when the income distance between middle- and lower-income earners decreases relative to the distance between upper- and middle-income earners. An empirical analysis confirms this relationship, but it has seen a comprehensive rebuttal in Luebker (2021), who shows that the results were driven by omitted variable bias. Conversely, Cavaillé and Trump (2015) show that inequality *decreased* redistribution support in Great Britain, which they attribute to reduced social affinity with the poor. In the same vein, Shayo's (2009) model implies that the role of inequality is ambiguous; it can lead to both increased and decreased redistribution support, which depends on whether poor people identify with their nation or fellow members of the lower class.

The approach to study the effect of objective inequality on political preferences, especially when normative motivations are theorized, has been critiqued by a literature that rather focuses on subjective beliefs regarding inequality (see Janmaat [2013] for an overview). It questions whether objective inequality and inequality aversion have a consistent relationship (Luebker

2007), not least because individuals tend to be misinformed about inequality (Engelhardt and Wagener 2018; Fernández-Albertos and Kuo 2018; Gimpelson and Treisman 2018).

One of the most robust findings from studies directly tapping into subjective judgements regarding income inequality is that inequality and inequality aversion are not necessarily related. While people do hold egalitarian views (Dawes et al. 2007; Sachweh 2012), it is widely accepted among diverse populations that those with higher individual merit, e.g. due to working harder, receive a higher income. To the extent that individuals believe that income differences in their country are warranted by differences in merits rather than circumstances, inequality is considered to be perfectly legitimate (Janmaat 2013; Lewin-Epstein et al. 2003; Mijts 2018b, 2021; Sachweh 2012). The philosophical foundation is a distributive ideal that has been coined *meritocracy*, *equity*, or *economic fairness* by different theorists, which typically includes demands for a proportionality of individual merit and reward as well as equality of opportunity (Aalberg 2003: ch. 2; Deutsch 1975; Konow 1996).

Further research shows that the endorsement of income fairness beliefs, which vary within and between countries, is crucial for individuals' redistribution support. García-Sánchez et al. (2020) find that the effect of (perceived) income differences on redistribution support decreases with the endorsement of income fairness beliefs. People who think that actual and ethical wages diverge (Ahrens 2019; Kuhn 2010) and those who think that others do not get what they deserve (Benabou and Tirole 2006) also demand more redistribution. Other studies show that not only outcomes but also processes matter. Those who think that income inequality results from unfair processes, e.g. because only those with a wealthy family can get ahead, demand more redistribution (Ahrens 2019; Alesina and La Ferrara 2005; Fong 2001). Furthermore, experiments show that the association between fairness perceptions and redistribution support is causal. Piff et al. (2020) show that people's preference for egalitarian policies increases when they are primed to attribute poverty to situational forces. Lastly, Becker (2020) shows that Americans adjust their redistribution preferences when they are informed about objective inequalities between people with different characteristics ascribed at birth (e.g., gender), which may serve as indicators for economic fairness.

To sum up, researchers in political economy continue to expect (and find) a relationship between objective inequality and redistribution support. This relationship is increasingly attributed to normative considerations. However, research on subjective income inequality questions this practice since (a) objective inequality and inequality aversion are not necessarily linked and (b) because people are generally misinformed about objective inequality. The following section will outline a theory that reconciles these theoretical approaches and critiques.

THEORY AND HYPOTHESES

This section will advance the theory that, due to citizens' normative concerns, objective inequality affects redistribution preferences. However, it is paramount to take the findings from research on subjective income inequality into account, which shows that the effect of inequality depends on whether inequality is seen as fair or not. Furthermore, fairness perceptions vary considerably between countries, with some countries endorsing much stronger income fairness beliefs than others.² While it would be simple to treat these perceptions as separate from reality, I rather expect that individuals in some countries experience more income unfairness than individuals in other countries.

² Descriptive statistics on the between-country dispersion of unfairness perceptions are available in Appendix A (see Figure A1.1).

I argue that the effect of inequality on redistribution support depends on the extent to which income fairness is empirically realized. Redistribution support increases with unfair inequality, which cannot be explained by differences in individual merit. Fair inequality that results from individual merit, on the other hand, should be less consequential. Public redistribution is a tool that can be used to equalize the income distribution. People will primarily support the use of this tool when they observe that income differences are not deserved because, as Fong (2001: 226) notes, “individuals care deeply that other people get what they deserve”.

The implication is that the relationship between objective inequality and redistribution preferences cannot be inferred from the overall level of inequality alone. Previous research has disregarded this point by using different measures of overall inequality as explanatory variables. For example, the widely used Gini coefficient measures an income distribution’s deviation from perfect equality, which does not conform to how popular perceptions of a legitimate income distribution are formed at all. The next section will thus outline an approach to solely measure unfair inequality. Beforehand, however, several theoretical refinements are appropriate.

The argument so far begs the question of what exactly unfair inequality is. I argue that unfair inequality is inequality that cannot be explained by differences in labor-related merits, i.e., attributes related to occupation, experience, skills, and effort, which I will refer to as occupational attributes. Research reliably shows that occupational attributes are paramount in defining individual deservingness. Cappelen et al. (2010) show in an experiment that labor effort and skill legitimize income inequality. Differences in remuneration resulting from differences in productivity (e.g., being able to type more words) are accepted while randomized differences are not. Lewin-Epstein et al. (2003) find that differences in individuals’ education, skills, and effort on the job warrant unequal reward. Two studies show that individuals in diverse settings support *substantial* income differences between different professions (Kuhn 2011; Osberg and Smeeding 2006). Lastly, individuals expect to earn as much their colleagues (Feldman and Turnley 2004) and employees in the same industry (Verhoogen et al. 2007), which supports the view that occupational attributes define deservingness. The implication is that income differences between people with the same occupational attributes are unfair.

The next question is how individuals form income fairness perceptions. Relying on equity theory (Ahrens 2019), I argue that people conduct comparisons with and between observable reference groups (c.f. Cruces et al. 2013; Dawtry et al. 2015; Mijs, 2018a). Distributive fairness is judged by comparing people with similar occupational attributes such as education and profession and inferring whether the rewarded income is similar (Sauer and May 2017). Income inequality is deemed to be fair when there is a proportionality of inputs (i.e., occupational attributes) and outputs (i.e., income). For example, people will compare themselves to colleagues who work at the same employer and others in the same profession to gauge whether their own income is appropriate. Of course, relevant occupational attributes and incomes are difficult to observe beyond one’s immediate social surrounding. Therefore, I expect that the estimated fairness of the own income, where proportionality is most easily assessed, is especially relevant for the formation of overall income fairness perceptions. Insofar as it is possible, however, people also use social comparisons between others to gauge whether the income distribution is fair.

My approach relies on the assumption that people can form a relatively valid estimate of unfair inequality in their society. This is debatable because recent research shows that individuals tend to be misinformed about inequality (Engelhardt and Wagener 2018; Fernández-Albertos and Kuo 2018; Gimpelson and Treisman 2018), presumably because they base their beliefs only on observable subsets of the income distribution (Cruces et al. 2013; Dawtry et al. 2015). I argue that, in the aggregate, individuals assess unfair inequality with less bias than overall inequality. Comprehensive knowledge about all other incomes in society would be required to

arrive at an unbiased estimate of both unfair and overall inequality. But since people tend to observe only local subsets of the income distribution, which tend to be much more homogenous than the overall distribution, individual estimates of both fair and overall inequality will be biased in reality (Cruces et al. 2013; Mijs, 2018a). However, there is a fundamental difference between individual estimates of overall and unfair inequality. Most people underestimate overall inequality because their reference groups tend to have similar incomes as themselves. When all overall inequality estimates in a society are summed up, the result will display this downward bias as well. Fairness estimates, on the other hand, do not have this predetermined bias. The homogeneity of observed reference groups allows people to form relatively valid local fairness estimates because perceived income fairness depends on whether people with similar attributes also have similar earnings (e.g., one's colleagues who work the same job, or friends with similar education). Based on how people themselves and others in their observable surrounding are treated, some will have local fairness estimates that are too low, and others will have local fairness estimates that are too high. When averaged across whole societies, the result should be less biased than estimates of overall inequality.

Overall, I argue that people primarily have an aversion to unfair inequality. Individuals' demand for redistribution increases when unfair inequality rises because they do not support inequality that does not reflect individual deservingness. It can be expected that *unfair inequality positively affects redistribution preferences (H1)*. Fair inequality, on the other hand, should be less consequential for redistribution preferences. It is questionable that people support a certain level of merit-based inequality and seek to reduce this inequality at the same time. Fair inequality may influence redistribution preferences if redistribution advances distributive ideals other than economic fairness (e.g., equality). Unfair inequality, however, is clearly more consequential because decreasing it via redistribution most often advances other ideals such as equality in addition to economic fairness. Thus, I expect that *unfair inequality affects redistribution preferences to a stronger degree than overall inequality (H2)*.

MEASURING UNFAIR INEQUALITY

Several empirical approaches to measure realized income (un)fairness in a society have been proposed (e.g., Almås et al. 2011; Devooght 2008; Krauze and Slomczynski 1985; Pignataro 2012). This study applies the approach by Almås et al. (2011) because, firstly, it focuses on distributive fairness rather than processual fairness norms such as equality of opportunity (see Pignataro 2012), which are also consequential but not the theoretical focus of this study. Secondly, the approach by Almås et al. allows the researcher to specify individual characteristics that do and do not legitimize inequality rather than having a pre-specified fairness model (e.g., Krauze and Slomczynski 1985). And thirdly, because the quantification results in a Gini coefficient that solely measures unfair inequality, which implies that empirical results can easily be compared to the conventional Gini coefficient that is frequently used in inequality research.

Almås et al.'s (2011) approach to measure unfair inequality requires representative micro datasets that contain information on income and individual characteristics. It involves estimating a hypothetical fair income distribution based on individuals' merits, calculating how much it differs from the actual distribution, and aggregating the results into an unfairness Gini index purged from fair income differentials. A fair distribution is defined as one where everybody has the same returns to their merits. This requires a choice of what individual characteristics are merits, which will be conceived of in broad terms for the purpose of this study. Merits are defined as all attributes related to occupation, skills, experience, and effort. This follows an intentionally minimal fairness principle, namely that individuals in similar employment with

similar skills and effort who do similar work should receive similar remuneration, i.e., non-discrimination. The methodology proposed by Almås et al. as well as the exact choice of data and variables used in the estimation procedure are presented below.

The measurement approach of Almås et al. (2011)

Incomes vary according to individuals' characteristics. These include *merits* that result in fair inequality and *circumstances* that produce unfair inequality. Estimating unfair inequality proceeds in the following steps. Firstly, the linear regression model given in equation (1) is fitted using log income as the dependent variable and all variables identified as merits and circumstances as independent variables.

$$(1) \quad \log y_i = \beta_m X_i^m + \beta_c X_i^c + \varepsilon_i$$

where y refers to income, X^m to all variables defined as merits, and X^c to all defined as circumstances of individual i . The vector of estimated coefficients β_m indicates the merits' average market remuneration irrespective of the circumstances' relationship to income, which effectively serve as control variables.

Secondly, equation (2) yields a fair income share for everyone based on the merits' coefficients β_m and individuals' observed values of the corresponding variables, denoted by lower-case letters.

$$(2) \quad \vartheta_i = \frac{\exp(\beta_m X_i^m)}{\sum_i \exp(\beta_m X_i^m)}$$

where the numerator of the fraction corresponds to the predicted income of individual i solely based on merit, and the denominator to an aggregation of all predicted merit-based incomes in society. The exponential function is used because of the log-transformation of the dependent variable in the initial regression. The logic of the fair income share ϑ_i is that everyone should receive an income share given by individual merit relative to aggregate merit. A hypothetical fair income y^f is then calculated with equation (3). It multiplies the fair share with the total available income, which is defined as the aggregate income in a country.

$$(3) \quad y_i^f = \vartheta_i \sum_i y_i$$

Lastly, the results are aggregated into an *unfairness Gini* index given by equation (4):

$$(4) \quad \text{Gini}_{\text{unf.}} = \frac{1}{2n(n-1)\mu(y)} \sum_i \sum_j |(y_i - y_i^f) - (y_j - y_j^f)|$$

where n refers to the number of individuals, $\mu(y)$ to mean income, and both i and j to individuals (see Almås et al. 2011: 489–90). This unfairness Gini indicates to what extent real incomes deviate from (hypothetical) fair incomes. In contrast, the conventional Gini index indicates to what extent real incomes deviate from perfect equality.

Empirical application

Unfair inequality is estimated just as proposed by Almås et al. (2011) using data from the Luxembourg Income Study (LIS 2020). The LIS offers a high-quality data infrastructure with harmonized micro datasets on, e.g., the income of the population in Germany in 2012. Each dataset is used to estimate aggregate unfair inequality for a specific country and year, the results of which will be merged to micro-level data from the ESS to assess the impact on redistribution preferences in a subsequent step. The sample selection of country-years depends on mutual data availability in the LIS and ESS data, which will be explained in detail in the ESS data description. Using all available data, unfair inequality can be estimated for 48 country-years from 16 countries.

The regression models (see equation 1 above) are estimated with hourly labor income, gross of taxes, as the dependent variable.³ Capital income is explicitly disregarded because it is unclear what characteristics legitimize capital income inequality. The samples are restricted to non-retired working age (16-65) individuals in dependent employment with an income above zero, weighted according to the LIS personal weights. Defined as merits are the variables education (dataset-specific categories), profession (10 categories based on ISCO-08), industry (nine categories), sector (public or private), age (five categories: <25, 25-34, 35-44, 45-54, >54), as well as interaction terms between education and profession.⁴ All job-related variables refer to the respondents' first job.⁵ Defined as circumstances are gender, a children dummy, an interaction of the gender and children dummies, region (dataset-specific categories), the father's education (dataset-specific categories), as well as dummies on the respondents' immigrant background, rural place of living, and permanent employment status. Unfortunately, not all variables are available for each individual regression. Table A1.1 in Appendix A lists which variables are excluded in which country-years. A sensitivity analysis shows that the results are robust to an exclusion of variables that are often not available.⁶

What qualifies the classification of variables as merits or circumstances? As previously stated, the guiding principle is a minimalist conception of income fairness, namely that individuals in similar employment with similar skills and effort who do similar work should receive similar remuneration. Accordingly, merits are defined as all attributes related to occupation, skills, experience, and effort. The merits profession, industry, and sector indicate respondents' *occupation*. Working hours, education, and profession show the *effort* that respondents deliver or have delivered in the past.⁷ Lastly, education and age relate to individuals' *skills and experience*. The variables defined as circumstances, on the other hand, are at most loosely related to individuals' occupation, skills, or effort.

Results

The unfairness Gini measuring unfair inequality is estimated for 48 country-years from 16 countries. In addition, five variants of the conventional Gini index measuring overall inequality are estimated from the same datasets to assess the relative explanatory power of unfair and overall inequality in the empirical analysis. These additional Gini indices measure overall inequality of (1) personal gross labor income among the working-age population in dependent employment

³ Incomes crucially depend on working time, and it is necessary to normalize incomes accordingly to make them comparable between individuals. This is achieved by dividing income by annual working time. My framework assumes it to be fair that people who work more receive a larger income. Thus, the normalization according to hours worked is a first consideration of distributive fairness. Hours worked could also be framed as a fair input and used as an independent variable in the income regressions instead. However, it is much cleaner to normalize according to working hours first because otherwise a single coefficient of working hours would have to be estimated for whole workforces.

⁴ The preferred specification is not available in some cases because the profession and industry dummies are recorded in rougher or dataset-specific categories. If the 10-category profession specification is not available, I use the three-category specification; and if this is not available, I use the dataset-specific categories. Likewise, I prefer the nine-category industry categorization over the three-category specification over the dataset-specific entry. Lastly, education is used as a continuous variable for the interactions with profession to keep the number of independent variables in check.

⁵ This slightly affects the results because some (but few) individuals also have a second job that is not considered in the income regressions. However, most LIS datasets do not collect information on respondents' second job, and including more variables would overload the regressions models.

⁶ I re-estimated unfair inequality and excluded the circumstance variables immigrant background, education of father, permanent employment, and rural place of living. The resulting unfairness Gini is highly correlated with the main specification ($r=.99$). This result is based on data from the countries Germany and Slovakia, which are the only countries that consistently have all four excluded circumstance variables available.

⁷ Although working hours is not used as a variable in the income regressions, it is used to normalize the dependent variable, which is a first consideration of distributive fairness.

FIGURE 2.1: Time series of unfair and overall labor income inequality by country



Note: No time series for Iceland and Luxembourg are shown because only one data-year is available, respectively.

(i.e., the same sample used to estimate unfair inequality), (2) personal labor income among the whole population, (3) personal gross total income among the working-age population in dependent employment, (4) personal gross total income among the whole population, and (5) household gross total income (equivalized).⁸ The full results are available in Appendix A.

Figure 2.1 plots estimated unfair inequality. The aim is to show how unfair inequality is distributed among countries and over time. The figure also includes overall inequality (specifically the conventional Gini index of personal labor income among the working-age population in dependent employment; i.e., the same income type and population unfair inequality is estimated from). This allows for a direct comparison of how adjusting the Gini according to distributive fairness affects the results. Figure 2.1 shows that unfair inequality varies considerably between countries, with the Netherlands having the lowest and Israel the highest value. Furthermore, unfair inequality is consistently lower than overall inequality because a proportion of overall inequality results from individual merit.⁹

⁸ People with zero income are excluded from the estimation sample, and LIS personal and household weights are used in each case. Furthermore, household income is equivalized by dividing it by the square root of household members. The aim is to make incomes comparable between households of different size.

⁹ It is principally possible that unfair inequality is higher than overall inequality, but this would require that the deviation of actual incomes from perfect equality is smaller than the deviation of actual incomes to fair incomes. This seems like an unlikely scenario because it could only result from grossly unequal returns to labor-related merits and thus labor markets with barely functioning labor pricing.

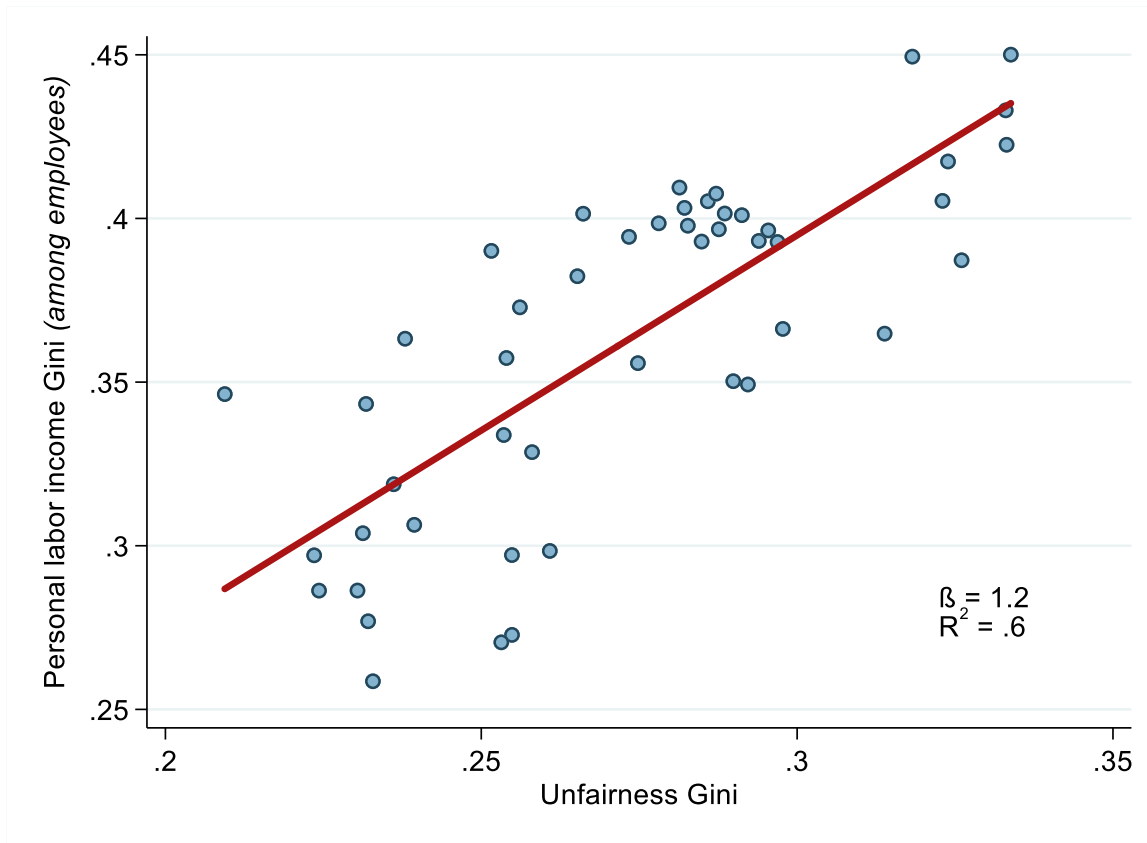
FIGURE 2.2: Scatterplot of unfair and overall labor income inequality

Figure 2.2 explores the relationship between unfair and overall inequality by depicting a scatterplot with linear fit. It becomes evident that higher overall inequality is associated with higher unfair inequality, but the relationship is not perfect ($R^2=.6$). As expected, unfair inequality is not given by a fixed proportion of overall inequality. This implies that unfair and overall inequality are related but distinct concepts.

Is the unfairness Gini a valid measure?

The main empirical analysis, where redistribution preferences are regressed on the unfairness Gini, crucially relies on the assumption that the unfairness Gini is a valid measure of experienced unfairness. This subsection assesses critically whether this assumption is reasonable. It proceeds in two steps. Critiques that can be raised from a theoretical perspective are discussed first; thereafter, the unfairness Gini is validated using empirical data.

Two critiques can be raised against the quantification of unfair inequality from a theoretical perspective. Firstly, one may question the indicator because it only registers inequality as unfair when people have unequal returns to characteristics defined as merits even though the populace may consider certain returns to be exorbitantly low or high. For example, do people with a university degree really deserve that, on average, they enjoy a sizable income advantage compared to those with non-tertiary education? The proposed unfair inequality measure cannot consider this question. It will only consider inequality as unfair when people with the same occupational attributes (such as a university degree) do not enjoy the same returns to these attributes.

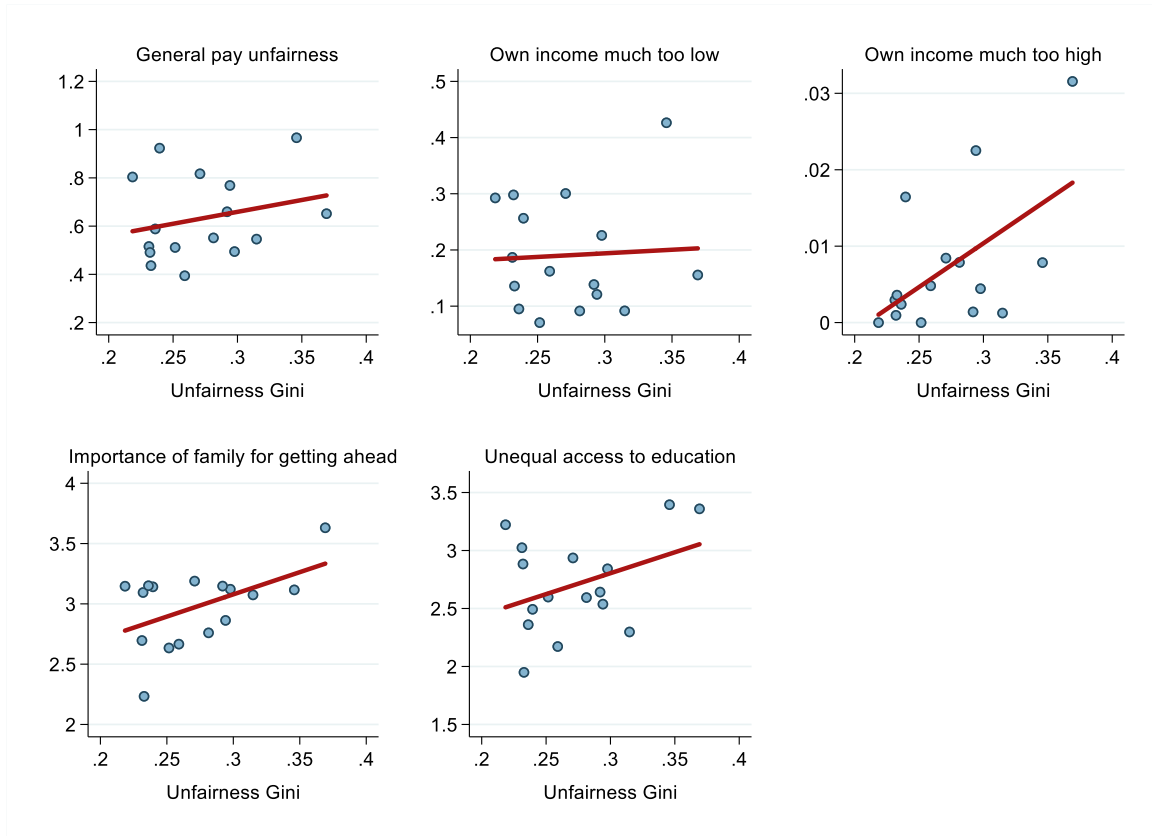
I acknowledge this critique but argue that the unfair inequality measure is nonetheless valid because it assesses the backbone of income fairness, i.e., non-discrimination. There is more to

income fairness than non-discrimination, but non-discrimination is fundamental. Returning to the example, people may not always agree that people with a university degree really deserve their high income, but it is likely that all agree that, *ceteris paribus*, degree holders should at least be treated equally in order to satisfy baseline distributional fairness. Furthermore, I expect that the critique has less bite than one may assume. Income fairness is not judged relative to an abstract standard of how much individuals with certain merits ought to earn in absolute terms. Individuals rather adapt their perception of how large income differentials should be to what they observe in reality (Trump 2018).

The second critique is that the choice of merits and circumstances may seem questionable. Is it sensible to lump labor-related variables together and define them all as legitimizing sources of inequality? Would it not be preferable to arrive at a more fine-grained definition of what labor-related characteristics legitimize inequality, for example by refraining from defining employment in the public vs. the private sector as a merit? I acknowledge that the choice to use all labor-related characteristics as merits is debatable. However, I argue that it is necessary in the context of the macro view that this study takes to follow such a minimalist conceptualization of distributive fairness. The empirical analysis covers 16 countries, and it should be expected that they have different conceptions of income fairness, for example because one country supports seniority-based income advantages more than another. It would be impossible to justify a more sophisticated model that reflects these differences due to the cross-country perspective. However, the proposed minimalist conceptualization of income fairness is a feasible strategy. The fundamental fairness principle that, *at least*, individuals with similar labor-related characteristics should receive a similar income should find broad support in all countries under consideration.

Moving on to the empirical validation of the measure, the goal is to assess whether higher values of the unfairness Gini empirically coincide with increased perceptions of experienced income unfairness. Such an analysis is difficult to implement because data on perceived income unfairness is unavailable in the European Social Survey, which will be used in the main analysis. To offer an empirical validation nonetheless, individual-level data from the 2009 Social Inequality module of the International Social Survey Programme (ISSP) will be used, which contain commonly used unfairness perceptions. Using all available data, sixteen additional datapoints of the unfairness Gini are quantified using LIS data and subsequently merged to the ISSP. Figure 2.3 plots the relationship between the unfairness Gini and country-level means of five different income unfairness perceptions. Detailed data and variable descriptions are available in Table A1.2 in Appendix A.

Figure 2.3 reveals that income unfairness according to the unfairness Gini generally coincides with unfairness perceptions on the individual level. In countries with a higher unfairness Gini, people think that ethical and actual labor remuneration diverges more and the proportion of individuals who see their income as much higher than deserved is larger. Likewise, the proportion of people who think that their income is much lower than deserved is also larger, but this relationship is weaker than in the other plots. Furthermore, the unfairness Gini correlates positively with perceptions that circumstances rather than merits determine who flourishes (specifically, the importance of a strong family background and unequal access to education). The results thus suggest that the unfairness Gini indeed taps into individuals' unfairness perceptions. However, it must be stressed that this validation is less perfect than one would prefer because it is only based on 16 countries and uses a different country-year sample than the main analysis.

FIGURE 2.3: Empirical validation of the unfairness Gini

Note: The figure plots bivariate relationships between the unfairness Gini and country-level means of different income fairness perceptions. The fairness perceptions are based on the 2009 Social Inequality module of the ISSP. Data, sample, and variable descriptions are available in the Appendix A.

DATA AND METHODS FOR THE MAIN ANALYSIS

In a next step, the unfair and overall inequality variables are merged to multiple waves of the European Social Survey (2002-2014) to estimate their effect on individual redistribution preferences with multilevel models. The ESS offers high-quality datasets used commonly in redistribution preference research. ESS data rather than other available datasets such as the ISSP are used because the ESS has a vastly superior cross-sectional and longitudinal coverage, which is necessary to reach an acceptable higher-level sample size. The choice of ESS country-waves depends on mutual availability with LIS data, which is assessed in a mutual exclusion process. All country-years with data from both the ESS and LIS that contain all crucial variables are included. Since multilevel models require a sufficient higher-level sample size, it was necessary in some cases to use LIS data from country-years preceding the ESS data by one year (see Appendix A, Table A1.3). This should not influence the results because of the high autocorrelation of labor market fundamentals. The selection process results in a sample of 48 country-years from 16 European countries. Akin to the populations used to estimate unfair inequality, the ESS samples are restricted to working-age individuals (16-65) in dependent employment. The rationale is that it is primarily individuals in dependent employment who (*a*) have the relevant information to gauge income fairness among employees and (*b*) who react to income fairness among employees.

The dependent variable is the demand for redistribution. Individuals indicated their support for the following statement on a five-point scale: “The government should reduce differences in income levels”, which I recode onto a scale from zero to one where higher values indicate

increased support. This variable is commonly used in studies on redistribution preferences (e.g., Finseraas 2009; Jæger 2013; Schmidt-Catran 2016).

Concerning the individual controls, I firstly use left-right ideology as measured by respondents' self-assessment on an 11-point left-right scale centered around zero. Secondly, I include net household income, which is found to be strongly associated with redistribution preferences (e.g., Alesina and La Ferrara 2005; Finseraas 2009; Schmidt-Catran 2016). Income is inconsistently measured as either absolute or relative categories in the ESS data. I recode the variable to country-specific quintiles following the approach of Schmidt-Catran (2016: 127). Furthermore, research shows that individuals support redistribution as a social insurance scheme. Those who expect to lose income in the future tend to increase support while those who expect to gain decrease support (Alesina and La Ferrara 2005). Following Rehm (2009), I use the occupation-specific unemployment rate¹⁰ to capture the objective unemployment risk. The remaining control variables are the highest level of education, age, a gender dummy, and household size (logged). Lastly, I include ESS-wave dummies indicating from which data wave the data stem (Fairbrother 2014).

The data have a three-level hierarchical structure with individuals on level one, country-years on level two, and countries on level three. The goal is to assess the impact of a country-year-level variable, i.e., unfair inequality, on individual redistribution preferences. Thus, I employ multilevel models with random intercepts for both country-years and countries, treating the dependent variable as continuous. Multilevel models allow the researcher to *(a)* regress micro-level variables on macro-level variables and *(b)* to analyze hierarchical data without invalidating hypothesis tests (Hox 2010). The advantage of the model is that the impact of macro-level variables can be assessed while controlling for individual characteristics.

RESULTS

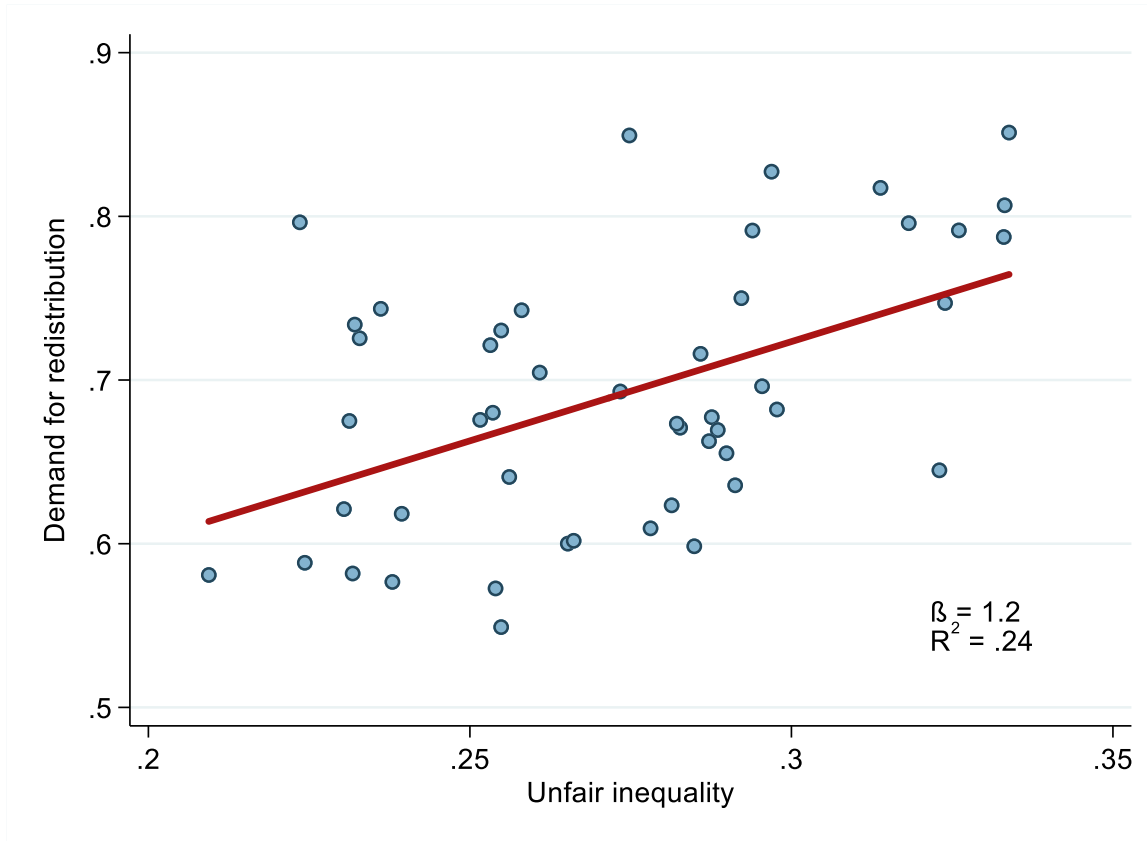
This section reports the results from several multilevel models. The analysis is conducted in three steps. A first set of regressions considers the relationship between unfair inequality and redistribution support. A second set of regressions subsequently compares the explanatory power of unfair inequality with the explanatory power of several measures of overall inequality, each based on a different definition of income type and baseline population. The third step analyzes the cross-sectional and longitudinal variance of unfair inequality separately. All regression models use a common sample of 31,309 individuals. Unless stated otherwise, the data are weighted according to the post-stratification weight of the ESS,¹¹ and the standard errors are derived from the observed information matrix (i.e., model-based standard errors).

Relationship between unfair inequality and demand for redistribution

Figure 2.4 plots the bivariate relationship between unfair inequality and mean redistribution support on the country-year level. The results indicate a positive relationship. Unfair inequality explains 24% of the variance in redistribution support on the country-year level, which is considerable given that preference formation is complicated and there should be multiple other factors driving variance.

¹⁰ Occupation-specific unemployment is estimated separately for each country-wave from the ESS data. It is based on the 1-digit ISCO-08 classification of occupations.

¹¹ Post-stratification weights aim to remove both sample error and non-response bias.

FIGURE 2.4: Scatterplot of unfair inequality and mean redistribution support

The results of a first set of regressions are displayed in Table 2.1. All four models assess the relationship between unfair inequality and the demand for redistribution. Model 1 only contains unfair inequality. Model 2, the main specification, then introduces all control variables. In both cases, unfair inequality has a positive and highly significant coefficient, which supports Hypothesis 1. The coefficient from Model 2 indicates that the demand for redistribution increases by 0.06 across its zero-to-one range when unfair inequality increases by two standard deviations.¹² This is roughly the same as the difference in redistribution support between the first and fourth income quintile. Therefore, the effect of unfair inequality is not only significant in a statistical but also in a substantive sense.

Unfortunately, the country-level sample size is smaller than one would prefer ($N=16$), which in the worst case is associated with a high type I error rate due to deflated standard errors (Maas and Hox 2004; Stegmueller 2013). Deflated standard errors are not necessarily an issue because the estimates rely on 48 country-years of unfair inequality, which is above the recommended higher-level sample size. There is, however, remarkable intra-country correlation of unfair inequality and it should thus be excluded that my inferences are biased by deflated standard errors. Model 3 thus uses robust standard errors clustered by countries, which prevent error deflation but are inefficient when the number of clusters is low (Maas and Hox 2004); and Model 4 uses standard errors corrected for denominator degrees of freedom, which have recently been shown to deal with biased standard errors (see Elff et al. 2021). The results of Models 3 and 4 show that unfair inequality retains its highly significant coefficient.

¹² Unfair inequality has a standard deviation of 0.032.

TABLE 2.1: The effect of unfair inequality on redistribution preferences

	(1)	(2)	(3)
Unfair inequality	0.94*** (0.32)	1.02*** (0.27)	1.02*** (0.18)
Left-right		-0.02*** (0.00)	-0.02*** (0.00)
Gender (ref.: female)		-0.03*** (0.00)	-0.03*** (0.00)
Age		0.00*** (0.00)	0.00*** (0.00)
Occupational risk		0.52*** (0.06)	0.52*** (0.15)
<i>Education</i>			
Below secondary		ref.	ref.
Lower secondary		-0.00 (0.01)	-0.00 (0.01)
Upper secondary		-0.01 (0.01)	-0.01 (0.01)
Post-secondary		-0.02** (0.01)	-0.02* (0.01)
Tertiary		-0.05*** (0.01)	-0.05*** (0.01)
<i>Income</i>			
1st income quintile		ref.	ref.
2nd income quintile		-0.01** (0.01)	-0.01*** (0.00)
3rd income quintile		-0.03*** (0.01)	-0.03*** (0.01)
4th income quintile		-0.05*** (0.01)	-0.05*** (0.01)
5th income quintile		-0.10*** (0.01)	-0.10*** (0.01)
Household size (log)		0.01*** (0.00)	0.01*** (0.00)
Constant	0.44*** (0.09)	0.37*** (0.08)	0.37*** (0.05)
Model	RI-ML	RI-ML	RI-ML
Year fixed effects	No	Yes	Yes
Standard errors	OIM	OIM	Robust
Weighted	Yes	Yes	Yes
Observations	31,309	31,309	31,309
Number of countries	16	16	16
Number of country-years	48	48	48

Note: Standard errors in parentheses. * $p < .1$ ** $p < .05$ *** $p < .01$. *RI-ML* refers to a random intercept multilevel model. *OIM* refers to standard errors derived from the observed information matrix, *Robust* to robust standard errors clustered by countries, and *DF-adjust* to degrees-of-freedom adjusted standard errors following the approach of Elff et al. (2021).

The results are corroborated by additional robustness checks based on the main specification (i.e., Model 2 from Table 2.1). Full regression results are available in Table A1.5 in Appendix A. The first robustness check drops the control variable *left-right ideology* because of possible endogeneity with redistribution preferences. Secondly, a model with random slopes for all variables

is estimated because the effect of the control variables and unfair inequality may vary considerably across countries. Thirdly, the regression is re-estimated without the post-stratification weights. All robustness estimates still yield a positive and highly significant coefficient for unfair inequality. Lastly, it is checked whether the results depend on the inclusion of certain countries in the sample. The main specification is re-estimated 16 times, dropping one of the included countries each time.¹³ The resulting coefficients remain stable (varying between 0.9 and 1.1) and are each significant at a $p < 0.01$ level. Overall, there is thus strong support for Hypothesis 1. The demand for redistribution is higher when there is stronger unfair inequality.

Comparison with simple inequality measures

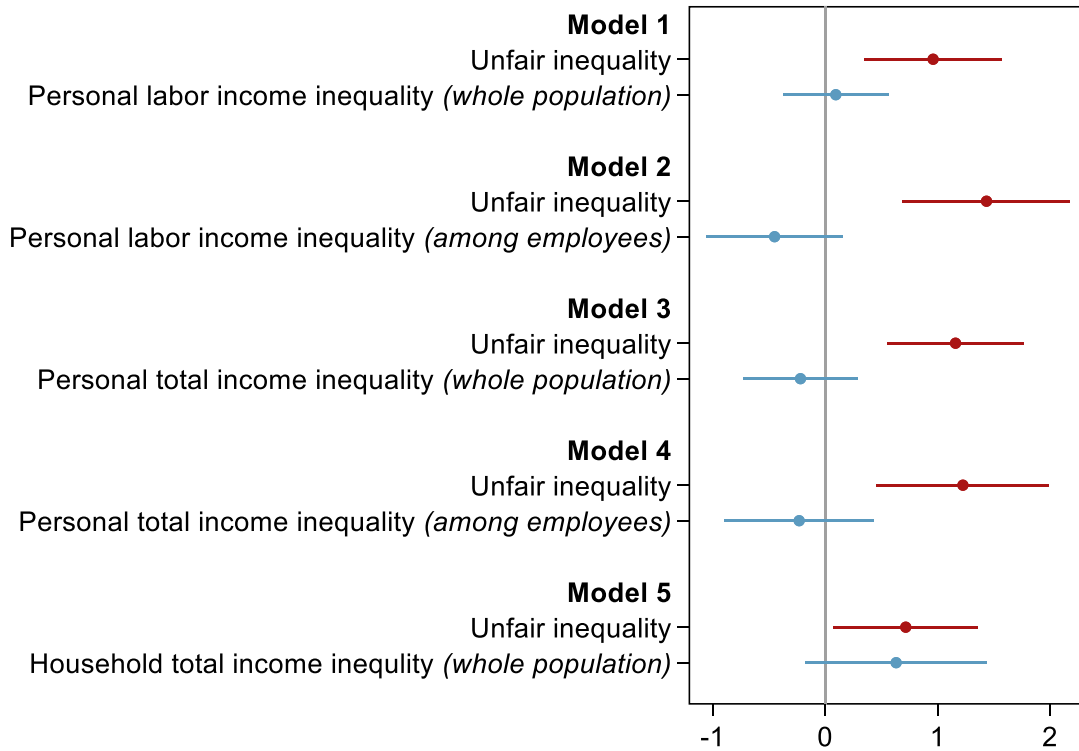
The second set of regressions assesses Hypothesis 2, which states that unfair inequality affects redistribution preferences to a stronger degree than overall inequality. This is achieved by evaluating the relative explanatory power of the unfairness Gini and common variants of the Gini index that measure overall inequality, i.e., the deviation of realized incomes from perfect equality. Unfair inequality is checked against five measures of overall inequality based on the following income types and populations: (1) personal gross labor income among the whole population, (2) personal gross labor income among the working-age population in dependent employment (i.e., the same population used to estimate unfair inequality), (3) personal gross total income among the whole population, (4) personal gross total income among the working-age population in dependent employment, and (5) household gross total income (equivalized).

Figure 2.5 depicts the results of several multilevel regressions that each include unfair inequality in addition to one of the five measures of overall inequality (as well as all control variables). The results show that it does not matter whether unfair inequality is entered into a common model with overall inequality of labor income or total income; whether overall inequality on the personal or household level is considered; and whether the population among which overall inequality is measured is restricted to the same population used to estimate unfair inequality or not. In every model, unfair inequality retains its positive and significant coefficient even though overall inequality measures are included in the same model. Furthermore, all overall inequality measures are insignificant and mostly have negligible effect sizes that are vastly smaller than the effect size of unfair inequality. However, there is one exception, namely overall inequality of total household income. Unfair inequality retains its positive and significant coefficient while overall inequality of household income has a smaller and insignificant coefficient when both are entered in the same model. But strictly speaking, the two coefficients are indistinguishable in size due to their overlapping confidence intervals.

The inconsistent results may emerge from the differing applicability of income fairness norms to personal and household income inequality (remember that only Model 5 compared the effect of unfair inequality to a measure of *household* income inequality). The fairness principle of input-output-proportionality is directly applicable to personal income. For example, it is widely supported that those who work more receive a higher personal income. I expect that, for this reason, unfair inequality consistently trumps naïve measures of overall inequality when the personal income level is considered. In contrast, income fairness is more obscure when it comes to household income inequality since it should generally be accepted that incomes are shared within households. To return to the example, consider that our hardworking person has a spouse who is not in the labor force. While it is considered as fair that this spouse has no *personal* income, it will also be accepted that the spouse bears the fruits of their partner's efforts. Fairness norms therefore follow different logics regarding personal and household income, and a quantification

¹³ The full results are available upon request.

FIGURE 2.5: Relative explanatory power of different Gini variants



Note: The figure plots coefficients of regressions using the demand for redistribution as the dependent variable. The grey bars represent 95% confidence intervals. All models also include the full set of control variables. See Appendix A for full results.

of unfair inequality appropriately applied to household income would be better suited for a comparison to overall household inequality.

Overall, the results offer support for Hypothesis 2, which expected that unfair inequality is more influential for redistribution preferences than overall inequality. There are some caveats regarding the comparison to household income inequality, but it remains difficult to compare the implications of unfair inequality of personal income to those of overall inequality of household income. Furthermore, the results further strengthen Hypothesis 1, which simply expected that unfair inequality is positively related to redistribution preferences.

Disaggregation into longitudinal and cross-sectional variance

The previous sections established that there is a relationship between unfair inequality and redistribution preferences that is independent from overall inequality. This section analyzes to what extent the relationship results from cross-sectional or longitudinal variance of unfair inequality. Distinguishing between longitudinal and cross-sectional variance is possible because the estimation sample consists of repeated cross-sections, at least for most countries.¹⁴ Unfair inequality and the demand for redistribution thus vary *within* and *between* countries. Fairbrother (2014) proposes a method to analyze cross-sectional- and longitudinal variance separately. It is implemented by generating two variants of the unfair inequality variable: Between-variance is captured by a variable measuring country-specific means of unfair inequality; and within-

¹⁴ There are, e.g., biannual data between 2002 and 2012 for Germany.

TABLE 2.2: Within- and between decomposition of unfair inequality (selected results)

	(1)	(2)	(3)	(4)	(5)
Unfair inequality (within)	0.83** (0.38)	0.97*** (0.32)	0.97*** (0.25)	0.88** (0.35)	0.83*** (0.27)
Unfair inequality (between)	1.21** (0.61)	1.16** (0.52)	1.16** (0.49)	1.14* (0.54)	
All controls included	No	Yes	Yes	Yes	Yes
Model	RI-ML	RI-ML	RI-ML	RI-ML	FE
Year fixed effects	No	Yes	Yes	Yes	Yes
Standard errors	OIM	OIM	Robust	DF-adjust	Robust
Weighted	Yes	Yes	Yes	No	No

Note: All models analyze a sample of 31,309 observations from 48 country-years. Standard errors in parentheses. * $p < .1$ ** $p < .05$ *** $p < .01$. *RI-ML* refers to a random intercept multilevel model and *FE* to a fixed effects panel model. *OIM* refers to standard errors derived from the observed information matrix, *Robust* to robust standard errors clustered by countries, and *DF-adjust* to degrees-of-freedom adjusted standard errors following the approach of Elff et al. (2021). Full regression results are available in Table A6 in Appendix A.

variance is captured by intra-country deviations from country-specific means, which is akin to the approach commonly used to implement unit fixed effects in panel models.¹⁵

Table 2.2 reports selected results of several regression models analyzing cross-sectional and temporal variance separately. Only the estimated coefficients of the within- and between-variants of unfair inequality are shown while the full results are available in Appendix A. Model 1 only contains the unfair inequality variables and Model 2, the main specification, additionally includes all control variables. As expected, both the within- and the between-variants of unfair inequality are positive and significant, which offers further support for Hypothesis 1. The effect sizes from Model 2 indicate that the demand for redistribution increases by 0.025 across its zero-to-one range when the within-variant of unfair inequality increases by two standard deviations and by 0.065 when the between-variant increases by two standard deviations.¹⁶ The between-variant thus has a fairly strong effect (again comparable to the difference in redistribution support between the first and fourth income quintiles) and the within-variant a moderate effect, which is roughly comparable to the difference between the first and third income quintiles.

Models 3 and 4 re-assess the results using cluster-robust and degrees-of-freedom adjusted standard errors (see above). Both the within- as well as the between-variant of unfair inequality remain to be positive and significant. Lastly, Model 5 uses a standard fixed effects panel specification that relies solely on intra-country variance of unfair inequality.¹⁷ Again, the results remain unchanged, which supports the validity of the strict exogeneity assumption required for the initial random intercept specifications (uncorrelated independent variables and errors) since fixed effects models do not make this assumption. The estimated coefficient of unfair inequality is therefore not biased by country-specific time-invariant confounders.

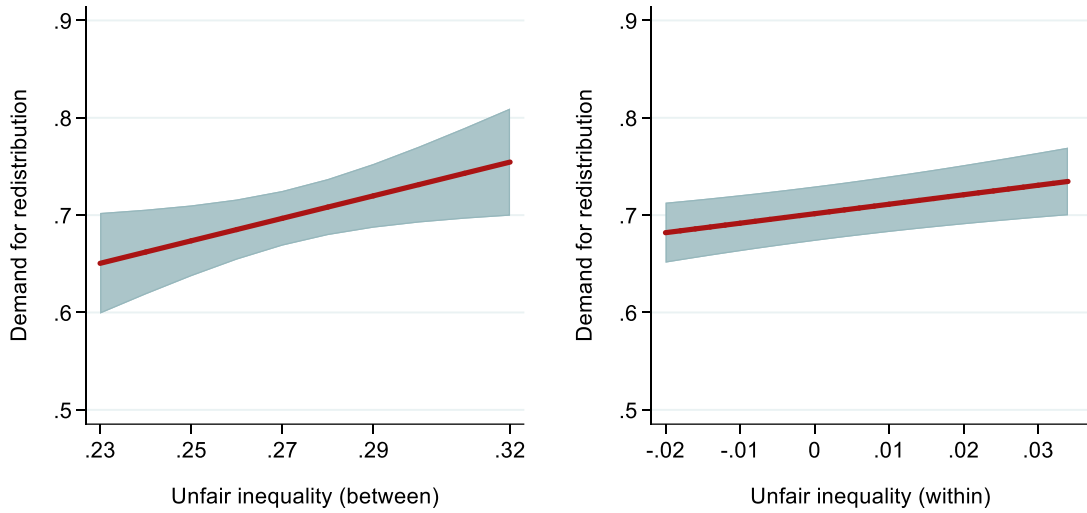
The results are corroborated with additional robustness checks (available in Table A1.5 in Appendix A). Again, the robustness checks drop the control variable *left-right ideology*, repeat the estimations with unweighted data, and estimate random slopes for all variables, all of which leave the results unchanged. Furthermore, the main specification (Model 2 in Table 2.2) is re-estimated 16 times, dropping one of the included countries each time.¹⁸ The results regarding

¹⁵ More formally, between-variance is assessed with the following variable: $X_c^{BE} = \bar{X}_c$, whereas the subscript c denotes countries; and within-variance with: $X_{ct}^{WE} = X_{ct} - \bar{X}_c$, where t denotes time.

¹⁶ The within-variant has a standard deviation of 0.01 and the between-variant of 0.03.

¹⁷ Serially correlated errors are dealt with via cluster-robust standard errors.

¹⁸ Again, the full results are available upon request.

FIGURE 2.6: Between- and within- effects of unfair inequality

Note: The marginal effects are calculated from Model 2 in Table 2. The areas represent 95% confidence intervals.

the within-variant of unfair inequality remain stable with coefficients that vary between 0.8 and 1.1 and p -values that consistently stay below 0.05. The between-variant also keeps a fairly stable coefficient (0.9-1.4), but p -values rise above 0.1 in three cases, with a maximum p -value of 0.13. This does not come as a major surprise because, after all, results regarding the between-variant rely on only sixteen unique observations; and besides that, the opposite result also holds: p -values decrease in several cases and reach the $p < 0.01$ threshold in two of them. Overall, there is strong and consistent evidence for a within-association between unfair inequality and the demand for redistribution; most likely owing to the low sample size, there is also weaker evidence for a between-association. These results offer further support for Hypothesis 1. They imply that both (a) countries with higher unfair inequality have stronger redistribution preferences and (b) that countries that increase their unfair inequality over time develop stronger redistribution preferences. It is a particularly strong result that the estimated coefficients of both unfair inequality variables have very similar coefficient sizes in all specifications.

Figure 2.6 plots predicted redistribution preferences by observed values of the between- and within-variant of unfair inequality. Resulting values of redistribution support increase from 0.67 to 0.73 (within-variant) and from 0.65 to 0.76 (between-variant) across the whole range of observed values.¹⁹ I conclude that unfair inequality is substantially consequential for individuals' demand for redistribution.

DISCUSSION AND CONCLUSION

I argued that the effect of objective income inequality depends on its unfairness, i.e., whether income differences cannot be explained by differences in labor-related merits. It is primarily unfair inequality that affects the demand for redistribution rather than overall inequality. The results of the quantitative analysis line up with this expectation. An empirical quantification of unfair inequality is associated positively with redistribution preferences. Countries with higher

¹⁹ The range of predicted redistribution support values is larger for the between-variant even though the between- and within-variants have similar coefficients similar because there is a wider range of empirically observed values of the between-variant.

unfair inequality have stronger redistribution preferences (although this result is associated with a degree of uncertainty), and countries where unfair inequality rises over time display rising redistribution preferences. Secondly, unfair inequality has superior predictive power compared to overall inequality.

How does this study compare to related research in political economy? In the tradition of the discipline, I argue that objective inequality increases redistribution support. However, the present study stands in obvious contrast with classical rational choice approaches (e.g., Jæger 2013; Johnston and Newman 2015; Schmidt-Catran 2016). Like some of these studies, I find a positive impact of inequality, but my approach differs in its theoretical foundation, which focuses on fairness norms, and its customized inequality indicator, which aims to measure unfair rather than overall inequality. My approach is similar to Schmidt-Catran (2016), who also uses repeated cross-sections to assess the impact of between- and within-variance of inequality on redistribution support. Schmidt-Catran only finds a within-effect of overall inequality, whereas unfair inequality can explain between-variance as well. This discrepancy may merely result from different country-year samples, but I expect that it reflects the importance of addressing income fairness in theory and inequality measurement.

Furthermore, the present study is closely related to recent scholarship that also expects an impact of objective inequality due to other-regarding preferences (Cavaillé and Trump 2015; Dimick et al. 2017; Luebker 2007; Lupu and Pontusson 2011; Shayo 2009). My approach is most similar to Dimick et al. (2017) and Luebker (2007), who also find that aggregate inequality increases redistribution support, at least once differences in distributive justice concerns across or within countries are accounted for. It contrasts most with studies that introduce the possibility that inequality may also *negatively* affect inequality aversion and redistribution support due to detrimental effects on social affinity (Cavaillé and Trump 2015; Shayo 2009) and people's ability to appreciate structural income differences (Mijs 2021). A direct comparison is difficult because of varying inequality measures and country-year samples, but my empirical results suggest the opposite, namely that (unfair) inequality rather exerts a positive influence.

Overall, the present study underscores that objective inequality matters for individuals' redistribution support, and that people are driven by normative concerns about this inequality. Countries have more or less fair income distributions, and it is not always the case that more inequality means more unfairness and thus a stronger taste for redistribution. Whether people are averse to inequality crucially depends on the empirical realization of distributive fairness. At the same time, this study is not without limitations. It was shown that the quantified unfairness Gini is positively associated with redistribution preferences, but it remains an assumption that this relationship can indeed be explained by people's fairness perceptions. It is required that (a) the unfairness Gini is a valid measure of how unfairly people are treated, (b) people's unfairness perceptions are triggered by this conception of unfairness, and (c) that, in the aggregate, people's unfairness perceptions are not fundamentally biased in the same way their beliefs regarding overall inequality are biased. Supportive theoretical arguments and empirical evidence have been presented to substantiate these claims, but they remain assumptions nonetheless.

Future research should be conscious about what part of inequality is accepted by the public and what part of inequality is not. It will be fruitful to assess how other quantifications of realized income fairness relate to policy preferences, for example by using another fairness model specification for a different version of Almås et al.'s (2011) unfairness Gini. Furthermore, there are various quantifications of realized equality of opportunity (see Pignataro 2012). As public opinion research shows (e.g., Alesina and La Ferrara 2005; Fong 2001), this is an influential fairness ideal that had to be disregarded in this study.

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The (a)symmetric effects of income and unemployment on popular demand for redistribution (Paper B)

Abstract

Numerous studies show that those with lower income and the unemployed support more redistribution, which is attributed to material self-interest. However, recent studies assessing within-individual changes result in smaller and less consistent effect estimates. To explain why preferences do not narrowly follow material self-interest, this study argues that the effects of income and unemployment may be asymmetric, implying that improving and deteriorating material circumstances exert differently sized effects. The claims are tested using panel data from Great Britain and a weighted difference-in-difference estimator. The results show that only income increases (negatively) affect redistribution support while income decreases have null effects. In contrast, unemployment is estimated to have a strong and symmetrical effect in line with self-interest theory. These results add further evidence for the validity of self-interest theories but suggest that individuals are only boundedly rational.

INTRODUCTION

Redistribution preferences structure political conflict about economic distributions. They are a determinant of both voting behaviour (Attewell 2021; Rueda and Stegmueller 2019) and redistributive public policies (Brooks and Manza 2006; Luebker 2014). Researchers have therefore devoted considerable attention to explaining individuals' demand for redistribution. Political economists emphasize the role of material self-interest, and their empirical work confirms that individuals' material circumstances are reliable predictors of public opinion. Those with lesser income and those without the ability to generate market income due to unemployment tend to demand more redistribution (e.g., Beramendi and Rehm 2016; Franko et al. 2013; Rehm 2009, 2011; Schwander 2019).

Despite the extensive empirical evidence, recent research has questioned to what extent income and unemployment assume a causal role in preference formation. O'Grady (2019) and Wehl (2019) argue that preferences are primarily formed during early-life socialization, which implies that adult income and unemployment experiences have limited impact. Such doubts about the causality of material circumstances' effects have important implications. If preferences do not (always) respond to economic circumstances, aggregate preferences in a country and resulting political outcomes may not react to increasing inequality or economic shocks such as

mass unemployment. Furthermore, these doubts question the importance attributed to self-interest in political economy research.

A burgeoning literature has set out to test the validity of self-interest theories with panel data, which track individuals over time and facilitate the estimation of income and unemployment effects based on more credible assumptions (see Margalit 2019 for an overview). These studies continue to support the self-interest argument, but they have more ambiguous results with smaller and less consistently significant effect estimates (Gidron and Mijs 2019; Margalit 2013; Naumann et al. 2016; O’Grady 2019; Owens and Pedulla 2014; Pahontu 2022). The evidence so far suggests that self-interest does matter but not as much as previously assumed.

I argue in this study that individuals are only boundedly and not fully rational, which helps to explain why preferences do not narrowly follow material self-interest. Specifically, I argue that the effects of both income and unemployment may be asymmetric. Effect asymmetry refers to the case when improvements and deteriorations of material circumstances exert differently sized effects (Haffert and Ergen 2019; Liebeson 1985), which has the potential to muddy naïve estimates of average effects. Such asymmetry can only be assessed with longitudinal data. From the aforementioned studies, however, only Margalit (2013) and Naumann et al. (2016) assess effect asymmetry, and only regarding unemployment and not regarding income. Building on their theoretical framework, I argue that effect asymmetries result from bounded rationality. Individuals neither have access to all information required for preference optimization nor do they necessarily include all relevant information in preference formation. Effect asymmetries arise when people are asymmetrically informed about the necessity of insuring against future income loss and when relevant information is only asymmetrically included in preference formation. People’s tendency for negativity bias, implying that more weight is assigned to negative experiences, serves as the primary theoretical justification for such asymmetries (Haffert and Ergen 2019; Soroka 2014).

The empirical analysis assesses whether income and unemployment affect redistribution preferences and whether these effects are asymmetric. It relies on a weighted difference-in-difference estimator and panel data from the British Election Study (BES) Internet Panel. This empirical strategy facilitates the estimation of asymmetric effects while avoiding bias from unobserved heterogeneity. The results generally confirm rationalist expectations with some caveats. Firstly, I find that income affects redistribution preferences, but only income increases exert a (negative) influence while income decreases leave preferences unchanged. Secondly, unemployment has a strong and symmetrical effect consistent with self-interest theory. Losing employment increases redistribution support to about the same extent as gaining employment reduces support.

This study makes several contributions to the literature. Firstly, it addresses the common assumption of effect symmetry and shows that it is not always viable. The finding that only income increases affect redistribution support contrasts with a narrow definition of individual rationality and implies that individuals should rather be characterized as boundedly rational. Secondly, this study informs a broader debate on the relative explanatory power of norms and values vs. self-interest (see Margalit 2019 for an overview). It was recently argued that the effects of both income and unemployment on redistribution preferences are spuriously created by a shared cause, namely normative predispositions formed during early-life socialization (O’Grady 2019; Wehl 2019). In contrast, this study suggests that self-interest does play a role in preference formation. This is a credible finding because the effect estimates are derived from within-individual changes, which partials out the impact of stable predispositions. Thirdly, this study presents nuanced evidence by explicitly addressing a vital assumption of commonly used panel models: the absence of unobserved temporal heterogeneity. This assumption may often be

unviable, and this study employs an empirical strategy that resolves associated shortcomings of previous research.

THE RELATIONSHIP BETWEEN MATERIAL CIRCUMSTANCES AND REDISTRIBUTION PREFERENCES

Two types of self-interest theories expect a causal relationship between individuals' current material circumstances and their support for redistribution. Firstly, *present-oriented* self-interest theory expects that individuals seek to optimize their current disposable income. Since the benefits of redistribution decrease with market income, redistribution support should depend negatively on market income. In Meltzer and Richard's (1981) seminal formalization of the argument, individuals' preferred level of redistribution negatively depends on the own income relative to the mean income (see also Romer 1975). Secondly, *future-oriented* self-interest theory expects that individuals seek to insure against possible material hardship in the future. Those who experience more risk for income loss, most importantly due to looming unemployment, should support more redistribution (Alt and Iversen 2017; Iversen and Soskice 2001; Moene and Wallerstein 2001; Rehm 2009).

Different hypotheses regarding the effects of income and unemployment, the most important circumstances addressed in the literature, can be derived from the two types of self-interest theory. Rising market income should weaken redistribution support due to redistribution's effect on current disposable income (and vice versa). At the same time, this decrease in support should be offset more or less due to a concurrent insurance motive because higher earners have more to lose from income loss (Moene and Wallerstein 2001). Furthermore, unemployment should increase redistribution support because unemployment reduces individuals' current market income (often to zero). Unemployment may also increase support due to an insurance motive because the event indicates that future market income is in peril: the unemployment spell may be prolonged, and future labour market opportunities and earning potentials may be negatively affected.

The effects of current income and unemployment have been evaluated extensively. Empirical studies offer overwhelming support for the expectations of self-interest theory. Firstly, studies that compare people with less and more income find that high earners have weaker redistribution support than low earners (e.g., Beramendi and Rehm 2016; Corneo and Grüner 2002; Franko et al. 2013; Rueda and Stegmueller 2019). Secondly, those who are currently unemployed support more redistribution compared to the employed (Cusack et al. 2006; Pahontu 2022; Rehm 2011; Schwander 2019; but see Wehl 2019).

Despite the strong support for self-interest theory, it remains possible that empirical estimates do not reveal causal relationships. Previous work predominantly relied on cross-sectional data, which must assume that confounders, i.e., shared causes of material circumstances and preferences, are sufficiently accounted for via control variables or matching (Keele 2015; Morgan and Winship 2015).¹ The assumption that cross-sectional studies have been successful in this regard is shaky because individuals differ in many ways that are unknown to the researcher and/or not captured by surveys (i.e., 'unobserved heterogeneity'). For example, Wehl (2019) and O'Grady (2019) forcefully argue that the relationship between material circumstances and redistribution preferences is confounded by normative predispositions shaped during early-life socialization (see also Ares 2020; Inglehart 2008).

¹ Of course, other pitfalls of effect identification and estimation must also be avoided (e.g., post-treatment controls).

Due to the shortcomings associated with cross-sectional data, researchers set out to test the claims of self-interest theory with methods that are less prone to unobserved heterogeneity. With some exceptions, researchers employed panel data and fixed effects models, which track how material circumstances and redistribution preferences develop within individuals over time (see Margalit 2019 for an overview). This setup allows the researcher to relax the assumption that time-invariant confounders such as normative predispositions are included as control variables or in a matching procedure.

Regarding the effect of income, Gidron and Mijs (2019) find a negative effect on redistribution support using Dutch data, and Owens and Pedulla (2014) conclude that income losses increase the demand for redistribution of US citizens. However, Margalit (2013) finds that temporal income changes only affect citizens with a Republican party identification in the US; and O’Grady (2019) finds substantially miniscule and partly insignificant effects of income on social policy and progressive taxation preferences in Switzerland. Furthermore, Doherty et al. (2006) employ a different research design by analysing a survey of winners of an US lottery that pays out an income as its prize. They show that received incomes decrease support for the estate tax but *not* for general redistribution support. This finding has high internal validity because income is exogenously assigned by the lottery, but its external validity is questionable since lottery participants are unlikely to be representative of US society.

Regarding unemployment, several studies find that unemployment leads to increased support for redistribution (Margalit 2013; Naumann et al. 2016; Owens and Pedulla 2014; Pahontu 2022), which is again based on panel data from the Netherlands, US, and Switzerland. On the contrary, Wehl (2019) finds that unemployment and redistributive attitudes are unrelated in most European countries; but note that Wehl’s study differs in its empirical approach because it uses cross-sectional data and carefully applied matching methods.

Overall, these studies underpin the bread-and-butter argument of political economists: that self-interest plays a role in the formation of redistribution preferences. However, their results are more ambiguous compared to previous research. Effect sizes are generally (much) smaller than in cross-sectional data, and material circumstances are sometimes found to leave preferences wholly unaffected.

BOUNDED RATIONALITY AND EFFECT ASYMMETRY

The previous section showed that studies relying on longitudinal data (and other studies with credible effect identification) have more ambiguous results than studies relying on cross-sectional data. This section presents a theory that can help to explain this discrepancy. I will argue that individuals are not fully but only boundedly rational, which is why preferences do not narrowly follow rationalist predictions. Crucially, bounded rationality implies that both income and unemployment may have asymmetric effects on redistribution preferences, which has the potential to muddy naïve estimates of average effects.

Effect asymmetry refers to the case when increases and decreases of explanatory variables have differently sized effects (Haffert and Ergen 2019; Liebeson 1985). For example, income has an asymmetric effect when increasing one’s income affects redistribution support to a different extent than decreasing one’s income does. Likewise, asymmetry implies that individuals change their redistribution support after becoming unemployed to a different magnitude than after gaining employment. The most well-known source of asymmetry in psychological processes is *loss aversion* or *negativity bias*, which entails that more importance is attributed to negative rather than positive developments (Haffert and Ergen 2019; Soroka 2014). It will serve as the primary theoretical driver of effect asymmetry.

Most previous studies that analyze longitudinal changes assume effect symmetry (most often implicitly). This cannot be avoided in cross-sectional designs. Longitudinal designs, however, make it possible to assess the effects of improvements and deteriorations in material circumstances separately. Two of the aforementioned studies do exactly this but only regarding the effect of unemployment. Margalit (2013) finds that unemployment increases redistribution support in the US while employment reduces support again. In contrast, Naumann et al. (2016) find persistent effects of unemployment in the Netherlands, which do not dissipate after gaining employment.

I argue that assuming symmetry in the effects of *both* unemployment and income is questionable because the required assumption of full rationality is unreasonable. Firstly, self-interest theories such as Meltzer and Richard (1981) assume that individuals have access to all relevant information. Two pieces of information are required to optimize preferences: one's relative income position (both current and expected), and the extent to which this position is affected by redistribution (i.e., knowledge about tax progressivity and the targeting of transfer income). Secondly, it is assumed that individuals actually make use of all this information in self-oriented preference formation.

I argue that it is more reasonable to assume bounded rationality with limited access to information and imperfect information processing, from which effect asymmetries may arise. Drawing from Margalit (2013) and Naumann et al. (2016), I firstly argue that individuals are equipped with imperfect information about themselves (c.f. Engelhardt and Wagener 2018; Fernández-Albertos and Kuo 2018). Changes in material circumstances lead people to adapt their priors about the probability of income changes in the future. This is especially true for (un)employment trajectories, which can strongly affect future labour market opportunities. To a lesser extent, income changes from sources other than (un)employment, for example from a promotion, job change, or working time change, can also provide information about future opportunities. Crucially, receiving this information affects redistribution support because social insurance via redistribution is deemed to be less or more valuable in response.

Effect asymmetry of unemployment and income may arise when changed priors persist even after a return to the previous employment and/or income level. Deteriorations in circumstances have a more significant effect on preferences when individuals persistently infer from income and employment loss that social insurance via redistribution is required due to looming income loss in the future (Moene and Wallerstein 2001; Rehm 2009). In contrast, improvements in circumstances have a stronger effect when gaining employment and/or a higher income position persistently informs individuals that insurance against income loss is not required because it poses less of a threat than previously assumed. However, the prevalence of negativity bias implies that a stronger effect of worsening circumstances is more likely.

The second (and hitherto neglected) source of effect asymmetry is that individuals may not always consider relevant information in the psychological preference formation process. Individuals attach different weights to information, and primarily salient information is incorporated in preference formation. Recall the two pieces of information required for self-oriented optimization: When individuals experience a change in material circumstances, they must consider that their relative income position changed, and that their (expected) payoff from redistributive policies changed as a result. The effects of income and unemployment have potential to be asymmetric when improvements and deteriorations in circumstances are associated with differing saliency regarding either of the two pieces of information.

Explicit theoretical expectations about the direction of asymmetry are difficult to derive even under the assumption of negativity bias. This is because an improvement in market income, a positive development, is associated with a concurrent negative development, namely an

increased tax rate and decreased transfer income (and vice versa). Assuming negativity bias, worsening material circumstances may have a stronger effect on preferences because associated information that the income position worsened is more salient than opposing information about an income increase would be. In contrast, improving circumstances may have a stronger effect because associated information about individuals' increased effective tax rate and decreased transfer income is more salient than opposing information following improving circumstances would be.

A further point of interest is that, while income and employment changes are related, they can differ substantially regarding information saliency. Both un- and re-employment lead to substantial and highly salient changes in market income. For example, unemployment most often reduces personal market income to zero.² In contrast, income changes from reasons other than (un)employment, for example a job change or promotion, tend to be more gradual and thus less salient. However, the reasons for such income changes are manifold, and they are associated with considerable variation in information saliency. For example, a promotion provides more salient information compared to an automatic salary increase designated by a collective labor agreement; and a change in personal market income is more salient than a change in household income due to another household member, who may not discuss minute details about their income trajectory.

The different information saliencies associated with shifts in employment and income have several implications for the theorized mechanisms underlying effect asymmetry. The high saliency of income changes caused by (un)employment implies that effect asymmetries of (un)employment should rather arise because of asymmetric changes in priors about future labour market chances. In contrast, effect symmetries of income changes in general should arise to a greater extent from asymmetric information saliency. This is because income changes tend to be more gradual, implying that overall information saliency is lower and that less information about future income trajectories is provided.

To sum up, both income and unemployment may have asymmetric effects on redistribution preferences. However, no explicit hypotheses regarding specific (a)symmetries are formulated. There are plausible arguments for effect asymmetries where either improvements or deteriorations in material circumstances are more influential. I thus follow Margalit (2013) and treat the (non)existence and direction of asymmetries as an 'empirical question' (p. 84).

METHOD

This section outlines the empirical strategy used to identify and estimate asymmetric effects of income and unemployment on redistribution preferences. It first introduces the strategy, namely a weighted difference-in-differences (DID) estimator. The latter part of the section discusses why this strategy resolves shortcomings associated with previous research.

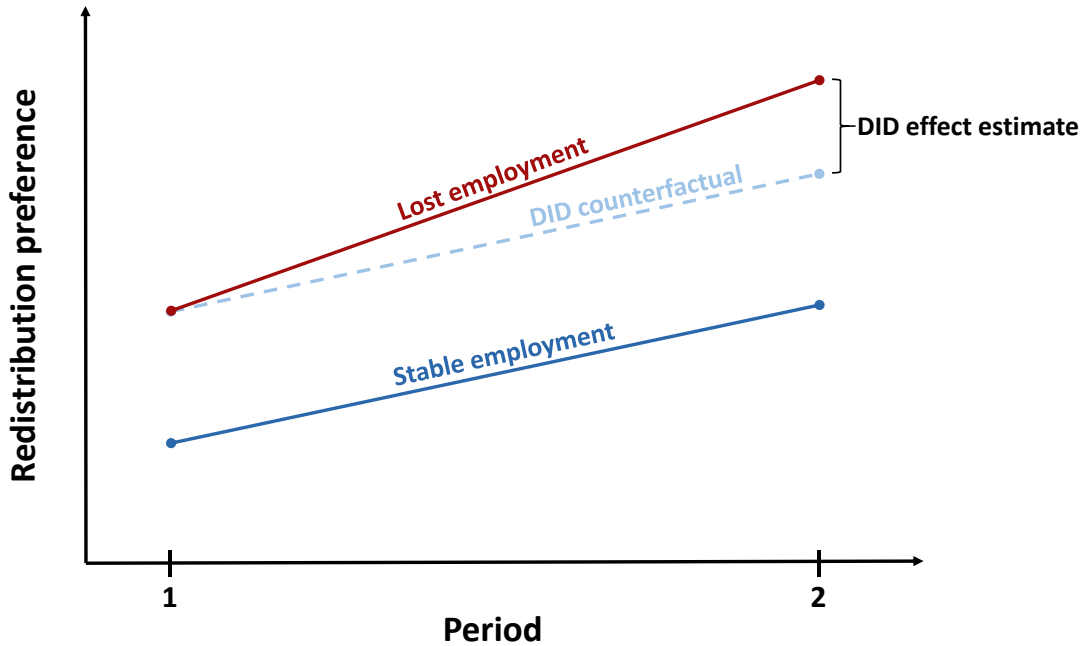
Empirical strategy

The empirical analysis relies on a weighted difference-in-difference (DID) estimator (Brüderl and Ludwig 2015; Lechner 2010). DID requires that panel data are set up in the following way: Individuals are observed exactly twice over time,³ and the explanatory variable is binary (e.g., unemployment vs. employment) and only varies in the second period (e.g., all individuals are

² This blow may be softened by the market income generated by other household members.

³ More than two periods can be used in DID, but canonical DID requires that included periods can be strictly categorized into pre- and post-treatment periods, which closely resembles the two-period data setup introduced here.

FIGURE 3.1: Effect identification in a difference-in-difference setup



Note: The figure depicts hypothetical results from a panel survey where individuals are observed twice over time. All individuals are employed in period one; a proportion becomes unemployed in period two (treatment group: “lost employment”) while the rest stays in employment (control group: “stable employment”). Average redistribution preferences of these two groups are indicated on the y-axis. The dashed line indicates the counterfactual development of redistribution preferences for the treatment group.

employed in the first period and some become unemployed in the second period). The effects of material circumstances can then be estimated with ordinary least squares (OLS) using the following regression model:

$$y_{i,t} = \alpha_i + \gamma_t + \beta_1 T_i * \gamma_t + \beta_k Z_{i,t} + \epsilon_{i,t} \quad (1)$$

where $y_{i,t}$ denotes the redistribution support of individual i in period t , α_i individual-specific constants (i.e., individual fixed effects), γ_t a time dummy that takes on the value one in the second period, T_i a time-invariant dummy that identifies whether a respondent belongs to the group of individuals who experience a ‘treatment’ such as unemployment in the second period, $Z_{i,t}$ a number of additional time-varying covariates, and $\epsilon_{i,t}$ the error term. The parameter of substantive interest is β_1 , which indicates an average treatment effect on the treated (ATT) (Lechner 2010).

Figure 3.1 depicts how causal effects are identified with DID using becoming unemployed as an example. To assess how unemployment changes the redistribution support of those who become unemployed, we are required to know their hypothetical level of redistribution support if they would have stayed in employment (i.e., the counterfactual). The causal effect is given by the difference between observed preferences and the counterfactual. The counterfactual is inherently unobservable, and it must therefore be estimated. DID does this by assuming that the redistribution support of those who become unemployed would have followed the same temporal trend as observed individuals in stable employment (common trend assumption), which is indicated by the dashed line in Figure 3.1.

DID is an attractive strategy because it effectively deals with unobserved confounders (i.e., shared causes that are not included as controls in the regression model) that plague both cross-

sectional and longitudinal designs. Bias from time-invariant confounders such as stable predispositions is completely prevented by solely assessing within-individual changes. Crucially, DID also allows for the presence of unobserved time-variant confounders, but these confounders must exert the same average impact on those who do and do not experience a treatment such as unemployment. For example, the emergence of unfavourable macroeconomic conditions such as mass unemployment may increase redistribution support between two periods (Kölln 2018; Neundorf and Soroka 2018). DID remains unbiased by this development even when unemployment is not included as a control in the regression model, but only if those who do and do not become unemployed are affected in the same way by these macroeconomic conditions (on average). If there are trend imbalances between the treatment and control group caused by developments that affect the treatment and control group differently, the corresponding variables must be included as controls in the regression model.

The second advantage of DID is that asymmetric effects can be estimated by virtue of the data setup. This is straightforward in the case of unemployment: one analysis tests whether those who switch into unemployment increase their redistribution support relative to those in stable employment, and another whether those who gain employment decrease their redistribution support relative to those in stable unemployment. However, the data setup is met with complications in the case of income since the variable is continuous and DID requires a binary explanatory variable. I use the following approach. The effect of income increases is evaluated by comparing those who increase their income to those with stable income, and the effect of income decreases by comparing those with decreased income to those with stable income. In both cases, those with stable income trajectories are used to construct the counterfactual.

To make the crucial DID assumption (i.e., common redistribution support trends) more plausible, matching methods are additionally applied (Gangl 2015; Ho et al. 2007). Matching is used to create balance between the treatment and control groups on selected pre-treatment variables, i.e., variables measured before treatment occurs. The goal is to base the estimates on comparisons between treatment and control groups that are initially very similar but then differ because the treatment groups experience changing material circumstances. Using age as a matching variable, for example, ensures that those who become unemployed are compared to a control group of individuals with a similar age composition, who are likely to have the same development of preferences. I rely on Hainmueller’s (2012) entropy balancing because it reliably creates covariate balance without requiring manual balance checks. The procedure creates balance on up to the first three moments (mean, variance, skew) of included matching variables. It results in a weight that can be used in regression analysis to re-weight the control group so that it artificially resembles the treatment group.⁴

Whether matching is successful in removing trend imbalances depends on included matching variables, which resembles the problem of choosing the right set of control variables (Keele 2015). But matching has the advantage that time-invariant characteristics such as gender can be used, which cannot be included as controls due to the individual fixed effects. Such time-invariant characteristics cannot bias the estimates themselves, but they are likely related to time-varying confounders (Abadie 2005). Furthermore, time-varying controls can be used *on top of matching* to get rid of remaining trend imbalances (Ho et al. 2007).

⁴ This is achieved by assigning all respondents in the treatment group a weight of 1 and respondents in the control group a weight of varying size.

Advantage over common modelling strategies

Previous longitudinal research on the effect of material circumstances predominantly relied on the two-way fixed effects (TWFE) regression model for its main inferences (Brüderl and Ludwig 2015):

$$y_{i,t} = \alpha_i + \gamma_t + \beta_1 X_{i,t} + \beta_k Z_{i,t} + \epsilon_{i,t} \quad (2)$$

where γ_t denotes time-specific constants (i.e., time fixed effects) and $X_{i,t}$ the explanatory variable of interest (i.e., income or unemployment).

TWFE and DID models are conceptually similar. When the data are set up correctly (i.e., clear pre- and post-treatment periods and a binary explanatory variable that only varies in the post-treatment period), DID can also be estimated using the TWFE regression model reported in equation (2), which returns identical results as the model reported in equation (1). However, TWFE estimates are primarily derived from more flexible data setups including continuous explanatory variables, explanatory variable variance in all periods, and no clear pre- and post-treatment periods.

TWFE is a popular strategy because it carries the same promise as DID, namely unbiasedness in the presence of unobserved confounders (both time-variant and -invariant), while being more flexible than DID. Whether the promise of unbiasedness is fulfilled also depends on the common trend assumption exemplified in Figure 3.1 (Brüderl and Ludwig 2015). It must be assumed that those with different income and employment trajectories would have the same counterfactual development of redistribution preferences after adjusting for control variables.

Weighted DID is chosen as the empirical approach because it has three advantages over TWFE. Firstly, DID facilitates an analysis of asymmetric effects by virtue of its data setup. For example, two separate regressions will assess the effect of losing and gaining employment. TWFE, in contrast, must assume that explanatory variables exert symmetrical effects.⁵ Secondly, DID estimates should be less biased because counterfactual states are estimated more carefully. For example, to assess the causal effect of moving into unemployment, one is required to estimate a counterfactual, i.e., the redistribution support of the unemployed if they would have remained in stable employment. DID derives this counterfactual from people in stable employment, which matches the counterfactual state of interest, whereas common TWFE setups often include other groups such as those in stable unemployment or those outside the labour force. This advantage of DID is solidified by the matching approach, which ensures that effect estimates are derived from treatment and control groups that are as similar as possible. Thirdly, recent work shows that, unlike DID, TWFE is only successful in adjusting for time-variant and -invariant confounders under additional modelling assumptions, which will often be violated in common data settings.⁶

The downsides of DID vis-à-vis TWFE are, firstly, a loss in estimation precision caused by splitting up the samples (e.g., the effect of moving in and out of employment are analyzed separately) and removing variance by dichotomizing the income variable. Secondly, dichotomizing income complicates the interpretation of estimated effects. Due to these disadvantages, it remains advisable to report the results from TWFE models alongside DID results.

⁵ Note that other models can be used for analyses of asymmetric effects, most notably models with first-differenced variables in which increases and decreases of explanatory variables are included separately (Allison 2019; Haffert and Ergen 2019). This approach can also incorporate continuous explanatory variables, but it is less straightforward to address unobserved heterogeneity.

⁶ TWFE requires the assumption of linear-additive effects (Imai and Kim 2021) with treatment effects that are constant across individuals and periods (Chaisemartin and D'Haultfoeuille 2020).

DATA AND MODEL SETUP

This section introduces the data and empirical setup that will be used to estimate the impact of income and unemployment on redistribution preferences. The data are sourced from the British Election Study (BES). Specifically, the Combined Waves 1-14 Internet Panel dataset is used. This is a fourteen-wave internet panel survey that was fielded between February 2014 and May 2018 by YouGov. Around 30,000 respondents were interviewed in each wave, whereas each cross-section is nationally representative of British citizens. Respondents below the age of 18 are dropped in the empirical analysis.

The BES Internet Panel is chosen because it offers individual-level panel data that cover a large sample, and because it includes repeated measures of all theoretically relevant variables, which unfortunately is rare. But the data have the disadvantage that only few panel waves contain all required variables⁷, namely waves 1, 10, 11, 12, and 14.⁸ This is further aggravated by limitations that relate to representativeness. After listwise deletion, only panel waves 1 and 14 retain an acceptable proportion of respondents (60% of respondents in wave 1 and 66% of respondents in wave 14), whereas the proportion of respondents left after listwise deletion in waves 10-12 is miniscule.⁹ This only leaves waves 1 and 14 for the analysis, spaced about four years apart.

A further analysis of panel dropout shows that 22% (6,866 from 30,590) of respondents who participated in wave 1 were re-surveyed in wave 14 on all theoretically relevant variables, indicating considerable attrition. These numbers may seem alarming, but 6,866 respondents surveyed twice over time still represents a substantial number of observations in the context of a panel regression. Furthermore, an analysis suggests that dropout is random. Tables A2.1-A2.3 in Appendix B show that mean values of socio-economic characteristics remain almost completely unaffected by dropping respondents within and over panel waves. This indicates that the smaller sample remains roughly representative.

The dependent variable is the demand for redistribution. Respondents were asked to what extent they agree with the statement ‘the government should redistribute income from the better off to those who are less well off’ on a 5-point scale. It is recoded to range from zero to one where higher values indicate stronger support. Therefore, the effects of independent variables can be interpreted in terms of percentage point changes.

The first independent variable is gross household income, which is recorded on a 15-point scale where each value represents a different income range (e.g., £20,000 to £24,999). Each respondent is assigned the midpoint of their respective range (e.g., £22,500).¹⁰ Using gross household income conforms to the theoretical argument behind self-interest approaches. Income should be gross rather than net of taxes because net income already factors in the redistributive impact of taxation, and household income rather than personal income is relevant because

⁷ These include the dependent variable: the demand for redistribution; the explanatory variables: income and employment; as well as matching and control variables: age, gender, and education. Other included variables (vote intention, perceived unemployment risk) are not included here because they are not required in the matching procedure for the analysis of unemployment.

⁸ The main reason for this inconsistent data coverage is that some concepts are simply not measured in a particular wave. Relatedly, some concepts are measured only periodically in the sense that, e.g., over five panel waves respondents only indicate information about their working status the first time they participate in the panel over these five waves.

⁹ The proportions of respondents left after listwise deletion are: 11% (wave 10), 5% (wave 11), and 2.5% (wave 12). These low proportions occur because some concepts are only measured the first time a respondent participates in the panel between, e.g., waves 6 to 12. The unfortunate implication of this design is that even when respondents participate in the same wave, it is not guaranteed that they all respond to the same items.

¹⁰ Respondents in the highest category are assigned an income of £175,000, which keeps the spacing constant compared to the preceding category.

income is shared within households.¹¹ However, the income variable would preferably measure market income instead of gross income since gross income includes transfers, i.e., one of the levers used to implement redistribution. Such fine-grained information is unfortunately not available, but it must be noted that this is generally not the case in panel datasets where redistribution preferences are also surveyed.

The DID regressions rely on income changes between panel waves 1 and 14 to define different treatment and control groups. The control groups are always constituted by 1,551 respondents with the same income in both waves.¹² These are compared to treatment groups who experience positive or negative income changes of varying sizes. Four different treatment groups consist of respondents who increase/decrease their income by 20% or more (observed in 1,487/761 respondents) and 40% or more (910/388 respondents).

The second independent variable is unemployment. Respondents were asked to indicate what best describes what they did last week. The empirical analysis of unemployment’s effect relies on categorical comparisons between those in employment (both full and part time) and the unemployed. The DID regressions use the following treatment and control groups. A first analysis compares those who move from employment to unemployment (observed in 33 respondents) to those who are employed over both waves (2,445 respondents). A second DID analysis compares those who (re-)enter employment from unemployment (67 respondents) to those who are unemployed in both waves (33 respondents). Furthermore, respondents’ employment situation is also used as a control and matching variable with four categories: full time employed, part time employed, unemployed, and not part of the labour force.

Two additional variables are used as time-varying controls. Education (recorded in six categories) is included because increasing one’s education can affect rational considerations regarding redistribution as well as the normative stance due to socialization effects (Bullock 2021; Gelepithis and Giani 2022). Respondents’ perceived unemployment risk (five categories) is included because it can affect future-oriented utility optimization (Cusack et al. 2006; Rehm 2009, 2011). Furthermore, the variables age, gender, and intended vote choice (‘And if there were a UK General Election tomorrow, which party would you vote for?’) are used in the matching procedure.

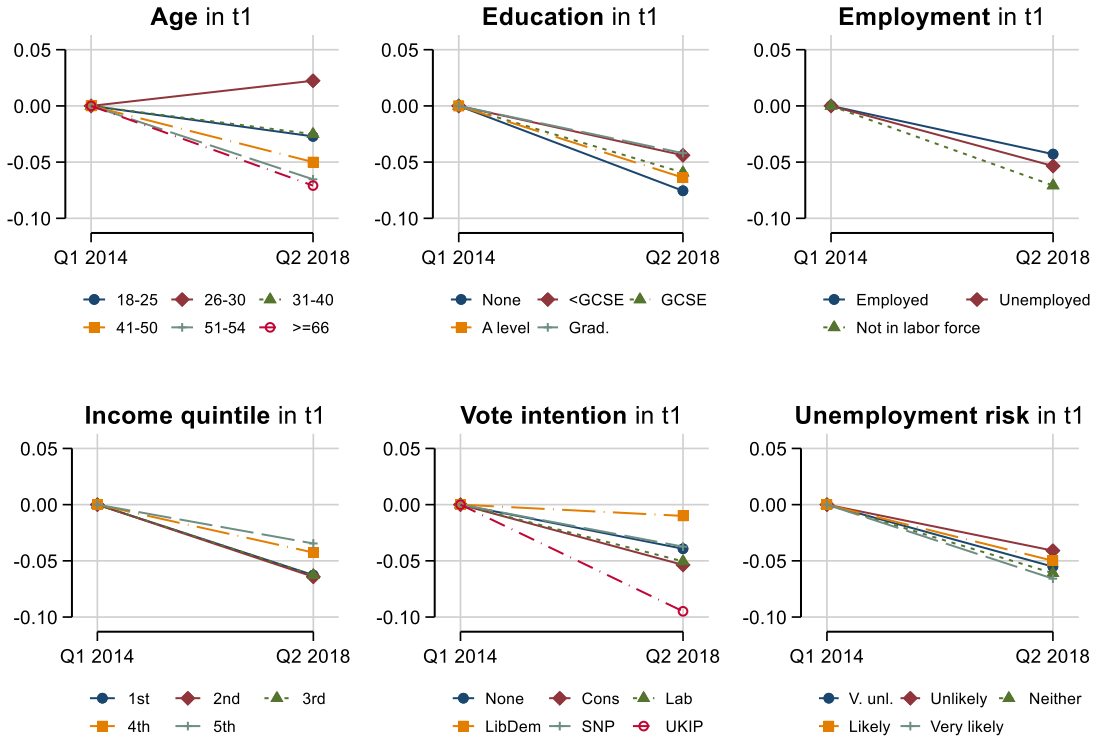
The empirical models are set up in the following way. The time-varying controls education, employment situation, and perceived unemployment risk are included in the analysis of income’s effect on redistribution preferences (all in categorical form). In the assessment of unemployment’s effect, in contrast, only education is used as a control to avoid overcontrol bias (Elwert 2013) – other time-varying characteristics (income and unemployment risk) are part of the causal chain that links unemployment to redistribution preferences.

The matching weights for the DID regressions are estimated using pre-treatment (i.e., wave 1) values of the following characteristics: age, gender, education, income, employment situation, and perceived unemployment risk. In the assessment of the effect of income, it is also possible to match on respondents’ vote intention. This is unfortunately not possible in the analysis of unemployment’s effects since the models use less observations, for which no appropriate matching weights could be found by the entropy balancing algorithm. The procedure is specified so that the variables’ first three moments (mean, variance, skew) are balanced between the treatment and control groups. More detailed information on the exact matching setup of the different

¹¹ The spouse of a rich individual, for example, benefits from their partner’s income and should adjust their preference accordingly. Furthermore, income is often taxed at the household rather than the personal level, but this is not the case in the United Kingdom.

¹² It must be noted that respondents with ‘constant income’ may experience a degree of income change. For example, a respondent within income category ‘£20,000-£24,999’ in both waves may experience income variation within this range.

FIGURE 3.2: Development of redistribution preferences by period one characteristics



Note: The figure shows the mean development of redistribution preferences between panel waves 1 and 14 in the BES Internet Panel (by respondents' characteristics observed in wave 1). The sample is restricted to respondents who are part of at least one estimation sample of the main analyses reported in the results section.

DID analyses is available in Table A2.4 in Appendix B. Furthermore, Figures A2.1-A2.6 compare the pre-matching distributions of included variables between the different treatment and control groups. They illustrate the necessity of applying matching because the variables' distributions are often unbalanced between the treatment and control groups.

Before turning to the empirical analysis, let me reflect on the period under study, i.e., the four years between 2014 and 2018. Firstly, considerable time passed between the two surveys, which is a potential shortcoming. It must be assumed that no time-varying characteristics which are causally related to material circumstances and redistribution preferences are left uncaptured by the matching weights and time-varying controls. However, people's lives can develop considerably in the span of four years, which includes changes such as household exit, marriage, and occupation switches. I acknowledge this weakness but expect that the empirical approach is well-equipped to deal with temporal heterogeneity because it involves careful estimation of counterfactual states via weighted DID. Secondly and relatedly, a monumental political event transpired during the observation period, namely the EU referendum. The result was an increasing polarization of the British population, pitting the young against the old, progressives against conservatives, the better against the worse educated, etc. This may well have been accompanied by divisions regarding redistribution support, for example because willingness to provide for others via redistribution depends on social cohesion (Magni 2021; Shayo 2009).

Figure 3.2 depicts the mean development of redistribution preferences by observed wave 1 values of the matching variables. The figure suggests that British politics experienced turmoil in the study period. Redistribution preferences were all but stable; on average, there is a

remarkable within-individual downward trend in redistribution support of about 5 percentage points. At the same time, there is heterogeneity around this overall trend. In line with the political divisions following Brexit, the starkest differences can be observed between respondents with different age and vote intention. For example, younger people tended to have flat or even increasing redistribution support while older respondents (strongly) decreased their support. This cursory analysis indicates that it is vital to ensure that the analysis remains unbiased by these trend imbalances. The weighted DID approach is therefore particularly fitting for the data at hand.

RESULTS

This section assesses the effects of income and unemployment on redistribution preferences with several regression models. The results of multiple models will be presented for each independent variable to facilitate a comparison between different approaches. Firstly, cross-sectional estimates based on wave 1 data are reported because cross-sections continue to be most common in the literature.¹³ Secondly, the results from TWFE models are reported because this is the standard approach in previous longitudinal analyses. As aforementioned, TWFE also enjoys advantages regarding precision vis-à-vis the weighted DID approach, which is why it makes sense to report TWFE results alongside weighted DID. Thirdly, the main results are derived from weighted DID models. All panel models use robust standard errors clustered by individuals that remain valid under serial autocorrelation and heteroskedasticity.

Income

Figure 3.3 presents the results of regressions using income as the explanatory variable (full regression results are available in Appendix B). The cross-section and TWFE model use income in logged form due to its skewed distribution. The coefficients are rescaled so that they indicate the effect of increasing one's income by 50% to roughly bring them on the same scale as the DID coefficients.¹⁴

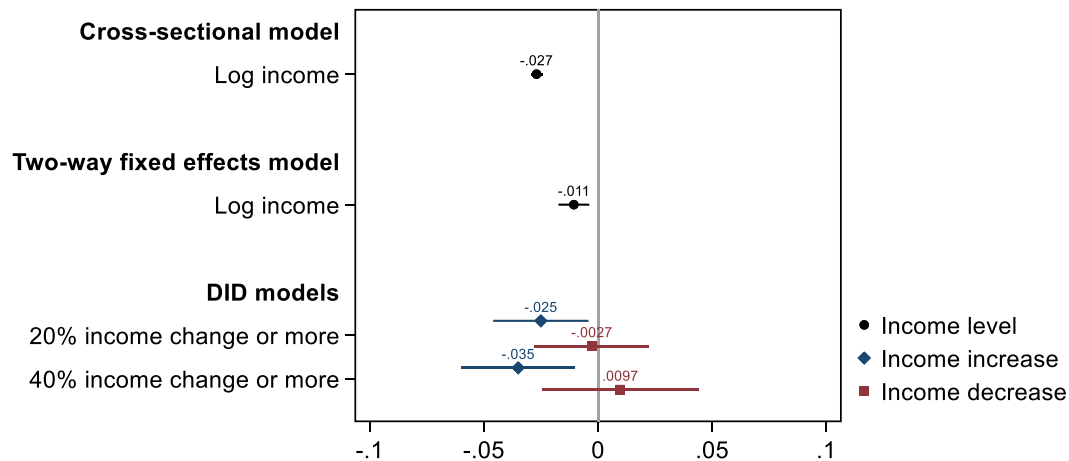
Figure 3.3 shows that the cross-section and TWFE model estimate a significantly negative effect of income, which is in line with the expectations of self-interest theory. The TWFE estimate is much smaller compared to the cross-section: it indicates that people reduce their redistribution support by about 1 percentage point after experiencing a 50% income increase, whereas the cross-sectional estimate is almost three times as large. This suggests that addressing unobserved heterogeneity with longitudinal models leads to a much smaller effect estimate. However, it must be noted that the effect estimates cannot be compared directly because they rely on different estimation samples.

The DID models assess the effects of income increases and decreases of varying magnitude separately. They have intriguing results. In line with self-interest theory, income increases are estimated to negatively affect redistribution support, whereas increasing the magnitude of income gains exacerbates this effect. Depending on the specification, the DID models predict preference decreases of 2.5 to 3.5 percentage points following upwards income shifts. Income decreases, on the other hand, are estimated to have null effects. Both coefficients are insignificant and substantially small.

¹³ The cross-sectional models include gender, age, and age squared in addition to the controls outlined in the data and model setup section (education, employment, and unemployment risk).

¹⁴ The median income changes of the different treatment groups are: +44% (20% increase or more treatment), -40% (20% decrease or more treatment), +67% (40% increase or more treatment), and -54% (40% decrease or more treatment).

FIGURE 3.3: The effect of income on redistribution support



Note: The coefficients of the cross-section and 2WFE model show the estimated effect of increasing one's income by 50%. The horizontal bars represent 95% confidence intervals. The complete results are available in the online appendix.
 N(Cross-section)=16,865; N(2WFE)=10,250 [n=5,125]
 N(20% incr. DID)=6,076 [n(treated)=1,487; n(control)=1,551]; N(20% decr. DID)=6,104 [n(treated)=761; n(control)=1,551]
 N(40% incr. DID)=4,922 [n(treated)=910; n(control)=1,551]; N(40% decr. DID)=5,184 [n(treated)=388; n(control)=1,551]

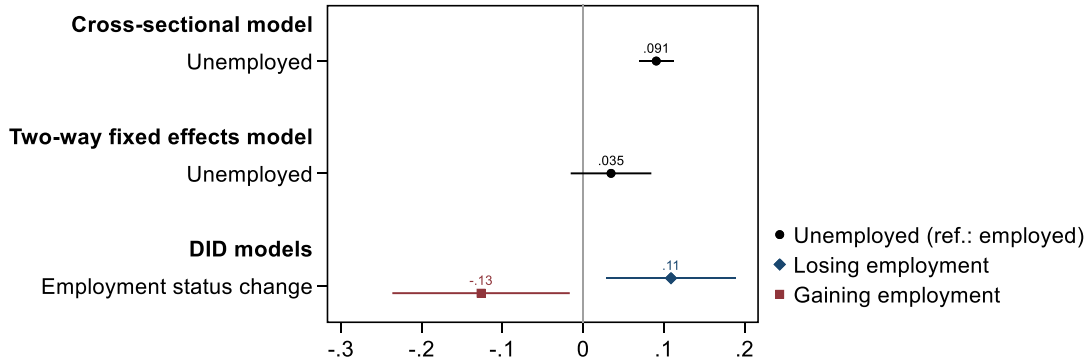
Overall, the different modelling approaches agree that income and redistribution preferences are negatively related. However, income is estimated to exert an asymmetric effect. Individuals only adapt their preferences according to self-interest when they experience income increases, which contradicts the expectations of self-interest theory that assumes full rationality. This suggests that the effects estimated from conventional panel models misrepresent the relationship between income and redistribution support because they do not distinguish between temporal increases and decreases of income.

The four DID regressions are re-estimated several times to gauge the robustness of the results (full results are available in Appendix B). Firstly, treatment groups are derived from absolute income changes instead of using changes expressed in percentages. Positive and negative income shocks of £10,000 or more and £20,000 or more are assessed. The corresponding control groups are identical to the main specifications. The results replicate. Secondly, percentage changes in *equivalized* household income are used to define the treatment and control groups.¹⁵ The treatment groups are given by those who increased/decreased their equivalized household income by 20% and 40% or more. Equivalization was not applied in the main specifications because household size does not necessarily affect the self-interest calculus regarding redistribution.¹⁶ However, the robustness test comes with a caveat. Information on household size is unavailable in panel wave 14 of the BES, which is why wave 10 is used instead as the second period for weighted DID. Unfortunately, all time-varying control variables but unemployment risk are either unavailable or only rarely available in wave 10, which is why only unemployment risk can be

¹⁵ Equivalence income scales aim to facilitate the comparison of consumption potential between households with different composition because households with more members benefit less from a given income amount than households with less members. Equivalization is applied by dividing household income by the square root of household members.

¹⁶ Household size does not inherently affect the relationship between market income and expected tax burden/transfer income. When the household size increases, e.g. due to a newborn child or older relative becoming a household member, using an equivalence scale would downscale household income. It is unreasonable to expect that, as a result, rational household members will increase their demand for redistribution. Their income decreases simply due to the equivalization; the amount of taxes the household pays does not necessarily decrease due to the 'lower' income, and the amount of transfer income it receives does not necessarily increase.

FIGURE 3.4: The effect of unemployment on redistribution support



Note: The coefficients of the cross-section and 2WFE model show the estimated effect of being unemployed relative to being employed. The estimation samples are restricted to respondents who are either employed or unemployed in both panel waves. The horizontal bars represent 95% confidence intervals. The complete results are available in the online appendix. N(Cross-section)=13,595; N(FE/2WFE)=7,236 [n=3,618] N(Losing employment DID)=4,586 [n(treated)=33; n(control)=2,258]; N(Gaining employment DID)=200 [n(treated)=67; n(control)=33]

included. Nevertheless, the results of the main specification replicate.¹⁷ This is also reassuring because the robustness test confirms the main results based on a different estimation sample, namely waves 1 and 10 rather than 1 and 14, between which less time passed (i.e., 2.5 years).

Unemployment

Figure 3.4 presents the results of regressions using unemployment as the explanatory variable (full regression results are available in Appendix B). The samples are restricted to those who are currently employed or unemployed. The cross-section indicates that unemployment increases redistribution support by nine percentage points, which is a large effect. The TWFE model, in contrast, estimates a comparably small (3.6 percentage points) and insignificant effect. Following the standard longitudinal approach would therefore lead us to believe that unemployment is rather inconsequential for individuals’ redistribution support.

The DID models, which rely on a more credible estimation strategy, paint a different picture. Firstly, unemployment is estimated to increase redistribution support, and the point estimate is *even larger* than the cross-sectional estimate (11 percentage points), although it must be noted that the confidence intervals are wide. Secondly, gaining employment is estimated to decrease redistribution support by 13 percentage points, which is also a large effect. I conclude that unemployment has a symmetrical and strong effect on redistribution preferences. This finding is in line with expectations of a narrow self-interest orientation, where individuals rationally increase redistribution support after losing employment and withdrawing support after (re-)gaining employment.

DISCUSSION AND CONCLUSION

This study assessed the claim that redistribution preferences depend on individuals’ material circumstances, specifically unemployment and income. It was argued that there are plausible theoretical arguments for effect asymmetry, implying that improvements and deteriorations of material circumstances have non-corresponding effects. An empirical evaluation was conducted

¹⁷ This is not a major surprise because unequivalised and equivalised household income are highly correlated ($r=.93$).

using longitudinal data from Great Britain and a modelling approach that aimed to facilitate asymmetric effect estimation with minimal bias.

The results show that, firstly, income is negatively related with redistribution preferences, which coincides with several previous studies analyzing longitudinal data (Gidron and Mijs 2019; Margalit 2013; Owens and Pedulla 2014). But in contrast to previous research, I find that this effect is asymmetric. Only income increases are estimated to affect redistribution support, whereas income decreases result in null effects. This finding may help to explain why previous longitudinal estimates generally resulted in smaller effects than cross-sectional estimates: the presence of asymmetric effects can muddy average effects estimated in longitudinal designs. Secondly, unemployment is estimated to have a symmetric and strong effect on redistribution preferences, where losing employment bolsters redistribution support to about the same extent as gaining employment reduces it. These results coincide with the general finding from related literature that unemployment increases the demand for redistribution (Owens and Pedulla 2014; Pahontu 2022; but see Wehl 2019). Furthermore, they mirror Margalit’s (2013) finding that unemployment has a symmetric effect but contrast with Naumann et al. (2016), who find that only job loss exerts an effect.

Overall, there is now considerable evidence speaking for a causal effect of income and unemployment on redistribution preferences, which implies that political economists’ characterization of humans as ‘rational’ (i.e., self-interested) is at least partially appropriate. However, the finding that only income increases affect redistribution support suggests that it is reasonable to characterize humans as only boundedly rational. Individuals have imperfect access to information and do not necessarily consider all relevant information in preference formation. Therefore, individual preferences do not narrowly follow the predictions of self-interest theory.

This study informs a broader debate on the relative explanatory power of normative dispositions and self-interest (see Margalit 2019 for an overview). Wehl (2019) and O’Grady (2019) advocated the position that the relationship between material circumstances and political preferences is confounded by normative predispositions rooted in socialization experiences. In contrast, the results of this study show substantially important effects of both income and unemployment. These results are derived from within-individual changes and are therefore unbiased by stable characteristics such as normative predispositions.

It must be noted that this study also has several limitations and that further research on bounded rationality and effect asymmetry is required. Firstly, it remains unclear to what extent the theorized mechanisms explain the asymmetry in the effect of income. Under the assumption that negativity bias drives asymmetry, it can be theorized that the increased effective tax rate due after an income increase is highly salient, which ‘rationally’ depresses redistribution support. Information about decreased tax burden after an income loss, on the other hand, is less salient and thus not considered in preference formation. However, there is no way to test the validity of this argument using the data at hand. Secondly, the analyzed panel waves are spaced four years apart, during which temporal heterogeneity may have developed that cannot be captured by the weighted DID approach. It must be noted that the empirical approach is comparatively well-equipped to deal with this shortcoming, and that a robustness test was conducted using waves that are spaced only 2.5 years apart. But it would be preferable if the analyzed panel waves were temporally closer.

Lastly, the generalizability of the findings is unclear. The empirical estimates returned average treatment effects on the treated (ATTs), and it is unclear how BES respondents *not* subjected to changing material circumstances would have reacted to income and employment

shifts.¹⁸ Furthermore, the generalizability across countries is also unclear. The analyzed data only pertain to Great Britain, and it is unclear whether effect asymmetry also exists outside its socio-political context. This question can only be answered by further research.

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¹⁸ A difference in treatment effects may arise, for example because of compositional differences between those with stable and changing circumstances. However, the evidence presented in Figures A6.1–A6.6 in Appendix B suggests that, while treated and untreated respondents do differ in their socio-economic background, the differences are not substantial. There is thus no strong evidence for compositional differences.

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Labor market risks and welfare preferences: a bounded rationality approach (Paper C)

Abstract

This study introduces a bounded rationality approach to welfare preference formation under exposure to labor market risks. It expects that risk exposure only increases welfare demand when it is reasonable to assume that workers are aware of their risk exposure and when future-related concerns are currently salient. An empirical analysis of longitudinal data from Switzerland and 28 European countries supports the theory. Swiss workers only optimize their welfare preferences in a forward-looking manner when they become unemployed, and only the national unemployment rate is found to increase welfare demand in European countries. In contrast, a variety of risks on the occupational and individual level drawn from previous research are found to be unrelated with welfare preferences. The implication is that the risk exposure of employed workers may matter less for their welfare preferences and downstream political phenomena such as voting behavior than commonly expected.

INTRODUCTION

One of the primary functions of the welfare state is to insure against risk (Moene and Wallerstein 2001). It redistributes income from those who generate income on the market to those who are less able to do so. Thereby, the welfare state stabilizes individuals' income over time and shelters against the loss of market income, for example caused by unemployment or old age (Jensen 2012).

Political economists expect that people are well-aware of the insuring function of welfare, and that they finely condition their support of the welfare state according to their current risk exposure. Guided by the usual self-interest assumption, it can be expected that those who experience more risk should demand more welfare because the probability is higher that they will become beneficiaries of welfare policies. Research primarily focused on exposure to labor market risks, above all regarding the probability of unemployment.

A voluminous body of work identified different risks that should affect demand for welfare. A first literature strand focuses on *occupational risks*, which derive individual risk exposure from aggregate characteristics of workers' occupations: occupational unemployment (Cusack et al. 2006; Rehm 2009, 2011), skill specificity (Iversen and Soskice 2001), the probability of job automation and digitization (Busemeyer and Sahm 2022; Dermont and Weisstanner 2020; Gallego et al. 2022; Thewissen and Rueda 2019), as well as globalization exposure (Walter 2010, 2017). A second literature strand focuses on *individual risks*, i.e., characteristics of individual workers such as atypical employment contracts (Burgoon and Dekker 2010; Marx 2014). Lastly,

unemployment, a *realized risk*, should be influential in preference formation (Ahrens 2022a; Cusack et al. 2006; Margalit 2013; Naumann et al. 2016; Pahontu 2022; Rehm 2011).

This study introduces a refined theoretical framework on the implications of labor market risk for welfare preferences that addresses the strong rationality assumptions underpinning common risk-focused theories. I argue that workers are boundedly rational, suggesting that they are not necessarily aware of the objective labor market risks they are exposed to. Furthermore, even when they are aware of their risk exposure, they do not necessarily consider this information in the formation of welfare preferences. While workers principally follow their material self-interest, labor market risks only shape welfare preferences when the risks are readily observable and when workers are primed to consider their future material wellbeing.

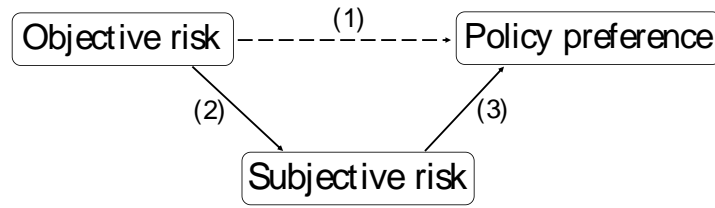
The empirical expectations of the bounded rationality framework are that especially the realized risk unemployment and to a lesser extent individual risks such as temporary employment should be influential in preference formation. These risks can be easily observed, and concerns about the future are especially salient in the case of unemployment. Occupational risks, in contrast, are difficult to observe and exposure to them does not prime workers to consider their future welfare. Bounded rationality theory therefore expects null relationships with policy preferences.

The theoretical expectations are largely confirmed with quantitative analyses of two datasets with more credible causal identification assumptions compared to previous research. Previous research almost exclusively relied on cross-sectional data, which are prone to omitted variable bias. In contrast, I use longitudinal data to assess whether over-time changes in labor market risks within individuals and occupations are related to welfare support. I first use individual-level panel data from the Swiss Household Panel (SHP) and evaluate whether different objective risk factors are related to perceived risks, and whether different risks (both perceived and objective) are related to welfare preferences. The results show that occupational risks such as occupational unemployment are only inconsistently and weakly related to perceived economic risk, while workers exposed to individual risks such as fixed-term contracts consistently feel threatened. Furthermore, only the realized risk unemployment consistently increases demand for welfare. The risks the currently employed are exposed to (occupational risks, individual risks, and even perceived risks) leave welfare preferences unchanged.

I then analyze repeated cross-sections from 28 European countries provided by the European Social Survey (ESS). The aim is to validate the findings on occupational risk exposure in a multi-country analysis. The analysis follows a similar empirical strategy as the SHP analysis since it assesses within-occupational changes in risk exposure over time (rather than within-individual changes). The results suggest that occupational unemployment and skill specificity are longitudinally unrelated to perceived risk, redistribution preferences, and support of unemployment assistance. However, the *national* unemployment rate increases demand for unemployment assistance, which is consistent with bounded rationality theory because national unemployment is widely reported and politicized.

This study suggests that risk can be important in the formation of welfare preferences, but this is not always the case. Only realized unemployment and the national unemployment rate are found to increase welfare demand. In contrast, the risk exposure of employed workers, especially on the occupational level, may matter less than commonly expected. This finding runs counter to a large literature on the political implications of labor market risks. Its substantial implication is that political phenomena related to policy preferences such as voting behavior or policymaking may also depend less on the distribution of risks among the employed than various theories suggest (e.g., Abou-Chadi and Kurer 2021; Marx 2014; Rehm 2011).

FIGURE 4.1: Causal mediation model of risk-focused self-interest theory



LABOR MARKET RISKS AND POLICY PREFERENCES

Theoretical argument

Political economists typically expect that individuals follow their material self-interest in their demand for redistributive welfare policies. In particular, people aim to optimize their disposable income. The most immediate implication of this argument is that there should be a negative relationship between current market income and support for redistributive policies (Meltzer and Richard 1981). But it can also be theorized that people optimize their *future* income, which implies a negative relationship between expected income and support for redistributive policies (Alesina and La Ferrara 2005; Becker 2021; Benabou and Ok 2001).

A large literature on the implications of labor market risks employs the future-oriented self-interest approach. It argues that workers use the welfare state to insure against possible market income loss in the future. Those who experience more risk for income loss due to un- or under-employment already demand more welfare in the present (Alt and Iversen 2017; Iversen and Soskice 2001; Marx 2014; Rehm 2009).

The theory can be broken down into a causal mediation model, in which the relationship between risk exposure and preferences is mediated by individual risk perceptions (see Figure 4.1). People have an objective risk exposure (i.e. the *actual* probability of negative income shocks) as well as a subjective risk exposure (i.e. the *perceived* probability of negative income shocks). As arrow 1 indicates, people are theorized to adapt their policy preferences directly to their objective risk exposure. However, I argue that workers can only act in response to risks they are aware of, i.e., their subjective risk (Anderson and Pontusson 2007; Cusack et al. 2006; Gallego et al. 2022; Walter 2010, 2017). Therefore, objective risk first influences subjective risk (arrow 2), which then impacts policy preferences (arrow 3). This mediation via subjective risk is only acknowledged by part of the literature, but I argue that it is in the spirit of self-interest theory. Put simply, one must know about a risk to insure against it via welfare.¹

Literature overview

The literature identified several objective risks that should affect welfare preferences. It can be broadly categorized into studies focusing on *occupational risks*, which derive risk exposure from aggregate attributes of workers' occupations; *individual risks*, which derive risk exposure from the attributes of specific workers and their jobs; and *realized risk* in the form of unemployment (c.f. Marx and Picot 2020; Vlandas 2020). These studies will be discussed consecutively.

¹ Some studies discuss that there may be alternative causal pathways between objective risk and preferences, such as self-esteem (Gallego et al. 2022; Walter 2010). While these pathways may exist, they do not reflect the utility optimization argument made by risk-focused theories.

The first occupational risk is *occupational unemployment* (Alt and Iversen 2017; Cusack et al. 2006; Rehm 2009, 2011; Vlandas 2020). The argument is that workers can most easily switch between jobs in the same occupational category because human capital and social networks are tied to occupations. Therefore, higher unemployment within an occupation indicates higher risk exposure, which should be associated with higher demand for social protection. This is supported empirically by all studies cited above. Cusack et al. (2006) also show that occupational unemployment correlates with perceived job insecurity, which supports the causal mediation model displayed in Figure 4.1.

The second occupational risk is *skill specificity*. Iversen and Soskice (2001) argue that workers who invested in specific skills tied to their occupations are at higher risk of being unable to find work appropriate for their skills in the case of unemployment. Their higher expected cost of unemployment leads workers to demand more social protection from the government. Several studies offer empirical support (Cusack et al. 2006; Iversen and Soskice 2001; Rehm 2009). Cusack et al. (2006) also show that skill specificity correlates with perceived job insecurity.

The third occupational risk relates to *technological risk*. Workers who perform routine tasks must fear that their job may be replaced by automation or digitalization, and they should demand more welfare as a result. Accordingly, Gallego et al. (2022) show that workers performing more routine tasks feel more insecure, and several studies find a positive relationship with welfare and redistribution preferences (Busemeyer and Sahm 2022; Dermont and Weisstanner 2020; Thewissen and Rueda 2019). However, two experimental studies find no effect of technological risk primes on support for compensatory welfare policies (Gallego et al. 2022; Zhang 2019).²

The fourth occupational risk is *globalization exposure*. Those who face a higher probability of losing their job due to job offshoring or international trade should demand more protection (Genschel 2004; Rodrik 1998). Walter's (2010, 2017) empirical analysis supports this argument based on an assessment of all paths of the causal mediation model. Workers in tradable industries and in jobs that can more easily be offshored report higher subjective insecurity and they demand more redistribution and welfare.

I now turn to individual risks that relate to worker-specific characteristics (Marx and Picot 2020). Firstly, workers with *atypical employment contracts*, i.e., those deviating from permanent full-time employment, are exposed to more risk and should demand more social protection. In support of this argument, two studies find a positive relationship between temporary employment and different welfare preferences (Burgoon and Dekker 2010; Marx 2014). The evidence on part-time employment is mixed, with Burgoon and Dekker (2010) finding positive effects on perceived insecurity and support for public unemployment benefits but Vlandas (2020) finding no effect on welfare preferences. Furthermore, Pahontu (2022) finds that workers who either work part-time, have low job tenure, or have low subjective job security demand more social protection. Secondly, individuals with *recent unemployment* are exposed to more risk because unemployment spells hinder future employment possibilities. Accordingly, Green et al. (2001) find that past unemployment spells increase subjective insecurity, and Burgoon and Dekker (2010) find that they also strengthen welfare demand.

Lastly, *unemployment* should be pivotal in preference formation. Unemployment is conceptually different because it is a realized risk, whereas the literature surveyed so far focused on the probability of not-yet-realized events. Unemployment therefore suits present-oriented self-interest theory, with an effect on welfare preferences that is transmitted via unemployment-induced loss in current market income. However, unemployment not only decreases current

² While Gallego et al. (2022) find no effect of a technological risk prime on support for compensatory welfare policies, they do find that it increases support for slowing down technological advancement.

market income but also puts future market income at risk. Becoming unemployed decreases the probability and quality of future employment. Firstly, employers place a penalty on unemployment spells in hiring and compensation, presumably because human capital depreciates. Secondly, at some point the unemployed may be forced to settle for employment that is incongruent with their skills or desired working hours, for example because welfare benefits expire. The unemployed should therefore demand more welfare even after accounting for their income loss, which is supported empirically by several studies (Cusack et al. 2006; Margalit 2013; Naumann et al. 2016; Pahontu 2022; Rehm 2011; Vlandas 2020).

Critique

I argue that the literature on labor market risks and welfare preferences has two shortcomings. Firstly, the theoretical argument relies on strong and partly unrealistic rationality assumptions. Individuals must be aware of the objective labor market risks they face, and they must be able and willing to derive welfare preferences that optimize their *future* welfare from this risk exposure. This requires considerable sophistication that cannot consistently be expected from humans. It is well known that information required for utility optimization is often not present (Engelhardt and Wagener 2018; Jensen and Zohlnhöfer 2020) and that human decision-making is less profound than political economy models suggest (Jones 1999; Simon 1985). To be sure, it is uncontroversial that human rationality is limited; political economists use full rationality assumptions to simplify their models, and it is unlikely that any theorist places high faith in full rationality. But as I will argue, when one takes the limits to rationality seriously, the empirical implications of labor market risks only hold under limited conditions. Therefore, their effects on political preferences should be less consistent than previously suggested.

Secondly, the bulk of previous empirical evaluations relied on cross-sectional data, implying that causal effects are possibly not identified. Cross-sectional estimates rely on covariate-adjusted comparisons of welfare preferences between people exposed to more and less risk. However, it is not difficult to imagine that the stronger welfare support of those exposed to more risk can be attributed to other causes. Individuals' normative predispositions and their labor market trajectories are shaped by common forces. Most importantly, it has been shown that socialization during childhood and early adolescence can introduce major bias to cross-sectional evaluations of self-interest theories (Jungkunz and Marx 2022; O'Grady 2019; Wehl 2019).

To tackle these shortcomings, this study introduces a refined theoretical approach to labor market risks and policy preferences that acknowledges the limits to human rationality. The theoretical implications are then tested in an empirical analysis that evaluates all steps of the causal mediation model depicted in Figure 4.1. It uses longitudinal data, which allows for more credible causal effect identification compared to the usual cross-sectional approach.

PREFERENCE FORMATION UNDER BOUNDED RATIONALITY

Theoretical framework

Self-interest theory on labor market risks requires the following assumptions: (a) people care about their future disposable income; (b) workers possess information about the objective labor market risk they face as well as information about welfare policies and how they affect their beneficiaries; and (c) workers use the information they possess to optimize their future disposable income in their policy preferences.

I argue that these assumptions only hold under certain conditions because humans are only boundedly rational (Jones 1999; Simon 1985). Bounded rationality theory expects that humans are indeed rational; they have goals and they behave in a way that is appropriate in a given

situation to achieve these goals. In specific, I continue to expect that workers are interested in receiving and maintaining disposable income, and that they adapt their policy preferences accordingly (assumption a). However, workers' rationality is bounded, i.e., limited. They possess incomplete information (assumption b) and an imperfect ability to utilize their information (assumption c). As a result, preferences are often not utility-optimizing.

Firstly, workers do not necessarily know about the objective labor market risks that they face, and they do not always possess information about the functioning of welfare programs (Jensen and Zohlnhöfer 2020). The central driver of this information deficiency is that humans only store a limited amount of information because their attention is highly limited (Simon 1985). It is crucial that it is not my aim to argue that workers possess no relevant information; rather, information availability differs significantly between various labor market risks and welfare programs. Generally, I expect that workers are more likely to possess information on risks and the alleviating impact of welfare programs when the information is easily observable and associated with little uncertainty.

Secondly, workers do not necessarily consider all relevant information in preference formation. Preference formation is a complex process, which humans simplify by using heuristics. One such heuristic is that humans only consider a limited amount of information in a decision (Simon 1985). What information is considered depends on what information is currently salient. The implication is that even when workers principally know about their risk exposure, they do not necessarily use this information to optimize their policy preferences. This is an especially important point because self-interest theory expects that workers optimize their *future* welfare, which requires substantial and often unrealistic attention to detail. I expect that future-related concerns are salient in preference formation when (a) a specific labor market risk is currently a politicized issue or (b) when it is abundantly clear for an individual worker that their future material wellbeing is at stake (i.e., workers face a pronounced economic risk that is clearly cushioned by the welfare state).

Theoretical expectations for different labor market risks

This section applies the bounded rationality framework to the different labor market risks identified in the literature. I begin with what labor market risks workers should be especially aware of (in other words, the relationship between objective and subjective risk). This relationship is important because workers must be aware of their risk exposure to conduct future-oriented utility optimization. As aforementioned, workers should be especially aware of their objective risk exposure when information is readily available and associated with little uncertainty.

I expect that workers should be most aware of their exposure to the *realized risk* unemployment and to *individual risks* such as temporary employment. Such worker-specific characteristics are readily observable and directly relevant for individuals' particular circumstances. In contrast, it is uncertain to what extent workers are aware of their exposure to *occupational risks* (occupational unemployment, skill specificity, technological risk, and globalization exposure). These are substantially important risks, but they are difficult to observe and assess because they relate to aggregate characteristics of the labor market. For example, unlike the national unemployment rate, occupational unemployment rates are not reported and cannot be observed directly.

I now move on to the relationship between subjective risks and demand for welfare. The bounded rationality framework expects that being aware of a risk is not sufficient to boost welfare demand. Future-related concerns must also be salient during preference formation.

My expectation is that primarily the *realized risk* unemployment affects welfare preferences. Workers are primed to consider their future material circumstances because (a) their future

income-generating capabilities are clearly at peril and (b) they can see clearly that welfare cushions this risk. In contrast, the effects of employed workers' risk exposure should be less consistent because they are less primed to consider their future welfare and the alleviating impact of social policies in preference formation. I expect that especially the *individual risks* have potential to spur welfare demand. Above all, temporary employment conveys to respective workers that they may require public transfer income in the future. The *occupational risks*, however, less clearly jeopardize future income, and the differential exposure to these risks between occupations does not tend to be widely politicized. I therefore expect that occupational risks are unrelated to welfare preferences.

Overall, I expect that labor market risks only inconsistently affect welfare preferences. This is because workers may not be aware of their risk exposure and/or because future-related concerns are not salient in preference formation. Especially the realized risk unemployment and to a lesser extent the individual risks should increase welfare demand. Occupational risks, in contrast, should leave preferences unaffected.

INDIVIDUAL-LEVEL EVIDENCE FROM SWITZERLAND

This section presents the data, methods, and results of an empirical analysis of individual-level panel data from Switzerland. It evaluates to what extent welfare preferences in Switzerland depend on a variety of risks: occupational risks, individual risks, the realized risk unemployment, as well as subjective risks. The section presents disaggregated analyses of all pathways of the causal mediation model depicted in Figure 4.1: the relationship between objective and subjective risk, between objective risk and preferences, and between subjective risk and preferences.

Data

I use data from waves 1-19 of the Swiss Household Panel (SHP), which is a stratified random sample of Swiss households that is representative of residents in Switzerland. The data were collected between 1999 and 2017, including two sample refreshments in 2004 and 2013. I retain data from respondents in dependent employment or unemployment aged between 16 and 65 years. Overall, I use 78,676 observations from 15,274 individuals (depending on the analyses, more or less of these observations are used due to listwise exclusion).

The SHP data are ideal to test the theoretical expectations because they contain extensive and high-quality measures of labor market participation, income, and welfare preferences. Furthermore, the SHP is a long-run panel that allows for precise effect estimation by virtue of the sheer amount of observations available for analysis. Lastly, Switzerland is a well-suited case to test the theoretical expectations because it is an open economy with weak employment protection, implying that workers' incentives to use the welfare state to insure against risk are high.

Welfare preferences. The first dependent variable measures general social spending preferences. Respondents were asked: “[a]re you in favor of a decrease or in favor of an increase in federal social spending?”, which they could respond to on a three-point scale: “in favor of an increase”, “neither”, or “in favor of a decrease”.

Using federal social spending preferences may be criticized because the most important welfare program used to insure against labor market risks, the unemployment insurance, is primarily financed via compulsory insurance payments in Switzerland. However, the federal government subsidizes the expenditures and absorbs financial shocks of the system. During the Coronavirus response, for example, expenditures financed by public subsidies exceeded those financed from insurance contributions. The federal social spending item is therefore a suitable measure.

I additionally use an item that specifically surveys preferences regarding unemployment benefits. Respondents were asked whether the government should spend less, the same amount, or more on unemployment benefits. This item is ideal because, as aforementioned, the most relevant welfare payments insuring against labor market risks are unemployment benefits. The downside is that the item is available in only three survey waves (13, 16, and 19).

Occupational risks. This section introduces the occupational risk measures, which are sourced from multiple datasets and then merged to the SHP data based on the ISCO88 classification of workers' occupations. Further information on the exact calculation of these occupational risks are available in Appendix C.

Occupational unemployment quantifies unemployment within occupations (Cusack et al. 2006; Rehm 2009, 2011). The rates are estimated from Eurostat Labor Force Surveys (LFS), which are available on a yearly basis between 1999 and 2009 and a bi-yearly basis from then on. They are calculated by assigning unemployed workers to the occupation of their previously held job and then calculating the share of unemployed workers in occupations. Following Rehm (2009), the main analyses rely on one-digit ISCO codes, but sensitivity tests include other possible specifications (two- and three-digit codes).

Skill specificity is measured with Iversen and Soskice's (2001) first relative skill specificity measure, which is also used in other studies (Cusack et al. 2006; Rehm 2009). It quantifies how many sub-categories an occupational category has relative to the share of the labor force working in that occupational category as well as the skill level of the occupation. The indicator "is high if an individual is in a very specialized occupation, but has relatively low levels of education or skills" and it "is low if the occupation is not very specialized, while the level of education or skills is high" (Cusack et al. 2006: 371). Following previous studies, the main analyses use two-digit ISCO codes, but sensitivity tests also use one-digit codes. The required labor force shares are, again, estimated from LFS data.

Technological risk is measured with the routine task intensity (RTI) indicator by Autor and Dorn (2013), which is commonly used in the literature (Dermont and Weisstanner 2020; Gallego et al. 2022; Thewissen and Rueda 2019). RTI measures how often workers perform routine tasks relative to manual and abstract tasks. The rationale is that routine tasks are most easily automated, implying that a higher intensity of routine tasks is associated with higher risk. Inspired by Sebastian (2018), I estimate the RTI of occupations in Switzerland from the European Working Conditions Survey (EWCS). The main analyses use two-digit ISCO codes while sensitivity tests also use one-digit codes and further breakdowns by industry. The EWCS contains two samples for Switzerland from 2005 and 2015, which are pooled to reach acceptable sample sizes per occupational category. RTI is therefore constant over time within occupational categories. The exact calculation of RTI differs slightly from Autor and Dorn (2013) and Sebastian (2018); full details are available in Appendix C.

Globalization risk is measured with the job offshorability indicator by Blinder (2009), which is also used by Walter (2010, 2017). It indicates the potential for a job to be moved abroad because it can be performed from distance without jeopardizing product or service quality. The indicator is available for the US Labor Department's Standard Occupational Classification (SOC), which I convert into ISCO codes. As RTI, the indicator is constant over time for occupational categories. Unfortunately, further globalization risk measures (notably, industry trade exposure) are not feasible because the SHP only has a broad categorization of workers' industries.

Subjective, individual, and realized risks. I now move on to risk measures that are available in the SHP data. Subjective risk is measured with two items. Firstly, respondents were asked how they evaluate the risk of becoming unemployed within the next 12 months on a scale from

zero (no risk at all) to ten (a real risk). Secondly, respondents were asked whether their job is very secure, quite secure, a bit insecure, or very insecure.³ Together, these items measure subjective labor market risks comprehensively. The first item indicates whether a respondent may be without a job altogether and the second item whether they may lose their current job.

Next, I use the following dummy variables to measure further individual-level risks. A temporary employment dummy captures whether a respondent holds a fixed-term employment contract (Burgoon and Dekker 2010; Marx 2014). A job tenure dummy indicates whether a respondent has been with their current employer for less than one year. The rationale is that low job tenure is associated with less employment security (Pahontu 2022). A third dummy captures whether a respondent is employed in the private rather than the public sector because private sector employment tends to be more flexible (Anderson and Pontusson 2007). Fourthly, an unemployment experience dummy captures whether respondents were unemployed during the previous year. The rationale is that unemployment spells indicate that future unemployment is more likely (Green et al. 2001). Lastly, the realized risk unemployment is measured with a dummy indicating whether respondents are currently unemployed.

Control variables. I control for the highest level of education on a four-point scale (no or primary, secondary, post-secondary non-tertiary, tertiary). I also control for income because it must be excluded that individuals derive their welfare preferences from their current and not their expected future income. I include both personal gross work income and equivalized household disposable income.⁴ It is common to control for household income because it is the best measure of all-around material wellbeing. I additionally use personal work income because, according to self-interest theory, present-oriented utility optimization is conducted based on market income gross of taxes and transfers (Meltzer and Richard 1981). Disposable household income, however, includes transfer income such as unemployment benefits.⁵ Both income variables are used in categorical form, specifically as income quintiles.⁶ The motivation is that quintiles reliably measure relative income levels over time, impervious to inflation, and that no functional form assumptions are required in regression modeling.

Method

I use ordinary least squares (OLS) to estimate two-way fixed effects (TWFE) regression models that include fixed effects for both individuals and panel waves:

$$y_{it} = \sum_{k=1}^K \beta_k \text{risk}_{it}^k + \sum_{m=1}^M \delta_m \text{control}_{it}^m + \alpha_i + \gamma_t + \epsilon_{it},$$

where y_{it} denotes the dependent variable (either welfare preferences or perceived risk) of individual i in wave t , risk_{it}^k a number of risk variables (comprised of either the subjective risks, occupational risks, or individual risks), control_{it}^m a number of control variables, α_i individual-specific constants, γ_t time-specific constants, and ϵ_{it} the residual.

TWFE accounts for unobserved confounders (i.e., common causes of risks and preferences). The approach solely assesses variation within individuals and therefore remains unbiased by any time-invariant characteristics such social background. Furthermore, the time fixed effects

³ Respondents also had the fifth response category “temporary” in waves 1-4, which I merge to the category “very insecure”.

⁴ Equivalization is applied by dividing household income by the square root of household members.

⁵ For example, when an individual becomes unemployed, they should demand more welfare because their current market income drops substantially and not only because their future income is in peril. Using the gross labor income variable captures this. The household income variable, in contrast, does not necessarily decrease substantially because a considerable proportion of previous labor income may be substituted by social security transfers.

⁶ The quintiles are estimated from the SHP data separately for each panel wave.

remove potential bias from unobserved confounders that individuals in particular panel waves are jointly subjected to (for example, the recession after the financial crisis, which affected both labor market risks and policy preferences [Limberg 2020]).

TWFE allows for more credible effect identification compared to the usual cross-sectional approach. Using TWFE trades one problem for another in the sense that TWFE can be biased by time-variant confounders instead of time-invariant confounders, as in the cross-sectional case. However, I expect that time-varying confounding poses less of a threat to identification. Firstly, there is stronger theoretical indication speaking for the presence of time-invariant confounders. In particular, political preferences and labor market profiles are jointly shaped during early-life socialization, which is difficult to capture via control variables (O’Grady 2019; Wehl 2019). Secondly, time-variant confounders are easier to capture even when they are unobserved because TWFE includes time fixed effects, which account for shared influences such as the aforementioned financial crisis. It must be noted that TWFE comes with an additional set of assumptions that are unlikely to be fully met in real world data.⁷ While a degree of bias must be expected, I expect that TWFE estimates more accurately reflect true causal effects than cross-sectional estimates.

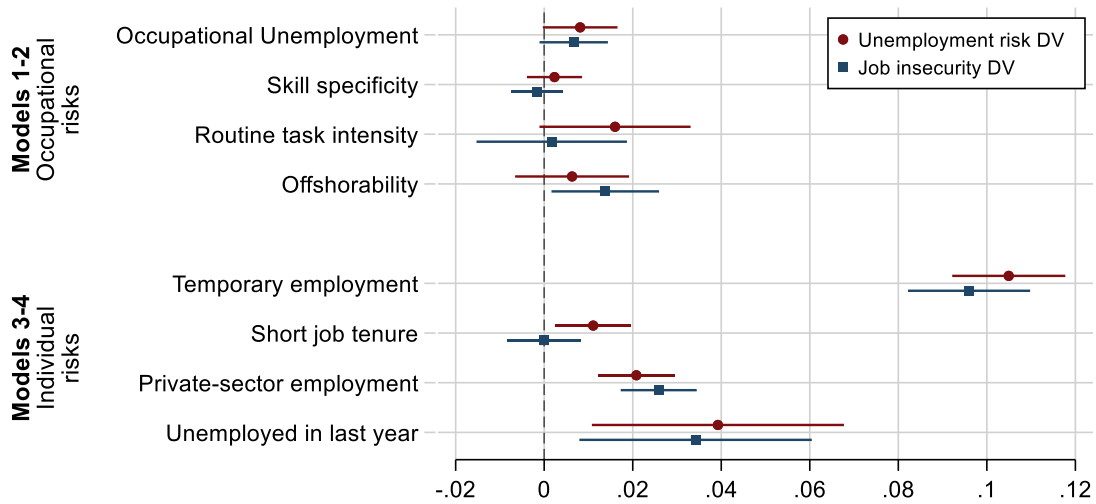
TWFE assesses within-individual variation in risk exposure over time, and it is important to note that such variation emerges for different reasons. Most variation originates from respondents taking up different employment (e.g., by switching from public to private sector employment). The occupational risks occupational unemployment and skill specificity also vary within occupational categories over time, implying that respondents’ risk exposure can vary even when they hold the same job as before. However, larger variations are primarily driven by occupation changes.

Analyzing variation in risk that mostly stems from job changes may spur doubts regarding the occupational risks because it could be assumed that people rarely change their occupational category. However, this is not the case. About 40% of all individuals in the analysis sample change their occupation at some point in the panel, and the median probability of a within-individual occupation change from one observation to the next is about 9%. Nevertheless, it must be noted that the effect estimates rely to a large part on job changers.

I now move on to how I rescale all variables to allow for meaningful comparison of effect estimates across variables and models. The dependent variables (welfare preferences and perceived risk) are always on a scale from 0 to 1, where higher values indicate stronger welfare demand or perceived risk. The independent variables on continuous scales are divided by two standard deviations, while the dummies are left as is. Therefore, all regression coefficients indicate how many percentage points the dependent variable is expected to change when the independent variable increases by two standard deviations (continuous independent variable) or by one (dummy independent variable). This approach makes the effects of dummies and continuous variables roughly comparable (Gelman 2008).

A last point to consider is that several of the occupational risks (occupational unemployment, skill specificity, and RTI) contain measurement error because they are estimates or use estimates in their calculation. For example, occupational unemployment rates are first estimated from LFS data and then used as an independent variable with SHP data. This measurement error must be reflected in the stage-two regressions. I rely on an imputation approach inspired by Blackwell et al. (2017). I treat the estimated values as missing and impute them 100 times using random draws from distributions that are informed by the point estimates and their sampling

⁷ TWFE requires linear-additive effects (Imai and Kim 2021) and a correct specification of causal dynamics over time (Plümper and Troeger 2019). Furthermore, the strict exogeneity assumption also requires that that past outcomes affect neither the current outcome nor the current treatment (Kim and Imai 2019).

FIGURE 4.2: The effect of objective risk on subjective risk

Note: The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by individuals. The control variables (education, personal income, household income) as well as both individual and time fixed effects are included in the regressions. Occupational unemployment is measured on the ISCO88 one-digit level, skill specificity and routine task intensity on the two-digit level, and offshorability on the four-digit level. The full results are available in Appendix C.

uncertainty. This introduces additional spread into the data informed by the amount of measurement error. For example, imputed values of occupational unemployment are on average equal to the calculated rates, but individual values deviate less or more from the point estimates depending on the occupational sample size in the LFS data. All regressions containing imputed risk variables are estimated 100 times, and their results are combined using Rubin's rule. More detailed information on the calculation of the respective occupational risks and their imputation is available in Appendix C.

Results

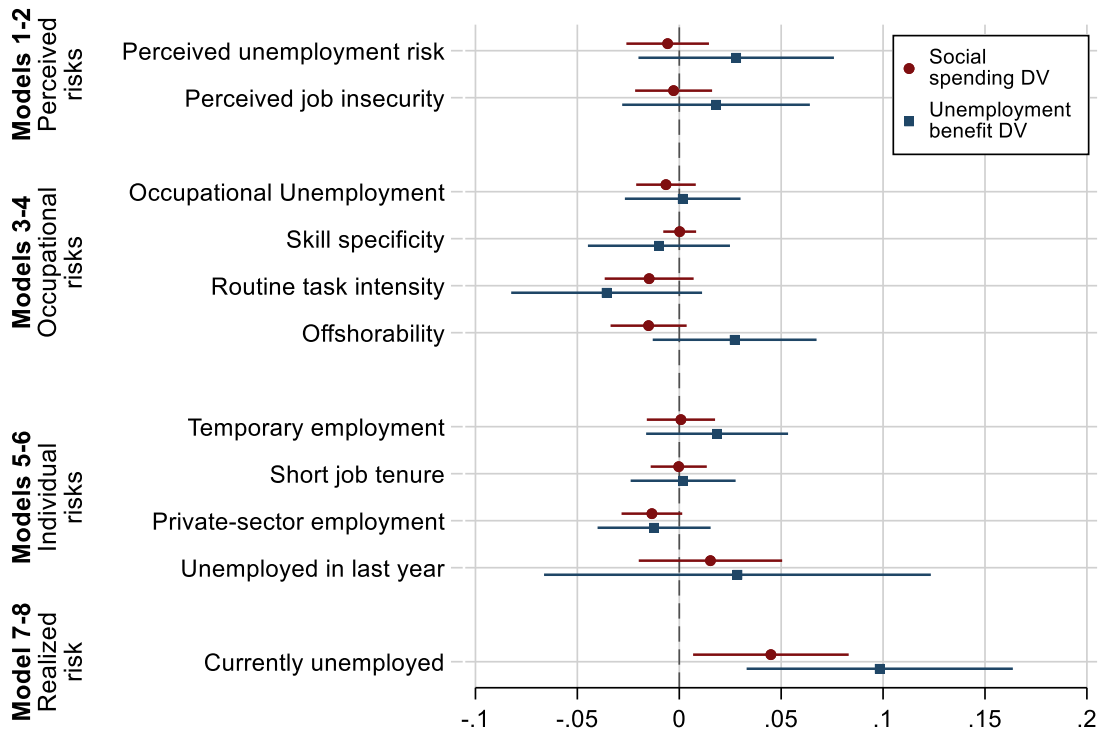
This section presents the results of an empirical analysis of all causal pathways in the mediation model (see Figure 4.1). It first evaluates whether individuals' perceived labor market risk depends on their objective risk exposure. The analysis then shifts to explanations of welfare preferences: do people adapt their preferences to their risk exposure?

The relationship between objective and subjective risks. I first assess the implications of different objective risks for subjective risks. The results of four regression models are displayed in Figure 4.2. The first two models jointly assess the effects of occupational risks on the two subjective risk perceptions, and the last two models jointly assess the effects of individual risks. The motivation of including the occupational and individual risks in common models is that they may be correlated among each other, but results from models containing only one risk variable at a time are available in Appendix C.

The results from the first two models show that workers only marginally adapt their subjective risk to their occupational risk exposure. The different occupational risks exert positive but mostly small effects on subjective risks. Effect sizes reach a maximum effect size of about two percentage points, but most coefficients are smaller. Furthermore, in most cases the null hypothesis cannot be rejected.

In contrast, the third and fourth models show that individual risks substantially increase perceived risks. The strongest effect is that of temporary employment, which increases subjective

FIGURE 4.3: The effects of risks on welfare preferences



Note: The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by individuals. The control variables (education, personal income, household income) as well as both individual and time fixed effects are included in the regressions. Occupational unemployment is measured on the ISCO88 one-digit level, skill specificity and routine task intensity on the two-digit level, and offshorability on the four-digit level. The full results are available in Appendix C.

risk by about 10 percentage points. The other individual risks have a less substantial size, but they are also mostly positive and significant, with effect sizes reaching up to four percentage points. The divergence makes sense from a theoretical perspective because temporary employment is bound to end, which gives an unambiguous signal to workers that they are exposed to risk. Short job tenure, private-sector employment, and recent unemployment spells also signify risk, but to a lesser extent.

Overall, the findings are consistent with theoretical expectations. Changes in individual risks over time (especially temporary employment) are positively associated with perceived risk, whereas the findings on occupational risks are weaker and inconsistent. This matches bounded rationality theory because workers should be best able to derive their risk exposure from directly observable individual attributes rather than more obscure aggregate characteristics of the labor market.

The relationship between risks and preferences. This section assesses the effects of all risks on welfare preferences. Figure 4.3 presents the results of eight regression models using either social spending or unemployment benefit preferences as the dependent variable. Models 1-2 evaluate the effects of subjective risks, models 3-4 of occupational risks, models 5-6 of individual risks, and models 7-8 of the realized risk unemployment. Note that the analysis of unemployment is unique because the regression model includes both employed and unemployed workers,

whereas all other analyses solely assess the implications of risks experienced by employed workers. This includes the subjective risk variables, which are only surveyed from employed workers.

The results show that none of the risks employed workers face affect welfare preferences. The effect estimates for perceived, occupational, and individual risks for both dependent variables are all insignificant. The point estimates also tend to be near zero.

In contrast, models 7-8 show that the realized risk unemployment substantially and significantly increases demand for welfare. The point estimates suggest that unemployed workers increase their social spending demand by about five percentage points and their unemployment benefit demand by about 10 percentage points. Especially the latter finding indicates a substantial effect of unemployment. However, it must be noted that the confidence intervals are wide in the analysis of unemployment benefit preferences because the variable is available in only three panel waves.

Overall, the results mostly align with bounded rationality theory. Unemployment is found to increase welfare demand while all risks faced by employed workers (even including subjective risks) leave preferences unaffected. This is consistent with bounded rationality theory because risk exposure should only affect welfare demand when (a) it can be assumed that workers are aware of their exposure to risks, and (b) future-related concerns are salient in preference formation. This is definitely the case when workers are unemployed. Regarding the other risk types, it is striking that even subjective risks have no effect on policy preferences. However, this is consistent with bounded rationality theory because knowledge of one's risk exposure is not sufficient to boost welfare demand; future-related concerns must also be currently salient, which is not guaranteed. The only finding that appears somewhat inconsistent with theoretical expectations is that even temporary employment has null effects because temporary employment is an unambiguous risk that may prime future-related concerns. The empirical analysis suggests that this is not the case.

Sensitivity analyses. Several robustness tests check the results' sensitivity (the full results are available in Appendix C). Firstly, the main regressions estimate the effects of different risks of the same type (e.g., occupational risks) jointly in one model. In the full regression tables in Appendix C, the effects of all risks are re-estimated only entering one risk variable at a time per regression. This has the advantage that the estimates become more precise due to less collinearity and that more observations can be used because listwise deletion does not limit the samples to individuals with available information on all risks. This approach changes the results only marginally and leaves all inferences intact.

Secondly, there are several approaches to quantifying the occupational risks depending on how fine-grained occupations are measured: at the ISCO one-, two-, or three-digit level. RTI can also be further broken down by industry (Sebastian 2018). Additional robustness tests use all possible specifications of occupational risks. Again, the main results replicate.

A last set of robustness checks uses logged version of occupational unemployment and skill specificity because the variables tend to have right-skewed distributions, especially when more fine-grained ISCO-classifications are considered. These robustness checks also use a version of the RTI indicator that, in line with Autor and Dorn (2013), uses logged task frequency variables in its calculation (see Appendix C for details on why the main analyses do not use logged frequencies). Once again, all robustness tests lead to similar results.

MACRO-LEVEL EVIDENCE FROM 28 EUROPEAN COUNTRIES

This section presents the data, methods, and results of an empirical analysis of survey data from 28 European countries.⁸ It addresses one of the main criticisms one could raise against the SHP data analysis: it is unclear to what extent the findings from Switzerland apply to other countries. Here, I present additional evidence on two occupational risks, namely occupational unemployment and skill specificity. I show that the results hold in a multi-country analysis, and conduct further research on the implications of risk exposure on the national level.

Data

I use data from waves 1-9 of the European Social Survey (ESS), which is a high-quality multi-country survey used extensively in related studies (Busemeyer and Sahm 2022; Rehm 2009; Thewissen and Rueda 2019; Walter 2017). I retain respondents in dependent employment aged 16-65 and merge data on their occupational unemployment and skill specificity based on the ISCO88 classification. These occupational risks are estimated for all available countries and years from Eurostat Labor Force Surveys (LFS). The resulting dataset contains 108,778 observations from 176 country-waves and 28 countries.

I use three dependent variables. The first variable measures perceived unemployment risk via an item that asks how likely it is that respondents will be unemployed and looking for work in the next year. The second and third dependent variables measure policy preferences. Support of public unemployment assistance is measured with an item that asks whether it should be the government's responsibility to ensure a reasonable standard of living for the unemployed, and support of redistribution with an item that asks whether the government should take measures to reduce income inequality. Data on redistribution preferences are available in all ESS waves, while the other two dependent variables were only surveyed in waves 4 and 8. I prefer the unemployment assistance item as a measure for policy preferences because unemployment is the primary labor market risk that workers are theorized to insure against. However, I also include the redistribution item because welfare programs protecting against risk are inherently redistributive. Furthermore, the item is commonly used in related research (e.g., Rehm 2009) and it has vastly superior availability across ESS waves.

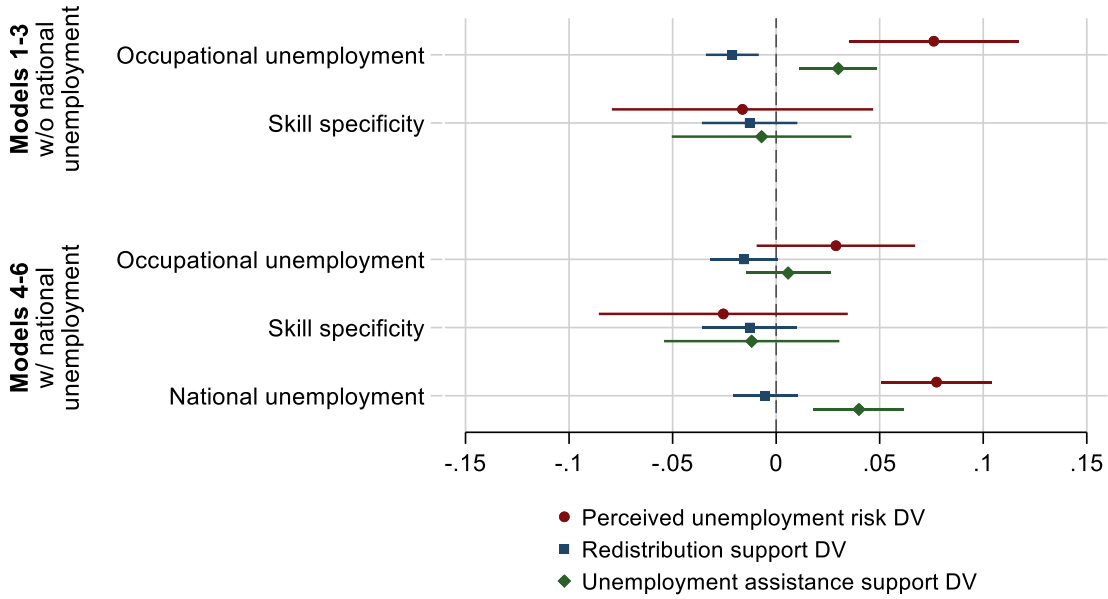
Method

Analyzing multiple ESS waves for each country (i.e., repeated cross-sections) makes it possible to use a within-occupation estimator to address bias from unobserved heterogeneity between occupations, which leads to more credible evidence than cross-sectional comparisons. The estimator assesses to what extent within-occupational changes in occupational risks and policy preferences are related over time. If classical self-interest theory on labor market risks is correct, then the average welfare support held by specific occupations should increase when their occupational unemployment and skill specificity rises over time. The advantage of this approach is that stable differences in policy preferences between occupations cannot bias the estimates. This follows a similar identification strategy as the previous analyses, which assessed within-individual rather than within-occupational changes. Again, it is noteworthy that the analyzed variance stems from (a) changing unemployment rates and skill specificity⁹ over time and (b) the changing composition of occupations (i.e., people switching in, out of, and between occupations).

⁸ Austria, Belgium, Croatia, Cyprus, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Iceland, Ireland, Italy, Latvia, Lithuania, Luxembourg, Netherlands, Norway, Poland, Portugal, Slovakia, Slovenia, Spain, Sweden, Switzerland, and United Kingdom.

⁹ Changes in skill specificity result from occupations' varying employment shares over time (see Appendix C for details on the calculation of skill specificity).

FIGURE 4.4: Effects of labor market risks in 28 countries



Note: The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by country-occupations (ISCO88 1d). Country-occupation and time fixed effects as well as the controls education years, household income quintiles, gender, age, and age squared are included in all regressions. Occupational unemployment and skill specificity are both measured on the ISCO one-digit level. The full results are available in Appendix C.

I estimate the following regression model with OLS:

$$y_{ioct} = \sum_{k=1}^K \beta_k occrisk_{oct}^k + \sum_{m=1}^M \delta_m control_{ioct}^m + \alpha_{oc} + \gamma_t + \epsilon_{ioct},$$

where y_{ioct} denotes the dependent variable (perceived unemployment risk, redistribution preferences, or unemployment assistance preferences) of individual i in occupation o , country c , and ESS-round t ; $occrisk_{oct}$ denotes occupational risks (occupational unemployment and skill specificity), $control_{ioct}$ several individual-level controls, α_{oc} country-occupation fixed effects, and γ_t ESS-round fixed effects. Due to the fixed effects structure, the regressions assess whether individuals in specific occupations change their redistribution support over time when their occupational risk exposure changes (relative to other country-occupations at specific points in time). I use gender, age, age squared, the number of full-time years in education, and household income quintile dummies as individual-level controls. The standard errors are clustered by country-occupations. Consistent with the analyses of SHP data, all dependent variables are put on a scale from zero to one and all independent variables are divided by two standard deviations. The regressions also take account of the measurement error of the estimated risk variables using the same imputation approach as in the SHP analysis (see the SHP method section above and Appendix C for additional details).

Results

Figure 4.4 displays the results of six regression models. The first three models assess the effects of occupational unemployment and skill specificity (both on the ISCO one-digit level) on the three dependent variables (perceived unemployment risk, redistribution preferences, unemployment assistance preferences). The last three models additionally include the national unemployment rate as an independent variable. The motivation is to check whether perceptions and

preferences really depend on occupational unemployment rather than on the national headline rate because there is considerable correlation between the two variables ($r = .62$).

The results of the first three models suggest that occupational unemployment has a positive impact on perceived unemployment risk and unemployment assistance preferences, which matches the theoretical expectations, but a negative impact on redistribution support, which counters theoretical expectations. Furthermore, skill specificity is found to be unrelated with all three dependent variables. All estimated coefficients are small and insignificant.

The results of the last three models show, however, that perceptions and preferences largely depend on the national unemployment rate rather than occupational unemployment. The national unemployment rate has a positive relationship with perceived unemployment risk and unemployment assistance support but no impact on redistribution preferences. In contrast, the effects of occupational unemployment become smaller and insignificant once national unemployment is included in the regression. Therefore, the ESS data again suggest that risks on the occupational level and policy preferences are unrelated.

Appendix C contains the results of several robustness tests that support these findings. Using ISCO two-digit specifications of occupational unemployment and skill specificity suggests that occupational unemployment increases perceived risk even when controlling for national unemployment, but an effect on policy preferences remains absent. Furthermore, individual regressions where only one risk variable is included at a time leads to similar results as the main analyses. I also show that the positive effect of national unemployment remains when a tailored statistical specification is used (fixed effects and error clustering on the country rather than the country-occupation level).

Overall, the results validate one of the main results from the SHP data in a multi-country analysis: employed workers do not seem to optimize their policy preferences according to their exposure to occupational risks. The ESS data rather suggest that employed workers partly adjust their preferences to the national unemployment rate. As unemployment rises, workers feel more threatened and demand stronger unemployment assistance with no concurrent effect on redistribution preferences.

CONCLUSION

This study argued that workers are boundedly rational. They follow their material self-interest, but they are ill-equipped with information on their labor market risk exposure, and they do not necessarily consider their risk exposure in preference formation. Labor market risks primarily influence welfare preferences when (a) information on risk exposure is readily available and (b) future-related concerns are made salient.

The empirical analysis of long-run panel data from Switzerland and repeated cross-sections from 28 European countries supported these expectations. Firstly, workers consistently feel threatened when they are exposed to individual-level risks such as temporary employment because these risks are easily observable. In contrast, risks on the occupational level are uncertain and difficult to observe, which is why they were found to only weakly and inconsistently affect perceived risk. There is, however, evidence suggesting that employed workers adjust their perceived risk to the national unemployment rate. This finding is in line with bounded rationality theory because, unlike unemployment on the occupational level, national unemployment rates are widely reported.

Secondly, unemployment is the only factor that is found to increase welfare demand. The most consistent finding is that workers adapt their welfare preferences when their risk is realized, i.e., when they are currently unemployed. This is consistent with theoretical expectations

because the unemployed face an unambiguous risk, which makes future-related concerns salient. Furthermore, employed workers increase their demand for unemployment assistance when the national unemployment rate rises (but not their redistribution demand). These findings are consistent with bounded rationality theory because rising unemployment tends to be a politicized issue, which primes workers to think about their future welfare. Also, strengthening unemployment assistance more clearly insures against unemployment compared to increasing redistributive efforts, which explains the divergence in findings between the two dependent variables.

All other risks employed workers are exposed to (individual risks, occupational risks, and even subjective risks) are found to be inconsequential for preferences. This makes sense from a bounded rationality perspective because risk exposure is not sufficient to boost welfare demand; workers must also be primed to consider their future welfare in preference formation. In my view, only the non-finding regarding temporary unemployment appears somewhat inconsistent with this perspective.

This study also has limitations that must be considered in the interpretation of its results. Many findings are based on a single country (Switzerland), and the longitudinal estimates draw much of their analyzed variance from individuals who changed their job or even their occupation over time. These job- and occupation-changers may differ in some unknown way from those in steady jobs and occupations. Both limitations suggest that it is unclear to what extent the results can be generalized across the workforce and across countries. However, I argue that there is no indication *per se* to doubt the results' external validity. In any case, the findings contrast with the expectations of self-interest theory that relies on strong rationality assumptions.

With these limitations in mind, this study suggests that labor market risks only affect policy preferences under advantageous conditions, and that the risk exposure of currently employed workers may matter less in preference formation than commonly expected. The substantial implication of this argument concerns political phenomena that are related to policy preferences. Several political science theories rely on the validity of a causal link between workers' risk exposure and their policy preferences. For example, Abou-Chadi and Kurer (2021) argue that risk exposure leads to radical right voting, Marx (2014) argues that risk exposure increases the probability left party voting, and Rehm (2011) argues that welfare generosity negatively depends on the inequality of risks. The theoretical justification of all contributions is that economic risk structures policy preferences, which in turn determines voting behavior and policymaking. If the causal link between risk and preferences is inconsistent, as this study suggests, such theoretical arguments should be constrained as well.

Overall, the research field will benefit from carefully assessing the validity of risk-focused theories in future research. It is noteworthy that all studies so far that deviate from the cross-sectional approach are in line with the findings from this study. Realized unemployment increases welfare and redistribution support (Ahrens 2022a; Margalit 2013; Naumann et al. 2016; Pahontu 2022), but studies have so far been unsuccessful in finding positive effects of experimental risk primes (Gallego et al. 2022; Zhang 2019) and within-individual changes in perceived unemployment risk (Margalit 2013; O'Grady 2019). It will therefore be beneficial to continue research the effects of labor market risks with methods that are less prone to omitted variable bias in the future, such as further panel studies or experiments.

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Theorizing the impact of fairness perceptions on the demand for redistribution (Paper D)

Abstract

Prior research shows that fairness judgements regarding the income distribution have a substantive impact on redistribution preferences. Those who perceive incomes as unfair demand more redistribution. However, the association is undertheorized in previous studies. This article adds to the literature by offering a comprehensive theoretical explanation of why incomes are perceived as unfair and how this influences the demand for redistribution. Based on equity theory from social psychology, it is argued that individuals develop a preference for redistribution if they consider their own income and incomes in general to be disproportional to relevant exchanged inputs. They assess proportionality by using social comparisons with observable reference groups such as colleagues, family members, or other labor market participants. Multilevel models with survey data from 39 diverse countries support this theory. Individuals who perceive their own income as disproportional in comparison to their efforts and those who perceive incomes in general as disproportional demand more redistribution. These findings have several implications for research on political economy and social policy. Most importantly, they explain the inconclusive results of empirical tests of rational choice theories such as the median-voter hypothesis.

INTRODUCTION

The demand for redistribution (DFR), i.e., the extent to which people support redistributive policies by the state, has received extensive attention from political scientists, economists, and sociologists. While rational choice explanations dominated the disciplines in the past (Meltzer and Richard 1981), recent research stresses the importance of normative beliefs. A mounting body of empirical research shows that fairness perceptions affect redistribution preferences irrespective of an individual's income. Those who believe that incomes are attained unfairly in their country, e.g. due to luck or nepotism, show greater support for redistribution from the top to the bottom; and those who believe that effort and skill determine incomes tend to show less support (Alesina and Giuliano 2010; Alesina and La Ferrara 2005; Corneo and Grüner 2002; Fong 2001; Fong et al. 2005; Iglesias et al. 2013; Kuhn 2010; Linos and West 2003; Miles 2014).

Overall, there is a strong consensus that fairness perceptions affect redistribution preferences. What remains is a lack of theoretical understanding of the association. Previous research is based on the contention that perceived unfairness leads to a higher DFR because redistribution offsets unfair outcomes, but there has been no comprehensive theoretical explanation as of yet. When exactly citizens perceive income distributions as unfair and how this influences

redistribution preferences is treated like a black box. Therefore, this article presents a unified theoretical framework that explains which economic outcomes are seen as unfair and how this relates to redistribution preferences.

Based on equity theory from social psychology, I argue that individuals view their income as an exchange where relevant inputs, i.e., skills and efforts, are traded for the income. They perceive earnings as unfair if these inputs are disproportional to the income. To assess the value of the inputs, individuals compare themselves and others to reference groups such as colleagues, friends, and family members. If they have the feeling that their own income is disproportional or incomes in general are disproportional to relevant inputs, they develop a demand for redistribution to compensate the unfairness.

The theoretic presumptions are substantiated by a quantitative analysis of cross-national survey data. Using the 2009 *Social Inequality Module* of the International Social Survey Programme (ISSP), multilevel models show for the first time that people's perception of their own income strongly affects redistribution preferences. Both the perceptions that the own income is worth *more* and worth *less* than the exchanged inputs are associated with a stronger DFR. In line with prior research, perceptions of general system unfairness are also associated with a stronger DFR. Overall, this article adds to the literature by developing a comprehensive theoretic framework and demonstrating that people's perception of the fairness of their own income is of consequence to the attitude towards redistribution (and not only the absolute value of their income).

This article has several implications for research on social policy and redistribution preferences, in particular for studies that deal with inequality and labor market disadvantage. It provides an explanation of the mixed empirical support for Meltzer and Richard's (1981) median-voter hypothesis (e.g., Dion and Birchfield 2010; Finseraas 2009; Kenworthy and McCall 2008; Luebker 2007; Schmidt-Catran 2016). As noted by Luebker (2007), the effect of inequality is most likely mediated by value judgements about the determinants of inequality. Furthermore, this article explains the mechanism that leads disadvantaged workers to support pro-redistribution parties (Emmenegger et al. 2015; Marx 2014; Marx and Picot 2013). Their voting decisions are not only based on rational motivations but also on normative judgements.

My argument is structured as follows. The next section offers a comprehensive overview of prior research on fairness perceptions and the demand for redistribution. It shows that while lots of empirical research suggests an effect of fairness perceptions on redistribution preferences, there is insufficient theoretic explanation. Thus, Section 3 presents a theoretical framework based on equity theory, arguing that individuals seek to reduce disproportionalities of incomes to attributes regarded as relevant with redistribution. Section 4 outlines empirical data and methods for a quantitative analysis of this theory. Section 5 presents the results, which offer support for the theoretical expectations. Lastly, Section 6 discusses the results.

PRIOR RESEARCH

Studies about the association between fairness perceptions and redistribution preferences can be classified into two major categories. The first focuses on individual beliefs about meritocracy, while the second focuses on social mobility. Within the first approach, a number of studies analyze how beliefs on whether incomes are determined by factors within or beyond individuals' control affect redistribution preferences (Alesina and Giuliano 2010; Alesina and La Ferrara 2005; Bjørnskov et al. 2013; Corneo and Grüner 2002; Fong 2001; Fong et al. 2005; Georgiadis and Manning 2012; Iglesias et al. 2013; Isaksson and Lindskog 2009; Kuhn 2010; Linos and West 2003; Miles 2014). These studies evaluate survey questions that ask to what extent luck, family

background, race, or social ties as well as effort or skill determine incomes. It is assumed that income differences due to factors beyond the individuals' control (luck etc.) are perceived as unfair and factors within individuals' control (skill and effort) as fair. The studies expect that the perceived importance of fair (unfair) factors negatively (positively) influences the DFR. All of the studies robustly support this. Furthermore, two studies present evidence that suggests a negative correlation between just-world beliefs, i.e., being convinced that people generally get what they deserve, and the DFR (Benabou and Tirole 2006; Frank et al. 2015).

Studies from the second category analyze the influence of social mobility on redistribution preferences. Dabalén et al. (2015) and Guillaud (2013) show that persons who experienced downward mobility in their lives, e.g. due to sudden unemployment, have a stronger DFR. The association is stronger the more recent, severe, and persistent the downward mobility was. Alesina and La Ferrara (2005) cannot replicate these results but find a negative relationship between expected upward mobility and redistribution preferences. Several studies corroborate this finding (Benabou and Ok 2001; Rainer and Siedler 2008; Ravallion and Lokshin 2000). There is also research on the perception of aggregate income mobility. Jaime-Castillo and Marqués-Perales (2014: 627) conclude that “[i]ndividuals who believe that there is a high level of inequality in opportunities are more supportive of state intervention and redistributive policies and reject market values such as meritocracy and competition” (see also Page and Goldstein 2016; Shariff et al. 2016).

The empirical results regarding social mobility can be explained with two theoretical approaches. On the one hand, a perception of high social mobility lowers the DFR because respondents anticipate upward mobility and, thus, a declining utility of redistribution (see Benabou and Ok [2001] for the “POUM hypothesis”). On the other hand, experienced and perceived mobility are also indicators of procedural fairness (Piketty 1995).

Overall, there is a strong consensus that fairness perceptions affect redistribution preferences. The theoretic explanation for the association put forth or implicitly assumed by prior research is simple: citizens regard redistribution as a compensation mechanism. When the market generates unfair outcomes, people tend to support state interventions that restore outcome fairness. However, the understanding of how perceptions of the income distribution influence redistribution preferences remains insufficient because prior approaches have been too simplistic. To explain why people support redistribution, it is necessary to understand what exactly is regarded as (un)fair in the income generation process. Therefore, this article theorizes explicitly and in a unified framework what leads citizens to assess the income generation process as (un)fair and how this influences their demand for redistribution. The next section outlines the theoretic approach. Empirical support for the theory is presented subsequently.

THE INFLUENCE OF FAIRNESS PERCEPTIONS ON REDISTRIBUTION PREFERENCES

In this section, I apply equity theory from social psychology to redistribution research. The first part deals with the question of which incomes are perceived as fair. Subsequently, I outline how these fairness perceptions relate to the demand for redistribution and derive empirically testable hypotheses.

Fairness evaluations of incomes

Which incomes and income distributions are perceived as fair? Prior research shows that perfect equality cannot be the answer to this question (Lewin-Epstein et al. 2003). In a literature overview of experimental studies, Starmans et al. (2017) show that humans generally accept unequal

income distributions if the inequality results from fair processes. For example, Cappelen et al. (2010) conduct an experiment where subjects work for a real monetary output. They are free to choose the duration and intensity of their work. The results show that subjects generally perceive inequalities that arise from different effort as fair. The experiment subsequently adds exogenous variation to the monetary outputs by paying out different rewards for the subjects' efforts. The resulting inequalities are generally perceived to be unfair because the subjects had no control over the arbitrary pricing. The experiment has two implications. First, people make investments and expect appropriate returns in the form of incomes. Second, what is perceived as appropriate is sensitive to the returns achieved by other people.

These findings can be explained by the *equity theory* put forth by Adams (1965). His explanation draws from Stouffer et al.'s (1949) concept of *relative deprivation* (RD), which posits that humans do not base their satisfaction on what they receive but rather what they receive in relation to what they think they should receive. A person with high education, for example, may be unsatisfied with a lower-middle class status because he feels that he is entitled to more. Another individual with low education, in contrast, will most likely be content with such a status. RD was later defined as "the judgement that one is worse off compared to some standard accompanied by feelings of anger and resentment" (Smith et al. 2012: 203). Once again, the important implication is that fairness evaluations are based on social comparisons.

Adams (1965) formulated equity theory to formalize the intuitions behind the concept RD. He framed employment as an exchange relationship where employees give inputs and expect an output in return. On the labor market, individuals sell skills and effort (inputs) and receive an income (output) in return. The exchange is perceived as fair only if the output is proportional to all directly relevant inputs. How inputs factor into the input-output-proportionality depends on the value assigned to the inputs. Inputs such as working hours or relevant skills do not have an inherent value in terms of an appropriate monetary compensation. Humans thus use social comparisons to assign a value to inputs. Someone doing the same work as his neighbor and receiving the same pay for it will perceive his income as fair. However, if the neighbor receives more despite delivering the same inputs, he will see his income as unfair. The neighbor who receives the disproportionately high pay may also perceive the distribution as unfair in this scenario. An income distribution is therefore fair if the input-output-proportionality is distributed equally in the society. This implies that income differences resulting from relevant inputs and their value should be perceived as fair (Lewin-Epstein et al. 2003). If someone brings less or less valuable inputs to social exchanges, he should receive a worse output.

First, I theorize that individuals evaluate their own income based on its income-outcome-proportionality. Individuals know about their income and about relevant inputs such as their effort, working hours, and education. The value assigned to these inputs depends on social comparisons to reference groups with observable input-output-proportionality, which can be colleagues, family members, and other labor market participants. This theory is largely supported by empirical research. Psychological studies show that humans constantly compare themselves to their peers (Buunk and Gibbons 2007). As a cognitive heuristic, they compare themselves to others who are similar in terms of relevant characteristics such as education or effort (Clark and Senik 2010). Individual attitudes towards the own income depend on the average income of colleagues (Feldman and Turnley 2004), employees in the same industry (Verhoogen et al. 2007), family members (Liebig et al. 2011), and similar people in general (Sauer and May 2017; Shamon 2014). It is a robust finding that the own position relative to others matters (Clark and D'Ambrosio 2015; Tao 2015).

Second, I theorize that individuals evaluate general income fairness in society based on input-output-proportionality. Of course, this is more difficult to theorize because individuals do not

possess substantive information about the inputs and incomes of fellow citizens. For example, it is unclear how a person should assess the income of an unknown person within an industry she has little information on. Thus, I theorize that general fairness evaluations are imperfect and vague. They are based on diffuse evaluations of observable patterns. For example, an individual might observe that many executives from her firm come from low socio-economic backgrounds. Based on this information, she can deduct that incomes vary according to effort, which makes input-output-proportionality more likely. An individual might also observe that nepotism is influential and deduct that incomes do not vary according to relevant inputs but according to other factors that should be irrelevant. She will perceive the incomes in her society as more unfair. Unfortunately, there is no empirical research on what determines general system fairness evaluations (see Piketty [1995] for a theoretical approach).

The question remains how individuals select inputs to consider in the evaluation of input-output-proportionality. Put differently, it is not clear which factors produce inequality that is perceived as (un)fair since incomes do not only vary according to relevant inputs such as working hours but also according to other factors such as luck. For example, it seems obvious that individuals who put more effort into their work and are more productive should receive a higher remuneration. However, it is unclear whether and to what extent regional differences in pay that cannot be explained by skill and effort are perceived as fair.

There is a broad theoretical and philosophical literature on which inputs are relevant. There is a consensus that only factors that are (a) related to the productivity and (b) under the control of individuals should be considered as relevant inputs (Arneson 1989; Cohen 1989; Dworkin 1981). The sparse empirical evidence on fairness evaluations supports this philosophical view. German data shows that survey respondents perceive income differences resulting from individual effort variables such as education and working time as fair. Firm-related (e.g., establishment size) and structural variables (e.g., region) correlate with fairness evaluations, but to a much lesser extent (Liebig et al. 2011; Sauer et al. 2016; Schwarze 2007).

In summary, equity theory asserts that people evaluate the fairness of their own and other people's income in their society based on social comparisons. They view incomes as fair that are proportional to input factors. The value of these inputs is determined by comparisons to observable reference groups. The next section theorizes how fairness evaluations affect the demand for redistribution.

Fairness evaluations and redistribution preferences

Adams (1965) discusses how individuals react to a perception of unfair remuneration. Perceived unfairness results in a negative emotional response such as, but not limited to, relative deprivation. The strength of this response is proportional to the magnitude of the unfairness. The emotional response will motivate the person to reduce or eliminate the tension. There are several possible reactions. First, a person can alter her inputs, which is the simplest method to react to perceived unfairness. When an employee feels that her income does not reflect her efforts, she can adapt her efforts until input-output-proportionality is restored. Second, a person can withdraw from the exchange relationship, e.g. by quitting her job. Third, she can try to alter the output. The most obvious method is to strive for a higher income by negotiating with employers. However, income redistribution is another method to alter the output. Individuals who perceive unfairness can form a demand for redistribution because they support a reduction of unfair income differentials. The aim is to equalize the proportionality of inputs and incomes. There are two relevant paths for this association between unfairness perceptions and the demand for redistribution.

First, individuals who perceive their own income as unfair develop a demand for redistribution. They feel that they are treated unfairly and, thus, wish for state intervention into the market that restores fairness. Personal income fairness should be highly relevant for redistribution preferences because people's perception of the world is mainly provided by their own experiences. They will see the need for redistribution by the state when they feel treated unfairly. In line with equity theory, perceived unfairness is caused by a disproportionality of the income and relevant inputs. Thus, I expect that *the demand for redistribution increases with the perception that individuals' own income is disproportional to their inputs (H1)*. Unfairness can mean two things here because it entails the perception that the own income is worth *less* as well as worth *more* than the inputs. There are various emotional responses linking both perceptions to redistribution preferences. People who think they are underpaid feel relatively deprived, which creates an incentive to resolve ensuing negative emotions. Typical responses are anger, frustration, and resentment (Smith et al. 2012). People who feel overpaid also suffer negative emotions, which may also lead to stronger redistribution preferences. Emotional responses include guilt, embarrassment, and compassion towards the disadvantaged.

Second, individuals who have a diffuse perception that incomes in their country are generally unfair form a stronger DFR. Because of normative concerns, they wish for state intervention that aims to restore fairness by redistributing incomes. The feeling of unfairness is caused by the perception that there is an inequality of input-output-proportionality in their society. Since individuals cannot observe the proportionalities directly, their system fairness evaluation is rather crude. Thus, I expect that *the demand for redistribution increases with the diffuse perception that incomes in general are disproportional to relevant inputs (H2)*.

Individuals derive redistribution preferences from systematic unfairness because they have an other-regarding taste for fairness. People are willing to forego personal gains to support others (Kamei 2018). Recent research suggests that altruism towards the disadvantaged is the relevant mechanism driving DFR (Dimick et al. 2017). However, Wim van Oorschot's (2000) analysis shows that the beneficiaries have to be seen as legitimately deserving. Among other deservingness criteria, he outlines that those who have earned the support by giving something in return (*reciprocity*) are perceived as deserving. According to equity theory, this criterion is always satisfied when someone is remunerated unfairly. This is because unfairness always entails at least one person who received less than the value of his efforts, which implies that targeted redistribution should be seen as a legitimate tool to restore fairness in society.

Lastly, the mechanisms behind the two hypotheses might not differ substantially. It is possible that people who view their own income as unfair only consider their own situation. If this were the case, then we should expect that only those who expect to benefit from redistribution derive a DFR from perceived unfairness. However, it is also possible that individuals use their own experience to gauge how fairly incomes are generally distributed. A worker who thinks that he ought to earn more than he does might infer that other people are treated similarly. If this is true, and people do have an other-regarding taste for fairness, then those who do not benefit from redistribution should also develop a DFR from perceived unfairness of the own income. This would be similar to the theorized mechanisms behind Hypothesis 2. It is not possible to give a definitive answer with the data at hand, but I will test to what extent the association between personal income fairness and the DFR differs with individuals' income to present suggestive evidence.

Before turning to the empirical tests, note that there are alternative mechanisms that link distributive ideals with redistribution preferences. Wim van Oorschot (2000, 2006) identifies five salient deservingness criteria: *control* (do individuals have control over their situation?), *need* (do those who receive need it?), *identification* (can individuals identify with those who

receive?), *attitude* (are those who receive grateful?), and *reciprocity* (have those who receive earned their support?). The theory presented in this study is congruent with the deservingness criteria reciprocity and, to a lesser extent, control. It focuses on a particular fairness ideal, i.e., merit-based proportionality of incomes, while disregarding others. For examples, populaces generally support redistribution to elders in need because they are unable to work. While it should be acknowledged that these alternative normative influences exist, I argue that the relationship between income fairness concerns and redistribution preferences is best understood by equity theory. However, alternative distributive ideals will be considered in the choice of control variables to ensure the validity of the results.

DATA AND METHODS

The data are obtained from the 2009 Social Inequality Module of the International Social Survey Programme (ISSP Research Group 2017). After listwise deletion, they cover 34,300 observations from 39 developed and developing countries. The social inequality modules deal with views on earnings and inequality. They are well suited for my analysis because they contain items on fairness perceptions and redistribution preferences. The dependent variable, the demand for redistribution, is captured by the reaction of respondents to the statement “It is the responsibility of the government to reduce the differences in income between people with high incomes and those with low incomes.”¹ Respondents answered on a five-point Likert scale where higher values indicate increasing approval of the statement.

Independent variables

The perception of personal income fairness is gathered by the following question: “Is your pay just? We are not asking about how much you would like to earn - but what you feel is just given your skills and effort”: (1) Much less than is just, (2) A little less than is just, (3) About just, (4) A little more than is just, (5) Much more than is just. The item perfectly captures the theory because the question explicitly states that respondents should assess the proportionality of their income to their inputs. To my knowledge, this or a comparable item has not been used before in redistribution research.

Furthermore, two items capture the diffuse perception of general income fairness. Respondents were asked to assess how important the following characteristics are for getting ahead in their country: coming from a wealthy family, and being corrupt. Higher values indicate increased importance. Since coming from a wealthy family and being corrupt should not be relevant inputs that justify income differentials, both items capture the perception of systematic unfairness in the income generation process. I expect that both correlate positively with the DFR.

Control variables

It is possible that other distributive ideals are correlated with income fairness perceptions. I consider the influence of individuals’ endorsement of *the importance of need*, *market performance*, and *egalitarianism* using three control variables. The perceived importance of how much need should determine income distribution is captured by the mean response to the following statements: “In deciding how much people ought to earn, how important should each of these things be, in your opinion”: “What is needed to support a family” and “whether the person has

¹ It must be noted that this dependent variable is not ideal. Preferably, an item on tax-spending preferences should be used to measure individuals’ support for redistribution. While a reduction of inequality implies redistribution in most practical situations, it is not the same concept. This shortcoming cannot be overcome with the data at hand. Furthermore, previous studies I rely on use this exact dataset and item (Corneo and Grüner 2002; Isaksson and Lindskog 2009).

children to support”. Market performance ideals are captured by the mean response to the following statements: “In deciding how much people ought to earn, how important should each of these things be, in your opinion”: “How much responsibility goes with the job”, “the number of years spent in education and training”, and “how hard he or she works at the job”. Egalitarianism is captured by a dummy variable that indicates whether respondents prefer a “society with most people in the middle”.

It is a robust finding of prior research that rational considerations strongly influence the demand for redistribution. Individuals who are richer, better educated, have a strong standing in the labor market, or expect to increase their status in the future have a lower demand for redistribution because they gain less from redistribution (e.g., Alesina and La Ferrara 2005; Finseraas 2009; Schmidt-Catran 2016). Thus, I use several variables as controls that aim to absorb rational motivations for redistribution preferences. First, a self-assessment of social class and country-specific household income quintiles. Second, the highest educational degree (five categories) and the current employment status (six categories) of the respondent. This variety of variables ensures that no rational motivations bias the results, which is especially important because it is possible that fairness evaluations mediate the effect of rational considerations. Third, I include the logarithm of household members because individuals with a larger household have increased financial needs and might favor more redistribution. The remaining control variables are age, gender, and marital status. Descriptive statistics are displayed in Table A4.1 in Appendix D.

Model

The data have a hierarchical structure with individuals nested in countries, which invalidates conventional hypothesis tests. I estimate multilevel models with random intercepts to deal with biased standard errors and country heterogeneity (Hox 2010). The dependent variable is available in only five categories and its distribution does not approximate a normal distribution since it is heavily skewed to the right. It is not advisable to estimate linear models since both coefficients and standard errors would be biased (Hox 2010: 141). I estimate a multilevel ordered probit model instead, which was developed for ordinal dependent variables and does not rely on a normally distributed outcome (Hedeker 2008; Hox 2010: 141–47). It models the demand for redistribution as a latent metric variable with cutoff points that determine which of the five ordinal categories is observed. To evaluate the hypotheses, I estimate model-based standard errors derived from the observed information matrix rather than cluster robust standard errors. The latter are robust against misspecifications such as heteroscedasticity or group-specific correlations of the residuals. However, such misspecifications invalidate logit and probit models because they cause a biased and inconsistent estimation of the coefficients. It does not make sense to estimate “robust” standard errors for coefficients that are biased in an unknown direction (Greene 2012: 693).

RESULTS

The results from the multilevel models are depicted in Table 5.1. Model 1 only contains the control variables. The results show that the endorsement of the alternative distributive justice ideals need, market performance, and egalitarianism correlate positively with the DFR. Income, top-bottom placement, university education, and household size correlate negatively with the demand for redistribution, which can be explained by rational motivations. Lastly, older individuals and women have stronger redistribution preferences. The effects of these control variables do not change substantively with the introduction of the main independent variables.

TABLE 5.1: Effects of fairness evaluations on the demand for redistribution

	(1)	(2)	(3)	(4)	(5)
<i>Fairness own income</i>					
Much less		0.345*** (0.018)		0.324*** (0.018)	0.304*** (0.039)
A little less		0.137*** (0.014)		0.128*** (0.014)	0.099*** (0.033)
About just			Reference		
A little more		0.012 (0.032)		0.005 (0.032)	-0.044 (0.075)
Much more		0.153** (0.068)		0.146** (0.068)	0.236 (0.153)
<i>Fairness own income * Income</i>					
Much less * Income					0.007 (0.013)
A little less * Income					0.010 (0.010)
About just * Income			Reference		
A little more * Income					0.016 (0.022)
Much more * Income					-0.030 (0.045)
Importance wealthy family			0.040*** (0.006)	0.037*** (0.006)	0.037*** (0.006)
Importance corruption			0.082*** (0.005)	0.077*** (0.005)	0.077*** (0.005)
Need	0.191*** (0.007)	0.186*** (0.007)	0.184*** (0.007)	0.179*** (0.007)	0.179*** (0.007)
Market performance	0.121*** (0.015)	0.106*** (0.015)	0.117*** (0.015)	0.103*** (0.015)	0.103*** (0.015)
Egalitarianism	0.036*** (0.012)	0.038*** (0.012)	0.036*** (0.012)	0.038*** (0.012)	0.038*** (0.012)
<i>Income</i>					
Lowest			Reference		
Low	0.002 (0.020)	0.015 (0.020)	0.004 (0.020)	0.016 (0.020)	-0.020 (0.049)
Medium	-0.009 (0.020)	0.011 (0.020)	-0.006 (0.020)	0.013 (0.021)	-0.059 (0.091)
High	-0.101*** (0.021)	-0.069*** (0.021)	-0.097*** (0.021)	-0.068*** (0.021)	-0.176 (0.135)
Highest	-0.229*** (0.024)	-0.180*** (0.024)	-0.222*** (0.024)	-0.176*** (0.024)	-0.318* (0.180)
Top-bottom self-placement	-0.079*** (0.004)	-0.069*** (0.004)	-0.072*** (0.004)	-0.063*** (0.004)	-0.063*** (0.004)
Age	0.001** (0.001)	0.001** (0.001)	0.001*** (0.001)	0.002*** (0.001)	0.002*** (0.001)
Sex	0.071*** (0.012)	0.060*** (0.012)	0.085*** (0.012)	0.074*** (0.012)	0.074*** (0.012)
Married	-0.015 (0.015)	-0.016 (0.015)	-0.013 (0.015)	-0.014 (0.015)	-0.014 (0.015)
<i>Education</i>					
No education			Reference		
Lowest formal qualification	0.023 (0.035)	0.022 (0.035)	0.024 (0.035)	0.024 (0.035)	0.024 (0.035)
Above lowest qualification	0.066* (0.034)	0.062* (0.034)	0.064* (0.034)	0.061* (0.035)	0.061* (0.035)
Higher secondary completed	0.003 (0.034)	0.005 (0.034)	0.006 (0.034)	0.008 (0.034)	0.008 (0.034)

Table 5.1 (continued)

Above higher secondary level	-0.039 (0.036)	-0.045 (0.036)	-0.035 (0.036)	-0.041 (0.036)	-0.041 (0.036)
University degree	-0.081** (0.036)	-0.088** (0.036)	-0.073** (0.036)	-0.080** (0.036)	-0.079** (0.036)
<i>Employment status</i>					
Employed, full time			Reference		
Employed, part time	0.026 (0.021)	0.038* (0.021)	0.026 (0.021)	0.038* (0.021)	0.038* (0.021)
Unemployed	0.016 (0.026)	0.024 (0.026)	0.015 (0.026)	0.022 (0.026)	0.022 (0.026)
Not in labor force	-0.004 (0.022)	0.015 (0.022)	-0.007 (0.022)	0.011 (0.022)	0.011 (0.022)
In education	-0.028 (0.040)	-0.000 (0.040)	-0.018 (0.040)	0.007 (0.040)	0.006 (0.040)
Retired	0.015 (0.021)	0.039* (0.021)	0.019 (0.021)	0.041* (0.021)	0.041* (0.021)
Household members (log)	0.037*** (0.014)	0.028* (0.014)	0.039*** (0.014)	0.030** (0.014)	0.030** (0.014)
Variance constant	0.17 (0.04)	0.16 (0.04)	0.14 (0.03)	0.13 (0.03)	0.13 (0.03)
Observations	34,300	34,300	34,300	34,300	34,300
Number of countries	39	39	39	39	39

Note: * <0.1, ** <0.05, *** <0.01. Standard errors in parentheses.

Model 2 introduces the dummy variables that indicate how fair a respondent perceives his income to be compared to his skills and effort. The reference category is the perception that the income is about just. The results show that individuals who perceive their income as too low and much too low have a stronger DFR compared to those who view it as fair. Furthermore, individuals who perceive their income as much too high have a significantly stronger DFR compared to those who view it as fair. Only the dummy variable for a little too much is insignificant. Overall, Model 2 strongly supports Hypothesis 1. The perception that the own income is unfair is associated positively with redistribution preferences. An intriguing finding is that this applies to people who feel underpaid as well as to people who feel overpaid.

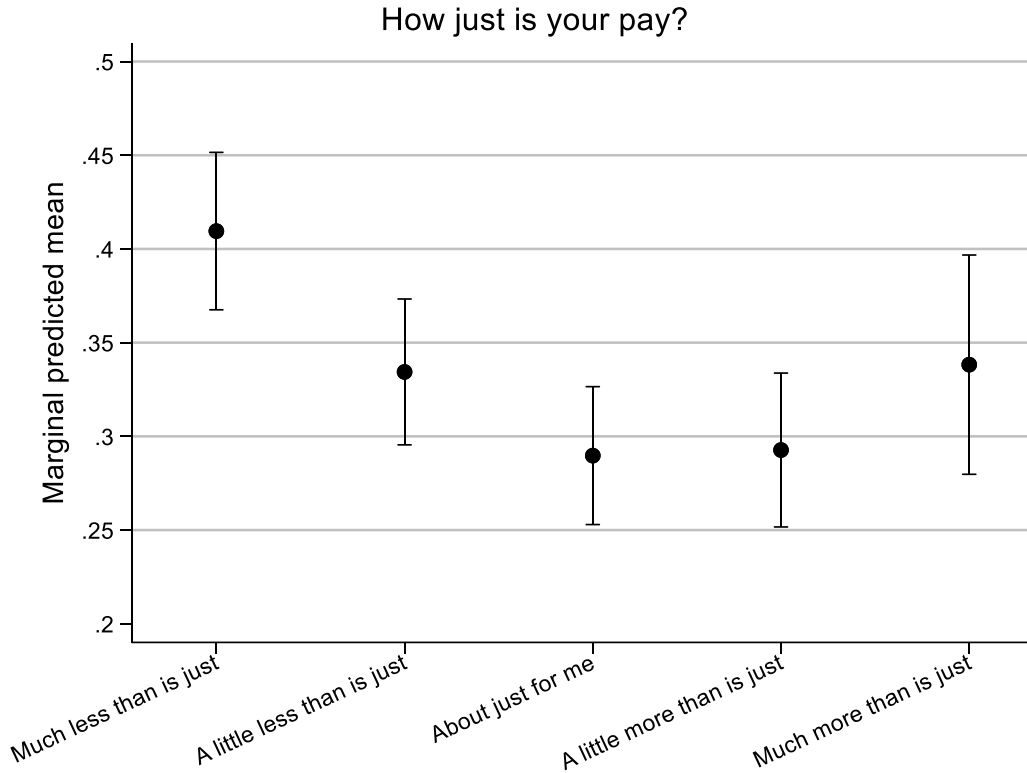
Besides the control variables, Model 3 contains the general unfairness perceptions. Both coefficients for the perceived importance of a wealthy family and being corrupt are highly significant and have the expected direction. Those who perceive systematic unfairness in their country, which means that having a wealthy family and being corrupt are important to get ahead, have a stronger DFR. This supports Hypothesis 2.

Model 4 estimates the coefficients from Model 2 and 3 in a unified model. The aim is to rule out that the inferences are biased by correlations between personal and general fairness perceptions. For example, it is conceivable that individuals who perceive their income as disproportionately low project their dissatisfaction and develop the attitude that a wealthy family is necessary to advance. The results show that the coefficients marginally reduce in magnitude but remain indistinguishable from the previous models.²

Model 5 adds interaction terms between personal income fairness and household income to the specification from Model 4. The result shows that the effect of personal income fairness does

² An analysis shows that personal and general income fairness variables do not correlate strongly. Recoding personal income fairness so that higher values indicate more unfairness (i.e. both feeling over- or underpaid) yields correlations of 0.09 (importance wealthy family) and 0.13 (importance corruption).

FIGURE 5.1: Influence of personal income fairness on the DFR

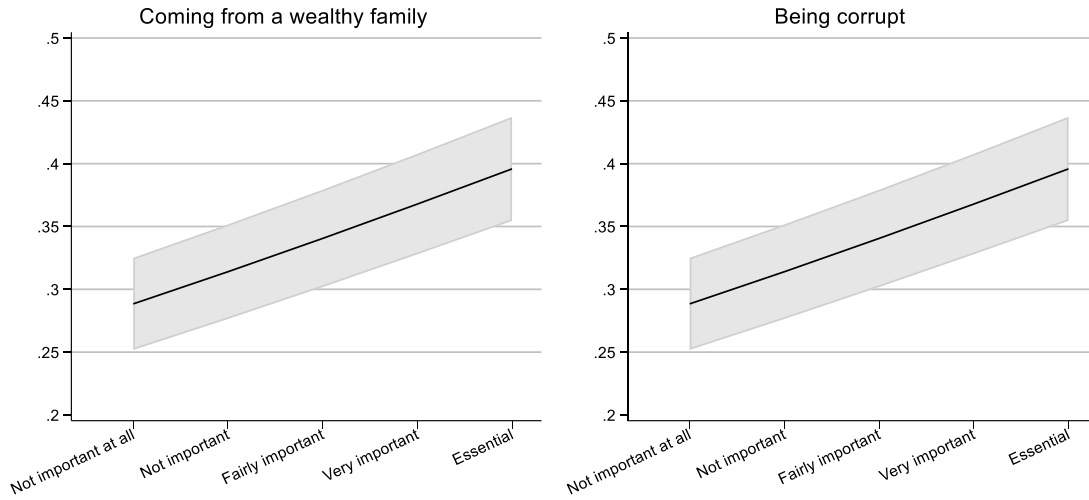


Note: The figure depicts the predicted probability that respondents have a very strong DFR (with 95% confidence intervals; calculations based on Model 4 in Table 5.1).

not differ with income. This suggests that the association to the DFR is other- rather than self-regarding. If individuals aimed to improve the fairness of their own income through redistribution, feeling underpaid (overpaid) would have a weaker (stronger) association with rising income. The results show that unfairness in general leads to a stronger DFR, which suggest that individuals have a taste for fairness and use their own experience to assess what level of redistribution is needed in society.

The coefficients from the regression table do not offer an intuitive apprehension of effect sizes. Statistical significance does not equal substantive significance. Thus, I will interpret the effects with an estimation of marginal effects based on Model 4. Figures 5.1 and 5.2 depict to what extent the independent variables influence the probability that a respondent has a very strong demand for redistribution, i.e., the highest category in the five-point Likert scale.

Figure 5.1 shows that the fairness evaluation of the own income has a substantial influence on the DFR. An individual who thinks that his income is much less than just has a 11 percentage points higher probability to strongly support redistribution than an individual who thinks his income is just. The difference is 4 percentage points between those who think that their income is a little more than just and those who think it is just. On the other hand, an individual who perceives his income as much more than just has a probability that is 5 percentage points higher. Figure 5.2 shows that general fairness perceptions also have a substantial influence. Respondents who perceive coming from a wealthy family as essential have a 6 percentage points higher probability to have a strong DFR than those who perceive it as not important at all. The differential is 10 percentage points for the being corrupt variable.

FIGURE 5.2: Influence of general income fairness on the DFR

Note: The figure depicts the predicted probability that respondents have a very strong DFR (with 95% confidence intervals; calculations based on Model 4 in Table 5.1). Respondents were asked how important coming from a wealthy family and being corrupt are for getting ahead in their country.

The results presented in this section are based on statistical models that include competing rational and normative explanations. This shows that fairness evaluations according to equity theory have an independent impact on redistribution preferences. To confirm the robustness of the results, I conduct several sensitivity tests based on Model 4 (Appendix D, Table A4.2). The first model uses a different measure for competing distributive fairness ideals, i.e., political ideology on a left-right scale. This variable is not used in the main estimations because values are missing in 49% of all observations, which might lead to biased estimates with listwise deletion. However, a re-estimation using left-right ideology confirms the main findings. Model 2 uses robust instead of model-based standard errors, which is an indirect specification test (Hox 2010: 263). The estimations do not lead to substantially different inferences. Model 3 is a simple ordered probit model with country fixed effects that deals with group heterogeneity without requiring the assumptions of random effects models, most importantly no correlation between independent variables and the errors terms. The inferences remain unaffected, which ensures that the results are not driven by omitted country-level variables that affect redistribution preferences. This is because the fixed effects control for all contextual influences such as government quality (see Svallfors 2013).

SUMMARY AND DISCUSSION

Prior research has reliably shown that fairness perceptions affect the demand for redistribution. People who perceive the income distribution as unfair show greater support for redistributive measures. However, the theoretic explanations underpinning prior research have been insufficient to understand the association. Thus, this study offered a comprehensive explanation of why incomes and income distributions are perceived as unfair and how this influences the demand for redistribution. Based on equity theory, it was argued that individuals see incomes as social exchanges where relevant inputs, i.e., skills and efforts, are traded against the income. They perceive earnings as unfair if these inputs are disproportional to the income. To assess the value of the inputs, individuals use social comparisons to reference groups such as colleagues,

friends, and family members. If they have the feeling that their own income is disproportional or incomes in general are disproportional, they develop a demand for redistribution to compensate the unfairness.

The empirical results strongly support the theoretical propositions. First, the DFR depends on the evaluation of the own income. People who perceive their income as unfair have a substantially stronger DFR. Second, the DFR also depends on the evaluation of systematic fairness. People who think that it is necessary to have a wealthy family and be corrupt to advance have a stronger DFR. Suggestive evidence indicates that the mechanism behind both associations is similar. The effect of personal income fairness on the DFR does not vary with income, which points towards other- rather than self-regarding motivations. It seems that people use their own income to gauge how much governmental redistribution is necessary overall.

These results advance the literature on redistribution preferences in several ways. To my knowledge, no prior study has analyzed how the evaluation of the own income influences redistribution preferences. A particularly interesting result is that individuals who feel overpaid develop a demand for redistribution and not only individuals who feel underpaid. This is surprising because high earners usually worry about forfeiting their advantageous position (Burleigh and Meegan 2013). Furthermore, no prior study has offered a comprehensive theoretic explanation of fairness perceptions and how they affect the demand for redistribution.

The present study confirms previous research on the impact of fairness judgements on the DFR (Alesina and Giuliano 2010; Alesina and La Ferrara 2005; Bjørnskov et al. 2013; Corneo and Grüner 2002; Fong 2001; Fong et al. 2005; Iglesias et al. 2013; Isaksson and Lindskog 2009; Kuhn 2010; Linos and West 2003; Miles 2014). It adds to a mounting body of evidence that shows the strong impact of normative evaluations of income distribution on redistribution preferences. The results have implications for scholars of social policy and political economy. They help to explain why income inequality breeds tolerance for income inequality (Schröder 2017; Shariff et al. 2016). Individuals mainly base their fairness perceptions on the incomes achieved by similar individuals while vertical comparisons are less important (see Tao 2015). This implies that fairness perceptions and resulting redistribution preferences are less sensitive to how incomes differ between stratified social groups. Thus, individuals might accept inequality because it does not affect social comparisons to similar individuals.

Furthermore, the findings stress the importance of normative judgements when it comes to policy preferences. Contrary to the widely cited model by Meltzer and Richard (1981), for example, high inequality might not be sufficient to boost redistribution preferences. My analysis suggests that it is necessary that the society values the inequality as offensive in a normative sense. The findings also explain the mechanism that leads disadvantaged workers to support pro-redistribution parties (Emmenegger et al. 2015; Marx 2014; Marx and Picot 2013). Their voting decisions are not only based on rational motivations but also on normative judgements. Especially in sub-disciplines dominated by rational choice theory, it will be a fruitful approach to incorporate normative motivations like these rather than maintaining a simple representation of humans as purely rational beings.

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The quality of government conditions political disagreement over redistributive policies (Paper E)

Abstract

What explains public opinion on redistribution across countries? This study argues that countries' government quality shapes the structure of public opinion. As government quality increases, preferences become more heterogeneous between those with different egotropic and sociotropic motivations regarding inequality. The reason is that higher-quality governments have more capacity and impartiality to implement redistribution effectively and fairly. An empirical analysis of survey data from 39 institutionally diverse countries supports this theory. Both income and perceptions of unfair inequality are associated more strongly with redistribution support in countries with higher government quality. As a result, preferences become more heterogeneous. The implication is that the economic left-right divide in political behavior and policymaking is exacerbated in higher-quality settings and muted in lower-quality settings.

INTRODUCTION

I argue in this study that quality of government (QoG) increases the heterogeneity of public opinion on redistributive policies within countries. Previous research shows that people support more or less income inequality due to their ego- and sociotropic concerns. My argument is that the extent to which this motivation translates into support for public redistribution is stronger when government quality is higher. The reason is that QoG increases governments' ability and willingness to redistribute because higher QoG comes with more redistributive capacity (e.g., the ability to tax the rich) and impartiality (e.g., incorruptibility). As a result, public opinion will be more heterogeneous between those with different views on inequality as QoG rises.

An analysis of comparative survey data from 39 institutionally diverse countries offers empirical support for the theory. Both household income (relating to egotropic concerns) and perceptions of unfair inequality (relating to sociotropic concerns) have substantially stronger associations with redistribution support in countries with higher QoG. Preferences diverge considerably between those with different income and unfairness perceptions when QoG is high while preferences are more homogenous in lower-quality settings.

The broader implication is that QoG shapes the prevalence of an economic left-right divide in political phenomena downstream from policy preferences. Redistribution preferences influence voting behavior (Rueda and Stegmueller 2019: ch. 8) as well as party competition and policy

outcomes (Adams et al. 2004). Since preferences are more diverse in higher-quality settings, the economic left-right divide should be more substantial in structuring political behavior and policymaking (Kasara and Suryanarayan 2015, 2020). In lower-quality settings, in contrast, disagreement about public redistribution is muted, which leaves more space for “second-dimension politics” focusing on issues such as identity (Beramendi and Rehm 2016).

THE DRIVERS OF REDISTRIBUTION PREFERENCES

Previous research identified a set of drivers (i.e., determinants) of redistribution preferences. At the risk of oversimplification, the literature can be separated into studies focusing on egotropic and sociotropic concerns. Firstly, individuals are characterized as motivated by material self-interest. Those who currently have less market income and those who expect to lose market income in the future demand more redistribution (Meltzer and Richard 1981; Moene and Wallerstein 2001). This expectation is supported by a large body of empirical research (Margalit 2013; Owens and Pedulla 2014; Rehm 2009; Rueda and Stegmueller 2019; Walter 2017).

Secondly, approaches focusing on sociotropic concerns theorize that people support more redistribution when they care about (specific) peers (Brezna 2010; Dimick et al. 2017). The research field is heterogeneous, but one of the most robust findings is that distributive fairness perceptions drive preferences. When the perception prevails that people do not get what they deserve, inequality is rejected and public redistribution receives more support (Fong 2001; Gee et al. 2017). In line with this argument, empirical research shows that those who perceive a rift between just and realized incomes demand more redistribution (Ahrens 2022b; Kuhn 2010). People also support more redistribution when they consider incomes to be determined by circumstances outside individuals’ control, such as family background (Fong 2001). Experimental research suggests that the relationship between unfairness perceptions and preferences really is causal (Becker 2020; Gee et al. 2017).

THE CONDITIONING ROLE OF QUALITY OF GOVERNMENT

This section advances the theory that preference formation crucially hinges on the quality of government (QoG). The core argument is that QoG assumes a moderating role that structures to what extent drivers of redistribution preferences exert an effect. The underlying assumption is that individuals have instrumental motivations. They support varying levels of income equality depending on both ego- and sociotropic considerations; however, this goal only translates into demand for redistribution if public institutions have sufficient quality to implement redistribution in a satisfactory manner.

It is beneficial to consider what QoG is to understand its role in preference formation. QoG is firstly determined by the *redistributive capacity* of government. Especially relevant is how well a government can enforce the tax code and provide transfers and services to appropriate recipients (Petrova 2021). Secondly, the *impartiality* of government refers to what extent citizens are treated alike. Especially relevant is whether corruption and other special relationships are absent in policymaking and administration (Rothstein and Teorell 2008).

Both redistributive capacity and impartiality are important for individuals with instrumental motivations who decide whether to turn to the state to pursue income equalization. In lower-quality settings, the redistribution demand of those who would gain from redistribution (both materially and ideologically) will be depressed because they must fear that redistributive efforts are ineffective, inefficient, and misguided. And those who would lose from redistribution are less opposed to it because they must not fear that redistribution will actually be implemented. They

may also trust that they will be the beneficiaries of corrupt public officials. In higher-quality settings, in contrast, preferences will diverge to a stronger extent between those with different ego- and sociotropic considerations because of high redistributive capacity and impartiality. In technical terms, QoG moderates the relationship between redistribution preferences and their drivers.

SITUATING THE ARGUMENT IN THE LITERATURE

It is important to note that I draw from previous literature which argues that people support more redistribution when they trust public institutions and officials (Edlund 1999; Rothstein et al. 2012; Svallfors 1999). Empirical evaluations of this expectation are inconclusive so far, with studies presenting a mix of supporting evidence (Edlund and Lindh 2013; Kuziemko et al. 2015; Rudolph and Evans 2005; Svallfors 2013) and refuting evidence (Edlund 1999, 2006; Peyton 2020; Svallfors 1999).

I depart from this literature by focusing on QoG rather than on political trust. To be sure, these are related concepts because QoG fosters trust in the state (Rothstein et al. 2012). I also contend that trust may explain a part of the causal chain that explains why certain individuals demand more redistribution in higher-quality settings. However, I will argue hereafter that this theory does not pertain to the whole society but rather only to those who already favor redistribution.

Secondly, I contend that it is misguided to characterize QoG (or political trust) as an independent determinant of preferences. QoG is rather a moderating factor that structures to what extent preferences diverge between people with different ego- and sociotropic concerns. Consider what happens when a country's QoG rises. The trust literature would expect that this increase leads to a uniform increase in political trust, which then translates into higher redistribution demand. However, I expect that this expectation only pertains to people who favor redistribution, i.e., poorer individuals and those who find inequality unfair. Those opposed to redistribution (i.e., the rich and those who find inequality fair), in contrast, should not react to higher QoG with higher trust and redistribution demand; in line with their distributive goals, they should rather fear QoG and lower their redistribution demand in response. This may explain the inconclusive findings from the trust literature.

My theory also reverberates Svallfors (2013), who argued that (perceived) QoG not only increases redistribution support but also moderates the effect of egalitarianism. In contrast, I expect that QoG should only assume a moderating role that structures preference heterogeneity rather than the overall level of redistribution support. And I argue that the moderating role of QoG is not limited to egalitarianism but extends to preference drivers in general. This includes drivers that relate to egotropic considerations, such as income. The theoretical and empirical scope of the argument widens considerably as a result.

DATA

The empirical analysis uses individual-level data from the 2009 Social Inequality Module of the International Social Survey Programme (ISSP Research Group 2017). The dataset is well suited because it contains comprehensive measures relating to economic inequality and redistribution. Furthermore, it covers 39 institutionally diverse countries with government quality ranging from countries such as Bulgaria and the Philippines to countries such as Finland. Appendix E contains a full list of countries and descriptive statistics on their government quality.

Redistribution preferences

Cavaillé and Trump (2015) demonstrated that support for inequality reduction and support for welfare provision to specific beneficiaries are distinctive dimensions of redistribution preferences, whereas the former elicits stronger egotropic and the latter stronger sociotropic considerations. I take up this insight by using two alternative dependent variables that map onto these dimensions. The first variable measures support of public income equalization via reactions to the statement “it is the responsibility of the government to reduce the differences in income between people with high incomes and those with low income”. The second dependent variable measures support of public unemployment assistance via reactions to “the government should provide a decent standard of living for the unemployed”. Both variables have five scale points ranging from disagree strongly to agree strongly.

Drivers of redistribution preferences

I use one independent variable that relates to egotropic and another variable that relates to sociotropic considerations. Firstly, material self-interest is captured with equalized household income. Household income is recorded in country-specific currencies in the ISSP data (often in coarsened form). These income values are first equalized by dividing by the square root of household members and then brought on the same scale across countries by dividing values by country-specific means. A value of 0.5 on the resulting variable, for example, indicates that a respondent earns half of the country mean. The variable’s distribution is highly skewed with far-out outliers on both the top and bottom side, as tends to be the case with income data. Therefore, the variable is winsored at the 1st and 99th percentiles and then logged.

The second independent variable measures perceived unfairness of realized income outcomes. Respondents were asked to estimate what individuals in five professions of varying income and status earn, as well as what they should earn. I first calculate the magnitude of divergence between estimated and just earnings for each profession.¹ The value one indicates perfect congruence between actual and just earnings, whereas values above one show that they diverge. A value of two, for example, shows that respondents think that a certain profession earns either twice as much or half as much as it should. This divergence is then averaged over all five professions. To deal with far-out outliers and a heavily skewed distribution of the resulting variable, the average is winsored at the 95th percentile and subsequently logged.

Quality of government

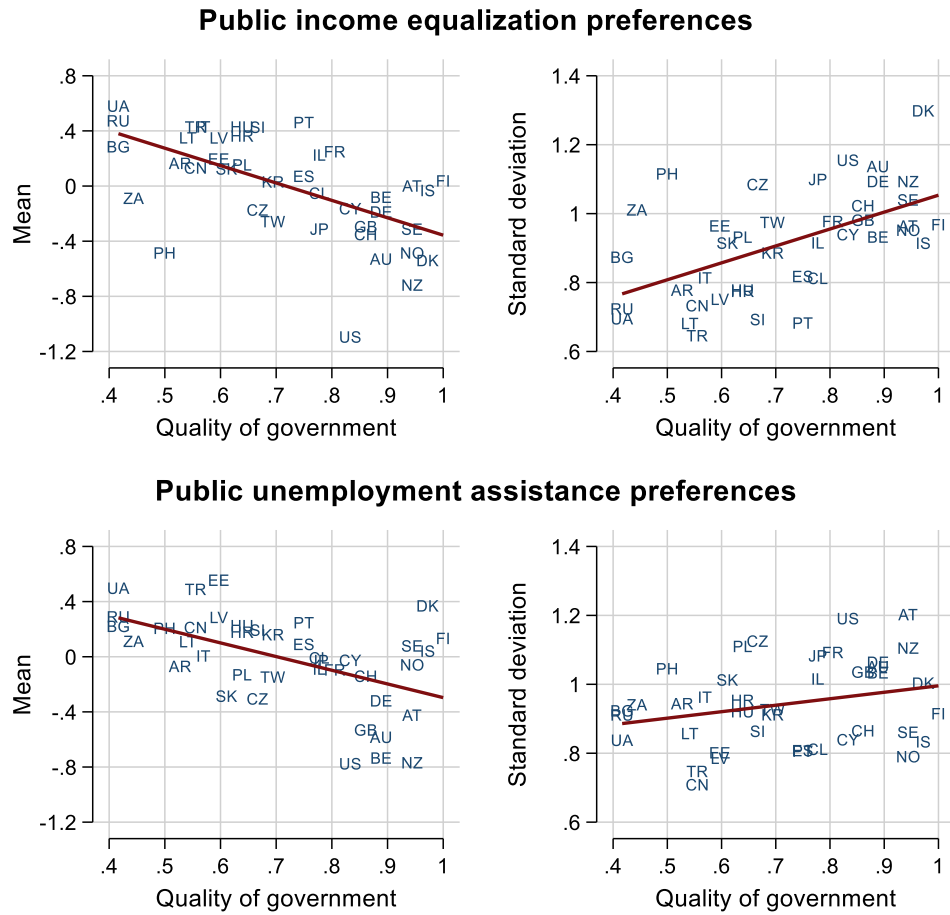
I use a country-level QoG indicator on a scale from zero to one from the International Country Risk Guide (ICRG) (PRS Group 2020), which has been widely used in comparative research (e.g., Kasara and Suryanarayan 2015, 2020; Petrova 2021; Rothstein et al. 2012). It is a summary measure of three sub-indices on bureaucracy quality (e.g., autonomy and competency of bureaucrats), corruption (e.g., bribes and nepotism), and law and order (e.g., observance of the law). The indicator is well-suited because all sub-components are directly relevant for the impartiality and redistributive capacity of public institutions. Figure A5.1 in Appendix E shows the distribution of QoG across countries.

Control variables

I also use several individual-level controls from the ISSP data. These include gender, age, age squared, the highest level of education (five categories: none or lowest, above lowest, higher

¹ This divergence d of profession i is defined as $d_i = \frac{a_i}{e_i}$ if $a_i \geq e_i$ and $d_i = 1/\left(\frac{a_i}{e_i}\right)$ if $a_i < e_i$, where a refers to estimated actual earnings and e to just earnings.

FIGURE 6.1: Means and standard deviations of preferences across countries



Note: The underlying variables (income equalization and unemployment assistance preferences) are standardized across the whole dataset with mean zero and standard deviation one.

secondary, above higher secondary, university), and employment status (four categories: employed, unemployed, in education, not in labor force).

RESULTS

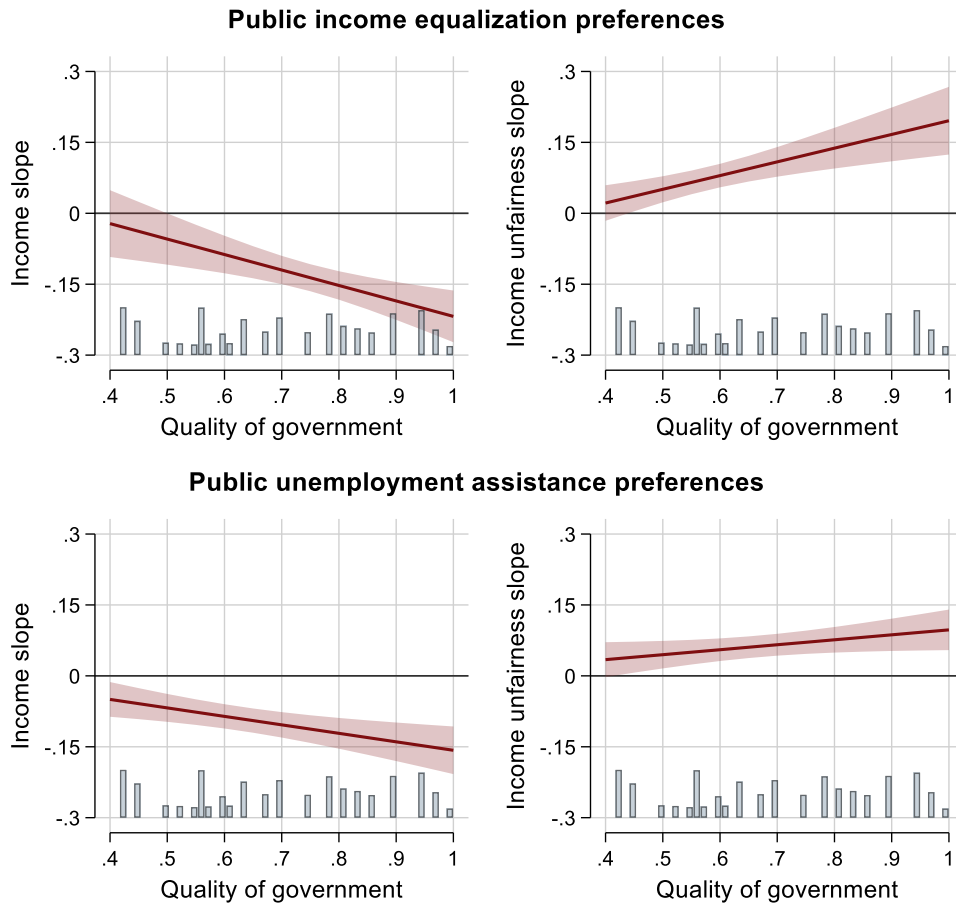
Figure 6.1 presents descriptive statistics on the means and standard deviations of the two policy preference variables across the covered countries. It shows that, as QoG increases, countries support less redistribution while the preferences also become more varied. This finding is inconsistent with the view that redistribution support increases with QoG, as theories on political trust would expect. In contrast, Figure 6.1 offers first support for the argument that QoG functions as a wedge that makes preferences more heterogeneous.

I move on to a direct evaluation of the theoretical argument and assess whether the effects of household income and unfairness perceptions on redistribution preferences become stronger as QoG increases. I estimate the following linear regression model with ordinary least squares:

$$pref_{ic} = \alpha inc_{ic} + \beta unfair_{ic} + \gamma (inc_{ic} \text{ OR } unfair_{ic}) * QoG_c + \delta cntrl_{ic} + \theta_c + \epsilon_{ic},$$

where $pref_{ic}$ refers to the redistribution preference of individual i in country c , inc_{ic} to household income, $unfair_{ic}$ to perceived income unfairness, QoG_c to government quality in country c , $cntrl_{ic}$

FIGURE 6.2: Estimated income and unfairness perception slopes by quality of government



Note: The figure depicts conditional marginal effects with 95% confidence intervals obtained from four regression models. The solid lines indicate the strength of the association between income/unfairness perceptions and redistribution preferences at different values of government quality, holding gender, age, age squared, education, and employment status constant. The full regression results are available in Tables A5.1-A5.2 in Appendix E.

to individual-level controls, θ_c to country fixed effects, and ϵ_{ic} to the error term. Crucially, the model also includes an interaction between QoG and either income or perceived income unfairness. I use this model to estimate whether the slope coefficients of income and unfairness perceptions within countries are stronger in countries with more QoG.

I standardize all main variables except QoG to allow for easy interpretation of the results. The estimated slope coefficients therefore indicate expected standard deviation changes of policy preferences when an independent variable increases by one standard deviation. The standard errors are clustered by countries to account for autocorrelation within countries (caused primarily by the QoG measure, which does not vary within countries) as well as heteroskedasticity. Lastly, the regressions are weighted so that each country has the same impact on the overall results because the primary theoretical interest (“do drivers exert a stronger effect in countries with higher QoG?”) concerns the country level.²

Figure 6.2 presents the findings. It plots the results of four regression models that use either income equalization or unemployment assistance preferences as the dependent variable, and

² This is achieved by assigning more (less) weight to countries with less (more) observations (see Appendix E for details).

that include an interaction term between QoG and either income or unfairness perceptions. Each panel depicts the slope (i.e., marginal effect) of one of the two drivers conditional on government quality.

The results support the theoretical expectations. The effect estimates of income are generally negative, but they crucially depend on QoG for both dependent variables. Income is only marginally associated with preferences in lower-quality settings, while the association becomes considerably stronger as QoG increases. Corresponding results are observed for the income unfairness variable. Perceived income unfairness covaries with preferences under higher QoG while its effect estimates remain negligible under low QoG. For both dependent variables, the effect estimates have moderate size under high QoG, generally reaching between 0.15 and 0.2 standard deviations of the policy preferences following a one standard deviation change in one of the drivers. At the same time, they become substantially indistinguishable from zero under lower QoG. Overall, the results suggest that QoG conditions to what extent drivers are associated with redistribution preferences. Preference heterogeneity is muted in low-quality contexts and exacerbated in high-quality contexts.

I run several robustness checks to ensure that the results hold under alternative specifications (see Appendix E for details). I firstly estimate logit regressions using dummies indicating whether people agree or agree strongly to the redistribution items as dependent variables (Figure A6.2). Secondly, I use an alternative country-level QoG indicator from the World Bank's Worldwide Governance Indicators (WGI), which is a summary measure covering the dimensions government effectiveness, control of corruption, and rule of law (Figure A6.3). Both tests have results that are almost identical to the main specifications.

Thirdly, I estimate separate OLS regressions for each country instead of a pooled model with QoG interactions and plot countries' slope coefficients against their QoG (Figure A6.4). The results show that the assumption of linear interaction effects underlying the main regression models is reasonable.

Lastly, an important threat to identification originates from country-level characteristics that are correlated with QoG and act as moderators themselves. There may be alternative explanations for why the associations of the drivers differ between countries. I safeguard against this possibility by adding additional interaction terms between the drivers and further country-level variables to the main specifications. Drawn from previous research, I use the Gini index of income inequality, tax and transfer progressivity, and GDP per capita for additional interactions (Beramendi and Rehm 2016; Dimick et al. 2017; Dion and Birchfield 2010; Schmidt-Catran 2016). I also use the labor share of GDP, the unemployment rate, the proportion of the population over 65, ethnic and religious fractionalization, social spending to GDP, and a democracy measure (see Appendix E for details and data sources). The results show that the main findings are robust to this range of additional interaction terms (Tables A5.4-A5.5).

DISCUSSION AND CONCLUSION

This study showed that the extent to which income and unfairness perceptions are associated with redistribution preferences is considerably stronger in countries with higher government quality. The consequence is that public opinion is more heterogeneous in higher-quality settings and more homogenous in lower-quality settings.

The findings imply that QoG also structures political phenomena downstream from policy preferences. Redistribution preferences increase the probability of voting for a pro-redistribution party on the left (Rueda and Stegmueller 2019: ch. 8). Citizens' stance regarding redistributive policies also influences how parties position themselves and what policies they implement

(Adams et al. 2004). Therefore, as QoG increases, political behavior and policymaking should become divided by an economic left-right conflict (Kasara and Suryanarayan 2015, 2020). Since political attention is limited, other cleavages consequentially become less important. Conversely, lower QoG implies that the economic left-right conflict becomes muted and that other issues become more important.

The findings are especially relevant when one moves the analytical lens away from well-developed countries. Dominant theoretical approaches on preference formation and downstream phenomena were formulated with highly-developed countries in mind since they continue to garner most attention in academic discourse. This study shows that such approaches are less applicable to countries without high government quality.

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

(Paper F)

Abstract

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

INTRODUCTION

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

Even though the expectation that welfare changes have electoral consequences is widespread, it is not backed up by comprehensive empirical evidence. Quantitative studies cannot confirm that there is a general relationship between welfare changes and votes (Armingeon and Giger 2008; Giger and Nelson 2013; Schumacher et al. 2013). They rather indicate that electoral consequences only arise conditionally. They are especially likely for pro-welfare parties such as social democrats (Giger and Nelson 2011; Horn 2021; Schumacher et al. 2013) as well as for parties that cannot engage in blame shifting (Wenzelburger 2014; Wenzelburger et al. 2020). However, the empirical evidence is mixed, and there is no consensus on the significance of the various conditional factors.

Researchers also argued that voters do not react to the implementation of welfare changes but to legislative decisions, for which comprehensive data only became available recently (Jensen and Wenzelburger 2021a). The scarce evidence indicates that the adoption of welfare reforms

impacts government popularity (Lee et al. 2020). However, it remains unclear whether this translates into actual electoral consequences.

The objective of this article is to put theories on the electoral consequences of welfare state change under scrutiny. We discuss the different theoretical approaches based on a unified theoretical model that allows us to lay open their pivotal assumptions, and we explain why these assumptions may be unreasonable from a theoretical perspective. We then conduct an empirical assessment of hypotheses on general and conditional electoral consequences using data on the electoral fates of government parties from 20 countries between 1970 and 2019. We draw from previous research but move beyond it in several ways. We assess whether welfare changes have electoral consequences across a range of empirical measures. Our analysis complements standard outcome measures of welfare state change (spending, welfare generosity) with new data on legislative changes to the welfare state (Jensen and Wenzelburger 2021a; Lee et al. 2020). The data are up-to-date, reaching up to 2019 for the outcome measures and 2014 for the reform measures. Furthermore, we conduct a wide range of statistical tests for electoral consequences under favorable conditions.

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

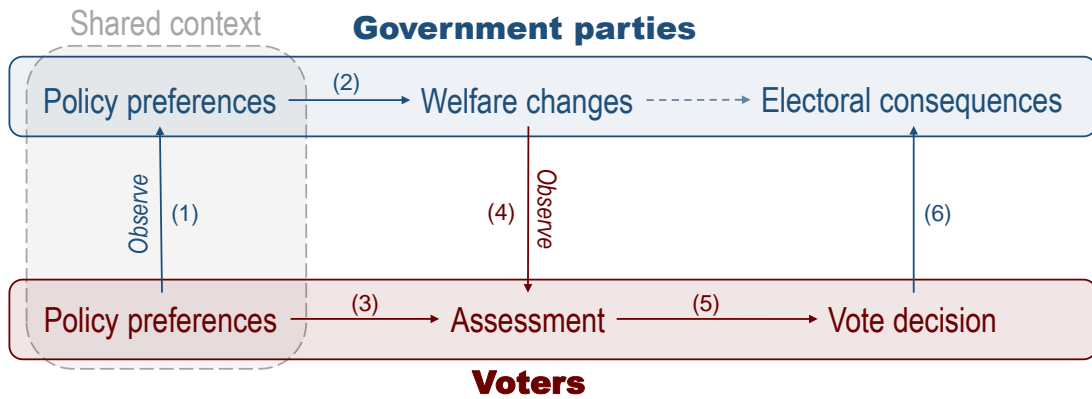
This study contributes to the literature by offering a critical examination of the assumptions underlying the supposed electoral consequences of welfare change, which are often taken for granted. It suggests that welfare policies matter less for electoral outcomes than commonly assumed. We believe that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

THE ELECTORAL CONSEQUENCES OF WELFARE STATE CHANGE

Paul Pierson's (1994, 1996) seminal work on the 'new politics of the welfare state' argues that the welfare state creates its own supporters, which is why welfare cuts are highly unpopular among large parts of the electorate. Voters thus turn away from governing parties that implement welfare cuts, which suffer electoral losses as a result. Correspondingly, governing parties may reap electoral gains when they expand popular welfare programs (Bonoli 2012). However, the negative electoral effects of retrenchment are expected to be higher than the gains resulting from welfare expansion because of a 'negativity bias' exhibited by individuals (Pierson 1996: 144–47; Weaver 1986).

Figure 7.1 offers a stylized representation of the causal mechanism, which serves as a backdrop for our discussion of the arguments in favor of and against electoral consequences of welfare changes. Government parties and voters both have policy preferences. These include attitudes on the ideal level of welfare entitlements and spending, but they also cover fiscal and economic

FIGURE 7.1: Theoretical model of electoral consequences of welfare state change



concerns, which are tightly linked with public provision of welfare. The policy preferences of government parties depend on party ideology, but they are also shaped by voters' policy preferences (arrow 1) due to vote-seeking behavior (Adams et al. 2004; Romeijn 2020). Government parties implement and oversee welfare changes based on their policy preferences (2). Voters then compare these welfare changes with their own preferences to assess the policy-preference congruence (3 and 4). Based on this assessment, voters punish or reward government parties in their vote decision (5). Lastly, the aggregated votes decide over the electoral consequences of welfare changes (6). Electoral consequences ultimately arise due to the degree of preference congruence between parties and voters according to the model.

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

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

Empirical research offers only weak support for the theoretical expectations. Individual case studies show that welfare changes can have electoral consequences (e.g., Arndt 2013; Schwander and Manow 2017). However, quantitative research consistently fails to confirm the prevalence of general electoral consequences (Armingeon and Giger 2008; Giger 2011: 135–41; Schumacher et al. 2013).

THEORETICAL REFINEMENTS AND FUNDAMENTAL CRITIQUES

Inspired by the lack of confirming evidence, researchers refined the theoretical argument on the electoral consequences of welfare change. Each contribution tweaks one of the building blocks

of Figure 7.1 to arrive at a more nuanced view on the conditions under which electoral consequences should arise. We review these theoretical refinements in this section. Our aim is to identify conditions under which electoral consequences are especially likely. We also draw from a wider political science literature and discuss why welfare changes may ultimately have no electoral consequences even under such favorable conditions.

Voter and party preferences

Researchers firstly refined the specification of voters' policy preferences. *Partisan theory* highlights that the social constituencies of political parties have distinctive policy preferences (Häusermann et al. 2013; Hibbs 1977). Voters of left parties, and to a lesser extent of Christian democrats (van Kersbergen 1995), hold especially strong pro-welfare preferences and assign more weight to their social policy preferences than other voters. Therefore, pro-welfare parties should face stronger electoral consequences than their competitors. Specifically, it is left parties that should be punished for retrenchment and rewarded for welfare expansion (Schumacher et al. 2013).¹

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

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

Moreover, the theory behind Figure 7.1 neglects that parties are able to influence the policy preferences of their voters, which prevents incongruences in the preferences of voters and parties (Bullock 2011; Slothuus and Bisgaard 2021). This may happen through successful framing efforts in public discourse. Research shows that parties prevent electoral punishment after welfare retrenchment by strategic framing, such as by proclaiming the undeservingness of recipients (Esmark and Schoop 2017; Slothuus 2007), highlighting popular reform elements (Nelson 2016), and pointing to the inevitability of the reform (Green-Pedersen 2001). But even without such efforts, committed voters align their policy preferences with observed welfare changes to protect their partisan identity or because they use party positions as informational shortcuts. Strikingly, Slothuus and Bisgaard (2021) show that supporters of the Danish People's Party and the Danish Liberals rapidly adapted their welfare preferences after their parties unexpectedly announced

¹ Following the competing 'Nixon goes to China' logic, left parties are supposed to be less vulnerable when they implement welfare cuts because of their higher credibility as defenders of the welfare state (Green-Pedersen 2001).

cutbacks to unemployment benefits and an early retirement program. The implication is that the potential for electoral consequences is more limited than assumed.

Voters' assessment of welfare changes

We now move on to theoretical refinements of voters' assessment of welfare changes (arrows 3 and 4 in Figure 7.1). In contrast to the stylized model presented in Figure 7.1, voters neither systematically nor always correctly observe welfare change, which limits their ability to assess whether policies are in line with their preferences or not. Research not only shows that voters' knowledge about welfare programs is often limited or inaccurate (Geiger 2018; Jensen and Zohlnhöfer 2020) but also that voters tend to misperceive the distributive effects of welfare reforms (Wilson et al. 2012). These misperceptions are aggravated by the fact that media attention is restricted to certain kinds of welfare reforms (Jensen and Wenzelburger 2021b).

There is an extensive literature on the different strategies of *blame avoidance* and *credit claiming*, in which parties exploit voters' limited ability to observe and assess welfare change (Bonoli 2012). Welfare scholars argue that parties strategically alter reform saliency via the timing of reforms and the choice of specific policy instruments (Jensen and Wenzelburger 2021a; Pierson 1994: 13–26; Vis 2016; Wenzelburger 2014; Wenzelburger et al. 2020). The timing argument states that voters have a short memory, which is why welfare reforms are quickly forgotten.² Parties may exploit this by strategically timing their reforms to maximize electoral outcomes, for example by avoiding cutbacks and favoring expansion as an election approaches (Jensen and Wenzelburger 2021a; Wenzelburger 2014; Wenzelburger et al. 2020). The policy instrument argument expects that voters only observe changes to the most visible welfare program characteristics such as replacement rates, while they have limited knowledge about more technical characteristics such as benefit indexation. Again, parties may exploit this by strategically targeting program characteristics in their reforms, for example by using more visible policy instruments such as benefit replacement rates when expanding the welfare state and by retrenching through less visible instruments such as benefit indexation (Jensen and Wenzelburger 2021a: ch. 6).

Voters' ability to correctly observe and attribute welfare changes also depends on the political context. Electoral consequences are especially likely under high clarity of responsibility, i.e., when voters are able to distinguish that a specific government party is responsible for implemented welfare change rather than other political entities (Giger 2011: 48–51). Low clarity of responsibility, in contrast, makes electoral consequences less likely, especially because parties can exploit the situation for strategic blame avoidance. Apart from institutional factors such as bicameralism or federalism, clarity of responsibility depends on government composition (Hobolt et al. 2013). Responsibility is clear when a government is dominated by a single party, when government parties are ideologically cohesive, and when there is no cohabitation. Maximum clarity can be achieved under single-party governments.

The theoretical discussion suggests that electoral consequences are especially likely when it can be reasonably assumed that voters are aware of welfare change and can attribute the change to government parties. This is the case when welfare change concerns visible program characteristics, when it is implemented shortly before an election, and when clarity of responsibility is high. However, the theoretical discussion can also be used to justify why welfare change should have little or no electoral consequences altogether. Possibly, voters are insufficiently able or

² For example, the literature on fiscal austerity indicates that reform-related drops in government popularity are often temporary and thus do not translate into electoral punishment when timed correctly (Hübscher and Sattler 2017; Jacques and Haffert 2021).

TABLE 7.1: Summary of conditions that favor electoral consequences

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

willing to observe and attribute welfare changes, for example because they do not care or pay little attention.

The link between voters' assessment and vote decision

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

Summary and expectations

Table 7.1 presents a summary of the theoretical arguments drawn from the literature. It lists the different conditions that make electoral consequences especially likely. If there is a tendency for electoral consequences, this is where we should observe them. We will use the table as a guide for our empirical analysis.

We start the empirical analysis by evaluating whether there are electoral consequences of welfare change across all parties and governments. However, both the theoretical discussion and previous empirical evidence suggests that we are unlikely to find such general effects. Therefore, we move on to refined analyses of conditional electoral consequences, successively going through all favorable conditions outlined in Table 7.1. But against the backdrop of the more fundamental critique, we contend that a predominance of null results may also be the outcome of our evaluation.

DATA AND METHOD

We compiled a dataset that tracks the electoral fates of government parties from 20 highly developed countries between 1970 and 2019.³ Each observation relates to the incumbency of a government party. For example, the German CDU and SPD governed in a coalition between

³ Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Japan, Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, and United Kingdom.

2005 and 2009. Our dataset contains one observation relating to the SPD and another observation relating to the CDU (both concerning their 2005-2009 incumbency and their election results in 2005 and 2009). Observations of parties are in many cases repeated because, for example, the SPD entered multiple governments during our observation period.

We include one observation per government party and election period. We consider a party to be in government between the start date of the first cabinet it participated in and either the end date of the last cabinet it participated in or the day of the subsequent election, whatever date is earlier. We exclude parties in caretaker governments, parties that governed less than one year, as well as special issue parties. The data cover 484 government parties after listwise deletion, formed by 117 unique parties in 236 governments.⁴ All data relating to parties, elections, and cabinets are sourced from the ParlGov database (Döring and Manow 2022).

Variables

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

We use three types of indicators to measure welfare state change during parties' incumbency. The first indicator is the development of social spending as a proportion of GDP between the years a party entered and left government (sourced from the Comparative Political Data Set by Armingeon et al. [2021]). For further analyses on whether changes implemented shortly before an election matter, we instead use social spending changes in the last government year.⁵ Social spending is a measure of the overall size of the welfare state. It indicates to what extent citizens *currently benefit* from welfare policies.

Secondly, we use the development of unemployment benefit and pension entitlement generosity between the years a party entered and left government (sourced from the Comparative Welfare Entitlements Dataset by Scruggs [2022]). Again, we also use alternative versions of the indicators that solely measure changes implemented in the last government year. The generosity indicators measure to what extent citizens can *expect to benefit* from the welfare state. Our main analyses rely on two summary generosity indicators that consider the following program characteristics to quantify the overall strength of social entitlements: average replacement rates, benefit duration (i.e., retirement age in the case of pensions), qualification period, waiting period (only unemployment benefits), the proportion of employee contributions (only pensions), and coverage rates. For further analyses of whether visible changes have electoral consequences, we use alternative summary generosity indicators that only record changes in the most visible subdimensions of the generosity indicators, namely replacement rates and benefit duration (Jensen and Wenzelburger 2021a).⁶

Thirdly, we use data on the adoption of legislative reforms to unemployment benefits and pensions during the incumbency of government parties (sourced from the Welfare State Reform

⁴ We exclude one observation with an abnormally large vote change of +157% from the dataset (the Norwegian Center Party and their 1989-1993 incumbency) because this is an outlier with potentially strong effects on OLS regression results (see Figure A6.1 in Appendix F).

⁵ For example, if parties governed between 2000 and 2005, we use changes between 2004 and 2005.

⁶ Replacement rates and benefit duration are weighted equally in these alternative summary indicators.

Dataset by Jensen and Wenzelburger [2021a]). Unfortunately, the reform data is only available for five countries.⁷ The data record each individual legislative reform of the two welfare programs, with separate entries for each dimension of change (e.g., replacement rate, duration period, etc.) and direction of change (cutback, retrenchment, introduction/abolishment). For example, a welfare reform package that decreases replacement rates but increases benefit duration of unemployment benefits is recorded in two separate entries in the dataset. The data only record that a reform happened at a specific point in time (e.g., pension replacement rates were lowered), but there is no information on the magnitude of change.

Following Jensen and Wenzelburger (2021a), we use net change indicators for pensions and unemployment benefits. They quantify the number of welfare-expanding reforms relative to the number of retrenching reforms under a government.⁸ For example, the value two indicates that a government party introduced two more expanding reforms than retrenching reforms during their incumbency. For the analyses of recent welfare changes, we use alternative reform measures that only record reforms implemented in the 12 months before the election; and for the analyses of visible welfare reforms, we use alternative measures that only consider reforms to the most visible program dimensions according to Jensen and Wenzelburger (2021a: ch. 6): benefit levels, age brackets, benefit duration, employability, and means-tests.

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

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

We use a standard set of controls inspired by previous studies on electoral consequences: parties’ vote share in the election that brought them into power are included to control for regression to the mean effects, i.e., parties with higher vote shares are likely to lose votes in the subsequent election. We use the effective number of parties measured by the indicator of Golosov (2010) because parties are expected to lose more votes when there are more alternatives. We control for the government duration in days to capture the “cost of governing”, i.e., a commonly observed decrease in government popularity over time. Furthermore, we choose a set of control variables based on the economic voting literature, which claims that voters retrospectively punish bad economic conditions and reward good economic conditions. Controlling for

⁷ Denmark, Finland, France, Germany, and United Kingdom.

⁸ Welfare program introductions are considered as welfare expansion and program abolishments as welfare retrenchment.

economic conditions is important because they should be a common cause of welfare changes and vote results. For example, low growth or a high budget deficit may lead governments to retrench welfare while such adverse conditions may also be punished at the polls due to economic voting. We use the central government budget balance, the unemployment rate, and GDP growth. Budget balance is measured in the first year of incumbency to avoid post-treatment bias since changes to the welfare state also affect the budget. Regarding unemployment and GDP growth, we use both their average levels during governments' incumbency as well as changes between the years a party entered and left government. The motivation is that it is unclear whether, for example, voters react to persistently high levels of unemployment or to a surge in unemployment during an election period. Lastly, we use time fixed effects to capture unobserved heterogeneity over time.

Method

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

$$\Delta vote_{pe} = \beta \Delta welfare_{pe} + \delta cntrl_{pe} + \gamma_{t+1} + \epsilon_{pe}$$

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

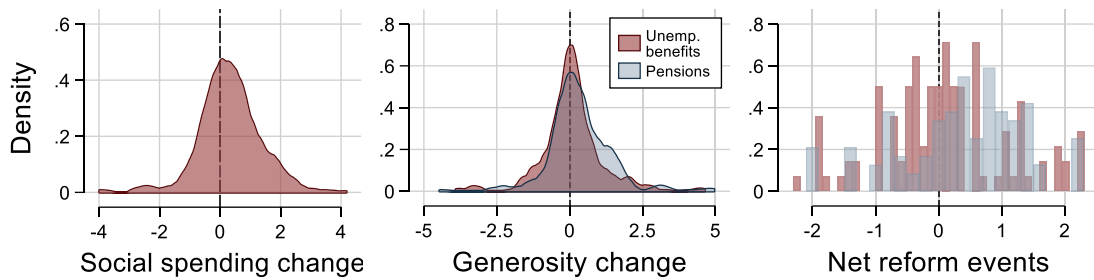
We also assess whether welfare expansion and retrenchment have different electoral implications. We split the welfare change indicators $\Delta welfare_{pe}$ into the two components $\Delta_{pos}welfare_{pe}$ and $\Delta_{neg}welfare_{pe}$, whereas the former only records welfare expanding changes and takes the value zero otherwise and the latter only records welfare retrenching changes and is zero otherwise.⁹ We then enter the two components in the regression model above to estimate separate coefficients for them.

We rescale all welfare change measures so that their standard deviations equal one. All regression coefficients therefore indicate the expected vote change when the respective welfare change measure increases by one standard deviation. This simplifies the interpretation of the results because the welfare change indicators are on different measurement scales, which are also difficult to evaluate (for example, it is unclear what exactly a one-unit increase in pension generosity entails). Further, we use robust standard errors with multi-way clustering by both governments and parties. This is motivated by the results of specification tests showing that there is intra-cluster correlation within governments (i.e., repeated observations within government coalitions) and parties (i.e., repeated observations of the same party across different governments). These standard errors are also heteroskedasticity-consistent.

Discussion

We believe that our empirical approach refines previous research in several ways. Firstly, we measure welfare change comprehensively by assessing the effects of several dimensions of change: social spending, welfare generosity, and legislative reforms, all with up-to-date data. Previous studies, in contrast, mainly relied on the development of program replacement rates to measure

⁹ For changes in social spending and entitlement generosity, the expansion and retrenchment indicators are constructed based on the overall change $\Delta welfare_{pe}$. For example, the welfare expansion indicator only records values above zero if the overall change is positive. For the reform indicators, we can use a more fine-grained measure. The expansion and retrenchment indicators are given by the number of welfare-expanding and -retrenching reforms, respectively, implying that both can deviate from zero concurrently.

FIGURE 7.2: Distributions of the welfare state change indicators

welfare state change (see Armingeon and Giger 2008; Giger and Nelson 2011; Schumacher et al. 2013). Replacement rates are a crucial feature of welfare programs, but they do not capture other important dimensions of welfare change, such as benefit duration. In addition, the effect of legislative decisions is understudied in quantitative research (Jensen and Wenzelburger 2021a). Lee et al. (2020) study effects on government support, but it remains unclear to what extent their findings extend to actual voting behavior.

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

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

Results

This section presents the results of the empirical analysis. We begin with descriptive statistics on welfare state change (a full set of descriptive statistics is available in Table A6.1 in Appendix F). Figure 7.2 shows the distributions of welfare changes according to five indicators. What is notable is that welfare expansion is at least equally as common as welfare retrenchment. It is therefore a fruitful endeavor to include welfare state expansion in the analysis.

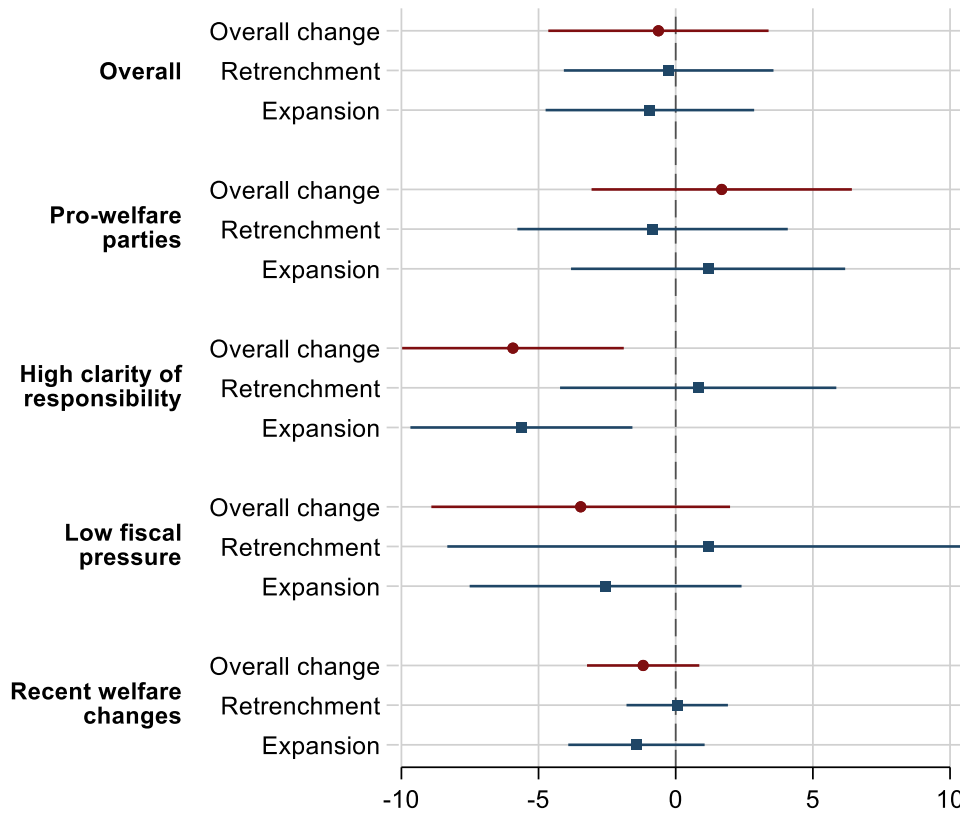
The further statistical analysis proceeds in three steps. We start with changes in social spending, which is the most general and coarse of our three indicators. We then turn to measures of welfare generosity and finally to legislative changes. In each case, we use the full set of observations available after listwise deletion. Note that this amount varies somewhat between the independent variables because of different data availability.¹⁰

The effects of social spending changes

Figure 7.3 depicts the empirical results regarding the electoral consequences of social spending changes. It plots selected slope coefficients with 95% confidence obtained from 10 regression models. The complete results are available in Appendix F. As with all results reported hereafter, the underlying regressions include the full set of control variables.

¹⁰ Most importantly, the welfare reform variables are only available up to 2014, whereas social spending and the generosity variables are available up to 2019 and 2018, respectively. Furthermore, the welfare reform variables are only available for five countries.

FIGURE 7.3: The effect of social spending changes on vote change

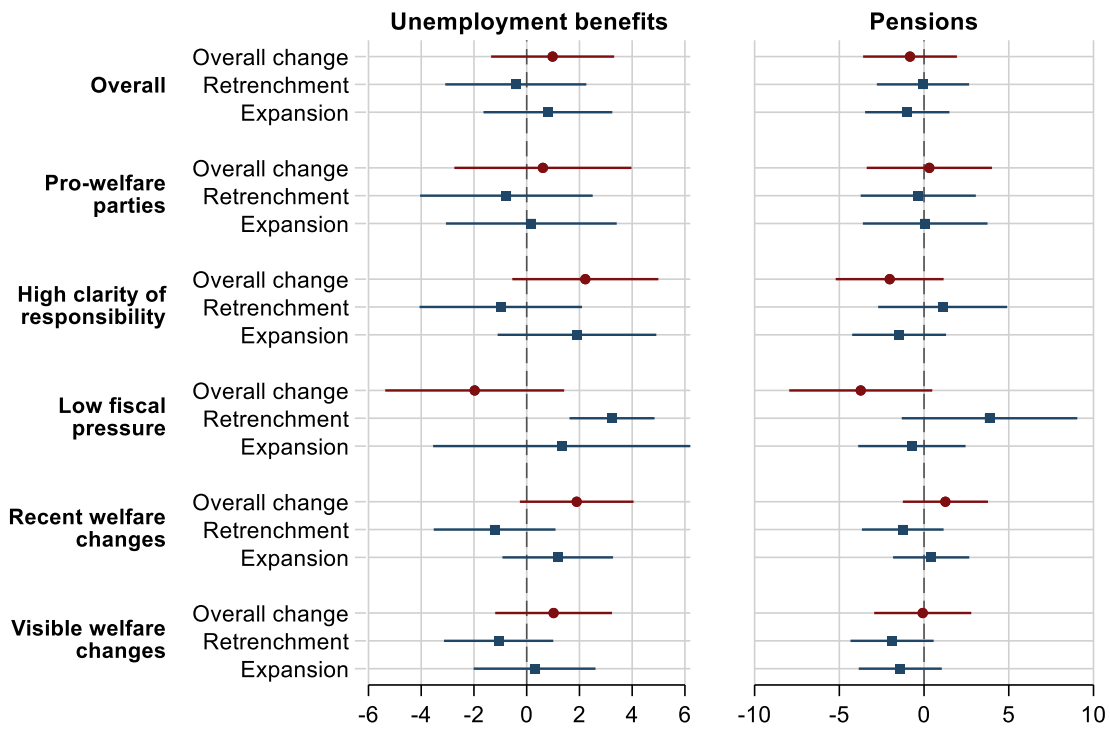


Note: The figure shows the results from 10 regression models that include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties. The full regression results are available in Appendix F.

The three regression coefficients on the top side of Figure 7.3 (labeled “overall”) indicate whether social spending changes have electoral consequences across the whole analysis sample. The top coefficient shows the effect of social spending changes in any direction, while the two coefficients right below present results from a disaggregated analysis of the effects of welfare retrenchment and expansion. To confirm the theoretical expectations, we should see a positive coefficient for overall welfare change, which would imply that expansion is rewarded and retrenchment punished; and we should see a positive coefficient for expansion as well as a negative coefficient for retrenchment.

Thereafter, Figure 7.3 presents results of analyses on whether social spending changes have electoral consequences under favorable conditions, i.e., when the government party is pro-welfare, there is high clarity of responsibility, under low economic pressure, and when the spending changes happen shortly before an election. To assess the former three conditional expectations, we estimate regression models with interaction terms between social spending and additional dummies (e.g., a pro-welfare dummy). We only report the relevant results that relate to the favorable conditions in Figure 7.3 (for example, we show the slope coefficients for pro-welfare parties but not for other parties). Again, the full results from the interaction models are available in Appendix F. For the analysis on recent social spending changes, we use the alternative change indicator that relates to changes in the last government year.

FIGURE 7.4: The effect of generosity changes on vote change



Note: The figure shows the results from 24 regression models that include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties. The full regression results are available in Appendix F.

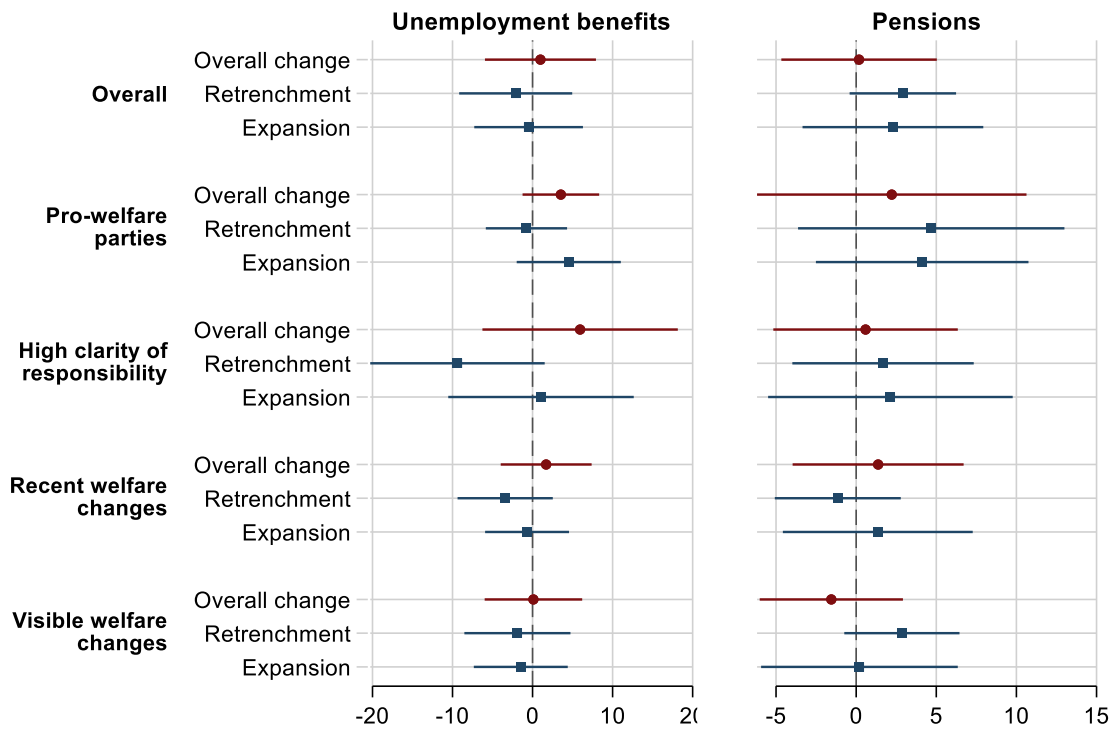
The results depicted in Figure 7.3 do not confirm that social spending has electoral consequences, neither overall nor under favorable conditions. Most estimated coefficients are small and insignificant, and the only coefficients with considerable effect size and/or significant hypothesis tests have results that point in the direction opposite of what the theoretical expectations are. Notably, the results suggest that social spending expansion has *negative* electoral effects under high clarity of responsibility.

The effect of generosity changes

We move on to the electoral consequences of welfare generosity changes. Figure 7.4 presents analyses on whether changes to unemployment benefit and pension generosity have electoral consequences across the whole sample as well as under favorable conditions. The empirical approach mirrors the approach of the social spending analysis above, except that we present an additional test of the effects of visible generosity changes, using our alternative generosity measure only relating to replacement rates and benefit duration.

The results depicted in Figure 7.4 do not confirm the prevalence of electoral consequences in line with theoretical expectations, neither across the whole sample nor under favorable conditions. The coefficients are mostly small and insignificant. There are some exceptions that hint at the prevalence of electoral consequences. Most importantly, welfare expansion has a positive and retrenchment a negative coefficient when unemployment benefits were changed under high clarity of responsibility and when the changes were recent. However, the coefficients remain small and the hypothesis tests are insignificant, implying that null effects cannot be ruled out. Furthermore, other results also directly contradict theoretical expectations, such as the positive

FIGURE 7.5: The effect of welfare reforms on vote change



Note: The figure shows the results from 20 regression models that include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties. The full regression results are available in Appendix F.

coefficients of unemployment benefit and pension retrenchment under low fiscal pressure. Overall, the results do not confirm the theoretical expectations.

The effect of welfare reforms

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

Again, the results depicted in Figure 7.5 do not confirm the prevalence of electoral consequences, neither across the whole sample nor under favorable conditions. There are some coefficients that are in line with theoretical expectations, although insignificantly. Notably, retrenchment of unemployment benefits under high clarity of responsibility has a negative and substantially important coefficient. However, other results also contradict the expectations, such as the positive coefficients of pension retrenchment under several conditions. Overall, the theoretical expectations are not confirmed.

Robustness tests

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

Firstly, we test whether we can tease out electoral consequences if we combine several favorable conditions that were analyzed separately above. We restrict the data to pro-welfare parties

and, in another analysis, to governments with high clarity of responsibility. We then re-estimate all conditional tests from above. This allows us to gauge whether, for example, pro-welfare parties under high clarity of responsibility are punished when they retrench the welfare state. Secondly, we repeat our analyses on conditional effects for pro-welfare parties using a different conceptualization of what constitutes a pro-welfare party. Following Schumacher et al. (2013), we additionally include Christian Democrats in the pro-welfare camp because they also tend to be proponents of the welfare state. Thirdly, we exclude the younger democracies Greece, Spain, and Portugal from the estimation sample. This is usually done in similar studies because these countries had a late transition to democracy, implying that there is no tradition of party competition over social policies (e.g., Armingeon and Giger 2008; Horn 2021). Fourthly, we omit all control variables from the regression models to show that the results are not a byproduct of our model specification. And lastly, we use a different specification of the dependent variable and measure vote change in terms of percentage *point* changes rather than percent changes, which follows previous quantitative research.

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

DISCUSSION AND CONCLUSION

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

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

We want to highlight that our study does not imply that welfare changes never have electoral implications. Several country studies suggest that welfare retrenchment can have crucial electoral costs (e.g. Arndt 2013; Schwander and Manow 2017). There is also evidence that governing parties paid a price for austerity measures implemented in the wake of the Great Recession (Bremer et al. 2020; Hobolt and Tilley 2016), although the evidence on austerity remains mixed overall (Jacques and Haffert 2021: 191). Based on both common sense and this evidence, we do

not question that governments which would implement more extreme reforms than what we observe in the data, such as halving pension payments, would be severely punished at the polls. It is also a different question whether welfare changes have long-term rather than immediate consequences, as Horn's (2021) analysis suggests. While welfare change may be mostly inconsequential in the short-term, for example because of stable partisan identification, prolonged discontent with welfare changes may lead to dealignment. Lastly, a more technical point is that, while we find no credible evidence for electoral consequences, statistically insignificant results do not necessarily imply the complete absence of effects. So, what should one make of our results?

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

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

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

Our results question the mechanistic worldview that underpins many theoretical approaches in welfare state and political economy research. People do not necessarily pay close attention to welfare changes to compare them to their (mostly fixed) preferences, which in turn informs their vote choice. This mechanism is pivotal, for example, for the insider-outsider dilemma of social democratic parties (Lindvall and Rueda 2014; Rueda 2005). Studies on the electoral backlash against austerity rely on a similar mechanism (Hübscher et al. 2021b; Jacques and Haffert 2021). While this research offers important insights, our findings emphasize that the potential

limitations regarding its required assumptions should be taken more seriously. For example, the finding that voters disapprove of spending cuts, as shown by survey experiments (Hübscher et al. 2021b), will not have the proposed electoral implications when voters do not observe actual spending cuts or when party elites are able to shift voter preferences in the political process. The insider-outsider and austerity literatures are only two examples, but the underlying mechanistic worldview exceeds them.

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Appendix A

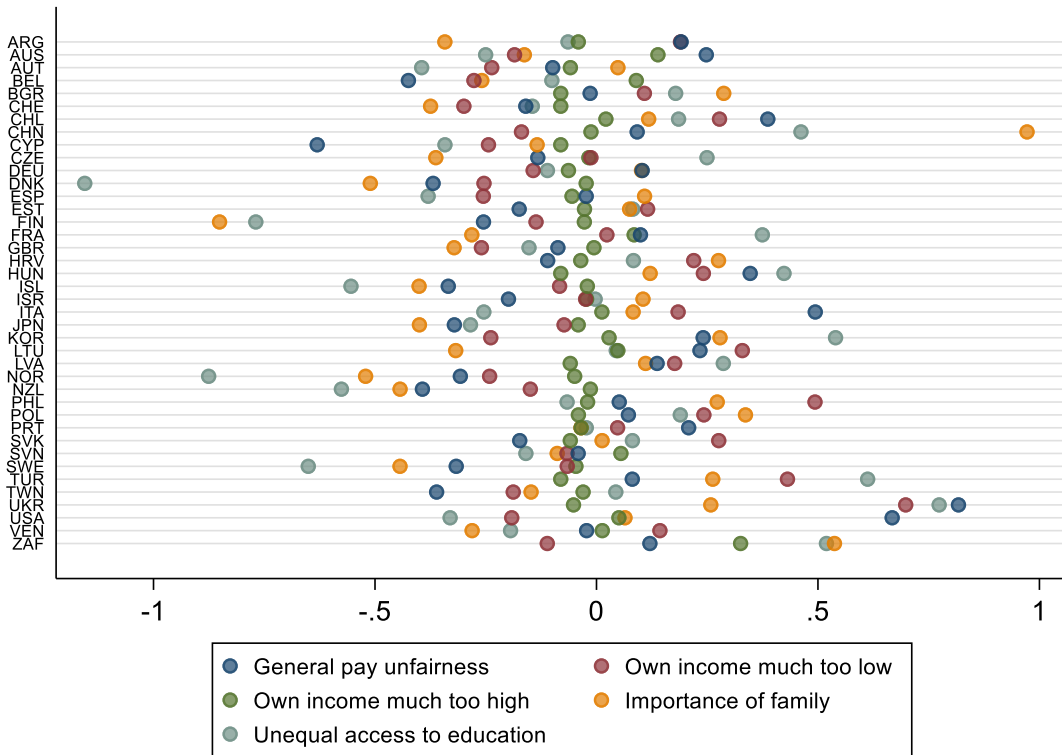
TABLE A1.1: Missing variables in the LIS income regressions

Country	Missing variables
Austria	Education father
Czech Republic	Education father, permanent employment dummy (only 2002)
Estonia	Education father (only 2007)
Finland	Immigrant dummy, education father
Germany	
Greece	Education father (only 2007)
Iceland	Region, education father
Ireland	Education father
Israel	Education father, permanent employment dummy
Lithuania	Immigrant dummy, education father
Luxembourg	Region, education father, rural place of living dummy
Netherlands	Region, education father, rural place of living dummy
Slovakia	
Spain	Education father (only 2007 & 2013)
Switzerland	Education father (only 2007 & 2013)
United Kingdom	Immigrant dummy, education father, permanent employment dummy (only 2004, 2007 & 2010), rural place of living dummy

TABLE A1.2: Data and variable description of empirical validation of the unfairness Gini

Data	2009 Social Inequality module of the International Social Survey Programme
Included countries (LIS data year in parentheses)	The following country-level sample resulted from a mutual exclusion process. Only countries are included which are represented in the ISSP data and for which the unfairness Gini could be estimated from a LIS dataset with temporal proximity to the ISSP data. The temporal match between the ISSP and LIS is unfortunately inferior to the match between ESS and LIS. Australia (2008), Austria (2007), Chile (2009), Czech Republic (2010), Estonia (2010), Finland (2010), Germany (2009), Hungary (2009), Iceland (2010), Italy (2008), Russia (2010), Slovakia (2010), South Africa (2008), Spain (2010), Switzerland (2010), United Kingdom (2010)
Sample	Working age population (18-65) in dependent employment
Var. 1: General pay unfairness	Summary measure. Respondents were asked to estimate what individuals in five professions actually earn and indicate what they should earn (the professions are unskilled workers, shop assistants, doctors in general practice, cabinet ministers, and chairmen of large corporations). In a first step, the magnitude of divergence between actual and ethical earnings is calculated for each profession. The value one indicates perfect congruence between actual and ethical earnings, whereas values above one show that actual earnings diverge from ethical earnings. A value of two, for example, shows that respondents think that a certain profession earns twice as much or half as much as it should. This divergence is averaged over all five professions. The variable is subsequently logged to deal with outliers and a heavily skewed distribution.
Var. 2: Own income is much too low	The proportion of respondents who answered “much less than just” to the following question: “Is your pay just? We are not asking about how much you would like to earn – but what you feel is just given your skills and effort”:
Var. 3: Own income is much too high	The proportion of respondents who answered “much more than just” to the following question: “Is your pay just? We are not asking about how much you would like to earn – but what you feel is just given your skills and effort”:
Var. 4: Importance of family for getting ahead	Unweighted average of responses to the following two questions. “To begin we have some questions about opportunities for getting ahead. Please tick one box for each of these to show how important you think it is for getting ahead in life.” “How important is coming from a wealthy family?” “How important is having well-educated parents?” Scale: 1-5
Var. 5: Unequal access to education	Unweighted average of responses to the following two questions. “To what extent do you agree or disagree with the following statements?” “In <R's country> only students from the best secondary schools have a good chance to obtain a university education.” “In <R's country> only the rich can afford the costs of attending university.” Scale: 1-5

FIGURE A1.1: Mean support of income unfairness perceptions across countries



Note: Figure A1.1 relies on the data and variables introduced in Table A2, whereas all countries available in the ISSP are included. To make the between-country variable comparable between countries, all variables are standardized across the whole dataset so that their mean equals zero and their standard deviation one.

TABLE A1.3: LIS and ESS datasets in use

Country	ESS 1	ESS 2	ESS 3	ESS 4	ESS 5	ESS 6	ESS 7
Austria		2004					2013
Czech Republic	2002	2004		2007	2010		2013
Estonia				2007	2010		
Finland				2007	2010		2013
Germany	2002	2004	2006	2008	2010	2012	2014
Greece				2007	2010		
Iceland		2004					
Ireland		2004		2007			
Israel	2001			2007	2010	2012	2014
Lithuania					2010		2013
Luxembourg		2004					
Netherlands		2004		2007	2010		2013
Slovakia		2004			2010		
Spain				2007	2010		2013
Switzerland				2007	2010		2013
United Kingdom		2004		2007	2010		2013

Note: The table displays which country-years of the unfairness Gini measure calculated from the LIS data are merged to which waves of the ESS data. The years in the cells indicate the data year from the LIS data. The ESS data refer to the following years: 2002 (ESS1), 2004 (ESS2), 2006 (ESS3), 2008 (ESS4), 2010 (ESS5), 2012 (ESS6), 2014 (ESS7). There are principally more datasets with mutual availability in the LIS and ESS, but in the remaining cases there were essential missing data in either LIS or ESS.

TABLE A1.4: Gini estimation results

Country	Year	Unfairness Gini	Personal labor income Gini (employee population)	Personal labor income Gini (whole population)	Personal total income Gini (employee population)	Personal total income Gini (whole population)	Household total income Gini (whole population)
AT	2004	0.2535626	0.384912	0.333839	0.382042	0.327691	0.318779
AT	2013	0.2939379	0.460317	0.393134	0.434938	0.36651	0.345628
CZ	2002	0.2548572	0.345051	0.297168	0.377822	0.276643	0.310341
CZ	2004	0.2304106	0.345511	0.286322	0.373841	0.269849	0.32144
CZ	2007	0.2243231	0.347601	0.286294	0.360067	0.269458	0.310123
CZ	2010	0.2312565	0.351019	0.303832	0.360266	0.287116	0.304068
CZ	2013	0.2393968	0.359877	0.3064	0.36019	0.294341	0.304639
EE	2007	0.2899007	0.522524	0.350278	0.451809	0.340014	0.366781
EE	2010	0.2977447	0.425344	0.366248	0.430095	0.351316	0.364659
FI	2007	0.2548706	0.430381	0.272776	0.396947	0.257545	0.330539
FI	2010	0.2328548	0.43575	0.258602	0.397698	0.246925	0.326078
FI	2013	0.2531793	0.438779	0.270492	0.392896	0.257552	0.325328
DE	2002	0.2652219	0.438528	0.382348	0.437221	0.367381	0.350805
DE	2004	0.2848896	0.445857	0.392944	0.43695	0.375152	0.355128
DE	2006	0.2912536	0.453217	0.401001	0.445822	0.38677	0.363362
DE	2008	0.2876091	0.448802	0.396694	0.441861	0.386094	0.363389
DE	2010	0.288545	0.45045	0.401516	0.439098	0.387574	0.35545
DE	2012	0.285867	0.449821	0.405254	0.438024	0.391026	0.358772
DE	2014	0.2733872	0.443336	0.394382	0.431486	0.380987	0.361616
GR	2007	0.2748025	0.445644	0.35579	0.434173	0.358334	0.374284
GR	2010	0.2235503	0.379271	0.297086	0.390024	0.298783	0.358092
IS	2004	0.2922138	0.42026	0.349285	0.39998	0.344604	0.306648
IE	2004	0.2954362	0.446623	0.39634	0.577649	0.370014	0.394469
IE	2007	0.2827006	0.462097	0.39779	0.568775	0.36894	0.373469
IL	2001	0.333025	0.442621	0.433035	0.442621	0.433035	0.421933
IL	2007	0.3238923	0.465922	0.417404	0.465922	0.417404	0.421403
IL	2010	0.3331662	0.474438	0.422516	0.474438	0.422516	0.437565
IL	2012	0.3338394	0.466593	0.450049	0.466593	0.450049	0.414343
IL	2014	0.3182458	0.46234	0.449443	0.46234	0.449443	0.404958
LT	2010	0.3260387	0.420645	0.387226	0.415025	0.374851	0.369248
LT	2013	0.3138558	0.441739	0.364808	0.437499	0.353142	0.389549
LU	2004	0.2561179	0.404068	0.372857	0.405345	0.367042	0.318625
NL	2004	0.2094011	0.444983	0.346339	0.426272	0.334101	0.325817
NL	2007	0.2539908	0.458033	0.357373	0.429937	0.342027	0.335707
NL	2010	0.2317674	0.446714	0.343328	0.41743	0.327791	0.320989
NL	2013	0.2379448	0.46513	0.363261	0.43532	0.349857	0.332952
SK	2004	0.2608595	0.32472	0.298421	0.35589	0.285325	0.318073
SK	2010	0.2320941	0.343685	0.276927	0.352092	0.267485	0.296377
ES	2007	0.258044	0.386857	0.328606	0.38475	0.320264	0.337691
ES	2010	0.2361435	0.373244	0.3188	0.389181	0.308636	0.351232
ES	2013	0.2969099	0.480747	0.392805	0.440284	0.373236	0.376915
CH	2007	0.2821779	0.447688	0.403235	0.434194	0.39116	0.3208
CH	2010	0.2516013	0.441719	0.390101	0.42099	0.374574	0.310442
CH	2013	0.2661356	0.446883	0.401452	0.424294	0.386262	0.311458
UK	2004	0.3230144	0.43904	0.405371	0.474585	0.399342	0.388095
UK	2007	0.2780944	0.430897	0.398523	0.484025	0.374722	0.384999
UK	2010	0.2813751	0.440507	0.409445	0.48042	0.382728	0.380897
UK	2013	0.2871877	0.435165	0.407576	0.456474	0.380169	0.375181

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TABLE A1.5: Further robustness checks

	(1)	(2)	(3)	(4)	(5)	(6)
Unfairness Gini	1.04*** (0.27)		0.95*** (0.27)		1.12*** (0.34)	
Unfairness Gini (within)		1.02*** (0.32)		0.87*** (0.31)		1.28** (0.54)
Unfairness Gini (between)		1.10** (0.51)		1.13** (0.51)		0.96** (0.43)
Left-right			-0.02*** (0.00)	-0.02*** (0.00)	-0.02*** (0.00)	-0.02*** (0.00)
Gender (ref.: female)	-0.04*** (0.00)	-0.04*** (0.00)	-0.03*** (0.00)	-0.03*** (0.00)	-0.03*** (0.00)	-0.03*** (0.00)
Age	0.00*** (0.00)	0.00*** (0.00)	0.00*** (0.00)	0.00*** (0.00)	0.00*** (0.00)	0.00*** (0.00)
Occupational risk	0.55*** (0.06)	0.55*** (0.06)	0.52*** (0.06)	0.52*** (0.06)	0.64*** (0.10)	0.64*** (0.10)
<i>Education</i>						
Below secondary	ref.	ref.	ref.	ref.	ref.	ref.
Lower secondary	-0.00 (0.01)	-0.00 (0.01)	-0.00 (0.01)	-0.00 (0.01)	-0.01 (0.01)	-0.01 (0.01)
Upper secondary	-0.01 (0.01)	-0.01 (0.01)	-0.00 (0.01)	-0.00 (0.01)	-0.02** (0.01)	-0.02** (0.01)
Post-secondary	-0.02* (0.01)	-0.02* (0.01)	-0.02* (0.01)	-0.02* (0.01)	-0.03*** (0.01)	-0.03*** (0.01)
Tertiary	-0.04*** (0.01)	-0.04*** (0.01)	-0.04*** (0.01)	-0.04*** (0.01)	-0.06*** (0.01)	-0.06*** (0.01)
<i>Income</i>						
1st income quintile	ref.	ref.	ref.	ref.	ref.	ref.
2nd income quintile	-0.01** (0.01)	-0.01** (0.01)	-0.02*** (0.01)	-0.02*** (0.01)	-0.01 (0.01)	-0.01 (0.01)
3rd income quintile	-0.03*** (0.01)	-0.03*** (0.01)	-0.04*** (0.01)	-0.04*** (0.01)	-0.02*** (0.01)	-0.02*** (0.01)
4th income quintile	-0.05*** (0.01)	-0.05*** (0.01)	-0.06*** (0.01)	-0.06*** (0.01)	-0.04*** (0.01)	-0.04*** (0.01)
5th income quintile	-0.11*** (0.01)	-0.11*** (0.01)	-0.11*** (0.01)	-0.11*** (0.01)	-0.10*** (0.01)	-0.10*** (0.01)
Household size (log)	0.01*** (0.00)	0.01*** (0.00)	0.01*** (0.00)	0.01*** (0.00)	0.01*** (0.00)	0.01*** (0.00)
Constant	0.37*** (0.08)	0.35** (0.14)	0.40*** (0.08)	0.34** (0.14)	0.34*** (0.10)	0.39*** (0.12)
Model	RI-ML	RI-ML	RI-ML	RI-ML	RS-ML	RS-ML
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Standard errors	OIM	OIM	OIM	OIM	OIM	OIM
Weighted	Yes	Yes	No	No	Yes	Yes
Observations	31,309	31,309	31,309	31,309	31,309	31,309
Number of countries	16	16	16	16	16	16
Number of country-years	48	48	48	48	48	48

Note: Standard errors in parentheses. * $p < .1$ ** $p < .05$ *** $p < .01$. RI-ML refers to a random intercept multilevel model and RI-MS to a random slope multilevel model (random intercepts are also included here). OIM refers to standard errors derived from the observed information matrix.

TABLE A1.6: Relative effect of other Gini measures

	(1)	(2)	(3)	(4)	(5)
Unfairness Gini	0.96*** (0.31)	1.43*** (0.38)	1.16*** (0.31)	1.22*** (0.39)	0.72** (0.33)
Personal labor income Gini (full sample)	0.09 (0.24)				
Personal labor income Gini (employee sample)		-0.45 (0.31)			
Personal total income Gini (full sample)			-0.22 (0.26)		
Personal total income Gini (employee sample)				-0.23 (0.34)	
Household total income Gini (full sample)					0.63 (0.41)
Left-right	-0.02*** (0.00)	-0.02*** (0.00)	-0.02*** (0.00)	-0.02*** (0.00)	-0.02*** (0.00)
Gender (ref.: female)	-0.03*** (0.00)	-0.03*** (0.00)	-0.03*** (0.00)	-0.03*** (0.00)	-0.03*** (0.00)
Age	0.00*** (0.00)	0.00*** (0.00)	0.00*** (0.00)	0.00*** (0.00)	0.00*** (0.00)
Occupational risk	0.52*** (0.06)	0.52*** (0.06)	0.52*** (0.06)	0.52*** (0.06)	0.52*** (0.06)
<i>Education</i>					
Below secondary	ref.	ref.	ref.	ref.	ref.
Lower secondary	-0.00 (0.01)	-0.00 (0.01)	-0.00 (0.01)	-0.00 (0.01)	-0.00 (0.01)
Upper secondary	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)
Post-secondary	-0.02** (0.01)	-0.02** (0.01)	-0.02** (0.01)	-0.02** (0.01)	-0.02** (0.01)
Tertiary	-0.05*** (0.01)	-0.05*** (0.01)	-0.05*** (0.01)	-0.05*** (0.01)	-0.05*** (0.01)
<i>Income</i>					
1st income quintile	ref.	ref.	ref.	ref.	ref.
2nd income quintile	-0.01** (0.01)	-0.01** (0.01)	-0.01** (0.01)	-0.01** (0.01)	-0.01** (0.01)
3rd income quintile	-0.03*** (0.01)	-0.03*** (0.01)	-0.03*** (0.01)	-0.03*** (0.01)	-0.03*** (0.01)
4th income quintile	-0.05*** (0.01)	-0.05*** (0.01)	-0.05*** (0.01)	-0.05*** (0.01)	-0.05*** (0.01)
5th income quintile	-0.10*** (0.01)	-0.10*** (0.01)	-0.10*** (0.01)	-0.10*** (0.01)	-0.10*** (0.01)
Household size (log)	0.01*** (0.00)	0.01*** (0.00)	0.01*** (0.00)	0.01*** (0.00)	0.01*** (0.00)
Constant	0.35*** (0.10)	0.41*** (0.09)	0.42*** (0.10)	0.39*** (0.09)	0.24** (0.12)
Model	RI-ML	RI-ML	RI-ML	RI-ML	RI-ML
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Standard errors	OIM	OIM	OIM	OIM	OIM
Weighted	Yes	Yes	Yes	Yes	Yes
Observations	31,309	31,309	31,309	31,309	31,309
Number of countries	16	16	16	16	16
Number of country-years	48	48	48	48	48

Note: Standard errors in parentheses. * $p < .1$ ** $p < .05$ *** $p < .01$. RI-ML refers to a random intercept multi-level model. OIM refers to standard errors derived from the observed information matrix.

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TABLE A1.7: Within- and between decomposition of unfair inequality

	(1)	(2)	(3)	(4)	(5)
Unfairness Gini (within)	0.83** (0.38)	0.97*** (0.32)	0.97*** (0.25)	0.88** (0.35)	0.83*** (0.27)
Unfairness Gini (between)	1.21** (0.61)	1.16** (0.52)	1.16** (0.49)	1.14* (0.54)	
Left-right		-0.02*** (0.00)	-0.02*** (0.00)	-0.02*** (0.00)	-0.02*** (0.00)
Gender (ref.: female)		-0.03*** (0.00)	-0.03*** (0.00)	-0.03*** (0.00)	-0.03*** (0.00)
Age		0.00*** (0.00)	0.00*** (0.00)	0.00*** (0.00)	0.00*** (0.00)
Occupational risk		0.52*** (0.06)	0.52*** (0.15)	0.52*** (0.06)	0.53*** (0.18)
<i>Education</i>					
Below secondary		ref.	ref.	ref.	ref.
Lower secondary		-0.00 (0.01)	-0.00 (0.01)	-0.00 (0.01)	-0.00 (0.01)
Upper secondary		-0.01 (0.01)	-0.01 (0.01)	-0.00 (0.01)	-0.01 (0.01)
Post-secondary		-0.02** (0.01)	-0.02* (0.01)	-0.02* (0.01)	-0.02 (0.01)
Tertiary		-0.05*** (0.01)	-0.05*** (0.01)	-0.04*** (0.01)	-0.05*** (0.01)
<i>Income</i>					
1st income quintile		ref.	ref.	ref.	ref.
2nd income quintile		-0.01** (0.01)	-0.01*** (0.00)	-0.02*** (0.01)	-0.02*** (0.00)
3rd income quintile		-0.03*** (0.01)	-0.03*** (0.01)	-0.04*** (0.01)	-0.03*** (0.01)
4th income quintile		-0.05*** (0.01)	-0.05*** (0.01)	-0.06*** (0.01)	-0.05*** (0.01)
5th income quintile		-0.10*** (0.01)	-0.10*** (0.01)	-0.11*** (0.01)	-0.10*** (0.01)
Household size (log)		0.01*** (0.00)	0.01*** (0.00)	0.01*** (0.00)	
Constant	0.37** (0.17)	0.33** (0.14)	0.33** (0.14)	0.34** (0.15)	0.65*** (0.03)
Model	RI-ML	RI-ML	RI-ML	RI-ML	FE
Year fixed effects	No	Yes	Yes	Yes	Yes
Standard errors	OIM	OIM	Robust	DF-adjust	Robust
Weighted	Yes	Yes	Yes	No	No
Observations	31,309	31,309	31,309	31,309	31,309
Number of countries	16	16	16	16	16
Number of country-years	48	48	48	48	48

Note: Standard errors in parentheses. * $p < .1$ ** $p < .05$ *** $p < .01$. RI ML refers to a random intercept multi-level model and FE to a fixed effects panel model. OIM refers to standard errors derived from the observed information matrix, Robust to robust standard errors clustered by countries, and DF-adjust to degrees-of-freedom adjusted standard errors following the approach of Elff et al. (2021).

Appendix B

TABLE A2.1: Observed values in wave 1 before and after listwise deletion

Variable	N (full sample)	Mean (full sample)	N (after listwise deletion)	Mean (after listwise deletion)
Age	29,072	52	18,300	51
Gender: Female	29,072	0.49	18,300	0.49
Household income	21,621	35,734	18,300	35,014
Education: No qualification	21,621	0.09	18,300	0.08
Education: Below GCSE	24,398	0.05	18,300	0.05
Education: GCSE	24,398	0.21	18,300	0.21
Education: A-level	24,398	0.2	18,300	0.2
Education: Undergraduate	24,398	0.34	18,300	0.34
Education: Postgraduate	24,398	0.11	18,300	0.12
Employment: Employed (full time)	29,052	0.13	18,300	0.13
Employment: Employed (part time)	29,052	0.39	18,300	0.42
Employment: Unemployed	29,052	0.03	18,300	0.03
Employment: Not in labor force	29,052	0.44	18,300	0.41

Note: Values rounded.

TABLE A2.2: Observed values in wave 14 before and after listwise deletion

Variable	N (full sample)	Mean (full sample)	N (after listwise deletion)	Mean (after listwise deletion)
Age	29,268	53	20,529	53
Gender: Female	29,268	0.54	20,529	0.53
Household income	21,719	36,021	20,529	36,183
Education: No qualification	27,286	0.07	20,529	0.07
Education: Below GCSE	27,286	0.04	20,529	0.04
Education: GCSE	27,286	0.21	20,529	0.21
Education: A-level	27,286	0.22	20,529	0.21
Education: Undergraduate	27,286	0.37	20,529	0.37
Education: Postgraduate	27,286	0.09	20,529	0.09
Employment: Employed (full time)	29,268	0.36	20,529	0.39
Employment: Employed (part time)	29,268	0.14	20,529	0.14
Employment: Unemployed	29,268	0.018	20,529	0.017
Employment: Not in labor force	29,268	0.48	20,529	0.45

Note: Values rounded.

TABLE A2.3: Observed values of respondents observed in both wave 1 and 14 after listwise deletion

Variable	N (after listwise deletion)	Mean (after listwise deletion)
Age	6,866	53
Gender: Female	6,866	0.49
Household income	6,160	34,023
Education: No qualification	6,866	0.08
Education: Below GCSE	6,866	0.05
Education: GCSE	6,866	0.23
Education: A-level	6,866	0.2
Education: Undergraduate	6,866	0.34
Education: Postgraduate	6,866	0.11
Employment: Employed (full time)	6,866	0.44
Employment: Employed (part time)	6,866	0.15
Employment: Unemployed	6,866	0.03
Employment: Not in labor force	6,866	0.38

Note: Values rounded.

TABLE A2.4: Detailed information on variables used in entropy balancing

DID analysis	Matching setup
20% income increase 20% income decrease 40% income increase 40% income decrease	Age Gender (categorical: men, women) Education (categorical: No qualification, below GCSE, GCSE, A level, undergraduate, postgraduate) Employment situation (categorical: full time employed, part time employed, unemployed, not in labor force) Household income (15 categories) Unemployment risk (categorical: very likely, likely, neither, unlikely, very unlikely) Vote intention (categorical: I would not vote, Conservative, Labour, Liberal Democrat, Scottish National Party, Plaid Cymru, United Kingdom Independence party, Green Party, British National Party, Other)
20% equivalized income increase 20% equivalized income decrease 40% equivalized income increase 40% equivalized income decrease	Age Gender (categorical: men, women) Education (categorical: No qualification, below GCSE, GCSE, A level, undergraduate, postgraduate) Employment situation (categorical: full time employed, part time employed, unemployed, not in labor force) Equivalized household income Unemployment risk (categorical: very likely, likely, neither, unlikely, very unlikely) Vote intention (categorical: I would not vote, Conservative, Labour, Liberal Democrat, Scottish National Party, Plaid Cymru, United Kingdom Independence party, Green Party, British National Party, Other) Household size
Lost employment	Age ⁺ Gender (categorical: men, women) Education (categorical: No qualification, below GCSE, GCSE, A level, undergraduate, postgraduate) Employment situation (categorical: full time employed, part time employed, unemployed, not in labor force) Household income Unemployment risk (categorical: very likely, likely, neither, unlikely, very unlikely)
Re-gained employment	Age ⁺ Gender (categorical: men, women) Education (categorical: No qualification, below GCSE, GCSE, A level, undergraduate, postgraduate) Employment situation (categorical: full time employed, part time employed, unemployed, not in labor force) Household income (categorical: above vs. below median income) (Unemployment risk could not be included because no matching weights could be found. However, an analysis shows that there is no major unbalance between the treatment and control groups)

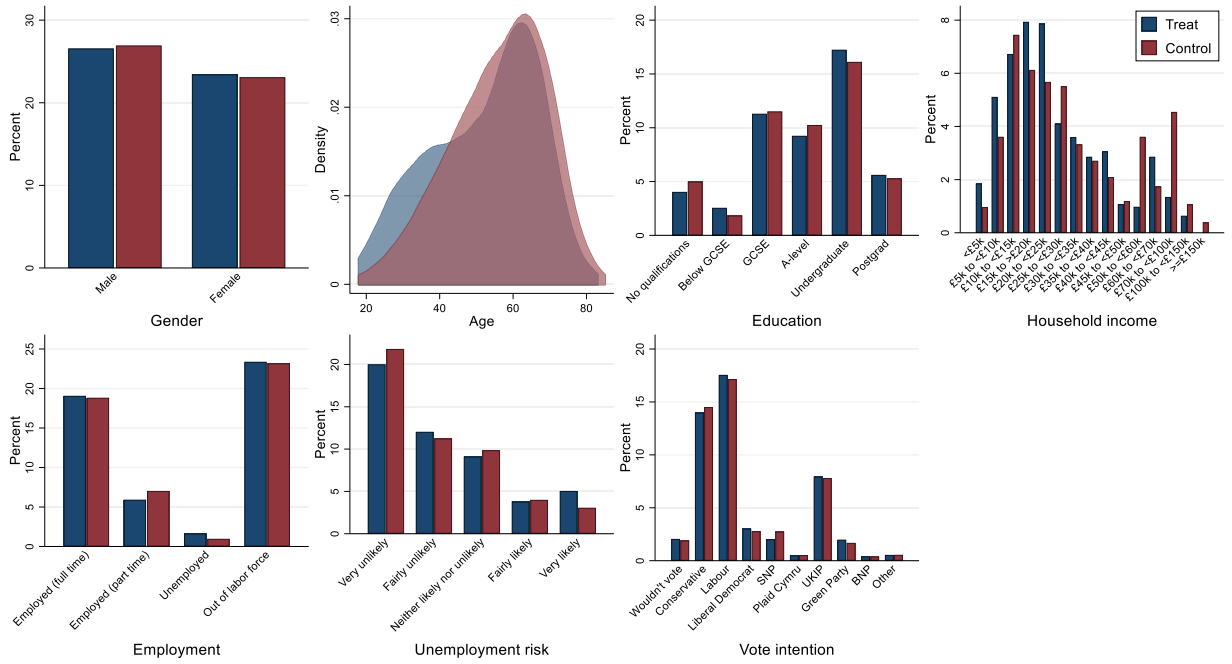
Note: Sometimes variables are included in categorical form in the matching procedure. This implies that dummy variables corresponding to the different categories are included as matching variables.

⁺ No respondents who were older than 60 in wave one became unemployed and no respondents older than 62 in wave one gained employment in the dataset. For this reason, respondents above these age thresholds are manually excluded from the respective control groups before matching.

Note regarding Figures A1.1-A1.6

The following figures each show the distribution of the variables used in the matching procedure within the different treatment and control group before applying entropy balancing. The data are re-weighted for the bar graphs so that the treatment and control groups each make up 50% of the sample. Variable balance (imbalance) is present when the bars of the treatment group (blue) and of the control group (blue) of a category (e.g., gender: male) have the same (different) height.

FIGURE A2.1: Distribution of matching variables before matching – 20% income gain analysis



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FIGURE A2.2: Distribution of matching variables before matching – 40% income gain analysis

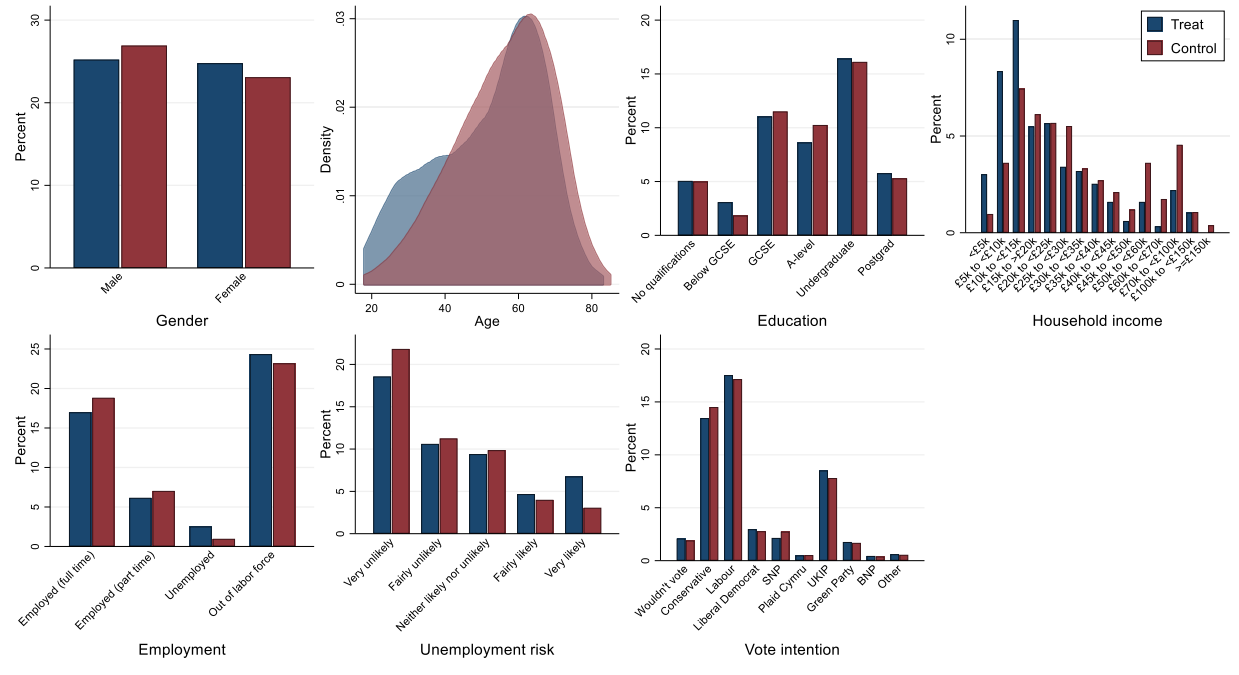


FIGURE A2.3: Distribution of matching variables before matching – 20% income loss analysis

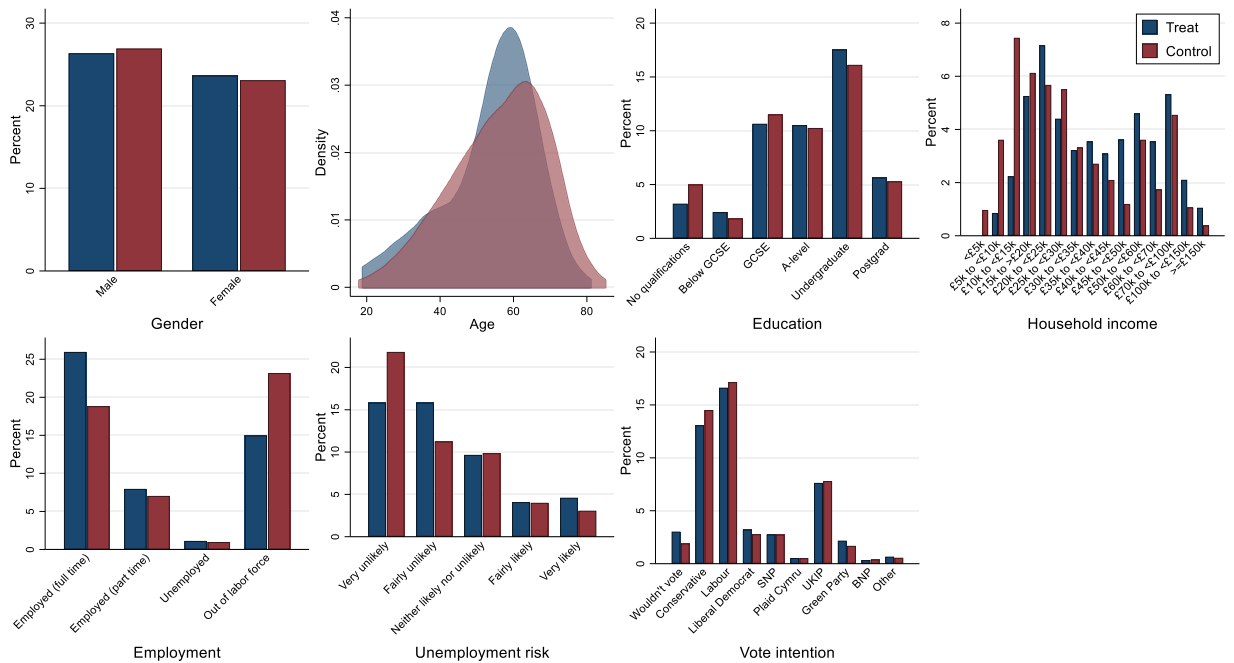


FIGURE A2.4: Distribution of matching variables before matching – 40% income loss analysis

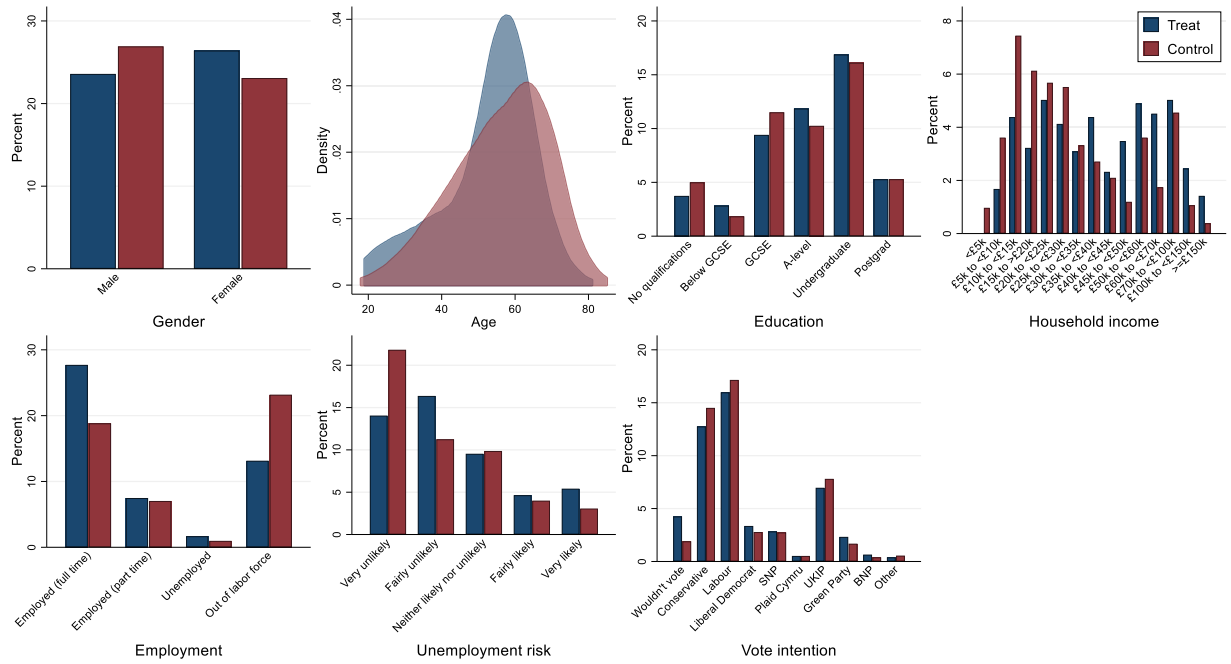
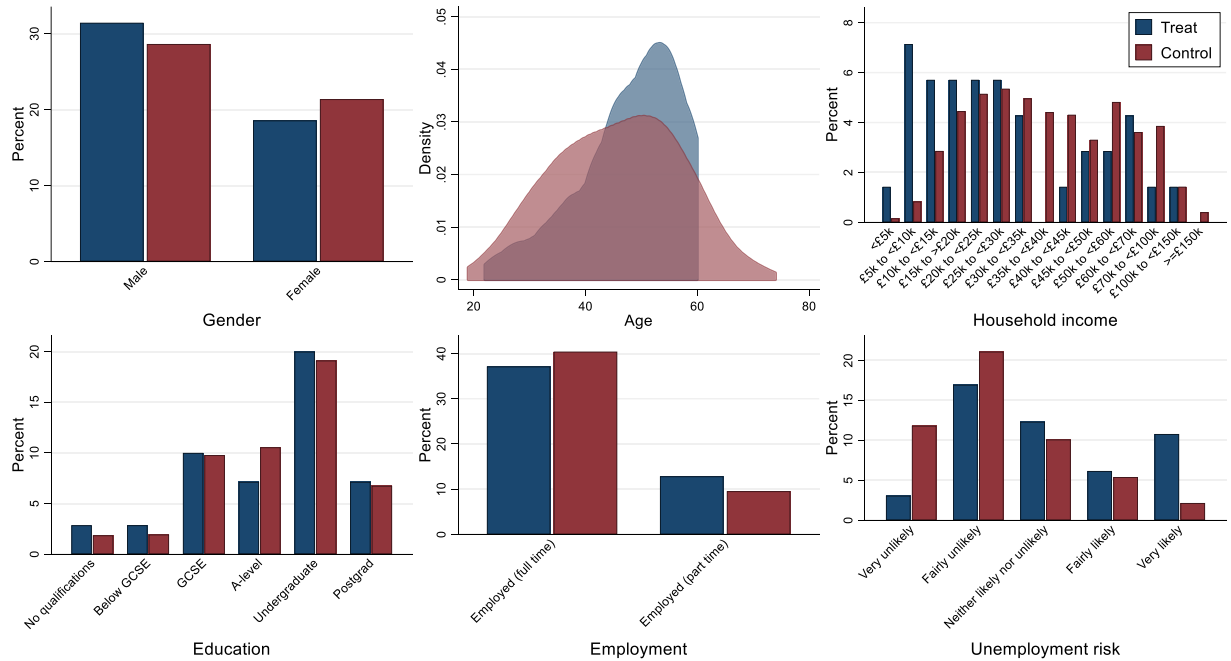


FIGURE A2.5: Distribution of matching variables before matching – losing employment analysis



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FIGURE A2.6: Distribution of matching variables before matching – gaining employment analysis

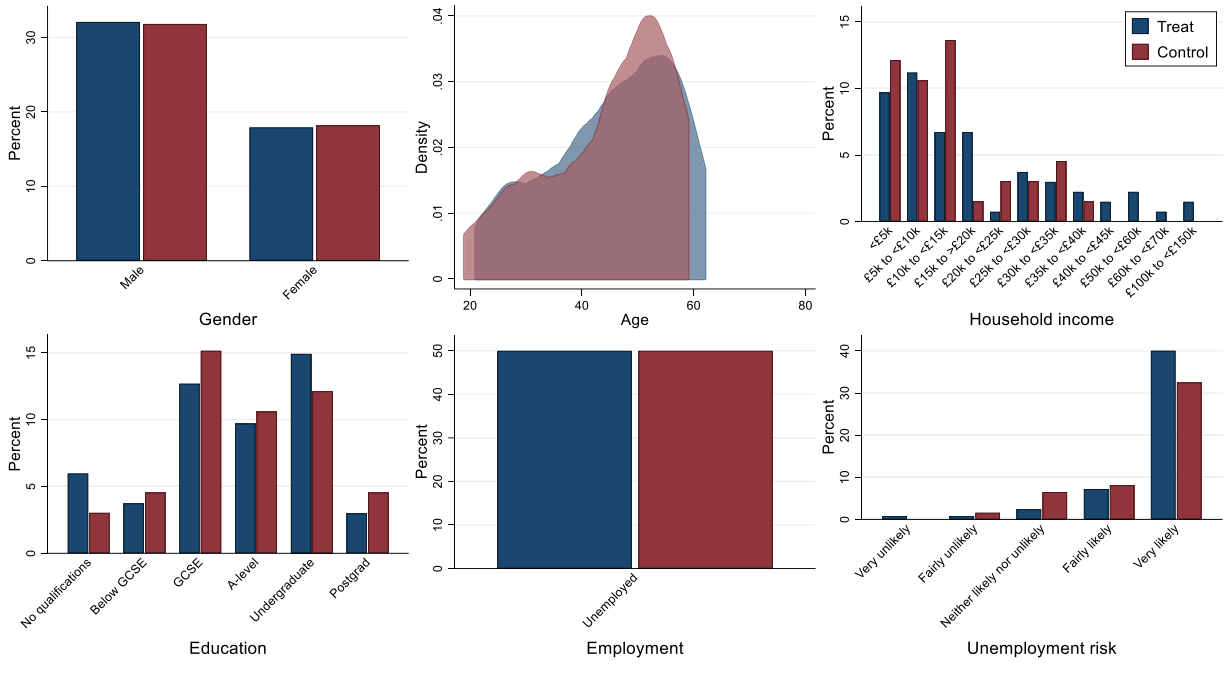


TABLE A2.5: Income and redistribution support (cross-section and FE/TWFE models)

	Cross-section	TWFE
Log income	-0.03*** (0.00)	-0.01*** (0.00)
Age	0.01*** (0.00)	
Age # Age	-0.00*** (0.00)	
Gender (ref.: male)	-0.03*** (0.00)	
Education (ref.: no education)		
Below GCSE	-0.04*** (0.01)	0.00 (0.03)
GCSE	-0.04*** (0.01)	0.03 (0.03)
A-level	-0.04*** (0.01)	0.03 (0.03)
Undergraduate	-0.01 (0.01)	0.02 (0.03)
Postgrad	0.03*** (0.01)	0.08** (0.04)
Employment (ref.: employed (full time))		
Employed (part time)	-0.01 (0.01)	-0.02 (0.01)
Unemployed	-0.01 (0.01)	-0.00 (0.03)
Out of labor force	0.01 (0.01)	-0.03*** (0.01)
Unemployment risk (ref.: very unlikely)		
Fairly unlikely	0.03*** (0.01)	0.00 (0.01)
Neither likely nor unlikely	0.06*** (0.01)	0.00 (0.01)
Fairly likely	0.08*** (0.01)	0.02* (0.01)
Very likely	0.10*** (0.01)	0.04*** (0.01)
Constant	1.11*** (0.04)	0.87*** (0.09)
Individual fixed effects	No	Yes
Time fixed effects	No	Yes
Observations	16,865	10,250

Note: Standard errors in parentheses. * $<.1$, ** $<.05$, *** $<.01$. The TWFE model uses cluster-robust standard errors.

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TABLE A2.6: Income and redistribution support (DID models)

	(1)	(2)	(3)	(4)
20% income increase or more # wave 14	-0.03** (0.01)			
20% income decrease or more # wave 14		-0.00 (0.01)		
40% income increase or more # wave 14			-0.04*** (0.01)	
40% income decrease or more # wave 14				0.01 (0.02)
Wave 14	-0.04*** (0.01)	-0.04*** (0.01)	-0.04*** (0.01)	-0.04*** (0.01)
Education (ref.: no education)				
Below GCSE	-0.03 (0.04)	0.05 (0.04)	-0.03 (0.05)	0.05 (0.06)
GCSE	-0.00 (0.04)	0.08** (0.03)	-0.02 (0.05)	0.09** (0.05)
A-level	-0.01 (0.05)	0.06 (0.04)	-0.04 (0.06)	0.05 (0.05)
Undergraduate	-0.01 (0.05)	-0.02 (0.05)	-0.03 (0.06)	-0.03 (0.07)
Postgrad	0.07 (0.06)	0.09 (0.06)	0.01 (0.07)	0.10 (0.09)
Employment (ref.: employed (full time))				
Employed (part time)	-0.01 (0.02)	0.00 (0.02)	-0.02 (0.02)	0.02 (0.02)
Unemployed	-0.01 (0.04)	0.03 (0.05)	-0.01 (0.04)	0.05 (0.05)
Out of labor force	-0.03 (0.02)	-0.00 (0.02)	-0.04* (0.02)	0.01 (0.02)
Unemployment risk (ref.: very unlikely)				
Fairly unlikely	-0.01 (0.01)	0.00 (0.01)	-0.01 (0.01)	0.02 (0.02)
Neither likely nor unlikely	-0.00 (0.01)	0.01 (0.01)	0.00 (0.01)	0.02 (0.02)
Fairly likely	0.01 (0.02)	0.02 (0.02)	0.02 (0.03)	0.04* (0.03)
Very likely	0.05** (0.02)	0.02 (0.02)	0.05** (0.02)	0.00 (0.03)
Constant	0.65*** (0.04)	0.59*** (0.04)	0.70*** (0.05)	0.58*** (0.05)
Observations	6,076	4,624	4,922	3,878

Note: Cluster-robust standard errors in parentheses. * $<.1$, ** $<.05$, *** $<.01$.

TABLE A2.7: Income robustness check: DID models based on absolute income changes

	(1)	(2)	(3)	(4)
£10,000 income increase or more # wave 14	-0.02* (0.01)			
£10,000 income decrease or more # wave 14		0.01 (0.02)		
£20,000 income increase or more # wave 14			-0.04** (0.02)	
£20,000 income decrease or more # wave 14				0.02 (0.02)
Wave 14	-0.04*** (0.01)	-0.04*** (0.01)	-0.04*** (0.01)	-0.03*** (0.02)
Education (ref.: no education)				
Below GCSE	0.00 (0.05)	0.09* (0.05)	-0.02 (0.10)	0.05 (0.09)
GCSE	0.02 (0.05)	0.10** (0.04)	0.03 (0.08)	0.07 (0.07)
A-level	-0.01 (0.05)	0.09* (0.05)	-0.04 (0.08)	0.06 (0.08)
Undergraduate	-0.03 (0.06)	-0.04 (0.07)	-0.11 (0.09)	-0.12 (0.10)
Postgrad	0.04 (0.07)	0.08 (0.08)	-0.02 (0.10)	0.04 (0.11)
Employment (ref.: employed (full time))				
Employed (part time)	0.01 (0.02)	-0.01 (0.02)	0.03 (0.03)	-0.00 (0.03)
Unemployed	0.03 (0.05)	0.04 (0.06)	0.01 (0.06)	0.09 (0.09)
Out of labor force	-0.02 (0.02)	-0.01 (0.02)	-0.02 (0.03)	-0.00 (0.03)
Unemployment risk (ref.: very unlikely)				
Fairly unlikely	-0.02 (0.01)	-0.00 (0.01)	-0.05** (0.02)	-0.01 (0.02)
Neither likely nor unlikely	-0.02 (0.01)	0.01 (0.02)	-0.03 (0.02)	0.02 (0.02)
Fairly likely	0.01 (0.02)	0.01 (0.03)	0.01 (0.03)	-0.02 (0.04)
Very likely	0.03 (0.03)	-0.00 (0.03)	-0.00 (0.04)	-0.05 (0.04)
Constant	0.63*** (0.05)	0.57*** (0.05)	0.70*** (0.08)	0.59*** (0.08)
Observations	5,210	4,344	3,938	3,406

Note: Cluster-robust standard errors in parentheses. *<.1, **<.05, ***<.01.

APPENDIX B

TABLE A2.8: Income robustness check: DID models using wave 1 & 10 data and equivalized income

	(1)	(2)	(3)	(4)
20% equivalized income increase or more # wave 10	-0.02** (0.01)			
20% equivalized income decrease or more # wave 10		0.00 (0.01)		
40% equivalized income increase or more # wave 10			-0.02* (0.01)	
40% equivalized income decrease or more # wave 10				0.00 (0.02)
Wave 10	-0.02*** (0.01)	-0.03*** (0.01)	-0.02** (0.01)	-0.03*** (0.01)
Unemployment risk (ref.: very unlikely)				
Fairly unlikely	0.01 (0.01)	0.02 (0.01)	-0.00 (0.02)	0.02 (0.02)
Neither likely nor unlikely	0.02 (0.01)	0.02* (0.01)	0.02 (0.02)	0.03 (0.02)
Fairly likely	0.04** (0.02)	0.05*** (0.02)	0.05** (0.02)	0.06** (0.03)
Very likely	-0.00 (0.02)	0.03 (0.02)	0.00 (0.02)	0.03 (0.03)
Constant	0.64*** (0.01)	0.62*** (0.01)	0.65*** (0.01)	0.63*** (0.01)
Observations	6,510	4,980	5,388	4,184

Note: Cluster-robust standard errors in parentheses. * < .1, ** < .05, *** < .01.

TABLE A2.9: Unemployment and redistribution support (cross-section and FE/TWFE models)

	Cross-section	TWFE
Unemployed (ref.: employed)	0.09*** (0.01)	0.03 (0.03)
Age	0.01*** (0.00)	
Age # Age	-0.00*** (0.00)	
Gender (ref.: male)	-0.02*** (0.01)	
Education (ref.: no education)		
Below GCSE	-0.04** (0.02)	0.05 (0.04)
GCSE	-0.05*** (0.01)	0.04 (0.03)
A-level	-0.06*** (0.01)	0.06 (0.04)
Undergraduate	-0.05*** (0.01)	0.03 (0.04)
Postgrad	-0.03** (0.01)	0.04 (0.05)
Constant	0.52*** (0.03)	0.58*** (0.03)
Individual fixed effects	No	Yes
Time fixed effects	No	Yes
Observations	13,595	7,236

Note: Standard errors in parentheses. * $<.1$, ** $<.05$, *** $<.01$. The TWFE model uses cluster-robust standard errors.

TABLE A2.10: Unemployment and redistribution support (DID models)

	(1)	(2)
Unemployed # wave 14	0.11*** (0.04)	
Re-employed # wave 14		-0.13** (0.06)
Wave 14	-0.06*** (0.01)	0.06 (0.04)
Education (ref.: no education)		
Below GCSE	0.04 (0.03)	0.13 (0.16)
GCSE	0.05 (0.05)	0.08 (0.14)
A-level	0.05 (0.06)	-0.01 (0.14)
Undergraduate	0.19*** (0.07)	-0.19*** (0.04)
Postgrad	0.20*** (0.07)	-0.25*** (0.05)
Constant	0.53*** (0.06)	0.78*** (0.07)
Observations	4,294	200

Note: Cluster-robust standard errors in parentheses. * $<.1$, ** $<.05$, *** $<.01$.

Appendix C

DETAILS ON THE CALCULATION AND IMPUTATION OF OCCUPATIONAL RISKS

Occupational unemployment. The occupational unemployment OU of occupation o is defined as (Cusack et al. 2006; Rehm 2009):

$$OU_o = \frac{n_{uo}}{N_o},$$

where n_{uo} refers to the number of unemployed persons u and N_o to the total number of persons in occupation o (i.e., the sum of employed and unemployed workers). To calculate these numbers, unemployed workers are assigned to the occupation of their previously held job.

Values of occupational unemployment are mere estimates containing measurement error because all quantities used to quantify it are only observed in survey samples. Values of the imputed versions of occupational unemployment are given by:

$$OUimp_o = \frac{\sum_{n_o=1}^{N_o} B(OU_o)}{N_o},$$

where B denotes random draws from a Bernoulli distribution with probability OU_o (i.e., the estimated occupational unemployment rate; see above) and N_o the total number of individuals observed within occupation o .

Skill specificity. The skill specificity SS of occupation o is defined as (Iversen and Soskice 2001):

$$SS_o = \frac{osg_o/tosg}{lshare_o} / skill_o,$$

where osg_o refers to the number of occupational subgroups of ISCO88 occupational category o on the next disaggregation level, and $tosg$ to the total number of subgroups on the most disaggregated level (i.e., 390). For example, ISCO88 one-digit code no. 8 (plant and machine operators and assemblers) is disaggregated into 70 two-digit codes, implying that the fraction equals $70/390=0.18$ (Iversen and Soskice 2001: 881). Furthermore, $lshare_o$ refers to the share of the labor force working in occupational category o , and $skill_o$ to this occupation's ISCO skill level.

The labor force shares used to calculate skill specificity are estimated quantities. Therefore, imputed values of skill specificity are given by:

$$SSimp_o = \frac{osg_o/tosg}{\sum_{n_o=1}^{N_o} B(lshare_o)/N_o} / skill_o,$$

where B denotes random draws from a Bernoulli distribution with probability $lshare_o$ (i.e., the estimated labor share) and N_o the total number of individuals observed within occupation o .

Routine task intensity. The routine task intensity (RTI) of occupation o is defined as:

$$RTI_o = routine_o - abstract_o - manual_o,$$

where $routine_o$ refers to the mean frequency of routine tasks performed by workers in occupation o , $abstract_o$ to the mean frequency of abstract tasks, and $manual_o$ to the mean frequency of manual tasks. All these quantities are estimated from the European Working Conditions Survey (EWCS) (see the main text and Sebastian 2018). The frequency variables in the EWCS are all on a scale from 0 to 1 and are best characterized as proportions. They describe what proportion of the time respondents perform a certain task.

RTI is calculated in a slightly different way compared to the original version of the indicator by Autor and Dorn (2013) and Sebastian’s (2018) adaption. They use logged versions of the skill frequencies of routine, abstract, and manual tasks:

$$RTI_o = \log(routine_o) - \log(abstract_o) - \log(manual_o).$$

Autor and Dorn (2013) presumably use these log-transformations because their skill frequency variables are count variables with a right-skewed distribution. I do not follow the approach because, as aforementioned, the frequency variables in the EWCS are best described as proportions, which are bounded between 0 and 1. The distributions of these frequency variables are also not right-skewed. However, robustness tests use the log-version of the RTI indicator.¹

Since the ECWS skill frequency variables should be understood as proportions, the imputed versions of the RTI indicator are given by:

$$RTI_{imp_o} = \left[\sum_{n_o=1}^{N_o} B(routine_o) / N_o \right] - \left[\sum_{n_o=1}^{N_o} B(abstract_o) / N_o \right] - \left[\sum_{n_o=1}^{N_o} B(manual_o) / N_o \right],$$

where B denotes random draws from Bernoulli distributions with probabilities $routine_o$, $abstract_o$, and $manual_o$ (i.e., the estimated task frequency proportions on a scale between 0 and 1); and N_o the total number of individuals observed within occupation o .² Therefore, separate random draws are used for routine, abstract, and manual tasks, and the results are then aggregated into the RTI measure.

Further notes on all estimated occupational risks. Random values are generally drawn on the occupational level. There is only one random draw per occupational category and imputation round. Put differently, respondents with the same occupational category receive the same random draw.

All occupational risk values derived from samples of less than 30 individuals are set to missing (for example, there are sometimes less than 30 individuals in ISCO three-digit occupations because the classification is so fine-grained). Such estimates based on less than 30 observations contain too much measurement error to contain any reliable information. High degrees of measurement error also have potential to bias the stage-two estimates because, for example, it becomes more likely that absurd occupational unemployment rates such as 50% are included in the analyses, which strongly affect least squares estimates. Experimentations with threshold

¹ The log-version of the RTI indicator used in the robustness tests does not consider measurement error, i.e. no differing “imputed” versions of the indicator are used. The reason is that random draws for the skill frequency variables often include the value zero, which cannot be log-transformed

² It would also be possible to use draws from normal distributions instead of Bernoulli distributions, so that: $RTI_{imp_o} = \log(N(routine_o, \sigma_{routine_o})) - \log(N(abstract_o, \sigma_{abstract_o})) - \log(N(manual_o, \sigma_{manual_o}))$, where N refers to random draws from normal distributions with, e.g., a mean of $routine_o$ (i.e. the estimated mean frequency of routine tasks in an occupation) and a standard deviation of $\sigma_{abstract}$ (i.e. the standard deviation of this estimate). The downside of this approach is that all frequency variables are bounded between 0 and 1. Random draws from normal distributions can fall outside these bounds, which is not the case with the Bernoulli approach.

values showed that excluding occupational samples of less than 30 is sufficient to get rid of such far-out outlier values, which is why I do not follow Rehm (2009) in top-coding occupational estimates at the 99th percentile.

FULL RESULTS OF THE SHP DATA ANALYSES

TABLE A3.1: The effect of occupational risks on perceived unemployment risk

	(1)	(2)	(3)	(4)	(5)
Occupational UE (ISCO 1d)	0.0081* (0.0043)	0.0061* (0.0035)			
Skill specificity (ISCO 2d)	0.0023 (0.0032)		0.00061 (0.0028)		
Routine task intensity (ISCO 2d)	0.016* (0.0087)			0.013 (0.0078)	
Offshorability	0.0063 (0.0066)				0.0050 (0.0056)
Secondary education	0.049** (0.022)	0.055** (0.021)	0.051** (0.022)	0.048** (0.022)	0.053** (0.021)
Post-secondary education	0.098*** (0.030)	0.096*** (0.028)	0.095*** (0.030)	0.094*** (0.029)	0.092*** (0.029)
Tertiary education	0.055** (0.023)	0.055** (0.022)	0.055** (0.023)	0.050** (0.023)	0.052** (0.022)
Personal income Q2	0.014** (0.0063)	0.013** (0.0061)	0.014** (0.0063)	0.015** (0.0063)	0.014** (0.0061)
Personal income Q3	0.0042 (0.0066)	0.0038 (0.0063)	0.0058 (0.0065)	0.0035 (0.0064)	0.0044 (0.0063)
Personal income Q4	-0.020*** (0.0076)	-0.018** (0.0071)	-0.019** (0.0075)	-0.020*** (0.0074)	-0.018** (0.0072)
Personal income Q5	-0.024*** (0.0085)	-0.020** (0.0080)	-0.023*** (0.0084)	-0.026*** (0.0083)	-0.022*** (0.0081)
Household income Q2	-0.0089 (0.0056)	-0.012** (0.0054)	-0.011* (0.0054)	-0.0090 (0.0055)	-0.012** (0.0055)
Household income Q3	-0.014** (0.0061)	-0.015*** (0.0058)	-0.013** (0.0059)	-0.014** (0.0060)	-0.015*** (0.0059)
Household income Q4	-0.0072 (0.0062)	-0.010* (0.0058)	-0.0080 (0.0060)	-0.0073 (0.0061)	-0.011* (0.0059)
Household income Q5	-0.0097 (0.0068)	-0.016** (0.0063)	-0.011 (0.0066)	-0.011* (0.0066)	-0.014** (0.0064)
Constant	0.15*** (0.025)	0.13*** (0.021)	0.14*** (0.021)	0.16*** (0.024)	0.14*** (0.021)
Total observations	64,896	74,930	67,182	67,941	71,989
Individuals	13,606	14,812	13,968	13,921	14,504
Individual FE	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes

Note: Robust standard errors clustered by individuals in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A3.2: The effect of individual risks on perceived unemployment risk

	(1)	(2)	(3)	(4)	(5)
Temporary contract	0.10*** (0.0065)	0.10*** (0.0064)			
Short tenure	0.011** (0.0044)		0.018*** (0.0043)		
Private-sector employment	0.021*** (0.0044)			0.013*** (0.0045)	
Unemployed in previous year	0.039*** (0.015)				0.044*** (0.014)
Secondary education	0.070*** (0.026)	0.080*** (0.025)	0.048** (0.021)	0.046* (0.025)	0.051** (0.021)
Post-secondary education	0.14*** (0.033)	0.14*** (0.032)	0.089*** (0.028)	0.095*** (0.033)	0.093*** (0.028)
Tertiary education	0.068** (0.027)	0.081*** (0.026)	0.046** (0.022)	0.042 (0.026)	0.050** (0.022)
Personal income Q2	0.031*** (0.0067)	0.033*** (0.0066)	0.016*** (0.0060)	0.018*** (0.0067)	0.016*** (0.0060)
Personal income Q3	0.035*** (0.0070)	0.036*** (0.0069)	0.0066 (0.0062)	0.0045 (0.0068)	0.0069 (0.0062)
Personal income Q4	0.019** (0.0078)	0.017** (0.0076)	-0.015** (0.0070)	-0.015* (0.0076)	-0.015** (0.0070)
Personal income Q5	0.024*** (0.0086)	0.019** (0.0084)	-0.016** (0.0079)	-0.014 (0.0084)	-0.016** (0.0079)
Household income Q2	-0.015*** (0.0057)	-0.016*** (0.0055)	-0.013** (0.0054)	-0.014** (0.0058)	-0.013** (0.0054)
Household income Q3	-0.018*** (0.0060)	-0.018*** (0.0058)	-0.016*** (0.0058)	-0.016*** (0.0061)	-0.016*** (0.0058)
Household income Q4	-0.015** (0.0061)	-0.014** (0.0059)	-0.012** (0.0058)	-0.013** (0.0061)	-0.011** (0.0058)
Household income Q5	-0.020*** (0.0066)	-0.020*** (0.0064)	-0.017*** (0.0063)	-0.018*** (0.0067)	-0.017*** (0.0063)
Constant	0.062** (0.026)	0.074*** (0.024)	0.14*** (0.020)	0.14*** (0.024)	0.14*** (0.020)
Total observations	67,026	71,496	76,098	67,252	76,161
Individuals	13,933	14,464	14,964	13,968	14,972
Individual FE	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes

Note: Robust standard errors clustered by individuals in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

APPENDIX C

TABLE A3.3: The effect of occupational risks on perceived job insecurity

	(1)	(2)	(3)	(4)	(5)
Occupational UE (ISCO 1d)	0.0067*	0.0044			
	(0.0039)	(0.0034)			
Skill specificity (ISCO 2d)	-0.0016		-0.00018		
	(0.0030)		(0.0025)		
Routine task intensity (ISCO 2d)	0.0017			0.00054	
	(0.0086)			(0.0080)	
Offshorability	0.014**				0.0098*
	(0.0062)				(0.0056)
Secondary education	0.033**	0.035**	0.032**	0.034**	0.036***
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
Post-secondary education	0.065***	0.062***	0.065***	0.067***	0.061**
	(0.025)	(0.024)	(0.025)	(0.025)	(0.024)
Tertiary education	0.015	0.018	0.012	0.015	0.017
	(0.016)	(0.016)	(0.016)	(0.016)	(0.016)
Personal income Q2	-0.0029	-0.0023	-0.0031	-0.0029	-0.0029
	(0.0059)	(0.0056)	(0.0058)	(0.0058)	(0.0057)
Personal income Q3	-0.020***	-0.019***	-0.019***	-0.021***	-0.020***
	(0.0067)	(0.0065)	(0.0068)	(0.0066)	(0.0065)
Personal income Q4	-0.035***	-0.036***	-0.035***	-0.037***	-0.037***
	(0.0076)	(0.0073)	(0.0076)	(0.0075)	(0.0073)
Personal income Q5	-0.049***	-0.048***	-0.050***	-0.051***	-0.051***
	(0.0089)	(0.0085)	(0.0088)	(0.0087)	(0.0085)
Household income Q2	-0.0070	-0.0068	-0.0075	-0.0074	-0.0061
	(0.0053)	(0.0049)	(0.0052)	(0.0052)	(0.0050)
Household income Q3	-0.0075	-0.0075	-0.0073	-0.0077	-0.0065
	(0.0057)	(0.0053)	(0.0056)	(0.0056)	(0.0054)
Household income Q4	-0.0048	-0.0066	-0.0047	-0.0058	-0.0054
	(0.0060)	(0.0056)	(0.0059)	(0.0059)	(0.0057)
Household income Q5	-0.014**	-0.015**	-0.014**	-0.016**	-0.015**
	(0.0066)	(0.0062)	(0.0065)	(0.0065)	(0.0062)
Constant	0.24***	0.25***	0.26***	0.26***	0.25***
	(0.019)	(0.015)	(0.015)	(0.019)	(0.015)
Total observations	65,216	75,287	67,519	68,267	72,345
Individuals	13,663	14,866	14,023	13,979	14,558
Individual FE	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes

Note: Robust standard errors clustered by individuals in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A3.4: The effect of individual risks on perceived job insecurity

	(1)	(2)	(3)	(4)	(5)
Temporary contract	0.096*** (0.0070)	0.093*** (0.0068)			
Short tenure	-0.000029 (0.0042)		0.0049 (0.0039)		
Private-sector employment	0.026*** (0.0044)			0.019*** (0.0044)	
Unemployed in previous year	0.034** (0.013)				0.035*** (0.013)
Secondary education	0.060*** (0.017)	0.061*** (0.016)	0.031** (0.014)	0.035** (0.015)	0.031** (0.013)
Post-secondary education	0.10*** (0.026)	0.097*** (0.025)	0.055** (0.023)	0.064** (0.025)	0.055** (0.023)
Tertiary education	0.040** (0.019)	0.041** (0.018)	0.0079 (0.016)	0.013 (0.017)	0.0081 (0.016)
Personal income Q2	0.014** (0.0060)	0.017*** (0.0059)	-0.00100 (0.0056)	0.00085 (0.0060)	-0.00058 (0.0056)
Personal income Q3	0.0066 (0.0069)	0.0098 (0.0067)	-0.019*** (0.0064)	-0.021*** (0.0069)	-0.018*** (0.0064)
Personal income Q4	-0.0074 (0.0077)	-0.0054 (0.0075)	-0.037*** (0.0072)	-0.038*** (0.0076)	-0.037*** (0.0072)
Personal income Q5	-0.0099 (0.0089)	-0.012 (0.0086)	-0.047*** (0.0083)	-0.045*** (0.0088)	-0.046*** (0.0083)
Household income Q2	-0.0086* (0.0051)	-0.0086* (0.0049)	-0.0072 (0.0049)	-0.0073 (0.0052)	-0.0074 (0.0049)
Household income Q3	-0.0080 (0.0055)	-0.0086 (0.0054)	-0.0079 (0.0053)	-0.0070 (0.0056)	-0.0076 (0.0053)
Household income Q4	-0.0080 (0.0059)	-0.0080 (0.0057)	-0.0064 (0.0056)	-0.0069 (0.0060)	-0.0061 (0.0056)
Household income Q5	-0.017*** (0.0064)	-0.017*** (0.0062)	-0.015** (0.0062)	-0.016** (0.0065)	-0.015** (0.0062)
Constant	0.18*** (0.018)	0.19*** (0.017)	0.26*** (0.015)	0.25*** (0.017)	0.26*** (0.015)
Total observations	67,362	71,843	76,455	67,586	76,518
Individuals	13,987	14,520	15,015	14,021	15,023
Individual FE	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes

Note: Robust standard errors clustered by individuals in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

APPENDIX C

TABLE A3.5: The effect of occupational risks on social spending preferences

	(1)	(2)	(3)	(4)	(5)
Occupational UE (ISCO 1d)	-0.0065 (0.0075)	0.000074 (0.0060)			
Skill specificity (ISCO 2d)	0.00014 (0.0041)		-0.0018 (0.0045)		
Routine task intensity (ISCO 2d)	-0.015 (0.012)			-0.013 (0.010)	
Offshorability	-0.015 (0.0095)				-0.012 (0.0082)
Secondary education	0.034 (0.029)	0.022 (0.031)	0.020 (0.032)	0.038 (0.028)	0.035 (0.028)
Post-secondary education	0.012 (0.044)	0.0082 (0.043)	0.010 (0.045)	0.024 (0.042)	0.018 (0.041)
Tertiary education	0.051 (0.031)	0.034 (0.033)	0.036 (0.034)	0.054* (0.030)	0.050* (0.030)
Personal income Q2	-0.0021 (0.0091)	-0.0019 (0.0084)	-0.0014 (0.0088)	-0.0033 (0.0088)	-0.0016 (0.0086)
Personal income Q3	-0.012 (0.011)	-0.013 (0.0098)	-0.012 (0.010)	-0.014 (0.010)	-0.011 (0.010)
Personal income Q4	-0.013 (0.012)	-0.010 (0.011)	-0.011 (0.011)	-0.013 (0.011)	-0.0089 (0.011)
Personal income Q5	-0.030** (0.013)	-0.027** (0.012)	-0.027** (0.013)	-0.028** (0.013)	-0.025** (0.013)
Household income Q2	-0.0058 (0.0096)	-0.0069 (0.0088)	-0.0071 (0.0092)	-0.0056 (0.0093)	-0.0056 (0.0090)
Household income Q3	-0.018* (0.010)	-0.014 (0.0094)	-0.016 (0.0099)	-0.014 (0.0097)	-0.012 (0.0097)
Household income Q4	-0.027*** (0.010)	-0.020** (0.0097)	-0.025** (0.010)	-0.025** (0.010)	-0.019* (0.010)
Household income Q5	-0.027** (0.011)	-0.024** (0.010)	-0.026** (0.011)	-0.026** (0.011)	-0.021** (0.010)
Constant	0.62*** (0.034)	0.64*** (0.032)	0.64*** (0.032)	0.61*** (0.032)	0.63*** (0.029)
Total observations	42,149	49,086	44,192	44,520	46,731
Individuals	11,433	12,542	11,826	11,769	12,209
Individual FE	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes

Note: Robust standard errors clustered by individuals in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A3.6: The effect of individual risks on social spending preferences

	(1)	(2)	(3)	(4)	(5)
Temporary contract	0.00081 (0.0085)	0.00047 (0.0082)			
Short tenure	-0.00027 (0.0070)		-0.00027 (0.0063)		
Private-sector employment	-0.013* (0.0076)			-0.014* (0.0076)	
Unemployed in previous year	0.015 (0.018)				0.016 (0.016)
Secondary education	0.045 (0.033)	0.024 (0.031)	0.026 (0.028)	0.044 (0.033)	0.024 (0.027)
Post-secondary education	0.025 (0.047)	0.010 (0.044)	0.014 (0.041)	0.028 (0.047)	0.013 (0.040)
Tertiary education	0.065* (0.034)	0.041 (0.033)	0.038 (0.029)	0.063* (0.034)	0.036 (0.029)
Personal income Q2	0.000029 (0.0093)	0.0026 (0.0089)	-0.0021 (0.0084)	0.0011 (0.0092)	-0.0020 (0.0084)
Personal income Q3	-0.010 (0.011)	-0.0067 (0.011)	-0.011 (0.0098)	-0.010 (0.011)	-0.011 (0.0098)
Personal income Q4	-0.0075 (0.012)	-0.0052 (0.012)	-0.0092 (0.011)	-0.0089 (0.012)	-0.0093 (0.011)
Personal income Q5	-0.029** (0.014)	-0.024* (0.013)	-0.026** (0.012)	-0.030** (0.013)	-0.026** (0.012)
Household income Q2	-0.0022 (0.0096)	0.00020 (0.0090)	-0.0060 (0.0088)	-0.0017 (0.0096)	-0.0058 (0.0088)
Household income Q3	-0.0099 (0.010)	-0.0092 (0.0098)	-0.014 (0.0095)	-0.0099 (0.010)	-0.014 (0.0094)
Household income Q4	-0.018* (0.011)	-0.018* (0.010)	-0.019* (0.0097)	-0.017* (0.011)	-0.018* (0.0097)
Household income Q5	-0.020* (0.011)	-0.022** (0.011)	-0.022** (0.010)	-0.020* (0.011)	-0.022** (0.010)
Constant	0.63*** (0.035)	0.63*** (0.033)	0.63*** (0.028)	0.63*** (0.034)	0.64*** (0.028)
Total observations	42,359	45,702	49,273	42,560	49,326
Individuals	11,696	12,177	12,584	11,730	12,594
Individual FE	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes

Note: Robust standard errors clustered by individuals in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

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TABLE A3.7: The effect of unemployment on social spending preferences

	(1)
Currently unemployed	0.045** (0.019)
Secondary education	-0.0024 (0.031)
Post-secondary education	-0.010 (0.042)
Tertiary education	0.0084 (0.032)
Personal income Q2	-0.0025 (0.0082)
Personal income Q3	-0.011 (0.0096)
Personal income Q4	-0.0084 (0.011)
Personal income Q5	-0.024** (0.012)
Household income Q2	-0.0060 (0.0087)
Household income Q3	-0.014 (0.0093)
Household income Q4	-0.019** (0.0096)
Household income Q5	-0.023** (0.010)
Constant	0.66*** (0.032)
Total observations	49,990
Individuals	12,718
Individual FE	Yes
Wave FE	Yes

Note: Robust standard errors clustered by individuals in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A3.8: The effect of occupational risks on unemployment assistance preferences

	(1)	(2)	(3)	(4)	(5)
Occupational UE (ISCO 1d)	0.0042 (0.026)	0.0057 (0.022)			
Skill specificity (ISCO 2d)	-0.017 (0.032)		-0.0011 (0.017)		
Routine task intensity (ISCO 2d)	-0.062 (0.045)			-0.043 (0.038)	
Offshorability	0.049 (0.038)				0.048 (0.033)
Secondary education	-0.037 (0.13)	-0.035 (0.12)	-0.063 (0.12)	-0.022 (0.12)	-0.041 (0.13)
Post-secondary education	0.034 (0.17)	0.035 (0.16)	0.020 (0.17)	0.053 (0.16)	0.041 (0.17)
Tertiary education	-0.018 (0.14)	-0.023 (0.13)	-0.063 (0.14)	-0.0020 (0.13)	-0.017 (0.14)
Personal income Q2	-0.020 (0.036)	-0.019 (0.032)	-0.019 (0.034)	-0.023 (0.033)	-0.023 (0.034)
Personal income Q3	0.019 (0.038)	0.010 (0.034)	0.020 (0.036)	0.017 (0.035)	0.010 (0.035)
Personal income Q4	0.0067 (0.042)	-0.0037 (0.037)	-0.0057 (0.040)	0.0073 (0.039)	-0.00065 (0.038)
Personal income Q5	0.013 (0.047)	-0.016 (0.043)	-0.0041 (0.045)	0.012 (0.045)	-0.0074 (0.044)
Household income Q2	0.11*** (0.035)	0.056 (0.035)	0.093*** (0.035)	0.083** (0.034)	0.094*** (0.034)
Household income Q3	0.051 (0.036)	0.022 (0.035)	0.037 (0.036)	0.035 (0.035)	0.047 (0.034)
Household income Q4	0.036 (0.037)	0.011 (0.036)	0.023 (0.037)	0.017 (0.036)	0.034 (0.035)
Household income Q5	0.050 (0.040)	0.016 (0.038)	0.039 (0.040)	0.037 (0.039)	0.038 (0.039)
Constant	0.91*** (0.14)	1.05*** (0.12)	1.06*** (0.12)	0.97*** (0.13)	1.02*** (0.12)
Total observations	10,235	12,083	10,739	10,941	11,389
Individuals	6,041	6,940	6,289	6,397	6,644
Individual FE	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes

Note: Robust standard errors clustered by individuals in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

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TABLE A3.9: The effect of individual risks on unemployment assistance preferences

	(1)	(2)	(3)	(4)	(5)
Temporary contract	0.034 (0.033)	0.027 (0.030)			
Short tenure	0.0035 (0.024)		-0.0075 (0.023)		
Private-sector employment	-0.023 (0.026)			-0.024 (0.026)	
Unemployed in previous year	0.053 (0.089)				0.062 (0.086)
Secondary education	-0.037 (0.14)	-0.016 (0.13)	-0.065 (0.12)	-0.042 (0.13)	-0.068 (0.12)
Post-secondary education	0.14 (0.18)	0.16 (0.17)	0.0043 (0.16)	0.13 (0.17)	0.0035 (0.16)
Tertiary education	0.024 (0.15)	0.049 (0.14)	-0.039 (0.13)	0.021 (0.14)	-0.040 (0.13)
Personal income Q2	-0.035 (0.038)	-0.035 (0.035)	-0.025 (0.033)	-0.041 (0.037)	-0.025 (0.033)
Personal income Q3	0.0064 (0.041)	0.0022 (0.037)	0.0033 (0.034)	-0.0066 (0.038)	0.0073 (0.033)
Personal income Q4	0.00046 (0.047)	-0.0083 (0.041)	-0.0064 (0.038)	-0.014 (0.043)	-0.00064 (0.037)
Personal income Q5	-0.0075 (0.052)	-0.014 (0.046)	-0.018 (0.044)	-0.022 (0.048)	-0.011 (0.043)
Household income Q2	0.060* (0.036)	0.059* (0.035)	0.073** (0.034)	0.054 (0.036)	0.073** (0.034)
Household income Q3	0.024 (0.035)	0.022 (0.034)	0.037 (0.034)	0.018 (0.036)	0.036 (0.034)
Household income Q4	0.021 (0.036)	0.013 (0.035)	0.028 (0.035)	0.015 (0.037)	0.027 (0.035)
Household income Q5	0.015 (0.040)	0.0084 (0.037)	0.029 (0.038)	0.012 (0.040)	0.027 (0.038)
Constant	1.04*** (0.14)	1.02*** (0.13)	1.07*** (0.12)	1.07*** (0.13)	1.06*** (0.12)
Total observations	11,128	11,650	12,108	11,150	12,117
Individuals	6,464	6,728	6,966	6,470	6,967
Individual FE	Yes	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes	Yes

Note: Robust standard errors clustered by individuals in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

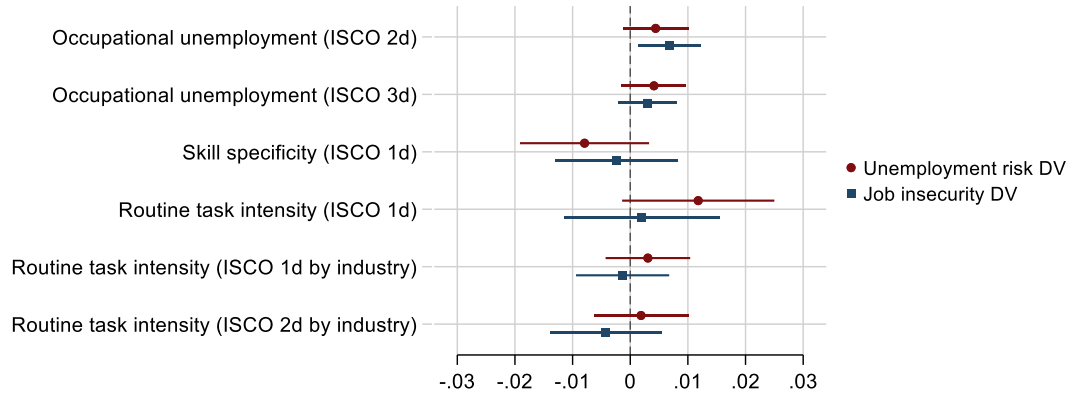
TABLE A3.10: The effect of unemployment on unemployment assistance preferences

	(1)
Currently unemployed	0.18*** (0.061)
Secondary education	-0.070 (0.12)
Post-secondary education	0.0073 (0.15)
Tertiary education	-0.048 (0.13)
Personal income Q2	-0.019 (0.032)
Personal income Q3	0.017 (0.033)
Personal income Q4	0.0047 (0.037)
Personal income Q5	-0.0025 (0.042)
Household income Q2	0.058* (0.034)
Household income Q3	0.024 (0.034)
Household income Q4	0.013 (0.035)
Household income Q5	0.020 (0.037)
Constant	1.08*** (0.11)
Total observations	12,304
Individuals	7,045
Individual FE	Yes
Wave FE	Yes

Note: Robust standard errors clustered by individuals in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

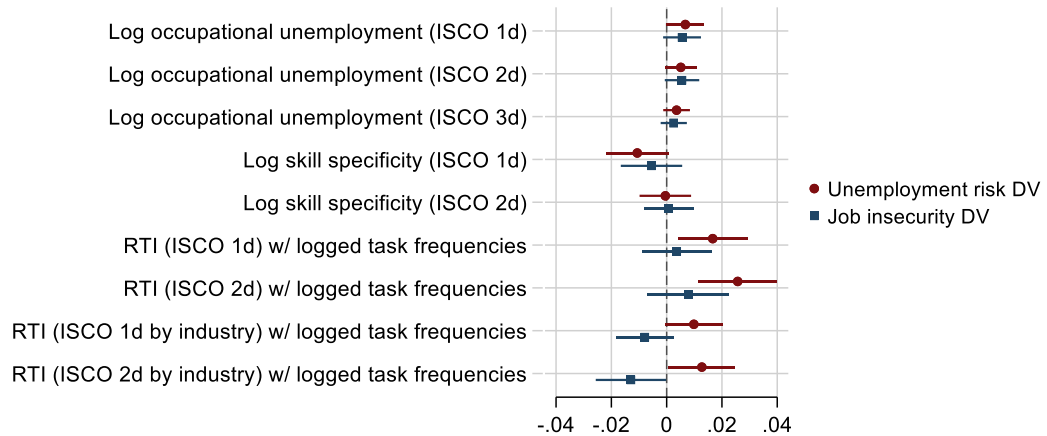
SENSITIVITY OF THE SHP DATA ANALYSES

FIGURE A3.1: Alternative occupational risk specifications and subjective risk



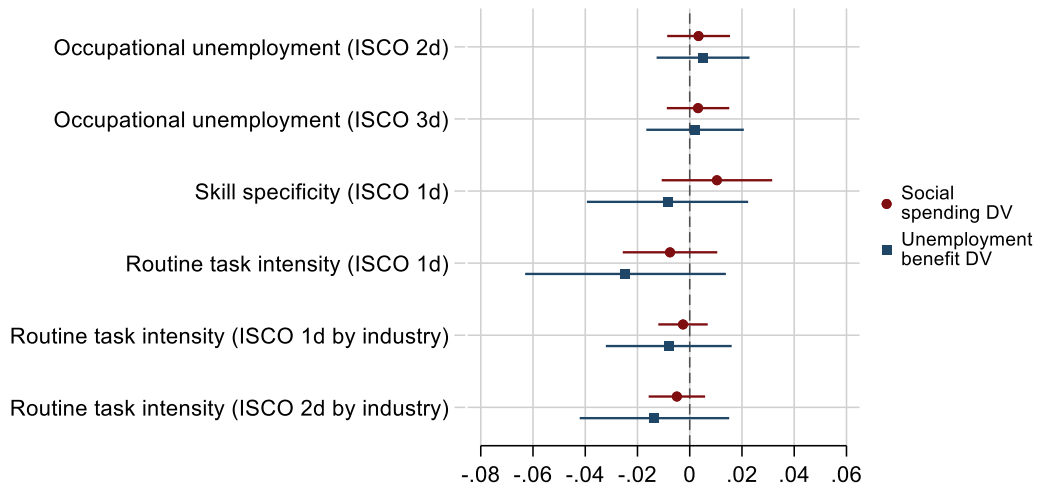
Note: The results are obtained from 12 separate regressions. That is, each displayed result is obtained from a separate regression model. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by individuals. The control variables (education, personal income, household income) as well as both individual and time fixed effects are included in the regressions.

FIGURE A3.2: Logged occupational risks and subjective risk



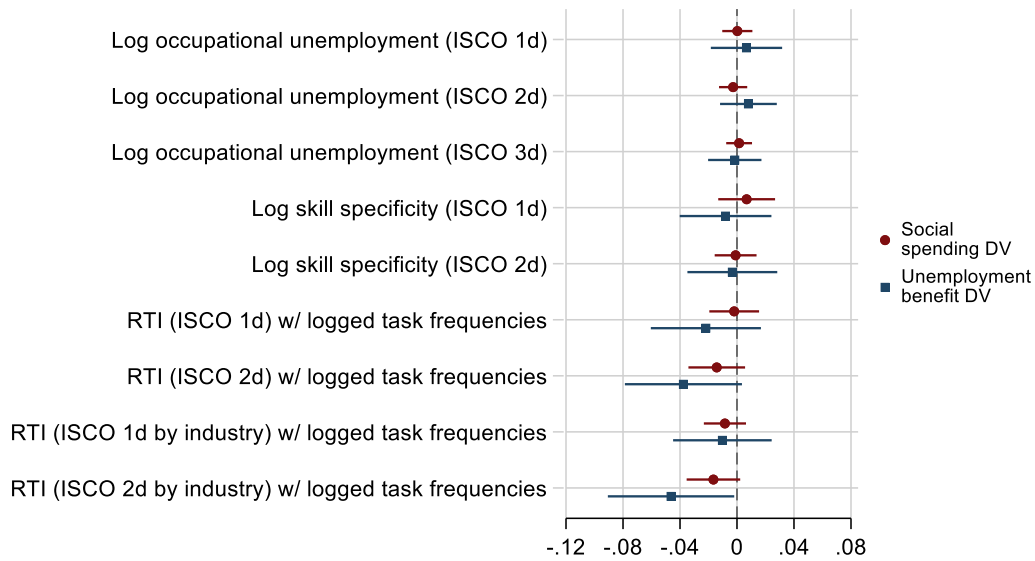
Note: The results are obtained from 18 separate regressions. That is, each displayed result is obtained from a separate regression model. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by individuals. The control variables (education, personal income, household income) as well as both individual and time fixed effects are included in the regressions.

FIGURE A3.3: Alternative occupational risk specifications and welfare preferences



Note: The results are obtained from 12 separate regressions. That is, each displayed result is obtained from a separate regression model. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by individuals. The control variables (education, personal income, household income) as well as both individual and time fixed effects are included in the regressions.

FIGURE A3.4: Logged occupational risks and social spending preferences



Note: The results are obtained from 18 separate regressions. That is, each displayed result is obtained from a separate regression model. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by individuals. The control variables (education, personal income, household income) as well as both individual and time fixed effects are included in the regressions.

FULL RESULTS AND SENSITIVITY OF THE ESS DATA ANALYSES

TABLE A3.11: The effect of labor market risks on perceived unemployment risk

	(1)	(2)	(3)	(4)
Occupational UE (ISCO 1d)	0.076*** (0.021)	0.029 (0.019)		
Skill specificity (ISCO 1d)	-0.016 (0.032)	-0.026 (0.030)		
Occupational UE (ISCO 2d)			0.064*** (0.018)	0.038** (0.019)
Skill specificity (ISCO 2d)			-0.0026 (0.0073)	-0.0017 (0.0072)
National unemployment		0.077*** (0.014)		0.059*** (0.015)
Gender	-0.0100 (0.0073)	-0.011 (0.0072)	-0.022*** (0.0076)	-0.022*** (0.0076)
Age	0.0020 (0.0016)	0.0017 (0.0016)	0.0023 (0.0017)	0.0020 (0.0016)
Age squared	-0.000047** (0.000018)	-0.000044** (0.000018)	-0.000048** (0.000020)	-0.000046** (0.000020)
Education years	-0.0029*** (0.0011)	-0.0032*** (0.0011)	-0.0023** (0.0011)	-0.0025** (0.0011)
2nd income quintile	-0.080*** (0.0099)	-0.078*** (0.0097)	-0.077*** (0.010)	-0.076*** (0.010)
3rd income quintile	-0.097*** (0.011)	-0.095*** (0.010)	-0.094*** (0.010)	-0.093*** (0.010)
4th income quintile	-0.13*** (0.012)	-0.12*** (0.011)	-0.12*** (0.011)	-0.12*** (0.011)
5th income quintile	-0.14*** (0.011)	-0.13*** (0.010)	-0.14*** (0.011)	-0.14*** (0.011)
Constant	0.42*** (0.051)	0.39*** (0.054)	0.41*** (0.040)	0.37*** (0.042)
Observations	25,829	25,829	25,064	25,064
Countries				
Country-occupations	222	222	602	602
Country-occupation FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes

Note: Robust standard errors clustered by country-occupations in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A3.12: The effect of labor market risks on redistribution preferences

	(1)	(2)	(3)	(4)
Occupational UE (ISCO 1d)	-0.021*** (0.0065)	-0.015* (0.0083)		
Skill specificity (ISCO 1d)	-0.013 (0.012)	-0.013 (0.012)		
Occupational UE (ISCO 2d)			-0.016*** (0.0044)	-0.0096* (0.0055)
Skill specificity (ISCO 2d)			0.0012 (0.0029)	0.0010 (0.0029)
National unemployment		-0.0053 (0.0079)		-0.0079 (0.0054)
Gender	-0.038*** (0.0026)	-0.038*** (0.0026)	-0.033*** (0.0030)	-0.033*** (0.0030)
Age	0.0025*** (0.00082)	0.0025*** (0.00081)	0.0026*** (0.00076)	0.0026*** (0.00075)
Age squared	-0.000020** (9.2e-06)	-0.000021** (9.1e-06)	-0.000022** (8.9e-06)	-0.000022** (8.9e-06)
Education years	-0.0027*** (0.00054)	-0.0027*** (0.00054)	-0.0028*** (0.00047)	-0.0028*** (0.00048)
2nd income quintile	-0.019*** (0.0045)	-0.019*** (0.0046)	-0.020*** (0.0041)	-0.020*** (0.0041)
3rd income quintile	-0.028*** (0.0041)	-0.028*** (0.0041)	-0.027*** (0.0039)	-0.028*** (0.0039)
4th income quintile	-0.048*** (0.0049)	-0.048*** (0.0049)	-0.047*** (0.0044)	-0.047*** (0.0044)
5th income quintile	-0.096*** (0.0063)	-0.096*** (0.0064)	-0.093*** (0.0055)	-0.093*** (0.0056)
Constant	0.71*** (0.023)	0.71*** (0.024)	0.69*** (0.018)	0.69*** (0.019)
Observations	108,595	108,595	105,664	105,664
Countries	28	28	28	28
Country-occupations	252	252	697	697
Country-occupation FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes

Note: Robust standard errors clustered by country-occupations in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

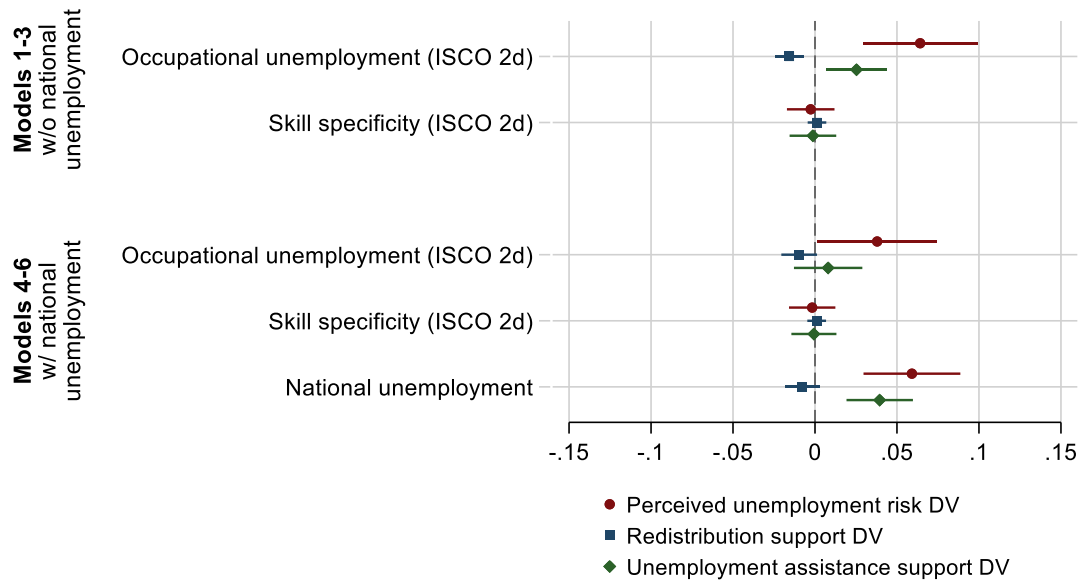
APPENDIX C

TABLE A3.13: The effect of labor market risks on unemployment benefit preferences

	(1)	(2)	(3)	(4)
Occupational UE (ISCO 1d)	0.030*** (0.0095)	0.0058 (0.010)		
Skill specificity (ISCO 1d)	-0.0070 (0.022)	-0.012 (0.021)		
Occupational UE (ISCO 2d)			0.025*** (0.0094)	0.0080 (0.011)
Skill specificity (ISCO 2d)			-0.0013 (0.0072)	-0.00067 (0.0070)
National unemployment		0.040*** (0.011)		0.039*** (0.010)
Gender	-0.0090** (0.0042)	-0.0093** (0.0042)	-0.0055 (0.0047)	-0.0058 (0.0048)
Age	-0.00062 (0.0013)	-0.00078 (0.0013)	-0.00052 (0.0013)	-0.00068 (0.0013)
Age squared	0.000017 (0.000015)	0.000019 (0.000016)	0.000016 (0.000015)	0.000018 (0.000015)
Education years	0.00029 (0.00081)	0.00016 (0.00081)	0.00020 (0.00087)	0.000046 (0.00088)
2nd income quintile	-0.040*** (0.0079)	-0.039*** (0.0079)	-0.037*** (0.0081)	-0.036*** (0.0082)
3rd income quintile	-0.048*** (0.0073)	-0.047*** (0.0073)	-0.048*** (0.0074)	-0.047*** (0.0074)
4th income quintile	-0.057*** (0.0087)	-0.056*** (0.0087)	-0.057*** (0.0090)	-0.056*** (0.0090)
5th income quintile	-0.067*** (0.0083)	-0.065*** (0.0082)	-0.065*** (0.0086)	-0.064*** (0.0086)
Constant	0.69*** (0.034)	0.67*** (0.034)	0.68*** (0.027)	0.65*** (0.028)
Observations	26,399	26,399	25,628	25,628
Countries	25	25	25	25
Country-occupations	222	222	602	602
Country-occupation FE	Yes	Yes	Yes	Yes
Wave FE	Yes	Yes	Yes	Yes

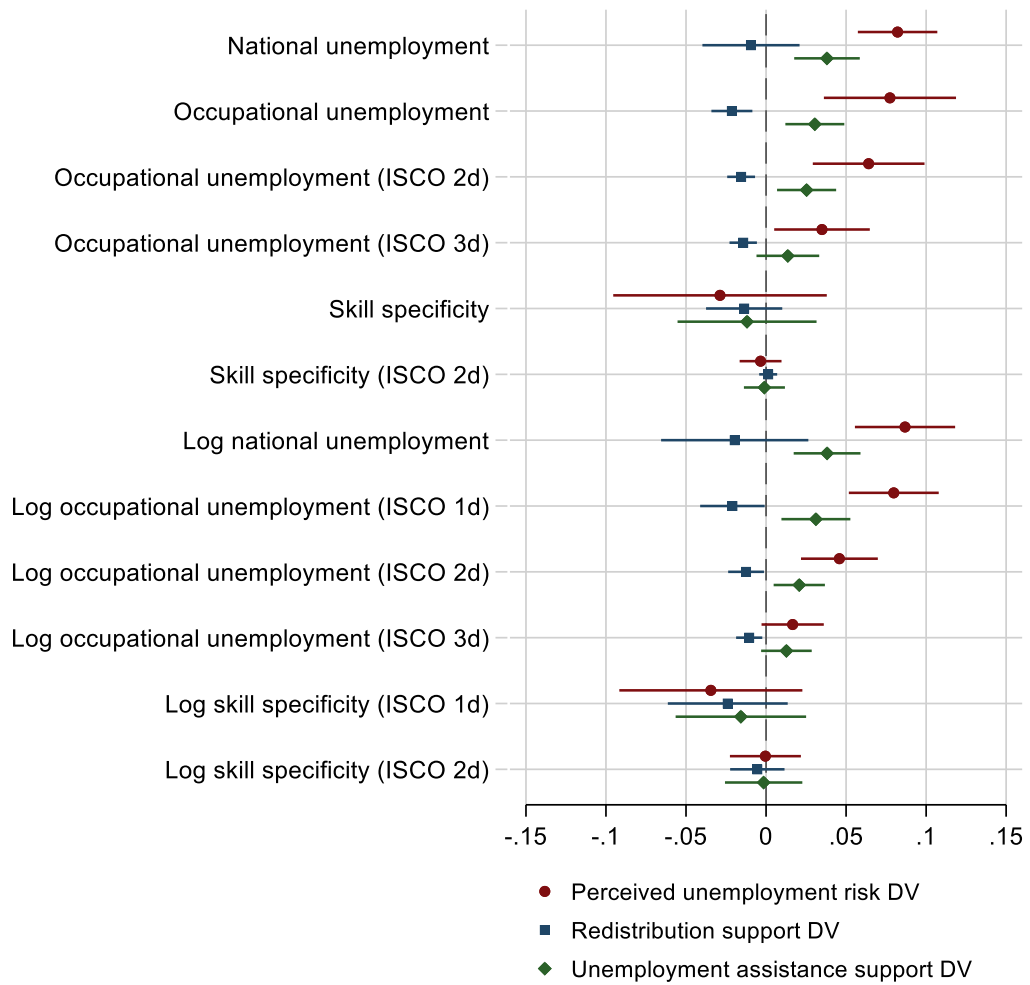
Note: Robust standard errors clustered by country-occupations in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

FIGURE A3.5: Replication of main analyses using ISCO 2-digit codes



Note: The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by country-occupations (ISCO88 2d). Country-occupation and time fixed effects as well as education years, household income quintiles, gender, age, and age squared are included as controls in all regressions. Occupational unemployment and skill specificity are both measured on the ISCO two-digit level.

FIGURE A3.6: Logged occupational risks and redistribution preferences



Note: The results are obtained from 36 separate regressions. That is, each displayed coefficient is obtained from a separate regression model. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by countries (when national unemployment rates are used as the independent variable) or by country-occupations (depending on the regression, ISCO88 one-, two, or three-digit codes). Country-occupation and time fixed effects as well as education years, household income quintiles, gender, age, and age squared are included as controls in all regressions.

Appendix D

TABLE A4.1: Descriptive statistics

	Mean	SD	Min	Max
Demand for redistribution	3.9	1.1	1	5
Fairness income				
Much less than just	0.2	0.4	0	1
A little less than just	0.39	0.49	0	1
About just	0.37	0.48	0	1
A little more than just	0.04	0.19	0	1
Much more than just	0.01	0.09	0	1
Importance wealthy family	2.9	1.2	1	5
Importance corruption	2.6	1.3	1	5
Need	3.4	1	1	5
Market performance	2.9	0.4	0.8	3.8
Egalitarianism	0.5	0.5	0	1
Income	2.9	1.4	1	5
Top-bottom placement	5.1	1.8	1	10
Age	47.6	15.9	16	98
Sex	1.5	0.5	1	2
Marital status	0.6	0.5	0	1
Education	2.9	1.4	0	5
Employment status				
Employed, full time	0.51	0.5	0	1
Employed, part time	0.10	0.3	0	1
Unemployed	0.07	0.25	0	1
Not in labor force	0.1	0.29	0	1
In education	0.02	0.16	0	1
Retired	0.21	0.4	0	1
Household members (log)	0.95	0.55	0	3.2

Note: The values are rounded. $N = 34,300$.

TABLE A4.2: Robustness tests

	(1)	(2)	(3)
<i>Fairness own income</i>			
Much less	0.323*** (0.024)	0.324*** (0.024)	0.357*** (0.025)
A little less	0.127*** (0.022)	0.128*** (0.022)	0.132*** (0.018)
About just		Reference	
A little more	0.006 (0.030)	0.005 (0.030)	-0.015 (0.039)
Much more	0.146** (0.069)	0.146** (0.069)	0.160** (0.079)
Importance wealthy family	0.037*** (0.010)	0.037*** (0.010)	0.051*** (0.008)
Importance corruption	0.076*** (0.010)	0.077*** (0.011)	0.086*** (0.007)
Need	0.180*** (0.022)	0.179*** (0.022)	
Market performance	0.037** (0.017)	0.038** (0.017)	
Egalitarianism	0.103** (0.043)	0.103** (0.043)	
Left-right ideology			-0.211*** (0.008)
<i>Income</i>			
Lowest		Reference	
Low	0.015 (0.024)	0.016 (0.024)	0.026 (0.026)
Medium	0.012 (0.027)	0.013 (0.027)	-0.008 (0.028)
High	-0.069** (0.035)	-0.068* (0.035)	-0.117*** (0.029)
Highest	-0.179*** (0.048)	-0.176*** (0.048)	-0.248*** (0.033)
Top-bottom self-placement	-0.063*** (0.006)	-0.063*** (0.006)	-0.065*** (0.006)
Age	0.002** (0.001)	0.002* (0.001)	0.001 (0.001)
Sex	0.073*** (0.021)	0.074*** (0.021)	0.104*** (0.016)
Married	-0.015 (0.023)	-0.014 (0.023)	-0.012 (0.020)
<i>Education</i>			
No education		Reference	
Lowest formal qualification	0.023 (0.042)	0.024 (0.041)	0.012 (0.048)
Above lowest qualification	0.060 (0.046)	0.061 (0.046)	0.003 (0.046)
Higher secondary completed	0.006 (0.047)	0.008 (0.047)	-0.104** (0.045)
Above higher secondary level	-0.042 (0.046)	-0.041 (0.046)	-0.165*** (0.047)
University degree	-0.081** (0.041)	-0.080** (0.041)	-0.210*** (0.047)
<i>Employment status</i>			

Table A4.2 (continued)

Employed, full time		Reference	
Employed, part time	0.039*	0.038	0.043
	(0.023)	(0.023)	(0.028)
Unemployed	0.023	0.022	0.032
	(0.030)	(0.030)	(0.036)
Not in labor force	0.012	0.011	0.037
	(0.034)	(0.034)	(0.030)
In education	0.007	0.007	0.004
	(0.032)	(0.032)	(0.049)
Retired	0.039*	0.041*	0.075***
	(0.022)	(0.022)	(0.028)
Household members (log)	0.031	0.030	0.051***
	(0.021)	(0.021)	(0.019)
Variance constant		0.13	0.13
		(0.03)	(0.03)
Standard errors	OIM	Cluster-robust	OIM
FE or RE	FE	RE	RE
Observations	34,300	34,300	19,550
Number of countries	39	39	34

Note: * <0.1, ** <0.05, *** <0.01. Standard errors in parentheses. Model 1 is an ordered probit model with country fixed effects. Model 2 is an ordered probit model with random effects and cluster-robust standard errors. Model 3 is an ordered probit model with random effects and model-based standard errors.

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Appendix E

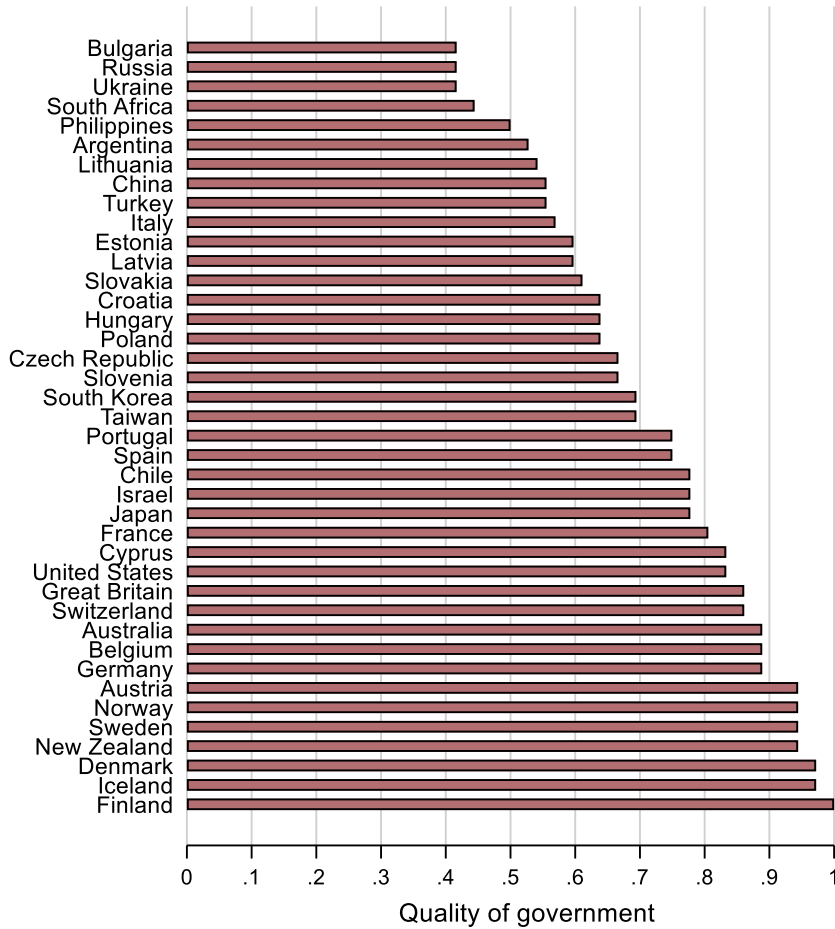
FULL LIST OF COUNTRIES INCLUDED IN THE ANALYSIS

Argentina, Australia, Austria, Belgium, Bulgaria, Chile, China, Taiwan, Croatia, Cyprus, Czech Republic, Denmark, Estonia, Finland, France, Germany, Great Britain, Hungary, Iceland, Israel, Italy, Japan, South Korea, Latvia, Lithuania, New Zealand, Norway, Philippines, Poland, Portugal, Russia, Slovakia, Slovenia, South Africa, Spain, Sweden, Switzerland, Turkey, Ukraine, and the United States.

Venezuela is excluded although data would be available in the ISSP data. The reason is that Venezuela in 2009 was an outlier that differed remarkably from the other countries. It had unusually low quality of government (a score of 0.2). Furthermore, scatterplots with linear fits suggest that Venezuela would have a strong impact on the overall results because the effect slopes of the drivers differed considerably from other countries with low quality of government. This may all be due to the fact that major political events unfolded in 2009 in Venezuela, with Hugo Chavez tightening his grip on power. However, note that including Venezuela in the analyses leads to substantially the same inferences.

DESCRIPTIVE STATISTICS

FIGURE A5.1: Distribution of government quality across countries



Note: The figure depicts the quality of government according to the International Country Risk Guide (ICRG) indicator by the PRS Group (2020).

DETAILS ON THE WEIGHTING PROCEDURE

Using unweighted data would mean that countries with larger sample sizes impact the overall results more strongly, implying that it is unclear regarding what entities inferences are drawn. All regressions are therefore weighted. Two weighting approaches appear reasonable. Firstly, regressions can be weighted so that inferences concern individuals, which is implemented by weighting according to population size (implying that larger countries affect the results proportional to their population size). The second approach is to weight each country so that it has the same overall impact on the results. I rely on this second approach because the quantity of interest, i.e., the effect size of drivers at different levels of government quality, concerns the country level. The weights are calculated with the following formula:

$$w_c = \frac{1/\text{Number of countries}}{\text{Observations for country } c / \text{Total observations}}$$

FULL REGRESSION RESULTS

TABLE A5.1: Main regressions – income equalization DV

	(1)	(2)	(3)
Household income	0.11 (0.072)	-0.11*** (0.018)	0.097 (0.071)
Perceived pay unfairness	0.11*** (0.016)	-0.094* (0.049)	-0.080* (0.047)
Household income * Quality of government	-0.33*** (0.095)		-0.31*** (0.092)
Household income * Quality of government		0.29*** (0.081)	0.27*** (0.078)
Male	-0.078*** (0.019)	-0.077*** (0.018)	-0.077*** (0.019)
Age	0.00088 (0.0021)	0.00012 (0.0021)	0.00087 (0.0021)
Age squared	5.2e-06 (0.000022)	0.000012 (0.000021)	5.1e-06 (0.000021)
Above lowest education	0.034 (0.021)	0.034* (0.020)	0.035* (0.021)
Higher secondary education	-0.055** (0.026)	-0.052** (0.025)	-0.052* (0.026)
Above higher secondary education	-0.12*** (0.028)	-0.11*** (0.028)	-0.12*** (0.028)
University education	-0.18*** (0.034)	-0.19*** (0.033)	-0.18*** (0.034)
Unemployed	-0.015 (0.026)	-0.018 (0.027)	-0.015 (0.026)
In education	-0.10*** (0.034)	-0.088** (0.033)	-0.10*** (0.034)
Not in labor force	-0.017 (0.017)	-0.013 (0.017)	-0.018 (0.016)
Constant	0.12** (0.061)	0.15** (0.062)	0.13** (0.061)
Country fixed effects	Yes	Yes	Yes
Observations	32,760	32,760	32,760

Note: Standard errors clustered by countries in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

TABLE A5.2: Main regressions – unemployment assistance DV

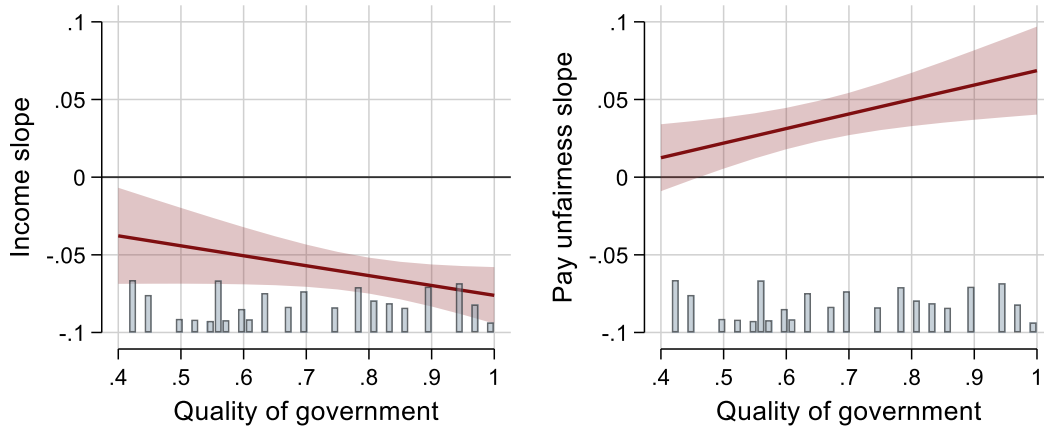
	(1)	(2)	(3)
Household income	0.022 (0.039)	-0.10*** (0.015)	0.018 (0.040)
Perceived pay unfairness	0.066*** (0.012)	-0.0077 (0.039)	0.00055 (0.040)
Household income * Quality of government	-0.18*** (0.059)		-0.17*** (0.060)
Household income * Quality of government		0.10* (0.056)	0.093 (0.057)
Male	-0.030* (0.015)	-0.029* (0.015)	-0.029* (0.015)
Age	0.0030 (0.0025)	0.0026 (0.0026)	0.0030 (0.0025)
Age squared	-0.000023 (0.000027)	-0.000020 (0.000028)	-0.000023 (0.000027)
Above lowest education	0.017 (0.027)	0.017 (0.027)	0.017 (0.027)
Higher secondary education	-0.042 (0.026)	-0.041 (0.027)	-0.042 (0.026)
Above higher secondary education	-0.11*** (0.031)	-0.11*** (0.031)	-0.11*** (0.031)
University education	-0.083** (0.039)	-0.085** (0.039)	-0.082** (0.039)
Unemployed	0.23*** (0.038)	0.23*** (0.038)	0.23*** (0.038)
In education	0.029 (0.034)	0.038 (0.035)	0.029 (0.034)
Not in labor force	0.065*** (0.020)	0.068*** (0.020)	0.064*** (0.020)
Constant	-0.049 (0.061)	-0.038 (0.061)	-0.047 (0.061)
Country fixed effects	Yes	Yes	Yes
Observations	32,828	32,828	32,828

Note: Standard errors clustered by countries in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

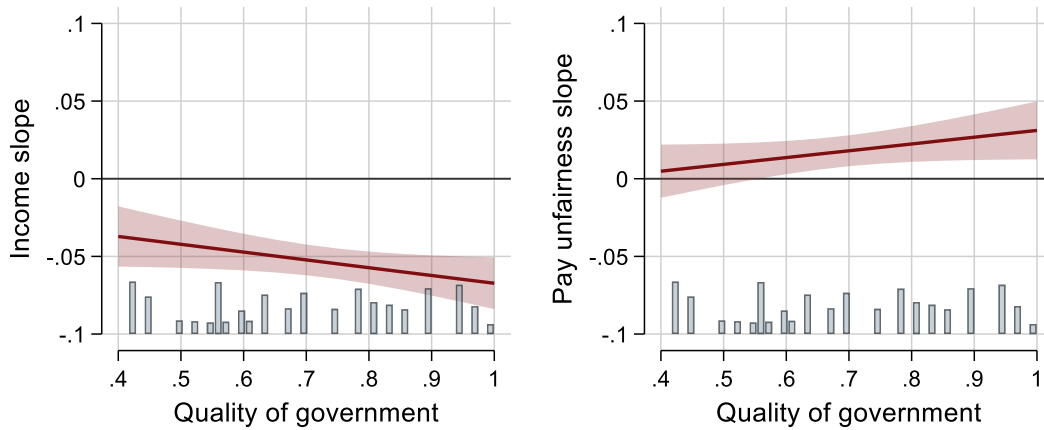
ROBUSTNESS CHECKS

FIGURE A5.2: Logit regression approach

Public income equalization preferences

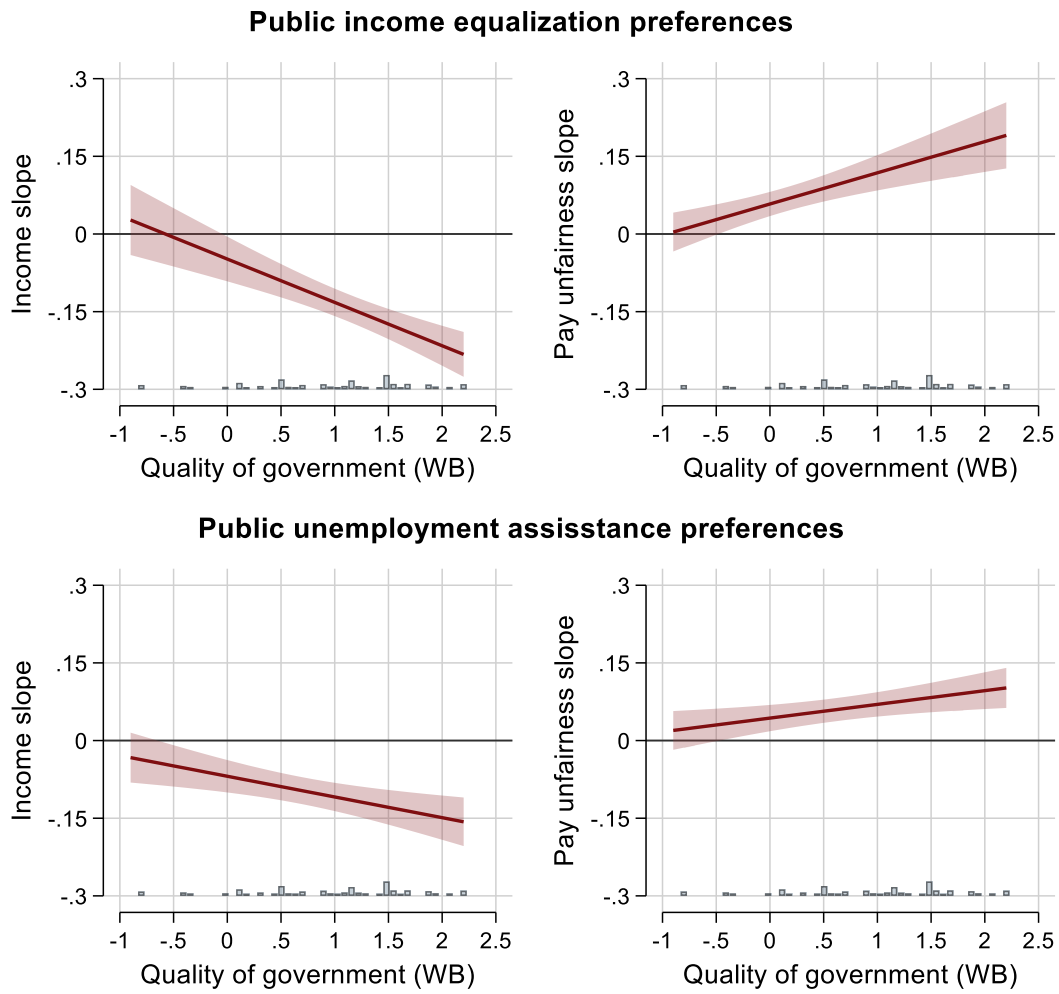


Public unemployment assistance preferences



Note: The figure depicts conditional marginal effects with 95% confidence intervals obtained from four logistical regression models using logit link functions. The dependent variables are dummies that indicate whether respondents either agree or strongly agree to public income equalization or unemployment assistance efforts. The solid lines indicate predicted percentage point changes to agree rather than to not agree to redistribution following a one standard deviation change of the respective independent variable at different values of government quality. The regressions hold gender, age, age squared, education, and employment status constant and also include country dummies.

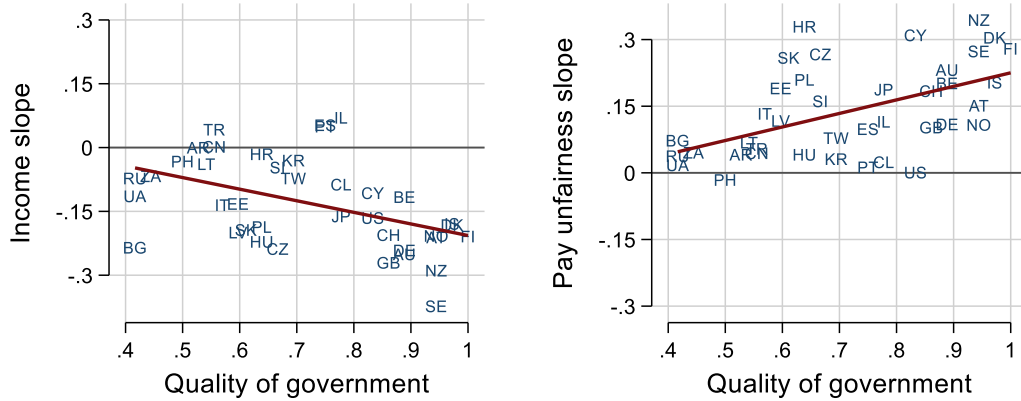
FIGURE A5.3: World bank quality of government indicator



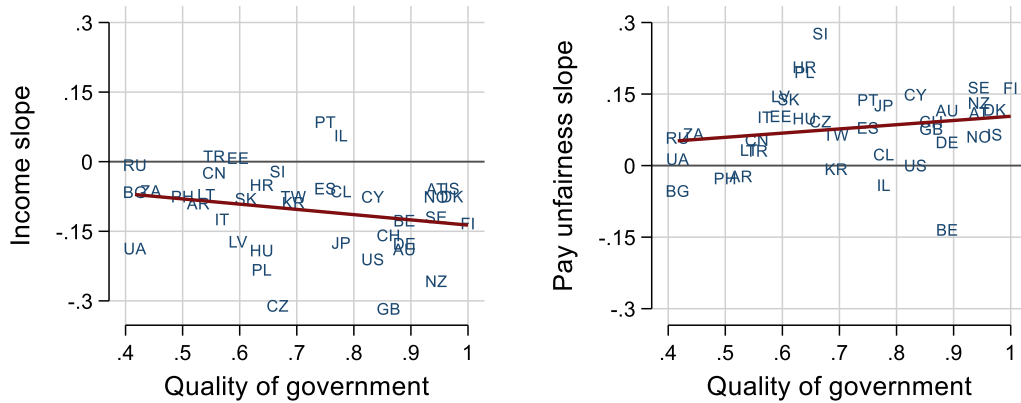
Note: The figure depicts conditional marginal effects with 95% confidence intervals obtained from four multiple regression models. The regressions use the World Bank’s (2021b) quality of government indicator instead of the ICRG indicator from the PRS group but have the same specification as the main regressions otherwise. The solid lines indicate the association between income/unfairness perceptions and redistribution preferences at different values of government quality, holding gender, age, education, and employment status constant.

FIGURE A5.4: Separate regressions for each country

Public income equalization preferences



Public unemployment assistance preferences



Note: The figure depicts scatterplots between quality of government and estimated slope coefficients for household income and perceived pay unfairness. The slope coefficients are obtained from separate OLS regression for each country and dependent variable, using either income equalization preferences (top panels) or unemployment assistance preferences (bottom panels). Each regression includes both household income and perceived pay unfairness as well as all controls as explanatory variables. The solid red lines represent linear fits between the countries' government quality and the slope coefficients.

TABLE A5.3: Information on additional country-level moderator variables

Variable	Source and comment
GDP per capita	Penn World Table 9.1 (Feenstra et al. 2015)
Labor share of GDP	Penn World Table 9.1 (Feenstra et al. 2015)
Unemployment rate	Modeled International Labor Organization (ILO) unemployment rate, sourced from the World Bank World Development Indicators (World Bank 2021a)
Market income Gini	Standardized World Income Inequality Database (SWIID) (Solt 2016)
Population over 65	World Bank World Development Indicators (World Bank 2021a)
Ethnic fractionalization	Fractionalization data from Alesina et al. (2003)
Religious fractionalization	Fractionalization data from Alesina et al. (2003)
Tax and transfer progressivity	Beramendi and Rehm (2016)
Social spending / GDP	Armington et al. (2019)
Democracy level	Freedom House Imputed ipolity2 score

TABLE A5.4: Additional cross-level interactions robustness test – income equalization DV

Conditional slopes of household income				
Additional interaction included between income and ...	at QoG = .4	at QoG = .6	at QoG = .8	at QoG = 1
GDP per capita	-0.084	-0.11***	-0.14***	-0.17***
Unemployment rate	-0.020	-0.086***	-0.15***	-0.22***
Market income Gini	-0.019	-0.086***	-0.15***	-0.22***
Labor share of GDP	-0.034	-0.093***	-0.15***	-0.21***
Population over 65	-0.066*	-0.11***	-0.16***	-0.20***
Ethnic fractionalization	-0.0025	-0.080***	-0.16***	-0.23***
Religious fractionalization	-0.011	-0.080***	-0.15***	-0.22***
Democracy level	-0.059	-0.10***	-0.15***	-0.19***
Social spending (% of GDP)	-0.049	-0.10***	-0.16***	-0.21***
Total tax and transfer progressivity	-0.085	-0.14***	-0.19***	-0.24***
Conditional slopes of pay unfairness perceptions				
Additional interaction included between pay unfairness and ...	at QoG = .4	at QoG = .6	at QoG = .8	at QoG = 1
GDP per capita	-0.0048	0.071***	0.15***	0.22***
Unemployment rate	0.021	0.079***	0.14***	0.20***
Market income Gini	0.021	0.079***	0.14***	0.20***
Labor share of GDP	0.038*	0.087***	0.14***	0.18***
Population over 65	0.044**	0.090***	0.14***	0.18***
Ethnic fractionalization	0.022	0.080***	0.14***	0.20***
Religious fractionalization	0.024	0.083***	0.14***	0.20***
Democracy level	0.032	0.084***	0.14***	0.19***
Social spending (% of GDP)	0.037*	0.088***	0.14***	0.19***
Total tax and transfer progressivity	0.048	0.096*	0.14***	0.19***

Note: Depicted are conditional marginal effects including 95% confidence intervals obtained from linear regressions (*<.1, **<.05, ***>.01). The marginal effects show how the two drivers covary with redistribution preferences at different values of quality of government. The results are obtained from regressions that include all independent variables (household income, pay unfairness) as well as two interactions terms: (a) between a selected driver such as income and quality of government, and (b) between a selected driver such as income and another country-level variable. All control variables are also included.

TABLE A5.5: Additional cross-level interactions robustness test – unemployment ass. DV

Conditional slopes of household income				
Additional interaction included between income and ...	at QoG = .4	at QoG = .6	at QoG = .8	at QoG = 1
GDP per capita	-0.075**	-0.095***	-0.12***	-0.14***
Unemployment rate	-0.052**	-0.087***	-0.12***	-0.16***
Market income Gini	-0.050**	-0.086***	-0.12***	-0.16***
Labor share of GDP	-0.051**	-0.086***	-0.12***	-0.16***
Population over 65	-0.071***	-0.097***	-0.12***	-0.15***
Ethnic fractionalization	-0.039	-0.081***	-0.12***	-0.17***
Religious fractionalization	-0.038*	-0.079***	-0.12***	-0.16***
Democracy level	-0.080***	-0.099***	-0.12***	-0.14***
Social spending (% of GDP)	-0.074***	-0.100***	-0.12***	-0.15***
Total tax and transfer progressivity	-0.29***	-0.23***	-0.17***	-0.11***
Conditional slopes of pay unfairness perceptions				
Additional interaction included between pay unfairness and ...	at QoG = .4	at QoG = .6	at QoG = .8	at QoG = 1
GDP per capita	0.018	0.050***	0.082***	0.11***
Unemployment rate	0.028	0.054***	0.079***	0.10***
Market income Gini	0.036**	0.056***	0.076***	0.096***
Labor share of GDP	0.056***	0.065***	0.074***	0.083***
Population over 65	0.055***	0.065***	0.074***	0.084***
Ethnic fractionalization	0.042*	0.058***	0.074***	0.091***
Religious fractionalization	0.036*	0.057***	0.079***	0.10***
Democracy level	0.046*	0.060***	0.074***	0.088***
Social spending (% of GDP)	0.046**	0.061***	0.076***	0.092***
Total tax and transfer progressivity	0.063	0.069*	0.075***	0.080***

Note: Depicted are conditional marginal effects including 95% confidence intervals obtained from linear regressions (* $<.1$, ** $<.05$, *** $>.01$). The marginal effects show how the two drivers covary with redistribution preferences at different values of quality of government. The results are obtained from regressions that include all independent variables (household income, pay unfairness) as well as two interaction terms: (a) between a selected driver such as income and quality of government, and (b) between a selected driver such as income and another country-level variable. All control variables are also included.

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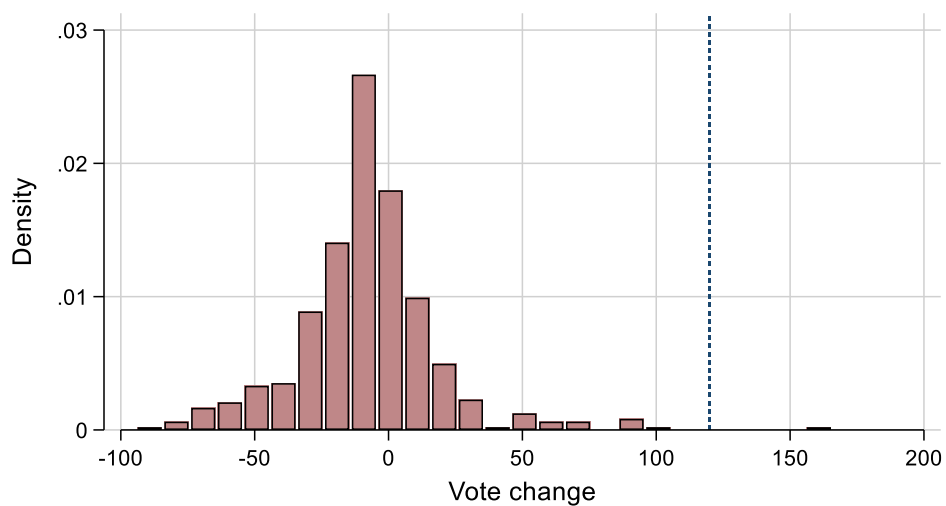
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Appendix F

INFORMATION ABOUT THE DATA

FIGURE A6.1: Excluded outlier values



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

TABLE A6.1: Descriptive statistics

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

FULL REGRESSION TABLES OF THE MAIN MODELS

TABLE A6.2: Effect of social spending changes

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

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

TABLE A6.3: Effect of social spending changes – interaction models

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

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

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TABLE A6.4: Effect of short-term social spending changes

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

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

TABLE A6.5: Effect of unemployment benefit generosity changes

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

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

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TABLE A6.6: Effect of unemployment benefit generosity changes – interaction models

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

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

TABLE A6.7: Effect of visible and short-term unemployment benefit generosity changes

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

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

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TABLE A6.8: Effect of pension generosity changes

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

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

TABLE A6.9: Effect of pension generosity changes – interaction models

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

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

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TABLE A6.10: Effect of visible and short-term pension generosity changes

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

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

TABLE A6.11: Effect of unemployment benefit reforms

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

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

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TABLE A6.12: Effect of unemployment benefit reforms – interaction models

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

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

TABLE A6.13: Effect of visible and short-term unemployment benefit reforms

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

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

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TABLE A6.14: Effect of pension reforms

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

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

TABLE A6.15: Effect of pension reforms – interaction models

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

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

APPENDIX F

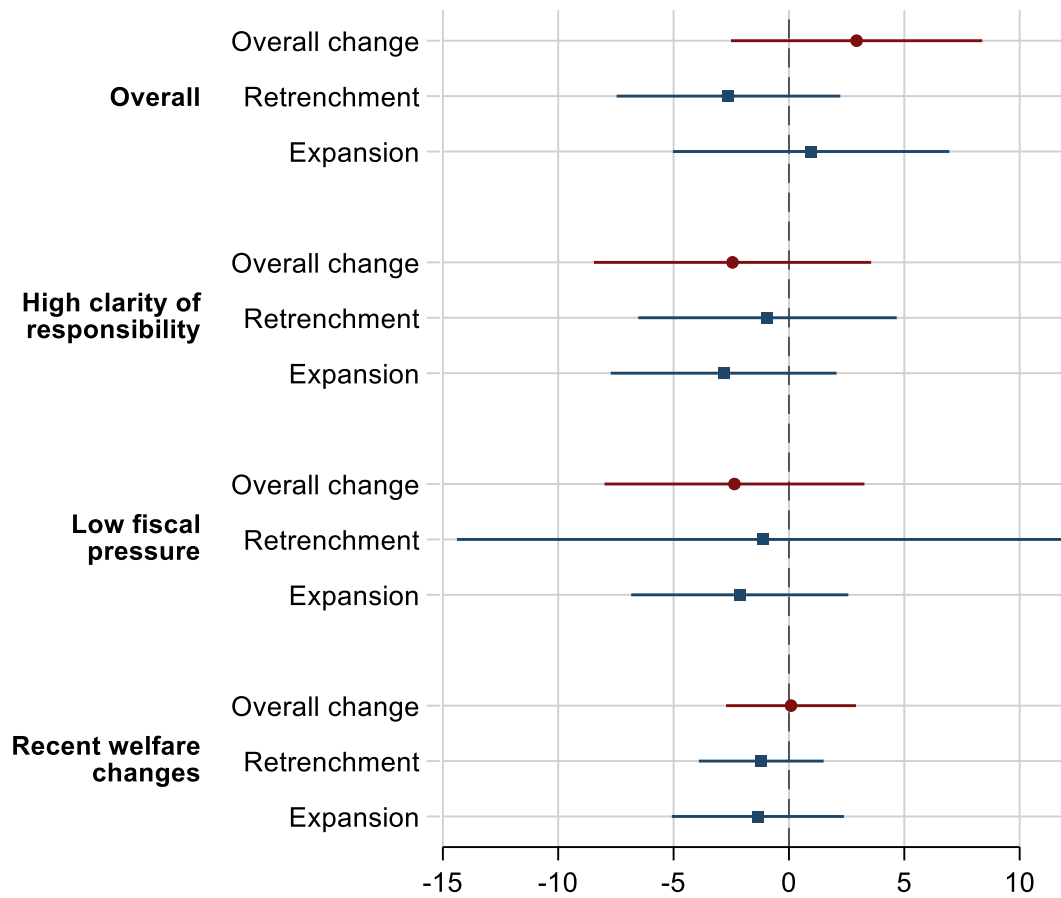
TABLE A6.16: Effect of visible and short-term pension reforms

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

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

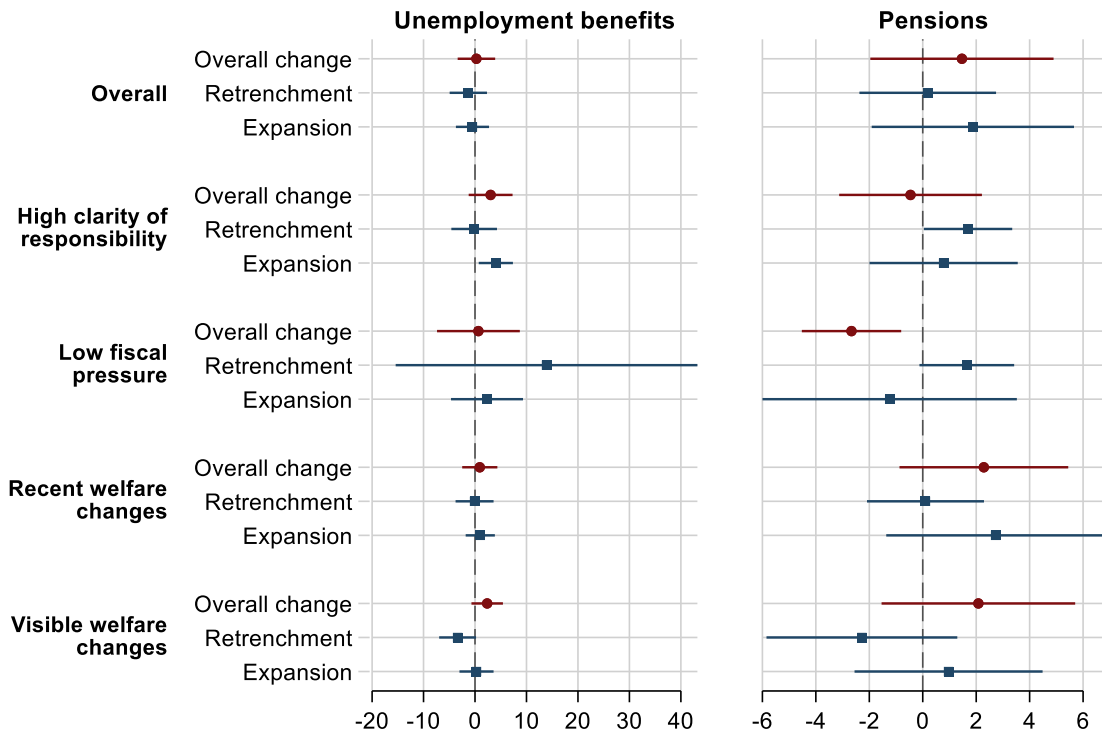
ROBUSTNESS TEST 1: SAMPLES RESTRICTED TO PRO-WELFARE PARTIES

FIGURE A6.2: The effect of social spending changes



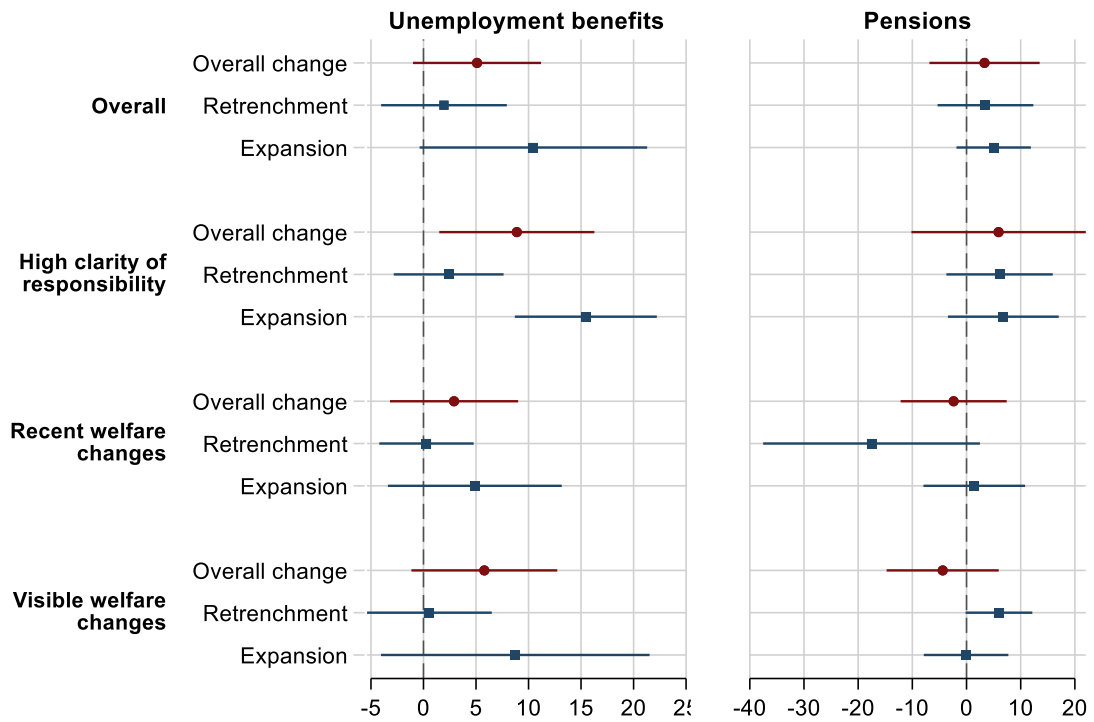
Note: All regressions include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties.

FIGURE A6.3: The effect of welfare generosity changes



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

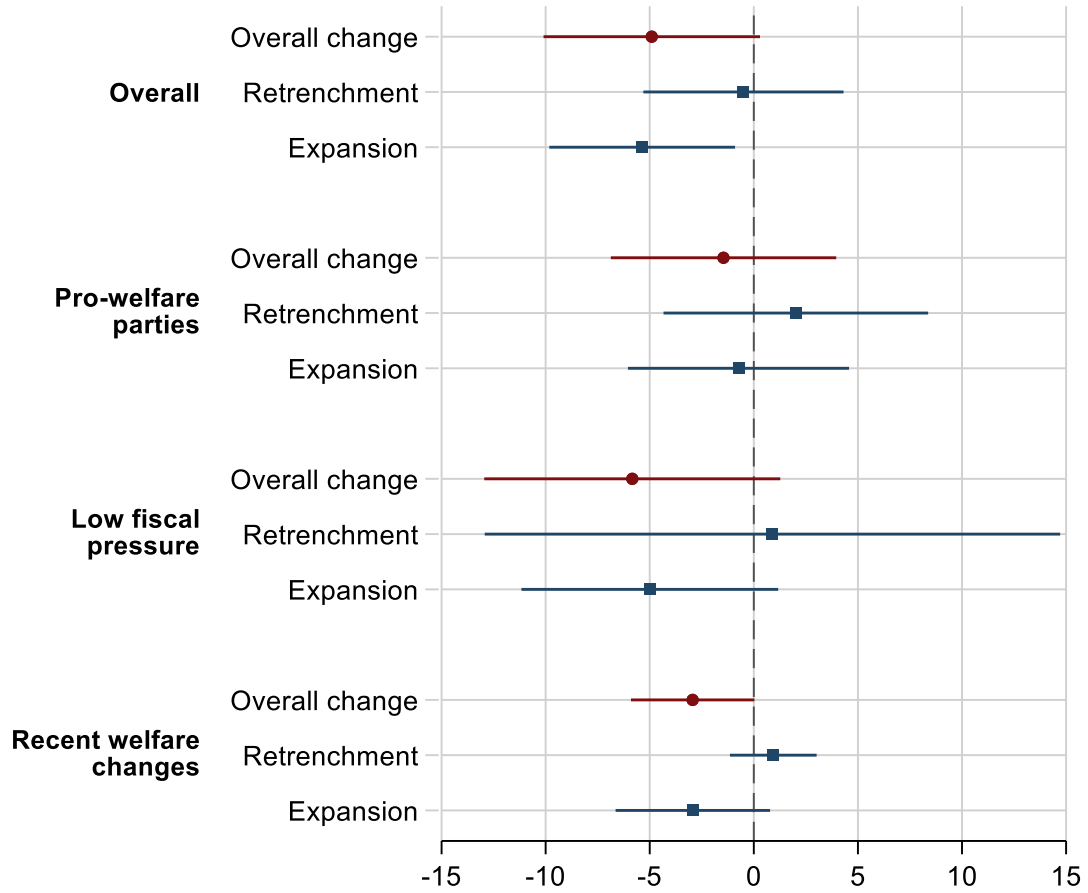
FIGURE A6.4: The effect of welfare reforms



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

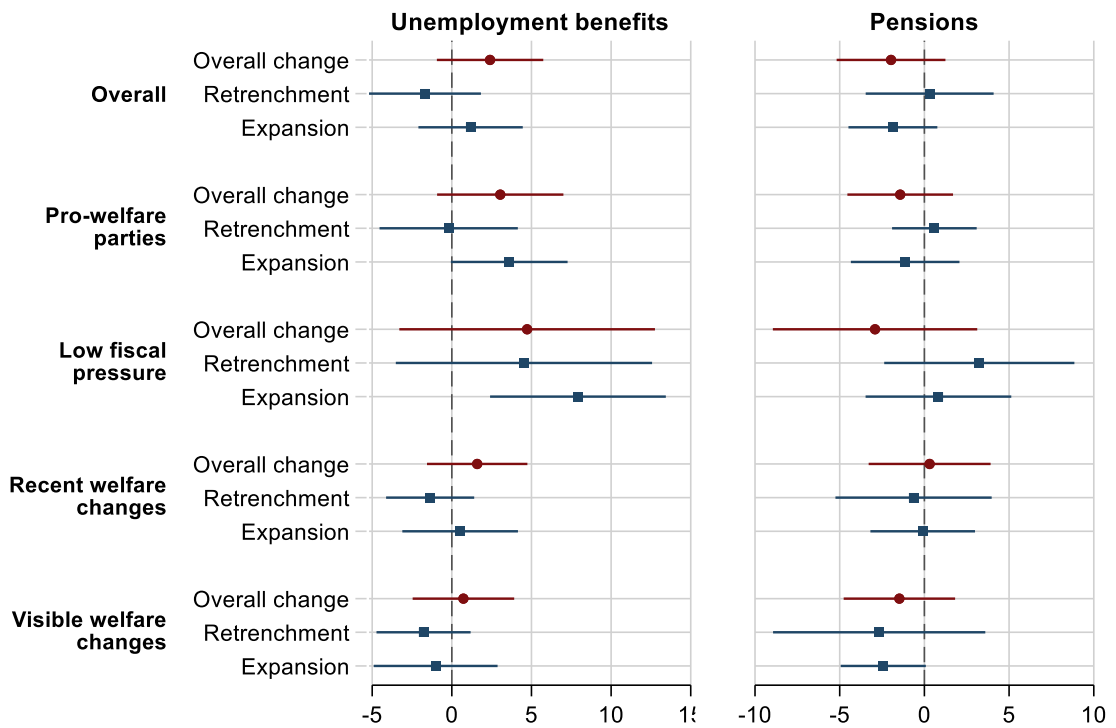
ROBUSTNESS TEST 2: SAMPLES RESTRICTED TO HIGH CLARITY OF RESPONSIBILITY GOVERNMENTS

FIGURE A6.5: The effect of social spending changes



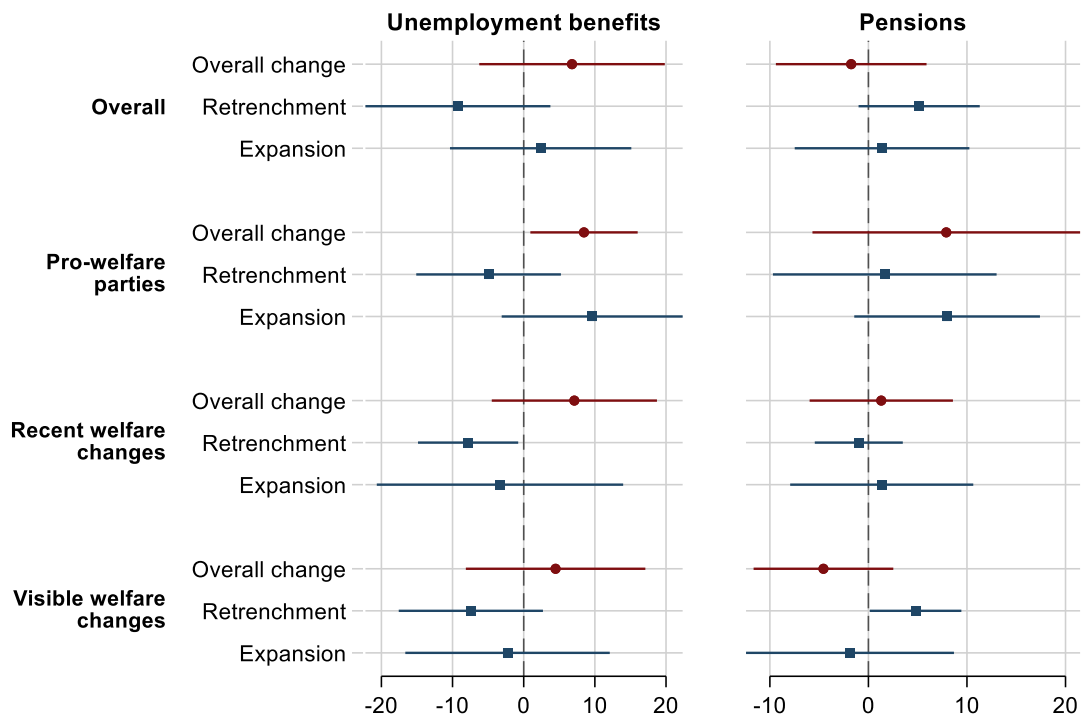
Note: All regressions include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties.

FIGURE A6.6: The effect of welfare generosity changes



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

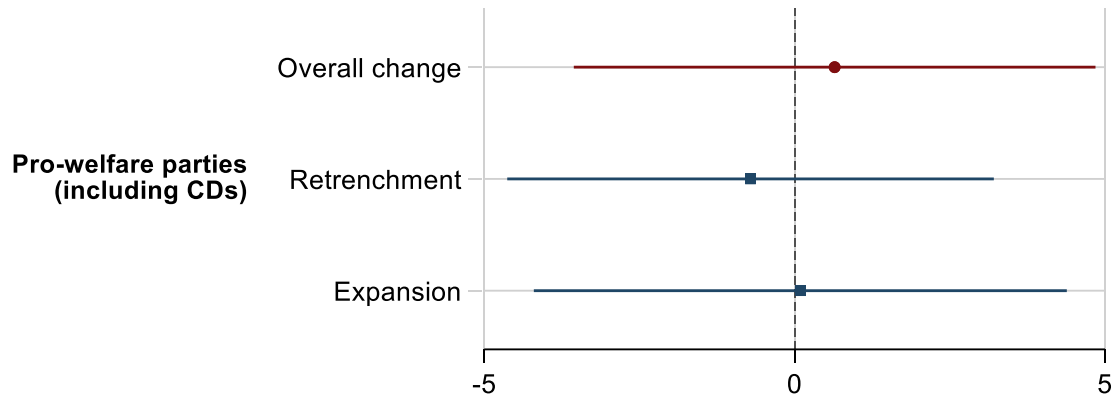
FIGURE A6.7: The effect of welfare reforms



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

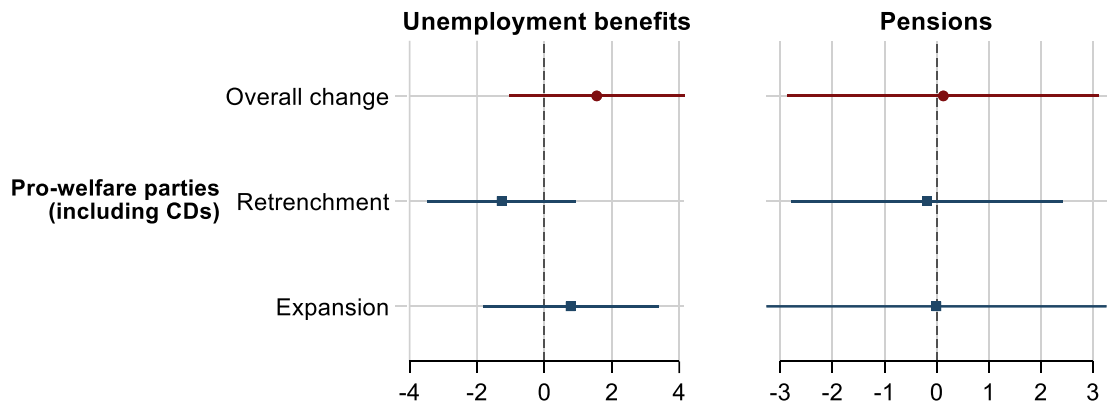
ROBUSTNESS TEST 3: DIFFERENT CONCEPTUALIZATION OF PRO-WELFARE PARTIES

FIGURE A6.8: The effect of social spending changes



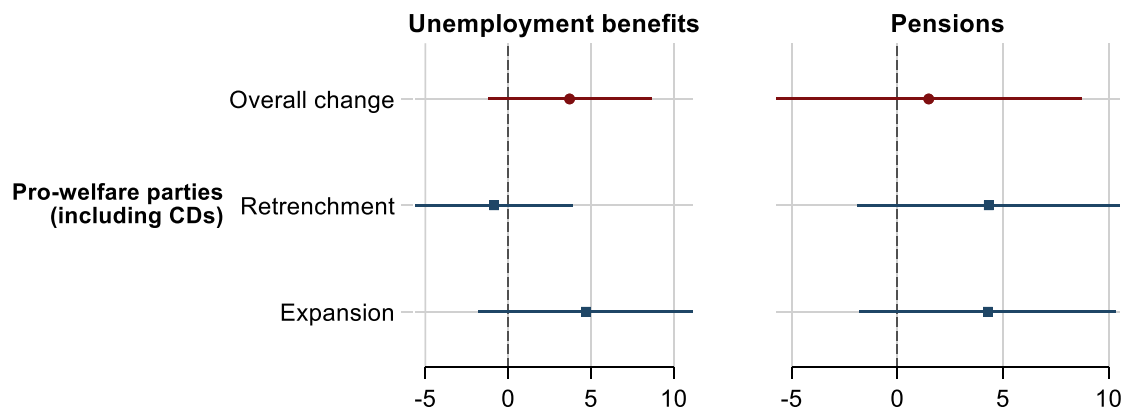
Note: All regressions include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties.

FIGURE A6.9: The effect of welfare generosity changes



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

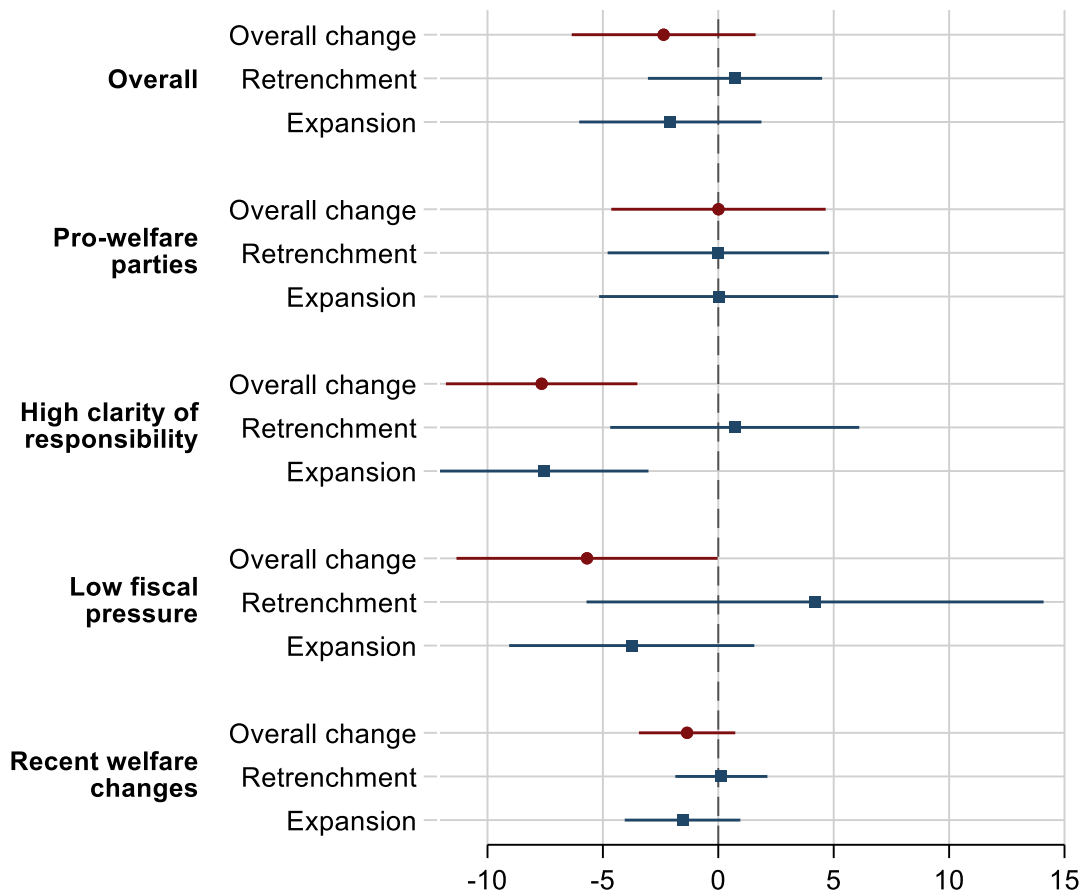
FIGURE A6.10: The effect of welfare reforms



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

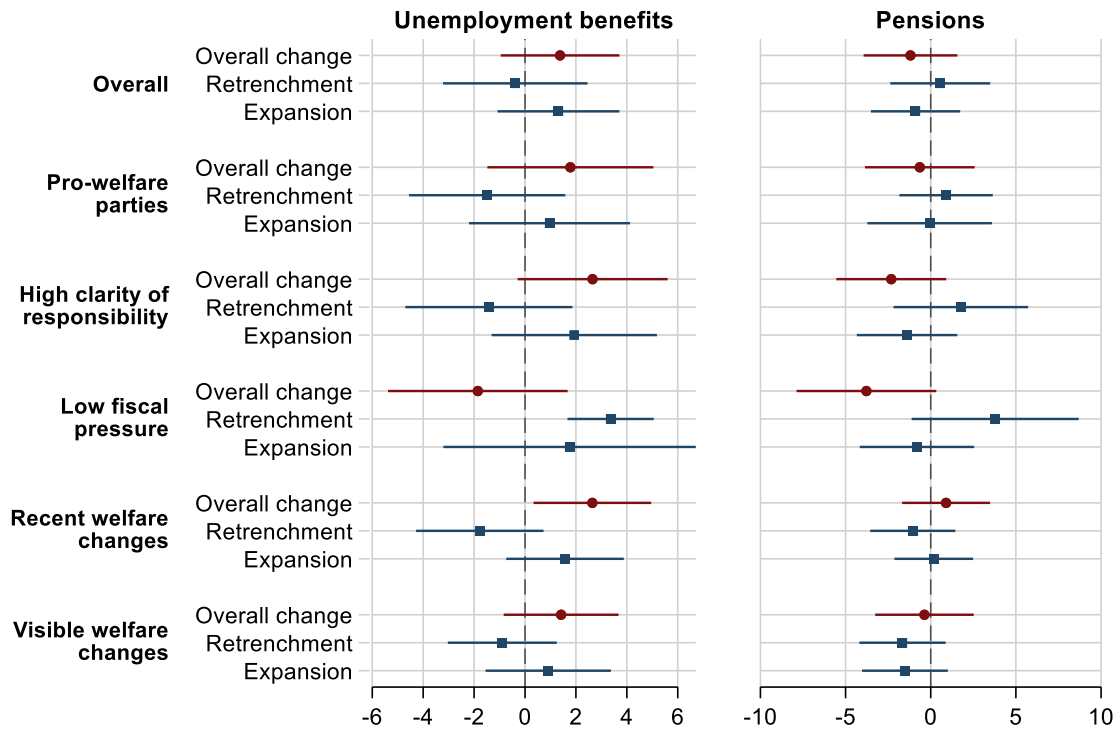
ROBUSTNESS TEST 4: YOUNG DEMOCRACIES EXCLUDED

FIGURE A6.11: The effect of social spending changes



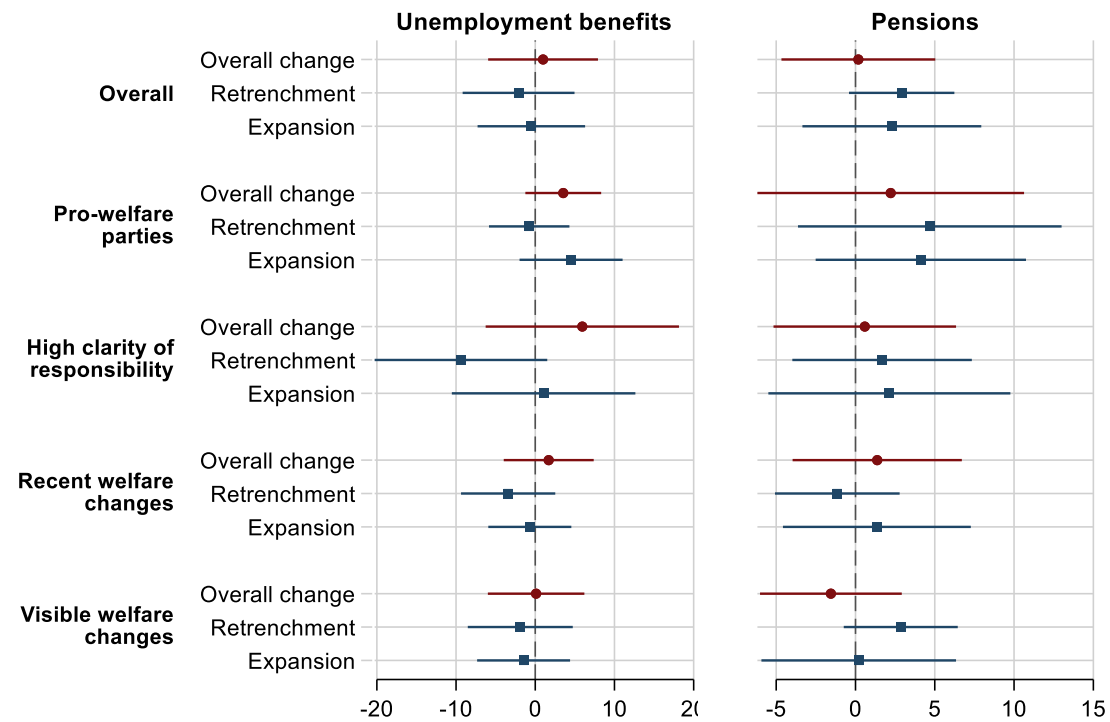
Note: All regressions include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties.

FIGURE A6.12: The effect of welfare generosity changes



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

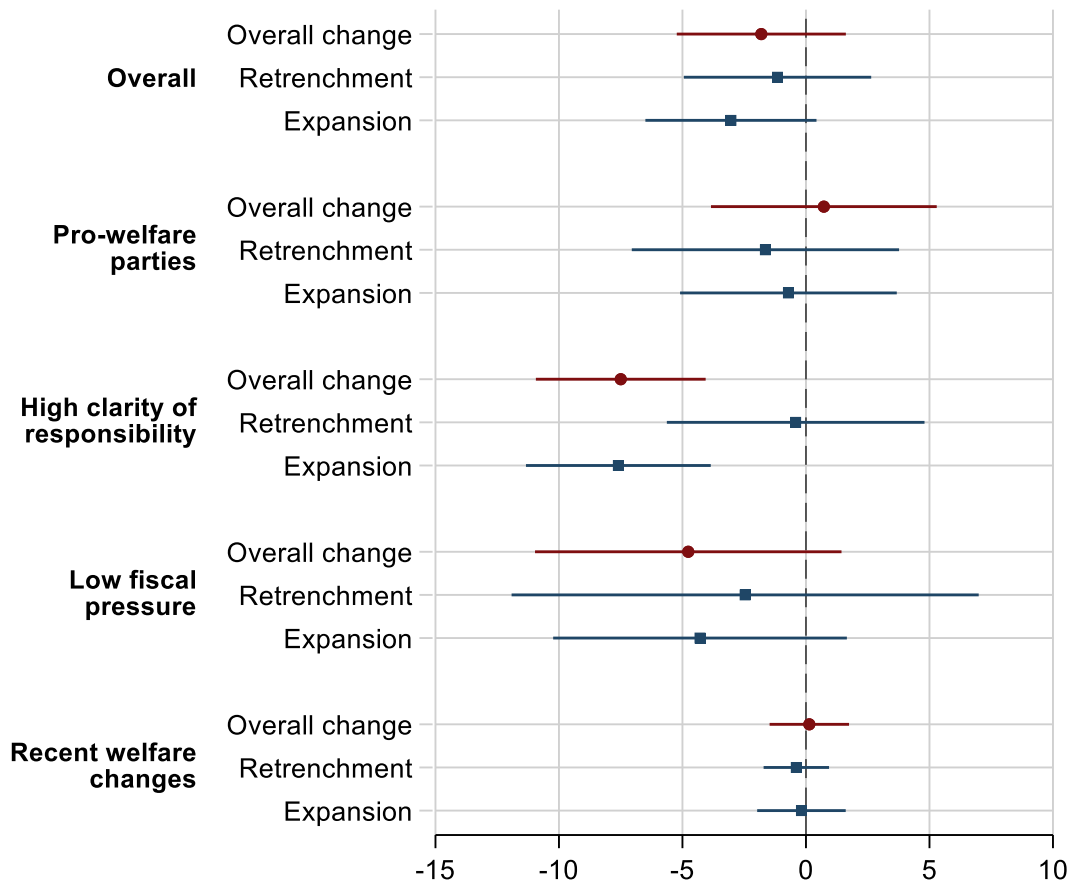
FIGURE A6.13: The effect of welfare reforms



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

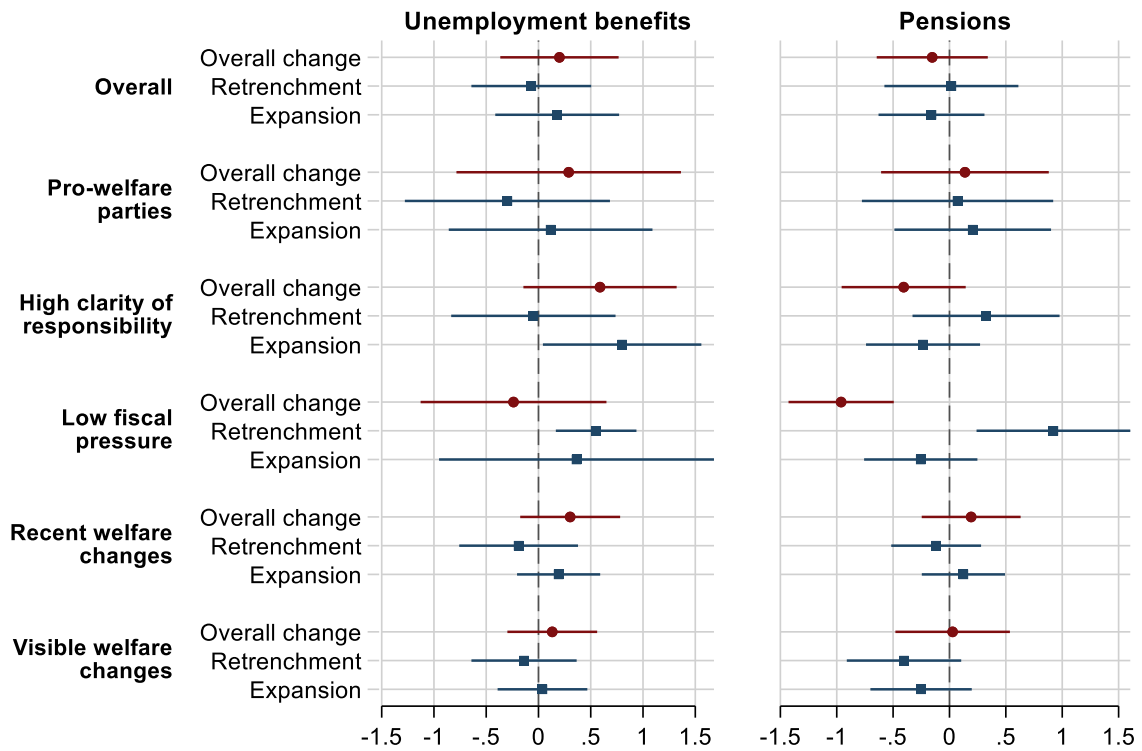
ROBUSTNESS TEST 5: NO CONTROL VARIABLES

FIGURE A6.14: The effect of social spending changes



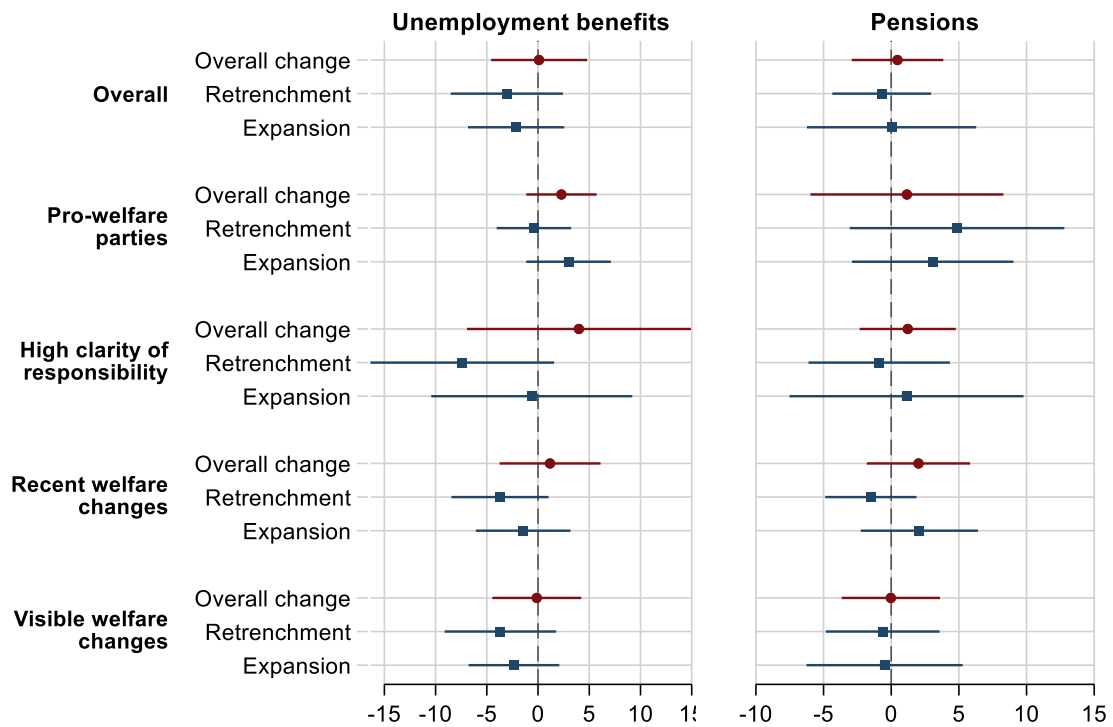
Note: All regressions include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties.

FIGURE A6.15: The effect of welfare generosity changes



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

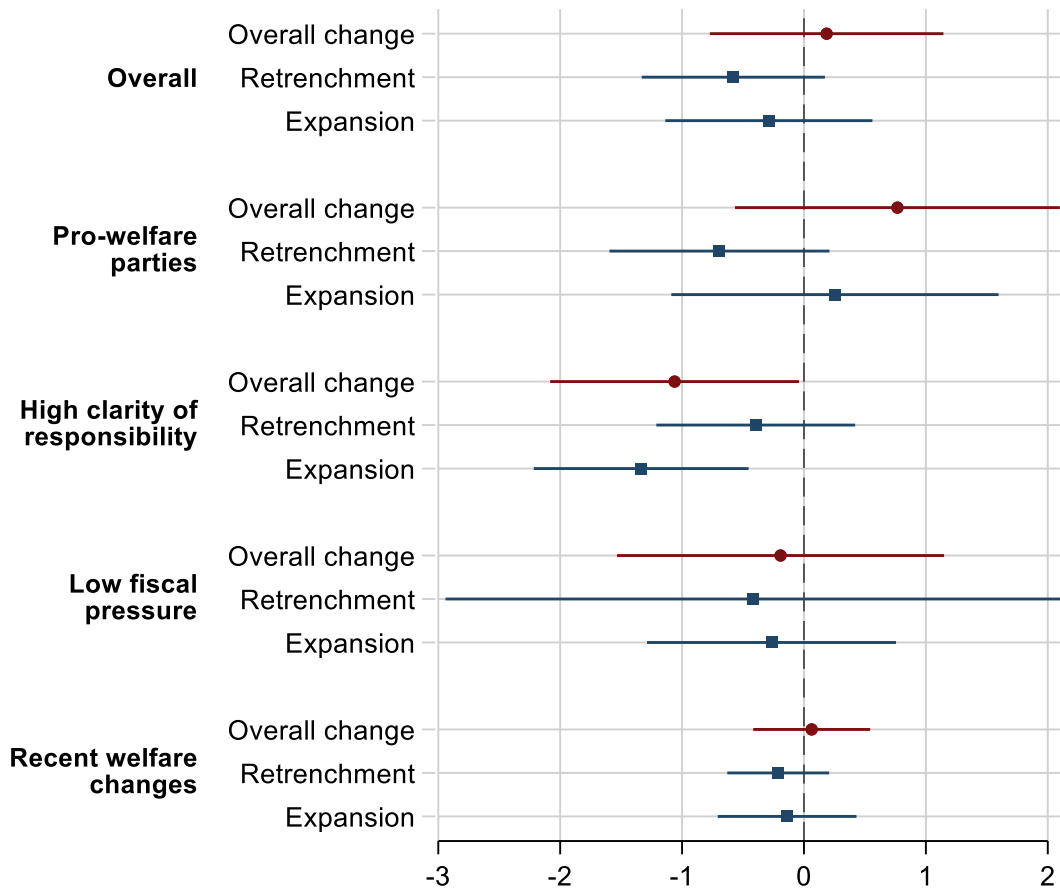
FIGURE A6.16: The effect of welfare reforms



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

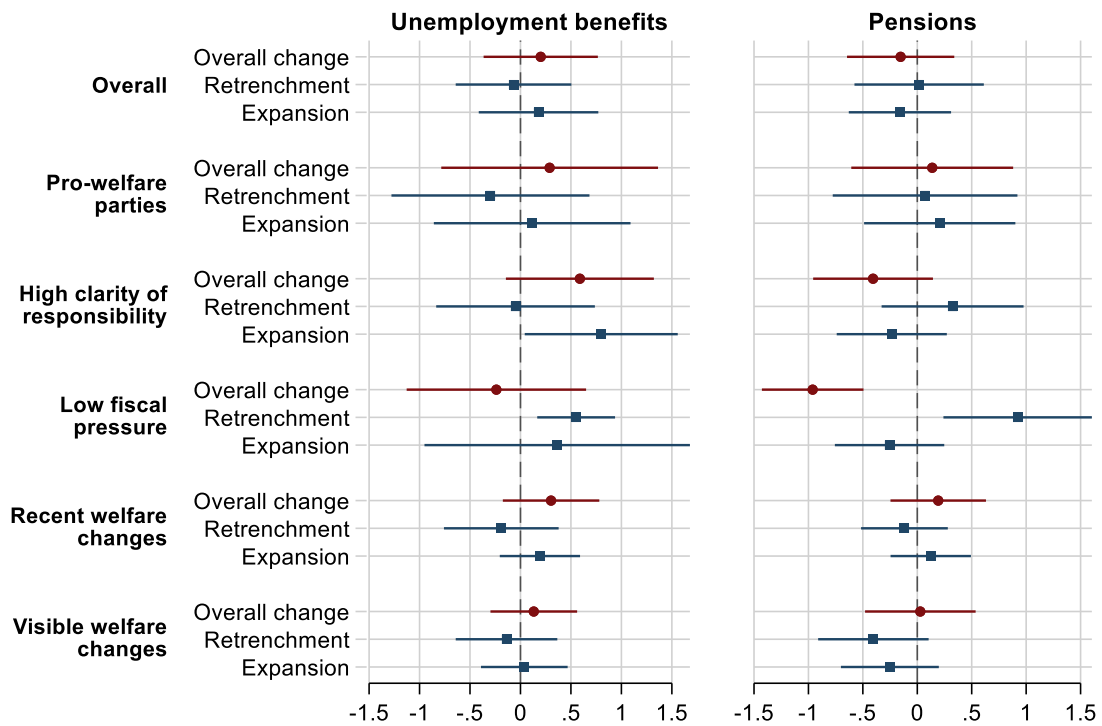
ROBUSTNESS TEST 6: DEPENDENT VARIABLE THAT MEASURES PERCENTAGE POINT CHANGES

FIGURE A6.17: The effect of social spending changes



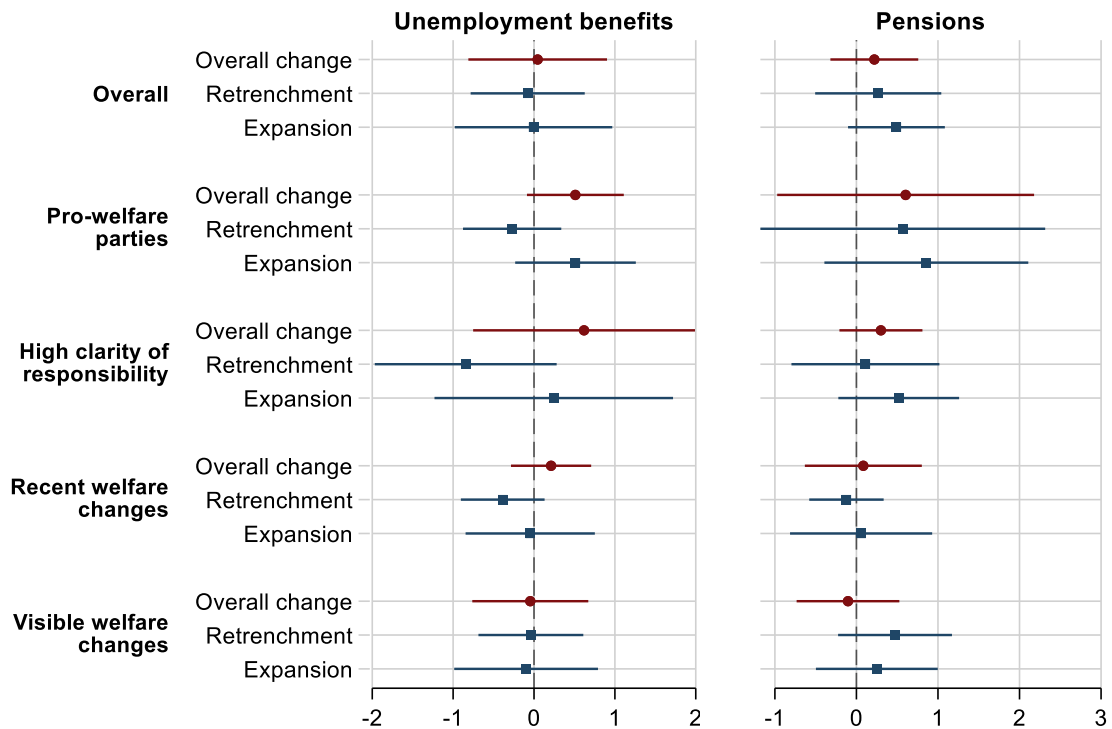
Note: All regressions include the full set of control variables. The horizontal bars represent 95% confidence intervals obtained from robust standard errors clustered by governments and parties.

FIGURE A6.18: The effect of welfare generosity changes



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

FIGURE A6.19: The effect of welfare reforms



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